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ARTICLE

Asymmetric adjustment of control

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Abstract

This study examines how principals adjust their control over agents based on their prior controlling experience. According to standard economic theory, principals should be equally willing to decrease their control as they are to increase it. However, I use psychological theory to predict that prior experience with exercising tight control reinforces a principal's belief that agents are self-interested and that they should be controlled. In contrast, I predict that the reinforcement of the belief that agents are socially interested and should not be controlled is weaker for principals who have prior experience with exercising loose control. I test my prediction using an experiment that exposes principals to either an increase or a decrease in the economic costs of control. The results support the predictions by exhibiting an asymmetric adjustment pattern. The data also show theory-consistent conditions under which the asymmetry in principals' control adjustments diminishes. Overall, my study suggests that prolonged experience with exercising high levels of control over agents may cause principals to hold on to their control disproportionately.

KEYWORDS

asymmetry, beliefs, control systems, controls, experiment, stickiness

Ajustement asymétrique du contrôle**Résumé**

Cette étude examine comment les mandants ajustent leur contrôle sur les mandataires en fonction de leur expérience antérieure en matière de contrôle. Selon la théorie économique standard, les mandants devraient être tout autant disposés à réduire leur contrôle qu'à l'accroître. Cependant, l'auteur utilise la théorie

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psychologique pour formuler l'hypothèse selon laquelle l'expérience antérieure de l'exercice d'un contrôle strict renforce la perception d'un mandant que les mandataires servent leur propre intérêt et qu'ils doivent être contrôlés. En revanche, l'auteur émet l'hypothèse selon laquelle le renforcement de la perception que les mandataires servent l'intérêt général et ne doivent pas être contrôlés est moindre pour les mandants qui ont déjà exercé un contrôle souple. L'auteur teste son hypothèse à l'aide d'une expérience qui expose les mandants à une augmentation ou à une diminution des coûts économiques du contrôle. Les résultats corroborent les hypothèses en présentant un modèle d'ajustement asymétrique. Les données indiquent également des conditions conformes à la théorie sous lesquelles l'asymétrie des ajustements de contrôle des mandants diminue. Globalement, cette étude suggère que l'expérience prolongée de l'exercice d'un contrôle renforcé sur les mandataires peut amener les mandants à conserver leur contrôle de manière disproportionnée.

MOTS-CLÉS

asymétrie, contrôles, expérience, perceptions, systèmes de contrôle, viscosité

1 | INTRODUCTION

Controls, such as monitoring, formal procedures, guidelines, and rules, are essential to ensure that agents undertake desirable rather than undesirable actions. Studies in the field of accounting have extensively researched the impact of controls on agent behavior, focusing on how agents make decisions in the presence of controls (Christ et al., 2008; Maas & Van Rinsum, 2013) and how they respond to changes in being controlled (Coletti et al., 2005; Emmett et al., 2019; Garrett et al., 2019; Tayler & Bloomfield, 2011). Although some studies have looked at the active role of principals in making decisions regarding controls (Cardinaels et al., 2022; Cardinaels & Yin, 2015; Evans et al., 1994), we lack knowledge about how principals adjust their control decisions over time and whether such adjustments align with predictions based on standard economic theory.

Understanding how principals adjust their control decisions is vital because the direct economic costs of control, also known as control costs, can change over time. Principals may encounter situations where control costs are not as favorable or unfavorable as they once were (Falk & Kosfeld, 2006; Jensen & Meckling, 1976, 1995). Anecdotal evidence suggests that technological advancements have caused control costs to decrease for some firms. For example, various applications and dashboards provide more cost-effective ways to implement stricter controls (Deloitte, 2021). However, there are also indications that control costs have increased due to the rise in demand for knowledge-based work (Kossek et al., 2021; Laber-Warren, 2022). Since knowledge-based work requires agents to work flexibly and at their own pace, there is typically a more significant efficiency loss for high than for low levels of control (Bartling et al., 2012; Walton, 1985).

Although principals experience changes in control costs, little is known about whether and how principals adjust their control in response to such changes, which is a gap I aim to fill with

this study. From a standard economic perspective, principals should adjust their control symmetrically when control costs change. Specifically, principals should be equally willing to increase control when control costs decrease, as they should be willing to decrease control when control costs increase. Furthermore, past experiences and interactions with former agents should have no bearing on their control adjustments, particularly when those experiences and interactions are uninformative about dealing with new agents under different circumstances.¹ If past experiences and interactions with former agents have little to no predictive power for how new agents respond to different control levels, principals should disregard these prior experiences and interactions when control costs change.

However, drawing from research in psychology, I predict that principals will not completely dissociate their past experiences and interactions with former agents from their control adjustments. Instead, I expect them to incorporate those experiences and interactions, resulting in asymmetry in how they adjust their control in response to changing control costs. My predictions are grounded in the well-established finding that different levels of control elicit distinct reactions from agents extending beyond the areas targeted by the control (Christ, 2013; Christ et al., 2008, 2012; Falk & Kosfeld, 2006; Fehr & Gächter, 2002; Tenbrunsel & Messick, 1999).² Specifically, when confronted with tight control, agents tend to adopt more self-interested responses, while loose control typically evokes more socially interested responses. Given these findings, I expect that principals develop beliefs informed by their past exposure to reactions from agents and that those beliefs are reinforced through experience. Moreover, I expect that principals' prior exposure to tight control and the resulting self-interested reactions of agents will yield more impactful and enduring beliefs than prior exposure to loose control and the resulting socially interested reactions.

Building on the literature about cognitive dissonance and information avoidance (Falk & Zimmermann, 2018; Festinger, 1957; Golman et al., 2017, 2022), I predict that this discrepancy in belief reinforcement will cause asymmetry in the willingness of principals to adjust their control over agents. Specifically, I expect that principals who have maintained high levels of control in the past will be relatively resistant to loosening control when related costs increase. As these principals have a reinforced belief that agents are self-interested and need to be controlled, they are more likely to resolve any cognitive dissonance on the topic by preserving their existing beliefs and ignoring information that contradicts or challenges it. Conversely, principals who have previously exercised lower levels of control should be relatively inclined to increase it when control costs decrease. These principals, who hold a more malleable belief that agents are socially interested and that tighter control is not necessary, find it relatively easy to revise this belief after learning about reduced control costs.

In addition to predicting asymmetry in how principals adjust their control in response to changes in control costs, I also predict when the asymmetry in principals' control adjustments weakens. Specifically, the asymmetry should be driven by the reinforced belief that agents are self-interested and need to be controlled among principals who have experience maintaining high levels of control. If this prediction is correct, then the strength of this reinforced belief will depend on the extent to which principals have prior experience maintaining high levels of

¹This study focuses on understanding how principals adjust their control decisions in response to changes in control costs under different agents. It does not examine how principals adjust their control decisions in response to changes in control costs under the same agent. Examining changes in control costs and principals' control adjustments in the context of managing different agents (rather than the same agent) reflects a realistic scenario because organizational changes and changes to employment modes are accompanied by agent turnover (Baron et al., 2001; Morrell et al., 2004).

²Although more control can elicit more compliant behavior from agents, it has also been shown to cause agents to use leftover discretion in a more self-interested manner or to retaliate in areas outside the scope of the controls (Falk & Kosfeld, 2006). Despite these negative effects, principals may still choose to control agents if control costs are low enough relative to the indirect costs generated by agents' reactions.

control over agents. Following research on belief formation and change (Hintzman, 1986; Wyer & Albarracin, 2005), I thus predict that less experience with exercising high levels of control should result in a weaker belief that agents are self-interested and need to be controlled.

To test these predictions, I conducted an experiment that assigned participants to the role of either principal or agent for a specified number of periods. At the start of each period, each principal was randomly and anonymously matched to an agent. Next, the principal-agent pairs played a control game in which the agent was responsible for distributing wealth between the agent and the principal. Before the agent decided on how to distribute the wealth, the principal had the option to implement a control to force the agent to allocate a minimum amount of wealth to the principal. However, implementing this control came at a direct cost to the principal (i.e., control costs), so the cost depended on the amount of control the principal chose to exercise. I manipulated whether control costs for principals decreased (from high to low) or increased (from low to high) at different points during the experiment. Initial exposure to low (high) control costs incentivizes principals to implement tight (loose) control over agents, which, in turn, should expose them to agents using their leftover discretion in a more self-interested (socially interested) fashion.

If the initial experience with control over agents does not asymmetrically influence how principals adjust their control, experiencing changes in control costs should lead them to adjust their control symmetrically. However, consistent with my psychology-based predictions, the data exhibit an asymmetric adjustment pattern in the predicted direction. Principals decrease their control less when control costs increase than they increase their control when control costs decrease. There is also support for the assumption that control has indirect costs for principals in addition to direct economic costs. Even in the relatively abstract setting of the experiment, agents positively (negatively) reciprocate the principals' decision to exercise lower (higher) control by using their leftover discretion in a more socially interested (self-interested) fashion.

My theory posits that the asymmetry in principals' control adjustments should be reduced when they have less prior experience with control decisions. To test this aspect of the theory, I manipulated the point in the experiment at which control costs changed: Some principals experienced earlier changes in control costs, while others experienced later changes. Furthermore, I restricted my analysis to a fixed number of periods after the change in control costs. By manipulating the timing of the control costs changes and keeping the number of periods after the changes fixed in my statistical analyses, I effectively varied principals' prior experience with control decisions under both high and low control costs. Although the results do not exhibit strong statistical significance, the consistent patterns found in the data align with the prediction that the asymmetric adjustment pattern weakens when principals experience the change in control costs earlier rather than later.

This study makes several contributions. First, it extends the control literature by showing that principals adjust their control decisions asymmetrically depending on how tightly they have controlled agents in the past. This finding offers a cautionary note for firms, regulators, and institutions; those responsible for control decisions may be quick to increase their control but may not reduce it with the same speed and determination. In addition, in contrast to the stylized conditions of my experiment, those responsible for controls in the real world may lack or even actively avoid information that would help them rethink their reinforced beliefs. As a result, introducing and increasing the strength of controls may lead to a control creep, where controls are progressively strengthened over time.³ Consistent with a control creep, it is frequently argued that the strength of controls has increased over time. Commonly cited reasons include stricter regulations (e.g., Sarbanes-Oxley Section 404) and lower control costs due to

³Specifically, a reinforced belief that agents should be controlled may not only cause the controlling parties to cling to their control but may also reinforce this belief further because it provokes even more self-interested reactions from agents. This "control creep" aligns with the so-called "self-fulfilling prophecy of mistrust" (Luhmann, 2014; Reuben et al., 2009).

advancements in information technology and data science (Labro & Stice-Lawrence, 2020). This study's results suggest that those responsible for designing control systems may have a hand in increasing the strength of controls due to disproportionate reinforcement of the belief that agents behave selfishly. To prevent this, firms and institutions may want to consider implementing time restrictions on those responsible for calibrating controls.

Second, this study contributes to the emerging experimental literature on the role of principals' profiles and prior experience. Although much of the experimental literature focuses on how controls affect agent behavior in terms of their benefits and (sometimes hidden) costs, recent work in management accounting has started to examine how principals' prior experience with the agent's decision or task influences their control choices. For instance, Cardinaels et al. (2022) find that principals without task-specific experience are more likely to offer incentive pay over a fixed wage than principals with task-specific experience. Another experimental study by Feichter (2022) shows that task-specific experience can negatively affect how principals set targets for agents. Specifically, it can lead to an "experience bias," with principals overemphasizing their own achievements when setting targets for agents. My experimental study adds to this research by examining how principals' prior controlling experience affects their flexibility in revising their control decisions.

2 | BACKGROUND AND HYPOTHESIS DEVELOPMENT

2.1 | Background

Controls, such as monitoring, formal procedures, guidelines, and rules, help ensure that agents' actions are in the principal's best interest. The primary drivers of control decisions in the traditional accounting and economic literature are control costs, which are direct economic costs that depend on the technology available to principals and how much flexibility the successful execution of agents' tasks requires. For instance, controlling agents' behavior and decisions requires buying and maintaining technology to measure and track their behavior and hold them accountable (Falk & Kosfeld, 2006; Jensen & Meckling, 1976, 1995). Also, when principals control the behavior and decisions of agents, they inherently limit the flexibility of those agents. Some decisions and tasks require agents to respond more flexibly to changing circumstances. When control reduces the flexibility of agents, those agents may respond less efficiently to changing circumstances (Bartling et al., 2012; Walton, 1985).

However, principals face additional costs besides control costs when implementing controls. Behavioral research in economics, psychology, and accounting suggests that controls may provoke adverse reactions from agents in areas that are outside the scope of the enacted controls. In addition to compliance effects, experimental evidence confirms that agents may exhibit more self-interested behavior in response to being controlled (Christ et al., 2008; Falk & Kosfeld, 2006). The behavioral literature discusses three interrelated reasons for these indirect costs of control: the use of controls signals distrust, restricts an agent's autonomy, and implicitly communicates that principals expect agents to exhibit self-interested behavior if given the opportunity (Bartling et al., 2012; Cardinaels & Yin, 2015; Christ et al., 2008; Falk & Kosfeld, 2006; Tenbrunsel & Messick, 1999). As a result, being controlled may cause agents to feel mistreated, leading them to react accordingly.

Principals sometimes do not anticipate these indirect costs (i.e., they are "hidden"), leading them to implement stricter control even though their payoffs could be higher with looser control over the agents. For instance, Evans et al. (1994) designed an experiment in which principals chose between restricting agents' discretion (tight control), which has a lower expected payoff, and expanding agents' discretion (loose control), which has a higher expected payoff. They find that, even if control reduces the principal's welfare, some principals still prefer to restrict agents'

discretion and hold them accountable. In another influential study by Falk and Kosfeld (2006), some principals controlled agents in their experiment even though they could have earned more by trusting agents, suggesting that these principals failed to comprehend that control can provoke adverse reactions from agents.

2.2 | Hypotheses

In this study, I develop and test a theory of how principals adjust their control over agents when control costs change. Standard economic reasoning suggests principals should adjust their control over agents symmetrically when control costs change. That is, they should increase their control when control costs decrease and decrease their control when control costs increase by the same degree. Moreover, principals should ignore past experiences and interactions with former agents when those experiences and interactions are uninformative about dealing with new agents under different control costs. Assuming the beliefs principals have formed based on past experiences and interactions with former agents, they have little to no predictive power for how new agents will react to different control levels warranted by the changes in the cost of control. Principals should discard their prior beliefs and form new ones when control costs change.

Contrary to this standard economic reasoning, I predict that principals will incorporate their past experiences with agents into their control adjustments, even when they are uninformative. Building on research from psychology, I expect a non-trivial role for the beliefs that principals formed before the change in control costs. Beliefs are cognitive acts or states in which a proposition is taken to be true and are one of the building blocks of conscious thought (Egan, 1986). People adopt beliefs to categorize others into social groups and to understand their relationship with that social group (McGarty et al., 2002). Moreover, they adopt and revise those beliefs based on facts, observations, and prior experience (Bandura, 1965; Gosen & Washbush, 2004; Kolb, 2014; Manz & Sims, 1981).

Prior experience with control decisions and agents' reactions are key determinants of principals' beliefs about whether agents should be controlled. When principals exercise tight control due to relatively low control costs, they put more pressure on agents to undertake desirable actions. However, tight control may provoke more adverse reactions from agents in areas extending beyond the implemented control's scope. When principals have witnessed such reactions, they are more likely to form the belief that agents are self-interested and require tight control. In contrast, when principals have exercised loose control due to relatively high control costs, they have compelled agents to undertake desirable actions to a lesser degree. This may have elicited more socially interested reactions from agents outside the implemented control's scope. Principals who have observed this behavior are more likely to form a belief that agents are socially interested and require little control.

Although I predict that past experiences with and prior beliefs about agents impact how principals adjust their control in response to changing control costs, I expect prior beliefs about self-interested agents to be particularly impactful. Principals who have exercised tight control are more likely to have been exposed to more self-interested reactions from agents, creating more negative events. In contrast, principals who have exercised loose control are more likely to have been exposed to more socially interested agent reactions, creating more positive events. Research in psychology suggests that negative events impact people more than positive events (Baumeister et al., 2001; Fiske, 1980; Smith & Trope, 2006). Due to their survival instincts, people are more inclined to keep track of events that negatively affect their well-being. Based on this research, I predict that (the more negative) self-interested agent reactions have a more substantial impact on principals' beliefs about agents than (the more positive) socially interested agent reactions. In other words, the reinforcement of beliefs about agents based on their

responses should be stronger for principals who have implemented tight control than principals who have implemented loose control.

Dating back to Festinger (1957), psychologists have long recognized that people dislike exposure to situations and information that conflicts with powerful, reinforced beliefs. People may maintain strong beliefs despite being confronted with situations and information that suggest acting against them (Bénabou & Tirole, 2016; Festinger, 1957; Golman et al., 2017, 2022).⁴ For instance, research on managerial decision-making finds that managers avoid exposing themselves to information that conflicts with their initial decisions (Schulz-Hardt et al., 2000). Also, research in finance shows that due to cognitive dissonance, investors disregard earnings news that contradicts their sentiment, causing muted announcement date price reactions to such news (Li et al., 2023).

Based on this collection of psychology research, I expect principals who have exercised tight control in the past to be relatively reluctant to decrease their control over agents when control costs increase. These principals will find it challenging to revise their reinforced belief that agents are self-interested and must be controlled. Therefore, they will resolve their cognitive dissonance by maintaining their reinforced belief and ignoring or avoiding information that contradicts or suggests acting against it (Golman et al., 2017, 2022; Hart et al., 2009; Sullivan, 2009). In contrast, principals with experience exercising loose control will be relatively inclined to increase their control when control costs decrease. That is, these principals will revise their more malleable belief that agents are socially interested and that they do not require control. They will resolve any cognitive dissonance by acting on information that contradicts their existing belief and suggests acting against it. Based on these theoretical arguments, the first hypothesis predicts asymmetry in principals' control adjustments: the increase in control when control costs decrease will be proportionally larger than the decrease in control when control costs increase.

Hypothesis 1 (H1). Principals are less likely to decrease control over agents when control costs increase than to increase control over agents when control costs decrease.

H1 predicts that principals asymmetrically adjust their control over agents when control costs change, but this effect will depend on the extent to which principals reinforce rather than revise their beliefs about the self-interest of agents. In a review of psychology research on belief formation and change, Wyer and Albarracin (2005) identify a critical factor predicting the retrieval and use of belief-relevant knowledge. Specifically, the extensiveness of people's experiences contributes to the strength of their adopted beliefs and the likelihood that they will revise these beliefs. According to Wyer and Albarracin (2005), belief adoption and reinforcement are not driven by the number of experiences alone. Instead, having multiple similar experiences increases the likelihood that people will connect the experiences to form a more general and resilient schema about the nature of the world in which they operate (Hintzman, 1986).

Following the literature on belief formation and change, I expect that the asymmetry in principals' control adjustments is affected by the extent of principals' past experiences with control decisions. Specifically, fewer past experiences with exercising tight control over agents will result in weaker disproportionate reinforcement of the belief that agents are self-interested and require tight control. In contrast, more past experiences with exercising tight control over agents will result in stronger disproportionate reinforcement of this belief. Based on these theoretical arguments, I formulate a second hypothesis predicting there will be weaker asymmetry in principals' control adjustments when principals have less prior experience with control decisions.

⁴Other psychological theories that explain how people may refuse to act on new information that contradicts their beliefs are conceptual conservatism (Nissani, 1990, 1994), belief perseverance (Anderson, 2007), confirmation bias (Nickerson, 1998), and motivated reasoning (Kunda, 1990).

Hypothesis 2 (H2). The asymmetry in the likelihood of principals adjusting control over agents is weaker when principals have less prior experience with control decisions.

3 | EXPERIMENT DESIGN

To test my hypotheses, I conducted an experiment in which principal-agent pairs played a control game for multiple periods. The first manipulation varied whether principals experienced an increase or decrease in control costs (low to high or high to low). In this way, the experiment not only confronted principals with either a decrease or increase in control costs but also facilitated the formation of different initial beliefs among principals. It did so by exposing principals to different amounts of initial control costs and incentivizing them to exercise varying initial levels of control. The second manipulation varied the timing of the change in control costs. By letting some principals experience the change in control costs sooner than later, my experiment effectively varied the extensiveness of their initial experience with control decisions. The primary dependent variable of interest is the difference in average control after and before principals experience the change in control costs.

3.1 | Experimental setting

Participants interacted for 12 periods and were randomly assigned to the fixed role of either principal or agent. At the beginning of each period, the experimental software assigned participants a different random, anonymous partner and informed them about this pairing procedure. Principal-agent pairs played a control game in each period, which was different from but inspired by other games that involve principal-agent interactions (Bartling et al., 2012; Evans et al., 1994; Falk & Kosfeld, 2006) and other games in which actors control others' decision-space (List, 2007). Both principals and agents had access to what happened to themselves in previous periods but not what happened to their randomly matched partners.

In the control game, the agent is responsible for distributing 20 points. In the absence of control, they are free to distribute the points as they see fit as long as they allocate at least 5 points and not more than 15 points to each party. However, before the agent distributes the points, the principal can choose whether and how much to control the agent's decision. Specifically, they can adjust the level of control along a continuum ranging from 0.00 (i.e., no control) to 1.00 (i.e., full control). Increasing the level of control means enforcing the principal's most profitable point distribution (i.e., 15 points for the principal and 5 points for the agent) and restricting the agent's discretion (i.e., the points the agent can still distribute). However, increasing the level of control is costly for the principal. Depending on how much control the principal exercises, the principal incurs a direct economic cost (hereafter, control costs). Thus, as the principal increases control, the principal not only enforces more of their most profitable point distribution but also increases the control costs.

Table 1 shows how the principal's and the agent's payoffs are calculated in each period as a function of their decisions. The principal chooses parameter a , which represents the level of control, and the agent chooses parameters b_1 and b_2 , which represent their preferred point distribution (subject to $15 \geq b_i \geq 5$ and $b_1 + b_2 = 20$). When the principal exercises complete control over the agent ($a = 1.00$), they incur control costs c . Also, under complete control, the agent has no discretion and cannot choose b_1 and b_2 . Instead, the agent automatically gives the principal 15 out of the 20 points while keeping 5 points. When the principal exercises no control ($a = 0.00$), the principal incurs no control costs, and the agent can distribute all 20 points the way they prefer by choosing parameters b_1 and b_2 . Accordingly, no control ($a = 0.00$) means the agent's preferred point distribution exactly equals the outcome of the control game.

TABLE 1 Principal and agent payoffs.

Role	Payoff
Principal	$a \cdot (15 - c) + (1 - a) \cdot b_1$
Agent	$a \cdot 5 + (1 - a) \cdot b_2$

Note: This table displays the agent’s and principal’s period payoffs. The principal first observes control costs c , which equals 1 or 9 points. Next, the principal chooses a , which is the level of control they would like to exercise, ranging from 0.00 to 1.00. If the principal chooses $0.00 \leq a < 1.00$, the agent chooses b_1 (b_2), which should satisfy $15 \geq b_1 \geq 5$ and $b_1 + b_2 = 20$.

When the principal chooses “some” control (i.e., $0.00 < a < 1.00$), the agent can still express a preferred point distribution by choosing b_1 points for the principal and b_2 points for the agent along a continuum ranging from 5 to 15 points; the sum must equal 20 points. However, the resulting actual point distribution does not equal the agent’s preferred point distribution because it is adjusted by how strongly the principal enforces their most profitable point distribution. Specifically, under “some” control (i.e., $0.00 < a < 1.00$), the principal enforces a minimum point distribution of $a \cdot 15$ points for the principal and $a \cdot 5$ points for the agent at control costs $a \cdot c$ points for the principal. Thus, under “some” control (i.e., $0.00 < a < 1.00$), the agent has leftover discretion to distribute $(1 - a) \cdot 20$ remaining points. Specifically, by choosing b_1 and b_2 , the agent will give $(1 - a) \cdot b_1$ points to the principal and keep $(1 - a) \cdot b_2$ points. Accordingly, the total number of points distributed to the principal is $a \cdot (15 - c) + (1 - a) \cdot b_1$ and to the agent is $a \cdot 5 + (1 - a) \cdot b_2$.

In the experiment, control costs c equal 9 points (hereafter, high control costs) or 1 point (hereafter, low control costs). High control costs are fixed to a maximum of 9 points to ensure that lowering control can only be driven by a principal’s belief that the agent will respond in a socially interested way. Specifically, if control costs are 10 points or higher, controlling the agent is not an economically meaningful option for the principal, and the principal may lower control purely out of self-interest without believing that the agent will respond in a socially interested fashion. Thus, when control costs equal 9 points or lower, only believing that the agent will use discretion in a socially interested way will incentivize the principal to lower control. Second, the minimum control costs are fixed at 1 point because it is the lowest possible integer that ensures the principal still incurs control costs. This way, varying low and high control costs is not confounded by varying whether or not the principal incurs control costs.

Since playing the control game is a non-trivial task, principals and agents did not have to make payoff calculations themselves or analyze large outcome tables. Instead, the experimental software automatically updated the expected payoffs while principals and agents indicated their decisions, such that both received real-time feedback on the consequences of their current selected decisions on their and their partner’s payoffs. In addition to observing each other’s expected payoffs, principals and agents also observed control costs c before making final decisions. Appendix S1 includes examples of how both expected payoffs were updated depending on the selected choice by principals and agents and the information available to them.⁵

3.2 | Experimental manipulations

3.2.1 | The direction of the change in control costs

I test H1 by manipulating whether principals experience an increase or decrease in control costs. For this purpose, about half of the principals initially experienced high control costs (i.e., $c = 9$), while the remainder initially experienced low control costs (i.e., $c = 1$). Initial exposure to high control costs induces principals to exercise low control over agents. Low control,

⁵See Appendix S1 in the Supporting Information.

in turn, will grant agents more discretion and should elicit more socially interested agent responses. To examine how principals adjust their level of control, I introduce a change in control costs in one of the periods (i.e., \hat{t}) and manipulated the direction of this change. Specifically, about half of the principals experienced a decrease in control costs from high (i.e., $c=9$) to low (i.e., $c=1$). In contrast, the remainder experienced an increase in control costs from low (i.e., $c=1$) to high (i.e., $c=9$). Therefore, the average control costs across all periods are constant across the two groups of principals.

3.2.2 | The timing of the change in control costs

I test H2 by varying when principals experience a change in control costs. Specifically, the period in which principals experience the change in control costs, period \hat{t} , varies gradually from period 4 to period 10. Furthermore, I keep the number of periods after the change in control costs constant by restricting my statistical analysis to three post-change periods. Thus, while the number of periods before the change in control costs varies due to this timing manipulation, I keep the number of periods after the change in control costs constant at three periods in the statistical analysis for H2. This way, I only test for the effect of principals' prior experience with control decisions without simultaneously testing for the effect of principals' subsequent experience. Together with the first manipulation, this second manipulation is akin to a moderation-of-process design (Asay et al., 2022). Specifically, manipulating the direction of the change in control costs tests the theoretical process of interest, while varying principals' prior experience with control decisions moderates that theoretical process. Such an experimental design enhances confidence in the causal role of the direction manipulation because the predicted interaction provides insight into the process of interest. Figure 1 shows both experimental manipulations.

Besides being a valid test for H2, manipulating the timing of the change in control costs also has an ancillary internal validity benefit. Together with the random-matching procedure, varying the timing of the change in control costs helps prevent agents from directly observing the change in control costs that principals experience. Therefore, these two design decisions reduce the likelihood that principals consider how agents will respond to the change in control costs principals experienced and principals' adjustment behavior. That is, varying the timing of the change in control costs helps randomly match agents to principals who may or may not have experienced a change in control costs. Similarly, principals are randomly matched to agents who have either been exposed to principals with low or high control costs.⁶

A setting where new agents accompany control adjustments is required to provide a clean test of my theory and hypotheses. If agents were to observe the change in control costs directly and if principals were to consider that agents observe how they respond to the change in control costs, principals would adapt their adjustment behavior based on how they expect agents to respond to their control adjustments. This would reflect a setting where agents are part of the principals' control adjustments. Given recent insights by Emmett et al. (2019), there is good reason to avoid this setting, who find evidence that agents respond differently to decreases than increases in control.

3.3 | Experimental procedures

I used oTree, a Python-based, open-source software platform for survey and experimental research, to program the experiment's software (Chen et al., 2016). Appendix S1 displays

⁶Although I do not perfectly vary the timing of the change in control costs from start to finish, varying the timing of the change in control costs from period 4 to period 10 means any asymmetry in control adjustments can be attributed to the direction of the change in control costs and not to principals' considerations about how agents will respond in the remaining periods.

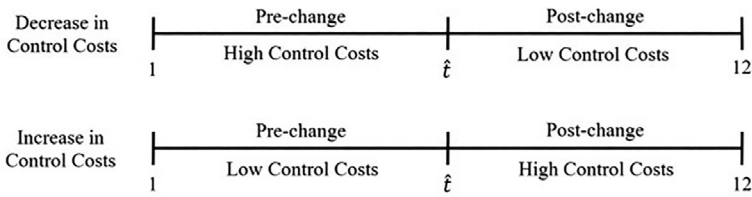


FIGURE 1 Direction and timing of the change in control costs. This figure presents a visualization of the two manipulations, that is, the direction and the timing of the change in control costs for the principal. About half of the principals experienced a decrease in control costs from high to low, while the remainder experienced an increase in control costs from low to high. The timing of the change in control costs, that is, period \hat{t} , varies randomly from periods 4 through 10.

several screenshots. I conducted the experiment at a research institute for economics and management at a large North-Western European university. Before conducting the experiment, I obtained formal approval from the research institute, including the evaluation of my research proposal and the key features of my experimental design.

From period 2 onward, participants had access to historical records of their prior experiences but not those of their matched partners. Participants could click on a button to review those historical records whenever they make decisions. Their historical records contain information about control costs, the principal's control decision, the principal's and agent's payoffs, and, when the principal did not exercise full control, the agent's preferred point distribution (b_1 and b_2). Before the experiment, participants also received instructions with quiz questions, and both principals and agents learned that control costs could not be lower than 1 point or higher than 9 points. Although control costs are observable to principals and agents before they make decisions in each period, the sequence of control costs realizations and the period in which principals experience the change in control costs are not explicitly disclosed to participants before the experiment.

3.4 | Participants

I recruited business and economics students from the same large North-Western European university to participate in the experiment. Business and economics students are an appropriate participant pool because their profile fits with the relatively abstract setting of the experiment (Libby et al., 2002). The experiment lasted approximately 45 min, and the number of participants in each experimental session ranged from 20 to 24 and was always a multiple of two. As an incentive to participate, participants received a modest amount of course credit (up to 5% of their total grade, depending on their educational track).⁷ In addition to this incentive, participants earned money based on how many points they earned during the experiment. Specifically, participants were paid €0.60 for every 10 points. Payouts ranged from €4.00 to €11.99, with an average rate of €6.60 for 45 min (an average of €8.80 per hour).

The raw sample consists of 100 principals and 100 agents. However, nine principals were excluded from the sample because they failed at least one of the four attention checks in the ex post questionnaire.⁸ This leaves a final sample of 91 principals and 100 agents. Given the relatively abstract nature of the experiment, the post-experimental questionnaire included three

⁷Participants were free to not participate in the experiment and could still receive course credit by doing an alternative assignment.

⁸These nine principals answered at least one of the following four attention checks incorrectly: (1) "In each period, I chose the value for a ," (2) "What best describes the value of c across the periods?" (3) "I was Player 2," and (4) "In each period, I chose the values for b_1 and b_2 ." Note that none of the agents failed their attention checks. Including these nine principals in my sample leads to inferentially equivalent results.

items to determine the perceived difficulty of the experiment.⁹ Since these three items show acceptable internal consistency ($\alpha = 0.768$), I created an average score based on all three (i.e., *Difficulty*). The average *Difficulty* reported by the sample is 2.422, which is significantly lower than the neutral midpoint of 4 ($t[190] = 17.029$, two-tailed p -value < 0.001)¹⁰ Thus, the participants report that the experiment was relatively clear and understandable.

4 | RESULTS

4.1 | Descriptive statistics

The data reveal that principals exercise less than full control 55% of the time (599 out of 1,092 times), enabling agents to exercise discretion over how points are distributed between them and their principals. Accordingly, principals do not exclusively expect agents to use discretion selfishly. The data also suggest that agents do not behave purely selfishly. First, agents gave their principals above the minimum 5 points 34% of the time they received discretion over how points were distributed (201 out of 599 times). Second, agents gave their principals more points when they exercised less control ($\rho = -0.1994$, two-tailed p -value < 0.001).

Table 2 presents the descriptive results for the two decisions in the experiment, split by the stage of the experiment and the direction of the change in control costs. First, it presents *Control*, which is the level of control exercised by the principal in a period, ranging from 0.00 to 1.00 (i.e., a). Second, it presents *Agent Contribution*, which is the portion of the agent's preferred point distribution allocated to the principal corrected for the minimum required allocation to principal wealth (i.e., $[b_1 - 5]/10$). Table 2 shows that principals control agents more when control costs are low compared to high (pre-change test: $Z = 5.230$, two-tailed p -value < 0.001 ; post-change test: $Z = 5.464$, two-tailed p -value < 0.001).¹¹ Also, when control costs are low, *Agent Contribution* is lower than when control costs are high (pre-change test: $Z = -3.035$, two-tailed p -value $= 0.002$; post-change test: $Z = -2.353$, two-tailed p -value < 0.001). These findings confirm that differences in control costs lead to differences in principals' control decisions; principals control agents more when control costs are low than when they are high while simultaneously provoking more self-interested use of leftover discretion. The last result is consistent with controls having indirect costs for principals (Falk & Kosfeld, 2006).

4.2 | H1: Direction of the change in control costs

H1 predicts that principals are less likely to adjust their control over agents when they experience an increase than a decrease in control costs. Table 2, Panel A, presents no evidence that principals decrease control over agents after control costs increase (two-tailed p -value > 0.100). However, Panel B shows that principals increase control over agents after control costs decrease ($Z = 8.716$, two-tailed p -value < 0.001). Figure 2 shows how the average control changes depending on the direction of the change in control costs, and it supports the idea that principals adjust their control asymmetrically. Specifically, while there is an upward and statistically significant trend that principals increase control over agents when control costs decrease, the

⁹Participants rated the following three items on a seven-point Likert scale, ranging from 1 (*disagree strongly*) to 7 (*agree strongly*): "The information presented to me in this study was clear," "This study was difficult for me to understand," and "It was not easy to understand the context described to me."

¹⁰There is no significant difference in *Difficulty* between principals ($M = 2.377$, $SD = 1.132$) and agents ($M = 2.463$, $SD = 1.406$); $t(189)$, two-tailed p -value $= 0.644$.

¹¹The Z -statistics are part of the Mann-Whitney U test, which is a non-parametric version of the t -test. In these and subsequent univariate tests, I use the Mann-Whitney U test if variables show indications of violating the t -test's normality assumption.

TABLE 2 Descriptive statistics.

Panel A: Increase in control costs										
	Pre-change (low control costs)					Post-change (high control costs)				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Control	0.815	0.254	0.000	1.000	234	0.752	0.334	0.000	1.000	258
Agent Contribution	0.086	0.189	0.000	1.000	131	0.113	0.205	0.000	1.000	152

Panel B: Decrease in control costs										
	Pre-change (high control costs)					Post-change (low control costs)				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Control	0.650	0.356	0.000	1.000	316	0.859	0.279	0.000	1.000	284
Agent Contribution	0.143	0.195	0.000	0.864	218	0.067	0.183	0.000	1.000	98

Note: This table displays descriptive statistics split by the stage of the experiment and the direction of the change in control costs. *Control* is the level of control exercised by principals in a period and ranges from 0.00 (i.e., no control) to 1.00 (i.e., full control). *Agent Contribution* is the portion of the agent’s preferred point distribution allocated to the principal corrected for the minimum required contribution to principal wealth; that is, $(b_1 - 5)/10$. Pre-change (low control costs) and Pre-change (high control costs) include period 1 until the period in which the change in control costs occurred, while Post-change (high control costs) and Post-change (low control costs) include the period of the change in control costs until period 12.

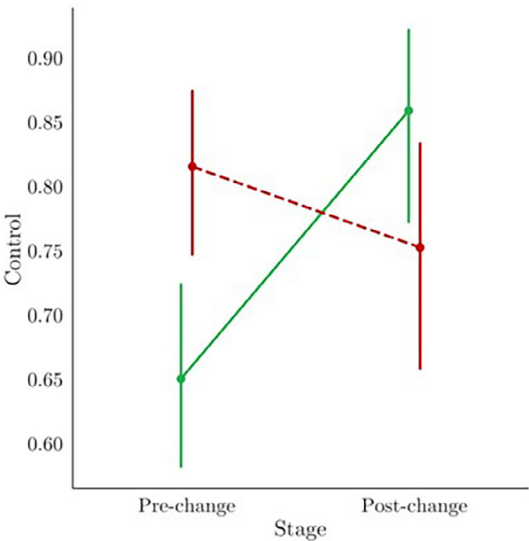


FIGURE 2 Average control. This figure displays the *Average Control* exercised by principals either before or after the change in control costs, and it ranges from 0.00 (i.e., no control) to 1.00 (i.e., full control). The solid line represents the experimental conditions in which principals experienced a decrease in control costs (from high to low control costs), and the dashed line represents the conditions in which principals experienced an increase in control costs (from low to high control costs). The perfectly vertical lines represent 95% confidence intervals. The stage of the experiment is displayed on the horizontal axis. Pre-change includes period 1 until the period in which the change in control costs occurred, while Post-change includes the period of the change in control costs until period 12.

downward controlling trend when control costs increase is insignificant. Descriptive statistics and Figure 2 support the prediction that principals adjust their control over agents asymmetrically after experiencing a change in control costs.

To test H1 formally, I conduct a change analysis that tests for a significant difference in control adjustment between principals who experienced an increase and principals who experienced

TABLE 3 Ordinary least squares (OLS) regression: *Control Adjustment*.

	(1) All changes
<i>Low-to-High Cost Coefficient</i>	−0.287*** (0.056)
Intercept	0.212*** (0.039)
<i>High-to-Low Control Change</i>	0.212***
<i>Low-to-High Control Change</i>	−0.075*
Asymmetry coefficient	0.137**
R^2	0.225
Model degrees of freedom	1
<i>F</i> -statistic	26.135***
<i>N</i>	91

Note: This table reports the result of an OLS regression with robust standard errors in parentheses. The dependent variable is *Control Adjustment*, which equals the difference in average control between all periods after the change in control costs and all the periods before the change in control costs; *Low-to-High Cost Coefficient* equals one for principals who experienced an increase in control costs from low to high and zero for other principals. The number of observations equals 91 because that is the total number of principals in the sample.

* $p < 0.100$; ** $p < 0.050$; *** $p < 0.010$ (two-tailed).

a decrease in control costs (Allison, 1990; van Breukelen, 2013).¹² Specifically, I use ordinary least squares (OLS) regressions with robust standard errors to predict *Control Adjustment*, which equals the difference between the average control principals exercised after and before the change in control costs.¹³ The independent variable in the OLS is *Low-to-High Cost Coefficient*, which equals one if control costs increased and zero if they decreased. Accordingly, the average control adjustment by principals who experienced a decrease in control costs is the intercept in the OLS regression, which I term *High-to-Low Control Change*. The average control adjustment by principals who experienced an increase in control costs, which I term *Low-to-High Control Change*, is calculated using the following linear combinations of coefficients: Intercept + *Low-to-High Cost Coefficient*. Table 3 reports that the average control adjustment under *High-to-Low Control Change* equals 0.212 (two-tailed $p < 0.001$), while the average control adjustment under *Low-to-High Control Change* equals $0.212 + -0.287 = -0.075$ (two-tailed p -value = 0.067).¹⁴

To assess whether principals' average control adjustments differ between principals who experience an increase in control costs and principals who experience a decrease in control costs, I calculate an asymmetry coefficient by subtracting the inverse of *Low-to-High Control Change*, that is, the inverse of the average control adjustment by principals who experienced an increase in control costs (i.e., 0.075), from *High-to-Low Control Change*, that is, the average

¹²An alternative way to measure principals' control adjustments is to calculate the absolute change in average control. The primary advantage of this approach is that it captures the principals' control adjustments alone, independent of their direction. The results are inferentially similar when using the alternative dependent variable.

¹³Unlike the statistical analysis for H2, which uses a constant number of three periods after the change in control costs, the statistical analysis for H1 includes all periods following the change in control costs. H1 predicts asymmetry, which does not justify a similar restriction on its statistical analysis. However, H2 does justify the need to maintain consistency in the periods after the change in control costs, as it predicts an impact of prior experience with control decisions.

¹⁴I also estimated fractional probit panel regressions for all principal-period observations ($91 \times 12 = 1,092$). These panel regressions predict principals' control over time according to the approach suggested by Papke and Wooldridge (2008), which can also control for correlation structures and trends in the data. However, since the OLS regressions in Table 3 offer a simpler test for H1 and the inferences remain qualitatively similar, I do not report or tabulate these results in the paper.

control adjustment by principals who experienced a decrease in control costs (i.e., 0.212). If the asymmetry coefficient significantly differs from zero, then the data reveal a distinct asymmetric adjustment pattern in the predicted direction. Consistent with H1, Table 3, Column 1, reports a positive and significant asymmetry coefficient ($\beta = 0.137$, two-tailed p -value = 0.017). Therefore, principals adjust their control over agents less after an increase in control costs than after a decrease in control costs.

4.3 | H2: Timing of the change in control costs

To test H2 formally, I conducted another change analysis using an OLS regression, predicting *Corrected Control Adjustment* as a function of *Low-to-High Cost Coefficient*, *Pre-change Experience*, and their interaction term. *Corrected Control Adjustment* is the difference in average control between (only) the first three periods after the change in control costs and all the periods before the change in control costs. Keeping the number of periods after the change in control costs constant in this analysis (i.e., three periods) means that I only alter the extensiveness of principals' prior experience with control decisions.¹⁵ The intercept in the OLS regression captures the average corrected control adjustment for principals who experienced a decrease in control costs in period 4. The estimated coefficient for *Low-to-High Cost Coefficient* captures the difference in the average corrected control adjustment between principals who experienced an increase and decrease in control costs in period 4. The estimated coefficient for *Pre-change Experience* reflects the linear progression in corrected control adjustments for principals who experienced a decrease in control costs as it happened a period later. Lastly, the interaction term *Low-to-High Cost Coefficient* \times *Pre-change Experience* is the test for H2; it captures the difference in linear progression in corrected control adjustments between principals who experienced an increase and decrease in control costs.

Table 4 reports the results of the OLS regression. The intercept, which captures the average corrected control adjustment for principals who experienced a decrease in control costs in period 4, is not statistically significant (Intercept = 0.126, two-tailed p -value = 0.224). The estimated coefficient for *Low-to-High Cost Coefficient*, which captures the difference in the average corrected control adjustment between principals who experienced an increase and decrease in control costs in period 4, significantly differs from zero ($\beta = -0.288$, two-tailed p -value = 0.026). The estimated coefficient for *Pre-change Experience*, which measures the linear progression in corrected control adjustments for principals who experienced a decrease in control costs as it happened a period later, is not significantly different from zero ($\beta = 0.023$, two-tailed p -value = 0.323). Lastly, the interaction term, *Low-to-High Cost Coefficient* \times *Pre-change Experience*, which is the test for H2, is not statistically significant ($\beta = -0.003$, two-tailed p -value = 0.924). Thus, there is no significant difference in linear progression in corrected control adjustments between principals who experienced increased and decreased control costs.

4.4 | Supplemental analyses

4.4.1 | Alternative analysis for H2

One of the limitations of the main analysis for H2 is that it tests whether the asymmetry in principals' control adjustments evolves linearly and on a period-by-period basis. However, the

¹⁵The window after the control costs is kept constant at three periods because this is the minimum number of periods principals experience after the change in control costs. I do not increase this window because a larger window would capture a less immediate response, and it would lead to a loss of data because observations in which principals experienced a shorter window after the change in control costs would be dropped.

TABLE 4 Ordinary least squares (OLS) regression: *Corrected Control Adjustment*.

	(1) All changes
<i>Low-to-High Cost Coefficient</i>	−0.288** (0.127)
<i>Pre-Change Experience</i>	0.023 (0.023)
<i>Low-to-High Cost Coefficient</i> × <i>Pre-Change Experience</i>	−0.003 (0.029)
Intercept	0.126 (0.103)
R^2	0.256
Model degrees of freedom	3
<i>F</i> -statistic	12.26***
<i>N</i>	91

Note: This table reports the result of an OLS regression with robust standard errors in parentheses. The dependent variable is *Corrected Control Adjustment*, which equals the difference in average control between the three periods after the change in control costs and all the periods before the change in control costs; *Low-to-High Cost Coefficient* equals one for principals who experienced an increase in control costs from low to high and zero for other principals. *Pre-change experience* ranges from 0 (change occurred in period 4) to 6 (change occurred in period 10). The number of observations equals 91 because that is the total number of principals in the sample. ** $p < 0.050$; *** $p < 0.010$ (two-tailed).

asymmetry in control adjustments might undergo a different transformation as principals experience the change in control costs later rather than earlier. Therefore, I divide principals into two relatively equal groups based on *Pre-change experience*: 50 principals who learned about the change to control costs in period 4, period 5, period 6, or period 7 (*Early Changes*), and 41 principals who learned about the change to control costs in period 8, period 9, or period 10 (*Late Changes*).¹⁶ Although this alternative approach to testing H2 does not assume the asymmetry progresses linearly on a period-by-period basis, research suggests caution because splitting samples based on continuous variables can lead to spurious significant results (Cohen, 1983; Maxwell & Delaney, 1993; McClelland et al., 2015; Rucker et al., 2015).

I ran the same OLS regression presented in Table 3 once for each subgroup and calculated the asymmetry coefficient for each. The primary dependent variable is *Corrected Control Adjustment* to keep the number of periods after the change in control costs constant and only vary the extensiveness of principals' prior experience with control decisions. Table 5 reports the results for *Early Changes* in Column 1 and *Late Changes* in Column 2. The results show that the asymmetry in principals' control adjustments is caused by those who experienced changes in control costs at a later stage rather than earlier. Principals who experienced an increase in control costs later changed their control less than those who experienced a decrease in control costs later (Asymmetry coefficient: $\beta = 0.242$, two-tailed p -value < 0.001). However, there is no evidence that principals adjust their control differently when they experience the change in control costs earlier (Asymmetry coefficient: two-tailed p -value > 0.100). The difference between these two asymmetry coefficients is statistically significant ($\chi^2_1 = 2.826$, two-tailed p -value = 0.093). These results support H2, predicting that the asymmetry in principals' willingness to adjust control over agents is weaker when principals have less prior experience with control decisions.

¹⁶My inferences are qualitatively similar if period 7 is used as the starting period for *Late Changes* (Asymmetry coefficient for *Early Changes*: $\beta = 0.018$, two-tailed p -value = 0.871; and Asymmetry coefficient for *Late Changes*: $\beta = 0.151$, two-tailed p -value = 0.022).

TABLE 5 Ordinary least squares (OLS) regressions: *Corrected Control Adjustment* split by timing of the change in control costs.

	(1) <i>Early Changes</i>	(2) <i>Late Changes</i>
<i>Low-to-High Cost Coefficient</i>	−0.298*** (0.093)	−0.302*** (0.062)
Intercept	0.156** (0.068)	0.251*** (0.041)
<i>High-to-Low Control Change</i>	0.156**	0.251***
<i>Low-to-High Control Change</i>	−0.141**	−0.051
Asymmetry coefficient	0.015	0.200***
R^2	0.177	0.366
Model degrees of freedom	1	1
F-statistic	10.313***	23.653***
N	50	41

Note: This table reports the result of two OLS regressions with robust standard errors in parentheses. The dependent variable is *Corrected Control Adjustment*, which equals the difference in average control between the three periods after the change in control costs and all the periods before the change in control costs; *Low-to-High Cost Coefficient* equals one for principals who experienced an increase in control costs from low to high and zero for other principals; Column 1 reports the results for earlier changes in control costs (i.e., period 7 or earlier), and Column 2 reports the results for later changes in control costs (i.e., period 8 or later). There are 50 principals in the sample who experience earlier changes in control costs and 41 principals who experience later changes in control costs. ** $p < 0.050$; *** $p < 0.010$ (two-tailed).

4.4.2 | Does exposure to agents’ reactions drive principals’ reluctance to lower control?

My theory proposes that principals’ experience with exercising high control over agents reinforces the belief that agents are self-interested and require tight control because principals are exposed to agents’ self-interested reactions. If the theory holds, prior exposure to agents’ reactions should drive their reluctance to relinquish control after experiencing the increase in control costs. To test this component of my theory, I exploit one of my experimental setting’s key features. Specifically, when principals exercise full control (i.e., $a = 1.00$) in a period, agents have no leftover discretion. Accordingly, under such circumstances, principals do not observe a reaction by agents, implying they cannot form and reinforce a belief that agents are self-interested. However, in any other circumstance, principals observe agents’ reactions, implying they can form such a belief. For the subset of 41 principals who experienced an increase in control costs, I calculate the percentage of times principals did not exercise full control and thus elicited a response from an agent before the increase in control costs (hereafter, *Observed Agent Reactions (Pre-change)*). I estimate an OLS regression predicting *Control Adjustment* for this subset of 41 principals who experienced an increase in control costs. I use robust standard errors and include *Observed Agent Reactions (Pre-change)* as the predictor of interest in the OLS regression.¹⁷

¹⁷I could also make a similar empirical specification for principals who experienced a decrease in control costs. However, there is not enough variation among this subset of principals because only one of them exercised full control in all periods while seven principals did in the subsample that experienced an increase in control costs. Another different approach would examine whether the principals’ control adjustments depend on the types of agents’ reactions they observed before the change. However, the experiment is designed to generate variation in control levels (and agents’ reactions) across experimental cells and not within experimental cells. With sample size limitations, the experiment is thus not designed nor powerful enough for detecting such effects.

Table 6 presents the results of this OLS regression, and the intercept shows that principals who have only exercised full control for all periods and have thus not elicited nor observed any reactions from agents before the increase in control costs decrease their control over agents after the increase in control costs (Intercept = -0.219 , two-tailed p -value = 0.005). However, as principals expose themselves to more self-interested agent reactions by exercising less than full control, they are more reluctant to decrease their control over agents after the increase in control costs ($\beta = 0.274$, two-tailed p -value = 0.005). These results support the idea that the asymmetry in principals' control decisions is driven by principals' exposure to agents' reactions and the belief formation and reinforcement process.

4.4.3 | Changes in payoffs

This section examines whether principals' asymmetric control adjustments affect how the principals' payoffs change and whether there is an asymmetry in how the payoffs change before and after principals experience the change in control costs. I use the same empirical strategy to test for asymmetry in principals' control adjustments predicted by H1. Specifically, the primary dependent variable equals the difference between the average payoff after the change in control costs and the average payoff before the change in control costs (*Principal Payoff Change*). Like the empirical strategy for H1, *Principal Payoff Change* uses the full range of periods before and after the change in control cost. The primary independent variable of interest is *Low-to-High Cost Coefficient*, which equals one for principals who experienced an increase in control costs and zero for the other principals. I estimate an OLS regression with robust standard errors.

Table 7 shows that *High-to-Low Payoff Change*, that is, the average change in payoffs for principals who experienced a decrease in control costs, equals an increase of 6.595 points (two-tailed p -value < 0.001). In contrast, *Low-to-High Payoff Change*, which is the average change in payoffs for principals who experienced an increase in control costs, equals a decrease of 6.590 points, that is, $6.595 + -13.181 = -6.590$ (two-tailed p -value < 0.001). I estimate the asymmetry coefficient by subtracting the inverse of the average change in payoffs for principals who experienced an increase in control costs (i.e., 6.590) from the average change in payoffs for principals who experienced a decrease in control costs (i.e., 6.595). If the asymmetry coefficient is

TABLE 6 Ordinary least squares (OLS) regressions: *Control Adjustment* on perceived agent reactions (pre-change).

	(1) <i>Control Adjustment</i>
<i>Observed Agent Reactions (Pre-change)</i>	0.274*** (0.091)
Intercept	-0.219*** (0.073)
R^2	0.135
Model degrees of freedom	1
F -statistic	8.995***
N	41

Note: This table presents the result of an OLS regression with robust standard errors in parentheses for principals who experienced an increase in control costs. The dependent variable is *Control Adjustment*, which equals the difference in average control between all periods after the change in control costs and all the periods before the change in control costs; *Observed Agent Reactions (Pre-change)* equals the percentage of times principals elicited responses from agents by not exercising full control before the increase in control costs. The number of observations equals 41 because that is the number of principals in the sample who experienced an increase in control costs.

*** $p < 0.010$ (two-tailed).

TABLE 7 Ordinary least squares (OLS) regressions: *Principal Payoff Change*.

	(1) <i>Principal Payoff Change</i>
<i>Low-to-High Cost Coefficient</i>	−13.184*** (0.365)
Intercept	6.595*** (0.302)
<i>High-to-Low Payoff Change</i>	6.595***
<i>Low-to-High Payoff Change</i>	−6.590***
Asymmetry coefficient	0.005
R^2	0.930
Model degrees of freedom	1
F -statistic	1301.847***
N	91

Note: This table presents the result of an OLS regression with robust standard errors in parentheses. The dependent variable is *Principal Payoff Change*, which equals the difference in average payoff between all periods after the change in control costs and all the periods before the change in control costs; *Low-to-High Cost Coefficient* equals one for principals who experienced an increase in control costs from low to high and zero for other principals. The number of observations equals 91 because that is the total number of principals in the sample.
*** $p < 0.010$ (two-tailed).

significantly different from zero, then the asymmetry in principals’ control adjustments also produces asymmetry in how their payoffs change during the experiment. However, Table 7 reveals an insignificant asymmetry coefficient (two-tailed p -value >0.100). Therefore, I find no evidence that the asymmetry in principals’ adjustment behavior is associated with asymmetry in principals’ payoffs.

5 | CONCLUSION AND DISCUSSION

This study presents experimental evidence that principals are less willing to relinquish control than take control over agents when control costs change. This experimental evidence is inconsistent with the standard economic prediction that principals will adjust their control over agents symmetrically in accordance with the changes in control costs. Notably, principals should incorporate changes in control costs into their control adjustment without considering their experiences with former agents when those experiences are uninformative about dealing with new agents under different circumstances. They should discard their prior beliefs that are based on experiences with former agents and form new beliefs based on the reactions of new agents to updated control levels. However, consistent with psychological theory, the results of my experiment reveal that the prior beliefs that principals have formed about former agents do impact their control adjustments, which leads to asymmetry in how they adjust their control. Specifically, when principals have experience exercising tight control, they struggle to revise their belief that agents are self-interested and should be controlled. In contrast, when principals have experience exercising loose control, they revise the prior belief that agents are socially interested more easily. My experiment also shows theory-consistent evidence when the asymmetry in principals’ control adjustments diminishes.

From a psychological perspective, the results have a bearing on the sources of people’s beliefs that other people tend to behave self-interestedly. For instance, some psychologists propose that people generally overestimate the influence of self-interested motives on agent

behavior (Miller, 1999) and attribute the behavior of others more to extrinsic incentives than intrinsic incentives (Heath, 1999). In support of these propositions, some practitioners have reported on how easily excessive control can spread throughout firms and have posited what firms could do to prevent this (Canner & Bernstein, 2016; Wilkins, 2014). My results suggest that beliefs about others' self-interest can be caused by people's past observations and experiences. Prolonged exposure to the self-interested behavior of others reinforces the belief that others are self-interested, making people less willing to revise that belief when new evidence comes to light or new circumstances warrant a revision of those beliefs. This phenomenon is akin to psychological theories such as confirmation bias (Nickerson, 1998), belief perseverance (Anderson, 2007), and conceptual conservatism (Nissani, 1990, 1994).

The results of this study also expand the existing list of sticky phenomena in economic literature. Stickiness refers to any economic variable that is resistant to change. It has been documented in prices (Kehoe & Midrigan, 2015), wages (Elsby et al., 2016), costs (Anderson et al., 2003), information (Dupor et al., 2010; Knotek, 2010), and decision authority (Bartling et al., 2014; Fehr et al., 2013). The results of this study suggest that control may also be sticky. Consistent with the idea that control is sticky, it is frequently argued that the strength of controls has increased over time. Reasons cited for this increase include stricter regulations (e.g., Sarbanes-Oxley Section 404) and the ease of controlling agents due to advancements in information technology and data science (Labro & Stice-Lawrence, 2020). The results of this study suggest that due to the disproportionate reinforcement of beliefs that agents behave selfishly, those responsible for control decisions may have had a hand in increasing the strength of controls.

This study has limitations, some of which are inherent to the nature of experiments. However, these limitations offer promising directions for future research. Although my experiment was designed to examine how principals adjust their control in response to changes in control costs with different agents, it was not designed to examine how they make these decisions with the same agents. As a result, a key feature of the experiment is that principals and agents were rematched randomly and anonymously each period. I designed the experiment in this way to ensure that principals' control adjustments could be directly attributed to the change in control costs rather than their anticipation of how agents may react to the change in control costs and their control adjustment. Not involving agents in the change in control costs and principals' control adjustments also reflects a relevant practical scenario since organizational changes and changes to employment modes are typically accompanied by agent turnover (Baron et al., 2001; Morrell et al., 2004).

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