

New Controls and the Development of Trust

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Abstract: Research suggests that cooperation-inducing controls provide an indirect benefit in the form of increased inter-employee trust. We examine whether this effect differs with employees' varying experiences prior to the control. We develop theory based on fundamental attribution error and base rate neglect to predict that cooperation-inducing controls increase inter-employee trust when employees have experience with *either* the task or the coworker prior to their exposure to the control, but to a lesser extent when employees have case-specific experience (i.e., experience with a specific coworker in a specific task). Experimental results support our theory. Further, we shed light on the mechanisms through which controls and experience affect trust development by providing evidence that case-specific experience mitigates the tendency to neglect base rate information about cooperative tendencies and misattributions of coworkers' behavior. Our results help inform practice regarding where the benefit of controls is likely to be greater.

Keywords: Trust; interpersonal trust; controls; management accounting controls; cooperation

I. INTRODUCTION

Trust is a critical component of virtually all social relations, providing mutual confidence and facilitating prosocial behavior in an efficient manner (Arrow 1972; 1974). Fostering a culture of trust between employees (“inter-employee trust”) is foundational to the success of firms. For example, group work is very prevalent in practice, and inter-employee trust is a key determinant of cooperation and efficiency in groups (Zand 1972; Boss 1978; Staples and Webster 2008; De Jong and Elfring 2010). Further, trust supports myriad firm-desired outcomes, such as increased organizational citizenship behavior and decreased turnover (Dirks and Ferrin 2001), making it important to understand how a firm’s decisions affect trust development between coworkers.

Firms often use formal controls to align employees’ interests with those of the firm (Ouchi 1979; Eisenhardt 1985). Research suggests effective controls that increase cooperation between coworkers (“cooperation-inducing controls”) can increase an employee’s trust of their coworkers as the employee over-attributes observed cooperation to the coworkers’ trustworthiness rather than to the control (Coletti, Sedatole, and Towry 2005; Garrett, Livingston, and Tayler 2019). This research suggests a dual benefit of controls: a direct benefit of increased cooperation on the controlled task and an indirect benefit of increased inter-employee trust that can increase future cooperation on uncontrolled tasks. Our study focuses on the indirect benefit of cooperation-inducing controls in the form of increased inter-employee trust. Specifically, we examine whether the effect of cooperation-inducing controls on inter-employee trust differs based on an employee’s experience with the task and their coworker prior to their exposure to the control.

The studies finding that cooperation-inducing controls increase inter-employee trust only consider settings where employees have no experience prior to working under the control. Understanding how prior experience affects the relation between cooperation-inducing controls

and inter-employee trust is important as a control is always “new” to an employee when they first encounter the control and employees likely differ widely in their experiences prior to performing a given task under a given control. Employees gain experience with how myriad coworkers behave on a given task absent a control (“task experience”) from interactions during school, training, or as they switch jobs. This last source is increasingly common as total voluntary turnover rose each year from 2009-2018 (United States Bureau of Labor Statistics 2019). Alternatively, employees can gain experience with how a given coworker behaves across myriad tasks (“coworker experience”) prior to performing the new task with a control, such as when employees remain at their firm for an extended period. However, experience need not be so general. Employees can gain experience working on a task with coworkers prior to a control’s introduction (“case-specific experience”) and continue working on the now-controlled task with the same coworkers.

Coletti et al. (2005) and Garrett et al. (2019) argue that cooperation-inducing controls increase inter-employee trust due to employees’ tendency to over-attribute cooperation to coworkers’ trustworthiness (a dispositional factor) rather than the control (a situational factor): a form of the fundamental attribution error (“FAE”; Ross 1977). Although the FAE likely plays a role in trust development in these studies, the greater trust observed in the presence of controls is also consistent with *rational* trust updating (Sonenshein, Herzenstein, and Dholakia 2011). Specifically, these studies implicitly assume that employees’ *ex post* trust in the absence of controls is equivalent to their initial trust. However, in the absence of experience, individuals have a tendency toward high trust (e.g., Dunning et al. 2014). This suggests that the difference in trust could be due to the observed *lack* of cooperation in the absence of the control decreasing trust (i.e., rational trust updating) rather than the high cooperation in the presence of the control increasing trust (i.e., the FAE). This alternative is less likely when employees have experience with a task

(coworker), as this experience allows them to form more accurate base rates of trustworthiness for the task (coworker). Thus, controls increasing trust when employees have experience prior to the control provides stronger evidence of the role of the FAE in the development of trust.

The base rate information generated from prior experience could alter the extent to which employees exhibit the FAE by making the influence of situational factors (i.e., the control) on observed cooperation more salient. However, research pertaining to a related attributional error—base rate neglect (“BRN”; Nisbett, Borgida, Crandall, and Reed 1976)—suggests employees will often not attend to base rate information, particularly if it is inconsistent with recent behavior. BRN would lead an employee to attribute recent control-induced cooperation to their coworker’s dispositional trustworthiness (the FAE). Thus, we predict that BRN leads to the FAE, allowing cooperation-inducing controls to increase inter-employee trust when employees have general experience with either the task *or* the coworker. By contrast, employees with experience with a specific coworker in the to-be controlled task prior to the control have case-specific knowledge of the coworker’s behavior and are therefore less prone to BRN and the FAE. Thus, we expect cooperation-inducing controls to increase inter-employee trust less for employees with case-specific experience than for those with no experience or general experience (i.e., task or coworker experience) prior to the control.

To study the effects of cooperation-inducing controls and employees’ prior experience on the development of inter-employee trust, we conduct a two-stage experiment. In stage one of our treatment conditions, participants complete a series of tasks that culminate in working with a constant coworker under a cooperation-inducing control. We manipulate whether participants have general experience (with the task or coworker) or case-specific experience with the coworker in the task prior to completing the task under the control. Further, we conduct one condition in which

participants have no experience absent the control (the control is always in place) and one condition in which participants never complete the task under the control to create two empirical benchmarks for measuring the development of trust. Stage two is identical for all participants and entails an investment game (Berg, Dickhaut, and McCabe 1995). Our dependent variable, trust, is captured by how much each participant sends to their coworker in this investment game. Comparisons to the condition where participants never complete the task under the control establish whether the control increases trust. The condition where participants have no experience prior to the control establishes the level of trust under the control without prior experience, making comparisons to this condition a test of whether experience mitigates the development of trust under the control. Participants then complete a questionnaire and a final task to create measures of the FAE and BRN.

We find that the control increases trust when employees have no experience prior to the control, as in Coletti et al. (2005) and Garrett et al. (2019). Consistent with our theory, we find the control also increases trust when employees have general experience with *either* the task or coworker prior to the control. Further, we do not find that either of these types of experience decreases the trust created by the control. Also consistent with our theory, we find that the control increases trust less when employees have case-specific experience prior to the control, relative to general or no experience. Supplemental tests suggest that employees with case-specific experience are less prone to BRN and the FAE than those with general task or coworker experience.

Our study makes several contributions to accounting research and practice regarding the effect of controls on inter-employee trust. First, our finding that cooperation-inducing controls increase inter-employee trust when employees lack prior experience replicates the results of Coletti et al. (2005) and Garrett et al. (2019), reducing our variance of belief around the true effect (Towry

2012). The reproducibility of results, even from seminal papers, is not a given (Camerer et al. 2018) and it is thus important to replicate results if they are to be relied upon. Arguably, the value of testing the reliability of results is even greater in applied sciences, such as accounting, where communications to practice may be damaged if based on unreliable results. Second, our finding that cooperation-inducing controls increase trust when employees have experience with the task or coworker prior to the control provides important robustness to the generalizability of claims that cooperation-inducing controls can increase trust. Collectively, these results should give greater confidence to arguments regarding the effect of cooperation-inducing controls on inter-employee trust in practice, where these forms of general prior experience are likely to exist.

Third, we extend the literature on the trust-control relation. Our theory and results suggest that the ability of cooperation-inducing controls to develop inter-employee trust is reduced for employees with case-specific experience. This result is important as it identifies a moderator of the effect of cooperation-inducing controls on trust, filling a gap in the trust-control literature (Long and Sitkin 2018). While several studies contribute to our understanding of the cost-benefit tradeoff firms face in their control decisions by studying moderators of the costs of controls (e.g., Christ et al. 2012; Christ 2013; Cardinaels and Yin 2015; Garrett, Holderness, and Olsen 2020), there is a paucity of research on moderators of the indirect benefits of controls, such as on inter-employee trust (Garrett et al. 2019). We further contribute to this literature by providing more direct evidence of the theoretic mechanisms by which controls increase trust. Our results suggest that BRN and the FAE help cooperation-inducing controls increase trust, but employees' case-specific prior experience attenuates these effects. Thus, our study helps explain how cooperation-inducing controls increase trust and when this benefit is less likely.

In sum, our study contributes to the collective understanding of an important aspect of the

trust-control relation. Firms' control system design, implementation, and execution are large multi-faceted decisions that weigh numerous costs and benefits. Our results suggest firms benefit less from increased inter-employee trust when placing controls on tasks already being performed by existing employees due to their case-specific experience reducing BRN and the FAE. Rather, the indirect benefit of introducing such a control is more likely to develop over time as the workforce turns over or jobs change in the firm. By better understanding these costs and benefits, managers can make more informed control decisions to benefit their firms.

II. THEORY AND HYPOTHESES DEVELOPMENT

Firms often use formal controls to align employee behavior with the interests of the firm. Research suggests that such controls not only affect employee behavior on the controlled task, but also affect two types of trust within firms: vertical trust between the firm and employees and horizontal trust between employees. The choice to use controls can signal a firm's distrust of employees and harm vertical trust (e.g., Christ 2013). However, the presence of controls can foster the development of horizontal trust between employees (Coletti et al. 2005). This latter finding illustrates that the benefit of formal controls can come from two sources: 1) a direct benefit of less opportunistic employee actions and 2) an indirect benefit of increased inter-employee trust. This indirect benefit is valuable because it reduces the need for further costly controls in motivating cooperation on alternative tasks and reduces losses from distrust in collaborations. In this study, we do not examine the effect of choosing to impose controls on vertical trust.¹ Rather, we examine the indirect benefit of control presence on the development of inter-employee trust.

¹ As with research focusing on the effect of controls on vertical distrust (e.g., Christ et al. 2012; Christ 2013), we do not model the direct costs of designing, implementing, and enforcing the control. Rather, as these studies focus on the relative cost of controls, we focus on the relative benefit of controls incremental to their direct benefit (improved performance on the controlled task) and direct cost (consultants, contracts, legal enforcement, etc.).

Controls and the Development of Trust

Trust, at its essence, is often defined as capturing one's assessment of the trustworthiness of another party's behavior (i.e., their likelihood to take advantage of one's vulnerability; Hardin 2002).² The assessment of trustworthiness depends on the trustor's propensity to trust, the trustor's institution-based trust, and the trustor's assessment of the competence (i.e., ability) and goodwill (i.e., integrity, benevolence and/or predictability) of the other party (Mayer, Davis, and Schoorman 1995; McKnight et al. 1998).³ The research on cooperation-inducing controls and inter-employee trust focuses on assessments of goodwill trust, as we do in our study.

Trust development is calculative and history dependent (Kramer 1999), as assessments of trustworthiness are calculated by the trustor based on weighing their cumulative prior experiences. Thus, one's trust in another party increases in the number of times one observes the other party exhibiting trustworthy behavior. Applying this logic to controls, controls that induce cooperative behavior from coworkers can increase trust between coworkers if employees assign the observed behavior a nonzero weight in their trust assessment of their coworker. In assessing trust, employees may (rightly) recognize that a coworker's cooperation is induced by the control rather than the coworker's trustworthiness, making observed cooperation under the control uninformative regarding the coworker's trustworthiness and suggesting that employees assign zero weight to the controlled behavior. In such a case, cooperation-inducing controls are unlikely to increase trust

² Formally, trust can take the form of beliefs, intentions, or behaviors, with each leading to the next (McKnight, Cummings, and Chervany 1998; Vidotto, Massidda, Noventa, and Vicentini 2012). Trusting beliefs reflect the expectation of trustworthy behavior by another party, which are antecedent to trusting intentions, which reflect the intention to engage in trusting behavior if the opportunity is present. As a result, factors that affect trusting beliefs are of paramount importance in determining realized trust behaviors (Schweitzer, Hershey, and Bradlow 2006).

³ Propensity to trust is a stable individual difference that reflects an innate likelihood to trust (McKnight and Chervany 1996). We test whether propensity to trust interacts with prior experience in supplemental analyses. Institution-based trust is less relevant in our setting as we are not examining employees' trust in the organization. Finally, although goodwill trust assessments are a function of how the trustor sees the other party's benevolence, integrity, and predictability, assessments of predictability are less impactful on trust than the other two (Vidotto et al. 2012).

development between employees. However, assessments of trustworthiness are rarely arrived at rationally (Misztal 1996; Emsley and Kidon 2007; Alarcon 2018; Baer et al. 2018), allowing for situational factors, such as a control, to be underappreciated (Hardin 2002).

Coletti et al. (2005) test whether a cooperation-inducing control affects inter-employee trust development. They find such controls increase inter-employee trust, which persists after the control is removed. Garrett et al. (2019) further test the effect of cooperation-inducing controls on inter-employee trust and find that the increase in trust is strong enough to carry over to a new, uncontrolled, task. As the development of trust is a function of how one interprets and attributes the source of prior experiences, both studies base their predictions on attribution theory.

Attribution theory explains how individuals perceive the cause of others' behavior, such as whether one perceives others' behavior as being due to situational or dispositional (i.e., about the individual) factors (Heider 1958). While it is reasonable for employees to attribute their coworkers' control-induced cooperation to the control (a situational factor), individuals often over-attribute the degree to which dispositional factors affect others' behavior (Jones and Harris 1967; Gilbert and Malone 1995). The tendency to over-attribute others' behavior to dispositional factors is so prevalent it is labelled the Fundamental Attribution Error ("FAE"; Ross 1977). Applied to our setting, the FAE suggests employees will misattribute some of their coworkers' control-induced cooperation to the coworkers' dispositional trustworthiness, increasing trust between coworkers. Such attribution errors are particularly likely when, as in Coletti et al. (2005) and Garrett et al. (2019), employees have no experience prior to the control, which makes it difficult for employees to accurately attribute their coworkers' behavior to a situational source. Specifically, an effective control makes both high and low trustworthiness coworkers observationally equivalent, despite different sources of cooperation. This creates a fertile ground

for the FAE. Our first prediction is a replication of these prior studies' main finding.

H1 (Replication): When employees have no experience prior to the control, inter-employee trust is higher following the use of cooperation-inducing controls than when no such controls are used.

Experience and the Development of Trust

To the extent that the FAE drives H1, it is important to consider elements of practice that can affect employees' propensity to attribute coworker behavior to situational factors instead of dispositional ones. Such examination enables a stronger test of the role of the FAE in the relation between cooperation-inducing controls and inter-employee trust, while providing important robustness regarding generalizability. To this end, we consider how an employee's experiences prior to working under the control affect trust development.⁴

In practice, employees often have experience with the task or with coworkers outside of the controlled task that could reduce their susceptibility to the FAE and, thus, the ability of controls to foster trust. Task experience refers to experience with the task absent the control, such as from training, working in another firm, or prior to the firm introducing the control. Coworker experience refers to experience with a given coworker in other tasks not subject to controls. These forms of prior experience allow employees to develop an understanding of the general base rate of cooperation on the task ("task base rate") or the general base rate of cooperation of a coworker

⁴ As a secondary benefit, our examination of prior experience allows us to draw stronger inferences regarding whether the predicted effects of our H1, as observed in Coletti et al. (2005) and Garrett et al. (2019), are driven by the FAE. If participants in their studies held an optimistic ex ante view of their coworker's trustworthiness, as individuals often do (Dunning et al. 2014), then the lack of cooperation absent the control could have driven down employees' trust in their coworkers while the observed cooperation under the control was (appropriately) ignored. This would be evidence of inaccurate prior beliefs about base rates of cooperation rather than evidence of the FAE as claimed. The lack of experience prior to the control does not allow these studies to rule out this alternative theory.

(“individual base rate”) absent a control.⁵ This general base rate information theoretically allows for better inferences regarding the role of the control in inducing cooperation, which would reduce the weight assigned to observed cooperation under the control and mitigate the extent to which employees exhibit the FAE. However, it is unlikely that base rate information will eliminate the FAE and, as a result, controls’ positive effect on trust as individuals commonly exhibit a related attributional error that supports the FAE—base rate neglect (“BRN”; Nisbett et al. 1976).

BRN and the FAE are related in that both pertain to individuals’ propensity to underweight relevant information when attributing the cause of another’s behavior (Nisbett and Ross 1980).⁶ BRN captures the tendency to underweight information about the base rate, or the unconditional probability of some outcome occurring, in favor of recent and case-specific information (Bar-Hillel 1980; Tversky and Kahneman 1980, 1985; Kahneman, Slovic, and Tversky 1982). BRN enables the FAE to occur when employees have general base rate information, as it causes employees to neglect the information that would otherwise help them make more accurate attributions of control-induced cooperation. Dunwoody, Goodie, and Mahan (2005) suggest that individuals rely on base rate information less when it is inconsistent with observed behavior, and the need for a control implies coworkers’ collective trustworthiness is insufficient to consistently motivate cooperation (Sliwka 2007). Thus, assuming an effective control, the base rate of cooperation (either task or individual) absent the control is likely lower than coworkers’ observed cooperation with the control. Due to this change in behavior, employees are likely to neglect the general base rate information (from task or coworker experience) in favor of the case-specific information (observed

⁵ It is important to note that experience (task or coworker) is best represented on a continuum in practice. Employees can have varying degrees of each, and these forms of experience are not mutually exclusive. For clearer testing of their effects, we manipulate them as present/absent in isolation when considering general base rate information.

⁶ BRN is most commonly tested and strongly observed in failures to apply Bayes’ Theorem in calculating the odds of an outcome occurring given summary probabilities (Koehler 1996; Dunwoody 2006). It is also observed when odds are stated in frequencies or gathered through exposure to a series of events (Gigerenzer and Hoffrage 1995).

cooperation under the control), allowing the FAE that leads to increased inter-employee trust.

Incremental to the overall theory regarding BRN, task and coworker experience each have attributes that further affect the likelihood of BRN. First, consider BRN with task experience. While task experience generates task base rate information for the population, research suggests individuals prefer to attend to individuating information (Ginosar and Trope 1980), making employees more likely to rely on observations of the coworker's cooperation under the control than on the task base rate of cooperation, leading to BRN and the FAE. Thus, cooperation-inducing controls likely increase inter-employee trust, relative to no controls, even with task experience.

While research suggests BRN will allow the FAE to occur when employees only have task experience, it is less clear what effect task base rate information has on the extent to which cooperation-inducing controls increase trust. The preceding theory suggests BRN allows for the FAE even with task experience, which supports controls' positive effect on trust relative to when there were no controls. It does not establish whether task experience reduces the effect of controls on trust relative to when employees have no experience prior to the control. Three possibilities exist regarding this effect. First, task experience could allow for better trust inferences, discounting behavior under the control and mitigating the positive effect of controls on trust. Second, task base rate information could be neglected entirely, leading to no effect of task experience on the FAE or the development of trust under the control. Third, the high levels of cooperation for this coworker, relative to the uncontrolled coworkers, could lead to greater misattributions and subsequent trust, as this coworker's high cooperation appears exceptionally trustworthy by comparison. Because of these offsetting theories, we present a null hypothesis regarding whether task experience affects controls' ability to increase trust.

In sum, theory for BRN suggests the FAE will occur when employees have prior task

experience, allowing cooperation-inducing controls to increase trust relative to when no controls are used (H2a). We also examine whether prior task experience affects cooperation-inducing controls' ability to increase inter-employee trust relative to when employees have no experience prior to the controls (H2b).

H2a: Relative to when no controls are present, cooperation-inducing controls increase inter-employee trust when employees have prior task experience.

H2b (null): Inter-employee trust is not different when employees have no experience prior to the control as compared to when employees have prior task experience.

The FAE seems less likely with coworker experience, as coworker experience speaks directly to dispositional aspects of a coworker. However, BRN suggests the FAE will occur with coworker experience because trust and perceptions of trustworthiness are often situation-dependent (Mayer et al. 1995; McKnight et al. 1998; Vidotto et al. 2012). In his treatise on trust, Hardin (2002) uses an example from Dostoyevsky's *Brothers Karamazov* to illustrate this notion. A Russian army lieutenant colonel (Lt. Col.) in charge of regiment funds illegally gives the funds to a merchant, Trifonov, to use after each audit. Trifonov uses the funds to buy and sell extra goods, then returns the capital and a share of the profits to the Lt. Col. personally. One day, the Lt. Col. is suddenly reassigned and approaches Trifonov to request the funds be returned as to hide their collusion. Trifonov replies that he has no such money and could not have it, as that would be illegal. Notably, the Lt. Col. is surprised by Trifonov's lack of dispositional trustworthiness despite the Lt. Col. having colluded with Trifonov to illegally use government funds! Hardin admonishes the Lt. Col. for failing to recognize the situation-dependent nature of trustworthiness.⁷

⁷ Hardin uses *Anna Karenina* for an additional example of situational trustworthiness. He quotes and comments on Tolstoy's description of Count Vronsky's situational code of trustworthiness: "The code categorically determined that though the card-sharper must be paid, the tailor need not be; that one may not lie to a man, but might a woman; that one must not deceive anyone, except a husband; that one must not forgive an insult, but may insult others, and so

The situation-dependent nature of trustworthiness is likely to support BRN in our setting. An employee who first observes low cooperation from their coworker in one task and then observes greater cooperation in a different, controlled task may attribute the increase in cooperation to the coworker's dispositional trustworthiness, neglecting the individual base rate of cooperation. Alternatively, an employee may attribute the low individual base rate to situational factors that caused low cooperation previously. For example, in many group settings, cooperation is a costly action that is only beneficial if the other party also cooperates. As a result, an employee may attribute low cooperation absent a control to a coworker's strategic uncertainty about the employee's behavior or a coworker's belief about the trustworthiness of the employee.

Based on the preceding discussion, we expect the situation-dependent nature of trustworthiness to contribute to BRN, the FAE, and increased trust from controls when employees have prior coworker experience relative to when there is no control (H3a). However, as with task experience, the degree to which coworker experience reduces trust relative to when the employee has no experience prior to the control is difficult to predict. As before, the base rate information could allow for better trust inferences, mitigating the positive effect of controls on trust. Alternatively, the base rate could be neglected entirely such that the FAE is similar to when there is no prior experience. Therefore, we examine whether prior coworker experience affects the development of inter-employee trust under cooperation-inducing controls relative to when employees have no experience prior to the control (H3b).

H3a: Relative to when no controls are present, cooperation-inducing controls increase inter-employee trust when employees have prior coworker experience.

H3b (null): Inter-employee trust is not different when employees have no experience

on.' Karenin therefore can be confident that Vronsky will be honorable in repaying a gambling debt but he cannot be confident that Vronsky will be honorable with his wife, Anna – and therein lies a long novel (p. 49)."

prior to the control as compared to when employees have prior coworker experience.

We have only discussed general base rate information thus far. In contrast to more general forms of experience, employees can have experience with a specific coworker *in the specific task*, such as when a firm imposes a control on employees' current work environment. Such experience provides case-specific information, which is less prone to BRN than general base rate information because it is individuating (Ginosar and Trope 1980), within the same context (Mayer et al. 1995; Hardin 2002), and seen as more relevant than more general base rate information (Bar-Hillel 1980). Notably, case-specific experience is not simply the sum of task and coworker experience. Task experience provides base rate information about the population, not a specific coworker. Coworker experience provides base rate information about a specific coworker, but not in the specific task. Adding these together does not provide information about whether a specific coworker cooperates in the specific task. Thus, this additive experience could still suffer from the idiosyncratic shortcomings of each form of general base rate information, leading to BRN. Rather, case-specific experience is more likely to reduce BRN. In effect, case-specific information makes the impact of the control on observed cooperation more apparent, making employees less susceptible to the FAE. Consequently, we expect case-specific experience to mitigate controls' effect on trust relative to general experience or no prior experience (H4b).

However, the extent to which case-specific experience reduces the trust developed from controls is difficult to predict. If case-specific information is seen as perfect indicant information (Bar-Hillel 1980), then we would expect similar trust as when no control is used. If case-specific information is not seen as perfectly indicant, we may observe an increase in trust from the control even with case-specific information. Therefore, we examine whether controls, preceded by case-specific experience, affect inter-employee trust relative to when no controls are present (H4a).

H4a (null): Relative to when no controls are present, cooperation-inducing controls do not increase trust when employees have prior case-specific experience.

H4b: Inter-employee trust is lower when employees have case-specific experience prior to the control than when employees have no prior experience, only task experience, or only coworker experience.

III. METHOD

Overview & Participants

We conduct a two-stage experiment with five conditions: *Baseline*, *No Prior Experience*, *Task Experience*, *Coworker Experience*, and *Case-Specific Experience*.⁸ We use the first half of stage one to create a history for participants, manipulating the form of experience participants have prior to the control. We use the second half of stage one to allow for cooperation under the control. In stage two we measure our main dependent variable, trust. Finally, participants complete a questionnaire and a final task to capture our process measures.

Five hundred and forty-three individuals recruited from Amazon's Mechanical Turk (MTurk) participated in our experiment.⁹ Any MTurk worker with a historical approval rating of at least 98 percent and who indicated they reside in the U.S. was eligible to participate. Participants earned a \$1.00 participation fee plus compensation from experimental points at a rate of 128 points = \$1.00. Total pay averaged \$5.83 across conditions. This equates to an hourly wage of \$14.58, above estimates of effective rates for MTurk workers (Farrell, Grenier, and Leiby 2017).

Basic Stage One Tasks

Our primary stage one task is based on Coletti et al. (2005) and Garrett et al. (2019) and has two forms: uncontrolled and controlled. We first describe each of these tasks before detailing

⁸ Approval for the experiment was obtained through the relevant Institutional Review Boards.

⁹ The web application was programmed using the oTree platform (Chen, Schonger, and Wickens, 2016).

how the tasks and coworker pairings differed by treatment.

The uncontrolled stage one task is structurally similar to a contextualized prisoner's dilemma (PD) game in that it is a simultaneous-choice game with a non-cooperative, socially inefficient Nash Equilibrium. Participants take the role of an R&D manager for a pharmaceutical company and decide to commit either a low or a high level of resources to a joint project with their coworker each period. Figure 1 summarizes the parameters of the game. Each manager receives half of the joint project's revenue and pays the cost of the resource level they commit to the joint project. If both managers commit low (high) resources to the joint project, the project's revenue is 10 (50). If only one manager commits high resources to the joint project, the project's revenue is 30. It costs each manager 15 (0) points to commit high (low) resources. These parameters create a setting where managers have individual incentives to contribute low resources to the joint project.

[INSERT FIGURE 1 ABOUT HERE]

The controlled stage one task uses the same structure and payoff as the uncontrolled task with one difference. In the controlled task, the firm implements a probabilistic audit and bonus that serves as a control to induce cooperation (i.e., high resources). The firm audits the joint project with 80% probability and gives each manager who contributed high (low) resources to the project a bonus of 15 (0) points if audited. As a result, the control shifts the Nash Equilibrium such that it is in each manager's best interest to contribute high resources. Following Coletti et al. (2005) and Garrett et al. (2019), we code the choice to provide high resources as cooperative.

Stage One Procedures by Condition

In all conditions, participants begin stage one by reading instructions describing the first task and answering a knowledge check to ensure understanding. Each participant is then randomly paired with another participant in the same experimental condition to begin the first of stage one's

twelve periods. The task details of stage one differ by condition and are summarized in Figure 2.

[INSERT FIGURE 2 ABOUT HERE]

Our manipulations only affect stage one. No one participated in more than one condition. Each condition entails twelve periods in stage one. The first two conditions replicate Coletti et al. (2005) and Garrett et al. (2019) and provide empirical benchmarks for inter-employee trust with and without the control. In the *Baseline* condition, participants repeat the uncontrolled PD game across all twelve periods of stage one with the same coworker (labelled “Manager B”) for all periods. This condition establishes a benchmark of ex post trust in a setting without the control, making comparisons of conditions with a control present to *Baseline* a test of whether cooperation-inducing controls increase trust. In the *No Prior Experience* condition, participants repeat the controlled PD game across all twelve periods of stage one with the same coworker (Manager B) for all periods. This condition establishes a benchmark of ex post trust under the control with no experience, making comparisons of conditions where participants gain experience prior to the control (*Task Experience*, *Coworker Experience*, *Case-Specific Experience*) to *No Prior Experience* a test of whether experience mitigates controls’ effect on trust.

To operationalize prior task, coworker, and case-specific experience, we conduct three more conditions, each of which splits stage one into an “experience half” and a “controlled half.”¹⁰ Each treatment differs in the experience half (periods 1-6).¹¹ In the *Task Experience* condition, participants complete the uncontrolled PD game for six periods while being randomly re-matched

¹⁰ An alternative design choice would be to simply provide participants with information about another person’s cooperation (in the same or a different task). However, research suggests secondhand information is more subject to BRN than information gained from firsthand experience (Gigerenzer and Hoffrage 1995; Goodie and Fantino 1999; Welsh and Navarro 2012). Thus, we provide participants with firsthand experience as a stronger test of our theory.

¹¹ In practice, individuals will have varying amounts of experience with tasks and coworkers prior to encountering a control. As our theory relates to perceptions of changes in observed behavior, we chose to have participants complete six periods in each half of stage one to create sufficient – and equal – experience with and without the control.

with a different coworker (never Manager B) each period.¹² After period 6, participants learn that the firm has adopted the control (audit) and that they will be paired with a single coworker (Manager B) for subsequent periods. In the *Case-Specific Experience* condition, participants complete the uncontrolled PD game for six periods with the same coworker (Manager B) each period. After period 6, participants learn that the firm has adopted the control and that they will remain paired with the same Manager B in subsequent periods.

Finally, to introduce experience with the coworker but not the task, the use of a different uncontrolled task in the experience half of stage one is required. Thus, in the *Coworker Experience* condition, participants are paired with a single coworker (Manager B) for the first six periods and complete a contextualized public goods game. The decision context is similar to the uncontrolled PD game in that participants decide what level of resources to devote to a joint project with their coworker.¹³ Participants are given six resource points to keep or allocate to the joint project each period. Any points allocated to the project are multiplied by 1.5 to determine the project's revenue. Each manager receives half of this revenue plus any points not allocated to the project. While the pair's total payoff is maximized when both managers contribute all their resources to the project, each manager's individual incentive is to contribute nothing. We code periods where a participant allocated more than four (less than five) of the possible six points as cooperative (uncooperative). After period 6, participants learn they will start a new task (the controlled PD) and remain paired with the same Manager B for subsequent periods.

In all conditions, participants receive feedback about their coworker's decision after each

¹² We randomly re-matched participants in this condition to expose participants to a larger number of coworkers, allowing them to better estimate the population base rate of cooperation on the task.

¹³ We use the public goods game as it presents a setting with a non-cooperative Nash Equilibrium, as in our uncontrolled PD task, allowing for meaningful observations of coworker cooperation. Importantly, this cooperation occurs under conditions that are not identical to the PD (i.e., resource allocation possibilities and payoff structures differ), allowing us to tell participants the form of their "project" changes when moving from periods 1-6 to 7-12.

period. After the experience half of stage one, participants complete a brief knowledge check to ensure their understanding prior to the controlled half of stage one. In the controlled half of stage one (periods 7-12) all participants complete the controlled PD game with the same coworker (Manager B) each period, receiving feedback about Manager B's decision after each period.

Stage Two

Stage two is identical for all conditions. After learning the points earned in stage one, participants read instructions for stage two. The first stage two task is an investment game (Berg et al. 1995) with Manager B. Specifically, participants learn the manager they had most recently been working with (Manager B) has a new project in which they can invest. Each participant has 100 points they can keep or invest in any portion to Manager B's project. Any point invested is tripled, with Manager B able to return any portion of the resultant profit they wish. As Manager B has no monetary incentive to return any of the points they receive, the self-interested decision is for the participant to keep all 100 points. Thus, sending any points demonstrates trust that Manager B will return more than the initial investment (Cox 2004). Following a brief knowledge check, participants are presented with a summary of all of their partner's/partners' decisions from stage one and indicate the number of points they wish to invest in Manager B's project.¹⁴ We measure trust as the number of points the participant invests in Manager B's project.

Participants then complete a second investment game in which they can invest in a second project managed by someone other than Manager B ("Manager C").¹⁵ We measure 3rd party trust

¹⁴ The summary of stage one actions provides base rate information pertaining to the cooperation of an individual's previous partner(s). Providing individuals with a recap of this information increases the salience of the information, which could bias against us finding any BRN when making trust-related future decisions. In *Task Experience*, the summary shows decisions by seven coworkers, six from the first six periods, and Manager B from the last six periods.

¹⁵ The focus of our study is on the effects of experience and controls on inter-employee trust for coworkers directly working together. Having participants complete a second investment game with another random coworker permits an exploratory investigation into possible effects of controls and experience on trust between two coworkers who have *not* worked together directly. Results of these tests are footnoted in the results section.

as the number of points the participant invests in this project. Participants complete this prior to learning results of the first investment game. Participants conclude this part of the experiment by acting in the receiver role in both investment games, indicating how many points they wish to return to each of the two other managers who invested in their projects (Manager B and Manager C). Then, participants receive their results from both investment games.

Questionnaire and Process Measures

After the stage two games, participants complete a post-experiment questionnaire (PEQ) to report demographic and process information. The PEQ includes an eight-question scale from Mayer and Davis (1999) to measure each participant's propensity to trust. For ease of interpretation, we create an indicator variable for participants with a high propensity to trust based on whether the participant is above the sample median. The PEQ also includes a measure to capture the extent to which each participant attributes Manager B's cooperation under the control to dispositional or situational factors. Specifically, participants report on a 7-point Likert scale the extent to which they attribute Manager B's cooperation in periods 7-12 to the fear of the audit versus Manager B's inherent cooperative tendencies (1 = fear of the audit alone, 7 = inherent cooperative tendencies alone). Higher responses on this measure are more consistent with the FAE.

Finally, participants complete a final task to allow us to measure their utilization of base rate information from stage one. Participants first complete an abstract PD game (with a defect-defect equilibrium) with a random participant (not Manager B). After making this decision, participants are given six points to wager on the decision, cooperate or defect, they believe Manager B played and six points to wager on the decision they believe Manager B's randomly matched partner played. Each point wagered on the decision actually made by each individual earns the participant three points, while all other points are lost. We measure BRN as the difference

between the participant's wager on Manager B to cooperate and the sum of cooperation the participant observed from their coworker(s) in the experience half of stage one.

IV. RESULTS

Table 1 presents descriptive statistics, and Figure 3 depicts average cooperation by period and condition. It is apparent in the three *Experience* conditions (*Task*, *Coworker*, *Case-Specific*) that cooperation increases once the control is introduced in period 7, while there is little change in cooperation around period 7 in the *Baseline* or *No Prior Experience* conditions. As both BRN and the FAE require that the control induce cooperation to increase inter-employee trust, we test the significance of the change in cooperation. Untabulated Wilcoxon rank-sum tests suggest the increase in cooperation from the control is significant when comparing periods 1-6 and periods 7-12 in each of the *Experience* conditions and when comparing cooperation between the *Baseline* and *No Prior Experience* conditions (each $p < 0.01$). Thus, the control we examine meets the criteria of being cooperation-inducing.

We also test for differences in cooperation across conditions. Untabulated results of pairwise comparisons with Bonferroni correction indicate that cooperation is higher in the *No Prior Experience* condition than in all other conditions during periods 1-6 (all p 's < 0.01), which is expected as the control is only in place in *No Prior Experience* in these periods. Further, cooperation is lower in the *Baseline* condition than in all other conditions during periods 7-12 (all p 's < 0.01), which is expected as the control is only absent in *Baseline* in these periods. We find no other differences in cooperation across conditions in periods 1-6 or 7-12 (all p 's > 0.30). In total, these results suggest our conditions do not create differences in cooperation beyond the effect of the control (as intended). In later analyses, we control for observed uncontrolled cooperation.¹⁶

¹⁶ As illustrated in Table 1, the proportion of individuals in each condition observing perfect cooperation (i.e., cooperation from their partner in all six periods) is generally consistent with overall cooperation. Untabulated Z-tests

[INSERT TABLE 1 AND FIGURE 3 ABOUT HERE]

Tests of Hypotheses H1-H4b

Our hypotheses relate to inter-employee trust. Our measure of trust is elicited once per participant (in stage two) and is truncated at 0 and 100, with many responses clustered at these endpoints (15% at 0 and 38% at 100). Thus, we use two-sided censored Tobit regressions when conducting tests of our hypotheses.

H1 replicates Coletti et al. (2005) and Garrett et al. (2019) and predicts that inter-employee trust is higher when employees have no experience prior to the control (*No Prior Experience*) than when there is no control (*Baseline*). Results in Panel A of Table 2 indicate that trust is higher in *No Prior Experience* than in *Baseline* (63.90 vs. 53.54; $p = 0.03$, one-sided). We next test the indirect effect of the control on trust through observed cooperation using the approach suggested by Mackinnon, Lockwood, and Williams (2004). Using 5,000 bootstrap samples, untabulated results suggest an indirect effect through cooperation that is significant at the 5% level. These results support H1.

[INSERT TABLE 2 ABOUT HERE]

H2a predicts that inter-employee trust is higher in *Task Experience* than in *Baseline*. H2b tests whether trust differs between *Task Experience* and *No Prior Experience*. Results in Panel A of Table 2 indicate that, trust is higher in *Task Experience* than in *Baseline* (64.22 vs. 53.54; $p = 0.02$, one-sided), supporting H2a and suggesting cooperation-inducing controls increase trust even when employees have prior task experience. This is consistent with our theory that employees

of proportions with Bonferroni corrections find the proportion of individuals observing perfect cooperation in periods 1-6 is lower in the *Task Experience* condition than all other conditions (all p 's < 0.01), which is reasonable as individuals in this condition are re-matched in each period with a new partner. However, there are no differences in *average* cooperation in periods 1-6 between *Task Experience* and the other uncontrolled conditions, which suggests participants in this condition observed similar levels of uncontrolled cooperation.

neglect task base rate information, enabling the FAE. Results in Panel B of Table 2 indicate that trust in *Task Experience* is not significantly different relative to *No Prior Experience* (64.22 vs. 63.90; $p = 0.94$, two-sided), suggesting task experience does not significantly mitigate the effect of the control on trust in our setting. Thus, we fail to reject our null H2b.

H3a predicts that inter-employee trust is higher in *Coworker Experience* than in *Baseline*. H3b tests whether trust differs between *Coworker Experience* and *No Prior Experience*. Results in Panel A of Table 2 indicate that trust is higher in *Coworker Experience* than in *Baseline* (66.18 vs. 53.54; $p < 0.01$, one-sided), supporting H2b and suggesting controls increase trust even when employees have prior coworker experience. This is consistent with our theory that employees neglect individual base rate information, enabling the FAE. Results in Panel B of Table 2 indicate that trust in *Coworker Experience* is not significantly different relative to *No Prior Experience* (66.18 vs. 63.90; $p = 0.70$, two-sided), suggesting coworker experience does not significantly mitigate the effect of the control on trust in our setting. Thus, we fail to reject our null H3b.

Although we are unable to predict directional effects of task and coworker experience ex ante in H2b and H3b, our inability to reject the null that general base rate information does not moderate the control's effect on trust is nevertheless important. The salience of base rate information in our design is high (experienced in close proximity to situational change, clearly observable, experienced personally, and summarized as additional feedback directly prior to trust measurement), and our operationalization of experience reduces the total observed cooperation relative to *No Prior Experience* (where the control is always present). Thus, our design creates a setting whereby the null results provide strong support for our theory. Specifically, these results provide evidence that the FAE plays a fundamental role in explaining the results of Coletti et al. (2005), Garrett et al. (2019), and our H1. To the extent trust arises when employees attribute their

coworker's cooperation to dispositional trustworthiness, the general base rate information generated by task and coworker experience should rationally lead to lower trust development relative to when employees have no prior experience. The fact that they do not do so suggests a major role of heuristic-driven trust assessments by employees (i.e., BRN and the FAE).

H4a tests whether trust differs between *Case-Specific Experience* and *Baseline*. H4b predicts that trust will be lower in *Case-Specific Experience* than in *No Prior Experience*, *Task Experience*, or *Coworker Experience*. Results in Panel A of Table 2 indicate there is no significant difference in inter-employee trust between *Baseline* and *Case-Specific Experience* (53.54 vs. 57.91; $p = 0.53$, two-sided). Thus, we fail to reject our null H4a. Further, results in Panel B of Table 2 indicate trust is lower in *Case-Specific Experience* relative to *No Prior Experience* (57.91 vs. 63.90; $p = 0.07$, one-sided), and results in Panel C of Table 2 indicate trust is lower in *Case-Specific Experience* relative to *Task Experience* and *Coworker Experience* ($p = 0.05$, one-sided, and $p = 0.02$, one-sided, respectively). Together, these results support H4b. The null result for H4a and the significant negative results for H4b are collectively consistent with our theory that by reducing BRN and the FAE, case-specific experience mitigates the positive effect of the control on trust. In sum, our hypotheses tests suggest cooperation-inducing controls increase inter-employee trust unless employees have case-specific prior experience, and case-specific experience reduces the positive effect of controls.¹⁷

¹⁷ Participants completed a second investment game with a previously unknown coworker (Manager C) to permit an exploratory analysis of the effects of experience and controls on 3rd-party trust. In untabulated results, we find that 3rd-party trust is highly correlated with coworker trust (0.59, $p < 0.01$) and similarly within each condition (all correlations 0.57-0.65, all p 's < 0.01). A one-way ANOVA with Bonferroni correction finds no significant differences in 3rd-party trust across any of our experimental conditions (all p 's > 0.52). Finally, we estimate censored Tobit regressions predicting 3rd-party trust as a function of combinations of coworker trust, control presence, individuals' propensity to trust, and observed cooperation in stage one. In all models in which coworker trust is included, the only significant predictor of 3rd-party trust is coworker trust. If coworker trust is excluded, propensity to trust is a significant predictor of 3rd-party trust ($p < 0.01$). Future research could further explore potential predictors of 3rd-party trust.

Additional Analysis

Trust Assessments when Controls are not Needed for Cooperation?

A main premise of our theory is that controls lead to trust because they increase cooperation. One consequence of giving participants experience prior to imposing the control is that participants may observe high cooperation absent the control (periods 1-6). Such instances offer less opportunity for the control to induce cooperation and may bias our tests, as high trust in such instances may be justified instead of an attributional error. To ensure our results are not driven by high levels of initial cooperation, we repeat our primary tests of our directional hypotheses (H2a, H3a, and H4b) controlling for the cooperation observed in periods 1-6. Untabulated results indicate continued support for H2a and H3a, with higher trust in *Task Experience* and *Coworker Experience* than in *Baseline* (both $p < 0.01$, one-sided). Further, trust is lower in *Case-Specific Experience* than in *Task Experience* and *Coworker Experience* ($p = 0.02$, one-sided, and $p < 0.01$, one-sided, respectively), supporting H4b.¹⁸

We next test the robustness of H2a, H3a, and H4b by excluding observations where participants observed perfect cooperation in periods 1-6 and repeating our primary tests of these predictions, still controlling for observed cooperation in periods 1-6. Untabulated results show trust is significantly higher in *Task Experience* (62.69) and *Coworker Experience* (52.65) than in *Baseline* (36.90; both $p < 0.01$, one-sided), supporting H2a and H3a. Further, trust is still lower in *Case-Specific Experience* (50.11) than in *Task Experience* and *Coworker Experience* ($p = 0.07$, one-sided, and $p < 0.01$, one-sided, respectively), supporting H4b.

¹⁸ It is difficult to make comparisons to *No Prior Experience* when controlling for cooperation in periods 1-6, as cooperation is confounded with the control's presence in this condition. Uncooperative behavior in *No Prior Experience* is more likely a signal of a coworker's poor understanding or antagonistic untrustworthiness (Guo, Lumineau, and Lewicki 2020) rather than a lack of goodwill, as the coworker harmed the employee at a personal cost to the coworker her/himself. Consistent with this, trust in imperfect cooperators is quite low in *No Prior Experience* (46.33).

Interestingly, results also show that trust is significantly higher in *Case-Specific Experience* than in *Baseline* ($p = 0.04$, two-tailed), contrary to our null result for H4a using all participants.¹⁹ Perfect cooperators exhibit similar trust in *Case-Specific Experience* and *Baseline* (71.22 vs. 77.49; $p = 0.41$, two-sided), but imperfect cooperators in periods 1-6 exhibit much less trust in *Baseline* (36.90) than in *Case-Specific Experience* (50.11). This suggests case-specific experience reduces, but does not fully mitigate, the FAE when the control changes the coworker's cooperation. Next, we examine the effect of experience on the FAE and BRN.

Effect of Experience on the Fundamental Attribution Error

Our theory suggests case-specific experience prior to the control will make employees less susceptible to the FAE. Thus, we first compare whether participants' attributions of cooperative behavior differ by experience type. The self-reported attributions of participants in *Case-Specific Experience* (4.68) do not differ significantly from those of participants in *Task Experience* or *Coworker Experience* (4.48 and 4.89, respectively; both $p > 0.34$, two-sided), failing to support our theory. However, the FAE is exhibited by a tendency to over-attribute the *increase* in cooperative behavior to the coworker's inherent cooperative tendencies. Thus, as a cleaner test of our theory, we measure the change in cooperation from periods 1-6 to periods 7-12 and test whether case-specific experience decreases the extent to which participants attribute increases in cooperation to Manager B's nature. Results in Table 3 show a negative interaction between the change in cooperation and *Case-Specific Experience*, relative to *Task* and *Coworker Experience* collectively ($p < 0.01$, one-tailed).²⁰ This suggests that case-specific experience decreases the extent to which increased cooperation is attributed to Manager B's cooperative nature relative to

¹⁹ Due to the aforementioned issues with comparisons to *No Prior Experience*, we cannot draw inferences for H2b and H3b's null predictions using this subsample.

²⁰ We observe similar results in untabulated tests comparing *Case-Specific Experience* to *Task Experience* and *Coworker Experience* individually (both $p < 0.01$, one-sided)

task experience and coworker experience, which provides support for the FAE playing a role in the increased trust from controls.²¹

[INSERT TABLE 3 ABOUT HERE]

Effect of Experience on Base Rate Neglect

Our theory also argues that employees with case-specific experience are less likely to suffer from BRN than those with only task or coworker experience. We measure BRN as the absolute value of the difference between the number of periods the participant's coworker cooperated in periods 1-6 and the number of points the participant bet that Manager B would cooperate in the final game. Larger differences indicate expectations that differ more from observed behavior in periods 1-6 (i.e., the base rate absent the control), suggesting greater BRN. We examine whether case-specific experience decreases the extent of BRN relative to task or coworker experience. Panel A of Table 4 indicates participants in *Case-Specific Experience* exhibit less BRN than those in *Task Experience* and *Coworker Experience* ($p = 0.02$, one-sided).²²

[INSERT TABLE 4 ABOUT HERE]

As an additional test of the effect of experience on BRN, we examine whether participants' bets on Manager B's cooperation are more likely to be influenced by the cooperation observed in

²¹ The coefficient on *Case-Specific Experience* is positive ($p < 0.05$), suggesting *Case-Specific Experience* increases attributions to a coworker's dispositional trustworthiness if a control does not affect cooperation. To probe this result, we examine participants who did not see a change in cooperation around the control. Of 41 participants with *Case-Specific Experience* that did not see a change in cooperation, 40 saw perfect cooperation in periods 1-6. Thus, these observations represent instances where participants observe actual trustworthy behavior without a control. Because participants with case-specific experience have case-specific information, it is reasonable they would be more likely to attribute continued cooperation to their coworker's dispositional trustworthiness. Importantly, high scores on this measure in such cases are not an attributional error, but an appreciation of a coworker's inherent trustworthiness without a control. Supporting this discussion, repeating this analysis while either controlling for cooperation in periods 1-6 or excluding those who observed perfect cooperation each yield an insignificant *Case-Specific Experience* term (each $p > 0.22$, two-sided) but retain a significant interaction term (each $p < 0.06$, two-sided).

²² Results are similar if we calculate BRN as an indicator variable that takes on the value of 1 if the employee's bet exceeds the number of periods of cooperation they observed from their coworker(s) in periods 1-6 (i.e., if the employee overestimates Manager B's cooperation), and 0 otherwise (0.19 vs. 0.26; $p = 0.06$, one-sided). We also observe greater BRN in *Case-Specific Experience* relative to *Coworker Experience* individually ($p < 0.01$, one-tailed), but not relative to *Task Experience* ($p = 0.17$, one-tailed), though the latter effect is directionally consistent with expectations.

periods 1-6 (i.e., base rate information) or Manager B's cooperation in periods 7-12. Results in Panel B of Table 4 indicate that the bets of participants in *Task Experience* are not associated with period 1-6 or period 7-12 cooperation. Consistent with BRN, the bets of participants in *Coworker Experience* are associated with period 7-12 cooperation ($p = 0.08$, two-sided). However, consistent with lower BRN, the bets of participants in *Case-Specific Experience* are associated with cooperation in periods 1-6 ($p < 0.01$, two-sided) and not periods 7-12. Further, as indicated in column 4, *Case-Specific Experience* increases the likelihood participants' bets are associated with period 1-6 cooperation ($p = 0.01$, one-sided).²³ These results suggest that case-specific experience mitigates BRN.

Finally, we test whether BRN helps explain the effect of experience on trust. Results in Panel A of Table 4 show *Case-Specific Experience* has a negative effect on trust, relative to *Task Experience* or *Coworker Experience* ($p = 0.02$, one-sided). Results indicate BRN also significantly predicts trust ($p < 0.01$, two-sided) and its inclusion diminishes slightly the effect of *Case-Specific Experience* on trust, which remains significant ($p = 0.03$, one-sided). We test the indirect effect of *Case-Specific Experience* on trust through BRN using the approach suggested by Mackinnon et al. (2004). Using 5,000 bootstrap samples, untabulated results show an indirect effect that is significant at the 10% level. Collectively, these results suggest that the reduction in inter-employee trust from controls due to case-specific experience operates, at least in part, through BRN.

Propensity to Trust

We next investigate whether the effect of case-specific experience on trust development is influenced by employees' innate propensity to trust.²⁴ Employees with a high propensity to trust

²³ Results are similar if we compare *Case-Specific Experience* to *Task Experience* and *Coworker Experience* individually ($p = 0.04$, one-sided, and $p = 0.01$, one-sided, respectively).

²⁴ Propensity to trust is a stable trait (McKnight and Chervany 1996) and is unlikely to be affected by our manipulations. In untabulated results, we find no differences in propensity to trust across conditions overall, for

are more likely to attribute cooperation to their coworker's trustworthiness, which could reduce the ability of case-specific experience to mitigate the FAE. Consistent with our main tests, results in column 1 (2) of Table 5 show, relative to *No Prior Experience* (*Task Experience* and *Coworker Experience*), *Case-Specific Experience* reduces trust for participants with low propensity to trust (both p 's < 0.01). However, we observe a positive interaction between *Case-Specific Experience* and high propensity to trust on trust ($p < 0.01$, two-sided and $p = 0.01$, two-sided, respectively), indicating that a high propensity to trust mitigates the effect of case-specific experience.

[INSERT TABLE 5 ABOUT HERE]

We also test whether propensity to trust interacts with case-specific experience to affect BRN. Consistent with our main tests, results in column 3 of Table 5 indicate that, relative to *Task Experience* and *Coworker Experience*, *Case-Specific Experience* reduces BRN for participants with a low propensity to trust ($p < 0.01$). However, we observe a marginally significant positive interaction between *Case-Specific Experience* and high propensity to trust ($p = 0.08$, two-sided), indicating a high propensity to trust mitigates the effect of case-specific experience.²⁵ Overall, these results suggest the effect of case-specific experience on the development of inter-employee trust and BRN is diminished for employees with a high propensity to trust. As individuals with a high propensity to trust are more likely to give the benefit of the doubt (akin to BRN for non-cooperative base rate information), this result provides further support for our finding that BRN plays a meaningful role in how cooperation-inducing controls increase inter-employee trust.

V. CONCLUSION

participants observing perfect cooperation, or for those observing imperfect cooperation in periods 1-6 (all Bonferroni-correct p 's > 0.99).

²⁵ We also test whether propensity to trust interacts with *Case-Specific Experience* to affect the extent to which participants based their bets on Manager B's cooperation on period 1-6 and period 7-12 cooperation. We find a negative interaction between *Case-Specific Experience*, period 1-6 cooperation, and propensity to trust ($p = 0.08$, two-sided). This provides additional evidence that propensity to trust affects the relationship between experience and BRN.

We conduct an experiment to study the effects of cooperation-inducing controls and employees' prior experience on the development of inter-employee trust. We find, for employees with no experience prior to the control and for employees who have experience with the task or coworker prior to the control, that the presence of a cooperation-inducing control increases inter-employee trust compared to when the control is absent. However, we find the presence of the control increases trust less when employees have case-specific experience (with a specific coworker on the task of interest) prior to the control. Our supplemental analyses provide evidence that case-specific experience mitigates employees' neglect of base rate information and subsequent misattributions of others' cooperative behavior.

Our results and theory contribute broadly to practice and research regarding the effects of controls on trust. Specifically, our result that cooperation-inducing controls increase inter-employee trust in settings where employees have general base rate information from prior task or coworker experience, coupled with our more direct measures of the fundamental attribution error, provide important improvements in theoretical specification and tests of robustness that support the generalizability of claims that controls can increase inter-employee trust. We further develop this theory by demonstrating the novel role of base rate neglect in enabling controls' positive effect on inter-employee trust, thus illustrating an environmental factor (employees' prior case-specific experience) that mitigates this effect.

Our study also contributes to our collective understanding of the control-trust relation. Decisions related to the design, implementation, and execution of controls are complex and multifaceted, with numerous costs and benefits for firms to consider. In this study, we focus on an important potential indirect benefit to the firm from cooperation-inducing controls—increased inter-employee trust. As contracts are rarely complete in practice, this increased trust is valuable

to the firm because it facilitates desirable behavior on uncontrolled aspects of tasks, such as increased cohesion and future cooperation (Ashforth and Mael 1989; Dirks and Ferrin 2001; Ashforth, Harrison, and Corley 2008). Our results suggest the direct benefit of controls is unaffected by employees' prior experience, but the total benefit of controls to firms, which is weighed against the total relevant costs, depends on employees' prior experience. Specifically, our results suggest firms can benefit from increased inter-employee trust even when employees have prior experience with either a task or coworker, as with employees who are new to the firm or whose team has experienced turnover in staff or a shift in job duties. However, firms are less likely to observe the same benefits if placing a new control on a task currently being performed by an existing team.

Our study also suggests possible avenues for future research. Research on potential moderators of the trust-control relation focus almost exclusively on the propensity for controls to damage vertical trust between employees and the firm (e.g., Christ et al. 2012; Cardinaels and Yin 2015). These studies ignore the indirect benefit of controls from improved horizontal trust between employees, just as the research related to horizontal trust ignores the indirect cost of controls from lost vertical trust between employees and the firm. As a result, research on the holistic, dynamic trust-control relation is fractured (Long and Sitkin 2018). Given the improved theoretical understanding of cooperation-inducing controls' effect on inter-employee trust from this study and the theories employed in the studies on the cost of control from losses of vertical trust, we encourage future research to explore the interactions and interplay of these two research streams together. For example, future research could examine whether the effect of employees' prior experience on inter-employee trust from controls depends on whether the control is perceived as being chosen by their superior.

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TABLE 1
Descriptive Statistics

	<i>Baseline</i> (n = 100)	<i>No Prior Exp</i> (n = 105)	<i>Task Exp</i> (n = 113)	<i>Coworker Exp</i> (n = 114)	<i>Case-Specific Exp</i> (n = 111)
Cooperation (1-6)¹	4.27 (1.66)	5.68 (0.70)	3.92 (1.60)	3.99 (2.22)	4.36 (1.66)
Cooperation (7-12)²	3.82 (2.34)	5.60 (0.99)	5.47 (1.21)	5.46 (1.11)	5.67 (0.82)
Perfect Cooperation (1-6)³	41%	80%	22%	40%	37%
Perfect Cooperation (7-12)⁴	44%	83%	78%	74%	82%
Coworker Trust⁵	53.54 (40.68)	63.90 (40.05)	64.22 (36.40)	66.18 (36.14)	57.91 (37.48)
Trust Returned to Manager B⁶	[n = 75] 30.13% (23.52%)	[n = 87] 29.72% (23.95%)	[n = 103] 37.06% (26.05%)	[n = 103] 32.79% (25.40%)	[n = 91] 37.93% (24.29%)
Trustworthy Attribution⁷		4.75 (1.50)	4.48 (1.71)	4.89 (1.62)	4.68 (1.69)
Base Rate Neglect⁸			2.03 (1.49)	2.41 (1.85)	1.83 (1.61)
Propensity to Trust⁹	31.31 (8.22)	30.78 (7.51)	30.66 (6.86)	31.98 (7.44)	30.74 (8.72)

Notes:

Mean (std. dev.) displayed for all variables except count variables (Perfect Cooperation).

¹ Range 0-6. The number of periods in the first six of stage one in which the participant observed cooperation from their coworker in the contextualized prisoner's dilemma game. In the *Coworker Experience* condition, we measure the number of periods (out of 6 again) in which the participants' partner contribution to the public good exceeded the mean contribution (4.83).

² Range 0-6. The number of periods in the last six periods of stage one in which the participant observed cooperation from their coworker in the controlled prisoner's dilemma game.

³ The percent of participants who observed cooperation in all of the first six periods of stage one.

⁴ The (percent of participants who observed cooperation in all of the last six periods of stage one.

⁵ Range 0-100. The number of points the participant sent to their coworker (Manager B) in the first trust game of stage two.

⁶ The percentage of points the participant returned to Manager B in the first trust game of stage two. Since participants receiving 0 points must be excluded, the number of participants represented is noted in brackets.

⁷ Range 1-7. Self-reported attribution of cooperative behavior in the post-experiment questionnaire, where higher (lower) values equate to the individual attributing cooperation to their coworker's trustworthiness (the presence of the control).

⁸ Absolute value of the difference between the participant's wager on Manager B to cooperate in the final game of the experiment and Cooperation (1-6) as defined in this table.

⁹ Range 0-56. Sum of participants' responses to eight post-experiment questionnaire items used to measure propensity to trust (Mayer and Davis 1999).

TABLE 2
Effect of Control Presence and Prior Experience on Inter-Employee Trust

Panel A: Effect of Control on Coworker Trust Relative to *Baseline* by Experience Type

Condition of Interest	<i>No Prior Exp</i> (H1)	<i>Task Exp</i> (H2a)	<i>Coworker Exp</i> (H3a)	<i>Case-Specific Exp</i> (H4a)
Control Presence	27.02** (1.92)	24.51** (2.19)	28.43*** (2.46)	7.02 (0.64)
Constant	59.55*** (6.02)	58.29*** (7.20)	58.45*** (7.02)	58.26*** (7.24)
Obs	205	213	214	211

Panel B: Effect of Experience, Given Control, Relative to *No Prior Experience*

Condition of Interest	<i>Task Exp</i> (H2b)	<i>Coworker Exp</i> (H3b)	<i>Case-Specific Exp</i> (H4b)
Task Exp	0.96 (0.08)		
Coworker Exp		4.50 (0.38)	
Case-Specific Exp			-16.91* (-1.48)
Constant	82.64*** (9.66)	83.28*** (9.45)	82.49*** (9.72)
Obs	218	219	216

Panel C: Effect of Control and Case-Specific Experience on Inter-Employee Trust

	All Conditions	<i>Task Exp</i> vs. <i>Case-Specific Exp</i> (H4b)	<i>Coworker Exp</i> vs. <i>Case-Specific Exp</i> (H4b)
Control Presence	25.29*** (2.74)		
Case-Specific Exp	-18.27** (-2.09)	-15.70** (-1.68)	-19.22*** (-2.00)
Constant	58.25*** (7.26)	79.88*** (11.80)	83.58*** (11.99)
Obs	543	224	225

Notes:

Each of the reported results are estimated using Tobit regressions, censored at the constrained minimum (0) and maximum (100) for our dependent variable.

The dependent variable in these analyses is Coworker Trust, defined as the number of points the employee sent to their coworker (Manager B) in the first trust game of stage two.

Control Presence is a dichotomous variable equal to one when the probabilistic audit was present in periods 7-12 in stage one (*No Prior Experience*, *Task Experience*, *Coworker Experience*, *Case-Specific Experience*) and equal to zero otherwise (*Baseline*).

Task Exp. is a dichotomous variable equal to one when an employee had experience with the task, but not their coworker (Manager B) prior to the control and equal to zero otherwise.

Coworker Exp. is a dichotomous variable equal to one when an employee had experience with their coworker (Manager B), in a different task prior to the control and equal to zero otherwise.

Case-Specific Exp. is a dichotomous variable equal to one when an employee had experience with their coworker (Manager B) in the to-be controlled task prior to the control and equal to zero otherwise.

The tests in Panel A are comparisons between *Baseline* and the Condition of Interest presented at the top of each column, excluding other data for that column's analysis.

The tests in Panel B are comparisons between *No Prior Experience* and the Condition of Interest presented at the top of each column, excluding other data for that column's analysis.

The tests in Panel C included the data for the conditions listed at the top of each column.

Reported results are coefficient (z-statistic) for each variable in each estimate.

*, **, and *** represent significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Tests of significance are two-tailed, unless related to a directional prediction. One-tailed significance tests are presented in **bold**.

TABLE 3
Effect of Form of Prior Experience and Observed Cooperation on Trustworthy Attribution

ΔCoop	-0.19*** (-3.73)
Case-Specific Exp	0.48** (2.09)
ΔCoop x Case-Specific Exp	-0.40*** (-3.86)
Constant	4.97*** (38.40)
Obs	338

Notes:

The reported results are estimated using OLS regressions.

The dependent variable in these analyses is Trustworthy Attribution, defined as the employee's self-reported attribution of their coworker's cooperative behavior on a 7-point Likert scale, with higher (lower) values indicating behavior is more attributed to the coworker's trustworthiness (the presence of the control).

Δ Coop is defined as the difference between the total cooperation observed by the participant in periods 7-12 (control in place) and the cooperation observed in periods 1-6 (no control in place).

Δ Coop x Case-Specific Exp. is the interaction of Δ Coop and Case-Specific Experience.

The test reported in this table only compares data from the *Task Experience*, *Coworker Experience* and *Case-Specific Exp.*

All other variables are as previously defined.

Reported results are coefficient (t-statistic) for each variable in each estimate.

*, **, and *** represent significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Tests of significance are two-tailed, unless related to a directional prediction. One-tailed significance tests are presented in **bold**.

TABLE 4

Effect of Case-Specific Experience on Base Rate Neglect and Inter-employee Trust

Panel A - Effect of Case-Specific Experience on Base Rate Neglect

Dependent Variable	Base Rate Neglect	Coworker Trust	Coworker Trust
Case-Specific Exp	-0.39** (-2.03)	-17.52** (-2.13)	-15.26** (-1.86)
Base Rate Neglect			5.66*** (2.36)
Constant	2.22*** (20.12)	81.83*** (16.61)	69.32*** (9.85)
Obs	338	338	338

Panel B – Effect of Case-Specific Experience and Observed Cooperation on Bet on Cooperation

Condition of Interest	Task Exp	Coworker Exp	Case-Specific Exp	All Exp
Coop 1-6	0.12 (1.03)	0.05 (0.56)	0.39*** (3.64)	0.09 (1.33)
Coop 7-12	0.02 (0.16)	0.34* (1.78)	-0.15 (-0.68)	0.16 (1.37)
Case-Specific Exp				0.42 (0.29)
Coop 1-6 x Case-Specific Exp				0.30** (2.24)
Coop 7-12 x Case-Specific Exp				-0.31 (-1.18)
Constant	2.47*** (2.72)	0.93 (0.94)	2.22* (1.83)	1.80*** (2.84)
Obs	113	114	111	338

Notes:

Each of the reported results are estimated using two-sided censored Tobit regressions for the last two columns in Panel A and using OLS regressions for all other results.

The dependent variable in Panel A is defined above each column as Base Rate Neglect or Coworker Trust. The dependent variable in Panel B analyses is *Bet on Cooperation*, defined as the employee's wager on Manager B to cooperate in the final game of the experiment.

Base Rate Neglect is defined as the absolute value of the difference between the employee's wager on Manager B to cooperate in the final game (Bet) and Coop 1-6.

Coop 1-6 is defined as the number of periods in the first six of stage one in which the employee observed cooperation from their coworker in the contextualized prisoner's dilemma game. In the *Coworker*

Experience condition, we measure cooperation as present if the employee's partner's contribution to the public good exceeded the mean contribution (4.83).

Coop 7-12 is the number of periods in the last six periods of stage one in which the employee observed cooperation from their coworker (Manager B) in the controlled prisoner's dilemma game.

Coop 1-6 x Case-Specific Exp. is the interaction of *Coop 1-6* and *Case-Specific Exp.*

Coop 7-12 x Case-Specific Exp. is the interaction of *Coop 7-12* and *Case-Specific Exp.*

The tests reported in this table only compare data from the *Task Experience*, *Coworker Experience* and *Case-Specific Exp.*

All other variables are as previously defined.

Reported results are coefficient (t-statistic for OLS and z-statistic for Tobit) for each variable in each estimate.

*, **, and *** represent significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Tests of significance are two-tailed, unless related to a directional expectation. One-tailed significance tests are presented in **bold**.

TABLE 5
Effect of Propensity to Trust on Inter-Employee Trust and Base Rate Neglect

	(1) Coworker Trust	(2) Coworker Trust	(3) Base Rate Neglect
Case-Specific Exp	-44.23*** (-3.00)	-36.11*** (-3.32)	-0.70*** (-2.69)
High Propensity	-8.42 (-0.52)	7.85 (0.85)	-0.29 (-1.32)
Case-Specific Exp x High Propensity	62.69*** (2.79)	42.19*** (2.61)	0.68* (1.77)
Constant	85.10*** (8.05)	77.73*** (12.28)	2.35*** (15.85)
Obs	216	338	338
Comparison Conducted	<i>Case-Specific Exp</i> vs. <i>No Prior Exp</i>	<i>Case-Specific Exp</i> vs. <i>Task Exp</i> and <i>Coworker Exp</i>	<i>Case-Specific Exp</i> vs. <i>Task Exp</i> and <i>Coworker Exp</i>

Notes:

The reported results are estimated using Tobit regressions, censored at the constrained minimum (0) and maximum (100) for our dependent variable for columns (1) and (2) and using OLS for column (3).

The dependent variable is shown above each column as Base Rate Neglect or Coworker Trust.

High Propensity is defined as employees with a high propensity to trust, defined as an above-median value for propensity to trust – measured as the sum of eight post-experiment questionnaire items used to measure adapted from Mayer and Davis (1999)

Case-Specific Exp. x High Propensity is the interaction of *Case-Specific Exp.* and *High Propensity*.

The tests reported in this table only compare data from *No Prior Experience*, *Task Experience*, *Coworker Experience* and *Case-Specific Exp.*

The data used for each comparison are presented at the bottom of each column.

All other variables are as previously defined.

Reported results are coefficient (t-statistic) for each variable in each estimate.

*, **, and *** represent significance at $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively.

Tests of significance are two-tailed.

FIGURE 1
Stage One Payoff Structures

Panel A: Summary of Payoff Structure

- If a manager devotes high resources to the project, that manager is charged 15 points.
- Joint project profits are increasing in the level of resources dedicated to the project:
 - If both managers choose low, joint project profit = 10.
 - If one manager chooses low and one chooses high, joint project profit = 30.
 - If both managers choose high, joint project profits = 50.
- Joint project profits are shared equally by the two managers.
- Control System in Place Only: if the auditor visits and a manager devoted high resources, that manager earns a bonus 15 points. (Probability of auditor visit = 80% each period.)

Panel B: Payoff Structure when no control is in place^a

	Manager B	
	Low	High
Manager A		
Low [*]	5, 5 ^{**}	15, 0
High	0, 15	10, 10

Panel C: Payoff Structure when control is in place^b

	Manager B	
	Low	High
Manager A		
Low [*]	5, 5 ^{**}	15, 12
High	12, 15	22, 22

Notes:

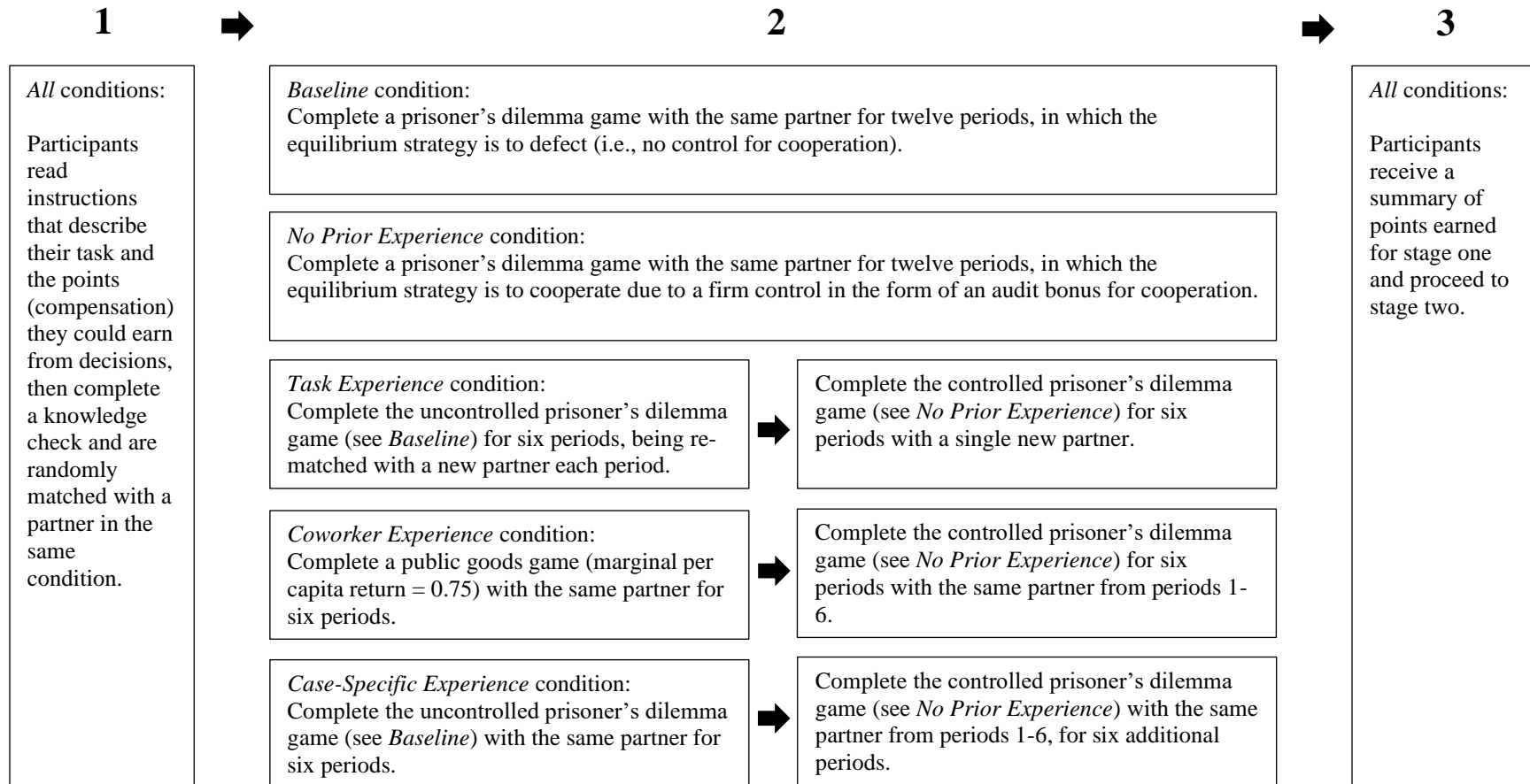
* Represents level of resources dedicated to the joint project (low or high).

** Represents the payoff to Manager A and Manager B, respectively.

^a Calculated as 50% of joint profit minus resource cost (15 points if the manager chooses high resources).

^b Calculated as 50% of joint profit minus resource cost (15 points if the manager chooses high resources) plus bonus (80% x 15 points if the manager chooses high resources).

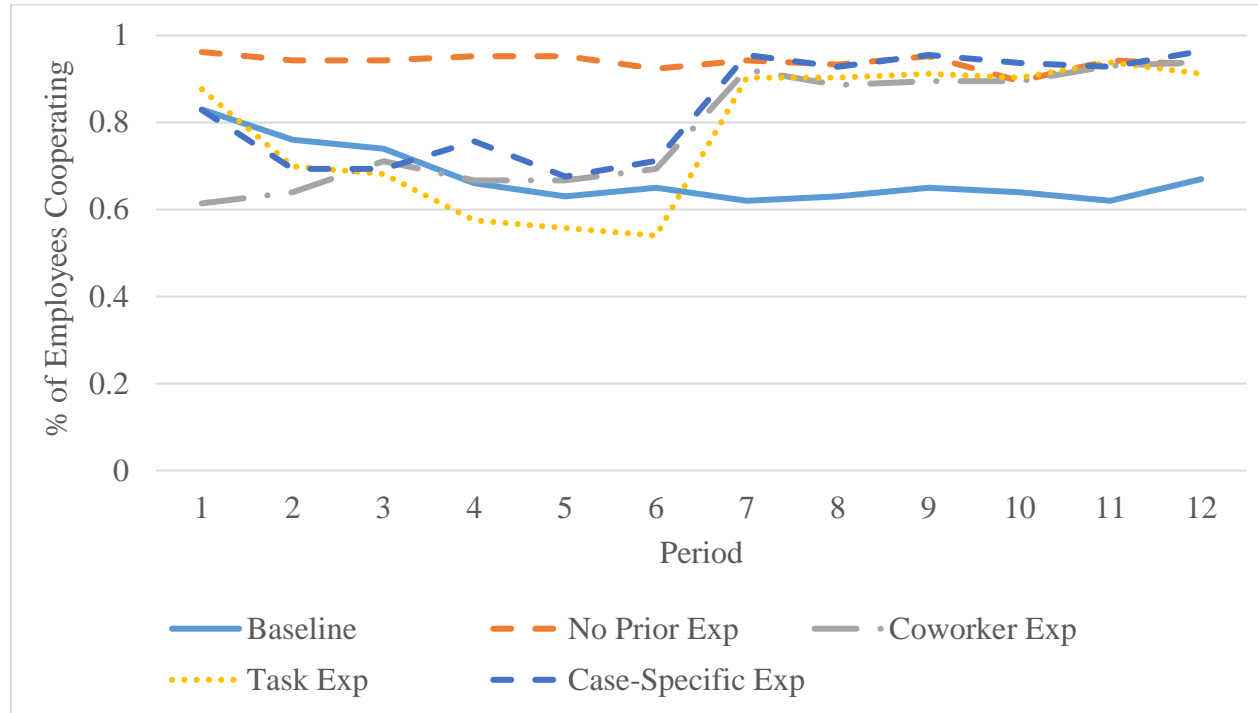
FIGURE 2
Stage One Procedures



Notes:

This figure illustrates the stage one task completed by participants, with manipulations for task experience, coworker experience, and the absence/presence of a control noted as applicable for each condition.

FIGURE 3
Cooperation Rate by Period and Condition



Notes:

This figure illustrates the percentage of participants who chose to cooperate in each of the 12 periods of stage one by condition. For participants in the *Coworker Experience*, *Task Experience*, and *Case-Specific Experience* conditions, period 7 is the first period in which the task is subject to the cooperation-inducing control. In the *Coworker Experience* condition, for purposes of this illustration, cooperation is defined as contributing 5 or 6 points (out of 6) to the public good in the uncontrolled public good task (periods 1-6).