WHEN THE CAT'S (FAR) AWAY…
THE EFFECT OF CONTROL CENTRALIZATION AND COMPENSATION INTERDEPENDENCE ON PERFORMANCE MISREPORTING

BY

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DISSERTATION

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ABSTRACT

This study investigates how control features influence employees' perceptions of controls and, subsequently, their reporting behaviors. I examine whether implementing a more centralized versus decentralized monitoring control influences employees' perceptions of whether the control will detect misreporting. In an experiment that holds the true detection rate constant, I find that participants perceive a relatively decentralized control as more likely to detect misreporting than a centralized control. Because decisions about monitoring controls and compensation systems are interrelated (O'Donnell 2000) and made contemporaneously (Brenner and Ambos 2013), I also examine whether compensation interdependence influences the effect that control centralization has on misreporting. Prior research shows that compensation interdependence leads to more aggressive reporting behavior (Sutter 2009). This aggressive reporting behavior is likely to reduce sensitivity to differences in subjective assessment of a control's detection likelihood resulting from control centralization. Consistent with this reasoning, I find that the difference in the extent of performance misreporting between a centralized and decentralized control is greater when compensation interdependence is low than when compensation interdependence is high. This study contributes to the accounting literature by defining control centralization and providing the first academic research on a consequence of control centralization. Moreover, this study provides theory and evidence of the joint influence of control centralization and compensation interdependence on misreporting, and it provides evidence that control design choices can influence employees' perceptions of control effectiveness.

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TABLE OF CONTENTS

CHAPTER 1: INTRODUCTION

Within an organizational hierarchy, performance information often flows from lower levels of the hierarchy to higher levels. For example, division managers in large firms report their division's performance to corporate managers. Similarly, some lower-level employees, such as staff consultants, self-report their hourly performance to their superiors. Reporting employees have both the opportunity and incentive to misreport their true performance. Opportunity arises from the inherent information asymmetry between reporting employees and their managers (Rajan and Saouma 2006), while incentives exist because superiors use reported information for compensation and/or promotion decisions (Guidry et al. 1999) as well as internal resource allocation (Heinle et al. 2014). To combat misreporting, firms develop and implement controls designed to detect performance misreporting (Baiman and Demski 1980; Morton 1993).

This study examines how two features of a control system jointly influence employees' propensities to misreport their performance. One feature is the centralization of a monitoring control. Control centralization is defined as the extent to which a control is consolidated and coordinated within a firm (Anderson et al. 2010). The centralization of a control is determined by the states of several components of the control, such as the social distance between an employee and the control personnel, the standardization of the control within the firm and, importantly, the proximity between the reporting employee and the control itself, which varies cross-sectionally among firms. Specifically, a survey by Ernst & Young (2012) finds that 49 percent of firms have their internal audit function centralized in one location while the remaining 51 percent use a more dispersed, decentralized internal audit function. The report also highlights several control centralization determinants, including geographic dispersion and organizational structure, which results in the cross-sectional variation. However, the consequences of control centralization have

received little to no attention. Knowing the consequences of control centralization is critical to understanding how managers' choices related to the different components of control centralization influence employees' perception of control effectiveness.

The second feature is the extent to which compensation is interdependent. That is, to what degree compensation is contingent upon group-based as opposed to individual-based performance. For example, bonuses contingent upon division performance have relatively high interdependence, while individual bonuses have relatively low interdependence (Lazear and Shaw 2007). Recent studies examine the direct effect that compensation interdependence can have on misreporting and generally find greater compensation interdependence leads to greater misreporting (Church et al. 2012; Maas and Van Rinsum 2013). However, these studies do not document how compensation interdependence influences the effectiveness of other controls designed to detect misreporting. This is important to consider because firms do not implement compensation-related controls in isolation, rather, they implement these controls within a more general control structure. As Grabner and Moers (2013) point out, "if an organization does not take the interdependencies among MC [management control] practices into account, it will forego some benefits or incur some unnecessary costs (p. 409).

I focus on the joint effect of control centralization and compensation interdependence for three reasons. Decisions about monitoring mechanisms and compensation systems are interrelated (O'Donnell 2000) and often made contemporaneously (Brenner and Ambos 2013). Also, both formal monitoring and compensation design are two controls used by large, multinational firms to manage foreign subsidiaries (O'Donnell 2000; Roth and O'Donnell 1996). Additionally, the determinants of control centralization, such as business structure, cost, and geographic diversity (Ernst & Young 2012) are also determinants of compensation design within

foreign subsidiaries (Govindarajan 1988; Roth and O'Donnell 1996). These findings highlight that firms are likely to make control centralization and compensation interdependence decisions concurrently and these decisions play a vital role in how a firm attempts to influence employee behavior, especially in large, multinational firms.

I rely on psychological distance theory to make my first prediction. Psychological distance is a subjective feeling of an object or event being close to, or far from, oneself (Magee and Smith 2013). Differences in psychological distance influence individuals' perceptions of an event, including its likelihood of occurrence (Trope and Liberman 2010). Specifically, events physically, temporally, or socially closer to an individual are perceived as more likely to occur than events farther from an individual (Trope, Liberman, and Wakslak 2007). In line with this theory, I predict that greater control centralization increases performance misreporting by decreasing employees' perceptions that the control will detect misreported performance. I also predict that compensation interdependence will moderate this effect. Prior research shows that compensation interdependence increases risky decision-making (Sutter 2009), suggesting a larger increase in perceived detection rate is required to reduce dishonest reporting with interdependent compensation. Put another way, the fixed change in perceived detection likelihood resulting from control centralization will have less of an effect on misreporting when compensation interdependence is high versus low. Therefore, I predict that higher compensation interdependence moderates the effect that control centralization has on misreporting.

To test these predictions, I use a 2 x 2 (between subjects) x 6 (within subjects) mixed experiment design. Participants assume the role of an employee making a reporting decision to a superior. In each of six rounds, participants privately receive the actual revenue of a project and then have the option to over-report the revenue to increase their compensation. After each

reporting decision, a review process takes place, operationalizing an investigation of the difference between participants' reported and actual revenues. Participants are unaware of the true detection likelihood of this review, but they are able to assess it subjectively. If the review process detects misreporting, the participant loses his or her compensation for the round. I manipulate the location of the review process at two levels between subjects: *Near Review* (i.e., decentralized control) and *Far Review* (i.e., centralized control). I also manipulate the extent of compensation interdependence at two levels between subjects: *High CI* and *Low CI*.

Results of my experiment support the prediction that control centralization influences misreporting through employees' subjective assessments that the control will detect misreported performance. Although true detection likelihood is consistent across conditions, participants in the *Near Review* condition assess a higher detection likelihood than participants in the *Far Review* condition, independent of compensation interdependence. However, compensation interdependence moderates the extent to which subjective detection likelihood influences reporting behavior. Specifically, given low compensation interdependence, less misreporting occurs with a decentralized control than with a centralized control. However, when compensation interdependence is high, participants misreport to the same degree, regardless of the assessed detection likelihood driven by control centralization.

Additional analyses indicate that participants' perceptions of other participants' misreporting also influences reporting behavior. When compensation interdependence is low, participants perceive other participants are more likely to misreport with a centralized control than with a decentralized control and increase their own misreporting. However, when compensation interdependence is high, control centralization has no effect on the perception that other participants are misreporting. These findings suggest that control centralization and

compensation interdependence jointly influence descriptive norms (i.e., perceptions of others' behavior), another mechanism that influences reporting behavior.

This study contributes to the accounting literature by offering theory and evidence about the interaction among controls and their joint influence on reporting behavior. While prior literature identifies the direct effect that compensation interdependence has on misreporting (Church et al. 2012; Maas and Van Rinsum 2013), my study contributes by highlighting a moderating effect. This moderating effect is important because it suggests that compensation interdependence can reduce the effectiveness of controls designed in part to reduce misreporting. In addition to documenting this moderating effect, this study also provides evidence of the cognitive processes underlying why it occurs. Specifically, this study provides a glimpse into how perceptions about a control itself and perceptions about how others respond to a control underlie the moderating effect. Practitioners, including designers of control systems and compensation systems, can leverage this study when implementing controls. In particular, these parties can consider the behavioral effect of control centralization on reporting behavior and can consider that greater compensation interdependence will mute this effect.

This study also examines one consequence of control centralization. While there is some understanding of the determinants of control centralization decisions (Ernst & Young 2012), there is little research on whether control centralization can influence behavior. This study documents that control centralization decisions can influence misreporting, which suggests that future research examining behavioral consequences of control centralization could be beneficial.

Finally, this study contributes to control literature by examining how features of two controls interact. Understanding one control's influence on another advances our understanding of how a control system holistically helps a firm meet its objectives. This study provides an

example of one control feature, compensation interdependence, which can reduce the effectiveness of another control feature, centralization, in changing reporting behavior.

CHAPTER 2: REVIEW AND IMPLICATIONS OF PRIOR RESEARCH

2.1 Overview of Literature Review

The intent of this literature review is to summarize prior research related to my dissertation topic and to highlight unanswered research questions that either I explore within this dissertation or others can explore with future research. I format the literature review as follows: I first summarize research related to dishonest reporting, my dependent variable of interest. Since both my independent variables are control design choices, I then summarize research related to control systems. Finally, I discuss research that has examined the effects that physical distance between a person and an object can have on a person's perception of an object. I review this stream of literature because the operationalization of one of my independent variables, control centralization, involves the physical distance between a control and an employee.

2.2 Literature on Dishonest Reporting through Budgetary Slack

A prevalent setting within accounting to study misreporting is participative budgeting. Many firms have a budgeting process that involves some level of subordinate participation (Shields and Shields 1998). Participative budgets typically involve divisional mangers requesting necessary funds and the firm allocating resources based on those requests. One of the difficulties associated with participative budgeting is the propensity for managers to over-report the amount of funds required for their division (Onsi 1973). Specifically, managers have incentives to build slack into their budgetary requests.

Early literature examines the determinants of budgetary slack with mixed results. Merchant (1985) finds that the propensity to create slack is low when managers actively participate in budgeting, especially when the administrative system is relatively stable. However, Young (1985) finds that managerial participation increases budgetary slack. Interestingly, Young

(1985) also finds that the average amount of budgetary slack is no different for those participants who have private information about productive capabilities versus those who do not have private information. This suggests that information asymmetry is not a significant determinant of budgetary of slack. However, subsequent research suggests that information asymmetry is a significant predictor of budgetary slack (Dunk 1993).

Early research also examines whether individual preferences are determinants of slack. Young (1985) finds that risk preferences influence slack creation such that risk-averse subordinates build more slack into budgets than non-risk-averse subordinates do. Building on this study, Kim (1992) shows that environmental factors can influence risk preferences and budgetary slack. Specifically, Kim (1992) finds that previous exposure to missing (meeting) a budgetary goal can change an individual's risk preferences, resulting in the individual building more (less) slack into future budgets.

Recent research finds that individuals with a stronger ethical position against lying tend to build less slack into their budget reports (Stevens 2002; Douglas and Wier 2000). Additionally, higher preferences for honesty by subordinates decreases the extent to which superiors build slack into their budget (Church, Hannan, and Kuang 2012). Taken together, this stream of research suggests that the determinants of budgetary slack are specific to both the environment and individual preferences of those involved in the budget process.

Future Research Questions

Research on slack creation has focused on both environmental and personality variables to explain slack levels. However, several unanswered questions remain. For example, how do different controls within a control system influence a manager's propensity to incorporate slack? Murphy (2001) shows that a majority of firms base their incentive contract targets on budgets.

However, there is little research on whether linking incentives to budgetary targets influences budgetary slack. Future research could also examine whether layers of managerial review influence slack. For example, most studies assume two people are involved in the budgeting process. Yet in practice, budgets go through several layers of approval before top management allocates resources. It is unclear whether the theories used to test budgetary slack creation with a single principal and a single agent generalizes to settings with more hierarchical levels of review.

Researchers could also examine the consequences of budgetary slack from the firm's perspective. The literature's focus has been on the agent's creation of slack. However, top management's response to the agent's budgetary request is the true output of the budgeting process. Future research could examine whether top management can correctly assess the subordinate's slack and adjust their resource allocation decision accordingly. Specifically, future research can expand beyond the creation of slack to investigate whether superiors incorporate slack in their final resource allocation decisions. In other words, the literature can begin to focus on the acceptance of slack by the principal. This perspective creates new research questions. For example, environmental and personality variables are important not only for slack creation, but also for the principal's ultimate approval of a slack budget.

2.3 Literature on Dishonest Reporting in Other Settings

In addition to reviewing literature on budgetary slack, I also review literature on misreporting more broadly (i.e., not just related to budgetary slack). I begin with Baiman and Lewis (1989). In this study, the authors create two compensation contracts for participants. In one condition, participants select their own incentive contract given their assigned skill and effort levels. In the other condition, participants communicate their assigned skill and effort level to a superior, who then assigns them to a contract. The authors find that the two contracts are not

identical with respect to the level of misreporting of both skill and effort. Participants in the first condition are more likely to misreport versus participants in the second condition. Interestingly, the authors also find that the threshold amount over which most participants lie is quite low. Based on the low threshold, they conclude that their findings support the agency prediction that the two contracts are identical. Overall, this study suggests that lying to increase compensation would take place, provided the increase in compensation was above a relatively low threshold. Evans III et al. (2001) examine whether employers can incorporate employees' preferences for honesty to reduce contracting costs and find evidence consistent with this prediction. This finding refutes Baiman and Lewis (1989)'s findings that misreporting thresholds are too low to make a difference in contracting.

Research also examines mechanisms that could reduce misreporting within an organization. For example, Antle and Fellingham (1990) examine whether an organization's commitment to multi-period contracts can reduce misreporting. They find that a control system must rely on performance history (i.e., it must have memory) to reduce misreporting. Hannan, Rankin, and Towry (2006) examine how the presence of an information system and the precision of the information it provides influence employee honesty. They find that the presence of an information system increases managerial honesty. Further, they find that honesty is lower with a precise versus coarse information system. The authors contend that this effect occurs because of the manager's desire to appear honest. This paper and the corresponding discussion paper (Salterio and Webb 2006) both call for additional research to examine how control systems can become "enablers" of honest behavior. Other methods to reduce dishonesty include information acquisition on reported performance (Church, Hannan, and Kuang 2014), using firm-wide informal communication to influence perceived norms (Newman 2014), using rank-order

performance evaluations (Brown, Fisher, Sooy, and Sprinkle 2014), and paying employees a wage that exceeds expected wages (Chen and Sandino 2012).

Related more closely to my dissertation, Bruner, McKee, and Santore (2008) examine how the actual detection likelihood of a control influences misreporting. They find that misreporting is negatively correlated with the actual probability of detection. While this study is informative, it makes an underlying assumption that employees know the actual probability of detection. In their study, the authors provide participants with a table informing them of the true detection probability. However, there are several situations where true detection likelihood is unknown to an employee. For example, internal audit would likely not disclose the number of transactions it audits to reduce the likelihood of employees gaming the system. Moreover, as described in this dissertation, there are choices a firm can make about their control system design that can influence employees' subjective assessments of detection.

Also related to my dissertation, Maas and Van Rinsum (2013) examine the direct effect that control design and compensation interdependence have on misreporting. They find that participants are more likely to overstate their performance if it increases versus decreases the monetary payoff to their peers. In addition, misreporting is lower under an open information policy, in which each individual's reported performance is public knowledge, compared to a closed information policy. The authors also test for an interactive effect of these two variables on misreporting, but do not find one. As described in Chapters 2 and 3 of this dissertation, I rely on this study to motivate my research question and develop my hypotheses.

Future Research Questions

While a number of studies have examined factors that can influence misreporting, there is still much work to do. Many studies consider misreporting to be a single construct. However,

misreporting can vary in terms of both the magnitude and the content of the misreported information. For example, a low-level employee within a service firm that misreports his or her hours spent working on a client is a far different setting than a manager who misreports division performance. In the former case, the employee is misreporting time, which leads to overbilling of the client. In the latter case, the employee is directly misreporting monetary amounts. However, no research has examined whether differences in content of the misreported information affects misreporting.

Future research can also examine whether the difficulty of performance targets in a contract influence misreporting. Generally, one might think that more difficult performance targets result in more dishonesty. However, if targets are framed to the employee as very difficult to achieve (e.g., stretch targets) does this framing lead to more or less dishonest reporting? Specifically, does classifying the target as one that is not likely to be met without substantial effort increase dishonest reporting in an attempt to achieve the unlikely goal or does the employee not feel pressure to meet the stretch goal and, therefore, does not feel the need to overstate performance? How do initial target level and the revision of targets over time influence misreporting? There is still much to explore in this area.

More closely related to my dissertation, a number of studies examine how environmental factors influence misreporting. However, these papers have not focused on whether features specific to a control itself influence misreporting. For example, existing research does not examine how the location of a control within a firm's hierarchy influences its perceived effectiveness. Controls incorporated at lower levels of the hierarchical structure have the advantage of being physically closer to the reporting individual. However, it is likely to be a supervisor who has daily interactions with the reporting employee who designs, implements, and

performs the control. This could result in low control effectiveness. This provides tension to the research question of how control centralization influences misreporting. My dissertation examines how the physical location of a control can influence employees' perceptions of the control's effectiveness.

2.4 Literature on Monitoring

Because opportunity for error and fraud within a reporting system exists, a firm implements a control system to serve a monitoring role within the organization. In this section, I focus on the reduction of intentional misstatements through control system design. I begin with research in the late 1990's. Evans III and Sridhar (1996) examined a firm's use of both a financial reporting system and a contracting system in the principal agent model with moral hazard. The authors find that the compensation system alone is not enough to generate truthful reporting. Specifically, the compensation system can solve the moral hazard problem, but to induce truthful reporting, the principal must rely on the financial reporting system and controls. Caplan (1999) finds that managers with a propensity to misreport prefer a weak control system. However, third parties could potentially be less inclined to search for fraud given a weak control system because the third party will likely associate a "bad" outcome with an error instead of fraud. With a strong control system, a "bad" outcome is more likely to be associated with fraud.

A shift in the control system literature began in early 2000. Researchers began to investigate not only formal controls, but also social controls that peers place on employees. For example, Towry (2003) provides evidence that, when team identity is high, horizontal monitoring systems, in which employees monitor and regulate their peers' performance, can be more effective than a vertical monitoring system, in which the employee's superior is responsible for monitoring. Coletti, Sedatole, and Towry (2005) examine whether a formal control system

influences cooperation within collaborative environments. They find that control systems improve trust and cooperation within collaborative environments, resulting in overall higher levels of performance within collaborative environments. Based on their results, the authors argue that firms will choose to implement stronger control systems than prior research suggests. Interestingly, contemporaneous research suggests the opposite. Falk and Kosfeld (2006) find that agents reduce their performance in response to the principal's decision to implement a control. Their manipulation is the presence or absence of a minimum performance level within the incentive contract, which is different from the control in Coletti et al. (2005). This suggests that a better understanding of how different controls can increase or decrease trust and their subsequent effect on employee performance could be fruitful.

Research also examines whether formal controls undermine trust (Christ et al. 2008). This literature examines how control framing influences employees' perceptions of trust and employees' task performance (Christ, Sedatole, and Towry 2012) and how control type (i.e., preventative, detective) influences employees' performance (Christ et al. 2012).

Recently, several new directions in control research have emerged. For example, Campbell, Epstein, and Martinez-Jerez (2011) examine how control "tightness" influences learning among employees. They find that relatively "loose" control systems lead to higher levels of learning than relatively "tight" control systems. Researchers have also expanded the definition of a control system to include employee selection into and out of the firm. Campbell (2012) examines the role that employee selection through different channels into the firm can play in their subsequent "match" with the firm. He finds that those employees who self-selected through a channel more likely to "match" their preferences to the firm's preferences are more likely to use decision-making authority and make less risky decisions.

Researchers have also examined the role that control systems have in creating norms for or against a behavior. Tayler and Bloomfield (2011) find that controls can create a norm of what behavior a firm expects of its employees. They also find that after a firm removes a control, the established norms are strong enough to govern behavior. This suggests that controls play a role in norm creation and can have an indirect influence on employees' behaviors by signaling appropriate and inappropriate behaviors.

Future Research Questions

There are several unanswered questions related to control system design. Prior studies have found divergent results when examining whether formal controls help or hurt performance. Future research can provide additional insight into moderating variables that help define this relation. Does the manner in which management implements the control influence how the control influences performance? Similarly, does the manner in which management communicates the implementation of a new control or the change of an existing control influence employees' perceptions of the control? Future research can also explore the interaction of controls within the control system. For example, does the implementation of one type of control, such as a compensation control, influence the effectiveness of another type of control, such as a formal monitoring control?

A firm does not implement controls in a vacuum. Future research can examine how the relationships between employees and their superiors influence the effectiveness of a control system. Research has recently begun to investigate whether controls create norms for or against specific behavior (Tayler and Bloomfield 2011). Future research can further investigate the role that controls play in the creation of both descriptive and injunctive norms. To what extent do formal controls serve as a signal to employees about the type of behavior the firm expects? How

can control design strengthen or weaken this signal? How do the actions of peers either strengthen or weaken the signal that controls send to employees? How does control ownership delegation to lower-level employees influence employees' perceptions of the control?

2.5 Literature on the Effect of Physical Distance

Few studies examine the role that geographic distance plays in performance reporting. This could be due to the assumption that monitoring costs increase with geographic dispersion (Jensen and Meckling 1976) and, given the higher monitoring costs, on the margin one would expect to see greater misreporting as geographic dispersion increases. However, this assumption needs to be tested. Consider a wheel-and-spokes model of a firm. Is it the distance between the reporting manager and the firm's central headquarters (the length of the spokes) or is it the distance among the different reporting managers (the distance between the spokes) that influence reporting decisions? The answer could have different implications for firms' organizational design choices. Moreover, how does the distance between reporting managers and a control itself influence reporting behavior?

Literature outside of accounting that has examined geographic dispersion has revealed some interesting patterns. For example, more geographically dispersed organizations tend to be less employee friendly and more likely to dismiss employees (Landier, Nair, and Wulf 2009). Firms that are more geographically dispersed also tend to have higher communication costs, which can result in lower communication with distant business units (Baaij and Slangen 2013). When deciding what business units or divisions to divest, on average, firms select those business units that are more geographically distant from the firm's central headquarters (Landier et al. 2009).

There have been a few studies in the auditing literature that have examined how geographic proximity influences audit quality. For example, Choi, Kim, Qiu, and Zang (2012) find that their proxies for audit quality decrease as a client is located farther from the local audit firm's headquarters. Similarly, DeFond, Francis, and Hu (2011) and Kedia and Rajgopal (2011) examine how geographic distances factor into SEC enforcements. They find that auditors are less aggressive with their auditing as the distance between the engagement office and the SEC regional office increases. This research, taken together, suggests that geographic distance plays a role in behavior. In the sections that follow, I describe how I extend this research by examining whether the physical distance between a control and a reporting manager influences the reporting manager's reporting behavior.

CHAPTER 3: BACKGROUND AND HYPOTHESES DEVELOPMENT

3.1 Background

3.1.1 Self-Reported Performance

Information asymmetry often forces a principal to rely on self-reported performance from multiple agents (Baiman and Evans III 1983). Although the principal prefers fully-verified information, she often must rely upon partially- or non-verified information because full monitoring is cost-prohibitive (Baiman and Rajan 1994). Monitoring controls partially verify information by probabilistically selecting and reviewing reports for misreported performance (Dye 1986). Analytic research typically sets the detection likelihood of a monitoring control as a parameter in the model (e.g., Narayanan and Davila 1998). However, in the natural setting, it is likely that the principal does not have a precise measure of the true detection likelihood. Even if the principal has a sense of the true detection likelihood, it is not necessarily in her best interest to reveal this information to the agents. Because agents are uncertain of true detection likelihood, their subjective assessment of detection likelihood drives reporting behavior.[1]

3.1.2 Control Centralization

Following prior literature, I define control centralization as the extent to which a control is consolidated and coordinated within a firm (Anderson et al. 2010). Within the context of control centralization, I examine an outcome of the control centralization decision, specifically, employees' decisions to misreport performance. While I focus on an outcome of control centralization, one should note that little research on the determinants of control centralization

[1] Detection likelihood can either be independent of performance or contingent upon performance (Baiman and Demski 1980). In both settings, the agent will likely not know the true detection likelihood, making the subjective detection likelihood important in both. I focus on the setting where the review is independent of performance. However, the theory I test would likely generalize to settings where detection likelihood is contingent upon performance.

exists. That which does exist comes from two public accounting firms. Ernst & Young (2012) highlights five determinants of control centralization: 1) the broader structure of the business, 2) the organization's risk profile, 3) cost, 4) independence requirements, and 5) geographic diversity. PriceWaterhouseCoopers (2012) adds the desire of maintaining a physical presence in multiple locations as another determinant.

I focus on the effect that control centralization has on misreporting because firms implement formal monitoring controls in part to reduce dishonest reporting. As such, a natural question to ask is how control design (i.e., centralization) influences reporting behavior. By examining whether control centralization reduces misreporting, this dissertation highlights an exogenous choice that management can make to reduce performance misreporting.

3.1.2.1 Control Centralization Framework

As mentioned, I define control centralization as the extent to which a control is consolidated and coordinated within a firm (Anderson et al. 2010). This definition implies that control centralization is a multi-faceted construct. In this section, I describe the five most important components of control centralization. I consider these five components to be the most important because prior literature shows that they differ both across and within firms, and, importantly, this variance will likely have a significant impact on the perception of a control as being relatively more centralized or more decentralized. The five components of control centralization are 1) the location where the control work occurs, 2) the location of the control within the firm's hierarchy, 3) the social distance between the reporting manager and control facilitator, 4) the consistency of the control within the firm, and 5) the use of a single or multiple administrative systems.

Figure 1 provides the control centralization framework. One can classify a control as relatively more centralized or more decentralized, depending on how the five components of control centralization are structured. In the middle of Figure 1, the vertical line with the "centralized" and "decentralized" endpoints represents the centralized/decentralized continuum along which one can classify a control. The placement of the control along that continuum is contingent upon the state of each of the five control centralization components. One can classify each component (the boxes in Figure 1) as relatively more centralized or more decentralized using the state of each component. As one classifies more components of a control as centralized (decentralized), the control classification moves further up (down) along the control centralization continuum. For example, *ceteris paribus,* one would place a control that has its manual work done relatively far from reporting managers higher up on the continuum than a control that has its manual work done relatively close to reporting managers. As such, understanding the control centralization components is critical to classifying a control as more centralized or more decentralized. In the following subsections, I describe each component of control centralization in detail.

3.1.2.2 Location where the Control Work Occurs

Many controls have a manual component to them, and where that manual work occurs is the first component of control centralization. Firms can design controls such that the manual control work occurs geographically far from or close to the reporting manager (centralized and decentralized states, respectively). For example, in a case study examining expatriates at Nokia Telecommunications, Tahvanainen (2000) describes how some middle manager expatriates had goal setting, performance evaluation, and training and development performed by senior managers located in the host country while some middle manager expatriates had senior

managers located at the primary headquarters of the firm perform those functions. Ernst & Young (2012) describes the work of the internal audit function as either taking place at each business unit or at a firm's central headquarters. Moreover, remote internal audits, in which internal audit team members on site work jointly and directly report to a remote lead audit team (on location at the firm headquarters) (Teeter, Alles, and Vasarhelyi 2010), are another example of the physical work of a control being performed relatively close to or far from the reporting manager.[2]

3.1.2.3 Location of the Control within the Firm's Hierarchy

Within a firm's hierarchy, management can implement a control at relatively higher or lower levels (centralized and decentralized states, respectively). For example, Indjejikian and Matějka (2012) show that the budgeting process is either implemented at the business unit level (decentralized) or at the corporate level (centralized) within a firm's hierarchy. Fjeldstad et al. (2012) describe how historically, large organizations, such as the railroad and steel companies of the late 1800s, had highly centralized controls. Contrast that to the currently decentralized control structure of the State of Illinois, where the creation and review of financial reporting forms takes place at each of the state's 821 primary government/fiduciary funds and 26 component units (Holland 2011). These examples suggest variation exists in where management implements controls within a firm's hierarchy.

3.1.2.4 Social Distance between the Reporting Manager and Control Facilitator

The third component of control centralization is the social distance between the individual subject to the control and the individual performing the control. The more (less) social

[2] As described in this article, remote audits can involve a virtual team, which "coordinates auditing activities among auditors who are physically present at the audit site and auditors who are located in other locations, such as [the client's] corporate headquarters."

distance between the two parties, the more centralized (decentralized) the state of the social distance component of control centralization is. By social distance, I mean the perceived "closeness" of the two parties. Consider a budget variance investigation performed by a business unit controller versus one performed by the corporate controller. The reporting manager is likely to be socially closer to the business unit controller than the corporate controller because they generally interact more frequently. Maas and Matějka (2009) explore this relationship by examining how a business unit controller must balance their role of local responsibility (i.e., a business partner) and functional responsibility (i.e., a corporate police officer). Compared to corporate controllers, the balancing act is more difficult for the business unit controllers because of the "closeness" the business unit controller has with people within the business unit relative to the greater social distance that the executive controller likely feels.

3.1.2.5 Control Consistency within the Firm

The fourth component of control centralization is the extent to which a control is consistent within the firm. The more (less) consistent a control is across the divisions of a firm, the more centralized (decentralized) the state of the consistency component of control centralization is. Grabner and Moers (2013) describe a performance evaluation system for salespeople that was highly formalized and closely monitored by the human resource department to "ensure compliance and consistency" across different banks. However, other studies suggest a fair amount of variation across controls within a firm. For example, Maas and Matějka (2009) point out that business unit managers and controllers within their sample have a great amount of influence over the performance evaluation process within their business unit. This suggests that performance evaluation across this firm's business units is not as standardized as the evaluations in Grabner and Moers' (2013) setting.

3.1.2.6 Single or Multiple Administrative Systems

The fifth component of control centralization is the use of a single or multiple administrative systems. The fewer (more) systems used to administer the control, the more centralized (decentralized) the state of the administrative system component of control centralization is. The use of a single administrative system is often associated with an enterprise planning resource (ERP) system, such as SAP. For example, Jørgensen and Messner (2009) point out in their case study that the decision to implement SAP is made to make the firm operate as if it were a single unit. A byproduct of this decision is that management can implement a control within the firm's single ERP system, making the control component more centralized. However, firms that do not use a single ERP system, such as those that grow through mergers and acquisitions, use several legacy systems and rely on the controls embedded within the different legacy systems (Abdinnour-Helm, Lengnick-Hall, and Lengnick-Hall 2003). The administrative system component of these controls is more decentralized in nature.

3.1.2.7 Additional Examples of Control Centralization

Examples of controls that can be centralized or decentralized include a firm's internal audit function, budgetary variance investigations, and formal performance reviews. A survey issued by Ernst & Young (2012) finds that 49 percent of surveyed firms have an internal audit function centralized in one location while the remaining 51 percent of surveyed firms have either a regional hub-and-spoke internal audit model (16 percent) or a decentralized internal audit function present at each major business unit (35 percent). Further, in a survey of 236 organizations, Anderson et al. (2010) find that approximately 25 percent of firms have a

decentralized internal control structure, 28 percent have a centralized control structure, and 47 percent have a partially decentralized structure.[3]

Maas and Matějka (2009) find that some business unit controllers have high levels of control over the business unit budgeting process while other business unit controllers have little to no control over the budgeting process. In the latter case, management implements the budgeting process at a level above the business unit level within the organization, making it more centralized. Similarly, the 10-K disclosure of D.R. Horton, provided as Appendix 1, reveals that each division is responsible for the review of division business plans, budgets, acquisition contracts, and inventory levels. Each of these reviews represents a control that the firm has chosen to decentralize.

3.1.2.8 Control Centralization Operationalization

I focus on the first component of control centralization. That is, I focus on the physical location of control work component of control centralization. I do so for two reasons. First, the location where control work occurs represents an exogenous choice variable that the firm can make with respect to how it designs its control system. Other components of control centralization, such as the social distance between a superior and a reporting employee, develop endogenously and are likely to be highly idiosyncratic. As such, the location of control work component occurs represents a dial that a firm can adjust relatively easily to create different employee perceptions about a control.

The second reason I focus on the location of control work component is that the practitioner literature appears to suggest that location is a first order effect. Specifically, both

[3] Partial decentralized structures, also called hub-and-spoke structures, consist of a primary central hub where the control takes place (usually within a firm's headquarters) and smaller "spokes" where the control also occurs on a smaller scale. For example, PriceWaterhouseCoopers (2012) provides an example of a global firm that has their internal audit "hub" in the United States and several "spokes" in their Asia, Australia, and Europe business units.

Ernst & Young (2012) and PriceWaterhouseCoopers (2012) describe geographic diversity as a primary determinant in the decision to centralize controls.[4] These reports also describe how a control can change based on the physical location where the work takes place. Therefore, these reports appear to suggest that location is a first order effect in control centralization decisions. Given our limited understanding of the consequences associated with a more centralized versus decentralized control, I focus on what prior research suggests is the most critical control centralization component to make the strongest contribution possible.

3.1.3 Compensation Interdependence

I define compensation interdependence as the extent to which compensation is contingent upon group-based or individual-based performance (Nalbantian and Schotter 1997). For example, bonuses based on a division's overall performance have greater interdependence than a bonus based on individual performance. Consistent with prior literature (e.g., Maas and Van Rinsum 2013; Sandino 2007; Malmi and Brown 2008), I consider compensation system design to be a feature of a firm's control system.

Management can implement interdependent compensation to increase knowledge sharing and coordination within a firm. For example, Hwang, Erkens, and Evans III (2009) show that manufacturing plants rely more on group-based performance measures when the value of sharing knowledge is high and more on individual-based performance measures when the value of sharing information is low. Similarly, FitzRoy and Kraft (1987) find that employee coordination and cooperation increases as a firm uses compensation that is relatively more group-based.

Increasing compensation interdependence can also lead to problems. A common problem associated with group-based pay is the decision of some employees to free ride on the effort of

[4] These studies describe the decision to centralize or decentralize controls. Using my framework, centralizing or decentralizing control components is a more precise way to state this.

other employees (Holmstrom 1982). Recent literature has also shown that interdependent compensation can lead to aggressive decision making (Sutter 2009). This aggressive decision making has been shown to increase the level of dishonest reporting among employees (Maas and Van Rinsum 2013; Church, Hannan, and Kuang 2012).

Despite numerous studies that have examined the direct effect of compensation interdependence on positive or negative outcomes, there remains little research on the moderating effect that compensation interdependence can have on the relation of other controls with these outcomes. In other words, prior literature has not established what effect compensation interdependence can have on other firm-level control choice variables. For example, does compensation interdependence increase or decrease the effectiveness of other controls within the control system? In this study, I investigate the moderating effect that compensation interdependence can have on the relation between control centralization and misreporting.

3.2 Theory and Hypotheses

Prior research shows that the physical distance between an individual and an object can influence the individual's perception of that object (Fujita et al. 2006). This suggests that the physical distance between a reporting employee and a control could influence both the employee's perception of control effectiveness and, subsequently, his or her reporting behavior. In the following subsections, I argue that increasing the physical distance between a reporting employee and a formal control, (i.e., control centralization) decreases an employee's subjective assessment of detection, which, *ceteris paribus*, increases dishonest reporting. Next, I argue that compensation interdependence moderates the relation between an employee's subjective

assessment of detection and dishonest reporting. Figure 2 provides a framework for my theory and predictions.

3.2.1 Control Centralization and Subjective Assessment of Detection

I first consider how the physical distance between a reporting employee and a formal control can influence the employee's subjective assessment of the likelihood of the control detecting misreporting. I predict that increasing the physical distance between a reporting employee and a formal control decreases the employee's subjective assessment of the likelihood of the control detecting misreporting. I base this prediction on psychological distance theory.

Psychological distance is a subjective feeling that an object or event is close to or distant from oneself in the current moment (Magee and Smith 2013; Trope and Liberman 2010). Four related dimensions of distance (spatial, temporal, social, and hypotheticality) determine this feeling of closeness. As an individual perceives an object or event to occur (1) physically closer rather than farther, (2) sooner rather than later, (3) to a socially similar as opposed to dissimilar individual, and (4) with higher probability rather than lower probability, psychological distance between the individual and the object or event decreases (Trope et al. 2007).

Prior research suggests that the four dimensions of psychological distance are interrelated such that an increase in one dimension will cause a perceived increased in the other three dimensions. For example, Fiedler et al. (2012) manipulated two dimensions of psychological distance, (spatial and social) and then measured participants' assessments of the two non-manipulated dimensions (temporal and hypothetical). Their findings suggest that the measured dimensions are positively associated with the manipulated dimensions such that an increase in two of the dimensions resulted in an increase in participants' perception of the other two dimensions. Similarly, Zhang and Wang (2009) show that participants' subjective assessment of

winning a trip to visit a city changes as the location of the city changes. Specifically, participants perceive a lower probability of winning the trip as the destination city increases in distance from the participant's current location. Yan (2014) finds similar results. He finds that participants seated closer to a laptop, which is the prize for a drawing, perceive their chances of winning the laptop to be higher than those participants seated further away from the laptop.

The Zhang and Wang (2009) and Yan (2014) studies both suggest that the perceived hypotheticality of an event is associated with the spatial distance between an individual and the salient outcome of the event (i.e., winning a prize). This salient event has both a low probability of occurrence and a high outcome effect on the individual relative to the counterfactual outcome (i.e., not winning a prize). Similarly, I contend that the salient outcome of a control designed to detect misreporting is the detection of misreporting. That is, the detection of misreporting is not likely to occur often within the organization. When a control does detect misreporting, there are also likely high consequences. Therefore, I predict that greater physical distance between where the control work occurs and the reporting employee (i.e., control centralization) increases the perceived hypotheticality of a control detecting performance misreporting. In other words, greater control centralization will decrease an individual's subjective assessment of a control's likelihood of detecting misreporting.

H1: Greater control centralization decreases employees' subjective assessments of detection likelihood.

The "H1" link in Figure 2 represents this prediction. It is important to note that this prediction is independent of compensation interdependence. In the next subsection, I describe how compensation interdependence moderates the relation between employees' subjective assessments of detection and misreporting behavior.

3.2.2 Compensation Interdependence and Misreporting

Prior literature suggests that group membership can lead to aggressive, risky decision making (Charness et al. 2007; Luhan et al. 2009; Yechiam et al. 2008).[5] Related literature also finds that individuals lie more when they share the benefit of the lie with another person than when there is no mutual benefit (Erat and Gneezy 2012; Gneezy 2005). Taken together, this literature suggests that high compensation interdependence will have an adverse effect on reporting behavior.

Two recent accounting studies have leveraged these findings to predict a positive main effect for compensation interdependence on misreporting. Church et al. (2012) show that managers create more budgetary slack when they share the benefits of slack with a subordinate than when they alone receive the benefits. Maas and Van Rinsum (2013) show that more dishonest reporting occurs when misreported performance positively influences peers' compensation than when it negatively influences peers' compensation. Consistent with this reasoning, I predict that compensation interdependence will have a positive main effect on dishonest reporting. The direct link from compensation interdependence to misreporting in Figure 2 represents this prediction.

I also predict that compensation interdependence will moderate the relation between an individual's subjective assessment of detection and reporting behavior. As discussed at the beginning of this subsection, high compensation interdependence leads to aggressive, risky decision making. Consider how this behavior will influence the effectiveness of a control in reducing misreporting. Assume two employees considering misreporting. Employee A is more aggressive in his reporting behavior than Employee B because of higher compensation

[5] Sutter (2009) finds that the mere notion of payoff commonality, as opposed to formal team membership, is sufficient to induce aggressive decisions.

interdependence. Consider the input into both employees' reporting decisions from Hypothesis 1: their subjective assessment that a control will detect misreporting. If management makes a control design decision, such as decentralizing a control, that increases by the same amount both employees' subjective assessments of detection (i.e., Hypothesis 1), how will this affect reporting behavior? Given Employee A's higher level of aggressive reporting from compensation interdependence, the control design decision is less likely to influence his reporting behavior than Employee B's reporting behavior. To elaborate, the presence of compensation interdependence increases aggressive reporting behavior, which reduces an individual's sensitivity to changes in the subjective assessments of detection.

This logic suggests that compensation interdependence will moderate the relation between an individual's subjective assessment of detection and misreporting behavior, as illustrated by the "H2" link in Figure 2 (i.e., moderated mediation). Specifically, individuals' subjective assessments of detection are less likely to drive reporting behavior with high compensation interdependence, suggesting a muting of the effect that control centralization has on reporting behavior. With low compensation interdependence, individuals' subjective assessments of control effectiveness are more pertinent to their reporting decision, suggesting that control centralization will have a stronger effect on reporting behavior. Put another way, employees' subjective assessments of detection, developed endogenously from control centralization, potentially translate into a different honest versus dishonest reporting decision by employees compensated interdependently versus compensated independently.[6]

[6] Of course, extreme detection likelihood assessments likely lead to the same reporting decision, regardless of compensation interdependence. That is, when assessed likelihood is sufficiently high (low), the moderating effect of compensation interdependence is likely insufficient to preclude employees from reporting honestly (dishonestly). My theory is more applicable to scenarios in which employees' reporting decisions are more ambiguous.

Ultimately, this discussion suggests that employees are more likely to misreport at a given level of perceived detection likelihood when their compensation is interdependent than when it is independent of others' performance reporting.

H2: The difference in the extent of performance misreporting between a centralized and decentralized control is greater when compensation interdependence is low than when compensation interdependence is high.

Figure 3 provides a graphical representation of Hypothesis 2. Taken together, my hypotheses suggest a model consistent with moderated-mediation. Specifically, subjective detection likelihood is the mechanism by which control centralization influences employees' propensity to misreport performance, and compensation interdependence moderates the effect of this mediator on employee performance reporting. As discussed in Chapter 6, I also investigate the extent to which compensation interdependence moderates the effect of control centralization on perceived detection likelihood (i.e., the hypothesized mediator underlying Hypothesis 1).

CHAPTER 4: METHOD

4.1 Overview

The experiment involves a setting in which participants make a series of independent reporting decisions. In each round, participants have the option to over-report performance to increase their compensation. However, they are subject to a control that probabilistically detects misreported performance. I use a 2 x 2 (between-subjects) x 6 (within-subjects) experiment design. The between-subjects manipulations (described in detail later) are (1) the spatial distance between a participant and the control (*Near Review / Far Review)* and (2) the extent of compensation interdependence (*High CI / Low CI*). The within-subjects manipulation reflects that participants make independent reporting decisions in six rounds.

4.2 Procedures

Upon entering the lab, participants read instructions, answered quiz questions about the instructions, and read a compensation example on their computer. The facilitator then demonstrated the review process, and participants completed a practice round. Participants then performed the reporting task for six rounds, answered post-experiment questions, collected their payment, and left the lab. Each session took between 50 and 60 minutes. Figure 4 provides a timeline of the experiment.

The instructions informed participants to assume that they were in charge of reporting a project's performance to their supervisor. Each round, the computer program randomly generated and privately informed each participant of the actual revenue (uniformly distributed between $2.00 and $6.00, inclusive) that his or her project generated. After having received their actual revenue, participants reported their project's revenue. They could report any amount of revenue between the actual revenue generated and $6.00 (i.e., participants could over-report, but

not under-report). At the end of each round, after all participants reported their project's revenue, a review process took place.

For the review process, the facilitator randomly selected a poker chip for each participant from a bowl containing 92 white poker chips and 8 red poker chips. Participants did not know the total number of poker chips in the bowl nor the ratio of red to white chips. If the facilitator selected a white poker chip, then the review process ended for that participant. However, if the facilitator selected a red poker chip, he compared that participant's reported revenue to their actual revenue. If the two amounts differed, the computer program adjusted their compensation to zero for the round.[7] If the participant had a red chip drawn and had honestly reported or had a white chip drawn, then no adjustment to their compensation occurred. Across all conditions and all rounds, the facilitator held up each individual's poker chip and announced its color to all participants in the session before placing it back into the bowl.

I set the probability of a participant being selected for a review in a given round intentionally low (8 percent) to increase the likelihood of observing my construct of interest, performance misreporting. I also hold the rate constant across conditions, which allows me to make meaningful comparisons across conditions.

Recall that a review demonstration took place after the instructions and before the practice round. During the demonstration, the facilitator explained how the poker chip selection would work, held up a transparent bowl with the 100 poker chips, and walked once up and down the center aisle of the computer lab while churning the poker chips in the bowl. Because the

[7] Participants input the color of the poker chip selected for their review into the computer program, and the program updated their compensation accordingly. The experimenter also kept a written record of the color of poker chip selected for each participant to validate participants truthfully input the outcome of the review process. Only 1 out of 516 color-reports was incorrectly reported. Results are inferentially identical when dropping this observation.

facilitator did not announce the true number of red and white poker chips, this process enabled participants to make a subjective assessment of the relative number of red and white chips. After the facilitator walked up and down the aisle once, he put the bowl containing the poker chips inside a non-transparent bag for the duration of the review process. This ensured that participants could not count the ratio of red and white chips during the rest of the session, and that neither the participants nor the facilitator knew the color of the chip as the facilitator selected it.

4.3 Variables and Compensation Detail

4.3.1 Dependent Variables

I focus on two dependent variables based on the difference between reported and actual revenue. The first dependent variable (Overstate) is the difference between reported revenue and actual revenue. I calculate the second dependent variable (Dishonesty) as follows: (Reported Revenue – Actual Revenue)/(Max Revenue – Actual Revenue) where max revenue is $6.00 in all conditions. This variable captures the extent to which participants misreported, given the amount they could misreport. This variable is also consistent with prior literature (Maas and Van Rinsum 2013).

4.3.2 Independent Variables

The first between-subjects manipulated variable is the location of the review process. In the *Near Review* condition, the poker chip selection and revenue comparison for each participant took place at each participant's desk. In the *Far Review* condition, the selection and comparison for each participant took place at the front of the room. Specifically, in the *Near Review* condition the facilitator would go to each participant's desk and draw a poker chip. If the chip was red, the participant's computer would display the reported and actual revenue and the facilitator would write down the information. In the *Far Review* condition, the facilitator would

stand at the front of the room to draw each participant's poker chip. If the chip was red, the computer program provided the participant's reported and actual revenue to the facilitator's computer (at the front of the room) where he would write down the information. Across all conditions, because the facilitator announced the color of the poker chip, participants knew whether each individual participant had a red or white chip drawn. However, if the facilitator selected a red chip, the review outcome was private in that no other participants discovered if performance had been misreported.

I made this design choice to keep the perceived non-economic costs of misreporting, such as shame and embarrassment, consistent across conditions. Specifically, if the outcome of the review process were public, one could argue that reducing the distance between the review process and a participant not only changes the perceived detection likelihood, but also increases the perceived non-economic costs of having misreported performance detected because the facilitator is standing relatively close and all other participants are watching. By not revealing whether the review process detected misreporting or not, the non-economic costs of misreporting should be constant across conditions. I validate this assumption in Chapter 7.

The second between-subjects manipulation relates to how participants earned compensation. Across all conditions, the computer program assigned a random, anonymous partner for the duration of the session to each participant. Each round, after the reporting decision but before the review, the computer program would inform participants of their partner's reported revenue (but not their actual revenue). In the *High CI* condition, compensation is the average of (1) the participant's reported revenue and (2) his or her partner's reported

revenue. Specifically, the program calculated per round compensation in the *High CI* condition using the following formula:[8]

$$\text{Compensation} = \frac{1}{2}(\text{Reported Revenue} - \$2.00) + \frac{1}{2}(\text{Partner's Reported Revenue} - \$2.00)$$

In the *Low CI* condition, participants were still able to view their partner's reported revenue each round, holding constant the presence of another participant's reporting decision across all conditions. However, in the *Low CI* condition, per round compensation was not contingent upon the partner's reporting decision. Specifically, the program calculated compensation per round using the following formula:

$$\text{Compensation} = \text{Reported Revenue} - \$2.00$$

4.3.3 Additional Compensation Detail

Across all conditions, compensation was the sum of three out of six randomly selected rounds. In addition to this amount, participants earn bonus compensation. Each session had a fixed bonus pool of $40.00 and each participant received a portion of the bonus pool based on his or her reported revenue. The allocation calculation is as follows:

$$\frac{\sum_{i=1}^{3} \text{Reported Revenue}}{\sum_{i=1}^{3} \text{All Participants in the session's Reported Revenue}} * \$40.00$$

Where *i* refers to the randomly selected rounds.[9]

The experiment design includes a bonus pool so participants were not only hurting the experimenter by dishonestly reporting, but also hurting other participants by taking a larger portion of the fixed bonus. By invoking a harm to others by misreporting performance this reduced the likelihood that participants would consider misreporting to be "costless." It is

[8] The $2.00 represents the per-round fixed cost of the project and is consistent across all conditions.
[9] On average, there were just under eleven participants per session. Average bonus pool compensation per participant was $3.72, or 26 percent of their total compensation.

important to note that the bonus was consistent across all conditions and, although it introduced negative compensation interdependence, I did not manipulate the presence of negative compensation interdependence across conditions.[10]

Across all conditions, the review also affected participants' bonus pool calculation. If a participant had a red poker chip selected and had misreported for any of the three randomly selected rounds used to calculate the bonus allocation in that round, the reported revenue for that round would be zero for the basis of calculating the bonus.[11]

[10] Prior research examines negative compensation interdependence (e.g., Maas and Van Rinsum 2013) where high reported performance from one employee decreases the payouts to other employees. My study manipulates positive compensation interdependence because research suggests firms are using compensation with positive interdependence more frequently (Lazear and Shaw 2007) and the theory predicting my interaction is more likely to play out in settings with positive compensation interdependence as opposed to negative compensation interdependence. I discuss how this limits the generalizability of my study in Chapter 9.

[11] Across all conditions, the detection of a participant's misreporting did not affect the payout of the participant's partner. I made this design choice to ensure the anonymity of partners. Specifically, because there were only a few red poker chips relative to white poker chips, a significant percentage of rounds (42 percent) had only one red chip drawn. Since only one person in those rounds was reviewed, a participant who saw a decrease in his or her compensation resulting from their partner's reporting decision in those rounds would know who their partner was. Prior research suggests that anonymity can influence misreporting decisions (e.g., Mayhew and Murphy 2008). Therefore, to provide a meaningful comparison of dishonest reporting by keeping anonymity consistent across conditions, I designed the experiment so misreporting detection did not change the payoffs of a participant's partner.

CHAPTER 5: PILOT STUDY

5.1 Overview of Participants

Fifty-four students from a large public university in the Midwest participated in the experiment. All students were sophomores, juniors, or seniors in the undergraduate Accountancy program. The average age of all participants was 20.8 years old and 37 percent of the participants were male. I examined if any demographic variables differed across conditions (e.g., age, gender). I found that age is significantly different across conditions. Therefore, all subsequent analyses in this chapter control for age.[12]

5.2 Manipulation Checks

To measure whether participants attended to the experiment manipulations, participants answered two manipulation check questions. The first question asked *during the session, where did the reviews take place?* Participants were able to select between the two choices: *at each participant's desk* or *at the front of the room*. Forty-nine of the 54 participants (91 percent) correctly answered the question. The second question asked whether *the compensation I received each round was influenced by the reporting decision of another participant.* Participants were able to select either *True* or *False*. Thirty-nine of 54 participants (72 percent) correctly answered the question.[13]

[12] The difference in age is likely due to the experimental setup and recruiting procedures. Because the *Near Review / Far Review* manipulation required the experimenter to walk around the room or stand at the front of the room when doing the review, each experimental session was a condition (i.e., I could not manipulate centralization within each session). When recruiting participants, a disproportionate amount of seniors signed up for a single session (which worked with their class schedule). I do not make any age-specific inferences from this study.

[13] The low correct response rate could be due to the wording of the question. As such, I changed the manipulation check question for my final experiment. Specifically, I made it clearer that the question was specific to the compensation earned each round and not to the bonus payout. To investigate further whether participants attended to the manipulation, I examined a quiz question following the instructions. Participants within the *Low CI (High CI)* condition were asked: *The higher your Reported Revenue* (*and your partner's Reported Revenue*), *the more money you make each round.* Participants could select either *True* or *False.* 54 out of 54 participants (100 percent) correctly answered this question on the first attempt, giving me comfort that participants attended to the manipulation.

5.3 Descriptive Statistics and Test of Hypotheses

Table 1 contains the descriptive statistics for variables used to calculate my dependent variable. Actual Revenue refers to the amount the project generated each round and is a random draw from a uniform distribution between 2.00 and 6.00.[14] Reported Revenue is the amount participants reported that their project generated each round and could be any value between their Actual Revenue and 6.00. Overstate is the difference between Reported Revenue and Actual Revenue. The dependent variable of interest, Dishonesty, measures the extent to which participants misreported their revenue, taking into consideration the actual revenue they received. I calculate it as Overstate / (6.00 – Actual Revenue). Prior literature has also used this measure (e.g., Evans et al. 2001; Maas and Van Rinsum 2013).

Table 1 provides preliminary evidence that Dishonesty is lowest in the *Low CI / Near Review* condition (0.69). The *High CI / Near Review* condition also had the lowest Actual Revenue and Reported Revenue. A t-test reveals that Actual Revenue is marginally higher in the *Low CI / Near Review* condition (4.17) than in the *High CI / Near Review* (3.87) condition ($p <$ 0.15, two-tailed). Therefore, in subsequent analyses, I provide results in which I control for Actual Revenue.[15]

To test my first hypothesis, I compare participants' responses to the following post-experiment question across conditions: *Within each individual round, what did you assess the likelihood of being selected for a review to be?* Participants responded with values between 0 percent and 100 percent. Consistent with my theory, I find that participants in the *Far Review* condition assess the likelihood of being selected for a review to be marginally lower than those

[14] The lower bound of 2.00 represents the cost of the project.
[15] I run both models because introducing *Actual Revenue* as a control variable on the right hand side of the equation could cause a mechanical relationship with the left hand side because *Actual Revenue* is used to calculate *Dishonesty*.

participants in the *Near Review* condition (7.6 percent compared to 12.2 percent, one-tailed t-test value of $p = 0.08$ non-tabulated).

My dataset consists of a panel of 324 (54 individuals x 6 rounds) pooled observations. Because correlation exists across rounds for the reporting decisions of any single participant, I use a panel estimation approach. Specifically I estimate all models with panel-specific heteroskedastic error terms (see e.g., Bruner, McKee, and Santore 2008). To test my second hypothesis, I use the following model:

$$\text{Dishonesty} = \alpha_0 + \alpha_1 \text{Far Review} + \alpha_2(\text{Far Review} * \text{High CI}) + \alpha_3 \text{High CI} + \alpha_4 \text{Age} + \epsilon$$

where Dishonesty is calculated as described above, Far Review is an indicator variable coded one if a participant was in the *Far Review* condition, zero otherwise, and High CI is an indicator variable coded one if a participant was in the *High CI* condition, zero otherwise. A negative and significant coefficient for α_2 would be evidence supporting Hypothesis 2.

Table 2, Model 1 provides the coefficient and standard errors from this model. Consistent with prior literature, I find support for the documented main effect that compensation interdependence has on dishonest reporting. Specifically, the coefficient for High CI is positive and significant ($p = 0.05$, one-tailed). I also find support for my second hypothesis. Specifically, I find a marginal negative interaction for Far Review * High CI ($p = 0.08$, one-tailed). This suggests that increased compensation interdependence decreases the effect that control centralization has on dishonest reporting. Figure 5 provides a graphical representation of these results.

As previously mentioned, I also control for the randomly generated actual revenue for each participant in each round. Model 2 in Table 2 provides the output when controlling for Actual Revenue. The inferences remain identical when controlling for Actual Revenue. I also

include round-specific indicator variables. These results (non-tabulated) provide evidence that round-specific effects are not significant within the data. Specifically, none of the round-specific indicator variables is significant when all are included within the model (all $p > 0.23$, two-tailed), and their inclusion does not change any inferences.

5.4 Evidence of Theory

I examine if the subjective detection likelihood variable used to test my first hypothesis mediates the relation between control centralization and dishonest reporting. Because compensation interdependence moderates this relation, I run the mediation analysis on both subsets of my sample (*High CI* and *Low CI*) separately (see, e.g., MacKinnon, Fairchild, & Fritz, 2007). I then compare the level of mediation that occurs within each subsample. I also control for age in all regressions performed in my regression analysis.

Figure 6 provides the results from this moderated mediation analysis. Panel A shows the results for those participants in the *High CI* condition. The second step in Baron and Kenny (1986) is to show that the independent variable affects the dependent variable (without the mediator). Within the *High CI* subsample, this test is not satisfied ($p = 0.74$, two-tailed). Therefore, within the *High CI* subsample, subjective detection likelihood does not mediate the relation between Control Centralization and Dishonesty. Panel B provides the results for those participants in the *Low CI* condition. In this analysis, the third step in Baron and Kenny (1986) is not met. Specifically, the relation between Detection Likelihood and Dishonesty is not significant ($p = 0.80$, two-tailed). Thus, perceived detection likelihood does not appear to mediate the relation between Control Centralization and Dishonesty in either subsample.

Unable to explicate the process underlying my results, I turn to exploratory analyses. I examine if increasing physical distance of the control decreases the perceived detection

likelihood, which, in turn, increases the perception that other participants dishonestly report. Perhaps the perception that other participants were dishonestly reporting induced increased dishonesty.

I asked participants the following post-experiment question: *How effective do you think the review was in preventing participants from overstating revenue?* Participants responded using a 7-point Likert Scale with higher values indicating higher perceived levels of effectiveness. I use this question as a measure of the extent to which participants believed the control prevented other participants from dishonestly reporting their performance. I label this variable Prevent Others. I examine if including Prevent Others as an additional step in the mediation path provides evidence of the underlying process driving my results.

Figure 7 provides the results from this additional mediation analysis. Recall that within the *High CI* subsample the second test of the Baron and Kenny (1986) was not satisfied in the original mediation, meaning there was no relation between the dependent and independent variables. Panel A reveals this holds true, as expected, in the new mediation analysis. However, Panel B reveals that all conditions for mediation are met in the *Low CI* subsample. Specifically, within the *Low CI* subsample, I find that, as controls become more physically distant, participants have lower subjective detection likelihood rates ($p < 0.01$, two-tailed). Controls perceived to have a lower detection likelihood are less likely to deter other participants from dishonestly reporting ($p < 0.01$, two-tailed). Controls less likely to deter other participants from dishonestly reporting are marginally associated with higher levels of dishonesty ($p = 0.16$, two-tailed). Finally, when including the two mediating variables the coefficient of the relation between Control Centralization and Average Dishonesty decreases from 0.14 to 0.11.

The results from my pilot study generally support my hypotheses. However, the results were contingent upon controlling for age. Moreover, a low percentage of participants correctly responded to one of my manipulation checks, and all statistical tests relied upon on a small sample that had only one session in each experiment condition. Therefore, I run a second study to address these issues. I discuss the results of my second experiment in the next section.

CHAPTER 6: FULL EXPERIMENT

I conduct my second experiment in a manner generally consistent with my first experiment. I conduct the experiment in a computer laboratory using z-Tree (Fischbacher 2007). Eighty-six accountancy students from a large public university in the Midwest participated in the experiment. All but one participant were sophomores, juniors, or seniors.[16] The average age of the participants is 20.4 years and 48 percent are male.[17]

6.1 Manipulation Checks

To measure whether participants attended to the control centralization and compensation interdependence manipulations, they answered two post-experiment manipulation check questions. The first question asked: *During your session, where did the individual performing the review stand while performing the review?* Participants selected between two responses: *at each participant's desk* or *at the front of the room.* Seventy-eight of the 86 participants (91 percent) responded correctly. The second question asked: *What calculation was used to determine how much you got paid each round?* Participants selected either *My reported revenue - $2.00* or *½ (My reported revenue - $2.00) + ½ (My partner's reported revenue - $2.00).* Eighty-four of 86 participants (98 percent) responded correctly. This suggests that participants attended to the manipulated information across conditions.[18]

[16] One participant was a graduate student. Results are inferentially identical when excluding the observation.
[17] Tests for demographic differences across conditions reveal that age and English as a primary language differ across the control centralization conditions. This is likely a result of the centralization manipulation being across sessions and random assignment of sessions to control centralization conditions (described later in detail). When including age and language as covariates in the ANOVAs described in Section IV, both independently and jointly, neither is significant at traditional levels (max p-value = 0.13, two-tailed). More importantly, all inferences are identical when including the covariates independently or jointly. This suggests that these demographic differences across conditions are not driving the results. Therefore, I do not discuss the difference in demographics further.
[18] When excluding those observations that missed either manipulation check question, the sample drops from 86 to 76. Both hypothesized results are marginally significant (all $p \leq 0.06$, one-tailed) after dropping those who missed a manipulation check question.

6.2 Data and Descriptive Statistics

Table 3 provides descriptive statistics for actual revenue, reported revenue, Overstate, and Dishonesty. Consistent with prior research, the data suggest that misreporting increases as compensation interdependence increases (Overstate: 1.71 vs. 1.40; Dishonesty: 0.72 vs. 0.56). Table 3 also shows that reported revenue, Overstate, and Dishonesty are all lowest in the *Near Review* / *Low CI* condition. This suggests that, of the four conditions tested, a decentralized control implemented in settings with low compensation interdependence results in the least misreporting.

Table 3 also reveals that the randomly generated actual revenues are highest in the *Near Review / Low CI* condition and lowest in the *Far Review* / *High CI* condition. Recall that actual revenue is randomly generated each round for each participant from a uniform distribution between $2.00 and $6.00. Therefore, on average, actual revenue should be $4.00. However, in both the *Near Review / Low CI* and *Far Review* / *High CI* conditions, total average actual revenue differs from $4.00 ($4.17; $p = 0.12$ and $3.81; $p = 0.04$, respectively, two-tailed t-tests). Given this difference, I examine the reporting patterns across all six rounds. Table 4 provides round-by-round detail for actual revenue and Dishonesty. In five of the six rounds, Dishonesty is lowest in the *Near Review* / *Low CI* condition. However, actual revenue is significantly higher than $4.00 in only two of the six rounds (Rounds two and six) ($p = 0.02$ and $p = 0.08$, respectively, two-tailed t-tests). This provides preliminary evidence that actual revenue is not driving the results. I also perform a series of robustness tests, described in detail later, to examine more fully whether differences in actual revenue explain the results.[19]

[19] The specific concern is within the *Near Review* / *Low CI* condition. Based on theory, I predict dishonest reporting to be lowest in this condition. However, because actual revenue is higher in this condition relative to the other conditions, less misreporting would be required to achieve to a specific economic payout. In other words, high actual revenue could lead to less dishonest reporting, and this could be driving the results.

6.3 Hypotheses Tests

Hypothesis 1 predicts that greater control centralization negatively influences employees' subjective assessment of detection likelihood. To test this hypothesis, I run an ANOVA with Compensation Interdependence and Control Centralization as the independent variables and the responses to the following post-experiment question as the dependent variable: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* Participants could respond with any value between 0 and 100, inclusive. Table 5 provides the results from this ANOVA. I find a significant main effect for control centralization. Specifically, participants in the *Near Review* condition assess a higher likelihood of being selected for a review relative to participants in the *Far Review* condition (33.9 percent compared to 23.9 percent, $p = 0.03$, one-tailed). This result comes through despite there being no difference in actual percentage of red chips relative to total chips drawn across the *Near Review* and *Far Review* conditions (9.3 percent compared 10.0 percent, respectively, $p = 0.78$, two-tailed t-test, non-tabulated). These findings support the hypothesis that greater spatial distance between a reporting manager and a control (i.e., control centralization) negatively influences employees' subjective assessments of detection likelihood.

Hypothesis 2 predicts that the difference in performance misreporting between a centralized and decentralized control is greater when compensation interdependence is low than when compensation interdependence is high. To test this hypothesis, I run a linear mixed ANOVA model with Dishonesty as the dependent variable and Control Centralization and Compensation Interdependence as the independent variables. I use a mixed model to control for correlation resulting from repeated observations from each participant over six rounds.

Table 6 provides the results from the ANOVA. Consistent with prior research (Church et al. 2012; Maas and Van Rinsum 2013), the main effect of Compensation Interdependence on Dishonesty is significant ($p < 0.01$, one-tailed). Panel B also reveals that the interaction effect is significant ($p = 0.04$, one-tailed), consistent with Hypothesis 2. Panel C shows a lack of significant round effects.[20] Simple effect tests shown in Panel D suggest that the interaction pattern is consistent with Hypothesis 2. Specifically, the effect of Control Centralization on Dishonesty is significant when Compensation Interdependence is low ($p = 0.04$, one-tailed), but not significant when Compensation Interdependence is high ($p = 0.44$, two-tailed). Figure 8 provides a graphical representation of the results.

Taken together, this evidence suggests that compensation interdependence has both a main and moderating effect on dishonest reporting. Compensation interdependence increases misreporting, and it also affects misreporting by reducing the effect that control centralization has on misreporting.[21]

6.4 Mediation Analysis

6.4.1 Subjective Detection Likelihood as a Mediator

I perform mediation analysis to test the process underlying the observed effects. I use the regression approach of mediation analysis (Baron and Kenny 1986) with Control Centralization as the independent variable, Average Dishonesty as the dependent variable, and Subjective

[20] I use a multi-period experiment to examine whether participants revised their reporting decision as they received more information about the control's effectiveness through seeing the number of red and white chips drawn each round. The lack of significant round effects suggests that participants did not receive information during the session that significantly influenced their reporting decision. I interpret this as evidence that participants were able to make a relatively accurate subjective assessment of the ratio of red and white chips during the review demonstration when they had a chance to estimate how many red and white chips were in the bowl. This is consistence with my findings from my pilot study.

[21] I perform the same mixed ANOVA using Overstate as the dependent variable. Results for the test of Hypothesis 2 are inferentially similar but weaker ($p = 0.06$, one-tailed t-test). I also run a random-effect ordered logistic model using an indicator variable coded one if a participant misreported and zero otherwise as the dependent variable. Results are inferentially similar but weaker ($p = 0.07$, one-tailed t-test) using the indicator dependent variable.

Detection Likelihood as the mediator. I perform mediation analysis for both the *High CI* and *Low CI* conditions separately to observe how the mediation differs across the two subsamples.[22] Figure 9 presents the results of these mediation analyses. Panel A contains the *High CI* subsample, and Panel B contains the *Low CI* subsample.

In both the *High CI* and the *Low CI* subsamples, the link between Control Centralization and Detection Likelihood is marginally significant, consistent with Hypothesis 1 ($p = 0.07$ and $p = 0.08$, one-tailed, respectively). In the *High CI* subsample, the link between Subjective Detection Likelihood and Average Dishonesty is not significant ($p = 0.42$, one-tailed), while it is marginally significant in the *Low CI* subsample ($p = 0.08$, one-tailed). Moreover, while no mediation occurs in the *High CI* subsample, a Sobel-Goodman test shows that Subjective Detection Likelihood mediates 17 percent of the total effect that Control Centralization has on Average Dishonesty in the *Low CI* subsample. Specifically, the coefficient and associated p-values go from 0.14 ($p = 0.04$, one-tailed) to 0.11 ($p = 0.07$ one-tailed), suggesting partial mediation. This evidence further supports Hypothesis 2 and suggests that, although participants perceive detection likelihood to be greater with decentralized controls, compensation interdependence reduces the effect that control centralization has on dishonest reporting.

6.4.2 Perception of Others' Misreporting Behavior as a Mediator

The mediation analysis shown in Figure 9 provides some evidence of the cognitive process underlying the interaction. However, subjective detection likelihood mediates only a small portion of the total effect that control centralization has on misreporting in the *Low CI*

[22] I also perform bootstrapped moderated mediation as suggested in model 14 of Hayes (2013). In this model, compensation interdependence moderates the effect between the mediator (perceived detection likelihood) and the dependent variable (average dishonesty). Results using 1,000 replications are inferentially identical to those presented in the paper. Specifically, compensation interdependence moderates the relation between the mediator and the dependent variable such that higher levels of compensation interdependence results in a less significant relation between these two variables.

subsample. As such, I perform an additional mediation analysis to understand whether another cognitive process underlies the interaction.

Control centralization could also affect misreporting by creating an environment in which individuals perceive others to be misreporting, increasing their own propensity to misreport. Specifically, control centralization could have different effects on the perceived descriptive norm for or against misreporting. Descriptive norms are beliefs about what is being done by most other members of a social group (Lapinski and Rimal 2005). If descriptive norms suggest others are misreporting, it could increase an individual's propensity to misreport.

To measure the perception that others are misreporting, I ask participants four post-experiment questions: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* (3) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (4) *Across all rounds, by how much do you think an average participant overstated his or her revenue?* Participants answered all four questions using a seven point Likert Scale with higher values indicating a higher perception that others were misreporting.[23] I run a principal component analysis on the responses to the four questions, which yields one factor with an eigenvalue greater than one. This factor, which I label Misreporting Norm, explains 63 percent of the total variance (Eigenvalue = 2.51) and each of the four questions has a loading score greater than 0.46.[24]

[23] Each question had the endpoints on the Likert Scale response labeled. For questions one and three, the endpoints were *No rounds* and *Every round*. For questions two and four, the end points were *Not at all* and *A large amount*. There was no label on the midpoints.

[24] The mean for Misreporting Norm across the four conditions is: *Near Review / High CI* = 0.18; *Near Review / Low CI* = -0.85; *Far Review / High CI* = 0.28; *Far Review / Low CI* = 0.20.

To examine whether a perceived descriptive norm is driving misreporting, I perform a Baron and Kenny (1986) mediation analysis with Control Centralization as the independent variable, Average Dishonesty as the dependent variable, and Misreporting Norm as the mediator. I split my sample based on *High CI* and *Low CI* to observe how the mediation is different across the two subsamples. Figure 10 provides the results from this mediation analysis.

The results show that control centralization has no effect on the perception that others were misreporting in the *High CI* subsample ($p = 0.84$, two-tailed). However, in the *Low CI* subsample, participants perceived others were misreporting more with a centralized control than a decentralized control ($p = 0.02$, two-tailed). In both subsamples, a greater perception that others are misreporting is associated with higher individual dishonest reporting (both $p \leq 0.02$, two-tailed). Moreover, while no mediation occurs in the *High CI* subsample, in the *Low CI* subsample, a Sobel-Goodman test reveals that Misreporting Norm mediates 66 percent of the direct effect from control centralization to average dishonesty. Specifically, the coefficient drops from 0.14 ($p = 0.08$, two-tailed) to 0.05 ($p = 0.50$, two-tailed). This evidence suggests that control centralization has a greater influence on the perception that others are misreporting and subsequent reporting behavior as compensation interdependence decreases.

6.5 Robustness Checks

Recall from Table 3 that Dishonesty is lowest in the *Near Review* / *Low CI* condition, but actual revenue is also highest in that condition. To ensure that differences in actual revenue do not explain the results, I perform a series of robustness checks. First, I run a mixed model with Dishonesty as the dependent variable, Control Centralization and Compensation Interdependence as the independent variables and Actual Revenue as a covariate. Table 7 reveals that inferences are unchanged when controlling for actual revenue. Specifically, the main effect of compensation

interdependence and the interaction effect are both still significant at conventional levels ($p < 0.01$ and $p = 0.03$, respectively, one-tailed).

One potential problem with including actual revenue as a covariate is that it introduces actual revenue to both sides of the equation underlying the ANOVA model. Specifically, because the dependent variable is (Reported Revenue – Actual Revenue)/(Max Revenue – Actual Revenue) and Actual Revenue is a covariate, actual revenue is present in the equation on both sides. This can lead to specification errors. I perform a second test to mitigate this issue.

I classify all observations by actual revenue into one of eight $0.50 increment bins. For example, bin 1 contains all observations with actual revenue between $2.00 and $2.49, bin 2 contains all observations with actual revenue between $2.50 and $2.99 and so on. Figure 11 provides a bar chart of the actual revenue in each bin across each of the four conditions. The red line in Figure 11 indicates the number of observations in each bin given a uniform actual revenue distribution. Figure 11 reveals that the relatively high average actual revenue in the *Near Review / Low CI* condition is the result of a small number of actual revenues between $2.00 and $2.49 (Bin 1) and a large number of actual revenues between $5.50 and $6.00 (Bin 8). Therefore, I run a mixed model ANOVA with Dishonesty as the dependent variable and Control Centralization and Compensation Interdependence as the independent variables excluding all observations in Bin 1 and Bin 8. After dropping these observations, the average actual revenue in the *Near Review / Low CI* condition is no longer significantly different from $4.00 ($4.03, $p = 0.78$, two-tailed t-test, non-tabulated). Table 8 provides the result of this test. The main effect for compensation interdependence is still marginally significant ($p = 0.09$, one-tailed), and the predicted interaction effect is significant ($p = 0.05$, one-tailed). This suggests that the results are robust to controlling for the effect that actual revenue has on reporting behavior.

CHAPTER 7: ADDITIONAL TESTS

In this section, I discuss the results of additional analyses designed to test the robustness of my findings. I first examine how robust my findings are to different specifications of the dependent variable.

7.1 Robustness of Dependent Variable

7.1.1 Overstate

I define Overstate as Reported Revenue – Actual Revenue. This variable differs from Dishonesty, my main variable of interest in that it does not consider to what extent a participant could misreport. For example, a participant who reported $6.00 of revenue after receiving actual revenue of $5.99 is the same as a participant who reported $3.01 of revenue after receiving actual revenue of $3.00.

To test the robustness of my findings, I perform a mixed ANOVA with Compensation Interdependence and Control Centralization as my independent variables and Overstate as my dependent variable. Table 9 provides the results from this analysis. The interaction remains marginally significant at traditional levels ($p = 0.06$, one-tailed). Additionally, the simple main effect of Control Centralization is significant when Compensation Interdependence is low ($p = 0.02$, two-tailed) and not significant when Compensation Interdependence is high ($p = 0.53$, two-tailed). Overall, this suggests that my findings are robust to using Overstate as the dependent variable.

7.1.2 Likelihood of Misreporting

Misreport is an indicator variable coded one if a participant overstated their reported revenue and zero otherwise. Therefore, this analysis does not consider the extent to which a participant misreported, just whether the participant did or did not misreport.

To test the robustness of my findings using an indicator variable as the dependent variable, I perform a logistic regression with Misreport as the dependent variable and Compensation Interdependence, Control Centralization, and the interaction of these two variables as the independent variables. Table 10 provides the results of this analysis. Panel A reveals that participants were least likely to misreport when compensation interdependence is low and the control is decentralized. This is consistent with my findings using both Dishonesty and Overstate.

Panel B also suggests a pattern consistent with the results for both Dishonest and Overstate. Because I run a logistic regression, the effects are all relative to the low compensation interdependence and *Near Review* condition.[25] Plotting out the pattern of results by using the coefficient of each line in the logistic regression reveals a pattern consistent with the ANOVA tests. Moreover, the interaction effect is marginally significant ($p = 0.06$, one-tailed), supporting Hypothesis 2. This suggests that the results are robust to using an indicator variable.

7.2 Robustness of Hypothesis 1 Tests

7.2.1 Participants Selected for a Review

In this section, I test potential alternative explanations for why I find support for my first hypothesis. Recall that Hypothesis 1 predicts that participants perceive the detection likelihood of a control to be greater with a relatively decentralized control than with a relatively centralized control. However, an alternative explanation is that more participants were selected for a review (i.e., had a red chip drawn) in the *Near Review* conditions relative to the *Far Review* conditions. To examine this alternative explanation, I first examine whether there is a significant difference in the subjective detection likelihood question for those participants selected for a review

[25] In this condition, both the indicator variables (compensation interdependence and control centralization) are set at zero. This becomes the intercept of the logistic regression and all effects are measured relative to that point.

compared to those participants not selected. Within my sample, 44 participants did not have a red chip drawn during any of the six rounds while 42 participants had at least one red chip drawn during the six rounds. When testing participants' responses to the perceive detection likelihood question across these two groups, I find, although close, no difference at traditional levels (24.5 percent versus 31.9 percent respectively, p = 0.14, two-tailed, non-tabulated).

I then examine each subsample in more detail. I first examine the 44 participants who did not have a red chip drawn during any of the six rounds. Within this subsample, there is a relatively equal split of participants in the *Near Review* condition (n = 20) and in the *Far Review* condition (n = 24). Participants in the *Near Review* condition assessed the detection likelihood higher than participants in the *Far Review* condition, although the difference in the two means is not statistically significant (26.9 percent versus 22.5 percent, p = 0.42, two-tailed, non-tabulated).

I then examine the 42 participants who had at least one red chip drawn during the six rounds. Within this subsample, there are fewer participants in the *Near Review* condition (n = 16) than in the *Far Review* condition (n = 26). Participants in the *Near Review* condition assessed the detection likelihood higher than participants in the *Far Review* condition, (42.6 percent versus 25.3 percent, p = 0.05, two-tailed, non-tabulated). This would suggest that most of the variation in participants' responses to this question is coming from those participants selected for a review.

I further examine whether a significant difference exists in the number of individuals who had multiple red chips drawn during the six rounds. Four participants in the *Far Review* condition and three participants in the *Near Review* condition had more than one red poker chip drawn during the six rounds. I investigate how the responses from those participants selected multiple times differed from those selected a single time. In both the *Far Review* and *Near*

Review condition, those participants selected multiple times assessed a lower probability of being selected in the future than those participants selected a single time, although the difference was not statistically significant (26.7 percent versus 17.5 percent, p = 0.49, two-tailed and 45.5 percent versus 30 percent, p = 0.44, two-tailed).

This evidence suggests that those participants who had no red chips or multiple red chips drawn during the session do not appear to be driving the results supporting my first hypothesis. Those selected for one review drive most of the variation. It is interesting to note that selection for a review could make the centralization manipulation more salient, and I cannot fully rule out that salience of the manipulation driven by review selection is partially driving my results.

7.2.2 Alternative Specification of Likelihood Detection.

Recall that I asked the following question to measure likelihood detection: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* In addition to this question, I also asked participants *Within each individual round, what did you assess the likelihood of being selected for a review to be? (0% - 100%).* In this section, I describe the results of my first hypothesis using this question to measure the subjective likelihood of detection. For this subsection, I refer to participants' responses to the first question as Session Review and their response to the second question as Round Review.

I first examine the correlation between Session Review and Round Review. The correlation between the measures is 0.24, which is significant at the 0.03 level. However, when performing a two-tailed t-test, I find evidence that only is directionally consistent with my first hypothesis. Specifically, the mean of Round Review is higher for those participants in the *Near*

Review condition than those in the *Far Review* condition; however, this difference is not statistically significant (14.7 percent vs 14.3 percent, $p = 0.92$, two-tailed, non-tabulated).

I examine why I observe a difference between Session Review and Round Review. I note that the standard deviation of Session Review is higher than the standard deviation for Round Review. I perform a variance ratio test to compare the standard deviations of these two variables. For this test, I divide the standard deviation of Session Review by the standard deviation of Round Review and I compare the resulting ratio to one. I find that the result of the variance ratio test is significantly different from one, suggesting that the standard deviations are significantly different from each other. Specifically, the standard deviation of Session Review is significantly higher than the standard deviation of Round Review (28.1 vs. 14.5, $p < 0.01$, two-tailed). This suggests that the Round Review question did not provide the participants enough variation in their responses to capture the difference that a decentralized versus centralized control created.

I then create composite measures combining Session Review and Round Review. The first composite measure I create is an average of Session Review and Round Review. Because both variables are percentages from 0 percent - 100 percent (the range was included in both questions), taking an average of the two measures is appropriate. Using this variable, which I call Average Review, provides results consistent with my first hypothesis. Specifically, participants in the *Near Review* condition perceive a marginally greater likelihood of being selected for a review than participants in the *Far Review* condition (24.2 percent vs. 19.1 percent, $p = 0.06$, one-tailed).

I create the second composite measure by running a principal component analysis (PCA) on Session Review and Round Review. The PCA creates a single factor (Eigenvalue = 1.24) that explains 62.1 percent of the variance. Both variables load positively on this factor with a

weighting of 0.71. The results using the single factor, which I call PCA Review, are also consistent with my first hypothesis. Specifically, I find that participants in the *Near Review* condition perceive a marginally greater likelihood of being selected for a review than participants in the *Far Review* condition (0.19 vs. -0.13, p = 0.10, one-tailed). This suggests that the results of Hypothesis 1 are marginally robust to different composite specifications of the variable used to measure subjective detection likelihood.

7.3 Robustness of Hypothesis 2 Tests

The focus of my second hypothesis is the interactive effect that control centralization and compensation interdependence have on dishonest reporting. My theory suggests that participants in the *Near Review*/*Low CI* condition engage in the lowest level of dishonest reporting. However, there could be alternative reasons why misreporting in that condition is lower than the other three conditions. In this subsection, I explore potential alternative explanations for my findings.

7.3.1 Robustness of Results across Rounds

I first examine the pattern of results within each of the six rounds. I am interested in determining if a single round is driving my results or if my results are consistent across all rounds. Figure 12 provides a graph of Dishonesty across the six rounds.

My theory predicts that less misreporting will occur with a decentralized control than a centralized control when compensation interdependence is low. I find a pattern of results directionally consistent with this prediction in five of the six rounds. The fourth round is the only round in which this pattern does not hold. In this round, a decentralized control has slightly more misreporting than a centralized control in the *Low CI* condition (0.55 vs. 0.53), although the difference is not statistically significant.

My theory also predicts that the difference in misreporting between a decentralized and centralized control will be greater when compensation interdependence is low than when it is high. I find a pattern consistent with this prediction in four out of the six rounds. When looking at the pattern of results across the six rounds, they all seem relatively consistent except the first and fourth rounds. In the first round, I find a significant main effect for control centralization given low compensation interdependence, as predicted. However, I also find a significant main effect for control centralization given high compensation interdependence that is opposite of what I would expect. Specifically, misreporting is higher with a decentralized control than a centralized control (0.82 vs. 0.48, $p = 0.01$, two-tailed). I perform additional tests on the first round results, described in detail later to examine further this finding.

In the fourth round, I find no simple main effects for control centralization given either high or low compensation interdependence. Therefore, my results appear not to be robust to the fourth round. Given my lack of a theoretical prediction as to why the fourth round would be any different from all other rounds, I attribute this null result to simple statistical properties that suggest doing the same test multiple times will create at least some null results.

7.3.2 Robustness of Results Excluding First Round

As mentioned in the previous subsection, the first round had an unexpected result. Recall that there was a simple main effect for control centralization given high compensation interdependence. However, the direction of this simple main effect was opposite of what I would expect. Specifically, Dishonesty was higher in the *Near Review* condition than in the *Far Review* condition when compensation interdependence was high (0.82 vs. 0.48, $p = 0.01$, two-tailed). I first examine potential reasons for this finding and, then, investigate whether my results change when excluding the first round.

I first examine whether including Actual Revenue in the ANOVA as a covariate changes the first round results. The effect of Actual Revenue is significant in the model ($p = 0.01$, two-tailed). However, the interaction effect is still significant ($p = 0.01$, two-tailed). Moreover, the simple main effect of Control Centralization given high compensation interdependence becomes weaker, but remains significant at traditional levels ($p = 0.04$, two-tailed). This suggests that the actual revenues generated in the first round are partially, but not fully, driving the simple effect. Given that this pattern occurred in the first round, before participants had seen their partner's first reported revenue and before the first review, I am unable to brainstorm what else could be driving this result. Given my inability to explicate why this pattern shows up in the first round, I examine whether my inferences remain unchanged if I exclude the first round.

Dropping the first round of observations reduces my sample size from 516 to 430 observations (86 participants * 5 observations). I rerun the ANOVA using this smaller sample size and compare these results to the results from my full sample. Excluding the first round decreases the significance of the interaction ($p = 0.15$, one-tailed). However, using the contrast codes described in Section 7.4, the interaction remains significant ($p = 0.01$, one-tailed). Moreover, the simple main effect of Control Centralization given low compensation interdependence remains significant ($p = 0.05$, two-tailed) while the simple main effect of control centralization given high compensation interdependence is not significant ($p = 0.95$, two-tailed). This suggests that my results are somewhat consistent when excluding the first round.

7.3.3 Robustness of Results Controlling for Partner's Report

My experimental design allowed participants to see the reported revenue of another participant in their session. z-Tree randomly selected a partner for each individual, and the partner remained the same through all six rounds. It is possible that a participant's decision to

misreport was only a function of their partner's reported revenue and not the control design. I examine this proposition by controlling for participants' partner's reported revenue. Specifically, I run a mixed ANCOVA with Dishonesty as the dependent variable and Control Centralization and Compensation Interdependence as the independent variables. I include the partner's reported revenue as a covariate. The control variable for the partners' reported revenue is a lagged variable. Specifically, because participants see their partner's reporting decision after they have made their own reporting decision, any influence that their partner's reported revenue has will occur in the subsequent round. Therefore, because participants made the first reporting decision prior to seeing their partner's reported revenue, I exclude this round. Thus, this test is a joint test of both the effect of the partners' reported revenue and the effect of control centralization and compensation interdependence on misreporting, excluding the first round.

Evidence from this test suggests that the combined effects of controlling for partners' reported revenue and dropping the first round does significantly change my results. Specifically, the interaction predicted in Hypothesis 2 is no longer supported ($p = 0.24$, one-tailed). Given that this test is a joint test, I perform additional tests to better understand what is driving this significant change in my inferences. Specifically, I rerun the analysis keeping the first round of data. For the partner's reported revenue in the first round, I put in $4.84. This was the fixed amount that all participants were informed that their partner reported in the practice round.[26] Since this number was consistent across all conditions, the extent to which it influenced reporting decisions should be the same across conditions.

[26] Participants were informed that their actual partner did not report this number. Specifically, they were told that the $4.84 was being reported to all participants so everyone would have a similar experience during the practice round. In other words, participants were not deceived into thinking that the reported revenue in the practice round was generated by their partner when, in fact, it was not.

I re-run the mixed ANCOVA with dishonesty as the dependent variable and control centralization and compensation interdependence as the independent variables and the participants' partner's reported revenue as a covariate. As mentioned, the covariate variable now includes $4.84 for all observations in the first round instead of a null value, which allows me to use all six rounds. Results from the mixed ANCOVA are consistent with my prediction, although weaker. Specifically, I find that the interaction is marginally significant ($p = 0.06$, one-tailed), indicating that controlling for participants' partner's reported revenue does somewhat decrease the significance of my findings. However, it does not qualitatively change my results such that my inferences would change. Therefore, the data do not support the alternative explanation that seeing another participant's reported revenue solely drives reporting behavior.

7.3.4 Robustness of Results Controlling for Cost of Misreporting

I intentionally designed my experiment to minimize the cost of misreporting. For example, although participants were able to see if the facilitator drew a red poker chip for each participant in the session, they did not know the outcome of the review for other participants. Specifically, they did not know if the individual who had the red chip drawn misreported or not. Therefore, the perceived cost of detection (i.e., having a red poker chip drawn) should be minimal across all conditions. However, it is possible that participants in the *Near Review* condition perceived higher cost associated with detected misreporting because the facilitator was physically closer to them relative to participants in the *Far Review* condition. Therefore, it is possible that differences in the perceived cost of misreporting could be driving my results. I anticipated this concern and asked three post-experiment questions designed to measure participants' perceived cost of misreporting. I describe these questions and my results when controlling for participants' perceived cost of misreporting in the next two subsections.

7.3.4.1 Perceived Monetary Cost

Although the actual monetary cost for being caught misreporting does not differ across conditions, it is possible that participants perceived differences across the *High CI* and *Low CI* conditions. To examine the potential effect that differences in perceived monetary cost has on misreporting, I ask participants the following post-experiment question: *With respect to your total earnings, how costly would it have been if you had overstated revenue and were selected for a review in the same round?* Participants responded using a 7-point Likert Scale with higher values indicating higher perceived monetary cost associated with being caught misreporting.[27]

I find no difference in the perceived monetary cost of misreporting across the *High CI* and *Low CI* conditions (4.47 vs. 4.31, p = 0.72, two-tailed). Interestingly, I do find a significant difference in participants' response to this question across the *Far Review* and *Near Review* conditions. Specifically, participants in the *Near Review* perceived higher monetary cost associated with being caught misreporting than participants in the *Far Review* condition (5.00 vs. 3.96, p = 0.02, two-tailed). This finding is somewhat surprising, and I describe what I believe is driving this result in subsection 7.3.4.3.

Given that I find a significant difference in perceived monetary cost across the *Far Review* / *Near Review* conditions, I examine whether perceived monetary cost is driving my results for either Hypothesis 1 or 2. To examine the effect that perceived monetary cost is having on Hypothesis 1, I run an ANCOVA with Detection Likelihood as my dependent variable and Control Centralization and Compensation Interdependence as my independent variables while controlling for perceived monetary cost. The results from this ANCOVA suggest that perceived monetary cost is a significant predictor of perceived detection likelihood (p = 0.05, two-tailed).

[27] The end points were labeled "Not at all costly" and "Very costly." The midpoint was not labeled.

Specifically, as participants perceive greater monetary cost associated with misreporting detection, they perceive the detection likelihood of the control to be greater. Controlling for the effect of perceived monetary cost also increases the significance of control centralization on perceived detection likelihood ($p = 0.01$, two-tailed). Therefore, although perceived monetary cost predicts perceived detection likelihood, it does not change the fact that participants in the *Near Review* condition perceived the control to be more likely to detect misreporting than do participants in the *Far Review* condition.

I also examine whether perceived monetary cost is driving my results for my second hypothesis. To examine this, I run a mixed ANCOVA with Dishonesty as the dependent variable and Control Centralization and Compensation Interdependence as the independent variables while controlling for perceived monetary cost of misreporting. I find that my results are inferentially consistent when including perceived monetary cost as a covariate. Specifically, the perceived monetary cost of misreporting is close, but not significant at traditional levels in the ANCOVA model ($p = 0.16$, two-tailed). Moreover, the interaction predicted by Hypothesis 2 remains significant ($p = 0.04$, one-tailed). Therefore, it does not appear that differences in the perceived monetary cost of misreporting are driving my results for either H1 or H2.

7.3.4.2 Perceived Social Cost

It is also possible that the perceived social cost of being caught misreporting could be driving my results. I designed my experiment to minimize the social cost associated with misreporting. For example, the other participants in the session did not know if a participant who had a red chip drawn had misreported or had reported honestly. In fact, the only person who knew this information was the facilitator of the experiment, and he kept a neutral face upon discovering if a participant had or had not misreported. Despite the experimental design,

participants could perceive differences in the social cost associated with being caught misreporting. For example, participants could perceive detection to be more socially costly when the facilitator was standing relatively close as opposed to when he was far away.

To examine the effect that differences in the perceived social cost of misreporting has on misreporting behavior, I asked participants two post-experiment questions. *With respect to <u>your reputation</u>, how costly would it have been if you had overstated revenue and were selected for a review in the same round* and *With respect to <u>social cost</u> (e.g., shame, embarrassment), how costly would it have been if you had overstated revenue and were selected for a review in the same round.* Participants responded using a 7-point Likert Scale with higher values indicating a higher perceived social cost associated with being caught misreporting.[28]

I first examine the correlation between participants' responses to these two questions. I find that these two questions have a high positive correlation (0.81). Given this high correlation, I combine these two variables to create a single composite variable. To do this, I run a PCA using the two variables. The PCA yields a single factor that explains 90.2 percent of the total variance (eigenvalue = 1.81). I label this factor PCA Social.

I then examine whether PCA Social differs across experimental conditions. I find that perceived social cost does not differ across the *High CI* and *Low CI* conditions ($p = 0.41$, two-tailed), but does differ across the *Near Review* and *Far Review* conditions ($p = 0.08$, two-tailed). This suggests that, despite my efforts to design the experiment so that the social cost of misreporting did not differ across conditions, the control centralization manipulation did influence participants' perceptions of the social cost of being caught misreporting.

[28] The end points were labeled "Not at all costly" and "Very costly." The midpoint was not labeled.

I investigate whether my results are robust to including perceived social cost as a covariate. To test the robustness of Hypothesis 1, I run an ANCOVA with Detection Likelihood as my dependent variable, Control Centralization and Compensation Interdependence as my independent variables, and PCA Social as a covariate. I find PCA Social is not a significant predictor of Detection Likelihood ($p = 0.62$, two-tailed). Moreover, Control Centralization remains a significant predictor of Detection Likelihood ($p = 0.02$, one-tailed). Thus, the results for Hypothesis 1 are robust to including the perceived social cost of detection as a covariate.

I also examine whether perceived social cost of detection is driving the results supporting my second hypothesis. To examine this, I run a mixed ANCOVA with Dishonesty as the dependent variable, Control Centralization and Compensation Interdependence as my independent variables, and PCA Social as a covariate. I find that my results are inferentially consistent when including PCA Social as a covariate. Specifically, PCA Social is close, but not significant at traditional levels in the ANCOVA model ($p = 0.13$, two-tailed). Moreover, the interaction predicted by Hypothesis 2 remains significant ($p = 0.03$, one-tailed). Therefore, it does not appear that differences in the perceived social cost of detection are driving my results for Hypothesis 2.

Overall, my results are robust to including controls for both the perceived monetary and social cost of detection individually. I also examine whether including both social and monetary cost variables concurrently changes my inferences. It does not. Specifically, Control Centralization is still a significant predictor of Detection Likelihood ($p = 0.01$, one-tailed), and the interaction effect predicted in Hypothesis 2 is still significant ($p = 0.03$, one-tailed).

7.3.4.3 Discussion on Perceived Cost of Misreporting

I find that the perceived social cost of detection differs across the *Near Review* and *Far Review* conditions. Specifically, participants perceived higher social cost when the facilitator was standing relatively close to them. This suggests that proximal controls not only influence employees' subjective assessment of detection likelihood, but also their perceived social cost of misreporting. Therefore, the use of decentralized controls can change employee behavior through multiple processes.

Interestingly, I also find that the perceived monetary cost of detection is higher with a decentralized control than a centralized control. I observe this difference although there was no actual difference in the monetary cost of misreported performance across conditions. I attribute this finding to participants having difficulty separating the social cost of detection from the monetary cost of detection. For example, prior research has documented that individuals have a difficult time separating affective considerations from financial considerations in an investing setting (Elliott et al. 2014). Consistent with that study, I contend that participants had a difficult time separating the monetary cost of detection from the social cost of detection. Therefore, because the social cost of detection was higher given a decentralized control than a centralized control, participants also assigned a higher monetary cost of detection in the *Near Review* condition than the *Far Review* condition.

7.4 Contrast Coding

I also consider how my results change using a contrast code based on my theoretical predictions. The contrast code I select to use is {-3, -1, 2, 2} with the code corresponding to the following conditions: {*Near Review*/*Low CI*, *Far Review*/*Low CI*, *Near Review*/*High CI*, *Far Review/High CI*}. This contrast coding is appropriate given my prediction of a simple main

effect of control centralization when compensation interdependence is low (*Near Review*/*Low CI* < *Far Review*/*Low CI*), a lack of a simple main effect when compensation interdependence is high (*Near Review*/*High CI* = *Far Review*/*High CI*), and a main effect for compensation interdependence documented in prior literature (*High CI* > *Low CI).*

Results for my interaction using all three dependent variables become stronger. The p-value of the interaction using Dishonest (Overstate) [Misreport] drops to two-tailed p-values of $p < 0.01$ ($p = 0.01$) [$p < 0.01$]. This suggests that a contrast code based on my theoretical predictions strengthens the results supporting Hypothesis 2.

7.5 Pattern of Reviews across Sessions and Rounds

Another potential alternative explanation for my results is that the pattern of red chips drawn across experimental conditions differed significantly. The specific concern is whether the number of red chips differed significantly across the *Near Review* / *Far Review* conditions. This is a concern because the design of my experiment necessitated that this manipulation take place across sessions as opposed to within sessions. Therefore, if participants in the *Near Review* condition saw a different number or pattern of red chips than participants in the *Far Review* condition, it could influence my results for both Hypothesis 1 and Hypothesis 2.

I first compare the number of red chips drawn per number of participants across the *Near Review* and *Far Review* conditions. In the *Near Review* condition, there were 300 poker chips drawn (50 participants * 6 rounds). Of the 300 poker chips selected, 30 were red. Therefore, the "hit rate" of red poker chips drawn in the *Near Review* condition is 10.0 percent. In the *Far Review* condition, there were 216 poker chips drawn (36 participants * 6 rounds). Of the 216 poker chips selected, 20 were red. Therefore, the "hit rate" of red poker chips drawn in the *Far Review* condition is 9.3 percent. I perform an odds ratio test to compare these two ratios across

the *Near Review* and *Far Review* conditions. The odds ratio test suggest that there is no significant difference between the two ratios (chi-square = 0.08, p > chi-square 0.78).

I next perform an odds ratio test for each round to determine if a significant difference exists between the ratio of red chips to total chips across the *Near Review* and *Far Review* conditions in any of the six round. Table 11 provides the ratio of red chips to total poker chips drawn across condition by rounds and the result of the odds ratio test for each round. In half the rounds, the ratio of red poker chips to total poker chips drawn is greater in the *Near Review* condition than in the *Far Review* condition. In the other three rounds, the ratio of red poker chips to total poker chips drawn is greater in the *Far Review* condition than in the *Near Review* condition. In only one round (round 3) does the ratio of red chips to total poker chips drawn significantly differ across the *Far Review* and *Near Review* conditions at traditional levels.

I examine how this difference in ratio of red chips to total poker chips drawn in the third round affects the reporting decisions in the next round (i.e., round 4). Given that there is a significantly higher ratio of red chips drawn in the *Far Review* condition than the *Near Review* condition, one would expect significantly less misreporting in Round 4 because participants perceived a higher number of red chips to be in the bowl of poker chips. However, the pattern of misreporting in Round 4 does not suggest that the ratio difference influenced misreporting. Specifically, there is no statistical difference in Dishonesty across the *Near Review* and *Far Review* conditions in Round 4. This suggests that a significant difference in the ratio of red chips to total chips drawn across the *Near Review* and *Far Review* conditions does not appear to be driving my results.

7.6 Other Manipulation Check Question

To measure whether participants attended to the control centralization manipulation I asked participants: *During the session, where did the reviews take place?* Participants were able to select between the two choices: *at each participant's desk* or *at the front of the room*. Forty-nine of the 54 participants (91 percent) correctly answered the question. However, one could argue that this question serves as a recall question instead of a manipulation check question. I consider this point in this subsection.

Psychological distance theory predicts that changes in one component of psychological distance will cause the other components of psychological distance to similarly change. Relevant to this study, as the spatial distance between the review and the reporting individual increases (decreases), the perceived hypotheticality of the review increases (decreases). Therefore, participants' responses to the perceived detection likelihood question serve as a manipulation check. Specifically, by observing a significant difference in the perceived detection likelihood question across the *Far Review* and *Near Review* conditions in the predicted manner, I feel confident that participants attended to the manipulation. I gain further comfort from the robustness checks I performed through which the effect of Control Centralization on Detection Likelihood remained significant.

I also examine how participants' perceptions of the control change across the *Near Review* and *Far Review* conditions. Construal level theory predicts that as psychological distance increases (decreases) between an individual and an object or event, the object or event will be viewed more abstractly (concretely). Therefore, participants in the *Near Review* condition should have a relatively more concrete perception of the control than do participants in the *Far Review* condition. One way prior literature has measured difference in abstract and concrete perceptions

is through classification exercises (e.g., Bar-Anan, Liberman, Trope and Algom 2007). Specifically, participants who have a relatively abstract (concrete) perception of an object or event tend to classify the object or event as belonging to relatively more (fewer) categories.

As a second manipulation check, I ask participants a post-experiment question in which they "consider how an internal control similar to the review process in today's session would be classified within a firm." Participants had a list of six types of control categories and definitions of each category. They could classify the control in as many categories as they wanted. Three of these control categories are the control classifications used by COSO to describe control activities (preventative, detective, and corrective). I compare the average number of COSO control categories that participants used to classify the control across the *Near Review* and *Far Review* conditions. If greater psychological distance exists in the *Far Review* condition than the *Near Review* condition, participants in the *Far Review* condition should have a more abstract view of the control and, therefore, classify it using more COSO categories than participants in the *Near Review*.

Results from this test suggest that participants in the *Far Review* condition classify the review using an average of 1.70 COSO control categories while participants in the *Near Review* condition classify the review using an average of 1.47 COSO control categories. The difference between these two values is significant at traditional levels ($p = 0.05$, two-tailed). This finding provides additional support that participants attended to the control centralization manipulation. It also provides further evidence that psychological distance theory is underlying the results supporting Hypothesis 1.

7.7 Mediation Analysis

To provide evidence of my theory, I perform a mediation analysis. In this section, I provide more detail about the different mediation analyses I perform.

7.7.1 Mediation Using Perceived Detection Likelihood

I first use the regression approach of mediation analysis (Baron and Kenny 1986) with Control Centralization as the independent variable, Average Dishonesty as the dependent variable, and Detection Likelihood as the mediator. I split my sample based on *High CI* and *Low CI* to observe how the mediation is different across the two subsamples. Figure 9 presents the results of the mediation analysis. Panel A contains the *High CI* subsample and Panel B contains the *Low CI* subsample. I discussed these results in Section 6.4.1.

I also perform bootstrapped moderated mediation as suggested in Model 14 of Hayes (2013). In this model, compensation interdependence moderates the effect between the mediator (perceived detection likelihood) and the dependent variable (average dishonesty). I bootstrap using 1,000 replications to check the robustness of my results. Panel A of Figure 13 provides the results from the model.

I find that the link from Control Centralization to Detection Likelihood is negative and significant ($p = 0.03$, one-tailed). This suggests that as a control goes from a decentralized control to a centralized control, the perceived detection likelihood decreases, consistent with my first hypothesis. I also find the link from perceived detection likelihood to average dishonesty is negative and marginally significant, suggesting that higher perceived detection likelihood led participants to be less dishonest in their reporting. The path from compensation interdependence to average dishonesty is positive, but not statistically significant ($p = 0.17$, one-tailed). This implies that, inconsistent with prior literature, compensation interdependence does not have a

direct effect on dishonest reporting when the mediators are present. I also find that the moderation path is positive, but insignificant at traditional levels ($p = 0.11$, one-tailed). Specifically, as compensation interdependence increases, it makes the link between detection likelihood and average dishonesty more positive. However, *ceteris paribus*, this relation is negative. Therefore, increased compensation interdependence weakens the association between detection likelihood and average dishonesty, consistent with the second hypothesis.

I then examine whether the mediated conditional indirect effects decrease as the moderator variable increases, consistent with recommendations on bootstrapping to examine moderated mediation (Preacher & Hayes 2008; Preacher, Rucker, and Hayes 2007). Specifically, I predict that aggressive reporting behavior resulting from higher compensation interdependence decreases the significance of the path between detection likelihood and average dishonesty. Therefore, as the moderating variable (compensation interdependence) increases, one would expect less of the indirect effects to be mediated because the path between detection likelihood and average dishonesty is becoming less significant.

Panel B of Figure 13 provides the bootstrapped indirect effect coefficient, standard error, z-score and p-value. As Compensation Interdependence increases, mediated conditional indirect effects decrease. Specifically, the coefficient goes from 0.03 to 0.01 to -0.00 and the associated p-values become less significant as compensation interdependence increases (0.27, 0.40, 0.98). This evidence, taken together with the regression approach of mediation analysis, suggests that the perceived detection likelihood does appear to play a role in the participants' responses to control centralization. Further, compensation interdependence moderates the effect that perceived detection likelihood has on dishonest reporting. In totality, it would appear that there is strong evidence of my theory in my experimental setting.

7.7.2 Mediation Using Perceived Social Norms

I also perform a moderated mediation analysis using the bootstrapping technique suggested in model 8 of Hayes (2013) with a measure of perceived descriptive norms as the mediator. In this model, compensation interdependence moderates the relation between the independent variable (control centralization) and the dependent variable (average dishonesty). I then bootstrap using 1,000 replications to check the robustness of my results using the moderated mediation analysis. Panel A of Figure 14 provides the results from the moderated mediation model.

I find that the path from control centralization to perceived descriptive norms is positive and significant. This suggests that as a control becomes more centralized, people perceive a stronger descriptive norm of misreporting. I also find a positive significant path from compensation interdependence to the descriptive norm of misreporting. This suggests that as compensation becomes interdependent, participants perceive a stronger norm of misreporting. However, the interaction effect of compensation interdependence and misreporting norm is negative, which suggests that increased compensation interdependence reduces the association between control centralization and misreporting norms. Specifically, it would appear that the direct effect of compensation interdependence reduces the significance of the path between control centralization and the perceived misreporting norm such that control centralization only affects perceived norms when compensation interdependence is low. This provides further evidence that control centralization is likely to reduce dishonest reporting only when compensation interdependence is low.

Panel B of Figure 14 provides the bootstrapped indirect effect coefficient, standard error, z-score and p-value. As Compensation Interdependence increases, condition indirect effects

decrease. Specifically, the coefficient goes from 0.07 to 0.04 to 0.01 and the associated p-values reveal the mediated indirect effects decrease as compensation interdependence increases (0.08, 0.13, 0.82). This evidence, taken together with the regression approach of mediation analysis, suggests that the perceived descriptive norms of misreporting appear to play a role in participants' responses to control centralization. Further, compensation interdependence moderates the effect that control centralization has on the creation of descriptive norms. I explain why I believe I find this effect in the next subsection.

7.7.3 Theoretical Support for Descriptive Norm Finding

I find evidence that compensation interdependence appears to reduce the effectiveness of control centralization in creating descriptive norms against misreporting. This is consistent with prior theoretical economic research that shows that under certain conditions economic incentives can crowd out social incentives (Huck, Kübler, and Weibull 2012). In my study, the shared economic incentives of misreporting and increasing compensation for both parties could have crowded out the motivation to adhere to any perceived descriptive norms against misreporting.

Moreover, research in the communication field suggests that as group identity increases, the norms associated with the group become more salient and can crowd out other behavior-influencing norms (Lapinski and Rimal 2005). Therefore, since compensation interdependence can increase group identity (Akerlof and Kranton 2005; Chen and Li 2009), one would expect that as compensation interdependence increases, participants pay more attention to the perceived norm in their small group compared the perceived norm of the session. To examine this proposition, I run a separate PCA on the two post-experiment questions designed to measure the perception that a participant's partner was misreporting and the two-post experiment questions designed to measure the perception that other participants in the session (other than their partner)

were misreporting. Specifically, I perform a separate PCA on questions 1 and 2 and on questions 3 and 4 of the following questions: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* (3) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (4) *Across all rounds, by how much do you think an average participant overstated his or her revenue?*

I label the two factors Partner Norm and Other Norm. I then perform two ANOVAs using these two factors as the dependent variables. Control centralization and compensation interdependence are the independent variables. Tables 12 and 13 provide the results from these ANOVAs and Figures 15 and 16 provide a graphical representation of the means.

I find that compensation interdependence has a direct effect on Partner Norm ($p = 0.07$, two-tailed). This suggests that as compensation interdependence increases, participants perceive that their partner engages in more dishonest reporting. I also find that the interaction of compensation interdependence and control centralization is significant ($p = 0.04$, two-tailed). Specifically, when compensation interdependence is low, participants perceive that their partner is misreporting his or her performance less with a decentralized versus centralized control ($p = 0.03$, two-tailed). However, when compensation interdependence is high, participants perceive their partner is reporting no differently with a decentralized versus centralized control ($p = 0.38$, two-tailed). This evidence suggests that the effectiveness of control centralization in changing an employee's perceptions that relatively socially similar employees are misreporting is contingent upon compensation interdependence.[29]

[29] I label the participant's partner as "relatively socially similar" because participants answered two post-experiment questions about how similar to their partner they perceived themselves to be and how similar to the other participants in the session they perceived themselves to be. Participants responded to these questions using a 7-point Likert Scale with higher values indicating a greater degree of similarity. The endpoints were *Not at all Similar* and

Related to *Other Norm*, I find a significant main effect for control centralization (p = 0.05, one-tailed) whereby participants perceived other participants to be less likely to misreport performance with a more decentralized control. However, the interaction effect and the main effect for compensation interdependence are both insignificant. This suggests that a different pattern is taking place as participants consider to what extent a relatively similar participant misreported versus a non-similar participant.

When comparing Figures 15 and 16, the high compensation interdependence line appears to be driving the difference in the results. More precisely, it appears that the *Near Review* / *High CI* condition is driving the results. The average perception that partners were misreporting was highest in that condition. However, that condition also had the third highest average perception that non-partner participants were misreporting. I explain this finding using social identity theory.

Social identity theory is the extent to which individuals identify with socially similar individuals (Hogg et al. 2004). Prior research has established that social identity increases as individuals share common outcomes (Akerlof and Kranton 2005; Chen and Li 2009) and individuals within a group with high group identity more likely view outsiders as a threat to the group if they have the ability to prevent the group from achieving its objectives (Cikara, Botvinick, and Fiske 2011). Research suggests that this phenomenon, labeled the "us versus them" effect, is present from an early age and can lead to significant differences in behavior (Mahajan and Wynn 2012).

Very Similar. There was no label on the midpoints. Participants perceived themselves to be more similar to their partner than a random other person in the session (4.87 versus 4.57, p = 0.04, two-tailed).

The “us versus them” mentality could be playing out in my setting. Specifically, a participant could see their partner as part of an in-group association that is not present when considering non-partner participants within the session. Increases in compensation interdependence likely increase this perception by increasing social identity. As the threat of detection moves closer to the participants (through the decentralization of a control), the response to this threat could be to fight back by increasing misreporting. Participants will likely think that their partner also increases their misreporting in response to this threat (or wishfully think they did) and, therefore, perceive a higher norm that their partner is misreporting. Because no such social identity exists with non-partner participants, the interactive effect of control centralization and compensation interdependence does not occur when examining non-partner participants. Instead, the creation of descriptive norms against misreporting is primarily driven by the proximity of the control to the reporting individual through control centralization. Admittedly, this is speculation at this point. I discuss how future research involving experiments designed to test this theory could shed more light on these findings in Chapter 9.

CHAPTER 8: WITHIN SUBJECTS CONTROL CENTRALIZATION MANIPULATION

8.1 Overview of Pilot Study - Within

In addition to my initial pilot and full experiment, I also run a pilot experiment in which I manipulate control centralization within subjects. The purpose of this experiment is to examine whether a change in the centralization of a control triggers changes in employees' behaviors. Additionally, I am interested in examining whether control centralization changes can lead to long-term changes in employee behavior.

8.2 Design of Pilot Study – Within

I design this pilot study similar to my full experiment. Participants make reporting decisions and receive compensation based on their reported performance. The participants are subject to the same review process whereby the facilitator draws a poker chip from a bowl and z-Tree adjusts compensation to zero if the chip is red and he or she misreported. The only difference between this pilot and my full experiment is the control centralization manipulation.

In this experiment, the control centralization manipulation was manipulated within-subjects after the third round across sessions. I randomly assigned each session to one of two conditions: *Near Review First* or *Far Review First*. I read the same script and showed the same review demonstration to those participants in the *Near Review First* condition as I did to those participants in the *Near Review* condition in my full experiment. Similarly, I read the same script and showed the same review demonstration to participants in the *Far Review First* condition as I did to participants in the *Far Review* condition in my full experiment. After the third round reviews were complete but before participants made their fourth round reporting decision, I informed participants that the control would change for the last three rounds. I informed participants in the *Near Review First* condition that the poker chip selection for each participant would take place at the front of the room for the final three rounds. I told participants in the *Far*

Review First condition that the poker chip selection for each participant would take place at each participant's desk for the final three rounds. I did not inform participants of the reason for the change.

I also randomly assigned all participants within each session to either the *High CI* or *Low CI* condition. The compensation scheme used in this pilot experiment is identical to the compensation scheme used in my full experiment.

8.3 Participants in the Pilot Study – Within

Sixteen students from a large public university in the Midwest participated in the experiment. None of these participants participated in my first pilot study or in my full experiment. All students were sophomores, juniors, or seniors in the undergraduate accountancy program. The average age of all participants is 20.0 years old, and 56.3 percent of the participants are male.

8.4 Results of Pilot Study – Within

Given the small size of the pilot study, I hesitate to make inferences from these results. Therefore, the reader should consider all results discussed in this section to be preliminary and should not make strong inferences until additional data collection occurs.

8.4.1 Exclusion of Compensation Interdependence

I first examine the pattern of Dishonesty across participants before and after the within subjects manipulation. For this analysis, I do not yet separate out the effect of compensation interdependence on behavior. Specifically, I only examine the difference in *Dishonesty* across the within-subjects control centralization manipulation. Table 14 provides the results.

Participants in the *Far Review First* condition slightly increased their misreporting from the first three rounds to the last three rounds (0.75 vs. 0.79, $p = 0.73$, two-tailed). However,

participants in the *Near Review First* condition significantly increased their misreporting from the first three rounds to the last three rounds (0.74 vs. 0.89, p = 0.14, two-tailed). In other words, participants significantly increased their dishonest reporting when the facilitator changed from drawing poker chips at each participant's desk in the first three rounds to drawing poker chips at the front of the room in the last three rounds. However, there was no significant change in dishonest reporting when the facilitator changed from selecting poker chips at the front of the room to selecting poker chips at each individual participant's desk.

I next examine the pattern of results across all six rounds. Recall that the announcement took place after the third round review process but before the fourth round reporting decision. Because the reporting decision in the fourth round preceded the manipulated review, I can attribute any change in the fourth round to the communication of the change in the control. Since participants were not subject to the new control regime (resulting from the within manipulation) until after their fourth round reporting decisions were made any sustained change from the within subjects manipulation would be found in the fifth and sixth rounds.

Table 15 provides the mean and standard deviation of Dishonesty across rounds. Figure 17 provides a graphical representation of average Dishonesty across rounds. The pattern of Dishonesty in Figure 17 provides evidence that the announcement of the control change had a significant effect on reporting behavior. After the facilitator announced the control would take place at the front of the room instead of at each participant's desk, dishonesty increased (0.81 to 0.93). However, after the facilitator announced the control would take place at each participant's desk instead of at the front of the room, Dishonesty decreased (0.85 to 0.62). This evidence suggests that the communication of a control change to employees can have a significant effect on their reporting behavior.

I also find that the change from a decentralized to a centralized control and from a centralized to decentralized control does not appear to create sustained behavior changes. In both Round 5 and Round 6, there are no significant differences between the *Near Review First* and *Far Review First* conditions. This suggests that employee behavior changes after the communication of a change in control centralization, but without additional interventions, the change does not appear to be permanent.

8.4.2 Consideration of Compensation Interdependence

In the analysis of the pilot study data up to this point, I make no consideration for compensation interdependence. In this subsection, I analyze the data and discuss whether compensation interdependence changes the results.

Recall that I randomly assigned participants to compensation interdependence conditions within each session. Because I randomly assigned participants, there was not a uniform distribution of participants in the *High CI* and *Low CI* conditions. There were two participants in the *Low CI / Near Review First* condition and six participants in the *High CI / Near Review First* condition. There were four participants in both the *Low CI / Far Review First* and *High CI / Near Review First* conditions. To tease out the effect of compensation interdependence, I compare participants' average dishonesty across the *Far Review First* and *Near Review First* conditions conditional upon participants being in either the *Low CI* or *High CI* conditions. I remind the reader that this reduces my already small sample to even smaller subsamples. Table 16 provides the results from this analysis, and Figure 18 provides a graphical representation of average dishonesty across rounds.

I first compare dishonest reporting for those participants in the *High CI* condition. I find an upward spike in misreporting in the fourth round when the facilitator moved the review to the

front of the room from each participant's desk. However, there was no change in reporting behavior when the facilitator moved the control from the front of the room to the participant's desk. I also find that participants in the *Near Review First* condition had higher average dishonesty in all rounds than participants in the *Far Review First* condition.

I then compare dishonest reporting for those participants in the *Low CI* condition. I find a downward spike in misreporting in the fourth round when the facilitator moved the review from the front of the room to each participant's desk. There was also a small dip in misreporting when the facilitator moved the review to the front of the room from each participant's desk. Therefore, it would appear that both control changes reduced misreporting when compensation interdependence was low, but the change from a centralized to decentralized control resulted in a greater decrease in dishonest reporting.

This evidence suggests that compensation interdependence could moderate the effectiveness of a change in control centralization. Again, I performed these analyses with limited observations. Before the reader makes inferences from these results, I should collect additional data to ensure the results are consistent. However, this evidence does suggest that future research examining how compensation schemes moderate the effectiveness of a control change could be fruitful.

CHAPTER 9: CONCLUSION

This study examines and finds evidence that compensation interdependence moderates the effect that control centralization has on misreporting. Specifically, when compensation interdependence is low, employees misreport less with a decentralized control than a centralized control. However, with high compensation interdependence, the difference in performance misreporting between centralized and decentralized controls decreases. Examination of post-experiment questions suggests that subjective detection likelihood increases as a control becomes more decentralized, independent of compensation interdependence. However, the difference in reporting behavior resulting from greater perceived detection likelihood decreases with greater compensation interdependence.

Additional analyses reveal that control centralization can change the perception that other employees are misreporting (i.e., the descriptive norm of misreporting). As employees perceive a stronger descriptive norm for misreporting, they increase their own dishonest reporting. However, compensation interdependence moderates the effect that control centralization has on the perception of a misreporting norm. Specifically, as compensation interdependence increases, employees are less likely to perceive that control centralization influences the descriptive norm of misreporting. This evidence suggests that compensation interdependence not only has the previously documented direct effect on dishonest reporting, but also an indirect effect by moderating how effective control centralization is at influencing reporting behavior.

This study contributes to control literature by examining a new construct, control centralization, and providing evidence that it can have a significant effect on employees' perceptions of control effectiveness. While this study focuses on one outcome of control centralization, misreporting, future research could examine both the determinants of control

centralization and other outcomes of control centralization. Future research can use the control centralization framework described in this study as a springboard for future exploration of the control centralization construct.

This study also contributes to the control literature by offering theory and evidence of the interaction among controls and their joint influence on reporting behavior. While prior literature identifies the direct effect that compensation interdependence has on misreporting, my study contributes by highlighting a moderating effect. This moderating effect is important because it suggests that compensation interdependence can mute the effectiveness of controls designed in part to reduce misreporting. In addition to documenting this moderating effect, this study also provides evidence of the cognitive process through which this effect occurs. Specifically, this study provides a glimpse into how perceptions about a control itself and perceptions about how others respond to a control underlie the moderating effect. Practitioners, including control system and compensation system designers, can leverage this study when implementing controls.

Limitations to this study provide opportunities for future research. Similar to other studies examining dishonesty in a lab setting, the penalty for misreporting is significantly lower than an individual would face in the natural setting. Moreover, detection likelihood was set intentionally low to induce sufficient dishonest reporting, the construct of interest. Future research could consider how stronger penalties for dishonest reporting and a higher probability of detection could influence performance misreporting.

This study also makes several design choices to cleanly test the theory outlined in this paper. For example, this study does not examine how negative compensation interdependence and control centralization jointly influence misreporting. Additionally, this study does not examine reviews contingent upon reported performance. If researchers were able to develop

theoretical reasons why these design choices would change the pattern of results presented in this study, it would certainly warrant future research.

The theory underlying my first hypothesis is that increasing the psychological distance between a control and a reporting individual decreases their perception that the control will detect misreporting. At a conceptual level, this would suggest that as control centralization increases, it decreases the perceived effectiveness of the control. I operationalize control centralization by manipulating the location where the control occurs. Given that control centralization is multi-faceted, one could question whether my chosen operationalization generalizes to the other dimensions of control centralization.

As previously discussed, as a control becomes more centralized there is likely to be greater social distance between the individual performing the control and the reporting manager. It is unclear how social distance influences the perceived effectiveness of the control. On one hand, low social distance could foster trust between the two parties, which could cause the reporting manager to believe the individual performing the control will be less ardent about the control. This could decrease the perceived effectiveness of the control. On the other hand, a high level of trust fostered by low social distance could lead the reporting manger to take actions to minimize the likelihood of having that trust broken. Knowing that a control is in place to detect misreporting, the reporting manager could be more honest in their reporting to prevent damaging the mutual trust. Because the control serves as a mechanism that could destroy trust, it becomes more effective in that the reporting manager will be more honest to preserve the trust. Therefore, it is unclear whether the location operationalization of control centralization generalizes to the social component of control centralization. This is an open empirical question.

It is also unclear how the use of a standardized versus idiosyncratic control affects perceived control effectiveness. On one hand, a centralized control will likely decrease the variance of the quality of the control within the firm. Because the firm implements and performs the same control across all business units, it is unlikely that differences across business unit controllers' personality, training, time, or resources will drive differences in control quality. This could increase the perceived effectiveness of the control. On the other hand, each business unit's controller could utilize their knowledge about the local operating environment when designing and performing a decentralized control. If the business unit controller has specific knowledge about how an employee could misreport within their business unit, then this information will potentially be lost with a centralized control. This could decrease the perceived effectiveness of the control. Therefore, whether a standardized control increases or decreases perceived control effectiveness is also an open empirical question.

Given the discussion in the last two paragraphs, there are some limitations to the extent to which my operationalization of control centralization generalizes to other operationalization. However, my chosen operationalization, the location where control work occurs, represents an exogenous choice variable that firms can change to influence employee reporting behavior. As such, despite this limitation in generalizability, this dissertation makes a significant contribution to the understanding of how control centralization influences reporting behavior.

Appendix 1: Excerpt from D.R. Horton, Inc. 2014 10-K

Decentralized Homebuilding Operations

We view homebuilding as a local business; therefore, most of our direct homebuilding activities are decentralized, which provides flexibility to our local managers on operational decisions. At September 30, 2014, we had 37 separate homebuilding operating divisions, many of which operate in more than one market area. Generally, each operating division consists of a division president; a controller; land entitlement, acquisition and development personnel; a sales manager and sales and marketing personnel; a construction manager and construction superintendents; customer service personnel; a purchasing manager and office staff. We believe that our division presidents and their management teams, who are familiar with local conditions, generally have the best information on which to base many decisions regarding their operations. Our division presidents receive performance based compensation if they achieve targeted financial and operating metrics related to their operating divisions. Following is a summary of our homebuilding activities that are decentralized in our local operating divisions, and the control and oversight functions that are centralized in our regional and corporate offices:

Operating Division Responsibilities

Each operating division is responsible for:

- Site selection, which involves
  - — A feasibility study;
  - — Soil and environmental reviews;
  - — Review of existing zoning and other governmental requirements;
  - — Review of the need for and extent of offsite work required to obtain project entitlements; and
  - — Financial analysis of the potential project;
- Negotiating lot option, land acquisition and related contracts;
- Obtaining all necessary land development and home construction approvals;
- Selecting land development subcontractors and ensuring their work meets our contracted scopes;
- Selecting building plans and architectural schemes;
- Selecting construction subcontractors and ensuring their work meets our contracted scopes;
- Planning and managing homebuilding schedules;
- Developing and implementing local marketing and sales plans;
- Determining the pricing for each house plan in a given community; and
- Coordinating post-closing customer service and warranty repairs.

Centralized Controls

We centralize many important risk elements of our homebuilding business through our regional and corporate offices. We have five separate homebuilding regional offices. Generally, each regional office consists of a region president, legal counsel, a chief financial officer and limited office support staff. Each of our region presidents and their management teams are responsible for oversight of the operations of a number of homebuilding operating divisions, including:

•Review and approval of division business plans and budgets;

•Review of all land and lot acquisition contracts;

•Review of all business and financial analysis for potential land and lot inventory investments;

•Oversight of land and home inventory levels;

•Monitoring division financial and operating performance; and

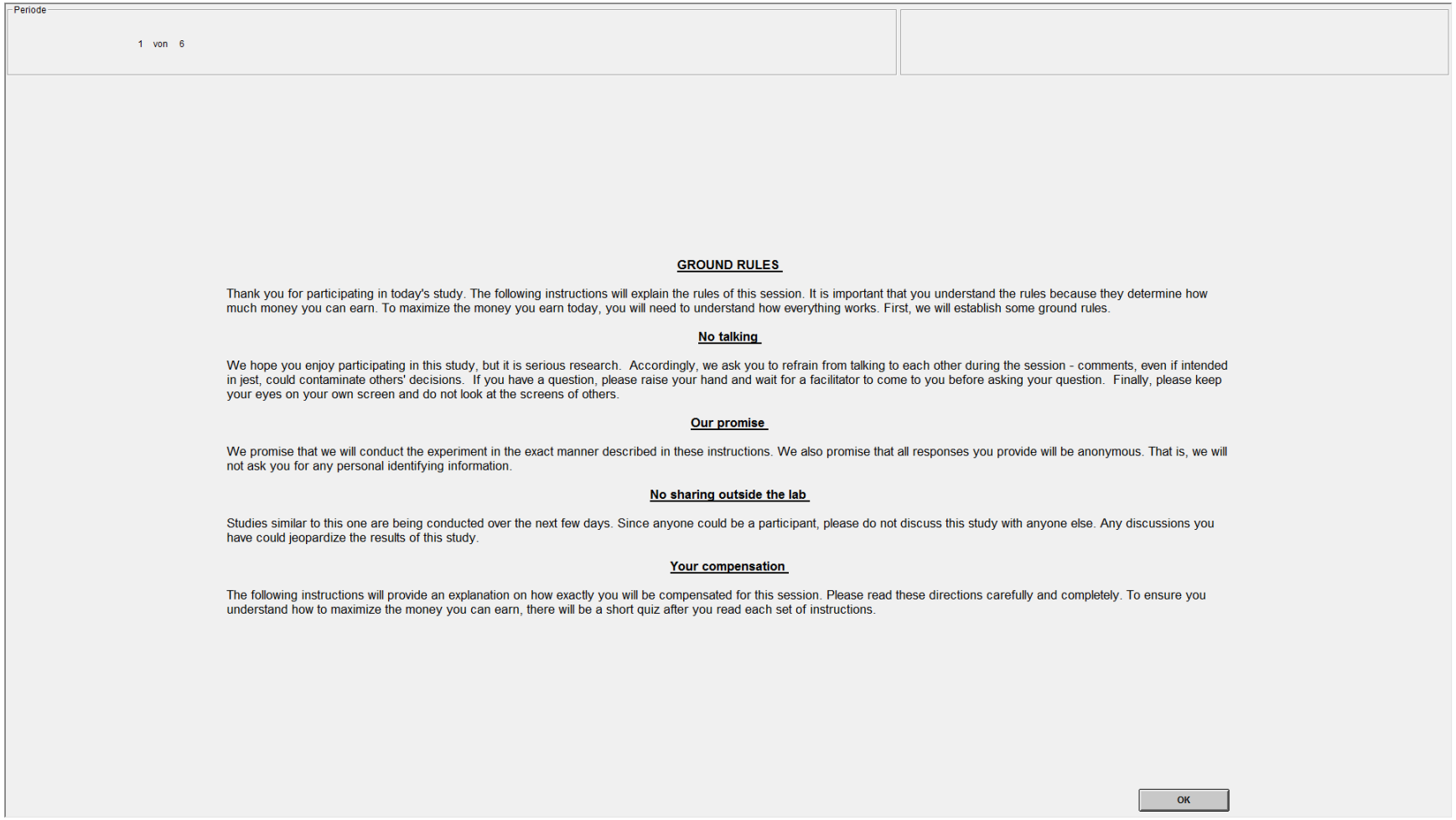
•Review of major personnel decisions and division incentive compensation plans.

Our corporate executives and corporate office departments are responsible for establishing our operational policies and internal control standards and for monitoring compliance with established policies and controls throughout our operations. The corporate office also has primary responsibility for direct management of certain key risk elements and initiatives through the following centralized functions:

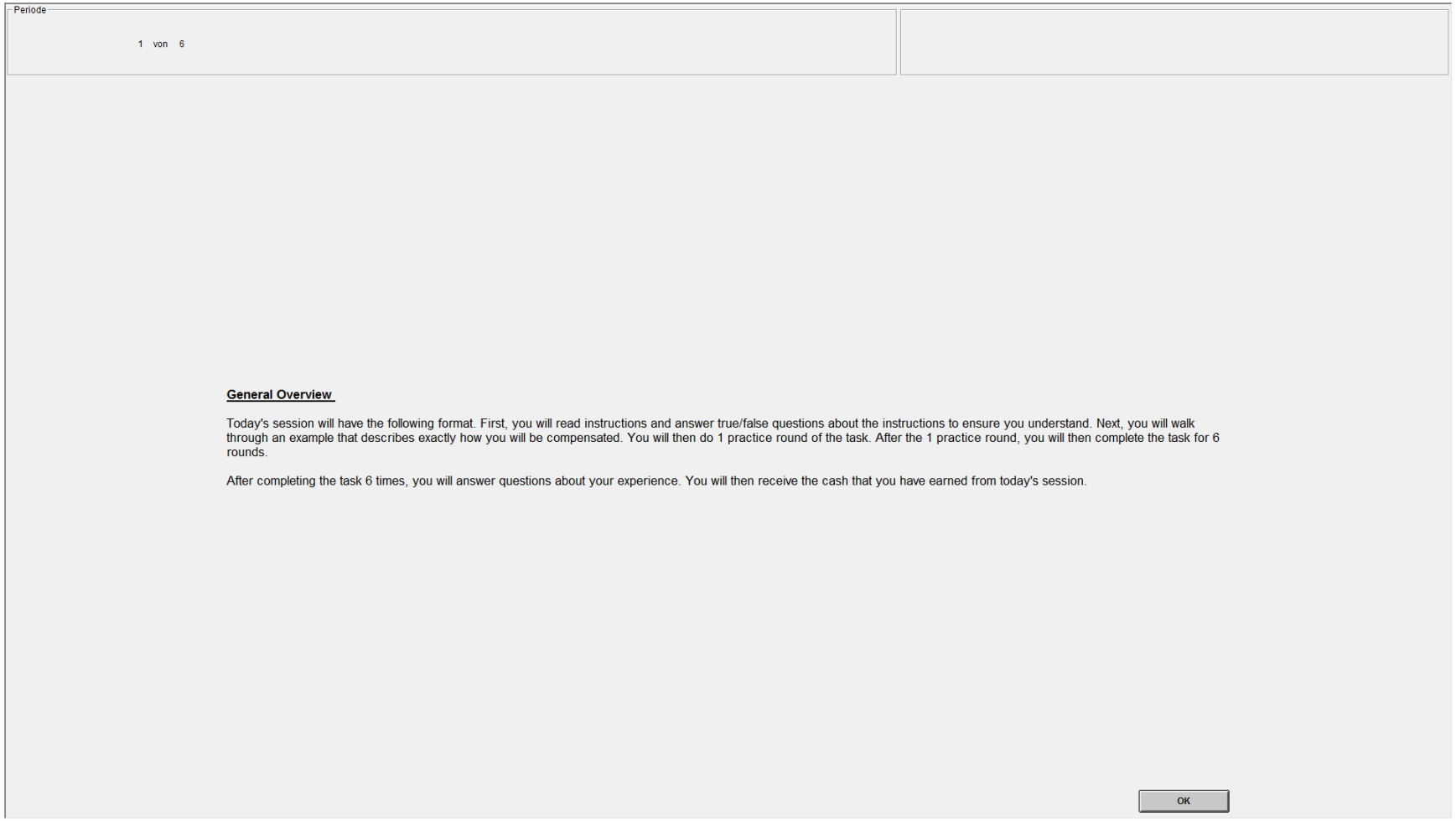
- Financing;
- Cash management;
- Allocation of capital;
- Issuance and monitoring of inventory investment guidelines to our operating divisions;
- Approval and funding of land and lot acquisitions;
- Monitoring and analysis of margins, costs, profitability and inventory levels;
- Risk and litigation management;
- Environmental assessments of land and lot acquisitions;
- Information technology systems;
- Accounting and management reporting;
- Income taxes;
- Internal audit;
- Public reporting and investor and media relations;
- Administration of payroll and employee benefits;
- Negotiation of national purchasing contracts;
- Administration of customer satisfaction surveys and reporting of results; and
- Approval of major personnel decisions and management incentive compensation plans.

Appendix 2: Experimental Materials

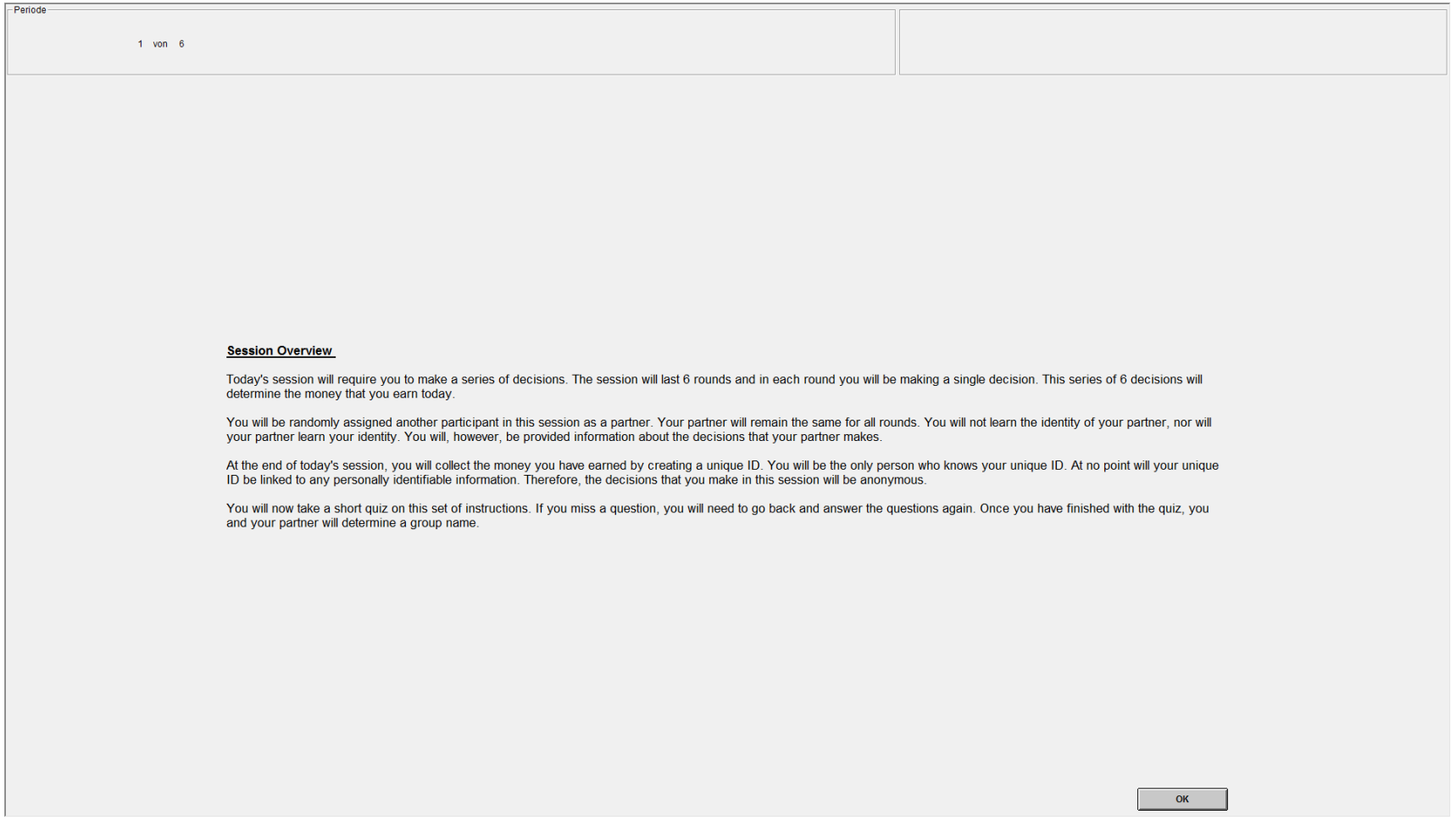
Ground Rules – All Conditions



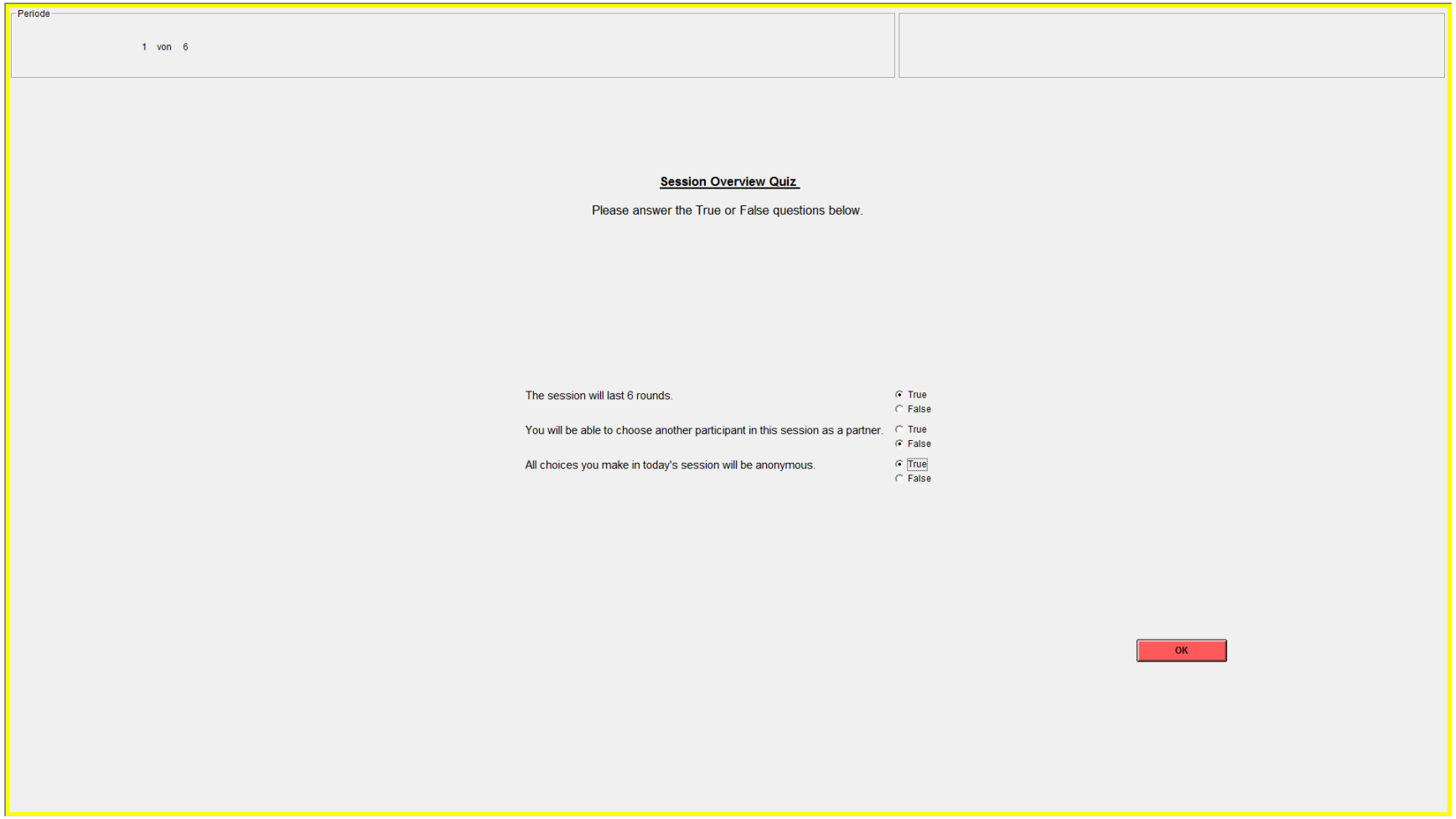
General Overview – All Conditions



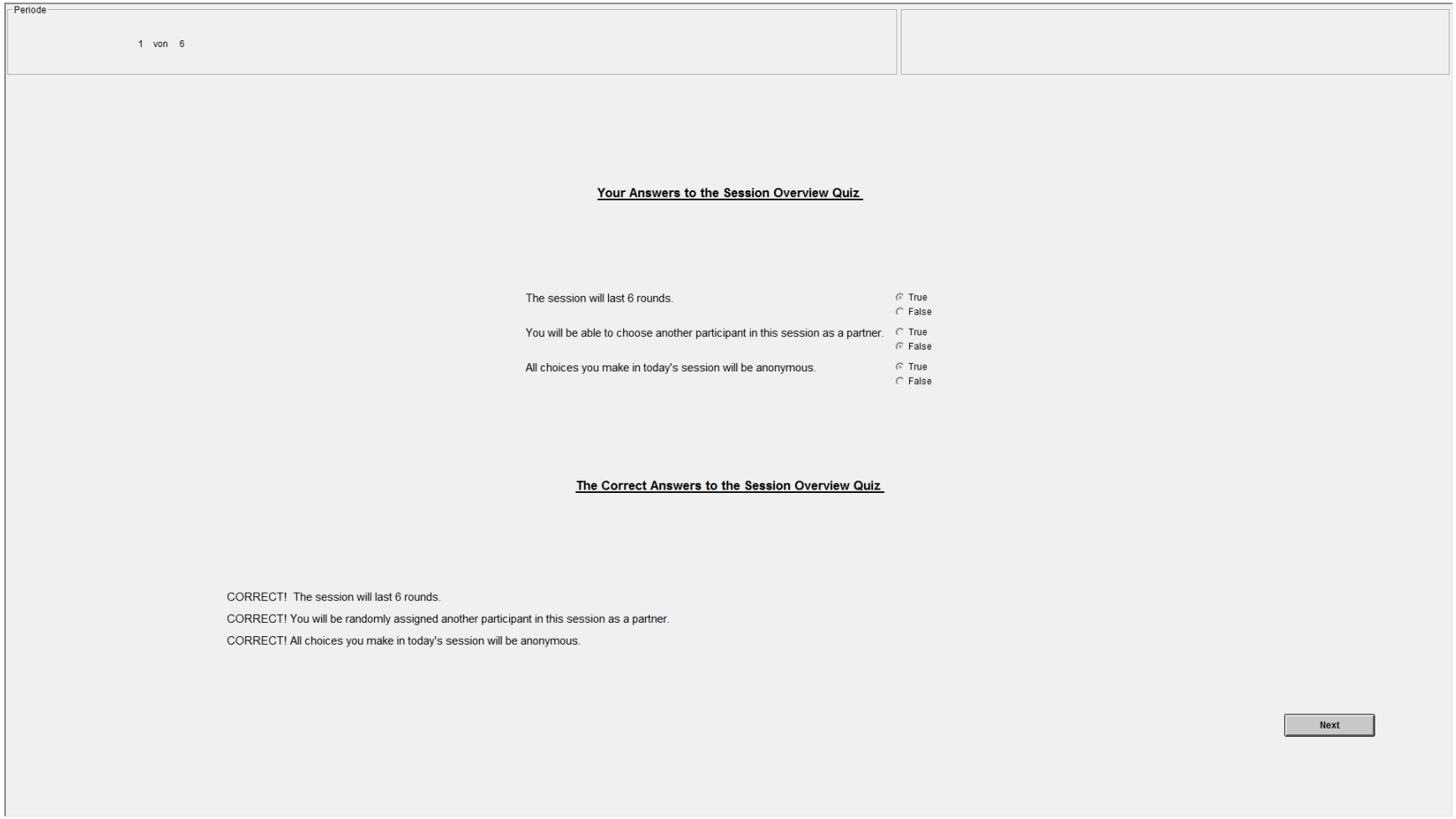
Session Overview – All Conditions



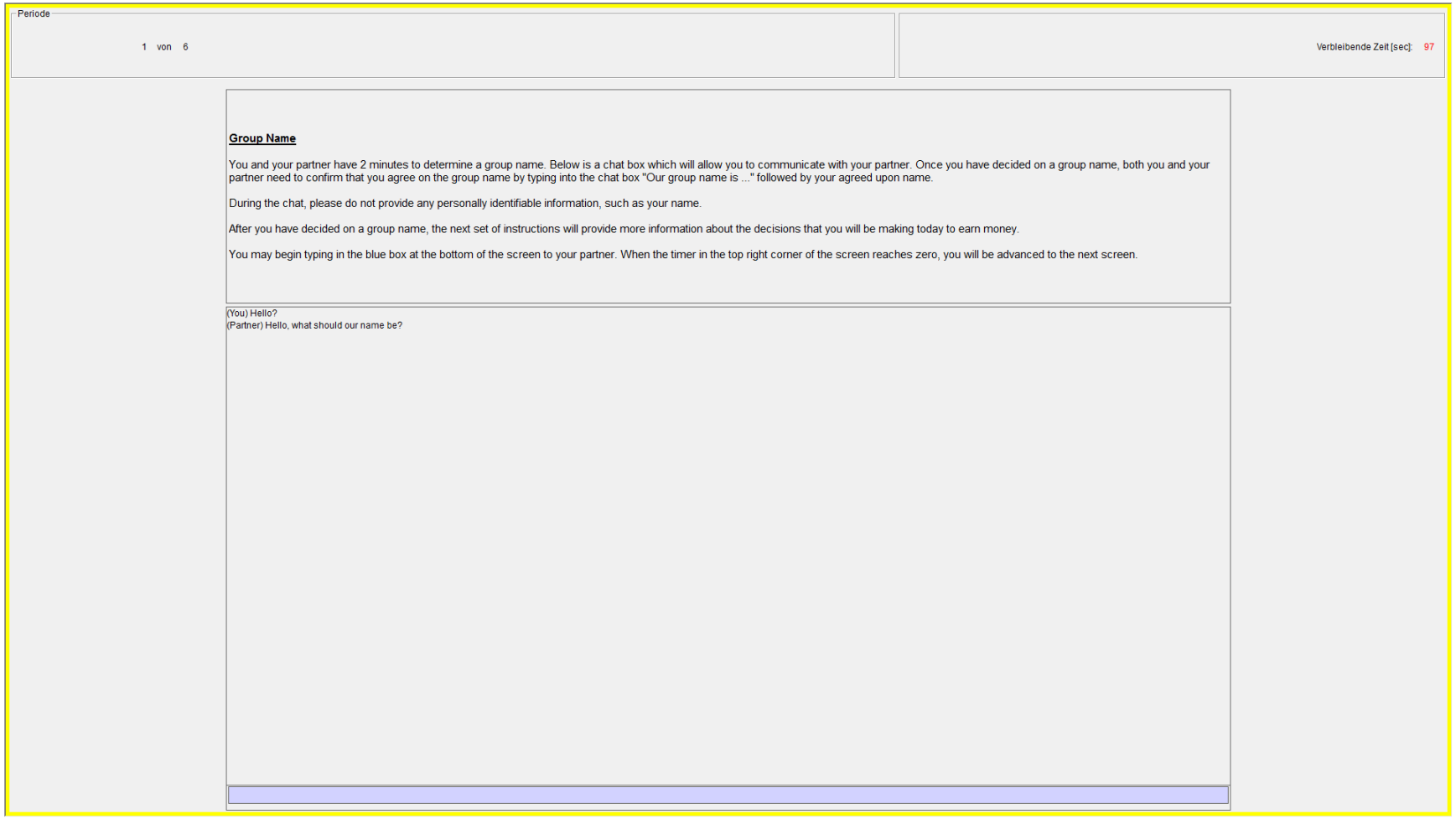
Session Overview Quiz – All Conditions



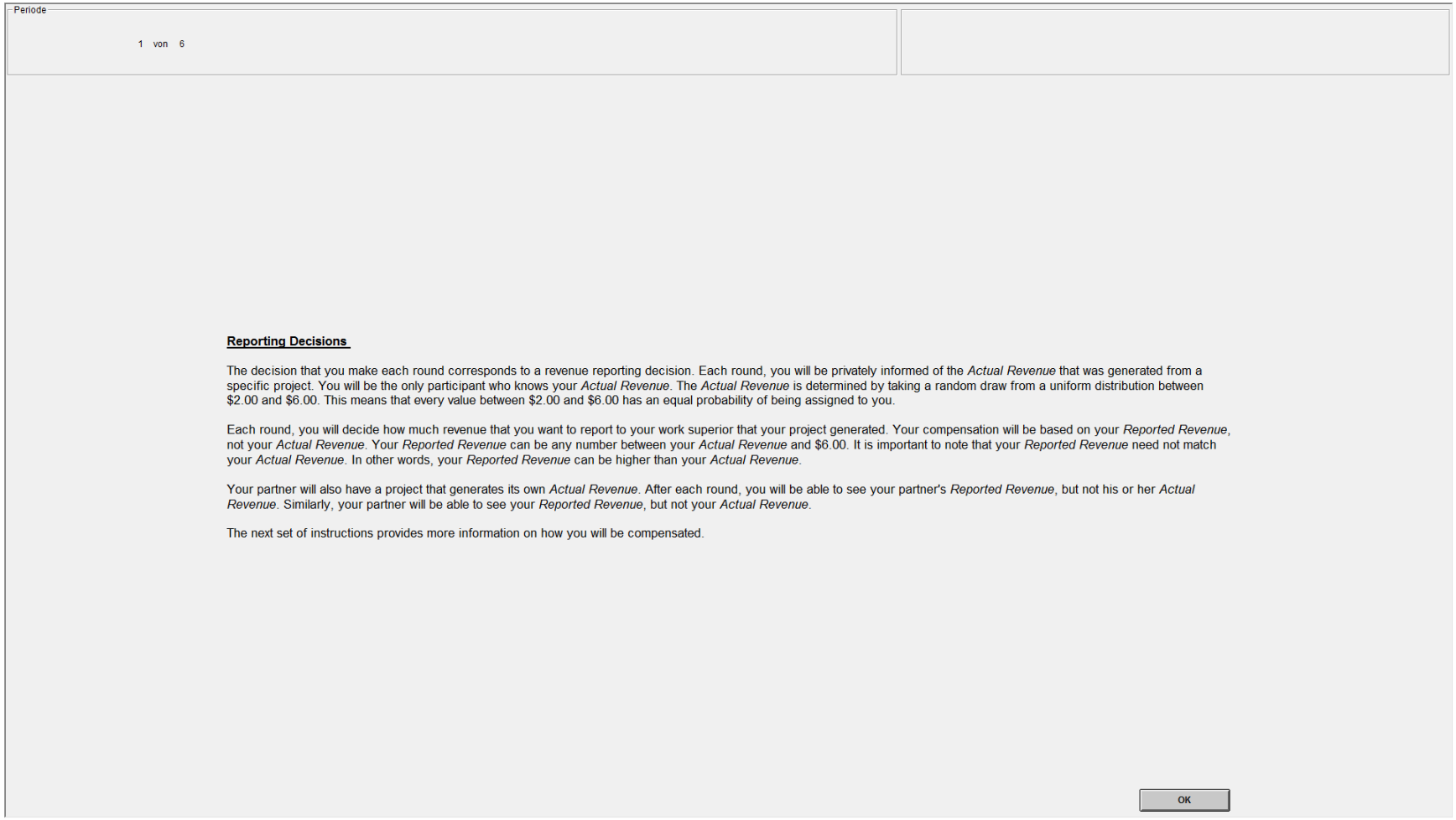
Answers to Session Overview Quiz – All Conditions



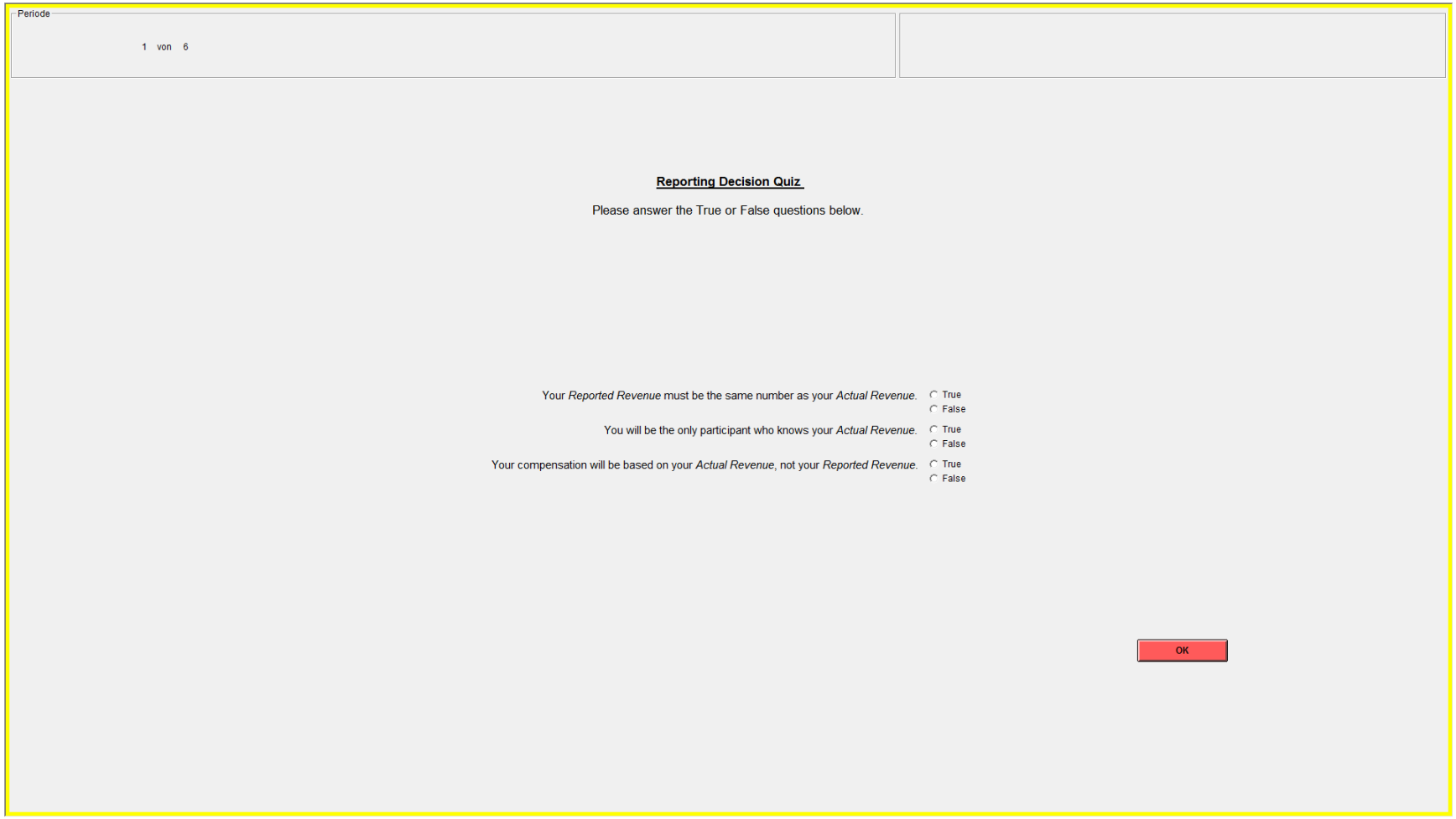
Partner Chat – All Conditions



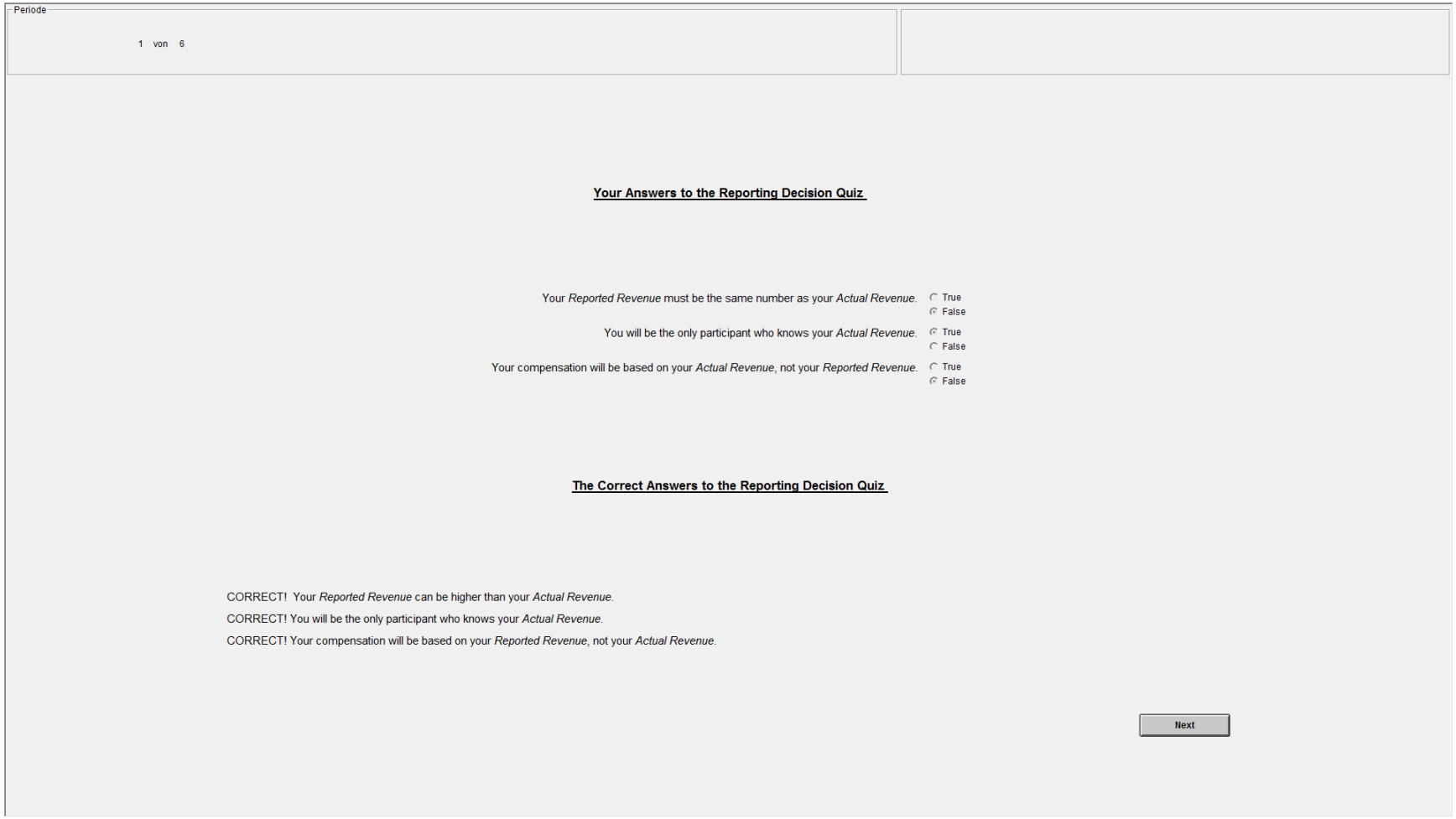
Reporting Decision Description – All Conditions



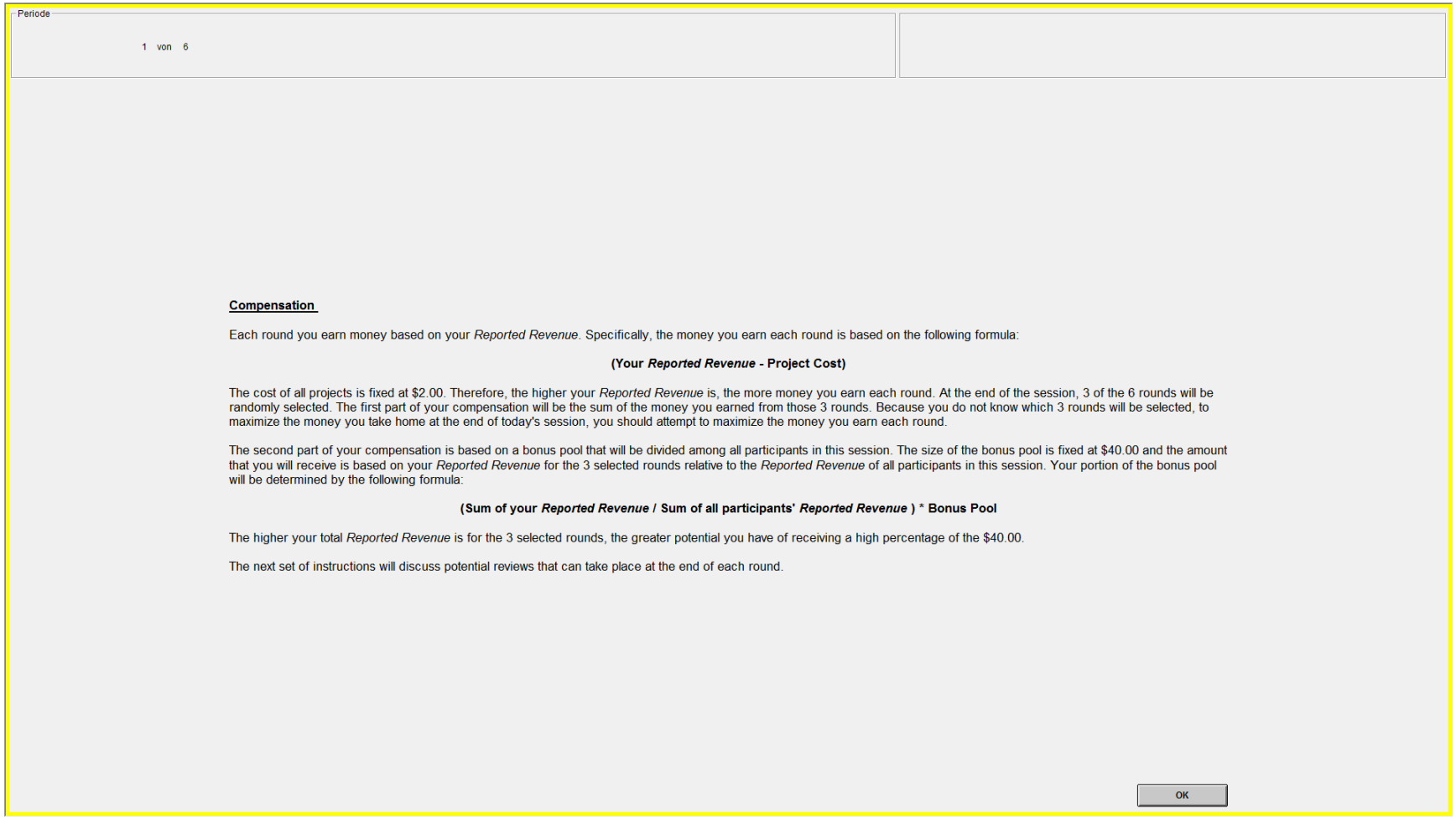
Reporting Decision Quiz – All Conditions



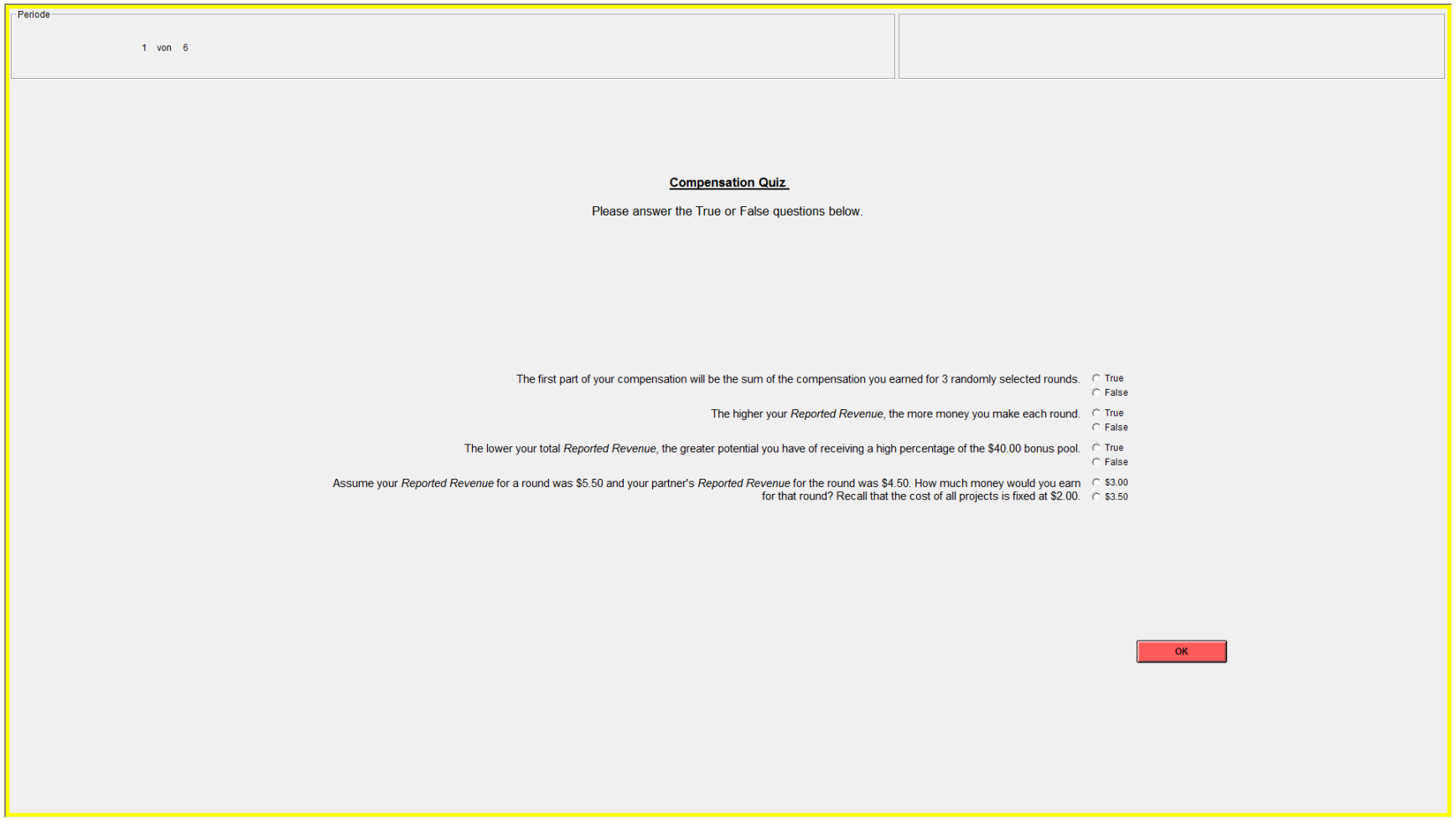
Reporting Decision Quiz Answers – All Conditions



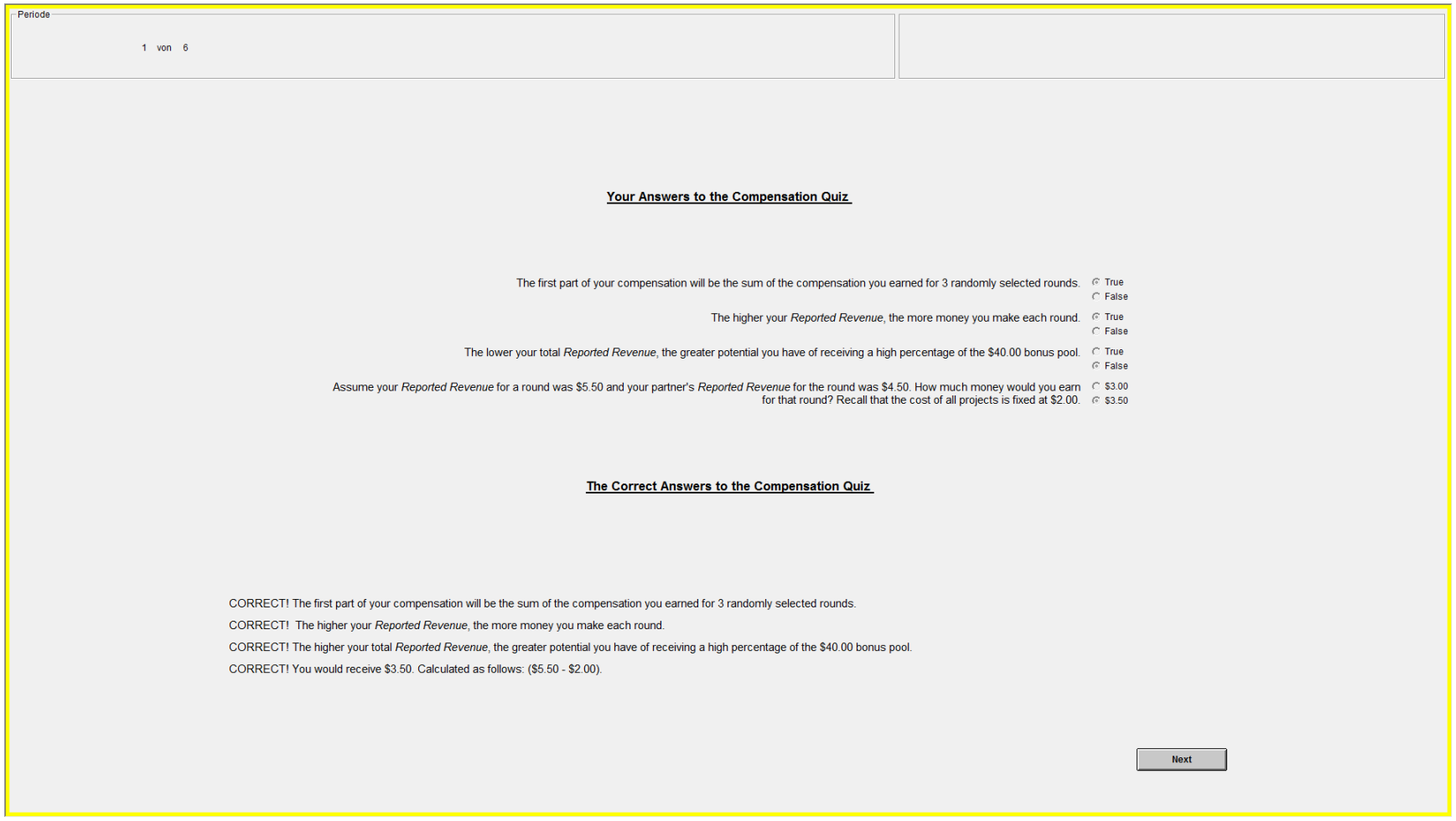
Compensation Description – CI Low Conditions



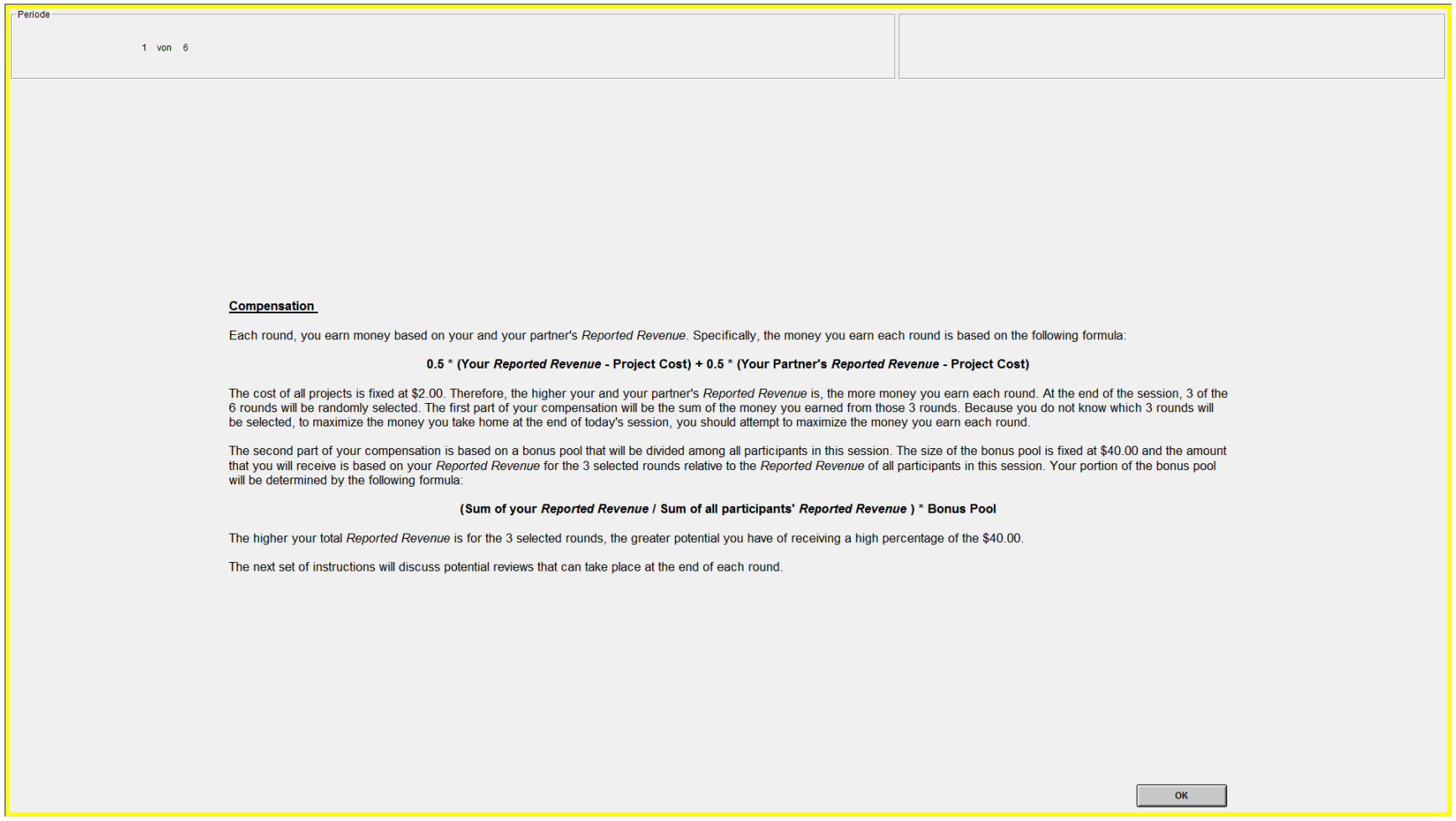
Compensation Description Quiz – CI Low Conditions



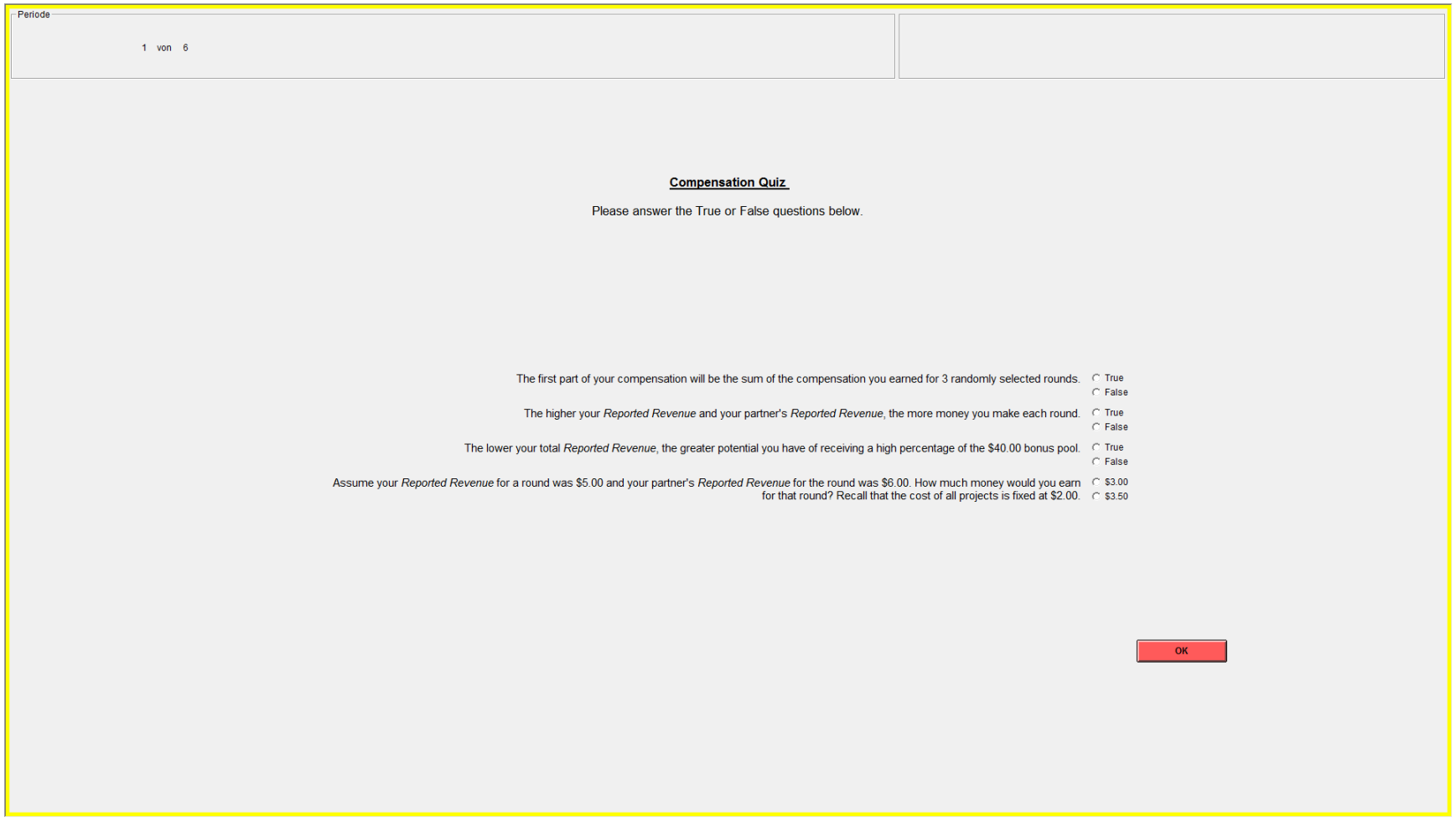
Compensation Description Quiz Answers – CI Low Conditions



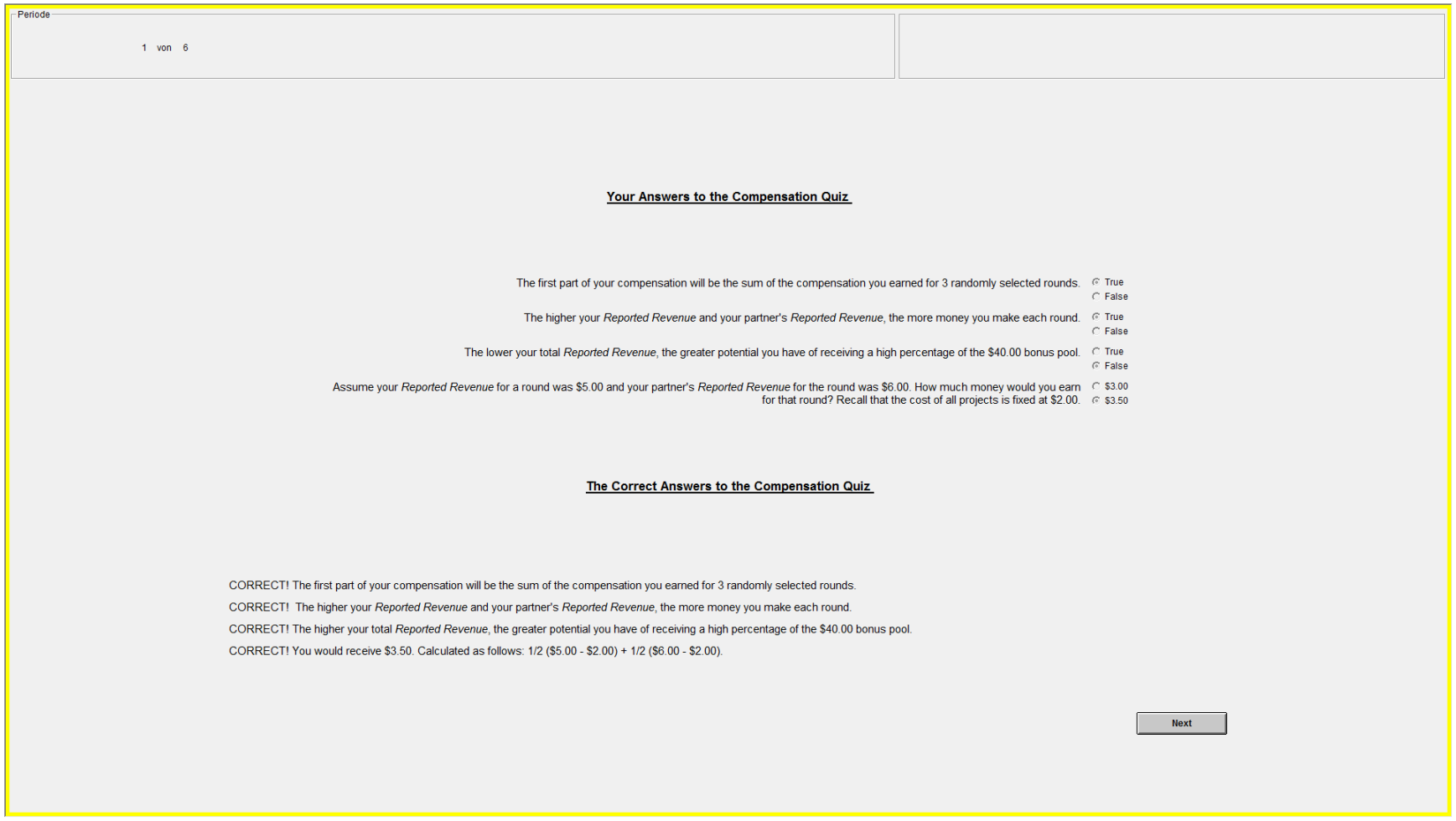
Compensation Description – CI High Conditions



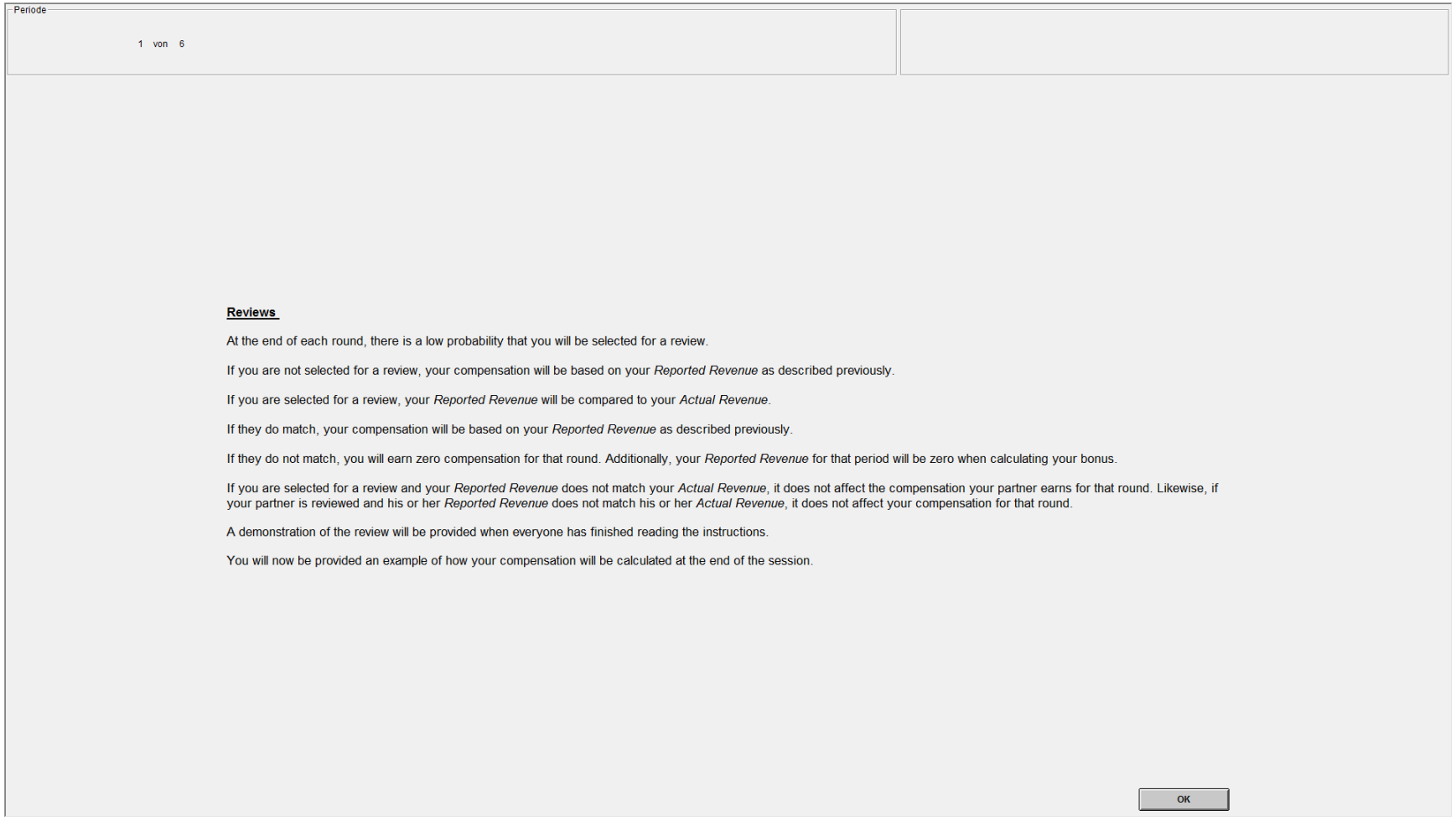
Compensation Description Quiz – CI High Conditions



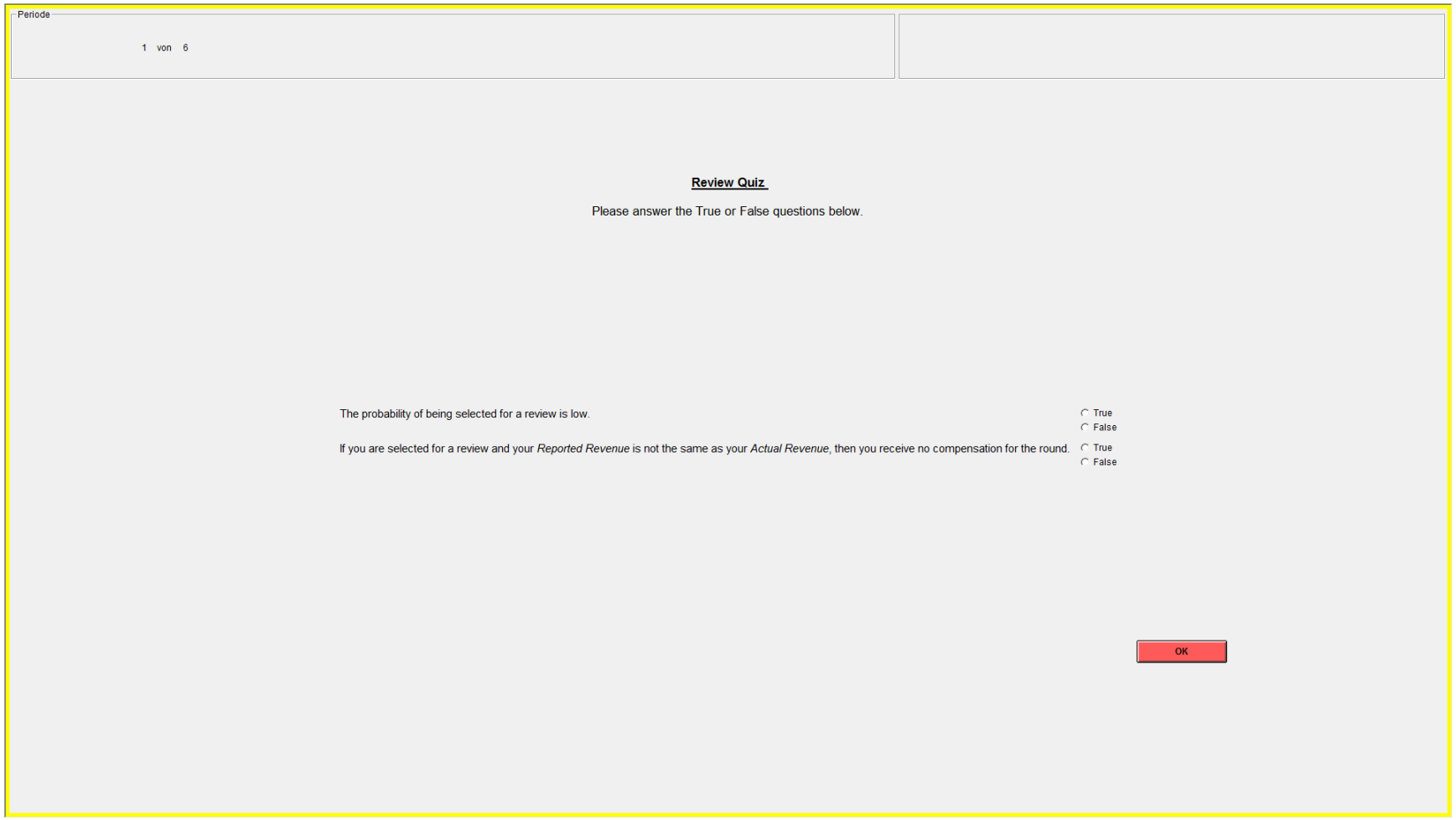
Compensation Description Quiz Answers – CI High Conditions



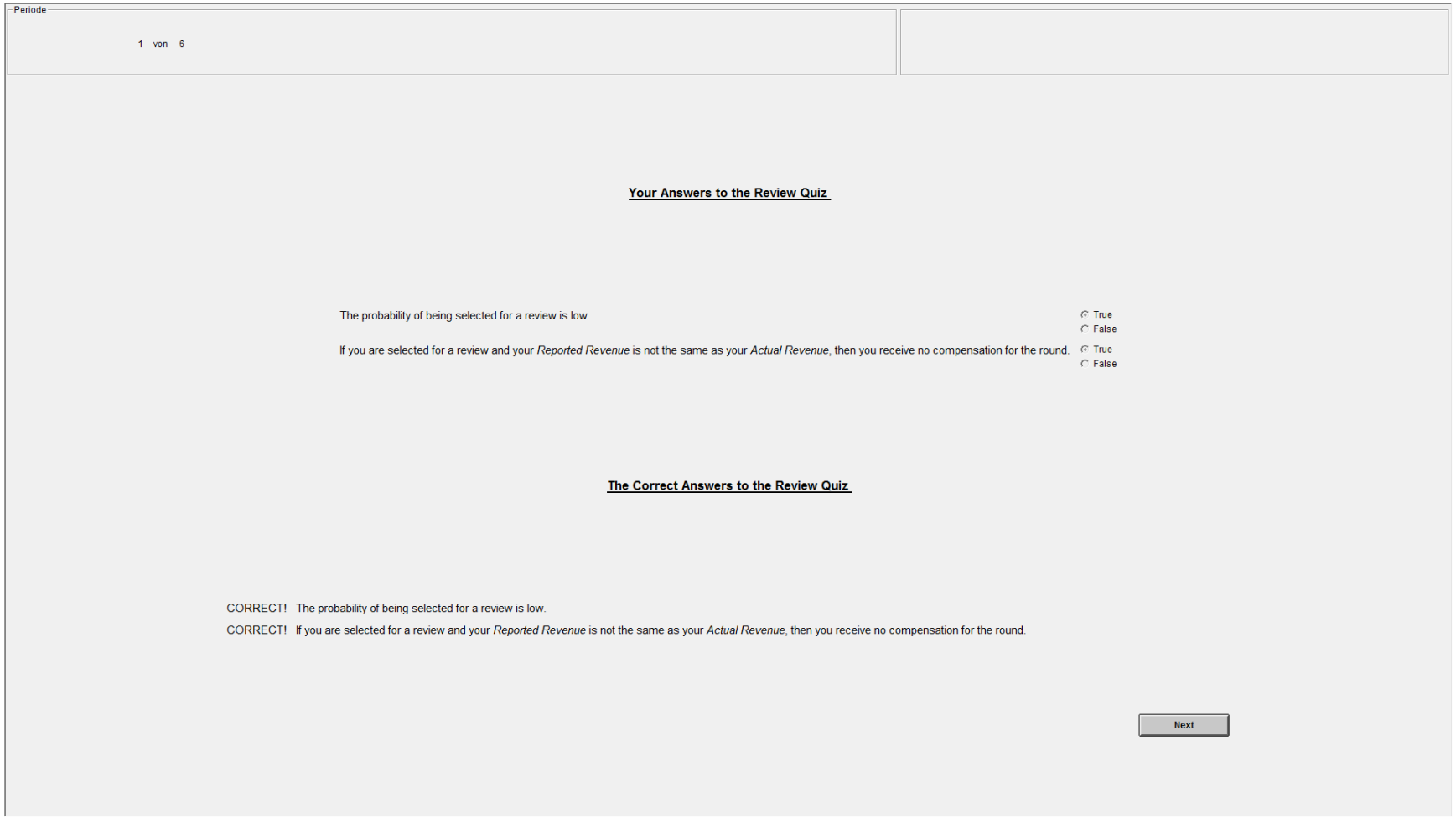
Review Description – All Conditions



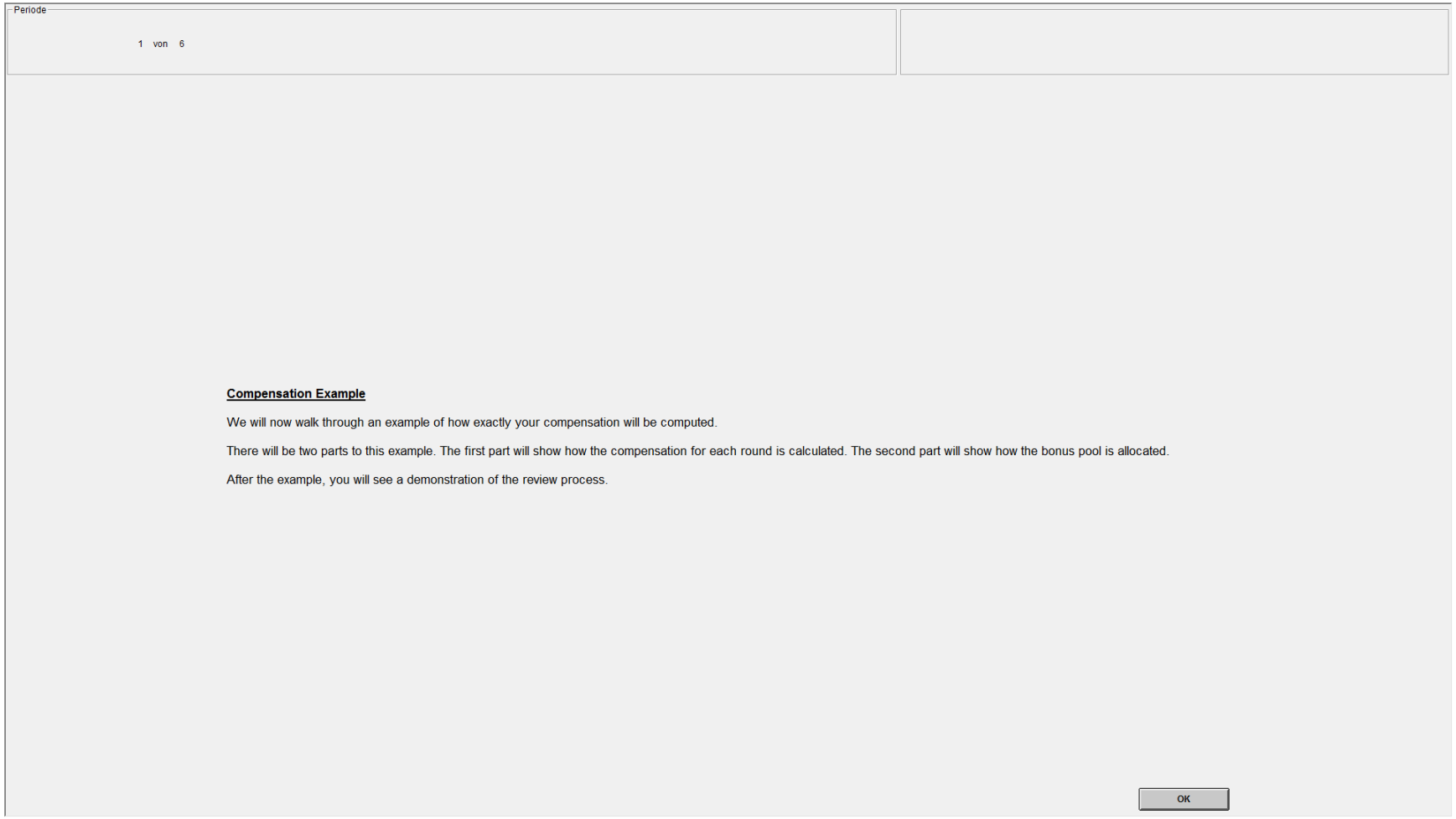
Review Description Quiz – All Conditions



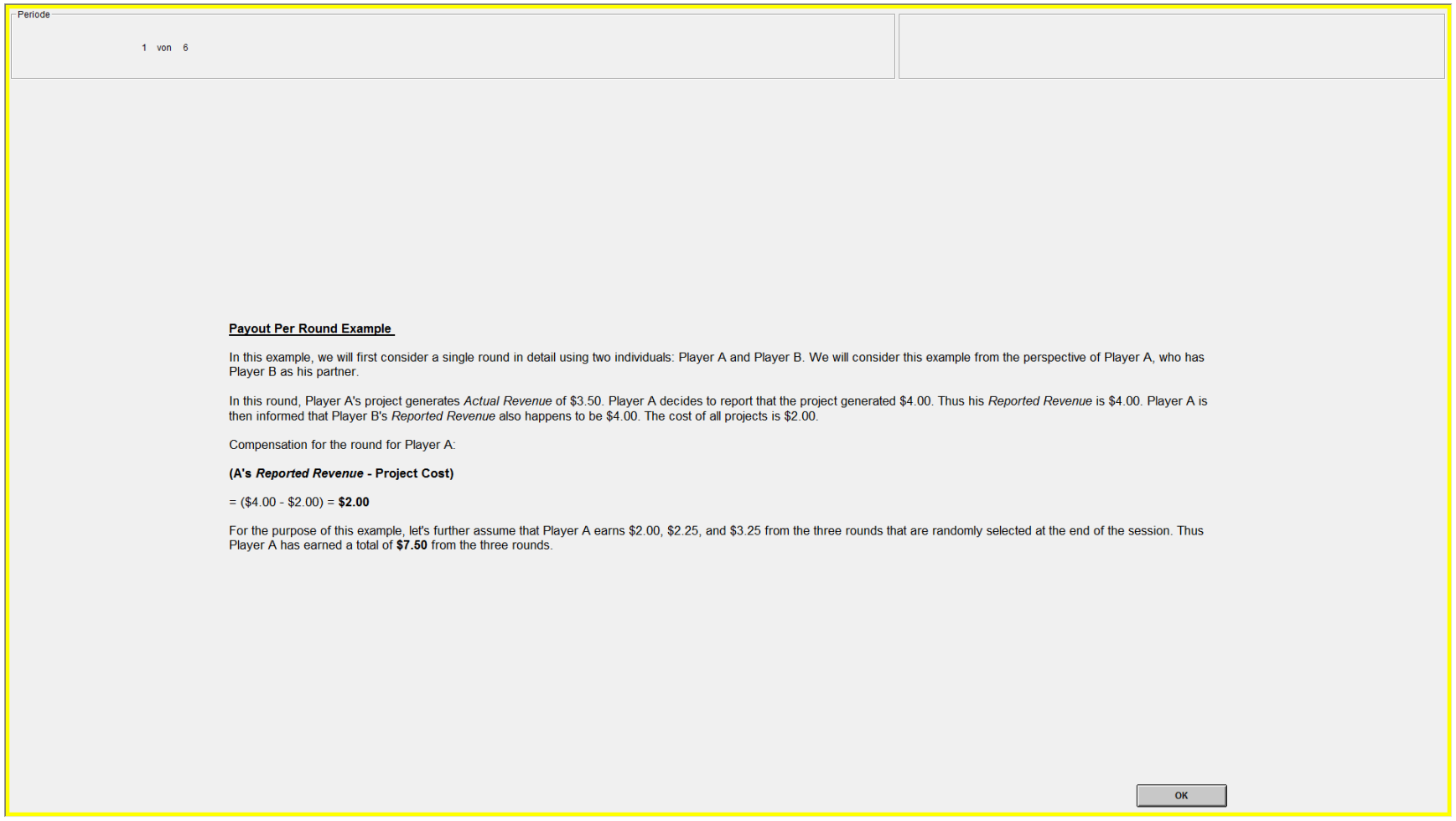
Review Description Quiz Answers – All Conditions



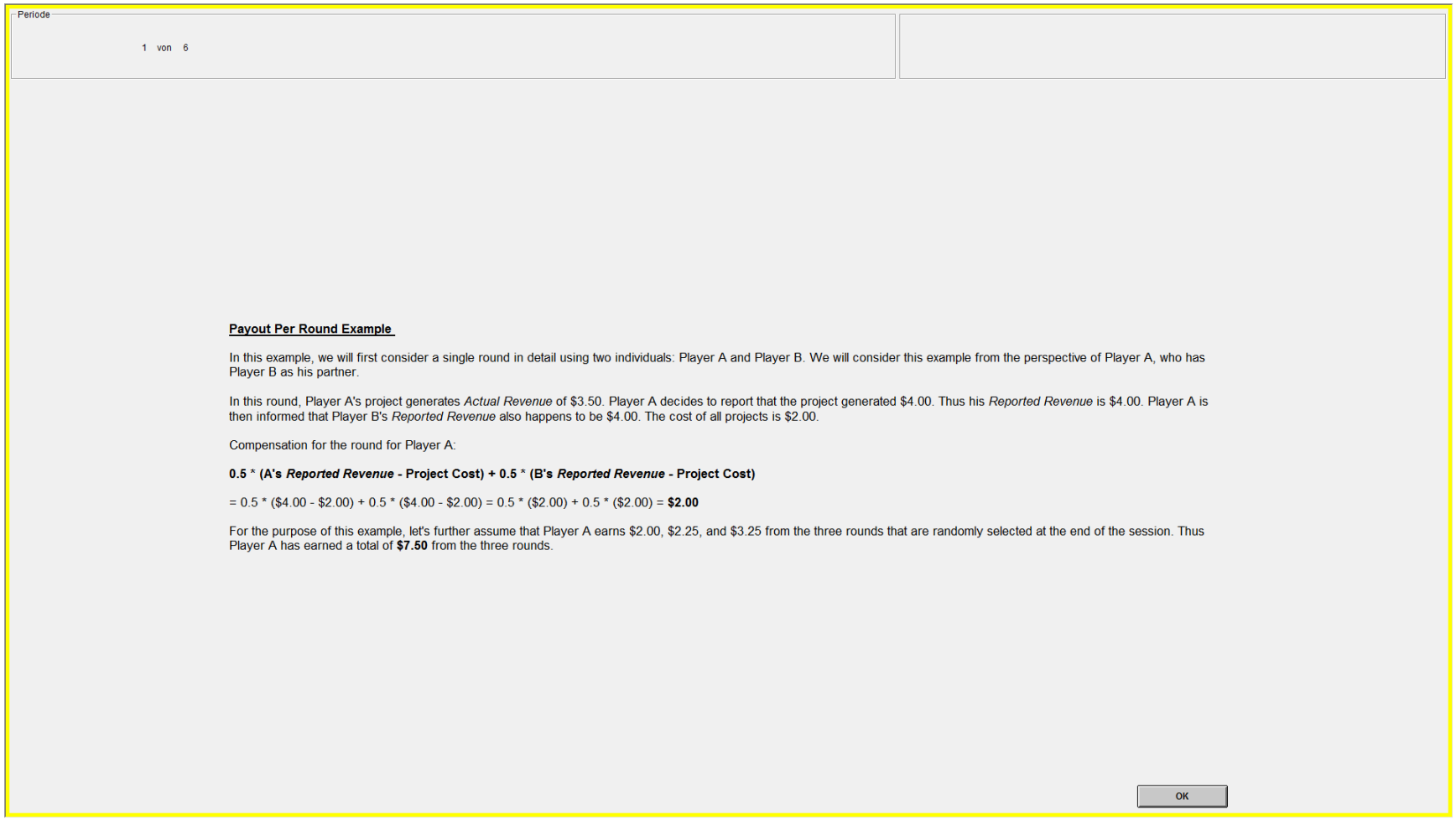
Compensation Example Introduction – All Conditions



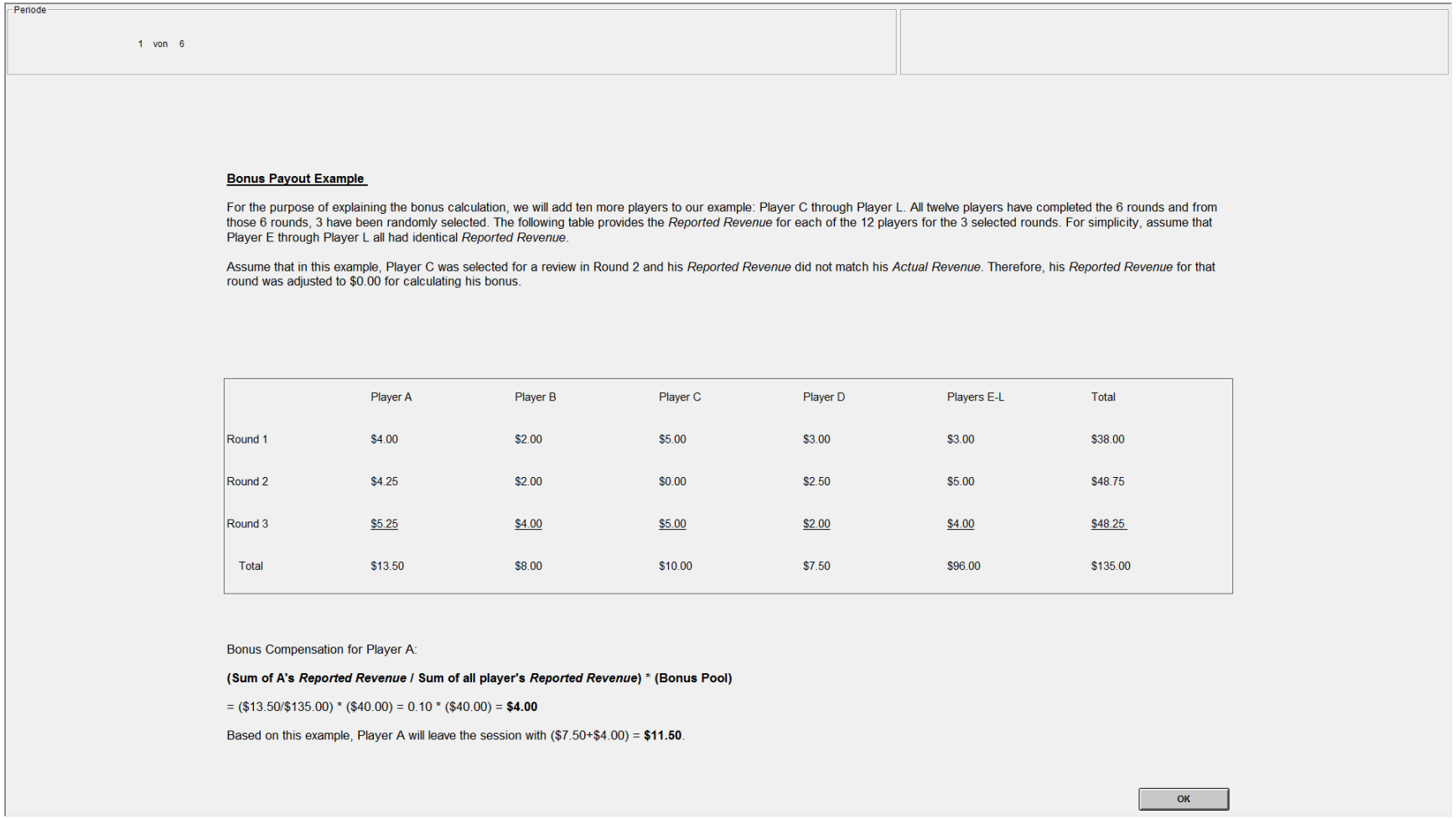
Compensation Example Part 1 – CI Low Conditions



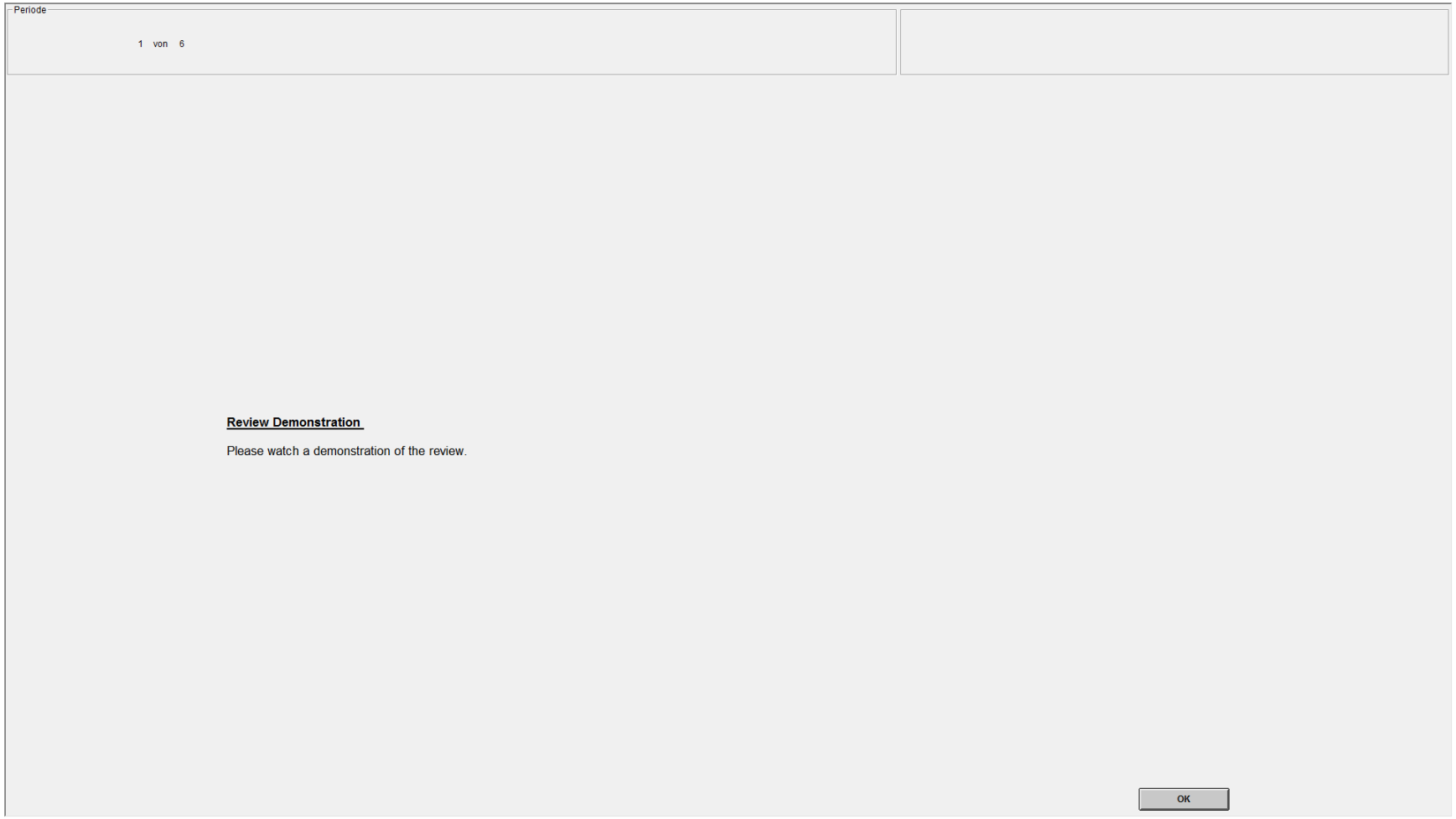
Compensation Example Part 1 – CI High Conditions



Compensation Example Part 2 – All Conditions



Review Demonstration Screen – All Conditions



Review Demonstration Script – Far Review Condition

I will now walk through a demonstration of the review process. Recall that the review will take place at the end of each round after everyone has made their reporting decision.

At the end of each round, for every participant in the room, I will draw a poker chip from this bowl where there are a large number of white chips and a small number of red chips. [WALK & CHURN]

If your chip is white, then you have **not** been selected for a review for that round. However, if your chip is red, then you **have** been selected for a review. I will then put that chip back in the bowl before I draw a poker chip for the next person. I will place the bowl inside of this bag so I cannot see what poker chip I am drawing.

Every individual will have a poker chip selected for him or her each round. I will stand at the front of the room and acknowledge the individual for whom the poker chip is being drawn. For example, if I were doing the process for this individual [point to someone], I would acknowledge that the chip selection was taking place for [him/her], reach into the bag, and draw a poker chip at random to determine if a review takes place for that individual. I will then hold the chip up and announce to the class the color of the poker chip.

During this process, you will have two buttons on your screen. One indicates you have **not** been selected for a review and one indicates you **have** been selected for a review. Please click the appropriate button based on the color of the poker chip that is drawn for you. Again, white means you are **not** selected for a review and red means you **are** selected for a review. If you are **not** selected for a review, you will advance to the next screen. If you **are** selected for a review, your computer will send my computer a message that informs me of your *Actual Revenue* and your *Reported Revenue*, which I will write down on this pad of paper. You will then be able to advance to the next screen.

Please click okay on your screen to start a practice round.

Review Demonstration Script – Near Review Condition

I will now walk through a demonstration of the review process. Recall that the review will take place at the end of each round after everyone has made their reporting decision.

At the end of each round, for every participant in the room, I will draw a poker chip from this bowl where there are a large number of white chips and a small number of red chips. [WALK & CHURN]

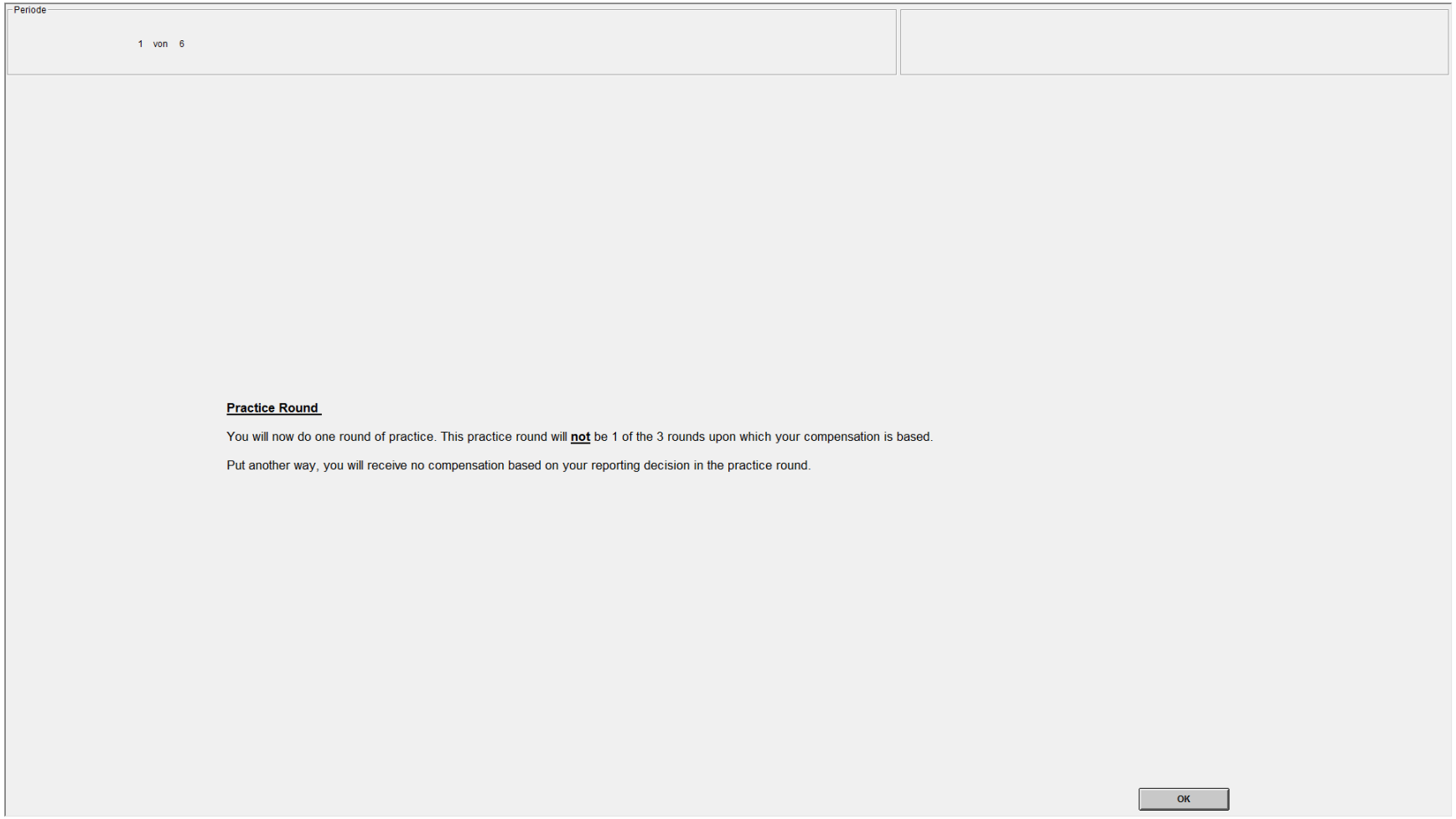
If your chip is white, then you have **not** been selected for a review for that round. However, if your chip is red, then you **have** been selected for a review. I will then put that chip back in the bowl before I draw a poker chip for the next person. I will place the bowl inside of this bag so I cannot see what poker chip I am drawing.

Every individual will have a poker chip selected for him or her each round. I will walk around to each individual's desk to acknowledge the individual for whom the poker chip is being drawn. For example, if I were doing the process for this individual [point to someone], I would acknowledge that the chip selection was taking place for [him/her] by walking over to [his/her] desk, reach into the bag, and draw a poker chip at random to determine if a review takes place for that individual. I will then hold the chip up and announce to the class the color of the poker chip.

During this process, you will have two buttons on your screen. One indicates you have **not** been selected for a review and one indicates you **have** been selected for a review. Please click the appropriate button based on the color of the poker chip that is drawn for you. Again, white means you are **not** selected for a review and red means you **are** selected for a review. If you are **not** selected for a review, you will advance to the next screen. If you **are** selected for a review, your computer will display a message that informs me of your *Actual Revenue* and your *Reported Revenue*, which I will write down on this pad of paper. You will then be able to advance to the next screen.

Please click okay on your screen to start the practice round.

Practice Round Transition – All Conditions



Practice Round Instructions 1 – Low CI Conditions

Practice Round Instructions 2 – Low CI Conditions

Practice Round Instructions 3 – Low CI Conditions

Periode

1 von 6

PAYOUT TABLE

This table provides detail about how your reporting decision affects your compensation independent of a review.

Please note that although the table is in $0.25 increments, you can choose to report any value between $2.28 and $6.00. In other words, your reporting decision does not need to be made in $0.25 increments. You can choose to report any dollar amount between $2.28 and $6.00.

Your Reported Revenue	Your Payout
$6.00	$4.00
$5.75	$3.75
$5.50	$3.50
$5.25	$3.25
$5.00	$3.00
$4.75	$2.75
$4.50	$2.50
$4.25	$2.25
$4.00	$2.00
$3.75	$1.75
$3.50	$1.50
$3.25	$1.25
$3.00	$1.00
$2.75	$0.75
$2.50	$0.50

PROJECT REVENUE REPORT

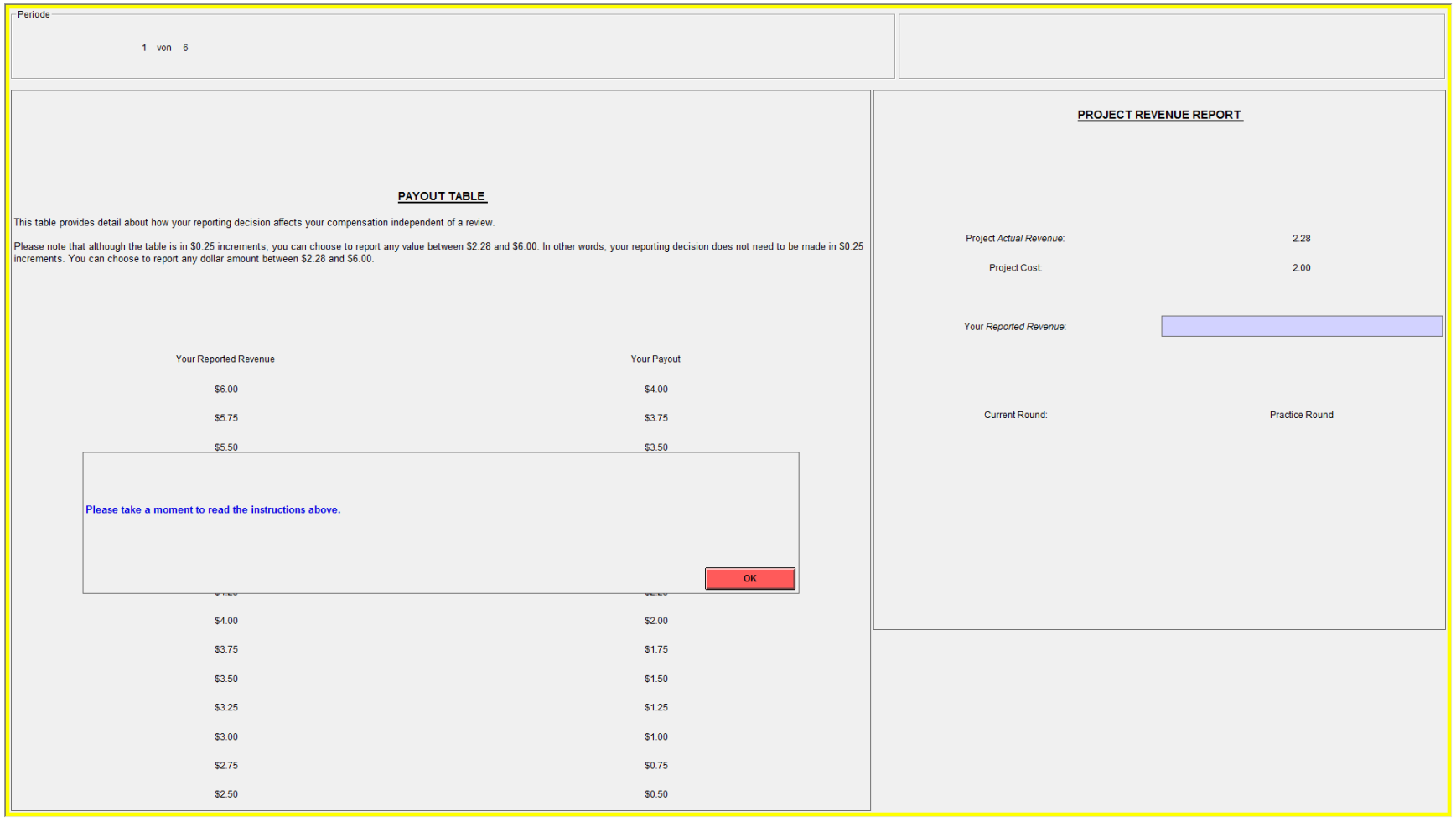
This section contains the actual revenue and cost of the project. Additionally, you make your reporting decision here.

OK

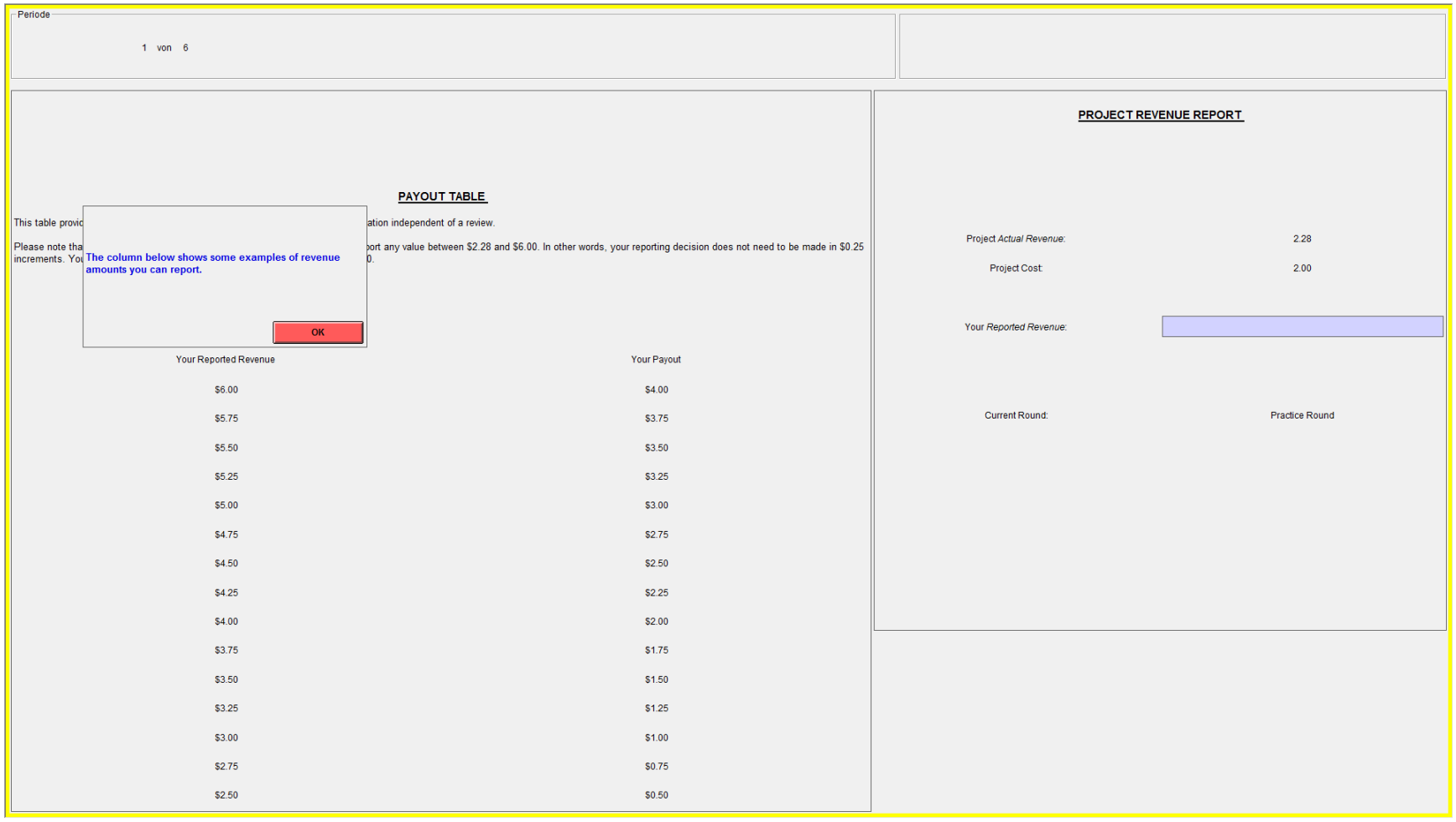
Your *Reported Revenue*:

Current Round: Practice Round

Practice Round Instructions 4 – Low CI Conditions



Practice Round Instructions 5 – Low CI Conditions



Practice Round Instructions 6 – Low CI Conditions

Practice Round Instructions 7 – Low CI Conditions

Practice Round Instructions 8 – Low CI Conditions

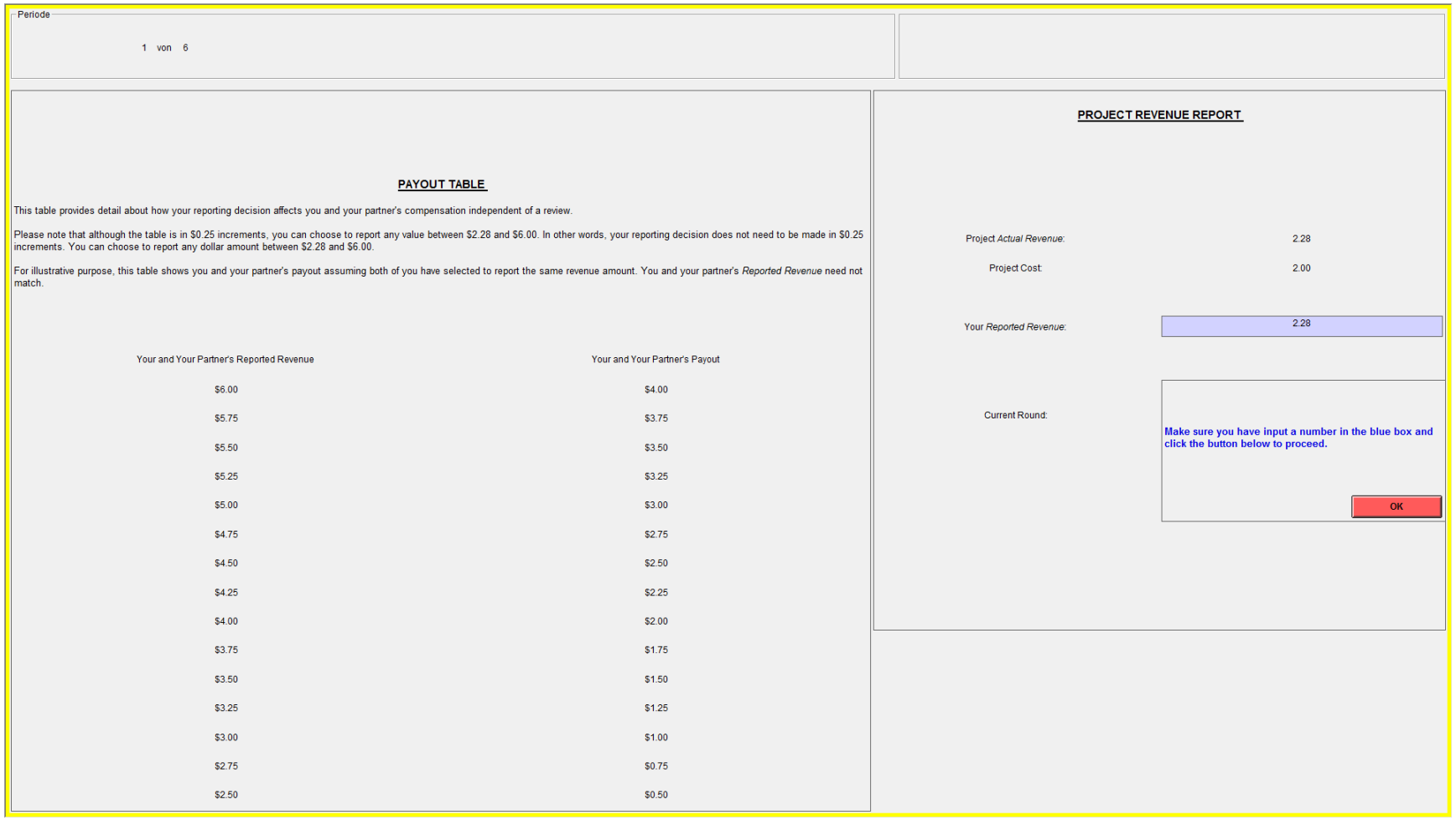
Practice Round Instructions 9 – Low CI Conditions

Practice Round Instructions 10 – Low CI Conditions

Practice Round Instructions 11 – Low CI Conditions

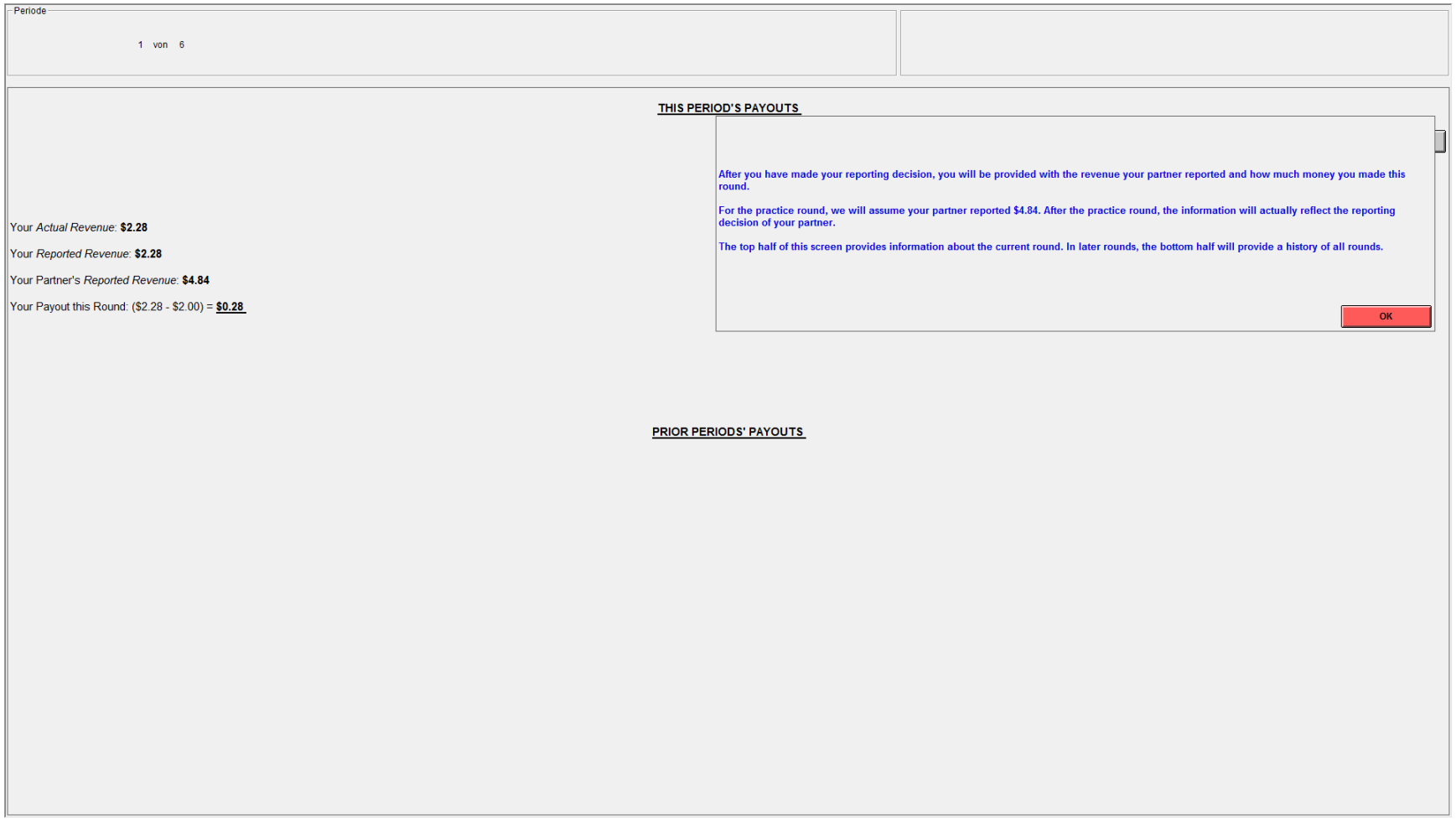
Practice Round Instructions 12 – Low CI Conditions

Practice Round Instructions 12 – High CI Conditions

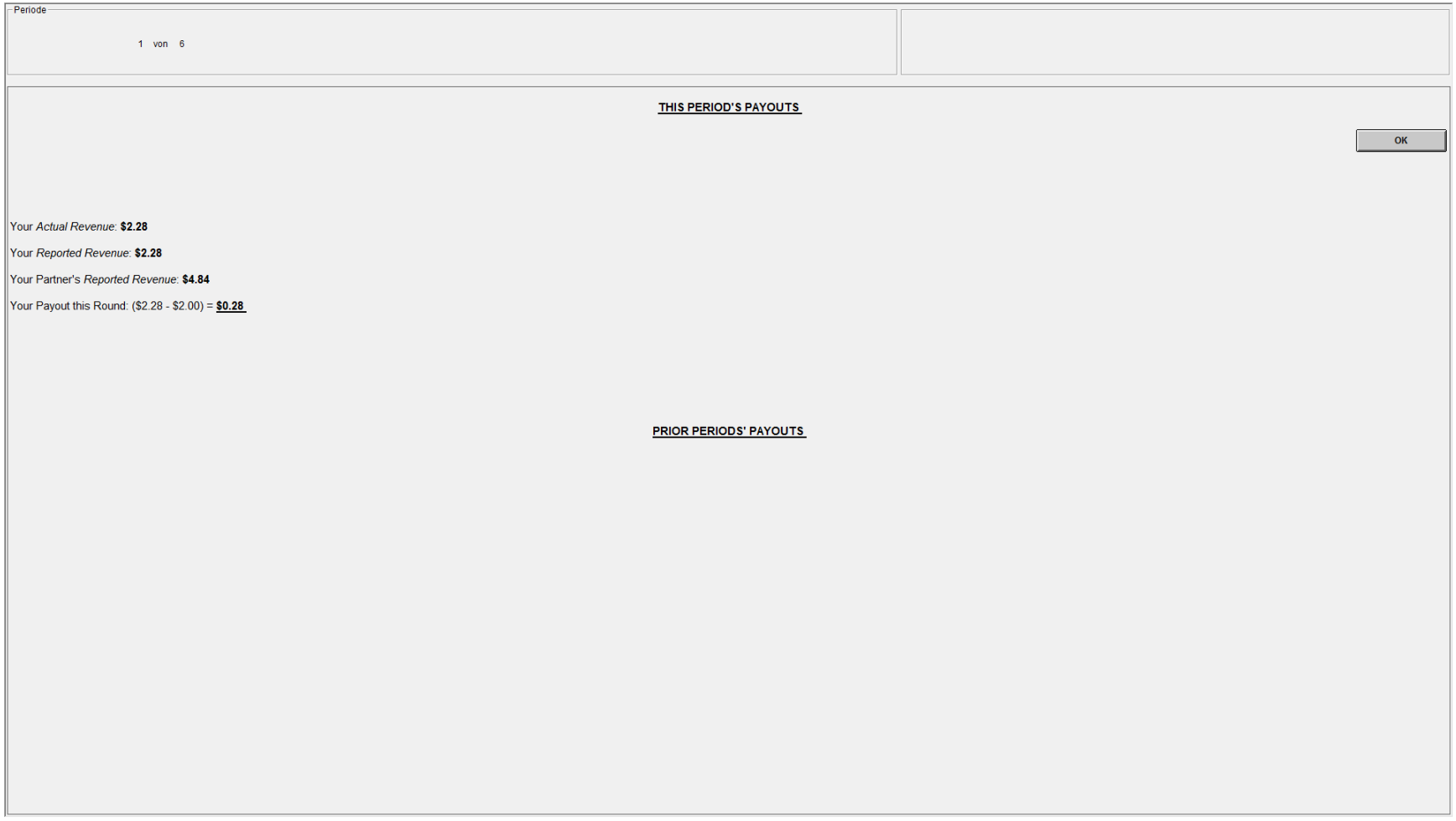


The only difference between High CI and Low CI conditions for all practice screens are the instructions at the top of the screen and the column headings. For brevity, I only show practice round instruction 12 screen for the High CI condition. However, participants in all conditions saw all 12 screens.

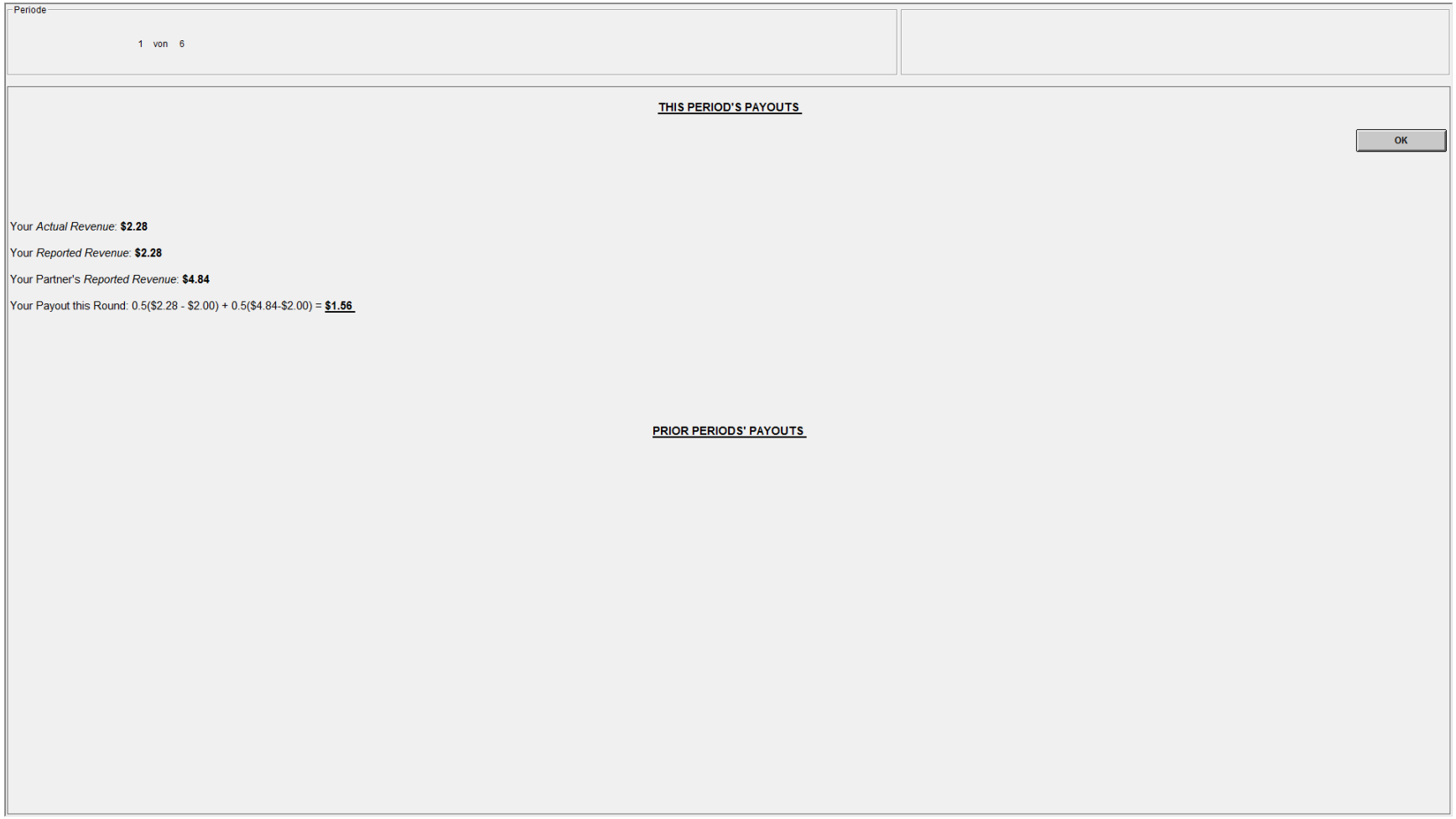
Reporting Information 1 – Low CI Condition



Reporting Information 2 – Low CI Condition

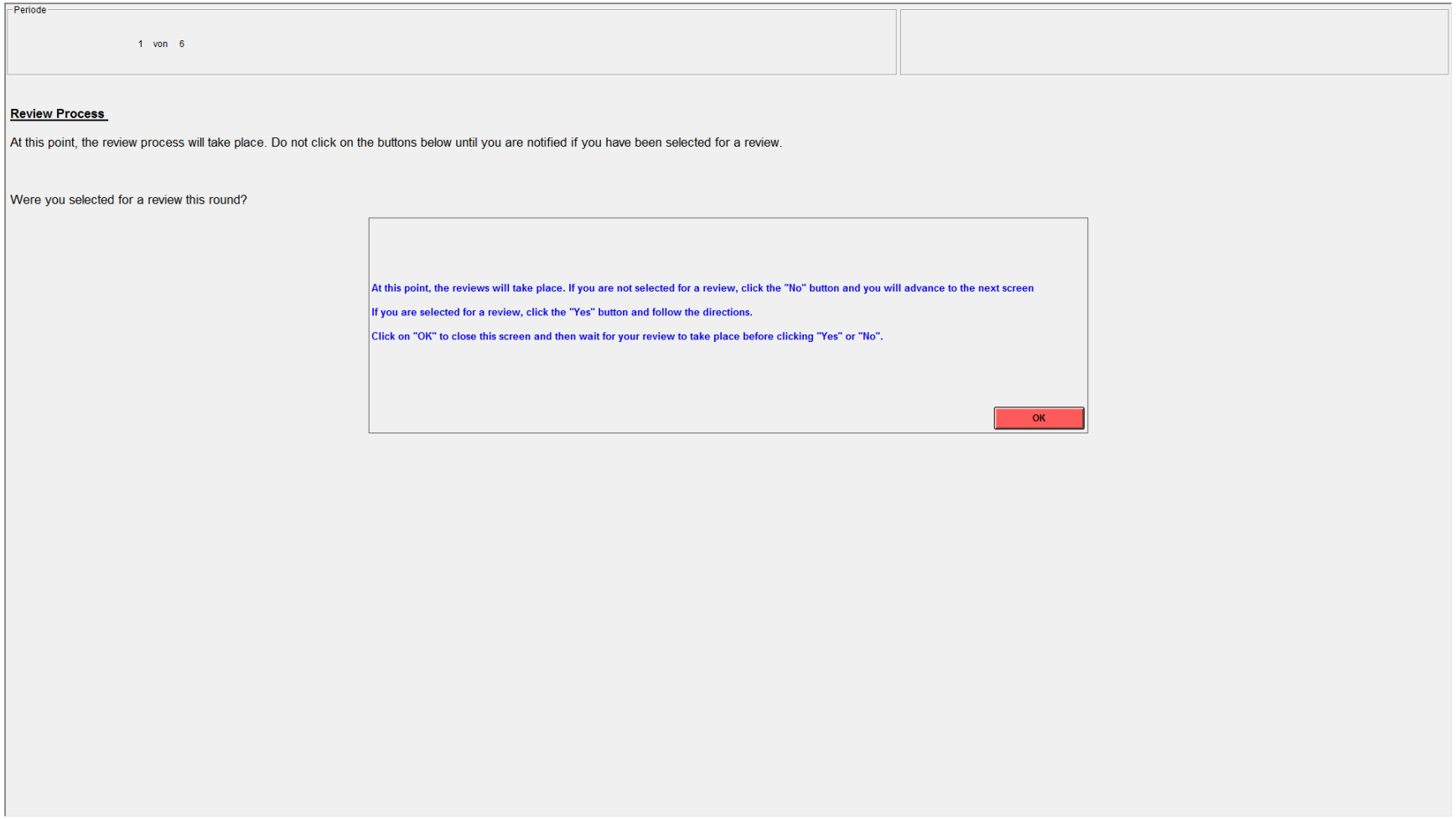


Reporting Information 2 – High CI Condition

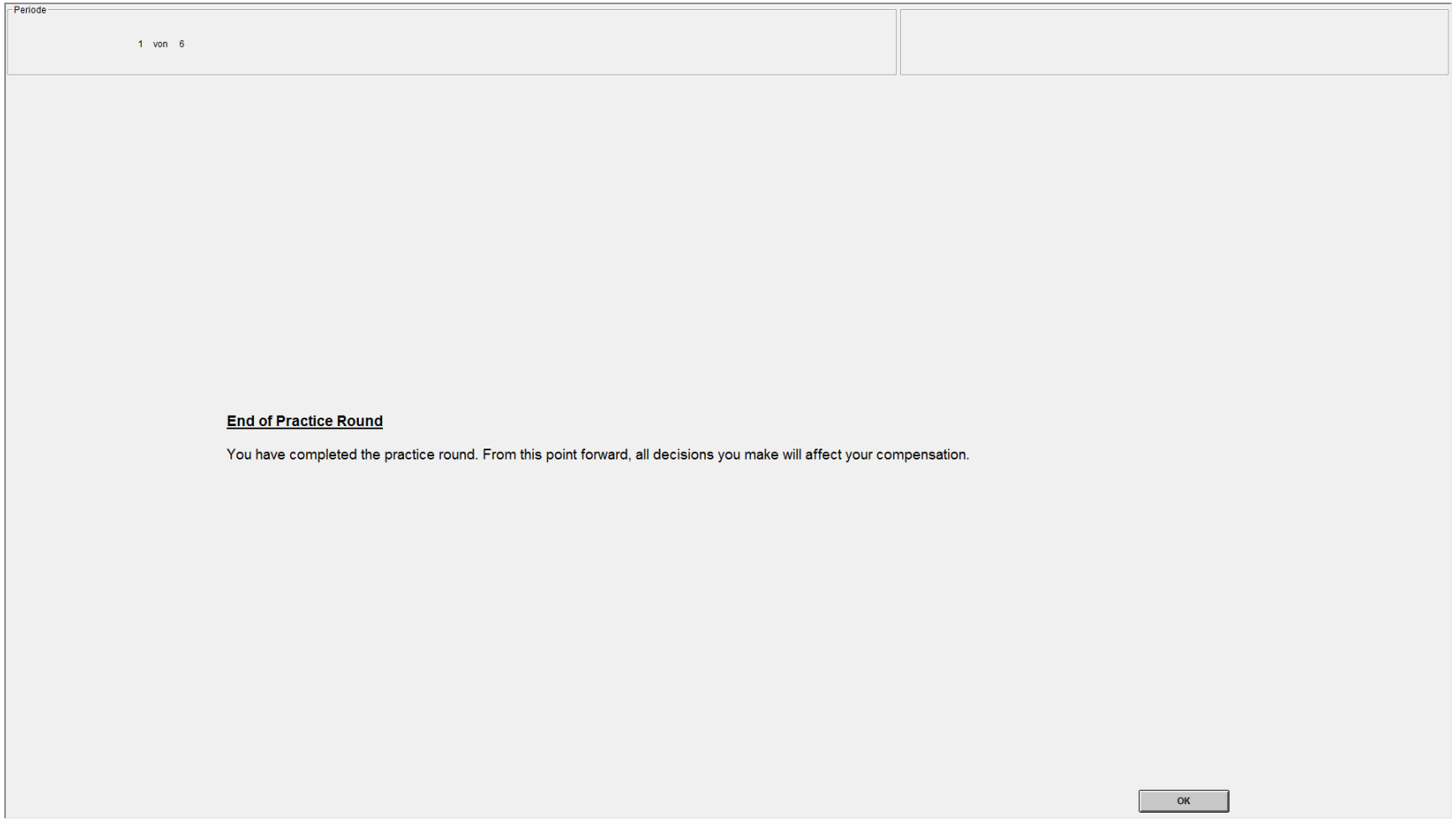


Participants in the High CI condition also saw the Reporting Information 1 Screen. For brevity, it is omitted.

Review Process 1 – All Conditions

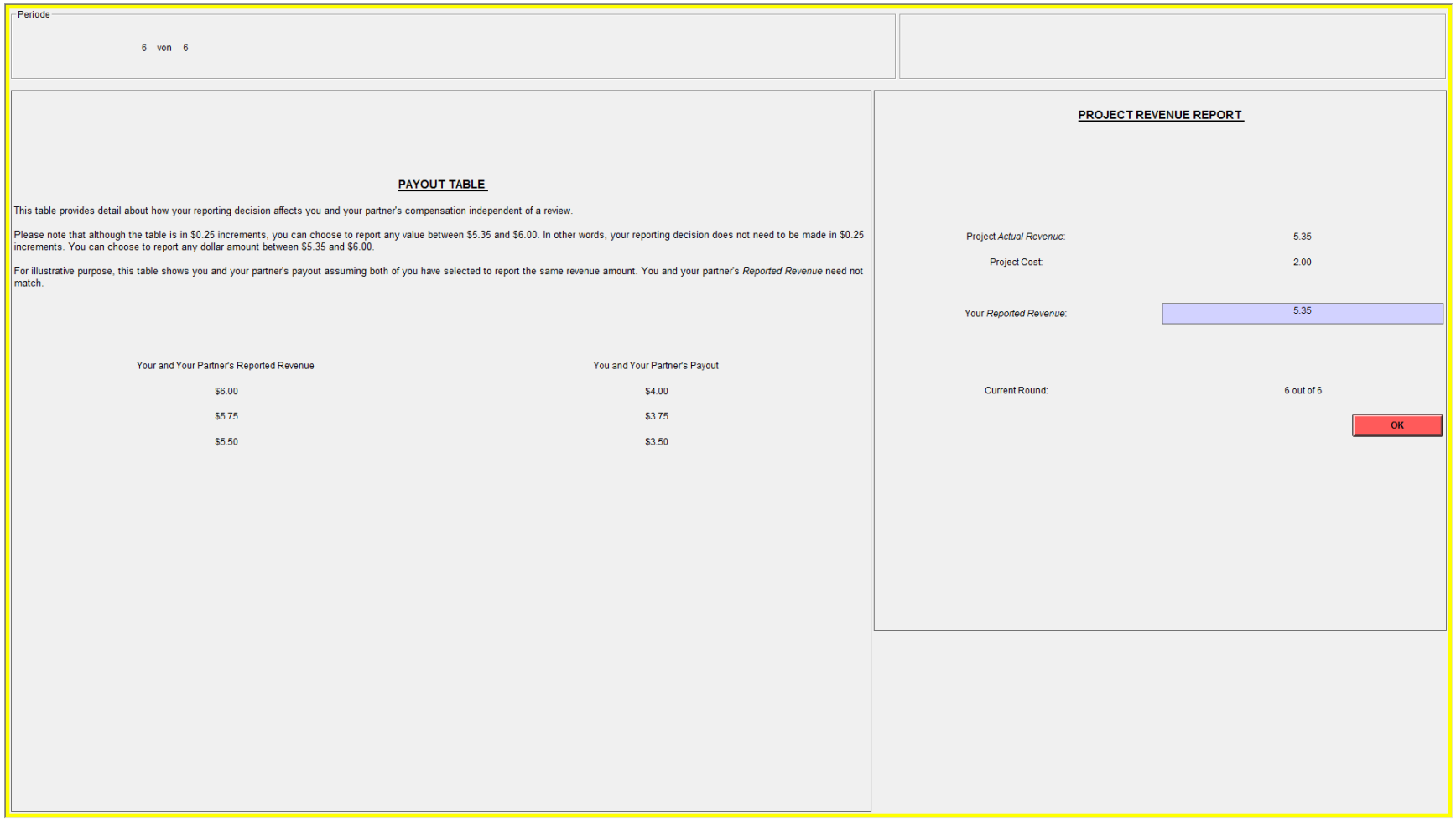


Practice Round Transition – All Conditions

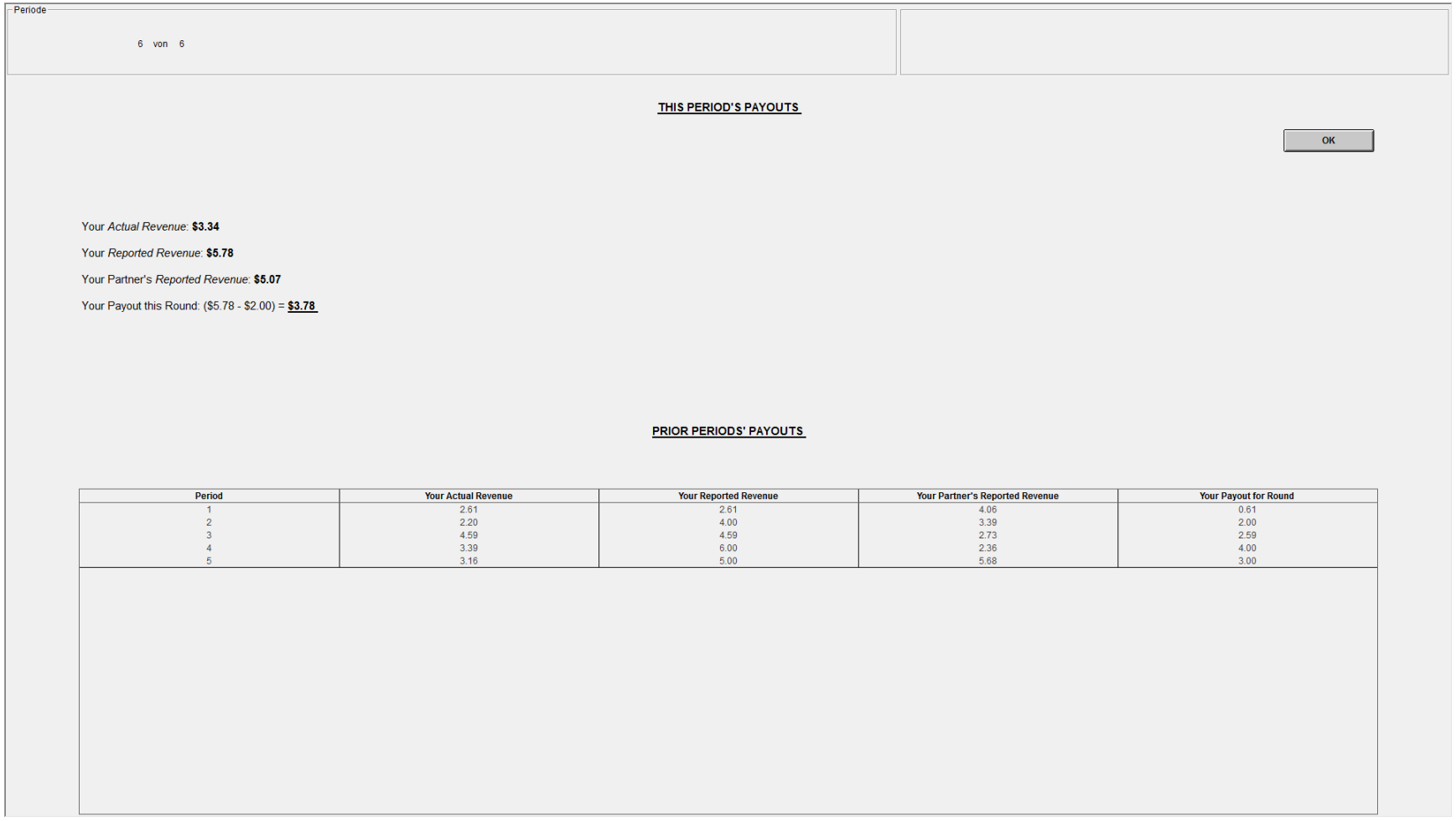


Reporting Screen - Low CI Condition

Reporting Screen - High CI Condition

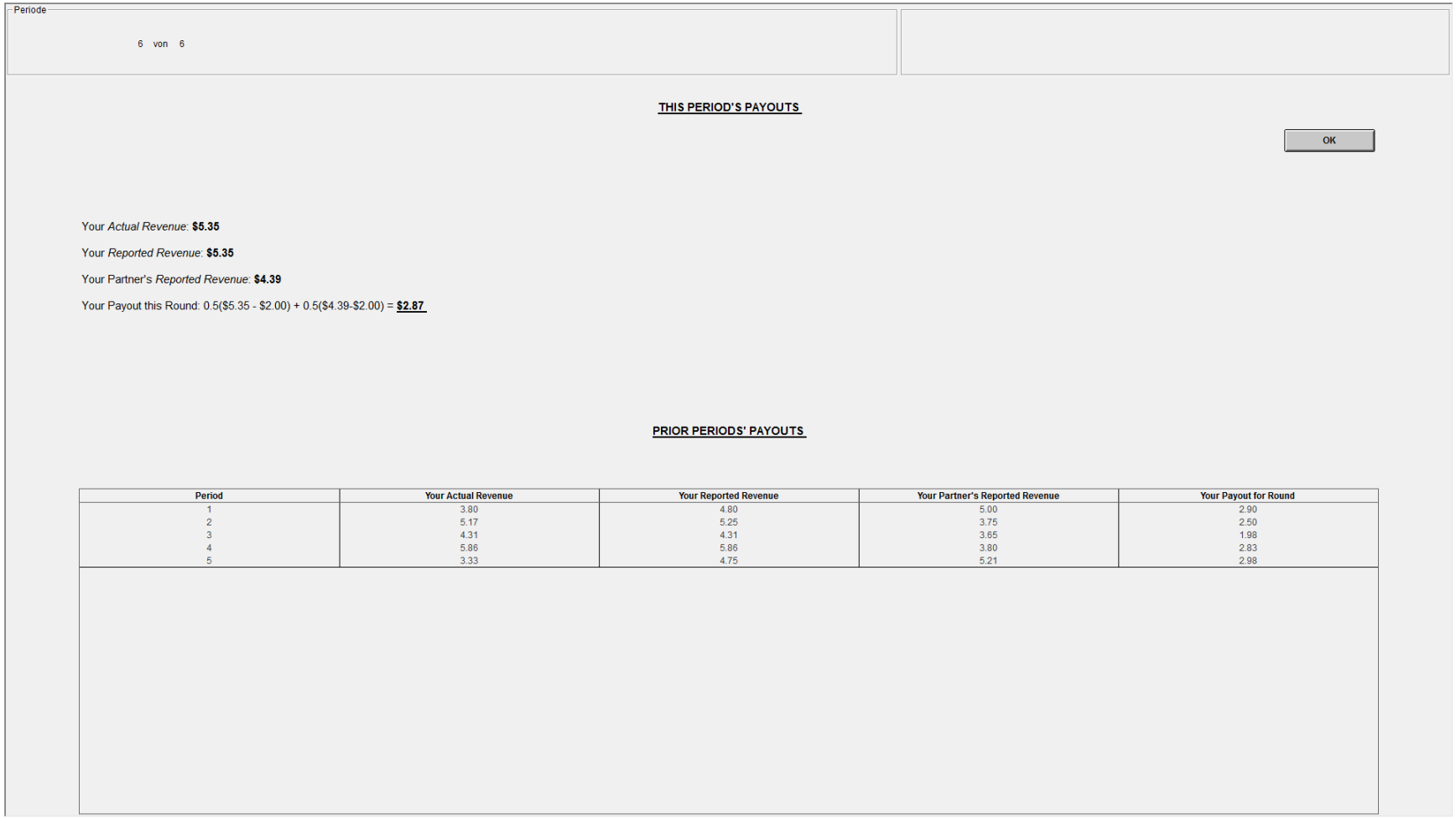


Reporting Information Screen – Low CI Conditions

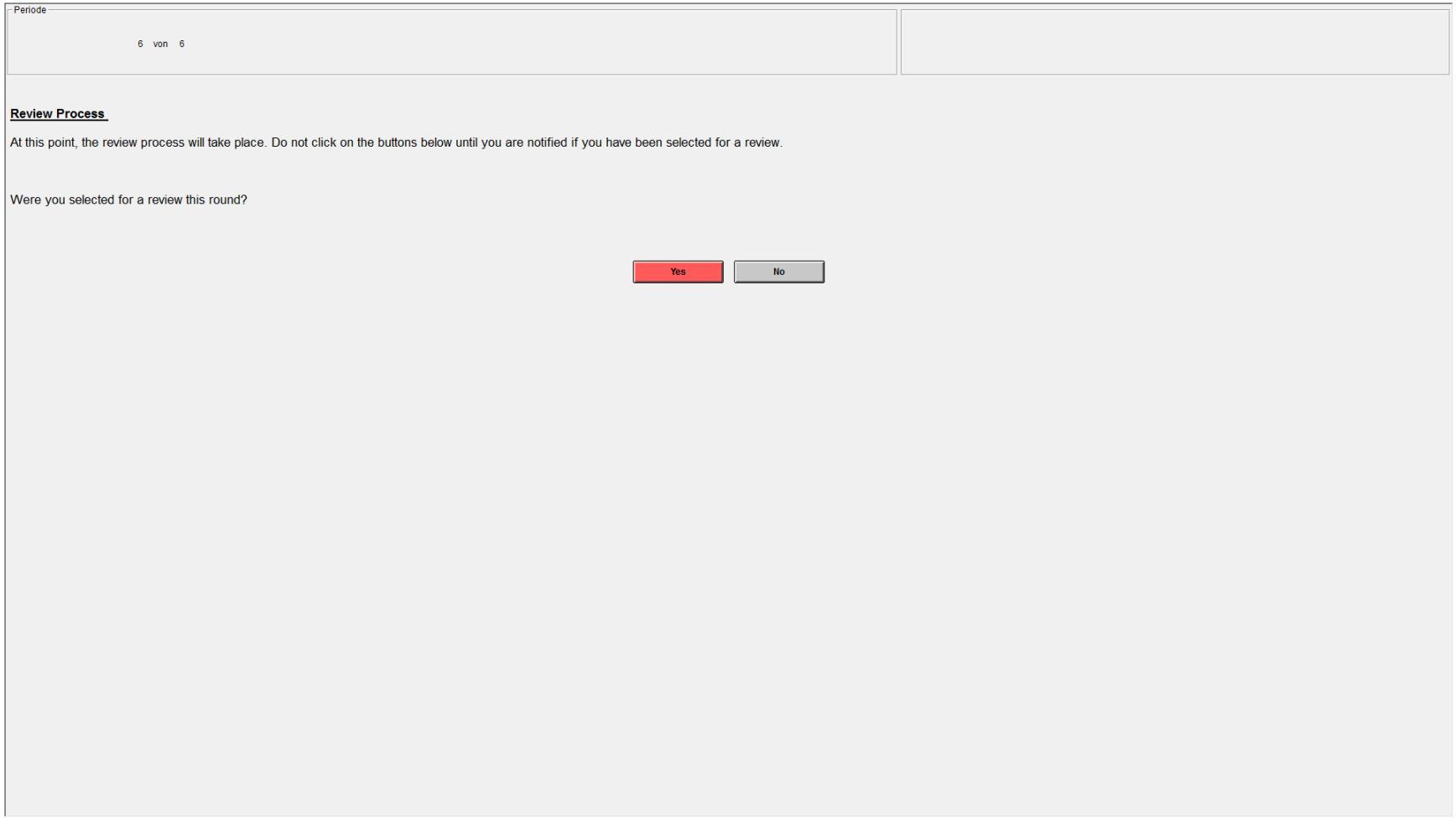


Period	Your Actual Revenue	Your Reported Revenue	Your Partner's Reported Revenue	Your Payout for Round
1	2.61	2.61	4.06	0.61
2	2.20	4.00	3.39	2.00
3	4.59	4.59	2.73	2.59
4	3.39	6.00	2.36	4.00
5	3.16	5.00	5.68	3.00

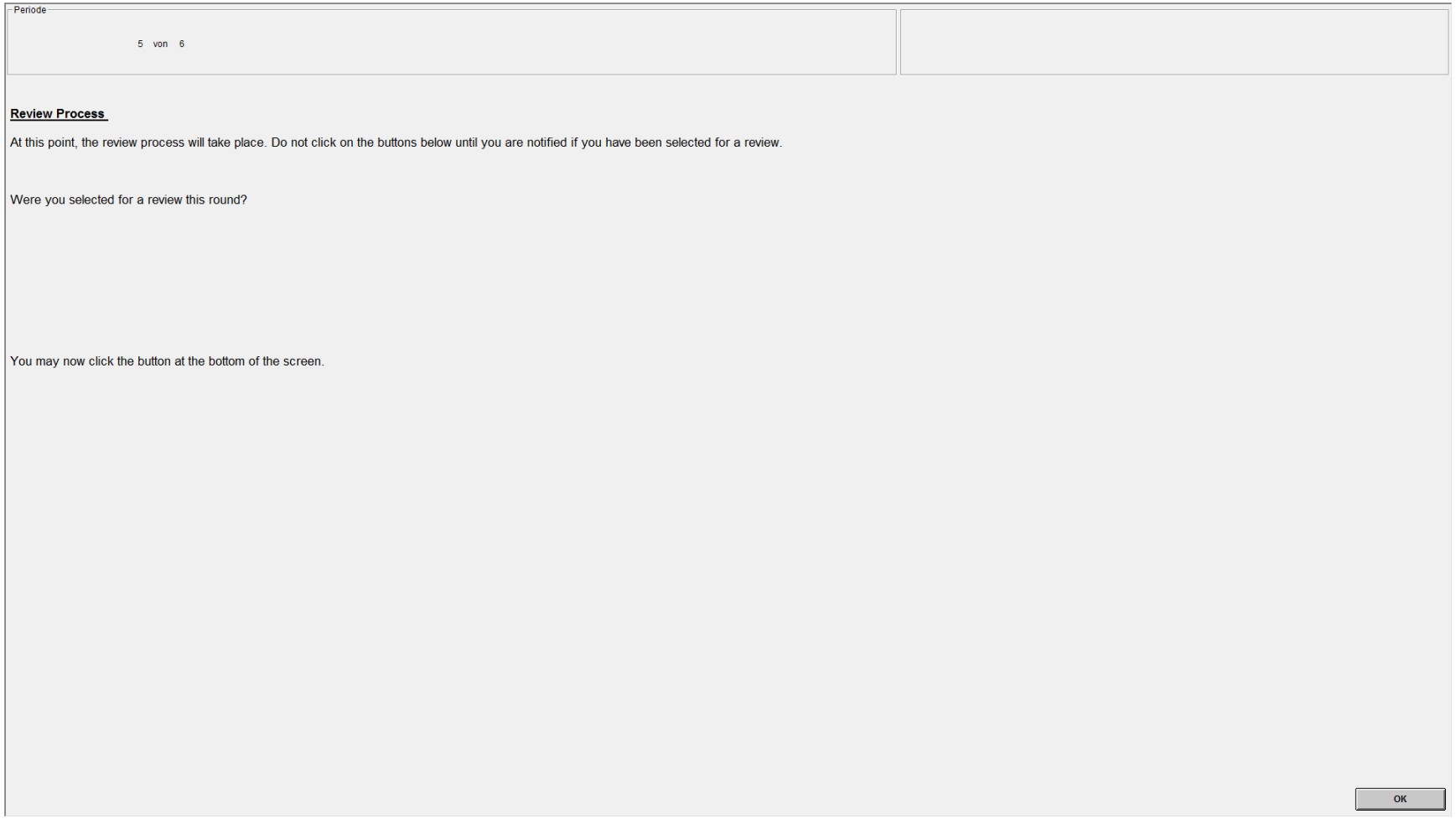
Reporting Information Screen – High CI Conditions



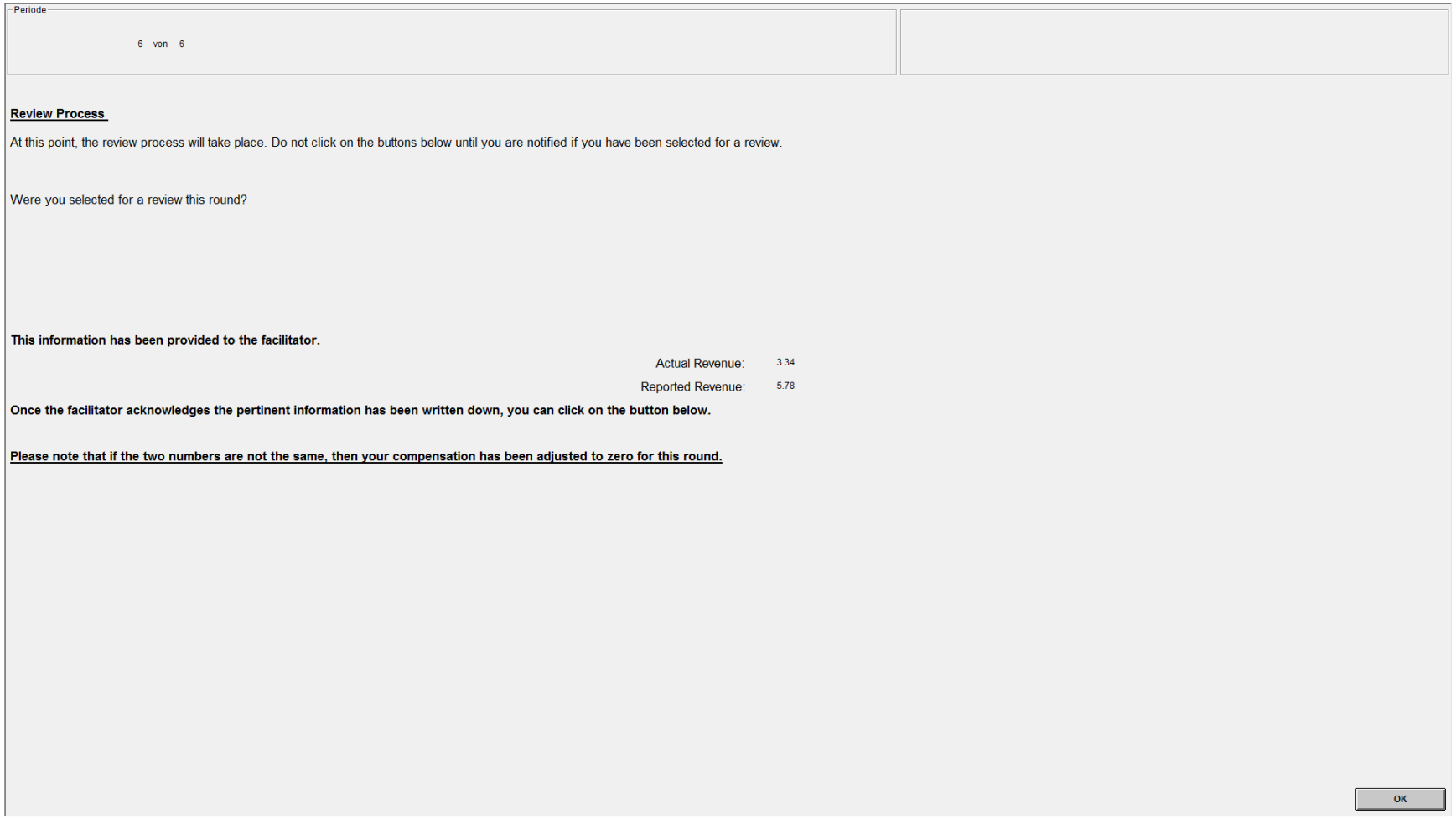
Review Screen – All Conditions



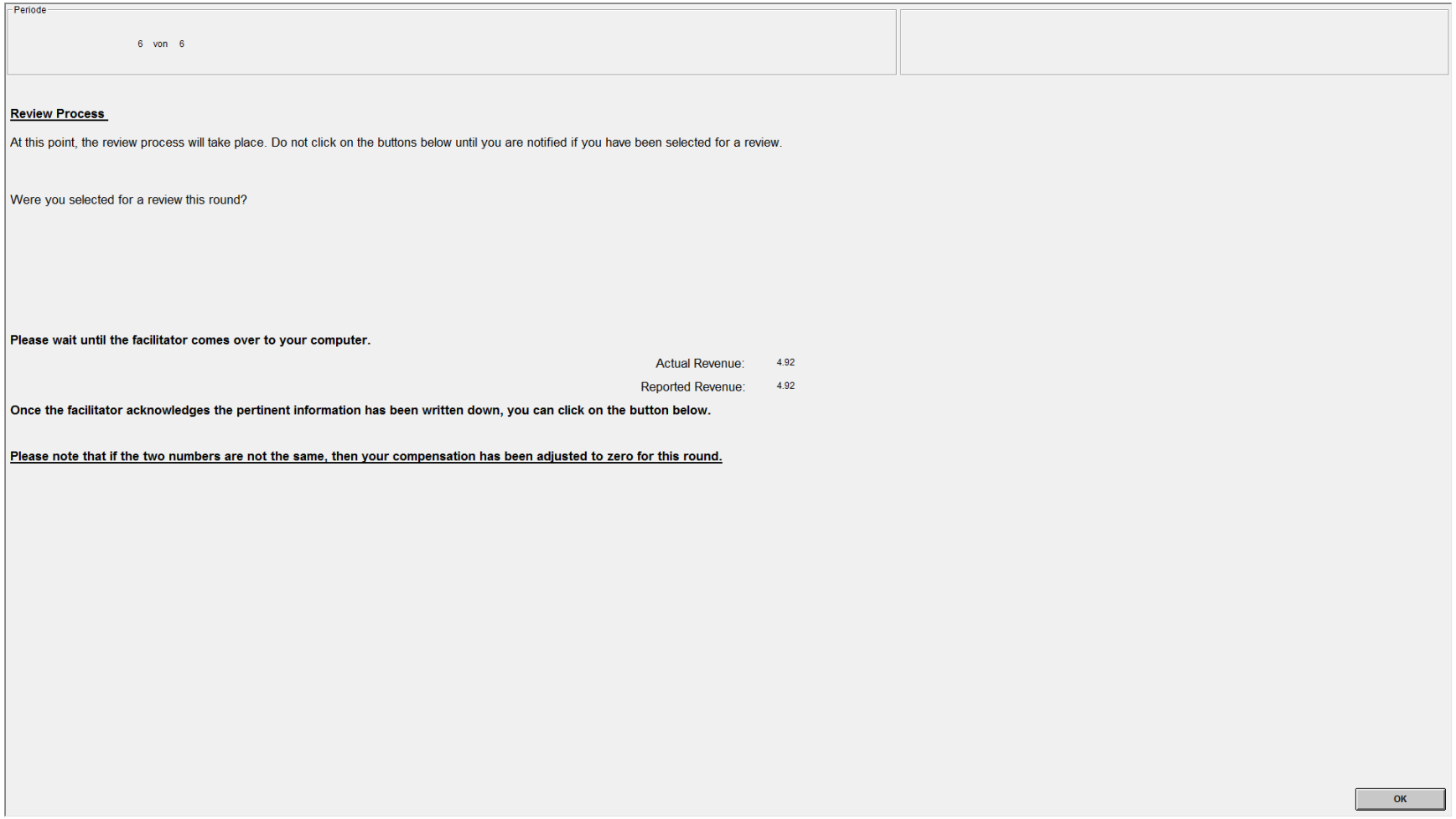
Review Screen – White Poker Chip - All Conditions



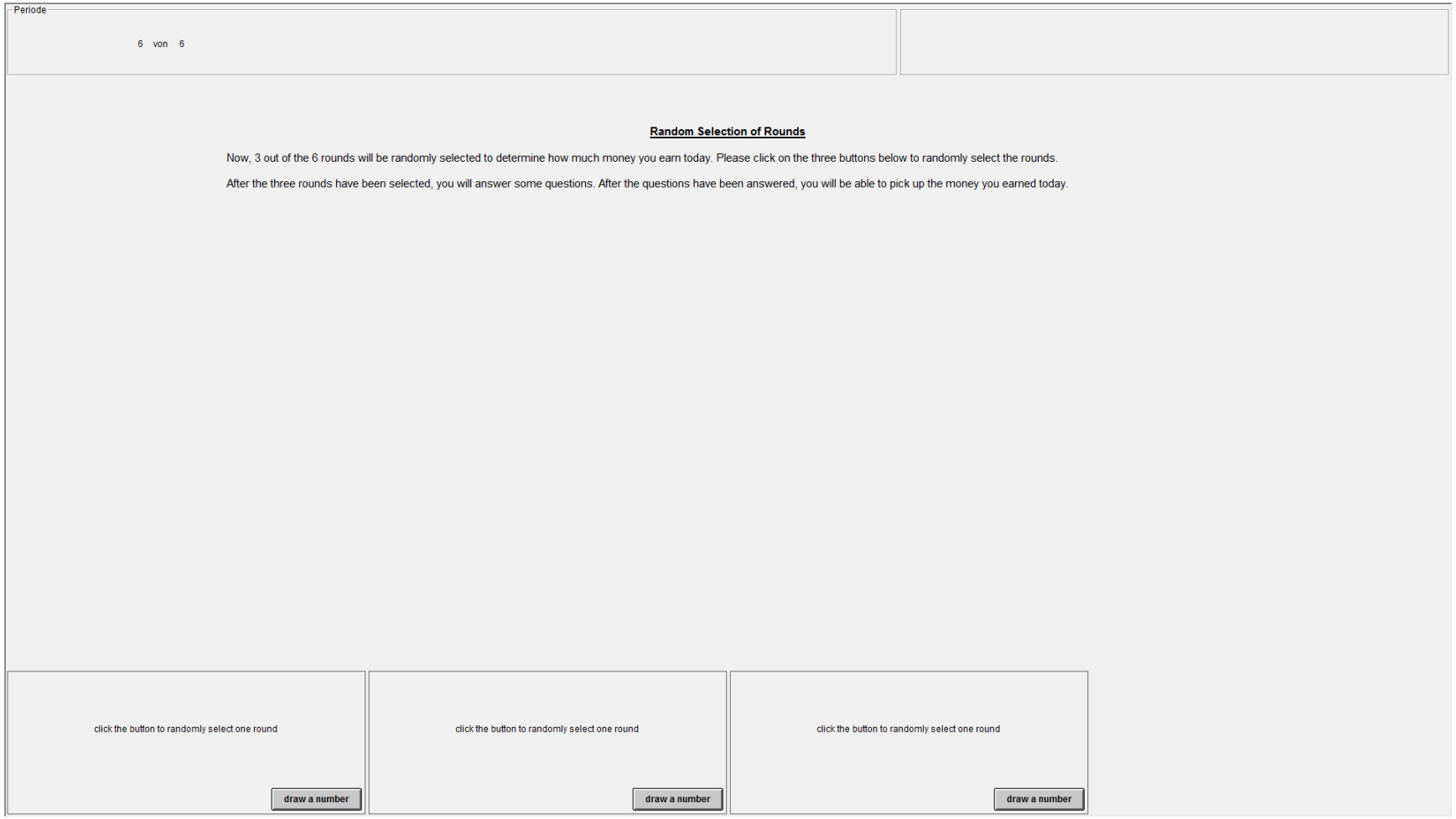
Review Screen – Red Poker Chip – Far Review



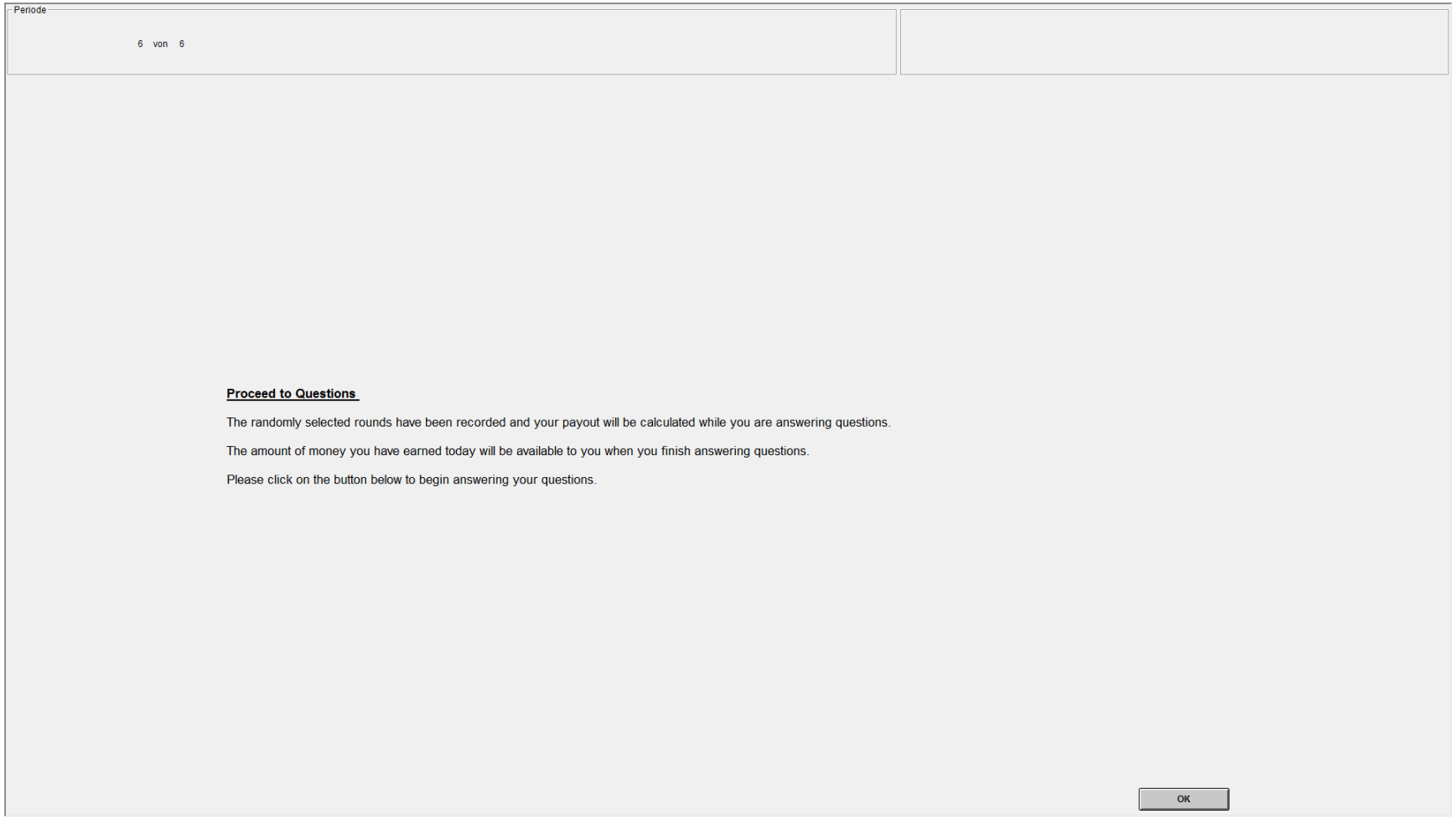
Review Screen – Red Poker Chip – Near Review



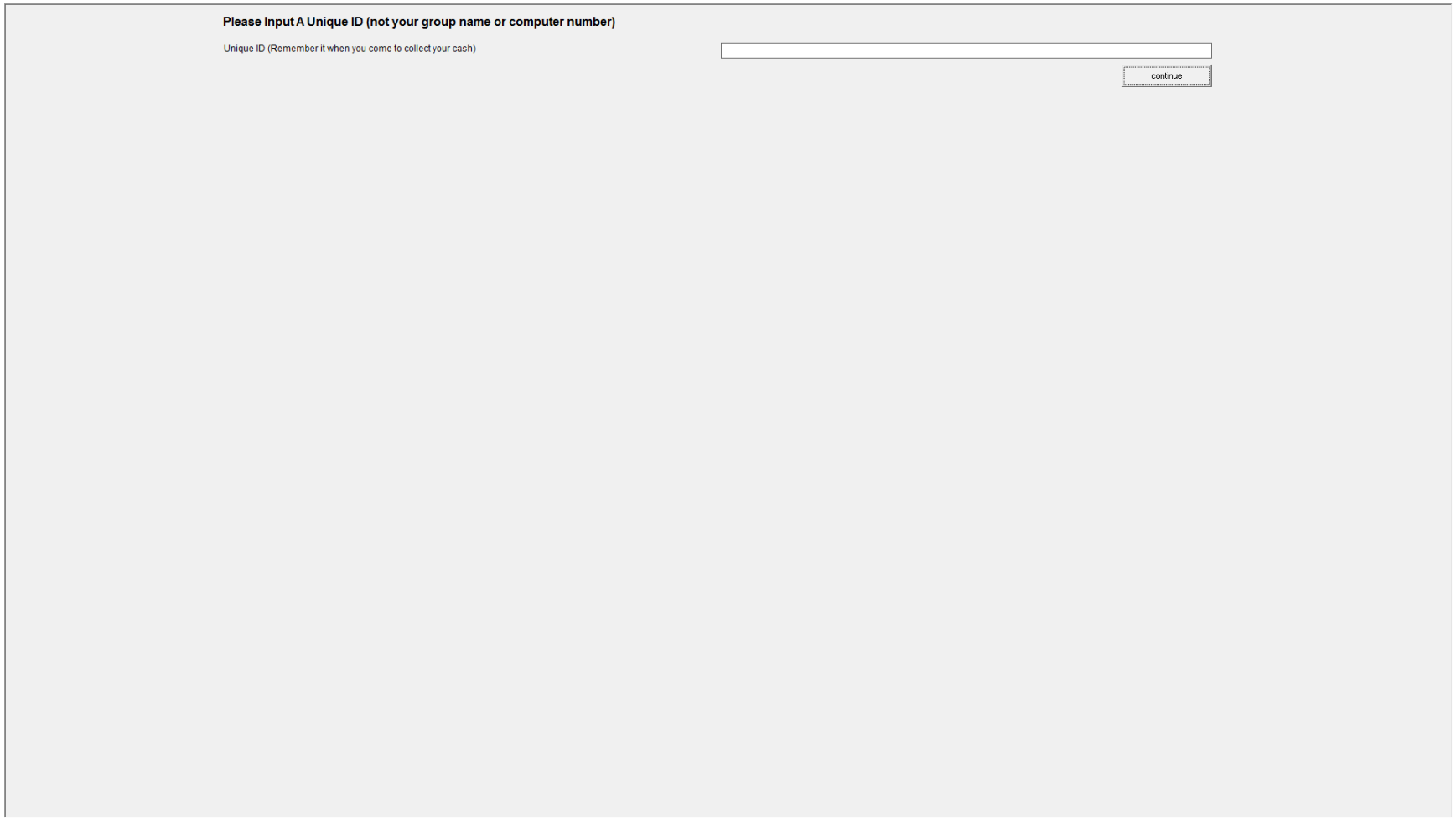
Round Selection Process Screen – All Conditions



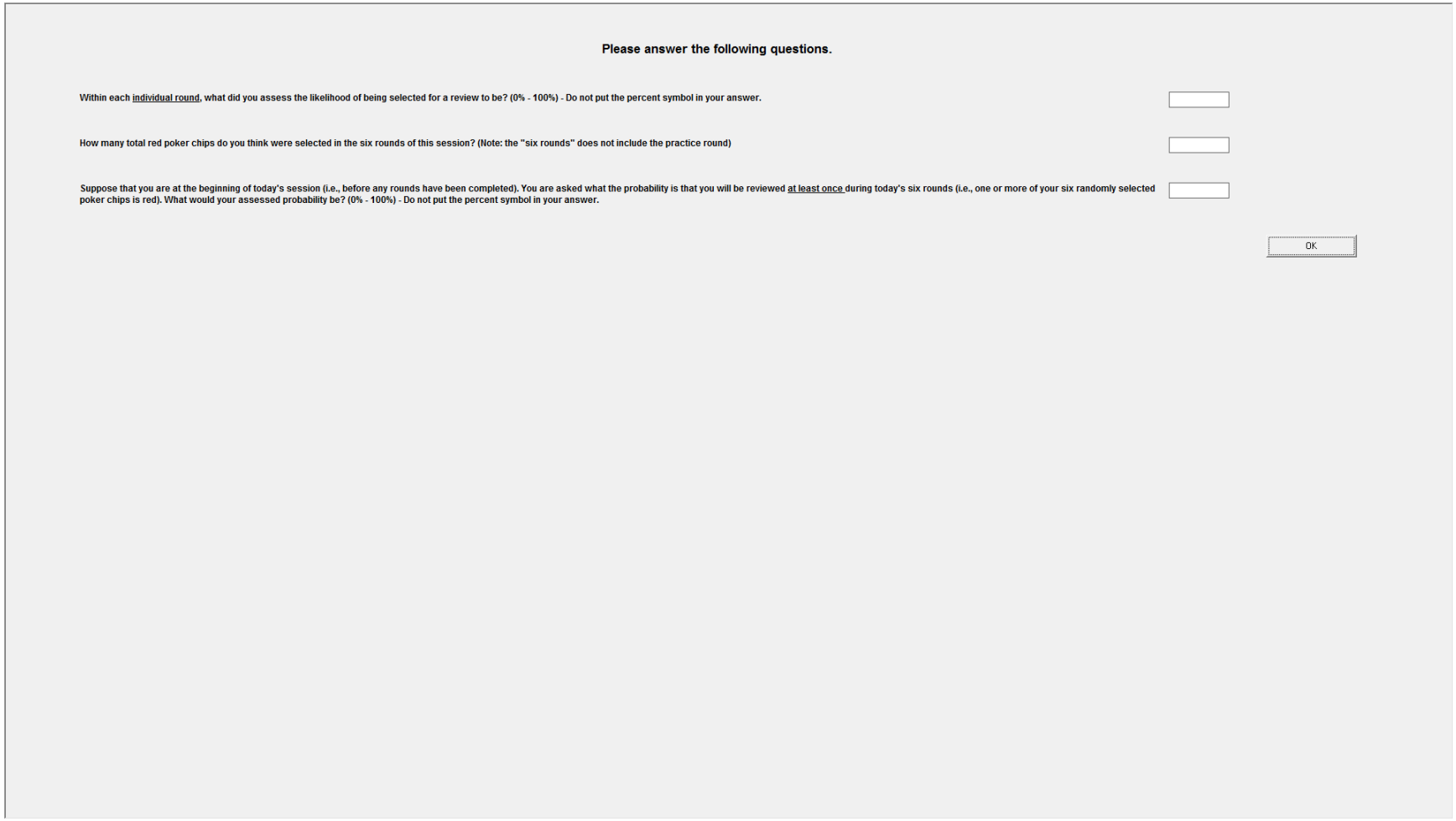
Transition to Post Experiment Question – All Conditions



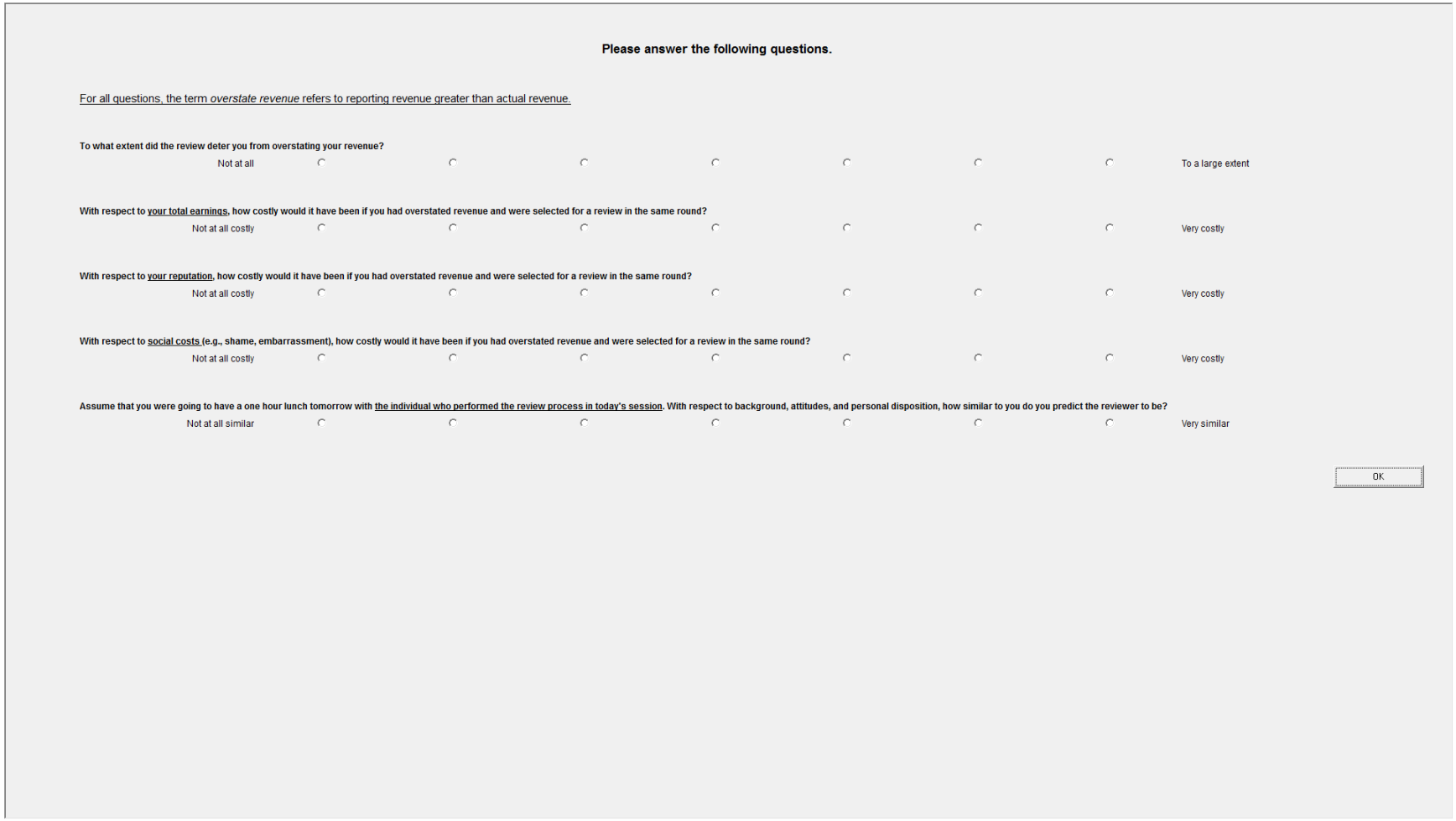
Post Experiment Question 1 – All Conditions



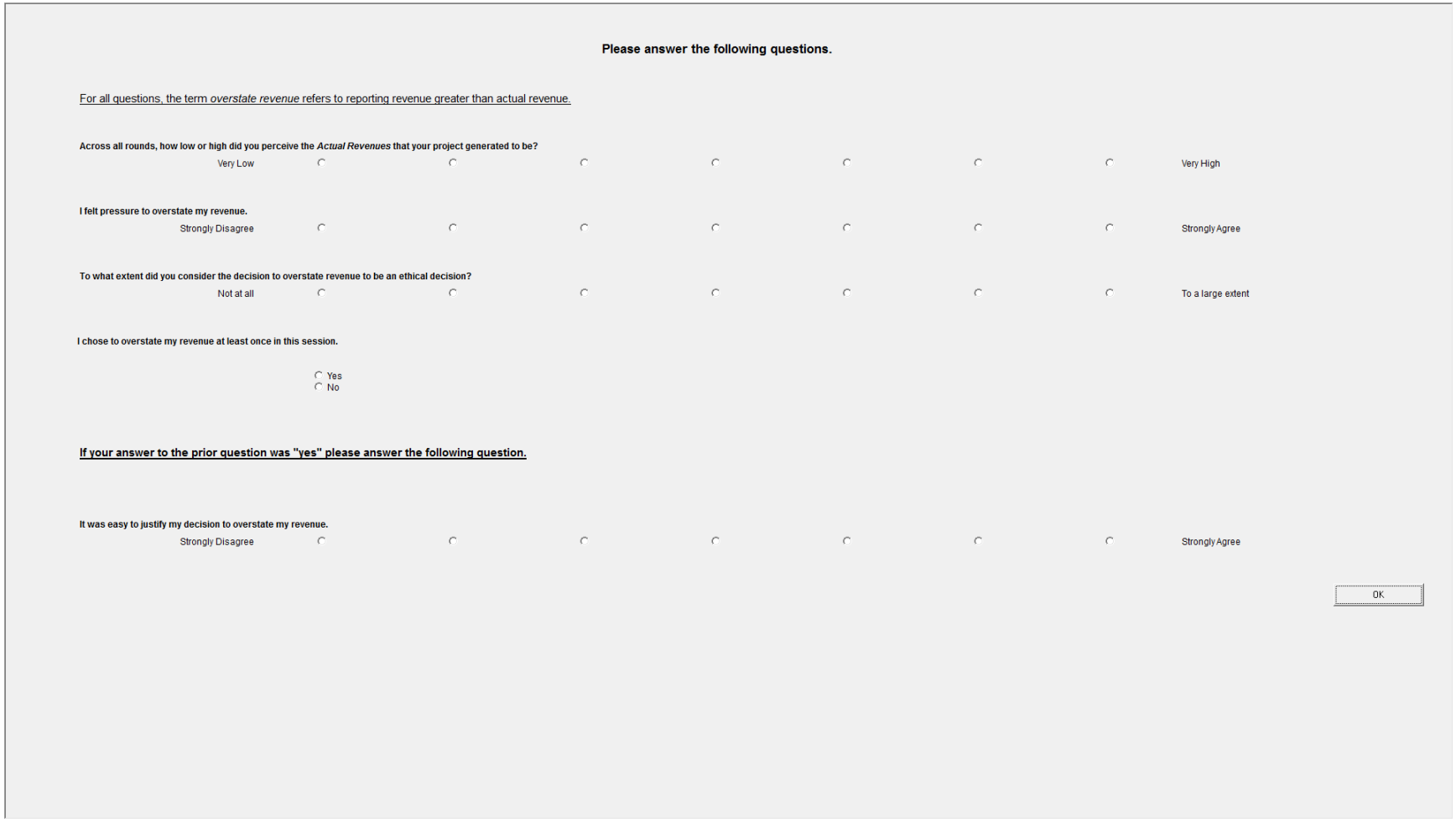
Post Experiment Question 2 – All Conditions



Post Experiment Question 3 – All Conditions



Post Experiment Question 4 – All Conditions



Please answer the following questions.

For all questions, the term *overstate revenue* refers to reporting revenue greater than actual revenue.

Across all rounds, how low or high did you perceive the *Actual Revenues* that your project generated to be?

Very Low ○ ○ ○ ○ ○ ○ ○ Very High

I felt pressure to overstate my revenue.

Strongly Disagree ○ ○ ○ ○ ○ ○ ○ Strongly Agree

To what extent did you consider the decision to overstate revenue to be an ethical decision?

Not at all ○ ○ ○ ○ ○ ○ ○ To a large extent

I chose to overstate my revenue at least once in this session.

○ Yes
○ No

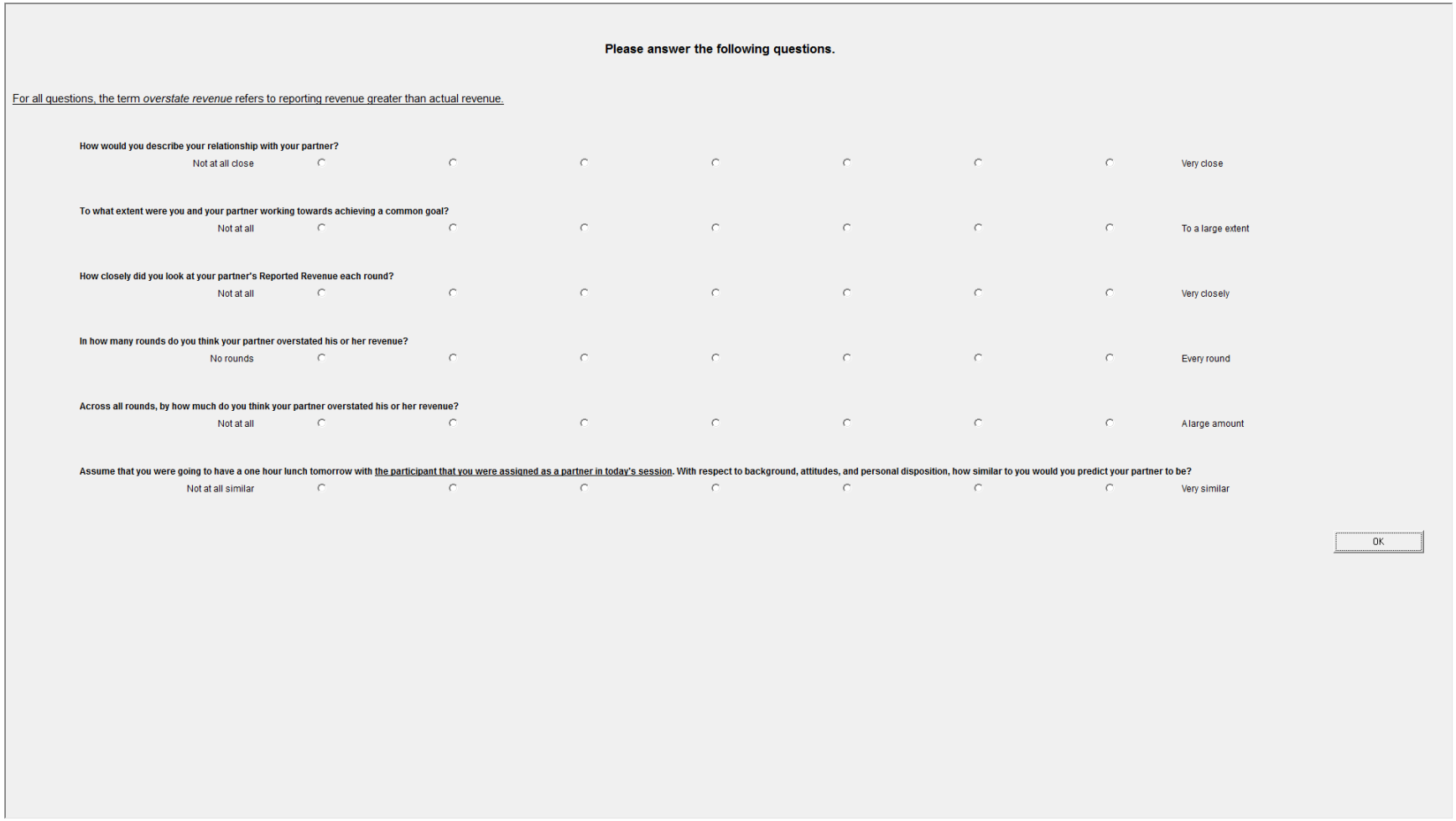
If your answer to the prior question was "yes" please answer the following question.

It was easy to justify my decision to overstate my revenue.

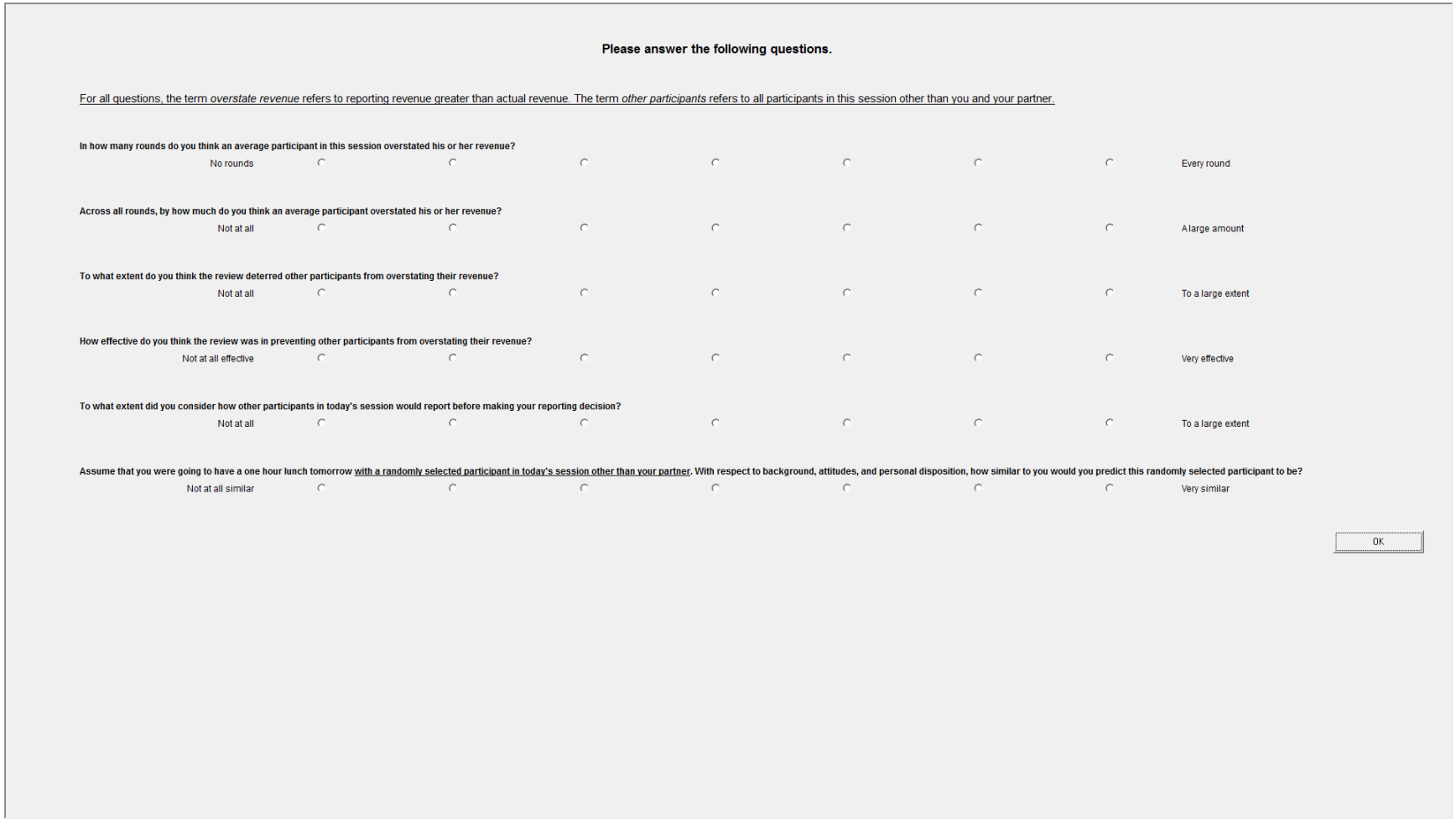
Strongly Disagree ○ ○ ○ ○ ○ ○ ○ Strongly Agree

OK

Post Experiment Question 5 – All Conditions



Post Experiment Question 6 – All Conditions



Please answer the following questions.

For all questions, the term *overstate revenue* refers to reporting revenue greater than actual revenue. The term *other participants* refers to all participants in this session other than you and your partner.

In how many rounds do you think an average participant in this session overstated his or her revenue?

No rounds ○ ○ ○ ○ ○ ○ ○ Every round

Across all rounds, by how much do you think an average participant overstated his or her revenue?

Not at all ○ ○ ○ ○ ○ ○ ○ A large amount

To what extent do you think the review deterred other participants from overstating their revenue?

Not at all ○ ○ ○ ○ ○ ○ ○ To a large extent

How effective do you think the review was in preventing other participants from overstating their revenue?

Not at all effective ○ ○ ○ ○ ○ ○ ○ Very effective

To what extent did you consider how other participants in today's session would report before making your reporting decision?

Not at all ○ ○ ○ ○ ○ ○ ○ To a large extent

Assume that you were going to have a one hour lunch tomorrow with a randomly selected participant in today's session other than your partner. With respect to background, attitudes, and personal disposition, how similar to you would you predict this randomly selected participant to be?

Not at all similar ○ ○ ○ ○ ○ ○ ○ Very similar

OK

Post Experiment Question 7 – All Conditions

Please answer the following questions.

Individuals' performance of a task can be described in many ways. For example, one person might describe a task as "typing a paper," while another might describe the same task as "pushing keys." Yet another person might describe the task as "expressing thoughts." The three following questions describe aspects of today's session. After each aspect will be two different ways in which the aspect might be identified. Here is an example:

Pre-Task Instructions
a) Reading words on a screen describing the task of today's session
b) Gaining understanding on how to perform the task of today's session

Your task is to choose the identification *a* or *b* that best describes the aspect for you. There are no right or wrong answers, just choose the description that *you personally believe* is more appropriate.

The review selection process
- ○ Randomly assessing the accuracy of reported revenue
- ○ Randomly selecting red or white poker chips

Your partner
- ○ A randomly selected individual in this room
- ○ A fellow participant in this study

The person who performed the review
- ○ Someone conducting a study to answer a research question
- ○ Someone documenting if overstating revenues occurred

OK

Post Experiment Question 7 – All Conditions

Please answer the following question.

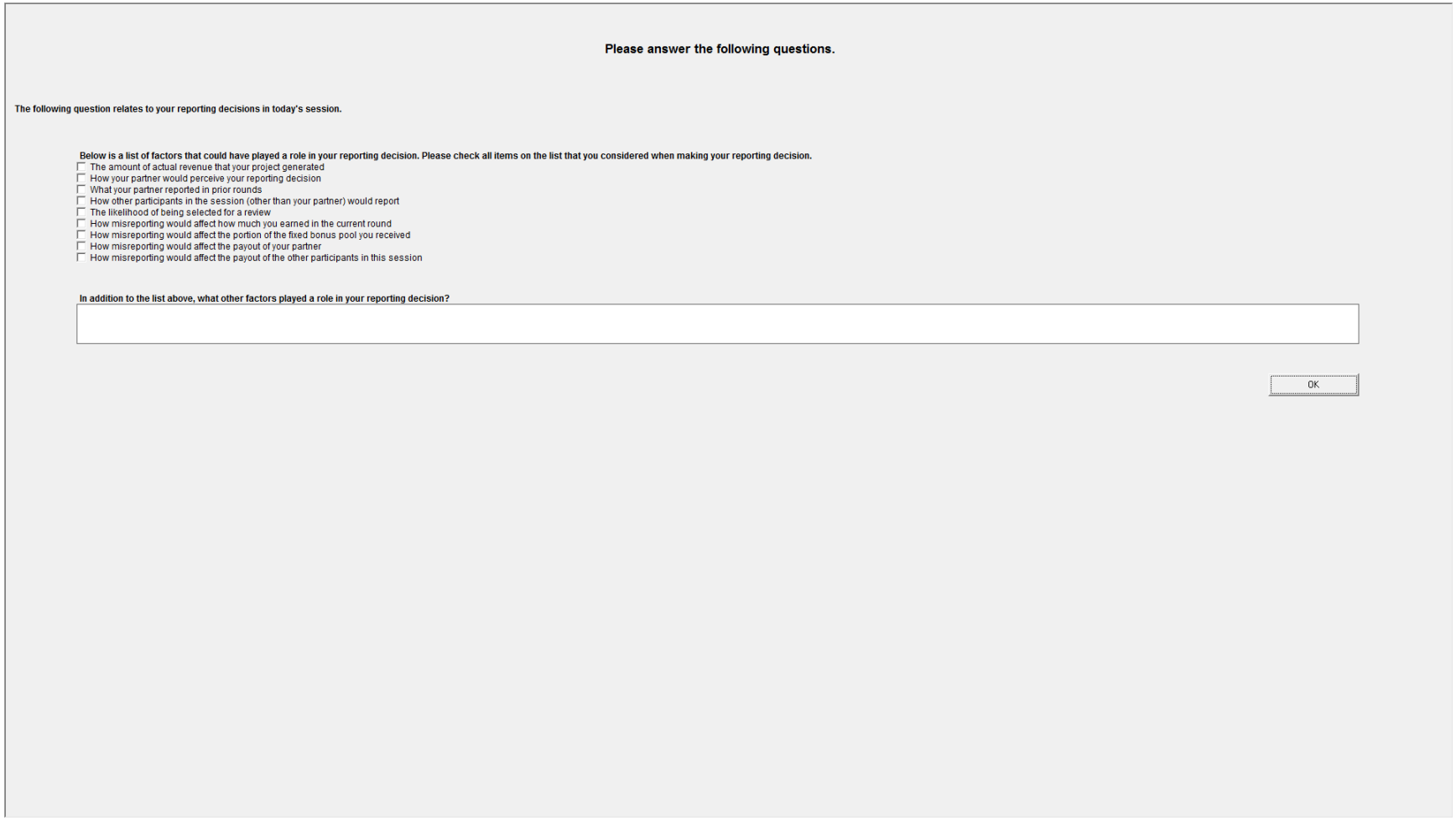
The next question asks you to consider how an internal control similar to the review process in today's session would be classified within a firm.

Internal controls within a firm can be classified into different categories. Below is a list and definitions of several control categories. Please select all the control categories under which you think the review process from today's session could be classified. (Check all that apply).

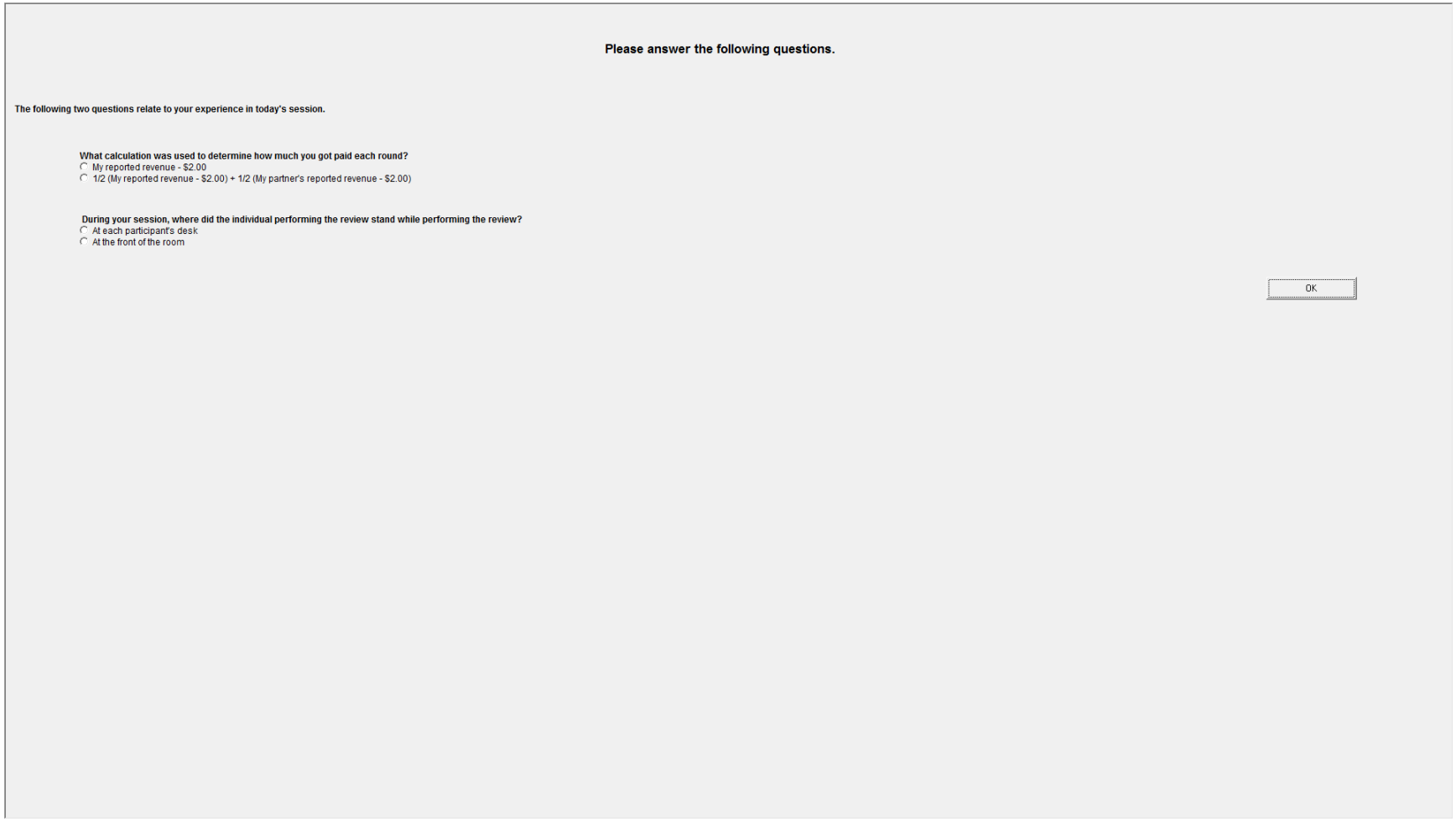
- ☐ Behavior Control: Monitoring ongoing employee activities and behaviors and regulating how work gets done.
- ☐ Bureaucratic Control: The use of rules, policies, hierarchy of authority, written documentation, reward systems, and other formal mechanisms to influence employee behavior and assess performance.
- ☐ Clan Control: The use of values, beliefs, corporate culture, shared norms, and informal relationships to regulate employee behaviors and facilitate the reaching of organizational goals.
- ☐ Corrective Control: A control that assists in the investigation and correction of problems that have been detected.
- ☐ Detective Control: A control designed to find errors or irregularities after they have occurred.
- ☐ Preventative Control: A control designed to discourage errors or irregularities from occurring.

OK

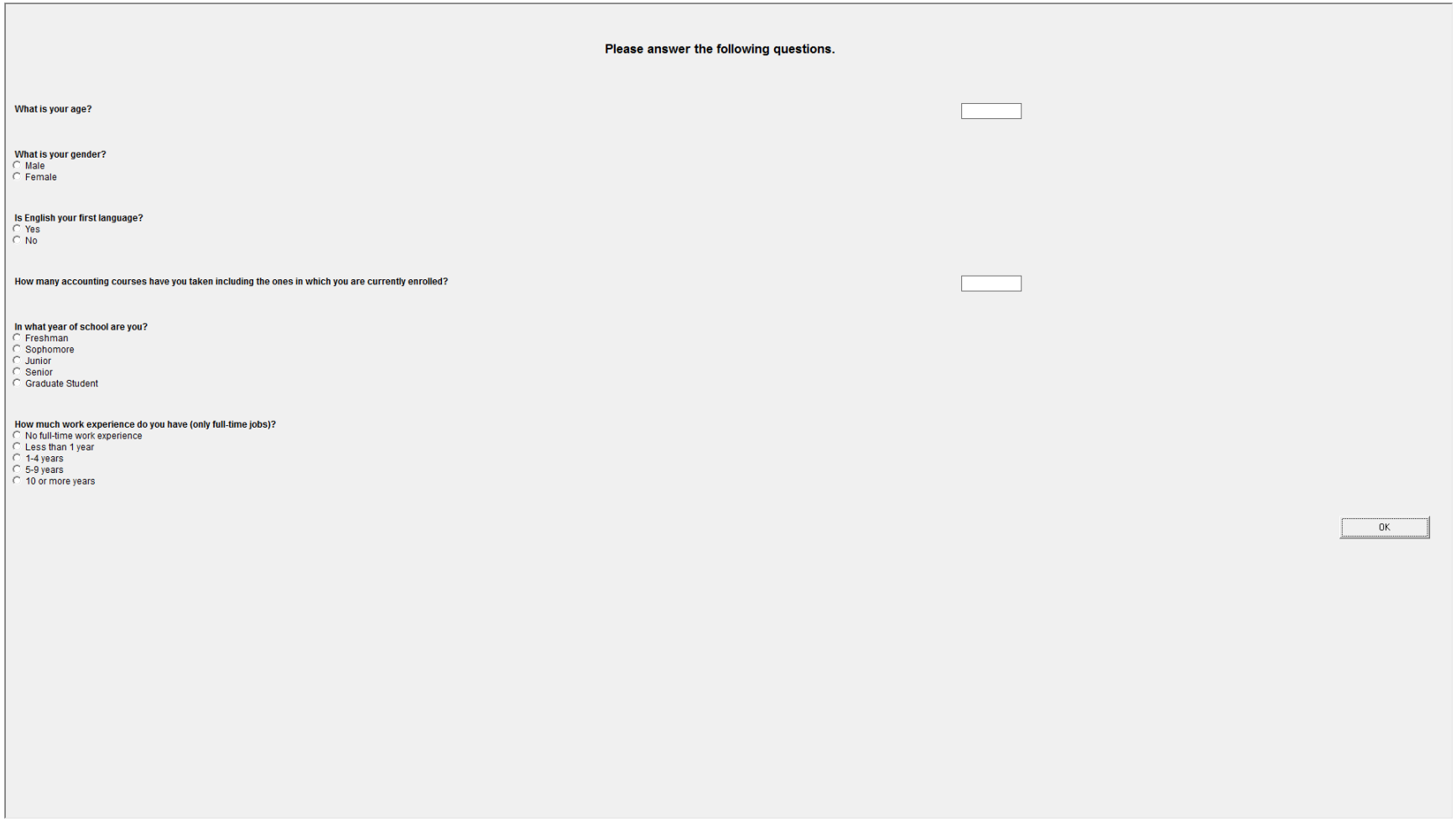
Post Experiment Question 8 – All Conditions



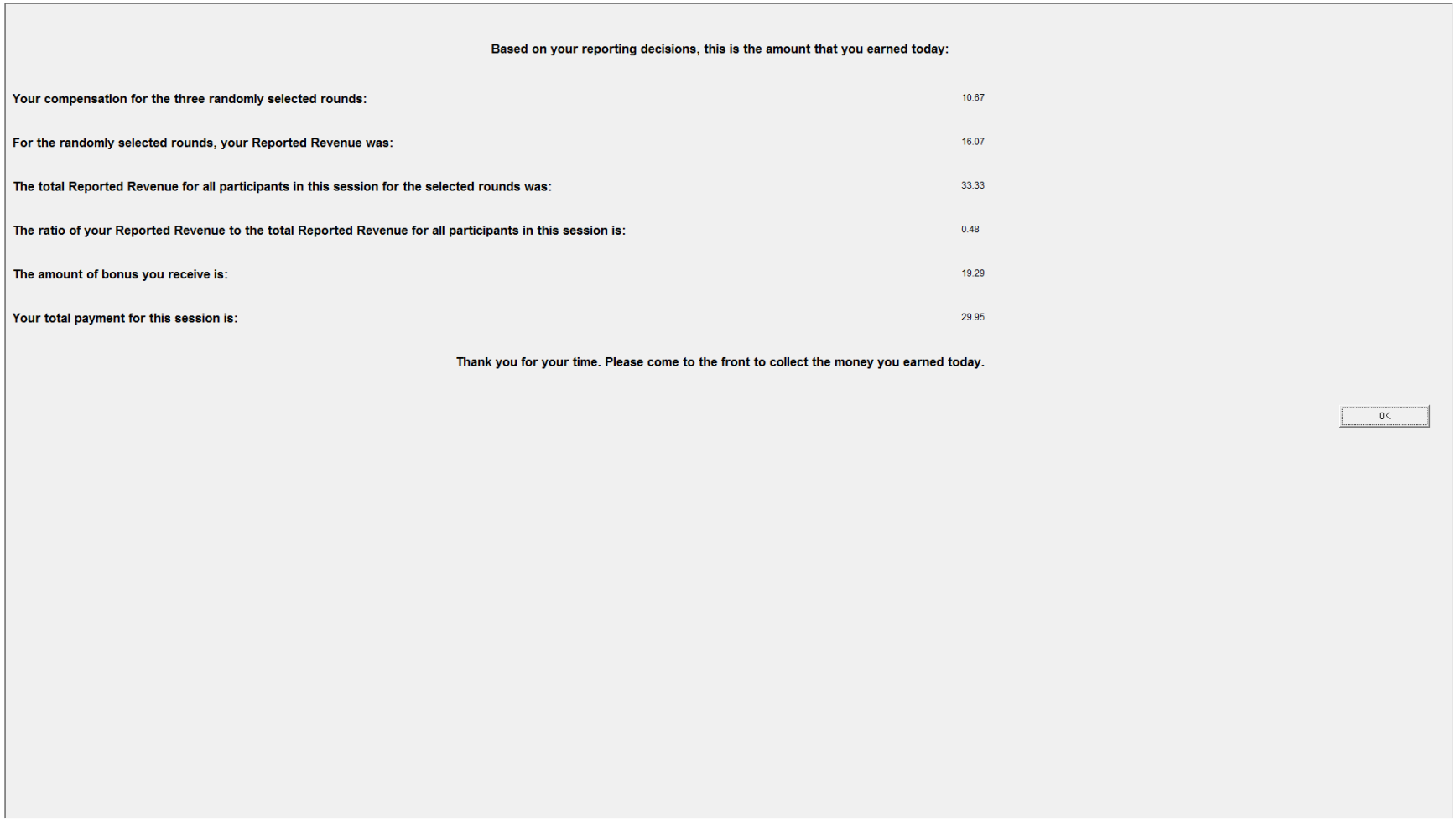
Post Experiment Question 9 – All Conditions



Post Experiment Question 10 – All Conditions



Payment Screen – All Conditions



This payment screen was created for illustration purposes with only two computers running on the computer program. Therefore, the cash payment on this screen is much higher than it was for participants because the bonus pool is divided between only two players.

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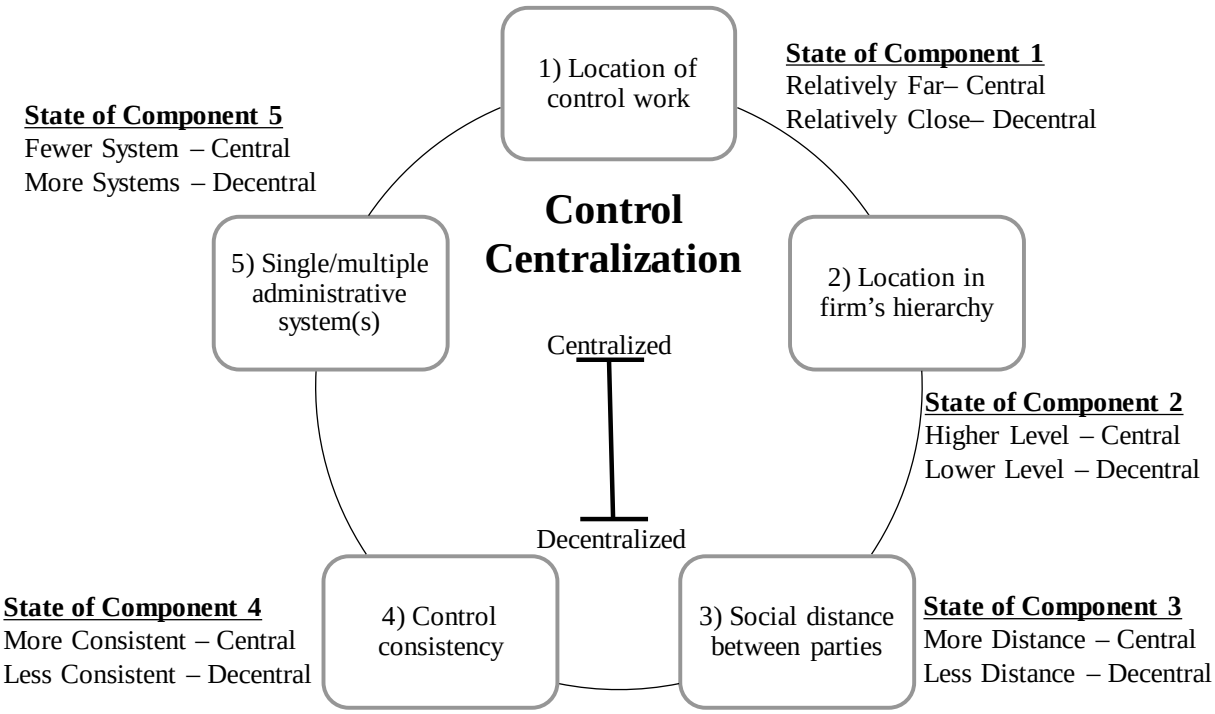
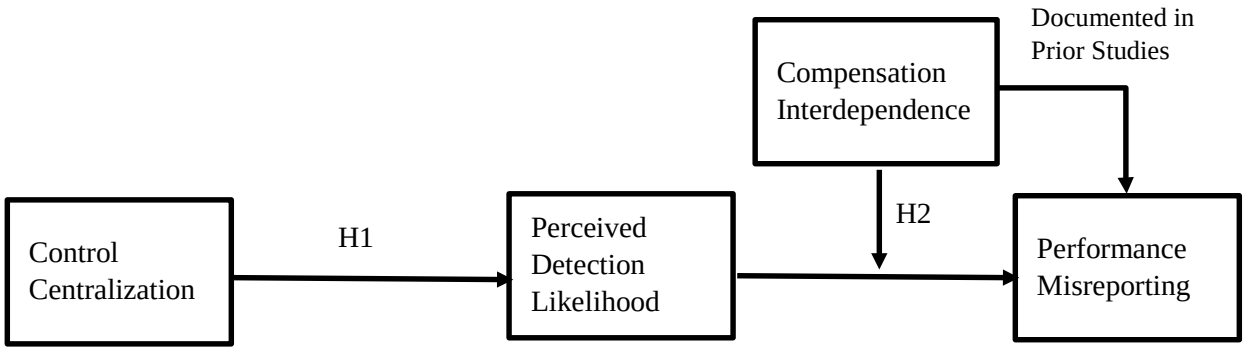
Figures

FIGURE 1
Control Centralization Framework



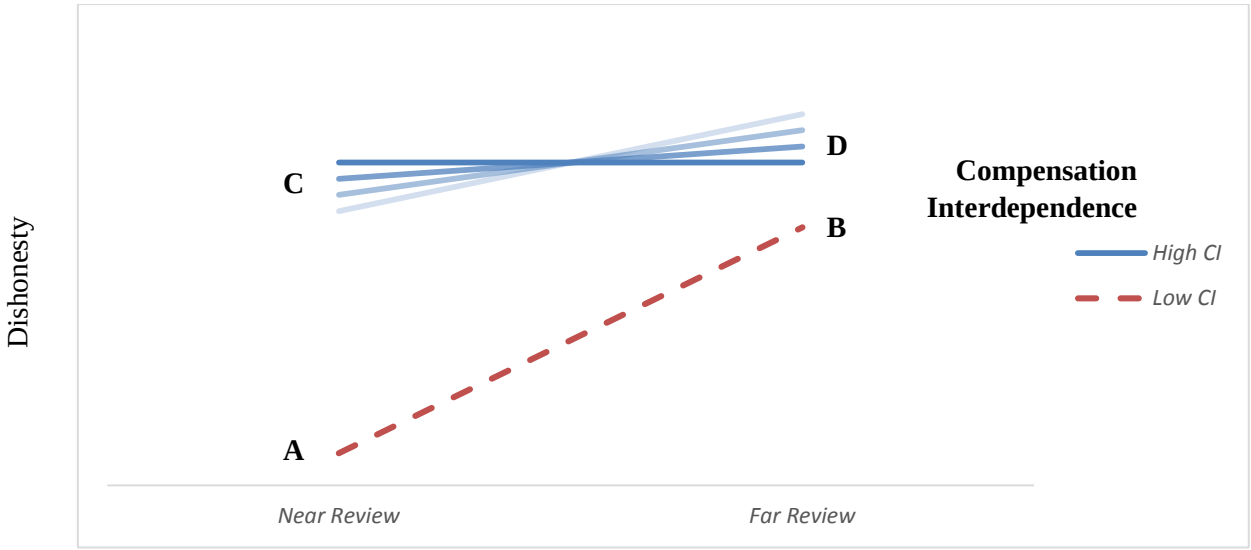
This figure provides a pictorial representation of the control centralization framework. The vertical line with the "centralized" and "decentralized" endpoints in the middle represents the centralized/decentralized continuum along which one can classify a control. The placement of a control along that continuum is contingent upon the states of the control centralization components. As one classifies more components of a control as centralized (decentralized), based on its state, the control classification moves further up (down) along the control centralization continuum.

FIGURE 2
Theoretic Framework and Hypotheses



This figure provides a graphical representation of the causal links underlying the predicted effects. H1 predicts greater control centralization decreases employees' subjective assessments of detection likelihood. H2 predicts the difference in the extent of performance misreporting between a centralized and decentralized control is greater when compensation interdependence is low than when compensation interdependence is high. Prior studies (Church et al. 2012; Maas & Van Rinsum 2013) show greater compensation interdependence increases performance misreporting.

FIGURE 3
Predicted Effect of Control Centralization and Compensation Interdependence on Dishonest Reporting



This figure provides a graphical representation of the predicted joint effect of compensation interdependence and control centralization on misreporting. Prior research predicts that the average of C and D will be greater than the average of A and B (i.e., the direct effect of compensation interdependence on misreporting). Hypothesis 2 predicts that the difference between B and A will be greater than the difference between D and C (i.e., the indirect effect of compensation interdependence on misreporting). I have no theoretical basis to predict whether D is greater than or no different than C. Therefore, the range of lines between C and D represents the uncertainty about the slope.

FIGURE 4
Timeline of Experiment

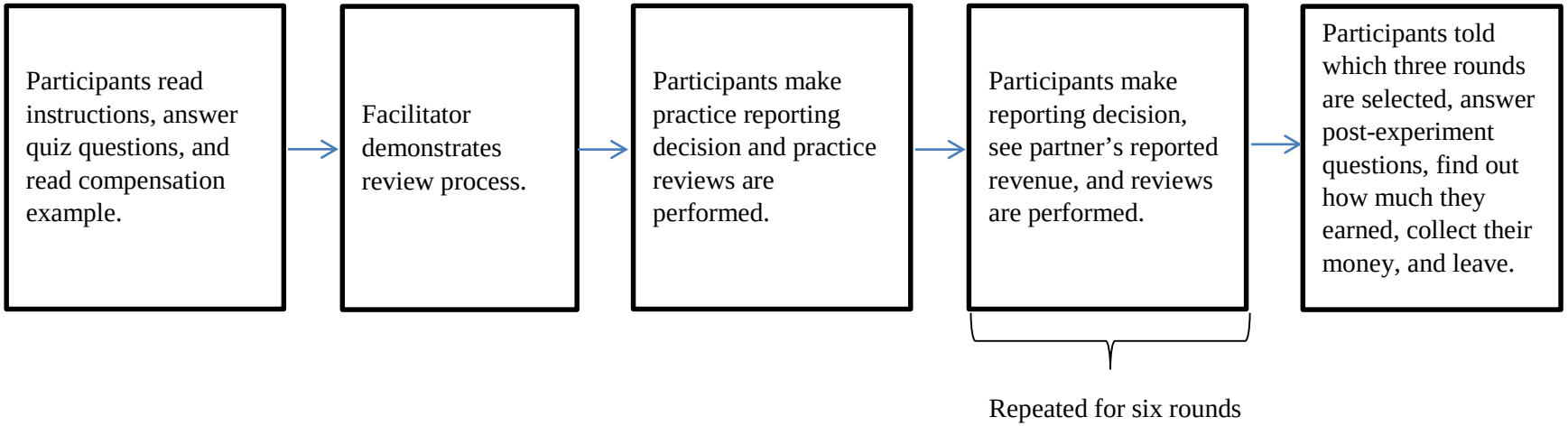
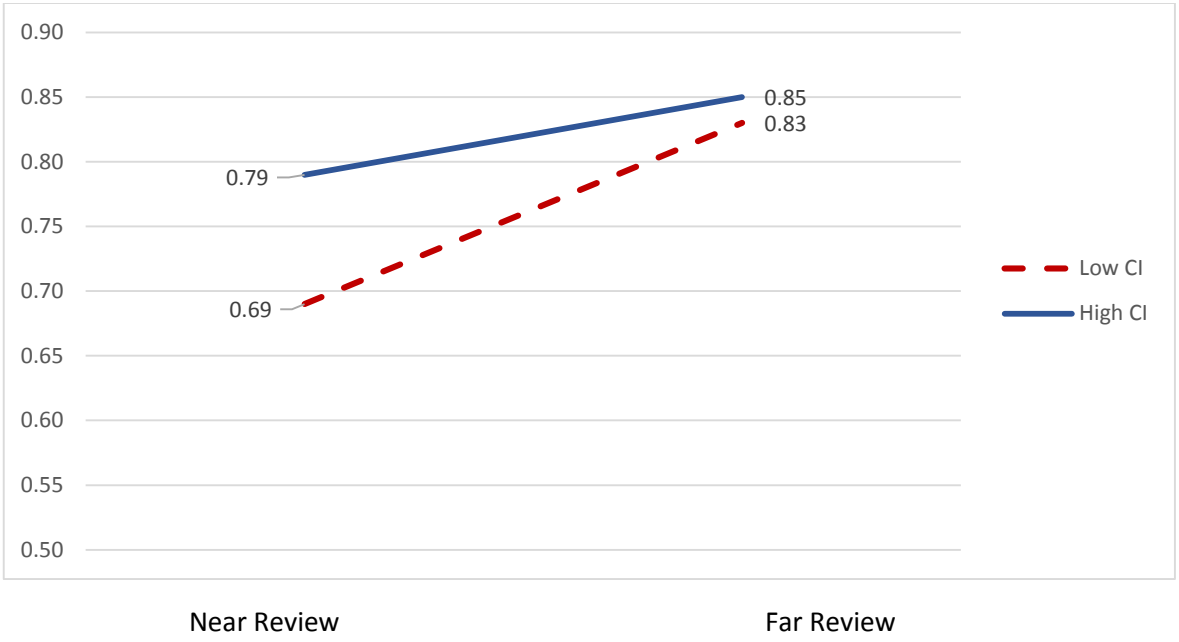


FIGURE 5
Pilot Experiment: Average Dishonesty across Conditions

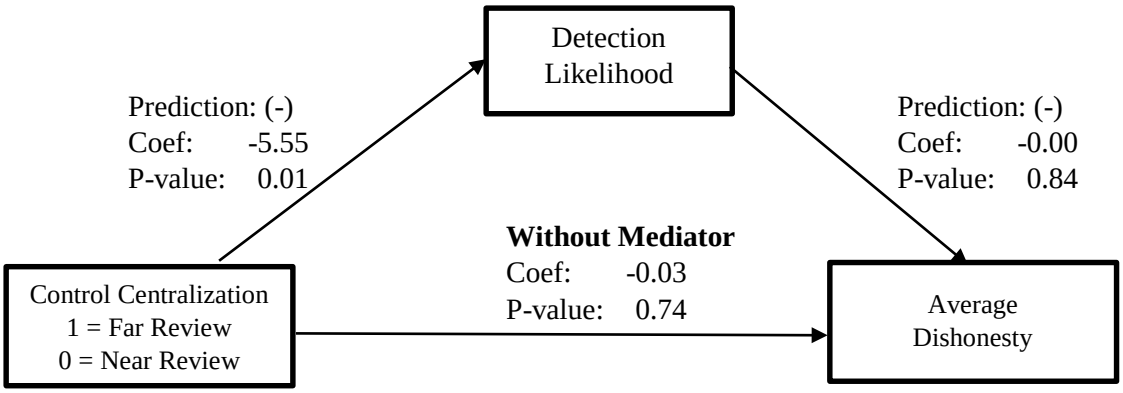


This figure provides a graphical representation of the effects of compensation interdependence and control centralization on dishonest reporting. Average Dishonesty for each condition is provided. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue).

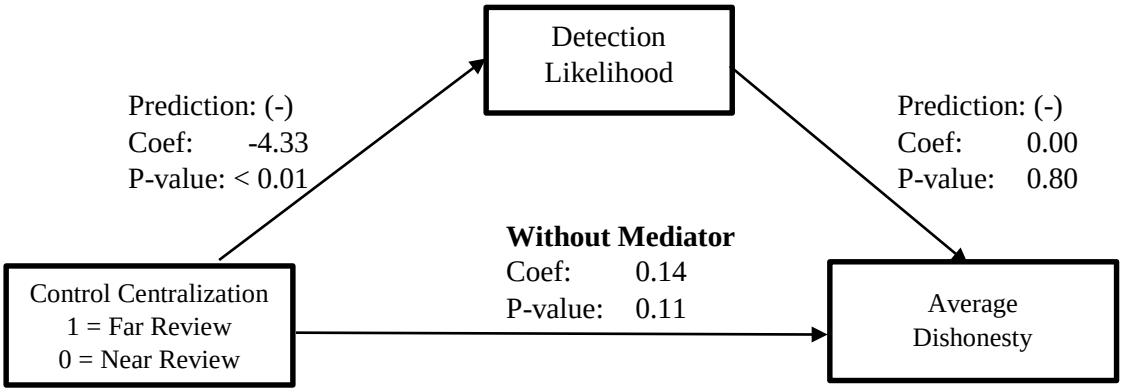
FIGURE 6

Pilot Experiment: Mediation Analysis using Perceived Detection Likelihood

Panel A: *High CI* subsample (n = 27)



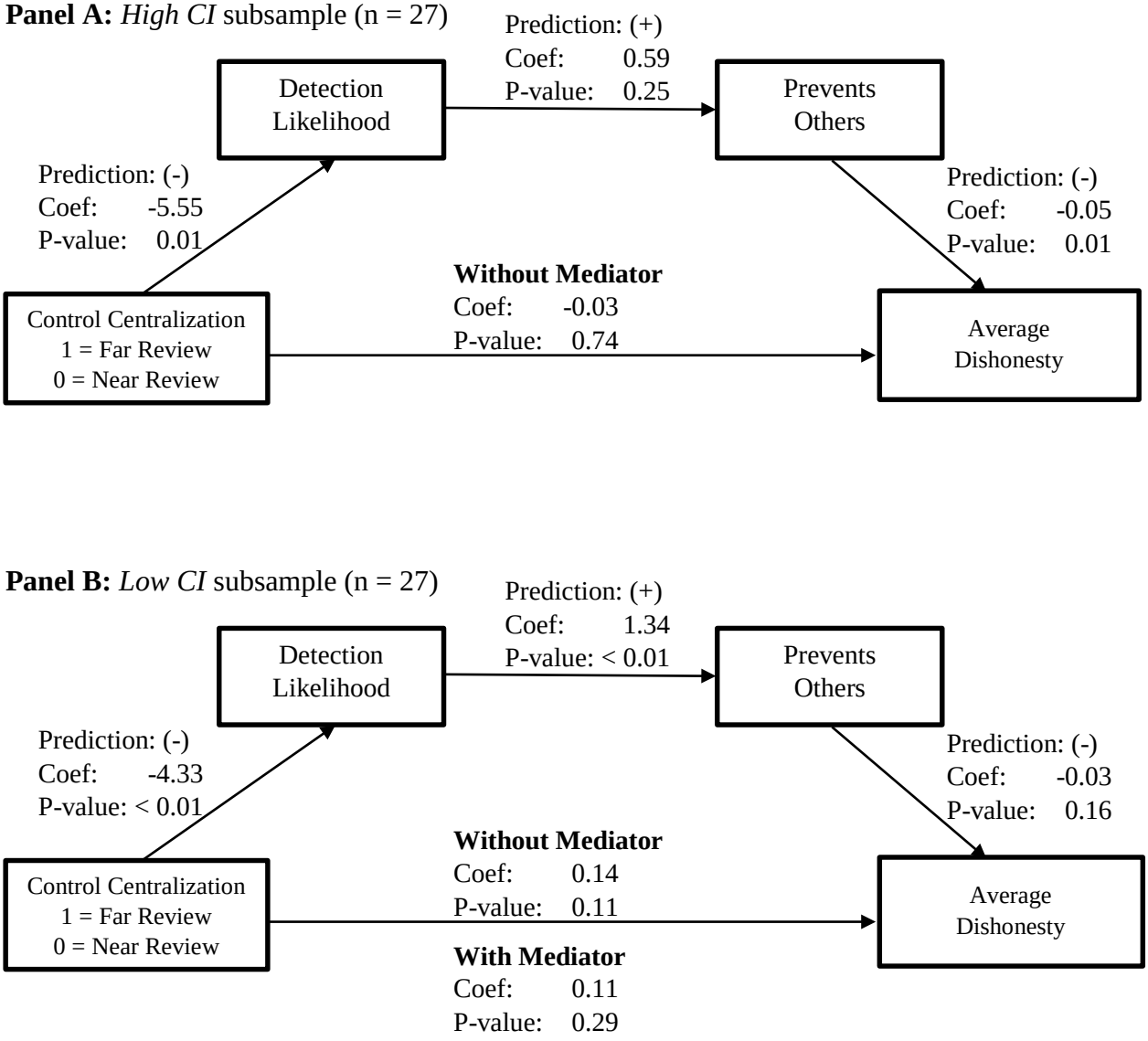
Panel B: *Low CI* subsample (n = 27)



This figure shows the results of two Baron and Kenny (1986) regression mediation analyses. Panel A provides the results for a mediation analysis for the *High CI* subsample. Panel B provides the results for the *Low CI* subsample. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Detection Likelihood is participants' respond to the post-experiment question: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* Participants could respond with any value between 0 and 100, inclusive. Age is included in all regressions as a covariate.

FIGURE 7

Pilot Experiment: Mediation Analysis using Perceived Detection Likelihood and Social Norms



This figure shows the results of two Baron and Kenny regression mediation analyses. Panel A provides the results for a mediation analysis for the *High CI* subsample. Panel B provides the results for the *Low CI* subsample. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Detection Likelihood is participants' responses to the post-experiment question: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* Participants could respond with any value between 0 and 100, inclusive. Prevent Others is participants' responses to the post-experiment question: *How effective do you think the review was in preventing participants from overstating revenue?* Higher values indicate a higher level of effectiveness.

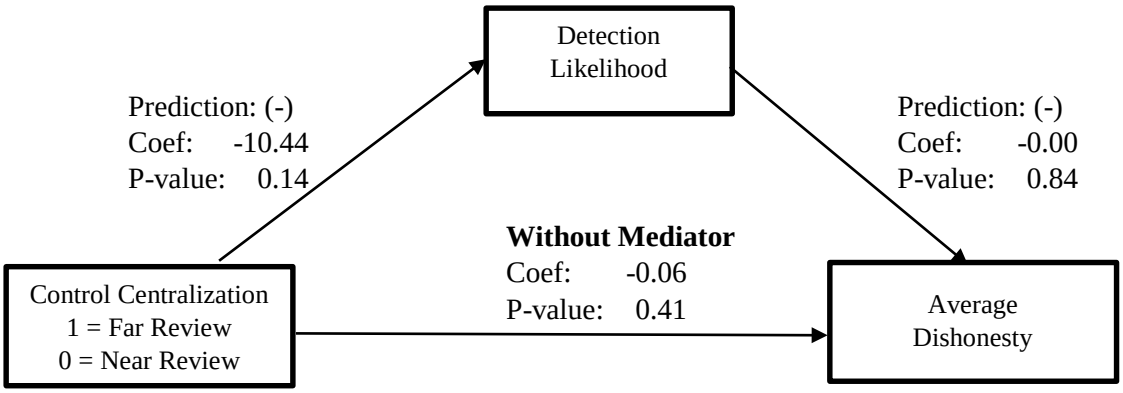
FIGURE 8

Full Experiment: Effect of Control Centralization and Compensation Interdependence on Dishonest Reporting

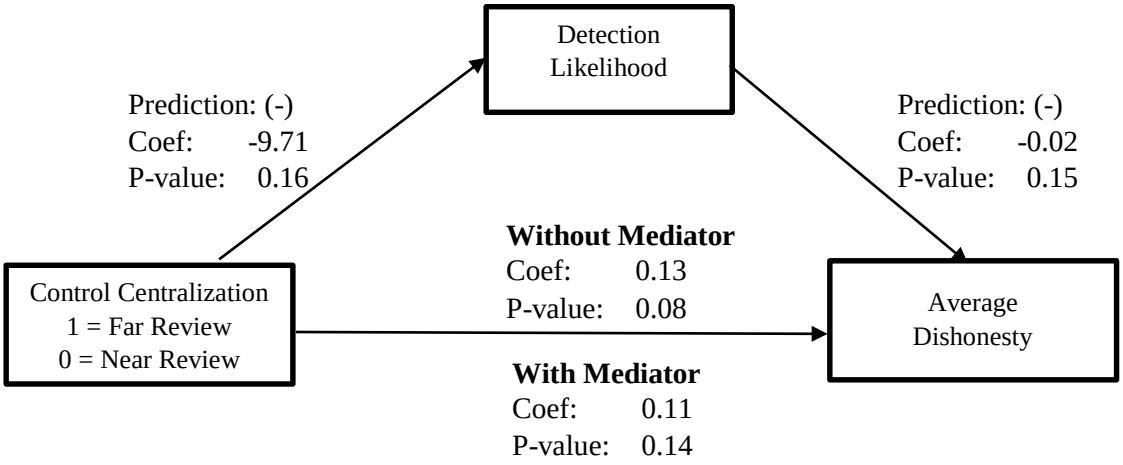
This figure provides a graphical representation of the effects of compensation interdependence and control centralization on dishonest reporting. Average Dishonesty for each condition is provided. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue).

FIGURE 9
Full Experiment: Mediation Analysis – Perceived Detection Likelihood

Panel A: *High CI* subsample (n = 42)



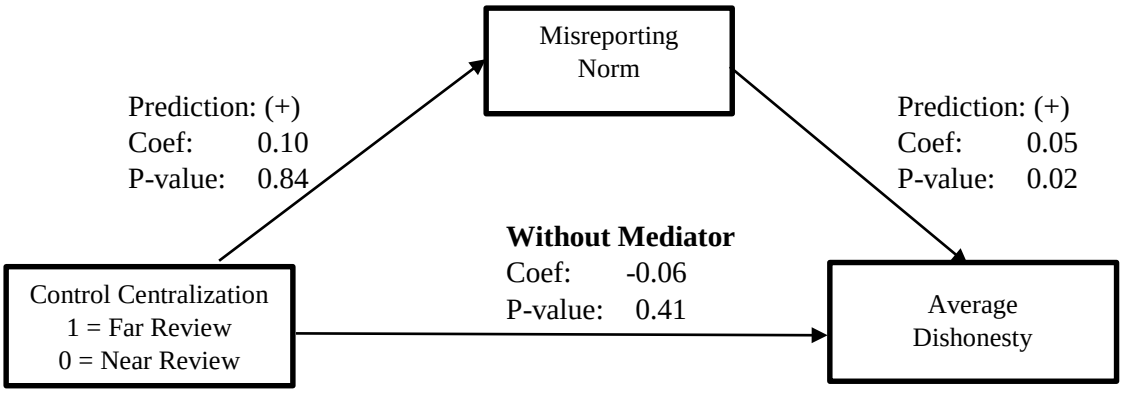
Panel B: *Low CI* subsample (n = 44)



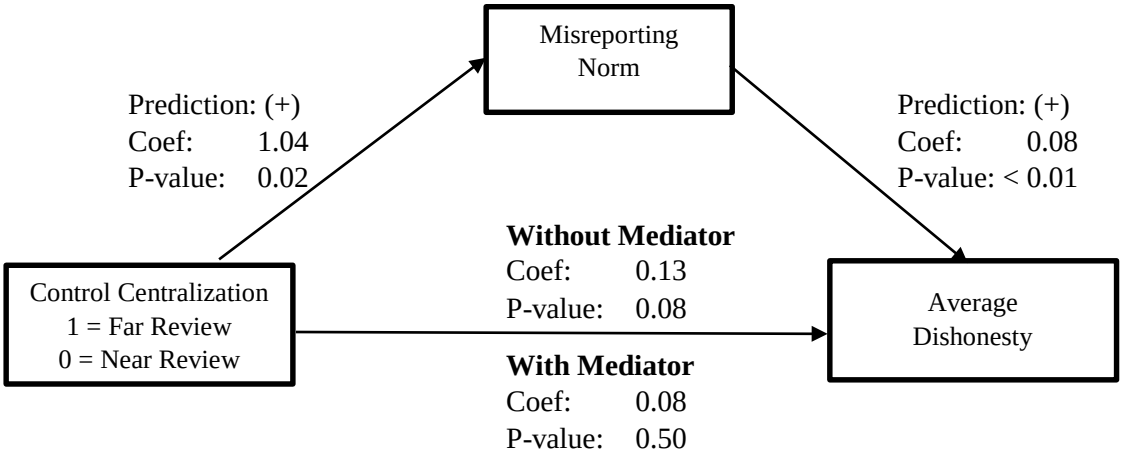
This figure shows the results of two Baron and Kenny regression mediation analyses. Panel A provides the results for a mediation analysis for the *High CI* subsample. Panel B provides the results for the *Low CI* subsample. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Detection Likelihood is participants' response to the post-experiment question: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* Participants could respond with any value between 0 and 100, inclusive. A Sobel-Goodman test reveals that Detection Likelihood mediates 17 percent of the total effect between Control Centralization and Average Dishonest in the *Low CI* subsample.

FIGURE 10
Full Experiment: Mediation Analysis – Misreporting Norm

Panel A: *High CI* subsample (n = 42)



Panel B: *Low CI* subsample (n = 42)



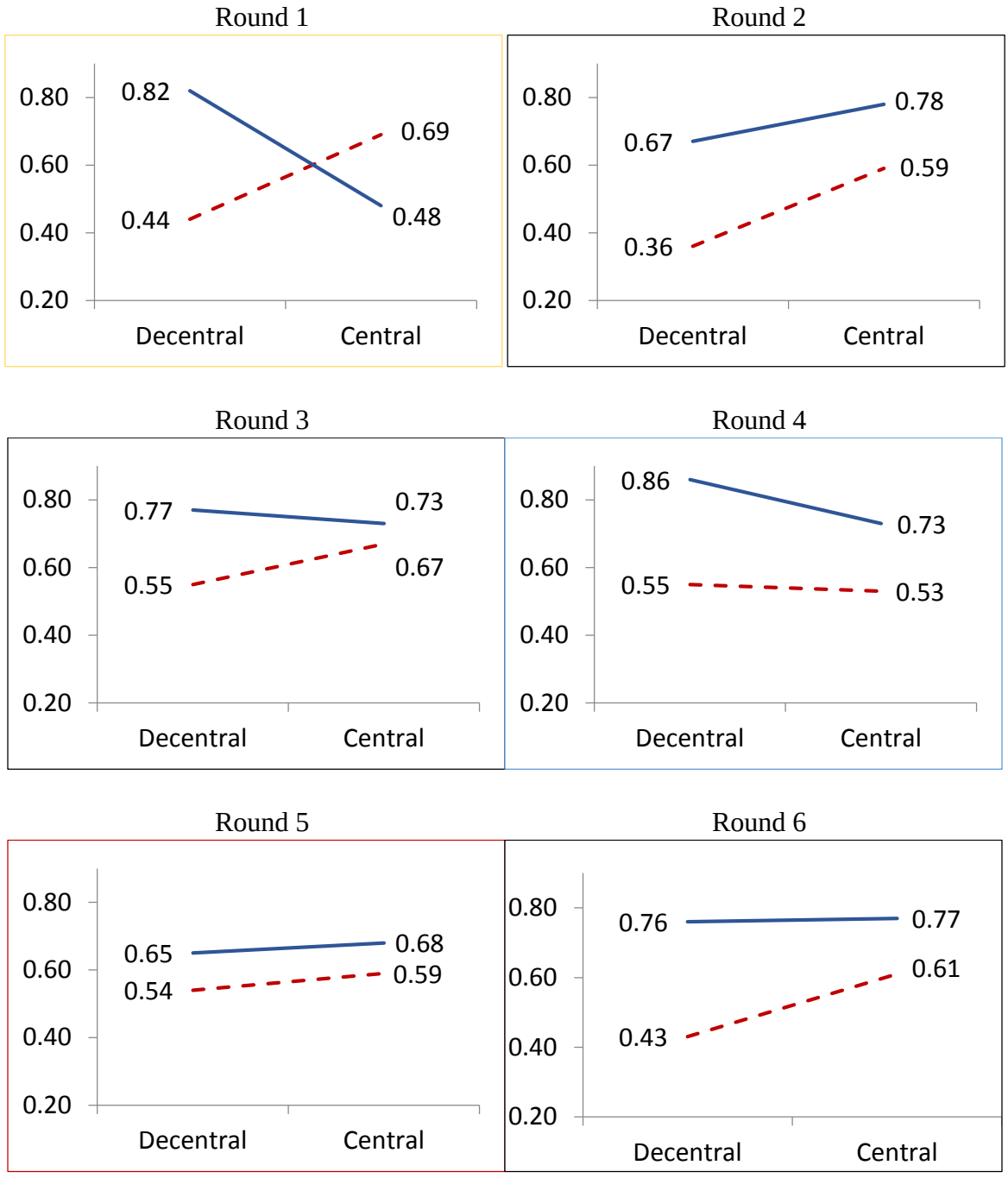
This figure shows the results of two Baron and Kenny regression mediation analyses. Panel A provides the results for a mediation analysis for the *High CI* subsample. Panel B provides the results for the *Low CI* subsample. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Misreporting Norm is calculated using a principal component analysis on the responses to four post-experiment question: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* (3) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (4) *Across all rounds, by how much do you think an average participant overstated his or her revenue?* Participants responded using a seven point Likert Scale with higher values indicating a higher perception that others were misreporting. A Sobel-Goodman test reveals that Misreporting Norm mediates 66 percent of the total effect between Control Centralization and Average Dishonesty in the *Low CI* subsample.

FIGURE 11

Full Experiment: Distribution of Actual Revenue across Conditions

The bar charts show the frequency of actual revenue across experimental conditions. The x-axis corresponds to different "bins" of actual revenue where each bin represents $0.50 increments of actual revenue. For example, bin 1 contains all observations with actual revenue between $2.00 and $2.49, bin 2 contains all observations with actual revenue between $2.50 and $2.99 and so on. The red line indicates the number of observations in each bin given a uniform actual revenue distribution across bins.

FIGURE 12
Full Experiment: Dishonesty across Rounds

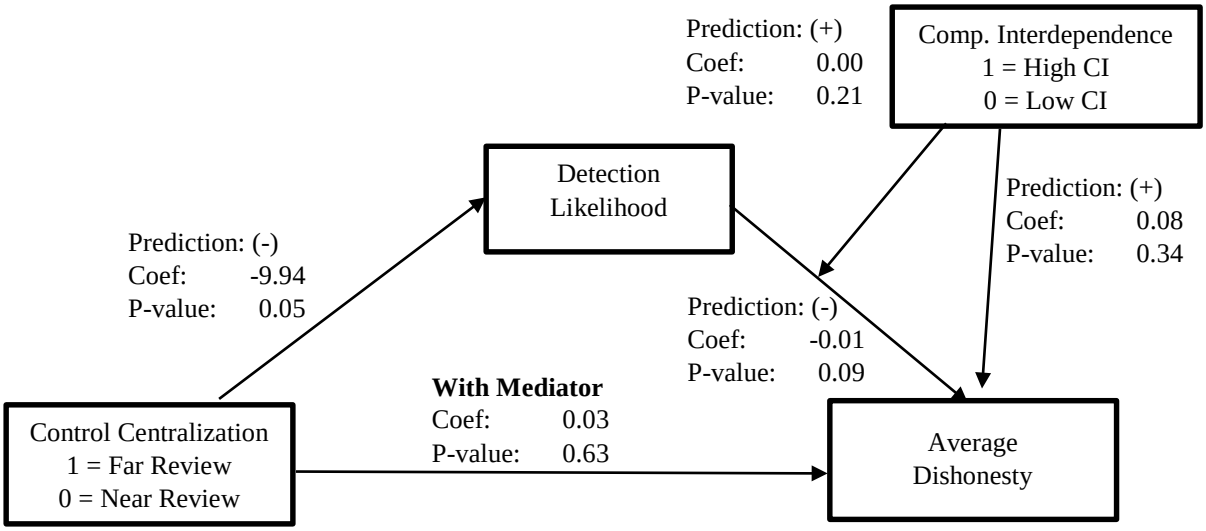


This figure shows the patter of dishonesty across the six rounds of the experiment. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue). The solid blue line is the *High CI* condition and the dotted red line is the *Low CI* condition.

FIGURE 13

Full Experiment: Moderated Mediation Model and Bootstrapping using Detection Likelihood

Panel A: Moderated Mediation Model



Panel B: Bootstrapped Conditional Indirect Effects (CIE)

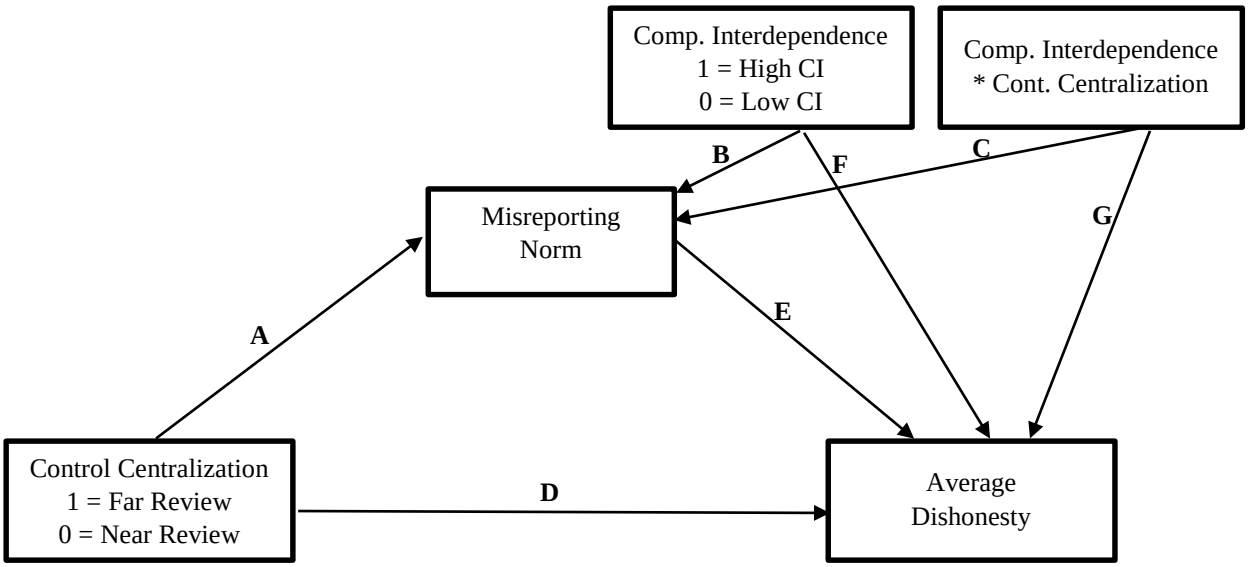
Value of Moderator	Coefficient - CIE	Bootstrap Std. Err.	Z-Value	P > |z|
Mean CI – 1 Std. Deviation	0.03	0.02	1.11	0.27
Mean CI	0.01	0.02	0.85	0.40
Mean CI + 1 Std. Deviation	-0.00	0.02	-0.03	0.98

This figure contains the results of a moderated mediation analysis. Panel A provides the significance of the different links in the model. Panel B provides the significance of the indirect effects as the value of the moderator increases. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Detection Likelihood is participants' response to the post-experiment question: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?* Participants could respond with any value between 0 and 100, inclusive. Compensation Interdependence is an indicator variable coded 1 for the *High CI* condition, zero for the *Low CI* condition. Panel B reveals that as the value of the moderator increases (as compensation becomes more interdependent), the mediation of the indirect effect by Detection Likelihood decreases. The bootstrapped conditional effects were generated using 1,000 replications.

FIGURE 14

Full Experiment: Moderated Mediation Model and Bootstrapping using Misreporting Norm

Panel A: Moderated Mediation Model



Link	Prediction	Coefficient	P-Value	Link	Prediction	Coefficient	P-Value
A	+	1.05	0.03	D	+	0.06	0.36
B	+	1.03	0.04	E	+	0.07	< 0.01
C	-	-0.95	0.15	F	+	0.21	< 0.01
				G	-	-0.13	0.18

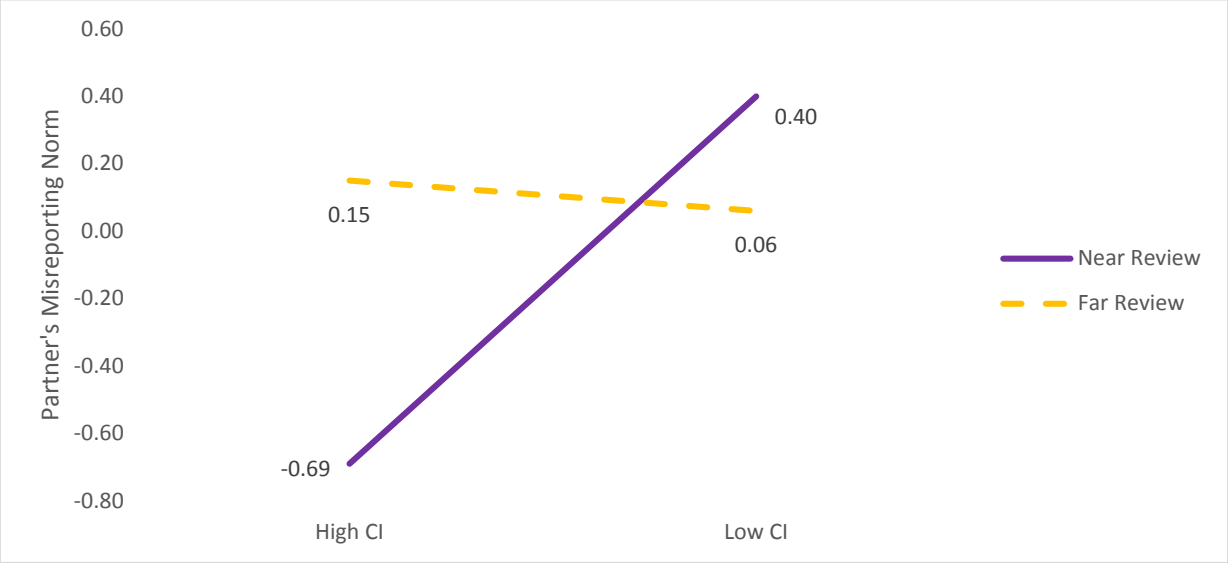
Panel B: Bootstrapped Conditional Indirect Effects (CIE)

Value of Moderator	Coefficient - CIE	Bootstrap Std. Err.	Z-Value	P > |z|
Mean CI – 1 Std. Deviation	0.07	0.04	1.78	0.08
Mean CI	0.04	0.03	1.51	0.13
Mean CI + 1 Std. Deviation	0.01	0.03	0.23	0.82

This figure contains the results of a moderated mediation analysis. Panel A provides the significance of the different links in the model. Panel B provides the significance of the indirect effects as the value of the moderator increases. All p-values are two-tailed. Control Centralization is an indicator variable coded one if a participant was in the *Far Review* condition, zero for the *Near Review* condition. Dishonesty is calculated for each round as (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue) and is averaged across the six rounds. Misreporting Norm is calculated using a principal component analysis on the responses to four post-experiment question: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* (3) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (4) *Across all rounds, by how much do you think an average participant overstated his or her revenue?* Participants responded using a seven point Likert Scale with higher values indicating a higher perception that others were misreporting. Compensation Interdependence is an indicator variable coded 1 for the *High CI* condition, zero for the *Low CI* condition. Panel B reveals that as the value of the moderator increases (as compensation becomes more interdependent), the mediation of the indirect effect by Misreporting Norm decreases. The bootstrapped conditional effects were generated using 1,000 replications.

FIGURE 15

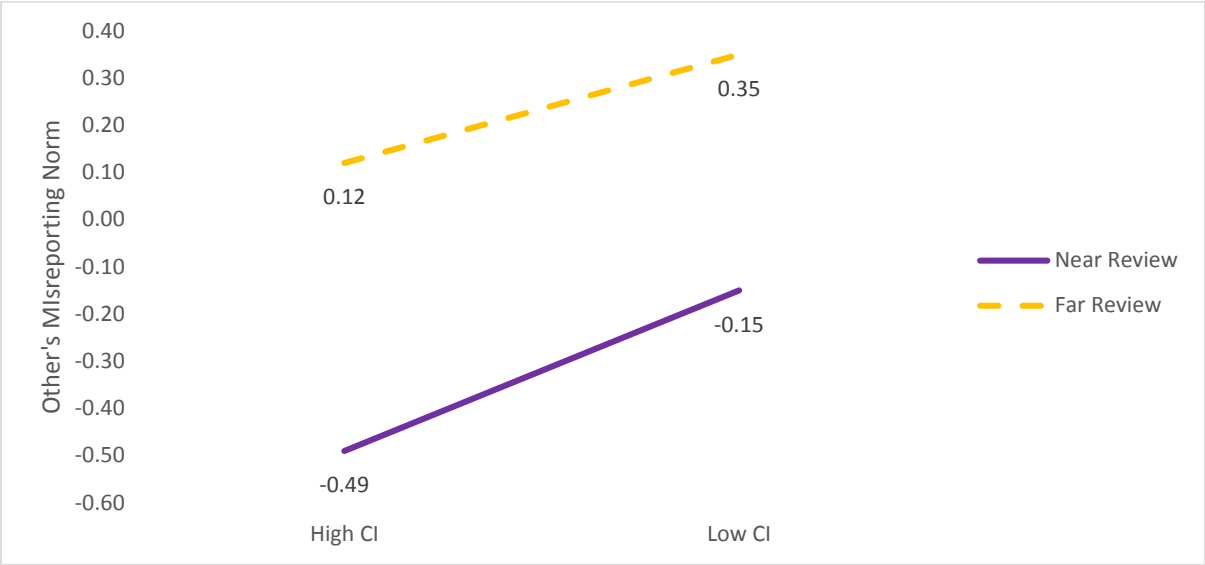
Effect of Control Centralization and Compensation Interdependence on Dishonest Reporting on Partner's Misreporting Norm



This figure provides a graphical representation of the effects of compensation interdependence and control centralization on the perceived norm that a participant's partner was misreporting. Partner's Misreporting Norm is calculated using a principal component analysis on the responses to two post-experiment question: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* Higher scores indicate a higher perception that a participant's partner overstated his or her revenue.

FIGURE 16

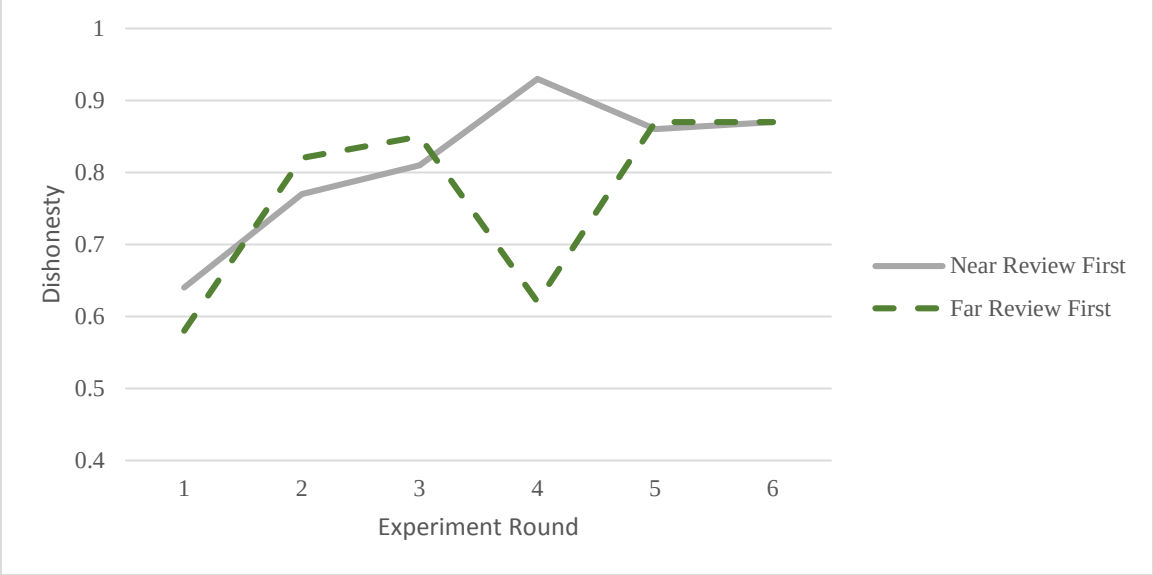
Effect of Control Centralization and Compensation Interdependence on Dishonest Reporting on Other Participant's Misreporting Norm



This figure provides a graphical representation of the effects of compensation interdependence and control centralization on the perceived norm that other participants in the session were misreporting. Other's Reporting Norm is calculated using a principal component analysis on the responses to two post-experiment question: (1) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (2) *Across all rounds, by how much do you think an average participant overstated his or her revenue?* Higher scores indicate a higher perception that an average participant overstated his or her revenue. The instructions specifically indicated that "an average participant" does not include their partner.

FIGURE 17

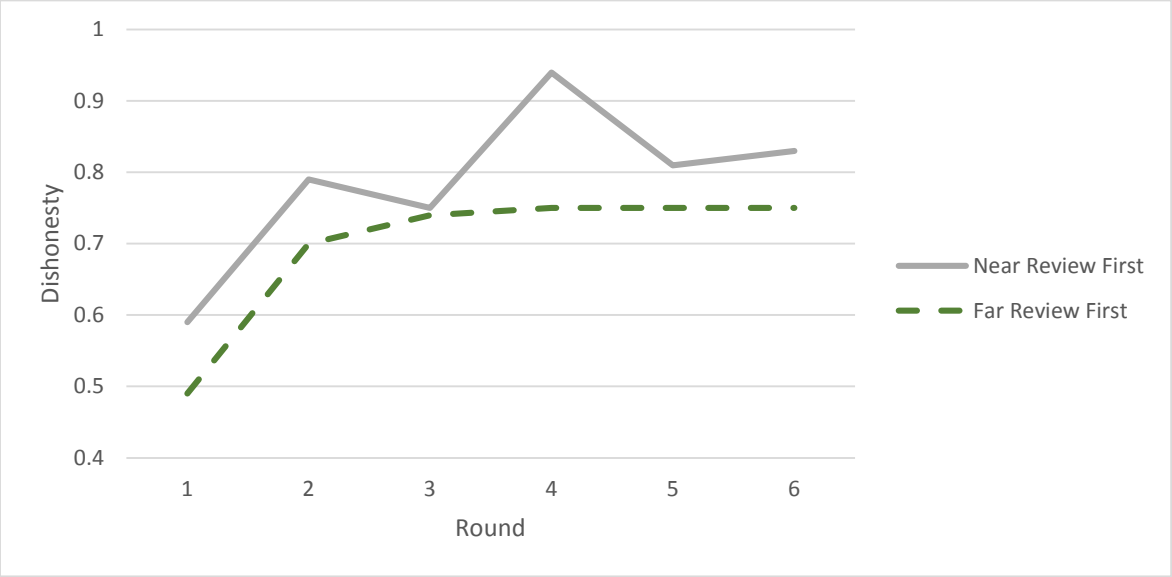
Pilot Experiment #2: Dishonesty across Rounds for Within-Subjects Manipulation



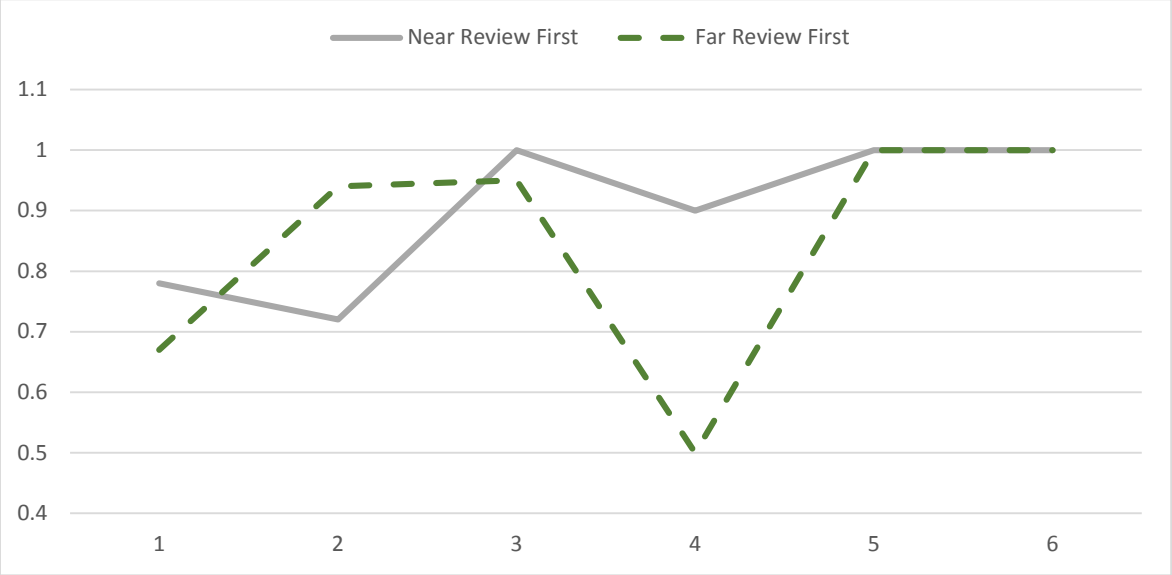
This figure provides a graphical representation of how average dishonest reporting changed across rounds. After the third round, the control centralization within-subjects manipulation was announced. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue).

FIGURE 18
Pilot Experiment #2: Dishonesty across Rounds for Within-Subjects Manipulation Contingent on Compensation Interdependence

Panel A: High Compensation



Panel B: Low Compensation



This figure provides a graphical representation of how average dishonest reporting changed across rounds. After the third round, the control centralization within-subjects manipulation was announced. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue).

Tables

TABLE 1

Descriptive Statistics by Condition – Pilot Experiment

	Control Centralization											
	Near Review				***Far Review***				**TOTAL**			
Low CI	n = 54				n = 108				n = 162			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual Revenue	3.89	1.08	2.01	5.86	3.96	1.13	2.11	5.99	3.94	1.11	2.01	5.99
Reported Revenue	5.70	0.51	4.25	6.00	5.81	0.55	2.31	6.00	5.78	0.54	2.31	6.00
Overstate	1.81	1.32	0.00	3.94	1.85	1.21	0.00	3.89	1.84	1.24	0.00	3.94
Dishonesty	0.69	0.45	0.00	1.00	0.83	0.36	0.00	1.00	0.78	0.39	0.00	1.00
High CI	n = 90				n = 72				n = 162			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual Revenue	4.17	1.12	2.04	5.94	4.01	1.18	2.01	5.94	4.10	1.15	2.01	5.94
Reported Revenue	5.71	0.66	2.63	6.00	5.91	0.20	5.05	6.00	5.80	0.52	2.63	6.00
Overstate	1.54	1.20	0.00	3.89	1.90	1.27	0.00	3.99	1.70	1.24	0.00	3.99
Dishonesty	0.79	0.39	0.00	1.00	0.85	0.35	0.00	1.00	0.82	0.37	0.00	1.00
TOTAL	n = 144				n = 180				n = 324			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual Revenue	4.07	1.11	2.01	5.94	3.98	1.15	2.01	5.99	4.02	1.13	2.01	5.99
Reported Revenue	5.71	0.60	2.63	6.00	5.85	0.45	2.31	6.00	5.79	0.53	2.31	6.00
Overstate	1.64	1.25	0.00	3.94	1.87	1.23	0.00	3.99	1.77	1.24	0.00	3.99
Dishonesty	0.75	0.42	0.00	1.00	0.84	0.35	0.00	1.00	0.80	0.38	0.00	1.00

This table provides summary statistics for each experimental treatment condition. The experiment had two manipulated factors: control centralization (*Near Review/Far Review*) and the level of compensation interdependence (*High CI / Low CI)*. *Actual Revenue* refers to the actual revenue generated from a project that was uniformly distributed between 2 and 6 and was randomly selected each round. *Reported Revenue* is the amount the participant elected to report the project generated each round. It was bounded between the *Actual Revenue* and 6. *Overstate* is *Reported Revenue – Actual Revenue*. *Dishonesty* is *Overstate /* (6 – *Actual Revenue)* and captures the extent to which a participant misreported given the amount they were able to misreport.

TABLE 2

Generalized Least-Squares Estimation of *Dishonesty* – Pilot

Variables	**Model 1**		**Model 2**	
Intercept	2.50***	(0.54)	2.76***	(0.53)
Far Review	0.14*	(0.09)	0.15**	(0.09)
*Far Review * High CI* **(H2)**	-0.19*	(0.13)	-0.20*	(0.13)
High CI	0.17**	(0.10)	0.20**	(0.09)
Age	-0.09***	(0.03)	-0.07***	(0.03)
Actual Revenue			-0.15***	(0.01)
Wald chi-square statistic	14.86***		105.34***	

*, **, *** indicates significance at the 10%, 5%, 1% level, respectively (hypothesized relations one-tailed, all others two-tailed)

This table provides the results of an OLS regression with *Dishonesty* as the dependent variables and indicator variables for each condition as the independent variables. Age is also included as a covariate. All models were estimated assuming panel-specific heteroskedastic errors. Standard errors of the coefficients are reported in parentheses. Dishonesty is calculated as Reported Revenue – Actual Revenue / (Max Revenue – Actual Revenue).

TABLE 3

Descriptive Statistics by Condition Aggregated Across all Rounds

	Control Centralization											
	Near Review				***Far Review***				**TOTAL**			
High CI	n = 108				n = 156				n = 264			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual revenue	4.17	1.11	2.02	5.98	4.06	1.23	2.01	6.00	4.10	1.18	2.01	6.00
Reported revenue	5.36	0.81	2.27	6.00	5.61	0.61	2.71	6.00	5.51	0.71	2.27	6.00
Overstate	1.19	1.33	0.00	3.98	1.55	1.39	0.00	3.99	1.40	1.38	0.00	3.99
Dishonesty	0.48	0.48	0.00	1.00	0.61	0.46	0.00	1.00	0.56	0.47	0.00	1.00
Low CI	n = 108				n = 144				n = 252			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual revenue	3.96	1.17	2.00	5.96	3.81	1.13	2.01	5.96	3.87	1.14	2.00	5.96
Reported revenue	5.72	0.56	3.45	6.00	5.47	0.85	2.36	6.00	5.58	0.75	2.36	6.00
Overstate	1.77	1.29	0.00	3.97	1.66	1.32	0.00	3.99	1.71	1.31	0.00	3.99
Dishonesty	0.76	0.40	0.00	1.00	0.70	0.43	0.00	1.00	0.72	0.42	0.00	1.00
TOTAL	n = 216				n = 300				n = 516			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual revenue	4.06	1.14	2.00	5.98	3.94	1.18	2.01	5.99	3.99	1.17	2.00	6.00
Reported revenue	5.54	0.72	2.27	6.00	5.54	0.74	2.36	6.00	5.54	0.73	2.27	6.00
Overstate	1.48	1.34	0.00	3.98	1.61	1.36	0.00	3.99	1.55	1.35	0.00	3.99
Dishonesty	0.62	0.46	0.00	1.00	0.65	0.45	0.00	1.00	0.64	0.45	0.00	1.00

This table provides summary statistics for each experiment condition. The experiment had two manipulated factors: control centralization (*Near Review/Far Review*) and compensation interdependence (*High CI / Low CI*). Actual revenue refers to the actual revenue from a project that was uniformly distributed between two and six and randomly generated each round. Reported revenue is the amount the participant elected to report the project generated each round. It was bounded between actual revenue and six. Overstate is Reported revenue – Actual revenue. Dishonesty is Overstate / (6 – Actual revenue). It captures the extent to which a participant dishonestly reported, given the amount they were able to misreport.

TABLE 4

Descriptive Statistics by Condition and Round

	Near Review / Low CI (n=18)				***Far Review / Low CI*** (n=26)				***Near Review / High CI*** (n=18)				***Far Review / High CI*** (n=24)			
	Mean	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
Round 1																
Actual revenue	4.03	1.03	2.69	5.96	3.99	1.36	2.11	5.93	3.61	0.97	2.03	5.10	4.27	1.32	2.30	5.96
Dishonesty	0.44	0.44	0.00	1.00	0.69	0.42	0.00	1.00	0.82	0.31	0.00	1.00	0.48	0.47	0.00	1.00
Round 2																
Actual revenue	4.53	0.91	2.74	5.89	4.09	1.24	2.15	5.93	4.22	1.25	2.04	5.95	3.76	1.10	2.07	5.77
Dishonesty	0.36	0.47	0.00	1.00	0.59	0.47	0.00	1.00	0.67	0.45	0.00	1.00	0.78	0.39	0.00	1.00
Round 3																
Actual revenue	3.65	1.19	2.02	5.81	3.91	1.24	2.01	5.97	4.16	1.03	2.00	5.93	3.71	0.88	2.17	4.98
Dishonesty	0.55	0.48	0.00	1.00	0.67	0.42	0.00	1.00	0.77	0.42	0.00	1.00	0.73	0.41	0.00	1.00
Round 4																
Actual revenue	4.07	1.18	2.09	5.84	4.14	1.33	2.07	5.98	3.80	1.20	2.24	5.72	3.67	0.97	2.18	5.80
Dishonesty	0.55	0.51	0.00	1.00	0.53	0.48	0.00	1.00	0.86	0.32	0.00	1.00	0.73	0.40	0.00	1.00
Round 5																
Actual revenue	4.27	1.23	2.25	5.98	4.23	1.08	2.28	6.00	3.90	1.24	2.19	5.56	3.77	1.20	2.03	5.75
Dishonesty	0.54	0.50	0.00	1.00	0.59	0.48	0.00	1.00	0.65	0.48	0.00	1.00	0.68	0.45	0.00	1.00
Round 6																
Actual revenue	4.45	1.02	2.89	5.87	3.97	1.18	2.01	5.99	4.05	1.31	2.10	5.96	3.68	1.22	2.01	5.57
Dishonesty	0.43	0.50	0.00	1.00	0.61	0.49	0.00	1.00	0.76	0.42	0.00	1.00	0.77	0.39	0.00	1.00

This table provides summary statistics for Actual revenue and Dishonesty for each of the six rounds. The experiment had two manipulated factors: control centralization (*Review Near / Review Far*) and compensation interdependence (*High CI / Low CI*). Actual revenue refers to the actual revenue from a project that was uniformly distributed between two and six and randomly generated each round. Dishonesty is (Reported revenue – Actual revenue) / (6 – Actual revenue). It captures the extent to which a participant dishonestly reported, given the amount they were able to misreport.

TABLE 5
ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Perceived Detection Likelihood

Panel A: Average (Std. Errors) Subjective Assessment of Detection Likelihood by Condition

	Near Review	*Far Review*	Overall
Low CI	36.67 (5.43)	26.96 (4.52)	31.81 (3.53)
High CI	31.11 (5.43)	20.67 (4.70)	25.89 (3.59)
Overall	33.89 (3.84)	23.81 (3.26)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	1.38	0.24
Control Centralization **(H1)**	4.00	0.05
Compensation Interdependence * Control Centralization	0.01	0.94

Panel C: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.93	0.34
Effect of Compensation Interdependence given Decentralized Control	0.52	0.47
Effect of Centralization given Low Compensation Interdependence	1.89	0.17
Effect of Centralization given High Compensation Interdependence	2.11	0.15

This table provides the results of an ANOVA with perceived detection likelihood as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. Detection Likelihood is participants' respond to the post-experiment question: S*uppose you were to do the same task with the same review process for six more rounds. What would you assess the likelihood to be that you would be selected for a review at least once in the six rounds?*

TABLE 6
Mixed ANOVA Examining the Effects of Control Centralization and Compensation Interdependence on Dishonesty

Panel A: Average (Std. Errors) Dishonesty by Condition

	Near Review	*Far Review*	Overall
Low CI	0.48 (0.04)	0.61 (0.03)	0.56 (0.02)
High CI	0.76 (0.04)	0.70 (0.03)	0.72 (0.03)
Overall	0.62 (0.03)	0.65 (0.02)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	11.23	< 0.01
Control Centralization	0.48	0.49
Compensation Interdependence * Control Centralization **(H2)**	3.23	0.08

Panel C: Repeated Measure Effects

Source	**F**	**p-value (two-tailed)**
Round	0.58	0.72
Control Centralization * Round	1.00	0.42
Compensation Interdependence * Round	0.76	0.58
Control Centralization * Compensation Interdependence * Round	1.30	0.27

Panel D: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	1.45	0.23
Effect of Compensation Interdependence given Decentralized Control	11.45	< 0.01
Effect of Centralization given Low Compensation Interdependence	3.16	0.08
Effect of Centralization given High Compensation Interdependence	0.60	0.44

This table provides the results of a mixed ANOVA with Dishonesty as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. I use a mixed model to correct for correlation resulting from multiple decisions from each participant. Dishonesty is calculated (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue).

TABLE 7
Mixed ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Dishonesty – Actual Revenue Control

Panel A: Average (Std. Errors) Dishonesty by Condition

	Near Review	*Far Review*	Overall
Low CI	0.51 (0.03)	0.63 (0.03)	0.58 (0.02)
High CI	0.75 (0.03)	0.66 (0.03)	0.70 (0.02)
Overall	0.63 (0.02)	0.64 (0.02)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Actual Revenue	64.92	< 0.01
Compensation Interdependence	6.86	0.01
Control Centralization	0.05	0.82
Compensation Interdependence * Control Centralization	3.84	0.05

Panel C: Repeated Measure Effects

Source	**F**	**p-value (two-tailed)**
Round	0.32	0.90
Control Centralization * Round	0.48	0.79
Compensation Interdependence * Round	0.89	0.49
Control Centralization * Compensation Interdependence * Round	1.00	0.42

Panel D: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.28	0.59
Effect of Compensation Interdependence given Decentralized Control	9.10	< 0.01
Effect of Centralization given Low Compensation Interdependence	1.47	0.22
Effect of Centralization given High Compensation Interdependence	2.42	0.12

This table provides the results of a mixed ANOVA with Dishonesty as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. I include actual revenue as a covariate in the ANOVA model. I use a mixed model to correct for correlation resulting from multiple decisions from each participant. Dishonesty is calculated (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue).

TABLE 8
Mixed ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Dishonesty

Panel A: Average (Std. Errors) Dishonesty by Condition

	Near Review	*Far Review*	Overall
Low CI	0.59 (0.04)	0.70 (0.04)	0.65 (0.03)
High CI	0.79 (0.05)	0.68 (0.04)	0.74 (0.04)
Overall	0.69 (0.03)	0.69 (0.03)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	1.87	0.17
Control Centralization	0.02	0.99
Compensation Interdependence * Control Centralization	2.72	0.10

Panel C: Repeated Measure Effects

Source	**F**	**p-value (two-tailed)**
Round	1.30	0.26
Control Centralization * Round	0.90	0.48
Compensation Interdependence * Round	0.75	0.59
Control Centralization * Compensation Interdependence * Round	1.44	0.21

Panel D: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.01	0.97
Effect of Compensation Interdependence given Decentralized Control	5.77	0.02
Effect of Centralization given Low Compensation Interdependence	1.18	0.28
Effect of Centralization given High Compensation Interdependence	2.17	0.14

This table provides the results of a mixed ANOVA with Dishonesty as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. I include actual revenue as a covariate in the ANOVA model. I use a mixed model to correct for correlation resulting from multiple decisions from each participant. Dishonesty is calculated (Reported Revenue – Actual Revenue) / (Max Revenue – Actual Revenue). This analysis excludes all observations with actual revenue < $2.50 and > $5.50

TABLE 9
Mixed ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Overstate

Panel A: Average (Std. Errors) Overstate by Condition

	Near Review	*Far Review*	Overall
Low CI	1.19 (0.12)	1.55 (0.10)	1.40 (0.08)
High CI	1.77 (0.12)	1.66 (0.11)	1.71 (0.08)
Overall	1.48 (0.09)	1.61 (0.07)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	5.80	0.02
Control Centralization	0.85	0.36
Compensation Interdependence * Control Centralization	2.68	0.11

Panel C: Repeated Measure Effects

Source	**F**	**p-value (two-tailed)**
Round	0.84	0.52
Control Centralization * Round	1.14	0.34
Compensation Interdependence * Round	0.68	0.64
Control Centralization * Compensation Interdependence * Round	1.00	0.42

Panel D: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.55	0.46
Effect of Compensation Interdependence given Decentralized Control	10.77	0.01
Effect of Centralization given Low Compensation Interdependence	5.07	0.02
Effect of Centralization given High Compensation Interdependence	0.39	0.53

This table provides the results of a mixed ANOVA with Overstate as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. I use a mixed model to correct for correlation resulting from multiple decisions from each participant. Overstate is calculated (Reported Revenue – Actual Revenue).

TABLE 10

Logistic Regression Examining the Effect of Control Centralization and Compensation Interdependence on Misreport

Panel A: Percentage of Misreporting per opportunity by Condition

	Near Review	*Far Review*	Overall
Low CI	56/108	104/156	160/264
	51.9%	66.7%	60.6%
High CI	86/108	110/144	196/252
	79.6%	76.4%	77.8%
Overall	142/216	214/300	
	65.7%	71.3%	

Panel B: Main Effects

Source	**Z**	**p-value (two-tailed)**
Compensation Interdependence	3.24	< 0.01
Control Centralization	1.88	0.06
Compensation Interdependence * Control Centralization	-1.61	0.11

This table provides the results of a logistic regression with Misreport as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. The model is estimated assuming panel-specific heteroskedastic errors to correct for correlation among participants. Misreport is coded one if a participant's reported revenue was greater than actual revenue and zero otherwise.

TABLE 11
Red Chips Drawn across Conditions and Rounds

	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Far Review	3/50	6/50	8/50	3/50	6/50	4.50
	6.0%	12.0%	16.0%	6.0%	12.0%	8.0%
Near Review	4/36	2/36	1/36	5/36	1/36	7/36
	11.1%	5.6%	2.8%	13.9%	2.8%	19.4%
Chi-Square	0.73	1.03	3.91	1.54	2.38	2.46
Pr > Chi-Square	0.39	0.31	0.05	0.21	0.12	0.12

This table provides the number of red poker chips drawn across rounds for participants in the *Far Review* and *Near Review* conditions. The results of an odds ratio test are provided in the bottom two lines. This examines whether the two percentage across the two condition significantly differs across the rounds.

TABLE 12
ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Perceived Partner's Misreporting Norm

Panel A: Average (Std. Errors) Perceived Partner's Misreporting Norm by Condition

	Near Review	*Far Review*	Overall
Low CI	-0.69 (0.30)	0.15 (0.25)	-0.27 (0.19)
High CI	0.40 (0.30)	0.06 (0.26)	0.23 (0.20)
Overall	-0.14 (0.10)	0.10 (0.18)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	3.37	0.07
Control Centralization	0.81	0.37
Compensation Interdependence * Control Centralization	4.70	0.03

Panel C: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.07	0.80
Effect of Compensation Interdependence given Decentralized Control	6.90	0.01
Effect of Centralization given Low Compensation Interdependence	4.78	0.03
Effect of Centralization given High Compensation Interdependence	0.79	0.38

This table provides the results of an ANOVA with perceived partner's misreporting norm as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. Perceived partner's misreporting norm is participants' respond to the two post-experiment questions: (1) *In how many rounds do you think your partner overstated his or her revenue?* (2) *Across all rounds, by how much do you think your partner overstated his or her revenue?* I run a principal component analysis on the responses to these two questions. Higher scores indicate a higher perception that a participant's partner overstated his or her revenue.

TABLE 13

ANOVA Examining the Effect of Control Centralization and Compensation Interdependence on Perceived Other Participant's Misreporting Norm

Panel A: Average (Std. Errors) Perceived Other Participant's Misreporting Norm by Condition

	Near Review	*Far Review*	Overall
Low CI	-0.49 (0.30)	0.12 (0.25)	-0.18 (0.19)
High CI	-0.15 (0.30)	0.35 (0.26)	0.10 (0.20)
Overall	-0.32 (0.21)	0.24 (0.18)	

Panel B: Main Effects

Source	**F**	**p-value (two-tailed)**
Compensation Interdependence	1.06	0.31
Control Centralization	4.09	0.05
Compensation Interdependence * Control Centralization	0.04	0.84

Panel C: Simple Effects

Source	**F**	**p-value (two-tailed)**
Effect of Compensation Interdependence given Centralized Control	0.41	0.52
Effect of Compensation Interdependence given Decentralized Control	0.65	0.42
Effect of Centralization given Low Compensation Interdependence	2.52	0.12
Effect of Centralization given High Compensation Interdependence	1.63	0.21

This table provides the results of an ANOVA with perceived other participant's misreporting norm as the dependent variable and compensation interdependence (*CI Low / CI High)* and control centralization (*Far Review / Near Review)* as the independent variables. Perceived other participant's misreporting norm is participants' responses to the two post-experiment questions: (1) *In how many rounds do you think an average participant in this session overstated his or her revenue?* (2) *Across all rounds, by how much do you think an average participant overstated his or her revenue?* I run a principal component analysis on the responses to these two questions. Higher scores indicate a higher perception that a participant's partner overstated his or her revenue. The instructions specifically indicated that "an average participant" does not include their partner.

TABLE 14

Descriptive Statistics by Within Manipulation Conditions

Control Design

	Review Near First (Rounds 1-3)				***Review Near First (Rounds 4-6)***				**TOTAL**			
	n = 8; obs = 24				n = 8; obs = 24				n = 16; obs = 48			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual Revenue	4.16	1.13	2.01	5.84	4.15	1.00	2.15	5.54	4.16	1.06	2.01	5.84
Reported Revenue	5.71	0.49	4.45	6.00	5.84	0.35	4.83	6.00	5.78	0.43	4.45	6.00
Overstate	1.55	1.22	0.00	3.99	1.69	1.06	0.00	3.70	1.62	1.14	0.00	3.99
Dishonesty	0.74	0.38	0.00	1.00	0.89	0.28	0.00	1.00	0.81	0.34	0.00	1.00
	Review Far First (Rounds 1-3)				***Review Far First (Rounds 4-6)***				**TOTAL**			
	n = 8; obs = 24				n = 8; obs = 24				n = 8; obs = 24			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Actual Revenue	4.04	1.03	2.18	5.93	3.99	1.13	2.10	5.79	4.01	1.07	2.10	5.93
Reported Revenue	5.50	0.88	3.00	6.00	5.88	0.29	4.85	6.00	5.69	0.67	3.00	6.00
Overstate	1.46	1.16	0.00	3.69	1.89	1.29	0.00	3.89	1.68	1.23	0.00	3.89
Dishonesty	0.75	0.37	0.00	1.00	0.79	0.41	0.00	1.00	0.77	0.39	0.00	1.00

This table provides summary statistics for the within-subjects manipulation of control design. The top (bottom) three quadrants contain summary statistics for the condition where the control was first centralized (decentralized) and then decentralized (centralized). Participants completed the task for six rounds with the manipulation coming after the third round. As such, *n* refers to the number of participants and *obs* refers to the number of observation taking into account multiple observations for a single participant. *Actual Revenue* refers to the actual revenue generated from a project that was uniformly distributed between 2 and 6 and was randomly selected each round. *Reported Revenue* is the amount the participant elected to report the project generated each round. It was bounded between the *Actual Revenue* and 6. *Overstate* is *Reported Revenue – Actual Revenue*. *Dishonesty* is *Overstate /* (6 – *Actual Revenue)* and captures the extent to which a participant dishonestly reported given the amount they were able to misreport

TABLE 15

Mean (Std. Deviation) of Variables across Rounds in Within Conditions

Near Review First

	Round 1	**Round 2**	**Round 3**	**Round 4**	**Round 5**	**Round 6**
	4.05	4.27	4.17	3.61	4.36	4.48
Actual Revenue	(1.33)	(0.76)	(1.36)	(1.07)	(0.82)	(0.98)
	5.55	5.72	5.87	5.82	5.83	5.86
Reported Revenue	(0.63)	(0.50)	(0.30)	(0.32)	(0.41)	(0.37)
	1.50	1.45	1.70	2.21	1.48	1.38
Overstate	(1.36)	(0.99)	(1.43)	(1.00)	(1.01)	(1.11)
	0.64	0.77	0.81	0.93	0.86	0.87
Dishonesty	(0.43)	(0.37)	(0.37)	(0.12)	(0.35)	(0.35)

Far Review First

	Round 1	**Round 2**	**Round 3**	**Round 4**	**Round 5**	**Round 6**
	3.77	4.53	3.84	4.23	3.60	4.13
Actual Revenue	(1.21)	(0.70)	(1.06)	(1.34)	(1.20)	(0.81)
	5.19	5.76	5.56	5.75	5.97	5.91
Reported Revenue	(0.97)	(0.54)	(1.05)	(0.42)	(0.07)	(0.24)
	1.42	1.23	1.72	1.52	2.38	1.78
Overstate	(1.45)	(0.87)	(1.20)	(1.57)	(1.26)	(0.97)
	0.58	0.82	0.85	0.62	0.87	0.87
Dishonesty	(0.40)	(0.34)	(0.35)	(0.52)	(0.35)	(0.35)

This table provides summary statistics for the within-subjects manipulation of control design. The top (bottom) portion of the table contain summary statistics for the condition where the control was first centralized (decentralized) and then decentralized (centralized). Participants completed the task for six rounds with the manipulation coming after the third round. *Actual Revenue* refers to the actual revenue generated from a project that was uniformly distributed between 2 and 6 and was randomly selected each round. *Reported Revenue* is the amount the participant elected to report the project generated each round. It was bounded between the *Actual Revenue* and 6. *Overstate* is *Reported Revenue – Actual Revenue*. *Dishonesty* is *Overstate /* (6 – *Actual Revenue)* and captures the extent to which a participant dishonestly reported given the amount they were able to misreport.

TABLE 16

Mean (Std. Deviation) of Variables across Rounds in Within Conditions

PANEL A: *High CI Condition*

	Near Review First					
	Round 1	**Round 2**	**Round 3**	**Round 4**	**Round 5**	**Round 6**
Dishonesty	0.59	0.79	0.75	0.94	0.81	0.83
	Far Review First					
Dishonesty	0.49	0.70	0.74	0.75	0.75	0.75

PANEL B: *Low CI Condition*

	Near Review First					
	Round 1	**Round 2**	**Round 3**	**Round 4**	**Round 5**	**Round 6**
Dishonesty	0.78	0.72	1.00	0.90	1.00	1.00
	Far Review First					
Dishonesty	0.67	0.94	0.95	0.50	1.00	1.00

This table provides Average Dishonesty for the within-subjects manipulation of control design. Panel A provides average dishonesty for each round for participants in the *High CI* condition. Panel B provides average dishonesty for each round for participants in the *Low CI* condition. Participants completed the task for six rounds with the manipulation coming after the third round. *Dishonesty* is *(Reported Revenue – Actual Revenue) /* (6 – *Actual Revenue)* and captures the extent to which a participant dishonestly reported given the amount they were able to misreport