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**The Effects of Auditor Rotation, Professional Skepticism, and  
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# **The Effects of Auditor Rotation, Professional Skepticism, and Interactions with Managers on Audit Quality**

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# The Effects of Auditor Rotation, Professional Skepticism, and Interactions with Managers on Audit Quality

## ABSTRACT

We examine whether the effect of mandatory auditor rotation on audit quality depends on the mental frame auditors adopt in evaluating management representations. In practice, auditors can alternately frame their assessments of management representations in terms of their potential dishonesty (what we term skepticism) or potential honesty. Using psychology theory and a laboratory experiment, we predict and find that mandatory rotation improves audit quality when an auditor takes an honesty frame, but that this effect reverses when an auditor takes a skeptical frame. Thus, the benefit of using a skeptical frame occurs when auditors do *not* rotate, but requiring rotation can *reduce* audit effort for auditors using a skeptical frame. An implication of our study is that focusing auditors on a skeptical assessment frame rather than mandating auditor rotation may be a less costly way to reduce low-effort audits and aggressive reporting.

Key Words: Auditor Rotation; Professional Skepticism; Audit Quality; Game Theory

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## I. INTRODUCTION

We test whether the effects of auditor rotation on audit quality depend upon the mental frame (Hanson 2011) with which auditors evaluate either the honesty or dishonesty of management representations about the financial statements. In particular, we explore whether auditor rotation and assessment frame interact, leading to low-effort audit and aggressive reporting.

In recent years, standard setters have proposed and implemented various forms of mandatory auditor rotation. For example, the SEC requires rotation of audit engagement and concurring review partners (SEC 2003), and the European Union (EU) directs that key audit partners rotate (Commission of the European Communities 2006). Recently, both the U.S. and Europe have revisited the recurring, controversial topic of audit firm rotation, with the EU, but not the U.S., moving forward with adoption of firm rotation. While opponents suggest that the loss of experience with the audit client due to rotation reduces audit quality (e.g., Myers, Myers, and Omer 2003; PwC 2011; AICPA 2011b), the PCAOB and other proponents argue that existing rotation requirements in the U.S. are insufficient and that firm rotation requirements will enhance audit quality and professional skepticism (e.g., PCAOB 2011a; PCAOB 2011b, footnote 2; Hall 2011).

Proponents of auditor rotation may not take into account that the auditor's frame when assessing management representations can vary among auditors, audit teams, audit firms, audits, and parts of audits (e.g., COSO 1992; Peecher 1996; AICPA 2007; Peecher, Piercey, Rich, and Tubbs 2010). Auditors can frame their assessments in terms of managers' potential *honesty* (e.g., client integrity assessments, COSO 1992; AICPA 2007; PCAOB 2007), or potential *dishonesty*

(e.g., fraud risk assessments, AICPA 2011a). Psychology theory suggests that the auditor's frame could interact with auditor rotation.

While auditing standards traditionally described professional skepticism as an attitude that includes an unbiased questioning mind (AICPA 2011c), regulators increasingly (though not uniformly) advocate for and characterize skepticism from a presumptive doubt perspective, in which auditors generally focus on the possibility of management dishonesty (Panel on Audit Effectiveness, PAE, 2008; PCAOB 2008; Nelson 2009; PCAOB 2011b; Doty 2012). This is a perspective observed in audit practice (Quadackers, Groot, and Wright 2014). Thus, we use the term "skeptical" to describe cases in which auditors assess management representations in terms of their potential dishonesty (see Nelson 2009, 4).<sup>1</sup>

We contribute to the literature by directly examining the interactive effects of auditor assessment frame and auditor rotation on audit quality. Support Theory from psychology suggests that an honesty (dishonesty) assessment frame will lead auditors to overestimate manager honesty (dishonesty), provided that auditors perceive themselves as competent to evaluate managers in this way. However, these effects could diminish and even reverse if rotation undermines auditors' comfort assessing managers' honesty (dishonesty). Therefore, we predict that when auditors assess the honesty of management representations, auditor rotation will increase audit effort and decrease the frequency of low-effort audits paired with aggressive financial reporting. On the other hand, when auditors assess the dishonesty of management representations, i.e., exercise skepticism, auditor rotation likely weakens the auditor's belief that management representations are *dishonest*, thereby *reducing* audit quality.

Building upon and extending theories from psychology and economics, and drawing on prior research in accounting (King 2002; Bowlin, Hales, Kachelmeier 2009), we use an

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<sup>1</sup> Accordingly, we use the phrases skeptical frame and dishonesty frame interchangeably.

experiment to examine the effects of auditor rotation and assessment frame in a game theoretic, strategic setting (e.g., Fellingham and Newman 1985; Newman, Patterson and Smith 2005). Our experiment follows the principles of experimental economics (e.g., Davis and Holt 1993; Friedman and Sunder 1994; Kagel and Roth 1995; Smith 2003), which is an ideal method to examine auditor rotation, because participants assuming the role of auditors and managers can interact repeatedly in a real microeconomic world that captures the key economic and strategic forces at play in the natural setting (Plott 1982, 1492; Smith 1982, 923). This approach maximizes internal validity while controlling for confounding and nonessential factors by using random assignment, inexperienced subjects, and non-contextually rich language.<sup>2</sup> We then rely on economic and psychology theory to generalize the results of our experiment to the natural setting.

In our experiment, a participant in the manager role chooses a level of financial reporting aggressiveness and makes a representation about that choice to a participant in the auditor role. The auditor assesses the honesty or dishonesty of the management assertion and chooses a level of audit effort. Consistent with prior auditing studies adopting similar methods, our design includes an incentive structure intended to parallel those encountered in real-world auditing interactions (e.g. King 2002; Fischbacher and Stefani 2007; Bowlin et al. 2009; and Bowlin 2011). For example, auditors prefer low-effort and low-cost audits, but only if managers are unlikely to choose aggressive financial reporting. However, managers prefer aggressive reporting when auditors conduct low-effort audits, but prefer conservative reporting when auditors are more diligent.

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<sup>2</sup> As with all experiments, to the extent that our setting does not incorporate some aspects of the natural environment, future research can expand on our contribution by considering how these other factors might interact with our findings outside of our theory to alter our results (Friedman and Sunder 1994, 16; Kachelmeier and King 2002; Libby, Bloomfield, and Nelson 2002, 795; Smith 1982, 937).

We manipulate three variables between subjects. First, we manipulate auditor rotation by requiring auditors to interact with the same or a different manager each round. We manipulate auditor assessment frame by having auditors assess the veracity of management representations as either the probability that managers are honest or the probability that managers are dishonest. Finally, as a robustness test, we manipulate whether auditors and managers are allowed to engage in interpersonal interaction by allowing half of the auditor-manager pairs to chat via text messages in an informal, yet controlled, environment.

Consistent with our predictions, we find that the effect of auditor rotation on audit quality depends on auditors' assessment frame. Specifically, when auditors assess the honesty of management representations (i.e., a client integrity frame), auditor rotation increases audit effort and decreases the frequency of low-effort audits paired with aggressive financial reporting, a pairing that increases the likelihood of audit failure (Peecher and Piercey 2008). However, when auditors assess the dishonesty of management representations (i.e., a skepticism frame), auditor rotation *decreases* audit effort and *increases* low-effort audits paired with aggressive reporting. Additional analysis demonstrates that auditors' assessment of management representations mediates audit effort, and ultimately the joint outcome of audit effort and aggressive reporting. These findings suggest that a skeptical, presumptive doubt auditor assessment frame (Nelson 2009; Quadackers et al. 2014) could actually lead to *increased* audit failure under mandatory auditor rotation.

We also find that increasing the level of interpersonal interaction between auditors and managers via informal chatting decreases audit effort but does not interact with our other independent variables. This suggests that our main findings could generalize to auditors who



have close interactions with managers (e.g., engagement partners), as well as those who do not (e.g., concurring partners and other audit team members).

Our findings have implications for theory and practice. By examining the joint effects of assessment frame and rotation, we provide evidence that the effects of auditor rotation on audit quality can depend on whether auditors assess management assertions through an honesty or skeptical frame. This suggests that the expected benefits of auditor rotation mandates could come at the cost of offsetting expected benefits of professional skepticism standards. Similarly, auditors should be aware of how these factors influence their judgment and decision-making (see Bell, Peecher, and Solomon 2005). For example, newly rotated auditors should be extra vigilant in fraud planning and procedures, perhaps focusing on best practices of high-quality fraud brainstorming (e.g., Brazel, Carpenter, and Jenkins 2010), and on falsifying (rather than verifying) management assertions during the audit (PAE 2008; Doty 2012).

These findings extend prior experimental and archival literature on auditor rotation and skepticism. We contribute to the experimental literature on rotation by focusing on unintentional under-auditing and auditors' assessments of management representations and choices of audit effort levels that occur prior to auditors' reporting decisions (Dopuch, King, and Schwartz 2001). We use an experiment to eliminate self-selection bias inherent in archival research on audit rotation. Finally, we show that the presumptive doubt perspective of auditor skepticism found to be prevalent in prior research (Quadackers et al. 2014) is potentially harmful when auditors rotate.

## II. THEORY

### Background Research

Over the past decade, a large archival literature has studied auditor rotation, primarily by testing whether proxies for earnings quality or audit quality improve or deteriorate with long-term auditor-client relationships. Reviews of this literature (e.g., Cameran, Prencipe, and Trombetta 2008) describe mixed results.<sup>3</sup> Myers et al. (2003), Cameran et al. (2008) and the PCAOB (2011b) note that self-selection bias limits the extent to which archival findings can address the effects of auditor rotation on audit quality. Using an experiment, Dopuch, King, and Schwartz (2001, 98) find that rotation discourages auditors from intentionally biasing their audit opinions in favor of management, despite incentives to compromise their independence. Wang and Tuttle (2009) examine the effect of rotation on auditor-manager negotiations. These studies call for future research to examine other aspects of auditor rotation.

### The Auditor-Manager Relationship

We examine the auditor-manager relationship as a strategic game between individuals, consistent with prior experimental and analytical research (e.g., Fellingham and Newman 1985; Kachelmeier 1991; Newman, Rhoades, and Smith 1996; King 2002; Mayhew and Pike 2004; Bowlin et al. 2009; Bowlin 2011). This relationship includes a key tension that mandatory rotation would presumably help alleviate: managers can benefit from aggressive financial reporting, but only if they can convince auditors to engage in low audit effort, while auditors can benefit from low audit effort, but only if managers engage in conservative financial reporting (e.g., King 2002; Bowlin et al. 2009).

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<sup>3</sup> For example, Myers et al. (2003), Mansi et al. (2004), Ghosh and Moon (2005), Chen, Lin, and Lin (2008), Gul et al. (2009), and Rice and Weber (2012) find negative effects of rotation on audit quality. Carey and Simnett (2006), Kealy, Yee, and Stein (2007), Dao, Mishra, and Raghunandan (2008), and Davis, Soo, and Trompeter (2009) find positive effects. Ghosh and Moon (2005), Knechel and Vanstraelen (2007), Chen et al. (2008), Chi, Huang, Liao, and Xie (2009), and Ruiz-Barbadillo, Gomez-Aguilar, and Carrera (2009) find no or mixed effects.

Auditors plan their costly effort based, in part, on management *representations* about financial reporting quality, such as misstatement risk in an account or explanations for unusual fluctuations observed during analytical review (Aghazadeh 2013). However, management representations are not necessarily honest (Dichev, Graham, Harvey, and Rajgopal 2013). Auditors can *assess* the honesty or dishonesty of management representations, but the audit environment provides auditors imperfect feedback about whether those assessments are accurate (King 2002). For example, auditors know when they have detected misstatements, but they generally do not know (and may never know) the private financial reporting decisions of their clients or whether there were undetected material misstatements (Peecher and Piercey 2008).

### **Auditors' Assessments of Management Representations**

Auditors can frame their assessments of management representations in terms of either their potential honesty or their potential dishonesty (Quadackers et al. 2014). These alternative frames may arise formally (e.g., COSO 1992; AICPA 2011a) or informally on different audit tasks.<sup>4</sup> Standard setters have recently noted that auditors often focus on verifying the honesty of management representations, and have encouraged auditors instead to evaluate them more skeptically in terms of their potential dishonesty (e.g., AICPA 2011a; PCAOB 2011b; and Doty 2012).

According to Support Theory in psychology (Tversky and Koehler 1994; Rottenstreich and Tversky 1997; and Brenner, Koehler, and Rottenstreich 2002; Brenner 2003), individuals do not make subjective probability assessments based on normative laws of probability, but instead base those assessments on the amount of subjective psychological support that comes to mind.

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<sup>4</sup> This is consistent with our conversations with partners and other auditors. For example, audit teams will often refer to their knowledge of and experience with management's integrity when deciding how much to rely on managers' explanations for unexpected fluctuations observed in analytical review. Auditors then may adopt a more skeptical mindset when participating in a fraud brainstorming session.

The Support Theory literature also finds that the ease with which such support comes to mind depends on how the probability assessment is framed. For example, individuals asked to assess the likelihood that they will be injured within the next year can easily retrieve mental support for this risk. As a result, they evaluate the subjective likelihood of injury in their minds and do not adequately consider the alternative complementary probability that they might *not* be injured. In contrast, asking individuals to assess the likelihood of an injury-free year leads them to *overstate* that possibility and inadequately consider the alternative, that they could be injured. Thus, individuals' subjective probability assessments of an uncertainty depend on how the question is framed. Accordingly, researchers commonly gather evidence consistent with Support Theory by finding that the two, separately assessed complementary probabilities exceed 100% (see, e.g., Ayton 1997 and Brenner et al. 2002 for reviews).

In our setting, Support Theory predicts that auditors assessing the probability of the honesty (dishonesty) of management representations will focus on the possibility of honesty (dishonesty), and will retrieve psychological support for it relatively easily. Consequently, these auditors will assess honesty (dishonesty) to be more likely and will, therefore, select low-effort (high-effort) audits more frequently. This can lead to judgment biases, because honesty and dishonesty assessments are two sides of the same coin (COSO 1992), and probability assessments of management honesty or dishonesty should be independent of the question's mental framing. Additionally, the features of a long-term auditor-client relationship further exacerbate such biases because extended, non-rotating experience with the client makes it even easier for auditors to retrieve psychological support for their mental frame. Thus, a traditional, non-rotating relationship would tend to magnify the tendency of auditors with an honesty frame to overstate the likelihood that a particular representation might be true, while also magnifying

the tendency of auditors with a skepticism frame to overstate the likelihood that a particular representation might not be true.

***Mandatory Auditor Rotation and Honesty versus Dishonesty Assessments: A Reverse Effect***

We suggest that the effect of auditors adopting an honesty or dishonesty frame on their assessments of management representations depends on auditor rotation in ways that neither auditors nor standard setters would likely anticipate. Specifically, while a *non-rotating* auditor/client relationship could exacerbate the effect of the auditor's mental frame on his or her assessments of management representations (as discussed above), further research in the Support Theory literature suggests that *rotating* auditors could actually diminish, or even reverse, this effect.

First, while the basic predictions of Support Theory were tested in settings familiar to participants, Macchi, Osherson, and Krantz (1999) theorized that the traditional findings of Support Theory would reverse in settings where individuals had *low* knowledge of the subject of the probability assessment. Their participants' assessments of two complementary probabilities often summed to *less than* 100% (in contrast to the overstated sum of greater than 100% in prior studies). Macchi et al. (1999, 213) attributed this behavior to participants' "low level of knowledge with the questions posed," leading to relative difficulty conjuring psychological support for the frame of the probability they considered. Idson, Krantz, Osherson, and Bonini (2001) extended this work by directly *manipulating* whether the subject matter being assessed was one about which participants would likely feel knowledgeable or unknowledgeable. Their findings suggest that the alternative complementary probability assessments sum to greater (less) than 100% when individuals feel knowledgeable (unknowledgeable) about the subject of the probability assessment.

Kilka and Weber (2001) also found a reversal effect and, importantly, showed that it influences not only probability assessments but also *incentivized decisions*. Participants judged complementary probabilities and placed bets on whether a company's stock returns would be inside or outside of a given range. Participants overweighted *both* of the complementary probabilities when evaluating a domestic stock, and underweighted *both* when evaluating a foreign stock. Because actual competence predicting any stock returns should be low across participants, Kilka and Weber (2001, 1713) attribute their findings to “the effect of a decision maker's *perceived* competence in evaluating the source of uncertainty” (emphasis added).

Similarly, Fox and Weber (2002) show directly that *perceived* (as opposed to *actual*) competence with the subject matter is sufficient to achieve this reversal effect. Specifically, participants placed bets on complementary events related to San Francisco weather, after being made to *feel* (but not actually become) either more or less competent with the subject matter. Participants bet more (less) on both an event and on its complement when they *felt* more (less) competent. The role of *perceived* competence suggests that the auditor does not need in-depth or long-term experience with the manager for our effects to occur. Indeed, we expect the effects to occur quickly and to persist over time. Research in Support Theory indicates that these effects are primitive, fundamental, and unconscious judgmental phenomena that persist under a variety of conditions (see Brenner et al. 2002).

The findings described above have potentially important implications for our setting. We posit that auditor rotation is likely to create a reverse effect similar to those found in the studies discussed above when auditors feel relatively unfamiliar with, and therefore less competent to evaluate, the probability that management representations are honest or dishonest.<sup>5</sup> A manager's

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<sup>5</sup> Similar reverse effects are found in other, related theories. For example, the “above average effect,” overestimating the probability that you are better than others (Brenner et al. 2002; Windschitl, Rose, Stalkfleet, and Smith 2008),

honesty or dishonesty is a private, latent characteristic about which auditors rarely receive clear feedback. Rotating auditors (aware that they will not be in a long-term relationship) will, as in the studies discussed above, likely perceive themselves to be less competent in evaluating the honesty or dishonesty of the manager relative to auditors who do not rotate. Thus, when assessing the probability of manager honesty, *rotating* auditors would likely find it more difficult to garner psychological support for the probability of manager honesty, leading them to be *less* likely to choose low levels of audit effort than non-rotating auditors. Similarly, when assessing the probability of manager dishonesty, *rotating* auditors would find it difficult to garner psychological support for the probability of manager dishonesty, leading them to be *less* likely to choose high levels of audit effort than non-rotating auditors. Thus, overall, rotating auditors will likely encounter difficulty finding psychological support for the probability of their current assessment frame, making them less likely to choose the audit action associated with that mental frame.<sup>6</sup>

The discussion above suggests the following hypothesis:

**H1a:** With rotation (no rotation), auditors assessing managers' dishonesty are more likely (less likely) to choose low effort than auditors assessing managers' honesty.

Ultimately, financial statement users and audit policy-makers are concerned with the joint choices of audit effort and manager financial reporting strategy and would prefer to minimize the frequency with which low-effort audits occur simultaneously with aggressive financial reporting.

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becomes the “below average effect” for difficult or unfamiliar tasks about which individuals feel incompetent (Kruger 1999). Similarly, hindsight bias, the belief that “I knew it all along,” becomes a reverse “I *never* would have known that” effect when individuals feel less competent with the subject matter (Ofir and Mazursky 1997).

<sup>6</sup> Our predictions for a reverse effect do not require that a rotating auditor explicitly consider the opposite complementary probability. For example, a rotating auditor assessing dishonesty (and therefore whether he or she should select a relatively higher-cost, less-trusting level of audit effort) may encounter difficulty finding psychological support for the probability of dishonesty, and therefore select high effort less frequently. This behavior translates into selecting lower effort more frequently by simple virtue of selecting higher effort less frequently. This point is made by Macchi et al. (1999, 211-213). In fact, a general premise of Support Theory is that people think of probabilities and actions in terms of the mental frame that they are in, and tend not to actively think in terms of the alternative frame (Brenner et al. 2002).

This outcome increases the likelihood of audit failures with negative legal, regulatory, and business implications for auditors, and is an important component of audit quality (e.g., Kadous 2000, 2001; King 2002, 268; and Peecher and Piercey 2008). Auditing standards (e.g., mandatory auditor rotation) and other regulations (e.g., the Sarbanes-Oxley Act of 2002) seek to minimize the frequency of this outcome. Thus, understanding conditions that make this outcome more or less frequent can help standard setters better anticipate the implications of proposed or adopted standards (Kachelmeier and King 2002).

We expect the frequency of this joint outcome to be influenced by the same phenomena we discuss in our predictions for H1a. Specifically, H1b only differs from H1a in that it predicts joint auditor and manager behavior rather than auditor behavior in isolation. We predict that managers will report aggressively when auditors pick low effort for two reasons. First, managers have incentives to report aggressively when auditors are less likely to detect aggressive reporting. Second, the imperfect feedback available to auditors in the natural environment increases managers' opportunity to report aggressively. As discussed in the next section, we ensure that managers in our experiment are aware of these opportunities (following King 2002), which allows us to test our predictions in a setting in which managers are likely to be opportunistic. Accordingly, we hypothesize the following:

**H1b:** With rotation (no rotation), audits will be more likely (less likely) to result in the joint occurrence of low effort and aggressive financial reporting when auditors assess manager dishonesty compared to when auditors assess manager honesty.

By definition, rotation status leads to differential feedback between the auditor and manager. Specifically, while auditors in our rotation condition learn about average manager behavior over time, auditors in the no-rotation learn about a specific manager's behavior. However, we do not expect the interactive results predicted in H1 to depend on or be mitigated



by repeated interactions and outcome feedback for two reasons. First, as described above, Support Theory finds that perceived competence, rather than actual competence, is sufficient to find the reverse effect predicted in H1 (e.g., Fox and Weber 2002). Specifically, Fox and Weber (2002) show directly that *perceived* (as opposed to *actual*) competence with the subject matter is sufficient to achieve this reversal effect. Further, research in Support Theory indicates that these effects are primitive, fundamental, and unconscious judgmental phenomena that persist under a variety of conditions and cannot be easily or consciously learned away (see Brenner et al. 2002). In an audit context, non-rotating (rotating) auditors anticipate being (not being) in a long-term relationship, and therefore likely anticipate being relatively more (less) competent in evaluating managers from very early on. Thus, a non-rotating (rotating) auditor does not need in-depth or long-term experience with the manager for our predicted effects to occur, but does need the sense that the auditor/client relationship is not (is) temporary. Second, as described in more detail below, feedback in the natural audit setting (as well as in our experiment) is sparse (King 2002; Peecher and Piercey 2008). Accordingly, we expect the effects in H1 to occur quickly and to persist over time.

### **Informal, Interpersonal Interactions Surrounding Management Representations**

In practice, auditors at different levels have varied degrees of informal interpersonal interactions with managers. The interpersonal interactions relevant to our study are those between auditors and the managers who typically make the major (aggressive or conservative) financial reporting decisions within the firm (Bell et al. 2005). For example, engagement partners have high levels of interaction with upper-level managers, while concurring partners, technical partners, advisory partners, and national office partners have virtually no interaction. Other audit team members, including engagement team members, can have interactions with managers that

range from very high levels to very low levels, depending upon their role in the audit firm and team.

Manipulating the availability of informal chat in our experiment allows us to test the robustness of H1 to both high and low levels of interaction. Such a test is helpful in assessing whether the potential unintended consequences of mandatory rotation extend to engagement partner rotation, concurring partner rotation, or audit firm rotation. For example, should our H1 interaction predictions hold only when informal chat is unavailable, we would expect our results to be less generalizable to engagement-partner-only rotation, since partners generally have a stronger relationship with management.

We predict that these chat interactions will result in less audit effort overall. Bazerman, Morgan, and Loewenstein (1997) argued that the interaction between auditors and managers could lead the auditor to unintentionally bias his or her decisions to favor the manager. King (2002) operationalized this concept as cheap talk or puffery in a strategic audit experiment, allowing managers to provide a one-way, restricted communication about their financial reporting choice. We extend King's (2002) research by manipulating the option to engage in two-way communication that is rich and restriction free.

The option to engage in informal chat with managers offers auditors the opportunity to gather social cues and other information about the potential reliability of a manager's assertions. Auditors who have the option to chat may use it when they sense the need to gather more information, or they may forego chatting when they are comfortable with management representations. In both cases, auditors may feel a greater, but perhaps illusory, amount of control (Langer 1975) over the actions of their clients (since chatting is at least as likely to serve the manager's goals as those of the auditor). Whether auditors are effective at using the *contents*

of chat to their benefit is a separate question, but we expect that the *option* to engage in informal chat will increase auditors' beliefs in the honesty of management representations and increase auditors' tendencies to select lower audit effort. Thus, our second hypothesis is:

**H2:** The opportunity to engage in interpersonal chat with managers will lead auditors to believe management representations more and to select lower levels of audit effort.

Support for H2 would have implications for auditors, who should understand factors that influence their beliefs about the financial statements and then select levels of audit effort. Support for this hypothesis would also provide new empirical support for the recent PCAOB standard requiring more involvement from concurring partners because those partners have less interpersonal interaction with client managers and would therefore be less susceptible to the effects predicted in H2 (PCAOB 2009; Peecher et al. 2010).

Note that H2 focuses on the *availability* of informal chat with a manager. Because of the difficulty in predicting the content of free and unstructured chat, it is not clear whether the *content* of any informal chat will affect auditors' beliefs about management or their audit effort.<sup>7</sup> For example, it is unclear whether the content of chat in this setting will lead auditors to know managers' intentions and possible actions, point out how little they actually know about them, or neither. In particular, the theory supporting the reverse effect in H1 focuses on the anticipated or perceived familiarity with the target. Thus, even short-term familiarity is unlikely to mitigate the reverse effect. As a result, we do not predict that unstructured chat will create the type of interactive effects that we predict in H1. However, our design allows us to test for these and other possible effects of unstructured chat on audit effort and audit quality.

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<sup>7</sup> Managers are likely to use the content of unstructured chat to try to persuade auditors of their honesty. However, the Persuasion Knowledge Model from psychology suggests that individuals can recognize and attempt to protect themselves from such persuasion attempts offered by parties with different strategic goals (e.g., Friestad and Wright 1994).

### III. METHOD

#### Experimental Setting

We operationalize the auditor-manager interaction as a strategic game (e.g., Fellingham and Newman 1985; Bowlin 2011), where an auditor and manager simultaneously make auditing and financial reporting decisions. Specifically, auditors choose a high or low-effort audit, while managers choose aggressive or conservative reporting. These paired choices lead to four possible outcomes, which determine each player's payoff in each round.

As illustrated in Table 1, each participant's payoff depends on exogenously determined probabilities. These probabilities represent (1) the likelihood that more or fewer misstatements occurred and (2) the likelihood that a misstatement has or has not been detected. Specifically, if the auditor selects low effort and the manager selects conservative reporting, the auditor has a 70% chance of earning 10 points and a 30% chance of earning 1 point, while the manager has a 10% chance of earning 4 points and a 90% chance of earnings 6 points. If the players select low effort and aggressive reporting, the auditor has a 30% (70%) chance of 10 (1) points, while the manager has a 10% (90%) chance of earning 1 (10) points. Third, if they select high effort and conservative reporting, the auditor has a 70% (30%) chance of earning 4 (6) points, while the manager has a 90% (10%) chance of earning 4 (6) points. Finally, an outcome of high effort/aggressive reporting yields a 30% (70%) chance of 4 (6) points for the auditor and a 90% (10%) chance of 1 (10) points for the manager.

[Insert Table 1 Here]

This auditor-manager structure does *not* result in a pure strategy Nash equilibrium of either conservative financial reporting or high-effort audits (Newman and Noel 1989). Rather, game theory predicts that in equilibrium each auditor (manager) will choose from among his or

her available strategies with probabilities that cause the manager (auditor) to be indifferent between his or her own available strategies (i.e., a mixed-strategy equilibrium). In our setting, game theory predicts a mixed-strategy equilibrium in which the auditor chooses a high-effort (low-effort) audit with a probability of 58.9% (41.1%) and the manager chooses aggressive (conservative) financial reporting with a probability of 61.4% (38.6%).<sup>8</sup>

Our parameterization captures five key concepts from the natural audit environment (Table 1).<sup>9</sup> First, misstatements are more likely to be detected when auditors choose high effort. Specifically, in our setting misstatements are detected 90% (10%) of the time when the auditor chooses high (low) effort. Second, high effort is costly, reducing the auditor's expected payoffs relative to low effort when the manager chooses conservative reporting (7.3 versus 3.7). Third, the auditor receives a larger expected payoff for choosing high effort (5.4 points) relative to low effort (3.7 points) when the manager chooses aggressive reporting. Thus, auditors are rewarded for avoiding both under-auditing (ineffective audits) and over-auditing (inefficient audits).

Fourth, larger misstatements are more likely when the manager reports aggressively rather than conservatively (70% and 30%, respectively). Fifth, the manager is always better off if misstatements are not detected. When misstatements are detected, managers face larger penalties when they engage in aggressive financial reporting (e.g., Sarbanes-Oxley Act of 2002, Section 906). However, when misstatements are not detected, managers receive greater benefits from

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<sup>8</sup> We analyze our parameterized model as a one-shot game even though we manipulate whether auditor-manager pairings are repeated or changed each round. As further described below, our participants know that the session will end after 20 rounds of the game, and therefore, because our game is repeated a finite number of times, analysis of the model as a repeated game yields the same equilibrium as that of the one-shot game (Fudenberg and Tirole 1991). This is true regardless of whether rotation occurs because the assumptions that make it so (backward induction, common knowledge of the game, and rationality) hold in both settings. To solve for the mixed strategy equilibrium, each player chooses a strategy such that the other player is indifferent between his or her strategies. Thus, if the auditor picks low (high) effort 41.1% (58.9%) of the time, the manager gets an expected payoff of 2.43, regardless of what the manager chooses. Likewise, if the manager chooses conservative (aggressive) reporting 38.6% (61.4%) of the time, the auditor gets 2.55, regardless of his or her choice. Consequently, as required by the logic of a Nash equilibrium, neither player has an incentive to deviate from these mixed strategies (Fudenberg and Tirole 1991).

<sup>9</sup> These experimental parameters, including payoffs and probabilities, are intended to create the key incentives and strategic tensions of the natural audit setting rather than mimic the real-world payoffs in a literal sense.

aggressive reporting. Specifically, the manager receives 6 (10) points when the manager chooses conservative (aggressive) reporting if misstatements are not detected and 4 (1) points when the manager chooses conservative (aggressive) reporting if misstatements are detected.

To summarize, this setting provides strategic tension for the auditor. On one hand, the auditor's highest expected payoff (7.3) occurs when he or she exerts low effort and the manager reports conservatively. This is analogous to an auditor's efficient use of resources when misstatement risk is appropriately assessed as low. On the other hand, the auditor's lowest expected payoff occurs when he or she exerts low effort and the manager reports aggressively, which captures the legal liability costs the auditor would eventually face when the aggressive reporting and low audit effort are revealed (e.g., Securities Exchange Act of 1934; Peecher and Piercey 2008).<sup>10</sup>

Finally, in our setting, each player learns only the points he or she earned at the end of each round, which provides only an imperfect signal of the other player's private choices, because payoffs for any given choice pair are probabilistic rather than deterministic, and identical payoffs can occur under multiple choice pairs. This is consistent with prior research (King 2002) and audit practice, in which auditors do not know in the near-term (and may never know) the private financial reporting choices of their clients, and managers do not know with certainty the auditor's strategy (Kennedy, Kleinmuntz, and Peecher 1997; Piercey 2009). That is, the revelation of an auditor's failure to detect aggressive reporting is often significantly, if not indefinitely, delayed. Furthermore, the probabilities in Table 1 confer an information advantage to managers. In practice, managers likely have a relative advantage at imperfectly observing

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<sup>10</sup> Overall, while unique to the current study, this payoff structure is related to that of King (2002), and is also consistent with structures in experiments by Fischbacher and Stefani (2008), Bowlin et al. (2009), and Bowlin (2011) and with the reasoning employed in analytical studies by Fellingham and Newman (1985), Newman and Noel (1989), Newman, Rhoades and Smith (1996), and Newman, Patterson and Smith (2005).

auditors' effort, compared to auditors' ability to infer managers' private financial reporting choices (King 2002; Bowlin et al. 2009).

## **Design**

We use a  $2 \times 2 \times 2$  fully-crossed, fixed-factorial between-subjects design manipulating three variables: *auditor rotation* (rotation vs. no rotation), *assessment frame* (honesty assessment frame vs. dishonesty assessment frame), and *availability of unstructured chat* (chat vs. no chat). The experiment is implemented using z-Tree software (Fischbacher 2007) and networked computers.

## **Participants**

Our participants are 226 undergraduate student volunteers from a large university.<sup>11</sup> Individual payouts range from \$18.80 to \$40.60, and average \$26.78. The experiment takes between 90 and 105 minutes, with approximately 30 of those minutes devoted to instructions. Participants are predominantly college sophomore (74%) business students (82%, of which 48% are accounting students) who have little work experience (80% with less than three years). A slight majority of participants is female (54%). These characteristics do not differ significantly between the levels of our manipulated conditions (all  $p > 0.10$ ).

## **Detailed Procedures**

The procedures for each experimental session are summarized in Figure 1. At the beginning of each session, participants receive written instructions, which are also read aloud. All laboratory materials contain neutral, non-contextual labels for player roles (i.e., “BLUE” and “GREEN”, for auditor and manager, respectively) and their choices (i.e., “UP” and “DOWN”, for low audit effort and high audit effort, respectively, “LEFT” and “RIGHT”, for conservative financial reporting and aggressive financial reporting, respectively); thus, we believe that

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<sup>11</sup> The university's Institutional Review Board approved the use of human participants in our study.

students are appropriate participants (Haynes and Kachelmeier 1998, Libby et al. 2002). The neutral labels reduce role-playing, hypothesis guessing, or other demand effects, while maintaining the economic incentives and strategic tensions present in actual auditor-manager interaction (Haynes and Kachelmeier 1998). All players are quizzed on the instructions they receive. The experimental software asks, grades, and displays correct answers for eight true/false quiz questions. Two questions emphasize the rotating or non-rotating nature of participants' roles. Four questions ask about game play and payoffs. Finally, two questions test knowledge about participants' payments.<sup>12</sup>

Next, participants are randomly assigned to the role of manager or auditor for the duration of the 20-round game. After participants are assigned to their roles, their computer screens display a brief review of a portion of their earlier instructions that are unique to their roles. In particular, managers are reminded of the outcome in Table 1 that gives them a 90% chance of winning 10 points, the highest chance at the highest possible points in the game. We then remind them that they can send messages to auditors about which action they would choose, such as saying they picked "left" (conservative reporting), but that they were not bound by the message they sent, and could actually pick "right" (aggressive reporting). Like King (2002), we offered these reminders to managers so that we could observe auditor behavior in an environment in which aggressive reporting and misleading manager representation are likely to occur.<sup>13</sup>

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<sup>12</sup> The primary intent of this quiz was to reinforce important information from the instructions. Participants were required to answer each question correctly before proceeding to the next question. Therefore, for each question answered incorrectly, the participant received an explanation of the relevant portions of the instructions and then answered the question again until all questions had been correctly answered.

<sup>13</sup> Although auditors and managers do not receive the same reminders, the instructions that all participants receive explain that the managers can choose whether or not to be truthful regarding their reporting choices (management representations).



### ***Procedures for Each Round***

Each auditor in the no-rotation (rotation) condition is randomly paired with a manager and remains with that manager for all 20 rounds (is randomly paired with a different manager at the beginning of each round).<sup>14</sup> At the beginning of each round (see Figure 1), participants in the chat conditions are given the opportunity to chat online for one minute. They receive the following on-screen instructions, but otherwise have no restrictions placed upon their chat: “You may use this chat box to send messages to your partner about your choices this round. Enter your messages to your partner in the field below. Please do not communicate identifying information about yourself.”

Next, managers in all conditions make their choices for that round (“You may select LEFT or RIGHT”). This is our measure of aggressive reporting. On the next screen, managers send auditors a standardized message (i.e., management representations) indicating which of their two options they (claim to have) selected for the round. The representation is either “I selected ‘LEFT’ this round” or “I selected ‘RIGHT’ this round.” This screen also reminds managers that their representation does not have to be truthful and that auditors are aware of this. Managers are also reminded that auditors have not yet made their choice (“UP or “DOWN”), and thus the manager’s representation could influence the auditor’s choice.

Next, the auditor receives the manager’s representation. Auditors in the honesty assessment (dishonesty assessment) conditions then assess the chance that this representation is honest (dishonest), using the following language for the honesty (dishonesty) frame: “On a scale

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<sup>14</sup> Following Kerlinger and Lee’s (2000, 459) experimental design tenet to “design, plan, and conduct research so that the experimental conditions are as different as possible” for the theoretical construct of interest, auditors and managers in the rotation condition are re-paired every round rather than, for example, every five rounds. Note that a design choice of rotating every five rounds, for example, while not being less arbitrary than rotating every one round, would have drawn a weaker distinction between our experimental rotation and no rotation conditions for the purpose of testing our generalizable theory within an abstract experiment. According to the theory we test, rotation impacts auditor judgment by its effects on auditors’ perceptions of their own competence evaluating the client, and we chose a strong manipulation of rotation to achieve a powerful test of this theory.

of 0 to 100, what is the chance that GREEN is being HONEST (DISHONEST) in that message?” This manipulation allows us to vary the frame in which the auditor assesses the probability that the management representation they receive is honest or dishonest. We call this auditor’s assessment “assessment of management representations” in the results section.

Finally, all auditors select their level of audit effort for the round (“UP” or “DOWN”). This is our measure of audit effort. Once both players have made their choices, each player learns the number of points that he or she earned that round (see Table 1). After all 20 rounds, participants complete a post-experimental questionnaire<sup>15</sup> and are paid based on the total points earned.

## IV. RESULTS

### Manipulation Checks

Most participants correctly recalled whether they were in the rotation or no-rotation condition (96%). Ninety-seven percent of participants understood that the management representations did not have to be truthful. Eliminating participants who failed these manipulation checks does not change our inferences.

### Tests of H1

Table 2 lists means and standard deviations for several key dependent measures across all of our independent variables. We find evidence for the interaction predictions in H1a and H1b in

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<sup>15</sup> The questionnaire asked manipulation check (discussed in Section IV) and demographic questions. On this questionnaire, participants reported finding the study quite easy to understand and complete (9.3 on a scale from 0= “Extremely Difficult” to 10 = “Extremely Easy”). The questionnaire also asked how participants felt about the person they were paired with (0= “Very Unhappy” and 10= “Very Happy”), and found that managers (7.83 vs. 7.14,  $t = 1.75, p = 0.08$ ) but not auditors in the chat condition, auditors (7.50 vs. 6.77,  $t = 1.97, p = 0.05$ ) but not managers in the no rotation condition, and auditors (7.64 vs. 6.63,  $t = 2.89, p < 0.01$ ) but not managers in the honesty frame condition felt happier about the other participant. Next, we asked whether participants could predict what the person they were paired with was going to do (0= “Not at all” and 10= “Extremely well”), and found that auditors (7.04 vs. 6.44,  $t = 1.72, p = 0.09$ ) and managers (7.50 vs. 6.23,  $t = 3.01, p < 0.01$ ) in the chat condition and auditors (7.23 vs. 6.33,  $t = 2.69, p < 0.01$ ) and managers (7.62 vs. 6.33,  $t = 3.08, p < 0.01$ ) in the no rotation condition felt they could. These results generally support our main findings. Principally, as our theory implies, auditors appear to feel more uncertain and less at ease when they rotate to a new manager.

the “*Low-Effort Audits*” and “*Low-effort/Aggressive*” rows. *Low-Effort Audits* is the percentage of rounds in which auditors choose low-effort audits, averaged over all 20 rounds for each participant (thus, one observation per participant). *Low-Effort/Aggressive* is the percentage of rounds in which the auditor chooses low effort and the manager reports aggressively (e.g., King 2002), an outcome that increases the likelihood of audit failures (Peecher and Piercey 2008). Regardless of *Chat* condition, low-effort audits and low-effort/aggressive outcomes are higher in an honesty frame than in a dishonesty frame when auditors are not rotating, while the reverse is true when auditors are rotating.

[Insert Table 2 here]

To formally test H1a and H1b, we conduct ANOVAs and planned contrast tests on our two primary dependent variables, *Low-Effort Audits* and *Low-Effort/Aggressive* outcomes. H1a predicts a disordinal interaction between rotation and assessment frame on *Low-Effort Audits*. We find support for this interaction in the ANOVA in Table 3, Panel A. Specifically, the *Rotation* × *Assessment Frame* interaction is significant ( $F = 7.39, p = 0.004$ ). Figure 2 shows our observed means for this interaction by experimental condition, and planned contrasts of these means appear in Table 3, Panel B. Under no-rotation, auditors choose low-effort audits more frequently when assessing the honesty of management representations than when assessing their dishonesty (63.1% vs. 53.6%,  $F = 2.70, p = 0.052$ ).

However, under rotation, auditors choose low-effort audits *less* frequently when assessing the honesty of those representations than when assessing their dishonesty (53.0% vs. 66.2%,  $F = 4.80, p = 0.015$ ). Thus, we find that rotation improves the audit by decreasing the frequency of low-effort audits when auditors use an *honesty frame* to assess management representations (63.1% vs. 53.0%,  $F = 3.14, p = 0.040$ ), but harms the audit by *increasing* the frequency of low-

effort audits when auditors are skeptical, using a *dishonesty frame* (53.6% vs. 66.2%,  $F = 4.25$ ,  $p = 0.021$ ).

[Insert Figure 2 and Table 3 here]

H1b predicts effects parallel to those above for the joint occurrence of low-effort audits and aggressive reporting, *Low-Effort/Aggressive*. Managers appear to generally respond to low-effort audits with more aggressive reporting, as can be seen in the “*Aggressive Reporting*” row of Table 2, as we would expect, given their incentives. Figure 3 presents the observed means for the interaction by experimental condition. Consistent with H1b, we find that rotation improves audit quality by decreasing the frequency of these outcomes when auditors use an *honesty frame* to assess management representations (33.1% vs. 23.0%,  $F = 5.13$ ,  $p = 0.013$ ). However, rotation harms audit quality by *increasing* the frequency of such outcomes when auditors use a *dishonesty frame* (25.4% vs. 37.8%,  $F = 6.58$ ,  $p = 0.006$ ). The ANOVA in Table 4, Panel A shows that the *Rotation*  $\times$  *Assessment Frame* interaction is significant ( $F = 11.71$ ,  $p < 0.001$ ). Thus, the results from testing H1a and H1b demonstrate that auditor rotation appears to offset and even reverse what standard setters would otherwise see as the benefits of auditor skepticism on the financial reporting outcome that is most likely to lead to audit failure.<sup>16</sup>

[Insert Figure 3 and Table 4 here]

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<sup>16</sup> As a test of our theory, we examine an alternative measure of auditors’ tendency to be unintentionally misled by managers. Specifically, we measure the difference between auditors’ assessments of manager honesty (or dishonesty) and managers’ actual honesty (or dishonesty). An untabulated ANOVA finds that results examining the *Rotation*  $\times$  *Assessment Frame* disordinal interaction remain robust to this variable ( $F = 4.62$ ,  $p = 0.017$ ), are in the expected direction, and generally replicate our results above. Furthermore, we find that this variable mediates the results of our tests of H1a (Goodman 1960 test = 1.63,  $p = 0.051$ ) and H1b (Goodman 1960 test = 2.02,  $p = 0.022$ ; MacKinnon, Warsi, and Dwyer 2005; Morgan-Lopez and Mackinnon 2006). This supplemental analysis suggests that the unintended consequences of mandatory auditor rotation extend to the accuracy of auditors’ assessments of management representations.

### ***Judgment Process Analysis for H1a and H1b***

In this section, we use two methods to validate the theory underlying H1a and H1b: first, a common empirical test of Support Theory to determine whether auditors' assessments of the probability of manager honesty and dishonesty exceed 100%, and second, a path analysis. Our theory underlying H1a and H1b suggests that auditors' probability assessments in the honesty frame conditions and in the dishonesty frame conditions will sum to (not sum to) more than 100% in the no-rotation (rotation) conditions. Consistent with the theory, within the no-rotation conditions, these complementary probability assessments sum to 108.0%, significantly greater than 100% ( $F = 3.51, p = 0.032$ , untabulated). In contrast, as our theory suggests, within the rotation conditions, these complementary assessments do not sum to more than 100% (95.5%,  $F = 1.03, p = 0.156$ ). These summed probability assessments are statistically different across the rotation and no-rotation conditions ( $F = 4.12, p = 0.022$ ).

Next, our theory suggests that auditors' assessments of management representations explain the interactive effects of rotation and assessment frame on *Low-Effort Audits*, which, in turn, explains the effects of rotation and assessment frame on *Low-Effort/Aggressive*. We test this with a multi-mediator model path analysis (Preacher and Hayes 2008). The first step in this path, auditors' *Assessment of Management Representations*, is equal to auditors' assessments of the probability that management representations are true in the honesty frame conditions, and 100% minus auditors' assessments that management representations are false in the dishonesty frame conditions.

As the "*Assessment of Management Representations*" row in Table 2 and Figure 4 show, the *Rotation*  $\times$  *Assessment Frame* interaction significantly influences auditors' *Assessment of Management Representations* ( $t = 2.03, p = 0.022$ ). Importantly, this first link in Figure 4 is

statistically equivalent to the Support Theory test (above) that the sum of honesty and dishonesty assessments is significantly different in the rotation and no-rotation conditions ( $F = 4.12, p = 0.022$ ). Thus, the remainder of this model shows how this psychological effect influences subsequent audit effort choices (H1a) and joint outcomes (H1b).

The path from *Rotation*  $\times$  *Assessment Frame* to *Assessment of Management Representations* to *Low-Effort Audits* is statistically significant (Goodman 1960 test = 1.93,  $p = 0.029$ ; MacKinnon et al. 1995; Morgan-Lopez and Mackinnon 2006). The significance of this path indicates that *Assessment of Management Representations* mediate our findings for H1a, the effect of *Rotation*  $\times$  *Assessment Frame* on *Low-Effort Audits* ( $t = 2.72, p = 0.004$ , Figure 4 and Table 3, Panel A). In turn, these *Assessment of Management Representations* (which reflect our predictions based on Support Theory) then affect *Low-Effort/Aggressive* outcomes ( $t = 3.77, p < 0.001$ , Figure 4), by way of their effect on *Low-Effort Audits* (Goodman 1960 test = 5.35,  $p < 0.001$ ).

Finally, the entire indirect path from *Rotation*  $\times$  *Assessment Frame* to *Assessment of Management Representations* to *Low-Effort Audits* to *Low-Effort/Aggressive* is statistically significant (Preacher and Hayes 2008 test = 2.35,  $p = 0.009$ ). These findings provide evidence that the psychological effects of rotation and assessment frame predicted by Support Theory drive the effects in our tests of H1a and H1b.<sup>17</sup>

[Insert Figure 4 here]

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<sup>17</sup> Our theory suggests that the results in Figure 4 would become even stronger if we examined managers who claimed to use conservative reporting, since that is the claim where higher *Assessment of Management Representations* would be more likely to result in more *Low-Effort Audits*. On average, managers in our experiment claimed to auditors that they were engaging in conservative financial reporting a majority, but not 100%, of the time (mean = 74.5%). As expected, when we examine only the half of the sample in which managers claim conservative reporting more frequently, we find stronger results than reported in Figure 4, with all mediation switching to full mediation from the partial mediation reported in Figure 4.

## Tests of H2

H2 predicts that the option to engage in unstructured chat will increase auditors' overall tendency to believe management representations and to choose low-effort audits. Consistent with H2, Table 2 shows that the opportunity to chat increases the rates of low-effort audits and aggressive reporting. As the ANOVA in Table 3 shows, we detect a significant main effect of unstructured chat on *Low-Effort Audits*. Overall, unstructured chat increases *Low-Effort Audits* from 48.7% to 69.2% ( $F = 23.98, p < 0.001$ ).<sup>18</sup> These results support H2.<sup>19</sup> Additionally, we do not detect any significant three-way interactions, which indicates that the two-way *Rotation* × *Assessment Frame* interaction is statistically similar within each level of *Unstructured Chat*, for both *Low-Effort Audits* (Table 3) and *Low-Effort/Aggressive* (Table 4). This suggests that our main findings for H1a and H1b may generalize to the judgment and decision-making of audit team members with different levels of opportunity to interact informally with managers (e.g., engagement partners vs. concurring partners and other audit team members).

## Supplemental Analyses of H2

Additional (untabulated) analysis of the content of chat confirms that it is a reflection, rather than a driver of our results. Specifically, we find that it is the opportunity to chat, and not the content of the chat, that increases *Low-Effort Audits*. First, we examine the total amount of chat between managers and auditors. We find a significant main effect of rotation, such that auditors in the rotation conditions engage in more chat than those in the no-rotation conditions ( $F = 4.11, p = 0.048$ ; see Table 2 and Figure 5). Next, we identify attempts to persuade or encourage

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<sup>18</sup> *Unstructured Chat* has similar effects on auditor assessments of management representations. As reported in Table 2, *Assessment of Management Representations* increases from 53.97% in the no-chat condition to 68.45% in the chat condition ( $F = 19.44, p < .001$ ). A repeated measures ANOVA indicates that this effect does not systematically change across rounds ( $p > 0.10$  for all interactions that include *Unstructured Chat* and *Round*).

<sup>19</sup> We also find that *Assessment of Management Representations* significantly mediates the main effect of *Unstructured Chat* on *Low-Effort Audits* (Goodman 1960 test = 3.46,  $p < 0.001$ ).

the other player to be honest.<sup>20</sup> We detect a significant ordinal *Rotation* × *Assessment Frame* interaction such that auditors use more of these attempts when jointly in the rotation and dishonesty frame conditions ( $F = 8.25, p = 0.006$ ; see Table 2).

These results could be a reflection of auditors in the rotation condition being inexperienced with the manager and therefore more carefully considering both sides of the assessment under question, which would be consistent with H1a and H1b. We do *not* find that either persuasion attempts or the total volume of chat influences *Assessment of Management Representations*, *Low-Effort Audits*, or *Low-Effort Aggressive* (all p-values  $\geq 0.15$ ), suggesting that the content of chat does not drive effects in these variables. Moreover, we do not find significant relationships between any other aspects of the contents of chat and any of our dependent variables.<sup>21</sup>

### **Additional Results**

Figure 5 presents auditor and manager behavior over time (Panels A-D). Overall, these figures show that auditor and manager behavior is consistent with our hypotheses. Further, these effects show no significant moderation over time. As discussed in the theory section, this is not surprising, given that Support Theory predicts that perceived, rather than actual, competence is sufficient to find the reverse effect predicted in H1 (e.g., Fox and Weber 2002) and given the

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<sup>20</sup> We used two independent coders. The coders were unfamiliar with the hypotheses and resolved any differences in their coding together.

<sup>21</sup> While informal chat could potentially be a source of useful audit evidence, our results from testing H2 and our supplemental analysis indicate that was not the case in our study. In practice, the content of chatting between auditors and managers likely includes various topics, many of which are not accounting-related assertions or other attempts to persuade. For example, auditors and managers might chat about the weather, sports, family, and other personal interests, in addition to audit-related issues. Further, the value of informal chat is likely reduced by its non-binding, cheap-talk nature.



sparse feedback participants receive.<sup>22</sup> Finally, Panels E and F show that chat volume generally increases over time, particularly in the *Rotation Condition*.

[Insert Figure 5]

## V. CONCLUSION

Our study sheds light on an important policy question that has been the subject of considerable debate—whether mandatory auditor rotation increases or decreases audit quality—by considering a critical related variable, skepticism. Regulators appear not to have considered that the frame auditors use to evaluate management representations can vary between assuming potential *honesty* to assuming potential *dishonesty* (i.e., skepticism). We extend the auditing literature by providing evidence of an unanticipated and potentially harmful interaction between rotation and skepticism. We develop a parameterized model of the auditor-manager relationship that includes attributes of audit quality, including audit effort, auditors' assessments of the honesty of management representations, and low-effort audits paired with aggressive reporting. Overall, we find evidence of a disordinal interaction such that mandatory auditor rotation increases audit quality for audit judgments focused on the likelihood of managers' honesty, but tends to *decrease* audit quality for audit judgments focused on the likelihood of managers' dishonesty.

Our findings suggest that rotation could result in an unintended consequence for professional skepticism standards. According to SAS No. 99 (AICPA 2011a), professional skepticism requirements are intended to elevate auditors' skepticism of their clients and, ultimately, audit quality. We present evidence that the benefits of skepticism could be offset

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<sup>22</sup> Inferences from our results are unchanged when we use repeated measures with *Round* as a main effect and its interactions with our other independent variables to analyze the dependent variables audit effort, auditors' assessments of management representations, the joint outcome of low-effort audits and aggressive reporting, and manager aggressive reporting (all  $p > 0.10$  for *Round* and its interactions).

under mandatory auditor rotation. Specifically, while prompting non-rotating auditors to adopt a skeptical mental frame results in higher levels of audit effort and a lower frequency of low-effort audits paired with aggressive reporting, this benefit disappears and even reverses when auditors rotate. That is, rotation and a skeptical mindset interact to the detriment of audit effort and financial reporting quality. Auditors are not likely aware of this subtle psychological effect about which decision-makers rarely have accurate insight (Peecher et al. 2010). Standard setters, auditors, investors, and academics should consider this effect when evaluating the relative costs and benefits of a rotation mandate. Given the significant costs associated with mandatory rotation, focusing auditors on a skeptical assessment frame without requiring mandatory rotation may be a less costly way for standard setters to improve audit quality.

We also find that this effect occurs independently of whether auditors have the opportunity to engage in informal, unstructured chat with clients. As a result, the potential unintended consequences of auditor rotation that we document would likely influence engagement partners (who have many opportunities to interact with clients), concurring partners (who have virtually no opportunity to interact with clients), and a variety of audit team members with both high and low opportunities to chat with managers involved in financial reporting decisions. As a result, the effects we study may generalize to both audit partner and audit firm rotation (cf. Gul et al. 2013).

In addition, we find that the opportunity to interact informally with management, beyond the content of what is actually said, exacerbates auditors' assessments of management representations and the frequency of low-effort audits. This finding suggests a potential advantage to concurring partners, who do not interact as closely with the client, contributing new evidence supportive of the PCAOB (2009) auditing standard increasing the role of concurring

partners (Peecher et al. 2010). Expanding upon unstructured chat between auditors and managers may be a fruitful avenue for future research.

Our results have implications for prior research. Myers et al. (2003), Cameran et al. (2008) and the PCAOB (2011b) note that self-selection bias limits the extent to which archival findings can address the effects of auditor rotation on audit quality. We use an experiment to eliminate this self-selection bias. We also contribute to the experimental literature on auditor rotation, which calls for additional research (Dopuch et al. 2001; Wang and Tuttle 2009). For example, while prior experimental research on auditor rotation focuses on auditors *intentionally* biasing their audit opinions (Dopuch et al. 2001; Wang and Tuttle 2009), we examine how auditors may succumb to management representations through *unintentional* under-auditing because they rely too heavily on those representations.

Thus, our study may be more relevant to audit failures involving auditor negligence, which are more common than those involving deliberate audit report bias (Messier, Glover, and Prawitt 2011). Additionally, Wang and Tuttle (2009) point out that the existing experimental research on rotation has focused on auditors' reporting decisions that occur only after auditors have planned the audit and collected evidence. Instead, we focus on auditors' choices of audit effort levels and their assessments of management representations that occur prior to auditors' reporting decisions.

Our research is subject to limitations. Like all experimental research, we test only some aspects of the real-world audit setting (Haynes and Kachelmeier 1998). Other factors, such as audit firm switching costs, auditor expertise, and client complexity, could affect the relative costs and benefits of mandatory auditor rotation. Future research could consider such factors. Additionally, our manipulations are not representative of all possible situations and outcomes.

For example, in our setting, rotation occurs every round. Our rotation manipulation instantiates the perception of familiarity or unfamiliarity with the client based upon the knowledge that the auditor will or will not be with the client for the duration of the experiment. Thus, our rotation condition could generalize to a variety of settings in which the auditor knows that he or she will be with the client only temporarily. Future research could address this issue by examining the effects of rotation length. Despite these limitations, we believe we provide a useful setting and supportive evidence for future research on the effects of mandatory auditor rotation on audit quality.



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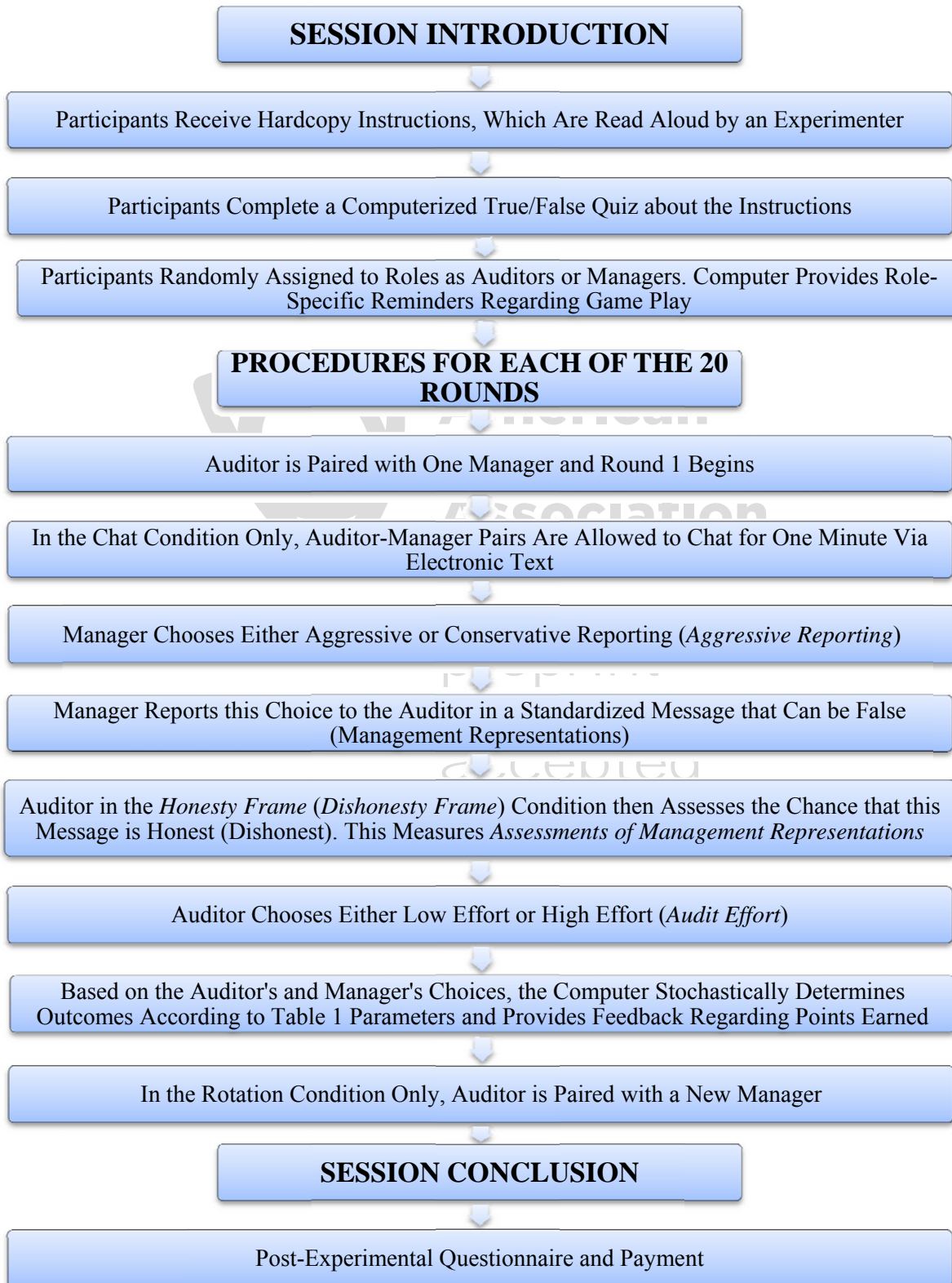


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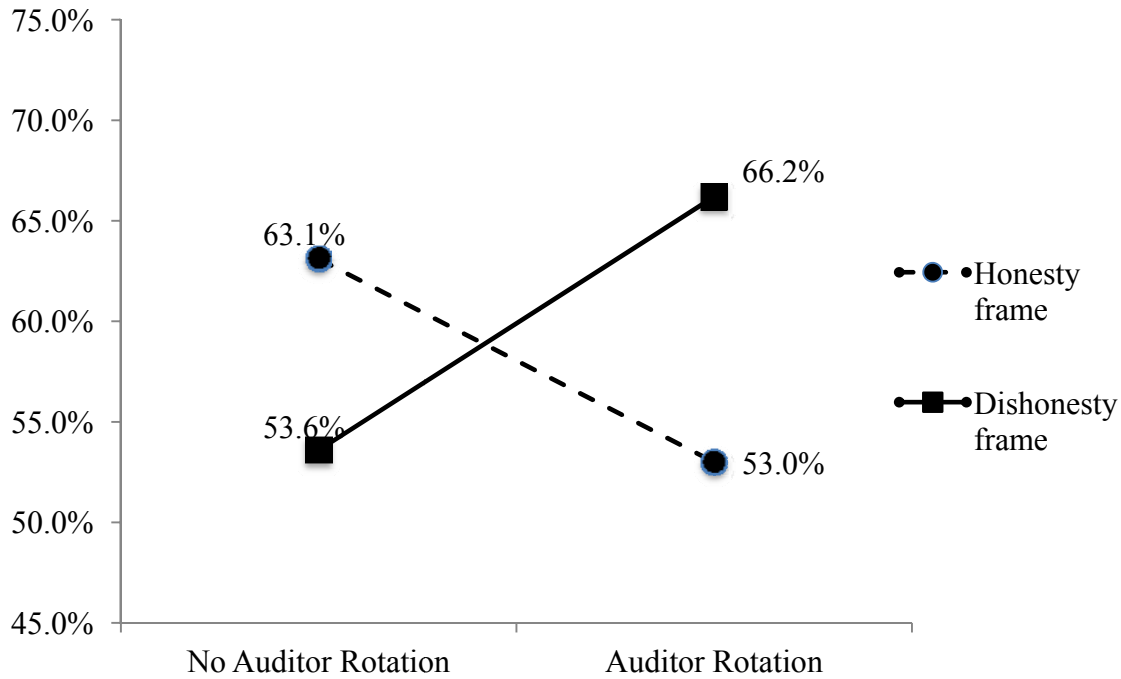


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**FIGURE 1**  
**Detailed Procedures**



**FIGURE 2**  
*Effects of Rotation<sup>a</sup> and Assessment Frame<sup>b</sup> on Low-Effort Audits<sup>c</sup>*

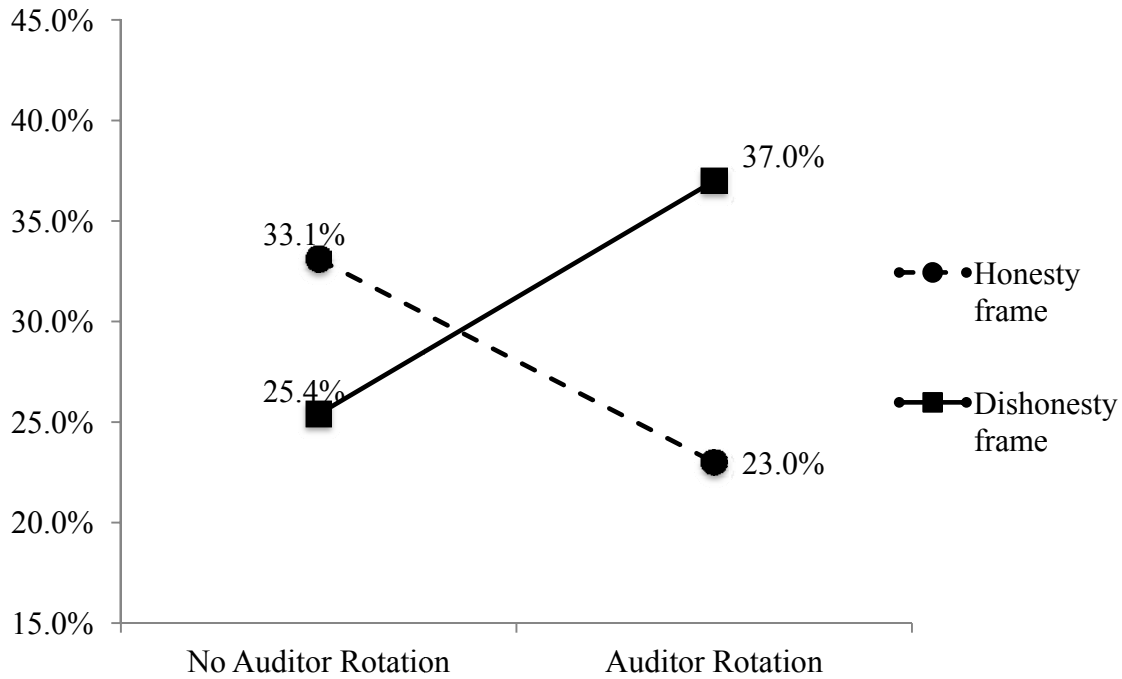


<sup>a</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).

<sup>b</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).

<sup>c</sup> *Low-Effort Audits* equals the percentage of rounds in which auditors select low-effort audits.

**FIGURE 3**  
*Effects of Rotation<sup>a</sup> and Assessment Frame<sup>b</sup> on Low-Effort/Aggressive Reporting<sup>c</sup> Pairs*

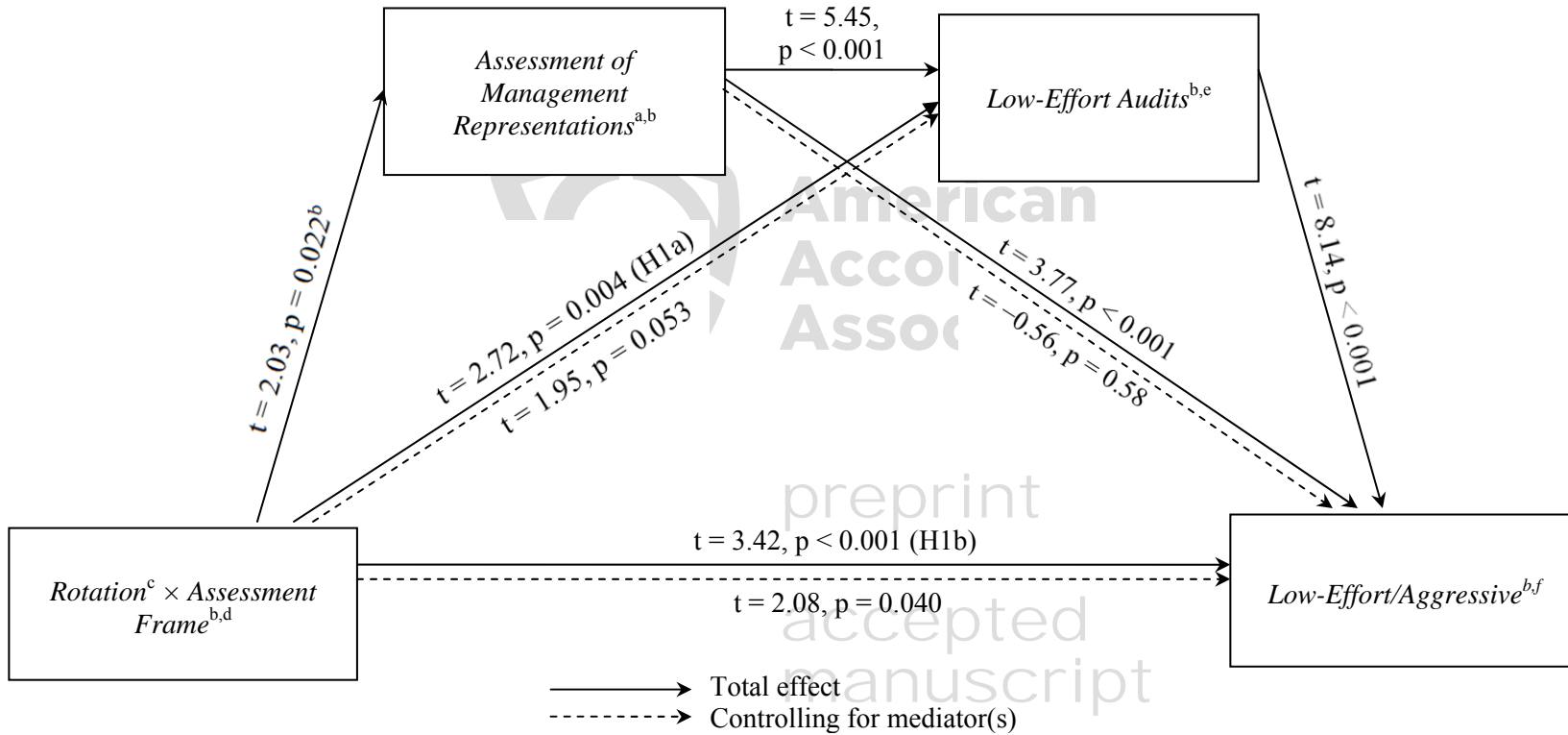


<sup>a</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).

<sup>b</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).

<sup>c</sup> *Low-Effort/Aggressive* equals the percentage of rounds in which auditors choose low-effort audits and managers choose aggressive reporting.

**FIGURE 4**  
**Judgment Process Analysis**

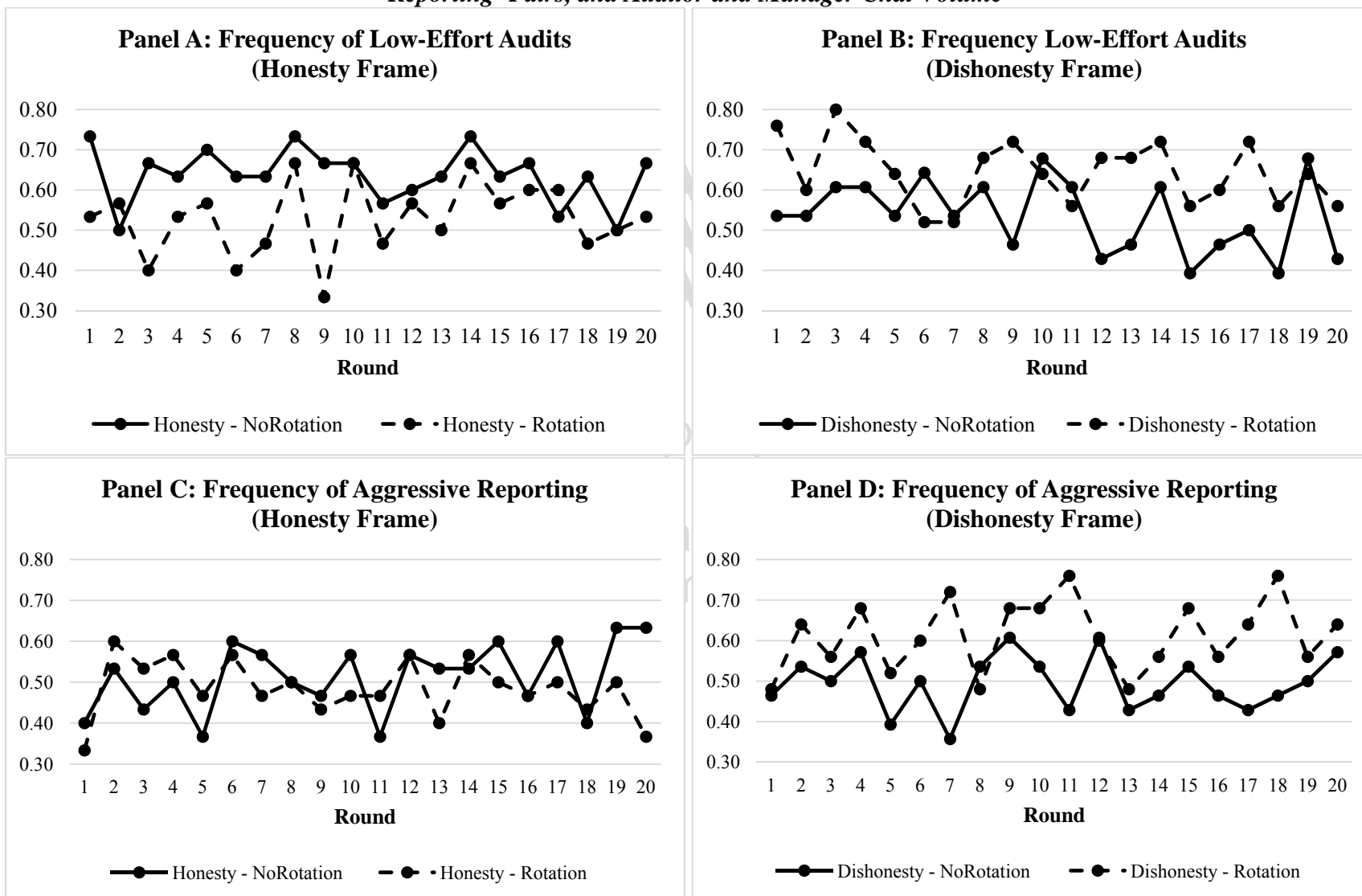


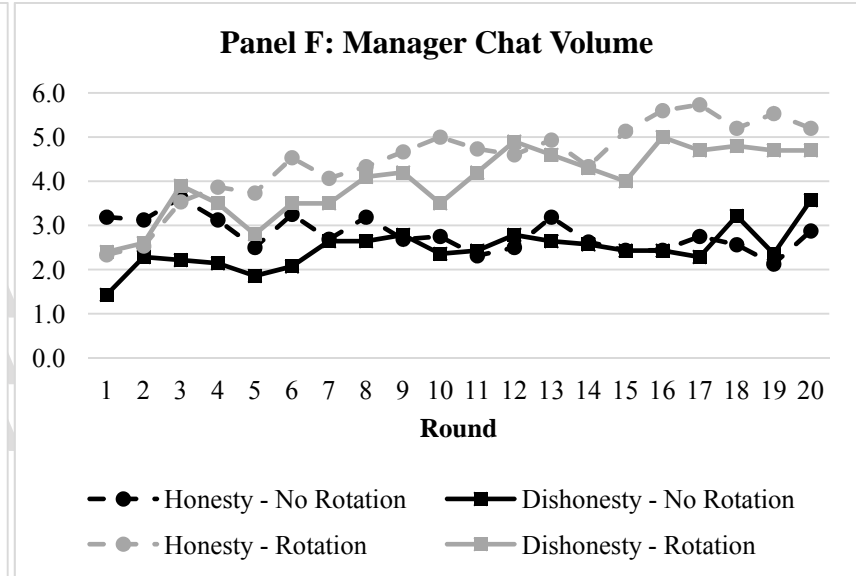
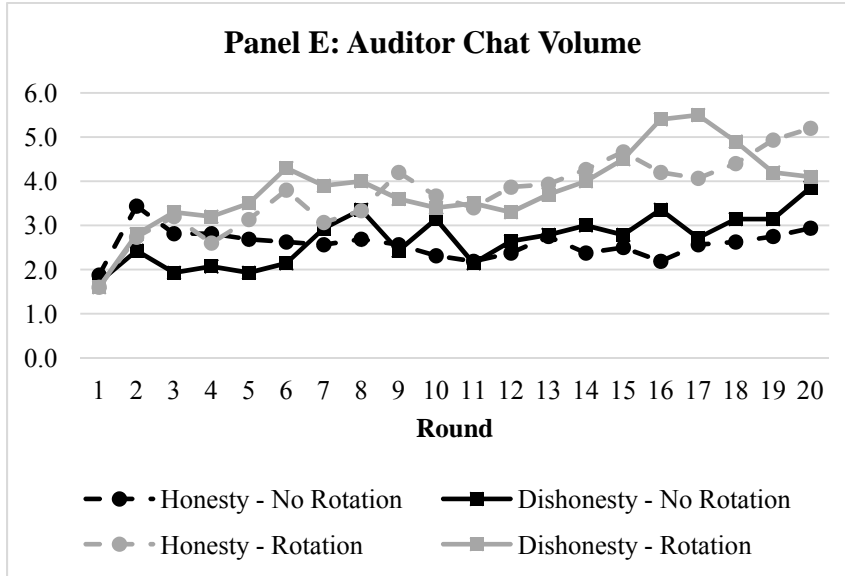
- <sup>a</sup> The mediator variable *Assessment of Management Representations* is equal to auditors' probability assessments of the honesty of management representations in the honesty assessment frame conditions, and 100% minus their probability assessments of the dishonesty of management representations in the dishonesty assessment frame conditions.
- <sup>b</sup> The test of the *Rotation* × *Assessment Frame* interaction on *Assessment of Management Representations* ( $t = 2.03$ ,  $p = 0.022$ ) is statistically equivalent to our earlier test that the sum of honesty assessments and dishonesty assessments within the rotation conditions (which is more than 100%) is significantly different ( $p = 0.022$ ) from the same sum within the no rotation conditions (which is not more than 100%). As a result, the statistically significant indirect path from *Rotation* × *Assessment Frame* to *Assessment of Management Representations* to *Low-Effort Audits* to *Low-Effort/Aggressive* (Preacher and Hayes 2008 test = 2.35,  $p = 0.009$ ) suggests that the judgmental effects predicted by our use of Support Theory are driving the *Rotation* × *Assessment Frame* effects for *Low-Effort Audits* (H1a), and, ultimately, *Low-Effort/Aggressive* outcomes (H1b).
- <sup>c</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).
- <sup>d</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).
- <sup>e</sup> *Low-Effort Audits* equals the percentage of rounds in which auditors choose low-effort audits.
- <sup>f</sup> *Low-Effort/Aggressive* equals the percentage of rounds in which auditors choose low-effort audits and managers choose aggressive reporting.

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**FIGURE 5**

*Descriptive Results Over Time of the Effects of Rotation<sup>a</sup> and Assessment Frame<sup>b</sup> on Low-Effort Audits<sup>c</sup>, Low-Effort/Aggressive Reporting<sup>d</sup> Pairs, and Auditor and Manager Chat Volume<sup>e</sup>*





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- <sup>a</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).
- <sup>b</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).
- <sup>c</sup> *Low-Effort Audits* equals the percentage of rounds in which auditors choose low-effort audits.
- <sup>d</sup> *Low-Effort/Aggressive* equals the percentage of rounds in which auditors choose low-effort audits and managers choose aggressive reporting.
- <sup>e</sup> *Auditor Chat Volume* equals the mean number of chat messages sent from the auditor to the manager each round. *Manager Chat Volume* equals the mean number of chat messages sent from the manager to the auditor each round.



**TABLE 1**  
**Parameters of the Auditor-Client Relationship:**  
**Game Choices, Payoffs, and Probabilities**

<b>Auditors' Audit Quality Choices:</b>	<b>Managers' Financial Reporting Choices:</b>	
	<u>Conservative Reporting</u>	<u>Aggressive Reporting</u>
<u>Low Effort</u>		
Managers' payoffs:		
Misstatement detected [payoff (probability)]	4 (10%)	1 (10%)
Misstatement not detected [payoff (probability)]	6 (90%)	10 (90%)
Expected value	5.8	9.1
Auditors' payoffs:		
Smaller misstatement [payoff (probability)]	10 (70%)	10 (30%)
Larger misstatement [payoff (probability)]	1 (30%)	1 (70%)
Expected value	7.3	3.7
<u>High Effort</u>		
Managers' payoffs:		
Misstatement detected [payoff (probability)]	4 (90%)	1 (90%)
Misstatement not detected [payoff (probability)]	6 (10%)	10 (10%)
Expected value	4.2	1.9
Auditors' payoffs:		
Smaller misstatement [payoff (probability)]	4 (70%)	4 (30%)
Larger misstatement [payoff (probability)]	6 (30%)	6 (70%)
Expected value	4.6	5.4

We operationalize the auditor-client interaction as a strategic game (e.g., Fellingham and Newman 1985; and Bowlin 2011), where an auditor and manager simultaneously make auditing and financial reporting decisions. Specifically, a manager participant is paired with an auditor participant for one (all 20) rounds in the rotation (no rotation) condition. The manager picks conservative or aggressive reporting and the auditor picks low or high effort. Their joint choice determines two possible probabilistic payoffs for each individual. The table lists these payoffs and their probabilities as well as their expected values. Each participant's precise payoff depends on exogenously determined probabilities. These probabilities represent (1) the likelihood that more or fewer misstatements occurred and (2) the likelihood that a misstatement has or has not been detected. Specifically, if the auditor chooses low effort and the manager chooses conservative reporting, the auditor has a 70% chance of earning 10 points and a 30% chance of earning 1 point, while the manager has a 10% chance of earning 4 points and a 90% chance of earnings 6 points. If the players choose low effort and aggressive reporting, the auditor has a 30% (70%) chance of 10 (1) points, while the manager has a 10% (90%) chance of earning 1 (10) points. Third, if they choose high effort and conservative reporting, the auditor has a 70% (30%) chance of earning 4 (6) points, while the manager has a 90% (10%) chance of earning 4 (6) points. Finally, an outcome of high effort/aggressive reporting yields a 30% (70%) chance of 4 (6) points for the auditor and a 90% (10%) chance of 1 (10) points for the manager.

**TABLE 2**  
*Means (Standard Deviation) by Experimental Condition*

**Panel A: Means by Assessment Frame and Rotation**

	No Rotation <sup>l</sup>		Rotation <sup>l</sup>	
	Dishonesty Assessment Frame <sup>m</sup>	Honesty Assessment Frame <sup>m</sup>	Dishonesty Assessment Frame <sup>m</sup>	Honesty Assessment Frame <sup>m</sup>
	<i>n</i> =28	<i>n</i> =30	<i>n</i> =25	<i>n</i> =30
<i>Low-Effort Audits<sup>a</sup></i> (Std. Dev.)	53.57% (24.86%)	63.67% (22.85%)	64.40% (23.73%)	53.00% (24.37%)
<i>Aggressive Reporting<sup>b</sup></i>	49.46% (16.01%)	51.33% (21.13%)	61.40% (16.62%)	48.50% (12.67%)
<i>Assessment of Management Representations<sup>c</sup></i>	60.84% (16.67%)	69.57% (21.32%)	58.66% (15.89%)	54.60% (16.13%)
<i>Low-Effort/Conservative<sup>d</sup></i>	28.21% (18.42%)	30.33% (19.82%)	26.00% (18.03%)	30.00% (17.22%)
<i>Low-Effort/Aggressive<sup>e</sup></i>	25.36% (15.39%)	33.33% (22.72%)	38.40% (17.60%)	23.00% (12.64%)
<i>High-Effort/Conservative<sup>f</sup></i>	22.32% (14.56%)	18.33% (14.46%)	12.60% (10.72%)	21.50% (11.53%)
<i>High-Effort/Aggressive<sup>g</sup></i>	24.11% (15.99%)	18.00% (12.84%)	23.00% (19.36%)	25.50% (16.99%)

**TABLE 2 (continued)**

**Panel B: Means by Assessment Frame, Rotation, and Chat (Includes Chat Variables)**

	No Rotation <sup>l</sup>				Rotation <sup>l</sup>			
	Dishonesty Assessment Frame <sup>m</sup>		Honesty Assessment Frame <sup>m</sup>		Dishonesty Assessment Frame <sup>m</sup>		Honesty Assessment Frame <sup>m</sup>	
<i>n</i> (Chat   No Chat)	<i>n</i> = 14   14		<i>n</i> = 16   14		<i>n</i> = 10   15		<i>n</i> = 15   15	
<i>Low-Effort Audits</i> <sup>a</sup> (Chat   No Chat)	66.43%	40.71%	72.19%	53.92%	75.00%	57.33%	63.00%	43.00%
<i>Aggressive Reporting</i> <sup>b</sup> (Chat   No Chat)	52.14%	48.93%	51.56%	43.36%	54.50%	28.00%	59.00%	44.00%
<i>Assessment of Management Representations</i> <sup>c</sup> (Chat   No Chat)	67.34%	54.33%	79.65%	58.05%	61.47%	56.79%	62.19%	47.01%
<i>Low-Effort/Conservative</i> <sup>d</sup> (Chat   No Chat)	34.29%	22.14%	36.56%	23.21%	40.50%	16.33%	38.67%	21.33%
<i>Low-Effort/Aggressive</i> <sup>e</sup> (Chat   No Chat)	32.14%	18.57%	35.63%	30.71%	34.50%	41.00%	24.33%	21.67%
<i>High-Effort/Conservative</i> <sup>f</sup> (Chat   No Chat)	17.86%	26.79%	15.00%	22.14%	14.00%	11.67%	20.33%	22.67%
<i>High-Effort/Aggressive</i> <sup>g</sup> (Chat   No Chat)	15.71%	32.50%	12.81%	23.93%	11.00%	31.00%	16.67%	34.33%
<i>Auditor Chat Volume</i> <sup>h</sup> (Std. Dev.)	2.68 (2.10)		2.58 (1.52)		3.84 (1.55)		3.71 (0.81)	
<i>Manager Chat Volume</i> <sup>i</sup> (Std. Dev.)	2.46 (1.64)		2.80 (1.99)		4.00 (0.50)		4.48 (2.12)	
<i>Auditor Persuasion Attempts</i> <sup>j</sup> (Std. Dev.)	0.38 (0.41)		0.17 (0.21)		0.62 (0.46)		0.43 (0.34)	
<i>Manager Persuasion Attempts</i> <sup>k</sup> (Std. Dev.)	0.28 (0.35)		0.25 (0.18)		0.69 (0.56)		0.68 (0.45)	

<sup>a</sup> *Low-Effort Audits* equals the percentage of rounds in which auditors choose low-effort audits.

<sup>b</sup> *Aggressive Reporting* equals the percentage of rounds in which managers choose aggressive reporting.

<sup>c</sup> *Assessment of Management Representations* equals the auditors' probability assessments of the honesty of management representations in the honesty assessment frame conditions, and 100% minus their probability assessments of the dishonesty of management representations in the dishonesty assessment frame conditions.

<sup>d</sup> *Low-Effort/Conservative* equals the percentage of rounds in which auditors choose low-effort audits and managers choose conservative reporting.

<sup>e</sup> *Low-Effort/Aggressive* equals the percentage of rounds in which auditors choose low-effort audits and managers choose aggressive reporting.

<sup>f</sup> *High-Effort/Conservative* equals the percentage of rounds in which auditors choose high-effort audits and managers choose conservative reporting.

<sup>g</sup> *High-Effort/Aggressive* equals the percentage of rounds in which auditors choose high-effort audits and managers choose aggressive reporting.

<sup>h</sup> *Auditor Chat Volume* equals the mean number of chat messages sent from the auditor to the manager each round.

<sup>i</sup> *Manager Chat Volume* equals the mean number of chat messages sent from the manager to the auditor each round.

- <sup>j</sup> *Auditor Persuasion Attempts* equals the mean number of chat messages sent from the manager to the auditor each round in which auditors attempt to persuade auditors to choose low effort.
- <sup>k</sup> *Manager Persuasion Attempts* equals the mean number of chat messages sent from the manager to the auditor each round in which managers attempt to persuade auditors to choose low effort.
- <sup>l</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).
- <sup>m</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assessed the chance that this message was honest (dishonest).



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**TABLE 3**  
*Effects of Rotation<sup>b</sup> and Assessment Frame<sup>c</sup> on Low-Effort Audits<sup>d</sup>*

**Panel A: ANOVA**

Source	Sum of Squares	df	F	p-value
<i>Rotation</i>	0.004	1	0.09	0.761
<i>Assessment Frame</i>	0.009	1	0.19	0.660
<i>Unstructured Chat</i>	1.156	1	23.98	< 0.001 <sup>a</sup>
<i>Rotation × Assessment Frame</i>	0.356	1	7.39	0.004 <sup>a</sup>
<i>Rotation × Unstructured Chat</i>	0.007	1	0.14	0.706
<i>Assessment Frame × Unstructured Chat</i>	0.005	1	0.09	0.759
<i>Rotation × Assessment Frame × Unstructured Chat</i>	0.017	1	0.34	0.558

**Panel B: Pairwise Contrasts**

Source	Sum of Squares	df	F	p-value
Effect of <i>Assessment Frame</i> under <i>No-Rotation</i>	0.130	1	2.70	0.052 <sup>a</sup>
Effect of <i>Assessment Frame</i> under <i>Rotation</i>	0.231	1	4.80	0.015 <sup>a</sup>
Effect of <i>Rotation</i> under an <i>Honesty Assessment Frame</i>	0.151	1	3.14	0.040 <sup>a</sup>
Effect of <i>Rotation</i> under a <i>Dishonesty Assessment Frame</i>	0.205	1	4.25	0.021 <sup>a</sup>

<sup>a</sup> These p-values are for effects that occur in the expected direction suggested by our theory (e.g., H1), and are therefore the one-tailed test of the signed *t*-statistic associated with this *F*-test (e.g., Kachelmeier and Williamson 2010, Table 2). McNeil, Newman, and Kelley (1996) discuss one-tailed tests of interactions with directional expectations (as in, e.g., Bowlin et al. 2009; Kachelmeier and Williamson 2010; Peecher et al. 2010). Other reported p-values are two-tailed.

<sup>b</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds of the game (were randomly paired with a different manager, at the beginning of each round).

<sup>c</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).

<sup>d</sup> *Low-Effort Audits* equals the percentage of rounds in which auditors choose low-effort audits.

**TABLE 4**  
*Effects of Rotation<sup>c</sup> and Assessment Frame<sup>d</sup> on Low-Effort/Aggressive Reporting<sup>e</sup> Pairs*

**Panel A: ANOVA**

Source	Sum of Squares	df	F	p-value	
<i>Rotation</i>	0.003	1	0.11	0.737	
<i>Assessment Frame</i>	0.033	1	1.11	0.295	
<i>Unstructured Chat</i>	0.037	1	1.23	0.135	<sup>a,b</sup>
<i>Rotation × Assessment Frame</i>	0.353	1	11.71	< 0.001	<sup>a</sup>
<i>Rotation × Unstructured Chat</i>	0.086	1	2.86	0.094	<sup>b</sup>
<i>Assessment Frame × Unstructured Chat</i>	< 0.001	1	< 0.01	0.969	
<i>Rotation × Assessment Frame × Unstructured Chat</i>	0.055	1	1.83	0.179	

**Panel B: Pairwise Contrasts**

Source	Sum of Squares	df	F	p-value	
Effect of <i>Assessment Frame</i> under <i>No-Rotation</i>	0.088	1	2.93	0.045	<sup>a</sup>
Effect of <i>Assessment Frame</i> under <i>Rotation</i>	0.290	1	9.62	0.001	<sup>a</sup>
Effect of <i>Rotation</i> under an <i>Honesty Assessment Frame</i>	0.155	1	5.13	0.013	<sup>a</sup>
Effect of <i>Rotation</i> under a <i>Dishonesty Assessment Frame</i>	0.198	1	6.58	0.006	<sup>a</sup>

<sup>a</sup> These p-values are for effects that occur in the expected direction suggested by our theory (e.g., H1) and are therefore the one-tailed test of the signed *t*-statistic associated with *F*-test (e.g., Kachelmeier and Williamson 2010, Table 2). McNeil et al. (1996) discuss one-tailed tests of interactions with directional expectations (as in, e.g., Bowlin et al. 2009; Kachelmeier and Williamson 2010; Peecher et al. 2010). Other reported p-values are two-tailed.

<sup>b</sup> While the main effect of *Unstructured Chat* is significant in *Low-Effort Audits* (Table 2), its effects are directionally consistent but not significant in *Low-Effort/Aggressive* ( $p = 0.135$ , above). The marginally significant *Rotation × Unstructured Chat* interaction ( $p = 0.094$ , above) indicates that the effect of *Unstructured Chat* on *Low-Effort/Aggressive* is significant within the no-rotation conditions ( $F = 4.09$ ,  $p = 0.023$ ), but not within the rotation conditions ( $F = 0.16$ ,  $p = 0.69$ ). However, this *Rotation × Unstructured Chat* interaction ( $p = 0.094$ ) is marginally significant in only this one test, is not robust to alternative model specifications or sensitivity analyses, and is not significant in even this instance after adjustment for the post-hoc nature of the finding (Sidak-corrected  $p = 0.92$ ; e.g., Piercey 2009). Thus, we find significant evidence of the main effect of chat on *Low-Effort Audits* predicted by H2, as well as a directionally similar effect of chat on *Low-Effort/Aggressive* outcomes, but one that is significant within the no-rotation conditions only. In contrast, our main finding of the *Rotation × Assessment Frame* interaction that occurs in the dependent variable *Low-Effort Audits* ( $p = 0.022$ , Table 2) remains a robust effect in *Low-Effort/Aggressive* as well ( $F = 11.71$ ,  $p < 0.001$ , above). Therefore, of our two main findings for *Low-Effort Audits* (H1a and H2), H1 is robust to the dependent variable *Low-Effort/Aggressive*.

<sup>c</sup> Auditors in the no-rotation (rotation) condition were randomly paired with a manager and remained with that manager for all rounds (were randomly paired with a different manager, at the beginning of each round).

<sup>d</sup> After choosing conservative or aggressive reporting, managers send auditors a nonbinding message about their choice. Auditors in the honesty assessment frame (dishonesty assessment frame) conditions then assess the chance that this message was honest (dishonest).

<sup>e</sup> *Low-Effort/Aggressive* equals the percentage of rounds where auditors choose low-effort audits and managers choose aggressive reporting.