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

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ABSTRACT

Standard setters are proposing new, controversial auditor rotation requirements with a stated objective to enhance audit quality through improved auditor skepticism, independence, and objectivity. However, while auditors are required to be independent and objective, their implementation of skepticism is allowed to vary. That is, their frame for evaluating managers’ representations can vary between skepticism (assessments of potential dishonesty) to assessments of potential honesty. Further, psychology theory suggests the auditor’s assessment frame may interact with the auditor’s familiarity with management. We use an experiment to test for this interaction between auditor skepticism and mandatory rotation. We find beneficial effects of mandatory rotation when an auditor assesses management’s honesty. However, these effects reverse when an auditor takes a skeptical mindset. This unanticipated, harmful effect of rotation informs regulators, auditors and researchers interested in mandatory rotation and skepticism. These findings are robust to both high and low levels of auditor interaction via unstructured chat, suggesting that our findings apply to both audit partner and firm rotation mandates.

Key Words: Auditor Rotation; Professional Skepticism; Audit Quality; Audit Failure; Game Theory
I. INTRODUCTION

We test whether the effects of auditor rotation on audit quality depend upon the mindset or mental frame (Hanson 2011) with which auditors evaluate managers’ representations about the financial statements. In recent discussions of mandatory rotation, the PCAOB (2011b) posits that mandatory audit firm rotation will increase audit quality through better independence, objectivity, and professional skepticism. However, while auditors are required to be independent and objective (AICPA 2012), their frame for evaluating managers’ representations can vary between assessments of potential honesty and assessments of potential dishonesty (i.e., skepticism). This variability in how auditors operationalize skepticism is important for the debate over auditor rotation, because psychology theory implies that the auditor’s assessment frame may interact harmfully with auditor rotation, leading to an unexpected decrease in audit quality.

Regulators and others are increasingly concerned that auditors are unintentionally biased towards management’s wishes (Doty 2012, 5). Ongoing audit inspections in the U.S. and around the world, as well as recent financial scandals have fueled concerns that auditors possess insufficient skepticism, objectivity, and independence, and that audit quality may deteriorate with long-term auditor-client relationships (DeFond and Francis 2005; PCAOB 2011a). As a solution to this risk, standard setters have proposed and implemented various forms of mandatory auditor rotation. In the U.S., the SEC requires rotation of audit engagement partners and concurring review partners (SEC 2003), and the GAO (2003) has recommended that the SEC consider making audit firm rotation mandatory. Recently, the PCAOB (2011a, 2011b) announced that it is considering audit firm rotation rules as a means to enhance audit quality. Former SEC Chief Accountant Lynn Turner argues that existing rotation requirements in the U.S. are insufficient, and that new firm rotation requirements will enhance professional
skepticism and audit quality (PCAOB 2011a; Hall 2011). Following the PCAOB’s announcement in the U.S., the European Commission announced its intention to pursue new rotation mandates (Dalton 2011; Brunsden 2011). On the other hand, audit firms have strongly objected to mandatory auditor rotation, arguing that the loss of experience with the client reduces audit quality (e.g., PwC 2011; Myers et al. 2003). The AICPA, likewise, opposes the new rotation requirements (AICPA 2011b).

In its concept release, the PCAOB (2011b, footnote 2) argues that audit firm rotation will increase audit quality through improved auditor independence, objectivity, and professional skepticism. However, regulators apparently do not consider that the frame the auditor uses when assessing management’s representations (e.g., managers’ explanations for unexpected fluctuations observed during analytical procedures) can vary both cross-sectionally and intertemporally among auditors, audit teams, audit firms, audits, and parts of audits. Formally or informally, auditors can assess representations in terms of their potential honesty (e.g., client integrity assessments, AICPA 2007; COSO 1992; PCAOB 2007), or in terms of their potential dishonesty (e.g., fraud risk assessments, AICPA 2011a). Auditing standards describe professional skepticism as an attitude that includes a questioning mind and an awareness of the possibility of fraud (AICPA 2011c). Nelson (2009, 4) defines professional skepticism as “a heightened assessment of the risk that an assertion is incorrect.” In practice, auditors choose whether to frame their evaluations of managers in terms of their honesty or their dishonesty (e.g., Peecher 1996; AICPA 2007; COSO 1992), and research suggests that auditors sometimes exercise such skepticism, but sometimes focus instead on the honesty of their clients (e.g., Peecher et al. 2010). Regulators increasingly advocate that auditors take a skeptical approach (Nelson 2009). For example, SAS No. 99 requires auditors to explicitly consider and be alert to
the possibility that management is dishonest. More recently, the PCAOB (2008, 2011b, fn. 11) and its Chairman (Doty 2012) indicate that auditors too often rely on the honesty of their clients, and the Panel on Audit Effectiveness (PAE 2008) calls for auditors to focus more on the possibility of management dishonesty. Importantly, psychology theory suggests that this skeptical assessment frame may interact detrimentally with auditor rotation. In fact, using these theories, we predict that a skeptical mindset for rotating auditors may actually increase the auditor’s belief that management’s representations are honest, thus reducing audit quality.

Building upon and extending theories from psychology and economics, and drawing upon prior research in accounting (Bowlin et al. 2009; King 2002), we examine the effects of rotation and assessment frame using an experiment designed according to the traditions of experimental economics. Student participants assume the roles of auditors and financial reporting managers in an interactive audit setting. Managers select a level of financial reporting aggressiveness while auditors select a level of audit effort. We manipulate auditor rotation and assessment frame (honesty frame vs. dishonesty / skepticism frame) between subjects. We design the experiment such that managers are incentivized to entice auditors into providing low effort. Thus, audit failure occurs when auditors reduce their audit effort because they incorrectly assess managers’ honesty.

Consistent with our predictions, we find that the effects of auditor rotation on audit quality differ depending on how auditors frame their assessments of managers’ representations. Specifically, when auditors assess the honesty of managers’ representations (i.e., a client integrity frame), auditor rotation increases audit effort and decreases audit failure (low-effort audits paired with aggressive financial reporting, see Peecher and Piercey 2008 and DeAngelo 1981). However, when auditors assess the dishonesty of managers’ representations (i.e., a
skepticism frame), auditor rotation decreases audit effort and increases audit failure. Additional analysis demonstrates that auditors’ assessment of managers’ honesty mediates this reduction in audit effort. These findings imply that audit areas requiring high levels of skepticism (e.g., fraud risk assessments and the presumptive doubt assessment frame advocated by the PCAOB 2011b and other regulators, Nelson 2009) would suffer under a mandatory auditor rotation scheme, contrary to the intentions of regulators.

We also manipulate whether auditors have the opportunity to chat informally with managers. By chatting informally, auditors and managers endogenously increase their interpersonal interaction. We find that this chatting decreases audit effort but does not interact with our other independent variables. This suggests that our main findings are robust to auditors who have close interactions with the client (e.g., engagement partners), as well as those who do not (e.g., concurring partners and other audit team members). Therefore, the effects we find would likely occur for mandatory engagement partner rotation, concurring partner rotation, and rotation of all audit team members by mandatory firm rotation. In addition, robustness to the level of interpersonal chat indicates that the auditor bias we document is distinct from biases traditionally referenced in the mandatory rotation debate dependent upon unintentionally impaired independence (PCAOB 2011b; Bazerman 1997), coercion, persuasion, etc.

These findings have several implications for theory and practice. Standard setters are not likely to anticipate that the effects of auditor rotation on audit quality depend on whether auditors assess manager assertions through an honesty or dishonesty evaluation frame, and that their preferred, skeptical, assessment frame interacts harmfully with rotation. Our findings suggest that what standard setters might otherwise consider the beneficial effects of auditor-client rotation mandates could come at the cost of undoing what they would also see as the beneficial
effects of professional skepticism standards. Moreover, auditors should be aware of how these factors influence their judgment and decision-making. For example, auditors are unlikely to anticipate that the benefits of auditor skepticism depend on their ability to remain with the same client. Finally, auditors may not recognize that informal interactions with clients can cause them to assess manager assertions as more honest and to select lower levels of audit effort (see Bell et al. 2005). As detailed below, these findings extend prior experimental and archival literature on auditor rotation and skepticism.

II. THEORY

Background Research

Over the past decade, a large archival literature has studied the issue of 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 et al. 2008) find mixed results. For example, various studies conclude that rotation would have negative (e.g., Chen et al. 2008; Ghosh and Moon 2005; Gul et al. 2009; Myers et al. 2003; Mansi et al. 2004; Rice and Weber 2011), positive (e.g., Carey and Simnett 2006; Dao et al. 2008; Davis et al. 2009; Kealy et al. 2007), or mixed or no effects (e.g., Carey and Simnett 2006; Chen et al. 2008; Ghosh and Moon 2005; Chi et al. 2009; Knechel and Vanstraelen 2007; Ruiz-Barbadillo et al. 2009) on audit quality. Myers et al. (2003) and Cameran et al. (2008) note that self-selection bias limits the extent to which archival findings can speak to the effects of auditor rotation on audit quality. For example, if more aggressive financial reporters swap auditors early in the auditor-client relationship, either by their own choice (e.g., “opinion shopping”), or by the choice of the auditor (e.g., client retention decisions), this self-selection bias would tend to drive aggressive financial reporters out of the later years of the auditor-client relationship. This would lead to a
positive relationship between the length of the auditor-client relationship and earnings quality, but not necessarily because the quality of the audit itself is improving over time. The PCAOB’s (2011b) recent concept release on mandatory firm rotation notes this self-selection in the literature and concludes that it confounds the conclusions that can be made about audit quality, long-term audit relationships, and auditor rotation. We extend this research by using an abstract experimental method that eliminates self-selection bias.

A small stream of experimental research has also examined the effects of rotation on audit quality. Dopuch et al. (2001) find that rotation discourages auditors from biasing their audit opinions in favor of management. Auditors in this study have incentives to provide a biased audit report, meeting the wishes of management. Thus, the study examines the problem of auditors compromising their independence to intentionally fulfill management’s desires because doing so is economically rational (98). Our study extends this research in three ways. First, in our study, the auditors’ highest payoff is to select low audit effort only if managers do not report aggressively. Managers’ highest payoff is to report aggressively only if auditors select low effort audits, providing managers with the incentive to mislead auditors. Thus, similar to King (2002) and Bowlin et al. (2009), audit failure in our study results when auditors incorrectly and unintentionally succumb to management, a situation of concern for regulators (Doty 2012). Second, we focus on assessments of management, selected levels of audit effort, and unintentional audit failures, rather than intentional audit report bias. Thus, our study may be more relevant to audit failures involving auditor negligence, which is more common than those involving deliberate audit report bias (Messier et al. 2011). Finally, we examine a longer retention period as encouraged by Dopuch et al. (2001, 116).
Wang and Tuttle (2009) find that rotation leads auditors to be less cooperative with managers in negotiations over reported asset values. Both this study and Dopuch et al. (2001) call for future experimental research to examine other aspects of auditor rotation. For example, 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.

The Auditor-Client Relationship

Our model views the auditor-client relationship as a strategic game, consistent with prior analytical and experimental research (e.g., Bowlin 2011; Bowlin et al. 2009; Fellingham and Newman 1985; Kachelmeier 1991; Mayhew and Pike 2004; Newman et al. 2005; Newman et al. 1996; and King 2002). Within this framework, managers and auditors respectively make financial reporting and auditing decisions based on their expectations of their opponents’ strategic choices (Bowlin et al. 2009). Specifically, auditors choose between more costly, higher effort audits and less costly, lower effort audits based upon their expectations of managers’ choices between conservative or aggressive financial reporting.

As Table 1 shows, each player earns points based on known probability distributions that depend on the auditors’ and managers’ joint choices (e.g., King 2002). These probabilistic outcomes give each player limited insights into the other player’s choices even after outcomes have been determined and reported. This is consistent with practice, where auditors generally do not immediately (or perhaps ever) know managers’ financial reporting choices with certainty.
We designed the game (payoffs and their probabilities) such that greater audit effort leads, imperfectly, to greater misstatement detection. Thus, as detailed in Table 1, misstatements are detected 90% (10%) of the time when auditors pick high (low) effort. This audit effort is costly to the auditor, resulting in a reduction in all auditor payoffs when the auditor exerts high effort. Misstatements are more likely when the manager reports aggressively rather than conservatively (70% versus 30% probability).

When the auditor exerts high effort, s/he receives 6 points when misstatements are present versus 4 points when they are absent. However, when s/he exerts low effort, s/he receives only 1 point when a misstatement is present versus 10 points when it is absent. The net effect of this design is that the auditor’s highest expected payoff and highest possible payoff occurs when the auditor expends low effort and the manager chooses conservative reporting. The manager is always better off when misstatements are not detected, and has a higher possible payoff when s/he chooses conservative reporting (10 points) versus when s/he chooses aggressive reporting (6 points). Thus, the manager’s highest possible and highest expected payoff comes when the manager manipulates earnings significantly and the auditor chooses to exert low effort.

This design, while not intended to mimic payoffs in practice, nevertheless creates the key tension that mandatory rotation would presumably alleviate—managers want to convince auditors that the financial statements are free of misstatements and auditors would prefer to

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1 Managers likely have a relative advantage at imperfectly observing auditors’ effort in practice, compared to auditors’ ability to infer managers’ private financial reporting choices (Bowlin et al. 2009; King 2002). The more extreme probabilities for managers’ payoffs (compared to auditors’) in Table 1 confer this imperfect information advantage to managers in the study.
believe them and exert less audit effort (Bowlin et al. 2009; King 2002). Notably, this auditor-client relationship 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 each auditor (manager) will choose from among his/her available strategies with probabilities that cause the manager (auditor) to be indifferent between his/her own available strategies (i.e., a mixed-strategy equilibrium).  

Our model also includes other features of the auditor-client relationship. First, while auditors in practice cannot directly observe the managers’ private financial reporting decisions, they will typically receive representations from managers about their financial reporting during the audit and adjust their testing accordingly (e.g., managers’ explanations for unexpected fluctuations observed by the auditor during analytical review and responses to auditors’ inquiries with managers regarding high-risk financial statement accounts or known internal control deficiencies). Second, auditors assess either the honesty or dishonesty of these representations. Third, interpersonal, informal discussions between the auditor and the client may or may not surround the managers’ representations about the financial statements (e.g., engagement partners versus concurring partners vs. other audit team members). Finally, we model the auditor-client relationship as a multi-period game in which an auditor and client must either maintain a long-term relationship or rotate frequently. We combine this model with theories from psychology that indicate the possibility that rotation and assessment frame will interact to build predictions about the joint effects of rotation and honesty vs. dishonesty frame on audit effort, as well as the effects of interpersonal chat.

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2 Game theory predicts a mixed-strategy equilibrium in our model in which the auditor chooses a high (low) effort audit with a probability of 58.9% (41.1%) and the manager chooses high (low) earning manipulation with a probability of 61.4% (38.6%).
Auditors’ Assessments of Managers’ Representations and Auditor Rotation

When managers make representations to auditors about the financial statements, the manager has private information about whether that representation is truthful. However, from the auditor’s perspective, assessing the likelihood that these representations are truthful or untruthful is a subjective probability assessment (French 1988) that is subject to unintentional psychological bias.

According to Support Theory (Brenner 2003; Brenner et al. 2002; Rottenstreich and Tversky 1997; Tversky and Koehler 1994), individuals will not assess the likelihoods of manager honesty and of manager dishonesty according to normative laws of probability, but rather will base them on the amount of subjective psychological “support” that is immediately accessible to the mind. For example, simply asking individuals to assess the likelihood that they might be seriously injured within the next year makes it relatively easy to conjure mental support for this risk. As a result, they elevate the subjective likelihood of injury in their minds and do not adequately consider the alternative complementary probability that they might not be injured within the next year. In contrast, asking individuals to assess the likelihood that they might have an injury-free year leads them to easily conjure mental support for this and inadequately consider the alternative possibility that they could be injured. As a common empirical test of this, Support Theory researchers test whether the two, separately assessed complementary probabilities (e.g., being injured and not being injured) exceed 100%, as an indication that individuals are over-imagining the probability posed to them. A widespread finding is that such probabilities sum to more than 100% in nearly all studies (see, e.g., Brenner et al. 2002, and Ayton 1997 for reviews). Support Theory is considered a unifying framework that helps explain a variety of psychological phenomena (e.g., Ayton 1997; Brenner et al. 2002; Peecher and Piercey 2008), including
hindsight bias or the “I-knew-it-all-along” effect (Fischoff 1975), and comparative ability assessment or the “above-average” effect (Svenson 1981).

Thus, individuals’ subjective probability assessments of an uncertainty tend to depend on how they pose the question to themselves. This is important in our setting (auditor assessments of management representations) because variance in an auditor’s mindset can arise both formally (e.g., COSO 1992; AICPA 2011a) and informally on different audit tasks. Standard setters have recently noted a tendency of auditors to focus on verifying the honesty of managers’ representations, and have encouraged auditors to evaluate them more skeptically in terms of their potential dishonesty (e.g., Doty 2012; PCAOB 2011b; AICPA 2011a). Our conversations with partners and other auditors indicate that auditors choose informally between skepticism-focused (assessing dishonesty) or client-integrity-focused (assessing honesty) mindsets on common audit tasks. 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, and adjust their audit tests for those accounts. Auditors may adopt a more skeptical mindset when participating in a fraud brainstorming session. An auditor can be in either mindset when determining when and how much oral inquiry or other audit evidence to collect, how to interpret it, and how much to rely on it, or how to reach conclusions about the control environment or the adequacy of substantive evidence for a large account. They can also rely on either mindset when assessing whether subjectively determined assurance levels are reached.

Thus, without considering further factors, a basic prediction from Support Theory would be that auditors assessing the honesty of managers’ representations will focus on the likelihood of honesty, conjure support for it relatively easily, but without similarly considering the
likelihood of dishonesty, and therefore select lower levels of audit effort. Those assessing
dishonesty will focus on its likelihood, conjure support for it, but without similarly considering
the likelihood of honesty, and select higher levels of audit effort.\textsuperscript{3}

\textit{Long-Term Auditor-Client Relationships and Honesty versus Dishonesty Assessments}

The features of a long-term auditor-client relationship make this basic prediction likely.
In such a relationship, assessments of the honesty or dishonesty of a manager occur in a setting
in which the auditor has experience with the client and his or her representations. This
experience would tend to magnify the ability of auditors with an honesty focus to imagine that a
particular representation might be true, while also magnifying the ability of auditors with a
skepticism focus to imagine that the same representation might not be true.

\textit{Mandatory Auditor Rotation and Honesty versus Dishonesty Assessments: A Reverse Effect}

Next, we suggest that this basic prediction might depend further on auditor rotation, and
in ways that neither auditors nor standard setters would anticipate. Specifically, Support Theory
and several related theories within the framework of Support Theory suggest that the effects just
discussed could diminish, or even reverse, when individuals lack experience with the target of
the subjective probability assessment. For example, the “above-average effect” occurs when
people easily conjure accessible support for the likelihood that they might be better at a task than
other, similar people might, without adequately considering the alternative probability that the
other, similar people might also be good at the task for similar reasons. In contrast, Kruger
(1999) found a reverse effect, such that, while individuals exhibit this effect for simple tasks,

\textsuperscript{3} This is a mistake, since auditors’ probability assessments should be independent of how they frame the question to
themselves. This is merely assessing alternative sides of the same coin; as far as professional standards are
concerned (e.g., COSO 1992), by assessing one, the auditor should be implicitly assessing the other. Thus, auditors
assess the honesty of management’s representations using the frame they have been given. These assessments lead
directly to auditors’ selection of level of audit effort. Auditors’ mistakes in assessing management’s honesty result in
inappropriately low audit effort, since management benefits by convincing auditors they will not manipulate
earnings.
they will also exhibit a reverse effect (i.e., a “below-average effect”, or systematically underestimating one’s relative performance) for other tasks in which they lack experience.

Similarly, hindsight bias or the “I-knew-it-all-along effect” occurs when individuals with knowledge of the outcome of an uncertainty (e.g., almanac question, Christensen-Szalanski and Willham 1991) overestimate the likelihood that they would have expected the outcome. However, Ofir and Mazursky (1997) found that hindsight bias was reduced or even reversed into a “reverse hindsight bias” (i.e., I never would have known that) when the outcome seemed surprising. This suggests that, when individuals sense conspicuous inexperience with the subject, people systematically underestimate the probability that they would have been able to predict the outcome, compared to their actual performance. This effect presumably occurs because of the apparent difficulty of finding accessible support for the likelihood of performing well on a subject with which one lacks experience.

Finally, Macchi et al. (1999) tested individuals’ ability to find support for various probability assessments in a traditional Support Theory setting, but when they were inexperienced with the subject. For example, Macchi et al. (1999) asked half of the participants to assess the likelihood that the freezing point of alcohol is more than that of gasoline, and half to assess the reverse probability (that it is less than that of gasoline). Unlike the conventional predictions of Support Theory, which would suggest that people posed a question about which they do have experience would over-imagine its probability, causing the two probabilities to sum to more than 100%, people posed a question about which they do not have experience seem to under-imagine its probability. Because participants in Macchi et al. (1999) were inexperienced with the subject they were assessing, they had relative difficulty conjuring support for the version of the question posed to them. In contrast to the widespread finding of Support Theory
studies, their assessments of the two complementary probabilities often summed to less than 100%. Overall, these effects suggest that the tendency to easily find accessible support for a subjective probability assessment can diminish and potentially even reverse when individuals are noticeably inexperienced with the target of their assessment.

This psychological effect is likely to occur in audit practice. Experience with a client likely breeds confidence in an auditor’s ability to assess the client’s honesty or dishonesty, whichever trait is the focus of that particular auditor’s assessment. Thus, within a long-term auditor-client relationship, auditors with an honesty focus will be more likely to believe managements’ representations are honest and engage in lower-effort audits than auditors focusing on dishonesty. However, this effect is likely to diminish and potentially even reverse when auditors are less experienced with their clients due to auditor-client rotation. Under these conditions, auditors may feel more aware that a manager’s honesty (or dishonesty) is a latent character trait that is difficult to observe in a mere acquaintance, particularly given the limited extent to which an auditor can truly know a manager’s character when annual audits with multiple clients are frequently rotated. That is, rotation may lead auditors in practice to unconsciously sense just how little they really know about their clients’ private personal integrity. Therefore, when assessing the likelihood that a client’s representation is honest (dishonest), an auditor sensing little experience with the client will encounter less ease conjuring support for the premise that the representation is truthful (false). As a result, when auditors consider one aspect of a client with whom they have little experience, they may be relatively more likely to consider the alternative possibility. Thus, the effects of an auditor’s skepticism focus (vs. honesty focus) will likely depend on auditor rotation as depicted in Figure 1. This suggests the following hypothesis:
H1: The effect of auditor-client rotation on audit effort will depend on the auditor’s assessment frame such that without rotation, auditors will select fewer low-effort audits when assessing the dishonesty of managers’ representations than when assessing the honesty of those representations. However, when auditors rotate, they will select more low-effort audits when assessing the dishonesty of managers’ representations than when assessing the honesty of those representations.

If supported, this theory suggests an interaction with interesting implications for both research and practice. Specifically, rotation would have positive effects on audit effort when auditors are evaluating the potential honesty of managers’ representation, but would also have negative effects when auditors use a skepticism focus (Figure 1).

Informal, Interpersonal Interactions Surrounding Managers’ Representations

In practice, auditors at different levels and in different roles have varied degrees of informal interpersonal interactions with managers while evaluating their representations. Engagement partners have very high levels of interaction with managers, concurring partners have (by definition) virtually no interaction, and other audit team members’ interaction can range from very high levels to very low levels depending upon their role in the firm. Manipulating the availability of informal chat in our experiment allows us to test the robustness of H1 to auditors with both high and low levels of interaction with managers. Such a robustness test can be helpful in determining whether the potential unintended consequences of mandatory rotation would 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 likely have a stronger, informal relationship with management.

We predict that these chat interactions will result in less audit effort. Bazerman et al. (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 expand King’s (2002) research by allowing this puffery, and, in addition, 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, when wanted, 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 intuitively feel that they have enough comfort to believe managers’ representations. In both cases, auditors are likely to feel a greater, but illusory amount of control (Langer 1975) over the actions of their clients. Thus, auditors who have the option to chat may sense that they have more tools to assess when they can believe managers, as needed. 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’ overall belief in the honesty of managers’ representations and increase auditors’ tendencies to select lower audit effort. This suggests the following hypothesis:

**H2:** The opportunity to engage in interpersonal chat with managers will lead auditors to believe their client’s representations in the financial statements more and to select lower levels of audit effort.

Support for H2 would have practical implications for auditors, who should understand what factors besides the financial statements influence their beliefs about the financial statements and selected 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 (PCAOB 2009; Peecher et al. 2010).
Note that H2 focuses on the *availability* of informal chat with a client. Whether the *content* of that chat affects auditors’ belief in management and audit effort is a separate research question. Because of the difficulty in predicting the content of free and unstructured chat, and the relative scarcity of prior research, additional effects of chat are open empirical research questions in our study.\(^4\) For example, it is unclear whether the content of chat in this setting will lead auditors to feel that they know the managers, point out how little they actually know about them, or neither. As a result, we do not necessarily predict that unstructured chat would create the type of interactive effects that we predicted auditor rotation would create in H1. Our design, however, allows us to test for these and other possible effects of unstructured chat on audit effort and audit outcomes. In particular, our design allows for a robustness check of whether the effects we posit in H1 would be likely to occur across different types of audit-client relationships.

**III. METHOD**

**Participants**

The experiment uses an abstract two-player game under the traditions of experimental economics. Because of the abstract nature of this task, students are appropriate participants (Haynes and Kachelmeier 1998, Libby et al. 2001). Our sample consists of 226 undergraduate student volunteers from a large university.\(^5\) Each participant was assigned to the role of either an auditor or a manager. Each participant earned points based on game outcomes as shown in Table 1. These points were converted into cash payments at the end of the experiment.\(^6\)

\(^4\) 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 1999). Ellingsen and Ostling (2010) find that behavior can diverge between one-way and two-way communication in a strategic setting, depending on the type of game being played.

\(^5\) Approval for the study was provided by the University’s Human Subjects Board.

\(^6\) Laboratory materials used neutral labels for players’ roles (i.e., “blue” and “green”) and their choices (e.g., “up” or “down” for different levels of audit effort, “left” or “right” for different levels of financial reporting quality). The
Experimental 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 frame vs. skepticism / dishonesty frame), and availability of unstructured chat (chat vs. no chat). Participants received oral instructions (with paper copies) and participated in the experiment over a computer network using z-Tree software (Fischbacher 2007).

Task Instructions

The instructions informed participants that they would be paid $5.00 for their participation plus $0.20 for every point that they earned. They were assigned to one of two roles in a 20-round game. Participants in the no-rotation (rotation) condition were told that they would be randomly paired with a player in the opposing role and remain with that player for all rounds of the game (they would be randomly paired with a player in the opposing role at the beginning of each round of the game). Because we wanted a clean test of the effects of auditor rotation, our manipulation follows 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” along the theoretical construct of interest. The instructions then explained the roles, choices, and payoffs for both players quizzed the participants on this information. Participants in the chat conditions then read an explanation that they would be able to “chat” online with the other player for one minute at the beginning of each round. Participants in the no-chat conditions did not receive these instructions.

In all conditions, participants were told that, after the manager submits her/his financial reporting decision, but before the auditor selects her/his level of audit effort, the manager would neutral labels control against role-playing, hypothesis guessing, or other demand effects, while still maintaining the economic realities and strategic tensions of the auditors’ and managers’ choices (Haynes and Kachelmeier 1998). For ease of exposition, we generally use the more meaningful terminology in this manuscript.
send the auditor a standardized message about the type of financial reporting s/he selected (adapted from King 2002). The instructions further told all participants that the managers could choose whether or not to be truthful in these representations (see King 2002). Having all managers make a representation about their financial reporting choice enables us to manipulate whether auditors assess the probability that this representation is honest or dishonest.

Next, the instructions told all participants that they would find out how many points they earned at the end of each round. Specifically, they would learn only their own point outcome. As noted earlier, however, players can infer incomplete information about the other players’ choices based on the amount of points that they earned. Finally, the instructions told participants that they would complete a post-experimental questionnaire and receive payment.

**Computerized Instrument and Experimental Task**

After reading the instructions, participants began the z-Tree program and were assigned to the role of either auditor or manager. The program also reminded those in the no-rotation (rotation) conditions that they would be paired with the same (a different) player each round. After participants learned their roles, auditors read a screen reminding them that the manager would send them a message about which action he or she had chosen, and that this information would not necessarily match that manager’s actual choice. It then told the auditors in the honesty assessment (dishonesty assessment) condition that they would rate the likelihood that the manager’s message is honest (dishonest).

Meanwhile, managers read a screen reminding them of information from the instructions about the strategic possibilities available to them in the game. In a similar auditor-manager game, King (2002) conducted a separate training session with managers before the experiment to help ensure that managers would engage in opportunistic behavior and select aggressive reporting
relatively frequently. While we did not conduct separate training sessions, our instruction screen does implement a more modest adaptation of King’s approach. Specifically, we reminded managers 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 reminded them that they could send messages to auditors about which action they would choose, such as saying they picked “left”, but that they were not bound by the message they sent (as in King 2002), and could actually pick “right”. Although auditors did not read this additional screen, all of this information was available to all players in the instructions, and we reminded managers that the other players were aware that their standardized messages might not be true. Similar to King (2002), we provided this additional instruction to help ensure that managers would engage in opportunistic behavior, better enabling us to observe auditors’ ability to infer their clients’ opportunism, by experimental condition.

After completing a quiz over the instructions, participants started the 20-round game. At the beginning of each round, participants in the chat conditions could chat online for one minute. Next, managers in all conditions made their choices for the round and then sent auditors a standardized message indicating which of their two options they (claimed to have) selected for the round. Auditors in the honesty assessment (dishonesty assessment) conditions then assessed the chance that this message was honest (dishonest), and then all auditors selected their level of audit quality for the round. Once both players had made their choices, each player learned the number of points that he or she earned that round (see Table 1). After all 20 rounds, participants

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7 All managers and auditors responded to a quiz of eight true/false questions testing their understanding of the game (e.g., whether they would stay with the same player, how many points they could earn for a certain combination of choices, whether the manager’s message must match his or her choice, etc.). After completing the quiz, participants immediately received feedback for each question, including explanations of any quiz questions answered incorrectly.
completed a post-experimental questionnaire, and were paid based on the total points earned. Individual payouts ranged from $18.80 to $40.60, and averaged $26.78.

**Dependent Variables and Other Supplementary Data**

Our primary dependent variable is auditors’ selected level of audit quality, *Low Effort Audits*. It is equal to the percentage of rounds in which auditors select low-effort audits, averaged over all 20 rounds for each participant (thus, one observation per participant). We also examine process and supplementary data. We test whether auditors’ assessments of the probability of manager honesty and dishonesty exceed 100% in the rotation and no-rotation conditions. Since our theory suggests that variation in this should influence auditors’ *Low Effort Audits*, we also examine this as a potential mediator. Additionally, we examine *Audit Failure Outcomes*, the proportion of audits that result in aggressive reporting paired with low-effort audits (e.g., King 2002). This particular accounting outcome is associated with audit failures that have legal, regulatory, and business implications for auditors (e.g., Kadous 2001; Peecher and Piercey 2008), and is an important component of audit quality (DeAngelo 1981). Auditing standards (e.g., mandatory auditor rotation) and other regulations (e.g., the Sarbanes-Oxley Act) explicitly or implicitly seek to minimize the frequency of this outcome, and so experimental conditions that make this outcome more or less frequent can help standard setters better understand the implications of both proposed and adopted standards (Kachelmeier and King 2002). Finally, we also examine the contents of participants’ unstructured chat.

**IV. RESULTS**

**Tests of H1**

To test H1, we conduct an ANOVA and planned contrast tests on the dependent variable *Low Effort Audits*. H1 predicts a disordinal interaction between rotation and assessment frame on
**Low Effort Audits.** We find support for this interaction in the ANOVA in Table 2 Panel A. Specifically, the \( \text{Rotation} \times \text{Assessment Frame} \) 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 2 Panel B. Specifically, under no-rotation, auditors select low effort audits more frequently when assessing the honesty of managers’ representations than when assessing their dishonesty (63.1% vs. 53.6%, \( F = 2.70, p = 0.052 \)). However, under rotation, auditors select 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 assessed the honesty of managers’ representations (63.1% vs. 53.0%, \( F = 3.14, p = 0.040 \)). However, rotation harms the audit by increasing the frequency of low effort audits when auditors assessed the dishonesty of managers’ representations (53.6% vs. 66.2%, \( F = 4.25, p = 0.021 \)). These findings support H1. They suggest that mandatory auditor rotation may have harmful unintended effects on audit effort, potentially reversing what regulators would otherwise see as the potential benefits of a skepticism assessment frame on audit effort.

![Insert Figure 2 and Table 2 here]

**Judgment Process Analysis for H1**

Our theoretical development of H1 suggests that auditors’ probability assessments in the honesty assessment conditions and in the dishonesty assessment conditions would sum to more than 100% in the no-rotation conditions, but would not in the 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, within the rotation conditions, these complementary assessments do not sum to more than 100%.
These differences in probability assessments in the rotation and no-rotation conditions are statistically significant (F = 4.12, p = 0.022). Thus, in the rotation condition, as we predict, results run counter to widely replicated Support Theory effects consistent with Macchi et al. (1999).

Our theoretical framework suggests that this phenomenon will be a driving factor in the joint effects of rotation and assessment frame on auditors’ selected levels of audit effort (H1). Specifically, when auditors assess management honesty as high, they reduce their audit effort. To examine this, we determined whether auditors’ assessments of the likelihood of manager honesty (vs. dishonesty) significantly mediates the effect of the Rotation × Assessment Frame interaction on Low Effort Audits ($\beta = 0.057$, $t = 2.72$, $p = 0.004$, Figure 3). The mediator is equal to auditors’ assessments of the likelihood of management honesty for auditors in the honesty assessment conditions, and 100% minus auditors’ assessments of management dishonesty for auditors in the dishonesty assessment conditions. Figure 3 Panel A shows this mediation test (Morgan-Lopez and MacKinnon 2006). First, Rotation × Assessment Frame similarly influences the potential mediator in a disordinal interaction ($t = 2.03$, $p = 0.022$). Second, the potential mediator, in turn, influences auditors’ selection of Low Effort Audits ($t = 5.45$, $p < 0.001$). Finally, in the presence of the mediator as a covariate, the effect of the Rotation × Assessment Frame interaction on Low Effort Audits is significantly reduced (from $\beta = 0.057$ to $\beta = 0.037$, Goodman test = 1.93, $p = 0.029$; MacKinnon et al. 1995), indicating significant mediation. This interaction is now

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8 The test of this disordinal interaction on the mediator ($t = 2.03$, $p = 0.022$) is statistically equivalent to the above test that the sum of auditors’ honesty and dishonesty probability assessments in the no-rotation conditions (108.0%) and in the rotation conditions (95.5%) are significantly different from one another (F = 4.12, p = 0.022, above). Thus, this mediation model is a test of the extent to which the influence of rotation and assessment frame on the additivity of auditors’ assessments of the complementary probabilities of managers’ honesty and dishonesty (above or below 100%), in the manner predicted by our theory, is a driving factor in the effects we observe in Low Effort Audits (H1). These mediation results suggest that it is.

9 All of our mediation tests results are robust to the Goodman, Aroian, and Sobel tests (MacKinnon et al. 2005).
marginally significant ($\beta = 0.037$, $t = 1.95$, $p = 0.054$), indicating an overall significant partial mediation.

[Insert Figure 3 here]

There are two potential reasons for partial (rather than full) mediation. First, there is a difference between auditors’ stated reliance on management honesty and their actual reliance on management honesty (i.e., their actual selection of low effort audits, the choice that is incentivized by actual monetary payoffs). Peecher et al. (2010) find that there are differences in auditors’ stated and actual reliance on management’s representations, and Piercey (2011) finds that requiring auditors to explicitly record their stated assessments (as we did) accentuates those differences. Second, while we expect that managers would generally tell auditors they picked conservative reporting, managers might sometimes acknowledge aggressive reporting. In such cases, auditors assessing that representation as honest would not be likely to select low effort audits, as the mediation model in Figure 3, Panel A suggests. 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%, median 75%). Full mediation would be most likely if we restrict our analysis to managers using their representations to claim conservative reporting.

We find this to be the case (Figure 3, Panel B). When we examine only the half of the sample in which managers claim conservative reporting most frequently, we find the same relationships among the variables in the mediation model, except now with full mediation. The effect of the $Rotation \times Assessment \ Frame$ interaction on $Low \ Effort \ Audits$ decreases significantly (Goodman test = 1.99, $p = 0.023$) and becomes insignificant in the presence of the mediator ($t = 1.05$, $p = 0.30$). Thus, we find that the influence of rotation and assessment frame
on auditors’ assessments of the probability of managers’ honesty is a driving factor in the effects we observe in *Low Effort Audits* (H1), consistent with our theoretical development of H1.

**Tests of H2**

H2 predicts that the option to engage in unstructured chat will increase auditors’ overall tendency to believe managers’ representations and to engage in low effort auditing. As the ANOVA in Table 2 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). These results support H2. We do not detect any evidence of higher-order interactions involving unstructured chat. This indicates that our main findings for H1 generalize regardless of interpersonal interaction between auditors and managers during the audit, suggesting that the potentially unintended consequences of auditor rotation would generalize to the judgment and decision-making of a variety of audit team members with various levels of opportunity to interact informally with managers (e.g., engagement partners vs. concurring partners and other audit team members).

**Judgment Process Analysis for H2**

We also find that auditors’ assessments of manager honesty partially mediate the main effect of *Unstructured Chat* on *Low Effort Audits* (not tabulated). Specifically, *Unstructured Chat* has a positive main effect on auditors’ assessments of management honesty (t = 4.41, p < 0.001), which, in turn, influences *Low Effort Audits* (t = 5.45, p < 0.001). Finally, the main effect of *Unstructured Chat* on *Low Effort Audits* (β = 0.102, t = 4.90, p < 0.001) is significantly reduced in the presence of the mediator (from β = 0.102 to β = 0.059, Goodman test = 3.46, p <
0.001), but remains significant ($\beta = 0.059$, $t = 2.92$, $p = 0.004$), indicating significant partial mediation.\(^{10}\)

**Supplemental Analyses of H2**

Additional analysis of the content of chat confirms that it is a reflection, rather than a driver of our results, consistent with our theory for H2. Further, 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. In an untabulated ANOVA, 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 ($F = 4.11$, $p = 0.048$). This could be a reflection of auditors in the rotation condition having less experience with the manager relative to those in the no-rotation conditions, which would be consistent with our theory predicting H1. However, we do not find that the total volume of chat elevates *Low Effort Audits* ($r = -0.003$, $p = 0.982$).

We also used two independent coders to identify attempts to persuade or encourage the other player to be honest. The coders were unfamiliar with the hypotheses and resolved any differences in their coding together. We detect a significant ordinal rotation $\times$ assessment frame interaction such that auditors use more of these attempts when jointly in the rotation and dishonesty assessment conditions ($F = 8.25$, $p = 0.006$). This 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 the

\(^{10}\) There is a noteworthy difference between the mediation analysis of *Rotation $\times$ Assessment Frame* (Figure 3) and this mediation analysis of *Unstructured Chat*. In the mediation analysis of *Rotation $\times$ Assessment Frame*, the effect of the *Rotation $\times$ Assessment Frame* interaction on the mediator is statistically equivalent to the test of *rotation* influencing whether the sum of auditors’ honesty assessments and dishonesty assessments (above and below 100%) significantly differ. In contrast, the effect of *Unstructured Chat* on the mediator is not equivalent to this test. Thus, consistent with our theory, the joint effect of *Rotation and Assessment Frame* on the additivity of auditors’ assessments of the complementary probabilities of managers’ honesty and dishonesty appears to be a significant driver of our results in H1, whereas a simpler effect of *Unstructured Chat* on assessments of manager honesty appears to be a significant driver of our results in H2.
unintended effects of rotation in our tests of H1. We again do not find that the total volume of these types of persuasion attempts influences Low Audit Effort \( (r = 0.09, p = 0.495) \), suggesting that this is a reflection rather than a driver of our main findings. Moreover, we generally do not find significant relationships between any aspects of the contents of chat and Low Audit Effort. These findings suggest that it is the opportunity to engage in chat rather than its contents that elevates Low Effort Audits.

**Supplementary Analysis of Audit Failure Outcomes**

As a supplementary analysis to H1, we also examine the effects of auditor rotation and assessment frame on a measure of audit failure. Specifically, we examine the concurrence of an auditor’s low-effort audit choice and a manager’s aggressive reporting choice (*Audit Failure Outcomes*), an outcome associated with accounting failures (Peecher and Piercey 2008). We use Low Effort Audits as our primary tests of H1, above, since this variable is most directly under the control of the auditor. In contrast, Audit Failure Outcomes includes both auditor and manager behavior. That said, the frequency of audit failures is an audit outcome variable that is particularly important to minimizing social costs associated with accounting failures. We expect the frequency of these outcomes to be influenced by the same phenomena we discuss in our predictions for H1. Accordingly, we examine whether the unintended consequences of mandatory auditor rotation on audit effort extend to the relative frequency of accounting failures.

Figure 4 shows the frequency of audit failures, as percentage of all final outcomes. The ANOVA in Table 3 Panel A shows that the Rotation × Assessment Frame interaction remains robust to this variable \( (F = 11.71, p < 0.001) \). Furthermore, as Figure 4 and Table 3 Panel B show, the results on audit failure outcomes are in the expected direction and generally replicate our prior results, consistent with H1. This further demonstrates that mandatory auditor rotation
appears to undo and even reverse what standard setters would otherwise see as the benefits of
auditor skepticism on financial reporting outcomes associated with audit failure.\(^{11}\)

\[\text{Insert Figure 4 and Table 3 here}\]

V. CONCLUSION

Our study sheds light on an important policy question that has been the subject of
considerable debate—whether mandatory rotation of auditors 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). Thus, we extend
the rotation literature by demonstrating a harmful interaction between rotation and skepticism.
Our study uses the comparative advantages of experimentation, including active manipulation of
auditor rotation, randomization of other confounding factors, and the ability to directly measure
audit process variables that are unobservable in extant data. We develop a model of the auditor-
client relationship that includes attributes of audit quality, including auditor effort, auditors’
assessment of the honesty of managers’ representations, and final financial reporting outcomes
associated with audit failures. We also extend a theory which suggests that the effects of auditor
rotation is likely to depend upon the critically linked variable—professional skepticism—in ways
that neither auditors nor standard setters likely anticipate. That is, we find evidence of a
disordinal interaction such that mandatory auditor rotation tends to increase audit quality for

\(^{11}\) As another test of our theory, we examine an alternative measure of auditors’ tendency to be unintentionally
misled by managers. Specifically, we measure the difference between managers’ assessments of manager honesty
(or dishonesty) and managers’ actual honesty (or dishonesty). An untabulated ANOVA finds that the Rotation \(\times\)
Assessment Frame interaction remains robust to this variable (\(F = 4.62, p = 0.017\)). Furthermore, results on auditor
honesty accuracy are in the expected direction and generally replicate our prior results above with statistical
significance. This supplemental analysis suggests that the unintended consequences of mandatory auditor rotation
extend to the accuracy of auditors’ assessments of managers’ representations. Overall, our results across several
dependent variables suggest an interaction between auditor rotation and auditor skepticism on audit quality that is
consistent with our theory, but inconsistent with the intention of auditor rotation mandates.
audit judgments focused on the likelihood of managers’ honesty, but tends to decrease audit quality for audit judgments focused on a likelihood of their dishonesty.

Our findings also suggest that rotation creates unintended consequences 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 such skepticism would be undone under mandatory auditor rotation. Specifically, non-rotating auditors benefit most from a skepticism orientation, in terms of audit effort, and, ultimately, audit failures. However, when rotation is imposed, we find that the benefits of a skepticism mindset disappear in auditors’ selected levels of audit effort, and final audit outcomes associated with audit failure. This suggests a new potential cost to the loss of the long-term relationship with mandatory auditor rotation. That is, the intended benefit of a skeptical orientation that focuses on the possibility of client dishonesty can depend on the auditor’s ability to remain with the same client. Auditors are not likely aware of this issue because it is a subtle psychological effect into 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.

We also find that this effect occurs independent 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 very high opportunity 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 particular judgmental effects we study would likely be found in rotation of an engagement partner, rotation of a concurring partner, or rotation of an entire audit team.

In supplemental analysis, we also find that the opportunity to interact informally with clients exacerbates auditors’ assessments of management honesty and the frequency of low-effort auditing. Auditors should be aware that the opportunity to chat with a manager can potentially increase auditors’ sense that they can believe managers, even beyond the content of what management actually says to them during that interaction. This finding suggests a potential advantage to concurring partners, who do not interact with the client and therefore would be less likely to be influenced by that opportunity, contributing new evidence supportive of the recent PCAOB (2009) auditing standard increasing the role of concurring partners (Peecher et al. 2010). Expanding upon unstructured chat between auditors and clients may be a fruitful avenue for future research. Finally, we also contribute to the general judgment and decision making research on Support Theory, with additional theory and new evidence that rotation can moderate the additivity of subjective probability assessments.

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 are likely to exist that affect the relative costs and benefits of mandatory auditor rotation. Future research should consider other factors from the real world that we ignore. Despite these limitations, we provide a useful model and supportive evidence for future research on the effects of mandatory auditor rotation on audit quality.


Hanson, J. D. 2011. Keynote Address at the *10th Annual Sec Financial Reporting Conference of the Center for Corporate Reporting and Governance,* California State University, Fullerton, Irvine, CA, Sept. 23.


FIGURE 1
Hypothesis 1

Willingness to rely on management and engage in low-effort audits

- Dishonesty (skepticism) frame
- Honesty (client-integrity) frame

No auditor rotation (i.e., Long-term auditor-client relationship) vs. Auditor rotation (i.e., Short-term auditor-client relationship)
FIGURE 2
Low Effort Audits

This figure shows means for Low Effort Audits, the percentage of rounds for which auditors select low-effort audits.
FIGURE 3
Judgment Process

Panel A: Entire Sample

Assessments of Management Honesty\(^a\) → Low Effort Audits

Rotation \(\times\) Assessment Frame

Direct effect:
\(\beta = 0.057, t = 2.72, p = 0.004\)

Controlling for mediator:
\(\beta = 0.037, t = 1.95; p = 0.054\)

Mediation of effect (\(\beta\)):
Goodman test = 1.93, \(p = 0.029\).

Panel B: Half of Sample with Managers Claiming Conservative Financial Reporting More Frequently

Assessments of Management Honesty\(^a\) → Low Effort Audits

Rotation \(\times\) Assessment Frame

Direct effect:
\(\beta = 0.075, t = 2.09, p = 0.021\)

Controlling for mediator:
\(\beta = 0.037, t = 1.05, p = 0.30\)

Mediation of effect (\(\beta\)):
Goodman test = 1.99, \(p = 0.023\).

\(^a\) The mediator variable Assessments of Management Honesty is equal to auditors’ probability assessments of the honesty of managers’ representations in the honesty assessment frame conditions, and 100% minus their probability assessments of the dishonesty of managers’ representations in the dishonesty assessment frame conditions.
FIGURE 4
Audit Failure Outcomes: Low-Effort Audits and Aggressive Reporting Outcomes\textsuperscript{a}

\textsuperscript{a} This figure shows the percentage of audits that resulted in the combined outcome of low-effort audits with aggressive reporting (e.g., King 2002).
| Auditors' Audit Quality Choices: | \begin{tabular}{l|c|c}
\hline
| Conservative Reporting | Aggressive Reporting |
\hline
\multicolumn{2}{l|}{Managers' Financial Reporting Choices:} \tabularnewline
\hline
Low Effort & \multicolumn{2}{l|}{\begin{tabular}{l|c|c}
\hline
| Misstatement detected [payoff (probability)] & 4 (10%) & 1 (10%) \\
| Misstatement not detected [payoff (probability)] & 6 (90%) & 10 (90%) \\
| Expected value & 5.8 & 9.1 \\
\hline
\end{tabular}} \\
\hline
Auditors' payoffs: & \multicolumn{2}{l|}{\begin{tabular}{l|c|c}
\hline
| No misstatement [payoff (probability)] & 10 (70%) & 10 (30%) \\
| Misstatement [payoff (probability)] & 1 (30%) & 1 (70%) \\
| Expected value & 7.3 & 3.7 \\
\hline
\end{tabular}} \\
\hline
High Effort & \multicolumn{2}{l|}{\begin{tabular}{l|c|c}
\hline
| Misstatement detected [payoff (probability)] & 4 (90%) & 1 (90%) \\
| Misstatement not detected [payoff (probability)] & 6 (10%) & 10 (10%) \\
| Expected value & 4.2 & 1.9 \\
\hline
\end{tabular}} \\
\hline
Auditors' payoffs: & \multicolumn{2}{l|}{\begin{tabular}{l|c|c}
\hline
| No misstatement [payoff (probability)] & 4 (70%) & 4 (30%) \\
| Misstatement [payoff (probability)] & 6 (30%) & 6 (70%) \\
| Expected value & 4.6 & 5.4 \\
\hline
\end{tabular}} \\
\hline
\end{tabular} |
### TABLE 2
*Low Effort Audits*

**Panel A: ANOVA**

<table>
<thead>
<tr>
<th>Source</th>
<th>Sum of Squares</th>
<th>df</th>
<th>F</th>
<th>p-value</th>
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</thead>
<tbody>
<tr>
<td>Rotation</td>
<td>0.004</td>
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<td>0.09</td>
<td>0.761</td>
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<tr>
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**Panel B: Pairwise Contrasts**

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<th>F</th>
<th>p-value</th>
</tr>
</thead>
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<td>Effect of Assessment Frame under No-Rotation</td>
<td>0.130</td>
<td>1</td>
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<td>Effect of Assessment Frame under Rotation</td>
<td>0.231</td>
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<td>Effect of Rotation under an Honesty Assessment Frame</td>
<td>0.151</td>
<td>1</td>
<td>3.14</td>
<td>0.040a</td>
</tr>
<tr>
<td>Effect of Rotation under a Dishonesty Assessment Frame</td>
<td>0.205</td>
<td>1</td>
<td>4.25</td>
<td>0.021a</td>
</tr>
</tbody>
</table>

*Low Effort Audits* equals the percentage of rounds for which auditors select low-effort audits.

* These p-values are for effects that occur in the expected direction suggested by our theory (e.g., Figure 1), 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 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.
TABLE 3
Audit Failure Outcomes: Low Effort Audits and Aggressive Reporting Outcomes

Panel A: ANOVA

<table>
<thead>
<tr>
<th>Source</th>
<th>Sum of Squares</th>
<th>df</th>
<th>F</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
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<td>Assessment Frame</td>
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<td>1.11</td>
<td>0.295</td>
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<td>Unstructured Chat</td>
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<td>1</td>
<td>1.23</td>
<td>0.135(^a)^ [^b]</td>
</tr>
<tr>
<td>Rotation × Assessment Frame</td>
<td>0.353</td>
<td>1</td>
<td>11.71</td>
<td>&lt; 0.001(^a)</td>
</tr>
<tr>
<td>Rotation × Unstructured Chat</td>
<td>0.086</td>
<td>1</td>
<td>2.86</td>
<td>0.094(^b)</td>
</tr>
<tr>
<td>Assessment Frame × Unstructured Chat</td>
<td>&lt; 0.001</td>
<td>1</td>
<td>&lt; 0.01</td>
<td>0.969</td>
</tr>
<tr>
<td>Rotation × Assessment Frame × Unstructured Chat</td>
<td>0.055</td>
<td>1</td>
<td>1.83</td>
<td>0.179</td>
</tr>
</tbody>
</table>

Panel B: Pairwise Contrasts

<table>
<thead>
<tr>
<th>Source</th>
<th>Sum of Squares</th>
<th>df</th>
<th>F</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect of Assessment Frame under No-Rotation</td>
<td>0.088</td>
<td>1</td>
<td>2.93</td>
<td>0.045(^a)</td>
</tr>
<tr>
<td>Effect of Assessment Frame under Rotation</td>
<td>0.290</td>
<td>1</td>
<td>9.62</td>
<td>0.001(^a)</td>
</tr>
<tr>
<td>Effect of Rotation under an Honesty Assessment Frame</td>
<td>0.155</td>
<td>1</td>
<td>5.13</td>
<td>0.013(^a)</td>
</tr>
<tr>
<td>Effect of Rotation under a Dishonesty Assessment Frame</td>
<td>0.198</td>
<td>1</td>
<td>6.58</td>
<td>0.006(^a)</td>
</tr>
</tbody>
</table>

The dependent variable in these analyses is the percentage of audits that resulted in the combined game outcome of low-effort audits with aggressive reporting (e.g., King 2002).

\(^a\) These p-values are for effects that occur in the expected direction suggested by our theory (e.g., Figure 1), 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.

\(^b\) While the main effect of Unstructured Chat is significant in Low Effort Audits (Table 2), its effects are directionally consistent but not significant in Audit Failure Outcomes (p = 0.135, above). The marginally significant Rotation × Unstructured Chat interaction (p = 0.094, above) indicates that the effect of Unstructured Chat on Audit Failure Outcomes 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). Overall, in the Audit Failure Outcomes, we find evidence of the effect of chat similar to the results of H2 within the no-rotation conditions only, but not significant evidence overall. 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 Audit Failure Outcomes as well (F = 11.71, p < 0.001, above). Therefore, of our two main findings for Low Effort Audits (H1 and H2), H1 is robust to the dependent variable Audit Failure Outcomes.