EFFECTS OF RISK-BASED INSPECTIONS ON AUDITOR BEHAVIOR

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EFFECTS OF RISK-BASED INSPECTIONS ON AUDITOR BEHAVIOR

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

SUMMARY

I examine how risk-based inspections influence auditor behavior in a multi-client setting. I conduct an experiment using an abstract setting that captures the theoretical constructs present in the audit ecology. I manipulate the presence of risk-based inspections between-participants and the level of client risk (higher vs. lower) within-participants. Consistent with the theoretical predictions, under conditions of high resource pressure, I find that auditor effort is higher under a regime with risk-based inspections as compared to a regime without inspections, and the auditor effort increases more for higher-risk clients than for lower-risk clients. More notably, following attentional control theory, I predict and find that risk-based inspections diminish the quality of auditor decision performance for lower-risk clients. Specifically, auditors' decision performance is worse (i.e., more suboptimal) for lower-risk clients than for higher-risk clients (ceteris paribus), but only under a risk-based inspections regime. Likewise, auditors' decision performance for lower-risk clients is worse in a regime with risk-based inspections than in a regime without inspections.

I theorize that accountability pressures from PCAOB inspections combined with pressures from high resource constraints (that naturally occur in the audit environment) induce task-related anxiety on auditors. Following attentional control theory in a multi-task setting, I predict anxiety interrupts auditors' decision-making processing shifting attention toward higher-risk clients contributing to the anxiety, and away from lower-risk (untargeted) clients, thereby decreasing the quality of decision performance for lower-risk clients. I perform several supplemental analyses to test the underlying theory. First, I

conduct a second experiment where auditors operate under relatively lower resource pressure and find that auditors' decision performance is no longer worse for lower-risk clients in an inspections regime. The results support the theory that it is the combined pressures of inspections and high resource constraints causing the negative effects. Second, I conduct a supplemental experiment and measure participants' levels of anxiety. In support of the underlying theory, participants' reported anxiety levels are higher under a regime with versus without inspections. Third, I perform several robustness checks to rule out alternative explanations of the findings.

The findings of this study contribute to the auditing literature, and they have practical and regulatory implications. First, by identifying higher auditor effort in a regime with inspections, I join others in documenting potential benefits of inspections on auditor behavior, and thus audit quality. Second, by examining the effect of risk-based inspections on auditor effort in a multi-task setting, I extend prior research by providing evidence that inspections increase auditor effort more for higher-risk clients than for lower-risk clients. Third, and most notably, by identifying diminished auditor decision performance for lower-risk clients under a risk-based inspections regime, this is the first study to provide theory and evidence on how risk-based inspections can lead to potential negative consequences on audit behavior, and thus audit quality.

CHAPTER 1

INTRODUCTION

The goal of the U.S. Public Company Accounting Oversight Board (PCAOB) inspection process is to improve audit quality. After almost a decade of inspections, relatively little is known about the effectiveness of the PCAOB inspection process in achieving this goal. Academics continue to call for research as to how PCAOB inspections influence audit quality (e.g., Bedard et al. 2008, 208; Church and Shefchik 2012, 62; Daugherty and Tervo 2010, 190; Houston and Stefaniak 2013, 25; Knechel et al. 2013, 36; Peecher et al. 2013). The purpose of this study is to provide theory and evidence as to how risk-based inspections influence auditor behavior, and how changes in auditor behavior potentially differ depending on client risk. When examining auditor behavior, I consider changes in auditor effort and changes in the quality of auditor decision performance. Throughout the study, I consider potential benefits and potential unintended consequences of risk-based inspections on auditor behavior.

The PCAOB inspection process is an independent, external review that essentially holds auditors accountable for the quality of their work. The PCAOB selects individual public-company clients for inspection and conducts detailed examinations on the quality of the audit process. Any identified auditing deficiencies are included in an annual report for the audit firm that is made available to the public. Thus, auditors can anticipate that their work may be evaluated by the PCAOB, and they can anticipate negative consequences for performing substandard work and incurring auditing deficiencies. Negative consequences potentially include: (1) economic costs as negative inspection results have been reported to influence auditors' promotions, compensation, and litigation risks (Houston and Stefaniak 2013), (2) psychological costs experienced from receiving negative feedback or penalties, and (3) reputational costs given that auditors' deficiencies are known by their superiors and peers and also given that the audit firms' auditing deficiencies are publicly available.

Consistent with other accountability mechanisms in auditing, I expect inspections motivate auditors to increase effort to enhance the audit process in order to reduce the chances of incurring auditing deficiencies and the negative consequences associated with deficiencies. Thus, compared to a regime without inspections, I predict inspections will increase the level of auditor effort, in general. Increased auditor effort is consistent with the PCAOB's goals of improving audit quality.

The PCAOB uses a risk-based approach in selecting issuer-clients for inspection. That is, higher-risk engagements are targeted for reviews. The risk-based nature of the inspection process is public knowledge and is underscored by the PCAOB. While a risk-based approach has certain appeal, some worry that it might have potential drawbacks. To the extent that auditors anticipate which engagements will be selected for inspection (i.e., higher-risk clients), they may direct special attention toward those clients. For example, Houston and Stefaniak (2013, 28) warn that inspection outcomes may be capturing auditors' "best attempts to ensure audit quality and not the typical audit." Recent survey data from audit partners of large public accounting firms supports this concern (Houston and Stefaniak 2013). Reported data indicate that to some extent a majority of audit partners try to predict which engagements will be selected for PCAOB inspection review. Further, recent studies with experienced auditors find that auditors' planning decisions are influenced by anticipation of PCAOB inspections, ceteris paribus (Stefaniak and Houston 2013; Winn 2014).

Given the use of a risk-based approach to selecting inspections, but a mission to protect the public's interest of all public companies, it is unclear whether special attention to or improved quality for some clients (i.e., higher-risk targeted clients) is consistent or inconsistent with the PCAOB's goals. However, when asked about her reaction to the findings reported above, PCAOB Board Member, Jeanette Franzel, stated that she "would be concerned if a partner is making decisions about staffing levels and hours based on that partner's assessment of whether or not that audit will be inspected. I would hope the *same levels* of quality and expertise would be applied to an audit regardless." She further added, "that makes me question what's happening on the audits that partners think are not being inspected?" (Franzel 2013, emphasis added).

As indicated, a potential concern is that while the inspection process may improve audit quality for some clients (i.e., higher-risk, targeted clients), improvements may not be uniform across all audit engagements. That is, to manage firms' "inspections risk" (Glover and Prawitt 2013; Prawitt et al. 2014), auditors may increase effort more for higher-risk clients than for lower-risk clients. In this study, I argue that the potential issue is further compounded by the environment auditors typically operate in: an environment with high resource pressure. Whether due to busy seasons when auditors have a limited amount of time and resources to do above-average amounts of work, or due to tight budgets resulting from client fee pressure, auditors have chronic constraints on their resources (Bowlin 2011; Lopez and Peters 2012). When auditors' resources are constrained, even if they want to increase effort for all clients in response to inspections, they may not be able to sufficiently do so to the desired levels. Further, at increasingly high levels of resource pressure, if auditors want to increase effort for some clients, they may have to decrease effort for others. Therefore, in an environment with relatively high resource pressure, I expect auditor effort will increase more for higher-risk clients than for lower-risk clients under a risk-based inspections regime compared to a regime without inspections. As described, the potential effect may or may not be an *unintended consequence* of inspections.

By contrast, drawing on theory from cognitive psychology, I theorize that risk-based inspections in an environment with high resource pressure can lead to *unintended consequences* on auditors' *decision performance* for lower-risk clients. Following prior research, I predict that pressures from anticipating inspections combined with pressures from tight resource constraints lead to task-related anxiety (Stone and Kadous 1997; DeZoort and Lord 1997). For instance, auditors likely want to increase effort in response to accountability pressures from inspections, but not being able to sufficiently increase effort to the desired levels because of resource constraints can increase auditors' task-related anxiety.

According to attentional control theory (Eysenck et al. 2007), anxiety impairs individuals' decision-making processing. It impairs the use of goal-directed processing and increases the use of stimulus-driven processing. That is, in order to reduce anxiety, individuals shift attention toward the stimulus causing the anxiety. Thus, in a multi-task setting, attention shifts toward tasks that cause anxiety and away from other tasks not contributing to anxiety (i.e., less-salient tasks), resulting in worse decision performance for the less-salient tasks (e.g., more suboptimal decision strategies, less accurate performance, etc.) (Eysenck et al. 2007).

Following attentional control theory, I predict that auditors with task-related anxiety will shift to the use of stimulus-driven processing and direct their attention more toward the higher-risk clients they anticipate being selected for inspection (i.e., those contributing to the anxiety under risk-based inspections) and away from lower-risk clients not targeted for inspection. As a result, I anticipate diminished decision performance for lower-risk clients under a regime with risk-based inspections, ceteris paribus. Lower-quality decision performance for audits of lower-risk clients, all else equal (e.g., not attributable to changes in auditor effort), is not consistent with the PCAOB's objective of improving audit quality and is therefore considered to be an unintended consequence.

To test my theoretical predictions as to how risk-based inspections influence auditor behavior, I conduct a 2 X 2 mixed-design experiment manipulating risk-based inspections between participants (present or absent) and client risk within participants (higher- and lower-risk). Following prior experimental economics research in auditing, I design an abstract setting that captures the theoretical constructs present in the audit ecology. Using a controlled experimental setting allows me to examine the effect of risk-based inspection on auditor behavior in a multi-client setting. Participants are assigned the role of auditors, and they make effort and reporting decisions concurrently for higher-risk and lower-risk clients, given a fixed amount of available audit resources (i.e., in an environment with high resource constraints). To avoid confounding effects, I use abstract terms in the experiment (e.g., "verifiers" versus auditors, "assets" versus clients, etc.).

Using economic incentives, I mimic the incentives auditors face in the real world. For example, auditors have incentives to report favorably for the client (i.e., issue clean reports), but also have incentives to report accurately to avoid potential costs associated with incorrect reports (i.e., litigation and reputation costs). To examine the effect of inspections on auditor behavior, half of the auditors are subject to risk-based inspections ("reviews"). Auditors can anticipate that their higher-risk clients will be selected if they are reviewed and that they may receive penalties based on the audit effort allocated to that client.

The findings indicate that auditor effort is higher with inspections than under a regime without inspections, and that auditor effort increases more for higher-risk clients than for lower-risk clients. In fact, auditor effort only increases for higher-risk clients. More notably, the findings indicate that risk-based inspections lead to diminished decision performance for lower-risk clients. Specifically, with risk-based inspections, auditors' decision performance is more suboptimal (i.e., of lower-quality) for lower-risk clients than for higher-risk clients, ceteris paribus. By contrast, in a regime without inspections, auditors' decision performance does not differ for lower- and higher-risk clients. To clarify, when making identical decisions and facing the same expected outcomes for both lower- and higher-risk clients and under both *Inspection* conditions, auditors' decision performance is worse for lower-risk clients than for higher-risk clients, but only under risk-based inspections. I also find that for lower-risk clients, decision performance with risk-based inspections is worse than decision performance in a regime without inspections. This further supports the prediction that risk-based inspections can negatively influence auditors' decision performance for lower-risk clients. The findings are consistent with the theory that accountability pressures from anticipating inspections combined with pressures from high resource constraints increase individuals' task-related anxiety which leads to lower decision performance for less-salient tasks (i.e., lower-risk clients in a risk-based inspections regime).

As my primary dependent measure of decision performance, I use suboptimal decisions related to auditors' reporting decisions: "Suboptimal Type II Errors" calculated as the number of times an auditor incorrectly reports a "high" value after receiving a "low" value signal (with 100 percent accuracy) as a percentage of low value signals during the 20 periods. Upon issuing an incorrect report, auditors in my study face a 50 percent chance of being detected and incurring a -6,000 incorrect report costs, or a 50 percent chance of not being detected and incurring no costs for the incorrect report. Alternatively, following the signal and issuing a "low" value report costs -500. Thus, the decision to report "high" after a "low" signal is very suboptimal in terms of maximizing wealth (it's the most suboptimal decision participants can make during the experiment). Importantly, the reporting decision and the expected outcomes are identical across both *Inspection* conditions and for lower- and higher-risk clients. Therefore, observed differences in the dependent measure are expected to be caused by the hypothesized effects of risk-based inspections. Nonetheless, I perform a number of robustness checks to rule out alternate explanations. Additional analyses supports that the results are not driven by individuals' risk preferences, participants' effort decisions, varying numbers of low value signals across conditions, or by behavior of a few individuals. Rather, I expect the results are driven by a shift in reporting strategy for lower-risk clients (i.e., to a riskier, more suboptimal strategy) when auditors are subject to risk-based inspections under high resource pressure. Finally, I confirm the results using a secondary measure of decision performance (a measure related to auditors' suboptimal effort decisions).

In order to further support my theoretical predictions, I perform several supplemental analyses. First, to support the theory that it is the *combined* pressures of inspections and high resource constraints that lead to diminished decision performance for lower-risk clients, I perform another experiment. I perform the same experiment except I reduce the level of resource pressure. Under this environment, in a regime with risk-based inspections, I no longer find diminished decision performance for lower-risk clients. Specifically, I find that decision performance does not differ across lower- and higher-risk clients in regime with inspections. Further, I find inspections increase auditor effort, but unlike Experiment 1, effort increases uniformly for lower- and higher-risk clients.

Second, to support the theory that the combined pressures of inspections and high resource pressure lead to *anxiety*, I perform a supplemental experiment. Using the same design as Experiment 1 (i.e., a 2 X 2 mixed-design and an environment with high resource constraints), I conduct a similar experiment but I measure participants' levels of anxiety immediately after their effort decisions in the first period. As expected, I find some evidence that anxiety levels are higher for participants in the *Inspections* condition than for participants in the *No Inspections* condition.

Finally, to support the theory that risk-based inspections and resource pressure lead to anxiety because auditors want to increase effort but are unable to sufficiently do so, I perform additional analyses. Specifically, I examine participants' self-reported "desire for more effort" elicited during the post-experimental questionnaires in

Experiment 1 (under high resource pressure) and Experiment 2 (under lower resource pressure). As expected, only participants in the *Inspections* condition *and* in Experiment 1 (under high resource pressure) indicate that they would have liked more effort above the constraint. Further, the rating of "desire for more" for participants in this condition was significantly higher than that of participants in the other three conditions (i.e., in the *No Inspections* condition with high resource pressure and in both *Inspection* conditions with lower resource pressure).

The remainder of the paper is organized as follows. In Chapter II, I provide a brief background on the regulation of accounting firms including PCAOB inspections and the prior self-regulation system (AICPA peer reviews). In Chapter III, I discuss the related literature, and in Chapter IV, I develop the hypotheses. The experimental method is detailed in Chapter 5. The results and supplemental analyses are included in Chapters 6 and 7, respectively. Finally, in Chapter 7, I discuss the implications of this study along with concluding remarks.

CHAPTER 2

BACKGROUND ON THE REGULATION OF ACCOUNTING FIRMS

2.1 Self-Regulation and AICPA Peer Reviews

Prior to the inception of the PCAOB, accounting firms were self-regulated. Accounting firms participated in peer reviews conducted once every three years, which were overseen by the American Institute of Certified Public Accountants (AICPA). The overwhelming majority of peer review reports were unqualified (i.e., "clean"). [1] Specifically, Hilary and Lennox (2005) find that 960 of 1,001 reports (96 percent) issued from 1997 to 2003 were unqualified, and Wallace (1991) finds that 306 of 351 reports (87 percent) issued from 1980 to 1986 were unqualified. A handful of empirical studies suggest that the AICPA's peer review system was beneficial at promoting audit quality. First, peer review reports, to some extent, affected users' perceptions of auditor quality. For example, peer review reports have been linked to accounting firms' ability to attract and retain clients (Hilary and Lennox 2005) and have been considered by audit committee members when recommending an auditor (Woodlock and Claypool 2001). Second, studies document positive associations between peer review results and proxies for audit quality (Casterella et al. 2009; Grant et al. 1996).[2] Finally, Colbert and Murray

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[1] Peer review reports fall into three categories: unqualified, modified, and adverse. The report is unqualified as long as the accounting firm does not have a serious weakness in its quality control procedures, compliance with such procedures, or compliance with membership requirements of the AICPA's Division of Firms. Otherwise, the report is modified (serious weakness) or adverse (very serious weakness).

[2] Using a proprietary dataset from an insurance company that covers local and regional accounting firms, Casterella et al. (2009) find an association between the number of weaknesses in comment letters

(1998) contend that the peer review program was beneficial in that firms improved their peer review ratings over time.

However, the peer review system has been the target of much criticism, most of which questions the effectiveness of the system enforcing high quality audits. First, peer reviewers were not independent: accounting firms were allowed to choose their own reviewers leading to overly friendly reviews (Fogarty 1996; DeFond 2010). Supporting this theory, Anantharaman (2012) provides evidence that accounting firms choosing friendly reviewers fare better in peer reviews than other firms. Second, the peer review system was essentially non-punitive. Accounting firms received light sanctions for performing substandard audit work (Fogarty 1996). Despite such criticism, the regulatory system remained intact as the only form of regulation on accounting firms until Congress passed the Sarbanes-Oxley Act of 2002 (SOX), which included a plan to overhaul the regulation of professional accounting firms.

2.2 Regulatory Reviews and PCAOB Inspections

SOX established the PCAOB to regulate the auditing profession. One of the main duties of the PCAOB is to conduct periodic inspections of audit engagements for all accounting firms that audit public companies ("issuers"). Beginning in 2004, the PCAOB began performing annual inspections for all accounting firms with over 100 issuer clients and triennial inspections (i.e., at least once every three years) for accounting firms with

accompanying unqualified peer review reports and malpractice claims alleging auditor negligence. Grant et al. (1996) conduct an experimental economics study and provide evidence that audit quality improves markedly with voluntary self-regulation, as long as participating auditors can sanction those providing low quality audits.

100 or fewer issuer clients. The inspection process includes detailed examinations and reviews of certain elements of selected issuer engagements ("Part I" of the reports) and an overall evaluation of the accounting firms' quality controls ("Part II" of the reports). The PCAOB uses a risk-based approach to conduct inspections. That is, they target the riskiest client issuers to be selected for review, and they select the riskiest areas of the audit for inspection. The PCAOB inspects roughly 50 to 75 issuers per report (i.e., annually) for large firms, and on average, less than three per report (i.e., triennially) for the small firms (Church and Shefchik 2012). During the inspections, the PCAOB focuses on the quality of the audit process as compared to the accuracy of the audit outcomes.

Once the annual inspections are completed, a report is prepared and shared with the accounting firm. Part I of the report describes identified auditing deficiencies (but does not identify issuers). On average, the PCAOB identifies roughly 14 deficiencies per report for large firms and less than two deficiencies per report for small firms (Church and Shefchik 2012). Part II of the report related to the quality control deficiencies is only disclosed if the firm fails to sufficiently remedy the problems within a one-year time frame from the date of the inspection report. The reports are publicly disclosed via the PCAOB's website at http://www.pcaobus.org. According to the PCAOB, the goal of the inspection process is to improve audit performance and to promote public trust in the auditing profession by reducing the risks of auditing failures in U.S. public companies.

Theoretically, the inspection process should improve audit quality due to a number of critical improvements in the regulation process compared to the prior self-regulation system. First, the inspection reviews are conducted by independent staff prohibited from being active auditing practitioners. Second, the PCAOB has punitive authority to issue powerful sanctions to accounting firms that fail to comply with the laws, accounting rules, and other professional standards.[3] Finally, the PCAOB focuses their review on both the high-level quality control procedures of the accounting firms as well as inspecting the audit quality of issuer audit engagements. The critical changes in the regulation of accounting firms are important for supporting improvements in overall audit quality.

2.3 PCAOB Inspections and Audit Quality

While many believe that audit quality[4] has improved with PCAOB inspections (e.g., Abbott et al. 2008; Church and Shefchik 2012; DeFond 2010; DeFond and Lennox 2011; Daugherty et al. 2011; Gunny and Zhang 2013), relatively little research has provided empirical evidence on this matter. A few studies offer descriptive analyses on the results contained in the PCAOB inspection reports for large (Church and Shefchik 2012) and small accounting firms (Hermanson et al. 2007), indicating that the number of audit deficiencies has declined over time. A few archival studies have examined changes in the audit market following the inception of PCAOB inspections or with deficiencies identified in the PCAOB reports. For example, DeFond and Lennox (2011) find that low-quality, smaller audit firms are more likely to exit the market following the

[3] For unintentional violations (e.g., negligent acts), the PCAOB has the authority to levy fines of up to $100,000 per person and up to $2,000,000 per firm. For intentional violations, the amounts increase to $750,000 and $15,000,000, respectively. The PCAOB also has the authority to prohibit accounting firms from conducting audits of public companies (via suspension or revocation of accounting firms' registration) (PCAOB 2003).

[4] While one of the PCAOB's goals is to improve "audit quality," audit quality is yet to be defined. That is, there is currently no unified, or commonly accepted, definition of audit quality (Knechel et al. 2013). Rather, divergent views as to what constitutes audit quality remain and depend on the "eye of the beholder" (e.g., regulators, auditors, investors, etc.). Throughout this paper, I discuss several different proxies for audit quality consistent with the prior literature (see Knechel et al. [2013] and Knechel and Shefchik [2014] for reviews).

implementation of the PCAOB, suggestive of an overall increase in the level of audit quality supplied in the market. Others have documented improvements in proxies for audit quality for public companies in the years following PCAOB inspections, including improved auditor reporting (Gramling et al. 2011) and higher-quality financial reporting (Carcello et al. 2011; Krishnan et al. 2014). Finally, the quality of all Deloitte's clients' financial reporting improved following the disclosure of Deloitte's Part II quality control disclosures (Drake et al. 2014).

There is also some evidence that the findings in the inspection reports may be useful in signaling audit quality. For example, clients of audit firms whose inspection reports contain more serious auditing deficiencies in Part I are associated with lower earnings quality (Gunny and Zhang 2013), and clients of small audit firms with Part II quality control disclosures are associated with lower financial reporting quality as compared to clients of firms who remediate their quality control issues (Buslepp and Victoravich 2014). Accordingly, users appear to react to the information included in the inspection reports. Specifically, auditors are more likely to be dismissed following more serious inspection deficiencies in Part I of the reports (Abbott et al. 2008; Daugherty et al. 2011; Acito et al. 2014). Likewise, disclosures of audit firm's Part II quality control criticisms are associated with decreases in the audit firm's client market share (Muriel 2013; Nagy 2014; Buslepp and Victoravich 2014) and negative abnormal returns for clients of the audit firm (Dee et al. 2011). At a more global level, Carcello et al. (2011) found that global markets reacted negatively to the PCAOB's announcement of their inability to inspect auditors in certain foreign countries. The findings suggest that the market perceives PCAOB inspections as valuable and adding credibility to the financial reporting process.

While the existing evidence is consistent with the claim that PCAOB inspections improve audit quality, empirical challenges limit the ability to draw causal inferences. First, because the PCAOB uses a risk-based approach to select issuer engagements for inspection reviews, the sample of engagements is not representative of the population (PCAOB 2008). Accordingly, improvements in the results from inspection samples may not be representative of the average audit quality in the overall audit market. Second, until 2010, the inspection reports did not disclose the *number* of issuer audit engagements inspected for large, annually-inspected firms. As such, the reported decrease in the auditing deficiencies over time could simply be due to a decrease in the *number* of issuer clients inspected. Finally, empirical findings based on changes in the overall audit market following PCAOB inspections are confounded with many other changes in the market during the same time period (e.g., changes in client managers' and auditors' behaviors due to significant changes after the passage of SOX).

With few exceptions, prior studies have not examined the direct causal effects of PCAOB inspections on auditor behavior. Recent experimental studies find that experienced auditors' planning decisions are influenced by anticipation of PCAOB inspections such that planned audit hours and fees are higher when auditors anticipate the client might be selected for inspection (Stefaniak and Houston 2013; Winn 2014). Further, Lamoreaux (2013) finds that auditors in jurisdictions allowing PCAOB inspections make higher quality reporting decisions (i.e., they are more likely to report going concern opinions and material weaknesses) than do auditors in jurisdictions barring

PCAOB inspections, but only in the post-PCAOB regulatory period. That is, prior to PCAOB inspections, there was no difference in the quality of auditor reporting across the two groups of audit firms, but after the jurisdiction of allowing PCAOB inspections the quality of auditor reporting is higher in countries allowing PCAOB inspections than it is for countries barring PCAOB inspections. The finding is consistent with improved auditor reporting quality due to the anticipation (or threat) of PCAOB inspections.

My study complements the prior literature that examines the causal effect of inspections on auditor behavior (e.g., increases in auditor effort), but it extends prior research by examining whether these benefits vary for higher-risk versus lower-risk clients. More notably, I extend prior research by examining whether there are unintended consequences to lower-risk clients resulting from risk-based inspections when auditors also are under high resource pressure. To the best of my knowledge, unintended consequences (i.e., negative effects on the quality of auditor decision performance) for lower-risk clients have not yet been studied.

CHAPTER 3

RELATED LITERATURE

3.1 Accountability Pressure

PCAOB inspections act as an accountability mechanism on auditors' behavior. Accountability refers to "the implicit or explicit expectation that one may be called on to justify one's beliefs, feelings, and actions to others," and usually implies that negative consequences will be suffered for insufficient actions (Lerner and Tetlock 1999, 255).[5] Accountability is one source of pressure that influences behavior and is comprised of feedback pressure of being evaluated and social pressure of justifying one's actions to others (DeZoort and Lord 1997).

In general, accountability pressure motivates individuals to increase effort, which often results in higher quality judgment and decision-making (Lerner and Tetlock 1999). However, the effects of accountability pressure are dependent on the perceived objective of the person held accountable to (e.g., a perceived objective to be accurate versus to reach a preferred outcome). Research in auditing has illustrated the effects of accountability pressure on auditor performance. When auditors are held accountable with a perceived objective to be accurate, auditors increase effort and improve judgment performance (e.g., Johnson and Kaplan 1991; Ashton 1990; Kennedy 1993). When

[5] Explicit justification is not a necessary condition of accountability. Accountability is often invoked with anticipation of being evaluated (i.e., when individuals expect that their performance will be assessed by another and with some implied consequences). Auditing research has shown that individuals perceive and react to accountability when they anticipate being evaluated, without anticipating having to *justify* their actions (e.g., Lord 1992; DeZoort and Lord 1997; Glover 1997).

auditors are held accountable to parties with known preferences, they tend to conform to those preferences (e.g., Lord 1992; Peecher 1996; Hoffman and Patton 1997; Wilks 2002), which may or may not yield better performance. For example, Wilks (2002) finds that when subordinate auditors learn their partner's view (i.e., a preferred outcome) before evaluating evidence, they unintentionally evaluate the evidence more consistent with the partner's view as compared to other auditors who did not learn of the partner's view beforehand.

3.2 Resource Pressure

Auditors suffer from chronic constraints on their resources. Whether through limited availability resulting from high workloads (e.g., during busy seasons) or due to tight fee- or time-budget pressures, auditors operate under high resource pressure (e.g., DeZoort and Lord 1997; Bowlin 2011; Lopez and Peters 2012). I refer to these types of resource pressure along with other pressures related to a lack of resources (e.g., time pressure, budget pressure) collectively as "time-related pressures." In general, auditors have negative attitudes toward time-related pressures, and they report experiencing stress from such pressures (Kelley and Seiler 1982; Azad 1994).

In their model, DeZoort and Lord (1997) describe how pressures act as a stimulus or antecedent to "stress responses," which result in "strain outcomes." Examples of stress responses from time-related pressures include increased levels of efficiency (McDaniel 1990; Spilker and Prawitt 1997), but also mentally filtering away information that seems less relevant to the judgment at hand (e.g., Glover 1997; Asare et al. 2000), and altering individuals' decision processing strategies in favor of employing less effortful, more heuristics-based strategies (Maule et al. 2000; Stone and Kadous 1997; Coram et al.

2004). Therefore, while time-related pressures can have a positive effect on effort (e.g., working harder or faster), much research shows an inverted-U relationship with time-related pressures and judgment performance (DeZoort and Lord 1997). That is, performance increases with low to moderate levels of pressure, but decreases with moderate to high levels of pressure (Choo 1995; DeZoort and Lord 1997).

In auditing, increasingly intense time-related pressures lead to undesirable effects on auditors' judgment and decision-making performance (Alderman and Deitrick 1982; McDaniel 1990; Coram et al. 2004). For example, time-related pressures (e.g., fee pressure, time pressure) reduce audit effectiveness when the risk of misstatement is low (McDaniel 1990; Agoglia et al. 2010), decrease audit effort, especially for low-risk audit areas (Houston 1999; Bowlin 2011), reduce the quality of auditors' decision-making processes (Solomon and Brown 1992), and increase auditors' willingness to engage in reduced audit quality acts, such as underreporting time, prematurely signing off on audit work, accepting doubtful audit evidence, and truncating sample sizes (e.g., Ponemon 1992; Alderman and Detrick 1982; Coram et al. 2004). The findings from individual judgment and decision-making research are reinforced at the market level by Lopez and Peters (2009) who find that the truncation of the busy season from 90 to 60 and 75 days (i.e., enhanced time-related pressures) is associated with proxies for lower quality audits.

3.3 Accountability Pressure and Resource Pressure

Relatively few studies have examined how accountability pressure influences auditor behavior when auditors also are under relatively high resource pressure (or other time-related pressures) (DeZoort and Lord 1997). Further, the limited evidence available produces mixed results. Glover (1997) finds no effect of accountability pressure at improving the quality of auditor judgments when auditors also are under high resource pressure. Specifically, he finds evidence of a dilution bias in auditors' fraud-risk assessments (i.e., overweighting irrelevant information in assessments). While time pressure reduces the dilution bias, accountability has no influence (i.e., no main effect of accountability or interaction with time pressure) on auditors' dilution bias during fraud-risk assessments. On the other hand, Asare et al. (2000) finds that accountability pressure improves auditor performance even when auditors also are under time-budget pressure. Specifically, they find that auditors increase the extent and breadth of audit testing (i.e., higher quality performance) when they are held accountable to a superior, and the results are consistent in the presence or absence of time-budget pressure.

My study complements and extends this line of research in auditing by further examining the joint effect of accountability pressure (imposed by inspections) and time-related pressure (high resource pressure) on auditor behavior, but in a multi-client setting. As stressed by Asare et al. (2000, 545), examining the joint effects of accountability and time-related pressure "is important given their simultaneous occurrence in practice and their potential countervailing effects." Further, using a multi-client setting allows for a finer examination on the effects of accountability pressure on auditor behavior when auditors also are under high resource pressure. For example, prior research has examined the combined effects of accountability and time-related pressures on performance but only for the tasks auditors are held accountable for. In my multi-task setting, auditors perform two tasks simultaneously: one task they anticipate being held accountable for, and one task they do not anticipate being held accountable for. I expect the combined effects of accountability and high resource pressures will influence auditor performance differently across the two tasks (i.e., higher- and lower-risk clients) because of varying levels of accountability.

While not audit-related per se, Stone and Kadous (1997) study the combined effects of accountability pressure (e.g., high levels of monitoring) and perceived time pressure on individuals' performance in a complex task. They find that individuals under the combined pressures experience task-related negative affect (measured by higher levels of nervousness). Further, they find individuals in this negative affective state have lower decision performance for complex tasks as compared to individuals who are not under such conditions. Specifically, their decision-processing strategies are more suboptimal (e.g., more heuristics-based), resulting in lower decision accuracy. Following Stone and Kadous (1997), I predict auditors under the combined pressures of accountability from anticipating inspections and high resource pressure will experience anxiety which will negatively influence their decision performance. Further, I extend the findings of Stone and Kadous (1997) by examining the effect in a multi-task setting.

3.4 Multi-client Settings

To the best of my knowledge, no studies have examined auditor behavior in a multi-client setting (e.g., with lower- and higher-risk clients). In a somewhat similar vein, Bowlin (2011) studies auditor behavior in a *multi-account* setting examining auditor decisions related to low- and high-risk accounts for the same client. Using a strategic-game setting with auditor and manager players, Bowlin (2011) finds auditors allocate fewer resources to low-risk versus high-risk accounts, thereby subjecting low-risk accounts to strategic misstatements by managers. Further, he finds that these results hold regardless of the level of resource pressure (high or low). The findings from Bowlin

(2011) illustrate a potential threat of risk-based auditing to low-risk accounts. Applying the implications to my setting would suggest that the risk-based inspections approach used by regulators might subject lower-risk clients to lower audit quality by auditors (strategically).

However, there are several key differences between my setting and that used in Bowlin (2011) that limit drawing such inferences. First, Bowlin examines strategic behavior of *managers* under a risk-based monitoring mechanism (i.e., audits). By comparison, I examine strategic *and non-strategic* behavior of *auditors* under a risk-based monitoring mechanism (i.e., regulatory inspections). Second, managers in the Bowlin (2011) setting have incentives to misstate accounts (high- or low-risk accounts); whereas auditors in my setting do not have incentives to provide lower quality audits to higher- or lower-risk clients. Third, the incentive structures and strategic interactions between auditors and managers (from Bowlin 2011) differ from those between regulators and auditors (in my study). Nonetheless, comparisons can be drawn as is it relates to strategic errors for lower-risk accounts (or in my case, lower audit quality for lower-risk clients) when individuals are under a risk-based monitoring mechanism. My study extends this line of research by examining the *non-strategic* effects of a risk-based monitoring mechanism on *decision performance* (i.e., the quality of judgment and decision making) for *multiple clients* which has not been previously examined.

Research in cognitive psychology, however, has studied individual's decision performance in a multi-task setting. In a review of the literature, Eysenck et al. (2007) discuss how anxiety impairs individuals' decision-making processing by disrupting the attentional control system. Anxiety impairs (distracts from) one's use of the goal-directed attentional system and increases one's use of the stimulus-driven attentional system. This effect arises because anxiety increases one's motivation to reduce the aversive or negative state. To reduce anxiety, individuals direct attention and resources toward the tasks contributing to anxiety. In doing so, they shift from a goal-directed attentional system to a stimulus-driven attentional system (i.e., to the stimulus adding to the anxiety). In a multi-task setting, if one task is more salient than another (e.g., if one task contributes more toward anxiety), anxiety impairs decision performance more on less-salient tasks (Eysenck et al. 2007). Alternatively, if neither task is perceived as more or less salient, then anxiety impairs decision performance similarly for both tasks.

In my study, following Stone and Kadous (1997), I expect accountability pressures from inspections, combined with high resource pressure, induces higher levels of anxiety on auditors. Following Eysenck et al. (2007), I make predictions as to how this expected anxiety influences auditors' decision performance in a multi-client setting. Specifically, I use attentional control theory to predict how risk-based inspections influence auditor behavior differently for varying types of clients (higher- versus lower-risk) when auditors are under higher resource pressure which naturally occurs in the audit environment.

CHAPTER 4

HYPOTHESES DEVELOPMENT

4.1 Risk-based Inspections on Auditor Effort

The PCAOB's inspection process is an independent, external quality review that holds auditors accountable for their work. In general, accountability influences individual behavior through feedback pressure (i.e., the expectation of being evaluated and suffering negative consequences), and social pressure (i.e., the expectation of having to justify one's actions to others) (Lerner and Tetlock 1999; DeZoort and Lord 1997).

PCAOB inspections likely induce feedback pressure. Auditors can anticipate that their work will be evaluated by PCAOB inspectors, and they can anticipate negative consequences for insufficient quality. The PCAOB selects public-company clients (issuers) for surprise inspection after the audit is complete and conducts a detailed examination on the quality of the audit process. In a recent survey, 100 percent of audit partners from large accounting firms (n = 107) indicate that at least one of their audit engagements has been selected for PCAOB inspection; further, a majority of partners report that they anticipate which engagements might be selected for PCAOB inspection (Houston and Stefaniak 2013). Therefore, auditors anticipate that their work will be evaluated by the PCAOB. The PCAOB identifies deficiencies in the audit process and includes those deficiencies in Part I of the report for the audit firm. Auditors suffer consequences from identified auditing deficiencies. First, inspection results have economic consequences. Audit partners report that negative inspection results influence their promotions, compensation, and litigation risk (Houston and Stefaniak 2013).

Further, in more extreme cases, the PCAOB has the authority to levy fines of up to $100,000 ($750,000) per person for unintentional (intentional) negligent acts, and can prohibit auditors from conducting audits of public companies (PCAOB 2003). Second, auditors may experience psychological costs of incurring auditing deficiencies (e.g., it feels bad to receive negative feedback on your work).

PCAOB inspections also likely induce social pressure. The results of inspections for individual audits are made known to auditors' superiors and peers in the audit firm. Thus, auditors likely experience personal reputational costs given that their superiors and peers are aware of deficiencies related to their work. Further, auditing deficiencies included in Part I of the report are available to the public. Even though individual partners and issuer clients are not identified in Part I of the report, auditors may still experience reputational costs given the reports are publicly available. Indeed, surveyed audit partners report that PCAOB inspections influence their professional reputation (Houston and Stefaniak 2013).

I expect accountability pressures induced by PCAOB inspections will influence auditor behavior via two mechanisms: from being held accountable to a party with a perceived objective to be accurate, and from being held accountable to a party with perceived preferences. First, auditors are aware that PCAOB inspectors examine the quality of the audit process. Accordingly, to satisfy inspectors, auditors are aware they need to conduct a thorough and high-quality audit in accordance with the generally accepted auditing standards (i.e., do a good job). Following prior research in auditing when auditors are held accountable to a party with the perceived objective of being accurate (e.g., Johnson and Kaplan 1991; Kennedy 1993), I expect inspections motivate auditors do a better job (i.e., to be more accurate). Auditors anticipating inspections likely want to be more vigilant and diligent in their work in order to avoid incurring inspection deficiencies and the consequences associated with those deficiencies.

Second, the sentiment gathered by the profession from PCAOB inspection results appears to be that the PCAOB prefers more work to less. Indeed, partners report high levels of documentation pressure resulting from inspections (Houston and Stefaniak 2013). Following prior research in auditing on accountability pressure when auditors are held accountable to a party with perceived preferences (e.g., Lord 1992; Peecher 1996; Hoffman and Patton 1997; Wilks 2002), I expect auditors under inspections with conform to inspectors' preferences. That is, I expect auditors will increase effort to appease inspectors (e.g., increase documentation), even if it produces inefficient outcomes (e.g., over-auditing). Both mechanisms are expected to influence auditor effort similarly. Auditors will likely want to increase auditor effort when anticipating PCAOB inspections in order to reduce their chances of incurring auditing deficiencies and suffering costs associated with those deficiencies. Accordingly, I expect the following prediction:

H1: Auditor effort will be higher under a regime with inspections than under a regime without inspections.

The PCAOB uses a risk-based approach to selecting issuer clients for inspections. Higher-risk clients are targeted for inspection. While the nature of what constitutes "high-risk" is not described in the detail, the use of a risk-based selection process is well-known by auditors and is underscored by the PCAOB on their website. Further, the definition of high-risk used by the PCAOB is likely similar to that used by audit firms when assessing client risk.[6] Because of the risk-based nature of the PCAOB inspection process, changes in auditor effort under an inspections regime may not be uniform across all clients. While the PCAOB stresses the importance of performing sufficiently high-quality audits to all clients, they focus their inspections on higher-risk clients. Accordingly, auditors anticipate that higher-risk (lower-risk) clients may (may not) be selected for inspection (Houston and Stefaniak 2013). Accordingly, under an inspections regime, auditors may increase effort more for higher-risk, targeted clients than for lower-risk, untargeted clients.

Further, auditors have chronic constraints on their resources (e.g., Bowlin 2011; Lopez and Peters 2012). Tight resource constraints are caused by busy seasons when auditors only have a limited amount of time and resources to complete above-average amounts of work, and caused by tight budgets which frequently occur due to pressure from clients to reduce fees (or at least to not increase fees). When auditors' resources are constrained, auditors may not be able to increase effort to the desired level for all clients in response to inspections. At higher levels of resource constraints, auditors are faced with making tradeoffs. That is, with increasing resource pressure, auditors may only be able to increase effort for some, but not all, clients. Further, at very high levels of resource pressure, if auditors want to increase effort for some clients (e.g., higher-risk

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[6] At the very least, audit firms are aware that their private company clients cannot be selected for inspection and can thereby be considered "lower-risk" in this setting. However, audit partners can likely anticipate which of their public company clients represent greater- or less-risk in a given year.

clients), at some point they may have to reduce effort for others (e.g., lower-risk clients). This discussion leads to the following prediction:

H2: When auditors are under relatively high resource pressure, compared to a regime without inspections, under an inspections regime auditor effort will increase more for higher-risk clients than for lower-risk clients.

4.2 Risk-based Inspections on Auditor Decision Performance

As described above, I expect PCAOB inspections induce accountability pressure on auditors. Likewise, tight resource constraints also create pressures on auditors. Research in psychology finds that increased pressures lead to stress responses and strain outcomes (DeZoort and Lord 1997; Stone and Kadous 1997). For example, Stone and Kadous (1997) find that accountability pressure from enhanced monitoring along with perceived time pressure induces task-related negative affect (i.e., a stress response). Further, individuals in this negative affective state have lower decision performance (e.g., they use more heuristic decision strategies and make less accurate decisions) (i.e., a strain outcome) than do others not in the negative affective state. This is consistent with research in psychology and auditing that finds an inverted-U relationship between pressure and individual performance whereby performance increases with low to moderate levels of pressure but deteriorates markedly with moderate to high levels of pressure (Choo 1995; DeZoort and Lord 1997).

Following Stone and Kadous (1997), I expect accountability pressure from PCAOB inspections combined with high resource pressure that naturally occurs in the audit environment causes task-related negative affect or “anxiety.” Specifically, I expect auditors experience accountability pressure under an inspections regime whereby they are likely motivated to increase effort in order to avoid negative feedback and negative consequences associated with incurring inspection deficiencies (e.g., economic, psychological, and reputational costs) (i.e., H1). Further, the pressure is enhanced with high resource constraints. Auditors are constrained from their desire of increasing effort to the desired levels under an inspections regime; they do not have the resources available to increase effort to the desired levels (i.e., without decreasing effort elsewhere). Therefore, I expect the combined pressures from PCAOB inspections and tight resource constraints will induce task-related anxiety because auditors are constrained from adequately responding to the accountability pressure.

Recall that according to attentional control theory, task-related anxiety negatively influences individuals' decision performance (Eysenck et al. 2007; Stone and Kadous 1997). Anxiety interrupts individuals' decision-making processing; it impairs the use of goal-directed processing and increases the use of stimulus-driven processing (Eysenck et al. 2007). That is, to reduce anxiety, individuals enhance their attention toward the tasks stimulating the anxiety (i.e., salient tasks). Consequently, anxiety impairs decision performance less-salient tasks not contributing toward the anxiety. For example, individuals under anxiety tend to increase their use of heuristics-based processing for less-salient tasks resulting in suboptimal, decremented decision performance (Adelberg and Batson 1978; Stone and Kadous 1997; Eysenck et al. 2007).

Following attentional control theory, I expect task-related anxiety of auditors will negatively include their decision performance, but only for lower-risk clients. Specifically, I expect anxiety will cause auditors to switch to the use of stimulus-driven processing. In a risk-based inspections regime, higher-risk clients are the stimulus causing the anxiety. That is, auditors are aware that the PCAOB is more likely to inspect higher-risk clients and they can anticipate suffering consequences for incurring any auditing deficiencies related to those clients if inspected. To reduce their anxiety, auditors may enhance their attention toward the audits of higher-risk clients (i.e., the stimulus contributing to the anxiety) and away from other audits (i.e., lower-risk clients untargeted by inspections). Accordingly, following prior research, under these conditions I expect the quality of auditors' decision performance for lower-risk clients to be weakened (i.e., more suboptimal, less accurate, etc.).

In sum, when auditors are under relatively high resource pressure and subject to risk-based inspections, I expect decision performance to diminish for lower-risk clients. Therefore, *all else equal*, with risk-based inspections, I expect decision performance to be worse for lower-risk clients than higher-risk clients. Likewise, I expect decision performance for lower-risk clients will be worse under a regime with risk-based inspections than under a regime without inspections. By contrast, when auditors are under a regime without inspections, auditors may not experience anxiety from high resource pressure alone; in this case, there would no threat to auditor decision performance. Furthermore, even if auditors did experience anxiety from high resource pressure, without risk-based inspections, higher-risk clients are not expected to stimulate the anxiety. Therefore, auditors with anxiety would not be expected to shift attention toward higher-risk clients and decision performance would not be expected to differ across higher- and lower-risk clients. This discussion leads to the following interaction hypothesis:

H3: When auditors are under relatively high resource pressure, under a risk-based inspections regime, decision performance will be worse for lower-risk clients than for higher-risk clients (ceteris paribus), but decision performance will not differ across clients under a regime without inspections.

CHAPTER 5

EXPERIMENTAL METHOD

5.1 Design Overview

I employ a 2 X 2 mixed-design experiment. I manipulate the inspections regime, present (*Inspections)* or absent (*No Inspections*), between-participants.[7] I manipulate the level of client risk *(*denoted *Client Risk),* lower- and higher-risk, within-participants. When operationalizing the inspections regime I attempt to capture the key structural constructs of the PCAOB's inspection process. Accordingly, under the *Inspections* condition, the following features are present: auditors are subject to an independent review; the review includes a risk-based selection of the client to be reviewed; auditors can anticipate penalties for insufficient levels of audit work; and identified audit deficiencies are publicly disclosed. For *Client Risk,* higher-risk clients have a higher probability of a misstatement than do lower-risk clients.[8]

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[7] Alternatively for the control condition, I could have included a regime with a peer review process rather than one with no inspections. I chose a control condition with no inspections rather than one with a peer review process in order to develop a clean test of my theory on the effect of risk-based inspections on auditor behavior. However, I would expect similar predictions and findings if the control condition had a peer review process. I would still expect the predicted differences in behavior across inspection regimes because the peer review program lacked several components that induce accountability pressure included in the inspections regime. For example, the peer reviewers were not independent and they did not have punitive authority to issue sanctions to firms who performed poorly. Thus, the inspection process induces higher accountability pressure (i.e., anticipated outside evaluation with perceived consequences). Further, the peer review process created less social pressure (i.e., reputational consequences) given that detailed results about individual performance evaluations were not publicly disclosed.

[8] By misstatement, I refer to the risk that the financial statements contain a material error.

For H1 and H2, the dependent variable is auditor effort. For H3, the dependent variable is auditor decision performance measured by suboptimal decisions (explained in detail in the results in Section 6.3).

5.2 Experimental Procedures

The experiment was conducted in a controlled laboratory with undergraduate students from a medium-sized state university. Consistent with prior experimental economic studies, the experimental setting is made as stark as possible to minimize the effect of any role-playing by participants (e.g., King 1991; Kachelmeier and King 2002). Participants are assigned the role of "verifiers" (auditors) and randomly assigned to *Inspection* conditions. The other audit market players are referred to as "sellers" (clients), "buyers" (investors), and "a review board" (regulators). Further, clients' financial statements are labeled "assets:" "Type A assets" for lower-risk clients and "Type B assets" for higher-risk clients. To reduce the complexity of the audit market, clients', investors', and regulators' behaviors are computerized (Schatzberg and Sevcik 1994).[9] Therefore, the design allows for isolating the effect of inspections on auditor behavior for multiple clients with varying levels of client risk.

Prior to beginning the experiment, participants were provided with written instructions. A full set of the instructions is included in Appendix A. The experimenter

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[9] The theoretical predictions across the *Inspection* conditions remain unchanged if actual participants are used for clients, investors, and/or regulators. The auditor participants are aware that the other parties' decisions are programmed, and they have full information as to how the decisions are programmed. Consistent with single-period settings, the other parties' decisions are not dependent on prior periods (i.e., they do not consider historical auditor behavior in their decisions). These design choices remove any strategic interaction and reputational effects that may arise between auditors and other parties. Further, I am interested in examining how the anticipation of PCAOB inspections influences auditor behavior rather than any strategic interaction between PCAOB inspectors and auditors.

read a summarized version of the instructions aloud to the participants, highlighting the key points of the experiment. The instructions explained that each participant would be assigned the role of a “Verifier.” They also were informed that they would make decisions for 20 periods and that each period was independent (i.e., that there were new assets each period). The instructions familiarized participants with the setting of the experiment, the choices to be made, the programmed behavior of the other players in the market, and the nature of the payoffs for different outcomes.

The experiment was implemented using the z-Tree experimental software (Fischbacher 2007). Following the instructions, participants completed a computerized true-false pretest to ensure they understood the key points of the experiment and the experimental manipulations. The pretest questions are included in Appendix B. Subsequently, participants completed three practice periods designed to help them understand the experimental protocol, become familiar with the computerized software, and understand how their decisions influence their outcomes and payoffs. Figure 1 below includes a screen print of the experimental design in z-Tree.

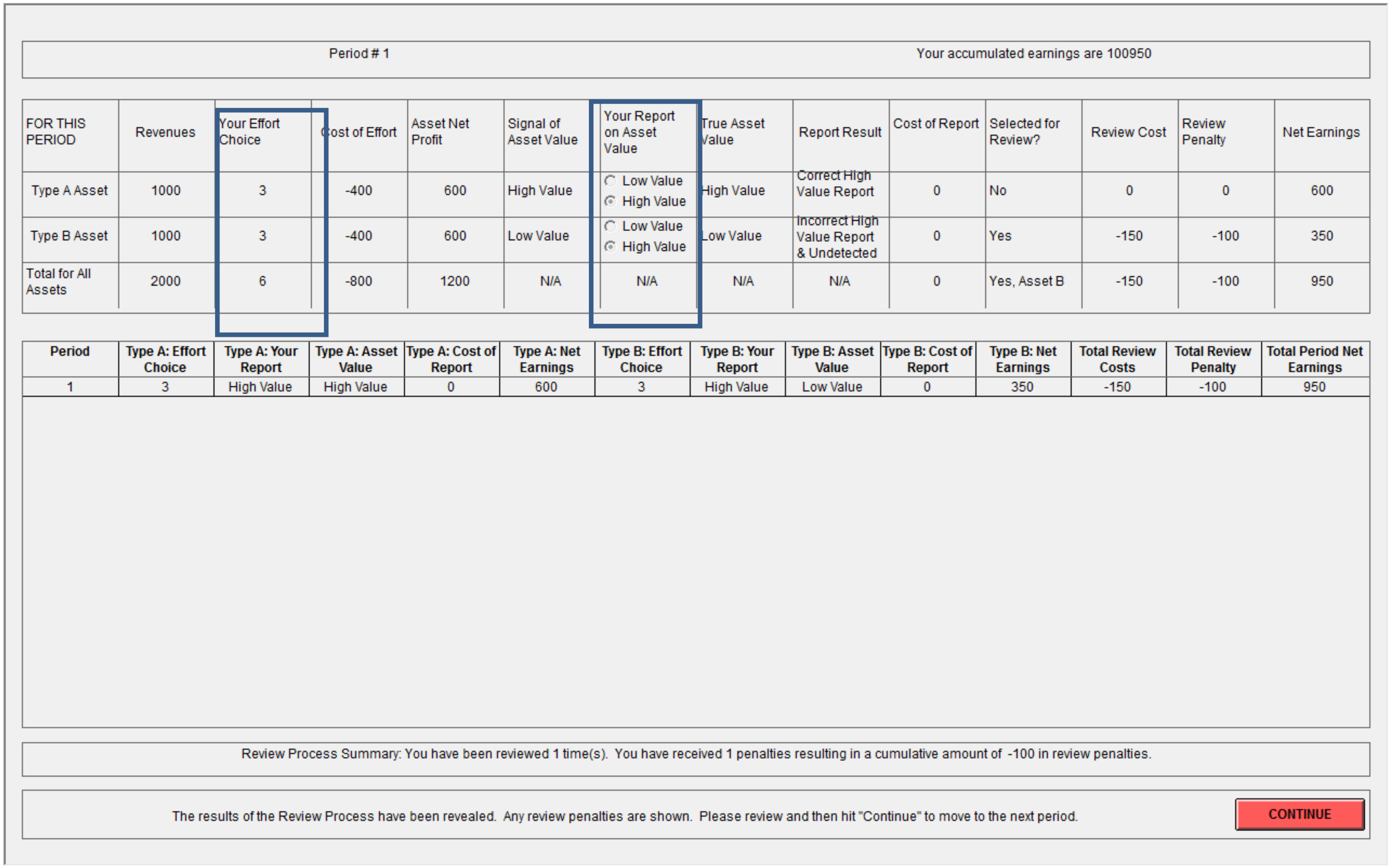

Figure 1: Screen Print of Experimental Task. The figure represents a screen print of the experimental task in z-Tree software (Fischbacher 2007). The items highlighted by the blue boxes are decisions that the participants make. The remainder of the information is feedback to the participants. For each period, feedback is not presented until the participants have made their effort and report decisions.

5.3 The Experimental Setting

The following describes the sequence of the steps in the experiments. A flowchart of the participants' decisions related to these experimental steps is included below in Figure 3, and a flowchart of the expected costs for the decisions is included in Figure 4 below. Verifiers make repeated effort and reporting decisions and accumulate experimental earnings (EE) over 20 periods. The probabilities and outcomes outlined below are modeled to mimic economic incentives in the real world audit environment. However, it's also important to note that by design, the experimental parameters are also set such that the wealth-maximizing effort and reporting decisions are held constant across conditions. The wealth-maximizing decisions are discussed in more detail in Section 5.5, in Appendix E, and are illustrated in Figure 4.

Experimental Steps:

1. Asset types. At the start of each period, verifiers receive revenues of 1,000 EE per asset to verify the values of one Type A asset and one Type B asset. For Type A assets (*lower-risk clients)*, the probability of a *low (high)* value is 0.20 (0.80). For Type B assets *(higher-risk client),* the probability of a *low (high)* value is 0.40 (0.60).

2. Effort-level choices. Verifiers make effort-level choices by allocating a fixed amount of resources among the two assets and keeping the remainder for their personal consumption. Verifiers are informed that the standard level of effort per asset is 3, but that they can allocate any amount greater than or equal to 1 for each asset, subject to the constraint that the total level of effort for both assets does not exceed 6.[10] Higher levels of effort cost more but also provide a more accurate signal about the true value of the asset.

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[10] I set the "standard" effort level equal to the wealth-maximizing level under the assumption that audit firms' standard levels of effort are the level at which the firm maximizes revenues (i.e., the point at which the marginal benefits of effort start to decline). Refer to Appendix E for a detailed discussion on the wealth-maximizing decisions. I constrained the total effort to "6" in order capture relatively high resource pressure whereby auditors have sufficient levels of resources to meet the firm's optimal standards, but do not have extra resources.

3. Asset value signals. Based on the effort-level choice, verifiers receive a signal about the true value of the asset (*high*, *low)*. When the true value of the asset is *high*, the signal will always be accurate. However, when the true value of the asset is *low*, the signal will be incorrect with some probability dependent on the effort-level choice (referred to as the "error rate"). By design, the error rates are higher for *higher-risk clients* (ranging from 12 percent to 40 percent) than for *lower-risk clients* (ranging from 6 percent to 20 percent). All participants received a table detailing the cost of effort and the corresponding error-rates of the signal for each effort-level choice. A copy of this table included below in Table 1.

Table 1: Cost of Effort and Signal Error Rates for Effort-Level Choices

Effort-Level Choice	Total Cost of Effort	Signal Error Rate – True Asset Value is "Low" but Signal says "High"	
		Type A Assets	Type B Assets
1	350	20%	40%
2	375	16%	28%
3	400	8%	16%
4	450	7%	14%
5	500	6%	12%

4. Reporting choices and outcomes. For each asset, verifiers report whether the asset has a *high* or *low* value. If the verifier reports that the asset has a *low* value (i.e., disagrees with the seller), they are charged a flat cost of 500 EE, regardless of the outcome. If the verifier reports that the asset has a *high* value (i.e., agrees with the seller), the report cost depends on the true value of the asset. If the true value of the asset is *low*, there is a 50 percent chance that the incorrect *high* report will be detected. If the incorrect *high* report is detected, s/he is charged an incorrect report cost of 6,000 EE.[11] The incorrect report cost represents a discovered Type II error which has significant consequences to audit firms (i.e., costs for litigation and reputational damage).

[11] Given the amount of incorrect report costs, there was potential for participants to accumulate negative earnings. Relatively few participants ended the experimental session in negative earnings (i.e., bankrupt). The percent of participants who went bankrupt did not differ across *Inspection* conditions (*p-value* > 0.10). For participants who went bankrupt, I informed them *after* the experimental session was completed that they would earn $0 for the experimental study and that they would *not* be required to pay any additional amounts for the negative earnings. Participants still received full compensation for the show-up fee, for the risk-preference task, and for completing the post-experimental questionnaire.

5. Review process. In the *Inspections* condition, verifiers are informed that there is a 50 percent chance in each period that they will be selected for review by a review board. Further, they are informed that if they are selected for review, one of their assets will be reviewed and there is a 0.05 (0.95) probability that their Type A, *lower-risk* (Type B, *higher-risk*) asset will be reviewed.[12] When the verifier is selected for review, s/he is charged a cost of 150 EE for the review. In addition, s/he is penalized based on the effort-level chosen for that asset.[13] The penalties range from 150 EE "severe" to 75 EE "moderate" to 0 EE "none." Only the maximum effort-level choice avoids receiving a penalty.[14] The summary of the review penalty costs that was provided to the participants is included below in Figure 2. Finally, participants are informed that at the conclusion of the experiment, the review board (the experimenter) will announce each person's review penalties to the group, one by one in order of severity (highest to lowest amounts).[15]

Effort-Level:	**1**	**2**	**3**	**4**	**5**
	Severe		**Moderate**		**None**
Penalty:	**150**	**125**	**100**	**75**	**0**

Figure 2: Review Penalties for Effort-Level Choices

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[12] While in the real world, a partner or an audit firm may have several clients selected for inspection review, it is doubtful that 100 percent of their clients would be selected. Therefore, I chose to only have a maximum of one asset selected for review. This design choice also allowed me to operationalize the risk-based nature of the selection process in that only a higher-risk client is anticipated for review and not a lower-risk client.

[13] Penalties are based on effort-level choices, rather than reporting outcomes, in order to better reflect the PCAOB's inspection practice, which is inherently a review of the audit *process* rather than of the audit *outcome*. Participants were explicitly made aware during the instructions that the review penalties were based on effort-level choices and *not* their reporting outcomes. Comprehension of this information was confirmed during the pretest that occurred prior to the experiment.

[14] The auditors' wealth-maximizing decision is an effort-level of "3" but auditors receive an inspection penalty for anything lower than the maximum effort-level choice. This design choice was implemented in order to demonstrate the PCAOB's preference that more effort is better than less. Also, it seems that if auditors' work and documentation is anything less than satisfactory, they will incur costs for deficiencies issued by the PCAOB and/or through incurring time to satisfy the PCAOB's inquiries. Essentially, this design choice recognizes an expectations gap PCAOB's standards and auditors' optimal model, whereby the standards for the optimal effectiveness of an audit are likely higher for the PCAOB than for an audit firm.

[15] Alternatively, I could have announced the review penalties after each period. Either way, I anticipate the public announcement of the review penalties to induce social pressure with effects similar to that induced by the PCAOB's practice of reporting the audit firm's deficiencies in a public report. I chose to announce the penalties one time at the end of the experiment due to time constraints of the experiment.

6. Feedback. Verifiers receive feedback about the asset values, report outcomes, review results, and earnings for the period. Then, the next period begins and the procedures are repeated.

To enhance comparability, for each period I randomly predetermine the states of the assets and reviews based on the probability parameters disclosed to the participants in the instructions. I hold these states constant across both *Inspection* conditions, which facilitates making comparisons of auditor behavior across conditions. The predetermined states each period include: (1) the true values of the assets, (2) the signals for each level of effort, (3) whether or not an incorrect high report will be detected, (3) whether or not a review will take place, and (4) the asset that will be reviewed.

5.4 Post-experimental Procedures and Payoffs

Participants completed a post-experimental questionnaire that included questions about demographics, comprehension questions, insights into how participants made their effort and reporting decisions, and other control variables (included in Appendix C). Participants also completed an option-choice task that measures their risk preferences (included in Appendix D).[16] The participants received compensation for the option-choice task, ranging from $0.10 to $3.80, based on the outcome of their decisions.

Participants were paid in cash at the end of the session. They received a show-up fee, plus their accumulated earnings in the experimental task, plus compensation from the option-choice task. Only the experimenter was made aware of their payoff, which is consistent with other similar experimental economics studies in the auditing literature (e.g., Dopuch and King 1992; Grant et al. 1996). The average payment was $18.10 and

[16] I measure individual risk preferences using a modified version of the Holt and Laury (2002) instrument.

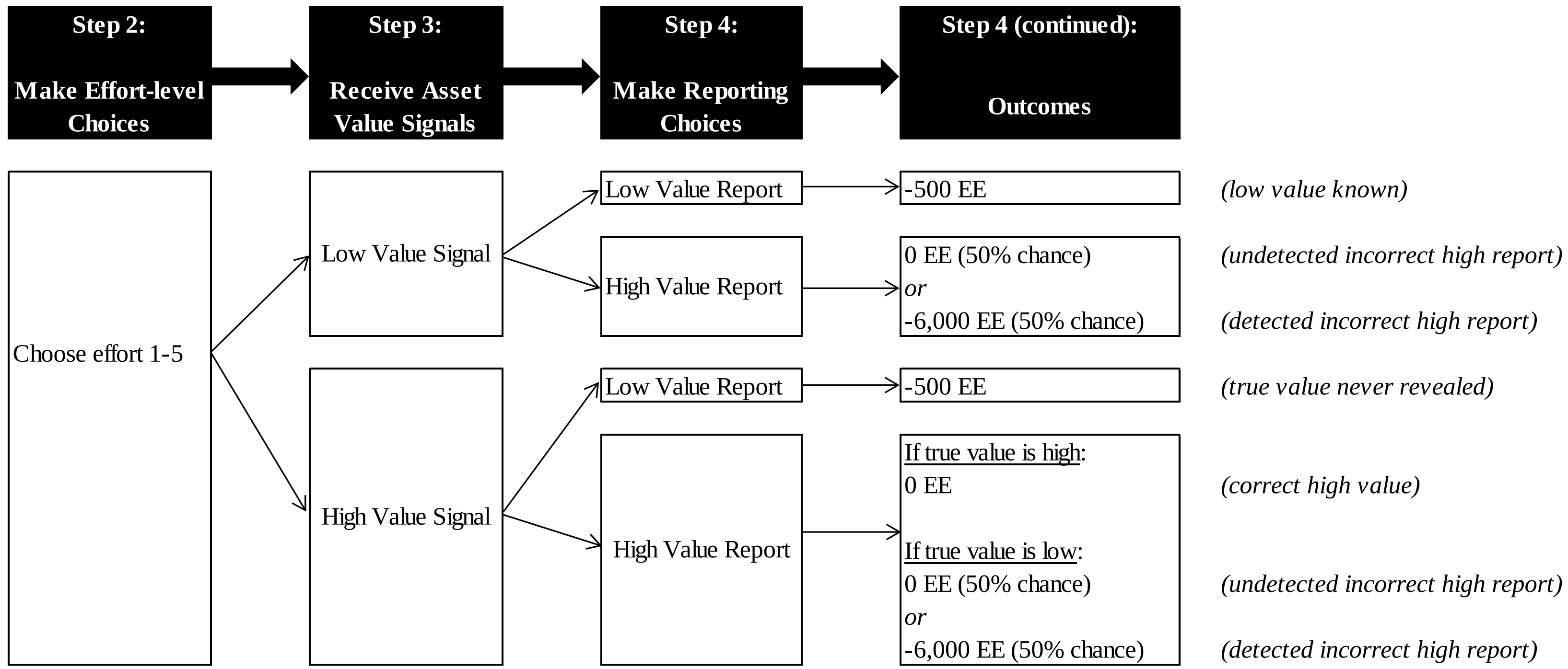

Figure 3: Flowchart of Participants' Decisions in the Experimental Steps. Represents a flowchart of the participants' decisions in experimental steps 2 through 4. Included are the possible actions participants can make (e.g., effort-level choices and reporting choices) and the corresponding possible outcomes. The probabilities and expected values for each potential outcome are detailed in Figure 4. The experimental steps outlined above are the same for higher-risk and lower-risk clients and do not differ across *Inspection* conditions.

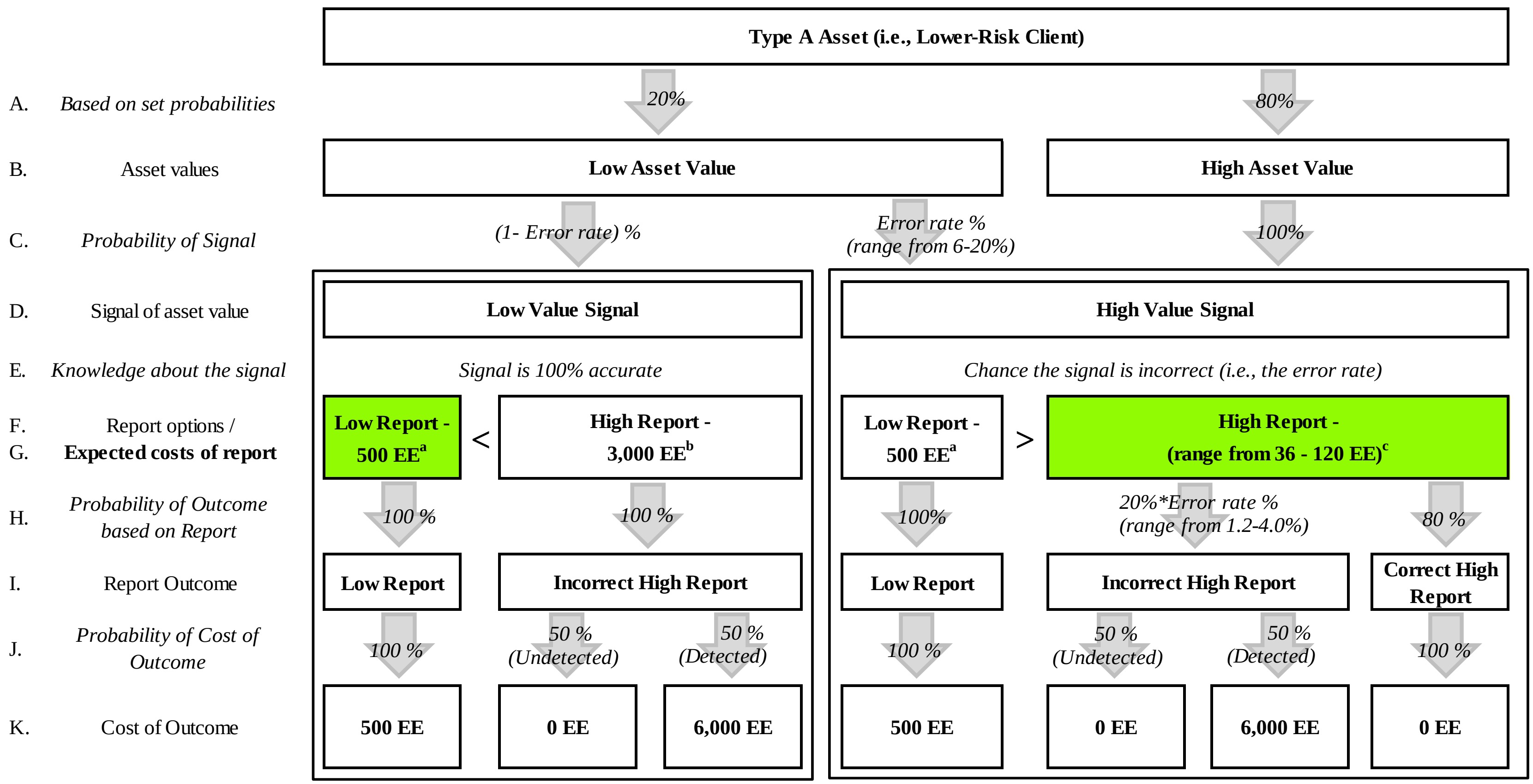

Figure 4: Panel A. Flowchart of Expected Costs for Report Decisions for Lower-Risk Clients. The boxes highlighted in green represent the wealth-maximizing report choices following the respective signals, which are to follow the signal.

a. The cost of reporting a *low* value is always 500 EE regardless of the actual value of the asset.

b. A "low value signal" is 100% accurate. If a participant reports a *high* value after receiving a "low value signal," there is a 50% chance that the incorrect high report will be detected. The cost of an incorrect *high* report is 6,000 EE if detected; therefore, the expected value is 3,000 EE (i.e., 6,000 EE * 0.5).

c. If the true value of the asset is *low*, the signal is not 100% accurate. The signal error rate for a *low* value asset ranges from 20% for an effort-level choice of "1" to 6% for an effort-level choice of "5." The expected cost of issuing a *high* report after receiving a *high* value signal is always less than the cost of issuing a *low* report (i.e., 500 EE). The expected cost is calculated as follows: 20% chance of being a low value asset * the error rate of the signal * 50% chance of detecting an incorrect high report * 6,000 EE cost of an incorrect high report. Therefore, the expected cost ranges from 36 to 120 EE.

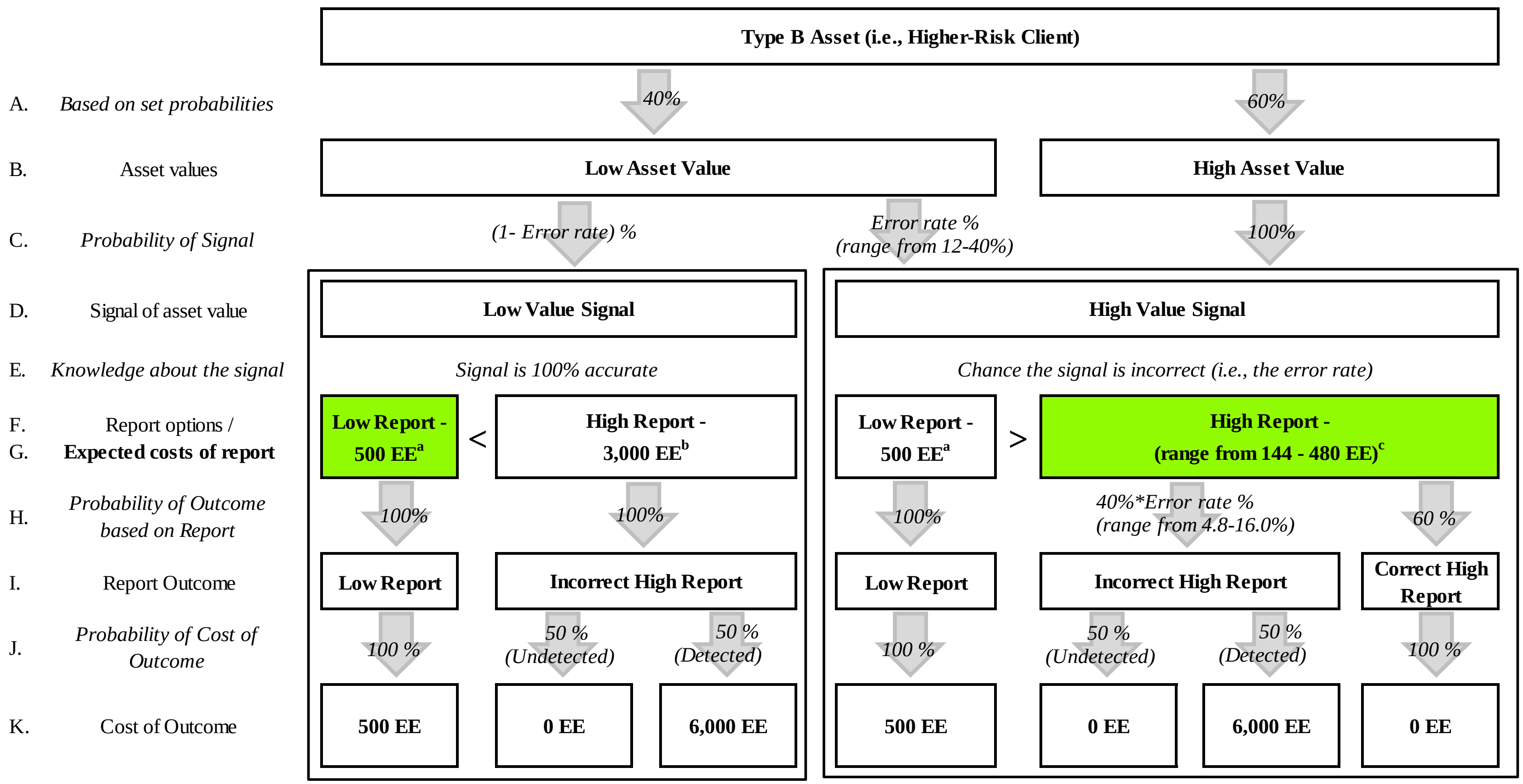

Figure 4: Panel B. Flowchart of Expected Costs for Report Decisions for Higher-Risk Clients. The boxes highlighted in green represent the wealth-maximizing report choices following the respective signals, which are to follow the signal.

a. The cost of reporting a *low* value is always 500 EE regardless of the actual value of the asset.

b. A "low value signal" is 100% accurate. If a participant reports a *high* value after receiving a "low value signal," there is a 50% chance that the incorrect high report will be detected. The cost of an incorrect *high* report is 6,000 EE if detected; therefore, the expected value is 3,000 EE (i.e., 6,000 EE * 0.5).

c. If the true value of the asset is *low*, the signal is not 100% accurate. The signal error rate for a *low* value asset ranges from 40% for an effort-level choice of "1" to 12% for an effort-level choice of "6." The expected cost of issuing a *high* report after receiving a *high* value signal is always less than the cost of issuing a *low* report (i.e., 500 EE). The expected cost is calculated as follows: 20% chance of being a low value asset * the error rate of the signal * 50% chance of detecting an incorrect high report * 6,000 EE cost of an incorrect high report. Therefore, the expected cost ranges from144 to 480 EE.

the range was $10 to $34. The average payment did not differ across *Inspection* conditions (*p-value* > 0.10).[17] The experimental sessions lasted approximately 60 minutes, ranging from 50 to 80 minutes.

5.5 Wealth-maximizing Behavior

In this study, I am interested in studying the non-wealth-maximizing effects of inspections on auditor behavior. In order to better isolate these effects, I construct a setting where the wealth-maximizing predictions are held constant across the *Inspection* conditions and also for lower- and higher-risk clients. Therefore, any differences in observed behavior across conditions cannot be attributable to wealth-maximizing reasons. I explain the parameters and wealth-maximizing behavior in detail in Appendix E, and I provide a brief summary below.

For effort-level decisions, I set the standard level of effort 3 equal to the wealth-maximizing effort-level choice. As can be seen in Panel A of Figure 2, the cost of effort has increasing net marginal benefits from effort-levels 1 to 3 and then has decreasing net marginal benefits beyond 3.[18] For reporting decisions (*high/low*), by design, the wealth-maximizing decision is always to follow the signal. Figure 4 (Panels A and B) illustrates the expected costs of the various report choices for lower-risk (higher-risk) clients. Following the signal always yields the lowest expect cost. Finally, the inspection review process does not alter the wealth-maximizing decisions. In summary, the wealth-

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[17] In an attempt for participants to earn a similar payment, on average, across the experimental conditions, I used different conversion rates from EE to $ for the two *Inspection* conditions. Therefore, while participants in the *Inspections* condition accumulated lower earnings in the experiment, their overall payment did not differ from those in the *No Inspections* condition.

[18] The "benefits" represent the accuracy of the signal. Obtaining an accurate signal is necessary in order to avoid incorrect report costs of 6,000 EE.

maximizing decisions are to always choose an effort-level of "3" and to follow the signal when reporting; this holds for both lower- and higher-risk clients under a regime with and without inspections. Given this design, any differences in effort or reporting decisions across conditions are assumed to be for reasons other than to maximize wealth.

CHAPTER 6

RESULTS

6.1 Participant Demographics, Comprehension Questions, and Other Control Variables

Participant demographic information is included in Panel A of Table 2. Forty-nine undergraduates participated in the experiment. Fifty-one percent of participants are female, and the average age is 20 years. None of the participant demographic variables significantly differ across *Inspection* conditions (p-values > 0.55). Therefore, the variables are not included in the models for hypotheses testing.[19]

Due to the amount of detail and complexity in the experimental instructions and procedures, it is important to ensure participants adequately understood the key concepts of the experiment. In addition to performing three practice periods, participants completed a series of true and false pretest questions prior to starting the experiment in order to measure their comprehension of the key concepts in the experiment. For example, the pretest questions asked about the details of allocating effort, the signals of the true asset values, the details of incorrect reports, and the review process. The pretest questions are included in Appendix B. There are five (eight) pretest questions for the *No Inspections* (*Inspections)* condition.

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[19] To ensure the demographic variables have no impact in the analyses for hypotheses testing, I include each of the demographic variables as covariates (separately) in each of the analyses used for hypotheses testing and note that the inferences of the hypotheses remain unchanged. Specifically, related to effort, the *Inspections* and *Inspections X Client Risk* effects are significant at the p = 0.05 level (H1, H2). Likewise, related to decision performance, the *Inspections X Client Risk* effect is significant at the p = 0.05 level (H3).

Table 2. Participant Demographics, Comprehension Questions, and Control Variables

Panel A: Participant Demographics	All conditions (n = 49)	No Inspections (n = 20)	Inspections (n = 29)	t-stat	*p*-value
Year in School (percent)					
1st	14.3	25.0	6.9		
2nd	24.5	15.0	31.0		
3rd	24.5	25.0	24.1		
4th	26.5	35.0	20.7		
Other	10.2	0.0	17.3		
Total	100.0	100.0	100.0	-0.155	0.877
Major (percent)					
Accounting	4.1	5.0	3.4		
Other-business	42.8	35.0	48.2		
Non-business	34.7	60.0	41.3		
Unknown	18.4	0.0	7.1		
Total	100.0	100.0	100.0	0.389	0.699
Female (percent)	51.0	40.0	65.3	-0.460	0.648
Age (mean)	20	20	20	-0.602	0.550

Table 2: Participant Demographics, Comprehension Questions, and Control Variables (Continued)

Panel B: Comprehension questions (percent accurate)	All conditions	No Inspections	Inspections	t-stat	*p*-value
Pretest questions (included in Appendix B)	85.9	84.0	88.2	1.143	0.260
Post-experimental questionnaire (included in Appendix C):					
If selected for review, Type B assets had a 95% chance of being reviewed	n/a	n/a	100.0	n/a	n/a
Review penalties are based on effort-level decisions	n/a	n/a	96.6	n/a	n/a
If the asset value was low, but you reported "high value," there was a 50% chance you would incur a cost of 6,000 EE	89.3	90.0	88.9	1.376	0.175

Panel C: Other control variables (means)	All conditions	No Inspections	Inspections	t-stat	*p*-value
Post-experimental questionnaire:					
How would you characterize your current economic situation? (on an 11-point scale from "poor" to "wealthy")	5.96	5.55	6.24	-0.996	0.325
How would you characterize the amount of money earned for participating in this experiment? (on an 11-point scale from "nominal amount" to "considerable amount")	5.65	6.05	5.38	0.836	0.407
How interesting did you find this experiment? (on an 11-point scale from "not very" to "very")	8.00	8.70	7.52	1.353	0.183
Option choice task:					
Risk preference from 1-10 with higher numbers representing higher risk preferences. Refer to Appendix D for the task.	6.34	6.30	6.39	-0.123	0.903
Task performance:					
Total profit earned during the experiment	3,662	5,038	2,714	1.529	0.133

As reported in Panel B of Table 2, participants correctly answered 84.0 (88.2) percent of the pretest questions in the *No Inspections* (*Inspections*) condition, and importantly, the percentages do not differ significantly across *Inspection* conditions (t_{47} = 1.143, p = 0.260). After participants answered each pretest question, the correct answer was displayed along with a detailed description about the key concept.

In addition to the pretest questions, additional comprehension questions were elicited at the end of the experiment during the post-experimental questionnaire (PEQ). I asked two questions specific for participants in the *Inspection* condition, and one question for all participants. In the *Inspection* condition, 29 out of 29 (or 100 percent) participants correctly indicated that Type B assets (i.e., higher-risk clients) had a 95 percent chance of being reviewed if selected for review, and 28 out of 29 (or 96.6 percent) participants correctly indicated that review penalties were based on their effort-level decisions. The third comprehension question asked all participants about the consequences of issuing an incorrect high report (i.e., a Type II error). Comprehension of this question adds validity to the results related to decision performance (i.e., H3). Approximately 89 percent of participants correctly answered the question, and importantly, there is no difference in percent correct across *Inspection* conditions (p-value = 0.175). Further, I note that the inferences from the hypotheses testing remain unchanged if I include or exclude the participants who failed one or more of the comprehension questions; therefore, I maintain all participants when conducting all analyses.

Several other control variables were obtained in order to rule out alternate explanations for hypotheses testing (refer to Panel C of Table 2). First, several questions were asked during the PEQ. For example, participants were asked to characterize their current economic situation, the amount of money earned for participating in the experiment, and how interesting they found the experiment. Second, I measure participants' risk preferences by asking them to complete the option choice task (included in Appendix D). Third, as a measure of task performance I examine participants' total profit earned during the experiment. Importantly, as shown in Panel C, none of the control variables differ significantly across *Inspection* conditions (*p-values* > 0.10). Further, the inferences from the analyses reported for hypotheses testing remain unchanged when each of the control variables are included as covariates in the models.[20] Accordingly, with the exception of risk preferences, I exclude these control variables from further analyses and discussion. I further consider risk preferences in the analyses due to its relative importance in participant decision-making during experiments with economic incentives.

6.2 Results of Risk-based Inspections on Auditor Effort

6.2.1 Tests of H1 and H2

The results of risk-based inspections on auditor effort for higher- and lower-risk clients are included in Figure 5 and Table 3.

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[20] Participants' perceptions about the amount of money earned for participating in the experiment and participants' total profit earned during the experiment were both significant in the auditor effort model (i.e., the main effects were significant at the $p = 0.05$ level). However, including the control variables did not influence the findings related to H1 and H2. The main effect of *Inspections* (H1) and the *Inspections X Client Risk* effect (H2) are still significant at the $p = 0.06$ level when including each of these covariates. Likewise, while participants' total profit earned during the experiment and participants' risk preferences are both significant in explaining decision performance ($p < 0.05$ for the main effects), including the covariates does not influence the *Inspections X Client Risk* effect (H3) on decision performance. In summary, the results reported for H1-H3 are robust to controlling for these other factors.

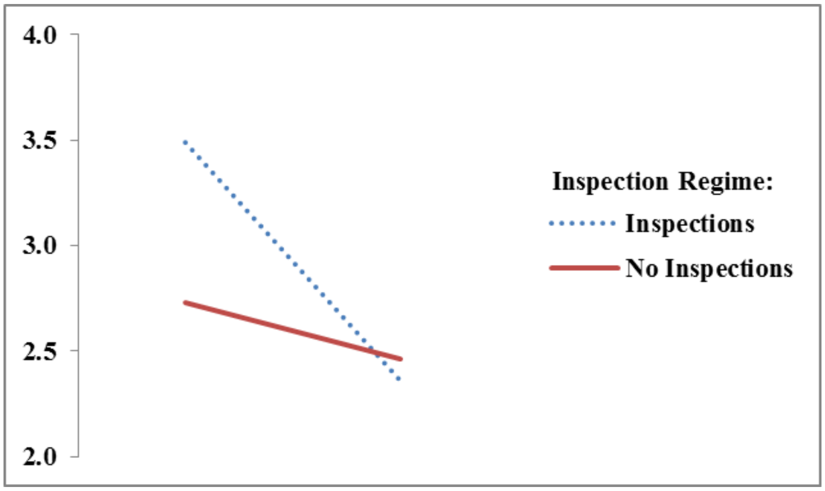

Figure 5: Results of Risk-based Inspections on Auditor Effort. The dependent measure, is *Auditor Effort*.

In order to construct independent observations of auditor effort, each participant's effort-level decisions are averaged across all 20 periods, such that each participant provides a single observation for higher-risk and lower-risk clients. Thus, auditor effort represents the average effort for each participant across the 20 periods. However, in supplemental analysis I complete robustness checks to ensure the results are not sensitive to changes in effort decisions over time (e.g., I control for period effects and analyze later periods alone).

H1 predicts that auditor effort will be higher under an inspections regime than under a regime without inspections. As reported in Panel A of Table 3, the mean total effort is 5.85 in the *Inspections* condition compared to 5.19 in the *No Inspections* condition. H2 predicts that under risk-based inspections, auditor effort will increase more for higher-risk client than for lower-risk clients as compared to a regime without inspections. The average auditor effort for higher-risk clients is 3.49 in the *Inspections* condition compared to 2.73 in the *No Inspection*s condition; the average auditor effort for

Table 3: Results of Risk-based Inspections on Auditor Effort

Panel A: Mean (standard deviation) Auditor Effort

	Higher-Risk	**Lower-Risk**	**Total**
No Inspections	2.73 (0.62) [n=20]	2.46 (0.62) [n=20]	5.19 (1.19) [n=20]
Inspections	3.49 (0.75) [n=29]	2.36 (0.72) [n=29]	5.85 (0.35) [n=29]
Combined	3.18 (0.79) [n=49]	2.40 (0.68) [n=49]	5.58 (0.86) [n=49]

Panel B: Repeated Measures ANOVA on Auditor Effort

Between-subjects effects:	df	SS	MS	*F*	*p*-value	
Inspections	1	2.598	2.598	8.048	0.003*	H1
Between-subjects error	47	15.170	0.323			
Within-subjects effects:						
Client Risk	1	11.547	11.547	17.977	<0.001	
Inspections X Client Risk	1	4.396	4.396	6.843	0.012	H2
Within-subjects error	47	30.189	0.642			

Panel C: Simple Effects Tests for Hypotheses (Univariate Analysis)

	df	*T*	*p*-value
Higher-risk clients: Effect of Inspections	47	-3.729	0.001
Lower-risk clients: Effect of Inspections	47	0.500	0.620

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

lower-risk clients is 2.36 in the *Inspections* condition compared to 2.46 in the *No Inspection*s condition.

To test H1 and H2, I use a repeated measures (for *Client Risk*) analysis of variance (ANOVA) on auditor effort (refer to Panel B). The main effect of *Inspections* on auditor effort is significant (p = 0.003) in support of H1. Likewise, the *Inspections X Client Risk* effect is significant (p = 0.012) in support of H2. To better interpret the interaction, I perform simple effects tests included in Panel C. Consistent with H2, compared to a regime without inspections, risk-based inspections significantly increase auditor effort but only for higher-risk clients (p = 0.001) and not for lower-risk clients (p = 0.620).

In Panel D, I report the results of an analysis of covariance (ANCOVA) on auditor effort controlling for participants' risk preferences. H1 and H2 are robust to controlling for participants' risk preferences evidenced by the significant *Inspections* (p = 0.024) and *Inspections X Client Risk* (p = 0.018) effects in the ANCOVA. Interestingly,

Table 3: Results of Risk-based Inspections on Auditor Effort (Continued)

Panel D: Repeated Measures ANCOVA on Auditor Effort

Between-subjects effects:	df	SS	MS	*F*	*p*-value	
Risk Preference (covariate)	1	0.683	0.683	1.681	0.203	
Inspections	1	1.700	1.700	4.183	0.024*	H1
Between-subjects error	35	14.221	0.406			
Within-subjects effects:						
Client Risk	1	0.429	0.429	1.094	0.303	
Client Risk X Risk Preference	1	0.057	0.057	0.145	0.705	
Inspections X Client Risk	1	2.411	2.411	6.143	0.018	H2
Within-subjects error	47	30.189	0.642			

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

however, the main effect of *Client Risk* is no longer significant in the ANCOVA (p = 0.303), which suggests that the main effect of *Client Risk* observed in the ANOVA (Panel B) is likely driven by participants' risk preferences.

6.2.2 Supplemental Analyses

To provide some additional context underlying the results of risk-based inspections on auditor effort, I perform additional analyses and examine whether auditor effort significantly differs from the standard, wealth-maximizing level of 3 for higher- and lower-risk clients under each *Inspection* condition (untabulated). The results indicate that in the *No Inspections* condition, auditor effort for higher-risk clients is significantly lower than the standard level of 3 (mean = 2.73, t_{19} = -1.952, p = 0.066, two-tailed) consistent with evidence of under-auditing. Whereas, auditor effort in the *Inspections* condition for higher-risk clients is at a level greater than the standard level (mean = 3.49, t_{28} = 3.506, p = 0.002, two-tailed) consistent with evidence of over-auditing. The findings suggest that risk-based inspections may be particularly effective at increasing auditor effort for higher-risk clients who formerly were receiving substandard levels of auditor effort. On the other hand, risk-based inspections appear to have no benefit on auditor effort for lower-risk clients. Auditor effort for lower-risk clients in the *No Inspections* condition is significantly lower than the standard level (mean = 2.46, t_{19} = -3.865, p = 0.001) (i.e., under-auditing) and remains at a substandard level in the *Inspections* condition (mean = 2.36; t_{28} = -4.755, $p < 0.001$).

6.3 Results of Risk-based Inspections on Auditor Decision Performance

H3 predicts that when auditors are under relatively high resource pressure, risk-based inspections lead to diminished decision performance for lower-risk clients. More specifically, that decision performance will be worse for lower-risk clients than for higher-risk clients (ceteris paribus) under a regime with inspections, but will not differ across clients under a regime without inspections.

6.3.1 Dependent Measures

The dependent variable for H3 is decision performance, specifically diminished or lower-quality decisions. Accordingly, I use measures of suboptimal decisions as the dependent measures. My primary dependent measure for suboptimal decision performance is "Suboptimal Type II Errors," obtained from participants' reporting decisions. As a secondary dependent measure, I use "Substandard Effort," obtained from participants' effort-level decisions. For both dependent measures, higher amounts of the dependent measure represent more suboptimal (i.e., worse) decision performance. To reiterate, the dependent measures represent proxies for poor decision-making quality (one related to the quality of effort decisions and one related to the quality of reporting decisions); they are not selected as measures of the quality of audit outcomes.

Primary Dependent Measure

Suboptimal Type II Errors is calculated as the number of times an auditor reported a *high* value after receiving a *low* value signal (i.e., an incorrect high report, also known as a "Type II error") as a percentage of *low* value signals received in the 20 periods. The measure is not dependent on whether or not the incorrect high report was detected (i.e., the outcome). It does not include Type II reporting errors that are unintentional (i.e., an incorrect *high* report after receiving a *high* value signal) because that report decision is not suboptimal. The dependent measure only includes Type II reporting errors that were suboptimal decisions. This reporting decision is suboptimal because a *low* value signal has 100 percent accuracy; therefore, the expected cost of issuing a *high* value report after receiving a *low* value signal is 3,000 EE (6,000 EE * 50 percent detection rate) as compared to the expected cost of 500 EE for following the signal and reporting a *low* value. While this reporting decision is suboptimal, it is not necessarily irrational; rather, it indicates that participants are willing to accept the risk (i.e., 50 percent chance) of being detected for issuing an incorrect high report.[21]

I use Suboptimal Type II Errors as the primary dependent measure for decision performance for two reasons. First, in terms of maximizing wealth, it is the most suboptimal decision participants can make during the experiment (i.e., the expected value of the decision is -3,000 EE).[22] Second, the decision is the same and the measure is constant across all conditions (i.e., *Inspection* conditions and *Client Risk* conditions). To clarify, the information related to Incorrect High Reports and the expected outcomes of reporting *high* after receiving a *low* value signal are the same across all conditions. For both higher- and lower-risk clients, participants are aware that a *low* signal is 100 percent accurate and that if they report *high*, there is a 50 percent chance of being detected and receiving an incorrect report cost of 6,000 EE. Further, the review process in the *Inspection* condition has no influence on participants' reporting decisions. That is, the

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[21] As a reminder, recall that approximately 89 percent of participants accurately answered "true" to the comprehension question "If the asset value was low, but you reported "High Value," there was a 50 percent change you would incur a 6,000 EE and a 50 percent chance you would incur a cost of 0 EE?" in the PEQ in support of the argument that participants had sufficient knowledge of the consequences for reporting *high* after receiving a *low* value signal.

[22] To provide relative amounts, participants only receive 1,000 EE in revenues to verify an asset, and the cost of following the "low" signal and reporting "low" is only 500 EE.

review penalties are only based on effort-level decisions and not on reporting decisions or reporting outcomes. Further, because the dependent measure is calculated as a *percent of low value signals*, the measure is not sensitive to (1) the varying number of *low* value assets across *Client Risk* conditions, or (2) the participants' effort-level decisions which vary across conditions. Therefore, the decision to misreport *high* after receiving a *low* value signal and the expected outcome are exactly the same across *Client Risk* and *Inspection* conditions allowing for a clean test of H3 (i.e., ceteris paribus).

Secondary Dependent Measure

As a secondary measure of suboptimal decision performance, I use a measure related to participants' effort-level decisions. I use "Substandard Effort," representing effort-level decisions below the standard, wealth-maximizing level of 3. Substandard Effort represents suboptimal decision performance because lower levels of effort yield higher error rates in the signal about the asset's true value. Therefore, chances of receiving an inaccurate signal and issuing an incorrect high report (i.e., incurring a cost of 6,000 EE) increase substantively. While effort-level decisions above the standard wealth-maximizing level of 3 are also "suboptimal," the decisions are less suboptimal than choosing substandard effort because the costs associated with selecting higher levels of effort are minimal (e.g., 50 EE) compared to the expected costs of being detected for issuing an incorrect report (e.g., 6,000 EE) which is more likely with substandard levels of effort.

I use two measures to calculate Substandard Effort: (1) an average amount of substandard effort across all periods ("Average Substandard Effort), and (2) the number of instances of substandard effort over the 20 periods ("Count of Substandard Effort").

For Average Substandard Effort, substandard effort for each period is calculated as 3 minus the effort-level decision if the effort-level decision was less than 3, and 0 otherwise. Average Substandard Effort is the average amount across the 20 periods (measures range from 0 to 2). For Count of Substandard Effort, substandard effort for each period is calculated as 1 if the effort-level decision was below the standard level 3 (i.e., a "1" or "2"), and 0 otherwise. Count of Substandard Effort is the sum of substandard effort for all 20 periods (measures range from 0 to 20).

6.3.2 Tests of H3

Primary Dependent Measure

The results of risk-based inspections on Suboptimal Type II Errors for higher- and lower-risk clients are included in Figure 6 and Table 4. Descriptive statistics are reported in Panel A of Table 4. The average Suboptimal Type II Errors (as a percent of low value

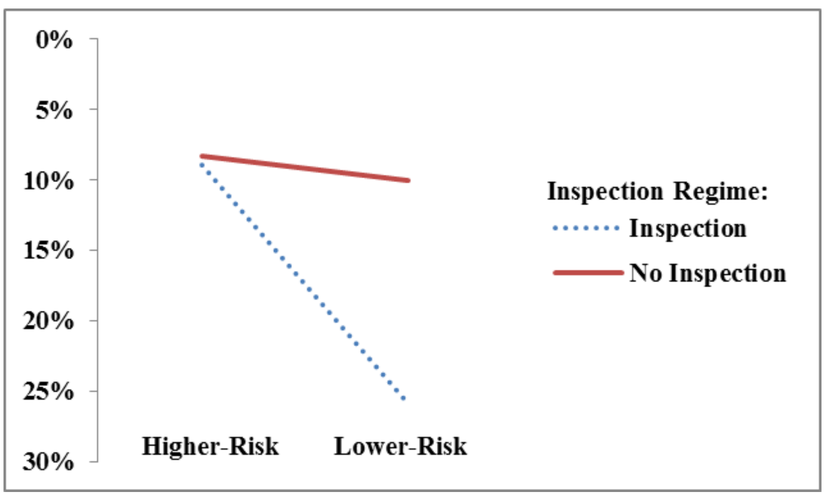

Figure 6: Results of Risk-based Inspections on Decision Performance – Primary Dependent Measure. The dependent measure is *Suboptimal Type II Errors*, a measure of suboptimal, worse decision performance. A reverse scale is used in order to better display performance where the best performance is 0 *Suboptimal Type II Errors*.

signals) is highest for lower-risk clients in the *Inspections* condition (mean = 25.9 percent, compared to a range of 8.3 to 10.0 percent in the other three conditions).

To test H3, I use a repeated measures (for *Client Risk*) ANOVA on Suboptimal Type II Errors (refer to Panel B). Consistent with H3, the *Inspections X Client Risk* effect is significant (p = 0.041). To better interpret the interaction, I test the prediction using simple effects tests included in Panel C. Consistent with H3, in the *Inspections* condition, Suboptimal Type II Errors are higher (i.e., worse) for lower-risk clients than for higher-risk clients (p = 0.009). Also consistent with H3, in the *No Inspections* condition, Suboptimal Type II Errors do not differ across lower- and higher-risk clients (p = 0.824). Further, for lower-risk clients, Suboptimal Type II Errors are higher in the *Inspections* condition than in the *No Inspections* condition (p = 0.042). Overall, the results support H3 that decision performance is diminished for lower-risk clients with risk-based inspections when auditors are under high resource pressure.

To ensure the results of H3 are robust to controlling for individual risk preferences, I conduct an ANCOVA on Suboptimal Type II Errors and include risk preferences as a covariate (refer to Panel D). The *Inspections X Client Risk* effect is significant in the ANCOVA (p = 0.039) indicating the results of H3 are robust to controlling for participants' risk preferences.

Other Robustness Checks

As described above, the reporting decisions following a *low* value signal should not be dependent on effort-level decisions (i.e., because a low value signal is 100 percent

Table 4: Results of Risk-based Inspections on Decision Performance – Primary Dependent Measure

Panel A: Suboptimal Type II Errors

	Higher-Risk	**Lower-Risk**
No Inspections	8.3	10.0
	(20.6)	(26.2)
	[25.0%]	[15.0%]
Inspections	8.9	25.9
	(15.6)	(33.8)
	[37.9%]	[44.8%]

Represents suboptimal decision performance using *Suboptimal Type II Errors*. The mean (maximum) low value signals for higher-risk assets is 11.53 (12) and 2.47 (3) for lower-risk clients. The standard deviations are included in parenthesis. The percent of individuals who had at least one Suboptimal Type II error are included in brackets.

Panel B: Repeated Measures ANOVA on Suboptimal Type II Errors

Between-subjects effects:	df	SS	MS	*F*	*p*-value	
Inspections	1	0.160	0.160	1.663	0.203	
Between-subjects error	47	4.519	0.096			
Within-subjects effects:						
Client risk	1	0.205	0.205	6.580	0.014	
Inspections X Client risk	1	0.138	0.138	4.435	0.041	H3
Within-subjects error	47	1.466	0.031			

Panel C: Simple Effects Tests for Hypotheses (Univariate Analysis)

	df	*T*	*p*-value
Inspections: Higher-risk vs. lower-risk clients	56	2.453	0.009*
No Inspections: Higher-risk vs. lower-risk clients	38	0.224	0.824
Higher-risk clients: Effect of Inspections	47	-0.111	0.912
Lower-risk clients: Effect of Inspections	47	-1.764	0.042*

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

Table 4: Results of Risk-based Inspections on Decision Performance – Primary Dependent Measure (Continued)

Panel D: Repeated Measures ANCOVA on Suboptimal Type II Errors

Between-subjects effects:	df	SS	MS	F	p-value	
Risk Preference (covariate)	1	0.709	0.709	9.173	0.005	
Inspections	1	0.172	0.172	2.219	0.145	
Between-subjects error	35	2.706	0.077			
Within-subjects effects:						
Client risk	1	0.008	0.008	0.251	0.619	
Client risk X Risk Preference	1	0.004	0.004	0.122	0.729	
Inspections X Client risk	1	0.141	0.141	4.619	0.039	H3
Within-subjects error	38	1.069	0.031			

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

accurate). Nonetheless, to ensure the results for H3 are not sensitive to effort-level decisions, I perform additional analysis on Suboptimal Type II Errors. I perform ANCOVA's on Suboptimal Type II Errors separately for higher- and lower-risk clients controlling for participants' effort-level decisions (untabulated). Consistent with H3, when controlling for effort-level decisions, the main effect of *Inspections* is marginally significant for lower-risk clients ($F = 2.789$, $p = 0.051$, one-tailed), but is not significant for higher-risk clients ($F = 0.001$, $p = 0.979$) consistent with the theory that risk-based inspections lead to diminished decision performance for lower-risk clients.

I conduct a few other robustness tests for the test of H3 using Suboptimal Type II Errors as the dependent measure. First, because the dependent measure is calculated as a percent of *low* value signals, I compare the number of *low* value signals across *Inspection* conditions noting that they do not differ for lower-risk clients ($t_{47} = 0.350$, $p = 0.728$, untabulated) or for higher-risk clients ($t_{47} = -0.827$, $p = 0.412$, untabulated). This finding rules out the alternative explanation that the number of *low* value signals is driving changes in reporting behavior. Second, I examine the number of participants who made at least one Suboptimal Type II Error. The number of participants with at least one Suboptimal Type II Error in each condition is not trivial and ranges from 15.0 to 44.8 percent (refer to Panel A of Table 4 reported in brackets). As expected the largest percent of individuals who had at least one Suboptimal Type II Error was in the *Inspections* condition for lower-risk clients (i.e., 44.8 percent). This evidence rules out the possibility that a few individuals are driving the results of H3.

Overall, the findings support that risk-based inspections lead to diminished decision performance for lower-risk clients and the effect is not driven by changes in effort, changes in signals, risk preferences, or a few individuals. Rather, I expect the effect is driven by risk-based inspections leading to a change in reporting strategy (i.e., shifting to a more risky, suboptimal strategy) for lower-risk clients when auditors are also under high resource pressure.

Secondary Dependent Measure

The results of risk-based inspections on auditors' decision performance for higher- and lower-risk clients using both measures of Substandard Effort are included in Table 5. The two measures yield consistent results. The descriptive statistics are reported in Panel A of Table 5. Decision performance is the worst for lower-risk clients in the *Inspection* condition for both Average and Count of Substandard Effort.

Table 5: Results of Risk-based Inspections on Decision Performance – Secondary Dependent Measure

Panel A: Mean (standard deviation) Substandard Effort

	Average Substandard Effort		Count of Substandard Effort	
	Higher-Risk	**Lower-Risk**	**Higher-Risk**	**Lower-Risk**
No Inspections	0.350	0.550	4.00	7.25
n=20	(0.600)	(0.637)	(6.67)	(7.44)
Inspections	0.126	0.717	2.07	9.72
n=29	(0.235)	(0.658)	(4.23)	(7.99)

Represents suboptimal decision performance using *Substandard Effort*.

Panel B: Repeated Measures ANOVA on Substandard Effort

	Average Substandard Effort				Count of Substandard Effort			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Inspections	1	0.019	0.045	0.833	1	1.746	0.031	0.861
Between-subjects error	47	0.428			47	56.622		
Within-subjects effects:								
Client Risk	1	3.707	21.533	<0.001	1	703.829	21.732	<0.001
Inspections X Client Risk	1	0.907	5.267	0.026	1	114.849	3.546	0.066
Within-subjects error	47	0.172			47	32.386		

Table 5: Results of Risk-based Inspections on Decision Performance – Secondary Dependent Measure (Continued)

Panel C: Simple Effects Tests for Hypotheses (Univariate Analysis)

	Average Substandard Effort			Count of Substandard Effort		
	df	T	*p*-value	df	T	*p*-value
Inspections: Higher- vs. lower-risk	56	4.560	<0.001*	56	4.563	<0.001*
No Inspections: Higher- vs. lower-risk	38	1.023	0.313	38	1.454	0.154

Panel D: Repeated Measures ANCOVA on Substandard Effort

	Average Substandard Effort				Counts of Substandard Effort			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Risk Preference (covariate)	1	0.612	1.274	0.267	1	201.630	3.375	0.075
Inspections	1	0.057	0.118	0.734	1	0.000	0.000	0.999
Between-subjects error	35	0.481			35	59.747		
Within-subjects effects:								
Client Risk	1	0.263	2.285	0.140	1	71.085	3.105	0.087
Client Risk X Risk Preference	1	0.000	0.000	0.992	1	1.242	0.054	0.817
Inspections X Client Risk	1	0.526	4.563	0.040	1	75.104	3.281	0.079
Within-subjects error	35	0.115			35	22.891		

To test H3, I use a repeated measures (for *Client Risk*) ANOVA on Substandard Effort (refer to Panel B). Consistent with H3, the *Inspections X Client Risk* effect is significant on Average Substandard Effort (p = 0.026) and is marginally significant on Count of Substandard Effort (p = 0.066). The simple effects tests reported in Panel C further support H3 whereby decision performance in the *Inspections* condition is worse for lower-risk clients than for higher-risk clients (*p-values* < 0.001), but does not differ across lower- and higher-risk clients in the *No Inspection* condition (*p-values* > 0.10). As shown in Panel D, the reported results for H3 using both measures of Substandard Effort are robust to controlling for individuals' risk preferences (i.e., the *Inspections X Client Risk* effects are significant at the p = 0.10 level).

CHAPTER 7

SUPPLEMENTAL ANALYSES

7.1 Effects over Time

7.1.1 Auditor Effort

When testing H1 and H2, I use an average measure of effort for each participant. However, because participants make repeated effort-level decisions over 20 periods, I consider the potential for an effect of period on auditor effort. The average effort-level decisions over the 20 periods for both *Inspection* conditions and for higher- and lower-risk clients are graphically displayed in Figure 7. There appears to be a slight decline in effort over time. To ensure that the results reported for H1 and H2 are not sensitive to any period effects, I perform additional analyses.

First, I perform a repeated-measures ANOVA including *Period* and *Client Risk* as repeated measures on auditor effort. The results are included in Table 6. Although the findings indicate a significant effect of *Period* on auditor effort, including *Period* does not influence the results reported for H1 and H2. Specifically, the *Inspections* (H1) and *Inspections X Client Risk* (H2) effects are significant at the $p = 0.05$ levels when controlling for period effects. To further support H2, as shown in Panel B, when controlling for *Period,* risk-based inspections still significantly influence auditor effort for higher-risk clients ($p < 0.001$), but do not influence auditor effort for lower-risk clients ($p = 0.620$). The results are consistent with the main findings reported.

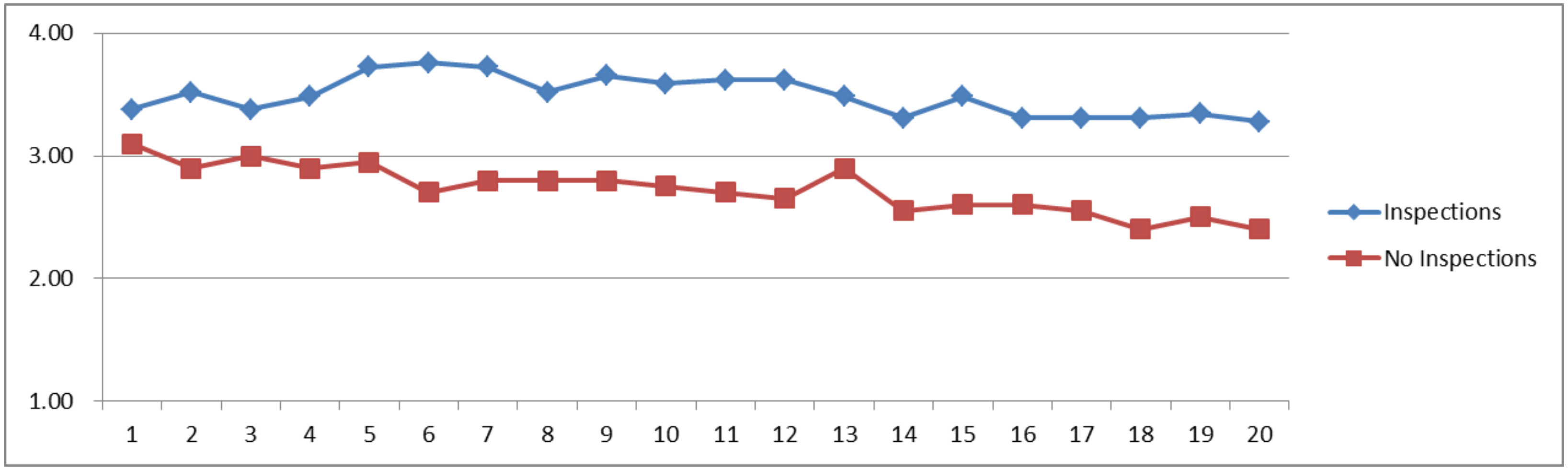

Figure 7: Panel A. Effort over Time for Higher-risk Clients

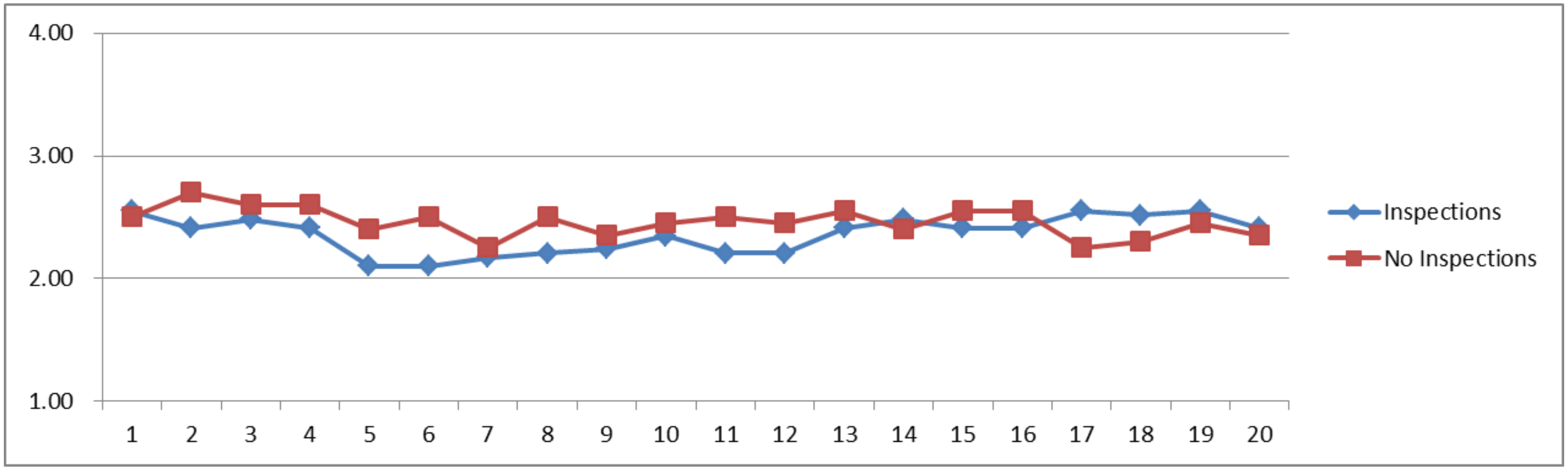

Figure 7: Panel B. Effort over Time for Lower-risk Clients

Table 6: Supplemental Analyses: Effect of Period on Auditor Effort

Panel A: Repeated Measures for Period and Client Risk ANOVA on Auditor Effort

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Inspections	1	51.952	51.952	8.048	0.003*
Between-subjects error	47	303.394	6.455		
Within-subjects effects:					
Period	19	10.948	0.576	2.719	<0.001
Period X Inspection	19	7.223	0.380	1.794	0.020
Client Risk	1	230.944	230.944	17.977	<0.001
Inspections X Client Risk	1	87.913	87.913	6.843	0.012
Period X Client Risk	19	18.175	0.957	2.023	0.006
Period X Client Risk X Inspections	19	9.083	0.478	1.011	0.445

Panel B: Repeated Measures for Period ANOVA on Auditor Effort – Separate for Client Risk

	Higher-Risk Clients				**Lower-Risk Clients**			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Inspections	1	137.515	13.903	<0.001*	1	19.240	0.250	0.620
Between-subjects error	47	9.891			47	9.411		
Within-subjects effects:								
Period	19	1.060	3.044	<0.001	19	0.473	1.405	0.116
Inspections X Period	19	0.450	1.294	0.178	19	0.408	1.211	0.240
Within-subjects error	47	6.614			47	0.337		

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

Table 7: Supplemental Analyses: Auditor Effort for Later Periods

Panel A: Mean (standard deviation) Auditor Effort

	Periods 11-20			**Periods 16-20**		
	Higher-Risk	**Lower-Risk**	**Total**	**Higher-Risk**	**Lower-Risk**	**Total**
No Inspections	2.59	2.44	5.02	2.49	2.38	4.87
n=20	(0.74)	(0.72)	(1.43)	(0.86)	(0.77)	(1.58)
Inspections	3.41	2.42	6.00	3.33	2.49	5.82
n=29	(0.75)	(0.72)	(0.47)	(0.81)	(0.75)	(0.65)

Panel B: Repeated Measures ANOVA on Auditor Effort

	Periods 11-20				**Periods 16-20**			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Inspections	1	3.827	7.986	0.003*	1	5.349	8.486	0.002*
Between-subjects error	47	0.479			47	0.630		
Within-subjects effects:								
Client Risk	1	7.687	12.945	0.001	1	5.357	8.394	0.006
Inspections X Client Risk	1	4.173	7.027	0.011	1	3.166	4.961	0.031
Within-subjects error	47	0.594			47	0.638		

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

Second, I perform additional analysis to mitigate any potential effects of participant learning over time on auditor effort. I re-perform the analysis on auditor effort, but exclude the earlier periods when learning may have occurred. Results of auditor effort for periods 11-20 (the second half of the experiment) and for periods 16-20 (the last five periods) are reported in Table 7. The results are consistent with those reported in the main analyses for periods 1-20. Importantly, as shown in Panel B, the *Inspections* and *Inspections X Client Risk* effects on auditor effort are significant at the $p = 0.05$ level for periods 11-20 and for periods 16-20. Further, like the main analyses, the effect of risk-based inspections on auditor effort is only significant for higher-risk clients (p-values = 0.001) and not for lower-risk clients (p-values > 0.10) for both reduced data sets (refer to Panel C).

Table 7: Supplemental Analyses: Auditor Effort for Later Periods (Continued)

Panel C: Simple Effects Tests (Univariate Analysis)

	Periods 11-20			**Periods 16-20**		
	df	T	*p*-value	df	T	*p*-value
Higher-risk clients: Effect of Inspections	47	-3.799	<0.001	47	-3.478	0.001
Lower-risk clients: Effect of Inspections	47	0.085	0.933	47	-0.497	0.621

7.1.2 Decision Performance over Time

Like auditor effort, it's important to consider the potential for period effects on auditor decision performance. However, examining the effect of inspections on decision performance over time is less straight forward, especially for Suboptimal Type II Errors (the primary dependent measure). Unlike effort decisions, participants' reporting decisions are dependent on other information and events that occur during the period such as participants' effort-level decisions and signals about the asset values. Further, Suboptimal Type II Errors represents the average number of times participants incorrectly report *high* after receiving a *low* value signal as a percentage of low value signals *for the entire experiment*. Therefore, performing a repeated-measure ANOVA on Suboptimal Type II Errors including *Period* as a repeated measure is not feasible.

Likewise, examining Suboptimal Type II Errors for later periods 11-20 and 16-20 has limitations because there are fewer opportunities for misreporting with a reduced number of periods. Specifically, during periods 11-20, for higher-risk clients there are eight *low* value assets, but for lower-risk clients there is only one *low* value asset. Nonetheless, I re-perform the ANOVA on Suboptimal Type II Errors for periods 11-20 in the supplemental analyses (included in Table 8). As reported in Panel A, the percentage of Suboptimal Type II Errors for periods 11-20 across conditions is consistent with that for periods 1-20. For instance, Suboptimal Type II Errors are the highest for lower-risk clients in the *Inspection* condition. However, the *Inspections X Client Risk* effect is not significant in the ANOVA when limiting the analyses to periods 11-20. While the lack of results could be attributable to changes in reporting behavior over time, it seems more likely there is a lack of power to observe an effect with the reduced data set.

On the other hand, using Substandard Effort to test for period effects on decision performance is not dependent on other information or events that occur during the period (i.e., similar to auditor effort) allowing for a more powerful test of period effects on

Table 8: Supplemental Analyses: Decision Performance for Later Periods – Primary Dependent Measure

Panel A: Suboptimal Type II Errors for Periods 11-20

	Higher-Risk	**Lower-Risk**
No Inspections	10.0	10.0
	(26.2)	(30.8)
	[15.0%]	[10.0%]
Inspections	5.7	13.8
	(11.5)	(35.1)
	[24.1%]	[13.8%]

Represents suboptimal decision performance using *Suboptimal Type II Errors*. The mean (maximum) low value signals for higher-risk assets is 7.65 (8) and 1.00 (1) for lower-risk clients for the latter 10 periods. The standard deviations are included in parenthesis. The percent of individuals who had at least one *Suboptimal Type II Error* is included in brackets.

Panel B: Repeated Measures ANOVA on Suboptimal Type II Errors for Periods 11-20

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Inspections	1	0.000	0.000	0.001	0.972
Between-subjects error	47	5.029	0.107		
Within-subjects effects:					
Client risk	1	0.039	0.039	0.959	0.332
Inspections X Client risk	1	0.039	0.039	0.959	0.332
Within-subjects error	47	1.887	0.040		

decision performance. Accordingly, I perform several additional analyses on Substandard Effort to further examine whether H3 is robust to period effects.

First, I perform a repeated-measures ANOVA including *Period* and *Client Risk* as repeated measures on Average Substandard Effort.[23] The results are included in Table 9. While there are some effects of *Period* on Average Substandard Effort, the *Inspections X Client Risk* effect remains significant ($p = 0.026$) in support of H3. Second, I re-perform the analysis to test H3 with Substandard Effort, but I limit the analysis to later periods (i.e., 11-20 and 16-20) in order to mitigate any effects of participant learning over time.

Table 9: Supplemental Analyses: Effect of Period on Decision Performance – Secondary Dependent Measure

Repeated Measures for Period and Client Risk ANOVA on Average Substandard Effort

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Inspections	1	0.383	0.383	0.045	0.833
Between-subjects error	47	402.086	8.555		
Within-subjects effects:					
Period	19	6.257	0.329	1.415	0.111
Period X Inspection	19	10.502	0.553	2.376	0.001
Client Risk	1	74.131	74.131	21.533	<0.001
Inspections X Client Risk	1	18.131	18.131	5.267	0.026
Period X Client Risk	19	6.918	0.364	2.362	0.001
Period X Client Risk X Inspections	19	3.000	0.158	1.024	0.429

[23] I do not perform a similar analyses using Count of Substandard Effort because the measure includes counts *over the 20 periods.* Thus, this measure is not susceptible to controlling for period effects.

The results for Average Substandard Effort and Count of Substandard Effort are included in Tables 9 and 10, respectively. Using both measures of decision performance, the results are consistent with those reported in the main analyses for periods 1-20.

Table 10: Supplemental Analyses: Decision Performance for Later Periods – Secondary Dependent Measure

Panel A: Mean (standard deviation) Average Substandard Effort

	Periods 11-20		**Periods 16-20**	
	Higher-Risk	**Lower-Risk**	**Higher-Risk**	**Lower-Risk**
No Inspections	0.46	0.57	0.55	0.62
n=20	(0.72)	(0.72)	(0.82)	(0.77)
Inspections	0.13	0.64	0.16	0.59
n=29	(0.26)	(0.66)	(0.32)	(0.68)

Represents suboptimal decision making using *Average Substandard Effort.*

Specifically, as reported in the repeated-measures ANOVA in Panel B of Table 10, the Inspections X Client Risk effect is significant on Average Substandard Effort for periods 11-20 (p = 0.015) and for periods 16-20 (p = 0.039). Likewise, as reported in Table 11, Panel B, the Inspections X Client Risk effect is significant on Count of Substandard Effort for periods 11-20 (p = 0.042) and for periods 16-20 (p = 0.098). Further, as reported in Panel C in Tables 10 and 11, for periods 11-20 and 16-20 decision performance is only worse for lower-risk clients than for higher-risk clients under an inspections regime (p-values < 0.01) and not in a regime with no inspections using both measures of Substandard Effort (*p-values* > 0.4). All together, the findings support that the effects of risk-based inspections on decision performance (H3) are robust to period effects.

Table 10: Supplemental Analyses: Decision Performance for Later Periods – Secondary Dependent Measure (Continued)

Panel B: Repeated Measures ANOVA on Average Substandard Effort

	Periods 11-20				**Periods 16-20**			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Inspections	1	0.393	0.691	0.410	1	1.035	1.526	0.223
Between-subjects error	47	0.568			47	0.678		
Within-subjects effects:								
Client Risk	1	2.328	15.137	<0.001	1	1.506	8.661	0.005
Inspections X Client Risk	1	0.982	6.381	0.015	1	0.786	4.521	0.039
Within-subjects error	47	0.154			47	0.174		

Panel C: Simple Effects Tests (Univariate Analysis)

	Periods 11-20			**Periods 16-20**		
	df	T	*p*-value	df	T	*p*-value
Inspections: Higher- vs. lower-risk	56	3.929	<0.001*	56	3.096	0.001*
No Inspections: Higher- vs. lower-risk	38	0.481	0.633	38	0.278	0.782

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

Table 11: Supplemental Analyses: Decision Performance for Later Periods – Secondary Dependent Measure

Panel A: Mean (standard deviation) Count of Substandard Effort

	Periods 11-20		**Periods 16-20**	
	Higher-Risk	**Lower-Risk**	**Higher-Risk**	**Lower-Risk**
No Inspections	2.55	3.60	1.50	1.90
n=20	(3.90)	(4.16)	(2.16)	(2.27)
Inspections	1.03	4.38	0.62	2.00
n=29	(2.26)	(4.01)	(1.29)	(2.10)

Panel B: Repeated Measures ANOVA on Count of Substandard Effort

	Periods 11-20				**Periods 16-20**			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	*F*	*p*-value
Inspections	1	3.208	0.172	0.680	1	3.594	0.640	0.428
Between-subjects error	47	18.613			47	5.613		
Within-subjects effects:								
Client Risk	1	114.310	15.954	<0.001	1	18.737	9.387	0.004
Inspections X Client Risk	1	31.168	4.350	0.042	1	5.676	2.844	0.098
Within-subjects error	47	7.165			47	1.966		

Panel C: Simple Effects Tests (Univariate Analysis)

	Periods 11-20			**Periods 16-20**		
	df	T	*p*-value	df	T	*p*-value
Inspections: Higher- vs. lower-risk	56	3.911	<0.001*	56	3.007	0.002*
No Inspections: Higher- vs. lower-risk	38	0.824	0.415	38	0.571	0.572

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

7.2 Review Penalties and Auditor Behavior

Recall from the theoretical development that anticipation of inspections is thought to influence auditor behavior in part due to the desire to avoid negative consequences associated with incurring deficiencies. The potential negative consequences include (1) economic costs of incurring auditing deficiencies (referred to as "economic costs"), (2) psychological costs associated with receiving negative feedback from inspections (e.g., feeling bad about performing insufficient work and/or receiving penalties) (referred to as "psychological costs"), and (3) reputational costs associated with incurring auditing deficiencies (e.g., feeling bad about others learning of your deficiencies and penalties) (referred to as "reputational costs"). In order to shed some light as to which of these expected costs may influence auditor behavior more or less under an inspections regime, I asked several questions during the PEQ.

Summary data of the review penalties along with the PEQ questions are reported in Table 12 for the *Inspections* condition. The maximum number of review penalties that could have been incurred over the 20 periods is 10. As shown in Panel A, more than 50 percent of the participants incurred all 10 review penalties. Only two participants did not incur any review penalties. Self-reported measures for the extent that participants were concerned about review penalties are included in Panel B. On average, when making effort-level decisions, participants report being more concerned about economic costs associated with incurring review penalties as compared to reputational costs ($t_{28} = 1.955$, $p = 0.061$). Participants also report feeling worse about incurring penalties (i.e., psychological costs) as compared to about having others learn about the review penalties they incurred (i.e., reputational costs) ($t_{28} = 3.239$, $p = 0.003$).

Table 12: Supplemental Analyses: Summary of Review Penalties

Panel A: Frequency of Review Penalties for Higher-Risk Clients

Frequency of review penalties	Number of Participants	Percent	Cumulative Percent
0	2	6.9	6.9
1	1	3.4	10.3
2	1	3.4	13.8
6	2	6.9	20.7
7	3	10.3	31.0
8	1	3.4	34.5
9	2	6.9	41.4
10	17	58.6	100.0
	29	100.0	

Panel B: Self-reported Effects of Review Penalties

On a scale from "a small extent" (1) to "a great extent" (11):	Mean	Std. Dev.
Economic costs: To what extent did "wanting to avoid the costs of the review penalty" influence your effort-level decisions?	4.62	2.74
Reputational costs: To what extent did "you didn't want to be the person with the highest amount of review penalties" influence your effort-level decisions?	3.52	3.07
Psychological costs: To what extent did you feel bad for incurring the review penalties?	4.48	3.32
Reputational costs: To what extent were you worried about others learning about the review penalties you incurred?	2.72	2.51

Paired t-tests	t-stat	*p*-value
During effort-level decisions:		
Economic costs v. reputational costs	1.955	0.061
After incurring review penalties:		
Psychological costs v. reputational costs	3.239	0.003

Table 12: Supplemental Analyses: Summary of Review Penalties (Continued)

Panel C: Pearson Correlations (Two-tailed p-value), N=29

	Effort for Asset B (higher-risk clients)	Amount of review penalties incurred	Concern for economic costs during effort decisions	Concern for reputational costs during effort decisions	Concern for psychological costs after penalties	Concern for reputational costs after penalties
Effort for Asset B (higher-risk clients)	1					
Amount of review penalties incurred	-0.846 (<0.001)	1				
Concern for economic costs during effort decisions	0.706 (<0.001)	-0.556 (0.002)	1			
Concern for reputational costs during effort decisions	0.210 (0.274)	-0.191 (0.320)	0.457 (0.013)	1		
Concern for psychological costs after penalties	-0.311 (0.101)	0.374 (0.046)	0.021 (0.915)	0.178 (0.356)	1	
Concern for reputational costs after penalties	-0.137 0.478	0.203 (0.292)	0.202 (0.292)	0.345 (0.067)	0.527 0.003	1

To shed more light as to how participants' concerns about review penalties influenced their effort-level decisions, I examine the correlations of effort-level decisions for the higher-risk asset, the review penalty results (i.e., cumulative amount of review penalties), and participants' self-reported concerns about review penalties. The correlation matrix is included in Panel C of Table 12. Interestingly, effort for higher-risk clients is only significant and positively correlated with participants' concern for economic costs of incurring review penalties ($p < 0.001$). Likewise, participants' reported concern about economic costs is negatively correlated with the cumulative review penalties incurred ($p = 0.002$). Unexplainably, participants' reported concern about psychological costs of incurring review penalties is negatively (positively) correlated with effort for higher-risk clients (cumulative review penalties incurred). Perhaps participants are unaware of their feelings about incurring review penalties and/or do not truthfully report their feelings (i.e., feeling bad about incurring penalties).

Future research can further investigate the underlying reasons as to *why* inspections influence auditor behavior, albeit economic, psychological or reputational effects of anticipating consequences from incurring auditing deficiencies.

7.3 Report Outcomes

I do not report whether inspections influence the audit report outcomes (e.g., Type I or Type II errors) because of the stylized setting of the experiment. The report outcomes are essentially driven by participants' effort-level decisions because effort-level decisions yield more or less accurate signals which likely result in more or less accurate reporting decisions. When analyzing reporting outcomes, as expected, the effect of inspections is not significant for Type II errors (i.e., a *high* report for a *low* value asset) for either higher-risk or lower-risk clients, with or without controlling for effort-level choices (*p-values* > 0.10, untabulated). The relatively small number of *low* value assets, especially for lower-risk clients, limits the power of detecting a significant variation in the quality of reporting across *Inspection* conditions. That said, the findings reported in support of H3 related to the significant effect of inspections on suboptimal Type II errors (i.e., a *high* report after a *low* value signal) are all the more astounding.

7.4 Experiment 2 with Lower Resource Pressure

In the hypotheses development, I theorize that it is the *combined* pressures of risk-based inspections and tight resource constraints that negatively influence auditor behavior for lower-risk clients. In short, in Experiment 1 I predict auditors want to increase effort in response to accountability pressure from inspections, but when they are under high resource pressure they may be unable to sufficiently increase effort to the desired levels. As a result, I predict auditors will increase effort more for higher-risk as compared to lower-risk clients with risk-based inspections (H2). I also predict that not being able to increase effort to the desired levels under an inspections regime will increase auditor's anxiety, which will negatively influence decision performance for lower-risk (less salient) clients. To further test this theory, I conduct a second experiment and reduce the level of resource pressure. I conduct the same experiment, but with one exception: I increase the total amount of resources available to a level slightly greater than the standard, wealth-maximizing level (i.e., a maximum equal to 7 instead of 6) thereby reducing the resource pressure.

7.4.1 Theory

When auditors are not under high resource pressure, I no longer expect auditor effort to increase more for higher-risk clients than for lower-risk clients with risk-based inspections. Prior research in psychology and auditing support that when individuals are held accountable to a party with known preferences, they tend to conform to those preferences, even it if produces inefficient outcomes (e.g., Lord 1992; Peecher 1996; Hoffman and Patton 1997; Wilks 2002). Auditors are aware that the PCAOB prefers more effort to less and that they expect high quality auditing for all clients. Therefore, I expect auditors will be willing to increase effort for higher-risk and lower-risk clients in accordance with the perceived preferences of the PCAOB, even if additional auditor effort is costly and results in suboptimal levels of effort (i.e., inefficiencies). Therefore, in Experiment 2 with lower resource pressure, I expect a main effect of inspections on auditor effort (prior H1), but I no longer expect a significant *Inspection X Client Risk* effect (prior H2).

Further, when auditors are not under high resource pressure, I no longer expect decision performance to be worse for lower-risk clients than for higher-risk clients under a regime with inspections. By reducing the resource pressure, I expect auditors will no longer experience task-related anxiety when they are under an inspections regime because they are no longer constrained from adequately responding to the accountability pressure from inspections. Absent anxiety, auditors are no longer expected to engage in stimulus-driven processing (i.e., directing more attention to higher-risk clients away from lower-risk clients) or suboptimal decision-making. Accordingly, in Experiment 2 with lower resource pressure, I no longer expect an *Inspection X Client Risk* effect on decision performance (i.e., decision performance should not be worse for lower-risk clients under an inspections regime) (prior H3).

7.4.2 Method

Fifty-five undergraduate students participated in Experiment 2, with 47 percent being female.[24] The average age was 20 years.[25] In Experiment 2, I employ the same 2 X 2 mixed-design. I manipulate the inspections regime (present or absent) between-participants, and client risk (lower- and higher-risk) within-participants. The independent variables and dependent variables remain the same as Experiment 1. The experimental procedures and experimental setting also are the same, except for one change: I increase the total amount of resources available to the participants to be allocated for their effort-level choices. Verifiers are still informed that the standard level of effort per asset is 3, but that they can allocate any amount greater than or equal to 1 for each asset, subject to the constraint that the total level of effort for both assets does not exceed 7 (i.e., as compared to 6 in Experiment 1).[26] All additional units of effort above 3 still cost 50 EE

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[24] Participants were not allowed to participate in more than one experiment or in more than one session. Therefore, Experiment 1 and 2 are independent.

[25] The average age and years of school significantly differed across *Inspection* conditions ($p < 0.05$). However, when I include these variables as covariates in the analyses for hypotheses testing, the covariates are not significant ($p > 0.10$) and they do not interact with any of the variables of interest ($p > 0.10$). Further, the inferences related to the hypotheses are unchanged when including these covariates. Therefore, I do not include the covariates in the analyses presented.

[26] I set the constraint of available audit resources to 7 based on results from a previous pilot study. In the pilot study, there were no restrictions on the total audit resources available. The standard was still set at the wealth-maximizing level of 3 but participants were allowed to allocate as much effort as they preferred, at increasing costs of effort. The average effort allocated across both clients was 7.11 and 8.26 in the *No Inspections* and *Inspections* conditions, respectively. Accordingly, I set the maximum amount of effort to 7 in Experiment 2 because it is approximately equal to the desired average choice with no constraints for *No Inspections* condition. Therefore, like the real world, auditors still have resource constraints but the pressure from resource constraints is lower than it is for Experiment 1. Further, the results of the pilot test are similar to that reported in H1 and H2 whereby auditor effort is higher with inspections, but does not increase more for higher-risk clients than for lower-risk clients.

and have decreasing marginal returns. Further, related to review penalties, as in Experiment 1, only the maximum level of effort (i.e., 6 in Experiment 2) avoids a review penalty. Importantly, the wealth-maximizing decisions do not change in Experiment 2. That is, the wealth-maximizing strategy is to choose an effort-level of 3 for each asset and report according to the signal. Finally, to ensure consistency and comparability across Experiments, I use the same predetermined states and outcomes from Experiment 1 for every period 1-20 (e.g., the same high and low value assets in the given period, the same signals for each effort-level choice in a given period, the same detection of incorrect reports in a given period, the same selection of reviews in each period, etc.).

7.4.3 Results

<u>Risk-based Inspections on Auditor Effort</u>

The results of inspections on auditor effort for higher- and lower-risk clients when auditors have relatively lower resource pressure are included in Table 13. As expected, the main effect of *Inspections* is significant ($p = 0.018$) indicating that auditor effort is higher under an inspections regime (mean = 6.24) than under a regime without inspections (mean = 5.58) (consistent with H1). Further, the simple effects in Panel C indicate marginally significant main effects of inspections on auditor effort for both higher-risk clients ($p = 0.083$) and lower-risk clients ($p = 0.057$), consistent with inspections leading to more uniform increases in effort. Likewise and as expected, the interaction of *Inspections X Client Risk* is not significant ($p = 0.667$) indicating that auditor effort does not increase more for higher-risk than for lower-risk clients with risk-based inspections when auditors are not under high resource pressure. The results are robust to controlling for individual risk preferences.

Table 13: Supplemental Analyses: Results of Risk-based Inspections on Auditor Effort for Experiment 2 (Lower Resource Pressure)

Panel A: Mean (standard deviation) Auditor Effort

	Higher-Risk	Lower-Risk	Total
No Inspections	3.09 (0.94) [n=28]	2.49 (0.65) [n=28]	5.58 (1.33) [n=28]
Inspections	3.49 (0.91) [n=27]	2.76 (0.75) [n=27]	6.24 (0.92) [n=27]
Combined	3.28 (0.94) [n=55]	2.62 (0.70) [n=55]	5.91 (1.19) [n=55]

Panel B: Repeated Measures ANOVA on Auditor Effort

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Inspections	1	3.047	3.047	4.605	0.018*
Between-subjects error	53	35.074	0.662		
Within-subjects effects:					
Client Risk	1	12.020	12.020	17.519	<0.001
Inspections X Client Risk	1	0.129	0.129	0.187	0.667
Within-subjects error	53	36.365	0.686		

Panel C: Simple Effects Tests (Univariate Analysis)

	df	*T*	*p*-value
Higher-risk clients: Effect of Inspections	53	1.406	0.083*
Lower-risk clients: Effect of Inspections	53	1.604	0.057*

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

Interestingly, when auditors are under relatively lower resource pressure, inspections appear to be especially beneficial to lower-risk clients. That is, in the *No Inspections* condition, auditor effort for lower-risk clients is significantly lower than the standard, wealth-maximizing level (mean = 2.49; $p < 0.001$, untabulated) (i.e., under-auditing), but increases in the *Inspections* condition to a level that is not significantly different from the standard level (mean = 2.76; $p = 0.104$, untabulated). This differs from Experiment 1 whereby average effort for lower-risk clients was significantly lower than the standard level with and without inspections. For higher-risk clients, in the *No Inspections* condition, auditor effort for higher-risk clients is not different from the standard level (mean = 3.09; $p = 0.634$), and increases in the *Inspections* condition to a level significantly higher than the standard level (mean = 3.49; $p = 0.010$, untabulated) (i.e., to an inefficient level or "over-auditing").

Risk-based Inspections on Auditor Decision Performance

The results of inspections on auditor decision performance for higher- and lower-risk clients when auditors have relatively lower resource pressure are included in Table 14 for the primary dependent measure, Suboptimal Type II Errors, and in Table 15 for the secondary dependent measure, Substandard Effort. Unexpectedly, the *Inspections X Client Risk* effect on Suboptimal Type II Errors is significant ($p = 0.030$; Panel B, Table 14); however, consistent with predictions the interaction is no longer significant when controlling for risk preferences ($p = 0.158$, untabulated). Further, as reported in Panel C of Table 14, under an inspections regime, Suboptimal Type II Errors do not differ for lower- and higher-risk clients ($p = 0.183$) consistent with the theory for Experiment 2

Table 14: Supplemental Analysis: Results of Risk-based Inspections on Decision Performance -Primary Dependent Measure for Experiment 2 (Lower Resource Pressure)

Panel A: Suboptimal Type II Errors

	Higher-Risk	Lower-Risk
No Inspections	11.3	6.5
(n=28)	(21.5)	(16.6)
	[39.3%]	[14.3%]
Inspections	8.0	17.3
(n=27)	(15.4)	(32.2)
	[33.3%]	[25.9%]

Represents suboptimal decision performance as using *Suboptimal Type II Errors*. The standard deviations are included in parenthesis. The percent of individuals who had at least one Suboptimal Type II error are included in brackets.

Panel B: Repeated Measures ANOVA on Suboptimal Type II Errors

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Inspections	1	0.038	0.038	0.524	0.472
Between-subjects error	53	3.862	0.073		
Within-subjects effects:					
Client risk	1	0.014	0.014	0.511	0.478
Inspections X Client risk	1	0.135	0.135	4.963	0.030
Within-subjects error	53	1.443	0.027		

Panel C: Simple Effects Tests (Univariate Analysis)

	df	*T*	*p*-value
Inspections: Higher-risk vs. lower-risk clients	52	1.348	0.183
No Inspections: Higher-risk vs. lower-risk clients	54	-0.927	0.358

Table 15: Supplemental Analysis: Results of Risk-based Inspections on Decision Performance -Secondary Dependent Measure for Experiment 2 (Lower Resource Pressure)

Panel A: Mean (standard deviation) Substandard Effort

	Average Substandard Effort		**Counts of Substandard Effort**	
	Higher-Risk	**Lower-Risk**	**Higher-Risk**	**Lower-Risk**
No Inspections	0.355	0.539	3.82	7.00
n=28	(0.650)	(0.637)	(6.57)	(7.61)
Inspections	0.141	0.391	2.07	5.79
n=27	(0.301)	(0.577)	(4.28)	(7.39)

Represents suboptimal decision performance using *Substandard Effort.*

Panel B: Repeated Measures ANOVA on Substandard Effort

	Average Substandard Effort				**Counts of Substandard Effort**			
Between-subjects effects:	df	MS	*F*	*p*-value	df	MS	F	*p*-value
Inspections	1	0.906	1.961	0.167	1	60.606	1.078	0.304
Between-subjects error	53	0.462			53	56.241		
Within-subjects effects:								
Client Risk	1	1.294	7.700	0.008	1	325.532	10.483	0.002
Inspections X Client Risk	1	0.030	0.179	0.674	1	1.895	0.061	0.806
Within-subjects error	53	0.168			53	31.054		

under lower resource pressure. The results are robust to controlling for auditors' individual effort-level choices (untabulated).

The results on decision performance using Substandard Effort further support the theory in Experiment 2. As shown in Panel B of Table 15, the *Inspections X Client Risk* effect on decision performance is not significant using Average Substandard Effort (p= 0.674) or Count of Substandard Effort (p = 0.806). The results are robust to controlling for individual risk preferences.

All together, the results of Experiment 2 support the theory that decision performance is no longer worse for lower-risk clients than for than higher-risk clients under an inspections regime when auditors are under relatively lower resource pressure. Thus, I conclude that it is the *combined pressures* of inspections and tight resource constraints that negatively influence auditor behavior for lower-risk clients in Experiment 1.

7.5 Experiment 3 to Measure Anxiety

7.5.1 Theory and Design

In the theory development of H3, I posit that *anxiety*, experienced in conditions of inspections plus high resource pressure, negatively influences decision performance for lower-risk clients. To better test this theory, I perform a supplemental experiment (Experiment 3) to measure participants' anxiety during the experimental task, specifically when they are making their effort-level decisions. I perform an abbreviated 2 X 2 experiment, similar to the main study. As in Experiment 1, I manipulate inspections (present or absent) across conditions, manipulate client-risk within participants, and hold resource pressure at a constant high level. Because the purpose of this supplemental experiment is to measure participants' anxiety while making effort-level decisions, several changes are made to the design of the experiment.

First, the experimental task is shortened to only one period. Participants are provided with the same full set of experimental instructions and materials in Experiment 1 (the main study), but they are told they will only be completing the first period. I only ask participants to complete one period because I elicit their level of anxiety immediately following their effort-level decisions during the first period; therefore, completing the remaining periods are unnecessary. Second, while participants are aware of the economic incentives from reading the full set of instructions, they are informed that they will receive a flat fee of $10 as compensation for participating in the study. Essentially, I ask participants to pretend they are completing the full experiment, and I ask them to put themselves in the mindset of someone who would be completing the full experiment. Third, for expositional ease, the experimental task is conducted via paper and pencil versus being computerized. Fourth, the review penalty results are not publicly announced after the experiment.

Finally and most importantly, I elicit questions to measure anxiety (explained below) immediately following their effort-level decisions, before they receive the signal, make reporting decisions, and learn the outcomes of their decisions. To ensure participants understand the full set of instructions, I read aloud a summary of key points and participants completed the same pretest questionnaire as in Experiment 1. I expect that each of these changes in the design weakens the strength of the manipulation effect of inspections on anxiety and auditor behavior as compared to Experiment 1. Nonetheless, I anticipate that inspection will influence the level of self-reported anxiety, and that anxiety will be higher in the *Inspections* condition than in the *No Inspections* condition.

To measure anxiety, participants answer a series of questions immediately after making their effort-level decisions for the higher-risk and lower-risk assets. First, I follow Stone and Kadous (1997) and ask participants to answer a series of two questions designed to measure their affective state. The first question assesses participants' negative affect experienced during the experimental task. I ask how nervous participants were while making their effort choices on a scale from "relaxed" (1) to "nervous" (5). The second question assesses participants' positive affect by asking how excited they were while making their effort-level choices on a scale from "drowsy" (1) to excited" (5). The second question is asked to ensure that the manipulation of inspections did not unintentionally influence positive affect; therefore, I control for this measure in the analysis. Asking only two questions mitigates any possible treatment effects of using a lengthy affect instrument, and it also mitigates the concern that a multiple-item scale may decrease the intensity of the participants' emotion states (Stone and Kadous 1997). Accordingly, I use the first question as my primary question of negative affect or anxiety.

However, as a secondary measure of anxiety, I also ask participants to answer the short form of the Speilberger State-Trait Anxiety Inventory (STAI) test (which is 40 items long). The short form is developed by Marteau and Bekker (1992) and includes six measures for anxiety including calm (reverse coded), upset, tense, relaxed (reverse-coded), content (reverse-coded) and worried. The composite score is used to measure *anxiety*, and has been found to yield consistent results with the STAI instrument (Marteau and Bekker 1992).

Table 16: Supplemental Analysis: Test of Anxiety for Experiment 3

Panel A: Means (standard deviations)

Variable Name	No Inspections (n=19)	Inspections (n=20)	t-stat	p-value
Nervous	2.63 (1.12)	3.05 (1.05)	-1.206	0.118*
Excited	3.37 (0.90)	3.00 (0.86)	1.312	0.198
Anxiety Scale[a]				
Calm (reversed score)	1.95 (0.78)	2.20 (0.70)		
Tense	1.90 (0.94)	1.95 (1.05)		
Upset	1.21 (0.54)	1.25 (0.55)		
Relaxed (reversed score)	2.26 (0.73)	2.25 (0.72)		
Content (reversed score)	2.26 (0.87)	2.20 (0.77)		
Worried	1.95 (0.97)	2.10 (0.91)		
Sum for anxiety	11.53 (3.88)	11.95 (3.43)	-0.362	0.360
Effort higher-risk assets	3.16 (0.77)	3.20 (0.89)	-0.158	0.876
Effort lower-risk assets	2.63 (0.76)	2.55 (1.05)	0.182	0.856
Importance of higher-risk asset (percent)	57.26 (11.41)	59.25 (15.50)	-0.427	0.336
Desire more effort	5.63 (2.89)	6.50 (3.12)	-0.900	0.187

Panel B: ANCOVA on Nervous

Between-subjects effects:	df	SS	MS	*F*	*p*-value
Gender (covariate)	1	5.651	5.651	5.464	0.025
Positive affect (covariate)	1	1.772	1.772	1.713	0.199
Inspections	1	2.992	2.992	2.893	0.049*
Between-subjects error	35	36.197	1.034		

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

a. As a secondary measure of anxiety, I use the 6-item, short-form of the Spielberger State-Trait Anxiety Inventory (STAI) scale develop by Marteau and Bekker (1992). Participants were asked to indicate how they felt while making effort choices from on a scale from (1) "Not at all" to (4) "Very much."

Nervous = "While making my effort choices, I felt: *Relaxed* 1----2----3----4----5 *Nervous*"

Excited = "While making my effort choices, I felt: *Drowsy* 1----2----3----4----5 *Excited*"

Importance of higher risk asset = "how important did you feel your effort decisions were related to the assets? Please rate the relative importance" for Type A and Type B assets.

Desire more effort = "to what extent would you have liked to have been able to choose a total effort level greater than 6?"

7.5.2 Results

Thirty-nine undergraduates participated in Experiment 3. Participants from Experiment 1 and Experiment 2 were not allowed to participate in Experiment 3 ensuring independent observations. The results are reported in Table 16. As reported in Panel A, the mean self-reported measure of anxiety from the negative affect scale (extent of “nervous”) is 3.05 in the *Inspection* condition compared to 2.63 in the *No Inspection* condition; but, the difference is not statistically significant ($t = -1.206$, $p = 0.118$ one-tailed test). Panel B reports the results of an ANCOVA on negative affect including gender and positive affect as covariates. Gender is included because it has a significant effect in explaining negative affect ($p = 0.025$); none of the other demographic variables are significant in the model (*p-values* > 0.10). The main effect of inspections on negative affect in the ANCOVA is significant ($p = 0.049$ one-tailed) indicating that participants experience higher levels of negative affect in the *Inspection* condition than in the *No Inspection* condition (controlling for gender and positive affect).

The secondary measure of self-reported anxiety using the short-form anxiety scale does not produce significant differences across *Inspection* conditions ($t = -0.362$; $p = 0.360$). The lack of significance could be attributable to there being no difference in the population, due to a lack of power to detect the effect, and/or due to treatment effects or decreased sensitivity in the effect due to the passage of time (i.e., concerns expressed by Stone and Kadous 1997).

Nonetheless, the findings in Experiment 3 provide some evidence that inspections lead to higher levels of anxiety when individuals are also under high resource pressure. Future research can further examine the underlying effect of anxiety when auditors are anticipating inspections and when they are under high resource pressure. For example, future research could explore more accurate measures of anxiety such as the use of functional magnetic resonance imaging (fMRI) technology. Alternatively, future research could consider using qualitative or survey methods to gather evidence about auditors' anxiety levels directly from accounting firms.

7.6 Desire for More Effort from Experiment 1 and Experiment 2

In H3 I predict task-related anxiety occurs under a regime with inspections when auditors are under high resource pressure because auditors want to increase auditor effort in response to accountability pressure from inspections, but they are unable to sufficiently do so because of resource constraints. To provide some evidence on this theory, I perform supplemental analysis examining the extent to which participants desired more effort (i.e., above the constraint). In the post-experimental questionnaires for Experiment 1 (high resource pressure) and Experiment 2 (lower resource pressure), I asked participants to what extent they would have liked to choose more effort on a scale from "a small extent" (1) to "a great extent" (11). Following the theory, I expect the desire for more effort to be the strongest in the *Inspection* condition for Experiment 1 when participants are under high resource pressure.

The results for this question are reported in Table 17. On average, only participants in the *Inspection* condition under high resource pressure (Experiment 1) indicate that they would have liked more effort (mean = 6.90, which is greater than the midpoint of 6.0 with marginal significant, *p-value* = 0.096, one-tailed). By comparison, the means of the *Inspection* condition under relatively lower resource pressure

Table 17: Supplemental Analysis: Desire More Effort from Post-Experimental Questionnaires in Experiment 1 and Experiment 2

Panel A: Means (standard deviations) [p-value of t-test from midpoint]

	Experiment 1 (High Resource Pressure)	**Experiment 2 (Lower Resource Pressure)**
No Inspections	5.90	4.04
	(3.46)	(3.00)
	[0.899]	[0.002]
	n = 20	n = 28
Inspections	6.90	5.26
	(3.60)	(3.75)
	[0.096*]	[0.314]
	n = 29	n = 27
Total	6.49	4.64
	(3.54)	(3.41)
	[0.338]	[0.005]
	n = 49	n = 55

Represents the average response to the following question in the PEQ, "to what extent would you have liked to choose more effort" on an 11-point scale with endpoints 1 "a small extent" to 11 "a great extent". The standard deviations are in parentheses. T-tests were conducted to compare the mean response to the scale midpoint of 6.

Panel B: Simple Effects Tests

	df	*T*	*p*-value
Inspections / Tight Resources condition vs. the other three conditions	102	-2.520	0.007*

* One-tailed *p*-value for directional tests. All other reported *p*-values are two-tailed.

(Experiment 2) and in the *No Inspection* conditions for Experiment 1 and 2 are less than 6.0. To test whether the desire for more effort is strongest in the *Inspections* condition for Experiment 1 with high resource pressure, I perform a simple effects t-test comparing this condition against the other three conditions (refer to Panel B). The mean rating is significantly higher than that of the other three conditions (t_{102} = -2.520, p = 0.007, one-tailed). The finding is consistent with the theory that auditors who are anticipating inspections and who are under high resource pressure experience anxiety because they want to increase effort in response to inspections pressure but are unable to because of resource constraints.

CHAPTER 8

IMPLICATIONS AND CONCLUSIONS

Congress passed SOX and established the PCAOB to regulate the auditing profession with the aim of improving audit quality. After more than a decade of regulatory inspections on audit firms under the new regime, relatively little is known about the effectiveness of the PCAOB inspection process. Researchers are just beginning to provide evidence on some potential benefits of inspections on audit quality. However, there is much research to be conducted. Academics and regulators continue to call for research as to how the PCAOB inspections influence auditor behavior and audit quality. Further, academics have started to caution about potential unintended consequences of PCAOB inspections on auditor behavior. For example, academics have begun discussing issues related to auditors managing “inspection risk” (e.g., Glover and Prawitt 2013; Glover et al. 2014). The concern is that auditors may alter their planned audit procedures and attention toward managing “inspection risk” (e.g., anticipate regulators’ moves and work to appease inspectors) thereby distracting auditors from performing audit procedures to manage “audit risk.”

In this study, I provide theory and evidence as to how risk-based inspections potentially influence auditor behavior, considering both potential benefits and unintended consequences. The findings of this study contribute to the auditing literature, and they also have important practical and regulatory implications.

First, I find that auditor effort is higher under a regime with inspections than under a regime without inspections. Thus, I join other concurrent studies that identify

PCAOB inspections as a source of accountability pressure that leads to increases in auditor effort (Stefaniak and Houston 2013; Winn 2014). However, by examining the effect of risk-based inspections in a multi-client setting, I extend this line of research by providing evidence that the benefit of inspections (i.e., increases in auditor effort) is limited to higher-risk clients or those clients auditors anticipate will be selected for inspection, and does not extend to lower-risk clients. Therefore, I identify a boundary condition to concurrent research in this area.

Second, and most notably, I develop theoretical predictions and provide supporting evidence that risk-based inspections can lead to diminished auditor decision performance for lower-risk (untargeted) clients. Specifically, I find that when auditors are under a risk-based regime with high resource pressure, their decision performance for lower-risk clients suffers. Ceteris paribus, decision performance is worse for lower-risk clients than higher-risk client under these conditions. That is, making identical decisions and facing the same expected outcome, auditors' decisions are more suboptimal for lower-risk clients than higher-risk clients, but only in a regime with inspections and not without inspections. Further, for lower-risk clients, decision performance is worse with risk-based inspections than without risk-based inspections. The finding suggests that risk-based inspections can lead to *lower audit effectiveness* for some clients (lower-risk clients) under certain conditions (when auditors are also under high resource pressure).

Third, in a supplemental experiment I provide evidence to support the theory that it is the *combined* effects of pressure from inspections plus pressure from high resource constraints that lead to the potential unintended consequences of risk-based inspections on auditors' decision performance for lower-risk clients. In doing so, I add to the auditing literature and answer calls for research as to how accountability pressures interact with other environmental pressures to influence auditor behavior (e.g., DeZoort and Lord 1997; Asare et al. 2000). To date, research on the combined effects of accountability pressure and other time-related pressures is sparse. I extend this line of research by examining the joint effect in a multi-task setting noting that the combined effects have a greater impact on the tasks that individuals are *not* anticipating being held accountable for (which has not been previously examined).

Finally, in a supplemental experiment, I provide some evidence on the underlying theory that the combined pressures of accountability and high resource constraints cause higher levels of *anxiety* thereby leading to impaired decision processing. Future research can examine this underlying mechanism more carefully. The findings are important in order to better understand the underlying mechanisms in order to work to mitigate these potential unintended consequences of risk-based inspections on auditors' decision performance.

This study is subject to a number of limitations that provide opportunities for future research. Consistent with traditional experimental-economics studies in auditing (Kachelmeier and King 2002), the experimental setting attempts to capture the essence of the audit ecology, but uses a stark setting. As such, I abstract away from many environmental and institutional factors present in the real-world that may affect auditors' decisions. For example, in the real-world, auditors have strategic interactions with managers and potentially with PCAOB inspectors which my study does not allow for. Further, I make a number of simplifying assumptions in the experimental setting (e.g., the cost of Type II errors are the same for lower- and higher-risk clients). Future research can continue to examine how risk-based inspections influence auditor behavior under different conditions and/or under different assumptions. However, I do not expect that changes to these factors would systematically affect the directional predictions or results observed in this study.

APPENDIX A

EXPERIMENTAL INSTRUCTIONS

[Note: The information related to the *Inspections* condition is in italics.]

Experimental Instructions

General

Thank you for participating in this experiment.

This is an experiment on decision making. You will receive $8 for completing the experiment. You will also earn money during the experiment based on your decisions, which will be paid to you privately in cash at the end of the experiment.

Be sure to ask any questions that you may have during the instructions period or ask for assistance, if needed, at any time during the experiment.

Please do not confer or talk with other participants at any time during the experiment. Thank you.

INSTRUCTIONS

Role

Everyone in this room has been assigned the role of a verifier. A verifier is someone who verifies the value of a seller's assets. Potential buyers rely on a verifier's report about the true value of the asset. *A review board randomly selects assets to be examined to ensure the verifier exerted sufficient levels of effort in verifying the assets.*

Payoffs

As discussed subsequently, your earnings for participating in this experiment are determined by your decisions. Each period you will accumulate experimental earnings (EE). At the end of the experiment your accumulated EE will be converted to dollars and paid to you in cash at a rate of *500* (600) EE equals one dollar.

Summary of Each Period

This experiment consists of a series of periods. An overview of each period is presented below. Additional details for each step will be discussed later.

You will be asked to verify two assets (for two different sellers and different assets each period). You will earn revenues for verifying the assets.

1. Each asset has either a high value or a low value. Sellers always claim that assets have a high value.
2. For each asset, you choose an effort level to investigate the asset's true value. Based on the effort level you choose, you will receive a signal about the true value of the asset (high or low).
3. You report whether you agree with the seller's claim that the asset has a high value. Then, the true value of each asset will be revealed.
4. If you report the asset has a high value, but the true value is low, you will be charged a cost for issuing an incorrect report.
5. *A computerized review board randomly selects assets to be reviewed. If your asset is reviewed, you may be penalized based on the effort-level you chose for the asset.*
6. After the period is over, you will be informed of your results and your accumulated earnings. Then you will proceed to the next period.
7. *At the conclusion of the experiment, the review board (the experimenter) will announce the review results to the group in this room. Each person's review penalties will be announced to the group, one by one in order of severity (highest to lowest amount).*

The following are relevant in making decisions throughout the experiment.

Revenues

At the start of each period, you will receive 1,000 EE to verify each of two assets.

Asset Values

All assets have either a high value or a low value. There are two types of assets, Type A and Type B. For asset Type A, there is an 80 percent chance that the asset has a high value and a 20 percent chance that the asset has a low value. For asset Type B, there is a 60 percent chance that the asset has a high value and a 40 percent chance that the asset has a low value. The values of each asset in each period are predetermined, before the experiment begins. The values are randomly generated from a normal distribution using a computer program.

Effort Levels, Cost of Effort, and Signals of Asset Values

Each period, you will choose a level of effort to verify each asset. The standard effort level for each asset is 3, but you can choose any level of effort you would like from 1 to 5 for a given asset.

The minimum amount of effort you can choose for any asset is 1 and the maximum total (combined) effort you can choose for both assets is 6. Notice that for each asset, each effort point not used below the standard effort level of 3 is worth 25 EE and each effort point above the standard effort level of 3 costs an additional 50 EE.

Based on the level of effort you choose, you will receive a signal about the true value of the asset (high or low). If the true value is high, the signal will always be high. However, if the true value is low, the signal is not 100 percent accurate. Higher levels of effort give you a signal with higher levels of accuracy when the true value is low. The following table describes the total cost of effort and the chance of an incorrect signal (i.e., the error rate) for each effort level for Type A and Type B assets.

Effort-Level Choice	Total Cost of Effort	Signal Error Rate – True Asset Value is "Low" but Signal says "High"	
		Type A Assets	Type B Assets
1	350	20%	40%
2	375	16%	28%
3	400	8%	16%
4	450	7%	14%
5	500	6%	12%

In summary, higher levels of effort reduce the error rate of the signal when the true value of the asset is low, but higher levels of effort cost more.

More about the Signal when the True Value of the Asset is Low

When the true value of the asset is low, there is a chance that the signal will be incorrect. The error rate is based on your effort-level choice. The experimenter will use a computer program to generate the signal based on your effort-level choice. To determine the signal, a number from 1 to 100 is randomly drawn. If the random number drawn is equal to or less than the error rate (e.g., 20 for an effort-level of 1 for a Type A asset), the signal will be incorrect (i.e., the signal will say "High" but the asset value is "Low"). If the random number drawn is greater than the error rate, you will get an accurate signal about the low asset value.

Your Report on the Asset Value

The sellers of the assets always report that the assets have high values. After you receive the signal, you will be asked to report whether you agree or disagree that the value of the asset is high. If you disagree with the seller and report that the asset has a low value, you will be charged an additional cost of 500 EE for disagreeing. There is no additional cost for agreeing with the seller and reporting that the asset has a high value.

After you report on the asset values, the true values are revealed. For any asset, if and only if you agree with the seller (i.e., you report "High") but the asset value is "Low", there is a 50 percent chance that you will be charged a cost of 6,000 EE for an incorrect report and there is a 50 percent chance that you will be charged a cost of 0 EE for an incorrect report.

Reviews

A computerized review board randomly selects assets to be reviewed. Assets are reviewed to ensure that sufficient levels of effort have been allocated to each asset. In each period, there is a 50 percent chance that you will be selected for review. If you are selected, one of the assets that you verified will be randomly chosen and reviewed. Type A assets have a 5 percent chance of being reviewed and Type B assets have a 95 percent chance of being reviewed. Selecting which asset will be reviewed is predetermined, before the experiment begins using a computer program.

If you are selected, the review process will cost you 150 EE. In addition, you may be penalized based on your effort-level chosen for the asset selected. The severity of the penalty varies based on the effort-level chosen. Below describes the review penalties for each effort-level. The penalties are the same for each asset type. Remember that only one asset (Type A or Type B) is selected to be reviewed. The penalty will be subtracted from your earnings for that asset.

Effort-Level:	**1**	**2**	**3**	**4**	**5**
	Severe		**Moderate**		**None**
Penalty:	**150**	**125**	**100**	**75**	**0**

At the conclusion of the experiment, the review board (the experimenter) will announce the review process results to the group in this room. For anyone who received a review penalty, each person's number and cumulative amount of review penalties (summed across the 20 periods) will be announced to the group, one by one in order by severity (highest to lowest amount).

Periods

There will be three practice periods before the experiment begins. You will not receive compensation for the earnings you accumulate during the practice periods. There will be 20 periods during the experiment.

Experimental Earnings per Period

For each period, your earnings will be equal to your revenues earned minus the costs of effort, costs for incorrect reports, *and review costs and penalties*. Your period earnings will accumulate across all periods.

APPENDIX B

EXPERIMENTAL PRE-TEST QUESTIONS

[Note: The information related to the *Inspections* condition only is in italics.]

Participants were asked to answer “true” or “false” to the following questions.

1. You can allocate any amount of effort points from 1 to 5 to an asset provided that the total combined effort is not more than 6?
2. You have to allocate the same amount of effort points to each asset?
3. The signals about the true asset values are always accurate?
4. The signals for Type A and Type B assets have different error rates?
5. There is a 50% chance you will be charged a cost of 6,000 EE if you report the asset has a "Low Value" but the true asset value is "High"?
6. *There is a 50% chance that you will be selected to be reviewed?*
7. *If you are selected to be reviewed, Type A Assets have a 5% chance and Type B Assets have a 95% chance of being reviewed?*
8. *The review penalties depend on your report decisions?*

APPENDIX C

POST-EXPERIMENTAL QUESTIONNAIRE

[Note: The information related to the *Inspections* condition only is in italics.]

Please complete the following questionnaire. You will earn money (up to $3.80) if you complete the questionnaire. Thank you.

1. What year are you in university (e.g., 1st, 2nd, 3rd, 4th)? _________

2. What is your major area of concentration (e.g., accounting, finance)? ___________

3. What is your gender? (check one) _____ Male _____ Female

4. How old are you? _______

5. In the study, if you were selected to be reviewed, Type B assets had a much larger chance of being reviewed than Type A assets (95% versus 5%)? (check one)

______ *True* _______ *False*

6. In the study, review penalties were based on your effort-level decisions? (check one)

______ *True* _______ *False*

7. In the study, if the asset value was low, but you reported "High Value", there was a 50 percent chance you would incur a cost of 6,000 EE and a 50 percent chance you would incur a cost of 0 EE? (check one)

______ True _______ False

8. To what extent did the following impact your <u>effort-level</u> decisions for verifying each asset? (circle the appropriate number for each one)

(a) You wanted to get a better (more accurate) signal on the asset value:

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

(b) You wanted to avoid the 50% chance of incurring a cost of 6,000 EE for issuing an incorrect "High Value" report:

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent *(c)* *You wanted to avoid the costs of the review penalty:*

A Small Extent *1----2----3----4----5----6----7----8----9----10----11* *A Great Extent*

(d) *You didn't want to be the person with the highest amount of review penalties:*

A Small Extent *1----2----3----4----5----6----7----8----9----10----11* *A Great Extent*

9. Please briefly describe, in general, how and why you made your <u>effort-level</u> decisions for each asset type.

____

____

____

____

10. To what extent would you have liked to have been able to choose a total effort level greater than 7? (circle the appropriate number)

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

11. To what extent would you feel bad for choosing an effort level below the standard level of 3 for an asset? (circle the appropriate number)

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

12. To what extent would you feel bad if others (e.g., the other individuals in this room including the participants and experiment) knew you chose an effort level below the standard level of 3 for an asset? (circle the appropriate number for each one)

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

13. To what extent did the following impact your <u>report</u> decisions for each asset? (circle the appropriate number for each one)

(a) You relied on the signal of the asset value when reporting:

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

(b) You reported high to avoid the additional cost of 500 EE to report "Low Value":

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

(c) You wanted to avoid the 50% chance of incurring a cost of 6,000 EE for an incorrect "High Value" report:

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

14. Please briefly describe, in general, how and why you made your report decisions for each asset type.

____

____

____

____

15. To what extent would you feel bad if you issued a "High Value" report but the actual asset value was "Low"? (circle the appropriate number)

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

16. To what extent would you feel bad for incurring an incorrect report cost of $6,000 for issuing a "High Value" report but the actual asset value was "Low"? (circle the appropriate number)

A Small Extent 1----2----3----4----5----6----7----8----9----10----11 A Great Extent

17. Did you incur any review penalties? (check-one) _____ Yes _____ No

(a) If so, did you feel bad for incurring the review penalties?

Not at All 1----2----3----4----5----6----7----8----9----10----11 To a Great Extent

(b) If so, to what extent were you worried about others learning about the review penalties you incurred?

Not at All 1----2----3----4----5----6----7----8----9----10----11 To a Great Extent

18. Please briefly describe, in general, how you were affected by the inspection process (i.e., did the concern of receiving inspection penalties or having your penalties announced to the group influence your effort and report decisions). Please explain.

____

____

____

____

____

19. How would you characterize your economic situation? (circle the appropriate number)

Poor 1----2----3----4----5----6----7----8----9----10----11 Wealthy

20. How would you characterize the amount of money earned for participating in this experiment? (circle the appropriate number)

Nominal Amount 1----2----3----4----5----6----7----8----9----10----11 Considerable Amount

21. How interesting did you find this experiment? (circle the appropriate number)

Not very Interesting 1----2----3----4----5----6----7----8----9----10----11 Very Interesting

APPENDIX D

OPTION CHOICE TASK

In this experiment you will complete the table below indicating your choice of Option A or Option B. You have ten consecutive decisions to make and you should record your choice in the final column of the table. However, **once you switch from Option A to Option B, while making decisions in consecutive order, you can no longer select Option A.**

In summary, for Option A, going from decision 1 to 10, the chance of earning $2.00 instead of $1.60 increases by 1/10 with each decision. For Option B, going from decision 1 to 10, the chance of earning $3.80 instead of $0.10 increases by 1/10 with each decision. The primary difference between Option A and Option B is the range of payouts.

Here's how your earnings will be determined.

Using a computer program, a number 1 to 10 will be randomly selected. This random number will indicate the Decision Number (1-10) that determines your earnings. Only one choice will determine your earnings and all choices are equally likely. Next, another number 1 to 10 will be randomly selected. This number will determine the Payoff Number for the option you chose. For example, suppose the first random number drawn indicates 1 and you chose Option A for decision 1. If the second random number drawn also indicates 1, your payoff is $2.00 and if the card indicates any number from 2 to 10, your payoff is $1.60. On the other hand, if you had chosen Option B for decision 1, your payoff would be $3.80 if the second random number drawn indicates 1 and if the second random number drawn was any number 2 to 10, your payoff would be $0.10.

In the computerized program, enter your Decision # (1-10) indicating at which decision number you want to switch from Option A to Option B. Then your payoff will be shown. Please raise your hand if you have any questions.

Decision	Option A	Option B	Your choice (A or B)
	$1.60 if the # is 1-10	$0.10 if the # is 1-10	Option A
1	$2.00 if the # is 1 $1.60 if the # is 2-10	$3.80 if the # is 1 $0.10 if the # is 2-10	
2	$2.00 if the # is 1-2 $1.60 if the # is 3-10	$3.80 if the # is 1-2 $0.10 if the # is 3-10	
3	$2.00 if the # is 1-3 $1.60 if the # is 4-10	$3.80 if the # is 1-3 $0.10 if the # is 4-10	
4	$2.00 if the # is 1-4 $1.60 if the # is 5-10	$3.80 if the # is 1-4 $0.10 if the # is 5-10	
5	$2.00 if the # is 1-5 $1.60 if the # is 6-10	$3.80 if the # is 1-5 $0.10 if the # is 6-10	

6	$2.00 if the # is 1-6 $1.60 if the # is 7-10	$3.80 if the # is 1-6 $0.10 if the # is 7-10	
7	$2.00 if the # is 1-7 $1.60 if the # is 8-10	$3.80 if the # is 1-7 $0.10 if the # is 8-10	
8	$2.00 if the # is 1-8 $1.60 if the # is 9-10	$3.80 if the # is 1-8 $0.10 if the # is 9-10	
9	$2.00 if the # is 1-9 $1.60 if the # is 10	$3.80 if the # is 1-9 $0.10 if the # is 10	
10	$2.00 if the # is 1-10	$3.80 if the # is 1-10	

APPENDIX E

WEALTH-MAXIMIZING BEHAVIOR

I constructed an experimental setting where the wealth-maximizing predictions are held constant across the *Inspection* conditions and held constant across *Client Risk* conditions. The purpose of this appendix is to explain the parameters and the wealth-maximizing behavior in detail.

For effort-level decisions, I set the standard level of effort 3 equal to the wealth-maximizing effort-level choice. Specifically, for both assets, the cost of effort has increasing net marginal benefits from effort-levels 1 to 3 (up to the standard level) and then has decreasing net marginal benefits beyond the standard level 3 (refer to Panel A of Figure 2). The "benefits" represent the accuracy of the signal. Obtaining an accurate signal is necessary in order to avoid incorrect report costs of 6,000 EE. The parameters related to effort-level decisions and signals are held constant across the *Inspection* conditions.

For reporting decisions (*high/low*), by design, the wealth-maximizing decision is always to follow the signal. Figure 4 illustrates the expected costs of the various report choices for lower-risk and higher-risk clients. As shown in Lines G of Figure 4 (Panels A and B), when there is a "low value signal" the expected cost of reporting *low* (i.e., following the signal) is 500 EE which is lower than the expected cost of reporting *high* of 3,000 EE. This result follows because (1) the cost of reporting *low* is always 500 EE, regardless of the outcome, and (2) a *low* value signal is 100 percent accurate, thus, the expected cost of reporting *high* after receiving a "low value signal" is the cost of an incorrect report 6,000 EE multiplied by the detection rate of 50 percent. Likewise, when there is a "high value signal" the expected cost of reporting *high* (i.e., following the signal) is lower than the expected cost of reporting *low* of 500 EE. Specifically, for lower-risk (higher-risk) clients, the expected cost of issuing a *high value* report upon receiving a "high value signal" ranges from 36-120 EE (144-480 EE) based on the effort-level chosen (refer to Figure 4 for more details). Accordingly, the reporting decisions do not differ across client types. These design choices are held constant across the *Inspection* conditions.

Finally, the inspection review process does not alter the wealth-maximizing decisions. Recall that the inspection review penalties range from 0 to 150 EE and are incurred based on the effort-level choices. While higher levels of effort reduce the amount of review penalties (if selected for review), the incremental cost of effort is greater than the expected value of savings for avoiding review penalties. For example, choosing an effort-level "4" costs 50 EE more than an effort-level "3," but *if* the asset is selected for inspection, the review penalty for an effort-level "4" is only 25 EE less than for an effort-level of "3." Therefore, increasing effort to avoid inspection penalties is not consistent with maximizing wealth. Further, in this setting, the participants' reporting choices have no bearing on the review process.

In summary, the above discussion indicates that the wealth-maximizing decisions are to always choose an effort-level of "3" for both lower- and higher-risk clients, under a regime with and without inspections. Given this design, any deviations in effort-level choices of "3" or in reporting choices that do not follow the signal are assumed to be for reasons other than to maximize wealth.

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Winn, A. 2014. Partner rotation and PCAOB inspections: Effects on end-of-term audit quality. Working paper, University of Washington.

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VITA

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EDUCATION

Georgia Institute of Technology, Atlanta, GA

- Doctorate of Philosophy (Ph.D.), Accounting, August 2014

University of Wisconsin-Whitewater, Whitewater, WI

- Masters of Professional Accountancy (MPA), May 2005
- Bachelor of Business Administration (BBA), Accounting, May 2004

PROFESSIONAL EXPERIENCE

Deloitte & Touche LLP, Milwaukee, WI

- Audit Senior Consultant, September 2007 – May 2009
- Audit Associate, September 2005 – August 2007

CERTIFICATION

- Certified Public Accounting (CPA), State of Wisconsin 2005 (inactive status)

ACADEMIC RESEARCH

Research Interests:

- Judgment and decision-making in accounting with specific interests in audit quality, audit regulation, and professional skepticism

Publications:

1. Knechel, W. R., and L. B. Shefchik. "Audit Quality." *The Routledge Companion to Auditing*, Ed. D. Hay, W.R. Knechel, and M. Willikens, Oxford (UK): Routledge, 130-147.
2. Knechel, W. R., G. V. Krishnan, M. Pevzner, L. B. Shefchik, and U. K. Velury. 2013. Audit Quality: Insights from the Academic Literature. *Auditing: A Journal of Practice & Theory* 32 (Special Issue).
3. Church, B. K. and L. B. Shefchik. 2012. PCAOB Inspections and Large Accounting Firms. *Accounting Horizons* 26 (1): 43-63.

Working Papers:

- Shefchik, L. B. "Effects of Risk-based Inspections on Auditor Behavior." (Dissertation job market paper)

- Krishnan, G. V., L. B. Shefchik, and W. Yu. "Potential Consequences of Violating Debt Covenants Imposed by Auditors." (Under second round of review at *Contemporary Accounting Research)*

  Received the "Best Paper Award" at the 2013 Center for Corporate Reporting & Governance Conference held at the College of Business and Economics at California State University, Fullerton.

  Presented at the 2010 AAA Annual Meeting and the 2014 Auditing Section Midyear Conference.

- Majors, T. M., L. B. Shefchik, and A. Vitalis. "The Interactive Effect of Ego Depletion and Skepticism on Auditor Performance" (Preparing to submit to *The Accounting Review*)

  Presented at the 2014 Auditing Section Midyear Conference.

- Gramling, A. A., A. Schneider, and L. B. Shefchik. "The Combined Effects of Prior Work and Type of Internal Control Deficiency on Internal Auditors' Evaluations of Internal Controls"

  Presented at the 2010 AAA Annual Meeting.

TEACHING EXPERIENCE

Teaching Interests:

- Auditing, Financial Accounting, and Managerial Accounting

Georgia Institute of Technology, Scheller College of Business, Atlanta, GA

- *Instructor*, Auditing & Financial Control Systems
  - Fall 2013, Course rating 4.8/5.0
  - Summer 2012, Course rating 5.0 / 5.0
  - Spring 2012, Course rating 4.5 / 5.0

RESEARCH PRESENTATIONS

- 2014 Deloitte Foundation/ University of Kansas Auditing Symposium. "Potential Benefits and Unintended Consequences of Risk-based Inspections on Auditor Behavior"

- Job interview workshops at Arizona State University, Chapman University, Georgia State University, Indiana University Bloomington, University of Arizona, University of Illinois at Urbana-Champaign, University of Massachusetts Amherst, University of Wisconsin-Madison. "Potential Benefits and Unintended Consequences of Risk-based Inspections on Auditor Behavior"

- 2014 AAA Auditing Midyear Meeting, San Antonio, TX. "The Interactive Effect of Ego Depletion and Skepticism on Auditor Performance: Why There *Can* Be Too Much of a Good Thing"

- 2013 Center for Corporate Reporting & Governance (CCRG) Conference, California State University, Fullerton. "Debt Covenant Violations and Associations with Viable Auditor Responses"
- 2012 AAA Annual Meeting, Washington D.C. "Regulatory Inspection Regimes and Auditor Behavior" (A previous version of my dissertation job market paper)
- 2012 AAA Annual Meeting, Washington D.C. "Effects of Prior Internal Audit Work on Internal Control Evaluations"
- 2010 AAA Annual Meeting, San Francisco, CA. "PCAOB Inspections and Large Accounting Firms"

ACADEMIC SERVICE

Ad Hoc Reviews for Scholarly Journals and Conferences:

- *Contemporary Accounting Research,* 2014
- *Managerial Auditing Journal,* 2013
- *Auditing: A Journal of Practice & Theory,* 2011, 2014
- AAA Auditing Section Midyear Conference, 2014 (two papers)
- AAA Annual Meeting, 2013 (two papers)
- AAA, Annual Meeting, Washington D.C., 2012 (two papers)
- AAA Accounting Behavioral Organization Conference, 2012

Discussant and Moderator for Conferences:

- AAA Accounting Behavioral Organization Conference, San Diego, CA, 2013, Discussant
- AAA Annual Meeting, Anaheim, CA, 2013, Discussant
- AAA Accounting Behavioral Organization Conference, Atlanta, GA, 2012, Discussant and Moderator
- AAA Annual Meeting, Washington D.C., 2012, Moderator
- AAA Annual Meeting, Denver, CO, 2011, Discussant and Moderator
- AAA Accounting Behavioral Organization Conference, Denver, CO, 2010, Discussant

CONFERENCES ATTENDED

- Deloitte Foundation/ University of Kansas Auditing Symposium, 2014
- AAA Auditing Section Midyear Conference, San Antonio, TX 2014
- AAA Accounting Behavioral Organization Conference & Doctoral Consortium, San Diego, CA 2013
- Center for Corporate Reporting & Governance (CCRG) Conference, California State University, Fullerton, 2013
- AAA Annual Meeting, Anaheim, CA 2013
- Southeast Summer Accounting Research Colloquium, Atlanta, GA 2013
- AAA Auditing Section Midyear Conference & Doctoral Consortium, New Orleans, LA 2013
- AAA Accounting Behavioral Organization Conference & Doctoral Consortium, Atlanta, GA 2012
- AAA Annual Meeting, Washington D.C. 2012
- Southeast Summer Accounting Research Colloquium, Atlanta, GA 2012
- AAA/Deloitte Foundation/J. Michael Cook Doctoral Consortium, Tahoe, CA 2012
- AAA Auditing Section Midyear Conference, Savannah, GA 2012
- IFREE's Graduate Student Workshop in Experimental Economics, Orange, CA 2012

- Southeast Summer Accounting Research Colloquium, Atlanta, GA 2011
- AAA Annual Meeting, Denver, CO 2011
- AAA Auditing Section Midyear Conference & Doctoral Consortium, Albuquerque, NM 2011
- AAA Accounting Behavioral Organization Conference & Doctoral Consortium, Denver, CO 2010
- AAA Annual Meeting, San Francisco, CA 2010
- AAA Auditing Section Midyear Conference & Doctoral Consortium, San Diego, CA 2010

HONORS AND AWARDS

- *Ashford Watson Stalnaker Memorial Award* for student excellence in the PhD program, 2013
- Outstanding Performance Award, Deloitte & Touche, LLP, 2009

REFERENCES

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