

Do Audit Actions Consistent with Increased Auditor Scepticism Deter Potential

Management Malfeasance?

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ABSTRACT

Effective audits are claimed to not only enhance the detection of fraud but also the deterrence of potential fraud. We conduct an experiment with 167 senior corporate managers in which we examine whether changes in auditor actions and behaviours consistent with auditors exhibiting a heightened level of professional scepticism deter managers from committing corporate malfeasance. We examine three audit action changes: an increase in management inquiry rigour, an increase in the extent of the audit sample and a change in the nature of the audit tests. All of these changes are situated in an experimental design where the objective probability of detecting managerial malfeasance does not change and managerial malfeasance can occur in the same period as the audit action changes. We find that changing the nature of the evidence collected decreases participating managers' intention to commit corporate malfeasance. Furthermore, we find that the most common auditor response in practice, increasing the extent of the sample without changing tests, does not reduce managers' intention to commit malfeasance. Finally, we find that combining the use of more critical client questioning with increasing extent of testing does result in less managerial intent to commit malfeasance. Implications of these findings are discussed.

Keywords: *professional scepticism; management malfeasance; fraud; deterrence; effective audits; types of audit evidence;*

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I. INTRODUCTION

The last three decades of audit standard setting have reflected a move back to the original focus of auditing, that being the prevention and detection of fraud and other malfeasance. While professional standards have always emphasized the need for the auditor to maintain an attitude of scepticism (e.g., SAS 1; ISA 200), recent standards (e.g., SAS 109; ISA 315) further emphasize the need for the auditor to maintain an attitude of professional scepticism when planning and performing an audit, particularly when considering the risks of misstatement due to fraud. Specifically, SAS 99 and the newly revised ISA 240 (both fraud related standards) are characterized by a dramatically increased emphasis on maintaining an attitude of professional scepticism and adapting audit procedures accordingly when it is heightened (Carpenter and Reimers 2009; Carpenter 2007; Marczewski and Akers 2005).

Effective audits are claimed to enhance not only the detection of fraud but also the deterrence of fraud (e.g., Kranacher 2006; Wells 2004; PAE 2000). Prior research has focused primarily on the role of the auditor in fraud detection (i.e., reducing detection risk in the audit risk model) by examining how auditors react to various risks, and how to increase the sensitivity of the auditor to those risks such that the audit program is more responsive to changes in those risks (e.g., Eining et al. 2006; Mock and Turner 2005; Glover et al. 2003; Knapp and Knapp 2001; Phillips 1999; Zimbelman 1997).

In addition to a detection role, it has long been posited that auditing plays a role in reducing the likelihood that fraud occurs in the first place (Mautz and Sharaf 1961;

ASOBAC (AAA) 1973 p. 13). The fact that an audit will occur is made known to management to motivate them to report honestly given management knows their actions will be subject to scrutiny. Indeed, this knowledge is a stylized fact employed to motivate many analytical models examining a variety of audit issues (e.g., Baiman et al 1987; Antle and Nalebuff 1991). However, our understanding of whether the audit has these deterrence effects in practice is limited (e.g., Nelson 2007; Uecker et al. 1981).

Nelson (2007) reviews related research on factors that may heighten auditor scepticism but research on the effects of any auditor action changes on deterring management fraudulent behaviour is scant. In the only other research in this area that we are aware of, Uecker et al. (1981) find that managers did not respond to the increased “aggressiveness” of external auditor’s actions versus the “normal” audit. Uecker et al. (1981) informed the participants, acting as CEO via a memo from the external auditor, of the CEO’s responsibility for the fairness of financial statements. In addition, the external auditors directly requested the CEO to review and confirm the reasonableness of an allowance for doubtful accounts. These changes in audit actions, relative to what would have been done in a normal audit at that time, were considered by participants to be “aggressive”, loosely analogous to the changes in audit actions due to heightened professional scepticism we study. These changes in audit actions did not reduce the participants’ propensity to commit malfeasance (i.e., understating the allowance for doubtful accounts) over the base case where no such audit actions were taken. However, much has changed since the 1970’s in our understanding of theories of human behaviour and in regulatory requirements regarding the exercise of professional scepticism in audits leading to the present study.

Employing theories from the attitudes, deterrence, and interpersonal deception literatures, we examine whether management intentions to commit corporate malfeasance are affected by management observing changes in audit actions vis-à-vis the previous year's audit. These auditor behaviour changes are consistent with an auditor exhibiting responses to heightened professional scepticism. Most managers can only observe the actions of auditors that may arise from increased professional scepticism, and may not directly know the reasons for an auditor's change in "attitude that includes a questioning mind and a critical assessment of the evidence" (ISA Glossary of Terms, 2008, p. 154).¹ We posit that in order for changes in auditor behaviour to act as a deterrent, management has to notice that the auditor has changed his/her audit approach from the prior year, perceive a resultant increase in the likelihood of the auditor detecting malfeasance and negative consequences associated with such detection, and thus choose not to undertake questionable accounting activities.

We conduct an experiment with senior corporate managers who possess significant experience as prior research has shown that most material financial statement fraud is committed by one or more members of management (Tillman and Indergaard 2007; Beasley et al. 1999). To strengthen our tests of the deterrent potential of changes in auditor behaviours, we make it highly likely that the objective probability of the auditor detecting management malfeasance in the experimental participant's division *remains unchanged* by having the changes in auditor's actions occur in the other two divisions of

¹ Certainly some financial managers and senior management of the company may have a more in-depth understanding of what the auditor is doing and why the auditor is doing it (e.g., Gibbins et al. 2001 on auditor client management negotiation). Nonetheless, auditors in recent years have been communicating less, rather than more, with their clients (see PCAOB 2005, 11-13).

the company under a known rotational audit plan.² We experimentally ensure that managers notice the audit actions have changed in the other divisions of a company versus the previous years. We also situate the experiment at the interim audit late in the fiscal year in a context when substantive procedures are being conducted. This setting allows the experimental participant to make a potentially fraudulent accounting decision in his/her division that will have immediate effects on the current year's financial statements.

We examine three audit action changes that are consistent with increased professional scepticism: change in management inquiry rigour (more pointed and critical questioning compared to Same As Last Year (SALY) audit tests) (CICA 2000); change in the extent of the sample collected holding evidence type and audit procedures constant (increased samples sizes with the same tests compared to SALY); and/or change in the nature of audit evidence collected (other than inquiry) and related procedures, other than inquiry (change in the nature compared to SALY) (Knechel et al. 2007 Chapter 10; ISA 315; SAS 109).³ Combinations of the three changes in actions are also studied where logically feasible.

We find that the auditor changing the nature of the evidence collected in the other divisions (i.e., moving from relying predominantly on internal documents and records to external confirmations with third parties) decreases managers' reported intention to

² We are careful to hold the objective probability of detection constant across experimental conditions (see experimental design section) to maintain experimental control. Holding the objective probability of detection constant also biases against finding support for our hypotheses.

³ Technically speaking, an increase of management inquiry rigour is a change in the nature of the audit tests. However, substantively speaking, this is a relatively weak response to an increase in professional scepticism as professional standards view inquiry as needing to be corroborated with other audit evidence (ISA 315; SAS 109). Further, based on recent releases by standard setters and regulators (e.g., PCAOB 2008-006 A4-3), more inquiry of management is not what they look for in audit program changes that reflect enhanced auditor scepticism.

commit corporate malfeasance in their own division relative to a SALY audit. Notably, changing the extent of the evidence collected (i.e., expanding sample size while holding type of evidence collected and tests constant) in the other divisions by itself does not reduce managers' reported intention to commit malfeasance in their own division vis-à-vis a SALY audit, despite it being the most common response to increased auditor scepticism in practice (Elder and Allen 2003; Bedard et al. 1999; Mock and Wright 1993). The addition of critical inquiry to changing the extent of the evidence collected in the other divisions is effective in reducing managers' reported intention to commit corporate malfeasance versus a SALY audit as well as versus expanding the sample size alone.

The remainder of this paper proceeds as follows. First, we develop our hypotheses based on attitudes, deterrence, and interpersonal deception theories combined with our understanding of the institutional environment. We then outline our research design, our experimental participants, the results of our experiments, and discuss the implications of these results for future research.

II. LITERATURE REVIEW AND HYPOTHESES DEVELOPMENT

We examine managers' reactions to the auditor behavior changes (i.e., observable audit actions) that are consistent with the activities regulators and professional standards associate with heightened auditor professional scepticism (e.g., PCAOB 2008-006 A4-3). Following Uecker et al. (1981), we also do not focus on the auditor's reasons for the changed audit actions in our research, as that would make the research question unmanageable.⁴ Instead, our study has managers observe changes in audit actions that are consistent with heightened professional skepticism and explores the effects of

⁴ See Nelson (2007) for a review of the factors that have been found to influence the auditor's level of professional scepticism that can trigger changes in audit actions.

managers' observation of those changes on their perceptions and behaviors.

Professional Scepticism and Audit Actions

Both IAS Glossary of Terms and SAS #1 define professional scepticism as “an attitude that includes a questioning mind and a critical assessment of audit evidence” and this view has been reinforced by recent PCAOB releases (e.g., PCAOB 2008-006, p. A4-3). Research shows generally that changes in attitude generally affect a person's behaviour or actions (see Petty et al. 1997 for a review; Eagly and Chaiken 1993). Thus, an attitudinal change in professional scepticism should be demonstrated by associated changes in auditor behaviour and actions. Recent audit standards (ISA 330, SAS 110) specify that exhibiting heightened professional scepticism requires the auditor to respond to heightened risk of material misstatement by considering changing the nature, timing, and extent of the auditing procedures to obtain reasonable assurance that the financial statements are free from material misstatements.^{5,6} The extant audit archival and experimental literature indicates that auditors may respond in principle to changes in audit risks in a variety of ways, ranging from retaining the audit plan from prior year without any adjustment to changing the nature and/or extent of audit tests to many combinations in between (see Bedard et al. 1999 for a review of this literature).

Consistent with standards and prior archival research, we examine three potential changes in auditor behaviour. First, as the definition of scepticism suggests, the auditor can change his/her “questioning mind (set)” and adopt a more “critical assessment of audit evidence”. In other words, the auditor can make more pointed enquiries of client

⁵ In this study, we focus on only changes in the nature and the extent of the auditing procedures, and ignore changes in timing because it would be difficult to meaningfully operationalize timing changes in an experimental context.

⁶ ISA 240 and SAS 99 on suspicion of fraud require similar actions to be taken.

management and subject management responses to more careful scrutiny than in previous years (CICA 2000; Liu 2006). Indeed, auditors have been criticized for relying too much on audit inquiry as a form of evidence in SEC enforcement actions against auditors (Beasley et al. 1999), hence increased inquiry is a probable practice based response.⁷ Moreover, prior studies indicate that auditor's heightened sceptical judgments translate into more costly sceptical actions only when they have reached a particular threshold (Nelson 2007; Shaub and Lawrence 2002). Given the low cost of using more critical audit inquiry, we expect that it would be the first auditor behaviour that would change with a higher level of professional scepticism.

Second, as suggested by professional standards (ISA 330; SAS 110) and confirmed by archival research as the most likely response to heightened professional scepticism (Nelson 2007; Bedard et al 1999), the auditor can increase the extent of the evidence collected through increasing sample size. In addition, prior experimental studies show that in response to increased audit risk, auditors change the extent of their audit tests by increasing the budgeted audit hours, but not the nature of their audit tests or the type of evidence collected (Nelson 2007; Glover et al. 2003; Zimbelman 1997). Implicitly, it appears that auditors assume that the optimal audit procedures and evidence are being employed and collected already, and that collecting and evaluating more of the same evidence is enough to reduce risk of material misstatement to a sufficiently low level (Mock and Wright 1993; Fellingham et al. 1989; Fellingham and Newman 1985).

Finally, standards (ISA 330, SAS 110) suggest the auditor can change what sort of evidence is sampled and/or what type of tests are performed such that the nature of the

⁷ However, others have argued the poor quality of the inquiry as practiced by most auditors, not inquiry per se, which has led to the SEC criticism on overreliance on inquiry (see CICA 2000 for a more rigorous approach to inquiry).

audit evidence collected is different from that collected previously (implicitly the standards presume towards more relevant and reliable evidence) (see PAE 2000, p. 90). While this approach may be the most effective at detecting potential fraud, it is also the least likely response to heightened professional scepticism found in practice or in the laboratory (Nelson 2007; Bedard et al 1999). Nevertheless, a few studies have shown the auditor responds with changes in the nature of the evidence collected in limited circumstances (Kizirian et al. 2005; Mock and Turner 2005; Shaub and Lawrence 1996).

In summary, there are three changes in audit actions (and combinations of those) that are consistent with heightened professional scepticism: the critical nature of management inquiry, the extent of audit sampling, and nature of audit evidence collected and tested. All three responses have been explicitly noted in recent PCAOB standards that link heightened professional scepticism to audit actions (PCAOB 2008-006, p. A4-2-A4-6).

Deterrence of Management Malfeasance

We use deterrence theory to examine whether audit actions consistent with heightened professional scepticism deter management malfeasance. Deterrence theory, in addition to being used in more traditional crime research, has been applied to analyze the reduction of the intention to commit crimes such as theft (e.g., Lochner 2007), taxpayer non-compliance (e.g., Fischer et al. 1992), consumer fraud (e.g., Cole 1989), white-collar crimes (Weisburd et al. 1995), and corporate environmental violations (e.g., Gunningham et al. 2005). Deterrence theory proposes three variables that deter people from engaging in illegal or undesirable activities, namely certainty, severity, and swiftness of punishment. When people perceive an increase in the certainty of being caught in an illegal or socially undesirable act that results in severe and quick punishment, the costs of committing the

act increases and hence the act's expected utility decreases, thereby reducing the likelihood of people committing the act in the first place (Mendes and McDonald 2001; Nagin and Pogarsky 2001; Scheider 2001; Becker 1968). Since the 1970's, deterrence theory has steadily expanded from its strict economics base to incorporate psychological theories (e.g., judgement and decision making) about offenders' interpretation of the theory's constructs (e.g., Nagin and Pogarsky 2003).

The deterrent effect of punishment certainty, in the form of a higher perceived probability of detection and punishment (i.e., arrest and conviction), is the one consistent empirical finding in the literature (Mendes and McDonald 2001; Nagin and Pogarsky 2001). The recent research shows that it is the perceived probability, rather than the objective probability, of detection and punishment that acts as a deterrent especially for first time offenders, a category for which many, if not most, managers contemplating a fraudulent accounting action would fall into (Fischer et al. 1992; Lochner 2007; Scheider 2001).⁸ Hence, in constructing our hypotheses, we focus on the increase in perceived probability of detection and punishment of corporate malfeasance as the deterrent for managers undertaking corporate malfeasance actions, holding constant the objective probability of detection and punishment.

Management's Knowledge of Auditor Behaviour Change and Hypotheses

Managers can observe, or know through others, the changes in audit actions (i.e., changes in inquiry, extent and nature of audit evidence collection) relative to the audit actions in previous years. Organizational behaviour research shows that managers attempt

⁸ Perceived punishment severity (see Mendes and MacDonald, 2001 for a review) and swiftness (see Nagin and Pogarsky 2001) have not been found to have a consistent significant effect on deterring criminal actions despite folk wisdom to the contrary.

to make sense of activities that occur in their environment that might affect them as a precursor to determining what their own behaviour will be (see Weick et al. 2005 for a review). Hence, managers noting changes in auditor behaviour will attempt to make sense of them prior to undertaking any actions involving corporate malfeasance.

Research on interpersonal deception indicates that both truth tellers and deceivers are able to perceive increased suspicion by others from observing behavioural changes in them (Burgoon et al. 1996; Burgoon et al. 1995). But people perceive more suspicion when they are lying than when they are telling the truth because of guilt and the fear of detection (Buller and Burgoon 1996; Burgoon et al 1995). Therefore, we expect managers to be sensitive to observable audit action changes and perceive increased suspicion by the auditor in general. In particular, we expect managers contemplating fraudulent acts would be more likely to attribute changes in audit actions to specific suspicions about fraud in the company rather than to audit changes due into professional standards requirements compared to those who do not contemplate such acts.

Therefore, managers contemplating a potential act of corporate malfeasance will be sensitive to auditors who become more critical in their questioning and challenge management assumptions and explanations more. Further, managers can see this increased critical inquiry as increasing the likelihood that corporate malfeasance will be detected and punished compared with previous years, albeit not a great increase due to the fact that inquiry relies solely on client management explanation (i.e. inquiry is considered weak evidence in professional standards and by regulators as discussed above). If managers perceive that the probability of detection and punishment of material misstatement is increasing due to this increased level of auditor inquiry, then there would

be a deterrence effect for management malfeasance. Our first hypothesis is:

H1: More critical auditor inquiry decreases managerial intent to carry out corporate malfeasance compared to a “same as last year” audit.

The same line of reasoning follows from managers’ perceptions of audit action changes with respect to increasing the sample size to which the same audit procedures will be applied. Managers who are contemplating malfeasance may attribute this increased sample size to auditor suspicions of fraud in the company, hence increasing the perceived likelihood that such malfeasance will be detected and punished. However, managers might not perceive a large increase in detection likelihood because managers know what audit tests were done on what types of evidence in the previous years and are unlikely to commit an act of corporate malfeasance in an area where they know an auditor is most likely to look (Fellingham and Newman 1985). However, if there is some increase in perceived probability of detection and punishment, then the deterrence effect would occur and our second hypothesis is:

H2: An increase in audit evidence sample size decreases managerial intent to carry out corporate malfeasance compared to a “same as last year” audit.

Our third hypothesis is about managers’ perceptions about whether the auditor’s change in the nature of the evidence collected (e.g., evidence selection procedures, tests applied to the new evidence, evidence types) increases the perceived probability of detection and punishment of corporate malfeasance. Assuming that manager’s knowledge of audit procedure effectiveness overlaps sufficiently with auditor’s knowledge, the use of more effective audit procedures will be considered as increasing the probability that malfeasance in the other divisions would be detected. Hence, where the change in the nature of the evidence collected has face validity, there would be a deterrence effect on

managers contemplating any questionable acts of malfeasance. Thus, we hypothesize that:

H3: A change in the nature of the audit evidence collected decreases managerial intent to carry out corporate malfeasance compared to a “same as last year” audit.

Numerous audit post-mortems have shown that managers committing financial statement fraud are unlikely to do so in areas where they have strong reasons to believe that the auditors will carry out extensive audit procedures (Fellingham and Newman 1985). Indeed, most audit failure post-mortems, albeit with the benefit of hindsight, are able to suggest relatively straightforward audit procedures that if employed would have detected the fraud or at least reduced the impact of the fraud when revealed at a later date (e.g., Beasley et al. 2001, 1999). Kadous (2000) also finds that as the consequences of audit failure become more severe, jurors require higher standards of care with respect to the nature of audit evidence collected but not with respect to the extent of audit sample size. Hence, we posit that from a manager’s perspective, changing the nature of audit evidence to evidence that is perceived to be more diagnostic, would have a stronger deterrence effect on corporate malfeasance behaviour than increasing existing procedures’ sample sizes. Hence, our fourth hypothesis is:

H4: A change in the nature of the audit evidence collected decreases managerial intent to carry out corporate malfeasance more than an increase in audit evidence sample size decreases managerial intent to carry out corporate malfeasance.

Although professional standards indicate that auditors should engage in more critical questioning of management and challenge weak or inconsistent explanations under increased professional scepticism, interpersonal deception theory suggests that people who are suspicious of others may actually use more accepting, rather than sceptical

questions, when interacting with suspected others (Buller and Aune 1987; Buller et al. 1991). This finding is stronger when people question others they know personally (Buller and Aune 1987; Buller et al. 1991; Buller and Burgoon 1996; Burgoon et al. 1996; Burgoon et al. 1995), which is characteristic of the audit environment. Hence, auditors may not necessarily adopt a more critical inquiry approach either because they are strategically concealing their suspicions from their client or to preserve good working relationships with their client (McCracken et al. 2008).⁹ However, prior research indicates that deceivers perceive more suspicion in the face of critical probing and therefore perceive a higher probability of detection (Buller et al. 1991; Burgoon et al. 1996).

Given that it is not automatic that both critical questioning and another audit action change will take place together, we test the effect of adding critical inquiry to a change in the extent or nature of audit procedures to help auditors better understand the impact of different combinations of audit action changes. We posit that using the two together will have at least an additive effect and increase managers' perceptions of the probability of detection and punishment more than either audit action change alone. Therefore, our fifth hypothesis is as follows:

H5a: An increase in audit evidence sample size combined with more critical auditor inquiry decreases managerial intent to carry out corporate malfeasance more than an increase in audit evidence sample size alone.

H5b: A change in the nature of the audit evidence combined with more critical auditor inquiry decreases managerial intent to carry out corporate malfeasance more than a change in the nature of the audit evidence alone.

In summary, our hypotheses suggest that managers will be less likely to commit

⁹ In fact, there are recommendations in practice that auditors keep their suspicions to themselves during client interviews targeted at fraud detection, unless expressing suspicions is intentionally planned (Hall 2005).

corporate malfeasance via accounting activities when they note that the auditor is changing actions versus the previous year. Our overall thinking, including possible manager attributions for changes in audit actions, is summarized in Figure 1.

[Insert Figure 1 Here]

III. EXPERIMENTAL DESIGN AND METHOD

Experimental Design

The experiment manipulates six between-subjects conditions (see Table 1 Panel A). The six conditions describe the audit action changes, if any, as not occurring in the experimental participant's division but taking place in the other two divisions of a three division company under a rotational audit plan (see "Managerial Judgment Case" and "Independent Variable – Changes in Audit Actions" sub-sections for descriptions of the conditions).¹⁰ The objective probability of fraud detection remains the same for the experimental participant's division across all six conditions because the audit action changes occur in the other two divisions due to the rotational audit plan.

Participants

A final set of 167 professional managers who occupied mid to high level management positions in their companies are reported on in this study. Participants were recruited from the MBA alumni who were at least five years post-graduation and EMBA alumni of a major Canadian business school that is ranked consistently high in the *BusinessWeek* rankings of MBA programs outside the USA.¹¹ Table 2 presents

¹⁰ We did not employ a 2X2X2 design as crossing the two manipulations, changing the nature of the evidence and increasing the audit evidence sample size while holding the procedures constant, did not make logical sense. Hence we eliminated those two cells (nature plus extent and nature plus extent plus critical inquiry) from our design, leaving us with a six-cell design.

¹¹ MBA students at this university average five years of work experience, mainly professional and technical, before entering the MBA program. Hence, the five year post-MBA delay was used to ensure comparability with EMBA

demographic information about our sample. Of particular note is the mean (13.7) and median (12.0) years of managerial experience.¹² Randomization appears to have been successful as there were no significant demographic differences across experimental cells (all p's \geq 0.233). However, we did find a significant correlation between a dependent variable and age; hence we include age as a covariate in our analysis.¹³

[Insert Table 2 Here]

Experimental Procedures

We enlisted prominent MBA faculty and administrators to send an invitation email directly to the alumni, with a follow-up email approximately one week later.¹⁴ We followed carefully the literature on maximizing responses to email solicitations combined with web administered surveys (e.g., Manfreda and Vehovar 2002; Schonlau et al. 2002). These studies note that unless one obtains a “click through” from the invitation email to the website hosting the study, one cannot even be certain that the email has been read, hence sample response rates are computed based on percentage of those who click through initially (Sheehan 2001) in addition to percentage of emails sent but not “bounced back”.¹⁵

participants. We did not keep track of MBA and EMBA participants separately given this control.

¹² Note this is not work experience as many of these managers have significant technical work experience prior to entering management ranks, a fact that this set of MBA programs sensitizes them to. Hence, they are unlikely to respond with anything other than actual “managerial experience” as asked for.

¹³ All other dimensions of participants’ demographic characteristic (e.g. gender, education) do not significantly correlate with the dependent variables. We also elicited answers to the question “how frequently have you experienced issues similar to the issue described in the case?” to determine the degree that participants had experience with our scenario. The mean response was at the mid-point of the scale anchored by “never” to “numerous occasions.” This variable is not significantly correlated with the dependent variables nor is it significantly different across experimental conditions.

¹⁴ In order to reduce clerical workload on the faculty members who helped us, with their permission, we created a temporary email account in their name. This eliminated the faculty members having to deal with undelivered emails and other such issues. None of authors had taught any of the participants due to the program type and the dates of graduation restrictions put on the sample.

¹⁵ Among the steps we took to maximize “click through” included having prominent faculty and administrators who were highly influential at the time the various MBA and EMBA alumni were active in the programs solicit their

Participants who responded to the invitation would click on the web-link provided in the invitation email that automatically took them to the website where our experimental instrument was located. Participants first read general information about the study, including all precautions being taken to ensure their anonymity, and clicked a consent button to participate in the study. Then they were randomly assigned by the survey software to one of the six experimental conditions.

The software proceeds to take participants through an online managerial judgment case. Participants read the case (which they could always access during the experiment), made their judgment choices, and then answered post-experimental and demographic questions (the case was not available in this last stage as we were eliciting in part what they recalled about the case).

Managerial Judgment Case

We adapted the case, with permission, from an earnings manipulation case developed by Cohen et al. (2000). In the case, the participant read about a project manager who worked in one of three divisions in a company, Pure Marine. The project manager was deciding how to account for \$2 million of Research and Development (R&D) expenditure. The \$2 million R&D expenditure was originally initiated and authorized for a nearly-completed Project K(3). The project manager was considering allocating the R&D expenditure to Project K(3) or to two new projects that would also likely benefit from the R&D expenditure. If the project manager allocated all of the \$2 million R&D expenditure

participation after completing the case themselves. We also sent the emails at a time when the school's alumni office was on a break from sending emails; hence the alumni are not as likely to be suffering from over-contact by the school. We engaged an independent web design expert that reviewed our in-house IT department aided design to ensure a user-friendly interface to encourage responses. Finally, we tested the case on recent MBA alumni (classes that were excluded from the study) to ensure that the web screens were user-friendly.

to Project K(3), it would result in a loss of \$1.2 million on the project and Pure Marine's senior management viewed substantial losses on projects very unfavourably. Allocating some of the \$2 million R&D expenditure to the two new projects could avert reporting a loss on Project K(3), and allocating all of the \$2 million to the new projects could help the project manager to earn a bonus of up to \$40,000 as Project K(3) would achieve its targeted profit of \$700,000. Any deferral of cost recognition (i.e., allocations to the new projects) would also help the company meet analysts' profit forecasts for the current year. Although allocating some or all of the \$2 million R&D expenditure to the two new projects is somewhat ambiguous in principle because arguments can be made that the two new projects will likely benefit from the R&D expenditure, it was against a significant company policy and could lead to charges of corporate malfeasance in which the manager received an undeserved bonus and financial statements were misstated for public markets.¹⁶

The case takes place late in the fiscal year during an interim audit when substantive testing can be carried out on projects already completed, which allows participants to make a potentially fraudulent accounting decision in the same year as the audit. In all six experimental conditions, participants received information that the external auditor used a rotational audit approach and was focusing detailed substantive testing this year on the other two divisions that made up the company. Of course, some limited substantive testing was done to the participant's division that was not the focus of the rotational audit this year, but the amount of work done in that division was said to be minimal compared

¹⁶ Research and development expenditures must be expensed under US GAAP and only development expenses meeting strict criteria can be capitalized under Canadian and International GAAP. Hence, from a financial accounting viewpoint, allocating these expenditures to future periods is a likely GAAP violation in the US and questionable internationally as the capitalization criteria were not discussed nor met.

to the other two divisions.

In the SALY condition (Condition 1), participants are told that audit in the other divisions is “going as smoothly as last year” with no noticeable changes in auditor behaviour compared to previous years (“auditing about the same number of projects”, “asking the same types of questions about the projects”, and “audit is going pretty much the same as it had in previous years”). In the other five conditions, participants received information that the external auditor exhibited various changes in their behaviour in their audit of the other two divisions. Note that the participants were managers in the division **not** subject to the detailed audit and they would **not** be affected by the audit action changes in the other divisions. Therefore, the objective probability of fraud detection by the auditor is unchanged this year across all six conditions. The changes in auditor behaviour are described in the following sub-section. Participants were then asked to assume that they were the project manager in the case and made their decision (and how their peers would make the decision) on how to allocate the \$2 million R&D expenditure to Project K(3) and the two new projects.

Independent Variables - Changes in Audit Actions

We designed the changes in audit actions based on our institutional understanding of auditor behaviour consistent with heightened professional scepticism, including approaches taken in existing professional standards (ISA 330; SAS 110) and practice (e.g., Knechel et al. 2007, Chapter 10).¹⁷ These action changes were also validated through discussions with four audit partners.

¹⁷ Subsequent to designing and carrying out our study, the US PCAOB issued a request for comments (2008-006, Oct. 21, 2008) where they explicitly linked professional scepticism to “modifying the planned audit procedures to obtain more reliable evidence” in the context of the “sufficiency (*i.e., the extent in our study*) and the appropriateness (*i.e., the nature in our study*) of audit evidence.”

Management inquiry. In the critical inquiry (CI) condition (Condition 2 in Table 1 Panel A), participants were told that the external audit team “is being much more inquisitive than in prior years”, “the auditor’s questions seem more pointed”, “the auditors are not as accepting of the answers provided by the project managers as they had been in the previous years”, and the auditors were “being more critical in their questioning than usual”. This followed guidance from the CICA Research Study on Audit Enquiry (CICA 2000) which is incorporated into the manuals and training programs of many audit firms.¹⁸

Extent of audit procedures. In the Extent condition (Condition 3), participants read the external audit team had “increased the number of projects audited by almost 50% over those audited in the previous years”, “the increase in number of projects audited placed more demands on support staff”, “the project managers are being asked more of the same types of questions as in the previous years and the volume of questions created more work for them”, and “the audit is more extensive than what it has been in previous years”. Hence, the types of audit evidence being examined are kept the same but the sample size is increased, resulting in more evidence of the same type being sampled.

Nature of audit procedures. In the Nature condition (Condition 4), participants were informed that the external audit team “had so completely changed their approach to selecting and auditing a project that Pure Marine staff could never anticipate what they would be asked for next”, “for the first time ever, the auditors are directly contacting external contractors and sub-contractors to verify work done on projects”, “the auditors are insisting that the contractors and sub-contractors provide written confirmation of

¹⁸ One of the authors has carried out this training for two of these firms.

work performed”, and “the audit is less predictable than what it has been in previous years”. Hence, the nature of the audit evidence had shifted from evidence internal to the company to external confirmation, a major shift in the nature of the evidence collected towards a more reliable and more costly form of evidence (Knechel et al. 2007, Chapter 10).

In CI+Extent condition (Condition 5), the auditor adopts more critical inquiry of management as well as increases the audit evidence sample size (Extent). Lastly, CI+Nature condition (Condition 6) the auditor is being more critical with his/her inquiries of management and also changing the nature of the audit evidence collected (Nature).

Multi-dimensional ethics scale. Prior research employing the original version of this case showed that ethical considerations were significant to the cost allocation decision (Cohen et al. 1996, 2007). Hence, we also included the multi-dimensional ethics scale (MES) (Cohen et al. 1996; Reidenbach and Robin 1990) in the post-experimental questionnaire so that ethical norms can be controlled for (see Appendix for the MES used).¹⁹ The MES measures four different modes of reasoning (i.e., moral equity, relativist, utilitarian, and contractual) that participants may use to evaluate the morality of an action.²⁰ In this case we elicited the MES measures relative to a manager allocating the entire \$2 million R&D expenditure to the two new projects. Participants evaluated the action along a series of items that captured the moral equity dimension (e.g., just - unjust),

¹⁹ Using the DIT or one of its variants may be better suited for controlling ethical norms (e.g., Cohen et al 2007). However, the on-line administration of the experiment combined with the length of time it takes to complete the DIT tasks made it impractical to do so in this case. Our instrument already took 25 minutes of concentrated time to complete and most participants with incomplete data (nearly 10% of our potential participants) did not complete a sequence of items at the end of the instrument. Finally, Cohen et al. (2007) report the MES replicated qualitatively their results obtained using the DIT.

²⁰ We dropped the fifth mode of reasoning, the egoist dimension, because the two items measuring the egoist dimension did not clearly load on any factor in Cohen et al. (1996).

the relativist dimension (e.g., traditionally unacceptable - traditionally acceptable), the utilitarian dimension (e.g., minimizes benefits while maximizes harm - maximizes benefits while minimizes harm), and the contractual dimension (e.g., does not violate an unwritten contract - violates an unwritten contract). The MES also includes a one-dimensional measure on whether the action is ethical-unethical, which we eventually used as our control variable for differences in ethical approaches given that it captured a high degree of the variation of the other ethical dimension subscales ($r^2=0.65$). We also used the moral equity dimension, the relativist dimension, the utilitarian dimension, and the contractual dimension subscales in place of the one-dimensional measure in our analyses, and the results were similar.²¹

Dependent Variables – Allocation of \$2m in R&D Costs

We elicit two allocation measures, the first is a measure of Self Allocation, that is, how much the participating manager would allocate (i.e., misallocate) to the two new projects. The second measure is Peer Allocation, which elicits how the participating manager believes his/her peers would make the same allocation. Table 1 Panel B reproduces both questions. The need to consider both measures arises because asking questions such as these raises the possibility of “socially desirable” responses (i.e., “I would never misallocate costs.” or at least “I would never admit to it.”). The standard research approach to mitigate social desirability bias is to use indirect questioning (i.e., “What would your peer do?”, which is our second question), rather than direct questioning (i.e., “What would you do?”, which is our first question) because it was

²¹ Factor analysis discovered a two factor solution. The first factor was a combination of the moral equity (3 items) and utilitarian (2 items) dimensions and accounted for 46% of the variance. The second factor was a combination of the relativist (3 items) and contractual (2 items) dimensions and accounted for 11% of the variance.

originally thought that people find it easier to respond truthfully when asked in an impersonal way and project their personal beliefs/attitudes onto their responses to indirect questions (e.g., Fisher 1993; Sherwood 1981).²² Analyzing solely responses to indirect questions, however, raises questions about construct validity and loses significant information about what managers would report for direct questions (Jo 2000). This occurs because managers may strategically and simultaneously under-report socially undesirable behaviours for themselves and over-report socially undesirable behaviours for their peers (Jo 2000). Thus we analyze our measures in two ways: first, as suggested by the traditional approach to mitigate social desirability bias, we use Peer Allocation as the dependent variable while controlling for Self Allocation; second, we employ a repeated measures design with both measures (Peer Allocation and Self Allocation) as the dependent variables.²³

[Insert Table 1 Here]

Pre-test of Experimental Instrument

The experimental instrument was pilot-tested with five graduating MBA students and 16 recent EMBA graduates from classes excluded from our email solicitation, in addition to the four faculty members who agreed to solicit participation from the graduates. We used a talk aloud verbal protocol with the five graduating MBA students to ensure that their understanding of the meaning of the instrument and ours were congruent. The 16 recent EMBA graduates were employed to determine if our manipulations were

²² To reduce social desirability bias, our experiment had extensive upfront information about its anonymous nature and reminders of the same throughout the instrument.

²³ Both of these approaches are consistent with Cohen et al (2007) who employed a seven point scale anchored on low to high probability of a complete cost misallocation as the dependent variable with the difference between the peer and self intent probability scales as a control variable.

interpreted as we intended (i.e., the participants passed the manipulation checks) when the instrument was done online and that the manipulations were strong enough. Seven out of the 16 EMBA failed one or more of the manipulation checks. The two pre-tests lead us to revise our manipulations to make them more salient in the final instrument. However, we did not have the option of pre-testing the revised instrument, due to time constraints and participant availability, given the short window we had access to the MBA email list service; hence we expected a somewhat high rate of failure on the manipulation checks.

IV. RESULTS

Manipulation Checks

We employed two manipulation checks to ensure participants understood their assignment to conditions: “Compared to prior years, you believe the external auditors have increased the number of projects audited in the other Groups.” and “Compared to prior years, you believe the external auditors changed the audit approach to require written confirmation from external contractors and sub-contractors.” Answers were elicited on a bipolar nine point scale (-4 “strong disagree” to 4 “strong agree” with 0 labelled as “uncertain”). Answering these manipulation check questions on the correct side of the scale in the context of their assigned condition (i.e., “agree” in the conditions where extent/nature were manipulated and “disagree” in the conditions where extent/nature were not manipulated) was required for inclusion in the final sample. Failing these basic manipulations suggests either a lack of attention to the case materials or an inability to recall the case details by the end of the case. As expected given our pre-test results and our inability to obtain a second pre-test sample after revising our manipulations, the manipulation failure rate was 31% of the 274 responses we received to

our email solicitations. This rate is significantly lower than the 44% failure rate in the pre-test. However, this appears to be more of a perceived failure of the manipulation checks rather than a substantive one as including all participants who fail these two manipulation checks does not substantially change our results, as will be discussed later.

Manipulation checks for critical inquiry indicated that all the five conditions with audit action changes viewed the auditors as being more critical with their questioning than the SALY condition (all p 's < 0.001). This indicates that managers infer that any change in audit procedures is implicitly associated with more critical questioning, even when critical inquiry is not explicitly manipulated. We did not eliminate participants based on this manipulation check given that including participants who fail this manipulation check biases against finding results.²⁴

Participants also indicated in post experimental questions that they understood that allocating enough costs to the two new projects such that Project K(3) met its profit target would significantly increase the likelihood that Pure Marine would meet financial analysts' expectations as well as the likelihood that the project manager would be eligible for a considerable bonus.²⁵ Further, participants understood that they were breaking company policy as they agreed with the statement that there would be negative financial (mean=1.75, median=2.00), career (mean=2.29, median=2.0) and social standing (mean=1.76, median=2) consequences should a cost allocation to the new projects be

²⁴ Practically speaking, it would result in eliminating so many participants in the non-critical inquiry conditions as to make statistical tests impossible to carry out.

²⁵ Over 95% percent of participants agreed with the statement "If Sue allocated enough costs to the new projects such that K(3) meets its profit target, it will significantly increase the likelihood that Sue will be eligible for a considerable bonus." Over 89% of participants also agreed with the statement "If Sue allocated enough costs to the new projects such that K(3) meets its profit target, it will significantly increase the likelihood that Pure Marine will meet financial analysts' expectations." There were no differences between conditions (all p 's > 0.498). Eliminating those participants who did not agree does not affect our analysis.

discovered (scale: 0 (no consequences) to 4 (very negative consequences)), and all highly significant greater than 0 ($p < 0.0001$) employing both parametric (i.e. t-test) and non-parametric (e.g. sign rank test) statistics.²⁶

Our study's response rate, based on the 274 responses, 12% of all working email addresses (2,231) and 42% of click throughs (657 actually went to (i.e. clicked through) to the website). This response rate of 12% and 42% falls within the range of 7% to 44% rates documented in a large scale analysis of e-mail and internet based surveys found in Chapter 3 and Appendix B of Schonlau et al. (2002). After excluding the 22 participants with one or more items of missing data critical to our analysis and the 84 participants who failed our manipulation checks, our final sample size was 167 participants (252 when including all manipulation check failures with complete data sets).²⁷

Overall Tests of Hypotheses

Table 3, Panel A and Table 4 presents the ANCOVA and Repeated Measures ANCOVA representing omnibus tests of our hypotheses.²⁸ The overall ANCOVA for peer allocation in Table 3 Panel A is significant ($F=6.26$, $p < 0.0001$), with marginal significance for the CONDITION variable ($F=-2.03$, $p < 0.077$).²⁹ The Repeated Measures ANCOVA in Table 4 indicates that the "Peer vs. Self Allocations" variable significantly interacts with CONDITION ($F=2.67$, $p < 0.024$). As it is the predicted between-conditions differences that we are concerned about in our hypotheses, we follow Rosenthal et al

²⁶ There was no significant variation in responses across conditions (all p 's > 0.15) after controlling for the allocation decisions made, gender, age and ethical disposition.

²⁷ Other than the SALY condition and the critical inquiry plus extent condition where manipulation check failure rate was near 20%, the other four conditions lost roughly an equal proportion of participants ($\chi^2=2.92$, $p > 0.404$), near 37% on average. While one might expect that the SALY condition would have a lower manipulation check failure rate, (given it is the simplest scenario), the reason for the lower manipulation check failure rate in the critical inquiry plus extent condition compared to the other conditions is not readily apparent.

²⁸ As expected, a paired t-test indicated a significant difference between Self Allocation and Peer Allocation ($t = -6.55$, $p < 0.0001$), hence the need to carry out tests that reflected our a priori concerns for social desirability biases.

²⁹ All p values reported are two-tailed unless otherwise specified.

(2000, p. 1-3) and carry out the planned tests.

[Insert Table 3, 4, and 5 and Figure 2 Here]

Individual Tests of Hypotheses

Table 3 (Panels B and C) and Table 5 (Panels A and B) present the descriptive statistics (i.e., least squares means adjusted for covariates and raw means) for each condition as well as the specified comparisons based on our hypotheses. H1, which predicted that critical inquiry by itself would be effective at reducing malfeasance compared to the “same as last year” (SALY) condition, is not supported (Peer: one-tailed $p > 0.206$, Table 3 Panel C; and Peer versus Self (PVS): one-tailed $p > 0.143$, Table 5 Panel B) although the reduction in cost misallocation is in the correct direction (Peer: $\$989,300 - \$846,869 = \$142,431$, Table 3 Panel B; and PVS: $\$538,654 - \$340,835 = \$197,819$, Table 5 Panel A). H2, which predicted increasing the audit evidence sample size had no significant effect on reducing cost misallocation relative to the SALY condition (Peer: one-tailed $p > 0.200$; and PVS: one-tailed $p > 0.307$), and the directional effect was an increase rather than decrease of misallocation (Peer: $\$135,469$; and PVS: $\$86,728$). Hence, H2 is not supported.

H3 is supported in that changing the nature of the evidence being collected (from mainly internal documents and records to external confirmations) reduces cost misallocation compared to the SALY condition. As can be seen in Table 3 Panels B and C, H3 is supported with a reduction in the Nature condition versus SALY of $\$320,036$ (one-tailed $p < 0.036$) in misallocation using the Peer allocation dependent variable (Table 3 Panels B and C) and a reduction versus SALY of $\$440,541$ (one-tailed $p < 0.010$) in misallocation using the PVS dependent variable (in Table 5 Panels A and B).

H4, about the relative effects of Nature and Extent conditions, is also strongly supported in that changing the nature of the evidence collected is more effective at reducing questionable cost misallocation than increasing the extent of the evidence collected. Indeed, increasing Extent has no significant effect on the cost misallocation relative to the SALY condition, as per the results for H2. In contrast, for the change in Nature, there is a significant reduction in cost misallocation versus the SALY condition documented in the results for H3 as well as a significant reduction versus the Extent condition (Peer: \$455,505, one-tailed $p < 0.006$, Table 3 Panels B and C; and PVS: \$527,269, one-tailed $p < 0.003$, Table 5 Panels A and B).

H5a examines the incremental effect of adding critical inquiry to increasing Extent. Supporting H5a, CI+Extent reduced the amount of cost misallocation versus Extent alone (Peer: \$399,978, one-tailed $p < 0.010$, Table 3 Panels B and C; and PVS: \$438,473, one-tailed $p < 0.008$, Table 5 Panels A and B). Comparing the CI+Extent condition to the SALY condition, there also is a significant reduction in cost misallocation (Peer: \$264,509, one-tailed $p < 0.054$, Table 3 Panels B and C; and PVS: \$351,745, one-tailed $p < 0.023$, Table 5 Panels A and B). Thus, it appears that managers react more to the double impact of being questioned more critically and expanding the sample size than they do to expanding the sample size alone.

H5b examines the incremental effect of adding critical inquiry to a change in Nature of evidence collected. H5b is not supported (Peer: one-tailed $p > 0.364$; and PVS: one-tailed $p > 0.372$) as can be seen in Table 3 Panel C and Table 5 Panel B. However, we find that CI+Nature condition continues to be significantly more effective than the SALY condition at reducing cost misallocation (Peer: \$251,243, one-tailed $p < 0.083$, Table 3

Panels B and C; and PVS: \$371,780, one-tailed $p < 0.027$, Table 5 Panels A and B). This finding is consistent with the results for H3 where a change in Nature reduces cost misallocation relative to the SALY condition.

Given our high manipulation check failure rate, we also ran the analysis using the 252 managers for whom we had a complete set of data. H1, H2, and H5b continue not to be supported, while H3, H4 and H5a continue to be supported at similar rates of significance and dollar amounts reduction to those reported in the main results, **regardless** of whether one employs peer allocation or the repeated measures design of peer versus self allocation.

Additional Analyses

Proportions instead of dollar amounts as dependent variable. While Uecker et al (1981) and Cohen et al (2007) both focused their analyses on the amount of misreporting, Uecker et al also reported the proportion of managers who misreported was unchanged across cells. We also examine the proportion of managers who misreported using multivariate categorical ANOVA. Using the same control variables as in Table 3, we found that using a dichotomous dependent variable (misallocation versus no misallocation) results in the same overall conclusion, a marginally significant ($\chi^2(1)=9.77$, $p < 0.082$) overall effect for Condition. Using categorical ANOVAs to test the individual hypotheses, we found the similar directional effects and levels of significance in all cases. H1, H2, and H5b remain unsupported, whereas H3, H4, and H5a remain supported. For example, in the test of H3 contrasting SALY condition versus Nature condition, there is a significant reversal ($\chi^2(1)=4.42$, $p < 0.035$) in the proportion of participants carrying out any misreporting (57% misallocated to the other two projects in the SALY condition

versus 46% in the Nature condition). Hence, using proportion of managers misreporting results in similar results to our analysis of dollar amounts of dollars misallocated, and again supports the deterrence role of auditing.

Perceived professional scepticism. In developing our hypotheses, we assumed that managers would attribute the manipulations of audit action changes to heightened auditor professional scepticism. To check this attribution, we measure professional scepticism using the average of the responses to two questions developed based on the definition of professional scepticism in auditing standards (SAS 1, ISA Glossary of Terms 2008). Participants were asked to indicate the extent to which they feel that the external auditors have maintained “an attitude consistent with a questioning state of mind” and “an attitude consistent with a critical assessment of the evidence”.³⁰ Professional scepticism (on a 10 point scale with 1 being low and 10 being high) was significantly lower in the SALY condition (mean=4.49, std. dev.=2.12) than in the other five conditions (means range from 6.48 to 7.74) (all one-tailed p 's \leq 0.0001). In addition, professional scepticism in the CI+Nature condition (mean=7.74, std. dev.=1.83) was significantly higher than other conditions (all one-tailed p 's \leq 0.033) except for the Nature condition (one-tailed p >0.260). Hence, managers' attribution of audit action changes to heightened professional scepticism is consistent with the beliefs of regulators and standard setters about audit actions that should be taken to reflect heightened professional scepticism, which was an underlying assumption in our hypotheses.

Perceived probability of detection and punishment. We created a composite variable based on our post-experimental questions to test if audit action changes affect the

³⁰ Cronbach alpha, measure of the convergence of the two items, was α =0.86. An acceptable alpha value is generally taken to be 0.70 or higher (Nunnally and Bernstein 1994).

perceived probability of detection and punishment. The questions employed were the participant's belief about the likelihood the external auditors would examine project files in the participant's division, the likelihood that inappropriate cost allocation would be detected if that file were selected for audit, and four questions on the severity of the consequences of detection of inappropriate cost allocation being detected (career, social standing within the company and the broader community, and financial). We found that consistent with our expectations, conditions (3 to 6) resulted in a significant increase (all p 's <0.05) in the perceived probability of detection and punishment over the SALY condition, except for marginally significant increase in the case of CI (Condition 2, one-tailed $p < 0.071$). This finding is consistent with our deterrence theory assumptions.

Perceived suspicion. Based on interpersonal deception theory, we theorize in our hypotheses development section that managers would be sensitive to audit action changes and associate those changes with increased suspicion by the auditor. We also expect managers who contemplate engaging in fraudulent acts to perceive more suspicion by the auditor than managers who do not. We took those managers who actually made allocations to the other two projects as a proxy for those contemplating engaging in fraudulent acts. We asked all participants two questions about reasons for changes in auditor behaviour: first whether changes in auditor behaviour are due to increases in the auditor's suspicions about the company's financial reporting honesty; and second to provide a contrast, attributing the audit action changes to changes in the regulatory environment. Participants indicated their agreement on a 9 point scale with -4 being "strongly disagree" and +4 being "strongly agree". Overall, managers attributed audit action changes to the auditor's suspicion of managerial honesty, as well as to changes in

the regulatory environment, significantly more (all one-tail p 's <0.029) in the five treatment conditions (2 to 6) than the SALY condition. Further, managers' attribution of audit action changes to auditor's suspicions of managerial honesty significantly increases ($t=2.82$, one-tailed $p<0.003$) as the costs that managers' allocated to the other new projects increases (i.e., as the degree of committed malfeasance increases). In contrast, managers' attribution of audit action changes to changes in the regulatory environment is not related to the increases in costs that managers' allocated to other new projects ($t=-0.09$, one-tailed $p>0.465$). These results are consistent with interpersonal deception theory that underlies our hypotheses.

V. CONCLUSIONS

We find that when the auditor changes the nature of the evidence collected, managers reduce the amount of corporate malfeasance versus the SALY audit, even in a setting where there is no increase the objective likelihood that the manager would be caught. Changing the extent of the evidence collected has no effect in reducing manager's malfeasance vis-à-vis the SALY audit, however, the addition of critical inquiry to changing Extent is effective in reducing manager's malfeasance compared to the SALY audit and compared to changing the extent of the evidence collected. The results for changing the extent of the evidence collected are important as both archival and experimental research in auditing have shown that after doing nothing (i.e., rationalizing away the risk), expanding sample size is the most common audit practice response to perceptions of heightened risk and hence it is the action that regulators are presented with most often when they are reviewing files or carrying out post mortems on frauds. Finding that expanding sample size alone is not an effective deterrent of fraud raises concerns,

since similar concerns exist about the effectiveness of using expanded sample size alone to detect a fraud that has occurred. Managers do not usually commit fraud where they know the auditor will look (i.e. based on previous year's audits) or they prepare elaborate ruses to hide the fraud from same as last year audit tests (e.g., Hammersley et al. 2009).

Of course there are limitations to our research. First, we constructed a single period scenario so that we could observe directly the effects of changed audit actions on manager accounting behaviour in the current period. Ongoing deterrence by auditors in practice is likely a more complex phenomenon, but we believe we have captured essential elements for study. Second, we focus on senior managers in our research, but not a member of the C-suite (chief executive officer, chief financial officer, etc.) nor members of the accounting department. Most financial reporting fraud is carried out by members of the C-suite and the accounting department (Beasley et al 1999), although 30% of financial statement frauds reported by the Association of Certified Fraud Examiners are carried out by managers other than top executives and accountants (ACFE 2008 p. 58). However, it should be noted that no response differences were found between those who had a professional accounting designation (roughly 14% of the sample) and those that did not in the experiment. Further, we argue that it is widespread deterrence, beyond the C-suite and the accounting department, that the auditing profession has frequently claimed as a positive effect of audits but has provided no evidence to support. Third, we do not directly manipulate the auditor's professional scepticism and it can be argued that client specific issues generally happen that heighten auditor professional scepticism and cause changes in audit actions. However, in any large organization, not all senior managers would necessarily be aware of those events that precipitated increased auditor

professional scepticism, and furthermore it is unclear to us why manager's knowledge of those events would change the deterrence effects. Certainly, participants' debriefing question responses made it clear that they attributed the changes in audit actions to the underlying components (i.e., more questioning state of mind and more critical assessment of the evidence) that make up the definition of professional scepticism. Finally, the manipulations of changes in audit actions, especially in the conditions where we obtained no reaction, may not be strong enough despite their face validity. This lack of manipulation strength could also explain the high manipulation check failure rate. The reported results, however, were not affected by the manipulation failure rate as when all participants were included in the analysis, the results were unchanged.

There are several implications to be drawn from our research. First, and possibly most important, we have developed empirical evidence to support the long posited claim by auditors that the existence of an audit and changes in auditors' action can have a deterrence effect on managers who are under audit. Second, we find that stronger deterrence effects are obtained from the same auditor behaviour changes that are also said to be more effective at detecting fraud, i.e., changing the nature of the evidence collected. Third, it appears that the combination of more critical inquiry of management and extending the size of the current sample verges on having the same deterrence effects as changing the nature of the evidence collected. However, in this day and age of relationship management between audit partners and client management (see McCracken et al. 2008), it is difficult to see this strategy readily being employed by audit firms who increasingly want to be "trusted business advisors" to their clients. In fact, it is an ongoing concern of regulators that auditors do not "sufficiently ... challenge

management's forecasts, views, or representations" which raise "concerns about the sufficiency of firms' application of professional skepticism and objectivity in some audits." (PCAOB 2008-008, p. 20).

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FIGURE 1: THEORETICAL MODEL
Management Attributions and Responses to
Audit Action Changes

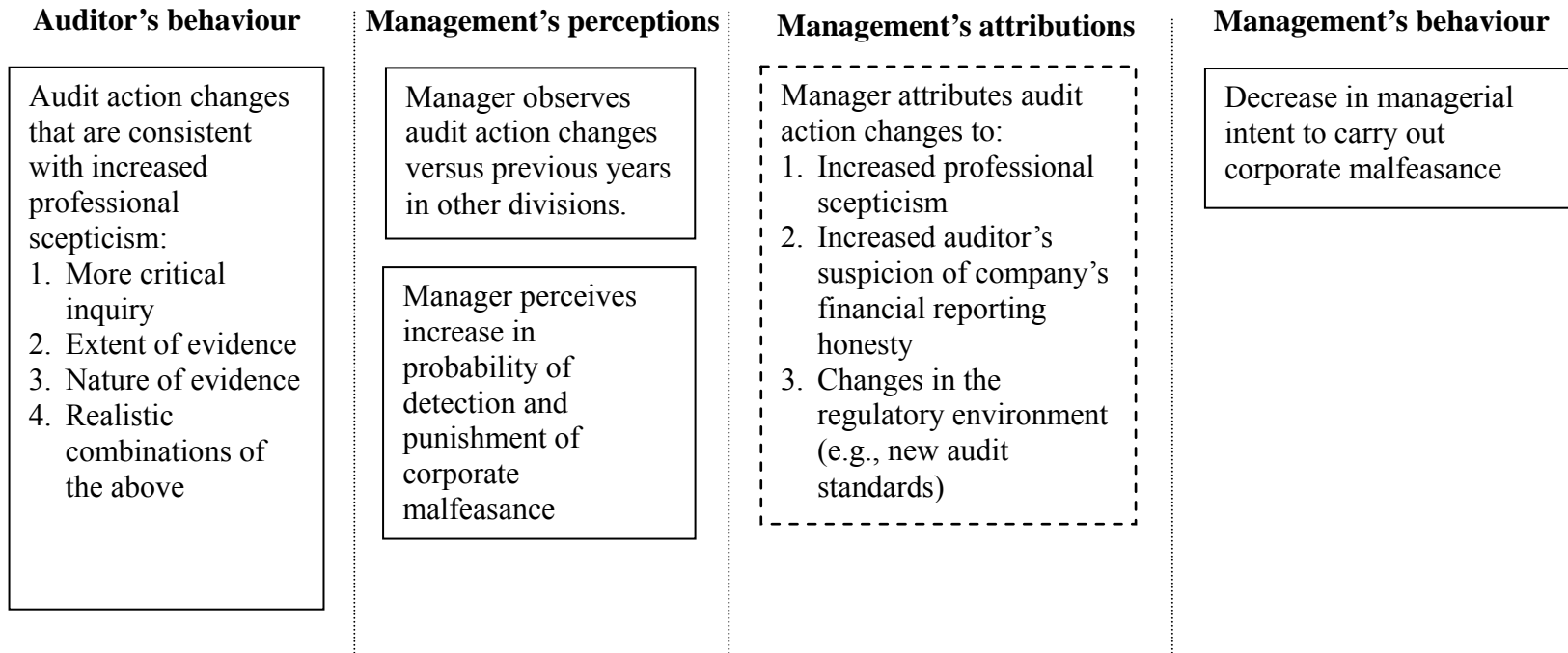
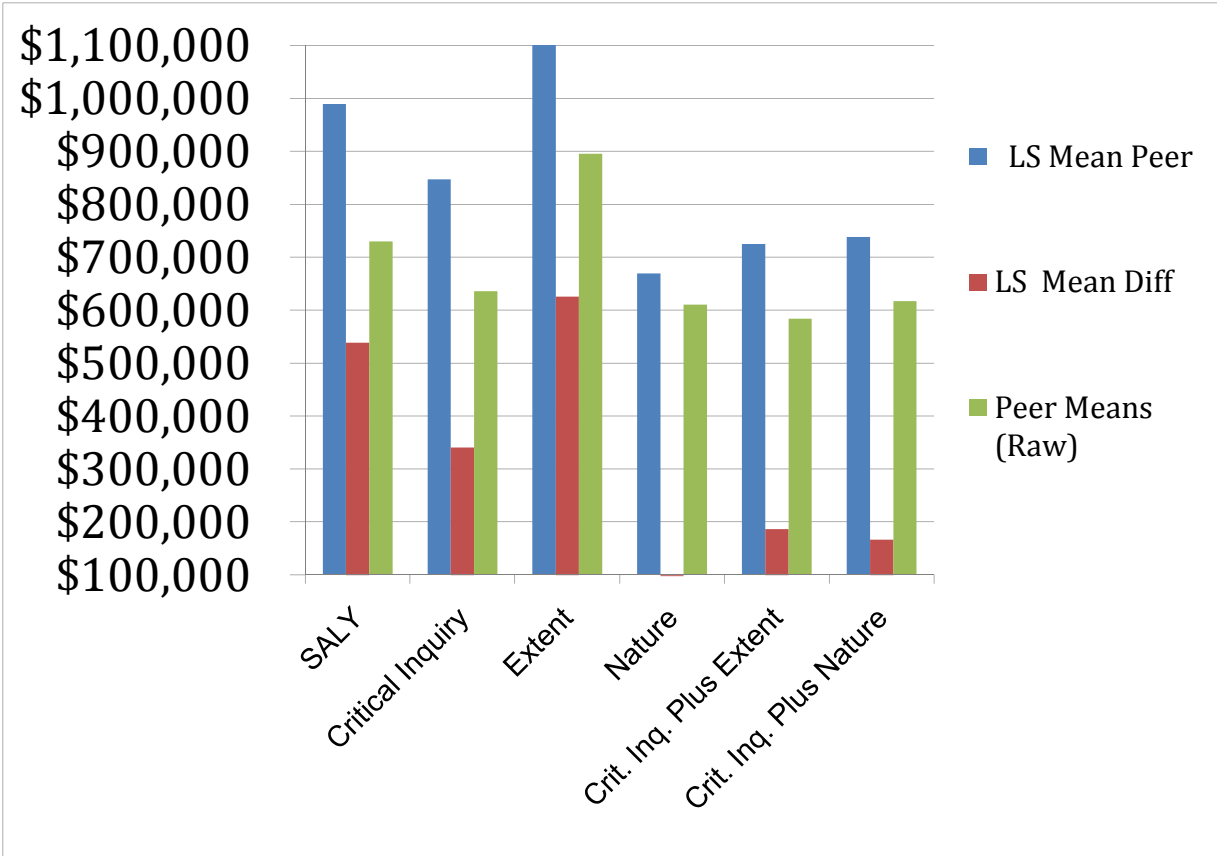


FIGURE 2: MEAN PARTICIPANT ALLOCATION OF COSTS TO OTHER PROJECTS



Notes:

LS Mean Peer: The least squares adjusted mean response to the question “If members of your peer group (e.g. fellow alumni) were making the allocation decision, how much of the \$2 million Technical Improvement R&D expenditure do you think they would likely charge to the new projects?” adjusted for Ethical, Age, and Self Allocation Other Projects (per Table 3 Panel A) and tabulated in Table 3 Panel B.

LS Mean Diff: The least squares adjusted mean difference between the responses to the peer and self version of the allocation question adjusted for Age and Ethical (per Table 4) and tabulated in Table 5 Panel A.

Peer Means [Raw]: The unadjusted mean responses to the above question on peer’s allocation.

TABLE 1
Experimental Design

Panel A: Between-Subjects Independent Variable Manipulations

Nature of Management Inquiry	Extent and Nature of Audit		
	Same as last year	Increase in audit evidence sample size	Change in nature of audit evidence
Same as last year	Condition 1 SALY	Condition 3 Extent	Condition 4 Nature
More critical inquiry	Condition 2 CI	Condition 5 CI + Extent	Condition 6 CI + Nature

Panel B: Dependent Variable Questions^a

1. Self Allocation

1. As Sue Davies, how much of the \$2 million Technical Improvement R&D expenditure would you likely charge to the new projects? (the unit "m" represents "millions")

|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|
 \$0 \$0.25m \$0.5m \$0.75m \$1.0m \$1.25m \$1.5m \$1.75m \$2.0m

Dollar allocation: \$ _____

2. Peer Allocation

2. If members of your peer group (e.g. fellow alumni) were making the allocation decision, how much of the \$2 million Technical Improvement R&D expenditure do you think they would likely charge to the new projects? (the unit "m" represents "millions")

|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|-----|
 \$0 \$0.25m \$0.5m \$0.75m \$1.0m \$1.25m \$1.5m \$1.75m \$2.0m

Dollar allocation: \$ _____

Note:

^aThis is the basis of the within-subjects independent variable (peer vs. self allocations denoted PVS) in the repeated measures design. See Table 4.

TABLE 2**Manager Participant Profile (N = 167)**

		Frequency
Age	26-40	38.6%
	41-50	44.6%
	51-60	15.7%
	61-70	<1%
	over 70	<1%
Years of managerial experience	Mean = 13.7 years Std. dev. = 7.9 years	Upper Quartile = 20 years Median = 12 years Lower Quartile = 8 years
Gender (% female)		15.0% ^a
Canada as country of birth		75.4%
Education		100% MBA ^b
Accounting Designation		14.5%
Lawyer or attorney		2.4%
Employment profile	Public company	43.1%
	Large private company	9.6%
	Medium or small private company	26.4%
	Government or not-for-profit organization	16.8%
	Other	4.2%

Notes:

^a Low female recruitment in technology-oriented full-time MBA program and in general executive MBA programs.

^b Consistent with drawing participants from an MBA only e-mail list.

TABLE 3

Amount Peers Would Allocate To New Projects (Peer Allocation) As Dependent Variable

Panel A: Analysis of Covariance (ANCOVA)

Source	DF	Sum of Squares ^a	Mean Square ^a	F Value	Pr > F
Condition ^b	5	4.31	0.86	2.03	0.077
Ethical ^c	1	0.10	0.10	0.23	0.634
Age ^d	4	2.81	0.70	1.65	0.164
Self allocation - other projects ^e	1	19.22	19.22	45.31	<0.0001
Error	154	65.33	0.424		

Notes:

^a All sum of squares and mean squares are in ‘000,000,000,000.

^b Condition is the experimental treatment as described in Table 1 Panel A.

^c Ethical is a measure of the ethical disposition of participants; see question in the Appendix question.

^d Age is as measured and reported in Table 2.

^e Self allocation: amount participant reported they personally would misallocate to other projects.

Panel B: Least Squares Means and Raw Means (Standard Deviations) of Peer Allocation

Condition (Table 1 Panel A)	N	LS Mean ^a	Mean (Std. Dev.)
Same as last year (SALY)	37	\$989,300	\$729,730 (780,354)
Critical Inquiry (CI)	24	\$846,869	\$635,833 (731,698)
Extent	32	\$1,124,769	\$895,000 (769,776)
Nature	24	\$669,264	\$610,417 (779,629)
CI + Extent	29	\$724,791	\$583,448 (762,151)
CI + Nature	21	\$738,057	\$617,143 (695,465)

Note:

^a LS Mean: Least squares mean adjusted for other variables in ANCOVA (Ethical, Age, and Self allocation).

Panel C: P-values Associated with All Pair-wise Comparisons of Least Squares Means of Peer Allocation^a

Condition	SALY	CI	Extent	Nature	CI + Extent
CI	0.206 H1				
Extent	0.200 H2	0.060			
Nature	0.036 H3	0.177	0.006 H4		
CI + Extent	0.054	0.251	0.010 H5a	0.381	
CI + Nature	0.083	0.289	0.019	0.364 H5b	0.472

Note:

^a All p-values are one tailed in keeping with the directional nature of hypotheses.

TABLE 4**Repeated Measures Analysis of Covariance (ANCOVA) for Amounts Participant Reports Peers and Self Would Allocate to Other New Projects (Peer Allocation and Self Allocation) as Dependent Variables**Between-Subjects Effects

Source	DF	Sum of Squares^a	Mean Square^a	F Value	Pr > F
Condition	5	1,021	204	0.30	0.910
Ethical	1	7,261	7,261	10.84	0.001
Age	4	9,024	2,256	3.37	0.0113
Error	155	103,827	670		

Within-Subject Effects

Source	DF	Sum of Squares^a	Mean Square^a	F Value	Pr > F
Peer vs. Self allocations (PVS) ^b	1	1,146	1,146	4.72	0.031
PVS*Condition	5	3,247	646	2.67	0.024
PVS*Ethical	1	1,017	1,017	4.19	0.043
PVS*Age	4	991	248	1.02	0.399
Error	155	37,663	243		

Notes:

^a All sum of squares and mean squares are in '000,000,000.^b PVS is a within subjects variable based on repeated measures of the same construct (Peer Allocation and Self Allocation). See Figure 2. All other variables are as defined in Table 3.

TABLE 5

Difference Between Amounts Peers and Self Would Allocate to New Projects (Peer Allocation less Self Allocation) as Dependent Variable

Panel A: Least Squares Means and Raw Means (Standard Deviations) of Difference in Peer Allocation less Self Allocation

Condition^a	N	LS Mean^b	Raw Mean (Std. Dev.)
Same as last year (SALY)	37	\$538,654	\$513,514 (663,916)
Critical Inquiry (CI)	24	\$340,835	\$396,250 (829,111)
Extent	32	\$625,382	\$629,375 (841,492)
Nature	24	\$98,113	\$125,000 (733,514)
CI + Extent	29	\$186,909	\$181,034 (490,520)
CI + Nature	21	\$166,874	\$188,571 (593,711)

Notes:

^a All conditions as defined in Table 1.

^b LS Mean: Least squares mean adjusted for other variables (Age and Ethical) in ANCOVA.

Panel B: P-values Associated with All Pair-wise Comparisons of LS Means of Difference in Peer Allocation less Self Allocation^a

Condition	SALY	CI	Extent	Nature	CI + Extent
CI	0.143 H1				
Extent	0.307 H2	0.069			
Nature	0.010 H3	0.118	0.003 H4		
CI + Extent	0.023	0.214	0.008 H5a	0.325	
CI + Nature	0.027	0.203	0.011	0.372 H5b	0.460

Note:

^a All p-values are one-tailed in keeping with directional nature of hypotheses.

APPENDIX – Ethics Questionnaire

(Explanations in *italics* were not in the participant version and the items were randomly ordered)

Ignoring your own answer to the case, suppose that Sue Davies charged the entire \$2 million Technical Improvement R&D expenditure to the new projects. Please indicate your views about this action on the following scales by choosing a value on the scale closest to the term that best describes your reaction to the statement at the top.

Charging the \$2 million to the new projects is:

Moral equity dimension

Just								Unjust
	1	2	3	4	5	6	7	8
Unfair								Fair
	1	2	3	4	5	6	7	8
Morally right								Not morally right
	1	2	3	4	5	6	7	8

Relativist dimension

Not Acceptable to my family								Acceptable to my family
	1	2	3	4	5	6	7	8
Culturally acceptable								Culturally unacceptable
	1	2	3	4	5	6	7	8
Traditionally unacceptable								Traditionally acceptable
	1	2	3	4	5	6	7	8

Utilitarian dimension

Produces the greatest utility								Produces the least utility
	1	2	3	4	5	6	7	8
Minimizes benefits while maximizes harm								Maximizes benefits while minimizes harm
	1	2	3	4	5	6	7	8

Contractual dimension

Does not violate an unwritten contract								Violates an unwritten contract
	1	2	3	4	5	6	7	8
Violates an unspoken promise								Does not violate an unspoken promise
	1	2	3	4	5	6	7	8

The probability that **I** would undertake the **same action** is:

Low								High
	1	2	3	4	5	6	7	8

The probability that members of **my peer group** (e.g. fellow alumni) would undertake the **same action** is:

Low								High
	1	2	3	4	5	6	7	8

Ethical (recoded such that 8 is ethical and 1 is unethical)-Used in analysis as control variable

The action described above is:

Ethical								Unethical
	1	2	3	4	5	6	7	8