

The Effectiveness of Alternative Risk Assessment and Program Planning Tools in a
Fraud Setting*

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Abstract

This study examines the impact of alternative risk assessment (standard risk checklist versus no checklist) and program development (standard program versus no program) tools on two facets of fraud planning effectiveness: (a) the quality of audit procedures relative to a benchmark validated by a panel of experts; and (b) the propensity to consult fraud experts. A between-subjects experiment, using an SEC enforcement fraud case, was conducted to examine these relationships. Sixty-nine auditors made risk assessments and designed an audit program. We found that auditors who used a standard risk checklist, structured by SAS 82 risk categories, made lower risk assessments than those without a checklist. This suggests that the use of the checklist was associated with a less effective diagnosis of the fraud. We also found that auditors with a standard audit program designed a relatively less effective fraud program than those without this tool but were not more willing to seek consultation with fraud experts. This suggests that standard programs may impair auditors' ability to respond to fraud risk. Finally, our results show that fraud risk assessment (FRASK) was not associated with the planning of more effective fraud procedures but was directly associated with the desire to consult with fraud specialists. This suggests that one benefit of improved FRASK is its relation with consultation. Overall, the findings call into question the effectiveness of standard audit tools in a fraud setting and highlight the need for a more strategic reasoning approach in an elevated risk situation.

Key Words: Fraud risk assessments, Standard audit programs, Audit planning, Fraud detection.

JEL Descriptors: M420, M400

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I. Introduction

Auditors are required to make fraud risk assessments (hereafter FRASK) as a basis for designing an audit plan that provides reasonable assurance of detecting material fraud in corporate financial statements (AICPA 1999, AU 316.12; AICPA 2002b). The design of the audit plan includes the choice of audit procedures (AICPA 1999, AU 316.28) as well as the decision to consult with fraud experts (AICPA 1999, AU 316.27; Salterio 1994). Accounting firms use alternative decision tools to aid in the FRASK (e.g., a standard risk checklist versus no risk checklist) and plan development phases (e.g., standard audit program versus no audit program).¹

This study examines the effect of these alternative tools on the effectiveness of auditors' fraud detection plans. We focus on two facets of effectiveness: (a) the quality of audit procedures relative to a benchmark developed from an SEC/AAER and validated by a panel of experts; and (b) the propensity to consult with fraud experts. In an experiment, 69 auditors were provided a realistic case based on an SEC/AAER involving a fraud perpetrated by inflating revenues (SEC 1998). Auditors received one of four versions of the case that vary by whether a standard risk checklist is present or absent and by whether a standard audit program is provided or not provided. Auditors made risk assessments and designed an audit program for the revenue cycle. Both the standard risk checklist and the standard audit program were based on professional standards and current practice.

The results show that auditors with a standard audit program designed a relatively less effective fraud program than those without this tool, providing the first direct evidence that standard programs can impair the ability to design fraud tests. Further, the audit program manipulation did not affect the propensity to consult. Overall, auditors

designed few effective fraud tests relative to the benchmark, suggesting that more fraud training is warranted. The extensive examples of appropriate responses to selected fraud risks (AICPA 2002a, 2002b) included in SAS 99 appear justified based on our results. Additionally, further training on the development of non-standard tests to address specific fraud risks appears promising.

We found that auditors who used a standard risk checklist made lower risk assessments than those without a risk checklist. This corroborates and extends the findings of Pincus (1989), whose auditors used an uncategorized risk checklist and were from an era where fraud detection was not an affirmative responsibility. Further, we found no significant relationship between FRASK and program effectiveness. However, a higher FRASK was associated with a greater tendency to seek the consultation of fraud specialists. Thus, the benefit of FRASK appears to lie in its relation with consultation. This finding underscores the importance of research that considers the complete action space available to auditors.

The remainder of this paper is divided into four sections. In the next section, we analyze relevant literature and formulate the research hypotheses. This section is followed by the method, presentation of the findings and the conclusions.

2. Literature analysis and research hypotheses

Background and research framework

The investing public relies on auditors to detect fraudulent financial reporting, which, while rare in occurrence, can have catastrophic consequences when it occurs (Albrecht et al. 2001; Hooks et al. 1994, Loebbecke et al. 1989). In spite of a professional obligation to detect fraud, auditors have very little specific formal training in this area and have limited experiential knowledge, due to the rarity of fraud (Nieschwietz et al.

2000; Jaenicke and Wright 1993). Audit firms, aware of this difficulty, provide various decision tools to lessen the burden on individual auditors. For instance, some firms provide a standard risk checklist as decision support in the FRASK phase while others encourage a listing of critical risk factors (Shelton et al. 2001, Mock and Turner 2001). Similarly, some firms provide a standard audit program as a starting point for developing fraud procedures while others use a blank template. Another important component of the decision-aiding resources widely made available to auditors is the ability to consult with auditors who specialize in fraud detection.

Figure 1 presents our research framework, which shows that auditors can use a standard risk checklist or a risk-listing approach (i.e., no risk checklist) to aid FRASK. The framework does not show a linkage between risk assessment tools and audit responses because we propose that FRASK mediates the effects of risk tools. That is, we expect the effect of these tools to be impounded into FRASK. Subsequent to FRASK, the auditor responds by revising a standard audit program or developing a program and then makes a determination of whether consultation is necessary. We define the effectiveness of fraud detection responses in terms of the quality of the procedures and the propensity to consult.²

The Effect of Standard Risk Checklists on FRASK

Standard risk checklists are widely used in practice and are typically built around the three broad categories of risk factors (red flags) identified in SAS 82 (Shelton et al. 2001; Mock and Turner 2001). The checklists are meant to facilitate FRASK by including a comprehensive list of factors identified from prior frauds. However, prior research suggests three factors may negate this benefit.

First, a checklist that is not organized by categories may prevent the auditor from seeing a pattern of risks suggestive of fraud. In effect, the checklist may be a hindrance to developing a “coherent story.” While current risk checklists are organized along the three categories suggested in SAS 82, these categorizations are not likely to facilitate FRASK, since they are not linked to form a clear picture of how fraud may be perpetrated and concealed.³ Second, auditors are expected to process risk factors less deeply when they check boxes on a checklist relative to when they are required to specifically identify and list risk factors. This is supported by prior research that shows that aided decision makers reduce the amount of effort required to complete tasks (Todd and Benbasat 1992) and are unable to reason strategically when reviewing a laundry list of cues (Wilks and Zimbelman 2002a). Third, a standard risk checklist may lead to “interference effects” (e.g., Bedard and Biggs 1991; Heiman-Hoffman et al. 1995) by inhibiting the auditor from recognizing the presence of unique risks for a particular client.

In contrast, if auditors are asked to use an open-ended form to identify risk factors, we expect them to process risk factors more deeply, better link these risk factors to fraudulent reporting and be freer from interference effects. We are aware of only two studies that have examined this issue. Pincus (1989) found that an uncategorized checklist resulted in less reliable FRASK for actual fraud cases than a no checklist condition. However, the ability to generalize from her study is limited because the research was conducted at a time when auditors had no affirmative responsibility to assess FRASK or detect fraud. Further, she examined the effect of a lengthy checklist without risk categories. Categorizing risks is a form of decomposition, which may facilitate more accurate FRASK (Zimbelman 1997, Wilks and Zimbelman 2002b).⁴

Mock and Turner (2001) found that auditors who used a checklist identified new risk factors in only a very few instances. Further, the longer the checklist, the more risk factors that were checked off. In contrast, auditors using open-ended forms (i.e., no structured checklist) identified the largest number of risk factors. However, Mock and Turner (2001) did not obtain direct evidence of auditors' FRASK. Based on our theoretical and empirical analysis, we pose the following hypothesis:

HYPOTHESIS 1. *In a fraud situation, auditors with no checklist will assess higher fraud risks than those with a standard risk checklist.*

The Effect of FRASK on Audit Plans

Responding effectively to FRASK appears to be the most challenging phase for practitioners (AICPA 2002a, p. 14) and has a direct impact on audit effectiveness. There are theoretical, practical and empirical foundations for expecting a linkage between FRASK and auditors' responses. Research in psychology and auditing suggests that decomposition increases accuracy by focusing decision-makers' attention to the component task (Kleinmuntz et al. 1996). Therefore, one would expect that explicit FRASK would lead auditors to develop audit plans that vary with FRASK (Zimbelman 1997). At the practical level, both the audit risk model and audit standards suggest that auditors should vary their audit plans in response to FRASK (AICPA 1988; 1997; 2002a, 2002b). Indeed, the maintained advantage of making a separate FRASK is that it focuses the auditor on the potential presence of fraud, which should lead to the selection of effective fraud procedures (Zimbelman 1997).

The realization of this advantage, however, depends on how and whether auditors are able to respond to an elevated FRASK. For instance, auditors may not be able to

engage in the high-order strategic reasoning needed for an effective response to risk (Zimbelman and Waller 1999; Wilks and Zimbelman 2002a). Further, the rarity of fraud may make it difficult to develop and refine the knowledge necessary for designing effective fraud procedures.

While several prior studies have examined the association between risk and audit plans, they focused primarily on non-fraud environments (e.g., Mock and Wright 1993; Mock and Wright 1999). Some of these earlier studies found a weak association (e.g., Mock and Wright 1993; DiPietro et al. 1994; Quadackers et al. 1996) but more recent studies found a stronger linkage between some client risk factors and audit responses (e.g., O’Keefe et al. 1994; Davidson and Gist 1996, Hackenbrack and Knechel 1997; Mock and Wright 1999). The absence of a linkage in a non-fraud setting may be justified to the extent that a robust standard audit program may cover all relevant audit assertions. This makes it difficult to generalize these results to the fraud environment, where there is strategic deception by the client and where an absence of a linkage may be *prima facie* problematic.

Two experimental studies (Zimbelman 1997; Glover et al. forthcoming) do not find a strong association between FRASK and the nature of planned tests. However, both studies constrain auditors to revise a closed set of procedures and hence did not provide an opportunity to add additional procedures. Responding appropriately to fraud risk may often require auditors to design tailored tests beyond those contained in a standard program (Nieschwietz et al. 2000). Moreover, Glover et al. (forthcoming) report that post-SAS no. 82 auditors are more aware of the need to modify audit plans and increased the extent of their audit tests in response to increased FRASK. Finally, anecdotal

evidence on a number of prior undetected fraud cases (e.g., Lincoln Savings and Loan Association (Mayhew et al. 1999) and Crazy Eddie Inc. (Knapp 1996) suggests that auditors respond to high fraud risk by merely expanding the scope of standard, traditional tests rather than designing effective fraud-directed procedures.

We provide a stronger test of the association between FRASK and audit procedures by allowing auditors to add as many procedures as they consider relevant. Although the evidence on whether auditors develop risk-adjusted program plans is mixed, we posit the quality of tests will be related to FRASK. This expectation is based on the more recent evidence that audit plans are responsive to variations in inherent risk (e.g., Mock and Wright 1999), the finding that post-SAS 82 auditors are more aware of the need to modify their audit plans (Glover et al. forthcoming), and our enhanced research design.

HYPOTHESIS 2. There is a positive association between auditors' fraud risk assessments and fraud program effectiveness.

It is commonplace among the major auditing firms to have a specialist group of auditors who have a work background and training in fraud prevention and detection. Such experts, if necessary, may be called upon to assist auditors to design an effective set of fraud tests. Therefore, consultation is an important component of the action set available to auditors and can enhance justifiability (Kennedy et al. 1997). This association is particularly important if auditors can identify an elevated fraud risk situation but are unable to modify their plans appropriately.

Two theoretical perspectives are relevant in considering the association between FRASK and consultation. First, because bringing in specialists inevitably results in

additional delays and costs, auditors will seek consultation only when they believe that they cannot address the elevated risks with their own audit plans. Prior research suggests that decision-makers tend to be over-confident in their abilities and decisions, a tendency that is perpetuated by a fraud detection environment, which is characterized by outcome irrelevant learning structures (Einhorn 1980). Second, as fraud risks are assessed to be higher, an auditor is expected to be more likely to recognize the need for bringing in fraud specialists. Accordingly, we pose the following hypothesis:

HYPOTHESIS 3: There is a positive association between FRASK and the desire to consult with fraud experts.

The Effect of Standard Audit Programs on Audit Plans

One advantage of a standard program is that it reduces the cognitive cost of generating a complete program and frees cognitive resources for the auditor to focus on revising the program. Further, because standard programs are designed to anticipate specific features of the typical audit environment, they tend to be comprehensive and enhance consensus. Finally, some tests have been added to the standard program, because with hindsight they would have been effective in detecting a particular fraud (e.g., observation of inventory in response to the classic McKesson and Robbins case (SEC Financial reporting Release No. 1; AU 331, AICPA 1999).

Against these advantages are various concerns derived from two lines of literature. First, in basic research, decision aids have been found to promote an action-oriented strategy (i.e., a desire to “act first” rather than thinking before acting) and lead to effort minimization (Todd and Benbasat 1992; Kottemann et al. 1994). Similarly, Cushing and Loebbecke (1986) argue that decision aids promote mechanistic processing

and inflexibility. An action-oriented strategy is likely to inhibit an auditor's ability to reason strategically (Wilks and Zimbelman 2002a). Further, effort minimization strategies, such as anchoring and adjustment, may impair auditors' ability to propose unique, tailored tests that may be called for in a fraud situation (Heiman-Hoffman et al. 1995; Frederick 1991). Second, empirical evidence in psychology and auditing suggests that "prompts" have an "interference effect" and impair a decision makers' ability to think about alternatives (e.g., Asare et al. 1998; Bell and Wright 1997; Heiman-Hoffman et al. 1995). In the same vein, the standard audit program may inhibit the auditor from identifying procedures that address the unique fraud risks present in a particular setting.⁵

In contrast, a "zero-based" program avoids potential interference effects, encourages greater cognitive processing, and may facilitate more creative and strategic thinking. We are unaware of any study that directly compares the effects of a standard program to a zero-based program. However, several prior archival studies indicate that auditors tend to plan the same tests from year to year, referred to as a "SALY" (same as last year) heuristic (e.g., Bedard 1989; Mock and Wright 1993, 1999). It is important to note that these studies do not examine program changes in response to FRASK. Standard programs are designed to address all audit assertions and unless there are indications of fraud, SALY may be an appropriate, albeit inefficient, heuristic. As noted previously, two experimental studies (Glover et al. forthcoming; Zimbelman 1997) suggest that auditors tend to expand existing procedures rather than design new relevant tests. However, these studies do not address the relative effectiveness of alternative program development approaches. Nor did they present auditors a clear opportunity to design new procedures.

Based on the findings from basic research that suggest the standard program will promote an action-oriented strategy and interference effects, we posit that auditors without the standard program will design more effective fraud procedures than those with a standard audit program.

HYPOTHESIS 4: Auditors without a standard program will develop a more effective set of fraud tests than those with a standard program.

In basic research, decision aids have been found to create an illusion of control, which leads decision makers to overestimate their effectiveness (Kottemann et al. 1994) and thereby reduce the propensity to seek consultation. The psychology literature reveals that consultation is obtained to enhance justification and confidence (Heath and Gonzales 1995). This suggests that auditors using the more structured standard program are less likely to seek consultation. Further, because the zero-based program requires significant retrieval and cognitive costs of processing, it can decrease confidence and thereby lead to a higher propensity to consult. The discussion above leads to the final research hypothesis:

HYPOTHESIS 5: Auditors without a standard program are more likely to seek consultation than those with a standard program.

3. Method

Experimental task

Participants completed an audit case based on an actual company that had issued fraudulent financial statements. The company materially overstated its net income by improperly recognizing revenue to its distributors less than two weeks before the fiscal year-end in connection with a marketing program that effectively resulted in consignment

sales. As a result of the fraud, the company had been investigated by the SEC and was forced to restate the financial statements in a subsequent year. The fraudulent financial statements were obtained from the company's annual report of the fraud year. The Management Discussion & Analysis and other parts of the annual report provided materials for the background information in the case and the SEC enforcement release provided additional information for setting up the fraud scenario.

The case, which was developed with the assistance of three engagement partners identified by a liaison for the ASB task force, had three sections.⁶ In the first section, participants were asked to assume that they had been assigned to finalize the audit program for the revenue cycle of a client. The client was described as a publicly traded manufacturer that was undergoing substantial changes and facing major strategic challenges. The client's business had historically been centered upon the sale of analog instruments and it had been slow in entering the emerging and growing digital market. A primary challenge was to devote increased efforts to the digital segment while also maximizing its traditional analog sales that continued to account for 70 percent of its revenues.

The participants were then presented comparative financial statements including the unaudited statements, which were the fraudulent ones issued by the disguised company. The control environment was described as positive with the audit committee and board members meeting regularly. The control system was described as reliable in recording routine transactions, and the president was described as maintaining a high degree of control over the reporting of results. Top management was compensated through a combination of salary, an earnings-based bonus plan, and stock options.

Further, management placed great importance on achieving or exceeding sales forecasts, which it had accomplished for the prior 12 consecutive quarters. Based on widely used guidelines and discussions with the three audit partners who helped develop the case, preliminary materiality was set at about 5 percent of net income. The industry analysis indicated a moderation in growth rates.

Participants were then provided an overview of the revenue cycle and were told that prior year's work, as well as testing in the interim period (completed in September), confirmed that computer and manual controls were in place and effective. Further, roll forward tests confirmed the controls remained effective. The only change since interim was implementation of a marketing program that increased revenue by \$22 million and net income by \$9 million (275 percent and 113 percent of planning materiality respectively).

The marketing strategy that was the basis of the fraud was also described. Specifically, participants learned that distributors were given the primary sales responsibility for analog products freeing the company to focus its marketing efforts on the digital market. The key element of this strategy was a marketing program implemented in November to provide distributors with incentives to buy analog products. As part of the portfolio of incentives, distributors were given access to large retail accounts and key customers previously serviced directly by the client, profit sharing opportunities, the "Premier Digital Program,"⁷ favorable financing terms, and warehousing and storage incentives for distributors with warehousing problems. The marketing initiative was described as largely successful with distributors signing up for large orders of analog systems. About 70 percent of the distributors signed immediately

with the rest being undecided. Follow-ups with the undecided distributors proved successful with only 4 not signing by year-end. Top management approved the credit limit increases based upon the potential strategic benefits of the Program.

After reading the market strategy, participants completed either a risk checklist (standard checklist condition) or were asked to list as many specific risk factors that came to their attention, which significantly heighten the likelihood of the presence of material unintentional or intentional misstatements. The checklist included 26 questions (see Exhibit 1) adapted from firm manuals and professional standards falling into three categories: management's characteristics and influence over the control environment; risk factors relating to industry conditions; and risk factors relating to operating characteristics and financial stability. After the checklist manipulation, participants were asked to assess inherent risk, control risk and fraud risk (defined as the risk of intentional misstatement) on a 10-point scale where 1 was identified as low risk and 10 as high risk.⁸

In the second part of the case, participants were asked to design or finalize the audit program for the revenue cycle. Depending on experimental condition, participants were provided with an audit program (standard program condition) or were asked to determine the tests to be conducted (no program condition). The standard audit program was compiled from firm materials and auditing textbooks regarding common tests performed in the revenue cycle (see exhibit 2).

Participants in the standard program condition were told that the program was based on last year and information up to the interim period. Their task was to review and update the program based on current circumstances. They were reminded that, as in practice, they are free to change the program plan in any way, either increasing or

decreasing the nature of tests. That is, they were free to add (or delete) as many additional procedures they believed necessary for the firm to achieve its desired audit risk.

Participants completed the task by placing a check mark by all procedures they felt were necessary and listing additional procedures. The participants in the no program condition were simply told to provide a complete program for the revenue cycle that will allow the firm to achieve its desired audit risk.

Finally, participants were asked to provide an opinion on the necessity of conferring with a risk management partner to finalize the proposed plan. To provide a context for this judgment, which was elicited on a 10-point scale (1=no necessity to consult, 10=high necessity to consult), they were asked:

“to assume that the engagement partner is interested in knowing your level of confidence that the proposed audit program addresses the firm's exposure in detecting material fraud that may be present. Therefore, he wants your opinion on the necessity of conferring with a risk management partner to finalize the proposed plan. The risk management partner is very experienced with fraud risk situations. Given the need for efficient audits, risk management partners are not consulted on every engagement but rather when the engagement team believes its audit program may not reduce the risk of detecting fraud to a tolerable level.”

In the third and final part of the experiment, subjects provided demographic information.

Participants

Three Big Five firms provided 74 participants with a mean (σ) of 9.7 years (5.1).⁹ Five individuals did not complete significant portions of the case and were excluded resulting in a final sample of 69. Contact persons identified by the AICPA, the sponsor for the project, distributed the cases to participants who have substantial experience in program planning. The participants indicated that they had been involved in a mean of 3.46 ($\sigma=3.67$) and .25 ($\sigma=.63$) audits in the last five years on which material errors and fraud were detected respectively. Mean years of audit experience did not vary by

treatment ($F = 0.25$; $p = 0.86$). One hundred and seventeen cases were initially distributed with a response rate of 63 percent (74/117).

Fraud Program Effectiveness

The SEC/AAER described the nature of the fraud and why the audit was alleged to have been deficient. For instance, in describing the meeting at which the program was presented to the distributors, the AAER notes that some of the distributors refused to participate in the program even though participation was required to maintain an authorized distributorship. Further, the AAER suggests that others who agreed to participate changed their minds at a later time. We reasoned, as implied by the AAER, that understanding of the “strong-arm” tactics applied, which nevertheless was rejected by some dealers, was critical to appreciating the deception.

The AAER also notes that management extended credit limits significantly without analyzing creditworthiness and the ability to pay; granted many distributors the right to return unsold products; offered storage facilities for distributors with limited warehousing capacity; and could not enforce payments when distributors defaulted. We reasoned, as insinuated by the AAER, that pursuing these issues would have exposed the transaction as a sham (consignment sales which should not be recognized as revenue).

Based on the SEC/AAER, we developed a preliminary benchmark program. We also provided another list of procedures made up of procedures from the standard program and the benchmark program and asked our three experienced liaisons to rate the effectiveness of each procedure in detecting the fraud that was described in the SEC/AAER. In addition, they could add any procedures that they thought would have been effective in detecting the fraud. After three rounds using a Delphi approach, we

obtained a finalized benchmark program, which is shown in Exhibit 3. It can be seen that the procedures are linked to the SEC/AAER: Procedures numbered 1 to 5 address the meeting with distributors; procedure 6 addresses the right of return; procedures 7 and 8 investigate the warehousing issues; procedures 9 and 13 address the override of the credit granting system; and procedure 12 relates to lack of enforcement in case of dealer default.

Two individuals independently coded participants' programs using the benchmark program. Coding was done on a blind basis without their knowledge of the research hypotheses or the participant's experimental condition. The coders had eight and four years of auditing experience respectively. A score of fraud program effectiveness was compiled for each participant based on the number of procedures identified in the benchmark program. The correlation between the coders' in effectiveness scores was 0.8643 ($p < .001$) with a Cronbach Alpha of 0.9062 (generally a score of .80 is considered to represent an acceptable level of agreement reliability). Thus, there is evidence of strong inter-coder concurrence.

In a fraud setting, it is unlikely that any single procedure will uncover the nature and extent of the fraud. Rather because of the deceit and the strategic posture of the perpetrators, the effective program will likely be that which provides multiple opportunities to detect the fraud. Based on this premise, we use the number of the identified benchmark procedures as a measure of fraud program effectiveness.¹⁰

4. Results

Hypothesis 1 posits that auditors in the no checklist condition will assess higher fraud risks than those with the standard checklist. The mean (σ) FRASK of auditors with

a standard checklist is 4.67 (2.00) compared to 5.46 (2.35) for those without a checklist. The difference is in the expected direction and is marginally significant ($t=1.549$, $p=0.063$), supporting Hypothesis 1.¹¹ Since fraud is closely linked to inherent risks, auditors without the checklist may have also assessed inherent risks higher than those with a checklist. To explore this, we compared the inherent risk assessments of auditors in both groups. The mean (σ) inherent risk assessed by auditors with a checklist is 5.47 (1.98) compared to 6.4 (1.88) for those without a checklist. The difference is significant ($t=2.068$, $p=0.021$). Taken together, these results indicate that auditors in the no checklist condition assessed a higher risk scenario than those with the standard checklist, providing support for Hypothesis 1.¹²

Hypothesis 2 addresses the association between auditors' FRASK and fraud program effectiveness, while Hypothesis 4 posits that auditors with no standard audit program will develop a more effective set of fraud tests than those with a standard program. The hypotheses were tested by a GLM that used FRASK and audit program as independent variables and participants' score using the case benchmark tests as the dependent variable. The checklist condition and level of auditing experience were used as control variables. Descriptive statistics bearing on the hypotheses are presented in panel A of Table 1 and the GLM results are presented in panel B. Neither the presence of the standard checklist nor FRASK ($F=0.001$, $p = 0.993$) seemed to aid auditors in identifying effective tests.¹³ Thus, Hypothesis 2 is not supported, suggesting that program plans are not meaningfully adjusted to reflect variations in FRASK.¹⁴

Consistent with Hypothesis 4, there is a main effect for audit program ($F= 4.980$, $p=0.029$). As expected, Panel A of Table 1 shows that auditors with a standard audit

program identified fewer of the benchmark tests (mean 2.19) than auditors in the no program condition (mean 3.03).¹⁵ We also show the number of participants who identified each of the 13 procedures in the second and third columns of Exhibit 3. This shows that the main differences between the program conditions reside in a propensity for those without the standard program to choose procedures 6 (review for side agreements), 9 (probative substantive analysis of increases in credit limits), 11 (evaluation of subsequent sales), 12 (cash receipts of large distributors) and 13 (compare marketing programs to credit limit). The frequency of identifying these procedures was at least 30 percent higher for the “zero-based” program auditors than those in the standard program regime. Procedure 1 (reading minutes) was the only procedure identified by the standard program auditors more frequently than the zero-based auditors.¹⁶

Finally, experience is marginally significantly associated with fraud program effectiveness ($F=3.365, p=0.072$). A Pearson correlation confirmed a positive association between years of experience and fraud program effectiveness ($r=0.226, p=0.034$). Across all participants, the mean number of benchmark tests identified is 2.64 out of a possible 13.

Hypothesis 3 suggests a positive association between FRASK and the desire to consult fraud experts. Hypothesis 5 posits that auditors without a standard audit program are more likely to consult fraud experts. The hypotheses were tested with a general linear model (GLM) with FRASK and audit program as independent variables. The checklist manipulation was included in the model as a control variable to see if it affected consultation. The assessed need for consultation was the dependent variable. Descriptive statistics bearing on this hypothesis are presented in panel A of Table 2 and the GLM

results are presented in panel B. As expected, FRASK is significantly associated with the penchant to consult ($F=39.37, p=0.0001$). The Pearson correlation confirmed a positive association between fraud risk ($r=0.605, p=0.001$) and the need for consultation. A similar relationship existed between inherent risk and the desire to consult ($r=0.351, p=0.002$). This supports Hypothesis 3. Contrary to expectations, the audit program manipulation was not significant ($F=0.988, p=0.324$). Thus, Hypothesis 5 is not supported. Neither the checklist ($F=0.332, p=0.566$), nor its interaction with audit program is significant ($F=0.866, p=0.356$).

In addition, there was no association between fraud program effectiveness and propensity to consult ($r=0.099, p=0.216$) suggesting that auditors were confident in their audit programs despite actual effectiveness. That is, it is expected that auditors would consult with experts when they are not confident in the ability of the audit program plans to address client risks. The fact that program effectiveness was not related to consultation thus suggests auditors were, generally, equally confident in their plans.

5. Discussion

This study is unique in that it used an actual fraud situation and evaluated audit procedures based on a benchmark validated by a panel of experts. In addition, the research examined auditors' propensity to consult with fraud experts thereby providing a more comprehensive consideration of auditors' responses to an elevated fraud risk situation. Appropriate consultation may mitigate concerns arising from auditors' inability to design effective fraud procedures.

Our primary findings were that fraud risk assessments (FRASK) were not associated with program effectiveness but were positively associated with the propensity

to consult. We also found that a standard audit program impaired program effectiveness but did not affect the propensity to consult with fraud experts. This suggests a potentially important cost associated with the use of standard programs in a fraud setting. Finally, we found that the use of a standard risk checklist, structured around the SAS 82 categories, led to lower risk assessments than a no risk checklist condition.

While our finding of no association between FRASK and program planning effectiveness is consistent with prior research (Mock and Wright 1993; Zimbelman 1997, Glover et al. forthcoming), it is particularly important given our research setting and design. Regarding the setting, we examined a fraud scenario where an absence of linkage has direct implications regarding audit effectiveness as compared to a normal, non-fraud setting where reliance on standard tests may be cost-effective. Regarding the design, as in practice, we allowed auditors to add new procedures to address past design limitations of prior studies. Thus, we are on a more robust foundation when we conclude that there is a tenuous linkage between FRASK and program effectiveness. The finding also raises an important policy question: if FRASK does not translate into effective programs, what is the justification for the FRASK phase? Our design allows us to address this policy question since we show that FRASK is associated with consultation. Thus, the benefit of FRASK may not necessarily lie in promoting more effective tests but rather in making participants more willing to consult with fraud experts.

While auditors with a standard audit program performed worse than the no program condition in designing effective fraud tests, they did not feel any greater need to consult with fraud specialists. In effect, the worst performers, from a procedures perspective, felt as confident as the better performers. This finding suggests that a “zero-

based program” may be desirable when auditors encounter a heightened fraud risk scenario. We recognize that it is possible that the standard program contains effective fraud detection tests for a given client situation. However, because fraud involves strategic deception by a client who likely is aware of the procedures in a standard program, this possibility should not be over-emphasized (see Zimbelman and Waller 1997; Wilks and Zimbelman 2002). Nonetheless, the no-program approach does not preclude such potential benefits. That is, in a heightened fraud risk setting an auditor may first design tests without the standard program and then later consult with the standard program.

In spite of providing the three categories of risk suggested by the professional literature, we found that auditors who used a risk checklist made less accurate FRASK than those without the checklist. This result essentially replicates the findings in the study by Pincus (1989), which was conducted prior to the issuance of SAS 82 and used an uncategorized checklist. Together, with the findings on standard audit programs, the results support the notion that traditional, standard audit decision-aids may not facilitate strategic reasoning needed to make appropriate FRASK and design effective procedures.

This research focused on FRASK and the design of tests. However, fraud detection also requires the auditor to evaluate test results and arrive at an appropriate conclusion. An important extension of this research would be to examine how the manipulated conditions affect performance at these subsequent phases. Further, we did not incorporate the cost of consulting with fraud experts. Such consultations can lead to delays, are expensive and can affect engagement profitability. Practicing auditors who have higher FRASK may thus not be willing to consult given these economic

considerations. Of course, a firm may anticipate such behavior and urge mandatory consultations at specified FRASK levels.¹⁷ Our classification of consultation as a positive mechanism rests on the assumption that fraud experts will be able to design more effective fraud detection tests than field auditors. Such behavioral possibilities and assumptions are not tested and thus limit the generalizability of the results.

Our research design was motivated by the assumptions that we made about the relations among our key variables. In particular, we assumed a FRASK phase, followed by program development and culminating in consulting decisions. Alternative representations of the relations among the variables are possible. For instance, the FRASK phase may be followed by consultations to determine what type of program approach (standard versus zero-base) to use. Our research focus was based on prior research, which suggests auditors typically arrive at an initial set of judgments prior to deciding whether to seek consultation (Salterio and Denham 1997; Kennedy et al. 1997; Salterio 1996; Heath and Gonzales 1995 and Salterio 1994). Nonetheless, auditors may seek consultation prior to developing program plans.

We found that auditors identified relatively few effective fraud procedures. Auditors may often need to “think outside the box” or use strategic reasoning in a fraud setting to design tests that fit the particular circumstances (Wilks and Zimbelman 2002a; Zimbelman and Waller 1997). A promising avenue for future research is to evaluate the efficacy of various means to assist auditors in this task such as training or consultation with fraud specialists. Finally, while we show evidence of statistical significance, we are less able to evaluate the practical significance of our results. Nevertheless, it is common

knowledge that, in the realm of fraud detection, even an omission of a single procedure can make a big difference (Albrecht 2003, chapter 10).

Exhibit 1
Risk Checklist

	YES	NO
Management's characteristics and influence over the control environment:		
Is a significant portion of management's compensation represented by bonuses, stock options, or other incentives, the value of which is contingent upon the entity achieving unduly aggressive targets for operating results, financial position, or cash flow?		
Is there an excessive interest by management in maintaining or increasing the entity's stock price or earnings trend through the use of unusually aggressive accounting practices?		
Is there a practice by management of committing to analysts, creditors, and other third parties to achieve what appear to be unduly aggressive or clearly unrealistic forecasts?		
Does management show an interest in pursuing inappropriate means to minimize reported earnings for tax-motivated reasons?		
Does management have an <i>ineffective</i> means of communicating and supporting the entity's values or ethics, or communication of inappropriate values or ethics?		
Is management dominated by a single person or small group without compensating controls such as effective oversight by the board of directors or audit committee?		
Does management fail to correct known reportable conditions on a timely basis?		
Does management set unduly aggressive financial targets and expectations for operating personnel?		
Does management display a significant disregard for regulatory authorities?		
Does management continue to employ an ineffective accounting, information technology, or internal auditing staff?		
Has there been a high turnover of senior management, counsel, or board members?		
Are there frequent disputes with the current or predecessor auditor on accounting, auditing, or reporting matters?		

Exhibit 1 (continued)
Risk Checklist

	YES	NO
Are there any known history of securities law violations or claims against the entity or its senior management alleging fraud or violations of securities laws?		
Risk factors relating to industry conditions:		
Is there a high degree of competition or market saturation accompanied by declining margins?		
Is the industry declining with increasing business failures and significant declines in customer demand?		
Risk factors relating to operating characteristics and financial stability:		
Is the client unable to generate cash flows from operations while reporting earnings and earnings growth?		
Are there significant pressure to obtain additional capital necessary to stay competitive considering the financial position of the entity-including need for funds to finance major research and development or capital expenditures?		
Are assets, liabilities, revenues, or expenses based on significant estimates that involve unusually subjective judgments or uncertainties, or that are subject to potential significant change in the near term in a manner that may have a financially disruptive effect on the entity-such as ultimate collectibility of receivables, timing of revenue recognition, realizability of financial instruments based on the highly subjective valuation of collateral or difficulty-to-assess repayment sources, or significant deferral of costs?		
Are there significant, unusual, or highly complex transactions, especially those close to year-end, that pose difficult “substance over form” questions?		
Are there difficulties in determining the organization or individual(s) that control(s) the entity?		
Has the company experienced an unusually rapid growth or profitability especially compared with that of other companies in the same industry?		
Is the company vulnerable to changes in interest rates?		
Does the company have an unrealistically aggressive sales or profitability incentive programs?		
Is there a threat of imminent bankruptcy or foreclosure, or hostile takeover?		
Is there a poor or deteriorating financial position when management has personally guaranteed significant debts of the entity?		

Exhibit 2: Standard Audit Program

1. Obtain an aged trial balance of trade receivables as of the date selected for confirmation procedures. Perform the following:
 - a. Cross-foot the totals and re-foot the total column and analysis columns.
 - b. Trace total to the general ledger control account and to the lead schedule or working trial balance.
 - c. On a test basis, trace entries for individual customers on the aging analysis (totals and aging detail) to the individual accounts in the accounts receivable subsidiary ledger and select individual accounts from the subsidiary ledger and trace totals and aging detail to the aged trial balance to determine if aging is correct. Test footings of individual customer accounts in the subsidiary ledger.

2. Select individual customer accounts for confirmation procedures from the aged trial balance and arrange for the preparation of confirmation requests to be mailed under the auditor's control and tested as follows:
 - a. Trace individual confirmation requests as to balances and addresses to the subsidiary accounts receivable records.
 - b. Send confirmations (using envelopes with the auditor's return address) and prepare confirmation statistics.
 - c. Trace confirmation replies to the trial balance and investigate replies with differences.
 - d. Obtain new addresses for all confirmations returned by the post office and re-mail.
 - e. Send second requests for all unanswered positive confirmation requests. Consider sending third requests by registered or certified mail and performing alternative auditing procedures.

3. Ascertain whether any accounts or notes have been assigned, pledged, or discounted by reference to minutes, review of agreements, confirmation with banks, etc.

4. Obtain or prepare an analysis of the allowance for doubtful accounts for the period and review adequacy of the allowance and related provision by:
 - a. Review the aged trial balance as of the balance sheet date with the client's credit manager or other responsible individual to identify accounts of a doubtful nature and allowances required; review correspondence files and other relevant data in support of client's representations. Items reviewed should include past-due amounts and significant amounts whether past due.
 - b. Examine credit reports for delinquent and large accounts.
 - c. Review confirmation exceptions for indication of amounts in dispute.
 - d. Consider requesting audited financial statements for large accounts that are past due and appear doubtful.

5. Perform cutoff tests for sales and returns:
 - a. Select sales invoices for testing from the sales register for several days before and after year-end and examine shipping records and determine that they were recorded in the proper period.
 - b. Select credit memos issued after year-end and examine underlying documentation (for example, record of receipt of returned goods) to determine period to which credit memo is applicable and whether it was recorded in the proper period.

6. Analytical procedures

Analyze and review trends for the following relationships:

- a. Accounts receivable to credit sales.
- b. Allowance for doubtful accounts to accounts receivable (in total and in relation to past-due categories per aging analysis).
- c. Sales to returns and allowances.
- d. Expense provisions for doubtful accounts to net credit sales.
- e. Expense provisions for doubtful accounts to write-offs.
- f. Moving average relationship of write-offs to trade receivables.
- g. Average balance per customer.
- h. Ratio of account receivable to current assets.

Exhibit 3
Benchmark Program developed from analysis of SEC/AAER and Validated by a
Panel of Experts

	Standard Program (n=31)	No Program (n=35)
1. If available, read minutes of the November 13th meeting with distributors.	10 32%	5 14%
2. Inquire of distributors who committed on November 13 as to their understanding of the terms of the sales.	13 42%	16 46%
3. Inquire of distributors who were at the November 13 meeting and who did not commit to participate in the marketing program as to their reasons for not committing.	-	-
4. Inquire of undecided distributors who changed their minds between November 13 and year-end, the reasons for changing their minds.	-	1 3%
5. If minutes (of November 13 th meeting with distributors) not available, inquire of sample of distributors at the meeting to ascertain their understanding of issues discussed.	1 3%	4 11%
6 Review correspondence file with distributors for evidence of side agreements (e.g., right of return; payment terms).	15 48%	21 60%
7. Investigate the rationale for Precision's involvement in the storage and warehousing of distributor sales.	9 29%	11 31%
8. Ascertain ability of distributors to store huge orders and the responsibility for paying storage costs.	4 13%	6 17%
9. For all increases in credit limits, review client analysis of distributor credit-worthiness. If no analysis exists, perform probing, substantive analysis of distributors' credit-worthiness.	9 29%	16 46%
10. Test with end-users to confirm that Precision was encouraging them to buy from the distributors.	2 6%	2 6%
11. Compare sales in the first quarter of 2000 to that indicated by Precision's operating plan.	1 3%	6 17%
12. Look at subsequent cash receipts for some of the large distributor sales.	4 13%	14 40%
13. Compare orders taken via the marketing program to authorized credit limit.	-	4 11%
Total Procedures Identified	68	106

Note: The second and third columns show the number and percentage of participants in the standard program and no standard program conditions who identified each procedure.

Table 1 (Panel A)
Descriptive Statistics on Auditors' Fraud Program Effectiveness Using Benchmark Program

Means with standard deviations in parentheses

	Standard Program	No Program	Overall
Checklist	2.60 (1.64) n=15	2.94 (1.59) n=18	2.79 (1.60) n=33
No Checklist	1.81 (1.47) n=16	3.12 (1.11) n=17	2.48 (1.44) n=33
Overall	2.19 (1.58) n=31	3.03 (1.36) n=35	2.64 (1.52) n=66

Note 1: There are 13 tests in the benchmark audit program, which were identified through an analysis of the SEC AAER and subsequently validated via a Delphi study. The Descriptive Statistics pertain to the number of tests identified in the benchmark program shown in Exhibit 3.

Note 2: The descriptive statistics and the associated GLM are based on 66 subjects because 3 subjects did not provide their years of experience. The GLM, which uses experience as a covariate, treat these subjects' responses as missing variables. Including these subjects do not alter any of the GLM results (means (n) for program/checklist = 2.43 (16); program/no checklist = 1.76 (17); no program/no checklist = 3.11 (18)).

Table 1 (Panel B)
Hypothesis Testing of Auditors' Fraud Program Effectiveness
General linear Model

Source	Type I sum of Squares	Df	F-value	p-value
Fraud Risk	.001	1	.000	.993
Experience	7.049	1	3.365	.072
Audit Program	10.433	1	4.980	.029
Checklist	1.631	1	0.778	.381
Audit Program x Checklist	3.744	1	1.787	.186
Error	125.69	60		

Fraud Risk: 1-10 (low risk-high risk; midpoint=moderate risk)

Experience: years of auditing experience

Audit Program: 0=no program; 1=standard program

Checklist: 0=no checklist; 1=checklist

Table 2 (Panel A)
Descriptive Statistics on Auditors' Willingness to Consult with Fraud Experts
Means with standard deviations in parentheses

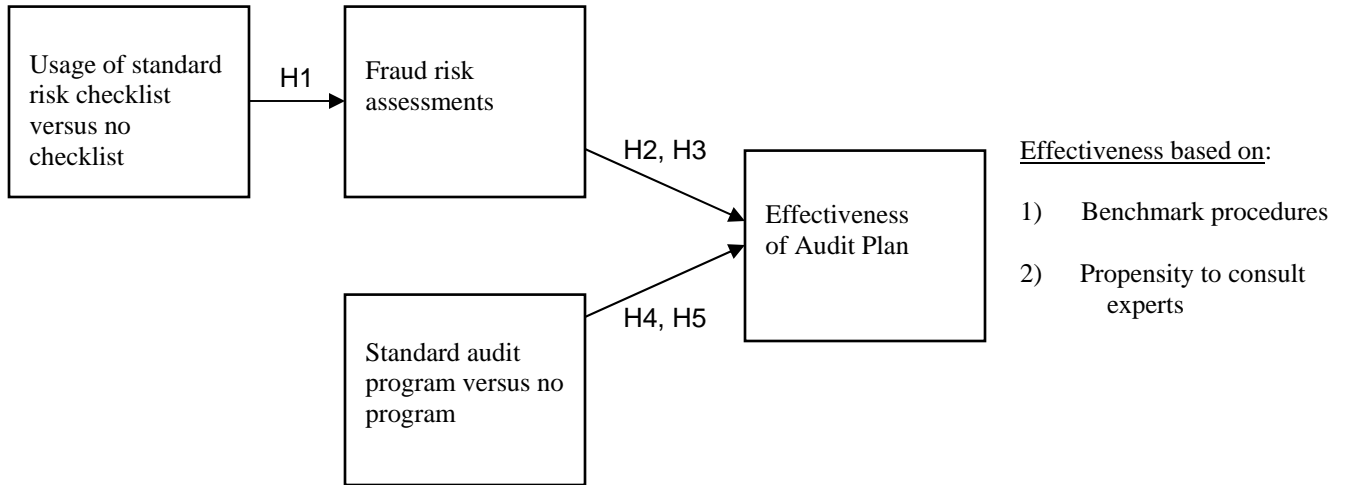
	Standard Program	No Program	Overall
Checklist	4.80 (2.48) n=15	5.37 (2.06) n=19	5.12 (2.24) n=34
No Checklist	5.47 (2.32) n=17	5.25 (2.70) n=18	5.36 (2.49) n=35
Overall	5.16 (2.38) n=32	5.31 (2.36) n=37	5.24 (2.36) n=69

Response scale: 1=no necessity to consult; 10=high necessity to consult

Table 2 (Panel B)
Hypothesis Testing of Auditors' Willingness to Consult with Fraud Experts
General linear Model

Source	Type III sum of Squares	Df	F-value	p-value
Fraud Risk	142.13	1	39.36	.0001
Checklist	1.20	1	.332	.566
Audit Program	3.568	1	.988	.324
Checklist x Audit program	3.125	1	.866	.356
Error	231.06	64		

Figure 1: Framework for Evaluating Effectiveness of Audit Responses to Fraud Risk



Endnotes

1. Auditors in a “no risk checklist” regime will, typically, identify and list client-specific risk factors while those in the “standard risk checklist” regime will have a pre-list of risk factors, from which they will check those that apply to a particular client. Similarly, auditors in a “standard audit program” regime will design a program using the standard program as an anchor while those without a standard audit program will design a program without any anchor. The no audit program regime is also referred to as “zero-based.”
2. Obviously, other factors affect FRASK and fraud detection effectiveness. These include knowledge, experience, incentives and training. Our study does not directly deal with these variables and wherever necessary, we use statistical analysis to control for their effects. Other papers provide a comprehensive discussion of how auditors generate audit plans based on FRASK and other fraud related issues (e.g., Nieschwietz et al. 2000; Wilks and Zimbelman 2002a).
3. SAS 99 categorizes the factors into management incentives, fraud opportunities and management attitudes. We believe this new categorization is more likely to facilitate the developing of a fraud story (see Wilks and Zimbelman 2002b).
4. Wilks and Zimbelman (2002b) found that a fraud-triangle decomposition of FRASK made auditors more sensitive to opportunity and incentive cues, although the increased sensitivity was offset by higher sensitivity to attitude risk when attitude cues suggest low fraud risk. They, however, do not evaluate the relative effectiveness of the use of checklist versus open-ended risk identification decision tools.
5. Standard programs may also have the dysfunctional “familiarity” effect in that clients through previous experience with the auditing firm anticipate the procedures and perpetrate frauds that are beyond the program’s reach. That is, the client gets acquainted with the standard procedures conducted and plans the fraud to avoid being detected by the use of that program. Surprise is an important element in fraud detection and that surprise is lost when a predictable set of tests are performed which the potential perpetrator knows in advance. Further, often the client’s accounting personnel have prior auditing experience, in some cases with the same firm that is conducting the engagement.
6. Earlier versions of the case were shared with the engagement partners who evaluated the case for realism and completeness and provided comments. Their comments resulted in further modifications.
7. Under this program retailers, who purchased analog systems from the distributors, earned frequent-flyer type points that could be used to obtain discounts on the digital units.

8. In our experiment, we use a case adapted from a real fraud scenario and did not vary the risk level. Therefore, our test of linkage focuses on the association between the subjective FRASK and audit responses. Of course, even when objective risk levels are varied, the subjective FRASK determines whether the manipulation is successful and is more informative (stronger independent variable) than the manipulated risk levels.

9. One firm provided 19 participants and the others provided 24 and 31 respectively. The participants included 54 managers, 12 partners and 8 seniors. Firm affiliation did not have a significant effect on any of our dependent variables: fraud risk ($F=.358$, $p=.700$); inherent risk ($F=2.359$, $p=.102$); program effectiveness ($F=1.776$, $p=.177$) and consulting ($F=.960$, $p=.388$). Thus, firm affiliation is not considered further in subsequent analyses. The effect of experience varied by the dependent variable and where necessary will be controlled in subsequent analyses. We also included various combinations of firm and experience in the model and obtain similar results.

10. We also attempted to use the effectiveness ratings of the three liaisons to weight the procedures. However, this weighting did not yield additional insights beyond that gleaned from using the simpler count of procedures. Therefore, and consistent with the principle of Ocam Razor (i.e., a preference for simplicity and parsimony), our subsequent analyses focus on the simpler effectiveness score.

11. We obtained similar results when we used the non-parametric Mann-Whitney procedure to test for the location difference ($Z=1.410$, $p=0.079$). We also run a GLM that used experience as a covariate but found experience was not significant ($F=0.002$, $p=0.965$) and checklist was marginally significant ($F=3.001$, $p=0.088$). All directional hypotheses are tested with 1-tail significance levels. Across subjects, the mean (σ) FRASK is 5.07 (2.21), which appears low considering that a fraud scenario was used. The median FRASK is 5.0 and the modal value is 4.

12. As expected, the checklist had no effect on control risk assessments. The mean (σ) control risk assessed by auditors with a checklist is 3.64 (1.82) compared to 3.70 (1.93) for those with no checklist. The difference is not significant ($t=0.145$, $p=0.885$).

13. In our framework, we noted that the checklist manipulation was not expected to have a direct effect on program effectiveness or propensity to consult. That is, the effect of checklist is hypothesized to be impounded in FRASK or alternatively FRASK mediates the effect of checklist. A necessary condition for testing this mediation is for the checklist to have an effect on these dependent variables. Mediation will then be evidenced by whether this effect persists or disappears after controlling for FRASK. Since the necessary condition fails (i.e., checklist had no direct effect on either variables), we are unable to test the mediation.

14. Inherent risk assessments were also used as covariates but were found not to be significant.

15. We obtain the same results using the non-parametric Mann Whitney U test ($Z=2.53$, $p= 0.006$). Finally, as an additional test to examine the robustness of the findings with respect to program effectiveness, we also examined the number of participants who identified at least one effective fraud detection test. The analyses show that 97 percent (i.e., 35 of 36) of auditors in the no standard program condition identified at least 1 fraud test compared to 79 percent (i.e., 26 of 33) in the standard program condition. The difference is significant (Fisher's exact test $p=0.020$), corroborating the primary findings that provision of the standard program was associated with lower program effectiveness.

16. Since we do not have any theoretical expectations to explain these variations, we present these data for exploratory purposes and present no statistical analyses.

17. Even such a mandate may be subject to "gaming" since the auditor, willing to save on consultation costs, will simply lower FRASK.

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