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An Experimental Investigation of Auditors' Evidence Evaluation and Work Ownership in a Compartmentalized Test of Details Setting

Christopher Pearson
University of Kentucky, chris.pearson@uky.edu
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Dr. Sean Peffer, Major Professor

Dr. Monika Causholli, Director of Graduate Studies

AN EXPERIMENTAL INVESTIGATION OF AUDITORS' EVIDENCE EVALUATION AND WORK OWNERSHIP IN A COMPARTMENTALIZED TEST OF DETAILS SETTING

DISSERTATION

A dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy in the
College of Business and Economics
at the University of Kentucky

By
Christopher A. Pearson
Lexington, Kentucky
Director: Dr. Sean Peffer, Professor of Accounting
Lexington, Kentucky
2022

ABSTRACT OF DISSERTATION

AN EXPERIMENTAL INVESTIGATION OF AUDITORS' EVIDENCE EVALUATION AND WORK OWNERSHIP IN A COMPARTMENTALIZED TEST OF DETAILS SETTING

Psychological ownership theory suggests that auditors are more likely to exert effort when they feel a sense of personal ownership of the audit. Ongoing trends towards a more compartmentalized and decentralized audit, however, may impair auditors' work ownership by decreasing their interaction with work, as well as by discouraging engagement with work performed by colleagues. In two experiments, I test the effects of audit work compartmentalization, where I consider whether auditors' evaluation of evidence is affected by the source of the sample to which evidence is related. Findings from my first experiment indicate lower performance when auditors evaluate evidence that relates to a sample selected for them by a colleague auditor compared to a sample selected by either themselves or a technological system. Additional analysis indicates that auditors were more likely to engage and take ownership of work when it related to a sample selected by either themselves or a technological system. In a second experiment, I consider whether a colleague's use of technology in sample selection mitigates auditors' tendencies to disengage with their work when a colleague auditor performs sample selection, as well as whether variation in a colleague's involvement in sample selection impacts evidence evaluation. Contrary to expectations, I find evidence that auditors perform worse when technology-assisted sample selection required greater colleague involvement. Collectively, findings indicate the importance of considering psychological ownership and engagement in decisions related to work design and indicate potential benefits of technology use in a sample-based test of details task.

KEYWORDS: Psychological Ownership, Sample-Based Testing, Auditor-Technology Interaction

Christopher A. Pearson

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By
Christopher A. Pearson

Dr. Sean Peffer
Director of Dissertation

Dr. Monika Causholli
Director of Graduate Studies

April 13, 2022
Date

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I. INTRODUCTION

Recent research indicates that financial reporting complexity is increasing over time, a product of expanding financial regulation and global economic trends (Loughran and McDonald 2014; Guay, Samuels, and Taylor 2016; Dyer, Lang, and Stice-Lawrence 2017). In response to growing complexity, accounting firms have changed their approach to the financial statement audit, such that work has become increasingly compartmentalized and likely to involve the collective effort of a network of professionals that includes specialists (e.g., Hux 2017), offshore auditors (e.g., Hanes 2013), and component auditors (e.g., Burke, Hoitash, and Hoitash 2020). At the same time, improvements in firms' technological capabilities have contributed to a rise in audit automation, where auditors often perform their work with the assistance of sophisticated technological tools. While changes in work structure present a variety of benefits to the audit, psychological ownership theory suggests that unintended costs may be imposed on individual auditors who feel less personal ownership of compartmentalized audit work. In this study, I consider the extent to which audit task compartmentalization affects performance, specifically, whether auditors' evaluation of evidence is affected when evidence relates to a sample selected by another auditor compared to a sample selected by themselves (Commerford, Hatfield, Houston, and Mullis 2017). I also consider variation in evidence evaluation when evidence relates to a sample selected by, or with the assistance of, technology, which is important given accounting firms' continued interest and investment in technological innovation (e.g., Deloitte 2016; KPMG 2017; EY 2018).

Psychological ownership theory indicates that people form attachments with inanimate objects to satisfy needs for efficacy, self-identity, and "having a place" (Furby

1978; Pierce, Kostova, and Dirks 2001, 2003). Owned objects – possessions – play a key role in defining self-identity, which research suggests leads people to view them as virtual "extensions of self" (Belk 1988, 2013; Tian and Belk 2005). As a result, people tend to be more willing to invest themselves and their resources in maintaining and improving possessions than they are non-possessions. Within the workplace, one of the most common targets of ownership formation, ownership manifests through workers' organizational commitment, job satisfaction, and performance, where ownership motivates workers to exert greater effort in their work (Van Dyne and Pierce 2004; Pierce, Jussila, and Cummings 2009; Wang, Law, Zhang, Liang, and Li 2019). While each of these positive ownership-related behaviors has implications for the audit workplace, the association between work ownership and effort holds particular meaning for auditors' judgment and decision-making, as auditors tend to be more effective when they exert greater effort (e.g., Zhao, Bedard, and Hoitash 2017), as well as when they are intrinsically motivated to perform their work (Kadous and Zhou 2019). As such, I expect that auditors' perceptions of work ownership are positively associated with their effort, motivation, and, ultimately, the quality of their judgment and decision-making processes.

Ongoing trends in the financial reporting environment have led accounting firms to adopt a more compartmentalized and decentralized approach to the audit, where work traditionally performed by an individual auditor is often, instead, divided between multiple auditors. In their utilization of offshore auditors, for example, firms typically divide larger tasks into smaller sub-tasks, which enables an allocation of simple and repetitive work to offshore auditors, while on-site engagement team members perform complex and subjective work (Downey 2018). While dividing work between multiple auditors poses a

variety of benefits to audit efficiency and effectiveness, unintended costs are likely imposed on individual auditors' perceptions of work ownership for several reasons. First, compartmentalization limits auditors' interaction with work and, thus impairs ownership formation by limiting auditors' self-investment in work, control of work, and the acquisition of work-specific knowledge (see Pierce et al. 2001, 2003). Second, compartmentalization increases the extent of colleagues' involvement in shared work, which may induce perceptions of competing ownership. Research indicates that responses to ownership competition are largely negative, as people exhibit territoriality when ownership is threatened (Kirk, Swain, and Peck 2018), as well as disengagement with others' possessions (Argo, Dahl, and Morales 2006). In the audit setting, I expect that auditors' perceptions of work ownership are negatively impacted by the involvement of a colleague in their work, which I also expect has a negative impact on the quality of their judgment and decision-making. Further, I expect that auditors are perceptive of, and sensitive to, variation in a colleague's level of involvement in shared work, such that I expect a stronger negative reaction when circumstances require a colleague to spend greater time and effort in shared work.

While I expect a negative reaction to work compartmentalization that involves another auditor, it is likely that auditors' reactions vary based on the extent to which technology is involved in compartmentalized work. While the use of technology in the audit is not necessarily a new phenomenon (e.g., Kinney 1986; Fischer 1996), recent reports indicate rapid developments in accounting firms' technological capabilities, as technology has come to be used in a variety of audit tasks ranging from the evaluation of complex estimates (Estep, Griffith, and MacKenzie 2021; Commerford, Dennis, Joe, and

Ulla 2022) to the automation of simple and repetitive processes (Moffitt, Rozario, and Vasarhelyi 2018; Cooper, Holderness, Sorensen, and Wood 2019). Although the precise nature of auditor-technology interaction is likely to vary between specific tasks and technological tools, research in the computer science and psychology domains indicates broad differences in how people view and approach work that they believe has been performed by technology. Hinds, Roberts, and Jones (2004), for example, find that people retain a greater sense of personal responsibility for work performed in collaboration with a robotic system compared to work performed in collaboration with another human, which the authors suggest may be due to fundamental differences in shared identity between humans and robots. Similarly, I expect that auditors experience greater personal ownership of compartmentalized work when it involves technology compared to a colleague human auditor, in addition to better performance when compartmentalized work involves technology compared to a colleague auditor.

My experimental setting draws on Commerford et al. (2017), who find that auditors are less likely to opportunistically avoid selecting sample items associated with difficult client managers when the responsibility for testing a selected sample falls to a colleague. I extend their findings by considering whether receiving a colleague-selected sample impacts evidence evaluating auditors' engagement, work ownership, and performance. I expect that auditors experience lower ownership and performance when evidence relates to a sample selected by a colleague compared to a sample selected by themselves. I further expect that auditors' ownership and performance are impacted to a greater extent when sample selection requires a colleague's greater involvement, due to greater disengagement with what is likely perceived as stronger colleague work ownership. Given differences in

how people perceive work performed by technology, I expect that evidence evaluating auditors experience higher ownership and stronger performance when technology performs sample selection compared to when a human colleague performs sample selection. I further expect that the benefits of technology-performed sample selection extend to a setting where technology assists a human colleague in sample selection, in addition to when it operates autonomously or free of human influence.

I conduct two experiments to test my expectations. The first experiment, a 3 x 1 between-subjects design, tasks inexperienced auditors with evaluating vendor invoices as part of a search for unrecorded liabilities, adapted from Westermann (2011) and Downey (2018). I manipulate my independent variable of interest, sample source, by informing participants about the source of the sample they are responsible for testing, specifically whether sample selection had been previously performed by either themselves, a colleague staff auditor, or a technological system. I measure my primary dependent variable, performance, through participants' identification of errors in client-provided evidence. I collect perceptions of work ownership (Brown, Pierce, and Crossley 2014) and engagement (Rich, Lepine, and Crawford 2010; Christian, Garza, and Slaughter 2011) to test a potential process through which sample source might indirectly impact work ownership (Wang et al. 2019). Additional collected measures capture participants' perceptions of task significance (Downey 2018), as well as their allocation of responsibility between themselves and sample selecting auditors for a possible negative testing-related outcome.

Statistical results support my expectations that the source of a sample impacts how auditors evaluate evidence. I find evidence of a direct and statistically significant relationship between sample source and performance, where auditors identified more errors

when sample selection was presented as having been previously performed by themselves compared to a colleague auditor. Likewise, auditors identified more errors when sample selection was presented as having been performed by a technological system compared to a colleague auditor. Process analysis indicates that perceptions of work ownership were indirectly affected by sample source through engagement, such that auditors were more likely to engage with their work when it related to a sample selected by either themselves or a system compared to a human colleague. Additional analysis indicates that auditors allocated marginally more responsibility for a potential negative testing-related outcome to a human colleague than they did a system, consistent with Hinds et al. (2004). These results are consistent with my expectations and demonstrate the importance of considering auditors' work ownership and engagement in decisions related to work design. Further, while separating sample selection and evidence evaluation appears to have some downside on evidence evaluation when sample selection is performed by a human colleague, involving autonomous technology in sample selection may enable firms to mitigate opportunistic sample selection without imposing costs on evidence evaluation.

Drawing on results from my first experiment, I conduct a follow-up 2 x 2 between-subjects experiment that considers whether variation in the time and effort a colleague spends in sample selection impacts evidence evaluating auditors' engagement, work ownership, and performance. I expect that a colleague's greater involvement in sample selection will be associated with lower ownership and worse performance in evidence evaluation due to heightened disengagement in response to stronger perceived colleague ownership of sample selection. In addition, I also consider whether the benefits of involving technology in sample selection extend to a setting where technology assists a

human colleague in sample selection. While I frame technology as operating independent of any specific human influence in Experiment 1, understanding how auditors perceive work performed by technology when technology serves in an assistive role is important given current audit practice, where technology is unlikely to serve as a perfect replacement for human auditors and professional skepticism (e.g., Harris 2017). Based on the results of my first experiment, I expect that a colleague's use of technology in sample selection will be associated with higher engagement, work ownership, and performance among evidence evaluating auditors, which I further expect to have the largest incremental impact when sample selection requires a colleague's to spend greater time and effort in sample selection.

I manipulate my first independent variable, sample selection involvement, through the amount of time and effort required for a colleague to select a sample and obtain evidence from the client. I presented this information to participants through an informal email from their sample-selecting colleague, where participants learned whether sample selection took either more or less time and effort than their sample selecting colleague had initially anticipated. I manipulate my second independent variable, technology use in sample selection, through background information indicating whether participants' hypothetical employer was in the process of implementing recently updated audit procedures or a novel proprietary technological tool across its various audit engagements. Similar to Experiment 1, I measure performance through the number of errors identified by each participant, which I supplement with a continuous measure of participants' confidence in the accuracy of their client's reported liabilities balance. I collect additional measures related to work ownership, engagement, perceived sample quality, and allocated responsibility for a possible negative testing-related outcome.

Results indicate differences in auditors' evidence evaluation based on the joint effects of colleague involvement in sample selection and technology use, in that I find a marginally significant interaction term for those variables. Contrary to expectations, however, I find that auditors identified significantly less errors when technology assisted a colleague in selecting a sample that required greater involvement (i.e., time and effort) than expected. Additional analysis indicates that auditors were less likely to engage with technology-assisted sample selection that required a colleague's greater involvement. While this pattern of results is not consistent with my expectations, it is indicative of possible expectancy violation (e.g., Burgoon and Hale 1988; Mendes, Blascovich, Hunter, Lickel, and Jost 2007), as auditors tend to associate technology use with a more efficient and less effortful audit (e.g., EY 2016; Emett, Kaplan, Mauldin, and Pickerd 2021). As a result of violated expectations, evidence evaluating auditors may have disengaged with their work when technology use did not result in a more efficient audit, consistent with research that indicates negative reactions to violated expectations (Afifi and Burgoon 2000; Burgoon 2015).

This study contributes to several growing areas of interest in the audit literature and should be of interest to accounting firms and academic researchers for several reasons. First, I contribute to a growing literature that documents the effects of auditors' attitudinal characteristics on their judgment and decision-making by demonstrating how work compartmentalization affects auditors' engagement and work ownership. While the positive effects of work ownership are well-documented in the organizational psychology and management literatures, little research has considered their antecedents and effects in the audit setting (see, e.g., Holmstrom 2021). Given the unique nature of audit work and

the audit regulatory environment, it is not readily apparent if the findings from prior research generalize to the audit setting, particularly given that additional effort is not always beneficial for auditors (e.g., Brazel, Jackson, Schaefer, and Stewart 2016). In addition to offering insight into auditors' judgment and decision-making, a deeper understanding of ownership offers likely benefits to other areas of concern for audit firms. A deeper understanding of the relationship between work ownership and organizational commitment, for example, may prove useful for firms' retention of highly skeptical auditors, who often become disillusioned with public accounting early in their careers (Cohen, Dalton, and Harp 2017).

In addition to the audit literature, my study also contributes to the broader work ownership literature, which is still in the process of developing an understanding of how collaborative work arrangements impact individual work ownership and performance (e.g., Pierce and Jussila 2010; Gray, Knight, and Baer 2020). While prior research has considered individuals' territorial responses to perceived violations of ownership, research has not yet considered how people interact with others' ownership when they have not yet had the opportunity to form ownership for themselves. In that sense, my study contributes by documenting disengagement with a colleague's work, a reaction that is distinct from the territorial and loss prevention-focused behaviors noted by prior research (e.g., Kirk et al. 2018; Wang et al. 2019).

Second, my study contributes to an ongoing discussion about the benefits and costs of audit automation. My study is the first to consider auditors' interaction with work performed by technology through a lens of work ownership and engagement, which contributes to both the audit and work ownership literatures. Results from my first

experiment indicate likely benefits to performance when auditors collaborate with technology to perform an objective task (i.e., search for unrecorded liabilities), consistent with recent research which indicates appreciation of technology in simple and objective settings (Castelo, Bos, and Lehmann 2019; Logg, Minson, and Moore 2019). That said, results of my second experiment indicate potential nuance in how work is perceived when it is performed with the assistance of technology, as auditors appear to respond negatively to work involving technology that does not result in a more efficient audit. While I am unsure as to the specific mechanism through which this effect may have occurred, findings are suggestive of violated expectations, as auditors may have responded negatively when technology use did not conform to their expectations of a more efficient and less effortful audit (e.g., Afifi and Burgoon 2000).

Finally, I contribute to practice by testing the implications of Commerford et al. (2017), who find that separating sample selection from evidence evaluation mitigates auditors' tendencies to avoid selecting sample items associated with difficult client managers. While their findings indicate that separating sample selection and evidence evaluation results in higher quality samples, my findings indicate a potential downside to evidence evaluating auditors' engagement, work ownership, and performance. That said, involving autonomous technology in sample selection may provide firms the ability to realize the benefits of separating sample selection and evidence evaluation without imposing engagement- and work ownership-related costs on evidence evaluating auditors. I expect that my findings generalize to other areas of the audit where auditors divide the performance of larger tasks with colleagues and may be useful for consideration in firms' work design- and staffing-related decisions.

This study proceeds as follows. Section II presents relevant background and motivates hypotheses. Section III outlines research design for Experiment 1. Section IV discusses tests of hypotheses and supplementary analysis for Experiment 1. Section V discusses the results and implications of Experiment 1. Section VI presents motivation for Experiment 2. Section VII outlines research design for Experiment 2. Section VIII discusses tests of hypotheses and supplementary analysis for Experiment 2. Section IX discusses findings from Experiment 2. Finally, Section X concludes the study.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

II.A Motivation

Recent research indicates that financial reporting complexity is increasing over time, a function of expanding financial regulation and changes in the underlying global business environment (Loughran and McDonald 2014; Guay et al. 2016; Dyer et al. 2017). In response to increased complexity, accounting firms have moved towards a more compartmentalized and decentralized model of the audit, where work often involves professionals who are not members of the traditional client-specific engagement team.[1] Firm frequently engage specialists, for example, to assist auditors in evaluating complex matters related to valuation, tax, and information technology (Glover, Taylor, and Wu 2017; Hux 2017). While specialists plan and perform testing procedures and may assist the engagement team in arriving at a recommended adjustment, the final responsibility for resolving any adjustments with the client generally falls to non-specialist engagement team

[1] Alternatively, audit teams may compartmentalize tasks as a means of mitigating auditor opportunism, where work may be split between members of the same engagement team (e.g., Commerford et al. 2017).

members (e.g., Gold, Kadous, and Leiby 2020; Commerford et al. 2022). Griffith (2020) notes that this division of responsibility for testing versus adjustment negotiation very often leads to one-sided friction between specialists and engagement team members, who frequently alter or disregard specialists' findings to maintain perceived "jurisdiction" over the audit.

As another example of a recent trend in work structure, offshoring involves a redistribution of work from the on-site engagement team to less costly, geographically distant auditors (Hanes 2013; Lyubimov, Sutton, and Arnold 2013; Canning, O'Dwyer, and Boomsma 2021). In addition to facilitating a more efficient audit due to time-zone related differences in offshore and on-site auditors' working hours, offshoring benefits the auditor-client relationship by allowing auditors to pass offshoring-related savings on to their "cost fatigued" clients (Daugherty and Dickins 2009). While offshore auditors are capable when assigned simple or repetitive pieces of audit tasks, they are less effective at completing complex or subjective work, which indicates a need to differentiate the work assigned to offshore auditors from the work assigned to the on-site engagement team. As such, on-site auditors tend to be responsible for complex or subjective work, as well as integration of any work performed by offshore auditors with work performed by the engagement team (Downey 2018). Motivated by offshoring-related work division, Downey (2018) indicates that dividing work between auditors can have unintended consequences on auditors' performance, in that auditors' documentation is less accurate when they complete unfinished work that is of low significance to the overall audit.

While the above examples - specialists and offshoring - differ widely in their capabilities and the circumstances in which they are used, a common finding in both

settings is that auditors interact with work differently when they believe it to involve another auditor. In that sense, the audit literature has yet to identify a unifying explanation for auditors' performance when multiple auditors engage with the same task, or a series of related tasks. The purpose of this study is to propose and assess one possible mechanism, psychological ownership, to explain how auditors interact with compartmentalized work, which I expect to generalize to any setting where auditors' work is influenced by another auditor. Given an increased involvement of technology in the audit, I also consider differences in auditors' responses to compartmentalized work that involves technology, which theory indicates is likely perceived differently than compartmentalized work that does not involve technology.

II.B Psychological Ownership Theory

Psychological ownership theory refers to the process by which people come to view a target object as their own (i.e., "mine"), which serves to satisfy needs for efficacy, self-identity, and "having a place" (Furby 1978; Pierce et al. 2001, 2003). In contrast to legal ownership, which forms through a formal transfer of title, psychological ownership forms through informal human-object interaction, irrespective of legal title (Beggan and Brown 1994). Specifically, research indicates that ownership forms through three primary paths: 1) investment of self in a target object, 2) perceived control of a target object, and 3) intimate knowledge of a target object (Pierce et al. 2001, 2003). Owned objects, or possessions, play a significant role in self-identity, where people view their possessions as virtual "extensions of self" (Belk 1988, 2013; Tian and Belk 2005). That is, people view possessions as reflections of their inner self and, as a result, tend to invest more time and resources in maintaining and improving possessions than they do non-possessions.

People tend to be adept at recognizing ownership, which research suggests is a fundamental part of early cognitive development. Friedman and Neary (2008), for example, document a "first possession" heuristic among preschool age children, where children ascribe toy ownership to whichever child first possessed a given toy. Interestingly, they note that children exhibit this heuristic without receiving any formal instruction to do so, which implies prior learning from either social observation or a more innate "property instinct" (e.g., Stake 2004). Similarly, Beggan and Brown (1994) note association as a justification for ownership, where experimental participants resolved property disputes between two parties in favor of whichever party was presented in a photograph with the disputed object.

In line with the above conceptualization of ownership, Wang et al. (2019) suggest that ownership could be best thought of as reflecting the triadic "self-object-other," where the relationship between "self" and "object" is a function of the relationships between "self" and "other" and "other" and "object." The extent to which people perceive "self" versus "not self" ownership influences perceptions of value, which is a concept reflected in several well-known psychological phenomena. A core premise of the endowment effect, for example, is that people value their possessions more than non-possessions (Kahneman, Knetsch, and Thaler 1990, 1991). While research has traditionally characterized endowment as a manifestation of loss aversion, a growing argument holds that endowment may be more adequately explained by psychological ownership theory, where a preference for one's own possessions is due to their incorporation in self-identity (Morewedge, Shu, Gilbert, and Wilson 2009; Morewedge and Giblin 2015). Dommer and Swaminathan

(2013), for example, note variation in endowment based on the strength of a self-object bond, a finding not supported by a loss averse-based theory of endowment.

A separate body of research indicates variation in perceived value based on the extent to which an object was previously handled or touched by another person. This research is motivated by anthropology research identifying a cross-cultural contagion heuristic, where objects are perceived to take on the properties of individuals with whom they come in contact (Rozin, Millman, and Nemeroff 1986; Rozin and Nemeroff 1990).[2] Argo et al. (2006), for example, find that consumers are averse to purchasing products recently handled by a fellow consumer. A subsequent study, however, indicates that reactions to others' touch are context-dependent, as consumers perceived products more positively when they had been previously handled by an attractive other consumer (Argo, Dahl, and Morales 2008).[3]

As one of the most common objects with which people interact, the workplace is one of the most well-documented targets of ownership formation. Research indicates that workplace ownership benefits both workers and organizations, as ownership is positively associated with organizational commitment and job satisfaction (Van Dyne and Pierce 2004; Avey, Avolio, Crossley, and Luthans 2009; Pierce et al. 2009; Peng and Pierce 2015). Other research indicates a positive relationship between workplace ownership and performance, where ownership motivates workers to exert greater effort in their work (Brown et al. 2014; Wang et al. 2019; Zhang, Liu, Zhang, Xu, and Cheung 2021). While

[2] While the contagion heuristic has been traditionally tested using tangible objects and physical contact (e.g., Argo et al. 2006), recent research suggests that tangibility may not be necessary, as people experience feelings of ownership for intangible objects (Belk 2013; Kirk et al. 2018; Morales, Dahl, and Argo 2018).

[3] Similarly, Newman, Diesendruck, and Bloom (2011) indicate that positive contagion may explain consumers' willingness to pay large sums of money for celebrities' former possessions.

organizational commitment and job satisfaction are likely important for the audit workplace, an association between ownership and effort holds particular meaning for auditors' judgment and decision-making, as auditors are more effective when they exert greater effort (e.g., Zhao et al. 2017), as well as when they are intrinsically motivated (Kadous and Zhou 2019). As such, I expect that auditors' perceptions of work ownership are positively associated with their effort and the quality of their judgment and decision-making processes.

As mentioned previously, ongoing changes in financial reporting complexity and the broad economy have led to changes in audit work structure, where multiple auditors are often involved in work traditionally performed by an individual. While work compartmentalization offers a variety of benefits to the audit, it may also harm individual auditors' perceptions of work ownership for several reasons. First, auditors are less likely to interact with compartmentalized work and, thus, are less able to form ownership through self-investment, control, and the acquisition of task-specific knowledge. Second, work division introduces colleagues' competing ownership, which may negatively impact ownership and performance by inducing territorial behavior and disengagement with shared work. Kirk et al. (2018), for example, find that consumers respond aggressively and defensively when another person threatens their ownership. Wang et al. (2019) note similar behavior in a work setting, where workers who are motivated to prevent the loss of their ownership are more likely to withhold knowledge and engage in unethical behavior than those who are promotion focused. When "self" ownership has not been established prior to the introduction of "other" ownership, however, research indicates a different reaction, disengagement, with work handled by a colleague (Argo et al. 2006; Morales et al. 2018).

Recent audit research provides evidence consistent with both territoriality and disengagement in response to "other" ownership. The auditors in Griffith (2020), for example, exhibit territoriality in their disregard for specialist-provided recommendations, similar to the territorial behavior noted by Kirk et al. (2018). In their worse performance of unfinished work that is perceived to be of low significance to the audit, participants in Downey (2018) exhibit behavior that is conceptually similar to the disengagement noted by Argo et al. (2006). I expect that this second behavior – disengagement – is most applicable for my experimental setting, where I do not allow auditors the opportunity to form ownership before introducing a colleague's ownership.

I follow Commerford et al. (2017), who find that auditors select non-representative samples to avoid interacting with difficult client managers when they are responsible for both sample selection and evidence evaluation. As a potential intervention, however, they note that auditors are less likely to consider client manager difficulty in selecting a sample for a colleague to test. I extend their findings by considering the implications of dividing sample selection and evidence evaluation from the perspective of auditors who receive and evaluate a sample selected by another auditor, with a specific focus on evidence evaluating auditors' work ownership, engagement, and performance. I expect that auditors who test a sample selected by a colleague will be less likely to engage and develop ownership than auditors who perform both sample selection and evidence evaluation. Further, I expect a difference in performance, such that auditors will identify less errors when they test a sample selected by a colleague compared to a sample selected by themselves.

> **H1:** Auditors will correctly identify a lower number of errors when they evaluate evidence that relates to a sample selected by a colleague auditor compared to a sample selected by themselves.

II.C Technology and Work Ownership

Reflective of broad societal trends in automation and technological advancement (e.g., Daquila and Shirer 2019), audit firms continue to indicate significant interest and investment in the development of technological tools (e.g., Deloitte 2016; KPMG 2017; EY 2018). Kapoor (2020), for example documents over $9 billion pledged towards technological innovation over a five-year window by three of the Big 4 firms. As a result of increased attention and investment, auditors are increasingly likely to interact with technology in their work, although the specific nature of auditor-technology interaction may be subject to change as technology becomes more capable and autonomous. Despite the expected benefits of technology to the audit, regulators continue to stress the need for appropriate auditor judgment, particularly when their work involves technology. This sentiment is reflected in a 2017 speech by former PCAOB board member Steven B. Harris, who indicated that, "as powerful as these tools are, or are expected to become, they nonetheless are not substitutes for the auditor's knowledge, judgment, and exercise of professional skepticism."

Given the growing importance of technology to the audit, a growing literature considers how auditors interact with technology in a variety of tasks and settings. In a complex estimate setting, Commerford et al. (2022) find that auditors are more hesitant to rely on an estimate produced by an artificial intelligence expert than they are a similar estimate produced by a human expert, a finding indicative of auditor algorithm aversion (see, e.g., Dietvorst, Simmons, and Massey 2015; Burton, Stein, and Jensen 2019). In less complex settings, however, research indicates potential appreciation of technology (e.g., Castelo et al. 2019; Logg et al. 2019). Cooper et al. (2019) note that auditors report a variety

of benefits associated with the automation of simple or repetitive processes, particularly efficiency-related bonuses as technology reduces much of the time associated with mundane, data-intense tasks. Likewise, Christ, Emett, Summers, and Wood (2021) indicate that drones and automated counting software can improve the accuracy of inventory counts, although they also note that firms appear hesitant to be "first movers" with respect to especially innovative technologies.

As the preceding discussion illustrates, technology has the potential to impact auditors both positively and negatively, which highlights the importance of understanding the conditions and settings that lead to a positive versus negative reaction to technology in the audit. While auditors may hesitate to work with technology in complex or subjective tasks, they are likely more willing in simple or objective tasks, an expectation that is supported by the psychology and computer science literatures (e.g., Castelo et al. 2019; Logg et al. 2019). Hinds et al. (2004), for example, note that people retain a stronger sense of personal responsibility for work performed in collaboration with a robotic system compared to another human. They note a similar difference in responsibility when collaboration involved a clearly non-human robotic system compared to a complex, human-like robot, which they interpret as evidence that differences in responsibility may be attributable to fundamental differences in humans' and robots' shared identity.

A separate body of research indicates variation in human-technology interaction based on whether technology is perceived as possessing human-like characteristics (i.e., anthropomorphism). Specifically, people tend to trust anthropomorphized (or human-like) technology more than they do non-anthropomorphized technology, given their perceptions of anthropomorphized technology as an autonomous social actor (Waytz, Cacioppo, and

Epley 2010; Waytz, Heafner, and Epley 2014). Similarly, Yang, Aggarwal, and McGill (2020), in a review of research related to technological anthropomorphism, note that people likely perceive anthropomorphized technology as a competitor, as well as a barrier or threat to their personal goals and autonomy, similar to how they might perceive another person. That said, people are less likely to perceive non-anthropomorphized technology as a competitor or threat. As I am primarily concerned with auditors' interaction with non-anthropomorphized technology in a simple audit task, I expect auditors to perceive technology as less of a threat to their work ownership and task control, which I further expect has a positive effect on performance when sample selection is performed by a technological system compared to a colleague human auditor.

Other research indicates that people view technology as exerting less effort than non-technology, which I expect to be associated with weaker "other" ownership due to less perceived "investment of self." Bechwati and Xia (2003), for example, find that consumers viewed a decision as less effortful when it was made with the assistance of an electronic decision aid compared to a non-electronic decision aid. In an audit setting, Emett et al. (2021) find that audit managers viewed staff auditors' use of technology-intense data and analytics (D&A) procedures more negatively than traditional sample-based testing, given their perceptions of D&A as lower in effort and quality than sample-based testing. While their findings are consistent with an "effort heuristic" (see, e.g., Kruger, Wirtz, Van Boven, and Altermatt 2004), they are surprising as D&A procedures have the ability to test entire populations, which is associated with higher actual audit quality. In an audit negligence setting, Libby and Witz (2020) note that jurors were less likely to find auditors negligent for potential audit failure when auditors performed work extensively with technology.

Specifically, jurors were more likely to view auditors as independent when their work involved technology, given that technology reduced jurors' perceptions of auditors abilities to inject their personal biases into their testing procedures. In short, the above research suggests that any ownership associated with a technological system is likely weaker due to a weaker perceived "investment of self" associated with technology. To the extent that sample selection performed by technology is associated with weaker perceived self-investment, I expect that evidence evaluating auditors will experience higher ownership and better performance.

To summarize, I expect that people are less likely to perceive technology as competing for work ownership compared to a human colleague given differences in how people perceive technology compared to other humans, or human-like technologies (Hinds et al. 2004; Yang et al. 2020). Further, when technology is viewed as a competitor, research indicates that it is likely associated with weaker "other" ownership, given reduced effort and self-investment (e.g., Emett et al. 2021), as well as reduced ability to exercise judgment or inject personal bias into a given task (e.g., Libby and Witz 2020). In my experimental setting, I expect weaker "other" ownership associated with the performance of sample selection by a technological system, which I expect will have less impact on evidence evaluating auditors' engagement and work ownership than sample selection performed by a human colleague. I further except auditors will exhibit better performance when sample selection is performed by a technological system compared to a human colleague, which I expect to manifest through auditors' identification of errors.

> **H2:** Auditors will correctly identify a higher number of errors when they evaluate evidence that relates to a sample selected by a technological system compared to another human auditor.

While I expect that auditors' ownership and performance will be higher when evidence relates to a sample selected by a technological system compared to a colleague, it is less clear whether participants' ownership and performance differ significantly when auditors evaluate evidence that to a sample selected by a technological system compared to sample selected by auditors themselves. Non-involvement in sample selection, regardless of whether sample selection is performed by a colleague auditor or a technological system, likely reduces ownership by reducing auditors' effort (i.e., investment of self) and task-specific knowledge. That said, it is less clear how auditors perceive a division of control with technology given prior research in human-technology collaboration (Hinds et al. 2004) and human-technology competition (Yang et al. 2020). As my experimental setting holds sample selection non-involvement constant across conditions, I do not expect significant variation in effort or task-related knowledge. As such, I am agnostic with respect to a directional prediction for how evidence evaluating auditors perceive sample selection performed by a technological system compared to participants themselves. I thus propose the following research question:

> **RQ:** To what extent does auditors' identification of errors differ when auditors evaluate evidence that relates to a sample selected by themselves versus a sample selected by a technological system?

III. EXPERIMENTAL DESIGN – EXPERIMENT I

III.A Overview

Experiment 1 is a 3 x 1 between-subjects experiment, where participants assumed the role of staff auditor in performing a sample-based search for unrecorded liabilities. Sample-based testing is ubiquitous to the financial statement audit and is used to test financial statement information as well as the operating effectiveness of internal controls

(Elder, Akresh, Glover, Higgs, and Liljegren 2013; Christensen, Elder, and Glover 2015). In addition, sample-based testing is a common part of a formal accounting education (e.g., Richardson and Louwers 2010; Dickins, Fallatah, and Higgs 2013). Given such widespread use, I expect prospective experimental participants at any level of actual experience to have at least a rudimentary understanding of sample selection and evidence evaluation, which is useful in mitigating the potential impact of experimental novelty. Further, sample-based testing is a simple and objective task, which I expect is less likely subject to the same confounds that may be present in complex and subjective audit tasks (e.g., algorithm aversion).

To perform sample-based testing, auditors first select a sample of testable items from a larger population. Although have considerable flexibility in determining how they conduct sample selection, current standards recommend that auditors consider tolerable misstatement, allowance for incorrect acceptance, and the underlying characteristics of a population in determining sample size and selection strategy (PCAOB 2016). Once a sample has been selected, it is then tested through an evaluation of relevant evidence, often obtained with some assistance from the client (e.g., Bennett and Hatfield 2013; Saiewitz and Kida 2018). Evidence is evaluated against the client's financial records, with any misstatements extrapolated to the larger population of untested items to account for potential undetected errors. While sample selection and evidence evaluation are clearly related, as a sample must be selected before evidence can be obtained and evaluated, the procedures and skillsets to perform sample selection and evidence evaluation are different. As such, auditors' consideration of evidence should not be predicated on the extent of their involvement, or lack of involvement, in sample selection.

III.B Participants

Prior research indicates that audit staff and seniors are typically responsible for performing sample-based testing procedures (Hall, Heron, Pierce, and Witt 2001; Sanders, Keune, and Hawkins 2021). Given significant barriers to the recruitment of practicing auditors for academic research studies, recent experimental research indicates that students may be acceptable proxies for inexperienced auditors, provided their experience and formal education align with a given task (Bennett and Hatfield 2013; Commerford et al. 2017). As my experimental task consists of an objective evidence evaluation task and does not require any specialized skillset, I consider students as reasonable proxies for inexperienced auditors and capable of performing my experimental task.[4] I recruit participants from students enrolled in an audit course at two public universities.[5] A total of 74 students participated in my experiment, with data collected in two experimental administrations.

Participants in the first administration were 30 students recruited from a master's level audit course. No incentive was offered for participation. Participants in the second administration were 44 students enrolled in an undergraduate audit course.[6] In contrast to the first administration, participants in the second administration were incentivized to participate with a $10 gift card. I fail to find any statistically meaningful difference in participants' ability between administrations, which I measure using three open-ended knowledge check questions administered near the beginning of the experiment (t [72] =

[4] It is worth noting that the experimental instrument I adapt for this study was previously used on a participant group that consisted of practicing auditors and accounting students (Downey 2018).
[5] Institutional Review Board approval was obtained prior to data collection.
[6] Participants in the first administration completed experimental materials at the beginning of the semester, where participants in the second administration completed experimental materials at the end of the semester. As a result, participants across administrations had virtually identical levels of formal audit education (i.e., one fully completed audit course).

0.91, $p = 0.37$; two-tailed). As such, participants appear equally capable with respect to purchasing and payables. As such, I combine responses across administrations in statistical analyses (e.g., Pyzoha, Taylor, and Wu 2017). However, given differences in both incentives and formal education between administrations, I control for participants' experimental administration and formal education status (junior, senior, or recent graduate/graduate student) in all analyses.

III.C Task Description

After providing their consent to participate in the experiment, participants responded to three open-ended knowledge check questions designed to assess their understanding of the accounting purchasing and payables cycle. Assessing participants' understanding of purchasing and payables is critical to ensure that participants understand the experimental setting, which is particularly important given that actual audit experience was not a prerequisite to participation. After responding to knowledge-check questions, participants were introduced to the experimental task and instructed to assume the role of staff auditor for Fluss, Ibaia, and Rio, a fictitious international accounting firm.[7] Participants were further told that they had been assigned to the audit of Carpenter & Weaver Construction (C&W), adapted from Westermann (2011) and Downey (2018). Participants were randomly assigned to one of three experimental conditions and presented information on their assigned condition. Participants were also provided information on the sample selection strategy used to test unrecorded liabilities, which indicated mandatory selection of all client disbursements above a $10,000 threshold, with random sampling to

[7] See Appendix A for the experimental materials used in Experiment 1.

form a final sample of either ten or twelve disbursements, depending on experimental administration.[8]

After proceeding through background materials, participants were asked to review C&W's year-end accounts payable listing, as well as a list of cash disbursements made within the first fifteen days of the end of the fiscal year. After verifying that they had reviewed C&W's financial information, participants proceeded to the main experimental task, where they were asked to evaluate vendor invoices against C&W's year-end payables and list of subsequent disbursements. For each invoice, participants were asked to provide their judgment of the year in which an expense should have been recorded (from the invoice), as well as the amounts that C&W had appropriately recorded versus inappropriately recorded (from the invoice and C&W's financial information). If a definitive conclusion was not supported by the provided evidence, participants were allowed to request additional information. Regardless of conclusion, however, participants were not required to evaluate any additional information to ensure that effort was held similar between participants.

After evaluating invoices, participants were asked whether they were aware of any errors in the sample selection process, in addition to whether there were any additional invoices they perceived as requiring follow-up.[9] Participants responded to two three-item inventories to measure their perceptions of work ownership and engagement. After responding to several additional post-experimental questions, participants were thanked for

[8] Participants in the first administration evaluated 12 invoices, whereas participants in the second administration evaluated only 10 invoices. I conduct all analyses on a set of nine invoices (three error; six non-error) that were presented identically across administrations.

[9] An error was intentionally coded into the sample selection process, where an item that objectively should have been selected (i.e., above the $10,000 threshold) was not selected. Only one participant indicated a relevant concern with sample selection.

their time and the study ended. Participants in the second experimental administration were redirected to a separate survey that collected their email addresses and names to allow for distribution of their $10 electronic gift card.

III.D Independent Variables

I manipulate my independent variable of interest, sample source, at three levels: evidence evaluating auditors (*Self*), a colleague staff auditor from participants' same firm (*Staff Auditor*), or a technological system (*System*). Participants in the *Self* condition were told that the evidence they were responsible for testing was related to a sample they had previously selected themselves. While this manipulation is weaker than allowing participants to select and test their own samples, holding samples and evidence constant across conditions was necessary to allow for a comparison of performance between conditions, which would have been difficult if participants been allowed to select their own samples. Further, recent research suggests that merely thinking about customization is sufficient to induce perceptions of ownership. Kirk et al. (2018), for example, note that asking participants to imagine customizing a beverage resulted in a measurable increase in ownership compared to others who were not asked to imagine customizing their own beverage. This difference in ownership corresponded with similar differences in ownership when individuals were allowed to customize a physical beverage.

Participants in the second condition, *Staff Auditor*, were told that sample selection has been previously performed by Taylor Zimmerman, a colleague staff auditor at Fluss, Ibaia, and Rio. I intentionally withheld information related to Taylor's gender, age, appearance, and prior performance to avoid introducing potential biases related to participants' perceptions of those characteristics (e.g., Argo et al. 2008; Bennett and

Hatfield 2013; Knobloch-Westerwick, Glynn, and Huge 2013).[10] The final experimental condition, *System*, presented sample selection as having been performed by the F.I.R. system, which was described to participants as a proprietary audit technology developed by the firm to assist with simple audit procedures. The use of automated technology in sample selection is consistent with recent research, which indicates that sample selection is one of several audit tasks that can be easily automated (e.g., Cooper et al. 2019). I do not provide any information on the F.I.R. system's error rate, nor do I provide any indicators of the overall quality of its work. Given the objective nature of sample selection and my use of random assignment, I do not expect individual participants' attitudes towards technology to have a significant effect on statistical results.

III.E Dependent Variables

I measure my primary dependent variable, performance, through participants' correct identification of three errors that were seeded in their evaluation of vendor invoices. Seeded errors differ in their nature and relationship with C&W's recorded liabilities and are presented in Table 1. Responses were scored as correct when participants identified either the amount of an invoice correctly recorded, or the amount of an invoice incorrectly recorded. Error 2, for example, consisted of an $81,000 invoice, where $40,500 had been correctly recorded, whereas the other $40,500 had been incorrectly recorded as a liability as opposed to a prepaid asset. Individual responses were thus considered correct if they noted that $40,500 of the total $81,000 was inappropriately recorded, that only $40,500 was appropriately recorded, or if they requested additional information. Non-errors were

[10] Any residual between-participants differences in perception would not be expected to significantly impact results given my use of random assignment.

scored as correct when participants indicated the correct amount recorded for each invoice.[11]

Table 1

Errors – Experiment 1

	Description	**% of Correct Responses**
Error 1	The invoice provided did not match accounts payable and cash disbursement listings, in that both company name and invoice numbers did not match. Participants should have withheld an opinion on whether the liability was appropriately recorded and, instead, requested additional support. Participants received a score of "1" if they either requested additional information or identified the invoice as inappropriately recorded.	61%
Error 2	The company inappropriately recorded an $81,000 invoice as a payable, when half of the amount (40,500) was prepaid for future services. Participants should have responded that $40,500 of the liability was appropriately recorded and that $40,500 of the liability was inappropriately recorded. Participants received a score of "1" if they identified either $40,500 of the invoice as appropriately recorded or $40,500 of the invoice as inappropriately recorded.	48%
Error 3	The company inappropriately excluded an expense for $11,682 that the invoice suggested should have been included in year-end accounts payable. Participants should have indicated that liabilities were underreported by the entire excluded invoice amount, $11,682. Participants received a score of "1" if they identified the $11,682 invoice as inappropriately recorded.	45%

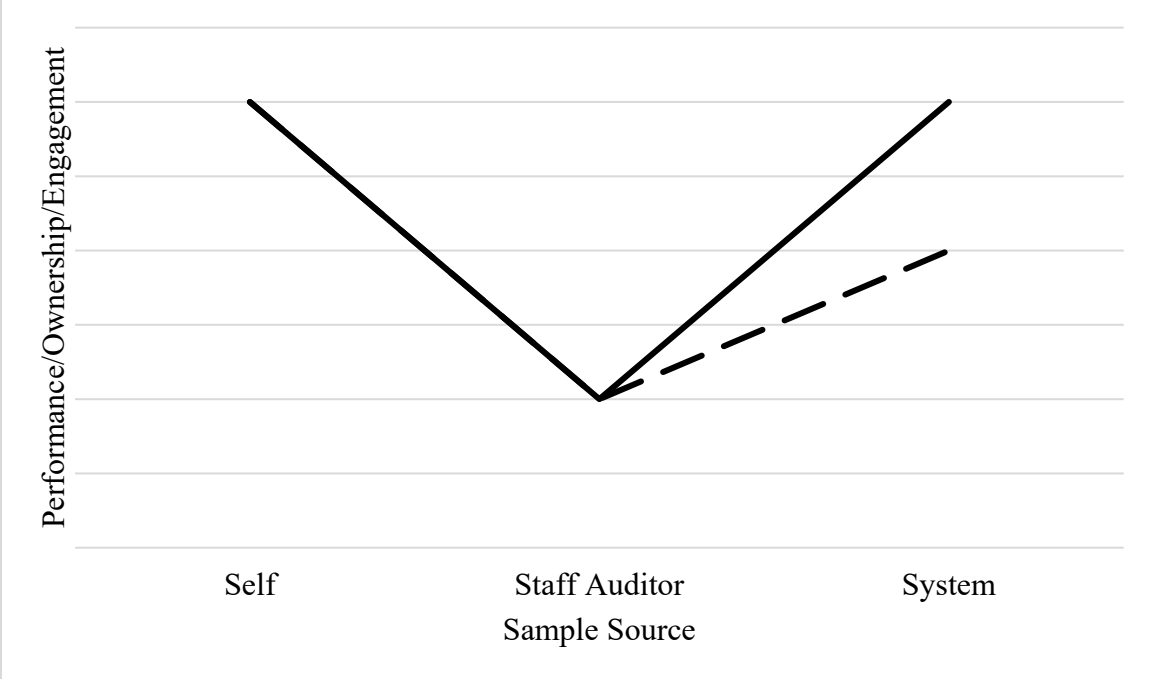
Note: The above table lists seeded errors, as well as the appropriate responses that would have been scored as correct. The table also lists the percentage of responses that correctly identified each error, out of the 56 total responses included in final analyses.

[11] While seemingly straightforward, several correctly recorded invoices involved client-recorded amounts that were different from the amount indicated by a given invoice. One invoice, for example, involved an equipment rental of $2,255.00 which was split between the end of the previous year as well as the beginning of the current year. Half of the invoice, $1,127.50, had appropriately been recorded as a liability, whereas the other half, $1,127.50, had appropriately not been recorded as a liability.

To measure my hypothesized process (see Wang et al. 2019), I collect participants' perceptions of work ownership, which I measure using the average of a three-item inventory adapted from Brown et al. (2014). I also collect participants' perceptions of work engagement, which I measure using the average of a three-item inventory adapted from Rich et al. (2010) and Christian et al. (2011). I collect additional measures of perceived task significance (Downey 2018), and, for participants in *Staff Auditor* and *System* conditions, the perceived division of responsibility for an adverse outcome related to deficient performance of evidence evaluation. Figure 1 depicts the expected pattern of results for performance, work ownership, and engagement. I include two potential values for the *System* condition in Figure 1 to account for ambiguity in my research question, as performance, work ownership, and engagement could be both significantly higher than the *Staff Auditor* condition and significantly lower than the *Self* condition.

Figure 1

Expected Pattern of Results – Experiment 1



Note: I measure my primary dependent variable, auditor performance, using the number of errors each participant correctly identifies, out of a maximum of three. I expect a similar pattern of results for perceived work ownership and engagement, which I measure using a scale adapted from Brown et al. (2014) and Rich et al. (2010), respectively. Given ambiguity in the relationship between *Self* and *System* conditions (RQ), I include two possible values for *System,* denoted by the solid line and dashed line between *Staff Auditor* and *System* conditions.

IV. RESULTS OF ANALYSES – EXPERIMENT I

IV.A Excluded Observations

Prior to accessing the experimental task, I require participants to answer three open-ended knowledge check questions to assess their understanding of the accounting purchasing and payables cycle, which is important given my use of students as proxies for inexperienced auditors. I require participants to answer at least one question correctly to be included in analyses.[12] I exclude eight participants without a single correct knowledge check response.[13] I exclude an additional eight participants for taking longer than 120 minutes to complete the experiment, given explicit instructions to complete the experiment in one sitting or 120 minutes.[14] Finally, I exclude two participants for providing exceptionally low-quality responses throughout the experiment, which results in a final sample of 56 observations.[15]

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[12] Results of my analysis of performance are robust to requiring participants to answer at least two questions correctly for their responses to be included in analysis ($F_{2,40} = 3.96$, $p = 0.01$; one-tailed).
[13] An additional multiple-choice question was posed to participants. I do not consider a correct response to this question as sufficient for inclusion in analyses, given that 1) an online query returned the correct answer, and 2) participants could have guessed the correct response.
[14] Results for my primary dependent variable, identified errors, remain statistically significant when I include observations with a completion time of more than 120 minutes ($F_{2,58} = 4.52$, $p < 0.01$; one-tailed).
[15] One excluded participant entered "idn" (i.e., I don't know) into each open text box when asked for a justification of their decisions to withhold a conclusion in favor of additional information. The second respondent copied and pasted numbers from the first invoice into each subsequent invoice, in addition to typing random numbers into other text boxes. Results are similar if these responses are not excluded ($F_{2,53} = 3.31$, $p = 0.02$; one-tailed).

IV.B Descriptive Statistics

Table 2 presents descriptive statistics for performance, work ownership, and engagement. Panel A presents descriptive statistics for performance, which is a count variable ranging from zero to three. This pattern of results, depicted in Figure 2, appears visually consistent with my predictions (Figure 1). Participants in the *Self* condition identified an average of 1.79 errors, compared to 1.16 errors in the *Staff Auditor* condition and 1.72 errors in the *System* condition. These values correspond with an identification rate of 60%, 39%, and 57% in the *Self, Staff*, and *System* conditions, respectively.

Panel B presents descriptive statistics for perceived work ownership, which I measure using the average of a three-item inventory adapted from recent workplace ownership research (Brown et al. 2014; Wang et al. 2019). I measure all three items using seven-point Likert scales, where a value of seven indicates higher ownership and a value of one indicates of lower ownership. Reliability analysis indicates that ownership measures have a high degree of internal consistency, with a Cronbach's α of 0.93. Reported ownership was above the midpoint, 4.00, across conditions, with an average of 4.98 in the *Self* condition, 4.57 in the *Staff Auditor* condition, and 5.04 in the *System* condition. Similar to the visual pattern of results for identified errors, the graphical pattern of reported ownership, depicted in Figure 3, appears visually consistent with my expectations.

Panel C presents descriptive statistics for my final process measure, work engagement, with graphical depiction of results in Figure 5. I measure work engagement using the average of a three-item inventory adapted from Rich et al. (2010) and Christian et al. (2011). I measure all three inventory items using seven-point Likert scales. A value of seven indicates high engagement and a value of one indicates low engagement.

Reliability analysis indicates high internal consistency between engagement measures, with a Cronbach's α of 0.96. Participants reported work ownership of 5.19 in the *Self* condition, 4.35 in the *Staff Auditor* condition, and 5.27 in the *System* condition. Correlation analysis indicates that ownership and engagement are significantly and positively correlated ($r = 0.40$, $p < 0.01$; two-tailed), consistent with Wang et al. (2019), who indicate that engagement is an antecedent to ownership.

Table 2

Descriptive Statistics – Experiment 1

Panel A: Mean (Std. Dev) of *Identified Errors*

Self	***Staff Auditor***	***System***
1.79	1.16	1.72
(1.03)	(0.96)	(0.90)
n = 19	n = 19	n = 18

Panel B: Mean (Std. Dev) of *Perceived Ownership*

Self	***Staff Auditor***	***System***
4.98	4.57	5.04
(1.45)	(1.40)	(1.87)
n = 19	n = 19	n = 18

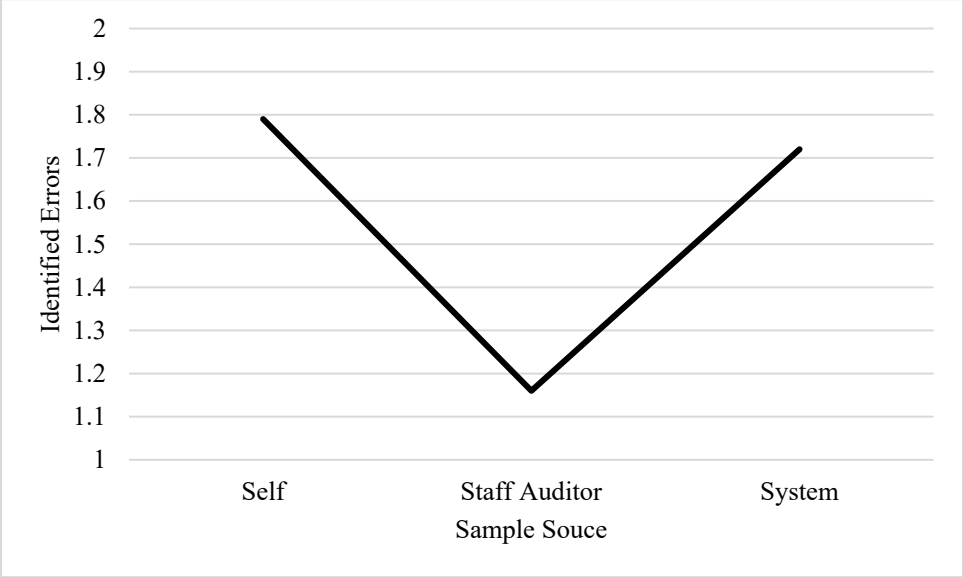
Panel C: Mean (Std. Dev) of *Task Engagement*

Self	***Staff Auditor***	***System***
5.19	4.35	5.27
(1.26)	(1.67)	(1.30)
n = 19	n = 19	n = 18

Note: Panel A provides descriptive statistics for identified errors, out of a maximum of three errors. Panel B presents descriptive statistics for perceived ownership, which I measure using the average of a three-item inventory, adapted from Brown et al. (2014). Panel C presents descriptive statistics for task engagement, which I measure using the average of a three-item inventory adapted from Rich et al. (2010) and Christian et al. (2011).

Figure 2

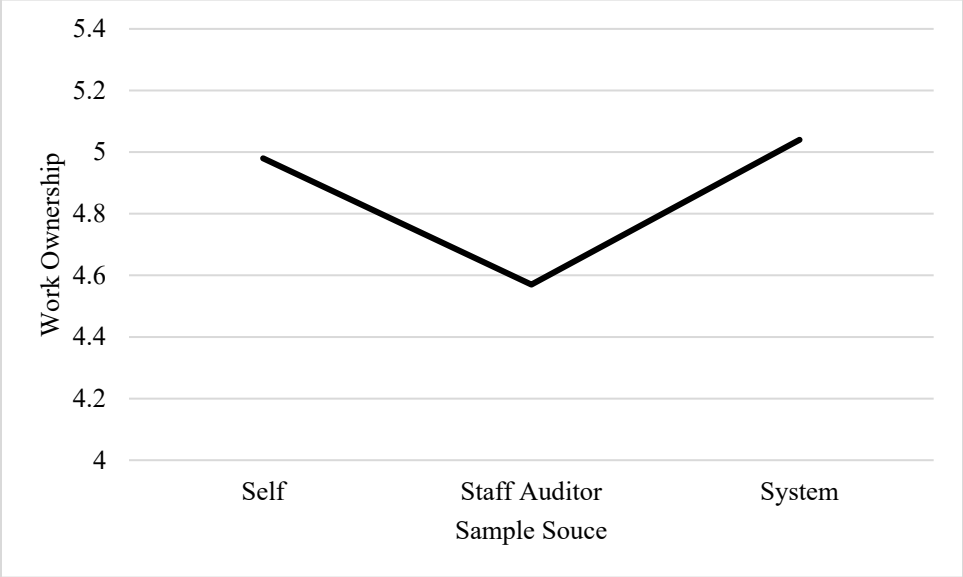
Identified Errors by Condition – Experiment 1



Note: Figure 3 depicts my primary dependent variable, auditor performance, which I measure using the errors identified by each participant. Values range from zero to three.

Figure 3

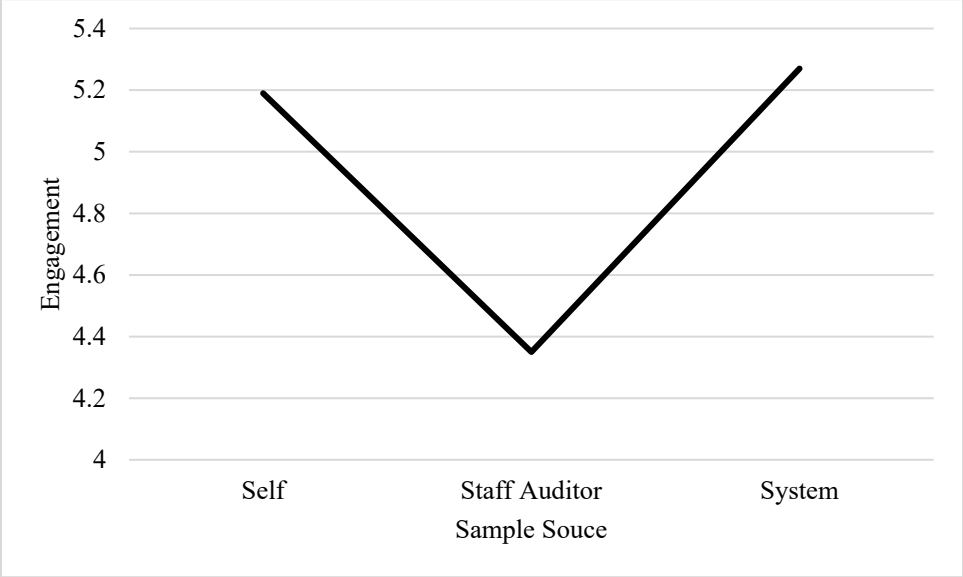
Work Ownership by Condition – Experiment 1



Note: Figure 4 depicts perceived work ownership, which ranges from one to seven and is measured using the average of a three-item inventory adapted from Brown et al. (2014).

Figure 4

Word Engagement by Condition – Experiment 1



Note: Figure 4 depicts engagement, which ranges from one to seven and is measured using the average of a three-item inventory adapted from Rich et al. (2010) and Christian et al. (2011).

VI.C Tests of Hypotheses

Hypothesis 1 predicts that auditors will identify less errors when they test a sample selected by a colleague auditor compared to a sample selected by themselves. Hypothesis 2 predicts that auditors will identify more errors when they test a sample selected by a technological system compared to a sample selected by colleague auditor. Given ambiguity in how auditors likely perceive their work when sample selection was performed by a technological system compared themselves, I pose an additional, nondirectional research question that compares identified errors between the *Self* and *System* conditions.

I test my hypotheses and research question using ANCOVA, planned contrasts, and mediation analysis. Table 3 presents the results of initial statistical analysis. Panel A reports the results of ANCOVA considering differences in identified errors between conditions. I

control for experimental administration and participants' educational status as covariates.[16] Results indicate a significant effect of sample source on identified errors ($F_{2,51} = 4.09$, $p = 0.01$; one-tailed), consistent with my expectations that sample source affects auditors' evidence evaluation. I note the absence of a statistically significant effect of both experimental administration and education experience on identified errors.[17]

Panel B reports the results of ANCOVA for auditors' work ownership. While the graphical pattern of results depicted in Figure 3 appeared similar to my expectations, I fail to find evidence of a statistically significant effect of sample source on perceived work ownership ($F_{2,51} = 0.85$, $p = 0.22$; one-tailed). This lack of significance is inconsistent with my expectations, although recent research indicates it is possible for sample source to have an indirect effect on ownership through engagement (Wang et al. 2019). Panel C reports the results of ANCOVA considering the effects of sample source on engagement. Consistent with my expectations, I find a significant effect of sample source on reported engagement ($F_{2,51} = 2.37$, $p = 0.05$; one-tailed), which suggests that participants' engagement with evidence evaluation was significantly affected by the source of the sample they were responsible for testing.

To better understand differences in performance between conditions, I perform contrast testing, with results reported in Panel D. In support of Hypothesis 1, participants

[16] Education and experimental administration are significantly correlated ($r = 0.81$, $p < 0.01$; two-tailed), which raises potential questions of multicollinearity when controlling for both in the same model. That said, regression analysis indicates a variance inflation factor of 3.03, which is safely below a generally accepted rule-of-thumb of 5.00. Further, neither administration nor education are significantly correlated with assigned sample selection condition, which indicates that a correlated omitted variable is unlikely.

[17] Results vary slightly when I do not include any covariates ($F_{2,53} = 2.31$, $p = 0.06$; one-tailed), educational status only ($F_{2,52} = 3.11$, $p = 0.01$; one-tailed), or experimental administration only ($F_{2,52} = 3.90$, $p = 0.01$; one-tailed). Results do not vary when covariates are interacted with sample source.

identified significantly less errors when they tested evidence related to a sample selected by a colleague auditor compared to a sample selected by themselves (t[53] = 2.02, p = 0.02; one-tailed). I find evidence of a similar difference in performance between *Staff Auditor* and *System* conditions, where participants identified more errors when they tested evidence related to a sample selected by an autonomous technological system compared to a colleague auditor (t[53] = 1.78, p = 0.04; one-tailed), consistent with Hypothesis 2. My final contrast compares *Self* and *System* conditions to test my nondirectional research question. I fail to find any statistical evidence of a difference in identified errors between *Self* and *System* conditions (t[53] = 0.21, p = 0.83; two-tailed). This pattern of results is consistent with recent research, which indicates that auditors view simple technology as beneficial to both audit efficiency and effectiveness (Cooper et al. 2019).

Table 3

Tests of Hypotheses – Experiment 1

Panel A: ANCOVA by *Sample Source: Identified Errors*

	Sum of Squares	df	MS	F	p-value
Sample Source	6.44	2	3.22	4.09	0.01[a]**
Education	0.60	1	0.60	0.76	0.39
Administration	1.14	1	1.14	1.45	0.23
Error	40.16	51	0.79		

Panel B: ANCOVA by *Sample Source: Perceived Ownership*

	Sum of Squares	df	MS	F	p-value
Sample Source	3.92	2	1.96	0.83	0.22[a]
Education	7.47	1	7.47	3.18	0.08*
Administration	0.89	1	0.89	0.38	0.54
Error	119.88	51	2.35		

Panel C: ANCOVA by *Sample Source: Engagement*

	Sum of Squares	df	MS	F	p-value
Sample Source	9.48	2	4.74	2.37	0.05[a]**
Education	5.11	1	5.11	2.56	0.12
Administration	4.36	1	4.36	2.18	0.15
Error	101.97	51	2.00		

Panel D: Contrasts Based on ANCOVA: *Identified Errors*

	df	*t*	p-value
H1: Staff Auditor < Self	53	2.02	0.02[a]**
H2: Staff Auditor < System	53	1.78	0.04[a]**
RQ: Self vs. System	53	0.21	0.83

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A presents results of ANCOVA for identified errors between conditions, controlling for experimental administration and education as covariates. Panel B presents results of ANCOVA for perceived ownership between conditions, controlling for experimental administration and education as covariates. Panel C presents results of ANCOVA for engagement between conditions, controlling for experimental administration and education as covariates. Panel D presents results of planned contrasts between conditions.

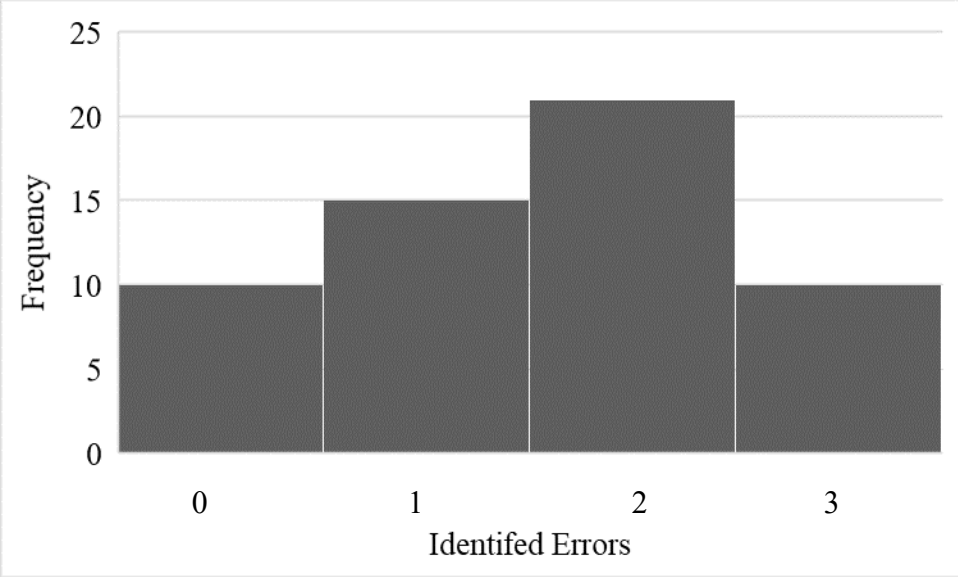
VI.D Tests of Robustness – Normality

My primary dependent measure, identified errors, is a count variable, with values ranging from zero to three. One concern with using ANCOVA to analyze count data is a violation of an assumption of normally distributed data. A histogram of identified errors, Figure 6, indicates that my data is not normally distributed, which I confirm through Shapiro-Wilk normality tests. I find that identified errors in all three conditions are not normally distributed [*Self* $W(19) = 0.86$, $p < 0.01$; *Staff Auditor* $W(19) = 0.85$, $p < 0.01$; *System* $W(18) = 0.87$, $p = 0.02$]. While ANCOVA is robust to violations of normality (e.g., Knief and Forstmeier 2021), I perform additional analysis using statistical procedures that are robust to potential violations of normality. I first conduct a Kruskal-Wallis test, which

is a nonparametric procedure that assesses differences between three or more independent groups on a dependent variable that is potentially not normally distributed (Kruskal and Wallis 1952; Breslow 1970). The results of this analysis indicate a marginally significant difference in identified errors between sample source conditions ($H(2) = 4.43$, $p = 0.06$; one-tailed), which is consistent with the results of previous analysis and indicative that results are robust to violations of normality.

Figure 5

Histogram of *Identified Errors* – Experiment 1



Note: Figure 5 histogram depicts the frequency with which participants identified errors, ranging from zero to three.

I next perform Poisson regression, where I code each condition using a dummy variable. In line with hypotheses, which predict greater identified errors in both *Self* and *System* conditions compared to the *Staff Auditor* condition, I report the results of a specified model where I code *Staff Auditor* as the "control" condition.[18] Consistent with previous

[18] Inferences are similar if *Self* is considered the control. That is, I find a negative and significant coefficient on *Staff Auditor* and a nonsignificant coefficient on *System*.

analysis, I also control for experimental administration and formal education. Table 4 presents the results of this analysis. I note a positive and significant effect of *Self* ($B = 0.48$, $X^2 = 3.10$, $p = 0.04$; one-tailed), in addition to a positive and significant effect of *System* ($B = 0.51$, $X^2 = 3.20$, $p = 0.04$; one-tailed), consistent with previous analysis and my hypotheses. Neither experimental administration nor formal education are statistically significant with respect to identified errors.

Table 4

Poisson Regression – Experiment 1

Variable	***β***	***Wald X^2***	***p-value***
Constant	0.33	0.10	0.75
Self	0.48	3.10	0.04 [a]**
System	0.51	3.20	0.04 [a]**
Administration	-0.32	0.39	0.41
Education	0.14	0.34	0.56

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.

VI.E Mediation Analysis

Although I fail to find evidence of a direct relationship between sample source and perceived work ownership, recent workplace ownership research suggests that perceptions of work ownership may, instead, be indirectly impacted through engagement (Wang et al. 2019). In my experimental setting, an indirect effect of sample source on ownership through engagement would be consistent with research that indicates individuals' tendencies to disengage with objects that have been previously handled by another person

(e.g., Argo et al. 2006, 2008; Morales et al. 2018).[19] I test mediation using Hayes Process Macro in SPSS, where I model task engagement as mediating the relationship between sample source and work ownership (Model 4) [Hayes 2018]. I next test sequential mediation of sample source on identified errors through work engagement and ownership (Model 6). I code my independent variable, sample source, as a multicategorical indicator, which allows for a simultaneous test of all three conditions (Hayes and Preacher 2014). Given hypotheses – and the results of previous analysis – which indicate lower performance in the *Staff Auditor* condition compared to the *Self* and *System* conditions, I code *Staff Auditor* as the "control" condition.

Figure 6 presents the results of analysis considering the indirect effects of sample source on perceived ownership through engagement. Results indicate a statistically significant indirect effect of *Self* on perceptions of work ownership through engagement (90% confidence interval of 0.01 to 0.89).[20] Results also indicate a statistically significant indirect effect of *System* on perceptions of ownership through engagement (90% confidence interval of 0.04 to 0.98). These results are consistent with theory and my expectations, in that auditors were less likely to engage with, and form ownership of, their work when a colleague auditor was involved in sample selection. To provide additional evidence that results are not dependent on coding *Staff Auditor* as the control condition, I reperform mediation analysis coding *Self* as the control, with results depicted in Figure 7. As expected, and consistent with Figure 6, I find a negative and statistically significant

[19] While Argo et al. (2006, 2008) test the effects of "touching" in a physical setting, recent research suggests that people interface, and perceive, interaction with intangible objects similarly to tangible objects (Belk 2013; Hoffman and Novak 2018).

[20] Given directional hypotheses, I test mediation using one-tailed statistical analyses (90% for "significant;" 80% for "marginally significant").

indirect effect of *Staff Auditor* on ownership through engagement (90% confidence interval of -0.90 to -0.01). That said, I fail to find any evidence of a statistically significant effect of *System* on ownership (90% confidence interval of -0.26 to 0.35), consistent with previous analysis that fails to identify a difference in performance between *Self* and *System* conditions.[21]

I next consider the sequential effects of sample source on identified errors through engagement and ownership, with results presented in Figure 8. While ownership and identified errors are marginally correlated (r=0.20, p=0.06; one-tailed), I fail to find evidence of a statistically significant relationship between ownership and errors while controlling for the effects of engagement and sample source on identified errors. As a result, the indirect relationships between sample source conditions and identified errors through ownership and engagement are not statistically significant. This is likely due to some collinearity between engagement and ownership, where both engagement and ownership are positively correlated with performance. Further, given my relatively small sample size, it is possible that a lack of statistical significance may be the result of low statistical power.[22]

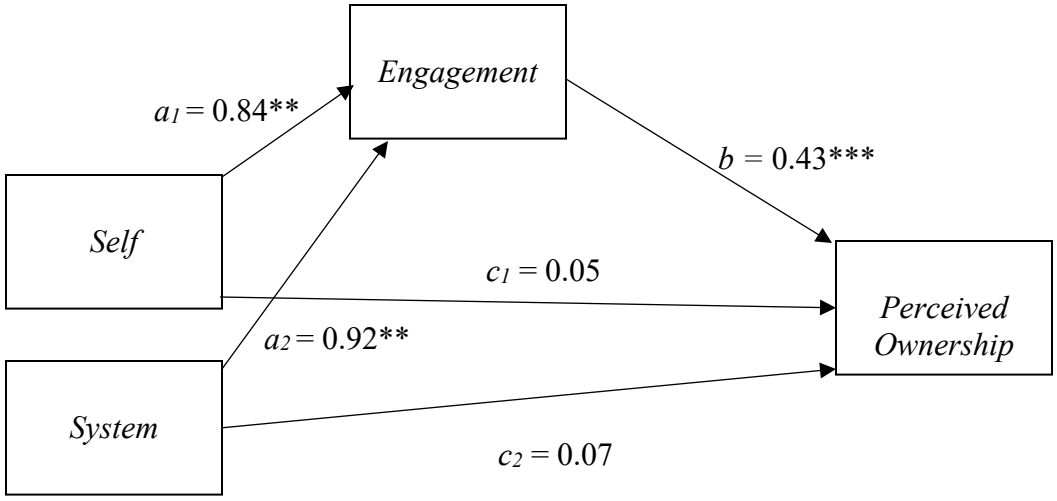
[21] While I do not report the results of controlling for *Education* and *Administration*, results remain significant when they are included as covariates in my ownership mediation models. Results are robust to alternate specification where either covariate is included by itself.

[22] I test an alternative custom Process model where I mute the relationships between *Self, System* and *Performance* and *Engagement* and *Performance*, following Wang et al. (2019). The results of this analysis indicate a marginally significant coefficient of *Ownership* on *Performance* (b = 0.13, p = 0.07; one-tailed), although the indirect effects of *Self* (Bootstrapped CI: -0.01 → 0.16) and *System* (Bootstrapped CI: -0.01 → 0.18) on *Performance* through *Engagement* and *Ownership* are not significant. A lack of significance appears attributable to low statistical power, as anecdotally doubling sample size resulted in statistically significant indirect effects for both *Self* and *System.*

Figure 6

Mediation Analysis – *Perceived Ownership*

Indirect Effects of Sample Source on Perceived Ownership
Self → *Engagement* → *Ownership:* 0.36**
(Bootstrapped CI: 0.01 → 0.89)[a]
System → *Engagement* → *Ownership:* 0.40**
(Bootstrapped CI: 0.04 → 0.98)[a]



The above diagram represents a mediation model, using a multicategorical independent variable where *Self* and *System* conditions are compared against *Staff Auditor*, consistent with predictions (Hayes and Preacher 2014). I use Hayes Process Model 4 with one mediator (Hayes 2018). *Engagement* is measured using the average of a three-item inventory adapted from Rich et al. (2010). *Perceived Ownership* is measured using the average of a three-item inventory adapted from Brown et al. (2014) and Wang et al. (2019). Results are robust to the inclusion of *Education* and *Administration* as covariates.

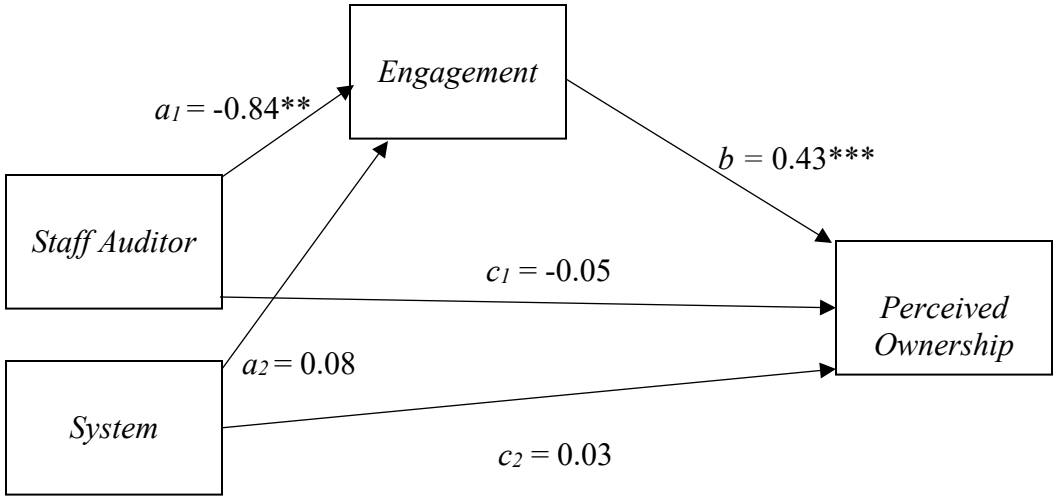
[a] I use confidence intervals from bootstrapped sampling distributions (based on 5,000 bootstrap samples) to test the significance of indirect effects. Consistent with my use of directional predictions, I use confidence intervals at 90% (80%) to test statistical significance at 0.05 (0.10).

* denotes statistical significance equivalent to $p<0.10$, one-tailed.
** denotes statistical significance equivalent to $p<0.05$, one-tailed.
*** denotes statistical significance equivalent to $p<0.01$ one-tailed.

Figure 7

Alternate Mediation Analysis – *Perceived Ownership*

Indirect Effects of Sample Source on Perceived Ownership
Staff Auditor → *Engagement* → *Ownership:* -0.36**
(Bootstrapped CI: -0.91 → -0.01)[a]
System → *Engagement* → *Ownership:* 0.03
(Bootstrapped CI: -0.26 → 0.35)[a]



The above diagram represents a mediation model, using a multicategorical independent variable where *Staff Auditor* and *System* conditions are compared against *Self* (Hayes and Preacher 2014). I use Hayes Process Model 4 with one mediator (Hayes 2018). *Engagement* is measured using the average of a three-item inventory adapted from Rich et al. (2010). *Perceived Ownership* is measured using the average of a three-item inventory adapted from Brown et al. (2014) and Wang et al. (2019). Results are robust to the inclusion of *Education* and *Administration* as covariates.

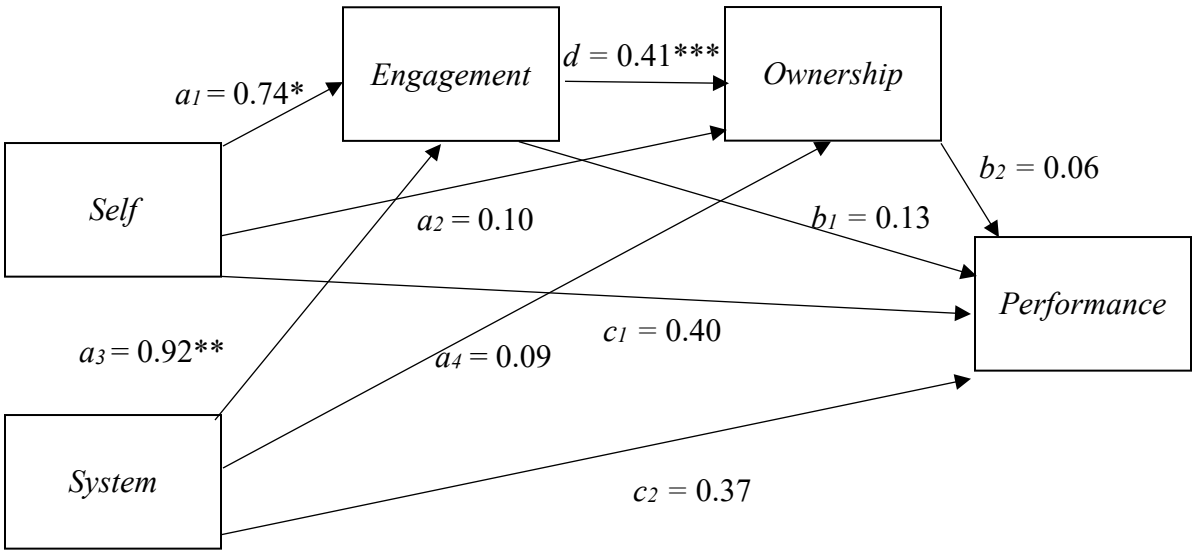
[a] I use confidence intervals from bootstrapped sampling distributions (based on 5,000 bootstrap samples) to test the significance of indirect effects. Consistent with my use of directional predictions, I use confidence intervals at 90% (80%) to test statistical significance at 0.05 (0.10).

* denotes statistical significance equivalent to $p<0.10$, one-tailed.
** denotes statistical significance equivalent to $p<0.05$, one-tailed.
*** denotes statistical significance equivalent to $p<0.01$ one-tailed.

Figure 8

Mediation Analysis – *Performance*

Indirect Effects of Sample Source on Performance
Self → *Engagement* → *Ownership* → *Identified Errors:* 0.02
(Bootstrapped CI: -0.04 → 0.10)[a]
System → *Engagement* → *Ownership* → *Identified Errors:* 0.02
(Bootstrapped CI: -0.04 → 0.11)[a]



The above diagram represents a sequential mediation model, using a multicategorical independent variable where *Self* and *System* conditions are compared against *Other Human Auditor* (Hayes and Preacher 2014). I use Hayes Process Model 6 with two mediators (Hayes 2018). *Engagement* is measured using the average of a three-item inventory adapted from Rich et al. (2010). *Perceived Ownership* is measured using the average of a three-item inventory adapted from Brown et al. (2014) and Wang et al. (2019). *Performance* refers to the number of errors each participant identified (out of three possible).

[a] I use confidence intervals from bootstrapped sampling distributions (based on 5,000 bootstrap samples) to test the significance of indirect effects. Consistent with my use of directional predictions, I use confidence intervals at 90% (80%) to test statistical significance at 0.05 (0.10).

* denotes statistical significance equivalent to $p<0.10$, one-tailed.
** denotes statistical significance equivalent to $p<0.05$, one-tailed.
*** denotes statistical significance equivalent to $p<0.01$ one-tailed.

VI.F Additional Analysis

While I expect that psychological ownership and effort will benefit auditors' error identification, I expect that it is less likely that ownership and effort benefit the evaluation of non-errors, given the diminished incremental benefits of effort when client-provided evidence does not contain any error. That said, it is possible that participants with low perceptions of work ownership and engagement responded randomly in their determination of which amounts were appropriately or inappropriately recorded. As such, differences could exist between conditions for participants' correct identification of both errors and non-errors. Thus, analyzing non-errors is important in determining the discriminant validity of my experimental instrument, as well as whether results are driven by high or low engagement and ownership. I compare participants across six non-errors that were presented identically across experimental administrations.[23]

Table 5 presents the results of this comparison, with descriptive statistics presented in Panel A. Participants in the *Self* condition correctly documented 3.79 invoices as non-errors compared to 3.16 and 3.72 invoices in the *Staff Auditor* and *System* conditions, respectively. The results of ANCOVA, presented in Panel B, fail to identify any statistical difference in documented non-errors based on sample source ($F_{2,51} = 1.06$, $p = 0.36$; two-tailed). As such, it does not appear that participants' documentation of non-errors is dependent on sample source. This is consistent with the lower incremental benefits of effort when the default – i.e., concluding that an invoice is correct – is appropriate. Further, my experimental instrument appears to have reasonable discriminant validity in differentiating

[23] An additional, seventh non-error was intended to be included in testing. That said, some participants in the second experimental condition reported trouble viewing the invoice. As such, I do not include that invoice in analysis. That said, results are robust to alternate specification of non-errors as a percentage versus absolute numbers.

between types of performance, as effort appears incrementally useful when participants consider errors, but not when they consider non-errors.

Table 5

Additional Analysis: Non-Errors – Experiment 1

Panel A: Mean (Std. Dev) of *Non-Errors*

Self	***Staff Auditor***	***System***
3.79	3.16	3.72
(1.72)	(1.80)	(1.18)
n = 19	n = 19	n = 18

Panel B: ANCOVA by *Sample Source: Non-Errors*

	Sum of Squares	**df**	**MS**	**F**	**p-value**
Sample Source	5.20	2	2.60	1.06	0.36
Education	8.66	1	8.66	3.51	0.06*
Administration	3.37	1	3.37	1.37	0.25
Error	125.71	51	2.47		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A provides descriptive statistics for identified non-errors, out of a maximum of six errors. Panel B presents the results of ANCOVA, analyzing participants' identification of non-errors by *Sample Source.*

I next consider the effects of sample source on perceived task significance, given recent research which indicates that auditors' interaction with unfinished work is influenced by the extent to which they view that work as important to the overall audit (Downey 2018). I measure task significance using a seven-point Likert scale where I asked participants to indicate how important the search for unrecorded liabilities task was to the overall performance of the audit. Descriptive statistics, reported in Table 6, Panel A, indicate average perceived significance of 6.20 in the *Self* condition, compared to 5.79 and

5.62 in the *Staff Auditor* and *System* conditions. Results of ANCOVA, presented in Panel B, do not indicate any statistically significant differences in perceived task significance between sample selection conditions ($F_{2,51} = 1.44$, $p = 0.25$; two-tailed).

While I do not find evidence of a significant difference in perceived task significance between conditions, a broad comparison of perceived significance between conditions may not be appropriate, as auditors may perceive differences in significance based on whether work was performed by themselves or other auditors (Downey 2018). In my setting, this implies a comparison between the *Self* condition and combined *Staff Auditor* and *System* conditions. Panel C presents the result of this contrast. I find a marginal difference in perceived task significance between *Self* and non-*Self* condition ($t[53] = 1.68$, $p = 0.06$; one-tailed). This is consistent with prior research and indicates that participants perceived their work as more important when it involved only themselves compared to either a technological system or a colleague.

Table 6

Additional Analysis: *Task Significance* – Experiment 1

Panel A: Mean (Std. Dev) of *Task Significance*

Self	***Staff Auditor***	***System***
6.19	5.79	5.62
(0.54)	(1.05)	(1.37)
n = 19	n = 19	n = 18

Panel B: ANCOVA by *Sample Source: Task Significance*

	Sum of Squares	**df**	**MS**	**F**	**p-value**
Sample Source	3.19	2	1.60	1.44	0.25
Education	0.24	1	0.24	0.22	0.64
Administration	0.23	1	0.23	0.21	0.65
Error	56.48	51	1.11		

Panel C: Contrasts of *Self vs. Non-Self Conditions: Task Significance*

	df	***t***	**p-value**
Self > Staff Auditor + System	53	1.66	0.06[a]*

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A presents descriptive statistics of perceived task significance by sample source (Downey 2018). Panel B presents results of an ANCOVA considering sample source on perceived task significance. Panel C presents results of contrasts using one-way ANOVA.

As a final analysis, I consider whether participants perceived variation in the division of responsibility for a possible adverse testing-related outcome when either Taylor Zimmerman or the F.I.R. system performed sample selection. I draw on Hinds et al. (2004), who find that people retain a stronger sense of personal responsibility when they collaborate with a technological system compared to another person. I measure participants' perceptions of allocated responsibility for both themselves and the sample selecting auditor or system for "errors in testing unrecorded liabilities," using two seven-point Likert scales. Table 7 presents the results of analysis. Panel A provides descriptive statistics of responsibility. Participants reported personal responsibility of 6.19 in the *Self* condition, compared to 5.79 in the *Staff Auditor* condition and 5.62 in the *System* condition. Participants reported "other" responsibility of 4.81 in the *Staff Auditor* condition and 4.43 in the *System* condition.

Given likely differences between participants in total possible responsibility for a possible adverse outcome, I create a scaled measure of responsibility, where I scale "other" responsibility by total responsibility (i.e., both self and other auditor responsibility). This value ranges from zero to one, where a higher value indicates greater "other" responsibility, and a lower value indicates greater "self" responsibility. Participants in the *Staff Auditor*

condition reported scaled responsibility of 0.49, compared to 0.45 in the *System* condition. Panel B reports the results of a contrast of *Staff Auditor* and *System* conditions. Consistent with Hinds et al. (2004), I expect a greater allocation of responsibility to a sample selecting human colleague compared to a technological system. Results are consistent with my expectations in that participants allocated marginally more responsibility for an adverse testing-related outcome to a colleague than they did to a technological system (t [35] = 1.38, p = 0.08; one-tailed).

Table 7

Additional Analysis: *Responsibility* – Experiment 1

Panel A: Mean (Std. Dev) of *Responsibility*

	Self	***Staff Auditor***	***System***
Self_Resp	6.19 (0.54) n = 19	5.79 (1.05) n = 19	5.62 (1.37) n = 18
Other_Resp	- - -	4.81 (1.50) n = 19	4.43 (1.28) n = 18
Other_Resp Scaled	- - -	0.49 (0.07) n = 19	0.45 (0.11) n = 18

Panel B: Contrast of *Staff Auditor vs. System: Other Responsibility-Scaled*

	df	***t***	**p-value**
Staff Auditor < System	35	1.38	0.08[a]*

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test.
[b] P-value based on two-tailed test.
Note: Panel A presents descriptive statistics of responsibility. Panel B presents results of contrasts using one-way ANOVA between *Staff Auditor* and *System* conditions.

V. DISCUSSION – EXPERIMENT 1

In this experiment, I provide evidence that auditors' evaluation of evidence is affected by the sample selection process, in particular, the nature of the auditor responsible for sample selection. Specifically, auditors were more likely to identify errors in client-provided evidence when evidence related to a sample selected by themselves compared to a sample selected for them by a colleague. I find a similar difference in performance when I frame sample selection as having been performed by an autonomous technological system compared to a human colleague, consistent with recent research which indicates individuals' tendencies to appreciate technology in simple and objective tasks (Logg et al. 2019; Castelo et al. 2019). In subsequent process analysis, I find that engagement mediates the relationship between sample source and ownership, where participants were more likely to engage and take ownership of their work when sample selection was performed by either themselves or a technological system. In additional analysis, I find that participants perceived their work as less important when sample selection was performed by a colleague or a technological system, consistent with Downey (2018). I also find differences in participants' allocation of responsibility for a negative testing-related outcome between a system and colleague auditor, such that auditors allocated greater responsibility to a human colleague than they did a technological system.

This experiment contributes to several areas of growing interest in the audit literature. First, I contribute to a growing literature that notes the importance of auditors' attitudinal characteristics on their judgment and decision-making processes. Despite the theoretical importance of work ownership to the audit setting, little prior research has considered the factors that influence how and why auditors develop ownership of their

work. My study indicates an association between work structure and work ownership, which may spark additional interest in other antecedents and outcomes of work ownership, including organizational commitment and job satisfaction, which are particularly important in the turnover-heavy audit industry (Cohen et al. 2017).

Second, this experiment demonstrates the benefits of auditors' interaction with work performed by technology in a simple and objective audit task. While recent research finds that auditors appreciate technology in simple processes (e.g., Moffitt et al. 2018; Cooper et al. 2019), little research has considered explanations for why auditors perceive efficiency and effectiveness-related benefits of technology. Instead, recent research has largely focused on auditors' interaction with complex and subjective tasks and technologies (e.g., Estep et al. 2021; Commerford et al. 2022). It is important to understand auditors' interaction with technology in simple settings, given differences in the factors that underlie human-technology interaction between simple tasks and tools and more complex tasks and tools. In that sense, my experiment is among the first to consider ownership- and engagement-related explanations for auditors' interaction with technology designed to assist with simple and objective tasks.

Finally, this experiment contributes to practice by directly testing the implications of Commerford et al. (2017), who indicate that dividing sample selection and evidence evaluation results in a higher-quality sample than when auditors are responsible for both sample selection and evidence evaluation. While providing a holistic test of their findings is beyond the scope of this experiment, my findings indicate a potential downside to evidence evaluating auditors' performance when they evaluate a sample selected for them by a colleague auditor compared to one selected by themselves. That said, firms may be

able to obtain a high-quality sample without compromising evidence evaluating auditors' performance and work ownership when sample selection is performed by a technological system that operates free of human influence.

VI. EXPERIMENT 2

VI.A Motivation

In Experiment 1, I consider how auditors' evaluation of evidence is impacted by the source of the sample to which their evidence relates, specifically whether auditors evaluate evidence differently when sample selection was previously performed by either a colleague auditor or technological system compared to themselves. Findings indicate differences in performance based on sample source, where auditors identified significantly more errors when evidence related to a sample selected by either themselves or a technological system compared to a colleague staff auditor. While I fail to find evidence of a direct relationship between sample source and work ownership, additional process analysis indicates an indirect effect of sample source on work ownership through engagement, where auditors were more likely to engage with evidence when it related to a sample selected by either participant auditors themselves or a technological system compared to a colleague auditor.

I expand on those findings in a second experiment, where I test the extent to which performance is impacted by variation in a colleague's level of involvement in sample selection, specifically the amount of time and effort a colleague requires to complete all pre-evidence testing activities. This investigation is important in developing an understanding of the underlying mechanisms through which ownership forms, as well as

the mechanism through which "other" ownership is perceived to form.[24] From a practical perspective, similar tasks may require varying amounts of time and effort between engagements, as well as within the same engagement over time. In a survey of experienced audit professionals, Bobek, Daugherty, and Radtke (2012) note clients' late delivery of information as the single-most cited cause of significant audit challenges, potentially straining audit performance by lengthening the time required to complete audit work. Likewise, client managers may be less forthcoming with information based on auditors' tone and choice of communication channel (e.g., Saiewitz and Kida 2018), which could necessitate additional follow-up requests for information or clarification. As such, considering whether auditors are affected by variation in a colleague auditor's involvement offers potential contribution to the audit literature and the broader work ownership literature.

In addition to considering variation in the time and effort a colleague spends in sample selection, the purpose of my second experiment is to consider auditors' perceptions of technology that is explicitly framed as being used by another auditor. While sample selection is the type of simple, repetitive task that could be performed by technology without human input (see Moffitt et al. 2018; Cooper et al. 2019), it is likely that some interaction with a human auditor will be necessary for the near future (e.g., Harris 2017). In that sense, given significant differences in performance favoring sample selection performed by technology compared to a human colleague in Experiment 1, it is also important to understand whether the benefits of involving technology in sample selection

[24] While the work ownership literature indicates that people respond defensively to perceived violations of ownership (e.g., Kirk et al. 2018), it has not considered whether people respond to variation in others' pre-existing ownership.

mitigate the negative effects of a colleague's involvement in sample selection. Understanding how auditors perceive a colleague's technology use offers contribution to other non-sample testing settings where auditors interact with work that is the joint product of a human colleague and a sophisticated technological tool (e.g., Emett et al. 2021).

VI.B Sample Selection Effort and Work Ownership

As noted in Experiment 1, psychological ownership forms through three primary paths: investment of self, perceived control, and the acquisition of intimate knowledge (Pierce et al. 2001, 2003). Research suggests that people are adept at differentiating their ownership from the ownership of others, a relationship that Wang et al. (2019) characterize as "self-object-other." Ross, Friedman, and Field (2015), for example, note that small children acknowledge both their own and others' ownership and act proactively to protect it from infringement. That said, ownership is not inherently zero-sum, in that both "self" and "other" ownership can exist for the same object. Kirk et al. (2018), in their study of consumers' territorial responses to ownership infringement, note that while consumers allocated some ownership to an infringing "other" person, their own perceptions of ownership remained statistically unchanged. In one experiment, for example, they note that consumers allocated some ownership of a beverage to a waiter who moved the beverage without permission, although their own perceptions of ownership with respect to the beverage remained unchanged.

While the discussed examples are not perfectly analogous with my experimental setting, I expect that auditors are able to differentiate ownership based on relative involvement and control of work. In contrast to Ross et al. (2015) and Kirk et al. (2018), however, participants in my study are not given the opportunity to form work ownership

before a colleague is involved in sample selection. This seemingly subtle difference is important, as auditors are unlikely to exhibit the same response to greater "other" ownership that they would, had there been an opportunity for them to form work ownership before a colleague was involved in sample selection. Instead, I find in Experiment 1, that auditors are more likely to disengage with sample selection performed by a colleague. Given a general human capacity to perceive variation in ownership strength, I expect that auditors will disengage with their work to a greater extent when sample selection involves greater colleague ownership, which I posit may form through greater time and effort invested by a colleague in sample selection.

Information on colleagues' involvement in shared work is accessible to auditors through several channels. Budgets, for example, are commonly used to plan the audit, track performance, and make consequential promotion and compensation decisions (Ettredge, Bedard, and Johnstone 2008; Mendoza 2020). While the effects of budgeting on work ownership have not been explicitly considered by prior research, I posit that budgets may also facilitate auditors' comparison of time and effort with their colleague's time and effort, which may affect ownership and performance. Yuksel, Darmody, and Venkataraman (2019), for example, note that online workers felt ownership of crowdsourced work only when they felt that their relative contributions were not less than those of other workers. Similarly, the audit budget may have a negative impact on auditors' work ownership when they perceive that they have not invested as much time or effort in their work compared to a colleague.

In addition to the formal audit budget, auditors may also glean information about colleague's involvement through formal and informal communication channels. Formally,

auditors may learn from shared or prior workpapers, which research indicates may facilitate learning between different engagements, as well as over time on the same engagement (Bonner and Majors 2022). Specifically, auditors may learn about colleagues' work ownership by observing their interaction with shared workpapers, in addition to comparing their own efforts again the efforts of auditors in a prior period. More informally, auditors may learn about colleagues' involvement from "small talk," which prior research indicates has the potential to significantly impact auditor behavior (King 2002; Gissel and Johnstone 2017; Hornok 2019). While extant research has considered "small talk" in the context of audit planning and information sharing, I posit that it may also serve as a channel through which auditors may learn about their colleague's involvement.

In summary, I expect that auditors are able to perceive differences in their colleague's involvement in shared work, which may be communicated through the audit budget, form workpapers, or informal small talk. I further expect that auditors' perceptions of work ownership are indirectly influenced by a colleague's involvement in sample selection, through engagement with shared work, which I demonstrate in my first experiment. I expect that auditors will perceive a colleague's greater involvement in sample selection negatively, which I expect will lead to less engagement and ownership compared to sample selection that requires less involvement. [25]

[25] I use the term "sample selection" to refer to *all* activities that take place before evidence can be evaluated; that said, there are a variety of other activities that may need to take place before an auditor is able to evaluate evidence. For example, auditors need to obtain, sort through, and clean information from the client's general ledger *prior* to selecting a sample. Once a sample has been selected, auditors communicate the sample to the client and then work with the client to obtain appropriate evidence. For the sake of parsimony, I refer to all pre-evidence evaluation activities, collectively, as "sample selection."

H3: Auditors will correctly identify a higher number of errors when sample selection required a colleague auditor to invest less time and effort than expected compared to when sample selection required more time and effort than expected.

VI.C Technology-Assisted Sample Selection

In Experiment 1, I find that auditors experience higher work ownership and engagement, and exhibit stronger performance, when they evaluated evidence related to a sample selected by a technological system compared to a sample selected by a colleague auditor. To ensure that participants' perceptions of technology-performed sample selection were not confounded by their perceptions of a human colleague, I withheld information on whether technology operated with or without any specific human assistance. While this design choice allowed for a clean measurement of the effects of technology-performed sample selection, current practice indicates that some human influence may be necessary for technology to function as intended (e.g., Harris 2017). As such, it is important to understand how technology is perceived when it is framed as being used by another auditor, specifically whether the benefits of technology use extend to situations where technology is explicitly framed as being used by another auditor.

I expect that a colleague's involvement in sample selection is perceived differently when technology is used to complete work, given technology's position as intermediary between worker and task. I find some support for this expectation in prior literature. Emett et al. (2021), for example, find that audit managers perceive staff auditors' use of technology-intense data and analytics (D&A) procedures as lower in effort and quality than traditional sample-based testing procedures. While their findings indicate a perception of lower effort and quality in staff auditors' use of D&A, I expect that non-managers may prefer working with a sample selected by a technology-assisted colleague given less perceived involvement of their colleague.

Other research indicates differences in perceived self-investment and control when technology or a decision aid is involved in audit work. In an audit negligence setting, Libby and Witz (2020) note that jurors perceived an audit firm as less negligent when work was performed by artificial intelligence compared to human auditors, given a perception of work performed by technology as more independent of human influence and bias. Similarly, Lowe, Reckers, and Whitecotton (2002) find that jurors were less likely to blame auditors for audit failure when auditors relied on a reliable decision aid. While their analyses do not shed much light on the process through which decision aid use impacted perceptions of negligence, it is plausible that jurors perceived a decision aid as limiting auditors' ability to inject their own biases or exert control of their work. More recently, Kipp, Curtis, and Li (2020) find that managers were less aggressive in reporting when they were advised by a robotic intelligence agent compared to a human adviser due to a diminished ability to diffuse responsibility for aggressive decision-making with a technological agent. Within my setting, I expect that auditors who evaluate a sample selected by a colleague with the assistance of technology will perceive their colleague as having less personal control of the sample selection process and will, thus, be less likely to attribute ownership to their colleague.

In summary, I expect that a colleague's use of technology in sample selection is associated with less perceived self-investment and task control, both of which I expect to be associated with weaker perceptions of colleague work ownership. Given weaker ownership, I expect that evidence evaluating auditors will be more likely to engage with a sample selected by a colleague with the assistance of technology, which I expect to have the biggest impact when a colleague's involvement and sample-selection, operationalized

through their sample selection time and effort, are reportedly high. It is possible, however, that auditors may perceive technology as controlling the sample selection task as opposed to their human colleague, in which case auditors may exhibit algorithm aversion and worse performance (Dietvorst et al. 2015; Burton et al. 2019). That said, I do not expect algorithm aversion in my experimental setting, given the presence of a colleague as intermediary between technology and evidence evaluating auditors.

> **H4**: Auditors will correctly identify a higher number of errors when a colleague auditor selects a sample with the assistance of technology, which has the greatest effect on evidence evaluators' ownership and performance when sample selection is reported to have taken more time and effort than expected.

VII. EXPERIMENTAL DESIGN – EXPERIMENT 2

VII.A Overview

Experiment 2 is a 2 x 2 between-subjects experiment where participants performed a search for unrecorded liabilities task modified from Experiment 1. Following Experiment 1, I recruited participants from students enrolled in either an undergraduate audit course or a graduate audit course.[26] All experimental materials were administered through Qualtrics. A total of 102 students participated in the experiment. 75 students were enrolled in an undergraduate audit course, where the other 27 students were enrolled in a master's-level audit course. In contrast to my first experiment, I do find a significant difference in participants' knowledge of purchasing and payables, which I measure using the same three knowledge check questions from Experiment 1 (t [100] = 2.65, p = < 0.01; two-tailed). That said, differences are not significant when comparing only those participants who answered at least one knowledge check question correctly, which process I discuss in more

[26] IRB approval was obtained prior to data collection.

detail in section "VIII.A Excluded Observations" (t [87] = 1.66, p = 0.11; two-tailed). Regardless, and consistent with Experiment 1, I control for participants' experimental administration and formal education (junior, senior, or recent graduate/graduate student) as covariates in all analyses.

VII.B Task Description

Following Experiment 1, participants were tasked with evaluating client-provided vendor invoices as part of a search for unrecorded liabilities for Carpenter & Weaver Construction.[27] Prior to accessing primary experimental materials, participants were required to respond to the same three open-ended knowledge check questions used in Experiment 1. Participants were then introduced to their assigned experimental condition with text describing whether audit firm management was emphasizing the implementation of recently clarified audit procedures or a recently developed proprietary technological system across audit engagements. Participants also received an informal email from Taylor Zimmerman, their colleague responsible for sample selection, which outlined the time and effort involved in sample selection, as well as whether Taylor had performed sample selection using clarified audit procedures or a technological system. Participants responded to two attention check questions to ensure that they paid attention to, and comprehended, experimental materials. Fifteen participants failed to correctly respond to both attention check questions correctly and were subsequently redirected to the experimental manipulation with instructions to pay closer attention, before being asked the same two attention check questions a second time.[28]

[27] See Appendix B for the experimental materials used in Experiment 2.
[28] Of the fifteen attention check failures, only three related to a condition involving technology, which suggests differences in manipulation salience between conditions. Only one participant failed attention check questions a second time.

After reading background information and colleague communication related to their assigned condition, participants proceeded to the main experimental task, evaluation of vendor invoices. Participants across conditions evaluated the same set of eight invoices, which had been provided by the client in response to Taylor Zimmerman's earlier requests. Four invoices contained either an error or ambiguity in the client's recorded accounts payable. The other four invoices contained no errors and indicated that selected disbursements had been appropriately recorded. A list of errors, along with the percentage of participants who correctly identified each error, is presented in Table 8. Following their review of vendor invoices, participants provided their responses to continuous measures that assessed their confidence in the accuracy of Carpenter & Weaver's underlying liabilities balance, as well as their perceptions of work ownership and engagement. After responding to several additional measures, participants provided demographic information and exited the survey.

Table 8

Errors – Experiment 2

	Description	**% of Correct Responses**
Error 1	The invoice provided did not match accounts payable and cash disbursement listings, in that both company name and invoice numbers did not match. Participants received a score of "1" if they identified either a mismatch in company name, or a mismatch in invoice number.	40%
Error 2	The company inappropriately recorded an $81,000 invoice as a payable, when half of the amount (40,500) was prepaid for future services. Participants should have responded that $40,500 of the liability was appropriately recorded and that $40,500 of the liability was inappropriately recorded. Participants received a score of "1" if they identified that the company had inappropriately recorded a prepaid expense as a liability.	19%

Error 3	The company inappropriately excluded an expense for $11,682 that the invoice suggested should have been included in year-end accounts payable. Participants should have indicated that liabilities were underreported by the entire excluded invoice amount, $11,682. Participants received a score of "1" if they indicated that the liability was incurred prior to year-end and not appropriately recorded.	52%
Error 4	The company-provided invoice indicated that goods were ordered prior to year-end and received after year-end. The company had not recorded a liability for the goods, although whether the goods were sold FOB destination or FOB shipping point was covered by Carpenter & Weaver's receiving stamp. Participants received a score of "1" if they requested additional information to verify shipping terms or problems with document visibility, including shipping terms and invoice number.	29%

Note: The above lists specific errors participants could have identified, as well as the appropriate responses that would have been scored as correct. The table also lists the percentage of responses that correctly identified each error, out of 89 responses included in final analyses.

VII.C Independent Variables

I manipulate my first independent variable, colleague involvement in sample selection, through the email provided to participants from Taylor Zimmerman, their colleague responsible for sample selection. Participants in the high involvement condition were told that Taylor had initially received the search for unrecorded liabilities workpaper two weeks previously and that it had taken much longer than initially expected to clean client-provided data and obtain evidence for testing from the client.[29] Participants in the low involvement condition were told that Taylor had initially received the search for unrecorded liabilities workpaper only two days previously and that it had taken much less time than initially anticipated to clean the data and obtain client-provided evidence.

[29] To avoid inducing perceptions of Taylor Zimmerman as incompetent, time delays were primarily attributed to messy data received from the client as well as client staff, who, the participant learned, had undergone a time-intensive end-of-month and required significant follow-up from Taylor to provide requested information.

My second independent variable, technology use in sample selection, was manipulated through background information provided to participants that described a recent firm initiative, as well as an email sent by Taylor Zimmerman, the colleague auditor responsible for sample selection. Participants in the non-technology condition were told that audit firm management was currently in the process of implementing a recently developed set of clarified audit procedures. While implementation was reported to be still ongoing, initial reports were positive and indicated no errors associated with the use of the clarified audit procedures. Participants in the technology condition were told that the firm had recently developed a proprietary technological system, EvalTech, with a specific emphasis of assisting auditors in performing routine work. While the firm was still in the process of implementing EvalTech across all engagements, initial reports were positive and indicated no errors associated with its use.

Additional information on technology use was also presented in the informal email communication from Taylor Zimmerman. In the email, Taylor described performing sample selection using either audit managements' clarified audit procedures or EvalTech to "identify the riskiest items for testing." Describing sample selection as risk-based represents a departure from how sample selection was presented in Experiment 1, where participants were told the specific criteria used in sample selection.[30] I characterize sample selection as risk-based in Experiment 2 to better approximate the enhanced capabilities of technology to test entire populations (e.g., Cooper et al. 2019; Emett et al. 2020), as well as to strengthen participants' perceptions of "other" ownership by decreasing their own

[30] Specifically, participants in Experiment 1 were told that all cash disbursements over $10,000 were required to be selected, with additional elements of random sampling to achieve a final sample of either ten or twelve tested disbursements.

knowledge of sample selection procedures while simultaneously strengthening perceptions of their colleague's control and knowledge of the sample selection process. While the algorithm aversion literature suggests that people are averse to the use of technology in subjective tasks, I do not expect algorithm aversion to affect my setting, given that technology is framed as assisting another auditor – i.e., not operating autonomously - for which prior algorithm aversion literature offers no clear prediction.

VII.D Dependent Variables

Following Experiment 1, I measure my primary dependent variable, auditor performance, through the number of errors identified by each participant. Instead of identifying errors through participants' documentation of appropriate versus inappropriate amounts, I instead asked participants to indicate whether each invoice was appropriately recorded, inappropriately recorded, or uncertain, with additional information required to reach a conclusion. When participants indicated that an invoice was either inappropriately recorded or uncertain, they were asked a follow-up question to document which error or ambiguity had led them to that conclusion. Requiring participants to describe identified errors is beneficial for several reasons. First, requiring additional effort to conclude that a disbursement was inappropriately recorded or uncertain is more closely aligned with real-world auditing, where conducting additional procedures requires auditors to expend potentially costly additional time and effort (e.g., Brazel et al. 2016). Second, and inversely, allowing participants to conclude that a disbursement was appropriately recorded without additional documentation establishes that conclusion as the "default" and disincentivizes incorrect or random responses. Finally, participants did not receive

compensation for "correct" responses. As such, the only incentive for participants to correctly identified errors was participants' own motivation to be correct.

In addition to identified errors, I collect two continuous variables as alternative measures of performance. The first measure, confidence, asked participants to indicate their confidence in C&W's reported liabilities using a seven-point Likert scale. In addition, participants indicated how many additional hours, out of ten possible, that the firm should allocate to testing C&W's reported liabilities. Following Experiment 1, I collect participants' work ownership and engagement to measure my hypothesized process. I also collect participants' perceptions of work ownership and engagement as measures of my hypothesized process (see Wang et al. 2019). Additional measures capture participants' perceptions of task significance (Downey 2018), perceived sample selection quality, and the division of responsibility for a potential adverse testing-related outcome between participants and Taylor Zimmerman.

VIII. RESULTS OF ANALYSIS – EXPERIMENT 2

VIII.A Excluded Observations

Following Experiment 1, participants answered three open-ended knowledge check questions prior to accessing primary experimental materials. I require participants to correctly answer at least one knowledge check question to be included in statistical analyses. I exclude 12 participants from analysis who failed to provide one correct response to knowledge check questions. I exclude an additional participant who failed attention check questions twice. My final sample thus consists of 89 observations across four conditions.

VIII.B Descriptive Statistics

Descriptive statistics for my primary dependent variable, performance, and process measures are presented in Table 9, with a graphical depiction of results presented in Figure 9. Panel A reports descriptive statistics for identified errors, which range from zero to four. Participants identified, on average, 1.39 errors across all conditions. The highest number of errors were identified by participants in the *High Involvement, Non-Technology* condition (1.67), compared to the lowest number of errors (0.90), which were identified by participants in the *High Involvement, Technology* condition.

Panel B reports descriptive statistics for work ownership, with results depicted visually in Figure 10. Following Experiment 1, I measure ownership using the average of a three-item inventory, adapted from Brown et al. (2014). Reliability analysis indicates a high degree of internal consistency between ownership inventory items with a Cronbach's α of 0.90. Values range from seven (i.e., high ownership) to one (i.e., low ownership). Reported ownership was above the midpoint, 4.00, in all conditions, with the highest reported ownership in the *High Involvement, Non-Technology* condition (5.17), and the lowest ownership in the *High Involvement, Technology* condition (4.63). My second process measure, engagement, is reported in Panel C, with a graphical depiction of results presented in Figure 11. I assess engagement using the average of a three-item inventory adapted from Rich et al. (2010) and Christian et al. (2011). Reliability analysis indicates a high degree of internal consistency between engagement inventory items, with a Cronbach's α of 0.93. The highest reported engagement was in the *High Involvement, Non-Technology* condition (5.83), with the lowest engagement was in the *High Involvement, Technology* condition (5.33).

Table 9

Descriptive Statistics – Experiment 2

Panel A: Mean (Std. Dev) of *Identified Errors*

	Non-Technology	Technology	Overall
Low Involvement	1.42 (1.06) n = 24	1.54 (1.02) n = 24	1.47 (1.03) n = 48
High Involvement	1.57 (0.93) n = 21	0.95 (0.94) n = 20	1.27 (0.98) n = 41
Overall	1.49 (0.99) n = 45	1.25 (0.99) n = 44	

Panel B: Mean (Std. Dev) of *Work Ownership*

	Non-Technology	Technology	Overall
Low Involvement	4.90 (1.36) n = 24	4.98 (1.41) n = 24	4.94 (1.37) n =48
High Involvement	5.17 (1.15) n = 21	4.64 (1.32) n = 20	4.91 (1.25) n = 41
Overall	5.03 (1.26) n = 45	4.82 (1.37) n = 44	

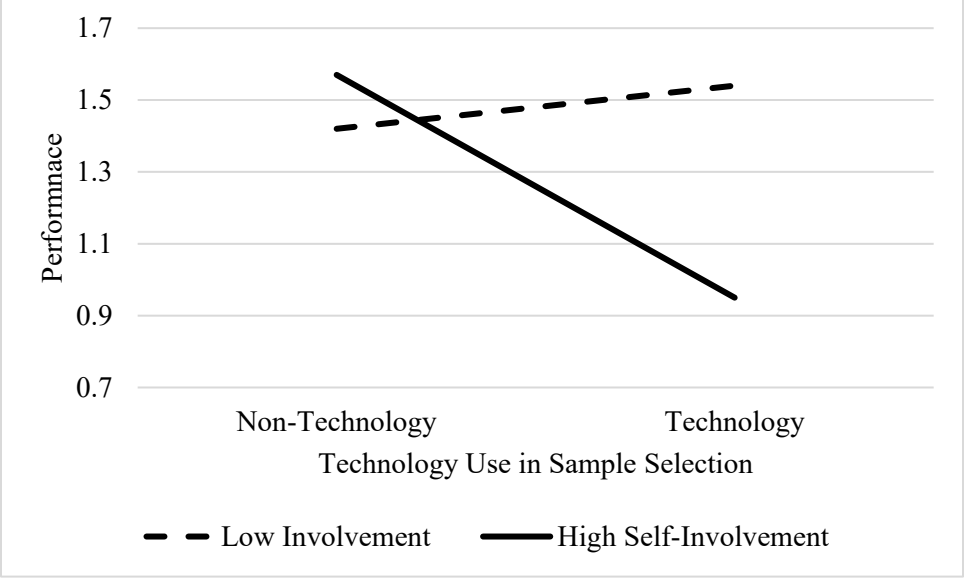
Panel C: Mean (Std. Dev) of *Engagement*

	Non-Technology	Technology	Overall
Low Involvement	5.38 (0.95) n = 24	5.66 (1.20) n = 24	5.52 (1.08) n = 48
High Involvement	5.83 (0.95) n = 21	5.33 (1.02) n = 15	5.58 (1.00) n = 41
Overall	5.59 (0.97) n = 45	5.51 (1.12) n = 44	

Note: Panel A provides descriptive statistics for identified errors, out of a maximum of four errors. Panel B presents descriptive statistics for perceived ownership, which consists of the average of a three-item inventory, adapted from Brown et al. (2014). Panel C presents descriptive statistics for task engagement, which consists of a three-item inventory adapted from Rich et al. (2010).

Figure 9

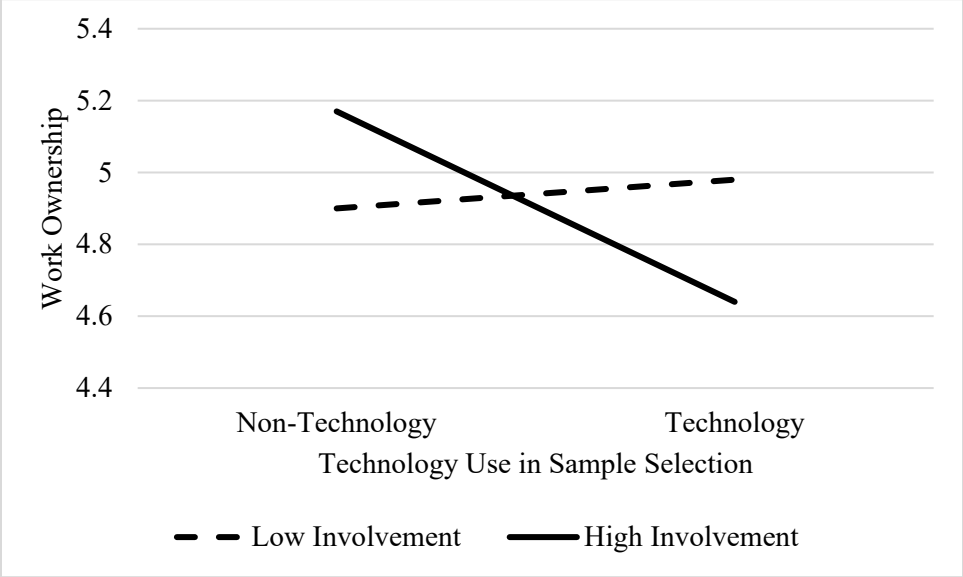
Identified Errors by Condition – Experiment 2



Note: Figure 9 depicts my primary dependent variable, auditor performance, measured using the number of errors identified by each participant, ranging from zero to four.

Figure 10

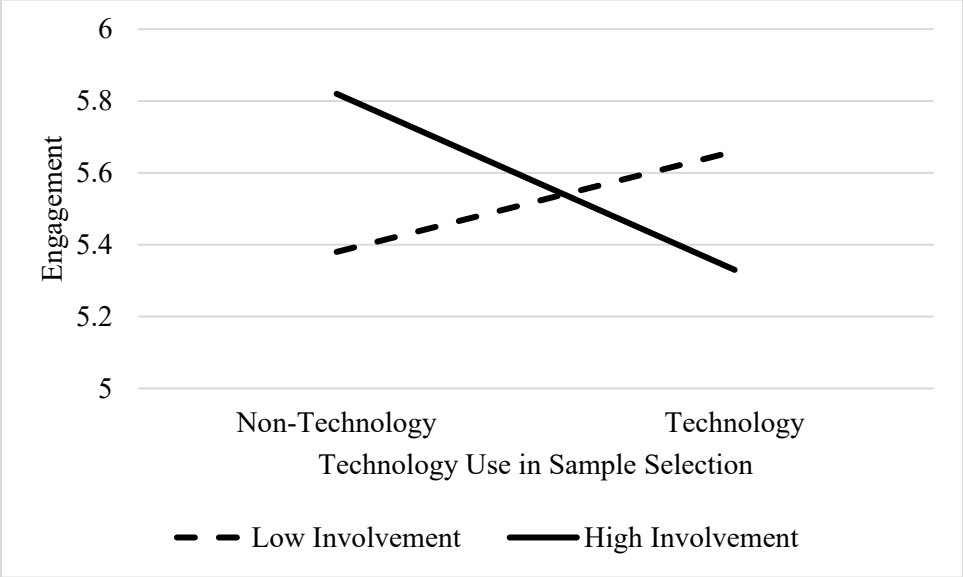
Perceived Work Ownership by Condition



Note: Figure 10 depicts work ownership, which ranges from one to seven and is measured using the average of a three-item inventory adapted from Brown et al. (2014).

Figure 11

Engagement by Condition – Experiment 2



Note: Figure 11 depicts engagement, which ranges from one to seven and is measured using the average of a three-item inventory adapted from Rich et al. (2010).

VIII.C Tests of Hypotheses

Hypothesis 3 predicts that auditors will identify more errors when evidence relates to a sample selected by a colleague who invested less time and effort than initially expected in sample selection. Hypothesis 4 predicts that auditors will identify more errors when a technological system assists in sample selection, with the greatest incremental effect when effort and time are reported to have been higher than expected. I test my hypotheses using a combination of ANCOVA, contrasts, and mediation analysis. Following Experiment 1, I control for variation in participants' self-reported formal education and experimental administration as covariates in ANCOVA.

The results of ANCOVA are presented in Table 10, Panel A. I find evidence of a marginally significant interaction between a colleague's level of involvement and technology use ($F_{1,83} = 3.14$, $p = 0.08$; two-tailed), although neither main effect for involvement ($F_{1,83} = 0.75$, $p = 0.39$; two-tailed) nor technology use ($F_{1,83} = 1.27$, $p = 0.26$; two-tailed) is statistically significant. I perform an additional contrast comparing the *High Involvement, Technology* condition against all three other conditions (i.e., 1, 1, -3, 1) in Panel B ($t[85] = 2.22$, $p = 0.03$; two-tailed). The results of this contrast indicate that participants in the *High Involvement, Technology* condition identified significantly less errors than participants in the other three conditions. An additional contrast, comparing the *High Involvement, Non-Technology* condition against other conditions (i.e., 3, -1, -1, -1) fails to identify a significant difference between conditions ($t[85] = 1.08$, $p = 0.28$; two-tailed), which indicates that my observed interaction is attributable solely to the value associated with the *High Involvement, Technology* condition. Given this pattern of results, I reject both Hypothesis 3 and Hypothesis 4.

As a potential explanation for differences in performance, I next consider my process measures, work ownership and engagement. The results of ANCOVA for work ownership are presented in Panel C. I fail to find evidence of a statistically significant main effect for both colleague involvement ($F_{1,83} = 0.01$, $p = 0.93$; two-tailed) and technology use ($F_{1,83} = 0.67$, $p = 0.41$; two-tailed), in addition to a statistically significant interaction term ($F_{1,83} = 1.02$, $p = 0.32$; two-tailed). To provide additional perspective on process, I next consider participants' reported engagement, with results of ANCOVA presented in Panel D. While I fail to find a main effect of involvement ($F_{1,83} = 0.14$, $p = 0.71$; two-tailed) and technology use ($F_{1,83} = 0.20$, $p = 0.66$; two-tailed), I do find a marginally significant interaction between colleague involvement and technology use ($F_{1,83} = 3.18$, $p = 0.08$; two-tailed), which indicates that participants were less likely to engage with their work when technology-assisted sample selection required greater involvement in the form of more time and effort.

Table 10

Tests of Hypotheses – Experiment 2

Panel A: ANCOVA of *Identified Errors*

	Sum of Squares	**df**	**MS**	**F**	**p-value**
Technology	1.24	1	1.24	1.27	0.26
Involvement	0.73	1	0.73	0.75	0.39
*Technology * Involvement*	3.06	1	3.06	3.14	0.08*
Education	0.11	1	0.11	0.11	0.74
Administration	1.63	1	1.63	166	0.20
Error	81.04	83	0.98		

Panel B: Contrast of *Specific Conditions: Identified Errors*

	df	***t***	**p-value**
High Involvement, Technology	85	2.22	0.03**
High Involvement, Non-Technology	85	1.08	0.28

Panel C: ANCOVA of *Perceived Ownership*

	Sum of Squares	df	MS	F	p-value
Technology	1.17	1	1.17	0.67	0.41
Involvement	0.01	1	0.01	0.01	0.93
*Technology * Involvement*	1.78	1	1.78	1.02	0.32
Education	0.30	1	0.30	0.17	0.68
Administration	3.26	1	3.26	1.87	0.18
Error	144.51	83	1.74		

Panel D: ANCOVA of *Work Engagement*

	Sum of Squares	df	MS	F	p-value
Technology	0.22	1	0.22	0.20	0.66
Involvement	0.16	1	0.16	0.14	0.71
*Technology * Involvement*	3.49	1	3.49	3.18	0.08*
Education	0.49	1	0.49	0.45	0.51
Administration	0.02	1	0.02	0.02	0.90
Error	91.05	83	1.10		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A presents ANCOVA of identified errors based on *Technology* and *Involvement*. Panel B presents results of a contrast to assess the magnitude of statistical difference between conditions. Panel C presents ANCOVA of perceived ownership based on *Technology* and *Involvement*. Panel D presents ANCOVA of work engagement based on *Technology* and *Involvement*.

VIII.D Test of Robustness – Normality

Similar to Experiment 1, a concern with using ANCOVA to analyze count data is potential violation of an assumption of normality. Figure 12 presents a histogram of identified errors, which provides visual evidence that an assumption of normality may not be met by my data. I find significant Shapiro-Wilk normality statistics for all four conditions: *Low Involvement, Non-Technology* (W [24] = 0.88, $p < 0.01$); *High Involvement, Non-Technology* (W [21] = 0.81, $p < 0.01$); *Low Involvement, Technology* (W [24] = 0.88, $p = 0.01$); *High Involvement, Technology* (W [20] = 0.84, $p < 0.01$). While

results of ANCOVA are robust to violations of an assumption of normality (e.g., Knief and Forstmeier 2021), additional analysis may be useful to ensure that results are robust to normality violation.

I first conduct a Kruskal-Wallis test, similar to Experiment 1 (Kruskal and Wallis 1952; Breslow 1970). Results of this analysis indicate a marginally significant difference in identified errors between conditions, which I assess using one-tailed significance following the results of ANCOVA (H [3] = 5.28, p = 0.07; one-tailed). One limitation of this statistical approach is an inability to determine where specific differences exist between conditions, in addition to an inability to specify control variables. To identify where specific differences might exist, I perform Poisson loglinear regression, which is used to model data with a nonnormal distribution. Given previous results, I code responses in the *High Involvement, Technology* condition using a dummy variable (i.e., "1") and code all three other conditions as the "control" (i.e., "0"). The results of this analysis are presented in Table 11. I find a significant and negative effect of *High Involvement, Technology* on identified errors (b = -0.44, X^2 = 3.12, p = 0.08; two-tailed, as well as nonsignificant effects of control variables, consistent with previous results of ANCOVA.

The results of Poisson regression are highly dependent on model specification, specifically the specification of "control" conditions. I compare the *High Involvement, Technology* condition against the combined other conditions given the results of previous ANCOVA and contrasts, which indicate a significant difference between that condition and all other conditions. Untabulated alternative specifications of this model, comparing the *High Involvement, Technology* condition as the "control" and assigned dummy variables to the other three conditions yields comparable results, in that I find positive and

marginally significant coefficients on the *High Involvement, Non-Technology* and *Low Involvement, Technology* conditions. In short, results suggest identification of less errors in the *High Involvement, Technology* condition compared to other conditions.

Table 11

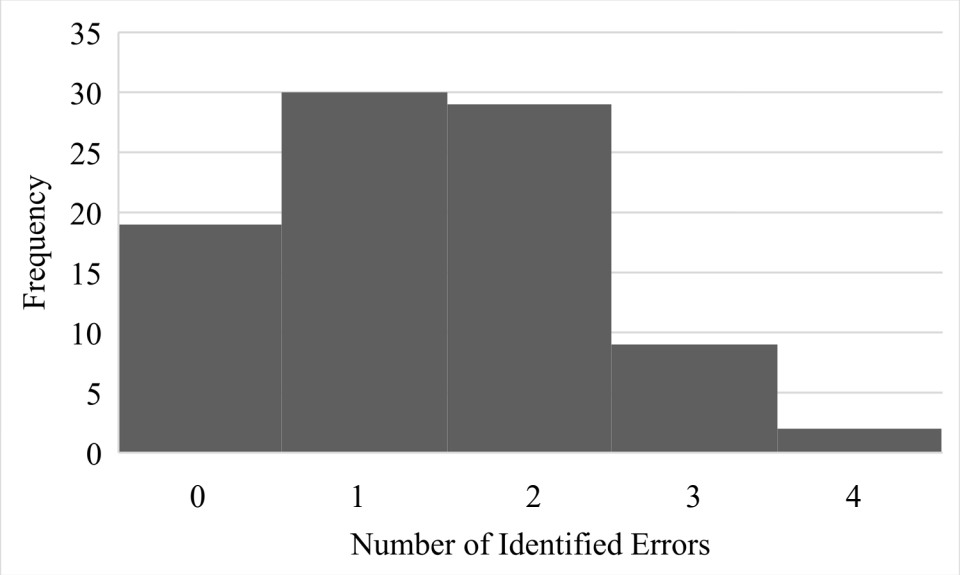
Poisson Regression – Experiment 2

Variable	*B*	*Wald* X^2	*p-value*
Intercept	0.02	0.01	0.97
High Involvement, Technology	-0.44	3.12	0.08*
Education	0.04	0.03	0.86
Administration	0.24	1.22	0.27

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.

Figure 12

Histogram of Identified Errors – Experiment 2



Note: Figure 12 depicts the frequency with which participants identified errors, which range between zero and four.

VIII.E Additional Analysis

As noted previously, the statistical results of Experiment 2 do not conform to my initial expectations and indicate the opposite of what I predict in Hypothesis 4, as a colleague's use of technology in sample selection resulted in significantly worse performance when sample selection required greater involvement. In this section, I explore several possible explanations using additional variables that were collected as part of the experiment. Following statistical analyses, I provide several possible explanations for observed results.

I first consider participants' confidence in Carpenter & Weaver's reported liabilities balance. Participants should have indicated less confidence in reported liabilities when they identified a higher number of errors, consistent with the logic that a materially misstatement in recorded liabilities is more likely when more errors are present in recorded liabilities. Simple correlation analysis supports this assumption, as identified errors are marginally correlated with participants' confidence in C&W's reported ($r = -0.17$, $p = 0.06$; one-tailed). Given that participants in the *High Involvement, Technology* condition identified significantly less errors than participants in the other three conditions, I expect to observe higher confidence in reported liabilities in that condition. Table 12 reports statistical results of confidence, which I measure using a seven-point Likert scale. Descriptive statistics, reported in Panel A, indicate that confidence was highest in both the *High Involvement, Audit Procedures* and *Low Involvement, Non-Technology* condition (3.67), with the lowest confidence in the *High Involvement, Technology* condition (3.40). Results of ANCOVA, reported in Panel B, indicate that differences are not statistically significant, with non-significant main effects and interaction terms, although it appears that

confidence was significantly influenced by experimental administration ($F_{1,83} = 6.41$, $p < 0.01$; two-tailed).

Table 12

Additional Analysis: Confidence – Experiment 2

Panel A: Mean (Std. Dev) of *Confidence*

	Non-Technology	Technology	Overall
Low Involvement	3.67 (1.31) n = 24	3.58 (1.28) n = 24	3.63 (1.28) n = 48
High Involvement	3.67 (1.43) n = 21	3.40 (1.43) n = 20	3.70 (1.42) n = 41
Overall	3.67 (1.35) n = 45	3.50 (1.34) n = 44	

Panel B: ANCOVA of *Confidence*

	Sum of Squares	**df**	**MS**	**F**	**p-value**[b]
Technology	0.63	1	0.63	0.36	0.55
Involvement	0.20	1	0.20	0.11	0.74
*Technology * Involvement*	0.45	1	0.45	0.26	0.61
Education	2.22	1	2.22	1.27	0.26
Administration	11.22	1	11.22	6.41	0.01**
Error	145.38	83	1.75		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A provides descriptive statistics for confidence, with a range between one and seven. Panel B presents ANCOVA of confidence based on *Technology* and *Involvement*.

I next consider variation in perceptions of sample quality, which could serve as an alternative explanation for observed differences in engagement and performance.

Specifically, participants may have perceived a technology-assisted sample as being lower in quality, which could have led them to disengage with testing evidence related to that sample. I assess participants' perceptions of sample quality using a seven-point Likert scale. Statistical results are presented in Table 13. Panel A provides descriptive statistics, which indicate highest perceived quality in the *High Involvement, Non-Technology* condition (4.93), compared to the lowest perceived quality, the *High Involvement, Technology* condition (4.64). That said, the results of ANCOVA, presented in Panel B, indicate that differences between conditions are not statistically significant, which suggests that differences in performance and engagement are not a function of differences in the perceived quality of a selected sample. In addition, neither formal education nor experimental administration is significant with respect to sample quality.

Table 13

Additional Analysis: Quality – Experiment 2

Panel A: Mean (Std. Dev) of *Quality*

	Non-Technology	Technology	Overall
Low Involvement	4.68 (1.20) n = 24	4.66 (1.66) n = 22	4.67 (1.42) n = 46
High Involvement	4.93 (1.02) n = 20	4.64 (1.22) n = 20	4.79 (1.12) n = 40
Overall	4.79 (1.12) n = 44	4.65 (1.45) n = 42	

Panel B: ANCOVA of *Quality*

	Sum of Squares	df	MS	F	p-value[b]
Technology	0.46	1	0.46	0.27	0.61
Involvement	0.56	1	0.56	0.33	0.57
*Technology * Involvement*	0.53	1	0.53	0.31	0.58
Education	1.31	1	1.31	0.76	0.39
Administration	0.06	1	0.06	0.04	0.85
Error	137.06	80	1.71		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A provides descriptive statistics for perceived sample quality, between one and seven. Panel B presents ANCOVA of perceived sample quality based on *Technology* and *Involvement*.

I next consider variation in the responsibility that participants allocated between themselves and Taylor Zimmerman for a possible negative testing-related outcome. Following Experiment 1, I measure responsibility using two seven-point Likert scales where participants indicated their responsibility for a negative testing-related outcome and Taylor's responsibility for a negative testing-related outcome. I combine these measures by dividing Taylor's allocated responsibility by total responsibility, which results in a value between zero and one. Descriptive statistics are presented in Table 14, Panel A. Participants allocated the least responsibility to Taylor, their sample selecting colleague, in the *High Involvement, Non-Technology* condition (0.42), and the most responsibility in the *Low Involvement, Non-Technology* and *High Involvement, Technology* conditions (0.52). Panel B reports results of ANCOVA, which demonstrate a marginally significant interaction between sample selection involvement and technology use ($F_{1,83} = 3.64$, $p = 0.06$; two-tailed). The results of additional untabulated contrasts indicate that this interaction is driven by differences between the *High Involvement, Non-Technology* condition and the three

other conditions, as participants allocated significantly less responsibility to Taylor Zimmerman when sample selection required greater involvement without the use of technology (t[85] = 2.60, p = 0.01; two-tailed).

Table 14

Additional Analysis: Responsibility – Experiment 2

Panel A: Mean (Std. Dev) of *Responsibility*

	Non-Technology	Technology	Overall
Low Involvement	0.52 (0.11) n = 24	0.50 (0.11) n = 24	0.51 (0.11) n = 48
High Involvement	0.44 (0.14) n = 21	0.52 (0.12) n = 20	0.48 (0.13) n = 41
Overall	0.48 (0.13) n = 45	0.51 (0.11) n = 44	

Panel B: ANCOVA of *Responsibility*

	Sum of Squares	**df**	**MS**	**F**	**p-value**[b]
Technology	0.03	1	0.03	1.88	0.17
Involvement	0.02	1	0.02	1.23	0.27
*Technology * Involvement*	0.05	1	0.05	3.64	0.06*
Education	0.02	1	0.02	1.38	0.24
Administration	0.04	1	0.04	3.12	0.08*
Error	1.11	83	0.01		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A provides descriptive statistics for allocated responsibility, between zero and one, where one indicates higher responsibility allocated to a sample selecting colleague. Panel B presents ANCOVA of responsibility based on *Technology* and *Involvement*.

Finally, I consider participants' correct classification of non-errors. While the primary purpose of this study is to consider performance with a specific emphasis on the error identification, it is also important to consider performance with respect to non-errors, which are much more likely to comprise the bulk of auditors' evidence evaluation activities in real-world auditing. Further, analyzing non-errors can offer some perspective on my experimental instrument's ability to differentiate between situations where additional effort is incrementally more beneficial (i.e., errors) than not (i.e., non-errors). As noted in Experiment 1, I do not expect differences between conditions in their identification of non-errors for two reasons. First, I do not expect that expending additional effort in evaluating non-error disbursements offers any incremental benefit to auditors, given participants' likely perceptions of a conclusion of "non-error" as the default. Second, in requiring additional effort for auditors' conclusions of "inappropriate" or "uncertain," participants were unlikely to respond randomly which, again, suggests a default conclusion of "non-error."

Table 15 presents the results of statistical analysis of non-errors. Panel A provides descriptive statistics for non-errors. It is worth noting that auditors across conditions identified, on average, numerically more non-errors correctly than they did errors (i.e., 2.75 vs. 1.38). Additional, untabulated, analysis indicates statistically significant within-participants' differences in identified non-errors compared to errors for every condition, which indicates higher performance in identification of non-errors, consistent with my underlying expectations. Panel B presents results of ANCOVA. I fail to find a significant main effect for both involvement and technology use, as well as their interaction term. Control variables related to education and administration were likewise not significant. As

such, my experimental manipulation did not significantly affect participants' identification of non-errors, consistent with an ability to differentiate between performance related to the accurate identification of errors and non-errors.

Table 15

Additional Analysis: Non-Errors – Experiment 2

Panel A: Mean (Std. Dev) of *Non-Errors*

	Non-Technology	Technology	Overall
Low Involvement	2.58 (1.06) n = 24	2.71 (0.86) n = 24	2.65 (0.96) n = 48
High Involvemnet	2.81 (0.88) n = 21	2.95 (0.69) n = 20	2.80 (0.81) n = 41
Overall	2.69 (0.97) n = 45	2.82 (0.79) n = 44	

Panel B: ANCOVA of *Non-Errors*

	Sum of Squares	**df**	**MS**	**F**	**p-value**[b]
Technology	0.48	1	0.48	0.61	0.44
Involvement	1.56	1	1.56	1.97	0.16
*Technology * Involvement*	0.01	1	0.01	0.02	0.90
Education	1.49	1	1.49	1.88	0.17
Administration	0.35	1	0.35	0.44	0.51
Error	65.49	83	0.79		

*, **, and *** denote significance at the 0.10, 0.05, and 0.01 levels, respectively.
[a] Directional prediction: p-value based on one-tailed test. All other p-values based on two-tailed tests of statistical significance.
Note: Panel A provides descriptive statistics for correctly classified non-errors, which range from zero to four. Panel B presents ANCOVA of correctly classified non-errors based on *Technology* and *Involvement*.

IX. DISCUSSION – EXPERIMENT 2

In this experiment, I consider whether evidence evaluating auditors' performance, work ownership, and engagement are affected by variation in a colleague's level of involvement in sample selection, in addition to whether a colleague used technology to complete sample selection. Based on findings from my first experiment, I expected auditors to identify less errors when sample selection was reported to have involved a colleague's greater involvement, which I manipulated through the time and effort required to select a sample and obtain evidence from the client. I also expected evidence evaluating auditors to react positively to a sample selecting colleague's use of technology in sample selection, with the greatest expected incremental impact when sample selection required a colleague's greater involvement. These expectations are not supported by the data, however, which indicates that auditors performed significantly worse when technology-assisted sample selection required greater involvement. Additional analyses fail to identify any significant differences in auditors' reported confidence in C&W's underlying liabilities balance, and perceived sample quality, although blame for a potential negative testing-related outcome was significantly lower when a colleague auditor invested greater time and effort to select a sample using traditional non-technological audit procedures.

This pattern of results, while inconsistent with my expectations, indicates that auditors' perceptions of technology vary based on the circumstances in which technology is used. The purpose of this section is to provide a discussion and potential explanations for observed results, specifically the extent to which expectancy violation and social presence may have the observed pattern of statistical results. While limitations in collected data prevent me from providing statistical support for either possibility, discussing their

potential impact on my data may prove useful in motivating additional discussion and future research about how auditors perceive work performed by colleagues with the assistance of technology.

Recent research indicates that auditors perceive technology as providing greater efficiency than more traditional non-technological methods (e.g., Lowe, Bierstaker, Janvrin, and Jenkins 2018). In a survey of audit partners, Westermann, Bedard, and Earley (2015), for example, note a widespread perception of technology as enabling a more efficient audit, although some partners expressed concern of a concurrent decline in staff auditors' critical thinking. Similarly, Emett et al. (2021) note that audit managers perceive D&A procedures as less effortful than traditional sample-based procedures. In their investigation of robotic process automation, Cooper et al. (2019) note efficiency as one of the most frequently mentioned benefits of automation, as technology has the capacity to process information up to 24 hours a day. Similarly, EY (2016) note that recent adoption of robotic process automation had shortened run times by an average of 76% compared to preadoption. In short, auditors generally expect work performed by, or with, technology to be more efficient than work performed without it.

Expectancy violation theory (EVT) is a communication theory that is primarily concerned with how people interpret others' behavior in light of expectations or norms (e.g., Burgoon and Hale 1988; Mendes et al. 2007). Expectancy violation occurs when behavior violates an expectation or norm. Where individuals may react positively to a positive expectancy violation, negative expectancy violations are typically associated with negative reactions (Burgoon 2015). Further, negative violators tend to be perceived as less socially attractive (Afifi and Burgoon 2000). In my second experiment, participants may

have felt some conflict between their expectations of a more efficient audit associated with technology use and reality, when their sample selecting colleague indicated that sample selection took longer than "initially expected." This negative violation would likely lead to less engagement and lower performance, as participants likely perceived their sample selecting colleague less positively for violating expectations. One shortcoming of this explanation is that I do not find an inverse effect when a colleague did not use technology, and sample selection took less time than expected.

As a complication to possible expectancy violation, participants may have encountered difficulty differentiating Taylor Zimmerman, their colleague responsible for sample selection, from the technological system that "assisted" in sample selection given the nature of my experiment where I do not allow participants to directly interact with their sample selecting colleague. Social presence theory (SPT) indicates that people interpret information differently based on 1) psychological distance from a message source, and 2) the extent to which they perceive a message as originating from a "real person" (e.g., Short, Williams, and Christie 1976; Gunawardena 1995; Lu, Fan, and Zhou 2016; Bennett and Hatfield 2018). While I did not expect participants to experience algorithm aversion when a colleague used technology to select a sample, it is possible that participants did not differentiate between technology-assisted sample selection and sample selection performed by technology itself. As a result, participants may have disengaged with what they perceived to be work that required greater involvement from technology due to algorithm aversion (e.g., Dietvorst et al. 2015; Burton et al. 2019).

In summary, results are not consistent with my expectations, which I posit could be due to either expectancy violation or diminished social presence. While I am unable to

provide statistical support for either possibility, additional research may prove helpful in developing a greater understanding of auditors' expectations of technology. Specifically, future research may wish to consider auditors' reactions to technology that does not conform to their expectations, both when technology operates autonomously as well as when it assists a colleague. Additionally, research may wish to consider whether expectations differ between subjective and objective tasks, which may help to provide insight into whether and how auditors are averse or appreciative of technology in their work.

X. CONCLUSION

In this study, I consider whether auditors' consideration of evidence and work ownership are impacted by sample source. I collect data in two experiments, where I find consistent evidence that auditors evaluate evidence differently based on sample source. Results from my first experiment indicate that auditors identify more errors in client-provided evidence when evidence relates to a sample selected by themselves compared to a colleague auditor, which suggests a potential cost when firms separate the responsibility for sample selection and evidence evaluation between separate auditors (see Commerford et al. 2017). Interestingly, I find evidence of a similar difference in identified errors when evidence relates to a sample selected by a technological system compared to a colleague, consistent with recent research which suggests that auditors realize a range of benefits associated with the automation of simple and objective audit tasks (e.g., Cooper et al. 2019).

Subsequent process analysis provides evidence that auditors' work ownership is indirectly impacted by their engagement with their work, consistent with a recent

workplace ownership model tested by Wang et al. (2019). Additionally, auditors appear more likely to engage with their work, and develop ownership, when evidence relates to a sample selected by either themselves or a technological system compared to a colleague auditor. Further analysis indicates that auditors ascribed greater responsibility for possible inadequacies in testing to a sample-selecting human colleague than they did a technological system, consistent with research that indicates differences in responsibility when work involves collaboration with a technological system versus collaboration with another human (e.g., Hinds et al. 2004).

Based on findings from my first experiment, I perform a second follow-up experiment where I hold sample source constant as a colleague auditor and manipulate the conditions under which sample selection was reported to have been performed. I manipulate a colleague's involvement in sample selection, which I operationalize through the amount of time and effort spent in sample selection. I also manipulate a colleague's sample selection methodology, specifically whether a colleague used traditional audit procedures or a technological system in sample selection. I find that auditors identified less errors when technology-assisted sample selection required a colleague to spend more time and effort in sample selection, in contrast to my expectations of better performance. Further, auditors were less likely to engage with evidence evaluation when technology-assisted sample selection required a colleague to spend more time and effort in sample selection that initially anticipated.

Although collected data does not allow for a statistical explanation of observed differences in engagement and performance, I speculate that differences in performance and engagement may be attributable to expectancy violation, given recent research which

suggests a widespread belief that technology is associated with a more efficient audit (e.g., Westermann et al. 2015; Lowe et al. 2018; Emett et al. 2021). That is, participants may have experienced expectancy violation when their expectations of technology (i.e., a more efficient audit) were not consistent with provided information (e.g., Burgoon and Hale 1988; Afifi and Burgoon 2000; Mendes et al. 2007; Burgoon 2015). As a result, participants may have reduced their engagement with their work. Alternatively, participants may have encountered difficulty in visualizing their sample selecting colleague as a "real person," which social presence theory suggests may have impacted their engagement with their work, and performance (e.g., Short, Williams, and Christie 1976; Gunawardena 1995; Lu et al. 2016; Bennett and Hatfield 2018).

Findings contribute to several emerging areas of interest in accounting and auditing research and indicate several opportunities for additional research. First, I contribute by providing evidence of the antecedents and expected effects of work ownership to auditors' judgment and decision-making. This finding is conceptually similar to recent research which indicates the effects of auditors' attitudinal characteristics on their judgment and decision-making outcomes, including, for example, auditors' skeptical mindsets (Griffith, Hammersley, Kadous, and Young 2015) and intrinsic motivation (Kadous and Zhou 2019). While work ownership is well-documented by the organizational psychology and management literatures, as a construct it has received significantly less attention in accounting and auditing research (e.g., Bauer, Estep, and Griffith 2018; McKenzie 2019; Holmstrom 2021). In addition to offering potential insight into auditors' evaluation, a deeper understanding of ownership may benefit accounting firms and academic researchers by providing deeper insight into auditors' organizational commitment and job satisfaction.

Second, my study contributes to an ongoing discussion about how technology impacts auditor behavior, which is important given audit firms' significant investment and interest in technological innovation (Deloitte 2016; KPMG 2017; EY 2018; Kapoor 2020). My study is among the first to consider auditor-technology interaction through the lens of engagement and work ownership, which contributes to both the audit and work ownership literatures. Further, my study complements recent research which indicates that auditors perceive a range of benefits from greater use of technology in simple and objective audit processes, as I find a positive effect of technology use on auditors' performance, engagement, and work ownership.

In addition, my study contributes by considering how auditors respond to shared work that involves a colleague's use of technology. Results from my second study indicate that auditors view technology negatively when it does not result in a more efficient audit, which is theoretically consistent with expectancy violation theory. While contrary to my expectations, this finding highlights the potential for context-dependent algorithm appreciation and aversion and indicates a need for additional research to understand auditors' expectations of technology, as well as their interaction with work performed by technology based on whether technology complies with previous auditors' efficiency-related expectations.

Finally, I contribute to audit practice by testing the implications of separating the performance of related tasks between multiple auditors. I extend Commerford et al. (2017), who note that auditors appear less likely to avoid selecting sample items associated with difficult client managers when the responsibility for testing a selected sample falls to a colleague. While separating sample selection and evidence evaluation may result in a

higher quality sample, my findings indicate potential downside to auditors responsible for evidence evaluation. That said, my findings are encouraging that involving technology in sample selection may give firms the ability to achieve higher sample quality without compromising evidence evaluation. That said, findings from my second experiment indicate the need for potential caution in discerning how auditors respond to sample selection performed by a colleague with the assistance of technology, as auditors may react negatively to technology use does not lead to a more efficient audit. These findings should be of interest to audit firms and academic researchers and highlight the importance of considering auditors' work ownership and engagement in work design- and staffing-related decisions.

This study is subject to limitations which present opportunities for future research. First, this study examines engagement and ownership in a simple setting that involves staff auditors' search for unrecorded liabilities. While this decision was useful in reducing the potential influence of complexity-related confounds (e.g., algorithm aversion), it may limit generalization to settings that involve a high degree of complexity or auditor specialization. A second limitation concerns participants' performance of a task they have not previously handled before. While this decision holds effort constant across conditions, research suggests that auditors may interact with their work differently when a colleague threatens their work ownership (e.g., Kirk et al. 2018; Wang et al. 2019). That is, findings of disengagement may not generalize to other collaborative work arrangements.

Third, while research suggests that ownership can form through suggestion (Kirk et al. 2018), results may differ when auditors are permitted to select their own samples. Finally, results may vary with team identity (e.g., Bauer 2015), where auditors' work

ownership may be impacted to a lesser extent when evidence relates to a sample selected by a colleague with whom they share a strong, positive relationship. Somech, Desivilya, and Lidogoster (2009), for example, note that strong team identity is associated with task performance in interdependent work. With that in mind, future research could consider whether team identity impacts willingness to engage with compartmentalized work.

In addition to these limitations, future researchers may wish to consider additional positive features of ownership not specifically investigated in this study. Work ownership research, for example, notes a negative association between ownership and turnover intentions (e.g., Peng and Pierce 2015). Investigating how work ownership and work structure impact turnover could be useful for firms' retention of highly skeptical auditors, who Dalton et al. (2017) indicate are more prone to disillusionment and turnover. Other future research may wish to consider auditors' expectations of technology and their reactions to technology that does not conform to expectations. Finally, future research could consider how providing information on a sample selecting colleague impacts auditors' willingness to interact with evidence, including prior performance, status within the firm, and experience.

APPENDIX A: EXPERIMENTAL INSTRUMENT – EXPERIMENT 1

Introductory Instructions

This exercise will ask you to assume that you are a relatively new staff auditor at an international public accounting firm. **PLEASE COMPLETE THE EXERCISE IN ONE SITTING.**

First, however, the following screens will ask you to answer **4 questions** about the purchasing/payables cycle. Please answer the questions to the **best of your ability** without using any additional resources (i.e., textbooks, Internet, or peers).

Knowledge Check 1: Performing a Search for Unrecorded Liabilities tests which financial statement assertion?_____

Knowledge Check 2: Your colleague, S.V., has prepared the following documentation related to an unrecorded liabilities test for ABC Co:

"Invoice 3456 is dated 12/26/X1 for $100,000 and pertains to rent owed to GO Management (for ABC Co.'s main office at 300 Harvard Place, Cambridge, MA) for January 20X2. Per review of the 12/31/X1 accounts payable ledger and 12/31/X1 accruals ledger, ABC Co. has properly excluded this amount from the Balance Sheet."

Please review her work and discuss any error(s) that she may have made in the space provided. If no errors were made, please write "No Errors" in the space provided. _____

Knowledge Check 3: An overstatement of reported earnings could be the result of a failure to record: 1) A dividend in arrears on preferred stock outstanding.; 2) An accrued revenue.; 3) An accrued liability.; 4) Amortization of premium on bonds payable.

Knowledge Check 4: Your colleague, J.R., has prepared the following documentation related to an unrecorded liabilities test for ABC Co.

"IBM Invoice 4567 is dated 12/29/20X1 for $50,000 and pertains to computers purchased by ABC Co. This invoice indicates that the shipping terms are FOB Destination. The receiving docket confirms that goods were received by ABC Co. on 1/02/20X2. Per review of the 12/31/20X1 accounts payable ledger and 12/31/20X1 accruals ledger ABC Co. has properly recorded a payable for $50,000 for the period ended 12/31/20X1."

Please review his work and discuss any error(s) he may have made in the space provided. If no errors were made, please write "No Errors" in the space provided. _____

Knowledge Check Transition Instructions

Thank you for answering the preliminary questions. On the next page you will begin the exercise.

For this exercise, assume that you are a relatively new audit staff with Fluss, Ibaia, and Rio LLP, an international public accounting firm. You have been assigned to the audit of Carpenter & Weaver Construction, for the year ended December 31, 20x1.

You have been instructed to complete the audit program work-step "Search for Unrecorded Liabilities," which is performed over cash disbursements made in the first 15 days of January 20x2.

The audit program instructs you to do the following:

Perform a search for unrecorded liabilities for the client, Carpenter & Weaver Construction, by:

1) Selecting transactions from the cash disbursement records for the period of January 1, 20x2 to January 15, 20x2 for testing. Sample consists of all disbursements greater than $10,000, with random selection of remaining disbursements for a final sample of 12 disbursements.

2) Review supporting documentation for selected cash disbursements and determine whether the item has been properly or improperly included or excluded as a liability as of the year-end balance sheet date. Documentation should include work performed, evidence reviewed, and conclusions reached.

Condition 1: Self

You previously selected a sample from cash disbursements made within the first 15 days after year end. Following firm guidance, **you** included all disbursements greater than **$10,000** in **your** sample, with random sampling of other disbursements to form a final sample of 10 items. **You** have been assigned to test the sample.

The client has recently made support available for **your** selected sample. As a result, you are now able to test the sample that **you** selected.

Prior to starting testing, you review the Search for Unrecorded Liabilities work folder, which includes the listings of accounts payable and subsequent disbursements:

Carpenter & Weaver Accounts Payable Listing 12.31.20X1

Carpenter & Weaver Disbursement Listing

In addition, you find the Unrecorded Liabilities Workpaper, which displays the sample you will be testing.

Audit Workpaper

You will be responsible for testing the sample contained in the workpaper. Additionally, you will be responsible for filling in any missing information. Please make sure to include the appropriate information on sampling (highlighted in yellow), which **you** were responsible for. (Please make sure to fill in sampling information with **your** initials).

Condition 2: Colleague Auditor

As you prepare to select a sample, you learn that **Taylor Zimmerman, another audit staff with Fluss, Ibaia, and Rio,** previously selected a sample from cash disbursements made within the first 15 days after year end. Following firm guidance, **Taylor** included all disbursements greater than **$10,000** in **Taylor's** sample, with random sampling of other disbursements to form a final sample of 10 items. **You** have been assigned to test the sample.

The client has recently made support available for **Taylor's** selected sample. As a result, you are now able to test the sample that **Taylor** selected.

Prior to starting testing, you review the Search for Unrecorded Liabilities work folder, which includes the listings of accounts payable and subsequent disbursements:

Carpenter & Weaver Accounts Payable Listing 12.31.20X1

Carpenter & Weaver Disbursement Listing

In addition, you find the Unrecorded Liabilities Workpaper, which displays the sample you will be testing.

Audit Workpaper

You will be responsible for testing the sample contained in the workpaper. Additionally, you will be responsible for filling in any missing information. Please make sure to include the appropriate information on sampling (highlighted in yellow), which **you** were responsible for. (Please make sure to fill in sampling information with **your** initials).

Condition 3: Technological System

As you prepare to select a sample, you learn that **F.I.R. a proprietary auditing technology system developed by Fluss, Ibaia, and Rio,** previously selected a sample from cash disbursements made within the first 15 days after year end. Following firm guidance, **the F.I.R. system** included all disbursements greater than **$10,000** in **the F.I.R. system's** sample, with random sampling of other disbursements to form a final sample of 10 items. **You** have been assigned to test the sample.

The client has recently made support available for **the F.I.R. system's** selected sample. As a result, you are now able to test the sample that **the F.I.R. system** selected.

Prior to starting testing, you review the Search for Unrecorded Liabilities work folder, which includes the listings of accounts payable and subsequent disbursements:

Carpenter & Weaver Accounts Payable Listing 12.31.20X1

Carpenter & Weaver Disbursement Listing

In addition, you find the Unrecorded Liabilities Workpaper, which displays the sample you will be testing.

Audit Workpaper

You will be responsible for testing the sample contained in the workpaper. Additionally, you will be responsible for filling in any missing information. Please make sure to include the appropriate information on sampling (highlighted in yellow), which **you** were responsible for. (Please make sure to fill in sampling information with **your** initials).

Accounts Payable Listing

Carpenter & Weaver Construction
Accounts Payable Listing
12/31/20X1

Vendor Balance Summary	Dec. 31, 20X1
AccounTemps	$ 3,250.00
ACE Fencing	$ 3,007.00
Boomtown Heavy Machinery Rental	$ 1,127.50
Capitol Scaffolding	$ 81,000.00
Contractor: Arlie, Harold	$ 19,244.00
Larry's Plumbing	$ 10,000.00
Office Plus	$ 4,323.74
Patton Hardware Supplies	$ 1,293.00
Randy's Steakhouse	$ 995.41
TripleLay Bricks	$ 3,646.50
TOTAL	$ 124,637.15

Subsequent Cash Disbursements Listing

Carpenter & Weaver Construction
Cash Disbursements 1/1/X2 to 1/15/X2

Date	Check/Trans #	Type	Invoice Reference	Vendor Name	Memo	Amount
1/03/20X2	616	Payment	4588	Contractor: Arlie, Harold	Kitchen Remodel 183 Bay Cir.	19,244.00
1/03/20X2	617	Bill Pmt - Check	869	BO Enterprises	Janitorial Prepayment - January	1,240.00
1/05/20X2	756	Check	245	TripleLay Bricks	8921 Aubrey Ave.	3,646.50
1/05/20X2	1863	Payment	76A	AccounTemps	Accounting Services	3,250.00
1/05/20X2	760	Check	1001138	State Board of California	License Renewal Fee - 20X2	300.00
1/05/20X2	621	Check	422-20X2	Boomtown Heavy Machinery	Backhoe Rental – 185 Always Ct.	2,255.00
1/07/20X2	624	Bill Pmt – Check	3456	Contractor: Larry's Plumbing	Plumbing 235 W 5th Street	10,000.00
1/08/20X2	759	Bill Pmt - Check	242	Capitol Scaffolding	Scaffolding Installments	81,000.00
1/08/20X2	618	Check	793	KD Ryan Trucking	Concrete Hauling - Elm Street	4,718.00
1/09/20X2	1866	Bill Pmt - Check	685	ADP	Monthly Payroll Services	11,682.00
1/09/20X2	620	Check	20X2-52	Robert Vance Oil Co.	Fuel Delivery	7,657.00
1/10/20X2	622	Check	1108	D.T. Orton Lumber	Lumber Delivery - 185 Always Ct.	12,115.00
1/12/20X2	763	Check	329-67	MBP CPA, LLC	Consulting Retainer	826.89
1/13/20X2	785-65	Bill Pmt - Check	243	Capital Tile	Tile 8921 Aubrey Ave.	17,765.67
1/13/20X2	1865	Check	115	Randy's Steakhouse	End-of-year Party	995.41
1/14/20X2	975	Check	657	Office Plus	Office Supplies and Equipment	4,169.93
1/15/20X2	851	Check	87	Contractor: Smith Electricians	Electrical Services 235 W 5th St.	28,100.00
1/15/20X2	1866	Sales Tax Pmt	245	State of California	ABCD 11-234567 – 12/20X1	2,721.80

Important Notes

Some important notes:

*Please do not refer to any outside materials (e.g., textbooks, Internet, or peers) other than those provided while completing this task.

*The following screens comprise the online work papers where you will document the Search. You may access support for each invoice by clicking on the relevant hyperlink. In addition, both Accounts Payable Listing & Cash Disbursement Listings are available on each invoice in case of accidental closure.

Sample Testing Item – Arlie Harold

Vendor: Arlie Harold

Arlie Harold Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

Arlie Harold

INVOICE

1120 Glenwood Ave.
Anaheim, CA 92808
Phone 805-333-3333

INVOICE #: 4588
DATE: DECEMBER 15, 20X1

To:
Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

For:
Kitchen Remodel
P.O. #324

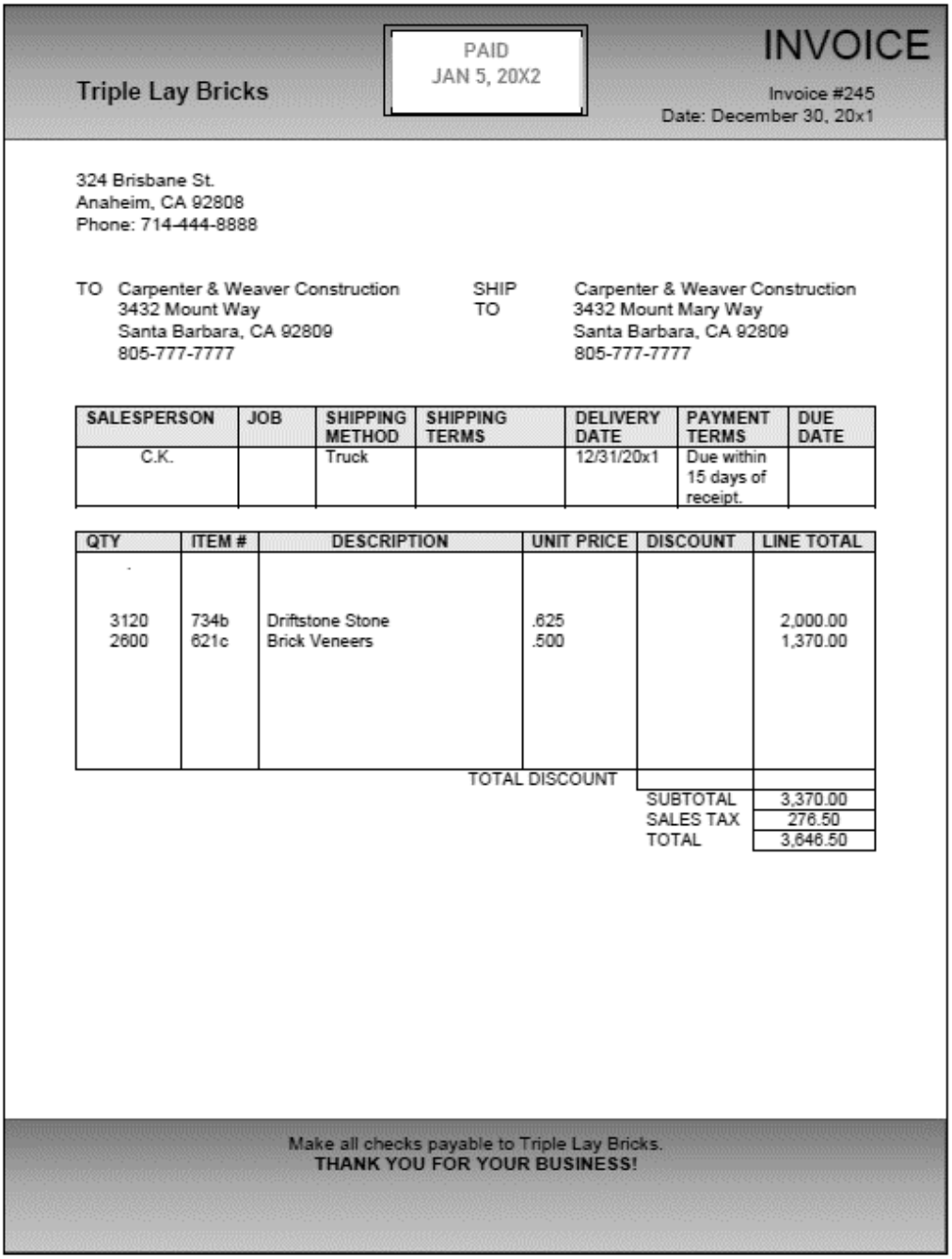
DESCRPTION	HOURS	RATE	AMOUNT
Kitchen Remodel (Labor and Supplies for the Period: 11/15/x1 – 12/15/x1)			
LABOR	40	100	4,000
SUPPLIES:			
Granite Tiles			8,000
Custom Cabinets			2,500
Lighting			2,500
Taxes			2,244
	TOTAL		19,244

PAID
JAN 3, 20X2

Sample Testing Item – Triple Lay Bricks

Vendor: Triple Lay Bricks

Triple Lay Bricks Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing



Triple Lay Bricks

PAID
JAN 5, 20X2

INVOICE

Invoice #245
Date: December 30, 20x1

324 Brisbane St.
Anaheim, CA 92808
Phone: 714-444-8888

TO Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

SHIP TO Carpenter & Weaver Construction
3432 Mount Mary Way
Santa Barbara, CA 92809
805-777-7777

SALESPERSON	JOB	SHIPPING METHOD	SHIPPING TERMS	DELIVERY DATE	PAYMENT TERMS	DUE DATE
C.K.		Truck		12/31/20x1	Due within 15 days of receipt.	

QTY	ITEM #	DESCRIPTION	UNIT PRICE	DISCOUNT	LINE TOTAL
3120	734b	Driftstone Stone	.625		2,000.00
2600	621c	Brick Veneers	.500		1,370.00
			TOTAL DISCOUNT		
				SUBTOTAL	3,370.00
				SALES TAX	276.50
				TOTAL	3,646.50

Make all checks payable to Triple Lay Bricks.
THANK YOU FOR YOUR BUSINESS!

Sample Testing Item – AccounTemps

Vendor: AccounTemps

AccounTemps Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

TMS Management
Accounting & Records Services

INVOICE

TMS Management
378 Harvard Drive
Reno, NV 89501
775-444-4477

Date: January 5, 20x2
Invoice: 67A

TO Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

SALESPERSON	JOB	PAYMENT TERMS	DUE DATE
B. Sabastini			February 8, 20X2

DESCRIPTION OF SERVICES RENDERED			
TMS	Monthly Account Reconciliations/ Close		$550.00
TMS	Financial Planning for 20X2: -Taxes -Investments (401k options) -Employee Health Plan -Debt Covenant Renegotiation *(Citi)*		$2,625.00
TMS	Notary Services		$50.00
		SUBTOTAL	$3,250
		SALES TAX	
		TOTAL	**$3,250.00**

Sample Testing Item – Boomtown Heavy Machinery *(Experiment 1 Only - Didn't Load Properly in Administration 2)*

Vendor: Boomtown Heavy Machinery

Boomtown Heavy Machinery Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

#

BOOMTOWN HEAVY MACHINERY RENTAL & LEASING, LLC
888 BISMARCK COVE RD.
LOS ANGELES, CA 91316
PHONE: (800) 747-7474

RENTOR/LEASOR	CONTRACTOR Carpenter & Weaver Construction	DATE December 28, 20X1
	CUSTOMER ID#: 1387292819	
	REFERENCE #: 422-20X2	

Machine: Backhoe Loader
Rental Rate: $275/day
Model: Case 590 Super N
Length of Rental: 8 days

Total Cost: $2,200.00

Preferred Customer Discount: -$50.00

Subtotal: **$2,150.00**
Sales Tax: **$105.00**

TOTAL DUE: **$2,255.00**

PAID
JAN 5, 20X2

Description: Backhoe Loader rental for use on a construction project at 185 Always Ct. Equipment to be delivered at location for use on emergency project from December 28, 20X1 through January 4, 20X2.

THANK YOU FOR CHOOSING BOOMTOWN FOR YOUR EQUIPMENT NEEDS!

TERMS OF USE

1) RENTER shall keep and maintain rented equipment during the terms of the rental period at his own cost and expense. Equipment shall be kept in a good state of repair, normal wear and tear excepted.
2) RENTER shall pay OWNER full compensation for replacement and/or repair of any equipment which is not returned due to loss or theft, or any equipment which is damaged and in need of repair to return it to the state in which it was rented, normal wear and tear excepted.
3) RENTER shall not remove the equipment from the address of RENTER or the location shown herein as the place of use for the equipment without prior written approval of the OWNER. RENTER shall inform the OWNER upon demand of the exact location of the equipment while RENTER's possession.

Sample Testing Item – Larry's Plumbing

Vendor: Larry's Plumbing

Larry's Plumbing Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

PLUMBING WORK ORDER No. 3456

PAID
OCT 23, 20X1

LARRY'S PLUMBING
License B0000123
65 Pine Avenue Suite 310
Long Beach, CA 90802
Phone: (800)-555-5151

CONTRACTOR/OWNER	CONTRACTOR Carpenter & Weaver Construction		DATE October 15, 20x1
	PROJECT ADDRESS 235 W. 5th Street	STATE/ZIP CA/92809	PHONE
	ALTERNATE ADDRESS 3432 Mount Way	STATE/ZIP CA/92809	PHONE 805-777-7777

Lead Engineer: T. Hurd

Work Performed at: 235 W. 5th Street **Description (if known):** Downtown Office Building

We will furnish all materials and necessary equipment, and perform all labor necessary to complete the following work (Describe labor, material and equipment necessary):

Lavatories installations ~SUMP and SEWAGE Pumps

Piping & Installation – GAS
Piping & Installation – DRAIN
Piping & Installation – WATER

PAID
JAN 07, 20X2

Time for Completion: **Begin:** November 3, 20x1 **Completion Date:** December 18, 20x1

Payment: This is a cash transaction. Contractor agrees to a price of $15,000. The payment schedule will be:

Down payment (retainer) of $5,000, (2) Payment of Services Rendered $10,000 due on project completion

CONTRACTOR/SELLER Signature: T. Hurd, Larry's Plumbing 10-15-x1

BUYER/OWNER Signature: E. Kent, C&WC 10-15-X1

Sample Testing Item – Capitol Scaffolding

Vendor: Capitol Scaffolding

Capitol Scaffolding Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

WORK ORDER

Date: December 3, 20x1
W.O. #242 (a)(b)

Capitol Scaffolding
6644 Forest Lance
San Luis Obispo, CA 90087
805-645-6456

JOB: 786-35

TO: Carpenter & Weaver
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

DESCRIPTION	LABOR	NOTES	LINE TOTAL
Project: 235 W 5th St. Office Building			
Refer to Contract ID #786-35 for full services rendered			
Add On:			
Independent Perimeter: Erect Base Lift			
Independent Perimeter: Erect Scaffold Lift 2			
Independent Perimeter: Erect Top Lift 2			
**Payment #1 – 20x1 Payable	Labor Expense 12/1/20x1 – 12/31/20x1	Current Period	$40,500
**Payment #2 – 20x2 Payable	Labor Expense 1/15/20x2 – 2/15/20x2	Billed in Advance	$40,500
		Subtotal	$40,500
		Sales Tax	$40,500
		Total	$81,000

PAID
JAN 8, 20X2

"We take you higher!"
Visit us at www.capitolscaffolding.com

Make all checks payable to Capitol Scaffolding

THANK YOU FOR YOUR BUSINESS

Sample Testing Item – KD Ryan Trucking

Vendor: KD Ryan Trucking

KD Ryan Trucking Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

KD Ryan Trucking
743 Osborne Rd.
Anaheim, CA 92804
Phone: 820-111-1111

Invoice #793
January 3, 20x2

Bill to: Carpenter & Weaver Construction, 3432 Mount Way, Santa Barbara, CA 92809

Qty	Description		Amount
1	Cement Delivery on Elm Drive – 01/03/20x2		4,142.40
	Sales Tax		575.60
	Total		4,718.00

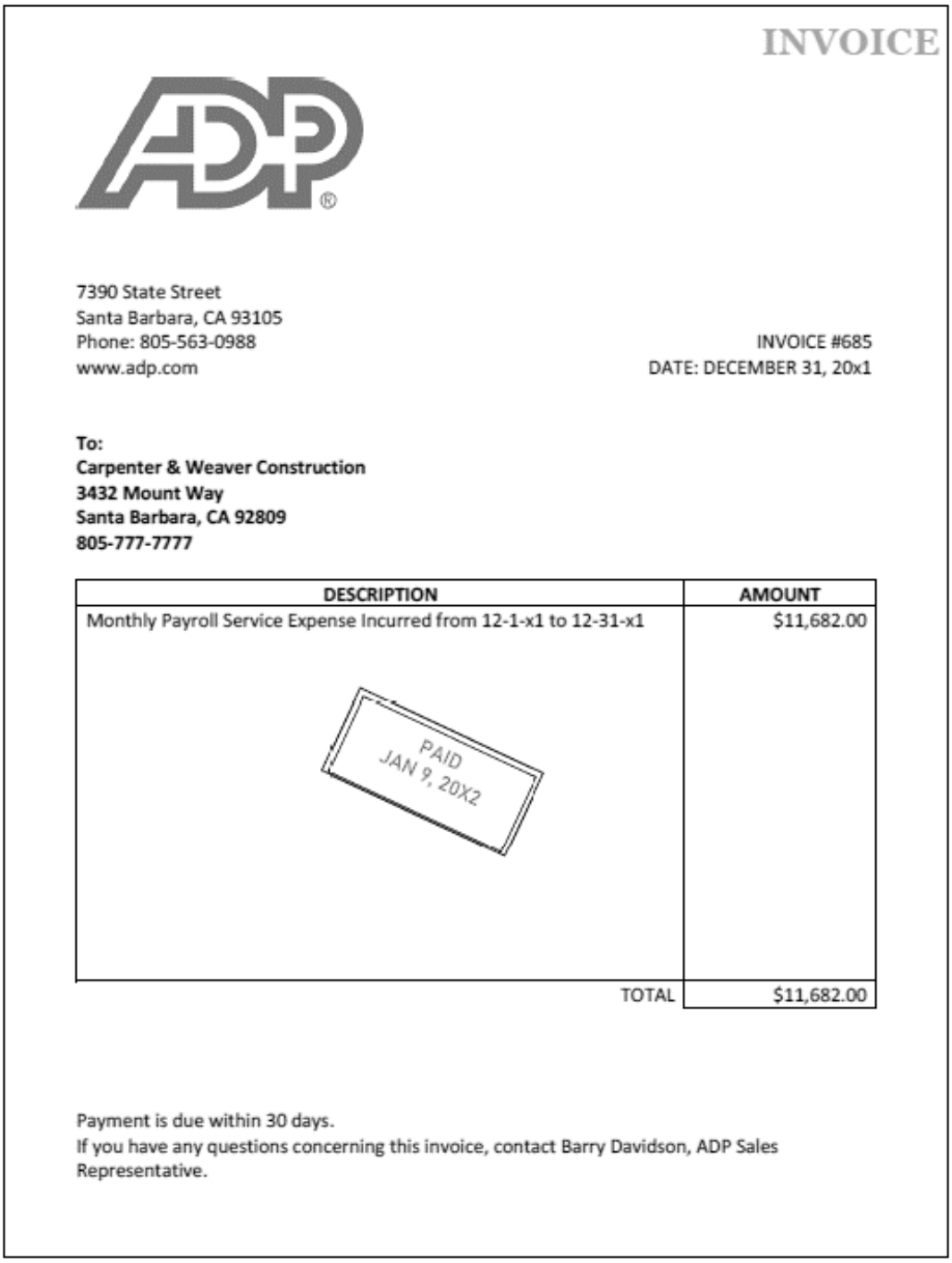
PAID
JAN 8, 20X2

KD Ryan Trucking
Serving Southern California for over 60 years
Thank you for your loyalty!

Sample Testing Item – ADP

Vendor: ADP

ADP Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing



INVOICE

7390 State Street
Santa Barbara, CA 93105
Phone: 805-563-0988
www.adp.com

INVOICE #685
DATE: DECEMBER 31, 20x1

To:
Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

DESCRIPTION	AMOUNT
Monthly Payroll Service Expense Incurred from 12-1-x1 to 12-31-x1	$11,682.00
TOTAL	$11,682.00

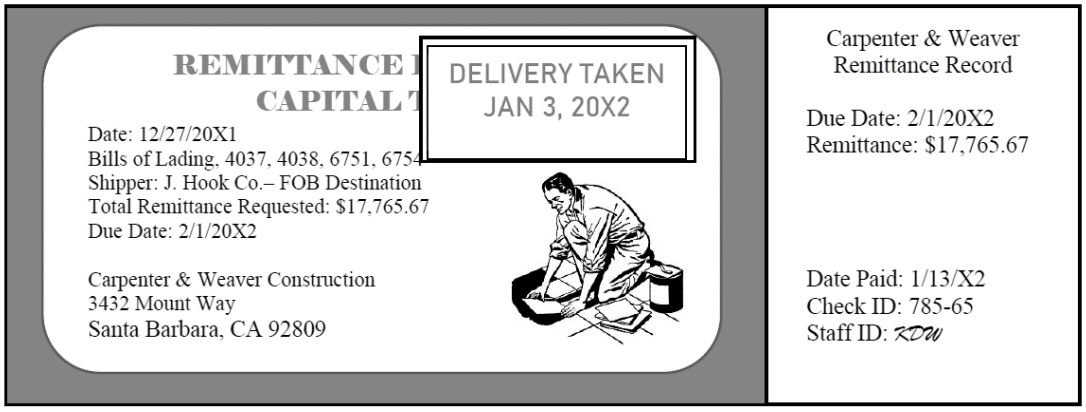
PAID
JAN 9, 20X2

Payment is due within 30 days.
If you have any questions concerning this invoice, contact Barry Davidson, ADP Sales Representative.

Sample Testing Item – Capital Tile

Vendor: Capital Tile

Capital Tile Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing



REMITTANCE I
CAPITAL T

DELIVERY TAKEN
JAN 3, 20X2

Date: 12/27/20X1
Bills of Lading, 4037, 4038, 6751, 6754
Shipper: J. Hook Co.– FOB Destination
Total Remittance Requested: $17,765.67
Due Date: 2/1/20X2

Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809

Carpenter & Weaver
Remittance Record

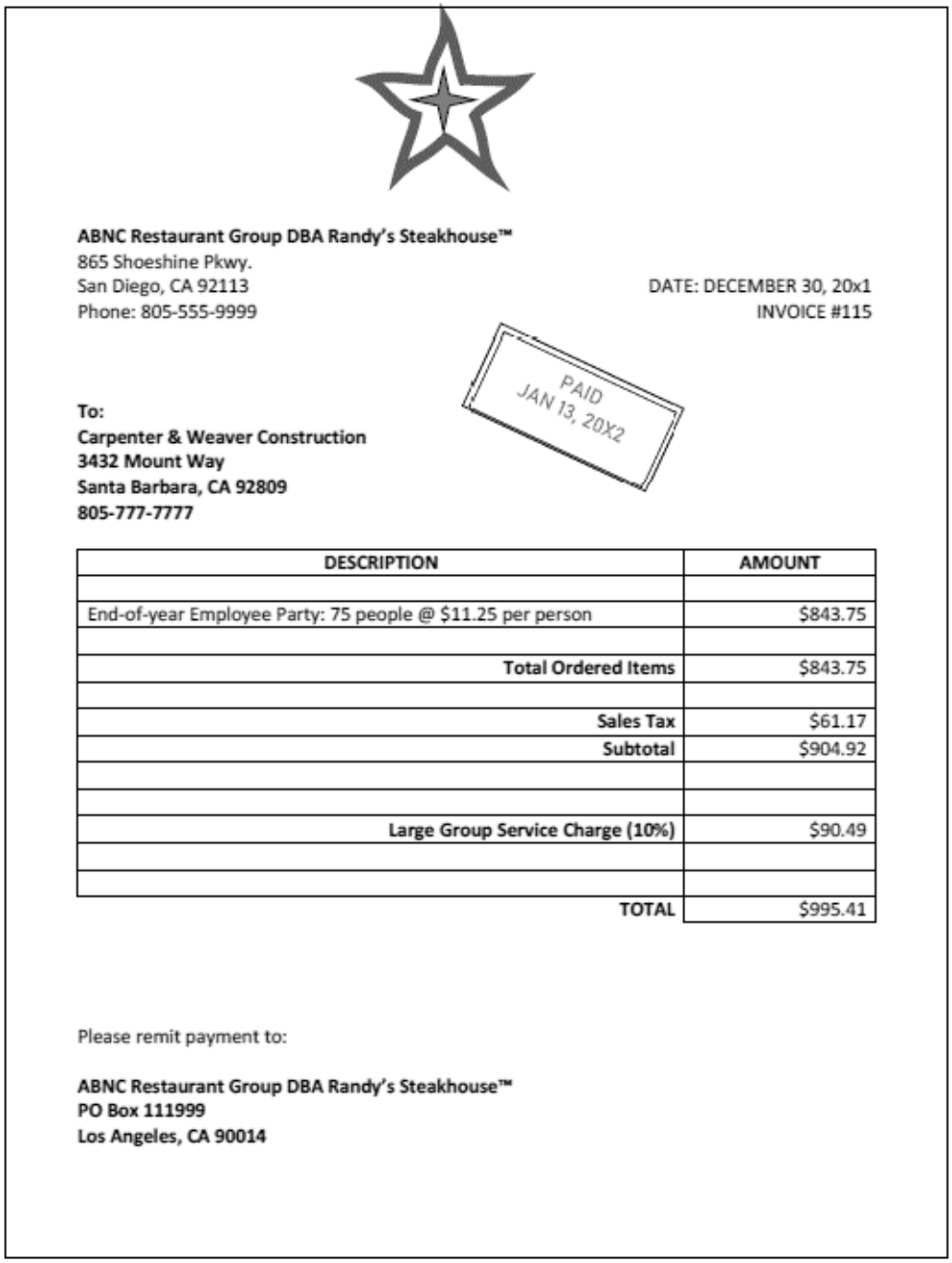
Due Date: 2/1/20X2
Remittance: $17,765.67

Date Paid: 1/13/X2
Check ID: 785-65
Staff ID: RDW

Sample Testing Item – Randy's Steakhouse

Vendor: Randy's Steakhouse

Randy's Steakhouse Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing



ABNC Restaurant Group DBA Randy's Steakhouse™
865 Shoeshine Pkwy.
San Diego, CA 92113
Phone: 805-555-9999

DATE: DECEMBER 30, 20x1
INVOICE #115

PAID
JAN 13, 20X2

To:
Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

DESCRIPTION	AMOUNT
End-of-year Employee Party: 75 people @ $11.25 per person	$843.75
Total Ordered Items	$843.75
Sales Tax	$61.17
Subtotal	$904.92
Large Group Service Charge (10%)	$90.49
TOTAL	$995.41

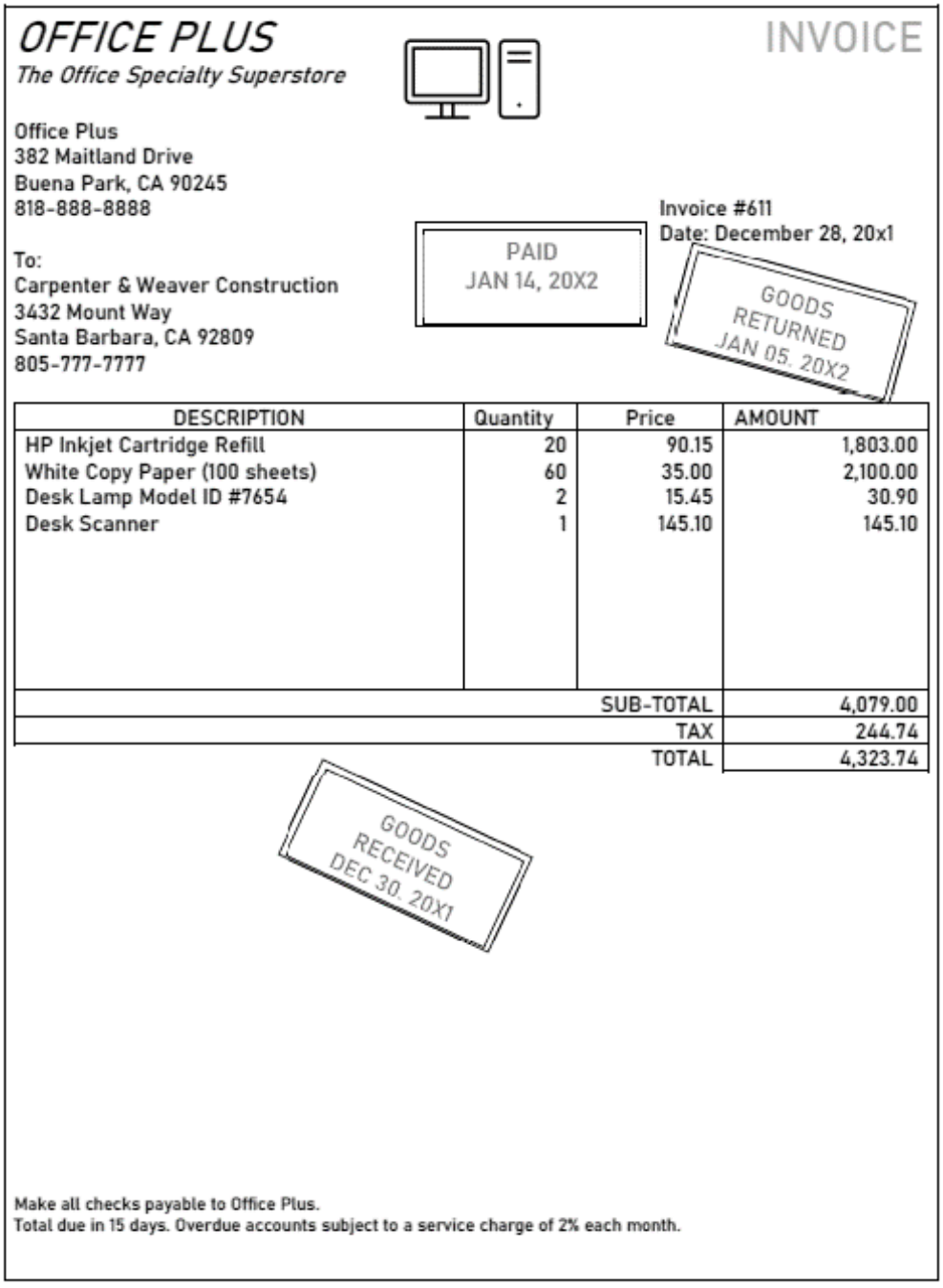
Please remit payment to:

ABNC Restaurant Group DBA Randy's Steakhouse™
PO Box 111999
Los Angeles, CA 90014

Sample Testing Item – Office Plus

Vendor: Office Plus

Office Plus Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing



OFFICE PLUS
The Office Specialty Superstore

INVOICE

Office Plus
382 Maitland Drive
Buena Park, CA 90245
818-888-8888

Invoice #611
Date: December 28, 20x1

PAID
JAN 14, 20X2

GOODS RETURNED
JAN 05, 20X2

To:
Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

DESCRIPTION	Quantity	Price	AMOUNT
HP Inkjet Cartridge Refill	20	90.15	1,803.00
White Copy Paper (100 sheets)	60	35.00	2,100.00
Desk Lamp Model ID #7654	2	15.45	30.90
Desk Scanner	1	145.10	145.10
		SUB-TOTAL	4,079.00
		TAX	244.74
		TOTAL	4,323.74

GOODS RECEIVED
DEC 30, 20X1

Make all checks payable to Office Plus.
Total due in 15 days. Overdue accounts subject to a service charge of 2% each month.

Sample Testing Item – Smith Electricians ***(****Administration 1 Only)*

Vendor: Office Depot

Office Depot Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

Smith
Electricians

"We'll turn your lights on!"

Invoice

Date: January 9, 20x2
Invoice: #87

PAID
JAN 15, 20X2

To: Carpenter & Weaver
3432 Mount Way
Santa Barbara, CA 92809
805-777-7777

Salesperson	Job	Payment Terms	Due Date
Jim Smith	235 W. 5th St. Office Building	Due on receipt	January 15, 20x2

Hours	Description	Hourly Rate	Line Total
	Journeyman Labor – expedite (12/13/X1 to 12/17/X1)		$9,500
	Journeyman Labor – expedite (1/3/X2 to 1/7/X2)		$9,500
	Apprentice Labor – expedite (12/13/X1 to 12/17/X1		$3,500
	Apprentice Labor – expedite (1/3/X2 to 1/7/X2)		$2,100
	Administrative Fees (12/29/X1 to 1/4/X2)		$3,500
		Subtotal	$28,100
		Taxes	
		Total	**$28,100**

Make all checks payable to Smith Electricians

Thank you for your business!

Smith Electricians, 870 Knott Ave, Buena Vista Park, CA 818-366-3666

Dependent Variable - Error Identification (For Each Invoice)

What year is the expense for XXXX related to?
Multiple Choice Options: 1) 20X1; 2) 20X2; 3) Both 20X1 and 20X2; 4) Neither 20X1 nor 20X2; 5) Adequate support not provided. Please explain:_____

Please document the liabilities amount(s) associated with XXXX's invoice in the appropriate category(ies).
Multiple Choice Options: 1) $_____ is properly included in 20X1; 2) $_____ is improperly included in 20X1; 3) $_____ is properly excluded in 20X1; 4) $_____ is improperly excluded in 20X1; 5) Adequate support not provided. Please explain:_____

Additional Issues

While performing testing, did you come across any problems with the sample you were testing? Were there items that should have been included in the sample, but weren't?
Multiple Choice: 1)Yes; 2) No

You indicated that you did happen across exceptions in the sample. Please document any identified irregularities in the box below. *(Only displayed if participant selected "Yes")*

_____

Instructions – Additional Questions

Thank you for your participation to this point. The following screens will pose several true/false statements. Please indicate your agreement/disagreement with each statement by moving the slider beneath each statement.

Psychological Ownership

I feel a high degree of personal ownership for the Search for Unrecorded Liabilities task.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I feel that this is my Search for Unrecorded Liabilities task.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I sense that the Search for Unrecorded Liabilities is my work.
Seven-point Likert (Strongly Disagree, Strongly Agree)

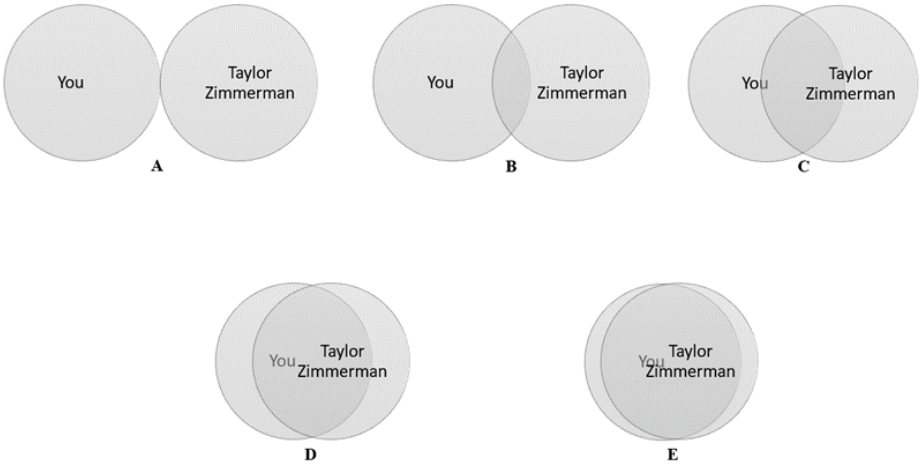
Task Engagement

My mind was focused on performing the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I paid a lot of attention to the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

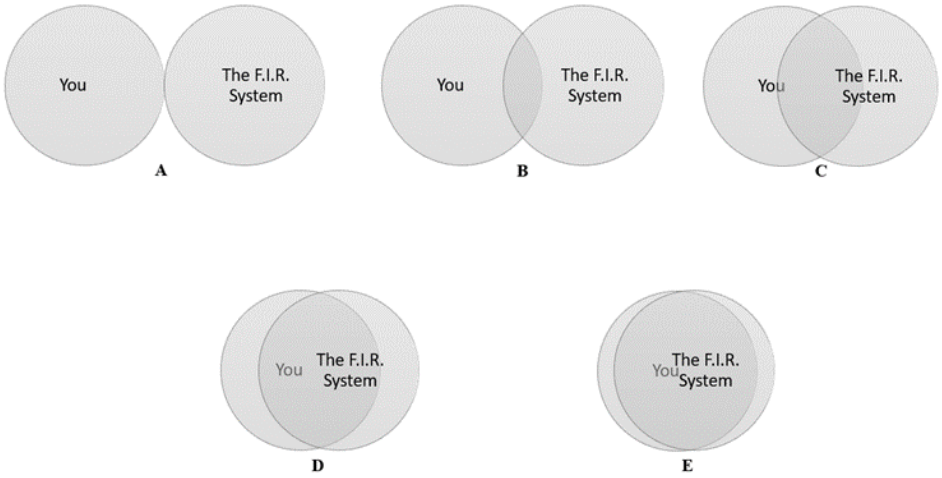
I concentrated on performing the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

Perceived Social Identity (*Condition "Colleague Auditor" Only*)



Select the picture which best describes how your personal attributes, qualities, and values **align or overlap** with the attributes, qualities, and values of **Taylor Zimmerman, the audit staff who selected the sample you tested.**
Multiple Choice: 1) A; 2) B; 3) C; 4) D; 5) E

Perceived Social Identity (*Condition "System" Only*)



Select the picture which best describes how your personal attributes, qualities, and values **align or overlap** with the attributes, qualities, and values of the **F.I.R. System, which selected the sample you were responsible for testing.**
Multiple Choice: 1) A; 2) B; 3) C; 4) D; 5) E

Perceived Effort

Please indicate your agreement with the following statements:

Constructing the sample for testing Unrecorded Liabilities required significant effort.
Seven-point Likert (Strongly Disagree and Strongly Agree)

Constructing the sample for testing Unrecorded Liabilities required a significant amount of time.
Seven-point Likert (Strongly Disagree and Strongly Agree)

Task Significance

How important is the Search for Unrecorded Liabilities testing to the overall performance of the audit?
Seven-point Likert (Not at all important and Extremely important)

Personal/Shared Responsibility

If there are errors in testing Unrecorded Liabilities, I should be held responsible.
Seven-point Likert (Strongly Disagree, Strongly Agree)

If there are errors in testing Unrecorded Liabilities, Taylor Zimmerman should be held responsible. *(Condition "Colleague Auditor" Only)*
Seven-point Likert (Strongly Disagree, Strongly Agree)

If there are errors in the testing of Unrecorded Liabilities, the FIR system should be held responsible. *(Condition "System" Only)*
Seven-point Likert (Strongly Disagree, Strongly Agree)

General Demographic Questions

What is your level of education?
Multiple Choice Options: 1) Undergraduate (Sophomore); 2) Undergraduate (Junior); 3) Undergraduate (Senior); 4) Recently Completed Undergraduate Degree; 5) Graduate Student

Have you previously completed an Auditing course? (Search for Unrecorded Liabilities)?
Multiple Choice Options: 1) Yes, I have completed **one** Auditing course; 3) Yes, I have completed **more than one** Auditing course; 3) No, I have not completed an Auditing course.

Have you previously held an audit internship with a public accounting firm?
Multiple Choice Options: 1) Yes; 2) No

Have you previously performed a Search for Unrecorded Liabilities (i.e., this task)?
Multiple Choice Options: 1) Yes; 2) No

APPENDIX B: EXPERIMENTAL INSTRUMENT – EXPERIMENT 2

Introductory Instructions

Thank you for agreeing to participate in this exercise. **Please complete the entire exercise in one sitting and answer all questions to the best of your ability without additional resources (i.e., textbooks, internet, or peers.)**

The following screens will ask you 3 questions about the purchasing/payables cycle.

Knowledge Check 1: Performing a Search for Unrecorded Liabilities tests which financial statement assertion?_____

Knowledge Check 2: Your colleague, S.V., has prepared the following documentation related to an unrecorded liabilities test for ABC Co:

"Invoice 3456 is dated 12/26/20 for $100,000 and pertains to rent owed to GO Management (for ABC Co.'s main office at 300 Harvard Place, Cambridge, MA) for January 2021. Per review of the 12/31/20 accounts payable ledger and 12/31/20 accruals ledger, ABC Co. has properly excluded this amount from the Balance Sheet."

Please review her work and discuss any error(s) that she may have made in the space provided. If no errors were made, please write "No Errors" in the space provided. _____

Knowledge Check 3: Your colleague, J.R., has prepared the following documentation related to an unrecorded liabilities test for ABC Co.

"IBM Invoice 4567 is dated 12/29/2020 for $50,000 and pertains to computers purchased by ABC Co. This invoice indicates that the shipping terms are FOB Destination. The receiving docket confirms that goods were received by ABC Co. on 1/02/2021. Per review of the 12/31/2020 accounts payable ledger and 12/31/2020 accruals ledger ABC Co. has properly recorded a payable for $50,000 for the period ended 12/31/2020."

Please review his work and discuss any error(s) he may have made in the space provided. If no errors were made, please write "No Errors" in the space provided. _____

Condition 1: High Effort, No Technology

For this exercise, assume that you are a relatively new staff auditor with Fluss, Ibaia, and Rio LLP, a large international accounting firm. Recently, firm management has emphasized the need for higher quality audit processes. With that in mind, the firm has developed a clarified set of procedures and guidelines for audit performance. While the firm is still in the process of rolling the clarified procedures out to all engagements, no problems have been noted.

You have been assigned to the audit of Carpenter & Weaver Construction, for the year ended December 31, 2020. You are responsible for performing a **Search for Unrecorded Liabilities**, which involves testing a sample of cash disbursements made within the first 15 days of year-end to determine if liabilities are appropriately recorded. Your manager informs you that **Taylor Zimmerman, another audit staff** with Fluss, Ibaia, and Rio, was assigned the preparatory work of cleaning the data, selecting a sample, and working with the client to obtain support for the sample. You are responsible for testing the sample that **Taylor** selected.

The client has recently provided all requested support and you may now begin testing. You receive the following email from Taylor Zimmerman:

From: taylor.zimmerman@flussibaiario.com
Subject: Unrecorded Liabilities

Hello,

I wanted to send a short email as part of the handoff of the Unrecorded Liabilities audit task. I received the workpaper **two weeks ago** and it took **much longer than I initially expected** to get C&W's data cleaned and sample selected. The accounting department at C&W were very busy given their month-end and I had to remind them more than once to send support for my sample. In short, this took a lot of effort.

I relied extensively on management's **clarified audit procedures**, which helped me to identify the riskiest items for testing. I hope you don't run into too many problems in your testing.

Best,

Taylor Zimmerman
Audit Staff

Fluss, Ibaia, and Rio, LLP

Condition 2: Low Effort, No Technology

For this exercise, assume that you are a relatively new staff auditor with Fluss, Ibaia, and Rio LLP, a large international accounting firm. Recently, firm management has emphasized the need for higher quality audit processes. With that in mind, the firm has developed a clarified set of procedures and guidelines for audit performance. While the firm is still in the process of rolling the clarified procedures out to all engagements, no problems have been noted.

You have been assigned to the audit of Carpenter & Weaver Construction, for the year ended December 31, 2020. You are responsible for performing a **Search for Unrecorded Liabilities**, which involves testing a sample of cash disbursements made within the first 15 days of year-end to determine if liabilities are appropriately recorded. Your manager informs you that **Taylor Zimmerman, another audit staff** with Fluss, Ibaia, and Rio, was assigned the preparatory work of cleaning the data, selecting a sample, and working with the client to obtain support for the sample. You are responsible for testing the sample that **Taylor** selected.

The client has recently provided all requested support and you may now begin testing. You receive the following email from Taylor Zimmerman:

From: taylor.zimmerman@flussibaiario.com
Subject: Unrecorded Liabilities

Hello,

I wanted to send a short email as part of the handoff of the Unrecorded Liabilities audit task. I received the workpaper **two days ago** and it took **much less time than I initially expected** to get C&W's data cleaned and sample selected. The accounting department at C&W were not very busy and I did not have to remind them to send support for my sample. In short, this did not take very much effort.

I relied extensively on management's **clarified audit procedures**, which helped me to identify the riskiest items for testing. I hope you don't run into too many problems in your testing.

Best,

Taylor Zimmerman
Audit Staff

Fluss, Ibaia, and Rio, LLP

Condition 3: High Effort, Technology

For this exercise, assume that you are a relatively new staff auditor with Fluss, Ibaia, and Rio LLP, a large international accounting firm. Recently, firm management has been pushing for a greater integration of technology in the audit. With that in mind, the firm has developed EvalTech, a proprietary audit technology designed to assist with fairly routine audit tasks. While the firm is still in the process of rolling EvalTech out to all engagements, no problems have been noted.

You have been assigned to the audit of Carpenter & Weaver Construction, for the year ended December 31, 2020. You are responsible for performing a **Search for Unrecorded Liabilities**, which involves testing a sample of cash disbursements made within the first 15 days of year-end to determine if liabilities are appropriately recorded. Your manager informs you that **Taylor Zimmerman, another audit staff** with Fluss, Ibaia, and Rio, was assigned the preparatory work of cleaning the data, selecting a sample, and working with the client to obtain support for the sample. You are responsible for testing the sample that **Taylor** selected.

The client has recently provided all requested support and you may now begin testing. You receive the following email from Taylor Zimmerman:

From: taylor.zimmerman@flussibaiario.com
Subject: Unrecorded Liabilities

Hello,

I wanted to send a short email as part of the handoff of the Unrecorded Liabilities audit task. I received the workpaper **two weeks ago** and it took **much longer than I initially expected** to get C&W's data cleaned and sample selected. The accounting department at C&W were very busy given their month-end and I had to remind them more than once to send support for my sample. In short, this took a lot of effort.

I relied extensively on **EvalTech**, which helped me to identify the riskiest items for testing. I hope you don't run into too many problems in your testing.

Best,

Taylor Zimmerman
Audit Staff

Fluss, Ibaia, and Rio, LLP

Condition 4: Low Effort, Technology

For this exercise, assume that you are a relatively new staff auditor with Fluss, Ibaia, and Rio LLP, a large international accounting firm. Recently, firm management has been pushing for a greater integration of technology in the audit. With that in mind, the firm has developed **EvalTech,** a proprietary audit technology designed to assist with fairly routine audit tasks. While the firm is still in the process of rolling **EvalTech** out to all engagements, no problems have been noted.

You have been assigned to the audit of Carpenter & Weaver Construction, for the year ended December 31, 2020. You are responsible for performing a **Search for Unrecorded Liabilities**, which involves testing a sample of cash disbursements made within the first 15 days of year-end to determine if liabilities are appropriately recorded. Your manager informs you that **Taylor Zimmerman, another audit staff** with Fluss, Ibaia, and Rio, was assigned the preparatory work of cleaning the data, selecting a sample, and working with the client to obtain support for the sample. You are responsible for testing the sample that **Taylor** selected.

The client has recently provided all requested support and you may now begin testing. You receive the following email from Taylor Zimmerman:

From: taylor.zimmerman@flussibaiario.com
Subject: Unrecorded Liabilities

Hello,

I wanted to send a short email as part of the handoff of the Unrecorded Liabilities audit task. I received the workpaper **two days ago** and it took **much less time than I initially expected** to get C&W's data cleaned and sample selected. The accounting department at C&W were not very busy and I did not have to remind them to send support for my sample. In short, this did not take very much effort.

I relied extensively on **EvalTech**, which helped me to identify the riskiest items for testing. I hope you don't run into too many problems in your testing.

Best,

Taylor Zimmerman
Audit Staff

Fluss, Ibaia, and Rio, LLP

All Conditions - Attention Check Questions

Taylor Zimmerman indicated that preparing the sample took:
Multiple Choice Options: 1) More time than expected; 2) No more or less time than was expected; 3) Less time than expected.

Taylor Zimmerman indicted that the sample was primarily constructed:
Multiple Choice Options: 1)Using updated audit procedures provided by management. 2) Using EvalTech, a proprietary technology developed by the firm; 3) Exclusively using random sampling without consideration of risk.

Important Notes

*Please do not refer to any outside materials (e.g., textbooks, Internet, or peers) other than those provided while completing this task. **Please complete the task in one sitting.**

*The following screens comprise the online work papers where you will document the Search. You may access support for each invoice by clicking on the relevant hyperlink. In addition, both Accounts Payable Listing & Cash Disbursement Listings are available on each invoice in case of accidental closure.

***For each invoice, you will be required to complete documentation and then note whether the related expense was appropriately recorded or not. If you indicate an expense as inappropriately recorded, you will then be asked to indicate why.**

The testing work folder contains the following documents. This information will be accessible while you are testing, in the event that either tab is closed.

Carpenter & Weaver Accounts Payable Listing 12.31.2020 *(Appendix A – Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*
Carpenter & Weaver Disbursement Listing *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Arlie Harold *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Triple Lay Bricks *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – AccounTemps *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Boomtown Heavy Machinery *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Larry's Plumbing *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Capitol Scaffolding *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – ADP *(Appendix A - Dates converted from 20X1 and 20X2 to 2020 and 2021, respectively)*

Sample Testing Item – Capital Tile *(Different from Experiment 1)*

Vendor: Capital Tile

Capital Tile Vendor Invoice
Carpenter & Weaver Accounts Payable Listing 12.31.2020
Carpenter & Weaver Disbursement Listing

REMITTANCE REQUEST
CAPITAL TILE

Date: 12/27/2020
Bills of Lading, 4037, 4038
Shipper: J. Hook Co.– FOB
Total Remittance Requeste
Due Date: 2/1/2021

Carpenter & Weaver Construction
3432 Mount Way
Santa Barbara, CA 92809

DELIVERY TAKEN
JAN 3, 2021

Carpenter & Weaver
Remittance Record

Due Date: 2/1/2021
Remittance: $17,765.67

Date Paid: 1/13/21
Check ID: 785-65
Staff ID: RDW

Each invoice included the below "virtual workpaper:"

		PER VENDOR INVOICE		
	Sample Selected By (Initials):	Invoice Number	Invoice Date	Invoice Amount
AccounTemps				

Dependent Variable - Error Identification (Shown for Each Invoice)

Please document your conclusion with respect to the liability for XXXX.
Multiple Choice Options: 1) Liability appropriately recorded; 2) Liability **not** appropriately recorded; 3) Adequate support not provided. Conclusion could not be reached.

Please explain the rationale for your conclusion with respect to the liability for XXXX.
(Only displayed if participant selected "Liability not appropriately recorded" or "Adequate support not provided. Conclusion could not be reached.") _____

Dependent Variable - Continuous

How confident are you that liabilities are appropriately recorded?
Seven-point Likert (Not at all Confident, Extremely Confident)

Suppose the engagement team has an additional 10 hours that COULD be allocated to testing liabilities. How many more hours (out of 10) should be allocated to testing liabilities?
Slider Bar (0, 10)

Instructions – Additional Questions

Thank you for your participation to this point. The following screens will pose several true/false statements. Please indicate your agreement/disagreement with each statement by moving the slider beneath each statement.

Psychological Ownership

I feel a high degree of personal ownership for the Search for Unrecorded Liabilities task.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I feel that this is my Search for Unrecorded Liabilities task.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I sense that the Search for Unrecorded Liabilities is my work.
Seven-point Likert (Strongly Disagree, Strongly Agree)

Task Engagement

My mind was focused on performing the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I paid a lot of attention to the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

I concentrated on performing the Search for Unrecorded Liabilities.
Seven-point Likert (Strongly Disagree, Strongly Agree)

Perceived Effort

How much effort and time were involved in constructing the sample that you were responsible for testing?
Seven-point Likert (Very Little Effort and Time; Extreme Effort and Time)

Task Significance

How important is the Search for Unrecorded Liabilities testing to the overall performance of the audit?
Seven-point Likert (Not at all Important, Extremely Important)

Sample Quality

Please rate how highly, you perceive the quality of the sample you received from Taylor Zimmerman.
Seven-point Likert (Terrible, Excellent)

Personal/Shared Responsibility

If there are errors in testing Unrecorded Liabilities, I should be held responsible.
Seven-point Likert (Strongly Disagree, Strongly Agree)

If there are errors in testing Unrecorded Liabilities, Taylor Zimmerman should be held responsible.
Seven-point Likert (Strongly Disagree, Strongly Agree)

General Demographic Questions

What is your level of education?
Multiple Choice Options: 1) Undergraduate (Sophomore); 2) Undergraduate (Junior); 3) Undergraduate (Senior); 4) Recently Completed Undergraduate Degree; 5) Graduate Student

Have you previously held an audit internship with a public accounting firm?
Multiple Choice Options: 1) Yes; 2) No

Have you previously performed a Search for Unrecorded Liabilities (i.e., this task)?
Multiple Choice Options: 1) Yes; 2) No

Have you previously participated in an experiment that uses the same task as this one (Search for Unrecorded Liabilities)?
Multiple Choice Options: 1) Yes; 2) No

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VITA

CHRISTOPHER A. PEARSON

EDUCATION	**Attended Ph.D. Program**, University of Kentucky	2017-2022
	Master of Accountancy, Southern Utah University	2015-2016
	B.S., Accounting, Southern Utah University	2012-2016
PROFESSIONAL PUBLICATIONS	**"Online worker fraud and evolving threats to the integrity of MTurk data: A discussion of virtual private servers and the Limitations of IP-based screening procedures."** With Sean Dennis of the University of Central Florida and Brian Goodson of Clemson University. *Behavioral Research in Accounting*, 2020, Vol. 32(1), 119-134.	
SCHOLASTIC HONORS	Teaching Excellence Award – 2021 - Gatton College of Business and Economics	
PROFESSIONAL EMPLOYMENT	Staff Auditor, Deloitte	2016-2017
	Accounting Data Entry – Leavitt Group	2015-2016
	Audit Intern – Deloitte	2015
	Regional Controller Assistant – Leavitt Group	2014-2015
PROFESSIONAL CERTIFICATION	Licensed Certified Public Accountant (Kentucky)	2018-Present