

Tax Audits and Accounting Labor Investment

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Abstract

We examine firms' internal organizational responses to tax enforcement. Using employment data and a proxy for IRS audits, we find that firms respond to IRS audits by hiring an average of three additional accounting employees (a 5.3% increase), including both tax and general (non-tax) accountants. We also document systematic evidence of a trade-off between firms' investments in internal versus external tax expertise around the audit. Further tests indicate that tax accountants are hired when greater tax savings are at risk, while general accountants are hired to meet the broad information demands of the audit. Tax (general) accounting hires are more senior (junior), consistent with hiring tax accountants for their tacit knowledge to assist with sustaining contested tax positions, and hiring general accountants to handle more routine accounting functions to free up capacity to manage audit-related information demands. Finally, path analyses indicate that accounting labor investments offset audit-induced increases in cash effective tax rates and partially explain the documented effects of tax enforcement on firms' information environments. Overall, we identify hiring in-house accountants as a key "real" organizational response to managing the risks and information demands of tax enforcement. In doing so, we answer calls for empirical research linking business taxes with firms' labor capital decisions.

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1. Introduction

Tax enforcement can generate positive externalities for firms, including improvements in the information environment and a reduction in agency costs (e.g., Desai et al. 2007; Guedhami and Pittman 2008; El Ghouli et al. 2011; Hanlon et al. 2014; Bauer et al. 2021). At the same time, it imposes a significant burden on firms' resources and has been associated with higher tax payments and reduced business activity (Hoopes et al. 2012; Fox et al. 2022; Belnap et al. 2024; Gallemore and Jacob 2025). This evidence for both benefits and costs of tax enforcement highlights the need to understand the internal organizational channels through which firms respond to the associated information demands and risks. A natural, though previously unexplored, response is labor investment in the internal accounting function. While costly, hiring in-house accountants can increase firms' capacity to collect and analyze information, provide robust documentation, and mitigate risks associated with a tax audit. In this study, we examine whether and how firms adjust their investments in accounting human capital in response to tax audits.

Firms should expand their accounting workforce in response to tax enforcement when managers expect that the benefits of additional internal expertise exceed the costs. On the one hand, tax audits create risks, including uncertainty regarding the sustainability of claimed tax positions and potential penalties. Firms may respond by expanding their internal expertise to mitigate these risks, consistent with tax enforcement motivating investment in compliance and information capacity (Hanlon et al. 2014; Bauer et al. 2021). Moreover, tax audits often require detailed information about the firm's transactions, leaving both internal tax and general (i.e., non-tax) accountants well-positioned to meet the information demands (Call et al. 2017; Giese et al. 2025). Newly hired accountants can provide tacit knowledge from prior experience or allow for reallocation of tasks among personnel (Arrow 1962; Rosenkopf and Almeida 2003; Song et al.

2003; Barrios and Gallemore 2024). If hiring accountants helps to preserve contested tax savings or reduce penalties, then investments in these personnel could be at least partially self-financing.

On the other hand, labor is an investment with high adjustment costs (Nickell 1986; Banker and Byzalov 2014). Firms may view tax audits as insufficient to justify expanding their in-house accounting workforce. Instead, they may opt to outsource work to external advisors, reduce the scope or aggressiveness of tax planning, or accept the costs of diverting accounting employees' time and attention from other tasks. Furthermore, persistent labor market frictions, such as challenges in hiring and retaining accounting expertise (Thompson Reuters 2024; Deloitte 2025a), may make alternative responses more attractive than investing in additional personnel. Accordingly, whether tax enforcement leads to incremental investment in accountants is an open empirical question.

In the U.S., Internal Revenue Service (IRS) audits provide a powerful setting to evaluate the influence of regulatory oversight on firms' labor investments. The U.S. government, via the IRS, is the *de facto* largest minority shareholder in most firms and seeks to ensure that reported taxable income accurately reflects underlying economic transactions (Desai et al. 2007). This direct interest in firms' profits, as well as the frequency and scope of IRS audits, uniquely distinguishes the IRS from other regulators, such as the Securities and Exchange Commission (SEC).¹ IRS audits impose substantial demands on firms, requiring significant time and attention from in-house accountants and management (Hanlon et al. 2014; Belnap et al. 2024). Additionally, these audits expose firms to significant tax risks. Because income taxes are a highly material

¹ For example, the Securities and Enforcement Empirical Database identified 993 SEC enforcement actions against 850 public companies and subsidiaries between October 1, 2009 and September 30, 2024, with 80 enforcement actions relating to approximately \$1.5 billion in monetary settlements in FY 2024 (NYU and Cornerstone 2024). In contrast, the IRS closed 1,659 examinations for corporate income tax returns with at least \$10 million in reported assets during FY 2024 *alone* and recommended over \$14.5 billion in additional tax related to those examinations (IRS 2025).

expense for most firms (Shackelford and Shevlin 2001; Hanlon and Heitzman 2010), they have strong incentives to support contested tax positions.

Because IRS audit data are not publicly available, we follow Yost and Shu (2022) and construct a firm-year-level indicator variable for IRS audits using time-varying IRS audit probabilities and IRS downloads of 10-K filings.² We construct the firm-year-level measures of tax and general accountants using comprehensive employee data from Revelio Labs. Our primary sample comprises 17,636 firm-year observations from 2008 to 2016.

We estimate two models to examine our research question. First, using a regression with industry fixed effects, we estimate the effect of an IRS audit on *changes* in the number of accountants.³ To mitigate correlated omitted variable concerns, we explicitly control for observable firm characteristics that are associated with the non-random component of IRS audit selection and that could also be associated with the number of in-house accountants, including size, profitability, foreign operations, and operational complexity (e.g., Ayers et al. 2019; Nessa et al. 2022). We document a statistically significant increase in both tax and general accountants around the IRS audit period. On average, firms increase the number of tax (general) accountants by approximately 7.6% (5.1%), representing an increase of 0.2 (2.8) individuals (a 5.3% overall increase in accountants). Thus, firms not only hire tax personnel but also general accountants to meet the internal resource demands arising from IRS scrutiny.

We next estimate a firm fixed effects model, where the dependent variable is the *level* of

² Yost and Shu (2022) perform extensive validation tests of this IRS audit measure, including documenting a significantly positive association with future tax settlements. While it is likely subject to some measurement error (e.g., the existence of IRS 10-K downloads when a firm is not actually under audit), it is unclear how such measurement error would be correlated with firms' accounting labor investment, which should ultimately bias against findings. Throughout, we refer to firms undergoing an IRS audit for ease of exposition, but we acknowledge that the Yost and Shu (2022) proxy is an *estimate* of true IRS audit activity.

³ In the industry fixed effects specification, we model *changes* in the number of accountants to reduce the influence of persistent cross-sectional differences in firms' baseline accounting labor levels (Wooldridge 2010).

accountants. Exploiting *within-firm* variation in IRS audit status and accounting personnel, this specification holds time-invariant firm characteristics constant, thereby further alleviating concerns about correlated omitted variables, such as inherent differences between large versus small firms. We again document a statistically significant increase in the number of tax and general accountants. Given the importance of controlling for time-invariant firm characteristics, we rely on this specification throughout, except when it is infeasible to include firm fixed effects.

Next, we conduct a series of cross-sectional analyses. First, we examine whether firms substitute between internal accountants and auditor-provided tax services (APTS). Consistent with a trade-off, we find that the hiring response for *tax* accountants is weakened as reliance on APTS increases, with limited evidence of a trade-off for general accountants. However, the trade-off between internal tax accountants and APTS depends on context. Firms with greater historical reliance on APTS, as well as loss firms, increase their investments in APTS in response to an IRS audit. However, those with less historical reliance on APTS, and those that are profitable, hire internal tax accountants. These patterns provide rich context for understanding the trade-off between internal and external tax expertise in response to tax enforcement pressures.

Second, we consider incentives to sustain claimed tax positions. We find that the hiring response for tax accountants becomes stronger (weaker) as tax avoidance increases (decreases). These results are also limited for general accountants, again reinforcing the unique role of tax expertise around IRS audits. Given the limited evidence for general accountants, we further examine whether their hiring varies with informational complexity and document a positive association. Collectively, these tests indicate that IRS audits not only increase the demand for tax expertise but also impose broad information demands that lead to the hiring of general accountants.

Next, we examine the seniority of new accounting hires. Firms might (1) hire experienced

employees with tacit knowledge to directly manage the IRS audit, or (2) hire less experienced employees to take on routine tasks, thereby freeing up existing personnel to assist with the IRS audit. The former (latter) predicts increased hiring of more senior (junior) personnel. Consistent with the differing roles of tax and general accountants in our setting, we find that new tax hires around IRS audits are more senior, whereas general accounting hires are (marginally) more junior. This pattern suggests that new tax hires are more likely to have the tacit knowledge from prior experience to directly sustain contested tax positions (Cohen and Levinthal 1990; Chatterji et al. 2016; Barrios and Gallemore 2024). In contrast, new general accounting personnel are more likely to take on basic accounting tasks, thereby freeing up existing accountants with firm-specific knowledge for the IRS audit (Becker 1962; Zander and Kogut 1995; Szulanski 1996).

We then turn to an examination of how internal accountant hiring mediates certain effects of tax enforcement previously documented in the literature (e.g., Guedhami and Pittman 2008; El Ghoul et al. 2011; Hoopes et al. 2012). To do so, we conduct path analyses using structural equation modeling (SEM). First, we find that the hiring of accountants partially offsets the increase in cash effective tax rates (ETRs) associated with tax enforcement. While IRS audits increase cash ETRs (Hoopes et al. 2012), the hiring of additional accountants mitigates this effect by approximately 14%. Thus, investment in accounting labor is partially self-funding through offsets to cash tax outflows to the tax authority. Second, regarding the information environment benefits of tax authority monitoring, we find that the increase in accountants explains approximately 5% of the effect of IRS monitoring on the cost of capital (e.g., Guedhami and Pittman 2008; El Ghoul et al. 2011). Thus, by meeting the information demands of the tax authority, new accountants improve the information environment, thereby reducing information asymmetry with capital providers.⁴

⁴ Guedhami and Pittman (2008) and El Ghoul et al. (2011) find that IRS monitoring is associated with a reduction in the cost of debt and equity, respectively. However, recent work by Hopkins et al. (2025) shows that, using more

Finally, to strengthen our inferences, we conduct a series of additional analyses. First, we examine the dynamics of the effect. IRS audits are conducted based on prior-year tax filings. We find that the increase in accountants occurs immediately around the audit, with no evidence of pre-period trends. This test mitigates several endogeneity concerns, including that firms consistently increasing accounting personnel – due to increased complexity or tax planning – are likely to be selected for an IRS audit. In such cases, hiring would likely manifest before the audit. Because the IRS audits prior-year returns, it is also unlikely that the audit decision is based on concurrent hiring (e.g., reverse causality).

Second, because our primary measure of IRS audits based on Yost and Shu (2022) is only available through 2016, and to ensure that our results are not driven by a unique feature of this measure, we verify the robustness of our inferences to using an alternative text-based measure of IRS scrutiny based on regulatory action words developed by Armstrong et al. (2025), available over a longer sample window. Third, our inferences hold using entropy balancing (EB) (Hainmueller 2012; Hainmueller and Xu 2013), which mitigates concerns about model misspecification. Lastly, in falsification tests, we find no evidence of a comparable change in hiring around IRS audits for *non-accounting* employees, indicating that the hiring response we document is accountant-specific around the audit and not reflective of broader hiring trends. Together, these tests, along with the cross-sectional analyses, substantially raise the bar for an alternative explanation. However, we acknowledge that, as with most observational studies, we cannot fully

granular data on IRS audit probability, there is a non-linear relation between tax enforcement and the cost of equity: smaller firms experience a reduction, whereas larger firms experience an *increase* (resulting in an on-average positive effect) because managers' tax avoidance opportunities are curtailed. Using a measure of *total* cost of capital from Gormsen and Huber (2025), we document an on-average negative association between IRS audits and the total cost of capital, indicating that the net information environment benefits of IRS audits, which reduce agency frictions with debtholders (e.g., Anderson et al. 2004), likely dominate the foregone benefits of tax avoidance for shareholders. Our finding that increased accounting personnel mediate this reduction in the total cost of capital is also consistent with Hopkins et al. (2025) since additional accountants reduce cash tax outflows to the tax authority.

rule out alternative explanations. Readers should interpret the results accordingly.

Our study makes several contributions. First, we use novel, comprehensive employment data to answer calls for research at the intersection of taxation and labor investments (Jacob 2022; Lester and Olbert 2025). For example, Lester and Olbert (2025) note that “despite the first-order economic importance of employment, empirical research on the link between business taxes and firms’ labor capital decisions is limited” (pg. 15). Furthermore, we answer the call for evidence on the effects of taxation on employment outcomes for *rank-and-file* workers rather than focusing on executives (Lester and Olbert 2025). In doing so, we help to reconcile work documenting tax enforcement’s positive information environment and governance effects with recent work highlighting its real costs, including suppressed business activity (Belnap et al. 2024; Armstrong and Glaeser 2025; Gallemore and Jacob 2025).

Second, we document a trade-off between internal and external tax expertise, and how this trade-off varies with historical investment in APTS and financial position. This evidence provides context for understanding the “build versus buy” decision for tax expertise and suggests that audit firms may benefit from tax enforcement by increasing the sales of tax services to some clients. Third, we provide insight into *how* firms utilize new accountants to meet IRS audit demands by analyzing the seniority of these hires. Lastly, we document hiring of accountants as a mechanism through which tax enforcement shapes firm behavior, partially explaining effects on tax avoidance and the information environment previously documented in the literature (e.g., Guedhami and Pittman 2008; El Ghouli et al. 2011; Hoopes et al. 2012). While prior studies have examined changes in employment-related outcomes in settings of regulatory change and uncertainty (e.g., Barrios 2022; Giese et al. 2025; Huang et al. 2025), existing work does not examine how tax enforcement affects the hiring of accountants.

2. Related Literature and Hypothesis Development

2.1. Related Literature

Prior work finds that tax enforcement reduces managerial diversion and self-dealing, improves financial reporting quality, and reduces suppression of bad news (Desai and Dharmapala 2006, 2009; Desai et al. 2007; Hanlon et al. 2014; Gallemore and Jacob 2020; Bauer et al. 2021; Yost and Shu 2022). Studies have also examined tax avoidance responses to regulatory enforcement. Hoopes et al. (2012) document a negative relation between IRS audit probability and tax avoidance, and Kubick et al. (2016) show that firms decrease tax avoidance in response to SEC scrutiny in the form of tax-related comment letters. Thus, the evidence suggests that regulatory monitors, including the tax authority, enhance firms' information environments and reduce tax avoidance.

However, the channels behind these associations are not well understood. Many studies emphasize the external monitoring role of the tax authority, whereby tax audits can uncover private information and constrain managerial opportunism (Bauer et al. 2021; Yost and Shu 2022). Tax audits can also mitigate agency costs, which in turn reduces the cost of capital (Desai and Dharmapala 2006, 2009; Desai et al. 2007; Guedhami and Pittman 2008; El Ghoul et al. 2011). Relatedly, tax enforcement may compel managers to reduce aggressive tax strategies and simplify financial reporting, thereby enhancing corporate transparency and lowering information asymmetry (Chen et al. 2018; Balakrishnan et al. 2019).

Notably, several studies also suggest an internal channel related to firms' information gathering to support tax positions under audit (e.g., Hanlon et al. 2014; Belnap et al. 2024; Ferguson et al. 2025). However, the way in which firms obtain and process this information is unclear. Firms may divert resources away from other value-added activities to meet the demands

of the tax audit (Belnap et al. 2024). Similarly, firms may rely on support from external advisors, as many Big 4 and other accounting and law firms offer tax audit and dispute resolution services.⁵ Firms could also increase their investment in accounting labor, thereby expanding their internal capacity to create, gather, and analyze information. However, until recently, detailed employee data have been largely unavailable, limiting researchers' ability to explore this mechanism.⁶

With the recent increase in the availability of employment and job posting data, several studies have examined investments in accounting-related human capital. For example, Jiang et al. (2020) document a reduction in tax rate volatility but no increase in tax avoidance for firms that hire former IRS employees. Chen et al. (2021b) find that larger tax departments are associated with greater tax avoidance and lower ETR volatility. Giese et al. (2024) show that firms tend to locate more tax employees in areas with higher tax complexity, particularly in countries with greater procedural complexity. Barrios and Gallemore (2024) document the diffusion of tax planning knowledge across firms. Giese et al. (2025) show that firm demand for tax personnel increased in response to the 2017 Tax Cuts and Jobs Act. Other studies examine accounting-related human capital more generally. For example, Huang et al. (2025) find that the issuance of the lease accounting and revenue recognition standards led to an increase in postings for accounting jobs requiring skills in these areas. Iselin et al. (2025) document a positive association between accounting human capital and investment efficiency, particularly among managerial accountants. However, no study has examined the labor market response to tax audits.

2.2. Hypothesis Development

Economic rationale dictates that firms should hire new accounting employees in response

⁵ For example, see <https://kpmg.com/us/en/capabilities-services/tax-services/tax-policy-legislation-and-controversy/tax-controversy-and-dispute-resolution.html>.

⁶ Lester and Olbert (2025) note a “scarcity of high-quality panel data on employment” (pg. 15).

to IRS enforcement if the expected marginal benefit of doing so exceeds the marginal cost (i.e., an expected net benefit). This cost-benefit analysis is likely to be firm-specific.

Hiring additional accounting personnel in response to IRS enforcement can alleviate the associated resource burden and mitigate risks. Firms may access the labor market to hire accountants with the tacit knowledge necessary to navigate the tax enforcement process and support contested positions under audit (Arrow 1962; Argote and Ingram 2000; Rosknkopf and Almeida 2003; Song et al. 2003). Additionally, IRS audits are often time-consuming and impose significant demands on a firm's resources (Hanlon et al. 2014). For example, there are many areas where book and tax reporting align, or where book amounts serve as a starting point for tax calculations. Resultingly, as part of its audit process, the IRS may request supporting documentation for internal controls or for financial reporting amounts that relate to tax amounts. These demands often fall not only on firms' internal tax personnel, but also on general accounting employees who produce supporting records for the transactions underlying contested positions. While the firm may have the necessary in-house accounting knowledge regarding these transactions, it may be concerned about diverting attention from these accountants. Thus, to the extent that certain general accounting tasks are explicit in nature and easily shareable through written procedures (Becker 1962; Cohen and Levinthal 1990; Chatterji et al. 2016; Barrios and Gallemore 2024), firms may also hire new accountants to assume these tasks and free up the capacity of those who are better equipped to handle the information demands of the tax audit.⁷

However, hiring in response to IRS enforcement is costly (Nickell 1986; Banker and Byzalov 2014), and such investments in personnel may be unnecessary. Instead, firms may choose

⁷ For example, firms undergoing an IRS audit must still perform routine accounting functions, including the recording, reconciliation, and closing of financial statement accounts. Because many of these tasks are often standardized and supported by formal documentation, firms can more readily shift them to new employees.

to engage an accounting or law firm for tax controversy services (e.g., Deloitte 2025b; EY 2025; KPMG 2025; PwC 2025). Survey evidence also suggests significant labor market frictions for top accounting talent (Thompson Reuters 2024; Deloitte 2025a). For example, a 2024 Thomson Reuters survey indicated that hiring and retaining top accounting talent is a significant challenge (Thompson Reuters 2024). Investing in external tax advice may be less costly than a long-term investment in internal personnel. Alternatively, firms could add to the workload of current personnel or reallocate existing employees to handle the demands of IRS audits. Firms could also invest in technology, although survey evidence also suggests significant frictions in technology adoption (Thompson Reuters 2024). Furthermore, investments in technology often require complementary investments in personnel for implementation.

We have no theoretical basis to motivate a *reduction* in accounting employees in response to IRS enforcement. Therefore, we state a directional hypothesis as follows.

Hypothesis: *IRS enforcement is positively associated with firms' investments in accounting labor.*

3. Data, Sample, and Research Design

3.1. Identifying Accounting Employees and IRS Audits

We obtain employment data from Revelio Labs, a leading employment analytics company that collects data from professional online profiles (e.g., LinkedIn) and other sources.⁸ These data enable us to identify the start and end dates of employees' positions at firms, along with their roles. Revelio's employment coverage becomes reliably populated beginning in 2008. We use Revelio's job role taxonomy to classify general and tax accountants.⁹ We classify individuals as an "Accountant" if Revelio categorizes their job role as "Accountant." We identify "Tax Accountants"

⁸ Revelio states that they are "the most comprehensive" and "the largest and most dynamic" workforce database (Wharton Research Data Services: <https://wrds-www.wharton.upenn.edu/pages/about/data-vendors/revelio-labs/>).

⁹ Revelio's job role taxonomy uses algorithms that incorporate the position title and text description, along with the individual's skills, associates, and previous experiences.

as the subset of positions in the “Accountant” category that Revelio categorizes as “Tax Specialist.” “General Accountants” are all accountants that are not categorized as tax accountants. We require a position to exist as of December 31 for an individual to be classified as an employee during the year. Although the accuracy and completeness of Revelio data are subject to limitations, the data are increasingly used and validated in academic studies (e.g., Li et al. 2022; Cai et al. 2024; Khavis and Szerwo 2025), and we have no reason to believe that any measurement error or coverage biases would be correlated with our measure of IRS audits.

In the U.S. setting, IRS enforcement has traditionally been measured using either data from Transactional Records Clearinghouse (TRAC), which captures IRS audit probability by asset-size and year (e.g., El Ghouli et al. 2011; Hoopes et al. 2012; Mason and Williams 2022; Bauer et al. 2021), or SEC EDGAR data of IRS downloads of firms’ 10-Ks (e.g., Bozanic et al. 2017; Fox and Wilson 2023). A recent IRS audit proxy developed by Yost and Shu (2022) combines these two measures to create a binary, firm-year-level indicator variable. We use this composite measure as our primary proxy for IRS audit activity.

To construct the Yost and Shu (2022) measure, we first use TRAC data to capture the probability of an IRS audit for a given asset-size group in each year. Second, we rank firms by descending IRS downloads within each asset-size group by year. Third, we select the top X% of firms by IRS 10-K downloads within a given asset-size-year group and code those as under IRS audit (i.e., *IRS Audit* = 1), where X is the IRS audit probability from TRAC. Any firm falling below the top X% in IRS 10-K downloads for an asset-size-year grouping is coded as not subject to an IRS audit (*IRS Audit* = 0). Thus, this composite measure provides a firm-specific, time-varying indicator variable for whether a firm is subject to an IRS audit, allowing for clear inferences regarding economic significance. Yost and Shu (2022) conduct various validation tests of this

measure, including a multivariate regression analysis showing a positive and significant association with future tax settlements. Since the IRS 10-K download data used to construct the Yost and Shu (2022) measure are only available through 2016, in additional analyses, we test the robustness of our inferences to using a recent text-based measure of IRS exposure (as captured through action words) created by Armstrong et al. (2025), which is available through 2019.

3.2. Research Design

We first estimate the following ordinary least squares (OLS) regression.

$$\Delta Accountant_{i,(t-1,t+1)} = \beta_0 + \beta_1 IRS\ Audit_{i,t} + \sum \beta_k Controls_{i,t} + IndustryFE + YearFE + \varepsilon_{i,(t-1,t+1)} \quad (1)$$

IRS Audit is an indicator variable equal to one if firm *i* is subject to an IRS audit in year *t*, based on the Yost and Shu (2022) measure, and zero otherwise. $\Delta Accountant$ is either the *change* in firm *i*'s tax accountants ($\Delta Tax Accountant$) or general accountants ($\Delta General Accountant$) from year *t-1* to *t+1*, which ensures that we capture hiring in both the year of and the year following the IRS audit. We include year *t+1* in this measurement because it is unlikely that most IRS audits would be fully completed in year *t*, particularly for those that begin later in the year (Hanlon et al. 2014). IRS downloads of a firm's 10-K likely indicate the *beginning* or early stages of IRS audit selection and fieldwork, consistent with the measure validation test in Yost and Shu (2022), which shows that these downloads peak in the year of the audit. However, they do not document increased settlements with the IRS until the following year (*t+1*).¹⁰ β_1 captures the effect of an IRS audit in year *t* on the change in the number of tax (general) accountants from year *t-1* to *t+1*. If IRS audits are associated with an increase in the number of internal accounting employees, then $\beta_1 > 0$.

Similar to Chen et al. (2021b) and Giese et al. (2025), we control for the natural logarithm

¹⁰ We examine the dynamics of this hiring response in subsequent analyses.

of total assets (*Size*), an indicator variable equal to one if the firm has foreign operations and zero otherwise (*MNC*), the market-to-book ratio (*MTB*), profitability (*ROA*), leverage (*Leverage*), intangible assets (*Intangible Intensity*), net property, plant, and equipment (*PPE*), auditor-provided tax services (*APTS*), and the number of unrelated business segments (*Segments Unrelated*).¹¹ To control for industry and macroeconomic events affecting all firms, we also include two-digit SIC industry and year fixed effects.

We begin with Equation (1) to estimate the effect *across* firms. This provides the benefit of utilizing variation from our entire sample and mitigates concerns that measurement error and high-order fixed effects create false positives (Breuer and deHaan 2024; Jennings et al. 2024). While this model allows for useful economic inferences, it is important to verify that our results are not driven by cross-sectional differences between firms that are subject to an IRS audit versus those that are not. Therefore, to strengthen inferences and control for time-invariant firm characteristics, we also estimate the following firm fixed effects model:

$$\text{Ln}(\text{Avg Accountant})_{i,(t,t+1)} = \beta_0 + \beta_1 \text{IRS Audit}_{i,t} + \sum \beta_k \text{Controls}_{i,t} + \text{FirmFE} + \text{YearFE} + \varepsilon_{i,(t,t+1)} \quad (2)$$

Since this specification holds the firm constant, we use the average *level* of accountants in years t and $t+1$ as the dependent variable, allowing us to capture within-firm variation in the number of accountants around an IRS audit.¹² $\text{Ln}(\text{Avg Accountant})$ is either the natural logarithm of the average number of tax accountants ($\text{Ln}(\text{Avg Tax Accountant})$) or general accountants ($\text{Ln}(\text{Avg General Accountant})$) in the year of and immediately following an IRS audit. Identification in this

¹¹ In an untabulated analysis, we confirm that the results are similar if we control for the natural log of total employees in year $t-1$ (either in addition to or instead of *Size*). However, since this measure is collinear with *Size*, we do not include it in our models. These size-related controls, in addition to our subsequent firm fixed effects model, mitigate concerns that we are simply documenting variation in hiring between large (subject to an IRS audit) and small (not subject to an IRS audit) firms.

¹² If we were to use a changes dependent variable as in Equation (1) for the firm fixed effects specification in Equation (2), we would effectively capture a change in a change, which is difficult to interpret.

model comes from within-firm variation in IRS audit status.

Importantly, we follow the suggestion of Jennings et al. (2024) and find that firm fixed effects only explain 26.5% of the variation in *IRS Audit* (untabulated), far below the cautionary threshold of 90%, where measurement error correlated with firm fixed effects can create false positives. Additionally, approximately 52% of firms in our sample experience a change in audit status during the sample period, which represents a meaningful subsample in which to examine within-firm variation (Breuer and deHaan 2024). We include the same controls as in Equation (1). Notably, while our industry fixed effects specification is helpful to examine variation across firms, the firm fixed effects specification in Equation (2) is more stringent and does not appear to suffer from concerns outlined in prior literature. Therefore, we rely on this specification to draw statistical inferences, except when firm fixed effects are infeasible. Continuous variables are winsorized at the 1st and 99th percentiles. We cluster standard errors by firm in both equations. All variables are defined in Appendix A.

We carefully control for many firm characteristics in our models to mitigate potential concerns about correlated omitted variables. However, we are cautious not to overcontrol to avoid issues related to “collider” variables (e.g., Gow et al. 2016; Whited et al. 2022). In our setting, collider variables are those that could be affected by both IRS audits and the number of accountants within an organization (e.g., R&D, tax loss carryforwards, effective tax rates, inventory, capital expenditures). Nevertheless, in robustness tests, we re-estimate our models after adding additional control variables. We also estimate both our baseline and expanded models using EB (Hainmueller 2012; Hainmueller and Xu 2013) to mitigate concerns about model misspecification. When implementing EB, we assign weights to control group observations (i.e., those not subject to an IRS audit) by matching them on the first three distributional moments of all control variables to

treated observations (i.e., those subject to an IRS audit).

3.3. Sample

Table 1 presents the sample selection. We begin with the universe of U.S.-incorporated, publicly traded firms in Compustat. We start the sample in 2008, which is the first full year that Revelio Labs employment data are reliably populated. The sample period ends in 2016 because that is the final year for which SEC EDGAR 10-K download data are available. Because we examine the effect of IRS audits on employment through year $t+1$, we utilize Revelio employment data through 2017. We require Revelio data to construct the dependent variables for tax and general accounting employees. We also require data to construct the Yost and Shu (2022) IRS audit measure, our independent variable of interest. Furthermore, we eliminate firms in the utilities, financial, and real estate industries, as well as those that provide tax preparation services. After further eliminating observations lacking the required data to calculate control variables, our final sample comprises 17,636 firm-year observations for 2,808 distinct firms.

4. Empirical Results

4.1. Descriptive Statistics

Table 2 presents descriptive statistics. The mean number of tax and general accountants (*Tax Accountant* and *General Accountant*) is approximately five and 91, respectively, suggesting that tax accountants comprise approximately 5% of in-house accounting departments ($5/(5+91)$). This average of five tax accountants is similar to the approximately seven reported for S&P 1500 firms in Chen et al. (2021b). The mean changes in tax and general accountants ($\Delta Tax Accountant$ and $\Delta General Accountant$) are 0.477 and 6.104, respectively, suggesting that fewer tax accountants are hired than general accountants. For firm-years subject to an IRS audit (i.e., $IRS Audit = 1$), the mean $\Delta Tax Accountant$ and $\Delta General Accountant$ is 1.032 and 13.217, respectively. The average

$\Delta Tax Accountant$ and $\Delta General Accountant$ for firm-years *not* subject to an IRS audit (i.e., $IRS Audit = 0$) is 0.320 and 4.096. This preliminary univariate evidence suggests an increase in hiring of accountants for firms subject to an IRS audit. However, such firms differ significantly from those not subject to an IRS audit on many observable dimensions, highlighting the importance of controlling for these characteristics in our research design.

Approximately 22% of observations are subject to an IRS audit, according to the Yost and Shu (2022) proxy (mean $IRS Audit = 0.220$). For robustness, we also use an alternative measure to capture IRS audit activity ($IRS Exposure$) from Armstrong et al. (2025), which is available through 2019. $IRS Exposure$ is the percentage of sentences in a firm's 10-K that include terms related to IRS action and enforcement. While this measure is not an indicator variable for being subject to an IRS audit, it captures the relative degree of concern about IRS scrutiny. The average value of $IRS Exposure$ is 0.001, corresponding to firms discussing the IRS in 0.1% of sentences in their 10-K filings. To create a binary indicator variable that is more similar to $IRS Audit$, we also calculate year-over-year changes in IRS exposure and quintile-rank those changes within the sample. We set $\Delta IRS Exposure High$ equal to one when a firm is in the top quintile of changes in $IRS Exposure$, and zero otherwise. By construction, the mean $\Delta IRS Exposure High$ is 0.200.

4.2. *IRS Audits and Accounting Labor Investment*

Table 3, Panel A, presents the results from estimating Equation (1). In columns 1 and 2 (3 and 4), the dependent variable is $\Delta Tax Accountant$ ($\Delta General Accountant$). The coefficient on $IRS Audit$ is positive and significant ($p < 0.01$) across all columns, consistent with an increase in the number of tax and general accountants when firms are subject to an IRS audit. Relative to the average value of $Tax Accountant$ for non-IRS audit observations (i.e., $IRS Audit = 0$), the coefficient in column 2 corresponds to an increase of approximately 7.6% in the firm's tax

accountant workforce (0.216/2.843). In column 4, the coefficient estimate on *IRS Audit* indicates an increase of 2.787 general accountants. Relative to the mean of *General Accountant* for non-IRS audit observations, this represents an increase of approximately 5.1% (2.787/54.141). These results indicate an increase of approximately three total accountants around an IRS audit, corresponding to an overall accounting workforce increase of 5.3%.¹³

Table 3, Panel B, presents the results from estimating Equation (2). This specification includes firm and year fixed effects, thereby estimating within-firm changes in the level of accountants when a firm is subject to an IRS audit. In columns 1 and 2 (3 and 4), the dependent variable is $\ln(\text{Avg Tax Accountant})$ ($\ln(\text{Avg General Accountant})$). Similar to Panel A, the coefficient on *IRS Audit* is positive and significant ($p < 0.05$, or stronger) across all columns, suggesting an increase in both tax and general accountants. Taking the coefficient estimates from columns 2 and 4, together with the mean tax and general accountants for observations subject to an IRS audit, we document a within-firm increase of 0.126 (0.011 x 11.414) and 1.937 (0.012 x 161.408) for tax and general accountants, respectively.¹⁴ This specification helps mitigate concerns that time-invariant firm characteristics influence the results in Panel A.

4.3. Cross-Sectional Analyses

We next conduct cross-sectional analyses to gain insight into the mechanism(s) underlying our results and rule out alternative explanations. First, we expect our documented effect to weaken (strengthen) as APTS increases (decreases). Firms with more APTS demonstrate a revealed preference for investment in external tax services and are more likely to rely on their external

¹³ This estimate should represent a *lower bound* for the effect because of (1) underreporting by employees of employment-related data (e.g., those without LinkedIn profiles), causing such individuals to not appear in Revelio, (2) not capturing temporary (temp) employees if their employer is listed as the temp agency (e.g., Robert Half) rather than the firm, and (3) other categories of employees that may have been hired to support the IRS audit process, such as lawyers, who would not be identified as accountants.

¹⁴ For these calculations, we use the mean number of tax and general accountants when *IRS Audit* = 1 for firms that exhibit over-time variation in both the number of accountants and in IRS audit status (untabulated).

auditor for assistance when faced with an IRS audit.¹⁵ Second, we expect the effect to strengthen (weaken) as firms engage in more (less) tax avoidance. Firms with greater (less) tax avoidance are likely to have more (fewer) positions scrutinized during an IRS audit, creating stronger (weaker) incentives for investments in internal capacity to support such positions. To test these predictions, we modify Equations (1) and (2) and interact *IRS Audit* with *APTS* and *HS ETR*, respectively. We use *HS ETR*, the Henry and Sansing (2018) measure of the difference between actual and expected cash taxes paid, as the proxy for tax avoidance because it is the least restrictive to our sample (i.e., it does not require us to exclude loss firms).

Table 4, Panel A, presents the results from estimating a modified Equation (1). In columns 1 and 3 (2 and 4), we interact *IRS Audit* with *APTS* (*HS ETR*). Consistent with our expectation, the coefficient on *IRS Audit* \times *APTS* is significantly negative in columns 1 and 3. Thus, the positive association between *IRS Audit* and the hiring of accountants weakens as investment in external tax advice from the auditor increases. In columns 2 and 4, the coefficient on *IRS Audit* \times *HS ETR* is also negative and significant. Because *HS ETR* is decreasing in tax avoidance, this result indicates that the positive association between *IRS Audit* and the hiring of accountants increases with the level of tax avoidance. In other words, firms hire more accountants in response to an IRS audit as they engage in greater tax avoidance. In such cases, more tax positions are likely to be scrutinized during an IRS audit, leading to greater incentives to support and preserve the cash tax savings associated with those positions.

In Table 4, Panel B, we present the results of these analyses from estimating a modified version of Equation (2). Columns 1 and 3 (2 and 4) interact *IRS Audit* with *APTS* (*HS ETR*). Using

¹⁵ We acknowledge that APTS do not capture tax services performed by providers other than the firm's auditor. Despite this limitation, we still view APTS as a useful proxy for investment in external tax services (e.g., Gleason and Mills 2011; McGuire et al. 2012; De Simone et al. 2015; Francis et al. 2019).

this firm fixed effects specification, we continue to find that the coefficients on the interactions of *IRS Audit* with *APTS* and *HS ETR* are negative when the dependent variable is *Ln(Avg Tax Accountant)* (columns 1 and 2), with the effect in column 2 falling to one-tailed significance. In contrast, the coefficients on the interaction terms are small and insignificant when *Ln(Avg General Accountant)* is the dependent variable (columns 3 and 4). These results underscore the distinct nature of tax versus general accountant hiring.

To provide more insight into the hiring of general accountants, we next examine informational complexity. Specifically, we conduct a principal component analysis (PCA) of *Size*, *Segments Unrelated*, *PPE*, and *Capital Exp*, each of which is plausibly associated with more complex information needs during an IRS audit.¹⁶ Interpreting this factor as capturing IRS audit-related information needs (*Information Needs*), we expect that more general accountants are hired around IRS audits as these information needs increase. In Table 4, Panel C, we examine the interaction between *IRS Audit* and *Information Needs*, focusing specifically on general accountants. In column 1, we report the result of estimating a modified Equation (1) with industry fixed effects, and in column 2, we report the result of estimating a modified Equation (2) with firm fixed effects.¹⁷ Consistent with our expectation, we find that the coefficient on the interaction term is positive and significant in both columns, suggesting that firms hire more general accountants in response to IRS audits as the complexity of the information environment increases.¹⁸ In general, these tests add context to our main results and eliminate many alternative explanations, as any

¹⁶ The first principal component is positively associated with all four variables (loadings: 0.38, 0.22, 0.65, and 0.62, respectively) and has an eigenvalue of 1.78, supporting its retention (Allee et al. 2022).

¹⁷ We do not control for *Size*, *Segments Unrelated*, and *PPE* in this analysis because we use these variables to construct the *Information Needs* variable. However, the results are consistent when these controls are included (untabulated).

¹⁸ We focus this analysis on general accountants to identify the mechanism driving the hiring of these personnel. In an untabulated analysis, we find similar results for tax accountants in both specifications. However, the effect for tax accountants is statistically weaker (one-tailed significance) in the firm fixed effect specification. To the extent that tax and general accountants are complements, this result is unsurprising.

plausible explanation would need to be consistent with these documented cross-sectional patterns.

4.4. Further Examining the Relation Between Tax Accountants and Auditor-Provided Tax Services

Our cross-sectional tests document a trade-off between hiring tax accountants and investing in APTS. When faced with an IRS audit, firms must decide whether to rely on internal tax expertise, external tax services, or a combination of both. To better understand the factors influencing this choice, we examine how it is shaped by institutional relationships and financial condition.

First, we examine the effect of IRS audits on investment in APTS by modeling it jointly with hiring of tax accountants using seemingly unrelated regressions (which allows for correlation among error terms), as internal tax hiring and investment in external tax services are likely to be jointly determined (e.g., Costello 2013; Chen et al. 2021a).¹⁹ Additionally, we examine the interactive effect of each measure with *IRS Audit* on the other outcome variable. We use the firm fixed effect specification for these tests.²⁰ Table 5 reports these results. Panel A shows that IRS audits are positively associated with both investment in APTS (column 1) and hiring of tax accountants (column 2) when estimated jointly. The *IRS Audit* coefficient in column 1 corresponds to an increase in APTS of approximately \$78,000.²¹ The coefficient in column 2 is very similar to that reported in Table 3, Panel B.

Table 5, Panel B, examines the effects of IRS audits on each outcome interacted with the other using joint estimation. In column 1, we estimate a significantly negative coefficient on *IRS Audit* \times *Ln(Avg Tax Accountant)* when *APTS* is the dependent variable. Consistent with the cross-

¹⁹ Because we estimate seemingly unrelated regressions in this section, the estimates for hiring of internal tax accountants differ slightly from prior analyses, although inferences are identical.

²⁰ To perform this estimation with Stata's SUREG command, we demean each variable by the firm average, effectively creating a firm fixed effect. Because we remove the fixed effect, the adjusted R-squared is lower in these analyses, effectively only capturing the explained within-fixed effect variation.

²¹ We use the average asset value in this sample of \$3.9 billion (untabulated) to estimate this effect. Note that APTS is multiplied by 100 for presentation purposes, so we divide by 100 for this calculation.

sectional results in Table 4, we estimate a significantly negative coefficient on $IRS\ Audit \times APTS$ in column 2 when $Ln(Avg\ Tax\ Accountant)$ is the dependent variable. These results suggest a trade-off between internal tax hiring and investment in APTS in the IRS audit setting.

Next, we examine firm-level characteristics that may influence this trade-off. First, we expect that firms that have historically engaged their auditor for tax services to a greater degree are more likely to rely on their auditor's tax expertise when facing an IRS audit. In Table 5, Panel C, we split the sample by median prior-year (i.e., lagged) APTS and re-estimate our analysis from Panel A. Consistent with our expectation, firms with below-median prior-year APTS hire additional internal tax accountants in response to an IRS audit (column 2), but they do not increase APTS (column 1). In contrast, firms with above-median prior-year APTS increase APTS around an IRS audit (column 3), but they do not increase hiring of tax accountants (column 4).²²

Next, we examine how this trade-off varies with financial condition. We propose that profit firms may be more willing to hire internal tax personnel in response to an IRS audit, whereas loss firms may rely more on external tax services due to hesitancy to commit to a long-term investment in personnel. We partition the sample into profit and loss observations, using two-year average EBIT as the measure of profitability. Table 5, Panel D, presents the results. Consistent with our expectation, profitable firms increase their hiring of tax accountants around IRS audits, whereas loss firms increase investment in APTS.

These results suggest that firms respond to IRS audits by trading off internal and external tax expertise. Firms that have historically relied more on their auditor for tax services, as well as firms in a weaker financial position, rely on more external tax services in response to IRS audits. However, firms with historically lower investment in APTS, as well as profit firms, respond by

²² The coefficient estimate in Panel C, column 3, corresponds to an approximately \$90,000 increase in APTS, using the average asset value in that subsample (untabulated).

hiring internal tax experts. These analyses provide rich evidence on the characteristics that determine whether firms internalize or outsource tax expertise in response to IRS audits. Furthermore, any plausible alternative explanation would need to be consistent with these documented trade-offs.

4.5. Seniority of New Accountants

We further investigate the mechanism underlying our results by examining the relative seniority of newly hired accountants, using Revelio's seniority variable.²³ The values range from 1 (indicating entry-level positions, such as an accounting intern) to 7 (senior executive, such as a CEO or CFO). We modify Equation (2) and use either *Avg Seniority – Tax* or *Avg Seniority – General* as the dependent variable. Because we cannot identify the *specific marginal* accountant(s) hired due to an IRS audit (as opposed to standard replacement from natural turnover), for each firm-year, we take the average seniority of *all* tax or general accountants hired in years t and $t+1$. While this analysis provides useful, suggestive inferences about the *direction* of seniority among new accounting hires, it could bias against finding results and makes the coefficient estimates challenging to interpret. Thus, we caution readers to interpret these findings with this limitation in mind. For the tax (general) accountant analysis, we restrict the sample to firm-years that *increase* their number of tax (general) accountants in year t or $t+1$, as firms that do not hire are uninformative for tests of seniority. This restriction reduces the sample for the tax (general) hire seniority tests to 5,397 (11,965) firm-years. Using the firm fixed effects model, we capture within-firm variation in the seniority of newly hired accountants around an IRS audit.

Table 6 presents these results. In columns 1 and 2 (3 and 4), we examine the average seniority of newly hired tax (general) accountants. Columns 1 and 2 indicate that the average

²³ Revelio measures seniority using information about an individual's current position (title, company, and industry), job history (including position duration and seniority), and age.

seniority of newly hired tax accountants increases around IRS audits. Alternatively, columns 3 and 4 suggest that the average seniority of general accountants decreases (just below significance at conventional two-tailed levels), providing limited evidence that the newly hired general accountants are, on average, more junior.

This analysis suggests that firms are more likely to hire *tax* accountants who have the experience to engage directly in the tax enforcement process by sustaining claimed tax positions. Although these new tax hires may not have extensive firm-specific knowledge of the transactions under audit, they can provide tacit knowledge from previous experiences to mitigate risks (Arrow 1962; Cohen and Levinthal 1990). Alternatively, general accountants, who are more likely to be hired to handle information demands, can take on more routine, basic tasks, thereby freeing up existing employees to directly assist with the IRS audit. As with the cross-sectional analyses, these tests provide context for our results and further narrow the set of plausible alternative explanations.

4.6. Hiring Accountants as a Mediating Effect

We next examine how hiring accountants may mediate certain effects of tax enforcement observed in previous studies. In particular, we examine how it influences the effect of IRS audits on tax avoidance (Hoopes et al. 2012) and the cost of capital (Guedhami and Pittman 2008; El Ghoul et al. 2011; Hopkins et al. 2025). To do so, we implement path analyses using generalized SEM. We include industry and year fixed effects, as in Equation (1), since SEM does not accommodate higher-order categorical predictors (Bauer et al. 2021). We estimate the following set of equations:

$$Y_{i,t+1} = \beta_0 + \beta_1 IRS\ Audit_{i,t} + \beta_2 \Delta Total\ Accountant_{i,(t-1,t+1)} + \sum \beta_k Controls_{i,t} + IndustryFE + YearFE + \varepsilon_{i,t+1} \quad (3)$$

$$\Delta Total\ Accountant_{i,(t-1,t+1)} = \beta_0 + \beta_1 IRS\ Audit_{i,t} + \sum \beta_k Controls_{i,t} + IndustryFE + YearFE + \varepsilon_{i,(t-1,t+1)} \quad (4)$$

$Y_{i,t+1}$ is either the three-year cash ETR (*Cash ETR*) or the cost of capital (*Cost of Capital*). We measure *Cash ETR* from year t to $t+2$ for consistency with prior research. We are limited in the cash ETR analysis to firm-year observations with positive cumulative three-year pre-tax income, as negative denominator values are uninterpretable in ETR measures. Cost of capital measures are often subject to many model assumptions. Thus, we utilize a recent measure of managers' perceived weighted-average cost of capital from Gormsen and Huber (2025), as gleaned from conference calls. Because we are interested in the tax and informational benefits of both tax and general accountants, and since firms are likely to hire along both dimensions, we aggregate them into a measure of the change in total accountants from year $t-1$ to $t+1$ ($\Delta Total Accountant$). We retain the same control variables as in Equations (1) and (2).

Table 7, Panel A (Panel B), reports the results for *Cash ETR* (*Cost of Capital*). In Panel A, *IRS Audit* is positively and significantly associated with *Cash ETR* along the *direct* path, consistent with prior research (Hoopes et al. 2012). The coefficient (β_1) suggests that an IRS audit is associated with an increase of approximately 0.63 percentage points in the three-year cash ETR. Next, we estimate a positive and significant effect of *IRS Audit* on $\Delta Total Accountant$ (γ_1), consistent with our primary results. We also find that $\Delta Total Accountant$ is *negatively* and significantly associated with *Cash ETR* (β_2). The total mediated path of about three additional accountants offsets the three-year cash ETR increase by 0.09 percentage points, which is 14.30% of the direct effect of 0.63 percentage points. While the effect may appear small in percentage point terms, taking the average three-year pre-tax income of \$1.58 billion (untabulated), the additional 3.02 accountants reduce total three-year cash taxes paid by approximately \$1.42 million (\$1.58 billion * 0.0009), or approximately \$471,000 per additional accountant.

Next, in Panel B, we find that *IRS Audit* is negatively and significantly associated with the

weighted-average cost of capital. For ease of interpretation, *Cost of Capital* is multiplied by 100, such that 1 represents 1%. The direct effect (β_1) indicates that an IRS audit is associated with a reduction in the weighted-average cost of capital of 0.0851 percentage points. Next, we again find that *IRS Audit* is positively associated with $\Delta Total Accountant$ (γ_1). Importantly, $\Delta Total Accountant$ is negatively and significantly associated with *Cost of Capital* (β_2). The total mediated path of hiring additional accountants represents a -0.0040 percentage point effect on weighted average cost of capital, or approximately 4.70% of the direct effect (-0.0040/-0.0851). This result is consistent with accounting labor facilitating part of the information improvements that reduce the cost of debt (Anderson et al. 2004; Desai et al. 2007; Guedhami and Pittman 2008) and reducing cash outflows to the tax authority, thereby lowering the cost of equity (Hopkins et al. 2025).

5. Robustness Checks

5.1. Dynamics

Using a modified Equation (2), we next examine the *dynamics* of the effect to further alleviate endogeneity concerns. Importantly, there is a time lag between the period covered by a tax return and the time when that return is audited by the IRS.²⁴ Still, IRS audits may *follow* a persistent increase in the hiring of accountants. For example, this scenario could occur if a firm continuously grows its accounting workforce to support tax planning activities and/or financial and operational complexity, representing the underlying drivers of an IRS audit. Therefore, if there exists a correlated omitted time-varying firm characteristic associated with increases in both hiring of accountants and IRS audit risk, then the increase in accountants should begin to manifest *prior* to the IRS audit.

²⁴ For example, a 2012 tax return would be filed with the IRS in 2013, and it may not be selected for audit until 2015. Thus, there is a lag between a firm's underlying economic transactions and activities (e.g., hiring) and when the tax return reflecting those activities is filed and subsequently selected for audit by the IRS.

Table 8 presents these results. We modify Equation (2) to use one-year measures of the number of accountants instead of two-year averages (years t and $t+1$) as in our main analysis. We vary the timing of the measurement of *IRS Audit*, ranging from year $t+2$ (column 1) to year $t-3$ (column 6). Thus, columns 1 and 2 capture hiring in the two years *preceding* the initial IRS audit year. Column 3 represents the year that the IRS audit begins, and columns 4 through 6 capture the effects for the *following* three years.²⁵

In Panel A, which focuses on the hiring of tax accountants, the coefficient on *IRS Audit* is positive and statistically significant in column (4), indicating that tax accountants are hired the year *after* the IRS audit begins. The coefficients are positive but statistically insignificant at conventional two-tailed levels in all other years (although statistically significant one-tailed for the initial year of the audit). Notably, the coefficients are small in the two years preceding the initial IRS audit year (0.002 and 0.003 in columns 1 and 2, respectively). The coefficients are 133% and 266% higher in the initial audit year (column 3) and the following year (column 4), respectively, than in the year directly preceding the audit (column 2). In the second and third years after the audit begins, the coefficient remains elevated (i.e., 0.007 in columns 5 and 6), but it is statistically insignificant. This result is consistent with general turnover, whereby firms slowly revert to their pre-audit number of personnel (i.e., by replacing employees who leave at a lower rate).

Panel B focuses on general accountants. The coefficient on *IRS Audit* is positive and statistically significant for both the initial year of the audit (column 3) and the following year (column 4). As in Panel A, this pattern aligns with our expectations. We document relatively small and statistically insignificant coefficients in the two years preceding the audit (i.e., columns 1 and

²⁵ The sample in column 4 corresponds to the primary sample from which we draw our inferences (i.e., $N = 17,636$). The other columns have a reduced sample size due to the relative availability of the IRS audit and employment variables.

2), and a coefficient that is 175% (150%) higher in the year of (directly following) the IRS audit relative to the year directly preceding the IRS audit. We observe a more rapid reversion to the mean in the second and third years following audit initiation (columns 5 and 6) than among tax accountants. This result is again consistent with natural turnover, whereby firms revert to pre-audit personnel levels, and with relatively larger groups of general accountants allowing for more rapid turnover.

We next examine these dynamics using an alternative research design. While our primary analyses are not based on a traditional difference-in-differences model with clearly defined pre- and post-treatment periods, our setting allows us to construct a subsample to evaluate the timing of the “treatment” effects from an IRS audit. To do so, we focus on firms first audited by the IRS near the midpoint of our sample period (2012 or 2013) and create a balanced panel spanning several years before and after the audit. To construct a counterfactual “control” group, we identify firms that are either outside a multi-year window surrounding an IRS audit (two years prior to or post audit), never audited, and firms that remain continuously under audit throughout the sample period. This approach enables us to approximate a difference-in-differences setting and observe how employment dynamics evolve around an identifiable treatment point.²⁶ We then conduct an event-time analysis, which examines the treatment effect for tax and general accountants, using the controls and firm fixed effects outlined in Equation (2) (e.g., Baker et al. 2022).

Figure 1 presents this analysis. Consistent with the results in Table 8, we find that the effect for both tax and general accountants is largely concentrated in the first year of the IRS audit. The level of tax accountants remains elevated for several years after audit initiation. However, the level of general accountants eventually mean-reverts, consistent with natural turnover (dismissals,

²⁶ This results in a subsample of 10,205 observations, of which 754 (90 unique firms) are considered “treated” by an IRS audit.

voluntary departures, or both) enabling a return to pre-audit equilibrium.

5.2. *Alternative Measure of Tax Enforcement*

An advantage of our primary measure of IRS audits, based on Yost and Shu (2022), is that it is a firm-specific, time-varying indicator variable that is a credible proxy for an IRS audit. However, this measure is only available through 2016 because the SEC EDGAR 10-K download data are not available after that time. To mitigate concerns that our documented results are spurious due to a factor unique to our primary measure, or that our results do not hold in a later sample period, we test the robustness of our inferences to using a recent measure of IRS exposure from Armstrong et al. (2025). This measure is available through 2019 and captures the percentage of sentences in firms' 10-Ks that include a reference to the IRS, along with associated agency action words such as "regulation," "audit," "authority," "enforce," and "investigation," among others.

We use this measure in two different ways to estimate the firm fixed effects specification from Equation (2). First, in Table 9, Panel A, we use the continuous measure (*IRS Exposure*), which captures the percentage of a firm's 10-K that references IRS action words. With firm fixed effects, this model captures the relation between within-firm changes in the discussion of IRS exposure and changes in the level of tax and general accountants. Consistent with our primary results in Table 3, the coefficient on *IRS Exposure* is positive across all four columns, and it is significant at conventional two-tailed levels in columns 3 and 4. In columns 1 and 2, the estimate is positive, yet it falls below two-tailed significance (although significant one-tailed).

Importantly, *IRS Exposure* is a continuous measure of firms' discussions of IRS scrutiny, which may be relatively sticky over time. To enhance statistical power and attempt to isolate "shocks" to IRS exposure, we use a modified measure of IRS exposure (*ΔIRS Exposure High*), which is an indicator variable equal to one for firms with a year-over-year change in *IRS Exposure*

in the top quintile, and zero otherwise.²⁷ Table 9, Panel B, reports these results. Across all four columns, we observe a significantly positive coefficient on *ΔIRS Exposure High*, indicating that large increases in IRS exposure are associated with a significant within-firm increase in accounting employees. These tests mitigate concerns that our results are driven by any unique feature of the Yost and Shu (2022) measure, as we obtain qualitatively similar inferences with a measure constructed in an entirely different manner.

5.3. Additional Controls and Entropy Balancing

We next conduct additional robustness tests. First, we employ EB, where we assign weights to control observations (i.e., those where *IRS Audit* = 0) to match them to treated observations (i.e., those where *IRS Audit* = 1) on the first three distributional moments (mean, variance, and skewness) of all included control variables. This methodology alleviates concerns related to model misspecification. We estimate Equation (2) using this EB-weighted sample. Table 10, Panel A, presents these results, and the findings are consistent with our primary analyses.

Next, we add additional controls. In our main analyses, we are careful not to overcontrol for characteristics that may be affected by both IRS audits and a firm's accounting workforce. For example, both IRS audits and internal accountants may influence R&D credits, inventory management, tax loss carryforwards, effective tax rates, or capital expenditures, which can result in potential bias from collider variables (Gow et al. 2016; Whited et al. 2022). However, to mitigate concerns that these represent correlated omitted variables, we add these controls to our Equation (2) estimation in Table 10, Panel B.²⁸ In Panel C, we employ EB using the expanded set of control variables. The results are again consistent with our primary analyses.

²⁷ We use the top quintile (20%) since it corresponds closely to the 22% of our sample coded as being subject to an IRS audit according to the Yost and Shu (2022) measure.

²⁸ To retain the full sample and eliminate the need to omit pretax loss observations, we utilize the Henry and Sansing (2018) measure of cash tax avoidance (*HS ETR*).

5.4. Falsification Tests

We next attempt to rule out that our results simply reflect broad hiring patterns rather than accountant-specific hiring responses. If IRS audits coincide with shocks that generally increase labor demand, we would expect to observe a similar hiring pattern in other employee categories outside of accountants. To rule out this explanation, we first replace the dependent variable with the level of total *customer service* employees, averaged over years t and $t+1$ ($\text{Ln}(\text{Avg Customer Service})$). This measure is advantageous because it provides a proxy for hiring that may be associated with significant changes to a firm's customer base. If a revenue shock is correlated with hiring *and* IRS audits, then we may observe a positive association between *IRS Audit* and $\text{Ln}(\text{Avg Customer Service})$. Next, we replace the dependent variable with the average number of *total non-accounting* employees over the same period ($\text{Ln}(\text{Avg Total Employees})$). While this measure may capture broader operational effects of tax audits that could influence headcount outside of the accounting function, it provides a useful benchmark for assessing the overall association between IRS audits and general hiring.

Table 11 presents these results. We use the firm fixed effect specification from Equation (2). For both customer service and all non-accounting employees, the coefficient on *IRS Audit* is small and statistically insignificant, which is inconsistent with the idea that our documented effect captures general labor expansion. Instead, they reinforce the interpretation that IRS audits elicit targeted increases in accounting-related human capital rather than broad-based hiring responses.

6. Conclusion

This study examines whether firms adjust their internal accounting workforce in response to IRS audits, providing novel evidence on firms' internal organizational responses to tax enforcement. Using a proxy for IRS audits based on Yost and Shu (2022) and Revelio employment

data, we document a significant increase in both tax and general accountants around IRS audits. The magnitude of this hiring response, corresponding to approximately three additional accountants per audit, is economically meaningful and consistent with a tax enforcement-driven demand for accounting expertise. These results are robust to the inclusion of firm fixed effects, using an alternative proxy for IRS audits based on Armstrong et al. (2025), and using EB. Our inferences are further strengthened by tests of the dynamics of the accountant hiring effect, as well as falsification tests using other employee groups.

We add context to the mechanism behind our results in several ways. We show that tax-specific accountants are hired when firms engage in more tax avoidance (where more tax savings are potentially at risk), and general accountants are hired in more informationally complex firms (where the information demands of IRS audits are likely the greatest). We also document that the hiring response around IRS audits for accountants decreases as firms invest more in external tax services from their auditor. Further examining the trade-off between internal and external tax expertise, we show that the hiring response for tax accountants is concentrated in firms that have not historically relied heavily on their auditor for tax services, as well as in profitable firms. We also find that the tax accountants hired around an IRS audit are more senior, while the general accountants are more junior. This result is consistent with the newly hired tax accountants having the experience to directly engage in the IRS audit process, while newly hired general accountants assist with more basic tasks, thereby freeing existing employees to help manage the IRS audit. Finally, we demonstrate that these labor investments partially offset the increased cash tax outflows associated with IRS audits and help explain a portion of the documented improvements in firms' information environments that reduce the cost of capital. Beyond providing context behind the mechanism, each of these tests also helps to rule out alternative explanations for our results.

By linking tax enforcement to internal hiring of accountants, our study contributes to the broad literature on the real effects of taxation and answers recent calls for more evidence on taxes and labor capital decisions (Jacob 2022; Lester and Olbert 2025). Our findings highlight how tax enforcement can both discipline and shape firm behavior, through mechanisms extending beyond external penalties to internal investment and adaptation.

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Appendix A Variable Definitions

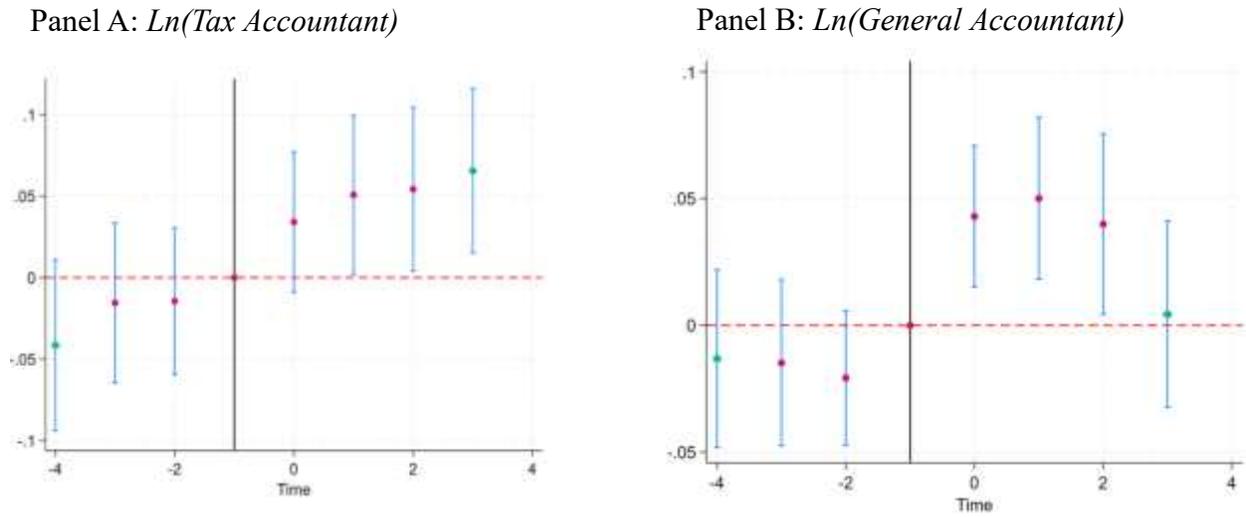
<i>Tax Accountant</i>	The number of tax accountants, measured as of December 31. Employees are considered tax accountants if their Revelio role_k1000 is “Tax Specialist.” Source: Revelio.
Δ <i>Tax Accountant</i>	The net change in <i>Tax Accountant</i> from year $t-1$ to $t+1$.
<i>General Accountant</i>	The number of total accountants less the number of tax accountants, measured as of December 31. Source: Revelio.
Δ <i>General Accountant</i>	The net change in <i>General Accountant</i> from year $t-1$ to $t+1$.
<i>Total Accountants</i>	The number of accountants, measured as of December 31. Employees are considered accountants if their Revelio role_k50 is “Accountant”. Source: Revelio.
Δ <i>Total Accountant</i>	The net change in <i>Total Accountants</i> from year $t-1$ to $t+1$.
$\ln(\text{Avg Tax Accountant})$	The natural log of (one plus the average of <i>Tax Accountant</i> for years t and $t+1$).
$\ln(\text{Tax Accountant})$	The natural log of (one plus <i>Tax Accountant</i>).
$\ln(\text{Avg General Accountant})$	The natural log of (one plus the average of <i>General Accountant</i> for years t and $t+1$).
$\ln(\text{General Accountant})$	The natural log of (one plus <i>General Accountant</i>).
<i>Avg Seniority – Tax</i>	The average seniority score from Revelio labs for all <i>Tax Accountant</i> hires for years t and $t+1$. Seniority score ranges from 1 (entry-level) to 7 (executive-level). Source: Revelio.
<i>Avg Seniority – General</i>	The average seniority score from Revelio labs for all <i>General Accountant</i> hires for years t and $t+1$. Seniority score ranges from 1 (entry-level) to 7 (executive-level). Source: Revelio.
$\ln(\text{Avg Customer Service})$	The natural log of (one plus the average of customer service employees for years t and $t+1$). Employees are considered customer service employees if their Revelio role_k50 is “Customer Service.” Source: Revelio.
$\ln(\text{Avg Total Employees})$	The natural log of (one plus the average of all firm employees for years t and $t+1$). Source: Revelio.
<i>IRS Audit</i>	An indicator variable equal to one if a firm is estimated to be under IRS audit, and zero otherwise. Measured following Yost and Shu (2022).

<i>IRS Exposure</i>	A firm's exposure to the IRS. Following Armstrong et al. (2025), this is measured as the number of sentences in which the IRS and an agency action word are mentioned in the firm's 10-K, divided by the total number of sentences in the 10-K. Source: https://www.daphnemarmstrong.com/ .
<i>ΔIRS Exposure</i>	The change in IRS Exposure from year t-1 to year t, multiplied by 100 for presentation.
<i>ΔIRS Exposure High</i>	An indicator variable equal to one for firms-years in the top quintile of <i>ΔIRS Exposure</i> , and zero otherwise.
<i>Information Needs</i>	The first principal component from a PCA using <i>Size</i> , <i>Segments Unrelated</i> , <i>PPE</i> , and <i>Capital Exp</i> . Each individual variable is described elsewhere in the variable appendix.
<i>Cash ETR</i>	The long-run cash effective tax rate, measured from years t to $t+2$, winsorized at 0 and 1. $\left(\frac{\sum_{t+2}^t TXPD}{\sum_{t+2}^t (PI-SPI)}\right)$. <i>Cash ETR</i> is set to missing if the denominator is negative. Source: Compustat.
<i>Cost of Capital</i>	The yearly average of the firm's internal estimate of its weighted average cost of capital, multiplied by 100 for presentation. Measured following Gormsen and Huber (2025). Source: https://costofcapital.org/ .
<i>Size</i>	The natural log of total assets (AT). Source: Compustat.
<i>MNC</i>	An indicator variable equal to one for firm-years reporting non-zero foreign earnings (PIFO) or non-zero foreign income tax (TXFO), and zero otherwise. Source: Compustat.
<i>MTB</i>	The market value of equity plus the book value of debt, scaled by total assets $((PRCC_F*CSHO + DLTT + DLC)/AT)$. Source: Compustat.
<i>ROA</i>	Return on assets, measured using earnings before interest and taxes (EBIT/AT). Source: Compustat.
<i>Leverage</i>	Long-term debt scaled by total assets (DLTT/AT). Source: Compustat.
<i>Intangible Intensity</i>	Intangible asset intensity, measured by intangibles scaled by total assets (INTAN/AT). Source: Compustat.
<i>PPE</i>	Capital intensity, calculated as net property, plant, and equipment scaled by total assets (PPENT/AT). Source: Compustat.
<i>APTS</i>	Auditor-provided tax services, measured by fees paid to the auditor for tax services scaled by total assets (TaxFees/AT), multiplied by 100 for presentation. Source: Audit Analytics and Compustat.

<i>Segments Unrelated</i>	The number of unrelated business segments reported in the Compustat Segments database. Unrelated business segments are defined as business segments that do not have overlapping three digit SIC codes. Source: Compustat.
<i>Tax Loss</i>	An indicator variable equal to one if a firm has a tax loss carryforward (TLCF), and zero otherwise. Source: Compustat.
<i>HS ETR</i>	The Henry and Sansing (2018) measure of tax avoidance. Measured as $(TXPD - (TXR - L.TXR) - PI * TaxRate) / (AT + PRCC_F * CSHO - CEQ)$. TXPD and TXR are replaced with zero if missing. TaxRate is calculated as 0.35 (0.21) before (on or after) 2018. Source: Compustat.
<i>Inventory</i>	Inventory scaled by lagged total assets (INVT/L.AT). Source: Compustat.
<i>R&D</i>	Research and development expense divided by lagged total assets (XRD/L.AT). Missings are replaced with 0. Source: Compustat.
<i>Capital Exp</i>	Capital expenditures scaled by total assets (CAPX/AT). Source: Compustat.

Figure 1

Event-time analysis for IRS audits first occurring in the middle of our sample window



This figure presents the results of an event-time analysis examining the “treatment” effects of an IRS audit on labor outcomes. To operationalize this, we capture a subsample of firms that are *first* audited in either 2012 or 2013 with a fully balanced panel of available pre- and post-periods. We then compare this treatment effect to counterfactual firms that are either not under audit in the prior or following two years or remain continuously under audit throughout the sample period. We use the controls and fixed effects outlined in Equation (2). Panel A (Panel B) presents results when $\ln(\text{Tax Accountant})$ ($\ln(\text{General Accountant})$) is the outcome variable. Circle points represent the coefficient estimate for each time interval, whereas diamond points represent the aggregation of the coefficient for the remaining pre- and post-periods. Whisker lines depict confidence intervals at the 90th percentile.

Table 1 Sample Selection

	Firm-Years	Firms
Compustat public, U.S. incorporated firm-years from 2008 to 2016	65,116	11,404
With Revelio employment data	32,846	5,301
With data to construct IRS Audit indicator	25,658	4,149
Not in regulated industries (SIC codes 4900 – 4999, 6000 – 6499, and 6600 – 6799)	20,291	3,354
Not tax preparation service providers (H&R Block and Robert Half)	20,273	3,352
With Compustat Annual controls data	19,606	3,306
With Audit Analytics data	19,521	3,299
With Compustat Segments data	<u>17,983</u>	<u>3,155</u>
Remaining firm-years excluding singletons	17,636	2,808

This table outlines sample selection criteria.

Table 2 Descriptive Statistics

	Full Sample						IRS Audit = 1		IRS Audit = 0	
	N	Mean	p25	Median	p75	SD	N	Mean	N	Mean
<i>Tax Accountant</i>	17,636	4.640	0.000	1.000	5.000	10.091	3,883	11.004	13,753	2.843
Δ <i>Tax Accountant</i>	17,636	0.477	0.000	0.000	1.000	1.759	3,883	1.032	13,753	0.320
<i>General Accountant</i>	17,636	90.938	6.000	22.000	72.000	206.969	3,883	221.268	13,753	54.141
Δ <i>General Accountant</i>	17,636	6.104	0.000	1.000	6.000	19.408	3,883	13.217	13,753	4.096
<i>Total Accountants</i>	17,636	96.083	7.000	23.000	77.000	218.256	3,883	233.813	13,753	57.196
Δ <i>Total Accountant</i>	17,636	6.559	0.000	1.000	6.000	20.372	3,883	14.189	13,753	4.405
<i>Ln(Avg Tax Accountant)</i>	17,636	0.975	0.000	0.693	1.705	1.111	3,883	1.658	13,753	0.782
<i>Ln(Avg General Accountant)</i>	17,636	3.194	2.015	3.157	4.311	1.668	3,883	4.097	13,753	2.939
<i>Ln(Avg Customer Service)</i>	17,636	2.462	0.693	2.303	3.902	1.952	3,883	3.418	13,753	2.192
<i>Ln(Avg Total Employees)</i>	17,636	6.283	4.850	6.438	7.889	2.196	3,883	7.339	13,753	5.985
<i>Avg Seniority – Tax</i>	5,397	3.509	2.500	3.500	4.750	1.231	1,858	3.466	3,539	3.532
<i>Avg Seniority – General</i>	11,965	3.059	2.500	3.000	3.500	0.898	2,878	2.930	9,087	3.099
<i>IRS Audit</i>	17,636	0.220	0.000	0.000	0.000	0.414	3,883	1.000	13,753	0.000
<i>IRS Exposure</i>	26,758	0.001	0.000	0.000	0.001	0.001	3,761	0.001	13,129	0.001
Δ <i>IRS Exposure</i>	26,758	0.001	-0.005	0.000	0.005	0.068	3,735	0.000	12,977	0.000
Δ <i>IRS Exposure High</i>	26,758	0.200	0.000	0.000	0.000	0.400	3,735	0.281	12,977	0.177
<i>Cash ETR</i>	11,280	0.247	0.126	0.236	0.321	0.184	3,042	0.251	8,238	0.245
<i>Cost of Capital</i>	13,748	9.136	8.334	9.131	9.927	1.097	3,209	8.722	10,539	9.262
<i>Size</i>	17,636	6.212	4.807	6.182	7.566	1.975	3,883	7.495	13,753	5.850
<i>MNC</i>	17,636	0.597	0.000	1.000	1.000	0.491	3,883	0.752	13,753	0.553
<i>MTB</i>	17,636	1.750	0.852	1.255	2.048	1.502	3,883	1.563	13,753	1.803
<i>ROA</i>	17,636	-0.008	-0.022	0.062	0.114	0.259	3,883	0.069	13,753	-0.030
<i>Leverage</i>	17,636	0.177	0.000	0.118	0.286	0.203	3,883	0.194	13,753	0.172
<i>Intangible Intensity</i>	17,636	0.184	0.011	0.112	0.303	0.201	3,883	0.216	13,753	0.175
<i>PPE</i>	17,636	0.235	0.063	0.149	0.335	0.230	3,883	0.237	13,753	0.234
<i>APTS</i>	17,636	0.028	0.000	0.006	0.031	0.054	3,883	0.022	13,753	0.030
<i>Segments Unrelated</i>	17,636	1.500	1.000	1.000	2.000	0.872	3,883	1.759	13,753	1.427
<i>Tax Loss</i>	17,636	0.641	0.000	1.000	1.000	0.480	3,883	0.623	13,753	0.646
<i>HS ETR</i>	17,636	0.017	-0.007	0.000	0.018	0.052	3,883	0.006	13,753	0.021
<i>Inventory</i>	17,636	0.112	0.001	0.070	0.173	0.132	3,883	0.123	13,753	0.109
<i>R&D</i>	17,636	0.072	0.000	0.009	0.084	0.140	3,883	0.040	13,753	0.081
<i>Capital Exp</i>	17,636	0.046	0.013	0.028	0.055	0.056	3,883	0.044	13,753	0.047

This table presents descriptive statistics. Continuous variables are winsorized at the 1st and 99th percentiles. All variables are defined in Appendix A.

Table 3 IRS Audits and Accounting Labor Investment

Panel A: Industry Fixed Effects

VARIABLES	(1) <i>ΔTax Accountant</i>	(2)	(3) <i>ΔGeneral Accountant</i>	(4)
<i>IRS Audit</i>	0.706***	0.216***	9.008***	2.787***
	(10.00)	(4.66)	(9.25)	(4.78)
<i>Size</i>		0.349***		4.627***
		(16.04)		(14.31)
<i>MNC</i>		-0.036		-0.909**
		(-0.99)		(-1.96)
<i>MTB</i>		0.060***		1.121***
		(4.92)		(7.53)
<i>ROA</i>		-0.314***		-3.848***
		(-5.32)		(-4.32)
<i>Leverage</i>		-0.549***		-6.829***
		(-5.65)		(-5.34)
<i>Intangible Intensity</i>		0.075		1.119
		(0.56)		(0.58)
<i>PPE</i>		-0.372***		-4.676***
		(-3.10)		(-3.32)
<i>APTS</i>		0.827***		12.477***
		(4.72)		(5.06)
<i>Segments Unrelated</i>		0.047		-0.028
		(1.43)		(-0.06)
Observations	17,636	17,636	17,636	17,636
Adjusted R-squared	0.039	0.137	0.054	0.190
Fixed Effects	Industry, Year	Industry, Year	Industry, Year	Industry, Year

Table 3 IRS Audits and Accounting Labor Investment (continued)

Panel B: Firm Fixed Effects

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2) <i>Ln(Avg Tax Accountant)</i>	(3) <i>Ln(Avg General Accountant)</i>	(4) <i>Ln(Avg General Accountant)</i>
<i>IRS Audit</i>	0.016*** (3.31)	0.011** (2.41)	0.019*** (4.57)	0.012*** (3.00)
<i>Size</i>		0.106*** (10.23)		0.188*** (18.05)
<i>MNC</i>		0.006 (0.40)		0.040*** (3.04)
<i>MTB</i>		0.005* (1.95)		0.010*** (3.38)
<i>ROA</i>		-0.071*** (-4.48)		-0.080*** (-4.07)
<i>Leverage</i>		0.016 (0.58)		0.024 (0.87)
<i>Intangible Intensity</i>		0.106*** (2.69)		0.045 (1.15)
<i>PPE</i>		0.032 (0.68)		0.047 (0.80)
<i>APTS</i>		0.204*** (3.81)		0.229*** (3.69)
<i>Segments Unrelated</i>		0.007 (0.74)		0.013 (1.55)
Observations	17,636	17,636	17,636	17,636
Adjusted R-squared	0.972	0.974	0.990	0.992
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

This table presents the results from estimating Equation (1) (Panel A) and Equation (2) (Panel B). In Panel A, the dependent variable in columns 1 and 2 (3 and 4) is $\Delta Tax Accountant$ ($\Delta General Accountant$), which is measured as the change in *Tax Accountant* (*General Accountant*) from $t-1$ to $t+1$. In Panel B, the dependent variable in columns 1 and 2 (3 and 4) is $Ln(Avg Tax Accountant)$ ($Ln(Avg General Accountant)$). $Ln(Avg Tax Accountant)$ and $Ln(Avg General Accountant)$ are measured as the natural log of one plus the average *Tax Accountant* (*General Accountant*) for years t and $t+1$. *IRS Audit* and all control variables (unless otherwise indicated) are measured in year t . All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.

Table 4 Cross-Sectional Analysis

Panel A: APTS and Tax Avoidance (Industry Fixed Effects)

VARIABLES	(1) <i>ΔTax Accountant</i>	(2)	(3) <i>ΔGeneral Accountant</i>	(4)
<i>IRS Audit × APTS</i>	-3.823*** (-5.41)		-40.040*** (-5.04)	
<i>IRS Audit × HS ETR</i>		-4.612*** (-5.53)		-67.547*** (-6.82)
<i>IRS Audit</i>	0.309*** (5.51)	0.251*** (5.07)	3.756*** (5.42)	3.306*** (5.31)
<i>HS ETR</i>		0.446 (1.63)		3.443 (1.05)
<i>APTS</i>	1.317*** (7.29)	0.818*** (4.71)	17.618*** (7.01)	12.393*** (5.06)
<i>Size</i>	0.345*** (16.05)	0.346*** (16.01)	4.583*** (14.33)	4.571*** (14.29)
<i>MNC</i>	-0.035 (-0.96)	-0.032 (-0.90)	-0.898* (-1.94)	-0.851* (-1.85)
<i>MTB</i>	0.059*** (4.88)	0.059*** (4.54)	1.116*** (7.49)	1.092*** (6.83)
<i>ROA</i>	-0.294*** (-5.00)	-0.288*** (-3.90)	-3.634*** (-4.12)	-3.877*** (-3.62)
<i>Leverage</i>	-0.543*** (-5.60)	-0.541*** (-5.59)	-6.760*** (-5.30)	-6.683*** (-5.26)
<i>Intangible Intensity</i>	0.078 (0.59)	0.072 (0.54)	1.154 (0.60)	1.080 (0.56)
<i>PPE</i>	-0.368*** (-3.08)	-0.368*** (-3.07)	-4.632*** (-3.31)	-4.620*** (-3.30)
<i>Segments Unrelated</i>	0.045 (1.36)	0.046 (1.39)	-0.054 (-0.12)	-0.050 (-0.11)
Observations	17,636	17,636	17,636	17,636
Adjusted R-squared	0.139	0.139	0.192	0.193
Fixed Effects	Industry, Year	Industry, Year	Industry, Year	Industry, Year

Table 4 Cross-Sectional Analysis (continued)

Panel B: APTS and Tax Avoidance (Firm Fixed Effects)

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2)	(3) <i>Ln(Avg General Accountant)</i>	(4)
<i>IRS Audit</i> × <i>APTS</i>	-0.282*** (-3.41)		-0.018 (-0.23)	
<i>IRS Audit</i> × <i>HS ETR</i>		-0.144 (-1.31)		-0.094 (-1.03)
<i>IRS Audit</i>	0.019*** (3.52)	0.013*** (2.62)	0.012*** (2.71)	0.012*** (3.18)
<i>HS ETR</i>		0.021 (0.35)		0.118* (1.96)
<i>APTS</i>	0.242*** (4.56)	0.205*** (3.84)	0.231*** (3.69)	0.229*** (3.69)
<i>Size</i>	0.106*** (10.24)	0.106*** (10.24)	0.188*** (18.05)	0.189*** (18.07)
<i>MNC</i>	0.006 (0.40)	0.006 (0.41)	0.040*** (3.04)	0.039*** (3.00)
<i>MTB</i>	0.005* (1.93)	0.005* (1.88)	0.010*** (3.38)	0.011*** (3.66)
<i>ROA</i>	-0.070*** (-4.43)	-0.069*** (-3.66)	-0.080*** (-4.07)	-0.063*** (-2.71)
<i>Leverage</i>	0.015 (0.57)	0.016 (0.58)	0.024 (0.87)	0.023 (0.84)
<i>Intangible Intensity</i>	0.105*** (2.66)	0.107*** (2.70)	0.045 (1.14)	0.048 (1.22)
<i>PPE</i>	0.031 (0.67)	0.032 (0.69)	0.047 (0.80)	0.045 (0.77)
<i>Segments Unrelated</i>	0.007 (0.75)	0.007 (0.74)	0.013 (1.55)	0.013 (1.56)
Observations	17,636	17,636	17,636	17,636
Adjusted R-squared	0.974	0.974	0.992	0.992
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 4 Cross-Sectional Analysis (continued)

Panel C: Informational Needs

VARIABLES	(1) <i>ΔGeneral Accountant</i>	(2) <i>Ln(Avg General Accountant)</i>
<i>IRS Audit</i> × <i>Information Needs</i>	2.309*** (4.08)	0.008** (2.19)
<i>IRS Audit</i>	6.163*** (7.75)	0.017*** (4.04)
<i>Information Needs</i>	2.424*** (9.27)	0.036*** (5.66)
<i>MNC</i>	3.566*** (7.58)	0.076*** (5.15)
<i>MTB</i>	1.341*** (8.39)	0.001 (0.41)
<i>ROA</i>	3.845*** (5.19)	0.076*** (3.68)
<i>Leverage</i>	-1.940 (-1.55)	0.093*** (3.06)
<i>Intangible Intensity</i>	12.666*** (6.34)	0.268*** (6.54)
<i>APTS</i>	-11.715*** (-5.08)	-0.026 (-0.37)
Observations	17,636	17,636
Adjusted R-squared	0.113	0.990
Fixed Effects	Industry, Year	Firm, Year

This table presents cross-sectional analyses of Equation (1) (Panel A) and Equation (2) (Panel B). *APTS* proxies for external accounting resources, and its interaction with *IRS Audit* is expected to be negative. *HS ETR* proxies for firm tax avoidance, and its interaction with *IRS Audit* is expected to be negative. Panel C presents an analysis of *Information Needs*, the first principal component from a PCA on *Size*, *Segments Unrelated*, *PPE*, and *Capital Expenditures*. In Panel C, we do not control for *Size*, *Segments Unrelated*, and *PPE* since these variables are used to construct *Information Needs*. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively. P-values are based on two-tailed tests.

Table 5 Trade-off between Internal and External Tax Expertise

Panel A: Main Analysis Using Seemingly Unrelated Equations

VARIABLES	(1) <i>APTS</i>	(2) <i>Ln(Avg Tax Accountant)</i>
<i>IRS Audit</i>	0.002** (2.38)	0.010** (2.16)
Observations	17,636	17,636
Adjusted R-squared	0.051	0.171
Controls	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year

Panel B: Trade-off Effects Using Seemingly Unrelated Equations

VARIABLES	(1) <i>APTS</i>	(2) <i>Ln(Avg Tax Accountant)</i>
<i>IRS Audit × Ln(Avg Tax Accountant)</i>	-0.005** (-2.16)	
<i>IRS Audit × APTS</i>		-0.292** (-2.40)
Observations	17,636	17,636
Adjusted R-squared	0.051	0.171
Controls	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year

Panel C: Split on Prior Year APTS

VARIABLES	(1) <i>APTS</i>	(2) <i>Ln(Avg Tax Accountant)</i>	(3) <i>APTS</i>	(4) <i>Ln(Avg Tax Accountant)</i>
Partition:	Below-Median Prior APTS		Above-Median Prior APTS	
<i>IRS Audit</i>	-0.000 (-0.31)	0.019*** (3.07)	0.004*** (3.32)	0.000 (0.06)
Observations	8,791	8,791	8,790	8,790
Adjusted R-squared	0.014	0.173	0.106	0.171
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 5 Trade-off between Internal and External Tax Expertise (continued)

Panel D: Split on Profitability

	(1)	(2)	(3)	(4)
VARIABLES	<i>APTS</i>	<i>Ln(Avg Tax Accountant)</i>	<i>APTS</i>	<i>Ln(Avg Tax Accountant)</i>
Partition:	Profit Firms		Loss Firms	
<i>IRS Audit</i>	0.001 (1.58)	0.013** (2.42)	0.005** (2.22)	0.005 (0.49)
Observations	12,630	12,630	5,006	5,006
Adjusted R-squared	0.024	0.210	0.081	0.084
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

This table presents the results from estimating Equation (2) using seemingly unrelated regression (Stata's SUREG command) to examine the trade-off between hiring tax accountants and purchasing auditor-provided tax services (*APTS*), considering that these outcomes are jointly determined. Panel A examines both Tax Accountant and *APTS* as joint decisions. Panel B examines the trade off between the two effects. For brevity, we only include the interaction, but the regression includes the standalone variables (i.e., *IRS Audit* and *APTS* or *Ln(Avg Tax Accountant)*). Panels C and D examine the result in subsamples based on firm's use of *APTS* in the prior year (Panel C) and firm profitability, using average EBIT for the prior and current year (Panel D). All analyses include the following controls: *Size*, *MNC*, *MTB*, *ROA*, *Leverage*, *Intangible Intensity*, *PPE*, and *Segments Unrelated*. We also control for *Ln(Avg Tax Accountant)* (*APTS*) when *APTS* (*Ln(Avg Tax Accountant)*) is the dependent variable. *Ln(Avg Tax Accountant)* is measured as the natural log of one plus the average *Tax Accountant* for years t and $t+1$. *IRS Audit*, *APTS*, and all control variables (unless otherwise indicated) are measured in year t . To include firm fixed effects with Stata's SUREG command, all variables are demeaned by the firm-average. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.

Table 6 Seniority of Hires

VARIABLES	(1) <i>Avg Seniority – Tax</i>	(2)	(3) <i>Avg Seniority – General</i>	(4)
<i>IRS Audit</i>	0.065* (1.72)	0.067* (1.75)	-0.029 (-1.60)	-0.030 (-1.64)
<i>Size</i>		-0.055 (-0.66)		-0.011 (-0.32)
<i>MNC</i>		0.011 (0.08)		-0.019 (-0.35)
<i>MTB</i>		0.053* (1.95)		-0.024* (-1.82)
<i>ROA</i>		-0.588* (-1.75)		-0.127 (-1.19)
<i>Leverage</i>		-0.104 (-0.45)		-0.066 (-0.66)
<i>Intangible Intensity</i>		0.223 (0.76)		0.228* (1.81)
<i>PPE</i>		0.140 (0.29)		0.204 (0.93)
<i>APTS</i>		0.473 (0.49)		0.062 (0.16)
<i>Segments Unrelated</i>		-0.100* (-1.81)		-0.049* (-1.78)
Observations	5,397	5,397	11,965	11,965
Adjusted R-squared	0.451	0.452	0.456	0.457
Fixed Effects	Firm & Year	Firm & Year	Firm & Year	Firm & Year

This table uses a modified version of Equation 2 to examine the relative seniority of tax and general accountant hires around IRS audits (*IRS Audit*). We limit our sample to only firm-years with net hires, ensuring that we do not have 0's in our analyses of seniority. Seniority measures are captured as the *average* seniority score from Revelio Labs for all new hires. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively. P-values are based on two-tailed tests.

Table 7 Path Analyses

Panel A: Effective Tax Rates

	Pred.	Coefficient	p-value
Direct path			
$p(\text{IRS Audit}, \text{Cash ETR}) = \beta_1$	(+)	0.0063	0.07*
Mediated Path for Cash Effective Tax Rate			
$p(\text{IRS Audit}, \Delta\text{Total Accountant}) = \gamma_1$		3.0158	0.00***
$p(\Delta\text{Total Accountant}, \text{Cash ETR}) = \beta_2$		-0.0003	0.00***
Total mediated path for Cash Effective Tax Rate ($\gamma_1*\beta_2$)		-0.0009	
Control Variables		Yes	
Fixed Effects		Industry, Year	
Observations		11,280	

Panel B: Cost of Capital

	Pred.	Coefficient	p-value
Direct path			
$p(\text{IRS Audit}, \text{Cost of Capital}) = \beta_1$	(-)	-0.0851	0.00***
Mediated Path for Cost of Capital			
$p(\text{IRS Audit}, \Delta\text{Total Accountant}) = \gamma_1$		2.3476	0.00***
$p(\Delta\text{Total Accountant}, \text{Cost of Capital}) = \beta_2$		-0.0017	0.00***
Total mediated path for Cost of Capital ($\gamma_1*\beta_2$)		-0.0040	
Control Variables		Yes	
Fixed Effects		Industry, Year	
Observations		13,748	

This table presents estimations of the path analysis in Equations (3) and (4) using a generalized SEM. Each $p(X_1, X_2)$ represents the path coefficient. The two mediated paths are cash effective tax rates and cost of capital. *Cash ETR* is measured from years t to $t+2$. *Cost of Capital* is measured in year $t+1$. $\Delta\text{Total Accountant}$ is the change in *Total Accountants* from year $t-1$ to $t+1$. *IRS Audit* is measured in year t , and all control variables (unless otherwise indicated) are measured in year t . Control variables included are *Size*, *MNC*, *MTB*, *ROA*, *Leverage*, *Intangible Intensity*, *PPE*, *APTS*, and *Segments Unrelated*. All variables are defined in Appendix A. All analyses include indicated fixed effects. P-values are based on two-tailed (one-tailed) tests (where predictions are made). ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Table 8 Dynamics

Panel A: Tax Accountants

	(1)	(2)	(3)	(4)	(5)	(6)
<i>DV:</i>	<i>Ln(Tax Accountant)</i>					
<i>IRS Audit</i> at time:	<i>t+2</i>	<i>t+1</i>	<i>t</i>	<i>t-1</i>	<i>t-2</i>	<i>t-3</i>
<i>Ln(Tax Accountant)</i> relative to <i>IRS Audit</i>	-2	-1	0	+1	+2	+3
<i>IRS Audit (timing varies)</i>	0.002	0.003	0.007	0.011**	0.007	0.007
	(0.28)	(0.52)	(1.37)	(2.16)	(1.19)	(1.10)
<i>Size</i>	0.102***	0.107***	0.103***	0.113***	0.106***	0.099***
	(7.15)	(8.56)	(9.09)	(10.52)	(9.67)	(8.04)
<i>MNC</i>	0.009	0.007	0.005	0.001	-0.004	-0.003
	(0.44)	(0.37)	(0.28)	(0.03)	(-0.21)	(-0.19)
<i>MTB</i>	0.002	0.004	0.006**	0.009***	0.009***	0.009**
	(0.76)	(1.41)	(2.06)	(2.99)	(2.81)	(2.47)
<i>ROA</i>	-0.062***	-0.070***	-0.059***	-0.066***	-0.032*	-0.021
	(-3.08)	(-3.61)	(-3.51)	(-3.99)	(-1.80)	(-1.09)
<i>Leverage</i>	0.023	0.029	0.032	0.005	-0.002	0.004
	(0.56)	(0.75)	(0.96)	(0.17)	(-0.08)	(0.13)
<i>Intangible Intensity</i>	0.077	0.093*	0.100**	0.104**	0.097**	0.096**
	(1.39)	(1.90)	(2.21)	(2.52)	(2.22)	(2.09)
<i>PPE</i>	-0.030	0.017	0.033	0.020	0.018	0.041
	(-0.52)	(0.31)	(0.63)	(0.41)	(0.36)	(0.72)
<i>APTS</i>	0.119	0.129*	0.143**	0.193***	0.204***	0.203**
	(1.56)	(1.84)	(2.24)	(3.30)	(2.98)	(2.57)
<i>Segments Unrelated</i>	0.010	0.015	0.008	0.011	0.012	0.013
	(0.98)	(1.61)	(0.86)	(1.19)	(1.22)	(1.27)
Observations	9,289	11,573	14,256	17,636	14,256	11,573
Adjusted R-squared	0.975	0.972	0.970	0.967	0.970	0.973
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 8 Dynamics (continued)

Panel B: General Accountants

	(1)	(2)	(3)	(4)	(5)	(6)
<i>DV:</i>	<i>Ln(General Accountant)</i>					
<i>IRS Audit</i> at time:	<i>t+2</i>	<i>t+1</i>	<i>t</i>	<i>t-1</i>	<i>t-2</i>	<i>t-3</i>
<i>Ln(General Accountant)</i> relative to <i>IRS Audit</i>	-2	-1	0	+1	+2	+3
<i>IRS Audit</i> (timing varies)	0.005	0.004	0.011***	0.010**	0.005	0.004
	(1.18)	(1.03)	(2.81)	(2.30)	(1.17)	(0.95)
<i>Size</i>	0.152***	0.175***	0.185***	0.188***	0.186***	0.190***
	(11.06)	(15.14)	(16.71)	(17.29)	(16.21)	(15.28)
<i>MNC</i>	0.018	0.025*	0.034**	0.039***	0.045***	0.048***
	(1.30)	(1.83)	(2.54)	(2.87)	(3.15)	(3.26)
<i>MTB</i>	0.012***	0.014***	0.015***	0.016***	0.018***	0.016***
	(3.46)	(4.20)	(5.03)	(5.22)	(4.95)	(4.07)
<i>ROA</i>	-0.039	-0.060**	-0.059***	-0.047**	-0.032	-0.036
	(-1.51)	(-2.43)	(-2.60)	(-2.19)	(-1.34)	(-1.38)
<i>Leverage</i>	-0.009	0.014	0.019	0.024	0.027	0.014
	(-0.27)	(0.42)	(0.65)	(0.82)	(0.88)	(0.44)
<i>Intangible Intensity</i>	0.024	0.036	0.067*	0.067	0.063	0.028
	(0.57)	(0.88)	(1.66)	(1.61)	(1.37)	(0.55)
<i>PPE</i>	0.043	0.061	0.005	0.003	-0.002	0.023
	(0.63)	(0.88)	(0.07)	(0.06)	(-0.03)	(0.37)
<i>APTS</i>	0.221***	0.210***	0.215***	0.219***	0.323***	0.367***
	(3.29)	(3.19)	(3.32)	(3.29)	(4.24)	(4.45)
<i>Segments Unrelated</i>	0.009	0.012	0.015*	0.015*	0.014*	0.011
	(1.13)	(1.57)	(1.88)	(1.77)	(1.75)	(1.33)
Observations	9,289	11,573	14,256	17,636	14,256	11,573
Adjusted R-squared	0.994	0.992	0.991	0.989	0.990	0.991
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year

This table presents a dynamic analysis of Equation (2), modified such that measures of accountants are captured in only one year. In columns 1 through 6, *IRS Audit* is measured as of years *t-3* through *t+2*, respectively. In both panels, the dependent variable is measured in year *t*, and all control variables (unless otherwise indicated) are measured in year *t-1*. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.

Table 9 Alternative Measure of Tax EnforcementPanel A: *IRS Exposure*

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2)	(3) <i>Ln(Avg General Accountant)</i>	(4)
<i>IRS Exposure</i>	0.059	0.046	0.098***	0.065**
	(1.64)	(1.34)	(3.29)	(2.44)
<i>Size</i>		0.081*** (12.30)		0.174*** (22.75)
<i>MNC</i>		0.001 (0.10)		0.050*** (3.74)
<i>MTB</i>		0.001*** (6.18)		0.002*** (4.34)
<i>ROA</i>		-0.018*** (-8.86)		-0.029*** (-8.00)
<i>Leverage</i>		0.036*** (2.75)		0.054*** (2.96)
<i>Intangible Intensity</i>		0.116*** (4.21)		0.043 (1.23)
<i>PPE</i>		0.004 (0.15)		-0.007 (-0.18)
<i>APTS</i>		0.100*** (5.05)		0.092** (2.46)
<i>Segments Unrelated</i>		0.015* (1.76)		0.007 (0.75)
Observations	26,758	26,758	26,758	26,758
Adjusted R-squared	0.968	0.970	0.986	0.988
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

Table 9 Alternative Measure of Tax Enforcement(continued)Panel B: High Change in *IRS Exposure*

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2)	(3) <i>Ln(Avg General Accountant)</i>	(4)
<i>ΔIRS Exposure High</i>	0.010*** (2.90)	0.007** (2.14)	0.016*** (4.73)	0.010*** (3.26)
<i>Size</i>		0.081*** (12.28)		0.174*** (22.76)
<i>MNC</i>		0.001 (0.09)		0.050*** (3.72)
<i>MTB</i>		0.001*** (6.17)		0.002*** (4.33)
<i>ROA</i>		-0.018*** (-8.86)		-0.029*** (-8.01)
<i>Leverage</i>		0.036*** (2.75)		0.054*** (2.95)
<i>Intangible Intensity</i>		0.116*** (4.19)		0.042 (1.20)
<i>PPE</i>		0.004 (0.15)		-0.007 (-0.18)
<i>APTS</i>		0.100*** (5.05)		0.092** (2.47)
<i>Segments Unrelated</i>		0.015* (1.75)		0.007 (0.74)
Observations	26,758	26,758	26,758	26,758
Adjusted R-squared	0.968	0.970	0.986	0.988
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year

This table estimates Equation (1), using *IRS Exposure* (Panel A) or *ΔIRS Exposure High* (Panel B) as the independent variable of interest. *ΔIRS Exposure High* is an indicator variable equal to one for firm-years in the highest quintile of *ΔIRS Exposure* and zero otherwise. *ΔIRS Exposure* is measured as the change in *IRS Exposure* from year $t-1$ to year t . *Ln(Avg Tax Accountant)* and *Ln(Avg General Accountant)* are measured as the natural log of one plus the average *Tax Accountant (General Accountant)* for years t and $t+1$. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.

Table 10 Robustness

Panel A: Entropy Balancing

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2) <i>Ln(Avg General Accountant)</i>
<i>IRS Audit</i>	0.013** (2.53)	0.011** (2.50)
<i>Size</i>	0.142*** (8.00)	0.191*** (14.57)
<i>MNC</i>	0.005 (0.20)	0.048*** (2.59)
<i>MTB</i>	0.003 (0.72)	0.006* (1.79)
<i>ROA</i>	-0.077*** (-2.70)	-0.048* (-1.86)
<i>Leverage</i>	0.032 (0.79)	0.039 (0.94)
<i>Intangible Intensity</i>	0.077 (1.33)	0.050 (1.06)
<i>PPE</i>	0.007 (0.09)	0.057 (0.73)
<i>APTS</i>	0.165* (1.79)	0.254*** (3.75)
<i>Segments Unrelated</i>	0.019* (1.89)	0.003 (0.24)
Observations	17,636	17,636
Adjusted R-squared	0.982	0.995
Fixed Effects	Firm, Year	Firm, Year

Table 10 Robustness (continued)

Panel B: Additional Controls

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2) <i>Ln(Avg General Accountant)</i>
<i>IRS Audit</i>	0.011** (2.33)	0.012*** (2.98)
<i>Size</i>	0.105*** (10.21)	0.188*** (18.05)
<i>MNC</i>	0.006 (0.39)	0.038*** (2.95)
<i>MTB</i>	0.005* (1.91)	0.011*** (3.53)
<i>ROA</i>	-0.069*** (-3.69)	-0.063*** (-2.90)
<i>Leverage</i>	0.020 (0.72)	0.024 (0.89)
<i>Intangible Intensity</i>	0.101** (2.55)	0.048 (1.24)
<i>PPE</i>	0.009 (0.18)	0.025 (0.40)
<i>APTS</i>	0.196*** (3.66)	0.227*** (3.68)
<i>Segments Unrelated</i>	0.007 (0.79)	0.013 (1.58)
<i>Tax Loss</i>	0.015 (1.42)	0.006 (0.72)
<i>HS ETR</i>	-0.018 (-0.31)	0.107* (1.93)
<i>Inventory</i>	-0.163*** (-3.10)	-0.008 (-0.14)
<i>R&D</i>	-0.023 (-0.72)	-0.003 (-0.06)
<i>Capital Exp</i>	0.075 (1.05)	0.124* (1.77)
Observations	17,636	17,636
Adjusted R-squared	0.974	0.992
Fixed Effects	Firm, Year	Firm, Year

Table 10 Robustness (continued)

Panel C: Entropy Balancing with Additional Controls

VARIABLES	(1) <i>Ln(Avg Tax Accountant)</i>	(2) <i>Ln(Avg General Accountant)</i>
<i>IRS Audit</i>	0.012** (2.39)	0.011** (2.47)
<i>Size</i>	0.142*** (8.03)	0.191*** (14.53)
<i>MNC</i>	0.003 (0.12)	0.047** (2.57)
<i>MTB</i>	0.004 (0.80)	0.006* (1.76)
<i>ROA</i>	-0.096*** (-2.80)	-0.042 (-1.43)
<i>Leverage</i>	0.037 (0.92)	0.040 (0.97)
<i>Intangible Intensity</i>	0.068 (1.19)	0.051 (1.08)
<i>PPE</i>	-0.032 (-0.38)	0.025 (0.30)
<i>APTS</i>	0.145 (1.58)	0.244*** (3.62)
<i>Segments Unrelated</i>	0.018* (1.90)	0.004 (0.25)
<i>Tax Loss</i>	0.027* (1.94)	0.000 (0.04)
<i>HS ETR</i>	-0.128 (-1.19)	0.059 (0.90)
<i>Inventory</i>	-0.202*** (-2.85)	-0.048 (-0.77)
<i>R&D</i>	-0.094 (-1.34)	-0.015 (-0.28)
<i>Capital Exp</i>	0.134 (1.29)	0.155* (1.90)
Observations	17,636	17,636
Adjusted R-squared	0.982	0.995
Fixed Effects	Firm, Year	Firm, Year

This table estimates Equation (2). Panel A uses an entropy-balanced sample, Panel B includes additional controls, and Panel C uses an entropy-balanced sample with additional controls. All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.

Table 11 Falsification Test – Other Employees

VARIABLES	(1) <i>Ln(Avg Customer Service)</i>	(2) <i>Ln(Avg Total Employees)</i>
<i>IRS Audit</i>	0.002 (0.57)	0.004 (1.35)
<i>Size</i>	0.095*** (10.63)	0.201*** (17.27)
<i>MNC</i>	0.010 (0.89)	0.034*** (3.07)
<i>MTB</i>	0.012*** (4.94)	0.014*** (4.60)
<i>ROA</i>	-0.026* (-1.69)	-0.094*** (-4.31)
<i>Leverage</i>	-0.006 (-0.27)	-0.018 (-0.72)
<i>Intangible Intensity</i>	0.081** (2.36)	-0.037 (-0.90)
<i>PPE</i>	0.109*** (2.77)	0.145*** (3.43)
<i>APTS</i>	0.001** (2.30)	-0.000 (-0.56)
<i>Segments Unrelated</i>	0.003 (0.34)	0.002 (0.29)
Observations	17,636	17,636
Adjusted R-squared	0.994	0.997
Fixed Effects	Firm, Year	Firm, Year

This table presents the results from a falsification analysis. Columns 1 and 2 estimate Equation (2) with *Ln(Avg Customer Service)* (column 1) or *Ln(Avg Total Employees)* (column 2) as the dependent variable. *Ln(Avg Customer Service)* (*Ln(Avg Total Employees)*) is measured as the natural log of one plus the average *Customer Service* (*Total Employees*) for years t and $t+1$. *IRS Audit* and all control variables (unless otherwise indicated) are measured in year t . All variables are defined in Appendix A. All analyses include indicated fixed effects and cluster standard errors by firm. T-statistics are in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% significance level, respectively.