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Tax Planning Knowledge Diffusion via the Labor Market

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Tax Planning Knowledge Diffusion via the Labor Market

Abstract
We examine the extent to which the labor market facilitates the diffusion of tax planning knowledge across firms. Using a novel dataset of tax department employee movements between S&P 1500 firms, we find that firms experience an increase in their tax planning after hiring a tax employee from a tax aggressive firm. This finding is robust to various research designs and specifications. Consistent with tax planning knowledge driving this result, we find that the tax planning benefit of hiring an employee from a tax aggressive firm is stronger when the employee has more tax experience and is hired into a senior tax department role, and when the hiring firm likely had less tax planning knowledge prior to the hire. Further tests suggest that tax planning knowledge is highly specific in nature: the increase in tax avoidance is larger when the hiring and former firms are similar (i.e., operating in the same sector or having similar foreign operations), and firms are more likely to hire tax department employees from firms with similar characteristics. Our study documents the first-order role of the labor market in the diffusion of tax planning knowledge across firms, and suggests that tax department human capital is a central determinant of tax planning outcomes.

Keywords: tax planning, tax avoidance, tax departments, employee movements, human capital

JEL Classification Codes: H25, H26, J23, J24, J44

1. Introduction

Income tax payments to the government represent one of the most substantial expenses corporations face, making tax planning an essential issue for most firms. While it is widely known that firms engage in tax planning, how firms acquire the requisite knowledge for tax planning is less understood. Several studies have started to explore the diffusion of tax planning strategies across firms, examining intermediaries such as auditors, banks, and board members (Brown 2011; McGuire, Omer, and Wang 2012; Brown and Drake 2014; Gallemore, Gipper, and Maydew 2019).[1] However, the literature has yet to examine one of the most prominent economic features in the tax field—the labor market for tax department employees. In this paper, we shed light on the labor market's role in facilitating the exchange of tax planning knowledge amongst firms by examining tax employee movements between publicly-traded U.S. corporations.

Our focus on the labor market for tax department employees stems from two general insights. First, a literature in labor economics theorizes and documents that employee movements serve as an important mechanism for the diffusion of knowledge (Jaffe, Trajtenberg, and Henderson 1993; Szulanski 1996; Song, Almeida, and Wu 2003; Franco and Filson 2006; Jara-Figueroa, Jun, Glaeser, and Hidalgo 2018). This literature finds that firms rarely generate all the knowledge necessary for continuous technological improvement, and therefore must turn to external sources for this knowledge. However, the extent to which firms can source this external knowledge is determined, in part, by firm-specific capabilities and resources (Cohen and Levinthal 1990) and the nature of the knowledge (Zander and Kogut 1995). Moreover, the most valuable knowledge to a firm is tacit knowledge, which is characterized by its complexity (Winter 1987). Tacit knowledge is usually built internally through experience (Cohen and Levinthal 1990) or

[1] See Hanlon and Heitzman (2010) and Wilde and Wilson (2018) for reviews of the corporate tax avoidance literature. We use "tax planning" and "tax avoidance" interchangeably to refer to actions that reduce firms' taxes.

acquired through learning-by-doing (Teece 1982). As a result, a significant portion of this knowledge is embedded in individuals. When these individuals move between firms, they can apply this knowledge to new contexts, thereby effectively transferring the knowledge across firms (Argote and Ingram 2000). Thus, human mobility can play an essential role in the knowledge-building process of the hiring firm, especially when knowledge tends to be "sticky" and has specificity to the firm's environment (Jaffe et al. 1993; Szulanski 1996; Lazear 2009; Jara-Figueroa et al. 2018).

Second, we posit that knowledge acquired via working in a corporate tax department, especially one successful in tax planning, is likely of first-order importance and tacit in nature. Successful corporate tax departments take a holistic view of tax planning, balancing tax minimization with other firm goals, such as market share and cost efficiency, while complying with tax regulations and relevant tax authorities. Simultaneously managing these objectives may require a unique set of experiences and skills that are difficult to acquire outside of a corporate tax department.[2] Additionally, some of these skills may exhibit significant specificity; that is, they have greater usefulness in certain tax environments. For example, individuals working in the corporate tax department of a multinational firm build experience setting up global supply chains and employing transfer pricing to balance tax minimization with other firm objectives (i.e., performance evaluation), which may be more beneficial to other multinationals than purely domestic firms. Therefore, a significant amount of tax employees' human capital is likely generated by their training and work experience in corporate tax departments, suggesting that tax

[2] This experience is especially important given the complex and uncertain nature of corporate tax planning (Guenther, Matsunaga, and Williams 2017; Dyreng, Hanlon, and Maydew 2019; Hoppe, Schanz, Sturm, and Sureth 2019). We do not claim that individuals cannot gain some of these skills outside corporate tax departments (e.g., in a public accounting firm). Instead, we posit that experience in corporate tax departments is unique because it requires managing a number of different objectives and interactions with multiple parties within the firm.

planning knowledge is tacit in nature. However, research on the role of individuals in corporate tax planning is relatively scarce, and generally focuses on top-level executives (Dyreng, Hanlon, and Maydew 2010; Koester, Shevlin, and Wangerin 2017), the tax department director (Armstrong, Blouin, and Larcker 2012; Klassen, Lisowsky, and Mescall 2016), and other types of tax employees (Jiang, Robinson, and Wang 2020), as opposed to tax department employees more generally. Furthermore, while these studies suggest the potential for individuals to matter with regards to the firm's tax planning outcomes, they do not directly examine the role of corporate tax department experience nor the diffusion of tax planning knowledge between firms.

These two motivating insights suggest that (a) individuals can matter in the diffusion of tacit knowledge and (b) employees can gain tacit tax planning knowledge while working in a corporate tax department. A natural question that arises from these two insights is: does tax employee mobility serve as a mechanism for the acquisition of externally developed tax planning knowledge, and specifically the knowledge gained while working in the tax department of tax aggressive firms?

To examine this question, we construct a novel dataset of tax department employee movements between publicly-traded U.S. corporations constructed from resumes posted to a major professional networking website. We use these data to explore the role of labor markets in the diffusion of tax planning knowledge by examining the association between the hiring of a tax employee who previously worked at a tax aggressive firm (which we define as a firm that maintained a low long-run cash effective tax rate prior to the employee’s departure) and changes in the hiring firm's tax planning outcomes using a difference-in-differences research design. We find that when a firm hires a tax department employee from a tax aggressive firm, it experiences an increase in tax planning: a decline in the cash effective tax rate (ETR) of approximately two

percentage points on average. We find that the pre-treatment trends in tax planning outcomes do not differ between firms that hire from tax aggressive firms and other firms, supporting the parallel trends assumption underlying the difference-in-differences design. Furthermore, this relation is robust to adjusting for the potential biases in difference-in-differences estimates from implementing a two-way fixed effect regression in a staggered setting (Barrios 2021). This evidence is broadly consistent with employee movements being a mechanism through which tax planning knowledge spreads across firms.

One concern with our primary design is that firms that hire tax department employees are systematically different from those that do not hire. To abstract from the hiring decision reflecting other unobserved confounders that may drive both hiring and changes in the firm's tax planning outcomes, we further refine our analysis by comparing tax planning changes only amongst hiring firms. Using this sample of employee movements, we continue to find that hiring a tax employee from a tax aggressive firm is associated with a materially important increase in the hiring firm’s tax planning compared to firms hiring from non-tax aggressive firms.

We conduct a number of additional tests to confirm that the transfer of tax knowledge through employee movements, rather than alternative explanations, drives our observed relation between hiring aggressive tax employees and tax planning outcomes. We find that the increase in tax planning that occurs after the hiring of a tax department employee from a tax aggressive firm is robust to accounting for the number of tax department hires, various alternative firm events (such as a merger or CEO turnover), or other inter-firm connections (i.e., auditor, bank, and board member overlaps). Furthermore, using a placebo test where we examine tax department hires from firms with high non-tax planning performance (using measures such as return on assets or foreign income), we fail to find that these hires explain changes in the hiring firm's tax planning. We also

fail to find that hiring from a tax aggressive firm is associated with changes in non-tax planning outcomes, such as pre-tax performance or capital investment. These results further support the idea that our main results are driven by the acquisition of tax-specific knowledge via the labor market rather than reflecting some other firm characteristic or contemporaneous event.

In our next set of analyses, we explore whether the tax planning benefit of hiring a tax employee from a tax aggressive firm varies in ways that are consistent with the employee's experience driving the diffusion of tax planning knowledge across firms. First, we predict that the benefit of hiring an employee from a tax aggressive firm will be stronger when hiring an employee who had more responsibility for the prior firm's tax strategy or will have more responsibility over the hiring firm's tax planning. Furthermore, we predict that employees with more tax experience overall, and more tax experience at the prior tax aggressive firm, will have gained greater tax planning knowledge to apply at the hiring firm. Consistent with these predictions, the tax planning increase associated with hiring from tax aggressive firms is generally larger when the employee hired held a director-level at the prior firm (and thus had played a significant role in defining the previous aggressive firm's tax strategy), and when the employee is hired into a director-level position at the new firm (and is thus likely plays a significant role in the hiring firm's tax planning). Furthermore, the tax planning increase associated with a tax aggressive hire is larger when the individual has more tax experience overall and specifically at the prior firm.

Second, we explore whether our documented findings are stronger when the firm likely lacked tax planning knowledge before the hire. We find that the tax planning benefit of hiring from a tax aggressive firm is larger for smaller hiring firms and for firms that do not engage their auditor for tax services. These results are consistent with the impact of tax planning knowledge diffusion mattering most when hiring firms likely lacked the tax knowledge prior to the hire—either because

the firm likely had a smaller tax department or because they were not receiving the tax planning knowledge from their auditor.

Third, we examine the role of specificity in tax planning knowledge. Becker (1962) proposes a distinction between two types of human capital: general human capital, which helps productivity at all firms (regardless of their type), and firm-specific human capital, which raises productivity at the current firm or similar firms, not elsewhere.[3] The concept of firm-specific human capital has been generalized to specific skills in a given industry or sector of the economy (Kletzer 1993; Neal 1995; Kim 1998). To the extent that the knowledge gained through on-the-job experience is specific in nature, firm performance may not simply be driven by the level of an employee's human capital, but rather its specificity, and therefore applicability, to the firm and its circumstances.[4]

We posit that tax planning knowledge gained working in a corporate tax department is similar to the notion of firm-specific human capital. While the typical tax department employee will have general tax knowledge (i.e., an understanding of the tax code), tacit tax planning knowledge is generated by combining this general knowledge with the specifics of the firm's business model and operating environment. This tacit tax planning knowledge will be specific to the circumstances in which it was developed. Thus, for a hiring firm to capitalize on this tax planning knowledge, its tax environment needs to be similar to that of the prior firm. In contrast, if tax planning knowledge is primarily general in nature, the benefit of the knowledge should not vary depending on the similarity between the hiring and prior firm. Consistent with the transfer of

[3] An extensive literature in economics has documented the importance of human capital as a critical determinant of economic growth and firm productivity (Nelson and Phelps 1966; Rauch 1993; Gennaioli, La Porta, Lopez-de-Silanes, and Shleifer 2013).

[4] Consistent with this idea, work experience, which is specific to the firm, location, and occupation, has been shown to affect the firm's performance (Jara-Figueroa et al. 2018).

tacit tax planning knowledge being responsible for our main findings, we find that the tax planning benefit of hiring an employee from a tax aggressive firm is stronger when the hiring and prior firms operate in the same sector or are similar in their global operations (i.e., primarily multinational or domestic).

In our final set of analyses, we examine where firms source their tax department employees. Since our prior findings suggest that tax planning knowledge is specific in nature, we predict that firms are more likely to acquire tax department employees from firms that face similar tax environments. Consistent with this prediction, we find S&P 1500 firms tend to hire from other S&P 1500 firms within close geographical proximity, in the same sector, and having similar multinational operations. This analysis allows us to speak to the recent debate on tax department outsourcing, which has attracted significant attention since General Electric spun off its tax department to PriceWaterhouseCoopers in 2017. Given that our findings suggest that tax planning knowledge exhibits specificity, this could limit the possibility of outsourcing the tax department for firms facing unique tax environments. Moreover, to the extent that there is some outsourcing, the public accounting firms' tax practices may have to specialize along specific dimensions, such as by sector or geography, in order to benefit from the transfers of tax planning knowledge.[5]

Our study makes several contributions to the accounting and tax literatures. First, we add to an emerging literature in accounting that examines the interaction between labor and accounting (Call, Campbell, Dhaliwal, and Moon Jr. 2017; Hoopes, Merkley, Pacelli, and Schroeder 2018; Choi, Gipper, and Malik 2019; Pacelli 2019; Barrios 2020; Merkley, Michaely, and Pacelli 2020). We extend this research by studying the role of the labor market in the diffusion of corporate tax

[5] We are unable to directly study the effect of outsourcing on tax planning for several reasons. First, we are unaware of databases that track the outsourcing of corporate tax functions. Second, this phenomenon is relatively new, and our data collection was primarily conducted before it began. Instead, we view our study as having implications for this trend, and we look forward to future research exploring tax department outsourcing more directly.

planning knowledge across firms, further demonstrating the potential that insights from labor economics can have in improving our understanding of accounting phenomena. Moreover, by exploiting employee movements, we establish a novel methodological approach that future research can implement to further explore the interactions between the labor market and other accounting outcomes.

We also make several contributions to the literature on corporate tax planning. First, we add to the literature examining how tax planning knowledge spreads across firms, which has explored the role of intermediaries such as board interlocks and banks in the diffusion of tax strategies amongst firms (Brown 2011; Brown and Drake 2014; Gallemore et al. 2019), by exploring the role of the labor market for the actual individuals in charge of developing and implementing tax strategies. Second, we contribute to the literature on the role of individuals in corporate tax avoidance, which has previously investigated specific individuals, such as firm executives (Dyreng et al. 2010) and certain tax department employees (Armstrong et al. 2012; Klassen et al. 2016; Koester et al. 2017; Ege, Hepfer, and Robinson 2020; Jiang et al. 2020; Chen, Cheng, Chow, and Liu 2020).[6] While these studies collectively suggest that employees can shape tax avoidance, they generally examine factors such as the employee's compensation or rank within the executive team. In contrast, we focus on the experience of tax department employees, which represents an important part of their tax planning knowledge, and how the movements of such employees serve as a mechanism to diffuse tax knowledge between firms. A contemporary study related to ours is Jiang et al. (2020), who examine hiring former IRS employees. However, their evidence suggests that hiring from the IRS does not affect the level of tax planning on average.

[6] Our study is also related to research that examines internal firm characteristics, such as expenditures on tax resources and internal information quality, and tax planning. Examples of such studies include Mills, Erickson, and Maydew (1998); Robinson, Sikes, and Weaver (2010); Gallemore and Labro (2015).

The difference between their findings and ours suggests that successful tax planning requires, in part, tacit knowledge, which is best developed via experience within a corporate tax department.

Finally, we contribute to the literature in personnel economics examining the relation between investments in human capital and organizational performance (Stiles and Kulvisaechana 2003). Prior studies have generally focused on overall firm outputs (Delaney and Huselid 1996); however, these studies suffer from a weak connection between human capital and unit performance given differing objectives amongst the firm's units (Becker and Gerhart 1996). By focusing on tax employees and tax outcomes, we provide insight into the role of business unit human capital and its closely linked outcomes.

2. Research design

2.1. Identifying employee movements

To conduct our analyses, we exploit data from resumes posted to LinkedIn, a prominent professional networking website, to construct a sample of employee movements between corporate tax departments. To build the sample, we begin by creating a directory of firms included in the S&P 1500.[7] We focus on S&P 1500 firms for several reasons. First, their SEC reporting requirements allow us to obtain data on firm characteristics and financial performance (including tax planning measures) that would be challenging to observe for private firms. Second, these firms are publicly traded and therefore relatively large, increasing the likelihood of having both (1) a tax department and (2) tax department employees posting profiles to the networking website. Third, the S&P 1500 index represents a diverse set of firms from different industries, which allows us to explore specific questions, such as whether the benefit of obtaining tax planning knowledge is stronger when the hiring and prior firms face similar tax environments (i.e., operate in the same

[7] We include any firm that was an S&P 1500 member between 1993 (to coincide with SFAS No. 109, which ensures consistent accounting for income taxes) and 2015 (as our data collection efforts occurred over 2016 and 2017).

sector). Fourth, significant academic, media, and political attention has been paid to these firms' tax planning, suggesting that examining these firms is likely of broad interest.[8]

Using the networking website, we identified individuals at each firm who either currently or previously worked in a tax position.[9] We classify an employee as being in the tax department if their profile contains a position with the word "tax" in the title.[10] For each tax department employee, we identify different job positions from career histories, including the company name, position title, start date, and end date. We determine the positions' chronological order using their arrangement on the profile and their beginning and end dates. We exclude individuals for which we cannot identify start and end years for their various positions. Next, we employ textual analysis algorithms to match the company names in these resumes to the S&P 1500 firms' names in the CRSP-Compustat merged database.[11] To minimize instances in which we incorrectly match an employee to a firm, we require either an exact match between the company names or a sufficiently high textual similarity score. While this minimizes the number of incorrect matches, it comes at the cost of potentially missing "good" matches.[12]

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[8] Importantly, limiting our search to S&P 1500 member firms makes our data collection effort more tractable. The downside to this requirement is that we are unable to speak to the impact of labor markets on the dissemination of tax planning knowledge in non-S&P 1500 member firms.

[9] Our search process employed simple variations of the firm's name. If the employee worked in the tax department of a subsidiary firm, our search will capture these employees to the extent that one of the following conditions is satisfied: (1) the employee listed the parent company's name in their resume or (2) the employee listed the subsidiary's name, which contains a variation of the firm's name. We are relatively confident that our search process will captures most of the tax employees who work at the firm and have listed their resume on the website, especially because the tax department is located at the corporate headquarters for most firms.

[10] Examples of these titles include "Tax Analyst," "Tax Associate," "Tax Manager," "Senior Tax Lawyer," "Tax Attorney," "Global Tax Manager" and "Chief Tax Counsel." We also collected profiles that included the word "tax" in the job description. However, a manual inspection of the data indicated that many profiles containing the word "tax" in the job description were unlikely to be in the corporate tax department. In contrast, nearly all of the titles that included the word "tax" were likely corporate tax department positions. Therefore, we only classify an individual as being in the tax department when their title includes the word "tax."

[11] This step is necessary because (1) resumes can misspell company names, which can be addressed using our approach and (2) individuals can work at multiple S&P 1500 firms, which we exploit in our empirical strategy.

[12] We first standardize company names to remove common words and abbreviations (i.e., "Inc."). Next, we identify for exact matches between the firm name in the resume and in the CRSP-Compustat database, conditioning on the dates of the position in the resume since firm names can change over time. Finally, with the remaining unmatched observations, we use the "token_sort_ratio" option in Python's "FuzzyWuzzy" package to generate

To construct our S&P 1500 tax department employee movements sample, we require individuals to meet several criteria. First, they need to have been employed by the originating (i.e., prior) S&P 1500 firm for at least one full year.[13] Second, they need to then move to another sample firm and remain there for at least one year, and the time between jobs must be no greater than twelve months. We focus on employees with general tax responsibilities (i.e., not specific non-income tax responsibilities) in both positions, as these employees are more likely to be involved in income tax planning. Furthermore, to classify prior firms based on their tax planning, we require the prior firm to have a non-missing 5-year cash ETR (measured in the employee's final year). Given these data requirements, we identify 1,083 employee movements during the period from 1998 to 2015.[14]

We present some descriptive evidence on these movements in Figure 1. In Panel A, we plot the number of movements in our sample across time. We are able to capture more movements in later years, which is a function of the LinkedIn website being more likely to capture individuals (and thus employee movements) over time. We account for this trend in our tests by utilizing year fixed effects. In Panel B, we present the percentage of firms in a sector with at least one hire from an S&P 1500 firm during our sample period (using the Fama-French 17 sector definition for brevity). The sectors with the largest percentage of firms with at least one hire are the machinery, fabricated products, oil and petroleum, and food; each sector has more than 20 percent of firms

text string similarity scores and retain matches with a score of 99 or 100. More details on the firm name cleaning and matching process are available from the authors upon request.

[13] Specifically, we require at least one full year between their beginning and start years to ensure that they spent at least one full year at the firm. This is because some profiles do not indicate which month a position began or ended, meaning that a position that began in one year and ended in the next year may have lasted less than a year.

[14] Because we are unable to observe employees who do not voluntarily list their resume, we likely undercount the actual number of employee movements. However, we believe that mobile employees are more likely to use the website, suggesting that our ability to capture employee movements is reasonable. To the extent that we fail to capture employee movements, this likely weakens our ability to find associations in our tests.

with at least one hire. On the other end, we find only one sector (steelworks) with less than 10 percent of firms with at least one tax department hire from an S&P 1500 firm.

2.2. Regression and sample

To examine whether hiring an employee from a tax aggressive firm leads to increases in tax planning, we implement a difference-in-differences design by estimating the following regression using a firm-year panel for S&P 1500 members over the 1995 to 2018 period:[15]

$$Cash\ ETR_{i,t} = \beta_1 Hire\ from\ TA\ Firm_i + Controls_{i,t} + Fixed\ Effects + \varepsilon_{i,t.} \qquad (1)$$

Following prior research (Dyreng, Hanlon, and Maydew 2008), we measure tax planning with the cash effective tax rate. Specifically, we employ *Cash ETR*, the hiring firm's cash effective tax rate (cash taxes paid scaled by pre-tax income minus special items), as the dependent variable. *Hire from TA Firm* is an indicator variable equal to one in the three years (inclusive of the hiring year) after the firm first hires an employee from a tax aggressive S&P 1500 firm, and zero otherwise. We classify a prior firm as tax aggressive if it has a five-year cash ETR in the bottom quartile of the sample of movements. We measure the prior firm's tax avoidance over a five-year window in order to capture a low ETR that is likely the result of deliberate and sustainable tax planning efforts, rather than unsustainable tax avoidance or transitory shocks.[16] On the other hand, we measure the hiring firm's tax planning using a one-year ETR to detect any shift in tax avoidance that occurs around the hiring of the new employee. We winsorize cash ETRs at 0 and 1, and set cash ETRs equal to missing if the denominator is negative, consistent with prior research (Dyreng

[15] We employ this period to allow us to observe firms several years before the first sample movements and several years after the final sample movements.

[16] Untabulated results show that the average five-year cash ETR for the prior firms designated as tax aggressive firms is 9 percent, consistent with these firms maintaining low cash ETRs over a long period. We intentionally define the quartile classification across the entire sample of movements to reflect the fact that tax planning can vary across time and certain firm characteristics (i.e., sector). We account for the potential impact of macroeconomic and sector-level factors on our findings via fixed effects, examine the robustness of our findings to within-year and within-sector definitions in section 3.2, and conduct placebo tests using non-ETR characteristics of the prior firm in section 3.6.

et al. 2008). We focus on the three-year period after the hiring to mitigate future events unrelated to the hiring affecting our inferences. The coefficient of interest is β_1, which captures the average change in the *Cash ETR* after hiring an employee from a tax aggressive firm, relative to other sample firms. We predict that this coefficient will be negative, consistent with an increase in tax planning after sourcing tax planning knowledge via the labor market.

To account for potential alternative explanations, we include a vector of time-varying firm-level controls that prior research has shown to be associated with firm tax planning outcomes: *ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change* (all defined in the appendix). Furthermore, we include sector (or firm) and year fixed effects to account for time-invariant sector-level (or firm-level) characteristics and macroeconomic conditions, respectively.

This design compares tax planning by firms hiring an employee from a tax aggressive firm to other firms. However, a firm hiring a tax department employee may be undergoing several other changes, and it might be those changes, rather than the employee's tax planning knowledge, that leads to an observed change in tax planning after the hire. Put differently, firms that hire tax department employees may systematically differ from those that do not hire. To mitigate this concern, in later analyses, we focus on our sample of employee movements and examine whether firms that hire tax department employees from tax aggressive firms experience differential changes in their tax planning relative to firms that hire tax department employees from non-tax aggressive firms. Specifically, we examine a six-year window around each employee's hiring, with three years pre-movement and three years post-movement (inclusive of the hiring year). Using a six-year window balances two competing factors: having a window that is (1) sufficiently long enough to observe the changes in tax planning that occur around the hiring and (2) sufficiently short

enough to minimize the chance that other non-hiring factors impact the firm's tax planning. This design is akin to the stacked event-by-event design suggested by Barrios (2021) and it mitigates the potential issues with a two-way fixed effect regression implemented in a staggered setting.

Using this window around each employee movements, we estimate a modified version of equation 1 that replaces *Hire from TA Firm* with *Hire from TA Firm x Post*. In this model, *Hire from TA Firm* is a time-invariant, movement-level indicator variable equal to one if the movement involves hiring from a firm with a five-year cash ETR in the bottom quartile of the sample of movements. *Post* equals one in the year that the employee joins the hiring firm and the following two years. The coefficient on the interaction of *Hire from TA Firm* and *Post* represents the average change in the cash ETR upon hiring an employee from a tax aggressive firm.

In this design, we include various fixed effects and control variables to account for alternative explanations. First, we include movement fixed effects; these fixed effects help account for time-invariant factors (within our window) that both influence the matching between employees or firms and the hiring firm's tax planning outcomes. Second, we include event-time fixed effects (i.e., fixed effects for each year in the window) to account for any tax planning trends that occur around hiring a tax department employee regardless of the former firm's tax planning outcomes.[17] Third, we include sector-year fixed effects to account for industry-level shocks (where sector membership is defined at the two-digit SIC code level) that impact tax planning outcomes. Finally, we include all of the time-varying firm-level controls defined above.

2.3. Other data sources and sample requirements

We employ financial accounting data from Compustat and equity data from CRSP.[18] We

[17] The movement fixed effect subsumes the coefficient for *Hire from TA Firm*, and the event-time fixed effect subsumes the coefficient for *Post*.

[18] Following Dyreng and Lindsey (2009), we set the following Compustat variables to zero if they are missing: advertising expense, R&D expense, tax loss carryforwards, intangible assets, special items, and long-term debt.

require firm-years to have a non-missing value for *Cash ETR* and each control variable to be included in the sample. When creating the movements-only sample, we require that each movement have at least one pre-movement and one post-movement observation to be included. To ensure all firms face the same federal tax environment, we require that the firm be incorporated in the United States. Furthermore, we follow prior tax research and exclude financial firms (those with one-digit SIC codes of 6) because of their unique operating and regulatory environments. We present statistics for the main variables employed in our movements-only analyses in Table 1.[19]

3. Results

3.1. Panel sample

We present the results of estimating equation 1 using the panel sample in Table 2. We employ several different models: no controls (col. 1), sector and year fixed effects (col. 2) firm and year fixed effects (col. 3), and firm and year fixed effects and time-varying firm-level control variables (col. 4). In each model, we document a significant negative coefficient on *Hire from TA Firm*.[20] The coefficient estimate in column 4 suggests that hiring a tax department employee from a tax aggressive firm is associated with a reduction in the cash ETR of approximately 2 percentage points, or 7 percent of the sample mean (25 percent in the panel sample).[21]

Additionally, we employ their method to correct for errors in foreign tax expense, foreign and domestic pre-tax income, total pre-tax income, federal current tax expense, and worldwide current tax expense.

[19] We winsorize non-ETR continuous variables at the 1st and 99th percentiles.

[20] While we employ a five-year cash ETR measure for our primary analyses because it captures sustained tax avoidance, inferences are qualitatively similar if we instead employ shorter-run ETRs (i.e., one-year or three-year; results untabulated).

[21] In untabulated tests, we expand the window over which we define *Post* (i.e., 4 years, 5 years, etc.). Our inferences are qualitatively similar (i.e., declines in the hiring firm's cash ETR), but the effect attenuates as we increase the *Post* window. This decline in the hiring effect over time is not surprising for two reasons: (1) the potential for alternative events and changes in the hiring firm's business model to confound our ability to detect changes in cash ETRs and (2) potential responses by the control firms that can lower their cash ETRs, including accessing alternative sources of tax planning expertise. In short, this decline in the treatment effect over time is consistent with diffusion of tax planning knowledge via the labor market providing a short-run advantage over other firms.

As with any difference-in-differences design, our approach only identifies the effect of hiring from a tax aggressive firm on a firm's tax planning under the assumption that the tax planning for the hiring and non-hiring firms would have been similar in the absence of a hiring. Although this assumption cannot be tested directly, we assess its plausibility by using an event study design to examine the pre-existing trends in firms' cash ETR. Specifically, we estimate a simple difference-in-differences model (similar to equation 1), where we saturate the model with event-time indicator variables interacted with *Hire from TA Firm*. We plot the coefficients on the interactions of *Hire from TA Firm* and the following time periods: year t-3, year t-2, year t (the hire year), year t+1, and year t+2. This design effectively documents the differences in *Cash ETR* changes between firms that hire from tax aggressive firms and other firms around the hiring, using the year prior to the hire (year t-1) as the base period.

We plot the coefficients of these event-year variables in Figure 1.[22] There are several findings to note. First, prior to the hire, firms hiring from a tax aggressive firm experience similar trends in their cash ETRs relative to other firms. The lack of substantial differences in pre-treatment trends between the two groups provides support for the parallel trends assumption. Second, while we find a decline in the cash ETR in the employee's first year at the hiring firm (year t), it is relatively small and statistically insignificant. The decline becomes significant in both an economic and statistical sense in employee's second year at the hiring firm (year t+1). This finding is consistent with the idea that an employee's ability to affect the firm's tax planning outcomes materially requires the employee to have been at the firm for a sufficient amount of time.

We conduct three additional robustness tests of these findings. In column 5, we re-estimate equation 1 only within firms that hire at least one tax department employee from another firm

[22] In Figure 1, we have included (but do not report) year and sector fixed effects. We find similar inferences if we also include the time-varying firm-level control variables.

during our sample period. In column 6, we include sector-year fixed effects to account for sector-specific shocks that could impact tax department hiring and tax planning outcomes. In each column, we continue to find a negative and significant coefficient on *Hire from TA Firm*. Finally, we address the concern from Barrios (2021) about heterogeneous treatment effects and negative weighting when implementing a difference-in-differences design with staggered treatments by implementing a stacked event-by-event design to estimate the average effect of an aggressive hire (similar to the approach in Cengiz, Dube, Lindner, and Zipperer (2019)). We report these findings in columns 7 and 8. We find that the stacked design estimate is very similar to our panel regression design, corroborating that our results are unlikely to be driven by the negative weighting issues present when implementing a two-way fixed effects regression in a staggered setting.

3.2. Movements sample

Next, we build on the stacked design and implement equation 1 using the movements-only sample. As discussed above, the benefit to this approach is that we hold constant the fact that the firm is hiring an employee into its tax department (i.e., we only explore hiring events), and examine whether the changes in hiring firm's tax planning depend on whether the employee is hired from a tax aggressive firm or not. Furthermore, we focus on a relatively short window around the hiring events to mitigate the likelihood that other events affect the hiring firm's tax planning. We employ this sample and approach for our remaining analyses.

We present the results of estimating equation 1 using the movements sample in column 1 of Table 3. We continue to find a significant negative coefficient on *Hire from TA Firm x Post*. In terms of economic significance, these findings are very similar to those in Table 2: hiring an employee from a tax aggressive firm is associated with a 2-percentage point reduction in *Cash ETR* on average, or 9 percent of the mean *Cash ETR* in the movements-only sample.

We conduct several robustness tests of this finding. First, to further address the non-random nature of employee hiring, and explicitly the fact that firms hiring employees from tax aggressive firms may differ systematically from those hiring from other firms, we employ entropy balancing. Specifically, we create weights for each observation such that the means and variances of the time-varying control variables are similar across movements where *Hire from TA Firm* equals one and where it equals zero, and employ these weights in our regression. We report this finding in column 2 and find that the interaction coefficient is slightly larger in magnitude. We also repeat the entropy balancing only using the year before the employee arrived at the new firm and continue to find similar results (column 3).[23] Next, we employ different thresholds for defining a tax aggressive prior firm: the bottom 20 percent (col. 4) and the bottom 30 percent (col. 5) and find that the coefficient on the interaction remains significant and close in value to that of column 1.[24]

3.3. Robustness: Contemporaneous firm actions

In our next set of analyses, we attempt to mitigate concerns that the increase in the hiring firm's tax planning is attributable to factors other than its exposure to the hired employee's tax planning knowledge developed while working at the prior tax aggressive firm. Specifically, we examine whether our findings are driven by other firm actions, firm executives outside of the tax department, other connections between firms, or other characteristics of the prior firm.

First, we examine whether the number of tax department hires, rather than their prior experience at a tax aggressive firm, is responsible for our findings. Chen et al. (2020) find that larger tax departments (measured using the number of tax department employees) are associated with greater tax avoidance. To examine whether the number of hires is driving our findings, we

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[23] Since we drop observations with missing cash ETRs, entropy balance weights cannot be calculated for a movement that does not have a year t-1 observation, leading to some sample attrition for this analysis.

[24] In untabulated analyses, we find qualitatively similar inferences if we define *Hire from TA* Firm using the bottom quintile within each movement year or the bottom quintile within each former firm's SIC2 sector.

create a count variable that captures the number of hires being made by the tax department in the year in which the employee starts at the new firm and interact this variable with *Post*. We report this finding in column 1 of Table 4, Panel A. We do not find that the number of employees is associated with the post-hiring tax planning, and we continue to find a negative and significant coefficient on *Hire from TA Firm x Post*.

Next, we examine whether mergers and acquisitions are responsible for our findings. An employee movement could be driven by a merger between the new and former firms, or be driven by compliance needs resulting from a merger between two firms. Furthermore, prior research suggests that tax avoidance may be one of the value drivers of mergers (Belz, Robinson, Ruf, and Steffens 2013; Devos, Kadapakkam, and Krishnamurthy 2009). To mitigate this alternative explanation, we re-estimate equation 1, including the interaction of *Large Merger* and *Post*, where *Large Merger* is an indicator variable equal to one if the hiring firm's acquisition-related cash flows are greater than 10 percent to lagged assets in either the year the employee is hired or in the year prior. We report these findings in column 2 of Table 4, Panel A. In short, our findings do not appear to be driven by mergers: the coefficient on *Large Merger x Post* is insignificant, and we continue to find a significant negative coefficient on *Hire from TA Firm x Post*.

3.4. Robustness: Executives

Next, we explore whether our findings are driven by firm-level executives, rather than tax department employees. Dyreng et al. (2008) find that executive fixed effects explain variation in cash ETRs, and argue that this is consistent with executives having a meaningful effect on corporate tax planning. However, the mechanism through which executives affect tax planning is not clear; Dyreng et al. (2010) examine several possibilities but find little support for any given explanation. We posit that a potential mechanism for the Dyreng et al. (2010) finding could be that

executives set the "tone at the top" for greater tax planning, leading the firm to access the labor market to acquire tax planning knowledge through hiring.

We conduct two tests to explore whether our findings are a manifestation of the impact of executives on tax planning. First, we examine whether our findings are robust to controlling for contemporaneous CEO turnovers. To implement this test, we obtain data on executive turnovers from Execucomp, and create the interaction of the indicator variable *CEO Turnover*, which equals one if the firm's CEO departing in either year t (the year the new tax department employee joins the firm) or year t-1, and *Post*. We re-estimate equation 1 with the inclusion of this interaction term, and report these findings in column 3 of Table 4, Panel A. We do not find that CEO turnovers are systematically related to changes in tax planning, and we continue to find that firms experience declines in their cash ETRs after hiring employees from tax aggressive firms.

Next, we recreate our design using executive movements between firms. We focus on CEOs and CFOs in the analyses: the former because of their ability to set the "tone at the top" and the latter because they are the C-suite executive most directly responsible for the tax department. Using Execucomp and our S&P 1500 sample, we identify 249 movements during our sample period that involved either hiring the former firm's CEO or CFO, or hiring a former firm executive into the hiring firm's CEO or CFO position. We create six-year windows around each of these movements and estimate a modified version of equation 1 that replaces *Hire from TA Firm* with *Hire Exec From TA Firm x Post*. We define *Hire Exec from TA Firm* as an indicator variable that equals one if the executive moves from a firm with a five-year cash ETR in the bottom quartile of the sample of movements.[25] We report these findings in Table 4, Panel B. In contrast to the findings examining tax department employees, we find no evidence that hiring an executive from a tax

[25] The average former firm's five-year cash ETR for movements defined as *Hire Exec from TA Firm* is 9 percent, similar to the average for our tax department movements.

aggressive firms are associated with increases in tax planning for the hiring firm: the coefficient on *Hire Exec from TA Firm x Post* is statistically insignificant (and is in fact positive).[26]

Overall, the findings from these two analyses suggest that our results are not driven by executive turnovers or hiring executives from tax aggressive firms, and therefore are distinct from the findings in prior research (e.g., Dyreng et al. (2010); Koester et al. (2017)). Furthermore, while our analyses are not intended to be a thorough test of whether the hiring of tax department employees from tax aggressive firms is the mechanism responsible for the findings in Dyreng et al. (2010), our findings suggest that this is not the case.

3.5. Robustness: Alternative connections

Prior research has documented that other connections between firms can facilitate the diffusion of tax planning strategies. For example, Brown (2011) and Brown and Drake (2014) find that common board ties between firms can lead to increased tax avoidance and the spread of specific tax planning strategies. Gallemore et al. (2019) document that certain banks are associated with increases in tax planning in new borrowers, consistent with banks facilitating tax planning via the spread of such strategies across borrowers. McGuire et al. (2012) find that industry specialist auditors facilitate tax avoidance. If these alternative connections facilitate employee movements between firms, our findings may simply reflect the fact that these connections enable the spread of tax planning information.

To examine whether alternative connections between our sample firms are responsible for our findings, we re-estimate equation 1, including an additional variable: *Alternative Connection x Post*. We define *Alternative Connection* as an indicator variable equal to one if the former and

[26] The reported analysis pools movements involving the former firm's CEO or CFO and movements involving the hiring firm's CEO or CFO to increase the sample size. In untabulated analyses, we find that our inferences are unchanged if we separately examine a specific combination (i.e., former firm CEOs, hiring firm CFOs, etc.).

hiring firms share a connection in the year of the hire, and zero otherwise. We employ three versions of *Alternative Connection* based on whether the connection is via a board member (identified using BoardEx), a lead bank lender (identified using Dealscan), or an auditor (identified using Audit Analytics). We report these analyses in columns 4 through 6 of Table 4, Panel A. Regardless of the connection (board, bank, or auditor), we do not find that our findings appear to be driven by these connections: the coefficient on *Hire from TA Firm x Post* continues to be negative and statistically significant, whereas the coefficient on *Alternative Connection x Post* is insignificant. Therefore, our findings represent a connection between firms that is distinct from those examined in the prior literature.

3.6. Placebo: Hiring from firms with high non-tax-planning performance

Next, we explore whether our results are being driven by employees hired from firms because of other non-tax planning characteristics of the prior firm, such as financial performance, which are correlated with the prior firm's long-run cash ETR. To address this concern, we create an additional indicator variable, *Hire from High Perf. Firm*, which equals one if the prior firm is in the top quartile of the sample of movements (similar to our *Hire from TA Firm* measure) along certain dimensions, and interact this variable with *Post*. We employ two variables that exhibit strong negative associations with cash ETRs in our sample to measure financial performance: *ROA* and *FI*. Examining *FI* also allows us to account for the possibility that firms seek tax department employees with cross-jurisdictional tax experience. We also employ the prior firm's *R&D*, since firms may seek individuals with tax experience related to intangibles and/or transfer pricing, which in turn could be correlated with the prior firm's tax planning outcomes. We measure these for the former firm over the same period over which we measure the long-run cash ETR (five years). We report the results in Panel C of Table 4; for each alternative performance measure, we include it

by itself first and then in addition to *Hire from TA Firm x Post*. Across all six regressions, we find consistent results: the coefficient on *Hire from High Perf. Firm x Post* is statistically insignificant and close to zero, whereas the coefficient on *Hire from TA Firm* continues to be negative and statistically significant. We conclude that our primary findings appear to be specific to hiring employees from firms with low long-run cash ETRs, and not driven by hiring from firms with high long-run financial performance or other non-tax characteristics.

3.7. Placebo: Non-tax-planning-related firm outcomes

In our final set of robustness tests, we examine whether we observe changes in non-tax firm performance or other outcomes in response to hiring tax department employees from tax aggressive firms. Such a finding could signal that a larger change in firm operations might be the driving factor behind both the employee hiring and the observed increase in tax avoidance. We examine three alternative firm outcomes: *ROA* (pre-tax income scaled by total assets), *R&D* (research and development expense, scaled by total assets), and *CapEx* (capital expenditures, scaled by total assets). We re-estimate equation 1 using these variables as the dependent variable in place of *Cash ETR*. We report these findings in Panel D of Table 4. In each regression, we find no evidence of an association between hiring a tax employee from a tax aggressive firm and changes in these variables, suggesting that our primary findings are attributable to the employee's impact on tax planning, rather than evidence of a broader change in the company's operations.

4. Mechanism analyses

Our next set of analyses explores whether and how the effect of sourcing tax planning knowledge via the labor market varies across employee and firm characteristics. The objective of these analyses is threefold. First, they allow us to explore whether the employee's tax planning knowledge developed at the prior firm is the mechanism responsible for our findings. Second, they

enable us to explore whether tax planning knowledge is more portable between firms that share similar characteristics; in other words, whether it is specific in nature. Third, finding that the tax planning effect of hiring employees from tax aggressive firms varies predictably in the cross-section helps further mitigate concerns over correlated omitted variables.

4.1. Employee role and experience

We begin our heterogeneity analyses by examining the importance of the employee's role, both at the prior firm and at the hiring firm. We predict that tax department employees who held director-level positions (i.e., director, vice president, etc.) at their prior firm were likely intimately involved in its tax planning, and the knowledge gained in this role will lead to a larger impact on the hiring firm's tax planning. To examine this idea, we sort the movement sample into two groups, depending on whether the employee held a director-level position at the prior firm. We re-estimate equation 1 within both samples, and report these findings in columns 1 and 2 of Table 5. We find that the coefficient on *Hire from TA Firm x Post* is twice as large in the subsample of movements involving a former director relative to the subsample involving other tax department employees. However, the coefficient in the former director subsample is only weakly significant (one-tailed p-value of 0.064), likely driven by the relatively small sample size leading to lower test power.

Next, we posit that tax department employees hired into a director-level role will have a more substantial impact on the hiring firm's tax planning as they will be in a position to use their tax planning knowledge to materially affect key tax department decisions. We implement a similar approach to prior-firm director analysis to examine this prediction: we sort movements into two subgroups based on whether it involved hiring the tax department employee into a director-level role or not. We report these findings in columns 3 and 4 of Table 5. Consistent with our predictions,

the interaction term is negative and economically significant when the employee was hired into a director-level role, with a smaller interaction coefficient for non-director movements.

Third, we focus on the role of prior tax experience. Employees with more time spent in tax-related roles, both overall and specifically at the prior tax aggressive firm, are more likely to have gained the knowledge and skills required for successfully developing and implementing tax planning strategies. To test these ideas, we re-estimate equation 4 within different subsamples. In columns 5 and 6 of Table 5, we split our sample of movements into subsamples based on whether the hired employee's total experience in tax positions was above the sample median. In columns 7 and 8 of Table 5, we split the movement sample into two groups based on whether the hired employee's experience in their prior position (i.e., at the tax aggressive prior firm) was above the sample median. We find a strong negative coefficient on the interaction in columns 5 and 7 (the high experience subsamples), but an insignificant coefficient in columns 6 and 8 (the low experience subsamples). These findings are consistent with the idea that greater tax planning knowledge, measured either by the employee's total tax experience or their experience at the tax aggressive prior firm, is responsible for our findings.[27]

4.2. Firm characteristics

Next, we explore whether the effect of tax planning knowledge dissemination on the hiring firm's tax planning outcomes varies by characteristics of the hiring firm. First, we explore whether the association varies by firm size. We hypothesize that since smaller firms likely have smaller tax departments pre-hiring, the hiring of an employee at a smaller firm likely has a bigger impact on the cumulative amount of tax planning knowledge relative to a similar hire at a larger firm.

[27] When conducting a similar split based on the employee's non-tax experience, we do not observe the same pattern as in columns 3 and 4; instead, the coefficients are similar across the two subsamples (results untabulated). This suggests that it is tax experience, rather than experience in general, that matters for tax planning performance.

Furthermore, smaller firms may have less access to alternative channels through which to obtain tax planning knowledge, such as large financial intermediaries. To examine this idea, we sort our movements into three subgroups based on the average number of employees (per Compustat) in the pre-hiring period. We report these findings in columns 1 through 3 of Table 6. We find evidence consistent with our prediction: the coefficient on the interaction term is strongly negative in the smallest size tercile, and smaller and statistically insignificant in the other two terciles.[28]

Second, we explore the role of auditor-provided tax services. Firms can engage their auditor for tax services, and prior research finds that the firm's auditor can influence its level of tax avoidance (McGuire et al. 2012). If a firm is engaging its auditor for tax services, it could signal that the firm is seeking to obtain tax planning knowledge from outside the firm, and this could lower the potential tax planning impact from an internal hire. In contrast, a firm that is not engaging its auditor for tax services may benefit more from the tax planning knowledge obtained from hiring someone from a tax aggressive firm. To examine this idea, we sort our movements into two subgroups based on whether the hiring firm reported non-zero fees paid to its auditor for tax services (obtained from Audit Analytics) in the three years pre-hiring. We report these findings in columns 4 and 5 of Table 6. We find that the tax planning benefit of hiring an employee from a tax aggressive firm is concentrated within the firms that did not pay tax service fees to their auditor, consistent with our prediction.

4.3. Similarity between hiring and former firms

Next, we explore whether the effect of sourcing tax planning knowledge via the labor markets depends on the similarity between the prior and hiring firms. At the heart of these tests is

[28] Inferences are robust to instead sorting based on total assets or using a grouping other than terciles. We do not use the count of tax department employees from LinkedIn because our ability to accurately measure the total number of tax department employees may vary across firms and time.

examining whether tax planning knowledge, and therefore tax-related human capital, is general or specific in nature. We posit that tax planning knowledge may exhibit a high degree of specificity. For example, certain sectors (such as extractive firms) are known for having unique tax environments because of the tax breaks and deductions tailored to their operation, and therefore tax planning knowledge developed in another sector may be less useful for an extractive firm. Tax planning knowledge may also exhibit a high degree of specificity related to the global scope of the firm's operations, in that tax planning for primarily domestic firms is quite different from tax planning for multinationals. For example, creating subsidiaries in tax havens and developing transfer pricing schemes to take advantage of the allocation of intellectual property (i.e., patents) across those subsidiaries likely requires a unique set of skills and experience that are unlikely to be developed or learned when working in the tax department of a purely-domestic firm. If tax planning knowledge is highly specific in nature, the extent to which employee movements facilitate the transfer of tax planning knowledge between firms may be determined by the extent to which the employee's tax planning knowledge "fits" the circumstances of the hiring firm (Becker 1964; Hashimoto 1981).

To examine the role of specificity in the impact of sourcing tax planning knowledge via the labor market on the hiring firm's tax planning outcomes, we examine whether our main findings vary with the degree of similarity between the former and hiring firms. Specifically, we examine three potential aspects of similarity: sector, global operations, and firm size. We classify former and hiring firms as being in the same sector if they either are in the same two-digit SIC code sector or are classified as similar based on the text-based measurement methodology developed by Hoberg and Phillips (2010, 2016).[29] To assess similarity in global operations and

[29] We obtain their data from https://hobergphillips.tuck.dartmouth.edu/industryclass.htm. We employ the Hoberg and Phillips methodology because it allows for changes in sector similarity over time and easily accommodates

firm size, we compute the absolute value of the difference in the foreign income (scaled by total assets) or total assets between the two firms, and then classify firms as being similar if they are below the sample median of this difference.[30] We classify firms into one of two subgroups for each similarity construct based on whether they are considered similar or not, and re-estimate equation 1 within each subgroup. We report these findings in Table 7, with columns 1 and 2 addressing sector similarity, columns 3 and 4 addressing global operations similarity, and columns 5 and 6 addressing size similarity.

When examining sector and global operations similarity, we find evidence consistent with our prediction: the coefficient on *Hire from TA Firm x Post* is negative and statistically significant in the same sector or have similar global operations, and larger than the same coefficient in the different sector or low global operations similarity subgroups. However, we do not find evidence consistent with size similarity; the coefficients across the high and low size similarity subgroups are similar. These findings largely support the idea that tax planning knowledge has a larger impact on hiring firm tax avoidance when that knowledge is built at a similar firm, consistent with such knowledge being specific in nature.

5. Additional analyses

We end our analysis by exploring the matching process between hiring firms and the firms from which they hire ("supplying firms"). In particular, we examine whether firms tend to hire from other similar or dissimilar firms along certain characteristics. If firms' operating and tax environments lead to unique tax scenarios that require specific types of tax planning knowledge,

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the creation of new sectors. In contrast, the SIC scheme is static over time and tends to aggregate new product markets into large, non-descript sectors (Hoberg and Phillips 2016). Furthermore, using their methodology allows us to broaden the sample of similar sector movements. We employ their TNIC-2 approach, which they argue is comparable to two-digit SIC codes.

[30] Specifically, we compute the hiring firm's foreign income and size using the average value in the three years prior to the employee's hiring and compare it to the prior firm's value in the year prior to the hiring. We measure global operations using foreign income because of a lack of reliable data on foreign assets.

then we expect that firms will be more likely to hire from similar firms, as the employees in those firms will have been more likely to develop the required tax planning knowledge. On the other hand, if tax planning knowledge is primarily general in nature, movements will likely occur without regard to the similarity between the hiring and supplying firm.

We focus on several firm characteristics that could proxy for unique tax environments: sector membership, firm size, geographic location, foreign operations, and the level of tax avoidance. Prior research shows that tax planning often involves industry-specific components (McGuire et al. 2012), suggesting that employee movements may be more likely to occur within an industry than across industries. Larger firms likely have more complex tax environments than smaller firms due to the scale of their operations. Similarly, multinational corporations are likely to have unique tax environments: operating in multiple tax jurisdictions leads to both additional complexity (e.g., complying with different regulations) as well as tax avoidance opportunities (e.g., shifting income from high-tax to low-tax jurisdictions). Likewise, firms within close geographic proximity likely face similar tax rules and regulations (e.g., state and local taxation). Furthermore, the costs (e.g., search costs, moving costs) of hiring an employee suggest that firms are more likely to hire within close geographical proximity. Finally, firms with low ETRs may be using aggressive tax strategies, the maintenance and/or development of which may require experience at a tax aggressive firm.

To explore this idea, we construct a sample of possible hiring firm-employee combinations, using all S&P 1500 firms that hired in a given year and all employees who moved in that year from S&P 1500 firms. Using this sample, we estimate the following model:

$$Match_{i,j,t} = \beta_1 Same\ Sector_{i,j,t} + \beta_2 Proximity_{i,j,t} + \beta_3 Size\ Similarity_{i,j,t} + \beta_4 Global\ Similiarty_{i,j,t} + \beta_5 ETR\ Similarity_{i,j,t} + Fixed\ Effects + \varepsilon_{i,t}. \quad (2)$$

The dependent variable is *Match*, an indicator variable equal to one if the firm hired that employee in that year and zero otherwise. We include various measures that capture the similarity between the hiring and supplying firms as independent variables: whether they are in the same sector (*Same Sector*), the geographic proximity between their headquarters (*Proximity*), the natural logarithm of the absolute difference in total assets times negative one (*Size Similarity*), the absolute difference in foreign income times negative one (*Global Similarity*), and the absolute difference between their 5-year cash effective tax rates times negative one (*ETR Similarity*).

We present these results in Table 8. We first include each "similarity" variable separately (cols. 1 through 5) and then jointly (cols. 6 and 7). We consistently find that firms are more likely to hire from firms that share industry membership, are closer in geographic proximity, and are similar in global operations. These findings support the idea that tax-related human capital is specific rather than general in nature: firms appear to on average demand specific types of tax-related human capital that fit their particular operating and tax environments. This finding is consistent with the findings in Table 7, which suggest that the acquisition of tax planning knowledge has a larger impact on the hiring firm's tax planning outcomes when the hiring and former firms are similar. The only exceptions are that we generally do not find evidence that firms seek tax department employees who worked in tax departments in similar-sized firms or in firms with a similar ETR. The former result mirrors the lack of significance in the Table 7 test examining similarity in the size of the hiring and former firm, and the latter finding is consistent with firms seeking employees from firms that have different tax planning outcomes; for example, firms with less tax planning seeking employees with tax planning knowledge built at tax aggressive firms.

Overall, these findings suggest that both the employee's development of and the hiring firm's need for specific types of tax planning knowledge matter in generating successful tax planning.[31]

6. Conclusion

Motivated by insights from the labor economics literature, we examine the role of the labor market in disseminating tax planning knowledge across firms. To examine this idea, we construct a novel dataset set of employee movements between the tax departments of S&P 1500 firms. Consistent with the labor market playing a critical role in the diffusion of tax planning knowledge through employee movements, we find that firms experience an increase in tax planning when hiring an employee who previously worked at a tax aggressive firm. While employee movements between firms are admittedly non-random, we employ several research designs and robustness tests to mitigate alternative explanations for our findings.

In additional analyses, we find that the tax planning benefit of hiring an employee from a tax aggressive firm is stronger when the employee has more tax experience, is hired into a senior tax department role, and when the hiring firm is smaller and does not engage its auditor for tax services. Further tests suggest that tax planning knowledge (and therefore tax-related human capital) is highly specific in nature: the increase in tax avoidance is larger when the hiring and former firms are similar (i.e., operating in the same sector or having similar foreign operations), and firms are more likely to hire tax department from firms with similar characteristics. These results further strengthen the conclusion that the hired employee's tax planning knowledge is the mechanism responsible for our main findings.

[31] Ideally, we would compare these percentages to employee movement in other business units or departments (e.g., marketing). Unfortunately, we are unaware of studies that conduct similar analyses for other business units or departments and doing so ourselves would require an extensive data collection effort on our part. Furthermore, it is unclear whether we would be able to accurately identify employees in other departments, given the heterogeneity in titles that can exist across firms. This is in contrast to the tax department setting, as tax department employees likely have the word "tax" in their title.

Our focus on the role of the labor market in diffusing tax planting knowledge contributes to the literature in accounting on the relation between labor and accounting generally, and on the diffusion of tax planning strategies and the role of individuals in corporate tax planning behavior more precisely. Based on our initial insights, we look forward to further research that connects labor economics, specifically the labor market, with accounting and taxation.

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Appendix: Variable definitions

Main variables

Variable	Definition
Cash ETR	Cash taxes paid scaled by pre-tax income (adjusted for special items)
Hire from TA Firm	Indicator variable equal to one if the prior firm's long-run (i.e., 5-year) cash ETR is in the bottom quartile of the sample of movements and zero otherwise
Post	Indicator variable equal to one in the year the employee is hired and the following two years, and zero otherwise

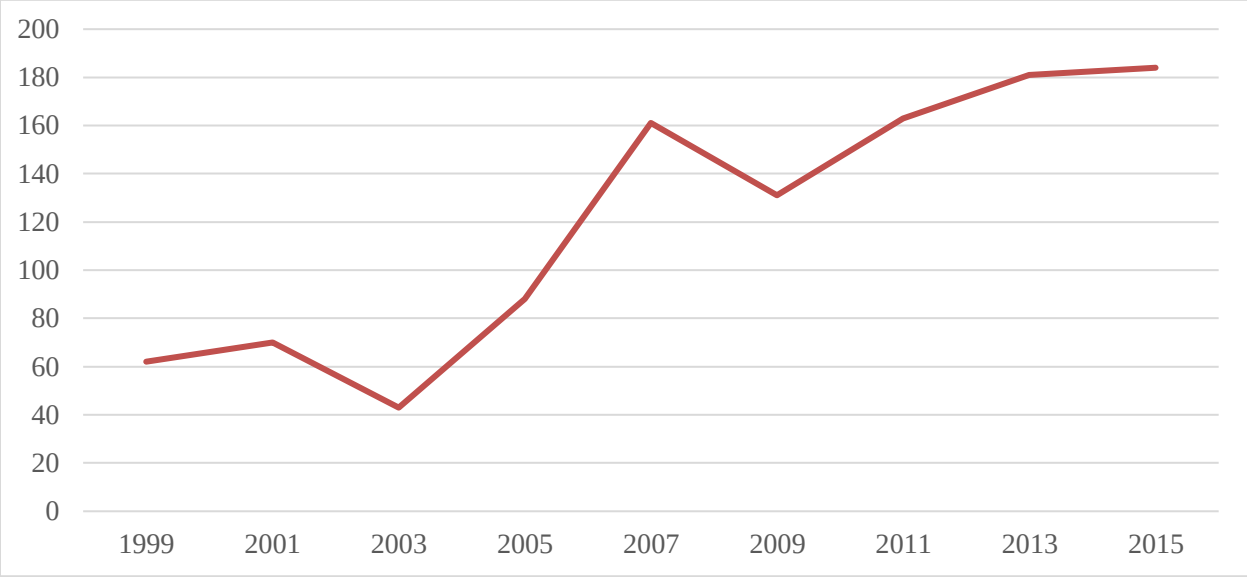
Control variables

Variable	Definition
ROA	Pre-tax income scaled by total assets
FI	Foreign pre-tax income scaled by total assets
Size	Natural logarithm of assets
Leverage	Long-term debt scaled by total assets
MTB	Market-to-book ratio
Sales Growth	Annual change in sales scaled by lagged sales
PPE	Property, plant and equipment scaled by total assets
Intangible Assets	Intangible assets scaled by total assets
R&D	R&D expense scaled by total assets
Advertising	Advertising expense scaled by total assets
Special	Special items scaled by total assets
Extraordinary	Extraordinary items scaled by total assets
NOL Indicator	Indicator variable equal to one if there was a tax loss carryforward at the beginning of the year, and zero otherwise
NOL Change	Change in the tax loss carryforward over the year scaled by total assets

Figure 1: Descriptive evidence on employee movements

This figure presents descriptive evidence on our sample of employee movements between the tax departments of S&P 1500 firms. Panel A plots the number of movements in our sample by each two-year period (i.e., 1998-1999). Panel B plots the percentage of the firms in each Fama-French 17 sector with at least one tax department hiring from another S&P 1500 firm during our sample period.

Panel A: Movements by year



Panel B: Percentage of firms in each sector with at least one hiring

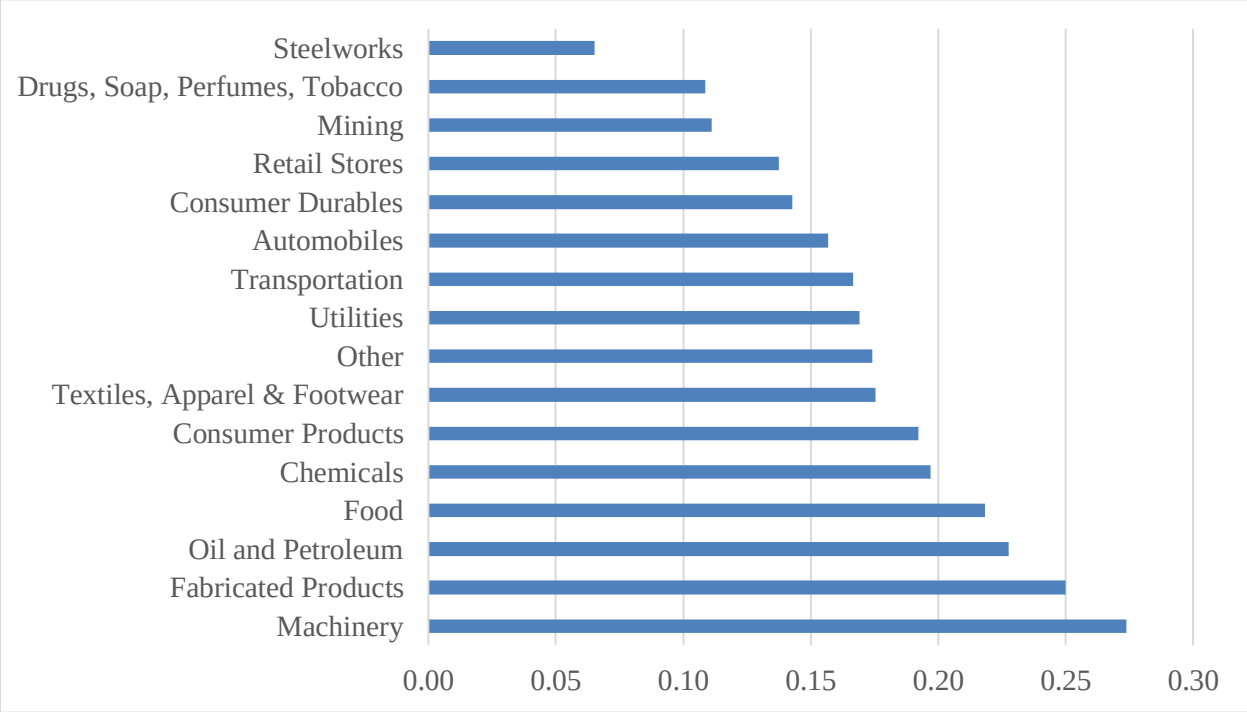


Figure 2: Treatment dynamics

This figure examines the pre-treatment and post-treatment dynamics in corporate tax planning around hiring tax department employees from tax aggressive firms. We estimate a simple difference-in-differences model (similar to equation 1), where we saturate the model with event-time indicator variables interacted with *Hire from TA Firm*, using year t-1 (the year prior to the hire) as the base period. We plot the coefficients on the interactions of *Hire from TA Firm* and the following time periods: year t-3, year t-2, year t (the hire year), year t+1, and year t+2. We also present 90 percent confidence intervals using standard errors clustered by firm.

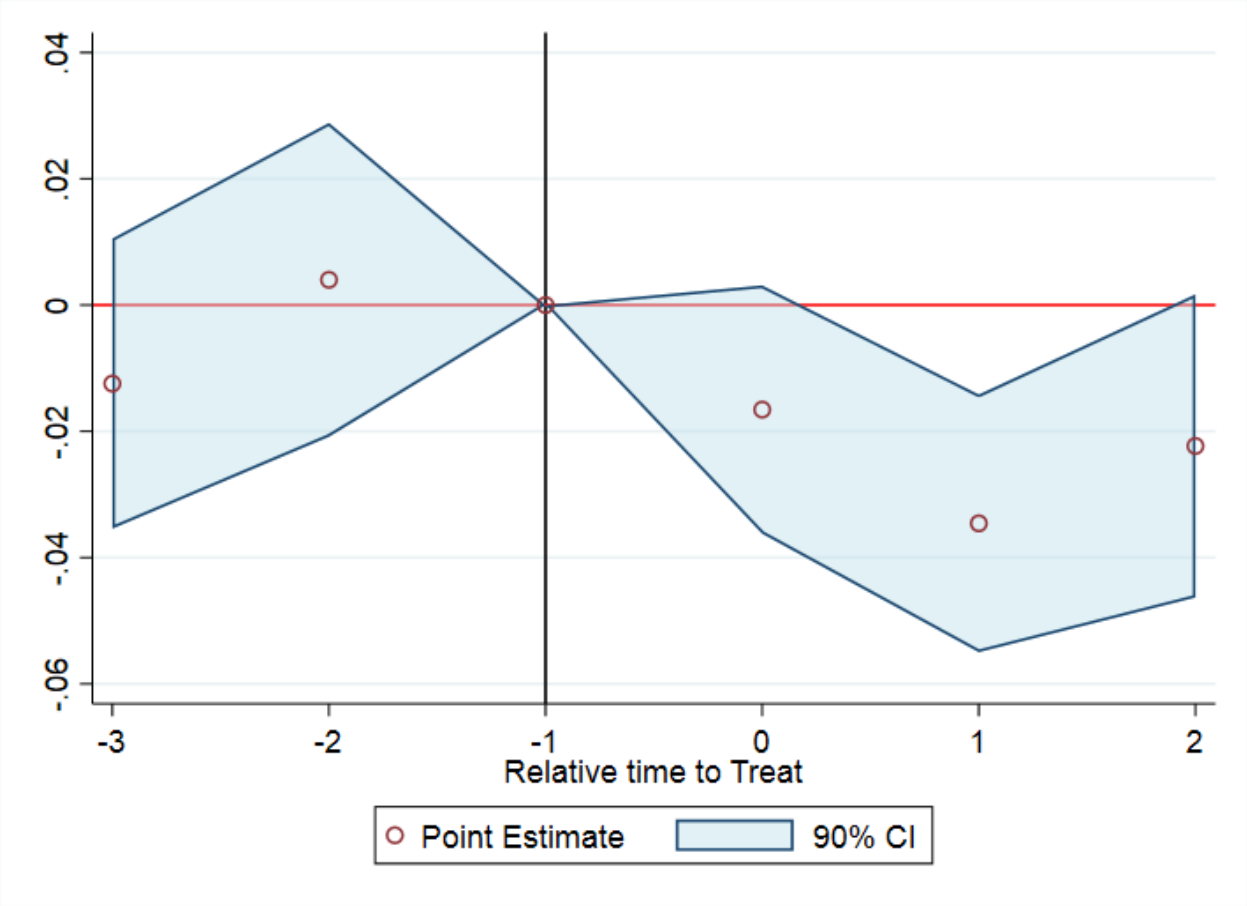


Table 1: Descriptive statistics for key variables

This table presents descriptive statistics for the variables employed in our analyses, using the movements-only sample (i.e., the sample employed in tables 3 through 7). All variables are defined in the appendix.

	Mean	**Std Dev**	**P25**	**P50**	**P75**
Cash ETR	0.233	0.165	0.128	0.214	0.305
Size	9.438	1.750	8.171	9.437	10.610
ROA	0.154	0.068	0.103	0.142	0.192
FI	0.040	0.044	0.002	0.026	0.063
Special	-0.011	0.028	-0.012	-0.003	0.000
Extraordinary	0.000	0.006	0.000	0.000	0.000
Advertising	0.015	0.030	0.000	0.000	0.016
R&D	0.034	0.044	0.000	0.016	0.053
Sales Growth	0.098	0.191	-0.004	0.067	0.162
PPE	0.236	0.194	0.089	0.164	0.331
Intangibles	0.227	0.195	0.067	0.171	0.364
Leverage	0.191	0.133	0.087	0.187	0.274
NOL Indicator	0.479	0.500	0.000	0.000	1.000
NOL Change	0.005	0.034	0.000	0.000	0.002
MTB	4.097	3.861	1.823	2.909	4.625

Table 2: Main results, panel sample

This table presents the results of estimating equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one in the year in which the firm first hired an employee from a tax aggressive S&P 1500 firm (a firm with five-year cash effective tax rate in the bottom quartile of the sample of movements) and the following two years, zero otherwise. Each column from 1 through 6 includes some combination of sector (defined at the SIC2-level) or firm fixed effects, year or sector-year fixed effects, and the time-varying firm-level control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*). Columns 1 through 4 and 6 use all firm-years for S&P 1500 firms between 1995 and 2018. Column 5 only includes firms that hired at least one employee from an S&P 1500 firm during our sample period. Columns 7 and 8 present the findings using a stacked regression design as described in Barrios (2021), Cengiz et al. (2019), and section 3.1, and includes group-firm and group-year fixed effects (both col. 7 and 8) and time-varying firm-level control variables (col. 8). Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:					*Cash ETR*			
Hire from TA Firm	-0.032***	-0.026***	-0.017**	-0.017**	-0.015**	-0.014*	-0.016**	-0.015*
	(0.009)	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.008)
Sample	All	All	All	All	Hiring Firms Only	All	Stacked	Stacked
Controls	No	No	No	Yes	Yes	Yes	No	Yes
Sector FE	No	Yes	No	No	No	No	No	No
Firm FE	No	No	Yes	Yes	Yes	Yes	No	No
Year FE	No	Yes	Yes	Yes	Yes	Yes	No	No
Sector x Year FE	No	No	No	No	No	Yes	No	No
Group x Firm FE	No	No	No	No	No	No	Yes	Yes
Group x Year FE	No	No	No	No	No	No	Yes	Yes
Observations	24,180	24,180	23,960	23,960	7,097	23,892	117,490	117,490
Adjusted R-squared	0.001	0.061	0.232	0.262	0.227	0.288	0.291	0.323

Table 3: Main results, movements sample

This table presents the results of estimating a modified version of equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample of movements and zero otherwise, and *Post*, an indicator variable equal to one in the year when the employee joins the hiring firm and each year afterward and zero otherwise. This regression is estimated on a six year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Column 1 employs the main sample and specification. In column 2, we employ entropy balancing that uses weights to minimize the differences in means and variances of the control variables between observations with *Hire from TA Firm* = 1 and other observations. In column 3, we again employ entropy balancing, but only create the weights using the year prior to the employee's starting at the new firm. Columns 4 and 5 employ the main sample and specification, except that they use different definitions for *Hire from TA Firm*: bottom 20 percent (col. 4) and bottom 30 percent (col. 5). Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*), movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dependent variable:			*Cash ETR*		
Hire from TA Firm x Post	-0.021**	-0.023**	-0.019**	-0.020**	-0.021**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Specification	Main	Entropy (all)	Entropy (t-1)	Main	Main
TA Firm definition	Bottom 25%	Bottom 25%	Bottom 25%	Bottom 20%	Bottom 30%
Controls	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,875	5,965	5,965
Adjusted R-squared	0.416	0.403	0.397	0.415	0.416

Table 4: Mitigating concerns about alternative events, firm connections, and former firm characteristics

This table presents the results of estimating a modified version of equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample of movements and zero otherwise, and *Post*, an indicator variable equal to one in the year when the employee joins the hiring firm and each year afterward and zero otherwise. This regression is estimated on a six year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Each panel contains a different set of robustness analyses. In Panel A, we additionally include *Alternative x Post* as an independent variable. In each column, we employ a different variable for *Alternative*: the number of tax department hires in year t (col. 1), an indicator variable equal to one if the firm had acquisition cash flows greater than 10 percent of lagged total assets in either year t-1 or t (col. 2), an indicator variable equal to one if the firm experienced a CEO turnover in either year t-1 or t (col. 3), an indicator variable equal to one if the hiring and former firm shared a board member in year t (col. 4), an indicator variable equal to one if the hiring and former firm shared a lead lender bank in year t (col. 5), or an indicator variable equal to one if the hiring and former firm shared an auditor in year t (col. 6). In Panel B, we re-estimate a modified version of equation 1 on a sample of CEO and CFO movements between S&P 1500 firms during our sample period. We replace *Hire from TA Firm* with *Hire Exec from TA Firm*, where the latter is an indicator variable equal to one if the executive's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample of executive movements and zero otherwise. In Panel C, we include (either by itself or in addition to the main variables) *Hire from High Perf. Firm x Post*, where *Hire from High Perf. Firm* is an indicator variable equal to one if the employee's former firm is in the top quartile of the sample of movements in a given performance measure and zero otherwise. Each column uses a different performance measure: pre-tax income (cols. 1 and 2), foreign income scaled by total assets (cols. 3 and 4), and research and development expense scaled by total assets (cols. 5 and 6). In Panel D, we re-estimate equation 1 using alternative dependent variables: *ROA* (operating income before depreciation scaled by total assets), *R&D* (research and development expense scaled by total assets), or *CapEx* (capital expenditures scaled by total assets). Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*) except when that control variable is used as the dependent variable, movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

Panel A: Accounting for alternative events and connections

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			*Cash ETR*			
Hire from TA Firm x Post	-0.021**	-0.021**	-0.022**	-0.021**	-0.022**	-0.021**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Alternative x Post	-0.001	0.008	-0.008	0.018	0.016	-0.006
	(0.003)	(0.013)	(0.011)	(0.019)	(0.010)	(0.010)
Alternative	Num. of hires	Large M&A	CEO turnover	Board overlap	Bank overlap	Auditor overlap
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,965	5,965	5,965	5,965
Adjusted R-squared	0.415	0.416	0.416	0.416	0.416	0.416

Panel B: Examining executive movements

	(1)
Dependent variable:	*Cash ETR*
Hire Exec from TA Firm x Post	0.031
	(0.034)
Controls	Yes
Movement FE	Yes
Event-time FE	Yes
Sector x Year FE	Yes
Observations	1,168
Adjusted R-squared	0.424

Panel C: Placebo analyses using alternative former firm characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			*Cash ETR*			
Hire from TA Firm x Post		-0.021**		-0.021**		-0.021**
		(0.009)		(0.009)		(0.009)
Hire from High Perf. Firm x Post	0.005	0.001	-0.004	-0.005	0.014	0.014
	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Performance measure	*ROA*	*ROA*	*FI*	*FI*	*R&D*	*R&D*
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,965	5,965	5,965	5,965
Adjusted R-squared	0.415	0.415	0.415	0.416	0.415	0.416

Panel D: Placebo analyses using alternative, dependent variables

	(1)	(2)	(3)
Dependent variable:	*ROA*	*R&D*	*CapEx*
Hire from TA Firm x Post	-0.000	-0.000	-0.000
	(0.002)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes
Observations	5,965	5,965	5,950
Adjusted R-squared	0.871	0.953	0.906

Table 5: Employee position and experience

This table presents the results of estimating a modified version of equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample and zero otherwise, and *Post*, an indicator variable equal to one in the year when the employee joins the hiring firm and each year afterward and zero otherwise. This regression is estimated on a six year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Column 1 (2) estimates the regression on the subsample of employees who held a director (non-director) position at the prior firm. Column 3 (4) estimates the regression on the subsample of employees who are hired into a director (non-director) position at the hiring firm. Column 5 (6) estimates the regression on the subsample of employees who have total tax-related experience above (below) the sample median prior to joining the hiring firm. Column 7 (8) estimates the regression on the subsample of employees whose experience at the prior firm is above (below) the sample median. Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*), movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:					*Cash ETR*			
Hire from TA Firm x Post	-0.049	-0.021**	-0.057**	-0.009	-0.034**	-0.005	-0.032**	-0.013
	(0.032)	(0.010)	(0.026)	(0.009)	(0.016)	(0.012)	(0.013)	(0.013)
Specification	Director at prior firm	Non-director at prior firm	Director at hiring firm	Non-director at hiring firm	High tax experience	Low tax experience	High experience at prior firm	Low experience at prior firm
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,134	4,613	1,708	3,978	2,630	3,115	2,842	2,866
Adjusted R-squared	0.353	0.421	0.327	0.454	0.379	0.432	0.409	0.401

Table 6: Hiring firm characteristics

This table presents the results of estimating a modified version of equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample and zero otherwise, and *Post*, an indicator variable equal to one in the year when the employee joins the hiring firm and each year afterward and zero otherwise. This regression is estimated on a six year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Columns 1, 2, and 3 estimate the regression on the subsample of hiring firms with total employees (per Compustat) in the top, middle, and bottom tercile of the sample of movements, respectively. Column 4 (5) estimates the regression on the subsample of hiring firms whose average fees paid to their auditor for tax services (scaled by operating income before depreciation) was greater than zero (zero) during the three-year period prior to the hiring of the employee. Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*), movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dependent variable:			*Cash ETR*		
Hire from TA Firm x Post	-0.008	-0.014	-0.040**	-0.014	-0.043**
	(0.008)	(0.015)	(0.019)	(0.010)	(0.021)
Subsample	Large firms	Medium firms	Small firms	APTS > 0	APTS = 0
Controls	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,894	1,913	1,857	4,562	1,274
Adjusted R-squared	0.733	0.430	0.408	0.416	0.462

Table 7: Hiring and former firm similarity

This table presents the results of estimating a modified version of equation 1. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had a five-year cash effective tax rate in the bottom quartile of the sample and zero otherwise, and *Post*, an indicator variable equal to one in the year when the employee joins the hiring firm and each year afterward and zero otherwise. This regression is estimated on a six year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Column 1 (2) estimates the regression on the subsample of employees who move between two firms in the same sector (different sectors), where same sector is defined as either having the same SIC2 or being peer firms as identified by Hoberg and Phillips (2010, 2016). Column 3 (4) estimates the regression on the subsample of employees who move between two firms where the absolute value of the difference between the hiring firm's pre-movement foreign income to asset ratio and the prior firm's long-run foreign income to asset ratio in the employee's final year is below (above) the sample median. Column 5 (6) estimates the regression on the subsample of employees who move between two firms where the absolute value of the difference between the hiring firm's pre-movement average assets and the prior firm's total assets in the employee's final year is below (above) the sample median. Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*), movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:			*Cash ETR*			
Hire from TA Firm x Post	-0.047**	-0.014	-0.054***	-0.001	-0.015	-0.026*
	(0.022)	(0.011)	(0.014)	(0.012)	(0.013)	(0.013)
Subsample	Same Sector = 1	Same Sector = 0	Similar *FI* = 1	Similar *FI* = 0	Similar *Size* = 1	Similar *Size* = 0
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,267	4,524	2,849	2,873	2,799	2,877
Adjusted R-squared	0.433	0.422	0.423	0.440	0.440	0.381

Table 8: Matching between hiring and prior firms

This table presents the results of estimating equation 2 on a sample of realized and possible movements between the corporate tax departments of S&P 1500 firms from 1998 to 2015. The sample is constructed by crossing all firms that hire a tax department employee from another firm in a given year with all tax department employees who were hired by any sample firm's corporate tax department in that year. The dependent variable is *Match*, an indicator variable equal to one if the firm hired the moving employee and zero otherwise (multiplied by 100). The primary independent variables are *Same Sector* (an indicator variable if the prior and hiring firms are in the same two-digit SIC sector or are peer firms as identified by Hoberg and Phillips (2010, 2016)), *Proximity* (the number of miles between the headquarters of the prior and hiring firms, scaled by -100), *Size Similarity* (the natural log of the absolute value of the difference between the total assets of the prior and hiring firm, times negative one), *Global Similarity* (the absolute value of the difference between the five-year foreign incomes (scaled by total assets) of the prior and hiring firm, times negative one), and *ETR Similarity* (the absolute value of the difference between the five-year cash effective tax rates of the prior and hiring firm, times negative one). Columns 1 through 6 include year fixed effects, column 7 includes hiring firm and year fixed effects. Standard errors clustered by firm are presented in parentheses below the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:				*Match*			
Same Sector	1.406***					1.282***	1.244***
	(0.144)					(0.139)	(0.139)
Proximity		0.086***				0.084***	0.095***
		(0.004)				(0.004)	(0.005)
Size Similarity			0.016			0.014	0.061***
			(0.023)			(0.023)	(0.013)
Global Similarity				2.144***		0.776	1.624**
				(0.667)		(0.715)	(0.774)
ETR Similarity					0.275	0.120	0.103
					(0.183)	(0.182)	(0.231)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	No	No	No	Yes
Observations	165,463	165,463	165,463	165,463	165,463	165,463	165,463
Adjusted R-squared	0.003	0.007	0.001	0.001	0.001	0.008	0.008