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Tax Planning Knowledge Diffusion via the Labor Market

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Abstract. We examine the extent to which the labor market facilitates the diffusion of tax-planning knowledge across firms. Using a novel data set 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 the result, we find that the tax-planning benefits are more substantial when the employee is involved in a director-level role and has more experience. 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. Finally, we do not find that the prior firm's tax planning changes after the employee leaves the firm, suggesting that the tax-planning knowledge simply spreads to the hiring firm and does not leave the prior firm. Our study documents the first order role of the labor market in the diffusion of tax-planning knowledge across firms, and our findings suggest that tax department human capital is a central determinant of tax-planning outcomes.

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

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. Whereas it is widely known that firms engage in tax planning, how firms acquire the requisite knowledge for tax planning is less understood. In particular, 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 among firms by examining tax employee movements between publicly traded U.S. corporations.

Our focus on tax knowledge diffusion stems from literatures in economics and organizational science, which propose that firms are unable to internally generate all the knowledge necessary for continuous technological improvement (Levitt and March 1988, Cohen and Levinthal 1990, March and Simon 1993). Whereas firms may acquire knowledge from various external sources (i.e., alliances and joint ventures), these literatures posit that

the labor market is a first order mechanism in transferring knowledge and innovation among firms (DiMaggio and Powell 1983, Barney 1991, Penrose 2009). Specifically, as individuals move between firms, they can apply their knowledge to new contexts, effectively transferring it across firms (Argote and Ingram 2000). Consistent with this idea, prior research documents that employee movements are associated with the transfer of knowledge (Arrow 1962, Levin et al. 1987, Rosenkopf and Almeida 2003, Song et al. 2003, Franco and Filson 2006). Moreover, locations exhibiting greater intraregional labor mobility between firms tend to have more knowledge flows (Jaffe et al. 1993, Almeida and Kogut 1999, Jara-Figueroa et al. 2018).

However, the extent to which firms can source knowledge via the labor market is determined in part by the nature of the knowledge being transferred (Zander and Kogut 1995, Szulanski 1996, Wu et al. 2018) and, in particular, whether it is explicit or tacit. Explicit knowledge is considered general in nature and easily shareable, such as through written material; its diffusion is, thus, unlikely to require employee movements. In contrast,

tacit knowledge is built through experience (Cohen and Levinthal 1990) or acquired through learning by doing (Teece 1982, Chatterji et al. 2016), usually in specific environments (Argote and Miron-Spektor 2011, Venkatchalam and Busch 2012). Because tacit knowledge originates from direct experience, it is embedded in the individual, and thus, its diffusion requires individuals to move from one environment to another.

Whether the labor market is a mechanism for the transferability of tax-planning knowledge is empirically ambiguous. If tax-planning knowledge is explicit in nature, then employee movements are unlikely to be a primary means through which tax-planning knowledge diffuses across firms. However, there are reasons to expect that tax-planning knowledge is primarily built through experience. Tax employees need to take a holistic view of tax planning, balancing tax minimization with other firm goals, such as market share and cost efficiency, and complying with tax regulations and the relevant tax authorities. Successfully managing these objectives requires a unique set of experiences and skills that are difficult to acquire outside of a corporate tax department, consistent with tax-planning knowledge being tacit in nature.¹ If so, then hiring a tax department employee from a successful tax-planning firm could play an essential role in the tax-planning knowledge-building process of the hiring firm.²

To explore the role of the labor markets in the diffusion of tax-planning knowledge, we construct a novel data set of tax department employee movements between publicly traded U.S. corporations assembled from resumes posted to a major professional networking website. Using these data and a difference-in-differences research design, we examine the relation 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 (ETR) prior to the employee's departure relative to both the overall economy and a sector- and size-matched peer group) and changes in the hiring firm's tax-planning outcomes.³ 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 ETR of approximately two percentage points on average. We find similar results when we focus on a sample of employee movements, which allows us to hold constant the fact that the firm is hiring into its tax department. We find, again, that firms hiring from a tax-aggressive firm experience a materially significant increase in their tax planning relative to firms hiring from non-tax-aggressive firms. Additional tests suggest that this tax-planning benefit is (a) sustained for several years after the hire and (b) not explained by alternative firm connections or contemporaneous events.

Next, we use our setting and data to shed light on the nature of tax-planning human capital (i.e., knowledge) and, thus, its applicability to the hiring firm. In economics, Becker (1962) proposes two types of human capital: general human capital, which helps productivity at any firm, and firm-specific human capital, which raises productivity at firms similar to the one in which it was generated but not elsewhere. If tax-planning knowledge is general in nature—that is, its usefulness does not depend on the characteristics of the hiring firm and their similarity to those in which the knowledge is developed—then there should be no difference in the effect of hiring an employee from a similar or dissimilar firm. In contrast, tax-planning knowledge could exhibit specificity, meaning that it is more applicable to environments similar to the one in which the knowledge was generated. Employees develop tax-planning knowledge by applying their understanding of the tax code to the unique characteristics of the firm's business model and operating environment to reduce the firm's tax burden. This suggests that tax-planning knowledge is specific to the circumstances in which it was developed. 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). This knowledge may benefit other multinationals more than domestic firms.⁴ If tax-planning knowledge is highly specific in nature, then (a) the benefit of hiring an employee with tax-planning knowledge should increase in the similarity between the hiring and former firms, and (b) firms should be more likely to seek tax employees from similar firms.

Understanding whether tax knowledge is specific or general in nature is valuable. For example, the extent of the specificity in tax-planning knowledge has implications for the role of incentive (i.e., compensation) schemes in explaining tax-planning outcomes, which is the focus of prior studies on the role of individuals in corporate tax planning (Phillips 2003; Armstrong et al. 2012, 2015; Rego and Wilson 2012; Brown et al. 2016). If tax-planning knowledge is highly specific in nature, then the ability of compensation incentives to encourage greater tax planning is inherently constrained by the applicability of one's tax-planning knowledge to the firm.⁵

We conduct two sets of analyses to explore the extent to which tax-planning knowledge is specific or general in nature. First, we examine from where firms source their tax department employees. If tax-planning knowledge is specific in nature, we expect that firms look to hire tax department employees from similar firms as the tax-planning knowledge developed in those prior firms is more easily applicable and valuable to the hiring firm. We find that firms are more likely to acquire

tax department employees from firms that face similar tax environments, such as those that are in close geographical proximity, in the same sector, have a similar focus on multinational operations, and are of a similar scale. Second, we examine whether the benefit of hiring an employee from a successful tax-planning firm varies with the similarity between the hiring and prior firms. If tax-planning knowledge is highly specific in nature, then for a hiring firm to capitalize on the tax-planning knowledge embedded in the hired employee, its tax environment needs to be similar to that of the prior firm. Indeed, we find that the benefit to hiring an employee from a successful tax-planning firm tends to be concentrated in movements in which the hiring and prior firms are more similar (measuring similarity using sector membership, the extent of global operations, or scale).⁶ Both sets of findings suggest that tax-planning knowledge transferred via the labor market is more consistent with the concept of specific human capital.

We also examine whether the tax-planning benefit of hiring from a tax-aggressive firm varies with certain employee- and hiring firm-level characteristics. We find that the increase in tax planning is concentrated within movements when the employee is hired into a director-level position (and, thus, likely plays a significant role in its tax planning) and has more tax experience at the prior firm (and, thus, likely had more familiarity with the successful tax strategies at that firm). We also find that the increase in tax planning is primarily attributable to hiring firms that are smaller and do not engage their auditor for tax services.

Our final set of analyses explores how employee movements are associated with tax-planning changes in the departing firms. *Ex ante*, it is unclear whether the prior firm's tax planning changes after an employee's departure. On the one hand, the firm's tax planning would not necessarily change if it can retain the tax-planning knowledge either in its remaining employees or via other means (e.g., internal information systems). On the other hand, if tax-planning strategies require an annual maintenance component, then the employee's departure could lead to a decline in the firm's tax planning if the employee is not replaced. We find that firms do not experience a significant change in their ETRs after an employee's departure, relative to a matched control sample that does not experience an employee departure. Our finding suggests that tax-planning knowledge does not dissipate as the employee departs the prior firm but, rather, simply spreads to the new firm.

Our study contributes to several strands of literature. First, we add to an emerging literature in accounting that examines the interaction between labor and accounting (Call et al. 2017, Hoopes et al. 2018, Choi et al. 2019, Pacelli 2019, Merkley et al. 2020, Barrios 2022). We extend this research by studying the role of the labor market in the diffusion of tax-accounting knowledge. Moreover,

our use of employee movements establishes a novel methodological approach that future research can implement to explore further the interactions between the labor market and other accounting outcomes.

We also contribute to the literature in organizational science on knowledge diffusion across firms, which, to date, mainly focuses on the diffusion of innovation and technology (Arrow 1962, Allen 1984, Levin et al. 1987, Rosenkopf and Almeida 2003, Song et al. 2003, Cassiman and Veugelers 2006). We extend this literature by emphasizing the importance of the labor market in transferring firm-level operational knowledge. Whereas we focus specifically on the diffusion of tax-planning knowledge, examining knowledge diffusion via employee movements in the tax setting has several advantages. First, tax planning likely requires specific experiences and training, making it more likely that its diffusion occurs through employee movements. Second, we can identify tax department employees for a large sample of firms, and we can observe an outcome (corporate tax planning) that is clearly linked to the efforts of those individuals.⁷

Finally, we make two contributions to the tax literature. First, we contribute to the literature that explores how tax planning strategies spread across firms; these studies focus on the role of intermediaries, such as board interlocks, banks, and auditors (Brown 2011, McGuire et al. 2012, Brown and Drake 2014, Gallemore et al. 2019, Chyz et al. 2021). We contribute by documenting the role of the labor market for the actual individuals in charge of developing and implementing tax strategies in the diffusion of tax knowledge. Second, we contribute to the literature on the role of individuals in corporate tax avoidance (Phillips 2003; Dyreng et al. 2010; Armstrong et al. 2012, 2015; Rego and Wilson 2012; Chyz 2013; Chyz et al. 2013; Brown et al. 2016; Klassen et al. 2016; Koester et al. 2017; Chen et al. 2021; Ege et al. 2021).⁸ These studies generally focus on either high-level firm employees, such as the CEO, or lower level employees who are unlikely to oversee or directly impact the tax department. In contrast, we focus on tax department employees who develop and implement the firm's tax strategy, which are generally ignored in prior work (likely because of data constraints). Furthermore, studies that examine tax department employees tend to focus on non-experience-related characteristics, such as the raw count of employees, their relevant rank within the management team, or the usage of incentives to motivate employee effort. In contrast, we directly examine tax-planning knowledge as a key mechanism in how these individuals shape the firm's corporate tax-planning outcomes.

There are two contemporaneous exceptions: Jiang et al. (2020) and Kubick et al. (2020). Jiang et al. (2020) find that hiring employees from the Internal Revenue Service does not affect the level of tax planning on average. The difference between their findings and

ours suggests that successful tax planning requires, in part, tacit tax knowledge developed via experience within the corporate tax department of a successful tax-planning firm. Kubick et al. (2020) find that tax-savvy executives are associated with greater tax avoidance. However, there are several key differences between our study and theirs. First, we focus on the broader tax department, whereas Kubick et al. (2020) only examine top-level managers. Second, Kubick et al. (2020) cannot attribute their findings to tax-planning knowledge; they note that their results may be driven by factors such as the “tone at the top” or facilitating connections with other intermediaries, such as auditors or banks. In contrast, we directly examine the role of experience in explaining tax-planning outcomes. In doing so, we provide novel evidence on the diffusion of tax-planning knowledge across firms and the role of individuals in corporate tax planning.

2. Data

2.1. Identifying Employee Movements

To conduct our analyses, we exploit data from resumes posted on 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 1500.⁹ We focus on S&P 1500 firms for several reasons. First, their Securities and Exchange Commission 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. This allows us to explore certain 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 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.¹⁰

Using the networking website, we identify individuals at each firm who either currently or previously worked in a tax position.¹¹ We classify an employee as being in the tax department if their profile contains a position with the word “tax” in the title.¹² For each tax department employee, we identify different job positions from career histories, including the company name, position title, and start and end dates. We determine the positions’ chronological order using their arrangement on the profile and their beginning and end dates. We

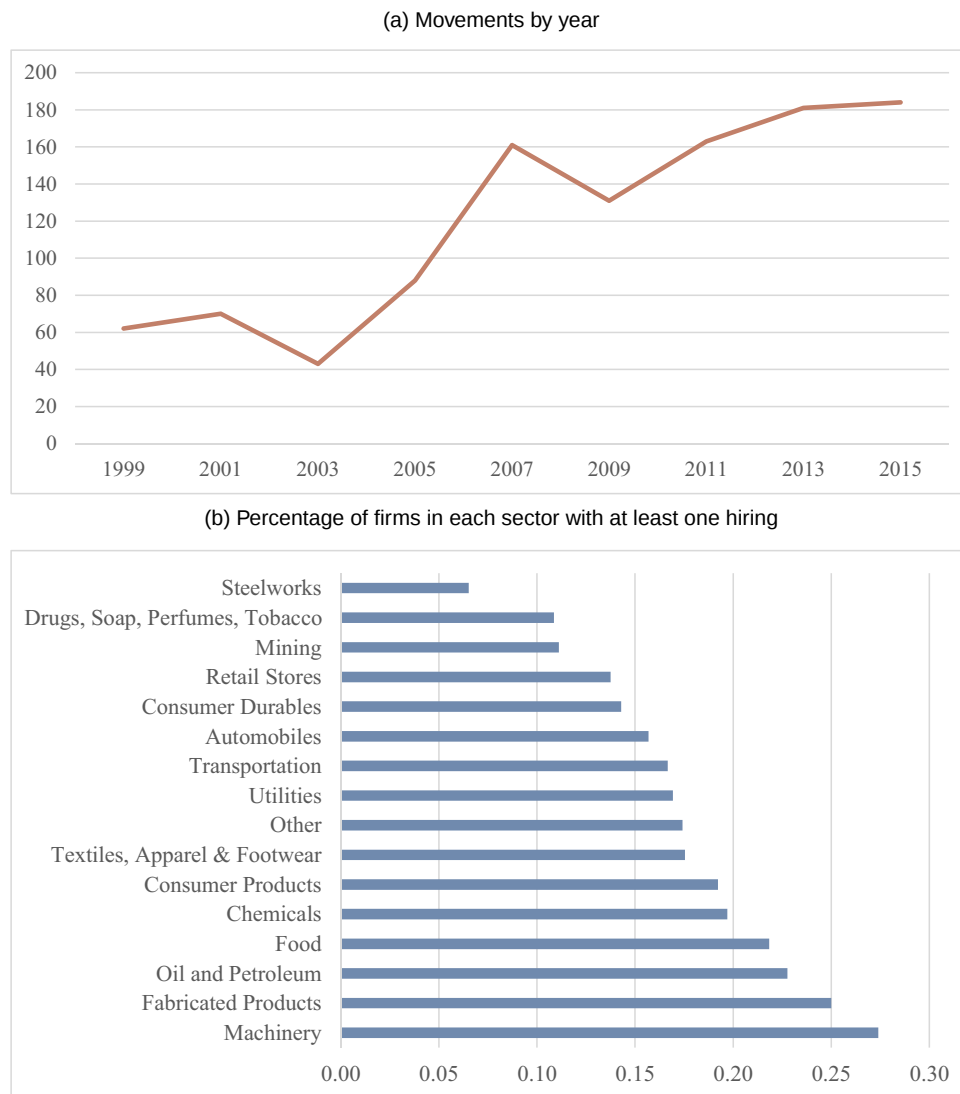
exclude individuals for whom we cannot identify start and end years for their various positions. Next, we employ textual analysis algorithms to match the company names in these resumes to the S&P 1500 firms’ names in the Center for Research in Security Prices (CRSP)–Compustat merged database.¹³ To minimize 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. Whereas this minimizes the number of incorrect matches, it comes at the cost of potentially missing “good” matches.¹⁴

To construct our sample of S&P 1500 tax department employee movements, we require individuals to meet several criteria. First, they must have been employed by the originating (i.e., prior) S&P 1500 firm for at least one full year.¹⁵ Second, they need to move to another sample firm and remain there for at least one year, and the time between jobs must be no greater than 12 months. We focus on employees with general tax responsibilities (i.e., not specific nonincome 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 nonmissing five-year cash ETR (measured in the employee’s final year). Given these data requirements, we identify 1,083 employee movements from 1998 to 2015.¹⁶

We provide some descriptive evidence on these movements in Figure 1. In panel (a), we plot the number of movements in our sample across time. We can 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 machinery, fabricated products, oil and petroleum, and food; each sector has more than 20% of firms with at least one hire. On the other end, we find only one sector (steelworks) with less than 10% of firms with at least one tax department hire from an S&P 1500 firm.

2.2. Other Data Sources and Sample Requirements

We employ financial accounting data from Compustat and equity data from CRSP.¹⁷ We require firm years to have a nonmissing value for *Cash ETR* and each control variable to be included in the sample. When creating the movements-only sample (discussed in Section 4.3), we require that each movement have at least one pre-movement and one postmovement observation to be

Figure 1. (Color online) Descriptive Evidence on Employee Movements

Notes. 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.

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 Standard Industrial Classification (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 1.¹⁸

3. Matching Between Hiring and Supplying Firms

Our first analysis explores the matching process between hiring firms and the firms from which they hire

from (supplying firms). In particular, we examine whether firms tend to hire from other similar or dissimilar firms along certain characteristics in order to understand whether tax-planning knowledge is specific or general in nature (Becker 1962). If firms' operating and tax environments lead to unique tax scenarios that require specific types of tax-planning knowledge, then we expect that firms are 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 likely occur without regard to the similarity between the hiring and supplying firm.

Table 1. Descriptive Statistics for Key Variables

	Mean	Standard deviation	P25	P50	P75
Cash ETR	0.233	0.165	0.128	0.214	0.305
Size	9.196	1.656	7.978	9.221	10.440
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

Notes. This table presents descriptive statistics for the variables employed in our analyses, using the movements-only sample (i.e., the sample employed in Tables 4–8). All variables are defined in the appendix.

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 because of 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- 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:

$$\begin{aligned} Match_{i,j,t} = & \beta_1 Same\ Sector_{i,j,t} + \beta_2 Proximity_{i,j,t} \\ & + \beta_3 Size\ Similarity_{i,j,t} + \beta_4 Global\ Similarity_{i,j,t} \\ & + \beta_5 ETR\ Similarity_{i,j,t} + Fixed\ Effects + \varepsilon_{i,j,t}. \end{aligned} \quad (1)$$

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 sales times negative one (*Size Similarity*), the absolute difference in foreign income times negative one (*Global Similarity*), and the absolute difference between their five-year cash effective tax rates times negative one (*ETR Similarity*).

We present these results in Table 2. We first include each similarity variable separately (columns (1)–(5)) and then jointly (columns (6) and (7)). When examining the similarity variables separately, we find that the coefficients on the similarity variables are all positive and statistically significant.¹⁹ When we examine them jointly, we document that firms are more likely to hire from firms that share industry membership, are closer in geographic proximity, and are similar in scale and global operations. However, in the joint specification, we do not find evidence that ETR similarity materially shapes hiring decisions. Overall, these findings support the idea that tax-related human capital is specific rather than general in nature: firms, on average, demand specific types of tax-related human capital that fit their particular operating and tax environments. The lack of a significant coefficient on ETR similarity is consistent with firms seeking employees from firms with different tax-planning outcomes; for example, firms with less tax planning pursuing employees with tax-planning knowledge built at tax-aggressive firms. These findings suggest that the hiring firm’s need for specific tax-planning knowledge shapes tax department hiring decisions.²⁰

4. Employee Movements and Tax Planning

In this section, we explore whether tax-planning knowledge can diffuse via employee movements by examining whether hiring an employee with tax-planning knowledge leads to an increase in tax planning.

4.1. Identifying Employee Movements with Tax-Planning Knowledge

To identify employee movements likely to lead to the diffusion of tax-planning knowledge, we focus on employees previously employed by firms that achieved low tax burdens for a sustained period (which we refer to as tax-aggressive firms). The underlying assumption is that, by working at a tax-aggressive firm, the employee is likely to have learned about the tax strategies and processes that led the firm to obtain successful tax-planning outcomes and then is able to apply this knowledge at the new firm.

Table 2. Matching Between Hiring and Prior Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	<i>Match</i>						
<i>Same Sector</i>	1.917*** (0.191)					1.747*** (0.184)	1.713*** (0.185)
<i>Proximity</i>		0.115*** (0.005)				0.111*** (0.005)	0.125*** (0.006)
<i>Size Similarity</i>			0.051** (0.025)			0.040 (0.027)	0.113*** (0.020)
<i>Global Similarity</i>				3.289*** (0.844)		1.259 (0.936)	2.265** (1.040)
<i>ETR Similarity</i>					0.413* (0.244)	0.213 (0.241)	0.304 (0.318)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	No	No	No	Yes
Observations	122,613	122,613	122,613	122,613	122,613	122,613	122,613
Adjusted R ²	0.005	0.010	0.002	0.002	0.002	0.012	0.011

Notes. This table presents the results of estimating Equation (1) 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 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 sales of the prior and hiring firms 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)–(6) include year fixed effects; column (7) includes hiring firm and year fixed effects. Standard errors clustered by the hiring 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.

To identify tax-aggressive firms, we consider that, when assessing whether employees are likely to have tax-planning knowledge, hiring firms likely compare the prior firm's tax-planning outcomes against various benchmarks.²¹ For example, a hiring firm may assume that an employee coming from a firm with a very low ETR relative to other firms in that year is likely to have tax-planning knowledge. Alternatively, hiring firms may assess the prior firm's tax-planning outcomes relative to a set of peer firms, such as other firms in that sector and of a similar scale (Balakrishnan et al. 2019). Therefore, to identify employee movements coming from tax-aggressive firms, we compare the prior firm's tax-planning outcomes relative to both the overall economy and a set of peer firms.

To operationalize this definition, we start with the prior firm's five-year cash ETR. We use the cash ETR because it reflects the cash tax savings associated with tax planning, including the effect of deferred tax payments.²² Following prior research (Dyreng et al. 2008), we define this as the sum of cash taxes paid over five years scaled by pretax income minus special items over that same period.²³ We use a five-year window to capture a low ETR that is likely the result of sustainable tax-planning efforts rather than unsustainable tax avoidance or transitory shocks. We then compare the prior firm's ETR to two benchmarks. First, to capture the firm's tax-aggressiveness relative to the overall

economy, we calculate the firm's economy-wide adjusted ETR, which is the five-year cash ETR minus the mean five-year cash ETR of all firms in that year. Second, to capture the firm's tax-aggressiveness relative to its peer firms, we calculate the firm's five-year ETR minus the mean five-year cash ETR of its peer group, which we measure as the mean five-year ETR of the firms in its SIC2 sector and sales quintile, similar to Balakrishnan et al. (2019).²⁴ Finally, we classify employee movements as originating from a tax-aggressive firm if the prior firm ranked in the bottom quartile of all sample movements on both the economy-wide and peer-group adjusted ETRs. By combining both benchmarks, we are agnostic whether tax-planning knowledge is best judged against an economy-wide or peer-group benchmark.²⁵

It is important to note that we do not require that the employee have worked for all five years over which we measure the prior firm's tax planning. Instead, as discussed in Section 2.1, we only require that the employee have worked there for at least the final year. The underlying assumption is that, by being there for at least the last year, the employee learned about the strategies that led to the firm's tax-planning success over the five years.²⁶

4.2. Firm-Panel Sample

To examine whether hiring an employee from a tax-aggressive firm leads to increases in tax planning, we first 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:²⁷

$$\begin{aligned} \text{Cash ETR}_{i,t} = & \beta_1 \text{Hire from TA Firm}_{i,t} + \text{Controls}_{i,t} \\ & + \text{Fixed Effects} + \varepsilon_{i,t}. \end{aligned} \quad (2)$$

Our dependent variable is *Cash ETR*, the hiring firm's cash effective tax rate in year t . Our primary variable of interest is *Hire from TA Firm*, 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 (as defined in Section 4.1) and zero otherwise. It is important to note that, in contrast to the long-run ETR that we use to measure the tax-aggressiveness of the prior firm, here we focus on a one-year ETR measure and examine a short period after the hiring. We employ this approach to detect the shift (if any) in tax avoidance immediately after the new employee's hiring and to mitigate the impact of future events unrelated to the hiring affecting our inferences.²⁸ 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 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 shows 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.

We present the results of estimating Equation (2) using the firm-year panel sample in Table 3. We employ several different models: no controls (column (1)), time-varying controls (column (2)), time-varying controls plus sector and year fixed effects (column (3)), and time-varying controls plus firm and year fixed effects (column (4)). In each model, we document a significant negative coefficient on *Hire from TA Firm*.²⁹ The coefficient 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 two percentage points or 7% of the sample mean (which is 25% in the panel sample).

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 be similar in the absence of a hiring. Although this assumption cannot be tested directly, we assess its plausibility using an event study design to examine the preexisting trends in firms' cash ETR. Specifically, we estimate a simple difference-in-differences model (similar to Equation (2)), in which we saturate the model with event-time indicator variables interacted

Table 3. Main Results, Panel Sample

Sample	(1) All	(2) All	(3) All	(4) All	(5) Stacked	(6) Stacked
Dependent variable:	<i>Cash ETR</i>					
<i>Hire from TA Firm</i>	−0.037*** (0.011)	−0.031*** (0.011)	−0.028*** (0.010)	−0.018* (0.009)	−0.018* (0.010)	−0.020** (0.010)
Controls	No	Yes	Yes	Yes	No	Yes
Sector fixed effects	No	No	Yes	No	No	No
Firm fixed effects	No	No	No	Yes	No	No
Year fixed effects	No	No	Yes	Yes	No	No
Group × Firm fixed effects	No	No	No	No	Yes	Yes
Group × Year fixed effects	No	No	No	No	Yes	Yes
Observations	24,180	24,180	24,180	23,960	122,119	122,119
Adjusted R ²	0.001	0.060	0.102	0.264	0.293	0.327

Notes. This table presents the results of estimating Equation (2) on the firm-year panel sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax income minus special items. The independent variable of interest is *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 firm and the following two years, zero otherwise. We define the prior firm as tax-aggressive if it had both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR in the bottom quartile of the sample of movements and zero otherwise. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. Column (1) includes no control variables or fixed effects. Columns (2)–(4) include some combination of sector (defined at the SIC2-level) or firm fixed effects, 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)–(4) use all firm-years for S&P 1500 firms between 1995 and 2018. Columns (5) and (6) present the findings using a stacked regression design as described in Barrios (2021) and Cengiz et al. (2019) and includes group-firm and group-year fixed effects (both columns (5) and (6)) and time-varying firm-level control variables (column (6) only). 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.

with *Hire from TA Firm*. We plot the coefficients on the interactions of *Hire from TA Firm* and the following periods: year $t - 4$ and before, year $t - 3$, year $t - 2$, year t (the hire year), year $t + 1$, year $t + 2$, and year $t + 3$ and beyond. 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 along with their 90th percentile confidence intervals in Figure 2.³⁰ There are several findings to note. First, firms hiring from a tax-aggressive firm experience similar trends in cash ETRs compared with other firms prior to the hire. The lack of significant differences in pretreatment trends between the two groups supports the parallel trends assumption. Second, we find a decline in the cash ETR in the employee's first year at the hiring firm (year t), consistent with the employee's hiring leading to an increase in tax planning. Finally, this figure speaks to the persistence of the effect. Our primary specification focuses on a short window around the employee's hiring to better attribute any tax-planning changes to the employee. However, this raises the question as to whether the tax-planning benefits of the employee's hiring are persistent or transitory. Figure 2 documents a significant

and negative effect for years three and beyond, suggesting that hiring from a tax-aggressive firm leads to a sustained tax-planning benefit.

We conduct additional analyses to 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 et al. 2019). The stacked design estimates (presented in columns (5) and (6) of Table 3) are very similar to our panel regression estimate, corroborating that our results are unlikely to be driven by the negative weighting issues.

4.3. Movements Sample

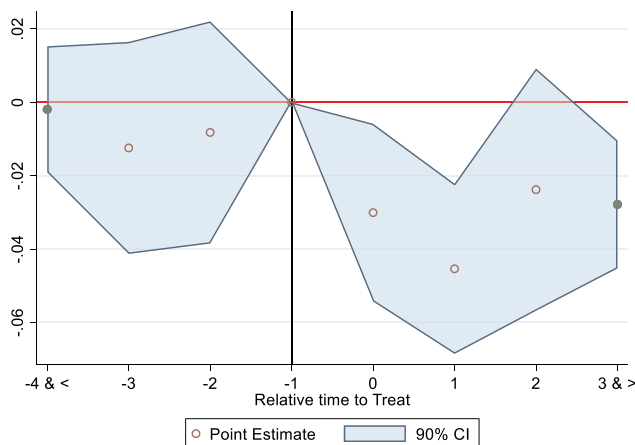
The panel design employed in Section 4.2 compares tax planning by firms hiring an employee from a tax-aggressive firm to other firms. However, a firm hiring a tax department employee may undergo several other changes simultaneously. Moreover, those changes, rather than the employee's tax-planning knowledge, might lead 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, we focus on a sample of employee movements and compare tax-planning outcomes for firms that hire tax department employees from tax-aggressive firms to firms that hire tax department employees from non-tax-aggressive firms for the remainder of our analysis. Specifically, we estimate the following regression:

$$\text{Cash ETR}_{i,t} = \beta_1 \text{Hire from TA Firm} \times \text{Post}_{i,t} + \text{Controls}_{i,t} + \text{Fixed Effects} + \varepsilon_{i,t}. \quad (3)$$

Hire from TA Firm is a time-invariant, movement-level indicator variable equal to one if the movement involves hiring from a firm identified as tax-aggressive, using the process described in Section 4.1. *Post* equals one in the year 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.

We estimate this regression using a six-year window around each employee's hiring with three years pre-movement and three years postmovement (including 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 nonhiring 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

Figure 2. (Color online) Treatment Dynamics



Notes. This figure examines the pretreatment and posttreatment 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 (2)), in which 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: years $t - 4$ and prior, year $t - 2$, year t (the hire year), year $t + 1$, year $t + 2$, and years $t + 3$ and beyond; these coefficients are represented by small circles. We also present 90% confidence intervals (the lines above and below the coefficient estimates) using standard errors clustered by firm. When these intervals include zero, this indicates that the coefficient estimate for that event-year is not statistically significant at conventional levels.

with a two-way fixed effect regression implemented in a staggered setting.

We include various fixed effects and control variables in this design 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 around hiring a tax department employee regardless of the former firm's tax-planning outcomes.³¹ Third, we include sector-year fixed effects to account for industry-level shocks (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 earlier.

We present the results of estimating Equation (3) using the movements sample in column (1) of Table 4. We continue to find a significant negative coefficient on *Hire from TA Firm* $\times$ *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 2.7-percentage-point reduction in *Cash ETR* on average or 11% of the mean *Cash ETR* in the movements-only sample.

We conduct several robustness tests of this finding. First, we employ different thresholds for defining a tax-aggressive prior firm: the bottom 20% (column (2)) and the bottom 30% (column (3)) of both adjusted ETRs. In both models, we find that the coefficient on the interaction remains significant and close in value to that of column (1). Second, we use different approaches to defining tax-aggressive former firms: the bottom quartile of movements by the sector size adjusted ETR (as described in Section 4.1) or the bottom quartile of movements by the raw (i.e., unadjusted) ETR. We continue to find a significant negative coefficient on *Hire from TA Firm* $\times$ *Post* using either approach (columns (4) and (5)). Finally, to further address the nonrandom 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 so that the means and variances of the time-varying control variables are similar across movements when *Hire from TA Firm* equals one and zero. We continue to find a similar coefficient on *Hire from TA Firm* $\times$ *Post* after using these weights in our regression (column (6)).³²

In our primary design, we focus on a ± 3 -year window around the employee hiring to attribute better the decline in the hiring firm's ETR to the arrival of the new employee. However, given our interest in sustained tax

Table 4. Main Results, Movements Sample

Specification	(1) Baseline	(2) Baseline 20% cutoff	(3) Baseline 30% cutoff	(4) Baseline Industry-size adjusted ETR	(5) Baseline Unadjusted ETR	(6) Entropy balanced Baseline
Low ETR Firm definition	Baseline					
Dependent variable	Cash ETR					
<i>Hire from TA Firm</i> $\times$ <i>Post</i>	−0.027*** (0.010)	−0.034*** (0.011)	−0.025*** (0.009)	−0.022** (0.009)	−0.021** (0.009)	−0.021** (0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector $\times$ Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,965	5,965	5,965	5,965
Adjusted R^2	0.418	0.418	0.418	0.418	0.417	0.416

Notes. This table presents the results of estimating Equation (3) on the movements sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax 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 both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR 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. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. 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. Columns (2) and (3) employ the main sample and specification except that they use different definitions for *Hire from TA Firm*: measured using the bottom 20% of both adjusted ETRs (column (2)) or bottom 30% of both adjusted ETRs (column (3)). In column (4), *Hire from TA Firm* is measured using the bottom quartile percentage of only the sector-size adjusted five-year cash ETR. In column (5), *Hire from TA Firm* is measured using only the unadjusted five-year cash ETR. In column (6), 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. 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.

planning, we explore whether the tax-planning benefits of hiring from a tax-aggressive firm are sustained over a longer window. Ex ante, this is unclear. On the one hand, the ETR benefit of hiring such an employee may be maintained if the tax-planning knowledge helps the hiring firm implement more sustainable tax strategies. On the other hand, the tax-planning advantage of hiring such an employee may only provide a temporary tax advantage over other firms if they subsequently improve their tax planning (either via hiring employees themselves or other means). We examine this idea by extending the posthiring window successively by one year up to an additional five years over our baseline specification (eight years posthiring in total). We present these analyses in Table 5. In each sample, we continue to find a negative and significant coefficient on *Hire from TA Firm* $\times$ *Post*. Furthermore, the coefficient is stable across models. Overall, these findings suggest that the tax-planning benefit of hiring from a tax-aggressive firm appears to be sustained for several years after the hiring.

We conduct several additional tests to assess the robustness of our findings. We briefly discuss these tests here and provide more details on the implementation and findings in the online appendix. First, we show that our findings are not driven by the number of tax department hires, but rather that the employee was hired from a tax-aggressive firm. Second, we document that our findings are robust to accounting for other contemporaneous events (large mergers, CEO and chief financial officer (CFO) turnovers, and executive hiring from tax-aggressive firms) and alternative connections

with the hiring firm that prior research has linked to tax planning, such as banks (Gallemore et al. 2019), auditors (McGuire et al. 2012), and boards (Brown 2011). Third, we show that our results are not 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. Finally, we find no evidence of an association between hiring a tax employee from a tax-aggressive firm and changes in non-tax-planning outcomes, such as pretax profitability or investment. This supports the notion 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.³³

5. 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 enable us to explore whether tax-planning knowledge is more portable between firms with similar characteristics; in other words, whether it is a form of specific human capital. Second, they allow us to explore whether the employee's tax-planning knowledge developed at the prior firm is the mechanism responsible for our findings. 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.

Table 5. Main Results, Extended Posthiring Window

	(1)	(2)	(3)	(4)	(5)	(5)
Posthire window	Three years (base)	Four years	Five years	Six years	Seven years	Eight years
Dependent variable	Cash ETR					
<i>Hire from TA Firm</i> $\times$ <i>Post</i>	−0.027*** (0.010)	−0.023** (0.009)	−0.023** (0.009)	−0.022** (0.009)	−0.021** (0.009)	−0.022** (0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector $\times$ Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	6,930	7,803	8,581	9,267	9,860
Adjusted R^2	0.418	0.409	0.407	0.400	0.397	0.400

Notes. This table presents the results of estimating Equation (3) on the movements sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax 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 both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR 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. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. We require at least one observation before and after the movement. Column (1) employs 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). In the subsequent columns, we expand the length of the posthiring window. 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.

5.1. Similarity Between Hiring and Former Firms

First, 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 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) have unique tax environments because of the tax breaks tailored to their operations. Therefore, tax-planning knowledge developed in another sector may be less valuable. 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 tends to require a unique set of skills and experience. These skills are unlikely to be developed or learned when working in 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 reestimate Equation (3) within subsamples of movements in which the hiring and former firms exhibit more or less similarity. We examine three potential aspects of similarity: sector, global operations, and firm size. We report these findings in Table 6 with columns (1) and (2) addressing sector similarity, columns (3)–(6) addressing global operations similarity, and columns (7) and (8) addressing size similarity.

First, to explore whether tax-planning knowledge exhibits sector specificity, we examine whether the tax-planning benefit of hiring from a tax-aggressive firm is concentrated within movements that occur between two firms in the same or different sectors. We classify former and hiring firms as being in the same sector if

Table 6. Hiring and Former Firm Similarity

Sample	(1) Same sector	(2) Different sector	(3) High FI similarity	(4) Low FI similarity	(5) Country overlap	(6) No country overlap	(7) High size similarity	(8) Low size similarity	(9) High similarity	(10) Low similarity
Dependent variable	<i>Cash ETR</i>									
<i>Hire from TA Firm</i> × <i>Post</i>	−0.037* (0.021)	−0.022* (0.012)	−0.061*** (0.017)	−0.001 (0.013)	−0.031** (0.015)	−0.023 (0.018)	−0.029* (0.017)	−0.019 (0.012)	−0.045*** (0.015)	−0.001 (0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,267	4,524	2,849	2,873	2,877	1,579	2,808	2,890	3,307	2,421
Adjusted R^2	0.434	0.423	0.425	0.441	0.448	0.450	0.354	0.488	0.402	0.446

Notes. This table presents the results of estimating Equation (3) on the movements sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax 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 both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR 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. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. 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), and 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 employee movements in which the absolute value of the difference between the hiring firm's premovement 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 when there is overlap (no overlap) in the countries in which the prior firm has a material subsidiary in the employee's final year and the countries in which the hiring firm has a material subsidiary in the employee's start year. Columns (5) and (6) are estimated only using the movements in which the hiring firm has nonzero pretax foreign income in the premovement period to focus on hiring firms with material foreign operations. Column (7) ((8)) estimates the regression on the subsample of employees who move between two firms when the absolute value of the difference between the hiring firm's premovement sales and the prior firm's sales in the employee's final year is below (above) the sample median. Column (9) ((10)) estimates the regression on the subsample of movements that are classified as being similar along at least (less than) two of the dimensions in columns (1), (3), (5), and (7). 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.

they 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).³⁴ We find evidence consistent with tax-planning knowledge exhibiting evidence of sector specificity: the coefficient on *Hire from TA Firm* $\times$ *Post* is negative and statistically significant for movements within the same sector.³⁵ We also find that movements across sectors lead to tax-planning increases, consistent with some tax-planning knowledge being applicable even outside of the sector in which it was developed.

Next, we examine whether tax-planning knowledge exhibits specificity in the tax jurisdictions in which the firm operates, which we operationalize using the extent of global operations. To measure global operational similarity, we compute the absolute value of the difference in the foreign income (scaled by total assets) between the two firms. Then, we classify firms as being similar if they are below the sample median of this difference.³⁶ We then reestimate Equation (3) within the two subsamples: high and low global operational similarity, which we report in columns (3) and (4) of Table 6, respectively. We find evidence consistent with tax-planning knowledge exhibiting strong tax jurisdiction specificity: the coefficient on *Hire from TA Firm* $\times$ *Post* is negative and statistically significant in the subsample of movements with similar global operations. It is statistically insignificant in the low global operations similarity subgroup.

We build on this result by examining whether there is an overlap in the hiring and prior firms' foreign footprints. If the prior and hiring firm share similar foreign footprints, then this could facilitate the diffusion of tax-planning knowledge as the tax strategies learned at the prior firm may be more applicable to the hiring firm. For this analysis, we focus on hiring firms that average positive pretax foreign income in the prehire period. These firms are likely to have material foreign operations and, thus, potentially be affected by the overlap (or lack thereof) in their foreign footprint. We measure foreign footprints using the countries in which the firm discloses it has material subsidiaries according to the Exhibit 21 of Form 10-K. We classify a movement as having a country overlap if the hiring firm has a material subsidiary in at least one country that the prior firm had in the premove-ment period. We reestimate Equation (3) within the subsamples of the movements with and without a country overlap and present the findings in columns (5) and (6) of Table 6. We find a negative and significant coefficient on *Hire from TA Firm* $\times$ *Post* in the subsample of movements when there is a country overlap. In contrast, this coefficient is statistically insignificant in the subsample of movements without country overlap. These findings suggest that overlap in foreign footprints facilitates the diffusion of tax-planning knowledge and are consistent

with tax-planning knowledge exhibiting specificity in global operations.

We also examine whether tax-planning knowledge exhibits specificity in the firm's scale. To measure size similarity, we compute the absolute value of the difference in total sales between the two firms and then classify firms as being similar if they are below the sample median of this difference.³⁷ We then reestimate Equation (3) within the high and low size similarity subsamples and report these results in columns (7) and (8) of Table 6. Again, we find evidence of size specificity: the coefficient on *Hire from TA Firm* $\times$ *Post* is negative and significant in the high similarity size group, whereas it is insignificant in the low size similarity group.

Finally, we examine a combined measure of the similarity between the hiring and prior firms, which classifies a movement as exhibiting high (low) similarity if the hiring and prior firms are similar in at least (less than) two of the previously examined characteristics: sector membership, global operations, subsidiary country overlaps, and firm size. We report these findings in columns (9) and (10) of Table 6. We again find evidence of specificity in tax-related human capital: the coefficient on *Hire from TA Firm* $\times$ *Post* is negative and significant in the high similarity group, whereas it is insignificant in the low similarity group. Furthermore, the coefficients across these two models are statistically different at the 5% level using a one-tailed test (p -value of 0.0275; results untabulated).³⁸

Overall, the findings in Table 6 largely support the idea that tax-planning knowledge diffusion is determined, in part, by the similarity between the tax environments in which the knowledge was developed (i.e., at the prior firm) and in which it will be applied (i.e., at the hiring firm). Combined with the Table 2 findings, these results also suggest that tax-planning knowledge is highly specific in nature.

5.2. Employee Role and Experience

In our next set of analyses, we examine whether the diffusion of tax-planning knowledge varies with the employee's role and experience. We first examine the employee's role at the prior firm and specifically whether the employee held a director- or non-director-level position. 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. However, it is unclear whether being centrally responsible for the prior firm's tax-planning success is a prerequisite for successfully transferring tax-planning knowledge to the hiring firm. Instead, simply being involved with the prior firm's tax planning may be sufficient to develop tacit tax-planning knowledge. 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 reestimate Equation (1) within both samples and report these findings in columns (1) and (2) of Table 7, panel A. In both subsamples, the coefficient on *Hire from TA Firm* $\times$ *Post* is negative; however, it is only statistically significant in the subsample of movements involving nondirectors. These findings suggest that the employee did not necessarily have to be in charge of the prior firm's tax planning to diffuse the tax-planning knowledge to the hiring firm.

Next, we examine whether the role that the employee is hired into matters in terms of whether the tax-planning knowledge is diffused. On the one hand, tax department employees hired into a director-level role

have a more substantial impact on the hiring firm's tax planning as they are able to use their tax-planning knowledge to materially affect key tax department decisions. On the other hand, the employee may be able to diffuse the knowledge even if not hired into a director position as long as the employee can communicate that information to other tax department employees. To examine this idea, 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 7, panel A. We find that the interaction term

Table 7. Employee Position and Experience

Panel A: Position						
Sample	(1) Director at prior firm	(2) Nondirector at prior firm	(3) Director at hiring firm	(4) Nondirector at hiring firm	(5) Director at either	(6) Nondirector
Dependent variable	<i>Cash ETR</i>					
<i>Hire from TA Firm</i> $\times$ <i>Post</i>	−0.044 (0.037)	−0.028*** (0.011)	−0.050* (0.029)	−0.015 (0.010)	−0.050** (0.025)	−0.013 (0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,134	4,613	1,708	3,978	2,028	3,664
Adjusted R^2	0.350	0.424	0.326	0.456	0.344	0.441
Panel B: Experience						
Sample	(1) High tax experience	(2) Low tax experience	(3) High prior firm experience	(4) Low experience at prior firm	(5) High experience	(6) Low experience
Dependent variable	<i>Cash ETR</i>					
<i>Hire from TA Firm</i> $\times$ <i>Post</i>	−0.042** (0.019)	−0.010 (0.014)	−0.038** (0.015)	−0.016 (0.015)	−0.040*** (0.014)	0.000 (0.017)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,012	2,728	2,842	2,866	4,001	1,706
Adjusted R^2	0.383	0.431	0.411	0.402	0.418	0.400

Notes. This table presents the results of estimating Equation (3) on the movements sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax 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 both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR 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. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. 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. In panel A, column (1) ((2)) estimates the regression on the subsample of employees who held a director (nondirector) position at the prior firm. Column (3) ((4)) estimates the regression on the subsample of employees hired into a director (nondirector) position at the hiring firm. Column (5) ((6)) estimates the regression on the subsample of employees that were either (neither) a director at the prior firm or at the hiring firm. In panel B, column (1) ((2)) estimates the regression on the subsample of employees with total tax-related experience above (below) the sample median prior to joining the hiring firm. Column (3) ((4)) estimates the regression on the subsample of employees whose experience at the prior firm is above (below) the sample median. Column (5) ((6)) estimates the regression on the subsample of employees who have either (neither) above-median total tax-related experience or above-median experience at the prior firm. 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.

is negative and economically significant when the employee was hired into a director-level role, whereas it is insignificant for nondirector movements. Finally, we combine these two tests in column (5) ((6)) of Table 7, panel A by examining whether employee movements are associated with larger increases in tax planning when the employee is either (neither) a director in the prior firm or the hiring firm. We find a negative and significant coefficient on *Hire from TA Firm* $\times$ *Post* in the subsample of movements in which the employee was a director at the prior firm or is hired into a director-level position but not in the subsample of other movements. Furthermore, using a one-tailed test, these coefficients are statistically different at the 10% level (p -value of 0.095; results untabulated).³⁹ Our results suggest that the diffusion of tax-planning knowledge is more likely to occur when the employee was in a position that directed the prior firm's successful tax planning or is hired into a position that is responsible for directing the hiring firm's tax-planning strategy.

Third, we focus on the employee's 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 required to successfully develop and implement tax-planning strategies. Thus, they may be better able to deploy this knowledge in the new firm. To test these ideas, we reestimate Equation (3) within different subsamples, which we report in Table 7, panel B. In columns (1) and (2), 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 (3) and (4), we split the movement sample into two groups based on whether the hired employee's experience in the prior position (i.e., at the tax-aggressive prior firm) was above the sample median.⁴⁰ We find a negative and statistically significant coefficient on the interaction in columns (1) and (3) (the high-experience subsamples) but an insignificant coefficient in columns (2) and (4) (the low-experience subsamples). Finally, as in the prior analyses, we estimate the regression on the subsample of employees that either (neither) have above-median total tax experience or above-median experience at the prior firm and present these results in column (5) ((6)). We document a negative and significant coefficient on *Hire from TA Firm* $\times$ *Post* in the high-experience subsample (column (5)) but not in the low-experience subsample (column (6)). Moreover, these coefficients are statistically different at the 5% level using a one-tailed test (p -value of 0.0435; results untabulated).⁴¹

Overall, the findings in Table 7, panel B are consistent with the idea that greater tax-planning knowledge, measured either by the employee's total tax experience or the experience at the tax-aggressive prior firm, is responsible for our findings.⁴² Furthermore, when combined

with the Table 2 findings, these results again suggest that tax-planning human capital is specific in nature in that the tax experience at the prior firm is most beneficial to economically similar hiring firms (Becker 1962). Additionally, these findings suggest that tax experience aids in the diffusion of tax-planning knowledge, potentially because it facilitates the communication and implementation of the knowledge within the hiring firm.

5.3. Firm Characteristics

We also 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, because smaller firms likely have smaller tax departments prehiring, 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. Furthermore, smaller firms may have less access to alternative channels to obtain tax-planning knowledge (i.e., large financial intermediaries). To examine this idea, we reestimate Equation (3) within three subgroups based on the average number of employees (per Compustat) in the prehiring period. We report these findings in columns (1)–(3) of Table 8. We find evidence consistent with our prediction: the coefficient on the interaction term is negative and significant in the smallest size tercile and statistically insignificant in the other two terciles.⁴³

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, Chyz et al. 2021). If a firm engages its auditor for tax services, it could signal that the firm is seeking to obtain tax-planning knowledge from outside the firm, which could lower the potential tax-planning impact from an internal hire. In contrast, a firm not engaging its auditor for tax services is likely to benefit 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 nonzero fees paid to its auditor for tax services (obtained from Audit Analytics) in the three years prehiring.⁴⁴ We report the results of reestimating Equation (3) within these subsamples in columns (4) and (5) of Table 8. Consistent with our expectation, we find that the tax-planning benefit of hiring an employee from a tax-aggressive firm is statistically significant within the sample of firms that did not pay tax service fees to their auditor. However, it is not significant within firms that did engage their auditor for tax services.⁴⁵

Third, we combine the prior two tests by estimating the regression on the subsample of employee movements in which the hiring firm is either (neither) a smaller firm

Table 8. Hiring Firm Characteristics

Sample	(1) Large firms	(2) Medium firms	(3) Small firms	(4) APTS > 0	(5) APTS = 0	(6) Small firm or APTS = 0	(7) Other firms
Dependent variable	Cash ETR						
Hire from TA Firm × Post	−0.013 (0.012)	−0.024 (0.016)	−0.040* (0.022)	−0.014 (0.016)	−0.032** (0.012)	−0.034*** (0.012)	−0.002 (0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Movement fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,964	1,837	1,872	2,944	2,888	4,101	1,716
Adjusted R ²	0.735	0.432	0.408	0.332	0.547	0.458	0.362

Notes. This table presents the results of estimating Equation (3) on the movements sample. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax 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 both an economy-wide adjusted five-year cash ETR and a sector-size adjusted five-year cash ETR 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. We provide more details on the measurement of *Hire from TA Firm* in Section 4.1. 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)–(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 years prior to the hiring of the employee. Columns (6) ((7)) estimates the regression on the subsample of hiring firms who are classified as either small firms or have no auditor-provided tax services (are nonsmall firms and have nonzero auditor provided tax services). 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.

or a firm that does not have any audited-provided tax service fees. We present these results in column (6) ((7)) of Table 8. We find a negative and significant coefficient on *Hire from TA Firm* × *Post* in the subsample of hiring firms that are either smaller or have no auditor-provided tax services. In contrast, this coefficient is not significant in the subsample movements involving other hiring firms, and these coefficients are statistically different at the 10% level using a one-tailed test (*p*-value of 0.077; results untabulated).⁴⁶ Again, we believe that these findings suggest that the tax-planning benefit of hiring from a tax-aggressive firm is stronger for firms that are either smaller or did not engage their auditor for tax services.⁴⁷

6. Employee Departures and Tax Planning

Our final analyses explore whether tax planning changes after the departure of a tax department employee. Ex ante, it is unclear whether we would observe a change. On the one hand, the movement of the tax department employee does not necessarily lead to the loss of tax-planning knowledge. Specifically, the employee moving between firms may simply be spreading the knowledge to the new firm, whereas that knowledge remains imbued in the remaining tax department employees at the prior firm. This suggests that the departure of a tax department employee is not necessarily associated with changes in the firm’s tax planning. On the other hand, if effective tax

planning requires repeated investments to be sustained, then the departure of a tax employee may be associated with a decline in tax planning (i.e., an increase in the ETR).

To examine this idea, we begin with our sample of employee movements and now focus on the prior firm (as opposed to the hiring firm as in the preceding analyses). To identify a set of control firms that do not experience an employee departure (at least according to our data collection), we match each departure firm to the S&P 1500 firm in the same industry and size portfolio (sales quintile as in our primary design) with the closest five-year cash ETR, requiring that the two ETRs are within two percentage points.⁴⁸ We create a ±3-year window around the employee’s departure with the employee’s final year at the firm being in the predeparture period, and the postdeparture period consisting of the three years after the departure. Using this sample, we estimate the following regression:

$$Cash\ ETR_{i,t} = \beta_1 Departure\ Firm \times Post_{i,t} + Controls_{i,t} + Fixed\ Effects + \varepsilon_{i,t}.$$

(4)

Departure Firm is an indicator variable equal to one if the firm was a prior firm in our employee movements sample. *Post* is an indicator variable equal to one if the year occurs in the postemployee departure period for either the treatment or control firms. Therefore, the coefficient on *Departure Firm* × *Post* captures the incremental change

Table 9. Employee Departures

Sample	(1)	(2)
	All firms	TA former firms only
Dependent variable	<i>Cash ETR</i>	
<i>Departure</i> × <i>Post</i>	0.005 (0.007)	0.004 (0.011)
Controls	Yes	Yes
Movement fixed effects	Yes	Yes
Event-time fixed effects	Yes	Yes
Sector × Year fixed effects	Yes	Yes
Observations	6,422	1,220
Adjusted R^2	0.420	0.384

Notes. This table presents the results of estimating Equation (4). We employ a sample of departure firms, which are firms that experienced a tax department employee movement to another firm, and matched control firms, which are S&P 1500 firms do not experience a tax department employee movement to another firm. Each departure firm is matched to the control firm in the same SIC2 sector and firm size quintile (measured using sales) with the closest five-year cash ETR in the year of the departure; we require the five-year cash ETRs to be within two percentage points. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pretax income minus special items. The independent variable of interest is the interaction of *Departure Firm*, an indicator variable equal to one if the firm experienced a tax department employee movement to another firm, and *Post*, an indicator variable equal to one for years after the event year (which is the employee's departure year for departure firms or the same year for the matched control firms). This regression is estimated on a six-year window around the movement, up to three years prior to and three years after the movement. Column (1) uses all departure firms. Column (2) only examines departure firms classified as tax-aggressive former firms, using the measurement scheme described in Section 4.1. 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.

in the ETR for the departing firms after the employee's departure relative to the matched control sample. We include the same control variables and fixed effects as in our primary analyses.

We report these results in Table 9. In short, we find little evidence of a significant change in the departure firm's ETR after the employee's departure, relative to the matched control firms: the coefficient on *Departure Firm* × *Post* in column (1) is statistically insignificant at conventional levels. In column (2), we focus on the subsample of departures that involve a firm identified as tax-aggressive in our prior analyses and continue to find little evidence of a change in ETR after the employee's departure.⁴⁹ Overall, these findings suggest that firms, including tax-aggressive firms, do not experience a change in their tax planning upon the departure of tax department employees. This evidence is consistent with tax-planning knowledge not leaving the firm after the employee departure as it is retained in the remaining tax department employees.

7. Conclusion

We examine the role of the labor market in disseminating tax-planning knowledge across firms. To examine this idea, we construct a novel data 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. Whereas employee movements between firms are admittedly nonrandom, we employ several research designs and robustness tests to mitigate alternative explanations for our findings.

Further tests suggest that tax-planning knowledge (and, thus, tax-related human capital) is highly specific in nature: the increase in tax avoidance is concentrated in movements in which the hiring and former firms exhibit greater similarity (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. We also find that the tax-planning benefit of hiring an employee from a tax-aggressive firm is concentrated in employee movements in which the employee has greater experience and is more likely to have control over the hiring firm's tax planning, consistent with tax-planning knowledge being the mechanism behind our findings. Finally, we do not document a tax-planning change upon an employee's departure, suggesting that employee movements simply spread tax-planning knowledge to the hiring firm without reducing it at the prior firm.

Our findings contribute to the accounting, organizational science, and tax literatures. First, we provide some of the first large-sample empirical evidence on the diffusion of accounting knowledge via employee movements across firms. Second, our findings suggest that tax-planning knowledge is specific in nature; it is more applicable to environments similar to those in which the knowledge was developed. This finding has implications for the literature examining the role of incentives in tax planning, suggesting that the applicability of the employee's tax-planning knowledge to the firm's operating environment is a natural limit to the efficacy of such incentives in encouraging greater tax planning. Third, we develop and implement a novel methodological approach that exploits employee movements between firms. The common theme behind these contributions is that we incorporate insights from labor economics and organizational science to explain tax and accounting outcomes. Future research in this area could explore whether tax department employee movements impact other tax-related outcomes, such as the book ETR or the unrecognized tax benefit liability, or whether the nontax accounting employee movements affect financial reporting and internal control outcomes.

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Appendix. Variable Definitions

Table A.1. Main Variables

Variable	Definition
<i>Cash ETR</i>	Cash taxes paid scaled by pretax income (adjusted for special items).
<i>Hire from TA Firm</i>	Indicator variable equal to one if the prior firm’s long-run (i.e., five-year) cash ETR is in the bottom quartile of the sample and zero otherwise.
<i>Post</i>	Indicator variable equal to one in the year the employee is hired and all later years and zero otherwise.

Table A.2. Control Variables

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

Table A.3. Matching Variables

Variable	Definition
<i>Match</i>	Indicator variable if the observation represents a hiring event and zero otherwise multiplied by 100.
<i>Same Sector</i>	Indicator equal to one if the hiring firm and the prior firm are in the same industry (either the same two-digit SIC industry or industry peers as defined by Hoberg and Phillips 2010, 2016) and zero otherwise.
<i>Proximity</i>	Miles (scaled by 100) between the hiring firm’s headquarters and the employee’s prior firm’s headquarters multiplied by -1 .
<i>Size Similarity</i>	Natural logarithm of the absolute value of the difference between the hiring firm’s total sales and the prior firm’s total sales multiplied by -1 .
<i>Global Similarity</i>	Absolute value of the difference between the hiring firm’s foreign income and the prior firm’s foreign income (each scaled by assets) multiplied by -1 .
<i>ETR Similarity</i>	Absolute value of the difference between the hiring firm’s five-year cash ETR and the prior firm’s five-year cash ETR multiplied by -1 .

Endnotes

¹ This experience is especially important given the complex and uncertain nature of corporate tax planning (Guenther et al. 2017, Dyreng et al. 2019, Hoppe et al. 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 various different objectives and interactions with multiple parties within the firm.

² This knowledge diffusion can occur regardless of whether the hired employee is placed in a position that allows the employee to direct the hiring firm's tax planning or instead communicate and help implement the strategies that aided in the success of the prior firm's tax planning.

³ We use "successful tax-planning firm" and "tax-aggressive firm" interchangeably to refer to firms that sustain low effective rates. We do not use these terms to imply that the underlying tax positions are risky or illegal.

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

⁵ The extant research on how individuals shape corporate tax planning generally ignores the role of experience and, therefore, implicitly assumes that tax-planning knowledge is general in nature.

⁶ By "concentrated," we mean that the cash ETR decline associated with hiring these employees is statistically significant in the predicted subsample of movements but not in the other subsample of movements.

⁷ Prior research linking human capital and organizational performance generally focuses 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 among the firm's units (Becker and Gerhart 1996).

⁸ 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 et al. (1998), Robinson et al. (2010), and Gallemore and Labro (2015).

⁹ We include any firm that was an S&P 1500 member between 1993 and 2015 (as our data collection efforts occurred over 2016 and 2017). If a firm is never a member of the S&P 1500 during this time period, it is not included in our analyses.

¹⁰ 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.

¹¹ 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 captures these employees to the extent that one of the following conditions is satisfied: (1) the employee listed the parent company's name in the employee's 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 captures most of the tax employees who work at the firm and listed their resume on the website, especially because the tax department is located at the corporate headquarters for most firms.

¹² 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 the title includes the word "tax."

¹³ 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.

¹⁴ 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 because 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 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.

¹⁵ 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 (i.e., the resume must indicate at least a one-year gap between the beginning and ending years for the position). 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.

¹⁶ 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.

¹⁷ 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. Additionally, we employ their method to correct for errors in foreign tax expense, foreign and domestic pretax income, total pretax income, federal current tax expense, and worldwide current tax expense.

¹⁸ We winsorize non-ETR continuous variables at the 1st and 99th percentiles.

¹⁹ We use two-tailed statistical tests throughout the paper unless stated otherwise.

²⁰ 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. In an untabulated analysis, we explore whether matches are more likely if the employee is coming from a tax-aggressive firm (as we define in Section 4.1). When we reestimate Equation (1) additionally adding *Hire from TA Firm* as an independent variable, we find that the coefficient on this variable is statistically insignificant, suggesting these employees are not more likely to be a match. Furthermore, the other coefficients remain unchanged in this model.

²¹ To the extent that we do not correctly attribute a prior firm's tax-aggressiveness, this would bias us toward not finding a tax-planning benefit from hiring an employee from that firm as tax-planning knowledge would be unlikely to be obtained via the employee's hiring.

²² Furthermore, focusing on a single tax-planning proxy allows us to simplify our analyses.

²³ We winsorize cash ETRs at zero and one and set cash ETRs equal to missing if the denominator is negative, consistent with prior research (Dyreng et al. 2008).

²⁴ Sector membership and sales are sorted independently. We use sales to identify peers of a similar scale as other measures, such as total assets, may understate scale for certain firms and industries (i.e., those with internally generated intangibles). Our findings are robust to using total assets or market capitalization rather than sales. We note that it is possible that variation in ETRs across these two characteristics (sector and scale) could reflect the concentration of tax-planning human capital along these dimensions. If so, controlling for those characteristics in determining a tax-aggressive firm would reduce our ability to capture the effects of tax-planning human capital.

²⁵ Untabulated results show that the five-year cash ETR for the prior firms designated as tax aggressive firms is 9% on average and are around 15 percentage points lower than the economy-wide and sector-size benchmarks on average. These statistics are consistent with these firms maintaining low cash ETRs over a long period both on an absolute basis and relative to key benchmarks. We examine the robustness of our findings to alternative measurement approaches in Section 4.3.

²⁶ In practice, however, most employees in our sample are at the prior firm for the entire five-year period. We examine how our results vary with the length of the employee's tenure at the prior firm in Section 5.2.

²⁷ 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.

²⁸ The downside to focusing on a short period around the employee's hiring is that it is unclear whether any change in tax planning after the employee's hiring is sustained. We examine whether these tax-planning changes are sustained for periods longer than three years subsequently and in Section 4.3.

²⁹ Whereas we employ a five-year cash ETR for our primary analyses because it captures sustained tax avoidance, inferences are similar if we instead employ shorter run ETRs (i.e., three-year; results untabulated).

³⁰ This figure is produced from a generalized difference-in-differences regression that includes sector fixed effects. We do not include the time-varying control variables as doing so can change the variation in the dynamics of the posttreatment trend analysis (Barrios 2021, Goodman-Bacon 2021); however, the trends are robust to their inclusion.

³¹ The movement fixed effects are akin to *Hire from TA Firm* fixed effects and absorb the main effect of *Hire from TA Firm*. These results are robust to replacing these fixed effects with either (or both) hiring firm or former firm fixed effects (results untabulated).

³² The firm-panel results are more sensitive to certain robustness tests conducted in this paragraph. For example, when examining the robustness of our firm-year panel results to alternative definitions of *Hire from TA Firm* (described in Section 4.1), we find that our results are robust to all the alternative definitions when using sector and year fixed effects but only two alternative definitions (the 20% cutoff and the unadjusted ETR approach) when using firm and year fixed effects. The sensitivity of the firm-year panel results to firm fixed effects is likely attributable to the fact that most observations have a *Hire from TA Firm* value of zero, meaning that there may not be sufficient variation when including these fixed effects. We circumvent this limitation by focusing on movements in the movements-only design.

³³ Additionally, these findings are consistent with the cash ETR being a better summary measure of a tax employee's influence on firm outcomes than other commonly used measures of nontax performance.

³⁴ We use a combined approach to capturing sector similarity because there are advantages and disadvantages to both sector classifications. The advantage of Hoberg and Phillips' (HP) approach is that it allows for changes in sector similarity over time and accommodates the creation of new sectors, whereas SIC does not. The disadvantage of the HP approach is that because it is text-based, it

suffers from potential noise. Using the combined approach also allows us to broaden our sample of "same sector" movements. In untabulated analyses, we examine the similar sector results using either the SIC or HP measures in isolation. Whereas we find coefficients of a similar magnitude, the results are not statistically significant at conventional levels, likely because of a lack of power. We obtain the HP data from <https://hobergphillips.tuck.dartmouth.edu/industryclass.htm>; we employ their TNIC2 approach, which they argue is comparable to two-digit SIC codes.

³⁵ We measure sector similarity using the parent company even if the employee worked at a subsidiary because we lack data on subsidiary sector membership. However, we expect that there is significant overlap between parent company and subsidiary sector membership. To the extent that the employee worked at the subsidiary and the prior parent and subsidiary are in different sectors, this introduces measurement error into this test.

³⁶ Specifically, we compute the hiring firm's foreign income 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.

³⁷ Similar to the global operation test, we compute the hiring firm's 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.

³⁸ When examining the statistical differences across partitions for the four individual proxies, we find that only the global operation similarity subsamples are statistically significant at conventional levels. We believe that this is likely attributable to the small sample sizes in these analyses and the fact that they each only examine one aspect of similarity. We overcome this power issue by combining these different similarity proxies in columns (9) and (10).

³⁹ When examining the statistical differences across the two individual partitions, we find that neither is significant at conventional levels. Again, this is likely attributable to the small sample sizes in these analyses, which is overcome by combining them in this analysis.

⁴⁰ We find a correlation of 0.304 between *High Tax Experience* and *High Experience at Prior Firm*, suggesting that there is some overlap between these two constructs and that these tests are not fully independent. We report the correlations between all cross-sectional sorting variables in Table OA.2 of the online appendix.

⁴¹ The differences across the two individual partitions are not statistically significant at conventional levels, which we again believe is attributable to power issues that are overcome in this combined test.

⁴² When conducting a similar split based on the employee's nontax experience, we do not observe the same pattern as in Table 7, panel B, columns (1) and (2) (results untabulated). This suggests that it is tax experience rather than experience in general that matters for tax-planning performance.

⁴³ Inferences are robust to instead sorting based on total sales or 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.

⁴⁴ The correlations between each of the size-based cross-sectional variables and the *APTS > 0* variable are all lower than 0.2, suggesting that these two different tests are reasonably independent.

⁴⁵ Prior research suggests that executives have materially important effects on tax planning (Dyreng et al. 2010). We conduct several untabulated analyses to examine whether executives are a key mechanism behind our findings. First, we find that changes in the key executives (CEO and CFO) are associated with a greater likelihood of tax department hiring in the year of the executive turnover

and the subsequent two years. However, we do not find that these executive changes are associated with an increased likelihood of hiring from tax-aggressive firms. Second, we do not find that the tax-planning benefits of hiring from a tax-aggressive firm are concentrated within hiring firms that recently experienced an executive (CEO or CFO) change. Specifically, whereas we do document a negative coefficient on *Hire from TA Firm* $\times$ *Post* in the subsample of movements concurrent with a CFO change, it is not significant at conventional levels. Overall, we are unable to provide compelling evidence showing that executives are a key mechanism behind our findings.

⁴⁶ The differences across the two individual partitions are not significant at conventional levels. We attribute this power issues given the small sample sizes in this analysis.

⁴⁷ In the subsamples examined in Tables 6–8, the percentage of movements for which *Hire from TA Firm* equals one ranges from 14% (director at prior firm) to 24% (same sector) and is generally close to 20%, which is the percentage for the overall sample. These statistics suggest that is sufficient variation in *Hire from TA Firm* across each of the subsamples.

⁴⁸ The reason we do not use only departure firms and sort on the prior firm's ETR to identify departures from aggressive firms is that this would effectively sort observations on the dependent variable. By matching each departure firm to a control firm, we can account for any mechanical changes in ETRs (i.e., mean reversion, etc.) that result from having a certain prior ETR. We find similar results when using stricter (i.e., <2 percentage point) or more relaxed (i.e., >2 percentage point) requirements for the difference in cash ETRs between the two firms.

⁴⁹ In untabulated analyses, we find similar results when we extend the postdeparture window (e.g., to five years postdeparture), when we focus on departures of employees that were directors at the prior firm, or when we focus on departures that had above-median tax experience or experience at the prior firm.

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