

ONLINE APPENDIX

to

“Tax Planning Knowledge Diffusion via the Labor Market”

by

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OA.1. Robustness: Contemporaneous firm actions

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

First, we examine whether the number of tax department hires, rather than their prior experience at a tax aggressive firm, is responsible for our findings. Chen, Cheng, Chow, and Liu (2020) find that larger tax departments (measured using the number of tax department employees) are associated with greater tax avoidance. To examine whether the number of hires is driving our findings, we create a count variable that captures the number of hires made by the tax department in the year when the employee starts at the new firm and interact this variable with *Post*. We report this finding in column 1 of Table OA.1, Panel A. We do not find that the number of employees is associated with the post-hiring tax planning, and we continue to find a negative and significant coefficient on *Hire from TA Firm x Post*.

Next, we examine whether mergers and acquisitions are responsible for our findings. An employee movement could be driven by a merger between the new and former firms or be driven by compliance needs resulting from a merger between firms. Furthermore, prior research suggests that tax avoidance may be one of the value drivers of mergers (Belz, Robinson, Ruf, and Steffens 2013; Devos, Kadapakkam, and Krishnamurthy 2009). To mitigate this alternative explanation, we re-estimate equation 3, including the interaction of *Large Merger* and *Post*, where *Large Merger* is an indicator variable equal to one if the hiring firm's acquisition-related cash flows are greater than 10 percent to lagged assets in either the year the employee is hired or in the year prior.

We report these findings in column 2 of Table OA.1, Panel A. In short, our findings do not appear to be driven by mergers: the coefficient on *Large Merger x Post* is insignificant, and we continue to find a significant negative coefficient on *Hire from TA Firm x Post*.

OA.2. Robustness: Executives

Next, we explore whether our findings are driven by firm-level executives, rather than tax department employees. Dyreng, Hanlon, and Maydew (2008) find that executive fixed effects explain variation in cash ETRs and argue that this is consistent with executives having a meaningful effect on corporate tax planning. However, the mechanism through which executives affect tax planning is unclear; Dyreng, Hanlon, and Maydew (2010) examine several possibilities but find little support for any given explanation. We posit that a potential mechanism for the Dyreng et al. (2010) finding could be that executives set the “tone at the top” for greater tax planning, leading the firm to access the labor market to acquire tax planning knowledge through hiring.

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

experience declines in their cash ETRs after hiring employees from tax aggressive firms. In column 4, we use CFO turnovers instead of CEO turnovers, and find similar results.¹

Next, we examine whether hiring executives from tax aggressive firms is responsible for our findings. We focus on CEOs and CFOs in the analyses: the former because of their ability to set the “tone at the top” and the latter because they are the C-suite executive most directly responsible for the tax department. We again use executive turnovers sourced from Execucomp, and classify executive hires as coming from a tax aggressive firm using an approach similar to the one described in section 3.1 of the paper. We then re-estimate our primary models additionally including the interaction between *Hire Exec from TA Firm*, which equals one if the firm hires a CEO or CFO from a tax aggressive firm in either year t (the year the new tax department employee joins the firm) or year $t-1$, and *Post*. We re-estimate equation 3 with the inclusion of this interaction term, and report these findings in column 5 of Table OA.1, Panel A. We continue to find robust evidence that hiring tax department employees from tax aggressive firms is associated with increases in tax planning, and we do not find that these executive hires are associated with changes in tax planning. Overall, the findings from these two analyses suggest that our results are not driven by executive turnovers or hiring executives from tax aggressive firms, and therefore are distinct from the findings in prior research (Dyreng et al. 2010; Chyz 2013; Koester, Shevlin, and Wangerin 2017).

¹ One concern with these robustness tests is that we do not isolate forced executive turnovers (which are more likely a confounder for our inferences), and instead pool together forced and unforced turnovers. However, in an untabulated test, we continue to find a tax planning benefit from hiring from an aggressive tax planning firm even if we eliminate all tax department employee movements that occur concurrently or with a lag to a CEO or CFO turnover. Thus, it is unlikely that executive turnovers—forced or unforced—are responsible for our findings.

OA.3. Robustness: Alternative connections

Prior research has documented that other connections between firms can facilitate the diffusion of tax planning strategies. For example, Brown (2011) and Brown and Drake (2014) find that common board ties between firms can lead to increased tax avoidance and the spread of specific tax planning strategies. Gallemore, Gipper, and Maydew (2019) document that certain banks are associated with increases in tax planning in new borrowers, consistent with banks facilitating tax planning via the spread of such strategies across borrowers. McGuire, Omer, and Wang (2012) find that industry specialist auditors facilitate tax avoidance, and Chyz, Gal-Or, Naiker, and Sharma (2021) find that firms that use auditor-provided tax services are more effective tax planners. If these alternative connections facilitate employee movements between firms, our findings may simply reflect the fact that these connections enable the spread of tax planning information.

To examine whether alternative connections between our sample firms are responsible for our findings, we re-estimate equation 3, including an additional variable: *Alternative Connection x Post*. We define *Alternative Connection* as an indicator variable equal to one if the former and hiring firms share a connection in the year of the hire, and zero otherwise. We employ three versions of *Alternative Connection* based on whether the connection is via a board member (identified using BoardEx), a lead bank lender (identified using Dealscan), or an auditor (identified using Audit Analytics). We report these analyses in columns 6 through 8 of Table OA.1, Panel A. Regardless of the connection (board, bank, or auditor), we do not find that our findings appear to be driven by these connections: the coefficient on *Hire from TA Firm x Post* continues to be negative and statistically significant, whereas the coefficient on *Alternative Connection x Post* is

insignificant. Therefore, our findings represent a connection between firms that is distinct from those examined in the prior literature.

OA.4. Placebo: Hiring from firms with high non-tax-planning performance

Next, we explore whether our results are being driven by employees hired from firms because of other non-tax planning characteristics of the prior firm, such as financial performance, which are correlated with the prior firm's long-run cash ETR. To address this concern, we create an additional indicator variable, *Hire from High Perf. Firm*, which equals one if the prior firm is in the top quartile of the sample of movements (similar to our *Hire from TA Firm* measure) along various dimensions and interact this variable with *Post*. We employ two variables that exhibit strong negative associations with cash ETRs in our sample to measure financial performance: *ROA* and *FI*. Examining *FI* also allows us to account for the possibility that firms seek tax department employees with cross-jurisdictional tax experience. We also employ the prior firm's *R&D*, since firms may seek individuals with tax experience related to intangibles and/or transfer pricing, which in turn could be correlated with the prior firm's tax planning outcomes. We measure these for the former firm over the same period over which we measure the long-run cash ETR (five years). We report the results in Panel B of Table OA.1; for each alternative performance measure, we include it by itself first and then, in addition, to *Hire from TA Firm x Post*. Across all six regressions, we find consistent results: the coefficient on *Hire from High Perf. Firm x Post* is statistically insignificant and close to zero, whereas the coefficient on *Hire from TA Firm* continues to be negative and statistically significant. We conclude that our primary findings appear to be specific to hiring employees from firms with low long-run cash ETRs, and not driven by hiring from firms with high long-run financial performance or other non-tax characteristics.

OA.5. Placebo: Non-tax-planning-related firm outcomes

In our final set of robustness tests, we examine whether we observe changes in non-tax firm performance or other outcomes in response to hiring tax department employees from tax aggressive firms. Such a finding could signal that a larger change in firm operations might be the driving factor behind employee hiring and the increased tax avoidance. We examine three alternative firm outcomes: *ROA* (pre-tax income scaled by total assets), *R&D* (research and development expense, scaled by total assets), and *CapEx* (capital expenditures, scaled by total assets). We re-estimate equation 3 using these variables as the dependent variable in place of *Cash ETR*. We report these findings in Panel C of Table OA.1. In each regression, we find no evidence of an association between hiring a tax employee from a tax aggressive firm and changes in these variables, suggesting that our primary findings are attributable to the employee's impact on tax planning, rather than evidence of a broader change in the company's operations.

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Table OA.1: Mitigating concerns about alternative events, firm connections, and former firm characteristics

This table presents the results of estimating a modified version of equation 3. The dependent variable is *Cash ETR*, defined as the cash taxes paid divided by pre-tax income minus special items. The independent variable of interest is the interaction of *Hire from TA Firm*, an indicator variable equal to one if the employee's prior firm had 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 of the paper. This regression is estimated on a six-year window around the movement, with up to three years prior to the movement and three years after the movement (inclusive of the hiring year). We require at least one observation before and after the movement. Each panel contains a different set of robustness analyses. In Panel A, we additionally include *Alternative x Post* as an independent variable. In each column, we employ a different variable for *Alternative*: the number of tax department hires in year t (col. 1), an indicator variable equal to one if the firm had acquisition cash flows greater than 10 percent of lagged total assets in either year t-1 or t (col. 2), an indicator variable equal to one if the firm experienced a CEO turnover in either year t-1 or t (col. 3), an indicator variable equal to one if the firm experienced a CFO turnover in either year t-1 or t (col. 4), an indicator variable equal to one if the firm hired a CEO or CFO directly from a firm that is classified as tax aggressive using an approach similar to the one described in section 4.1 (col. 5), an indicator variable equal to one if the hiring and former firm shared a board member in year t (col. 6), an indicator variable equal to one if the hiring and former firm shared a lead lender bank in year t (col. 7), or an indicator variable equal to one if the hiring and former firm shared an auditor in year t (col. 8). In Panel B, we include (either by itself or in addition to the main variables) *Hire from High Perf. Firm x Post*, where *Hire from High Perf. Firm* is an indicator variable equal to one if the employee's former firm is in the top quartile of the sample of movements in a given performance measure and zero otherwise. Each column uses a different performance measure: pre-tax income (cols. 1 and 2), foreign income scaled by total assets (cols. 3 and 4), and research and development expense scaled by total assets (cols. 5 and 6). In Panel C, we re-estimate equation 3 using alternative dependent variables: *ROA* (operating income before depreciation scaled by total assets), *R&D* (research and development expense scaled by total assets), or *CapEx* (capital expenditures scaled by total assets). Each column includes the control variables (*ROA*, *FI*, *Size*, *Leverage*, *MTB*, *Sales Growth*, *PPE*, *Intangible Assets*, *R&D*, *Advertising*, *Special*, *Extraordinary*, *NOL Indicator*, and *NOL Change*) except when that control variable is used as the dependent variable, movement fixed effects, event-time fixed effects, and sector-year fixed effects. Standard errors clustered by firm are presented underneath the coefficient estimates. *, **, and *** represent coefficients that are statistically significant at the 0.10, 0.05, and 0.01 levels, respectively.

Panel A: Accounting for alternative events and connections

Alternative	(1) Num. of hires	(2) Large M&A	(3) CEO turnover	(4) CFO turnover	(5) TA exec hire	(6) Board overlap	(7) Bank overlap	(8) Audit overlap
Dependent variable:	<i>Cash ETR</i>							
<i>Hire from TA Firm x Post</i>	-0.027*** (0.010)	-0.027*** (0.010)	-0.028*** (0.010)	-0.027*** (0.010)	-0.027*** (0.010)	-0.027*** (0.010)	-0.028*** (0.011)	-0.027*** (0.011)
<i>Alternative x Post</i>	-0.000 (0.001)	0.003 (0.013)	-0.007 (0.011)	-0.001 (0.011)	-0.023 (0.046)	0.019 (0.018)	0.017* (0.010)	-0.006 (0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,965	5,965	5,965	5,965	5,965	5,965
Adjusted R-squared	0.418	0.418	0.418	0.418	0.418	0.418	0.418	0.418

Panel B: Placebo analyses using alternative former firm characteristics

Performance measure	(1) <i>ROA</i>	(2) <i>ROA</i>	(3) <i>FI</i>	(4) <i>FI</i>	(5) <i>R&D</i>	(6) <i>R&D</i>
Dependent variable:	<i>Cash ETR</i>					
<i>Hire from TA Firm x Post</i>		-0.027*** (0.010)		-0.027*** (0.010)		-0.028*** (0.010)
<i>Hire from High Perf. x Post</i>	0.005 (0.008)	0.001 (0.008)	-0.004 (0.008)	-0.004 (0.008)	0.013 (0.009)	0.015* (0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,965	5,965	5,965	5,965	5,965	5,965
Adjusted R-squared	0.417	0.418	0.417	0.418	0.417	0.418

Panel C: Placebo analyses using alternative, dependent variables

	(1)	(2)	(3)
Dependent variable:	<i>ROA</i>	<i>R&D</i>	<i>CapEx</i>
<i>Hire from TA Firm x Post</i>	-0.000	-0.000	-0.001
	(0.003)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Movement FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Sector x Year FE	Yes	Yes	Yes
Observations	5,965	5,965	5,950
Adjusted R-squared	0.870	0.951	0.906

Table OA.2: Correlations between cross-sectional variables

This table presents a correlation matrix for the cross-sectional split variables. These variables are defined in section 5 of the paper.

	Director at prior firm	Director at hiring firm	High tax experience	High exp. at prior firm	Same sector	High FI similarity	High country overlap	High size similarity	Large firms	Medium firms	Small firms
Director at prior firm											
Director at hiring firm	0.510***										
High tax experience	0.286***	0.277***									
High exp. at prior firm	0.143***	0.097***	0.304***								
Same sector	-0.023*	0.007	0.031**	0.043***							
High FI similarity	-0.009	0.014	0.008	-0.049***	0.067***						
High country overlap	0.014	0.067***	0.024*	0.021	0.114***	0.01					
High size similarity	0.032**	0.060***	0	-0.049***	0.065***	-0.014	0.076***				
Large firms	-0.013	-0.103***	-0.026**	0.011	-0.088***	0.018	-0.204***	-0.338***			
Medium firms	-0.047***	-0.02	0.018	-0.027**	-0.021*	-0.026**	0.078***	0.110***	-0.502***		
Small firms	0.061***	0.123***	0.008	0.016	0.109***	0.007	0.132***	0.229***	-0.504***	-0.494***	
APTS > 0	0.047***	0.048***	0.024*	0.078***	0.039***	-0.115***	0.145***	0.080***	-0.151***	0.011	0.141***