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Tax Knowledge Diffusion via Strategic Alliances

Abstract

We empirically identify tax knowledge diffusion via strategic alliances by documenting economically meaningful decreases in effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to pseudo treated high-tax firms in strategic alliances with other high-tax firms. Additional analyses reveal that elapsed time facilitates tax knowledge diffusion. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. Furthermore, we find that shared industry affiliation rather inhibits tax knowledge diffusion. Our inferences persist when analyzing shared audit firms and board ties as alternative channels. We also show that tax knowledge diffusion appears ex ante unintended by analyzing (i) abnormal returns to the announcements of strategic alliances and (ii) differences between the partners' market shares. Overall, our study documents the impact of close cooperation and continued exchange in strategic alliances on undersheltered firms' willingness to engage in tax planning.

- Working Paper -

Keywords: Tax Planning, Strategic Alliance, Knowledge Diffusion

Data: Sources identified in the text

Declaration of Interest: The authors declare no conflicts of interest

1 Introduction

This study provides a novel tax perspective on the question "when you work with a superman, will you also fly?" (Tan and Netessine 2019). Focusing on strategic alliances, a highly relevant form of contract-based collaboration between at least two firms (PwC 2018), we elucidate undersheltered "high-tax" firms' changes in tax planning. Specifically, our analysis reveals that high-tax firms increase their tax planning after initiating strategic alliances with tax aggressive "low-tax" firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. In essence, we document the impact of close cooperation and continued exchange in strategic alliances on firms' willingness to engage in tax planning. Our empirical evidence, thus, complements the interview insights by Mulligan and Oats (2016), suggesting that informal private exchange may reduce the expected costs of tax planning. Analyzing changes in tax planning, as a matter outside the scope of an alliance's main business purpose, further highlights the complex tension between knowledge diffusion and protection in alliances (e.g., see Palomeras and Wehrheim (2021)). Taken together, our study reveals tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010).

Tax planning, conceptually, results from a firm specific equilibrium of expected tax costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). Its key benefits, lower tax payments, are rather simple to predict, also because specific tax planning tools are mass-market tax advisory products (e.g., see Lisowsky (2010) and Wilson (2009)). Low-tax firms are particularly good in managing and reducing actual tax costs (e.g., transfer price documentation that has been accepted by the IRS) or expect low potential tax costs (e.g., audit or reputational costs). If this tax knowledge diffused to high-tax firms, the assessment of tax costs by high-tax firms could change, too. Observing changes in tax planning in our analysis would then be the consequence of an updated equilibrium of expected costs and benefits of tax planning. Importantly, strategic alliances are not subject to corporate income taxation because they do not establish a separate legal entity, but all partners of a strategic alliances individually. This institutional characteristic precludes confounding mechanical tax effects upon investment and allows us to isolate the effect of tax knowledge diffusion and overcomes a key challenge that occurs with other forms of investment, such as M&A.

Strategic alliances are expected to foster their main business purposes and to facilitate (intended) transfers of related knowledge between the cooperating firms (e.g., K. Li, Qiu, and Wang (2019) identify significant increases in firms' innovative capacity when investing in R&D strategic alliances). Conceptually, a strategic alliance could also stir tax knowledge diffusion because information exchange, due to trust, and mutual commitment, as a consequence of collaboration, may exceed the initially intended scope (e.g., see Boone and Ivanov (2012) and Yin and Shanley (2008) on increased mutual commitment in alliances). However, the ex ante unintended diffusion of tax knowledge would be a valuable "private benefit" for the high-tax firm (e.g., see Anderson et al. (2014)) for which the low-tax firm is not compensated, e.g. in form of joint tax planning. Analyzing tax knowledge diffusion in strategic alliances, thus, is distinct and independent from intentional transfers of tax knowledge in peer-to-peer relationships to facilitate joint tax planning (Cen et al. 2017, 2020) and from intentional transfers and acquisitions via intermediaries (e.g., the client-bank-client relationships in Gallemore, Gipper, and Maydew (2019)).

To address our research question, we utilize information on strategic alliances that are initiated between publicly traded US firms from 1994 to 2021. Given that accounting data are available for an alliance's partners, we reshape the data from the alliance to the partners' levels (i.e., one observation per partner of an alliance). Our variable of interest to measure tax planning is the effective tax rate as the commonly employed proxy for a firm's nonconforming tax planning behavior. We then classify the partners in an alliance as low-tax and high-tax firms depending on their industry-multiperiod-mean adjusted cash effective tax rates in the run-up to the initiation of an alliance. To tease out tax knowledge diffusion, we analyze changes in our tax planning measure of high-tax firms in strategic alliances with low-tax firms in comparison to high-tax firms in strategic alliances with high-tax firms. Our control-group, thus, represents pseudo treated firms with on average identical chances and challenges after initiating a strategic alliance as the treated firms except for the fact that their alliance has no low-tax partner.

Our main analysis reveals that high-tax firms increase their tax planning (i.e., decrease their (cash) effective tax rates) after initiating strategic alliances with low-tax firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. First, we employ descriptive statistics and a univariate analysis to understand whether eventual treatment effects would stem from unexpected variation in the outcome variables of interest among the control observations. We then corroborate this analysis by multivariable regression analyses in which we control, based on textual analysis, for the alliances' business purposes, partner characteristics, alternative channels, and within-firm determinants of tax planning. Because these covariates account for a broad range of alternative explanations, we find it plausible to associate the (relative) increase in tax planning for treated high-tax firms with the presence of a low-tax firm in the alliance. Additionally and even though data indicate that the high-tax firms in our sample are in very similar situations except for potentially experiencing tax knowledge diffusion, we employ, in the interest of caution and methodological thoroughness, entropy-balancing weighting (Hainmueller 2012; Hainmueller and Xu 2013) and use multiple calculations of entropy weights to re-estimate the main analysis. Overall, our findings indicate that tax planning responses to "working with superwoman", suggesting tax knowledge diffuses via strategic alliances. Importantly, the average treatment effect on the treated is neither surprisingly large nor negligibly small. Our estimates indicate, consistent across a broad set of specifications, a reduction of 2.5 to 3 percentage points in a treated firm's effective tax rate, supported by a 95% confidence interval of $[-0.0552, -0.0052]$ in the preferred specification. Considering a sample average of ~29% pretreatment, the identified decreases are both statistically and economically significant.

We then test whether our treatment assignment is indeed plausibly exogenous by analyzing abnormal returns to the announcements of strategic alliances. Our idea is that if tax knowledge diffusion could be anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study and find, consistent with the findings by Chan et al. (1997), that cumulative abnormal returns are on average positive across all announcements in our sample. Importantly, we observe, counter to the idea of anticipation of tax knowledge diffusion, that abnormal returns of control firms' announcements exceed the abnormal returns for the treated firms. We conclude that tax knowledge diffusion via strategic alliances is not anticipated, reducing remaining concerns about endogenous treatment assignment.

Next, we turn to the mechanisms of tax knowledge diffusion via strategic alliances. First, we focus on elapsed time because knowledge diffusion is a gradual, multi-stage process (Bresman, Birkinshaw, and Nobel 2010; Inkpen 2000; Szulanski 1996) and elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv 2003; Isaksson, Simeth, and Seifert 2016). To test whether elapsed time facilitates tax knowledge diffusion, we extend the posttreatment period gradually over multiple specifications of our main analysis. The results indicate that the treatment effect particularly increases in magnitude with elapsed time since the initiation of an alliance. In particular, the estimates for our treatment variable of interest turn significant and expand economically with elapsed time. We conclude that elapsed time, increasing the probability of information exchange due to trust and mutual commitment, facilitates tax knowledge diffusion via strategic alliances. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. We find, broadly consistent with the inferences by Brown and Drake (2014) and Brown (2011), that a shared industry affiliation rather inhibits tax knowledge diffusion via strategic alliances. Finally, we turn to differences in the partners' market shares to test whether tax knowledge diffusion is indeed unintended or originates from power-dynamics. We find no evidence for a power-induced mechanism but, consistent with trust and mutual commitment, that tax knowledge diffusion is unintended. Throughout all cross sectional analyses the baseline estimate for the treatment persists in direction, magnitude, and statistical significance in comparison to the inferences from our main analysis.

Furthermore, we examine how tax knowledge diffusion via strategic alliances is impacted when partners share an audit firm because we are interested in whether the identified effects are robust to alternative channels of intentional tax knowledge transfers. We find that the results from our main analysis persist and that we do not spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances. The same holds when we analyze board ties between the firms in our sample. We then run a battery of robustness checks and analyze alternative tax planning measures, pretreatment volatility in effective tax rates, low-tax firms, and probabilities of valuation allowance releases. The results from these analyses support our identification strategy and inferences: we capture tax planning responses of the treated high-tax firms due to tax knowledge diffusion.

Our study reveals tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010). Specifically, we document the impact of close cooperation and continued exchange in strategic alliances on firms' willingness to engage in tax planning. Our empirical evidence not only complements the interview insights by Mulligan and Oats (2016), suggesting that informal private exchange may reduce the expected costs of tax planning, but also contributes to the literature that formalizes tax planning as an equilibrium of expected tax costs and benefits (Jacob, Rohlfing-Bastian, and Sandner 2021). We utilize an institutional feature of strategic alliances, the absence of mechanical tax effects at the firm level upon investment, that allows us to tie observed changes in tax planning to an update of this equilibrium. Our findings, thus, not only inform research but also offer valuable insights for practitioners and policymakers by elucidating how fostering collaboration through strategic alliances can influence firms' tax planning decisions.

Analyzing tax knowledge diffusion via strategic alliances, furthermore, contributes to the literature on cross-firm connections as determinants of tax planning. Importantly, our analysis is distinct and independent from intentional transfers of tax knowledge in peer-to-peer relationships to facilitate joint tax planning (Cen et al. 2017, 2020) and from intentional transfers and acquisitions via intermediaries (e.g., the client-bank-client relationships in Gallemore, Gipper, and Maydew (2019)). The focus in these and related studies (e.g., on board-ties (Brown 2011; Brown and Drake 2014), human capital turnover (Barrios and Gallemore 2024), auditors (McGuire, Omer, and Wang (2012), Klassen, Lisowsky, and Mescall (2016)), and law firms (Acito and Nessa 2022)) is on selective transactions that can be ex ante intended to affect a firm's tax planning. We empirically test whether capital markets anticipate tax knowledge diffusion via strategic alliances and find that this is not the case when comparing returns at announcements for treated and pseudo treated firms. Consistent evidence from analyzing differences in the partners' market shares further suggests that tax knowledge diffusion is unintended and not power-induced. Thus, our study underscores the importance of considering tax knowledge diffusion as unique and economically important yet unintended effect of a relevant cross-firm connection: cooperation in strategic alliances.

Finally, our study theoretically builds on and contributes to research that examines knowledge in the context of strategic alliances. Related findings frequently highlight the knowledge-related benefits of investments in strategic alliances but the focus is typically on knowledge in the context of a network's main business purpose (e.g., see K. Li, Qiu, and Wang (2019) on R&D alliances and their effects on firms' innovative capacities). Consistently, analyses of knowledge protection in strategic alliances suggest that firms especially attempt to safeguard themselves with respect to the main business purpose of the network (D. Li et al. 2008; Palomeras and Wehrheim 2021). We argue that close cooperation, trust, mutual commitment, and continued exchange with a low-tax firm in a strategic alliance could also stir the diffusion of tax knowledge, as matter outside the scope of an alliance's main business purpose, and find consistent evidence. Our evidence thus contributes to a deeper understanding of knowledge diffusion via strategic alliances, particularly concerning tax knowledge.

2 (Tax) Knowledge Diffusion: Conceptual Underpinnings

Generally, knowledge diffusion requires communication through channels over time among members of a social system (Rogers 2003). Moreover, a firm must not only gain access to knowledge but must also deploy an approach to utilize the knowledge. Otherwise, knowledge diffusion cannot contribute to a firm's knowledge profile (Kale, Singh, and Bell 2009; Mazloomi Khamseh, Jolly, and Morel 2017). We deduce that (tax) knowledge diffusion conceptually comprises gaining access to and being willing to and capable of employing relevant tax knowledge.

Within this framework, there are several aspects that speak in favor of tax knowledge diffusion via strategic alliances. Generally, strategic alliances force firms to commit investment and other support to common goals (Yin and Shanley 2008). Consistently, cooperation is found to mitigate cultural differences between partners (Kogut and Singh 1988). Furthermore, Kale, Singh, and Bell (2009) argue that firms should create a dedicated management structure to oversee and support their alliance activities. While research on knowledge protection in strategic alliances emphasizes firms' efforts to safeguard proprietary knowledge pertinent to the main business purpose of an alliance (D. Li et al. 2008; Palomeras and Wehrheim 2021), it is evident that strategic alliances facilitate transfers of such critical knowledge between the cooperating firms (e.g., K. Li, Qiu, and Wang (2019) identify significant increases in firms' innovative capacity when investing in R&D strategic alliances). We conclude that close cooperation, trust, mutual commitment, and continued exchange with a low-tax firm in a strategic alliance could also stir the diffusion of tax knowledge, as matter outside the scope of an alliance's main business purpose.[1] Consistently, Mulligan and Oats (2016) note that "sharing information, particularly about tax plans and technical advice about dealing with ambiguities in tax laws serves to provide legitimacy to preferred tax positions, yielding a form of power [...] when taking tax positions in dealing with Revenue Authorities" (p. 70). These insights suggest that informal private exchange may particularly reduce the expected costs of tax planning (including, for instance, the expected reputational costs (Austin and Wilson 2017; Gallemore, Maydew, and Thornock 2014; Graham et al. 2014; Hanlon and Slemrod 2009)). Tax planning might, thus, response as a consequence of an updated equilibrium of expected costs and benefits of tax planning (Jacob, Rohlfing-Bastian, and Sandner 2021).

However, "not all corporate practices diffuse in the same way" (Y. Cai et al. 2014, 1087). Exemplary barriers are knowledge-related factors, such as limits to a recipient's absorptive capacity (Dyer and Hatch 2006; Szulanski 1996). Additionally, imposing constraints on knowledge diffusion increases a firm's return from having a sophisticated knowledge profile (Akcigit and Ates 2019). Furthermore, tax knowledge diffusion is a private benefit (i.e., it is especially valuable outside the scope of an alliance). Such private benefits can harm the partners' efforts to strive for the alliance's common benefits (Khanna, Gulati, and Nohria 1998), shift partners' bargaining power, and finally induce instability to the alliance (Inkpen and Beamish 1997; Khanna, Gulati, and Nohria 1998). Additionally, Desai, Foley, and Hines (2004) suggest that shared ownership of equity joint ventures impacts the fine-tuning of tax planning of these entities, and both cooperation (Chen, King, and Wen 2015) and tax planning (Dyreng, Hanlon, and Maydew 2019) are found to induce uncertainty, which may induce marginal disutility from tax knowledge diffusion. Corporate culture and governance further impact a firm's decisions on tax planning (Armstrong et al. 2015; Klassen, Lisowsky, and Mescall 2017). Thus, it is an empirical question if and when tax knowledge diffuses via strategic alliances.

[1] Note that protection against tax knowledge diffusion is aggravated because tax knowledge comes with little to no legal protection, as exists, for instance, for intellectual property (for the general implications of weak knowledge protection, see Zhao (2006)).

3 Data & Identification Strategy

[Table 1 about here.]

3.1 Strategic Alliances

We exploit data on strategic alliances from Refinitiv's SDC Platinum (SDC) database on strategic alliances over the 1994-2021 period. SDC is widely used in relevant research on corporate cooperation (Anand and Khanna 2000; Boone and Ivanov 2012; Y. Cai and Sevilir 2012; Chen, King, and Wen 2015; Ishii and Xuan 2014; K. Li, Qiu, and Wang 2019) and tracks a very wide range of agreement types (Schilling 2009). SDC issues data at the strategic alliance level. Initially, we deflate our sample to observations that are flagged as strategic alliances by excluding equity joint ventures from the data. We then reshape data from the alliance to the partner level (i.e., one observation per firm in an alliance) because strategic alliances are (unlike equity joint ventures) not subject to corporate taxation but the investing partners. To illustrate: a strategic alliance between *two* partners translates to one observation for each of the two partners (i.e., *two* observations). Compustat data (via Wharton Research Data Services) provide firm-year-level accounting information, and we merge SDC and Compustat data by using a firm's historical six-digit CUSIP number (at the level of the ultimate parent of the participant). Although SDC provides reliable network observations from the beginning of 1990 onward, we start in 1994, consistent with many tax studies. Furthermore, we respectively consider strategic alliances between publicly traded firms incorporated and headquartered in the US and in which all contracting parties are identified in Compustat data.

3.2 Measuring Tax Knowledge Diffusion

We argue in our conceptual framework (see Section 2) that tax knowledge diffusion via strategic alliances would impact a firm's equilibrium of expected costs and benefits of tax planning. Therefore, we operationalize tax knowledge diffusion by measuring changes to a firm's nonconforming tax planning behavior. The lingua franca in determining the degree to which a firm engages in tax planning is the effective tax rate, which puts tax expenses and pre-tax book income into perspective.[2] We base our inferences on the cash effective tax rate (*cash ETR*) because *cash ETR* also captures tax deferral strategies (Edwards, Schwab, and Shevlin 2016; Hanlon and Heitzman 2010). Furthermore, we apply a multiperiod (3-year) form of *cash ETR* (Barrios and Gallemore 2024; Brown and Drake 2014; Gallemore, Gipper, and Maydew 2019):

$$cash\ ETR3_{i,t=1} = \frac{\Sigma_{t=1}^{3}(txpd_{i,t})}{\Sigma_{t=1}^{3}(pi_{i,t} - spi_{i,t})} \tag{1}$$

The terms txpd, pi and spi in equation (1) correspond to their Compustat data item equivalents of cash taxes paid, pre-tax income and special items. Missing spi values are reset to 0, while any *cash ETR3* with a negative denominator is reset to missing. Nonmissing *cash ETR3* are winsorized at 0 and 1.

For every t_1 in which a strategic alliance is initiated, we are interested in whether firms are rather undersheltered "high-tax" firms or rather tax aggressive "low-tax" firms. For the identification of low-tax and high-tax firms, we consider *pre cash ETR3*, which is constructed identically to *cash ETR3* but over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$:

$$pre\ cash\ ETR3_{i,t=0} = \frac{\Sigma_{t=-2}^{0}(txpd_{i,t})}{\Sigma_{t=-2}^{0}(pi_{i,t} - spi_{i,t})} \tag{2}$$

Next, we require to observe *cash ETR3* and *pre cash ETR3* of all partners for an alliance to be considered in our analysis.[3] We classify firms as high-tax or low-tax based on their *pre cash ETR3* which we industry-year-mean adjust for this purpose (i.e., we are interested in firms that show low/high multiperiod effective tax rates among their industry peers just before an alliance is initiated). We then allocate this adjusted *pre cash ETR3* into four bins according to the quartiles

[2]While nonconforming tax planning should be indicative of a low effective tax rate, a low effective tax rate is not indicative of tax planning by all means (Drake, Hamilton, and Lusch 2020; Schwab, Stomberg, and Xia 2021). However, the importance of effective tax rates as a measure of and proxy for tax planning continues to remain highly important in corporate practice. We employ effective tax rates as tax knowledge measure for the latter reason but carefully check the robustness of this choice by (i) considering the effects from presumable valuation allowance releases (Drake, Hamilton, and Lusch 2020), and (ii) utilizing a different measure of a firm's tax planning activities (e.g., the cash tax differential by Henry and Sansing (2018)).

[3]We also refer to *cash ETR3* and *pre cash ETR3* as $cash\ ETR3_{[t_1;t_3]}$ and $pre\ cash\ ETR3_{[t_{-2};t_0]}$ to highlight the respective timing around the initiation of a strategic alliance in t_1. Our analyses, thus, consider strategic alliances between 1997 (1994-1996 for *pre cash ETR3*) and 2019 (2019-2021 for *cash ETR3*).

of its distribution. Industry adjustment (Brown and Drake 2014) and a multiperiod measure (Dyreng, Hanlon, and Maydew 2008; Dyreng et al. 2017) help us to validate the identification of undersheltered firms and more aggressive tax planners. A partner is treated as a low-tax firm in a strategic alliance when its adjusted *pre cash ETR3* is in the first bin (i.e., lowest quartile). Conversely, firms that do not qualify as low-tax firms are classified as high-tax firms.

Since we are interested in changes to firms' tax planning and our sample consists of one observation per firm in an alliance (i.e., our sample mirrors a pooled cross section and not a panel), we utilize the first difference as outcome variable of interest:

$$change\ cash\ ETR3_{i,t=1} = cash\ ETR3_{[t_1;t_3]} - pre\ cash\ ETR3_{[t_{-2};t_0]} \tag{3}$$

Values of > 0 for *change cash ETR3* from equation (3) indicate increases in cash effective tax rates, whereas values of < 0 indicate decreases in cash effective tax rates which would be consistent with more tax planning.

3.3 Focal Independent Variable: *hightolow*

Strategic alliances may be composed of low-tax firms only, high-tax firms only, or a combination of high-tax and low-tax firms (see Section 3.2). In our analyses, we focus on high-tax firms as potential beneficiaries of tax knowledge diffusion ("work with superwoman") and discriminate between high-tax firms that initiate strategic alliances with low-tax firms ($hightolow = 1$) and high-tax firms that initiate strategic alliances with other high-tax firms ($hightolow = 0$):

$$\boldsymbol{hightolow} = \begin{cases} 1, & \textbf{high-tax firm in alliance to low-tax firm(s),} \\ 0, & \textbf{high-tax firm in alliance to high-tax firm(s).} \end{cases} \tag{4}$$

Applying the above described identification strategy leads us to 284 observations of $hightolow = 1$ and 965 observations of $hightolow = 0$. Overall, our sample selection and identification strategy ensures that high-tax firms are in very similar situations except for potentially experiencing tax knowledge diffusion. This implies that mechanical tax effects associated with strategic alliances would affect both groups of *hightolow* similarly (the same would hold for mean-reversion). Therefore, high-tax firms establish the treatment group and the control group for our analyses whereas their allocation to the treatment group exogenously depends on a partner's tax knowledge (see also Section 4.2). Our control-group, thus, represents pseudo treated high-tax firms with on average identical chances and challenges after initiating a strategic alliance as the treated high-tax firms except for the fact that their alliance has no low-tax partner.

3.4 Regression Design

We estimate the following linear regression model by OLS:

$$
\begin{aligned}
change\ cash\ ETR3_{i,t=1} = \beta_0 \ &+\ \beta_1 hightolow_{i,t=1} \ +\ \Sigma_n\, \beta_n\, partner\ controls^n_{i,t=1} \\
&+\ \Sigma_l\, \beta_l\, alliance\ controls^l_{i,t=1} \ +\ \Sigma_k\, \beta_k\, firm\ controls^k_{i,t=1} \\
&+\ \delta_{ind} \ +\ \tau_t \ +\ \varepsilon_{i,t}
\end{aligned}
\tag{5}
$$

The outcome variable of interest in equation (5) is *change cash ETR3* which is the first difference estimator *cash ETR3* minus *pre cash ETR3* (see also equation (3)). It captures changes in (high-tax) firms' tax planning from before to after the initiation of a strategic alliance in t_1. The independent variable of interest in equation (5) is *hightolow*, which is constructed as an indicator variable to distinguish between high-tax firms that invest in strategic alliances with low-tax firms ($hightolow = 1$) and high-tax firms that invest in strategic alliances with other high-tax firms ($hightolow = 0$). In essence, *hightolow* captures eventual differences in *change cash ETR3* between treatment and control observations that stem from the treatment. A negative coefficient for *hightolow* would suggest that effective tax rates of treated firms in strategic alliances decrease relative to the effective tax rates of control firms. Our conceptual framework indicates that this finding would identify tax knowledge diffusion via strategic alliances.

Additionally, we include multiple vectors of control variables that are constructed at the partner-, alliance-, and firm-level. The *partner controls* capture how the partners' organizational structures and their operational environments relate to each other. From Compustat data, we infer whether the partners in an alliance share the audit firm and/or industry affiliation in the year of alliance initiation (i.e., we construct the indicator variables *PartSameAuditor* and *PartSameInd*). We also observe whether their headquarters are located in the same region as defined by the Bureau of Economic Analysis (*PartSameBEARegion*).[4] In the main analysis, we substitute *PartSameBEARegion* with a manually collected measure of the geographical distance (as the crow flies) between the zip codes of the partners' headquarters (*HQDistance*) to control for the potential impact of geographical proximity in tax knowledge diffusion.

[Figure 1 about here.]

Furthermore, characteristics of a strategic alliance related to its business purpose might facilitate or impede tax knowledge diffusion. SDC provides information on an alliance's business purpose with a description of every strategic alliance. We apply textual analysis to these deal descriptions to derive the main business purposes of the strategic alliances in our sample (*alliance controls*). The word cloud depicted in Figure 1 shows the 40 most common words used in the descriptions. We base regular expressions on selected features and use pattern matching to identify *Purpose Develop*, *Purpose Licensing*, *Purpose Marketing*, *Purpose Manufacturing*, *Purpose Service*, *Purpose Supply*, *Purpose Tech*, and *Purpose Wholesale* as main activities. We construct indicator variables for each of these activities and include them in equation (5).

Finally, we control for within-firm determinants of tax planning by including a vector of *firm controls*. We basically follow Dyreng, Hanlon, and Maydew (2010) and consider *AdExp3*, *CapEx3*, *Cash3*, *EBITDA3*, *GrowthSale3*, *Intangibles3*, *Leverage3*, *MNE3* (indicator), *NOL3* (indicator), *NOL3*, *PPE3*, *RnDExp3*, *SGA3*, and *Size3*, whereas continuous *firm controls* are included as first differences ($[t_1; t_3] - [t_{-2}; t_0]$). Additionally, we employ data by Gentry et al. (2021) on CEO turnover, to control for the tension between tax knowledge transfers via labor markets and top management continuity as eventual facilitator of knowledge diffusion. We construct the indicator variable *CEOTurnover3*, which equals one when a firm experiences a CEO turnover between t_1 and

[4] The respective BEA regions are Far West, Great Lakes, Mideast, New England, Plains, Rocky Mountains, Southeast and Southwest.

t_3. We include year τ_t and industry δ_{ind} fixed effects in equation (5) and cluster robust standard errors at the firm level (Petersen 2009).

3.5 Descriptive Statistics

[Table 2 about here.]

Table 2 contains descriptive statistics for the outcome variables of interest (e.g., *change cash ETR3*) and the *firm controls* (see Section 3.4) conditional on the classifications by *hightolow* and *lowtohigh*, whereas *lowtohigh* is an indicator variable that is constructed inversely to *hightolow* by classifying whether low-tax firms are in strategic alliances with other low-tax firms or with high-tax firms (for an analysis, see Section 5). Conditioning on *hightolow*, we do not observe economically significant differences in the *firm controls* between the treatment group and the control group. In particular, only few differences in the averages of the *firm controls* are statistically significant at the 10% level (differences in *change Cash3*, *change Leverage3*, and *NOL3* are statistically different and lower between treatment and control group with p-values of 0.0786, 0.0923, and 0.0274). We conclude that our control-group represents pseudo treated firms with on average identical chances and challenges after initiating a strategic alliance as the treated firms except for the fact that their alliance has no low-tax partner. Consequently, these observations lend additional credibility to our control group identification.

4 Results

4.1 Main Analysis

[Figure 2 about here.]

Before we turn to the multivariate analysis, we descriptively analyze changes in the tax planning behavior of high-tax firms in strategic alliances with low-tax firms in comparison to high-tax firms in strategic alliances with high-tax firms. In Figure 2, we depict density plots and box plots of *change cash ETR3* conditional on *hightolow*. Generally, we observe reductions in cash effective tax rates for treatment and control groups and test for the difference between groups (and between periods). The right edge of the figure depicts the difference in the averages of *change cash ETR3* between the *hightolow* conditions (including the 95% error bar). The difference is negative and economically and statistically significant (difference of -0.0298 and p-value of 0.01). We interpret this finding as a first indication of the existence of tax knowledge diffusion via strategic alliances. In particular, we conclude that this and subsequent results do not reflect unexpected variation or increases in effective tax rates among the control observations.

[Table 3 about here.]

The main variable of interest in our regression analysis is *hightolow* because it isolates the incremental effect a low-tax partner exerts on a high-tax firm's tax knowledge and eventually on its tax planning behavior. In Table 3, we show the results for estimating equation (5) with *change cash ETR3* (columns (1) and (2)), *delta cash ETR3* (column (3)), and *cash ETR3* (column (4)) as dependent variables, whereas *delta cash ETR3* is constructed as *cash ETR3* scaled by *pre cash ETR3* minus one. The estimates for *hightolow* are negative and significant in all specifications. In the specification that includes all control variables and has *change cash ETR3* as the dependent variable (column (2), preferred specification), the estimate for *hightolow* has a magnitude of -0.0302 (p-value 0.018). Economically, this and the other results in Table 3 are consistent with our descriptive inferences in terms of direction and magnitude for both post-levels of and changes in tax planning behavior. Notably, we test for differences in the development of effective tax rates between high-tax firms conditional on their treatment status. If reversion to the mean influenced high-tax firms' post-initiation effective tax rates, our research design would account for this factor and persist in identifying an incremental treatment effect because mean reversion should occur irrespective of the treatment status. Because the covariates of partner, alliance, and firm controls additionally account for a broad range of alternative explanations, we conclude that the (relative) increase in tax planning for high-tax firms in alliances to low-tax partners stems from tax knowledge diffusion altering the equilibrium of expected costs and benefits of tax planning.

Furthermore, the estimates for the control variables are consistent between the specifications. In comparison to the estimates for *hightolow*, however, these estimates are either economically marginally influencing the outcome variables of interest or are statistically not significant at conventional levels. For instance, the estimate for *Purpose Develop* loads negative but is insignificant in the preferred specification (p-value 0.209). We cautiously interpret the coefficient to be consistent with research that shows that strategic alliances in R&D lead to higher patent output (K. Li, Qiu, and Wang 2019) and that patents have a causal effect on corporate tax planning that is incremental to the effect of R&D expenses on tax planning (Cheng et al. 2021). If strategic alliances in R&D, however, allowed firms to employ specific tax credits (as broadly suggested by Demirkan, Olson, and Zhou (2024)), this effect would be unconditional to our classification of alliances/observations as of the *hightolow* type. In several cross-sectional analyses (see Section 4.3), we additionally focus on interactions of *hightolow* and control variables to investigate whether these interactions eventually remove or complement the identified effects from the preferred specification.[5]

[Table 4 about here.]

Thus far, the data indicate that the high-tax firms in our sample are in very similar situations except for potentially experiencing tax knowledge diffusion. In the interest of caution and methodological thoroughness, we employ entropy-balancing weighting (Hainmueller 2012; Hainmueller and Xu 2013) and use the entropy weights to re-estimate equation (5). These weights approach to control for any characteristics of high-tax firms investing into low-tax alliances that could differ from the characteristics of high-tax firms investing in high-tax alliances and drive differences in the outcome variable. Specifically, Table 4 presents results for multiple variations of our preferred specification which employ entropy weights that are constructed over different moments and variables. The first specification uses the continuous *firm controls* over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$ to calculate the entropy weights so that control observations are reweighted to satisfy the balance constraint that the averages and variances (i.e., two moments) match the corresponding moments of the treated units. The entropy weights in the second specification are constructed identically but respectively use the first moment as balance constraint. We use the singleperiod firm controls in the year of the treatment (t_1) (as, for instance, in Gallemore, Gipper, and Maydew (2019)) to calculate the entropy weights in the third specification. The fourth specification uses the singleperiod observations of *cash ETR* in the preperiod $[t_{-2}; t_0]$ to calculate the weights. Throughout all specifications, we observe estimates

[5] An alliance's business purpose is selected by the partners. Therefore, we refrain from interacting *hightolow* with the *alliance controls* in additional analyses.

for *hightolow* that closely mirror the estimates from our preferred specification in Table 3, both in terms of magnitude and significance, reducing concerns about unobserved characteristics among treated and control observations explaining our results.

Overall, our main analysis reveals that high-tax firms increase their tax planning after initiating strategic alliances with low-tax firms vis-à-vis pseudo treated high-tax firms in strategic alliances with other high-tax firms. These insights indicate that tax planning responses to "working with superwoman", offering a unique perspective on the longstanding puzzle on firms' (dis)engagement from tax planning (Weisbach 2001; Desai and Dharmapala 2006; Hanlon and Heitzman 2010). Importantly, the identified average treatment effect on the treated is neither surprisingly large nor negligibly small. Our estimates indicate, consistent across a broad set of specifications, a reduction of 2.5 to 3 percentage points in a treated firm's effective tax rate, supported by a 95% confidence interval of $[-0.0552, -0.0052]$ in the preferred specification. Considering a sample average of ~29% pretreatment, the identified decreases are both statistically and economically significant.

4.2 Anticipation?

In a strategic alliance, partner choice is evidently driven by the alliance's scope. Our identification strategy, however, is agnostic about the scope of a strategic alliance and respectively considers partners' classification as low-tax or high-tax firms to determine treatment status. Therefore, we argue that firms are plausibly exogenously treated. However, if high-tax firms anticipated the beneficial diffusion of tax knowledge and selected low-tax firms as partners because of their sophisticated tax knowledge, endogenous treatment assignment would affect the inferences from OLS estimators (Lennox, Francis, and Wang 2012). Consequently, we are interested in whether treatment indeed is exogenous.

[Figure 3 about here.]

Generally, any intention to benefit from tax knowledge diffusion via an investment in a strategic alliance must be a byproduct of other main incentives. When investing in strategic alliances, firms pool their resources to achieve strategic objectives (Meier et al. 2016). Thus, it would be an economic pitfall if firms weighted the potential diffusion of tax knowledge over the selection of a partner that best suits the network's main business purpose. Furthermore, Baxamusa, Jalal, and Jha (2018) emphasize that there are considerably less due diligence analyses when investing in strategic alliances than when investing in M&As. As effective tax rates, unlike firms' underlying tax planning strategies, are publicly available, we take these arguments to an empirical analysis. Specifically, we analyze abnormal returns at the announcement dates of the strategic alliances in our sample. The idea is that if tax knowledge diffusion could be anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study in WRDS using a market adjusted model with 100 days as estimation window and a $[-1; 1]$ day event window. Generally, we find, consistent with the findings by Chan et al. (1997), that cumulative abnormal returns (CAR) are on average positive across all announcements in our sample (average CAR of 0.007 with a t-statistic 6.2). Next, we depict density plots and averages of CAR by *hightolow* in Figure 3. We observe, counter to the idea of anticipation of tax knowledge diffusion, that abnormal returns of control firms' announcements exceed the abnormal returns for the treated firms (p-value 0.0313).[6] We conclude that tax knowledge diffusion via strategic alliances is not anticipated, reducing remaining concerns about endogenous treatment assignment.

4.3 Mechanisms

Next, we are interested in facilitators and inhibitors of tax knowledge diffusion via strategic alliances. In particular, we focus on elapsed time, CEO turnover, geographical proximity, industry affiliation, and market power.

Elapsed Time

[Table 5 about here.]

Knowledge diffusion is a gradual, multi-stage process which requires continuous exchange (Bres-

[6] In an untabulated analysis, we run a regression that includes all controls from the preferred specification (incl. the fixed effects) and CAR as dependent variable. We find that the difference from the univariate test (presented in Figure 3) prevails economically and statistically.

man, Birkinshaw, and Nobel 2010; Inkpen 2000; Szulanski 1996) and elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv 2003; Isaksson, Simeth, and Seifert 2016). To test whether elapsed time facilitates tax knowledge diffusion via strategic alliances, we estimate three specifications of equation (5) and present the results in Table 5. We extend the posttreatment period (keeping the preperiod $[t_{-2}; t_0]$ constant) by one year with each specification. For the construction of the dependent variables (*change cash ETR*), we use the singleperiod *cash ETRs* at t_1, t_2, and t_3, calculate first differences against *pre cash ETR3*, and then calculate the average of the sum of these calculations over the number of periods since treatment.[7] The results indicate that the treatment effect particularly increases in magnitude with elapsed time since the initiation of an alliance. Although differences for the estimates of *hightolow* between the specifications are not statistically significant at conventional levels, the estimates within the specifications themselves turn significant with elapsed time. Our findings, thus, are consistent with the evidence by Kim et al. (2019), who suggest that firms are generally able to adjust their tax planning behavior within three years and that high-tax firms may increase their tax planning behavior faster. Furthermore, and consistent with our conceptual expectations, we conclude that elapsed time facilitates tax knowledge diffusion via strategic alliances.

CEO Turnover

[Table 6 about here.]

Next, we turn to cross sectional analyses. We estimate specifications of equation (5) with *change cash ETR3* as dependent variable and include interaction terms of *hightolow* with the cross-sectional variable of interest (X, see column headers). First, we employ data by Gentry et al. (2021) on CEO turnover and construct the indicator variable *CEOTurnover3*, which equals one when a firm experiences a CEO turnover between t_1 and t_3. In particular, we are interested in top management continuity as eventual facilitator of knowledge diffusion since management research suggests an impact for a multitude of soft factors. Prominent examples are communication (Bresman, Birkinshaw, and Nobel 2010; Bushee, Kim-Gina, and Leung 2020), partner trustworthiness (Jiang et al. 2016), commitment (Bushee, Kim-Gina, and Leung 2020), managerial flexibility (Chan et al. 1997;

[7]For instance, the dependent variable for the second specification is calculated as $[(cash\ ETR_{t_1} - pre\ cash\ ETR3) + (cash\ ETR_{t_2} - pre\ cash\ ETR3)]/2$.

Chen, King, and Wen 2015), partnering mindset (Kale, Singh, and Bell 2009), and learning intent (Hamel 1991; Mazloomi Khamseh, Jolly, and Morel 2017). Frank et al. (2021) focus on knowledge in the relationship between third-party insurers and audit firms and present interview evidence that "…one-on-one consultations tend to be most effective because they can make the necessary reductions in tacitness, ambiguity, and complexity of knowledge during the process…" (p. 38). A CEO turnover could, thus, inhibit tax knowledge diffusion. The results are depicted in Table 6 and indicate that the baseline effect for *hightolow* prevails economically and statistically. Consistent with our expectations, the interaction of *hightolow* with *CEOTurnover3* loads positive but falls short of conventional levels of statistical significance, indicating that tax planning responses when there are fewer changes in a high-tax firm's management. This result is broadly consistent with the notion that top management continuity enables the building of trust and supports exchange in the facilitation of knowledge diffusion.

Distance

Brown (2011) hypothesized that tax shelter adoption may spread regionally because "local business elites are connected through a range of formal and informal institutions that facilitate communication, from the country club to local charity organizations" (p. 34). Therefore, we are interested in whether geographical distance between the partners' headquarters removes (i.e., distance as alternative channel) or facilitates/inhibits (i.e., distance and treatment as two complementary diffusion mechanisms) tax knowledge diffusion via strategic alliance. We manually collect the geographical distance (as the crow flies in miles) between the zip codes of the partners' headquarters (*HQDistance*) and interact the standardized values with *hightolow* in the second specification of Table 6. The baseline and interaction estimates for *zHQDistance* indicate that increasing geographical distance between the partners' headquarters mitigates the observed decreases in *change cash ETR3*.[8] The interaction term, however, is respectively marginally different from zero and beyond common levels of statistical significance. The baseline effect for *hightolow* is economically and statistically similar to the results from the main analysis. These findings, generally consistent with the inferences by Brown (2011), suggest that geographical distance between firms can affect tax planning

[8] *z* indicates standardization at mean zero and a standard deviation of one. We find consistent evidence when we replace *HQDistance* with the indicator variable *PartSameBEARegion* (untabulated).

but particularly underscore our treatment indication as unique channel for tax knowledge diffusion between firms.

Same Industry

Next, we focus on partners that are in the same industry because industry peers can influence tax planning (Bird, Edwards, and Ruchti 2018) and a shared industry affiliation could speak to partner similarity. However, eventual effects of shared industry affiliation could be moderated by competition (e.g., see Bourveau, She, and Žaldkokas (2020) and J. Cai and Szeidl (2018) on the opposing effects of competition on collusion and diffusion of information; and Lavie, Lunnan, and Truong (2022) on restrictions in alliances from business similarity). We construct the indicator variable *PartSameInd* which equals one when the partners of an alliance belong to the same industry.[9] The results in column (3) of Table 6 indicate, consistent with the other cross sectional analyses, that the baseline effect for *hightolow* prevails economically and statistically. The baseline coefficient for *PartSameInd* (negative) and the estimate for *hightolow* × *PartSameInd* (positive) show different signs, are economically meaningful but fall short of statistical significance. These results suggest that a shared industry affiliation rather serves as substitute to our treatment indication and subsumes part of the treatment effect identified in the main analysis. Consistent with the inferences by Brown and Drake (2014), Brown (2011), and (research on) the effects of competition (see above), we find that *PartSameInd* does not facilitate but moderates tax knowledge diffusion for the treated.

Market Shares

Finally, we focus on differences in the market shares of the partners in strategic alliances as these differences could reflect relative power of the partners vis-à-vis each other. In particular, analyzing differences in market shares allows us to determine whether tax knowledge diffusion is driven by power dynamics or unintended. If a firm with a substantial market share could influence its alliance partner to share and transfer tax knowledge or engage in tax planning, it would suggest a power-induced mechanism, indicating intended transfers of tax knowledge. Conversely, if differences in market shares do not significantly influence (i.e., decrease) *change cash ETR3*, it would indicate

[9] When we exclude firms that belong to the Fama-French-12 industry classification "other" in this analysis (untabulated), we find consistent evidence to the results presented in Table 6.

that tax knowledge diffusion is indeed unintended.

We construct *DiffPartMarketShare* which is the difference between a high-tax firm's minus its alliance partner's *MarketShare*. The variable *MarketShare*, thereby, is constructed consistent with the market share calculations of the *HHI* (i.e., the percentage of a firm's sales within industry and year). Thus, *DiffPartMarketShare* increases with a firm's market share and decreases when the partner's market share increases. For the analysis, we standardize *DiffPartMarketShare* at mean zero and standard deviation one and include the baselines and an interaction of *hightolow* and *zDiffPartMarketShare*.[10] The results are depicted in column (4) of Table 6. The estimate for *hightolow* persists in direction, magnitude, and statistical significance. Interestingly, the interaction is positive and statistically significant (p-value 0.076). This finding indicates that with a one standard deviation increase in *DiffPartMarketShare* the baseline effect of *hightolow* on *change cash ETR3* is weakened by an economically meaningful 1.5 percentage points. Thus, we find no evidence for a power-induced mechanism but, consistent with trust and mutual commitment, that tax knowledge diffusion via strategic alliances is unintended (see also Section 4.2).

Overall, the evidence in Section 4.3 suggests that mechanisms are neither mutually exclusive nor reinforcing. In particular, we find evidence that elapsed time, consistent with an increased probability of information exchange due to trust and mutual commitment, facilitates tax knowledge diffusion via strategic alliances. Weaker evidence indicates directionally consistent findings for CEO continuity and spatial proximity between partners. We also find that a shared industry affiliation rather inhibits tax knowledge diffusion via strategic alliances. Results from analyzing differences in firms' market shares further suggest that tax knowledge diffusion is unintended and not power-induced. Throughout all cross sectional analyses the baseline estimate for *hightolow* persists in direction, magnitude, and statistical significance.

[10] z indicates standardization at mean zero and a standard deviation of one. Market shares are calculated within the Compustat universe (i.e., before merging Compustat to SDC data). We neither square firms' market shares nor include the Fama-French-12 industry classification "other" in this analysis.

4.4 Alternative Channels

[Table 7 about here.]

Same Audit Firm

Next, we examine how tax knowledge diffusion via strategic alliances is impacted when partners share an audit firm (*PartSameAuditor*) because we are interested in whether the identified effects are robust to alternative channels of intentional tax knowledge transfers. Generally, evidence on the impact of audit firms on tax planning outcomes is mixed.[11] Brown (2011) does not find significant tax shelter adoption via shared audit firms, and Klassen, Lisowsky, and Mescall (2016) show that less tax aggressiveness in the past is associated with the auditor preparing a firm's tax return. In contrast, Lim et al. (2018) and Cen et al. (2020) suggest that shared auditors facilitate tax planning. Consistent with the mixed evidence from prior literature, Nesbitt, Persson, and Shaw (2020) suggest that there are limits to the relation between auditor-provided tax services and clients' tax planning.

We construct the indicator variable *PartSameAuditor* that equals one when the partners in an alliance share an audit firm. We estimate equation (5) with *change cash ETR3* as dependent variable and include the interaction of *hightolow* with *PartSameAuditor*. Column (1) of Table 7 depicts the results. We observe that the baseline effect for *hightolow* is negative and economically meaningful (p-value 0.096). The negative estimate for the interaction term for *hightolow* × *PartSameAuditor* does not surpass common levels of statistical significance but is economically particularly sizable. We conclude that the results from our main analyses persist and that we do not spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances.

Board Ties

Since Brown and Drake (2014) indicate that board ties can impact tax planning of connected firms, we consider board ties among the partners in the alliances of our sample. We use data from ISS

[11] E.g., see Aobdia (2015); Y. Cai et al. (2016); Dhaliwal et al. (2016); McGuire, Omer, and Wang (2012); Klassen, Lisowsky, and Mescall (2016); Lim et al. (2018); Frey (2018); Bianchi et al. (2018); Nesbitt, Persson, and Shaw (2020); Hux, Bedard, and Noga (2022).

to construct the indicator variable *BoardTie3* that equals one when the partners have at least one common member among their board of directors (at any point over the $[t_1; t_3]$ period). First, we note that board ties are rare among the observations in our sample. This is particularly true for the treated observations (we observe an overlap of seven board ties among the 284 *hightolow* = 1 observations). Therefore, we do not estimate a specification that includes an interaction but respectively add *BoardTie3* as additional control variable to the analysis. Results for this specification are depicted in column (2) of Table 7. We observe that the inferences from our main analysis effectively remain unchanged because the coefficient for *hightolow* is estimated as − 0.0303 (main analysis − 0.0302). In seemingly unrelated regressions (untabulated), we additionally analyze whether our inferences change when we remove observations with board ties from the sample (both for control and treated observations). We find that the estimated coefficient for *hightolow* remains effectively unchanged (difference − 0.001). Therefore, we conclude that the results from our main analyses persist and that we do not spuriously pick up an alternative channel (here board ties) when identifying tax knowledge diffusion via strategic alliances.

5 Robustness Checks

[Table 8 about here.]

Alternative Tax Planning Measures

We turn to the robustness of our results by employing alternative tax planning measures. Table 8 depicts results for specifications of equation (5) with *change CTD3* (column (1)) and *change GAAP ETR3* (column (2)) as dependent variables. We utilize the cash tax differential (*CTD*) developed by Henry and Sansing (2018) which allows us to identify whether high-tax firms become rather tax-favored relative to the control observations. *GAAP ETR3* utilizes a GAAP measure of taxes paid (i.e., total income tax expense) instead of cash taxes paid in the numerator of the effective tax rate. For both *change CTD3* and *change GAAP ETR3*, a negative estimate for *hightolow* would be consistent with our main analysis. We find consistent and economically meaningful evidence. For instance, the estimate for *change GAAP ETR3* is just marginally shy to the estimate in our preferred specification with *change cash ETR3* as dependent variable.

Pretreatment ETR Volatility

Next, we focus on volatility in the singleperiod *cash ETR* observations in the pretreatment period because we want to ensure that we do not interpret volatility in high-tax firms' tax planning measures as eventual treatment effect (see also the fourth specification in Table 4, which uses pretreatment singleperiod *cash ETRs* to calculate the entropy weights). Initially, we construct a volatility measure σ that captures the volatility in the cash effective tax rates in the pretreatment period:

$$\sigma = \sqrt{\frac{1}{3}\sum_{i=-2}^{0}(cash\ ETR_i - pre\ cash\ ETR3)^2} \tag{6}$$

We then test (untabulated) whether the measure from equation (6) differs between treatment and control group and find no statistically significant difference. Finally, we exclude all observations that belong to the top-quintile of the volatility measure from the analysis and re-estimate equation (5) (column (3) of Table 8). We find that the identified effect from our main analysis prevails and conclude that our findings do not reflect eventual pretrends in effective tax rates of high-tax firms.

Low-Tax Firms

In our main analysis, we do not consider low-tax firms because we are interested in whether high-tax firms' tax planning responses to "working with superwoman". Furthermore, our conceptual framework of (unintended) knowledge diffusion suggests that there is little reason to expect a tax planning response for low-tax firms. However, if tax knowledge is transferred intentionally for joint tax planning (e.g., see Cen et al. (2017); Cen et al. (2020)), low-tax firms' effective tax rates could also change. To empirically control for this notion, we construct *lowtohigh*, which is an indicator that equals one for low-tax firms in alliances with high-tax firms and zero for low-tax firms in alliances with low-tax firms. We then re-estimate equation (5) (column (4) of Table 8). We find that the coefficient estimate for *lowtohigh* is (i) beyond common levels of significance (p-value 0.753) and (ii) economically not meaningful different from zero.

Valuation Allowance Releases

[Table 9 about here.]

Table 9 depicts results of specifications that consider advances by Drake, Hamilton, and Lusch (2020) on the effect of valuation allowance releases on effective tax rates. The authors document how effective tax rates decrease when valuation allowances are released and conclude that this effect challenges the assumption that lower effective tax rates indicate tax planning. Therefore, we follow the insights in Drake, Hamilton, and Lusch (2020) and calculate the probability for such events from Compustat data. We then construct first differences between the $[t_1; t_3]$ and $[t_{-2}; t_0]$ periods (*change Val. All. Release3*) and test whether our treatment indication increases the probability for a valuation allowance release (column (1) of Table 9).

We find no evidence for such an increase. Rather, the estimate for *hightolow* is negative and beyond common levels of statistical significance. Next, we include *change Val. All. Release3* as additional control variable, interact it with *hightolow*, and regress these and the control variables from equation (5) on *change cash ETR3*. If releases of valuation allowances confoundingly captured decrease in effective tax rates, we would expect that the baseline effect for *hightolow* diminishes. Again, we find no evidence for this influence. Instead, the estimate is respectively marginally different from our main analysis. Finally, we are interested in whether our classification of low-tax firms (as an prerequisite for the treatment assignment of high-tax firms) captured firms that show low effective tax rates due to valuation allowance releases. Therefore, we capture not the own firm's but the partner's probability for a valuation allowance release over the $[t_{-2}; t_0]$ period (*Part Pre Val. All. Release3*) and include and interact this variable with *hightolow* (column (3)). We find that the inferences from our analyses remain unchanged when controlling for this variable. In essence, these findings support our identification strategy. We conclude that our results are robust to the influence of valuation allowance release and indeed capture tax planning responses by the treated high-tax firms due to tax knowledge diffusion.

6 Conclusion

The purpose of this study is to shed light on whether "working with superwoman" triggers tax planning responses suggesting a novel perspective on the puzzle on firms' (dis)engagement from tax planning. Utilizing data on strategic alliances between publicly traded U.S. firms allows us to distinguish between alliances that bring together high-tax and low-tax firms. We empirically identify tax knowledge diffusion via strategic alliances by robustly documenting an economically meaningful decrease in cash effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to the effects on high-tax firms in strategic alliances with other high-tax firms.

Building on our conceptual framework we investigate several mechanisms which may facilitate tax knowledge diffusion via close cooperation and continued exchange between alliance partners. We observe that elapsed time, likely indicative of enhanced information exchange facilitated by trust and mutual commitment, promotes the diffusion of tax knowledge through strategic alliances. While there are indications supporting this assertion, albeit weaker, regarding CEO continuity and spatial proximity between partners, the evidence suggests that shared industry affiliation inhibits the diffusion of tax knowledge through such alliances.

Our findings suggest that informal private exchange may reduce the expected costs of tax planning. This effect seems to be unforeseen prior to the establishment of a strategic and on average it only benefits high-tax firms. Hence, our research underscores the significance of integrating tax considerations into the managerial frameworks governing strategic alliances. Additionally, it provides insights for policymakers regarding how promoting collaboration through strategic alliances can impact firms' decisions regarding tax planning.

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Appendix

Table A1: Variable Definitions

Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
Sampling Unit	
Strategic Alliance	Contract based cooperation between publicly traded U.S. firms in the sample period 1994 to 2021; data are from SDC Platinum (STRATEGICALLIANCE/SAF); for analyses, data are considered at the firm × alliance level.
Main Outcome Variables	
change cash ETR3	First difference between *cash ETR3* and *pre cash ETR3*, calculated as: *change cash ETR3* = *cash ETR3* - pre *cash ETR3*
pre cash ETR3	See *cash ETR3* for construction; difference: numerator and denominator are calculated over three preceding periods to the initiation of an alliance.
cash ETR3	Multiperiod cash effective tax rate: $cash\ ETR3_{i,t=1} = \frac{\Sigma_{t=1}^{3}(txpd_{i,t})}{\Sigma_{t=1}^{3}(pi_{i,t}-spi_{i,t})}$ Defined as cash taxes paid (txpd) divided by pre-tax income (pi) before special items (spi); special items are reset to 0 when missing; numerator and denominator are constructed as the sum of the current and two subsequent fiscal years (with alliance initiation in t_1); observations with a negative denominator are reset to missing; winsorized at 0 and 1.
delta cash ETR3	Alternative to *change cash ETR3*; constructed as *cash ETR3* scaled by *pre cash ETR3* minus one; reset to missing when numerator or denominator equal 0; winsorized at p1 and p99.
Main Variables of Interest	
high-tax firm	Inverse to *low-tax firm*; indicator variable; constructed at alliance initiation t_1; equals 1 if the firm's industry adjusted *pre cash ETR3* does not belong to the lowest quartile; 0 for low-tax firms.
hightolow	Treatment indicator; indicator variable; equals 1 for high-tax firms in strategic alliance with low-tax firms; equals 0 for high-tax firms in strategic alliance with high-tax firms; see *high-tax firm* and *low-tax firm* for details.
low-tax firm	Indicator variable; constructed at alliance initiation t_1; equals 1 if the firm's industry adjusted *pre cash ETR3* does belong to the lowest quartile; 0 for high-tax firms.
lowtohigh	Indicator variable; equals 1 for low-tax firms in strategic alliance with high-tax firm; equals 0 for low-tax firms in strategic alliance with low-tax firm; see *high-tax firm* and *low-tax firm* for details.
Partner Controls	
HQDistance	Distance (in miles as the crow flies) between the partners of an alliance according to the zip code of the partners' historical headquarters (addzip) at t_1; collected from freemaptools.com; standardized for regressions.
PartSameAuditor	Indicator variable; equals 1 when all partners in an alliance share the same audit firm (au) in t_1; 0 otherwise.
PartSameInd	Constructed identically to *PartSameAuditor* but for industry affiliation; industry is classified using Fama French 12 industries (sic).

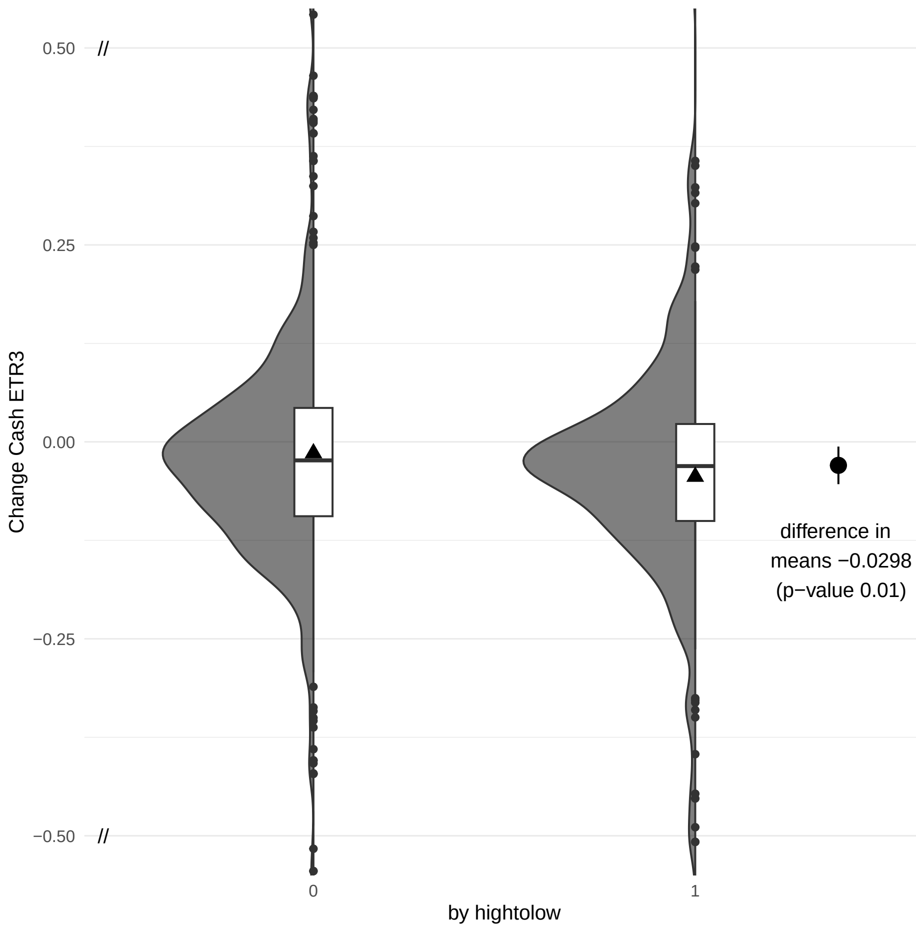
Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
PartSameBEARegion	Substitute to *HQDistance*; constructed identically to *PartSameAuditor* but for HQ-locations; equals 1 when all partners in an alliance are located in the same BEA region in t_1; 0 otherwise; the respective regions, as defined by the Bureau of Economic Analysis, are Far West, Great Lakes, Mideast, New England, Plains, Rocky Mountains, Southeast and Southwest.
Alliance Controls	
Σ alliance controls	Indicator variables; indicative of the main business purpose of a strategic alliance; derived from an alliance's deal description (DEALTEXT) in SDC; comprise *PurposeDevelop, PurposeLicense , PurposeManufacture, PurposeMarketing, PurposeService, PurposeSupply, PurposeTech, and PurposeWholesale.*
Firm Controls (Indicators)	
CEOTurnover3	Indicator variable, equals 1 when a firm experiences a CEO turnover in the current or subsequent two firm-years $[t_1; t_3]$, 0 otherwise; data from Gentry et al. (2021).
MNE3	Indicator variable; equals 1 for nonmissing, nonzero sum of pre-tax income from foreign operations (pifo) over $[t_1; t_3]$; 0 otherwise.
NOL3	Indicator variable equals 1 for nonmissing, nonzero sum of tax loss carry forwards (tlcf) over $[t_1; t_3]$; 0 otherwise.
Firm Controls (Continuous)	
AdExp3	Advertising expense (xad) divided by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
CapEx3	Reported capital expenditures (capx) divided by gross property, plant, and equipment (ppegt); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Cash3	Cash and cash equivalents (che) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
GrowthSale3	The annual average growth rate (geometric mean) of net sales (sale) over three years; when missing reset to annual growth rate, thereafter reset to 0.
EBITDA3	Earnings before interest, taxes, depreciation and amortization (ebitda) scaled by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Intangibles3	The ratio of intangible assets (intan) to total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.

Variable	Definition (Compustat (low)/SDC (CAPITAL) data items)
Leverage3	The sum of long-term debt (dltt) and long-term debt in current liabilities (dlc) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
NOL3	The sum of tax loss carry forwards (tlcf) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0; this variable is included as *change NOL3* in analyses (see below) and *NOL3* is included as indicator (see above).
PPE3	Gross property, plant, and equipment (ppegt) divided by total assets (at); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
RnDExp3	Research and development expenses (xrd) scaled by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0
SGA3	Selling, general, and administrative expense (xsga); divided by net sales (sale); numerator and denominator are constructed as the sum of the current and two subsequent years; when missing reset to annual measure, thereafter reset to 0.
Size3	The natural log of total assets (at) for the respective and two subsequent periods; when missing reset to annual measure, thereafter reset to 0. **Note**: continuous *firm controls* are constructed as first differences for the analyses (e.g., see *change cash ETR3*).
Other Variables	
BoardTie3	Indicator variable; we use identifiers of the members of firms' boards of directors from ISS (formerly Riskmetrics) to construct a variable that equals one when the partners in an alliance have at least one common board member (i.e., a board tie); zero otherwise.
CAR	Cumulative abnormal return; we run an event study (to the announcements of the strategic alliances on our sample) in WRDS using a market adjusted model with 100 days as estimation window and a $[-1; 1]$ day event window.
CTD	Cash tax differential; calculated following Henry and Sansing (2018); captures the extent to which a firm is tax-favored (< 0) or tax-disfavored (> 0).
DiffPartMarketShare	Difference between a high-tax firm's and its alliance partner's *MarketShare*. *MarketShare* is constructed consistent with the market share calculations of the HHI (i.e., the percentage of a firm's sales (sale) within industry and year (fyear)); *MarketShare* (which is not squared) is constructed within the Compustat universe; industry "other" is neglected; standardized for regressions.
GAAP ETR	see *cash ETR3*; the *GAAP ETR* utilizes a GAAP measure of taxes paid (i.e., total income tax expense, txt) instead of cash taxes paid (txpd) in the numerator of the effective tax rate.
Valuation Allowance Release	Probabilities for valuation allowance releases are calculated following Drake et al. (2020); linear combination that considers previous loss years (pi), tax loss carry forwards (tlcf, both as indicator and continuous change variable), and free cash flows (oancf, capx).

Figure 1: Main Business Purposes of Strategic Alliances

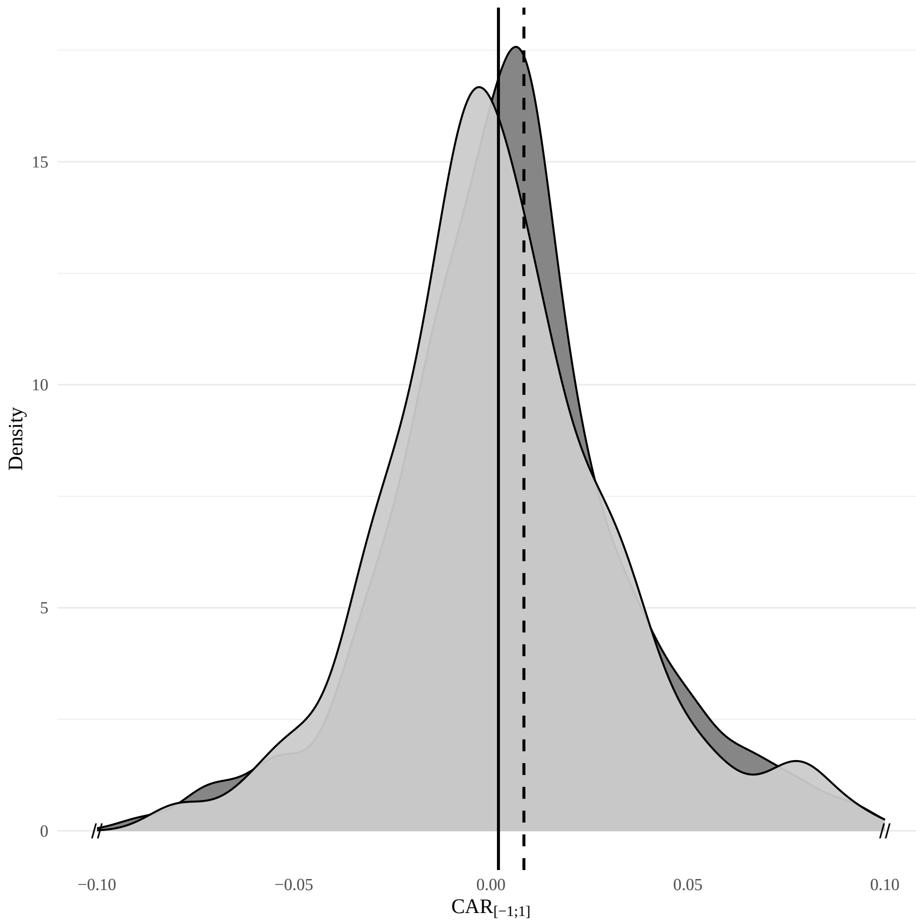
The word cloud depicted in this figure shows the 40 most common words used in the deal description of the alliances in our sample (as provided by SDC). By systematically searching through the deal descriptions (using regular expressions based on the above presented terms), we identify developing, licensing, manufacturing, marketing, services, supply, technology, and wholesale (in alphabetical order) as main business purposes of the alliances in our sample. We construct indicator variables accordingly and include them in our regression analyses (*alliance controls*). All variables are defined in detail in the Appendix.

Figure 2: Changes in Tax Planning - Treatment & Control



This figure depicts density plots and box plots (triangles mark the averages) of *change cash ETR3* conditional on *hightolow* (the depicted distributions of *change cash ETR3* in this figure include the $[-0.5; 0.5]$ range). Generally, we observe reductions in cash effective tax rates for treatment and control groups and test for the difference between groups (and between periods). The right edge of the figure depicts the difference in the averages of *change cash ETR3* between the *hightolow* conditions (including the 95% error bar). All variables are defined in detail in the Appendix.

Figure 3: Abnormal Returns at Announcement



This figure depicts density plots and averages of cumulative abnormal returns (*CAR*) by *hightolow*: light-gray fill and solid vertical line if *hightolow* equals 1; dark-gray fill and dashed vertical line if *hightolow* equals 0. If tax knowledge diffusion was anticipated from a partner's publicly available tax information (i.e., whether or not the partner is a low-tax firm (treatment)), such anticipation would, all else equal, be reflected at capital markets through higher abnormal returns for the treated firms. Therefore, we run an event study in WRDS using a market adjusted model with 100 days as estimation window and a $[-1; 1]$ day event window. We find (i) that *CARs* are on average positive across all announcements in our sample, and (ii) that abnormal returns for treated observations are on average lower, reducing remaining concerns about endogenous treatment assignment. The depicted distributions of *CARs* in this figure include the $[-0.1; 0.1]$ range. All variables are defined in detail in the Appendix.

Table 1: Sample Selection

Panel A: Strategic Alliances

Selection Step SDC Platinum (UPPER) and Compustat (lower) data items in parentheses	Alliances	Firms	Firm × Alliance
Compustat (cusip) and SDC Platinum (ULTPARENTCUSIP) data merged according to year of alliance initiation (DATEEFFECTIVE / fyear) & strategic alliances between U.S. firms (loc, fic, curcd, cik) & sample period 1994 - 2021	26,148	6,667	31,418
./. Identify all firms in a strategic alliance in Compustat data (NUMBEROFPARTICIPANTS)	4,654	3,676	9,425
./. Identify *pre cash ETR3* and *cash ETR3* (txpd, pi, spi) of all firms in a strategic alliance	808	845	**1,629**

Panel B: Classification

High-Tax Firms	
high-tax firm in alliance to low-tax firm(s) = treated	**284**
high-tax firm in alliance to high-tax firm(s) = control	**965**
Treated + control = number of obs. in main analysis	**1,249**
Low-Tax Firms	
low-tax firm in alliance to high-tax firm(s)	285
low-tax firm in alliance to low-tax firm(s)	95

We exploit data on strategic alliances from Refinitiv's SDC Platinum (SDC) database on strategic alliances over the 1994-2021 period (see Panel A). Initially, we deflate our sample to observations that are flagged as strategic alliances by excluding equity joint ventures from the data. We then reshape data from the alliance to the partner (i.e., firm × alliance) level. To illustrate: a strategic alliance between two partners translates to one observation for each of the two partners (i.e., two observations at the firm × alliance level.) Compustat data (via Wharton Research Data Services) provide firm-year-level accounting information, and we merge SDC and Compustat data by using a firm's historical six-digit CUSIP number (at the level of the ultimate parent of the participant). Next, we require to observe *cash ETR3* and *pre cash ETR3* of all partners for an alliance to be considered in our analysis. We classify (see Panel B) firms as high-tax or low-tax based on their *pre cash ETR3* which we industry-year-mean adjust for this purpose (i.e., we are interested in firms that show low/high multiperiod effective tax rates among their industry peers just before an alliance is initiated). We then allocate this adjusted *pre cash ETR3* into four bins according to the quartiles of its distribution. Consequently, strategic alliances may be composed of low-tax firms only, high-tax firms only, or a combination of high-tax and low-tax firms. In our analyses, we focus on high-tax firms as potential beneficiaries of tax knowledge diffusion ("work with superwoman") and discriminate between high-tax firms that invest in strategic alliances with low-tax firms (*hightolow* = 1, treated) and high-tax firms that invest in strategic alliances with other high-tax firms (*hightolow* = 0, control). All variables are defined in detail in the Appendix.

Table 2: Firm ×Alliance Observations

N (1,629)		284			965			285			95	
hightolow		=1			=0							
lowtohigh								=1			=0	
	mean	p50	SD	mean	p50	SD	mean	p50	SD	mean	p50	SD
change cash ETR3	−0.0442	−0.0308	0.1783	−0.0144	−0.0235	0.1856	0.0736	0.0430	0.1455	0.0851	0.0721	0.1611
pre cash ETR3$_{[t_{-2};t_0]}$	0.2917	0.2778	0.1373	0.2804	0.2643	0.1296	0.0934	0.0932	0.0578	0.0947	0.0894	0.0610
cash ETR3$_{[t_1;t_3]}$	0.2475	0.2249	0.1449	0.2660	0.2344	0.1755	0.1670	0.1427	0.1501	0.1798	0.1571	0.1676
delta cash ETR3	−0.0377	−0.1151	0.6189	0.0547	−0.0857	0.7829	1.5080	0.4804	3.0103	1.7000	0.6339	3.2187
Partner Controls												
z HQ Distance	−0.0195	−0.3071	1.0534	0.0058	−0.1881	0.9842	.	.	.	.	.	.
PartSameAuditor	0.2183	0.0000	0.4138	0.2010	0.0000	0.4010	0.2175	0.0000	0.4133	0.2526	0.0000	0.4368
PartSameInd	0.4366	0.0000	0.4968	0.4767	0.0000	0.4997	0.4386	0.0000	0.4971	0.6000	1.0000	0.4925
Alliance Controls												
Purpose Develop	0.2606	0.0000	0.4397	0.2705	0.0000	0.4444	0.2632	0.0000	0.4411	0.3053	0.0000	0.4630
Purpose Licensing	0.1725	0.0000	0.3785	0.1679	0.0000	0.3739	0.1719	0.0000	0.3780	0.1263	0.0000	0.3340
Purpose Manufacturing	0.2570	0.0000	0.4378	0.2394	0.0000	0.4269	0.2561	0.0000	0.4373	0.2526	0.0000	0.4368
Purpose Marketing	0.1268	0.0000	0.3333	0.0705	0.0000	0.2561	0.1263	0.0000	0.3328	0.0842	0.0000	0.2792
Purpose Service	0.0387	0.0000	0.1933	0.0249	0.0000	0.1558	0.0386	0.0000	0.1930	0.0000	0.0000	0.0000
Purpose Supply	0.0634	0.0000	0.2441	0.0466	0.0000	0.2110	0.0632	0.0000	0.2437	0.0947	0.0000	0.2944
Purpose Tech	0.3838	0.0000	0.4872	0.4321	0.0000	0.4956	0.3860	0.0000	0.4877	0.3684	0.0000	0.4849
Purpose Wholesale	0.0880	0.0000	0.2838	0.0456	0.0000	0.2087	0.0877	0.0000	0.2834	0.1263	0.0000	0.3340
Firm Controls												
CEO Turnover3	0.3063	0.0000	0.4618	0.3523	0.0000	0.4779	0.3509	0.0000	0.4781	0.2947	0.0000	0.4583
change AdExp3	−0.0010	0.0000	0.0067	−0.0001	0.0000	0.0099	−0.0008	0.0000	0.0089	−0.0010	0.0000	0.0120
change CapEx3	−0.0162	−0.0055	0.0579	−0.0126	−0.0051	0.0577	−0.0120	−0.0042	0.0756	−0.0317	−0.0179	0.0748
change Cash3	−0.0084	0.0011	0.0837	−0.0183	−0.0033	0.0841	−0.0086	−0.0028	0.0856	−0.0103	−0.0064	0.0872
change EBITDA3	−0.0256	−0.0175	0.0591	−0.0303	−0.0207	0.0623	−0.0265	−0.0132	0.0634	−0.0285	−0.0238	0.0567
change GrowthSale3	−0.0484	−0.0309	0.1443	−0.0385	−0.0257	0.1398	−0.0556	−0.0339	0.1503	−0.0356	−0.0376	0.1398
change Intangibles3	0.0266	0.0010	0.0806	0.0258	0.0018	0.0803	0.0326	0.0081	0.0910	0.0292	0.0064	0.0699
change Leverage3	0.0096	0.0008	0.0833	0.0187	0.0037	0.0793	0.0016	0.0000	0.1149	0.0170	0.0000	0.0940
MNE3	0.7148	1.0000	0.4523	0.7492	1.0000	0.4337	0.7684	1.0000	0.4226	0.8000	1.0000	0.4021
NOL3	0.2958	0.0000	0.4572	0.3668	0.0000	0.4822	0.4175	0.0000	0.4940	0.4000	0.0000	0.4925
change NOL3	0.5794	0.0293	3.9612	0.4683	0.0136	3.6939	0.0136	0.0153	4.0831	0.3067	0.0057	4.1486
change PPE3	−0.0284	−0.0180	0.0996	−0.0188	−0.0065	0.0955	−0.0140	−0.0040	0.1015	−0.0081	−0.0112	0.1011
change RnDExp3	0.0015	0.0000	0.0245	−0.0007	0.0000	0.0337	−0.0044	0.0000	0.0357	−0.0005	0.0000	0.0309
change SGA3	−0.0009	0.0000	0.0440	−0.0011	0.0000	0.0475	0.0046	0.0000	0.0470	0.0075	0.0000	0.0500
change Size3	0.3172	0.2248	0.3605	0.3490	0.2169	0.4099	0.3830	0.2751	0.4240	0.3209	0.3025	0.3627

This table shows descriptive statistics for our main variables of interest (e.g., *change cash ETR3*), *partner controls*, *alliance controls*, and *firm controls*. Statistics are presented by conditioning the observations on *hightolow* and *lowtohigh*. All variables are defined in detail in the Appendix.

Table 3: Main Analysis

	Dependent Variable:			
	change cash ETR3	*change cash ETR3*	*delta cash ETR3*	*cash ETR3*
hightolow	−0.0250	−0.0302	−0.1274	−0.0253
	(0.0136)	(0.0127)	(0.0478)	(0.0111)
Partner Controls				
zHQDistance		0.0099	0.0073	−0.0021
		(0.0049)	(0.0207)	(0.0047)
PartSameAuditor		−0.0183	−0.0296	−0.0105
		(0.0139)	(0.0437)	(0.0104)
PartSameInd		−0.0181	−0.0792	−0.0157
		(0.0123)	(0.0473)	(0.0112)
Alliance Controls				
PurposeDevelop		−0.0167	−0.0987	−0.0193
		(0.0133)	(0.0558)	(0.0113)
PurposeLicensing		−0.0266	−0.0674	−0.0139
		(0.0168)	(0.0683)	(0.0135)
PurposeManufacturing		−0.0218	0.0012	0.0277
		(0.0255)	(0.1027)	(0.0184)
PurposeMarketing		0.0083	0.0782	0.0028
		(0.0132)	(0.0551)	(0.0110)
PurposeService		−0.0286	−0.0772	−0.0267
		(0.0232)	(0.0952)	(0.0323)
PurposeSupply		−0.0135	−0.0069	0.0204
		(0.0249)	(0.0736)	(0.0229)
PurposeTech		−0.0206	−0.0649	−0.0138
		(0.0130)	(0.0485)	(0.0119)
PurposeWholesale		−0.0171	−0.0010	0.0180
		(0.0264)	(0.1043)	(0.0195)
Firm Controls				
CEOTurnover3		0.0062	−0.0004	0.0050
		(0.0152)	(0.0628)	(0.0160)
All Firm Controls	No	First Diff.	First Diff.	Levels
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1228	1249
Adjusted R^2	0.0365	0.1434	0.1690	0.1430

This table show the results for estimating equation (5) with *change cash ETR3* (columns (1) and (2): column (2) is the preferred specification), *delta cash ETR3* (column (3)), and *cash ETR3* (column (4)) as dependent variables, whereas *change cash ETR3* is constructed as first difference *cash ETR3* minus *pre cash ETR3*, and *delta cash ETR3* is constructed as *cash ETR3* scaled by *pre cash ETR3* minus one. Our focal variable of interest is *hightolow*. In specifications with *change cash ETR3* and *delta cash ETR3* as the dependent variables, first differences of continuous firm controls (if included) are applied. *z* indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 4: Entropy Weights

	Dependent Variable:			
	*change cash ETR*3	*change cash ETR*3	*change cash ETR*3	*change cash ETR*3
hightolow	−0.0294	−0.0273	−0.0300	−0.0206
	(0.0121)	(0.0120)	(0.0123)	(0.0114)
Entropy Weights	Two Moments	One Moment	Treatment Year	Pre cash ETRs
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1249	1205
Adjusted R^2	0.1854	0.1765	0.1849	0.2060

This table depicts results for multiple variations of our preferred specification which employ entropy weights that are constructed over different moments and variables. The first specification uses the continuous *firm controls* over the three-year preceding period to the initiation of an alliance $[t_{-2}; t_0]$ to calculate the entropy weights so that control observations are reweighted to satisfy the balance constraint that the averages and variances (i.e., two moments) match the corresponding moments of the treated units. The entropy weights in the second specification are constructed identically but respectively use the first moment as balance constraint. We use the singleperiod firm controls in the year of the treatment (t_1) to calculate the entropy weights in the third specification. The fourth specification uses the singleperiod observations of *cash ETR* in the preperiod $[t_{-2}; t_0]$ to calculate the weights. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 5: Elapsed Time

	Dependent Variable:		
	change cash ETR	*change cash ETR*	*change cash ETR*
Elapsed Time	from $t_{[-2;0]}$ to t_1	from $t_{[-2;0]}$ to $t_{[1;2]}$	from $t_{[-2;0]}$ to $t_{[1;3]}$
hightolow	−0.0178	−0.0215	−0.0223
	(0.0132)	(0.0118)	(0.0108)
Partner Controls	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm
Observations	1227	1200	1158
Adjusted R^2	0.1128	0.1143	0.1254

This table depicts results for three specifications of equation (5). We extend the posttreatment period (keeping the preperiod $[t_{-2}; t_0]$ constant) by one year with each specification. For the construction of the dependent variables (*change cash ETR*), we use the singleperiod *cash ETRs* at t_1, t_2, and t_3, calculate first differences against *pre cash ETR3*, and then calculate the average of the sum of these calculations over the number of periods since treatment (for instance, the dependent variable for the second specification is calculated as [(*cash* ETR_{t_1} - *pre cash ETR3*) + (*cash* ETR_{t_2} - *pre cash ETR3*)]/2). Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 6: Mechanisms

	Dependent Variable:			
	change cash ETR3	*change cash ETR3*	*change cash ETR3*	*change cash ETR3*
X equals	*CEOTurnover3*	*zHQdistance*	*PartSameInd*	*zDiffPartMktShare*
hightolow	−0.0331	−0.0303	−0.0389	−0.0256
	(0.0147)	(0.0127)	(0.0170)	(0.0126)
X	0.0042	0.0096	−0.0229	0.0037
	(0.0169)	(0.0058)	(0.0140)	(0.0067)
hightolow $\times X$	0.0091	0.0012	0.0197	0.0156
	(0.0238)	(0.0124)	(0.0247)	(0.0088)
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	1249	1167
Adjusted R^2	0.1428	0.1427	0.1432	0.1161
β_1 vs. β_3 [p-value]	[0.0562]	[0.0515]	[0.0454]	[0.0669]

We estimate specifications of equation (5) and include interaction terms of *hightolow* with the cross-sectional variable of interest (X). Our focus is on CEO turnover, geographical proximity, market power, and industry affiliation. *CEOTurnover3* is an indicator variable which equals one when a firm experiences a CEO turnover between t_1 and t_3. *HQDistance* is the handcollected geographical distance (as the crow flies in miles) between the zip codes of the partners' headquarters. *PartSameInd* is an indicator variable that equals one when the partners of an alliance belong to the same industry. *DiffPartMarketShare* is the difference between a high-tax firm's and its alliance partner's *MarketShare*. *MarketShare* is constructed consistent with the market share calculations of the *HHI* (i.e., the percentage of a firm's sales within industry and year). Column (4) neglects firms from the "other" industry. z indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 7: Alternative Channels

	Dependent Variable:	
	change cash ETR3	*change cash ETR3*
Channel	*PartSameAuditor*	*BoardTie3*
hightolow	−0.0209	−0.0303
	(0.0125)	(0.0127)
PartSameAuditor	−0.0077	−0.0185
	(0.0138)	(0.0140)
hightolow × *PartSameAuditor*	−0.0433	
	(0.0375)	
BoardTie3		−0.0126
		(0.0300)
Partner Controls	Yes	Yes
Alliance Controls	Yes	Yes
Firm Controls	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
SE Cluster	Firm	Firm
Observations	1249	1249
Adjusted R^2	0.1443	0.1428

We focus on channels of intentional tax knowledge transfers to analyze whether we spuriously pick up an alternative channel when identifying tax knowledge diffusion via strategic alliances. In column (1), we focus on partners that share an audit firm. The indicator variable *PartSameAuditor* equals one when the partners in an alliance share an audit firm in t_1. In column (2) we consider board ties among the partners in the alliances of our sample. We use data from ISS to construct the indicator variable *BoardTie3* that equals one when the partners have at least one common member among their board of directors (at any point over the $[t_1; t_3]$ period). Note that board ties are rare among the observations in our sample (we observe an overlap of seven board ties among the 284 *hightolow* = 0 observations). Therefore, we do not estimate a specification that includes an interaction but respectively add *BoardTie3* as additional control variable to the analysis. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 8: Robustness Checks

	Dependent Variable:			
	change $CTD3$	*change* $GAAP\ ETR3$	*change* $cash\ ETR3$	*change* $cash\ ETR3$
Specification	Alt. Measure	Alt. Measure	Pre-Volatility	Low-Tax Firms
hightolow	−0.0018	−0.0215	−0.0258	
	(0.0010)	(0.0103)	(0.0119)	
lowtohigh				−0.0058
				(0.0184)
Partner Controls	Yes	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm	Firm
Observations	1249	1249	999	380
Adjusted R^2	0.2112	0.0978	0.1475	0.0771

This table depicts results for specifications of equation (5) with *change CTD3* (column (1)) and *change GAAP ETR3* (column (2)) as dependent variables. *CTD* is constructed by following by Henry and Sansing (2018). *GAAP ETR3* utilizes a GAAP measure of taxes paid (i.e., total income tax expense) instead of cash taxes paid in the numerator of the effective tax rate. The directional interpretation of these measures is identical to our main outcome variable of interest. In column (3), we focus on volatility in the singleperiod *cash ETR* observations in the pretreatment period (i.e., we calculate a volatility measure σ that captures the volatility between the singleperiod cash effective tax rates and *pre cash ETR3* in the pretreatment period) because we want to ensure that we do not interpret volatility in high-tax firms' tax planning measures as eventual treatment effect (see also the fourth specification in Table 4, which uses pretreatment singleperiod *cash ETRs* to calculate the entropy weights). Therefore, we exclude observations that belong to the top-quintile of this volatility measure from the analysis. The fourth specification focuses on low-tax firms. We construct *lowtohigh*, which is an indicator that equals one for low-tax firms in alliances with high-tax firms and zero for low-tax firms in alliances with low-tax firms and estimate its impact on *change cash ETR3*. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.

Table 9: Valuation Allowance Releases

	Dependent Variable:		
	change Val. All. Release3	*change cash ETR3*	*change cash ETR3*
X equals		*z change Val. All. Release3*	*z Part Pre Val. All. Release3*
hightolow	−0.0283	−0.0297	−0.0280
	(0.0353)	(0.0128)	(0.0132)
X		−0.0009	−0.0025
		(0.0093)	(0.0070)
hightolow × *X*		0.0030	−0.0021
		(0.0139)	(0.0100)
Partner Controls	Yes	Yes	Yes
Alliance Controls	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
SE Cluster	Firm	Firm	Firm
Observations	1249	1249	1249
Adjusted R^2	0.1246	0.1430	0.1432

We follow the insights by Drake, Hamilton, and Lusch (2020) and calculate the probability for valuation allowance releases from Compustat data. We then construct first differences between the $[t_1; t_3]$ and $[t_{-2}; t_0]$ periods (*change Val. All. Release3*) and test whether our treatment indication increases the probability for a valuation allowance release (column (1)). In column (2), we include *z change Val. All. Release3* as additional control variable, interact it with *hightolow*, and regress these and the control variables from equation (5) on *change cash ETR3* (note that we exclude *MNE3*, *NOL3*, and *change NOL3* from the firm controls because loss information enter the construction of *change Val. All. Release3*). In column (3), we capture not the own firm's but the partner's probability for a valuation allowance release over the $[t_{-2}; t_0]$ period (*Part Pre Val. All. Release3*) and include and interact this variable with *hightolow*. *z* indicates standardization at mean zero and standard deviation one. Superscripts are not used to indicate statistical significance. Robust standard errors are clustered at the firm level and are depicted in parentheses. All variables are defined in the Appendix.