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# Tax Knowledge Diffusion via Strategic Alliances

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**Abstract**

We utilize the characteristics of tax knowledge, knowledge diffusion processes, and strategic alliances' institutionalization to examine whether strategic alliances serve as channels for tax knowledge diffusion between firms. We empirically identify tax knowledge diffusion via strategic alliances by documenting economically meaningful decreases in cash effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to high-tax firms in strategic alliances with other high-tax firms. We identify dynamic treatment effects and find that elapsed time is an important facilitating mechanism of tax knowledge diffusion. Furthermore, we show that geographical proximity, identical industry affiliation, and shared audit firm serve as substitutes rather than as complements for strategic alliances to low-tax firms. Finally, we triangulate our findings to effects on the textual sentiment of 10-K filings and the disclosure of tax haven operations. Overall, our results provide robust evidence for tax knowledge diffusion via strategic alliances.



**Keywords**: Corporate Tax Planning/Avoidance, Knowledge Diffusion, Network, Strategic Alliance

**JEL Qualification**: C31, G34, H26


**Data Availability**: Public and/or subscription-based sources identified in the paper

**Online Supplement**: https://github.com/taxknowledge/diffusionviastrategicalliances

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## 1. Introduction

Do strategic alliances serve as channels for tax knowledge diffusion? In the form of contractual-based cooperation between firms, strategic alliances are expected to foster their main business purposes and to facilitate the respective diffusion of knowledge between the partners. For instance, Li et al. [2019] identify significant increases in firms' innovative capacity when investing in R&D strategic alliances. Clearly, the main business purpose of a strategic alliance is different from tax knowledge. Rather, firms pool their resources to achieve strategic objectives. Our analyses, however, document substantial decreases in the cash effective tax rates of high-tax firms in strategic alliances with low-tax firms relative to high-tax firms in strategic alliances with high-tax firms. Based on the applied research design and the theoretical framework, we conclude to identify tax knowledge diffusion via strategic alliances. This carries two important implications. Firstly, our results indicate that firms not only create and explore anticipated synergies but also gain knowledge that is unrelated to the main business purpose of a strategic alliance. Thus, a firm's management should, when trading off the inherent tension between value creation and value protection (Palomeras and Wehrheim [2021], Sampson [2007]), incorporate tax knowledge diffusion into their management frameworks for strategic alliances. Secondly, investments in strategic alliances should not mechanically impact a firm's available options and incentives for tax planning. The identification of tax knowledge diffusion via strategic alliances thus builds on theory which highlights the behavioral aspects of tax planning processes. Our study documents the outcome of this process and shows that a strategic alliance establishes a cross-firm connection through which firms benefit from their partners' tax knowledge and change their tax planning behavior.

Specifically, we provide and test a theoretical framework of tax knowledge diffusion via strategic alliances that does not depend on the assumption of an intentional transfer of tax knowledge via an intermediary (e.g., see client-bank-client relationships in Gallemore et al. [2019]). Within this framework, tax knowledge diffusion comprises gaining access to and being willing and capable of employing relevant tax knowledge (see also Rogers [2003]). Therefore, we consider tax knowledge diffusion as an inadvertent outcome.

Consequently, the characteristics of tax knowledge impact its diffusion because the more tax knowledge qualifies as explicit, the more easily diffusible it should be (Meier [2011], Bresman et al. [2010]). While explicit knowledge can easily be codified and is systematically diffusible, tacit knowledge is difficult to formulate and communicate because it "is deeply rooted in action, commitment, and involvement in a specific context" (Nonaka [1994]). The substantial complexity of corporate taxes (Hoppe et al. [2021]), the increased uncertainty (Dyreng et al. [2019], Guenther et al. [2017]) and the costs (Hundsdoerfer and Jacob [2019]) of tax planning emphasize the tacit elements of tax knowledge. Tax knowledge may also comprise know-how that is more tacit than pure information (Kale et al. [2000]). However, observing that tax planning strategies often serve as mass-market tax-saving ideas and are not limited to a particular industry (e.g., see Lisowsky [2010]) highlights the explicit elements of tax knowledge. The inferences on corporate-owned life insurance shelters (Brown [2011]) or lease-in, lease-out transactions (Wilson [2009]) support this notion. We contend that tax knowledge is characterized by both explicit and tacit elements that facilitate and impede tax knowledge diffusion.

The institutional characteristics of strategic alliances enlarge this tension. Strategic alliances, which we also refer to as "networks", are not only a relevant (PwC [2018]) but also a unique choice of institutionalization (Lindsey [2008], Chan et al. [1997]). Mutual commitment exceeds that of simple market transactions but falls short of complete integration (Boone and Ivanov [2012], Yin and Shanley [2008]). In particular, strategic alliances are not subject to corporate income taxation because they do not establish a separate legal entity. Consequently, the diffusion of tax knowledge is particularly valuable outside the scope of a network. Earning such private benefits, however, harms the strive for the network's common benefits (Khanna et al. [1998]) and could further affect the bargaining power between the partners. The instability of networks, however, can be associated with shifts in partners' bargaining power (Inkpen and Beamish [1997], Khanna et al. [1998]). Additionally, uncertainty is inherent to both cooperation (Chen et al. [2015]) and tax planning (Dyreng et al. [2019]). This requires management to evaluate the marginal disutility of uncertainty. These examples reiterate how important behavioral aspects are when analyzing tax knowledge diffusion. In this regard, a benefit of tax knowledge diffusion could be to better be able to assess the costs and benefits of certain tax planning strategies when observing strategies effectively implemented by partners. If this caused a firm's management willingness to also implement the respective tax planning activity, tax knowledge diffusion could overcome the fear of reputational costs from engaging in tax planning (Gallemore et al. [2014], Hanlon and Slemrod [2009], Graham et al. [2014], Austin and Wilson [2017]).

The absence of corporate income taxation at the level of a strategic alliance is useful for our analysis since we are interested in whether strategic alliances serve as channels for tax knowledge diffusion between firms and not in whether investments lead to mechanical tax effects. This translates into our identification strategy: we empirically measure tax knowledge by observing the outcome of a firm's nonconforming tax planning behavior. We exploit information on strategic alliances that were established between publicly traded US firms from 1994 to 2016. Given that accounting data are available for a network's partners, we reshape the data from the alliance to the partners' levels (network-firm observations). We classify the partners in a network as low-tax and high-tax firms depending on their industry-year-mean adjusted multiperiod cash effective tax rates in the run-up to the initiation of a network. To tease out tax knowledge diffusion, we 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.

Our main analysis robustly documents a substantial decrease in cash effective tax rates of high-tax firms in strategic alliances with low-tax firms ($hightolow = 1$) relative to high-tax firms in strategic alliances with high-tax firms ($hightolow = 0$). Our results are also economically meaningful because our analyses suggest reasonable cash effective tax rate levels for high-tax firms in low-tax networks of 25.57%. This translates into a difference in differences in our tax knowledge proxy of 4.3 percentage points in univariate tests. We corroborate this finding by multivariable regression analyses in which we control, based on textual analysis, for the networks' business purposes and partner characteristics. Furthermore, we apply a difference in differences regression design with the employment of entropy balancing weights under the exclusion of overlapping events. Our results are consistent throughout these tests and additional robustness checks.

Next, we turn to the potentially facilitating mechanisms of tax knowledge diffusion. Our theoretical framework suggests that knowledge diffusion is a gradual, multistage process. We contend that elapsed time increases the likelihood of tax knowledge diffusion and find consistent evidence. Our results suggest that high-tax firms on average are able to significantly decrease their cash effective tax rates two years after a network's initiation. We also investigate whether characteristics of the partnering firms' environments, i.e., geographical proximity and shared industry affiliation, intensify or mitigate the identified effects. Our findings indicate that these characteristics serve as a substitute rather than as a complement for strategic alliances to low-tax firms. We find corresponding results when testing for an alternative channel by interacting $hightolow$ with $SameAuditor$, an indicator variable for partnering firms with a shared audit firm.

Finally, we investigate whether tax knowledge diffusion can be associated with tax related changes in the disclosure behavior of firms. In particular, we investigate the textual sentiment of 10-K filings and exhibit 21 disclosures of subsidiaries in tax havens. We find a negative response of textual sentiment of 10-K filings to low-tax networks in comparison to high-tax networks for high-tax firms. This finding is consistent with prior research indicating increased tax planning when observing decreasing textual sentiment of 10-K filings (Law and Mills [2015]). Our findings, however, do not suggest that the identified decreases in textual sentiment are accompanied by increases in firms' financial constraints. Furthermore, we show that the identified effects from our analyses seem on average not to stem from increases in the reported operations in international tax havens or the State of Delaware. This indicates that firms use existing, yet not fully exploited, structures for tax planning.

Our study builds on research that examines whether firms benefit from the knowledge that they gain in strategic alliances (Boone and Ivanov [2012], Mohanram and Nanda [1996], Chen et al. [2015], Anand and Khanna [2000], Baxamusa et al. [2018], Chan et al. [1997], Porrini [2004], Cai and Sevilir [2012], Higgins and Rodriguez [2006], Ishii and Xuan [2014], Gomes-Casseres et al. [2006], Li et al. [2019]). The findings frequently highlight the knowledge related benefits of investments in strategic alliances but focus on knowledge in the context of the networks' main business purposes. Consistently, existing research on knowledge protection in strategic alliances suggests that firms especially attempt to safeguard from knowledge leakage with respect to the main business purpose of the network (Palomeras and Wehrheim [2021], Li et al. [2008]). Notably, protection against tax knowledge diffusion is aggravated because tax knowledge comes with little to no legal protection, as there is, for instance, for intellectual property (for the general implications of weak knowledge protection, see Zhao [2006]). Furthermore, the characteristics of tax knowledge and strategic alliances' institutional characteristics induce ambiguity when analyzing the diffusion of tax knowledge because not all corporate practices diffuse in the same way (Cai et al. 2014). To the best of our knowledge, we are the first to measure knowledge diffusion via a contractual cooperative organizational form (i.e., strategic alliances) based on a firm's tax planning behavior.

We also contribute to the emerging accounting literature that identifies cross-firm connections to determine increases in the tax planning behavior of firms. With the focus on close relationships via intermediaries and intentional transfers of tax knowledge, board ties (Brown [2011], Brown and Drake [2014]), banks (Gallemore et al. [2019]), human capital turnover (Barrios and Gallemore [2021]), and auditors (Frey [2018], Lim et al. [2018]) are analyzed. In contrast, strategic alliances are established on a peer-to-peer basis without an intermediary. In this regard, recent work on peer-to-peer relationships by Cen et al. [2017], [2020] is related to our study. The authors document that tax planning spreads from principal customers to their dependent suppliers. Their findings suggest that customers and dependent suppliers intentionally share tax planning benefits through lower product prices. Thus, the identification of intentional tax knowledge transfers aligns their work with research on intermediaries. Our study generally builds on a different theoretical framework because our focus is on the inadvertent diffusion of tax knowledge. For instance, many facets of tacit tax knowledge characteristics (e.g. reevaluating tax planning strategies) are inherent to the diffusion process. Investigating the substantially diverse relationships in strategic alliances further distinguishes our study from Cen et al. [2017], [2020]. In particular, strategic alliances' business purposes cover both horizontal and vertical value chain localizations. We gain insights into these business purposes by applying textual analysis of the networks' descriptions and we control for the respective $network\ controls$ throughout our analyses. Finally, we contend that elapsed time after a network's initiation should be a relevant facilitator of knowledge diffusion (Bresman et al. [2010]), and our dataset allows for a precise identification of a network's initiation.

## 2. Theoretical Framework & Prior Literature

The extensive research that considers within-firm determinants of tax planning by firms underlines the perceived importance of corporate taxes in economic theory, politics and society (for comprehensive reviews, see Hanlon and Heitzman [2010] and Wilde and Wilson [2018]). Given the substantial economic impact of tax planning activities (Clausing [2016], Tørsløv et al. [2018], Blouin and Robinson [2020]), tax advisors are intuitively linked to observations of tax planning. However, recent analyses of "tax planning ecosystem[s]" (Dyreng and Maydew [2017]) suggest that cross-firm connections impact tax planning. We review the literature on these channels to embed strategic alliances into the theoretical framework of tax knowledge diffusion in the following.

### 2.1 CROSS-FIRM CONNECTIONS & TAX KNOWLEDGE

#### *2.1.1 Prior Research on Intentional Transfers of Tax Knowledge*

Brown [2011] examines the spread of a specific tax planning tool, the corporate-owned life insurance shelter, and finds that board interlocks increase the probability that a firm adopts the tax shelter from a prior user. Consistently, Brown and Drake [2014] suggest that firms with greater board ties to low-tax firms increase tax planning themselves. Further disentangling the role of intermediaries, Barrios and Gallemore [2021] document that firms exhibit increasing tax planning when they hire tax staff from sophisticated tax planners. Gallemore et al. [2019] show that firms experience meaningful tax reductions when they start a relationship with a bank whose existing clients engage in tax planning. Additionally, studies focus on whether auditors impact firms' tax planning behavior (Aobdia [2015], Cai et al. [2016], Dhaliwal et al.

[2016], McGuire et al. [2012], Klassen et al. [2016]) but provide mixed inferences. Calibrations from the audit firm level to the individual audit engagement partner (Lim et al. [2018], Frey [2018], Bianchi et al. [2018]) leave this question open. In recent work, Nesbitt et al. [2020] suggest that there are limits to the relation between auditor-provided tax services and clients' tax aggressiveness.

These studies are conceptually aligned by the presence of intermediaries who implement tax planning expertise in their set of contracts and intentionally transfer the tax knowledge to other parties with whom they are contracting. Consistently, intermediaries are found to play a key role in the acquisition and dissemination of information in many research fields (e.g., see Di Maggio et al. [2019]). In contrast, strategic alliances are based peer-to-peer without an intermediary. Cen et al. [2017], [2020] perform peer-to-peer analyses and investigate transfers of tax knowledge along supply chains. They document that both customers and suppliers increase their tax planning activities once their relationship is considered dependent. The evidence suggests that customers and suppliers share tax planning benefits through lower product prices. Although evidence for the intended sharing of tax benefits is scarce (for instance, see Erickson [1998] and Erickson and Wang [1999]) and supply chains could incentivize firms to pass along inaccurate information (Bushee et al. [2020]), the identification of an intentional transfer of tax knowledge aligns the supply chain channel with research on intermediaries.

#### *2.1.2 The Diffusion of (Tax) Knowledge*

Knowledge diffusion requires communication through channels over time among members of a social system (Rogers [2003]). This definition suggests that, in addition to knowledge access, a firm must also deploy an approach to utilize the knowledge. Otherwise, knowledge diffusion cannot contribute to a firm's knowledge profile (Kale et al. [2009], Mazloomi Khamseh et al. [2017]). From this, we deduce that tax knowledge diffusion conceptually comprises gaining access to and being willing and capable of employing relevant tax knowledge.

Within this theoretical 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 et al. [2009] argue that firms should create a dedicated management structure to oversee and support their alliance activities. For instance, observing effectively implemented tax planning strategies by partners may not only reveal unknown tax knowledge but also increase a firm's management willingness to implement the respective tax planning activity. Moreover, it may provide a better assessment of the costs and benefits of certain tax planning strategies. 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." Furthermore, tax knowledge diffusion could overcome the fear of costs from engaging in tax planning (Gallemore et al. [2014], Hanlon and Slemrod [2009], Graham et al. [2014], Austin and Wilson [2017]). Finally, existing research on knowledge protection in strategic alliances suggests that firms especially attempt to safeguard from knowledge leakage with respect to the main business purpose of the network (Palomeras and Wehrheim [2021], Li et al. [2008]). Notably, protection against tax knowledge diffusion is aggravated because tax knowledge comes with little to no legal protection, as there is, for instance, for intellectual property (for the general implications of weak knowledge protection, see Zhao [2006]).

However, not all corporate practices diffuse in the same way (Cai et al. [2014]). Major barriers are knowledge-related factors, such as limits to a recipient's absorptive capacity (Szulanski [1996], Dyer and Hatch [2006]). Additionally, constraints on knowledge diffusion increase returns to having a sophisticated knowledge profile (Akcigit and Ates [2019]). Furthermore, tax knowledge diffusion is especially valuable outside the scope of a network. Earning private benefits, however, raise tension between partners by impacting firms' strive for common benefits (Khanna et al. [1998]). Additionally, tax knowledge diffusion could lead to shifts in bargaining power between the partners because a partner with sophisticated (non-sophisticated) tax knowledge potentially gains bargaining power in the run-up to a (established) strategic alliance when identifying possible (earning the private benefit of) tax knowledge diffusion. However, the instability of networks can be associated with shifts in the partners' bargaining power (Inkpen and Beamish [1997], Khanna et al. [1998]). Furthermore, Desai et al. [2004] suggest that shared ownership of equity joint ventures could impact the fine-tuning of tax planning of these entities. Corporate culture and governance further impact a firm's behavior with regard to implementing tax planning strategies (Klassen et al. [2017], Armstrong et al. [2015]). Additionally, both cooperation (Chen et al. [2015]) and tax planning (Dyreng et al. [2019]) are found to induce uncertainty. Even prudent managers could expect the marginal disutility of uncertainty to exceed the benefits of received tax knowledge. Consequently, it remains an empirical question whether strategic alliances actually serve as channels for tax knowledge diffusion.

## 2.2 FACILITATORS

If one observed that strategic alliances served as channels for tax knowledge diffusion, documenting the facilitating mechanisms would allow for a better understanding of the underlying process. Prior research discusses a multitude of facilitators, of which "elapsed time" is generally highlighted as most important. This assessment theoretically builds upon knowledge diffusion being a gradual, multistage process (Inkpen [2000], Szulanski [1996], Bresman et al. [2010]). Consequently, elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv [2003], Isaksson et al. [2016]).

**[Figure 1]**

The black box model in Figure 1 aligns our conceptual framework with potentially relevant facilitators. Network characteristics themselves might facilitate or impede knowledge diffusion. Kepler [2019] and Palomeras and Wehrheim [2021] suggest that the ability to share more and a broader range of information increases with the number of partners in an alliance and its breadth of scope (see also Acemoglu et al. [2011] and Blonski [1999]). However, the mere quantity of networks does not translate to relational capabilities needed for building and managing alliances (Kale et al. [2009]). Facilitators could also emerge from firms' organizational structures and their environments (Fiol and Lyles [1985], Yin and Shanley [2008]). For instance, interfirm interactions could be reinforced with decreasing geographical distance between firms, as hypothesized by Brown [2011]. Potential effects of shared industry affiliation could be moderated by competition (i.e., industry concentration). Generally, industry concentration may promote collusive behavior (Bourveau et al. [2020]). However, Cai and Szeidl [2018] find that competition reduces diffusion rates of business-relevant information. Furthermore, competition could diminish the effects of technological overlap (Bena and Li [2014], Palomeras and Wehrheim [2021]). Initially, "technological relatedness" could also speak as to with whom firms form an strategic alliance (Diestre and Rajagopalan [2012]). Research on knowledge diffusion also suggests an impact for a multitude of soft factors. Prominent examples are communication (Bresman et al. [2010], Bushee et al. [2020]), partner trustworthiness (Jiang et al. [2016]), commitment (Bushee et al. [2020]), managerial flexibility (Chen et al. [2015], Chan et al. [1997]), partnering mindset (Kale et al. [2009]), and learning intent (Hamel [1991], Mazloomi Khamseh et al. [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).

## 3. Data & Methodology

**[Figure 2]**

### 3.1 SAMPLE SELECTION

We exploit data on strategic alliances from Refinitiv's SDC Platinum (SDC) database on strategic alliances over the 1994-2016 period. SDC is widely used in relevant research on corporate cooperation (Anand and Khanna [2000], Boone and Ivanov [2012], Cai and Sevilir [2012], Chen et al. [2015], Ishii and Xuan [2014], Li et al. [2019]) and tracks a very wide range of agreement types (Schilling [2009]). SDC issues data at the strategic alliance level. Initially, we exclude equity joint ventures from the data. This deflates our sample to observations which are flagged as strategic alliances by SDC. We then reshape data from the alliance to the partner level (network-firm observation) because strategic alliances are not subject to corporate taxation but the (publicly traded) contracting partners are. A strategic alliance between two partners, for instance, translates to one network-firm observation for each of the two partners. Compustat provides firm-year-level accounting information, and we merge SDC and Compustat data by using a firm's six-digit CUSIP number (at the level of the ultimate parent of the participant) as an identifier. Although SDC provides reliable network observations from the beginning of 1990 onwards, we start in 1994 consistent with many tax studies. We end our sample in 2016 to exclude any influences from the 2017 US tax reform. Furthermore, we consider strategic alliances between publicly traded firms incorporated and headquartered in the US and in which all contracting parties are identified in Compustat data (Figure 2).

## 3.2 IDENTIFICATION STRATEGY

### *3.2.1 Measuring Tax Knowledge*

We proxy for tax knowledge by measuring a firm's nonconforming tax planning. The lingua franca in determining the degree to which a firm succeeds in this attempt is the effective tax rate, which puts tax expenses and pre-tax book income into perspective. We base our inferences on the cash effective tax rate ($cash\,ETR$) because $cash\,ETR$ also captures tax deferral strategies (Hanlon and Heitzman [2010], Edwards et al. [2016]). Furthermore, we apply a multiperiod (3-year) form of $cash\ ETR$ (Brown and Drake [2014], Barrios and Gallemore [2021], Gallemore et al. [2019]) because we expect the likelihood of tax knowledge diffusion to increase with elapsed time:

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

The terms $txpd$, $pi$ and $spi$ correspond to their Compustat data item equivalents of cash taxes paid, pre-tax income and special items. Missing $spi$ 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. By the nature of this approach, $cash\,ETR3$ would always be missing for the final and penultimate firm-year of a firm in the panel. For these firm-years, we substitute $cash\,ETR3$ with $cash\,ETR$.[1] Applying a forward-looking multiperiod

[1] Example: given our sample period, $cash\ ETR3$ would always be missing for fiscal-year 2016. In this case, we construct the numerator and denominator over one year, respectively. In robustness checks we exclude firm-edge-

$cash\ ETR3$ has the advantage that potential tax knowledge diffusion via strategic alliances can be directly linked to the year of network initiation.

### 3.2.2 Focal Independent Variable: $hightolow$

For tax knowledge diffusion to occur, at least one of the network's partners must possess sophisticated tax knowledge (i.e., low-tax firm). Applying a forward-looking $cash\ ETR3$ for our analysis has the advantage of aligning an influence on the $cash\ ETR3$ directly with the year of network initiation. However, identifying low-tax firms based on a forward-looking measure would entail the disadvantage of concluding the type of input based on the output. For the identification of firms with sophisticated tax knowledge, we therefore consider $pre\ cash\ ETR3$, which is constructed over a three-year preceding period:

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

For every $t = 1$ in which a new network is initiated, we consider the partners' initial $pre\ cash\ ETR3$, which spans from $t = -2$ to $t = 0$.[2] We must observe $cash\ ETR3$ and $pre\ cash\ ETR3$ of all partners for a network to be considered in our analysis. Figure 2 provides additional information regarding how we identify low-tax and high-tax observations. We classify firms based on their industry-year-mean adjusted $pre\ cash\ ETR3\ [t_{-2}; t_0]$ and allocate firm-year observations into four bins according to the quartiles of the distribution. Industry adjustment (Brown and Drake [2014]) and a multiperiod measure (Dyreng et al. [2008], Dyreng et al. [2017]) help us to validate the identification of sophisticated tax planners. In $t = 1$ (network initiation), a partner is treated as a low-tax firm in a network when its adjusted $pre\ cash\ ETR3\ [t_{-2}; t_0]$ is in the first bin (i.e., lowest quartile). Conversely, firms that do not qualify as

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years from our sample to ensure that variation in $cash\ ETR3$ does not stem from this substitution (Table OS 7 Panel A).

[2] We also refer to $cash\ ETR3$ and $pre\ cash\ ETR3$ as $cash\ ETR3\ [t_1; t_3]$ and $pre\ cash\ ETR\ [t_{-2}; t_0]$ to highlight the respective timing around a network initiation.

low-tax firms are classified as high-tax firms.[3] Consequently, any strategic alliance in our sample 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 and discriminate 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 ($hightolow = 0$):

$$hightolow_{i,t} = \begin{cases} 1, & \boldsymbol{high}\text{-}tax\ firm\ in\ network\ \boldsymbol{to}\ \boldsymbol{low}\text{-}tax\ firm(s) \\ 0, & \boldsymbol{high}\text{-}tax\ firm\ in\ network\ \boldsymbol{to}\ \boldsymbol{high}\text{-}tax\ firm(s) \end{cases} \quad (3)$$

The high-tax firms are in very similar situations except for potentially experiencing tax knowledge diffusion. Consequently, mechanical effects, if any, from investments in strategic alliances on our tax planning measure would affect both groups of $hightolow$ similarly. Therefore, high-tax firms establish the treatment group and the control group for our analyses. The numbers of observations per group equal one observation per high-tax firm in a strategic alliance (network-firm observation) because we collapse the data based on the multiperiod-measured variables for the main analyses. Our identification strategy leads us to 201 observations of $hightolow = 1$ and 627 observations of $hightolow = 0$.

The identification of our focal independent variable is completely agnostic about the root of a strategic alliance and respectively considers partners' tax knowledge.[4] However, if high-tax firms anticipated beneficial diffusion of tax knowledge and unexceptionally selected low-tax firms as partners because of their sophisticated tax knowledge, endogenous treatment assignment would affect the inferences from OLS estimators (Lennox et al. [2012]). Clearly, any intention to gain tax knowledge via an investment in business cooperation must be a byproduct of other incentives. When investing in strategic alliances, firms pool their resources to achieve strategic objectives (e.g., R&D undertaking) (Meier et al. [2016]). Thus, it would be

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[3] In a robustness check, we show that our results are not sensitive to the classification of firms based on quartile-bins. We apply a modified identification strategy and enter high-tax and low-tax firms in a specification of equation (4) and regress $cash\ ETR3$ on the firms' own $pre\ cash\ ETR3$, the industry-year-mean adjusted $partner\ pre\ cash\ ETR3$, and an interaction term of the two independent variables (Table OS 4).
[4] This approach may be applied in various fields of research. For instance, Tan and Netessine [2019] use the catching title "When You Work with a Superman, Will You Also Fly?".

an economic pitfall if firms weighted the potential diffusion of tax knowledge over selection of a partner that best suits the network's main business purpose. Rather, firms could simply acquire tax planning strategies from tax advisors if this was the prioritized concern of a firm's management. Additionally, ex ante to the investment, a firm cannot publicly observe details of the partner's tax planning strategy even if it was aware of the tax planning outcome. This strongly increases the risk of a bet benefiting from tax knowledge diffusion. In contrast, our theoretical framework defines the process of tax knowledge diffusion as gaining access to and being willing and capable of employing relevant tax knowledge. In this regard, Baxamusa et al. [2018] emphasize that there is considerably less due diligence performed when investing in strategic alliances than when investing in M&As. Additionally, Owen and Yawson [2013] suggest that statutory tax rates are not negatively associated with investment decisions on strategic alliances in multicountry investigations. Finally, firms could be incentivized to become rather opaque about a network if its aim was to facilitate tax planning. These cases, however, are likely excluded from our sample since we require the identification of all partners in a network. These facts support our notion that strategic alliances are not intended to establish tax planning investments.

### *3.2.3 Information on Networks and Firms*

**[Figure 3]**

Figure 3 maps networks and the investing firms from our sample.[5] Each vertex (square) displays a firm in its classification as low-tax (black vertices) or high-tax (gray vertices). A link between two gray vertices translates into an observation of $hightolow = 0$ for both high-tax firms. Correspondingly, a link between a gray vertex and a black vertex translates into an observation of $hightolow = 1$ for the high-tax firm. It can be observed that there are firms with only one network observation in our sample (n = 324). In contrast, there are also firms with multiple investments in strategic alliances during our sample period (n = 178).

[5] Networks of two partners may be displayed, respectively. However, the vast majority of networks in our sample combine two firms.

Interestingly, these prominent partners largely consist of high-tax firms. This indicates that firms do not strategically choose low-tax firms as partners in expectation of tax knowledge diffusion.

**[Table 1]**

Table 1, Panel A contains descriptive statistics with regard to firm-level accounting information ($firm\ controls$). Generally, we follow Dyreng et al. [2010] and measure $EBITDA3$, $RnDExp3$, $AdExp3$, $SGA3$, $CapEx3$, $GrowthSale3$, $Leverage3$, $Cash3$, $MNE3$, $NOL3$, $Intangibles3$, $PPE3$, and $Size3$ as $firm\ controls$. Consistent with $cash\ ETR3$, $firm\ controls$ are constructed over rolling three-year periods $[t_1; t_3]$. Conditioning on $hightolow$, we do not observe economically significant differences in the $firm\ controls$ between the treatment group and the control group. This lends additional credibility to our control group choice.

Tax knowledge diffusion could be impacted by partners' organizational structures and their operational environments. Therefore, we collect information on $partner\ controls$. From Compustat data, we can infer whether network partners share the audit firm and/or industry affiliation in the year of network initiation ($SameAuditor$ & $SameInd$) and whether their headquarters are located in the same region as defined by the Bureau of Economic Analysis ($SameBEARegion$).[6] To increase the accuracy of our measures, we manually collect the geographical distance (as the crow flies) between the zip codes of the network partners' headquarters ($Proximity$) to control for the potential impact of geographical proximity in tax knowledge diffusion (Table 1, Panel B). We multiply the collected distances by minus one so that $Proximity$ can be interpreted in accordance with the sign of the indicator variable $SameBEARegion$ (as, for instance, in Brown [2011]).

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

**[Figure 4]**

Network characteristics might facilitate or impede tax knowledge diffusion, as business activities in a strategic alliance are generally not limited. SDC provides information on a network's activities with a deal description of every strategic alliance. We apply textual analysis on these deal descriptions to derive the main business purposes of the strategic alliances in our sample ($network\ controls$). The word cloud depicted in Figure 4 shows the 40 most common words used in the deal descriptions of our sample. We base regular expressions on selected features of the respective word frequencies (untabulated) and use pattern matching to identify $PurposeWholesale$, $PurposeRnD$, $PurposeLicensing$, $PurposeService$, $PurposeMarketing$, $PurposeSupply$, $PurposeTech$, and $PurposeManufacturing$ as major network activities. Panel B of Table 1 shows the distribution of the respective indicator variables among the $hightolow$ and $lowtohigh$ observations. Furthermore, Panel C of Table 1 presents information on the industry affiliation of networks and firms. Industry affiliation is determined on clusters of two-digit SIC-codes. The majority of networks and investing firms operate in business services and manufacturing.

## 3.3 REGRESSION DESIGN

### *3.3.1 Baseline Model*

Our main variable of interest $hightolow$ is constructed as an indicator variable to distinguish between high-tax firms that enter into high-tax networks ($hightolow = 0$) and high-tax firms that engage in low-tax networks ($hightolow = 1$). This approach ensures that we control for firms' seek to invest in strategic alliances. Consequently, $hightolow$ isolates the incremental effect a low-tax network exerts on the high-tax firm's tax knowledge. We estimate the following linear regression model by OLS:

$$\begin{aligned} cash\ ETR3_{i,t=1} = {} & \beta_0 + \boldsymbol{\beta_1 hightolow_{i,t=1}} + \sum\nolimits_n \beta_n\, partner\ controls_{i,t=1}^n \\ & + \sum\nolimits_l \beta_l\, network\ controls_{i,t=1}^l + \sum\nolimits_k \beta_k\, firm\ controls_{i,t=1}^k \\ & + \delta_{ind} + \tau_t + \varepsilon_{i,t} \end{aligned} \tag{4}$$

By using indicator notation ($hightolow$), the coefficient describes the effect of moving from one to another condition. We measure tax knowledge by $cash\ ETR3$, the two change-indicating variables $delta\ cash\ ETR3$, which is $cash\ ETR3\ [t_1; t_3]$ scaled by $pre\ cash\ ETR3\ [t_{-2}; t_0]$ minus one, and $change\ cash\ ETR3$, which is the first difference estimator $cash\ ETR3\ [t_1; t_3] - pre\ cash\ ETR3\ [t_{-2}; t_0]$. A negative coefficient for $hightolow$ would suggest that differences in the outcome level and in the change of high-tax firms' tax knowledge would be driven by the partners' tax knowledge. Our theoretical framework suggests that this would identify tax knowledge diffusion via strategic alliances. Because strategic alliances go beyond linking high-tax and low-tax firms, we include vectors of variables on $partner\ controls$ ($SameAuditor, Proximity$) and $network\ controls$ ($PurposeWholesale$, $PurposeRnD$, $PurposeLicensing$, $PurposeService$, $PurposeMarketing$, $PurposeSupply$, $PurposeTech$, $PurposeManufacturing$) in equation (4). Furthermore, we control for within-firm determinants of tax planning by including a vector of $firm\ controls$. We generally follow Dyreng et al. [2010] and consider $EBITDA3$, $RnDExp3$, $AdExp3$, $SGA3$, $CapEx3$, $GrowthSale3$, $Leverage3$, $Cash3$, $MNE3$, $NOL3$, $Intangibles3$, $PPE3$, and $Size3$. We include year ($\tau_t$) and industry ($\delta_{ind}$) fixed effects and cluster robust standard errors at the firm level (Petersen [2009]).

### *3.3.2 Difference in Differences (DiD)*

The multiperiod variables in the specifications of equation (4) allow us to tie tax knowledge diffusion to the year of network initiation. An alternative approach for measuring tax knowledge diffusion is to maintain the panel structure of our data and apply a DiD design. In this model, $treatment$ is generally in alignment with $hightolow$. Furthermore, we create an embargo period around a network observation during which a firm may not invest in another network (exclusion of overlapping events). Given a suggested average lifespan for strategic alliances of five years (Chan et al. [1997]), this embargo period contains the three years preceding ($post = 0$) and five years subsequent ($post = 1$) to network initiation.[7] We adjust $cash\,ETR$ and $firm\ controls$ from multiperiod measures to their single-year versions and continue to measure $partner\ controls$ ( $SameAuditor$ & $Proximity$) and $network\ controls$ at $t = 1$:

$$\begin{aligned} cash\,ETR_{i,t} = {} & \beta_0 + \beta_1 treated_i + \boldsymbol{\beta_2 treated_i * post_t} \\ & + \sum\nolimits_n \beta_n\, partner\ controls_{i,t=1}^{n} + \sum\nolimits_l \beta_l\, network\ controls_{i,t=1}^{l} \\ & + \sum\nolimits_k \beta_k\, firm\ controls_{i,t}^{k} + \gamma_{emb} + \tau_t + \delta_{ind} + \varepsilon_{i,t} \end{aligned} \tag{5}$$

We include embargo period ($\gamma_{emb}$) fixed effects that subsume the $post$ indicator (Gallemore et al. [2019]). Additionally, year ($\tau_t$) fixed effects capture influences that affect tax planning behavior across all sample firms within a given year. We also include industry ($\delta_{ind}$) fixed effects. In this model, $treated$ measures the baseline difference in $cash\ ETR$ that is not due to the presence of the treatment. The parameter of interest is the interaction $treated * post$, which measures the effect on $cash\,ETR$ due to the treatment (i.e., low-tax network of high-tax firm).

Generally, the high-tax firms are in very similar situations except for potentially experiencing tax knowledge diffusion. However, if the characteristics of high-tax firms investing into low-tax networks differed from the characteristics of high-tax firms investing in high-tax networks, a concern about equation (5) would be that these differences drive the observed differences in $cash\,ETR$. In addition to excluding overlapping events, we also employ entropy-balancing weighting (Hainmueller [2012], Hainmueller and Xu [2013]) and use the entropy weights to re-estimate equation (5). Observations are balanced using continuous $firm\ controls$ so that the variables' means and variances in the reweighted control group match the treatment group (balanced sample).

[7] We use an expected lifespan of five years because SDC does not provide sufficient information on the termination of networks.

**[Figure 5]**

DiD specifications rely on the parallel trend assumption because one needs to empirically proxy for the posttreatment outcome absent the treatment. Accordingly, Panel A of Figure 5 provides visual documentation that the trends of $cash\ ETR$ run both parallel and at comparable levels for treatment and control firms prior to the treatment. Furthermore, Patel and Seegert [2015] introduce an empirical approach to alleviate concerns about parallel trends. They suggest regressing the dependent variable on the treatment indicator, time fixed effects (i.e., embargo period ($\gamma_{emb}$) fixed effects which we base in the treatment year $t_1$) and the interaction of the treatment indicator and fixed effects. In Panel B of Figure 5, we therefore present the coefficient and the 95% confidence interval of the interaction of the treatment indicator and embargo period fixed effects for the pretreatment years. Additionally, we fail to reject that the coefficient estimates for the interaction terms for $\gamma_{emb} * treated$ are jointly zero in the pretreatment period (p-value 0.1741). These insights provide support for the parallel trend assumption.[8]

## 4. Results & Discussion

### 4.1 DESCRIPTIVE ANALYSIS

**[Table 2]**

We start our analyses by performing a descriptive analysis of the 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. First, our focus is on $cash\ ETR3\ [t_1; t_3]$. We compare the before after changes from $pre\ cash\ ETR3\ [t_{-2}; t_0]$ to $cash\ ETR3\ [t_1; t_3]$ within the $hightolow$ groups. We observe reductions in cash effective tax rates for both groups (Before After Change in Panel A of Table 2). While these decreases could comprise reversion to the mean, they would not explain differences in the development between the

[8] In a robustness check, we implement an interaction weighted two way fixed effects estimator for $hightolow$ under consideration of the recent advances by Sun and Abraham [2020] (Table OS 3). The result of this test generally supports our parallel trend assumption.

groups. Therefore, we test for the difference between groups and between periods (Difference in Differences). The respective difference of 4.3 percentage points is highly significant (p-value 0.0143).[9] Observations of $hightolow = 1$ are accompanied by a mean $cash\,ETR3\,[t_1; t_3]$ of 25.57%, and networks solely among high-tax firms ($hightolow = 0$) are aligned to an average $cash\,ETR3\,[t_1; t_3]$ of 28.04%. We interpret these findings as a first indication of the existence of tax knowledge diffusion via strategic alliances. A potential concern, however, could be that the identification of differences in averages is induced by some increases in $cash\,ETR3$ for high-tax firms in high-tax networks. Therefore, we test not only for the difference of the mean of $delta\,cash\,ETR3$ (p-value 0.0422, Table 2 Panel B) but also for the difference of the median of $delta\,cash\,ETR3$ ($change\ cash\,ETR3$). The results indicate negative and significant differences (p-values 0.0897 (0.0643)). We conjecture that our inferences are not biased from potential increases in $cash\,ETR3$ in the control group.

## 4.2 REGRESSION RESULTS

**[Table 3]**

The main variable of interest in our regression analysis is $hightolow$ because it isolates the incremental effect a low-tax network exerts on a high-tax firm's tax knowledge. In Panel A of Table 3, we show the results for estimating equation (4) with $cash\,ETR3\,[t_1; t_3]$, $delta\,cash\,ETR3$ and $change\ cash\,ETR3$ as dependent variables. The estimates for $hightolow$ are negative and significant in all specifications. In the specification with $cash\,ETR3\,[t_1; t_3]$ as the dependent variable, the estimate for $hightolow$ has a magnitude of -0.0278 (p-value 0.0488). Extending equation (4) to the change-indicating variables $delta\,cash\,ETR3$ and $change\,cash\,ETR3$ yields corresponding statistical implications (p-values 0.0137 and 0.0069). Economically, these results are consistent with our descriptive inferences in terms of direction and magnitude for both levels of and changes in tax planning behavior. Because the covariates of

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[9] Equivalently, testing for the differences in means for the first difference estimator $change\ cash\,ETR3$ by $hightolow$ yields corresponding results.

$partner, network$ & $firm\ controls$ account for a broad range of alternative explanations, we find it plausible to associate the (relative) increase in tax planning for high-tax firms in low-tax networks to be induced by the presence of a low-tax firm in the network.

For brevity and in support of refraining from discussing marginal effects of control variables (Hünermund and Louw [2020]), we generally focus the coefficient estimates for $hightolow$ and $partner\ controls$. While the estimates for $SameAuditor$ do not surpass the common levels of significance in either specification, the estimates for $Proximity$ are consistently negative but only once statistically significant when $change\ cash\ ETR3$ is the dependent variable. In several additional analyses, we focus on interactions of $hightolow$ and $partner\ controls$ to investigate whether $partner\ controls$ complement or substitute the identified effects. We do not observe that the business purposes of the strategic alliances ($network\ controls$) drive our findings. Solely the coefficients for $PurposeRnD$ load negative and significant with $delta\ cash\ ETR3$ and $change\ cash\ ETR3$ as the dependent variables. We cautiously interpret the coefficients for the $network\ controls$ to be consistent with research that shows that strategic alliances in R&D lead to higher patent output (Li et al. [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. [2020]). If strategic alliances in R&D further allowed firms to employ specific tax credits, this effect would be unconditional to our classification of networks as of type $hightolow$. Generally, we conjecture that these results support our notion that strategic alliances are not intended to establish tax planning investments.

Next, we turn to our DiD analysis, which excludes overlapping events from the data. Panel B of Table 3 depicts two specifications of equation (5). In the second specification, entropy balancing weights are applied (balanced sample).[10] The estimates for the interaction $treated * post$ are negative and significant

[10] Our results are robust to using alternative calculations (moments and timing) of the entropy weights (Table OS 6).

in both specifications. Consistent with our results for equation (4), we find a negative $cash\ ETR$ response to low-tax networks in comparison to high-tax networks for high-tax firms.[11] Consequently, applying a DiD regression design not only corroborates our main findings but also hedges our conclusions from errors due to potential confounding, overlapping events or systematic differences between treatment and control groups. Taken together, the results from our descriptive and regression analyses convey that a firm's tax planning behavior is driven by the partner's tax planning behavior. Consequently, we argue to identify inadvertent tax knowledge diffusion via strategic alliances.

## 5. Facilitators

### 5.1 ELAPSED TIME

**[Table 4]**

Knowledge diffusion is a gradual, multi-stage process (Inkpen [2000], Szulanski [1996], Bresman et al. [2010]) and elapsed time is suggested to increase the probability of uniformity of actions in networks (Gale and Kariv [2003]). To test whether elapsed time facilitates tax knowledge diffusion via strategic alliances, we estimate five specifications of equation (5). We extend the posttreatment period by one year with each specification. We notate $post = 1$ only for the year of network initiation $[t_1]$ first and finish with $post$ equaling one for the entire posttreatment embargo period $[t_1; t_5]$. The coefficients of $treatment * post$ present the cumulative adjustment of a high-tax firm's tax planning behavior with elapsed time ($t_1$ to $t_5$) when cooperating with low-tax firms. The results of this analysis are presented in Table 4. In accordance with our theoretical framework, the coefficient of $treatment * post$ is statistically insignificant when the posttreatment period is limited to the year of network initiation. This finding is consistent with our notion that strategic alliances, per se, do not aim at facilitating tax planning. The coefficient of the interaction,

[11] Our results are robust to (i) excluding $firm\ controls$, $\delta_{ind}$, and $\tau_t$ from the model (untabulated) and (ii) using the cash tax differential developed by Henry and Sansing [2018], which reflects the extent to which a firm is tax-favored (Table OS 5). Additionally, we implement an interaction weighted two way fixed effects estimator under consideration of the recent advances by Sun and Abraham [2020] (Table OS 3).

however, becomes significant when $post$ spans from $t_1$ to $t_2$. Furthermore, the estimated effect continues to be significant when extending the posttreatment period to $t_3$, $t_4$, and $t_5$. This estimation of treatment effects shows that high-tax firms are, on average, able to adjust their tax planning behavior within two years of network initiation.[12] This finding is consistent with recent research 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. Our results suggest that elapsed time facilitates tax knowledge diffusion via strategic alliances.

## 5.2 DISTANCE, INDUSTRY AFFILIATION, AUDIT FIRM

**[Table 5]**

Our theoretical framework highlights the behavioral aspects of tax planning processes and suggests that especially soft factors (e.g., the time-consuming building of trust) facilitate tax knowledge diffusion via strategic alliances. Tax research on cross-firm connections, however, often considers firms' organizational structures and their environments. Consistently, we investigate whether geographical proximity ($SameBEARegion$)[13] and shared industry affiliation ($SameInd$) intensify or mitigate the identified effects. However, we treat these analyses as open empirical questions for strategic alliances without clear predictions. We do so because the outlined theoretical framework for tax knowledge diffusion via strategic alliances suggests that facilitators particularly appear neither mutually exclusive nor reinforcing when firms' organizational structures and their environments are analyzed (e.g., see Bourveau et al. [2020] and Cai and Szeidl [2018] on the opposing effects of competition on collusion and diffusion of information).

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[12] We document consistent evidence for specifications of equation (4) with single-year $cash\ ETR$ and five-year $(delta)\ cash\ ETR5$ as dependent variables. The coefficients for $hightolow$ are negative albeit statistically significant for the multiperiod dependent variables (untabulated). Our results are also robust to estimating an interaction weighted two way fixed effects approach (Table OS 3).

[13] Using $SameBEARegion$ is consistent with Brown [2011]. The implications remain generally unchanged for intereactions of $hightolow$ with $Proximity$ (untabulated).

In Panel A of Table 5, we interact $hightolow$ with the indicator variable $SameBEARegion$. We observe significant and negative coefficient estimates of $hightolow$ in both specifications. However, the estimates for $SameBEARegion * hightolow$ do not surpass common levels of statistical significance. While this finding is generally consistent with the inferences by Brown [2011] on geographical proximity, Cen et al. [2020] report that the correlation of effective tax rates in supply chains is stronger for firms that are located within the same region. This emphasizes the importance of various channels for the diffusion and transfer of tax knowledge and distinguishes strategic alliances from other cross-firm connections. Our inferences for interacting $hightolow$ with $SameInd$ are similar. In particular, we cannot reject that the interactions of $hightolow$ and $SameInd$ are significantly different from zero. This finding is consistent with Brown and Drake [2014].

Next, we turn to shared audit firms ($SameAuditor$) to test for the impact of an alternative channel. Whether auditors impact firms' tax planning behavior is much debated in the literature. In our analysis, the interaction terms for $hightolow * SameAuditor$ do not surpass common levels of statistical significance (Panel C of Table 5). Our findings indicate that a shared audit firm would serve as a substitute rather than as a complement for low-tax networks of high-tax firms. This inference takes place alongside a range of mixed inferences concerning auditors' impact on firms' tax planning behavior. Brown [2011] does not find significant tax shelter adoption via shared audit firms, and Klassen et al. [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 et al. [2020] suggest that there are limits to the relation between auditor-provided tax services and clients' tax planning.

## 6. Effects on the Reporting of Operations

**[Table 6]**

To triangulate our findings, we analyze the effects of low-tax networks on firms' reporting of operations. Thereby, we identify drivers of the changes in tax planning behavior and corroborate our evidence that these changes are due to tax knowledge diffusion. We are also able to shed light on whether firms modify their organizational structure in response to varying network types.

### 6.1 TEXTUAL SENTIMENT OF 10-K FILINGS

Law and Mills [2015] show that linguistic cues in firms' qualitative disclosures provide incremental information beyond traditional accounting variables to predict tax planning. They provide evidence that the use of negative words (i.e., textual sentiment) in firms' 10-K filings suggests (future) tax planning. Quantifying language to measure firms' fundamentals has received massive interest in accounting and finance literature since Tetlock et al. [2008] and Loughran and McDonald [2011] pioneered in this field (for comprehensive reviews, see Loughran and McDonald [2016], [2020b] and Teoh [2018]). Commonly, accounting-specific dictionaries (bag of words) that share common sentiments (e.g., positive, negative) are used to measure a document's textual sentiment. While word classifications can largely differ according to the investigated setting (Loughran et al. [2019], Loughran and McDonald [2020a]), the focus is generally on the use of negative words.

We analyze whether the textual sentiment in firms' 10-K filings changes differently for high-tax firms in strategic alliances with low-tax firms relative to high-tax firms in high-tax strategic alliances. We estimate specifications of equation (5) with $Sentiment$ ($Use\ of\ Negative\ Words$) as dependent variables.[14] The results are depicted in Table 6. The estimate for the interaction of $treated * post$ is negative (positive) and statistically significant in both specifications. This negative response of 10-K filings' textual sentiment can be associated with tax planning behavior (Law and Mills [2015]). Law and Mills (2015), however, identify local average treatment effects by employing exogenous shocks to external financial constraints as instrumental variables for decreasing textual sentiment. Therefore, we estimate equation (5) with $Whited\ Wu\ Index$ as a proxy for firms' financial constraints as the dependent variable (Whited and Wu

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[14] Data on textual sentiment of 10-K filings is shared publicly by Bill McDonald. Please refer to the variable definitions in the Appendix for further details. A graphical investigation of the parallel trend test is presented in the GitHub repository.

[2006]). The coefficient for $treated * post$ in this test indicates that the identified decreases in textual sentiment of 10-K filings are not accompanied by increases in the firms' financial constraints (untabulated). Consequently, the treated firms in our sample seem not to increase their tax planning as a substitute for a more expensive source of external financing. We conclude that this supports the identification of tax knowledge diffusion via strategic alliances. Furthermore, our results indicate that changes in firms' tax planning may be accompanied by changes in their disclosure behavior.

### 6.2 TAX HAVEN OPERATIONS

Exhibit 21 of a 10-K filing lists all subsidiaries of a registrant, the state, or other jurisdiction of incorporation or organization of each, and the names under which such subsidiaries do business. Exceptions apply for subsidiaries, which are not considered "significant" (see Dyreng et al. [2020] and Demeré et al. [2020] for detailed explanations). Utilizing these data allows us to test whether the high-tax firms' changes in tax planning are accompanied by increases in the reported operations in international tax havens (Dyreng and Lindsey [2009]) or the State of Delaware (Dyreng et al. [2013]).[15]

Dyreng and Lindsey [2009] use exhibit 21 disclosures to show that US firms with material operations in at least one tax haven country generally report lower effective tax rates. Therefore, we estimate equation (5) with the indicator variable $Use\ of\ Tax\ Haven$ as the dependent variable.[16] The estimate for the interaction of $treated * post$ is negative but not significant (Table 6). This suggests that high-tax firms, on average, are able to increase their cash tax savings without increasing the reported operations in international tax haven countries. Furthermore, we focus on firms' reported operations in the State of Delaware because Dyreng et al. [2013] suggest that firms employ tax planning strategies that involve the strategic location of subsidiaries in the state. Consistent with Dyreng et al. [2013], we construct the indicator variable

[15] Exhibit 21 disclosure data are shared publicly by Scott Dyreng. Please refer to the variable definitions in the Appendix for further details. A graphical investigation of the parallel trend test is presented in the GitHub repository.
[16] In an untabulated test we employ the continuous variable $Num\ of\ Tax\ Haven\ Subs$ as dependent variable and find consistent results. The appendix contains detailed variable definitions.

$Delaware\ Strategy$ to approximate the role that Delaware plays as domestic tax haven. We find that the estimate of $treated * post$ is positive but not statistically significant when estimating equation (5) with $Delaware\ Strategy$ as the dependent variable. This suggests that low-tax networks are not generally accompanied by increases in high-tax firms' operations in tax havens. However, data from exhibit 21 cannot quantify the magnitude, extent, or legal structure of all tax haven activities. In particular, one cannot identify specific tax planning transactions (Law and Mills [2019]). Therefore, we cautiously conclude that our findings suggest that high-tax firms use existing, yet not fully exploited, structures for tax planning.

## 7. Robustness Checks

In the online supplement, we examine the robustness of our results. In particular, we show (i) that there is no economically meaningful tax knowledge effect for low-tax firms. Furthermore, we perform (ii) falsification tests with 1000 iterations of placebo indicators for $hightolow$ among untreated high-tax firms. Additionally, we implement (iii) an interaction weighted two way fixed effects estimator under consideration of the recent advances by Sun and Abraham [2020]. We examine (iv) an alternative identification strategy, (v) an alternative tax knowledge measure, (vi) the employment of alternative entropy balancing weights in the DiD analysis, and (vii) alternative explanations regarding nonsurvivors and effects on profitability. Throughout these tests, our results remain robust.

## 8. Conclusion

The purpose of this study is to shed light on whether tax knowledge diffuses between firms via strategic alliances. Our study builds on theory which highlights the behavioral aspects of tax planning processes because knowledge diffusion is shaped by soft factors such as communication and trust. In particular, the theoretical framework of tax knowledge diffusion does not depend on the assumption of an intentional transfer of tax knowledge via an intermediary. Utilizing data on strategic alliances between publicly traded US firms allows us to distinguish between networks 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 high-tax firms in strategic alliances with other high-tax firms. Furthermore, we find that the adjustment occurs on average within two years after a strategic alliance's initiation. This suggests that elapsed time is an important facilitator for tax knowledge diffusion.

Our findings are subject to the caveat that respectively the outcome (i.e., change in the tax planning behavior) of tax knowledge diffusion is measurable. Consistent with, for instance, MARTENS and Sextroh [2021] who investigate information flows via common analysts, we cannot observe the actual mechanisms of the diffusion process. In particular, publicly available data impede the identification of mechanisms when tacit tax knowledge diffuses because firms are generally able to make use of peers' salient information for tax planning purposes (Bird et al. [2018]). Therefore, relevant mechanisms that are based on trust- and communication-based may empirically be proxied for by elapsed time respectively. Consequently, we encourage future research to further investigate the nature of knowledge diffusion and hope that our study highlights the importance of considering taxes in interdisciplinary contexts.

## Appendix: Variable Definitions

| Variable | **Definition** [Compustat (low)/SDC (CAPITAL)/other (*ITALICS*) data items] |
|---|---|
| $\boldsymbol{cash\ ETR3}$ | Multiperiod cash effective tax rate: $$cash\ ETR3_{i,t=1} = \frac{\sum_{t=1}^{3}(\,txpd_{i,t})}{\sum_{t=1}^{3}(\,pi_{i,t} - spi_{i,t})}$$ 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 years; observations with a negative denominator are reset to missing; for the final and penultimate firm-year of a firm substituted by annual cash ETR; winsorized at 0 and 1. |
| $\boldsymbol{cash\ ETR5}$ | Constructed as cash ETR3 but with numerator and denominator constructed over five years. |
| $\boldsymbol{change\ cash\ ETR3}$ | First difference estimator of cash ETR3 [$t_1$; $t_3$] and pre cash ETR3 [$t_{-2}$; $t_0$]: $$change\ cash\ ETR3 = cash\ ETR3_{[t_1;t_3]} - pre\ cash\ ETR_{[t_{-2};t_0]}$$ |
| $\boldsymbol{CTD\ \&\ change\ CTD3}$ | Cash Tax Differential calculated as in Henry and Sansing [2018]; winsorized at p1 and p99. |
| $\boldsymbol{Delaware\ Strategy}$ | Indicator variable; equals 1 for firm-years with a relatively large number (upper third) of subsidiaries in Delaware (*STATECOUNT* and *STATECODE*; consistent with Dyreng et al. [2013]); 0 otherwise; calculated at the level of the global exhibit 21 data. |
| $\boldsymbol{delta\ cash\ ETR3}$ | Cash ETR3 [$t_1$; $t_3$] scaled by pre cash ETR3 [$t_{-2}$; $t_0$]: $$delta\ cash\ ETR3 = \frac{cash\ ETR3_{[t_1;t_3]}}{pre\ cash\ ETR_{[t_{-2};t_0]}} - 1$$ winsorized at p99; reset to 0 when denominator equals 0. |
| $\boldsymbol{delta\ cash\ ETR5}$ | Constructed as delta cash ETR3 but with cash ETR5 as numerator; winsorized at p99; reset to delta cash ETR3 when missing. |
| $\boldsymbol{embargo\ period}$ | Period of three years before [$t_{-2}$;$t_0$] and five years subsequent [$t_1$;$t_5$] to a network initiation during which a firm may not invest in another strategic alliance (exclusion of overlapping events). |
| $\boldsymbol{Exhibit\ 21\ Data}$ | Data on exhibit 21 disclosure is shared publicly by Scott Dyreng: https://sites.google.com/site/scottdyreng/Home/data-and-code/EX21-Dataset. We consider disclosed tax haven subsidiaries (*TAXHAVEN*) and rely on the classification of the jurisdictions as tax havens. We count *TAXHAVEN* occurrences ourselves and do not use *COUNT* because manual inspections of 10-K filings show that country names are counted multiple times when occurring in the subsidiaries' names (*NAME*) as well (as indicated by the data description). |
| $\boldsymbol{high\text{-}tax\ firm}$ | Inverse to low-tax firm; indicator variable in $t_1$; equals 1 if the firm's industry mean adjusted pre cash ETR3 in $t_0$ does not belong to the lowest quartile; 0 for low-tax firms. |
| $\boldsymbol{hightolow}$ | Indicator variable; equals 1 for high-tax firms in low-tax networks; equals 0 for high-tax firms in high-tax networks. |
| $\boldsymbol{low\text{-}tax\ firm}$ | Indicator variable in $t_1$; equals 1 if the firm's industry adjusted pre cash ETR3 in $t_0$ belongs to the lowest quartile ("first bin"); 0 for high-tax firms. |
| $\boldsymbol{lowtohigh}$ | Indicator variable; equals 1 for low-tax firms in high-tax networks; equals 0 for low-tax firms in low-tax networks. |
| $\sum \boldsymbol{network\ controls}$ | Indicator variables for the main business purpose of a network, which is derived from a network's deal description (DEALTEXT) in SDC; comprises PurposeWholesale, PurposeRnD, PurposeLicense, PurposeService, PurposeMarketing, PurposeSupply, PurposeTech and PurposeManufacture. |

| Variable | **Definition** [Compustat (low)/SDC (CAPITAL)/other (*ITALICS*) data items] |
|---|---|
| $\boldsymbol{Num\ of\ Tax\ Haven\ Subs}$ | Measure of material tax haven operations, calculated as $$Num\ of\ Tax\ Haven\ Subsidiaries = \ln\left(1 + \sum TAXHAVEN_{i,t}\right)$$ See exhibit 21 for data origin. |
| $\boldsymbol{\sum partner\ controls}$ | See Proximity and SameAuditor for main analysis (equation (4)). |
| $\boldsymbol{post}$ | Indicator variable; equals 0 in pretreatment period and 1 in posttreatment period. |
| $\boldsymbol{pre\ cash\ ETR3}$ | Constructed as cash ETR3 but with numerator and denominator constructed as the sum of the current and two preceding periods: $$pre\ cash\ ETR3_{i,t=0} = \frac{\sum_{t=-2}^{0}(txpd_{i,t})}{\sum_{t=-2}^{0}(pi_{i,t} - spi_{i,t})}$$ Industry adjusted pre cash ETR $[t_{-2};t_0]$ is used to identify low- and high-tax firms in $t_1$. |
| $\boldsymbol{Proximity}$ | Distance (in miles as the crow flies) between the partners of a network according to the zip code of the partners' headquarters (addzip); collected from freemaptools.com; calculated as -1*Distance, standardized for regressions. |
| $\boldsymbol{SameAuditor}$ | Indicator variable; equals 1 when all partners of a network share the same auditor firm (au) in $t_1$; 0 otherwise. |
| $\boldsymbol{SameInd}$ | Constructed as SameAuditor but for industry affiliation; industry is classified using two-digit SIC codes (sic); see also Table 1, Panel C. |
| $\boldsymbol{SameBEARegion}$ | Constructed as SameAuditor; equals 1 when all network partners 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. |
| $\boldsymbol{Sentiment}$ | Measure of textual sentiment of the underlying 10-K filing, calculated as $$Sentiment_{i,t} = \frac{(n_{pos} - n_{negation}) - n_{negative}}{n_{total}}$$ See Textual Sentiment for data origin. |
| $\boldsymbol{Strategic\ Alliance\ (Network)}$ | Contractual-based cooperation between US firms in the sample period 1994 to 2016, extracted from SDC Platinum (STRATEGICALLIANCE/SAF). |
| $\boldsymbol{Textual\ Sentiment\ Data}$ | Data on textual sentiment is shared publicly by Bill McDonald: https://sraf.nd.edu/textual-analysis/resources/. We consider 10-K filings (*FORM_TYPE*), positive, negative, total words (*N_POSITIVE*, *N_NEGATIVE*, *N_TOTAL*), and negations (*N_NEGATION*). |
| $\boldsymbol{treated}$ | Treatment is set according to hightolow if strategic alliance falls into embargo period (exclusion of overlapping events). Control observations are weighted to treatment observations by entropy balancing (weighting) on continuous firm controls (mean and variance) in the balanced sample. |
| $\boldsymbol{treated * post}$ | Interaction of treated and post; main variable of interest in the difference-in-differences model. |
| $\boldsymbol{Use\ of\ Negative\ Words}$ | Measure of textual sentiment of the underlying 10-K filing, calculated as $$Use\ of\ Negative\ Words_{i,t} = \frac{n_{negative}}{n_{total}}$$ See Textual Sentiment for data origin. |
| $\boldsymbol{Use\ of\ Tax\ Haven}$ | Measure of material tax haven operations; Indicator variable, equals 1 if $\sum TAXHAVEN_{i,t} > 0$ (at least one tax haven subsidiary in Exhibit 21 in firm-year); 0 otherwise; see exhibit 21 for data origin. |

| Variable | Definition [Compustat (low)/SDC (CAPITAL)/other (*ITALICS*) data items] |
|---|---|
| **Firm Controls** | |
| Continuous $firm\ controls$ are winsorized at p1 and p99 and are neither mean-centered nor standardized for analyses. First difference estimators of the continuous $firm\ controls$ are used in specifications with $change\ cash\ ETR3$ as dependent variable. For DiD specifications, annual measures of the $firm\ controls$ are employed. | |
| $\boldsymbol{AdExp3}$ | 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. |
| $\boldsymbol{CapEx3}$ | 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. |
| $\boldsymbol{Cash3}$ | 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. |
| $\boldsymbol{GrowthSale3}$ | The annual average growth rate (geometric mean) of net sales (sale) over three years $\left(\sqrt[3]{\frac{sale_{t3}}{sale_{t1}}}-1\right)$; when missing reset to annual growth rate from $t_0$ to $t_1$, thereafter reset to 0. |
| $\boldsymbol{EBITDA3}$ | 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. |
| $\boldsymbol{Intangibles3}$ | 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. |
| $\boldsymbol{Leverage3}$ | 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. |
| $\boldsymbol{MNE3}$ | Indicator variable; equals 1 if $\sum_{t=1}^{3}(pifo_{i,t}) > 0$ (nonmissing, nonzero value for pre-tax income from foreign operations); 0 otherwise. |
| $\boldsymbol{NOL3}$ | Indicator variable equals 1 if $\sum_{t=1}^{3}(tlcf_{i,t}) > 0$ (nonmissing, nonzero value of tax loss carry forward); 0 otherwise. |
| $\boldsymbol{PPE3}$ | 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. |
| $\boldsymbol{RnDExp3}$ | 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. |
| $\boldsymbol{SGA3}$ | 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. |
| $\boldsymbol{Size3}$ | The natural log of total assets (at) for the respective and two subsequent periods; when missing reset to annual measure, thereafter reset to 0. |

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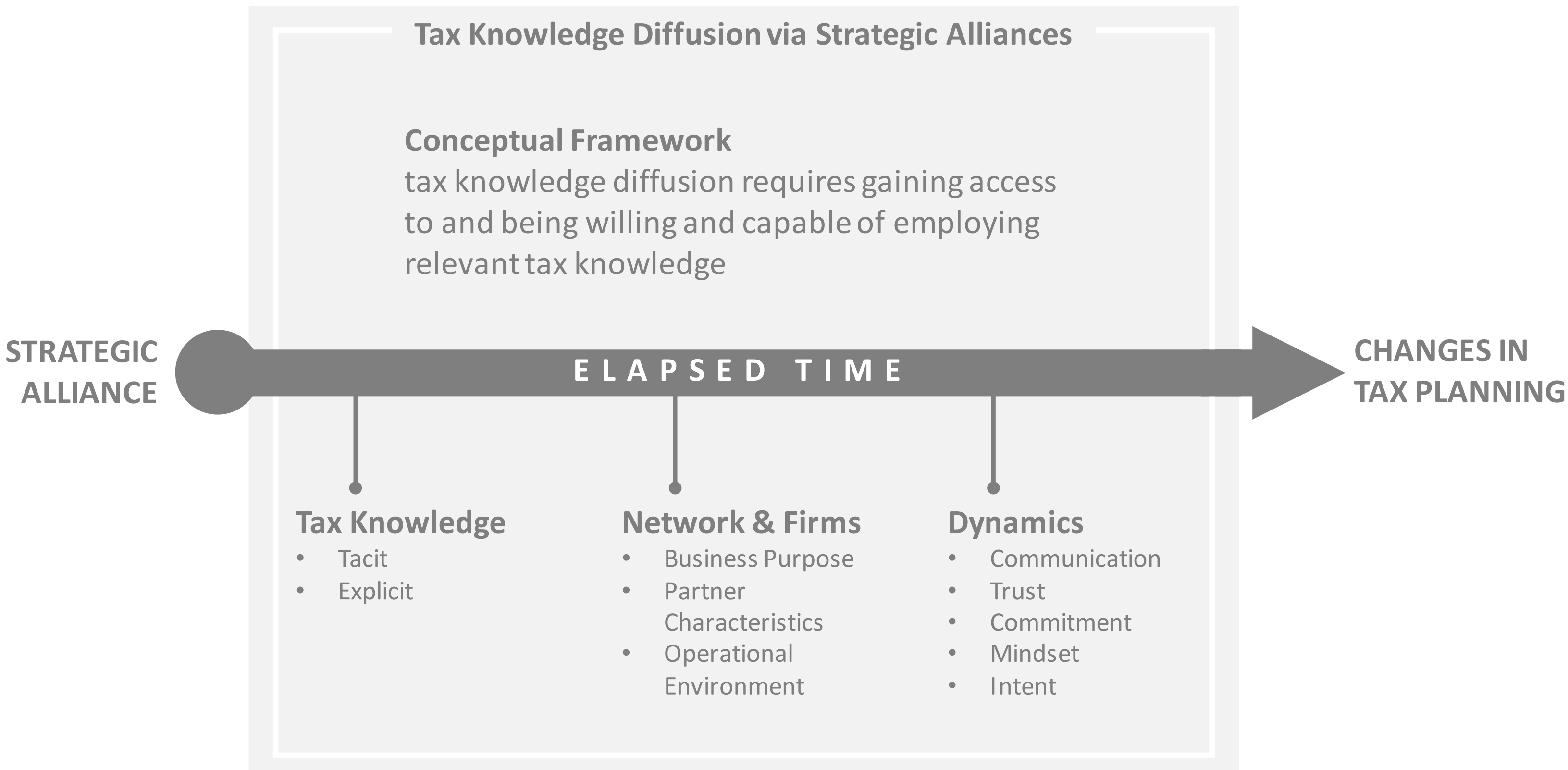
YIN, X., AND M. SHANLEY. "Industry Determinants of the "Merger Versus Alliance" Decision." *Academy of Management Review* 33 (2) (2008): 473–491.

ZHAO, M. "Conducting R&D in Countries with Weak Intellectual Property Rights Protection." *Management Science* 52 (8) (2006): 1185–1199.

## Figures

**Figure 1**

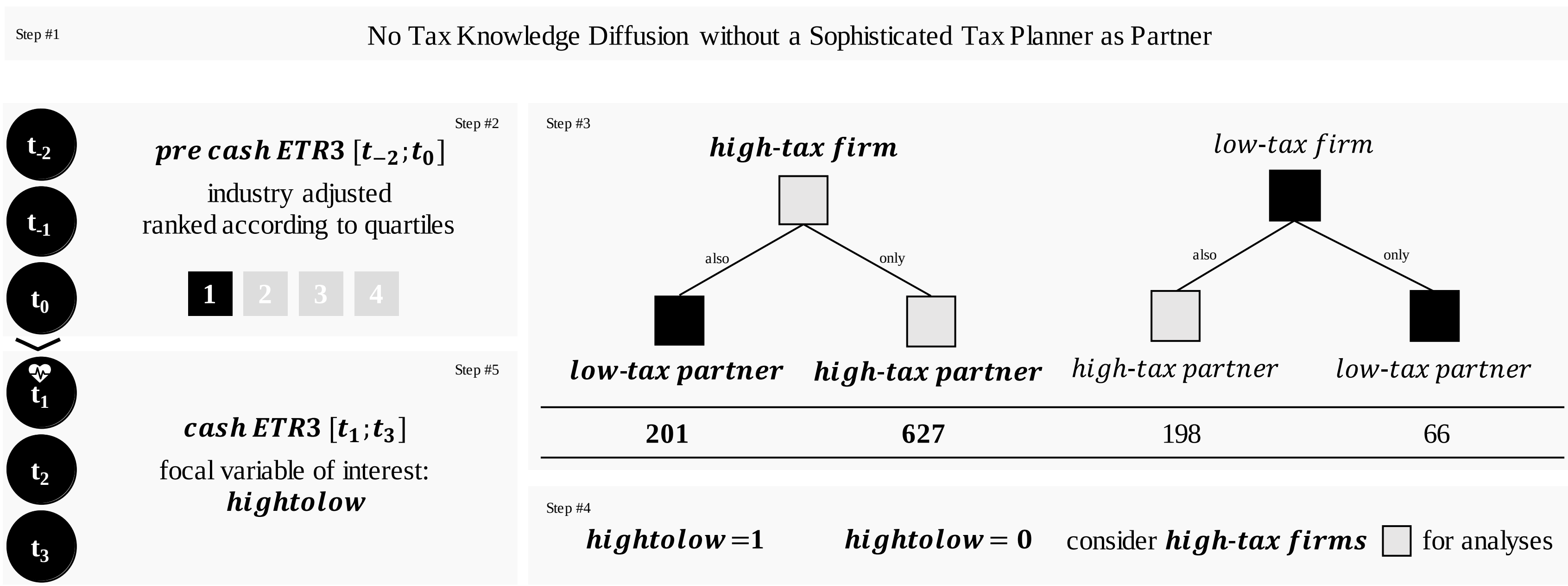



This figure depicts a black box model for tax knowledge diffusion via strategic alliances.

## Figure 2

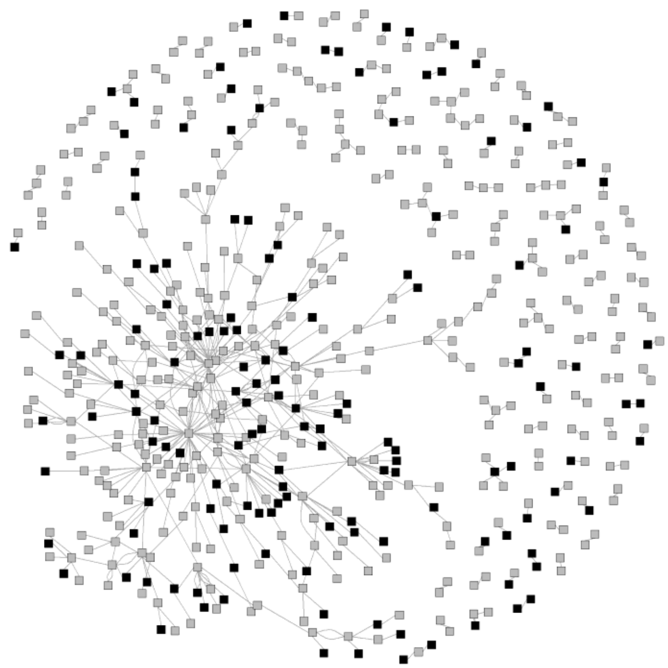
**Sample Selection & Identification Strategy**

| **Sample Selection**<br>SDC Platinum (UPPER) and Compustat (lower) data items in parentheses | **Network-Firm Obs.** | **Networks** | **Firm-Year Obs.** | **Firms** |
|---|---|---|---|---|
| Compustat and SDC Platinum data matched (ULTPARENTCUSIP / cusip) according to year of network initiation (DATEEFFECTIVE / fyear) // networks between US firms (loc, fic, curcd, cik) // sample period 1994 - 2016 | 17,126 | 14,838 | 51,566 | 3,445 |
| Identify all participants in a network | 3,967 | 1,969 | 22,352 | 1,545 |
| Identify $pre\ cash\ ETR3\ [t_{-2};t_0]$ and $cash\ ETR3\ [t_1;t_3]$ of all participants | **1,092** | 543 | 9,206 | 502 |




This figure summarizes our sample selection and identification strategy. The heartbeat pictogram at $t_1$ indicates the year of initiation of a strategic alliance. The number of observations equals one observation per firm, which covers the first three years after the initiation of a strategic alliance.

**Figure 3**



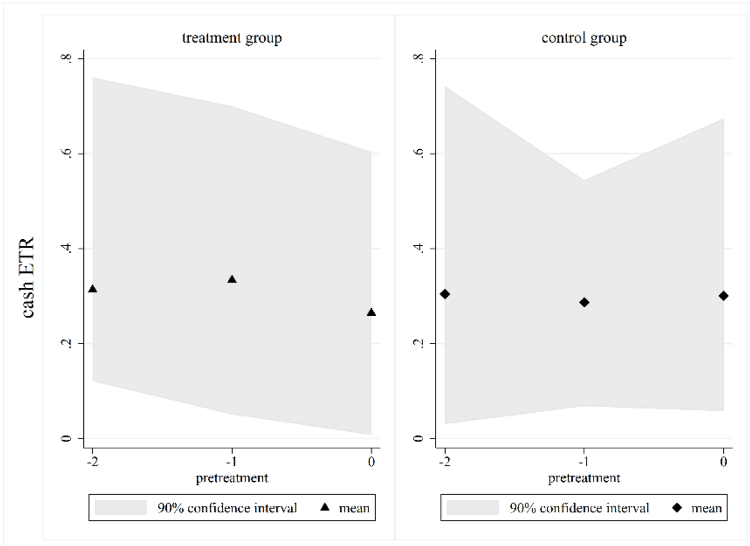
This figure maps networks and the investing firms from our sample (for networks of two partners). Each vertex (square) displays a firm in its classification as low-tax (black vertices) or high-tax (gray vertices). A link between two gray vertices translates into an $hightolow = 0$ observation for both high-tax firms. Correspondingly, a link between a gray vertex and a black vertex translates into an $hightolow = 1$ observation for the high-tax firm. There are 324 firms each of which has one network observation in our sample. In contrast, there are 178 firms with > 1 investments.

**Figure 4**


The word cloud depicted in Figure 3 shows the 40 most common words used in the SDC's deal description of the networks in our sample. By systematically searching through the deal descriptions, we identify *wholesale* , *licensing* , *marketing* and *manufacturing* activities as well as *research and development*, providing *services*, engaging in *supply functions* and *technology* as major business purposes of the networks in our sample. The respective indicator variables are included in equation (4). All variables are defined in detail in the Appendix.

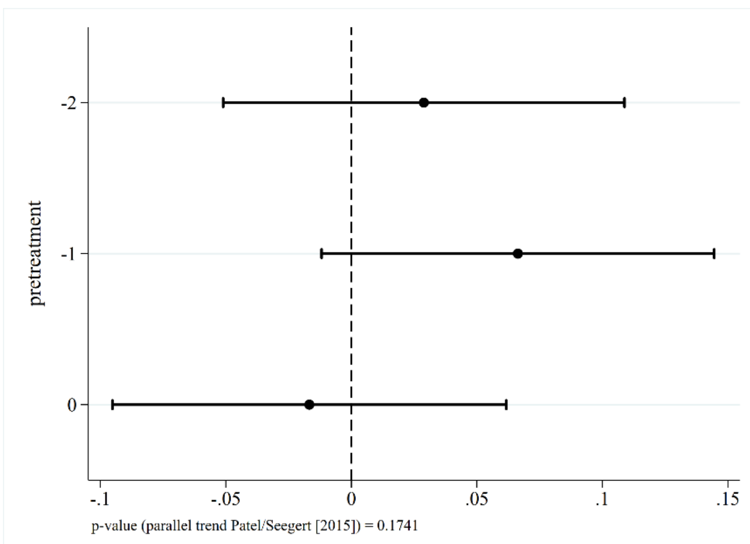
**Figure 5**

**Panel A**




Panel A of this figure provides visual evidence that the trend of $cash\ ETR$ is similar for the treatment and control firms prior to the treatment.

**Panel B**




In Panel B, we apply the approach of Patel and Seegert [2015] to provide statistical support for the parallel trend assumption. The figure reports the coefficient and 95% confidence interval of the interaction of the treatment indicator and embargo period fixed effects (with $t_1$ as baseline) for pretreatment years. The p-value for the parallel trend test is reported at the bottom of Panel B.

**Tables**

**Table 1 Information on Networks and Firms**

**Panel A Descriptive Statistics**

| N | 201 | | | 627 | | | 198 | | | 66 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| $\boldsymbol{hightolow}$ | | **== 1** | | | **== 0** | | | | | | | |
| $lowtohigh$ | | | | | | | | == 1 | | | == 0 | |
| | mean | p50 | SD | mean | p50 | SD | mean | p50 | SD | mean | p50 | SD |
| $pre\ cash\ ETR3$ | 0.3039 | 0.2631 | 0.1789 | 0.2856 | 0.2637 | 0.1341 | 0.0900 | 0.0862 | 0.0532 | 0.0976 | 0.0924 | 0.0632 |
| $cash\ ETR3$ | 0.2557 | 0.2341 | 0.1874 | 0.2804 | 0.2449 | 0.1936 | 0.1557 | 0.1359 | 0.1493 | 0.1631 | 0.1475 | 0.1108 |
| $delta\ cash\ ETR3$ | - 0.0411 | - 0.1005 | 0.6658 | 0.0871 | - 0.0446 | 0.8096 | 1.0424 | 0.2530 | 2.4787 | 1.7487 | 0.3520 | 3.7796 |
| $change\ cash\ ETR3$ | - 0.0481 | - 0.0267 | 0.2337 | - 0.0051 | - 0.0117 | 0.2103 | 0.0657 | 0.0376 | 0.1480 | 0.0655 | 0.0355 | 0.1263 |
| $EBITDA3$ | 0.1551 | 0.1504 | 0.0705 | 0.1606 | 0.1517 | 0.0768 | 0.1321 | 0.1255 | 0.0607 | 0.1273 | 0.1279 | 0.0498 |
| $RnDExp3$ | 0.0741 | 0.0517 | 0.0766 | 0.0677 | 0.0475 | 0.0745 | 0.0965 | 0.0834 | 0.0869 | 0.1193 | 0.1208 | 0.0917 |
| $AdExp3$ | 0.0172 | 0.0036 | 0.0271 | 0.0145 | 0.0000 | 0.0255 | 0.0112 | 0.0000 | 0.0260 | 0.0132 | 0.0018 | 0.0244 |
| $SGA3$ | 0.2671 | 0.2720 | 0.1826 | 0.2719 | 0.2613 | 0.1798 | 0.3099 | 0.3052 | 0.2038 | 0.3847 | 0.3704 | 0.2296 |
| $CapEx3$ | 0.1300 | 0.1159 | 0.0754 | 0.1365 | 0.1170 | 0.0866 | 0.1498 | 0.1382 | 0.0902 | 0.1487 | 0.1368 | 0.0705 |
| $GrowthSale3$ | 0.0386 | 0.0275 | 0.0867 | 0.0484 | 0.0267 | 0.1047 | 0.0572 | 0.0484 | 0.1088 | 0.0587 | 0.0447 | 0.0871 |
| $Leverage3$ | 0.1911 | 0.1977 | 0.1347 | 0.1932 | 0.1914 | 0.1563 | 0.1781 | 0.1318 | 0.1959 | 0.1674 | 0.1531 | 0.1638 |
| $Cash3$ | 0.1797 | 0.1254 | 0.1572 | 0.1725 | 0.1154 | 0.1626 | 0.2448 | 0.2179 | 0.1989 | 0.2901 | 0.2460 | 0.2273 |
| $MNE3$ | 0.6169 | 1.0000 | 0.4874 | 0.6411 | 1.0000 | 0.4800 | 0.6667 | 1.0000 | 0.4726 | 0.6818 | 1.0000 | 0.4693 |
| $NOL3$ | 0.2786 | 0.0000 | 0.4494 | 0.2823 | 0.0000 | 0.4505 | 0.3889 | 0.0000 | 0.4887 | 0.2879 | 0.0000 | 0.4562 |
| $Intangibles3$ | 0.1909 | 0.1577 | 0.1692 | 0.1765 | 0.1067 | 0.1877 | 0.1509 | 0.0864 | 0.1626 | 0.1251 | 0.0698 | 0.1450 |
| $PPE3$ | 0.3958 | 0.3112 | 0.2821 | 0.4141 | 0.3244 | 0.3232 | 0.3165 | 0.2194 | 0.2785 | 0.2947 | 0.2326 | 0.2381 |
| $Size3$ | 9.8628 | 10.6128 | 2.0736 | 9.6368 | 10.0497 | 2.0674 | 9.3460 | 9.5647 | 2.0564 | 9.2779 | 9.0031 | 2.1442 |

Panel A of Table 1 shows descriptive statistics for our tax knowledge proxies and $firm\ controls$ in $t_1$ (year of network initiation). The number of observations equals one observation per firm, which covers the first three years after the initiation of a strategic alliance. The $firm\ controls$ are constructed as multiperiod measures $[t_1; t_3]$. In specifications with $change\ cash\ ETR3$ as dependent variable, first differences of continuous $firm\ controls$ are applied. All variables are defined in detail in the Appendix.

## Table 1 Information on Networks and Firms (continued)

**Panel B Partner and Network Controls**

| | *Partner Controls* | | | | *Network Controls* (Purpose_) | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| n (mean) | SameAuditor | SameInd | SameBEARegion | Proximity | Wholesale | R&D | Licensing | Service | Marketing | Supply Chain | Manufacturing | Technology |
| $hightolow$ == 1 | 44 (0.2189) | 81 (0.4030) | 45 (0.2239) | (-1233) | 24 (0.1194) | 51 (0.2537) | 30 (0.1493) | 7 (0.0348) | 51 (0.2537) | 16 (0.0796) | 30 (0.1493) | 81 (0.4030) |
| $hightolow$ == 0 | 122 (0.1946) | 260 (0.4147) | 130 (0.2073) | (-1187) | 30 (0.0478) | 179 (0.2855) | 112 (0.1786) | 26 (0.0415) | 158 (0.2520) | 44 (0.0702) | 40 (0.0638) | 315 (0.5024) |
| $lowtohigh$ == 1 | 44 (0.2222) | 81 (0.4091) | 46 (0.2323) | (-1247) | 24 (0.1212) | 50 (0.2525) | 30 (0.1515) | 7 (0.0354) | 51 (0.2576) | 15 (0.0758) | 29 (0.1465) | 80 (0.4040) |
| $lowtohigh$ == 0 | 18 (0.2727) | 38 (0.5758) | 28 (0.4242) | (-917) | 4 (0.0606) | 16 (0.2424) | 6 (0.0909) | 2 (0.0303) | 30 (0.4545) | 0 (0) | 6 (0.0909) | 34 (0.5152) |

**Panel C Industry Affiliation of Networks and Firms [two-digit SIC-code]**

| | | | Industry of Networks (Network-Firm Observations) | | | | | | | | | | | Σ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | I | II | III | IV | V | VI | VII | VIII | IX | X | XI | XII | |
| Industry of Firms | Agriculture, Forestry, & Fishing [01-09] | I | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 3 |
| | Mining [10-14] | II | 0 | 6 | 0 | 1 | 2 | 0 | 2 | 0 | 0 | 0 | 1 | 0 | 12 |
| | Construction [15-17] | III | 0 | 0 | 0 | 0 | 0 | 3 | 0 | 0 | 0 | 0 | 1 | 0 | 4 |
| | Manufacturing: Chemical & Allied Products [28] | IV | 0 | 0 | 0 | 23 | 10 | 0 | 19 | 0 | 13 | 5 | 32 | 0 | 102 |
| | Manufacturing [20-39, except 28] | V | 0 | 4 | 2 | 12 | 110 | 16 | 30 | 3 | 42 | 109 | 43 | 7 | 378 |
| | Transportation & Public Utilities [40-49] | VI | 0 | 3 | 2 | 0 | 3 | 28 | 3 | 0 | 2 | 29 | 9 | 2 | 81 |
| | Wholesale Trade [50-51] | VII | 0 | 1 | 0 | 0 | 2 | 2 | 15 | 1 | 1 | 7 | 5 | 0 | 34 |
| | Retail Trade [52-59] | VIII | 0 | 0 | 0 | 0 | 4 | 3 | 9 | 1 | 5 | 10 | 3 | 2 | 37 |
| | Finance, Insurance, & Real Estate [60-67] | IX | 0 | 0 | 0 | 0 | 1 | 0 | 2 | 0 | 19 | 18 | 5 | 3 | 48 |
| | Services: Business Services [73] | X | 0 | 0 | 0 | 0 | 25 | 8 | 9 | 3 | 25 | 231 | 24 | 2 | 327 |
| | Services [70-89, except 73] | XI | 0 | 0 | 0 | 0 | 1 | 2 | 2 | 0 | 2 | 7 | 14 | 0 | 28 |
| | Nonclassifiable Establishments/Other | XII | 0 | 0 | 0 | 3 | 4 | 5 | 1 | 0 | 3 | 15 | 7 | 0 | 38 |
| | | | 0 | 14 | 4 | 40 | 162 | 68 | 92 | 8 | 112 | 431 | 145 | 16 | 1092 |

Panel B of Table 1 shows the distribution of the *partner & network control* variables among $hightolow$ and $lowtohigh$ observations. The variable $Proximity$ is standardized for regression analyses. Panel C of Table 1 presents information regarding the industry affiliation of networks and firms. Industry affiliation is determined on a two-digit SIC-code basis. All variables are defined in detail in the Appendix.

**Table 2 Descriptive Analysis**

**Panel A Change from $\boldsymbol{pre\ cash\ ETR3}$ $[\boldsymbol{t_{-2}};\boldsymbol{t_0}]$ to $\boldsymbol{cash\ ETR3}$ $[\boldsymbol{t_1};\boldsymbol{t_3}]$**

| N | | 201 | | 627 | |
|---|---|---|---|---|---|
| $hightolow$ | | == 1 | | == 0 | |
| | | mean | (SD) | mean | (SD) |
| $pre\ cash\ ETR3$ $[t_{-2};t_0]$ | I | 0.3039 | (0.1789) | 0.2856 | (0.1341) |
| $cash\ ETR3$ $[t_1;t_3]$ | II | 0.2557 | (0.1874) | 0.2804 | (0.1936) |
| | | | | | |
| Before After Change | I to II | - 0.0481*** | | - 0.0051 | |
| (SE) | | (0.0165) | | (0.0084) | |
| | | | | | |
| Difference in Differences | | | **- 0.0430**** | | |
| (SE) | | | **(0.0175)** | | |

**Panel B Differences in $\boldsymbol{delta\ cash\ ETR3}$ and $\boldsymbol{change\ cash\ ETR3}$**

| $hightolow$ | ==1 | ==0 | difference | (SE) |
|---|---|---|---|---|
| - mean $delta\ cash\ ETR3$ | - 0.0411 | 0.0871 | - 0.1282** | (0.0630) |
| - p50 $delta\ cash\ ETR3$ | - 0.1005 | - 0.0446 | - 0.0560* | (0.0329) |
| - p50 $change\ cash\ ETR3$ | - 0.0267 | - 0.0117 | - 0.0150* | (0.0081) |

All variables are defined in detail in the Appendix. Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. We follow Conroy [2012] and apply quantile regression to test for the differences in medians in Panel B. The results of these tests are robust to using (i) Mann-Whitney/Wilcoxon rank-sum and (ii) K-sample equality-of-medians tests (untabulated).

## Table 3 Main Analysis

**Panel A Baseline Regression**

| Dependent Variable | ***cash ETR3*** | | ***delta cash ETR3*** | | ***change cash ETR3*** | |
|---|---|---|---|---|---|---|
| | Coefficient | (SE) | Coefficient | (SE) | Coefficient | (SE) |
| ***hightolow*** | **- 0.0278**** | **(0.0141)** | **- 0.1301**** | **(0.0526)** | **- 0.0465***** | **(0.0171)** |
| *Proximity* | - 0.0017 | (0.0065) | - 0.0192 | (0.0293) | - 0.0139* | (0.0074) |
| *SameAuditor* | - 0.0048 | (0.0154) | 0.0342 | (0.0619) | 0.0144 | (0.0171) |
| *EBITDA*3 | - 0.6078*** | (0.1880) | - 1.7484*** | (0.5943) | - 0.5162** | (0.2383) |
| *RnDExp*3 | 0.1396 | (0.1835) | 1.1304 | (0.8668) | 0.2258 | (0.4751) |
| *AdExp*3 | 0.2153 | (0.3439) | - 0.2850 | (1.1296) | 1.8520 | (1.4296) |
| *SGA*3 | - 0.0080 | (0.0682) | 0.3059 | (0.3048) | 0.4390 | (0.2726) |
| *CapEx*3 | - 0.1320 | (0.1686) | - 0.0899 | (0.5790) | 0.2803 | (0.2294) |
| *GrowthSale*3 | - 0.3495** | (0.1362) | - 1.4437*** | (0.5173) | - 0.2668*** | (0.0862) |
| *Leverage*3 | - 0.2090** | (0.0884) | - 0.6533** | (0.3024) | - 0.0240 | (0.1442) |
| *Cash*3 | - 0.1190 | (0.1154) | - 0.8412* | (0.4491) | 0.2615 | (0.1627) |
| *MNE*3 | 0.0210 | (0.0198) | 0.1115 | (0.0842) | 0.0459** | (0.0232) |
| *NOL*3 | 0.0205 | (0.0224) | 0.0277 | (0.0870) | 0.0165 | (0.0235) |
| *Intangibles*3 | 0.0641 | (0.0656) | 0.2477 | (0.2302) | 0.2058 | (0.1496) |
| *PPE*3 | 0.0698 | (0.0587) | 0.2123 | (0.1813) | 0.0375 | (0.1394) |
| *Size*3 | - 0.0108* | (0.0056) | - 0.0173 | (0.0187) | - 0.0602** | (0.0252) |
| *PurposeWholesale* | 0.0123 | (0.0271) | - 0.0041 | (0.1223) | - 0.0358 | (0.0338) |
| *PurposeR&D* | - 0.0160 | (0.0130) | - 0.1131** | (0.0573) | - 0.0409** | (0.0159) |
| *PurposeLicensing* | - 0.0021 | (0.0181) | 0.0174 | (0.0782) | - 0.0076 | (0.0219) |
| *PurposeService* | - 0.0366 | (0.0422) | - 0.1344 | (0.1300) | - 0.0314 | (0.0354) |
| *PurposeMarketing* | - 0.0014 | (0.0179) | 0.0162 | (0.0861) | 0.0115 | (0.0204) |
| *PurposeSupply* | - 0.0104 | (0.0298) | - 0.0464 | (0.1089) | - 0.0067 | (0.0322) |
| *PurposeManufacturing* | 0.0296 | (0.0216) | - 0.0131 | (0.0863) | 0.0001 | (0.0289) |
| *PurposeTech* | - 0.0022 | (0.0160) | 0.0239 | (0.0576) | - 0.0271 | (0.0186) |
| *Intercept* | 0.5132*** | (0.0705) | 0.5342** | (0.2411) | - 0.0265 | (0.0219) |
| Firm Controls | | | | | First Differences | |
| Fixed Effects | Industry & Year | | Industry & Year | | Industry & Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | | Cluster @ Firm | |
| N | 828 | | 828 | | 828 | |
| Adjusted $R^2$ | 0.1224 | | 0.1108 | | 0.1221 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. The results for equation (4) are presented in Panel A. Our focal variable of interest is *hightolow*, which is an indicator variable set equal to 1 for observations of high-tax firms cooperating with low-tax firms and set equal to 0 for high-tax firms cooperating with high-tax firms. All variables are defined in detail in the Appendix.

**Table 3 Main Analysis (continued)**

**Panel B Difference in Differences**

| Dependent Variable | $\boldsymbol{cash\,ETR}$ | | $\boldsymbol{cash\,ETR}$ | |
|---|---|---|---|---|
| Embargo Period | Yes $[t_{-2}; t_5]$ | | Yes $[t_{-2}; t_5]$ | |
| Entropy Balancing | - | | Balanced Sample | |
| | Coefficient | (SE) | Coefficient | (SE) |
| $treated$ | 0.0057 | (0.0166) | 0.0149 | (0.0184) |
| $\boldsymbol{treated * post}$ | **- 0.0361*** | **(0.0212)** | **- 0.0486**** | **(0.0237)** |
| Controls | Partner & Network & Firm (Annual Levels) | | Partner & Network & Firm (Annual Levels) | |
| Fixed Effects | Industry & Year & Embargo Period | | Industry & Year & Embargo Period | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 1945 | | 1945 | |
| Adjusted $R^2$ | 0.0516 | | 0.0686 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. Panel B depicts the results for equation (5). We create an embargo period around a network observation during which a firm may not invest in another network (exclusion of overlapping events). The embargo period contains the three years preceding ($post = 0$) and five years subsequent ($post = 1$) to a network initiation. The exclusion of overlapping events deflates treatment and control observations, while measuring annual levels of variables over time ($[t_{-2}; t_5]$, DiD) inflates the sample in comparison to measuring multiperiod variables at a point in time ($[t_1]$, Panel A). This approach leads to $n = 1{,}945$. Including $embargo\ period$ fixed effects subsumes the $post$ indicator. In the second specification, we employ entropy-balancing weighting and use the entropy weights to reestimate equation (5). Observations are balanced using continuous $firm\ controls$ so that the variables' means and variances in the reweighted control group match the treatment group (balanced sample). All variables are defined in detail in the Appendix.

**Table 4 Facilitators: Elapsed Time**

| **Dynamic Treatment Effects** | | |
|---|---|---|
| Dependent Variable | $\boldsymbol{cash\ ETR}$ | |
| Embargo Period | Yes $[t_{-2}; t_5]$ | |
| Entropy Balancing | Balanced Sample | |
| (#) of Specification | Coefficient | (SE) |
| (1) $treated * post\ [t_1]$ | - 0.0415 | (0.0347) |
| (2) $treated * post\ [t_1; t_2]$ | - 0.0569** | (0.0277) |
| (3) $treated * post\ [t_1; t_3]$ | - 0.0461* | (0.0242) |
| (4) $treated * post\ [t_1; t_4]$ | - 0.0495** | (0.0243) |
| (5) $treated * post\ [t_1; t_5]$ | - 0.0486** | (0.0237) |
| Controls | Partner & Network & Firm (Annual Levels) & Treated | |
| Fixed Effects | Industry & Year & Embargo Period | |
| SE | Cluster @ Firm | |
| N | 1120; 1363; 1579; 1770; 1945 | |
| Adjusted $R^2$ | 0.0598; 0.0689; 0.0701; 0.0662; 0.0686 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. This table depicts the results for five specifications of equation (5) in which dynamic treatment effects are considered by binning posttreatment periods. The posttreatment period is extended by one year for each specification (from $post = 1\ for\ [t_1]$ to $post = 1\ for\ [t_1; t_5]$). $post$ equals 0 for $t_{-2}$ to $t_0$ throughout all specifications. The coefficient estimates of $treatment * post$ comprise the cumulative adjustment (i.e., effect of elapsed time) of a high-tax firm's tax planning behavior with progressing time when cooperating with low-tax firms. All variables are defined in detail in the Appendix.

**Table 5 Facilitators: Distance, Industry, Audit Firm**

**Panel A Distance**

| Dependent Variable | $cash\ ETR3$ | | $change\ cash\ ETR3$ | |
|---|---|---|---|---|
| | Coefficient | (SE) | Coefficient | (SE) |
| $hightolow$ | - 0.0272* | (0.0164) | - 0.0350** | (0.0174) |
| $SameBEARegion$ | - 0.0144 | (0.0211) | - 0.0369* | (0.0220) |
| $hightolow * SameBEARegion$ | - 0.0021 | (0.0370) | - 0.0466 | (0.0516) |
| Controls | Partner & Network & Firm | | Partner & Network & Firm | |
| Fixed Effects | Industry & Year | | Industry & Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 828 | | 828 | |
| Adjusted $R^2$ | 0.1221 | | 0.1267 | |

**Panel B Industry**

| Dependent Variable | $cash\ ETR3$ | | $change\ cash\ ETR3$ | |
|---|---|---|---|---|
| | Coefficient | (SE) | Coefficient | (SE) |
| $hightolow$ | - 0.0414** | (0.0184) | - 0.0436** | (0.0216) |
| $SameInd$ | - 0.0200 | (0.0163) | - 0.0160 | (0.0178) |
| $hightolow * SameInd$ | 0.0317 | (0.0321) | 0.0007 | (0.0343) |
| Controls | Partner & Network & Firm | | Partner & Network & Firm | |
| Fixed Effects | Year | | Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 828 | | 828 | |
| Adjusted $R^2$ | 0.1272 | | 0.1188 | |

**Panel C Audit Firm**

| Dependent Variable | $cash\ ETR3$ | | $change\ cash\ ETR3$ | |
|---|---|---|---|---|
| | Coefficient | (SE) | Coefficient | (SE) |
| $hightolow$ | - 0.0356** | (0.0167) | - 0.0555*** | (0.0199) |
| $SameAuditor$ | - 0.0146 | (0.0169) | 0.0029 | (0.0184) |
| $hightolow * SameAuditor$ | 0.0370 | (0.0340) | 0.0437 | (0.0541) |
| Controls | Partner & Network & Firm | | Partner & Network & Firm | |
| Fixed Effects | Industry & Year | | Industry & Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 828 | | 828 | |
| Adjusted $R^2$ | 0.1224 | | 0.1222 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. In Panel A, we test for the impact of geographical distance between the headquarters of cooperating firms. Distance is measured by an indicator variable that is set equal to one if network partners are headquartered in the same region, as defined by the Bureau of Economic Analysis, at network initiation. Panel B (Panel C) follows the approach of Panel A for the industry affiliation (shared audit firm) of network partners. For specification with $change\ cash\ ETR3$ as the dependent variable, first differences of continuous $firm\ controls$ are employed. All variables are defined in detail in the Appendix.

**Table 6 Additional Analyses: Effects on Reporting of Operations**

**Textual Sentiment of 10-K Filings & Tax Haven Operations**

| Dependent Variable | ***Sentiment*** | | ***Use of Negative Words*** | | ***Use of Tax Haven*** | | ***Delaware Strategy*** | |
|---|---|---|---|---|---|---|---|---|
| Embargo Period | Yes $[t_{-2}; t_5]$ | | Yes $[t_{-2}; t_5]$ | | Yes $[t_{-2}; t_5]$ | | Yes $[t_{-2}; t_5]$ | |
| Entropy Balancing | - | | - | | - | | - | |
| | Coefficient | (SE) | Coefficient | (SE) | Coefficient | (SE) | Coefficient | (SE) |
| *treated* | 0.0005 | (0.0007) | - 0.0001 | (0.0006) | - 0.0079 | (0.0615) | - 0.0137 | (0.0770) |
| ***treated * post*** | **- 0.0013**** | **(0.0006)** | **0.0012***** | **(0.0005)** | **- 0.0521** | **(0.0513)** | **0.0460** | **(0.0655)** |
| Controls | Partner & Network & Firm (Annual Levels) | | Partner & Network & Firm (Annual Levels) | | Partner & Network & Firm (Annual Levels) | | Partner & Network & Firm (Annual Levels) | |
| Fixed Effects | Industry & Year & Embargo Period | | Industry & Year & Embargo Period | | Industry & Year & Embargo Period | | Industry & Year & Embargo Period | |
| SE | Cluster @ Firm | | Cluster @ Firm | | Cluster @ Firm | | Cluster @ Firm | |
| N | 1641 | | 1641 | | 1712 | | 1313 | |
| Adjusted $R^2$ | 0.1848 | | 0.2464 | | 0.2790 | | 0.2949 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. Table 4 depicts the results for equation (5) with measures of textual sentiment of 10-K filings (*Sentiment*, *Use of Negative Words*) and Tax Haven Operations (*Use of Tax Haven, Num of Tax Haven Subsidiaries*) from Exhibit 21 disclosures as dependent variables. Data on textual sentiment (Exhibit 21 disclosures) are shared publicly by Bill McDonald (Scott Dyreng). All variables are defined in detail in the Appendix.

# Tax Knowledge Diffusion via Strategic Alliances

## Online Supplement

August 2021

# 1. Robustness Checks

## 1.1 FALSIFICATION TESTS

### *1.1.1 Low-Tax Firms*

**Table OS 1**

**Effect on low-tax Firms**

| Dependent Variable | $\boldsymbol{cash\ ETR3}$ | | $\boldsymbol{change\ cash\ ETR3}$ | |
|---|---|---|---|---|
| | Coefficient | (SE) | Coefficient | (SE) |
| $\boldsymbol{lowtohigh}$ | **- 0.0227** | **(0.0205)** | **- 0.0044** | **(0.0180)** |
| $Proximity$ | - 0.0013 | (0.0095) | 0.0055 | (0.0082) |
| $SameAuditor$ | - 0.0065 | (0.0209) | - 0.0157 | (0.0169) |
| Network Controls | Yes | | Yes | |
| Firm Controls | Yes | | Yes | |
| Fixed Effects | Industry & Year | | Industry & Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 261 | | 261 | |
| Adjusted $R^2$ | 0.0864 | | 0.1449 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests.

Our main analysis identifies tax knowledge diffusion for high-tax firms via low-tax networks. We do not consider the tax positions of low-tax firms because there is little reason to expect incremental tax knowledge diffusion/negative tax effects for low-tax firms bound to high-tax firms. To empirically control for this notion, we construct $lowtohigh$, which is an indicator that equals one for low-tax firms in networks with high-tax firms and zero for low-tax firms in low-tax networks. The results for this specification are presented in Table OS 1. The coefficient estimates (Table OS 1) for $lowtohigh$ are (i) beyond common levels of significance and (ii) economically not meaningful different from zero.

### *1.1.2 Placebo $hightolow$*

**Table OS 2**

**1000 Placebo Tests**

| Dependent Variable | $\boldsymbol{cash\ ETR3}$ | | | $\boldsymbol{change\ cash\ ETR3}$ | | |
|---|---|---|---|---|---|---|
| | Coefficient | (SE) | [p-value] | Coefficient | (SE) | [p-value] |
| Average Coefficient | 0.0009 | | | 0.0011 | | |
| Average SE | | (0.0175) | | | (0.0190) | |
| Average p-value | | | [0.5000] | | | [0.4798] |
| Controls | Partner & Network & Firm | | | Partner & Network & Firm (First Differences) | | |
| Fixed Effects | Industry & Year | | | Industry & Year | | |
| SE | Cluster @ Firm | | | Cluster @ Firm | | |
| N | 1000 x 627 | | | 1000 x 627 | | |

Table OS 2 depicts results of placebo tests. We create the indicator variable $placebo\text{-}hightolow$, which is 1000 times randomly assigned among observations of $hightolow = 0$ but within the initial propotions of $hightolow$. We then estimate equation (4) with $(change)$ $cash\ ETR3$ as the dependent variables. The averages of the coefficient estimates are economically marginally different from zero with large standard errors and the average p-values are beyond common levels of significance but match (approach) 0.5. A full display of the estimated coefficients is available at the GitHub repository.

## 1.2 TWO WAY FIXED EFFECTS

**Table OS 3**

**Interaction Weighted Two Way Fixed Effects**

| Dependent Variable | ***cash ETR*** | | ***cash ETR*** | |
|---|---|---|---|---|
| Specification | bins pre & post | | fully dynamic post | |
| | Coefficient | (SE) | Coefficient | (SE) |
| $pretreatment\ bin\ [t_{-2};t_{-1}]$ | 0.0176 | (0.0193) | 0.0178 | (0.0193) |
| $baseline\ period\ t_0$ | 0.0000 | - | 0.0000 | - |
| $posttreatment\ bin\ [t_1;t_5]$ | - 0.0259* | (0.0142) | | |
| $post\ t_1$ | | | - 0.0239 | (0.0174) |
| $post\ t_2$ | | | - 0.0340* | (0.0190) |
| $post\ t_3$ | | | - 0.0321* | (0.0194) |
| $post\ t_4$ | | | - 0.0306 | (0.0231) |
| $post\ t_5$ | | | - 0.0082 | (0.0266) |
| Firm Controls | Yes (Annual Measures) | | Yes (Annual Measures) | |
| Fixed Effects | Firm & Year | | Firm & Year | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 6911 | | 6911 | |
| Adjusted $R^2$ | 0.2015 | | 0.2000 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests.

In Table OS 3 we implement an interaction weighted two way fixed effects estimator under consideration of the recent advances by Sun and Abraham [2020]. The authors show that estimates of relative time indicators in event studies (i.e., settings with variation in treatment timing across units) can be contaminated when heterogenous treatment effects are present. We follow Sun and Abraham and generate relative time indicators to our treatment variable $hightolow = 1$. We treat our control group ($hightolow = 0$) as never treated units. We leave out distant leads and lags of the treated units to create pretreatment and posttreatment periods ($[t_{-2}; t_5]$ with treatment in $t_1$). We bin observations in the pretreatment and posttreatment periods relative to the last relative year before the treatment occurs ($t_0$ as baseline) in specification (1). In specification (2), we employ a fully dynamic posttreatment period. We then estimate a linear model with firm $\alpha_i$ and time $\lambda_t$ fixed effects and include interaction weighted relative time periods $\delta_{e,l}$ to the treatment using `eventstudyinteract` by Sun (https://github.com/lsun20/eventstudyinteract). We include annual levels of $firm\ controls$ and cluster robust standard errors at the firm level:

$$cash\ ETR_{i,t} = \alpha_i + \lambda_t + \sum_l \delta_{e,l} * treated_{it}^l + \sum_k \beta_k\, firm\ controls_{i,t}^k + \varepsilon_{i,t} \qquad \text{(OS 2)}$$

The results of estimating equation (OS 2) suggest that our analyses are robust to potential contamination of two way fixed effects estimators. The coefficient estimates and standard errors show that the posttreatment (pretreatment) bin is (not) statistically significantly different from the baseline period $t_0$. Furthermore, the estimates from the second specification underline the importance of elapsed time for tax knowledge diffusion to occur. We interpret the results to corroborate our identification strategy and main analysis.

## 1.3 ALTERNATIVE IDENTIFICATION STRATEGY

**Table OS 4**

**Interaction of Continuous Variables**

| Dependent Variable | $\boldsymbol{cash\ ETR3}$ | |
|---|---|---|
| | Coefficient | (SE) |
| $pre\ cash\ ETR3$ | 0.3952*** | (0.0700) |
| $adj.partner\ pre\ cash\ ETR3$ | - 0.0953 | (0.0690) |
| $\boldsymbol{pre\ cash\ ETR3 * adj.partner\ pre\ cash\ ETR3}$ | **0.7744**** | **(0.3588)** |
| $Proximity$ | - 0.0066 | (0.0049) |
| $SameAuditor$ | - 0.0095 | (0.0126) |
| Network Controls | Yes | |
| Firm Controls | Yes | |
| Fixed Effects | Industry | |
| SE | Cluster @ Firm | |
| N | 1092 | |
| Adjusted $R^2$ | 0.1686 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. To alleviate potential concerns about our identification strategy, we apply a modified identification strategy to check our strategy's robustness. We enter high-tax and low-tax firms in a specification of equation (4) and regress $cash\ ETR3$ on firms' own $pre\ cash\ ETR3$, on the industry-year-mean adjusted $partner\ pre\ cash\ ETR3$, and on an interaction term of the two independent variables:

$$cash\ ETR3_{i,t=1} = \beta_0 + \beta_1\ pre\ cash\ ETR3_{i,t=0} + \beta_2\ adj.partner\ pre\ cash\ ETR3_{i,t=0}$$
$$+ \boldsymbol{\beta_3\ pre\ cash\ ETR3 * adj.partner\ pre\ cash\ ETR3}$$
$$+ \sum_n \beta_n\ network\ controls^n_{i,t=1} + \sum_n \beta_l\ partner\ controls^l_{i,t=1} \quad \text{(OS 1)}$$
$$+ \sum_k \beta_k\ firm\ controls^k_{i,t=1} + \delta_{ind} + \varepsilon_{i,t}$$

The results suggest that our identification strategy and our results are not sensitive to the classification of firms based on quartile-bins when classifying $hightolow$. Table OS 1 further indicates that the effects for low-tax firms are negligible.

## 1.4 ALTERNATIVE TAX KNOWLEDGE MEASURE

**Table OS 5**

**Panel A DiD: CTD**

| Dependent Variable | $\boldsymbol{CTD}$ | |
|---|---|---|
| Embargo Period | Yes [t-2; t5] | |
| Entropy Balancing | Balanced Sample | |
| | Coefficient | (SE) |
| $treated$ | 0.0034 | (0.0022) |
| $\boldsymbol{treated * post}$ | **- 0.0067**** | **(0.0026)** |
| Controls | Partner & Network & Firm (Annual Levels) | |
| Fixed Effects | Industry & Year & Embargo Period | |
| SE | Cluster @ Firm | |
| N | 2063 | |
| Adjusted $R^2$ | 0.3261 | |

**Panel B Baseline Regression: change CTD3**

| Dependent Variable | $\boldsymbol{change\ CTD3}$ | |
|---|---|---|
| | Coefficient | (SE) |
| $\boldsymbol{hightolow}$ | **- 0.0038**** | **(0.0018)** |
| $Proximity$ | - 0.0010 | (0.0007) |
| $SameAuditor$ | 0.0004 | (0.0014) |
| Controls | Network & Firm (First Differences) | |
| Fixed Effects | Industry & Year | |
| SE | Cluster @ Firm | |
| N | 726 | |
| Adjusted $R^2$ | 0.1870 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. We apply the measure ($CTD$) developed by Henry and Sansing [2018] to identify whether high-tax firms become rather tax-favored relative to the control observations. The results of these tests support the identified effects in the main analysis.

## 1.5 ALTERNATIVE ENTROPY WEIGHTS

**Table OS 6**

**Difference in Differences: Alternative Entropy Weights**

| Dependent Variable | $\boldsymbol{cash\ ETR}$ | | $\boldsymbol{cash\ ETR}$ | |
|---|---|---|---|---|
| Embargo Period | Yes [$t_{-2}$; $t_5$] | | Yes [$t_{-2}$; $t_5$] | |
| Alternative Entropy Weights | Treatment Period [$t_1$] | | Pretreatment Average [$t_{-2}$; $t_0$] | |
| | Coefficient | (SE) | Coefficient | (SE) |
| $treated$ | 0.0075 | (0.0167) | 0.0048 | (0.0168) |
| $\boldsymbol{treated * post}$ | **- 0.0428*** | **(0.0227)** | **- 0.0405*** | **(0.0225)** |
| Controls | Partner & Network & Firm (Annual Levels) | | Partner & Network & Firm (Annual Levels) | |
| Fixed Effects | Industry & Year & Embargo Period | | Industry & Year & Embargo Period | |
| SE | Cluster @ Firm | | Cluster @ Firm | |
| N | 1945 | | 1945 | |
| Adjusted $R^2$ | 0.0661 | | 0.0602 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests. In our main analysis we balance covariates on two moments over the full embargo period. We check the robustness of this approach by balancing the covariates (i) in the treatment period $t_1$ (e.g., see Gallemore et al. [2019]), and (ii) on the firm-average of the first momentum in the pretreatment period. Our results remain statistically and economically robust.

## 1.6 ALTERNATIVE EXPLANATIONS

**Table OS 7**

**Panel A Exclude Nonsurvivors**

| Dependent Variable | $\boldsymbol{change\ cash\ ETR3}$ | |
|---|---|---|
| Specification | exclude nonsurvivors | |
| | Coefficient | (SE) |
| $\boldsymbol{hightolow}$ | **- 0.0518*** ** | **(0.0181)** |
| $Proximity$ | - 0.0086 | (0.0077) |
| $SameAuditor$ | 0.0146 | (0.0205) |
| Network Controls | Yes | |
| Firm Controls | First Differences | |
| Fixed Effects | Industry & Year | |
| SE | Cluster @ Firm | |
| N | 660 | |
| Adjusted $R^2$ | 0.1914 | |

**Panel B Effect on Profitability**

| Dependent Variable | $\boldsymbol{change\ EBITDA/AT3}$ | |
|---|---|---|
| Specification | First Difference ROA | |
| | Coefficient | (SE) |
| $\boldsymbol{hightolow}$ | **0.0044** | **(0.0051)** |
| $Proximity$ | 0.0007 | (0.0018) |
| $SameAuditor$ | 0.0019 | (0.0042) |
| Network Controls | Yes | |
| Firm Controls | First Differences | |
| Fixed Effects | Industry & Year | |
| SE | Cluster @ Firm | |
| N | 828 | |
| Adjusted $R^2$ | 0.4051 | |

Superscripts ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively, for two-tailed tests.

In Panel A of Table OS 7 we exclude firm-edge-years from the analysis (i.e., firms with network-firm observations within the last three years of their presence in our panel) because prior research indicates that strategic alliances can serve as preliminary ties between successive acquirers and targets (Ishii and Xuan [2014], Porrini [2004]). This approach also ensures that variation in $cash\ ETR3$ does not stem from the substitution with $cash\ ETR$ for the final and penultimate firm-year of a firm in our panel.

Panel B of Table OS 7 depicts results for estimating equation (4) with the first difference of ROA ($change\ EBITDA/AT3$) as dependent variable. The estimate for $hightolow$ is beyond common levels of statistical significance and economically small. This strenghtens our main result as a tax effect rather than an effect on profitability.

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