

Does Information Asymmetry Affect Corporate Tax Aggressiveness?

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Abstract

We investigate the effect of information asymmetry on corporate tax avoidance. Using a difference-in-differences matching estimator to assess the effects of changes in analyst coverage caused by broker closures and mergers, we find that firms avoid tax more aggressively after a reduction in analyst coverage. We further find that this effect is mainly driven by firms with higher existing tax planning capacity (e.g., tax haven presence), smaller initial analyst coverage, and a smaller number of peer firms. Moreover, the effect is more pronounced in industries where reputation matters more, and in firms subject to less monitoring from tax authorities.

JEL classification: G24; G32; G30

Keywords: Information asymmetry; Analyst coverage; Corporate tax aggressiveness; Natural experiment; Broker closures and mergers

I. Introduction

What factors affect corporate tax avoidance?¹ This question is drawing increasing attention from both academics and policy makers around the world. Between 1998 and 2005, 30.5% of large U.S. firms reported zero tax liability (U.S. General Accounting Office (2008)), and the U.S. Internal Revenue Service (IRS (2012)) reports that the official tax compliance rate is estimated to be only 83.1%. A 2013 special report in the *Economist* estimated that the amount of money stashed away in tax havens might be above \$20 trillion.² The recent “Panama Papers” revelations have also stimulated public attention and interest in the use of offshore tax havens for tax evasion.³ Despite their obvious importance to both academics and policy makers, the factors that have first-order effects in driving or reducing corporate tax avoidance have, until recently, received limited attention in finance and accounting research. In their survey of the literature, Hanlon and Heitzman ((2010), p. 145) conclude that “the field cannot explain the variation in tax avoidance very well,” and they call for more research.

Recently public media and lawmakers have called for greater transparency from

¹ Following Hanlon and Heitzman (2010), we define tax avoidance broadly as the reduction of explicit cash taxes, which includes all types of transactions from investing in a municipal bond to using tax shelters.

² See “Tax Havens: The Missing \$20 Trillion,” *The Economist* (Feb. 16, 2013).

³ See “What Are the Panama Papers? A Guide to History’s Biggest Data Leak,” *The Guardian* (Apr. 5, 2016); “Panama Papers’ Puts Spotlight on Boom in Offshore Services,” *The Wall Street Journal* (Apr. 6, 2016).

companies to help reduce tax avoidance.⁴ Although recent studies have linked tax avoidance to various factors,⁵ few studies have examined the causal effect of information asymmetry on tax avoidance. This gap is surprising, as managers clearly face conflicts between financial reporting quality and tax planning (e.g., Scholes and Wolfson (1992)).⁶ The paucity of research might be partially driven by potential endogeneity concerns. The primary source of endogeneity is reverse causality, as seen in the inconsistent conclusions of previous studies. Using cross-country data, Kerr (2012) finds that information asymmetry leads to tax avoidance. In contrast, several other studies find that aggressive tax planning affects earnings quality and information asymmetry (e.g., Hanlon (2005), Ayers, Jiang, and Laplante (2009), Comprix, Graham, and Moore (2011), and Balakrishnan, Blouin, and Guay (2012)). Therefore, the question of whether the information environment affects or is affected by tax avoidance is under debate, as the direction of the causality between these two constructs is unclear. Furthermore, unobservable factors could be correlated

⁴ See "U.K. has Unwinnable Battle on Tax Avoidance, Panel Says," *Bloomberg* (Apr. 26, 2013).

⁵ These factors include firm characteristics (e.g., Wilson (2009), Lisowsky, Robinson, and Schmidt (2013)), corporate governance (e.g., Desai and Dharmapala (2006)), ownership structure (e.g., Chen, Chen, Cheng, and Shevlin (2010), McGuire, Wang, and Wilson (2014)), executives (e.g., Dyreng, Hanlon, and Maydew (2010)), cultural norms (DeBacker, Heim, and Tran (2015)), and labor unions (Chyz, Leung, Li, and Rui (2013)), etc.

⁶ On the one hand, managers desire to report low levels of earnings to tax authorities. On the other hand, they desire to report higher levels of earnings to investors. Therefore, firms often face a trade-off between tax avoidance and lower reported earnings (e.g., Matsunaga, Shevlin, and Shores (1992), Maydew (1997)).

with both information asymmetry and tax avoidance at the same time. In this study, we use analyst coverage as a proxy for information transparency between firms and their investors. Specifically, we rely on two natural experiments, brokerage closures and brokerage mergers, which generate exogenous variations in analyst coverage, to examine the effect of information on tax avoidance. Both brokerage closures and brokerage mergers directly affect firms' analyst coverage, but are not directly related to individual firms' corporate decisions and policies.⁷

The change in the information environment caused by a reduction in analyst coverage could affect corporate tax avoidance in the following ways. First, financial analysts care about corporate tax policies, as a firm's tax shield is associated with capital budgeting, cost of capital, and eventually firm valuation. Analysts also analyze firms' tax planning strategies, effective tax rates, and abnormal tax patterns, and distribute both public and private information to institutional and retail investors and various other information users through research reports and media outlets such as newspapers and TV programs. In the process, analysts assess the tax risks that firms take and distribute this information to firms' investors; this scrutiny discourages firms from using risky tax strategies. Ultimately, if firms take on excessive tax risk, it is the investors who bear the cost when the government imposes penalties and extracts taxes. Therefore, a drop in analyst coverage could lead to an increase in tax avoidance.

Anecdotal evidence suggests that analysts pay attention to risky tax strategies. For

⁷ These settings have been used in recent studies, such as Kelly and Ljungqvist (2012), Derrien and Kecskes (2013), and He and Tian (2013).

example, in a 2012 article in the Daily Telegraph, Bruce Packard, an analyst at Seymour Pierce, said “Barclays risks ‘a fierce customer backlash’ if it does not reduce its exposure to offshore tax havens or limit legitimate tax avoidance.”⁸ We further verify analysts’ interest in firms’ tax avoidance activities by conducting a content analysis of 6,010,450 analyst reports downloaded from Investext database. This sample includes all of the reports in the database for all firms in the 2007–2013 period. We implement keyword searches related to tax planning, and find that on average 12.46% of the universal analyst reports are concerned with firms’ tax avoidance activities, indicating that analysts pay substantial attention to the tax policies of the firms they cover.⁹

Second, analysts are well trained in finance and accounting, with substantial background knowledge of the industry. They track firms on a regular basis and are therefore able to identify potential irregularities in tax planning in a timely manner. As

⁸ See “Barclays ‘Risks Backlash’ Unless Its Tax Affairs Are Simplified,” *The Daily Telegraph* (Jan. 4, 2012).

⁹ Specifically, we include the following keywords: tax* avoid*, avoid* tax*, tax* avoidance, evad*, evas*, tax*, tax* shelter*, effective tax rate, and tax* aggress*. We also read through some of the reports and confirm that analysts are indeed expressing interest in tax avoidance. For example, in a report on Affordable Residential Communities Inc (ARC) issued by Wachovia Securities on Mar. 7, 2006, Stephen Swett concluded that “ARC was giving up its real estate investment trust status to escape from tax penalties.” In their Aug. 20, 2009 report on Northrop Grumman (NOC), Joseph F. Campbell, Harry Breach, and Carter Copeland from Barclays Capital pointed out that the main choice for the divestiture of The Analytical Science Corporation (TASC) is whether to sell and pay taxes, or spin and avoid taxes by using a reverse Morris trust.

active information intermediaries, analysts disseminate information about a firm throughout the financial market, and the dissemination of information about tax aggressiveness to the general public could tarnish a firm's reputation.¹⁰ Evidence from the field indicates that corporate executives rate reputation concerns and the risk of adverse media attention as important or very important factors in their decisions to not engage in aggressive tax planning; these two factors are also more important for firms with more analyst following (Graham, Hanlon, Shevlin, and Shroff (2014)). In our analysis, we find that the effect of analyst coverage is more pronounced in firms in consumer-oriented industries where customers' perception is more important. Therefore, a reduction in analyst coverage increases information asymmetry and thus decreases the reputation concern related to tax avoidance, leading firms to avoid tax more aggressively.¹¹

Third, one strand of research on tax avoidance suggests that the complexity and obfuscatory nature of tax avoidance requires opacity (e.g., Desai and Dharmapala (2006), (2009b), Kim, Li, and Zhang (2011)). Therefore, as a reduction in analyst coverage leads to higher levels of information opacity, it also gives firms more opportunities for aggressive

¹⁰ Hanlon and Slemrod (2009) argue that firms risk being labeled bad corporate "citizens" if the general public is aware of their tax aggressiveness, and they find negative market reactions for firms detected in tax sheltering.

¹¹ Also, there is a view that analysts do not really understand complicated tax issues (e.g., Plumlee (2003), Hoopes (2014)). If analysts indeed fail to clearly understand tax issues, there is a possibility that analyst coverage will have little effect on corporate tax avoidance. In response, this study directly examines whether analyst coverage affects firms' incentives to engage in tax avoidance and avoidance activities.

tax avoidance. In addition, a drop in analyst coverage results in less competition among analysts (Hong and Kacperczyk (2010)), which might further reduce the incentives of the remaining analysts to produce more in-depth information. The weakened information production incentives of financial analysts further increase information asymmetry, and consequently increase managers' ex-ante incentives for aggressive tax planning.

In sum, financial analysts have both the abilities and incentives to produce and distribute tax-related information and hence reduce information asymmetry between the firms they cover and their investors. This reduction in information asymmetry might make it more difficult for a firm to hide earnings through tax sheltering or complicated financial structures, as the transaction costs for tax avoidance will tend to be higher. The increased likelihood of being detected also raises firms' concerns about reputation and financing costs.¹² Therefore, more intensive analyst coverage could deter corporate tax avoidance. Note that we are not arguing that analysts are pushing firms to pay more taxes, and we do not take a stance on whether tax avoidance increases or decreases firm value.¹³ Our study

¹² For instance, Shevlin, Urcan, and Vasvari (2013) find that tax avoidance increases the cost of public debt, whereas Hasan, Hoi, Wu, and Zhang (2014) show that tax avoidance increases the cost of bank loans.

¹³ In theory, firms are expected to maximize shareholders' value by reducing tax liabilities, as long as the incremental benefit exceeds the incremental cost as it reduces tax burdens and minimizes cash outflows. The empirical literature has mixed findings. Armstrong, Blouin, and Larcker (2012) find that tax managers are compensated to reduce ETRs. According to the agency view of tax avoidance, however, the cost will eventually outweigh the benefit of tax avoidance (Desai, Dyck, and Zingales (2007)). Graham et al. (2014) also provide evidence suggesting that firms benefit from

suggests that because analysts distribute their findings on firms' tax structure to the investors through analyst report and mass media, analyst coverage increases the cost of tax avoidance¹⁴ and reduces managers' incentives to aggressively avoid taxes.

To test this hypothesis, we use a comprehensive set of 9 measures of tax avoidance drawn from the literature (e.g., Hanlon and Heitzman (2010)).¹⁵ We run simple regressions of our measures of tax avoidance on the number of analysts following a firm and a battery of control variables and find a negative correlation between analyst coverage and tax avoidance. We then strive to establish the causal effect of information asymmetry on tax avoidance by using two natural experiments. Using both Abadie and Imbens' (2006) matching and propensity score matching strategies, our difference-in-differences (DID) estimation results indicate that our measures of tax avoidance significantly increase after a firm loses an analyst, compared to similar firms that do not experience a reduction in analyst coverage. These results are robust to various alternative matching criteria. Looking at the economic magnitude, treated firms' CETR drops by 0.9 percentage points relative to the control firms that do not experience a reduction in analyst coverage. The evidence therefore suggests a strong negative effect of analyst coverage on tax avoidance.

avoiding taxes, but if tax avoidance hurts the firm's overall reputation among customers, such firms are more cautious in their tax avoidance behavior.

¹⁴ As analyzed above, the cost includes both direct cost and indirect cost. Direct costs are the risk of being detected by tax authorities, and the indirect costs include reputation costs and financing costs.

¹⁵ The measures include 5 measures of book-tax difference, 1 measure of tax sheltering, and 3 measures of effective tax rates. Out of these 9 measures, we use 5 in the main test, and use the other 4 for robustness checks.

We further examine the factors that influence the link between analyst coverage and corporate tax avoidance to explore the underlying economic channels. Our findings of a strong effect of analyst coverage on tax avoidance rely on one important underlying assumption: managers have the ability to change tax planning quickly. Many firms with unused tax planning capacity may not engage in aggressive tax planning if information transparency is high. In such cases, an increase in information asymmetry due to broker closures and mergers might provide managers with the opportunity to incrementally use a firm's existing tax planning capacity; in this situation, we expect that firms would avoid tax more aggressively when there is a decrease in analyst coverage. As expected, we find that our results are only significant for multinational firms and firms with more segments, which can rapidly adopt more aggressive tax planning activities.

We also consider a specific form of existing tax planning capacity: tax haven presence. Following Dyreng and Lindsey (2009), we collect this information from the subsidiary disclosure in Exhibit 21 of firms' annual reports. Consistent with our expectation, we find that a significant effect of analyst coverage on tax avoidance only exists in the subset of firms with tax haven presence, firms with a greater number of tax haven subsidiaries or subsidiaries in more tax haven countries.

We then look at firms' initial level of analyst coverage. As expected, we find that a significant increase in tax avoidance only exists in the subsample of firms with low initial analyst coverage. This finding further strengthens our main hypothesis that information asymmetry materially affects corporate tax avoidance activities. We also find that the strong effects of analyst coverage on tax avoidance are mostly concentrated in the

consumer-oriented industries, as customer perception of a firm is more important in these industries (see Hanlon and Slemrod (2009) and Graham et al. (2014)). Specifically, Graham et al. (2014) provide evidence that if tax avoidance hurts a firm's overall reputation among customers, it will be more cautious in its tax avoidance behavior. We also find that the effect of a reduction in analyst coverage on tax avoidance is more pronounced for firms with a smaller number of peer firms and when tax authority monitoring is low.

Our study contributes to several strands of research. Primarily, our study adds to the tax avoidance literature (e.g., Johnson, Kaufmann, and Zoido-Lobaton (1998), Johnson Kaufmann, McMillan, and Woodruff (2000), Crocker and Slemrod (2005), Desai and Dharmapala (2006), (2009a), Hanlon and Heitzman (2010), and Beck, Lin, and Ma (2014)) by investigating a new potential factor in firms' tax avoidance activities. Due to the renewed intellectual and policy interest in tax avoidance from governments, media and academics, it is important to empirically identify the factors that influence tax avoidance. Our study is among the first to document that information asymmetry affects firms' incentives to engage in tax avoidance activities. In a recent study, Hanlon, Maydew, and Thornock (2015) find that country-level information-sharing agreements with tax havens decrease the use of tax havens at the individual investor level. Our study finds that firm-level information transparency decreases corporate tax avoidance. In this regard, our study also contributes to the broader literature of tax avoidance.

Our study also adds to the literature on the role played by financial analysts in corporate policies (e.g., Derrien and Kecskes (2013), He and Tian (2013), and Chen, Harford, and Lin (2015)). We show that a decrease in analyst coverage caused by broker

mergers and closures lead to more tax avoidance. In a parallel study, Allen, Francis, Wu, and Zhao (2016) examine the effect of analyst coverage on tax avoidance. We differ with their study in two main aspects. First, our study uses a more comprehensive set of measures of tax avoidance (5 measures of book-tax difference, 1 measure of tax sheltering, and 3 measures of effective tax rates) along with many alternative matching criteria, whereas their study uses effective tax rates as their main measure. Second, we explore in detail the economic channels through which information asymmetry affects tax avoidance by looking at existing tax planning capacity (e.g., tax haven presence), providing direct evidence for the information production incentives of the analysts, and exploring industry heterogeneity (i.e., differences between consumer-oriented industries and other industries).

The remainder of the study is organized as follows: Section II presents the construction of our sample and summary statistics. Section III describes our identification strategy and presents the DID results. Section IV conducts further explorations of tax avoidance. Section V provides additional robustness tests, and our conclusions are presented in Section VI.

II. Sample Construction and Summary Statistics

This section describes the construction of our sample and presents the summary statistics for the major variables used in the study.

A. Sample Selection

To construct our sample, we first extract financial and accounting data from Compustat's

North America Fundamentals Annual database for the 1999–2011 period. We choose listed U.S. firms that are not financials or utilities, and that have the Center for Research in Security Prices (CRSP) data. We eliminate firm-year observations for which information on total assets is not available, and we also exclude observations with negative cash holdings, sales, or total assets. Analyst coverage data are obtained from the Institutional Brokers' Estimate System (IBES) database. We collect information about broker closures from IBES, Factiva, and Kelly and Ljungqvist (2012) and information about broker mergers from the SDC M&A database.

B. Measuring Tax Avoidance

Following the literature, we adopt 5 main measures of tax avoidance: total book-tax difference (BTD), Desai and Dharmapala's (2006) residual book-tax difference (DDBTD), SHELTER, DTAX, and cash effective tax rate (CETR). The total book-tax difference is the most commonly used measure of book-tax difference, calculated by book income less taxable income standardized by lagged assets. Previous studies argue that total book-tax gap does not necessarily reflect tax avoidance, and might partially capture earnings management activities (e.g., Phillips, Pincus, and Rego (2003), Hanlon (2005)). Thus, we follow Desai and Dharmapala (2006) and adjust book-tax difference for earnings management by using an accruals proxy to isolate the component of the difference that is due to earnings management (DDBTD). SHELTER measures an extreme form of tax avoidance, tax sheltering. We estimate the probability that a firm uses a tax shelter using Wilson's (2009) tax sheltering model. DTAX is based on the work of Frank, Lynch, and Rego (2009), who attempt to measure the discretionary portion of tax avoidance by removing

the underlying determinants of tax avoidance that are not driven by intentional tax avoidance. CETR is the cash effective tax rate based on Chen et al. (2010), calculated as cash taxes paid divided by pretax income. We use CETR as our main measure of effective tax rate as it is less affected by changes in tax accounting accruals.¹⁶

In the robustness tests, we use 4 additional measures of tax avoidance: Manzon and Plesko's (2002) book-tax difference (MPBTD), ETR differential (ETRDIFF), effective tax rate (ETR), and cash flow effective tax rate (CFETR). ETRDIFF is based on the measures given in Frank et al. (2009) and Kim et al. (2011). ETR is the effective tax rate based on Zimmerman (1983). Our final measure is CFETR, which is the cash flow effective tax rate, calculated as cash taxes paid divided by operating cash flows. This measure only uses information from the cash flow statements, which could further separate out the earnings management effect. All of the detailed definitions and calculations are reported in Appendix A.

C. Measuring Analyst Coverage and Other Control Variables

Analyst information is obtained from IBES, and our major variable is the natural log of the total number of analysts following the firm during the year. Following the tax avoidance literature (e.g., Chen et al. (2010)), we include a vector of firm characteristics that could affect corporate tax avoidance. These control variables include firm size (SIZE), Tobin's Q (Q), tangibility (PPE), foreign income (FI), leverage (LEV), ROA, NOL, Δ NOL, intangibility,

¹⁶ Note that we cannot use the long-term measure of tax effective rates (Dyreng, Hanlon, and Maydew (2008)) as we find that our exogenous events are temporary shocks to the firms and the effects disappear after 3 years, which is consistent with Derrien and Kecskes (2013). We discuss this in detail in the persistence test in Section V.B.

and EQINC. Detailed information about the variable definitions is provided in Appendix A.

D. Summary Statistics

After merging the tax avoidance data with analyst coverage, our final sample consists of 23,475 firm-year observations from 5,401 publicly traded U.S. firms covering the 1999–2011 period. We winsorize all of the parameters excluding the dummy variables at the 1st and 99th percentiles to minimize the effect of outliers. Table 1 reports the descriptive statistics on our measures of tax avoidance, analyst coverage, and other control variables for this full sample prior to matching. For example, we find that the mean (median) value of Desai and Dharmapala’s (2006) residual book-tax difference is 0.023 (0.004), and the mean (median) value of the cash effective tax rate is 0.270 (0.256). The measures have significant variations, as shown by their large standard deviations: 0.293 and 0.211, respectively.

[Table 1 about here]

III. Identification and Estimation Results

In untabulated results, we run regressions of our main measures of tax avoidance on the number of analysts following the firm and other firm-level control variables, and find that firms followed by a larger number of analysts practice less tax avoidance.¹⁷ A potential concern in the interpretation of this result is that analyst coverage is likely to be endogenous. Many studies have shown that analysts tend to cover higher quality firms

¹⁷ The regressions control for year and firm fixed effects, and standard errors are clustered at the firm level. The results are available from the authors.

(Chung and Jo (1996)) and firms with less information asymmetry (e.g., Lang and Lundholm (1996), Bhushan (1989)). Therefore, firms with certain levels of tax avoidance and information asymmetry may attract more analyst coverage. In addition, unobservable firm heterogeneity that is correlated with both analyst coverage and corporate decisions and policies could also bias the estimation results. In this section, we introduce our identification strategy and present our empirical tests by adopting a DID approach built on broker merger and closure events.

A. Natural Experiments

Our identification strategy uses two natural experiments. The first one is brokerage closure. As argued by Kelly and Ljungqvist (2012),¹⁸ broker closures provide an ideal source of exogenous shocks to analyst coverage as these closures are mostly driven by business strategy considerations of the brokers themselves and not correlated with firm-specific characteristics. Therefore, such closures should only affect a firm's tax avoidance through their effect on the number of analysts covering the firm. The second natural experiment is broker mergers, first adopted by Hong and Kacperczyk (2010) in their study of how competition affects earnings forecast bias. As documented by Wu and Zang (2009), when two brokerage firms merge, they typically fire analysts due to redundancy or cultural clashes. Consequently, broker mergers also provide exogenous variations in analyst coverage.

To identify broker closures, we use the IBES database to find a list of brokers who

¹⁸ Using closures as shocks to the supply of information, Kelly and Ljungqvist (2012) document the importance of information asymmetry in asset pricing.

disappear from the database between 2000 and 2010, and then search Factiva to confirm that the exit is due to closure. We also complement our sample with a list of brokerage closures provided by Kelly and Ljungqvist (2012). The final sample consists of 30 brokerage closures. The construction of the broker merger sample follows Hong and Kacperczyk (2010). We first collect broker merger events using Thomson Reuters SDC M&A database, by searching for both the acquirer and target primary Standard Industrial Classification (SIC) codes 6211 or 6282. We only consider completed deals and deals in which 100% of the target is acquired. Then we manually match all of the acquirers and targets with the names of brokerage houses in the IBES database. Our procedure produces 24 merger events.¹⁹ Together with the broker closure sample, our list of 54 broker exits is similar to those of Kelly and Ljungqvist (2012) and Hong and Kacperczyk (2010) combined.²⁰

To obtain a sample of affected firms, we merge our final sample of broker exits with the IBES unadjusted historical detail data set. For broker closures, we need the covered firms to remain in the IBES sample in year $t+1$. For broker mergers, we restrict the firms to those

¹⁹ We select only those mergers where both merging houses analyze at least two of the same firms (Hong and Kacperczyk (2010)). Note that Lehman is not in our sample, as it is not a suitable shock for identification purpose, as also pointed out by Kelly and Ljungqvist (2012), because Barclays, which had no U.S. equities business of its own, took over Lehman's entire U.S. research department. The data for Merrill Lynch and Bank of America are retrieved from data downloaded at an earlier date, as their observations have been dropped from the current IBES database.

²⁰ We examine whether our main results are driven by broker mergers or broker closures, and we find no qualitative difference between the 2 groups.

that are covered by both the acquiring and target houses before the merger and continue to be followed by the remaining broker after the merger. In addition, we choose listed U.S. firms that are not financials or utilities, and that have CRSP and Compustat data in years $t-1$ and $t+1$. Following recent studies (e.g., Derrien and Kecskes (2013)), we only keep the firm-year observations of $t-1$ and $t+1$ to ensure that we capture only the direct effects of the drop in analyst coverage. It is possible that in the long run the entry of other brokers will make up for the diminished research or that the terminated analyst could find a job in another brokerage house. Therefore, this setting enables us to make good use of short-term deviations from the equilibrium in our analysis of how analyst coverage affects tax avoidance.

B. Variable Description and Estimation Methodology

We match our sample of affected firms with our measures of tax avoidance. The final sample consists of 1,415 firm-years for 1,031 unique firms (associated with 47 broker exits) in the 1999–2011 period. Appendix B shows the number of broker exits and the corresponding number of affected firms each year from 2000 to 2010. We find that there is no obvious evidence of clustering in time and the exits are spread out evenly over the sample period. On average, treated firms experience a 0.96 reduction in the number of analysts. Firms in the top, median, and bottom quartiles all have precisely 1 analyst missing.

To investigate the effects of analyst coverage on corporate tax avoidance, we use a DID matching estimator approach to minimize the concern that the variations in analyst coverage and in tax avoidance are caused by cross-sectional or time-series factors that affect both analyst coverage and avoidance. We use two matching strategies: Abadie and

Imbens' (2006) matching and propensity score matching.

Abadie and Imbens' (2006) matching estimator simultaneously minimizes the Mahalanobis distance between a vector of observed matching covariates across treated and non-treated firms.²¹ The primary matching variables include firm size (SIZE), Tobin's Q (Q), tangibility (PPE), foreign income (FI), and analyst coverage (COV) prior to broker terminations. We also make sure that both the treatment firm and the control firm are in the same Fama–French (1997) 48 industry and the data are from the same fiscal year.

Alternatively, we adopt the nearest-neighbor logit propensity score matching strategy developed by Rosenbaum and Rubin (1983). The control pool is the remainder of the Compustat universe that has analyst coverage and valid matching variables. We construct a control sample of firms that are matched to the treated firms along a set of relevant firm characteristics measured in the year prior to the broker exits. First, we estimate a logit regression where the dependent variable equals 1 if a particular firm-year is classified as treated, and 0 otherwise; our matching variables are the independent variables. We use a panel of 1,415 treatment firm-years and the remainder of the Compustat universe of premerger firm-years with analyst coverage and valid matching variables. Second, the estimated coefficients are used to predict the propensity scores of treatment, which are then used to perform a nearest-neighbor match.

To measure the effect of the decrease in analyst coverage on tax avoidance, we compare, for each matching approach, the differences in tax avoidance for treated firm i

²¹ The Abadie and Imbens' (2006) matching estimator approach has been used by, among others, Campello, Graham, and Harvey (2010) and Campello and Giambona (2013).

($\Delta\text{TAX_AVOIDANCE}_i^{\text{TREATED}}$) between 1 year after the broker exit and 1 year prior to the exit, to the differences of its matched control firm ($\Delta\text{TAX_AVOIDANCE}_i^{\text{CONTROL}}$) for the same years. We then take the mean of the DID across all of the firms in our sample. Specifically, the average treatment effect of the treatment group (DID) is calculated as:

(1) DID(TAX_AVOIDANCE) =

$$\frac{1}{N} \sum_{i=1}^N \Delta\text{TAX_AVOIDANCE}_i^{\text{TREATED}} - \frac{1}{N} \sum_{i=1}^N \Delta\text{TAX_AVOIDANCE}_i^{\text{CONTROL}},$$

where N refers to the number of treatment and control firms.

C. Estimation Results

Table 2 presents the DID estimation results. Panel A reports the summary statistics for the matched samples prior to broker terminations. The balance test shows that the treatment firms and the control firms are similar across all of the matching variables in the pre-event year, ensuring that the change in tax avoidance is caused only by the drop in analyst coverage.

[Table 2 about here]

A key identifying assumption central to our DID estimation results is that treatment and control firms share parallel trends in tax avoidance prior to broker events. Following Kausar, Shroff, and White (2016), we conduct a parallel trend test, and the results shown in Panel B of Table 2 indicate that the pre-treatment trends in our measures of tax avoidance are indeed indistinguishable for both $t-1$ and $t-2$.

In Panel C of Table 2, we present the DID results using Abadie and Imbens' (2006) matching estimator. Panel D presents the results using a nearest-neighbor logit propensity score matching estimator. The dependent variables are our main measure of tax avoidance. Higher values for the first 4 measures (BTD, DDBTD, SHELTER, and DTAX) indicate more tax avoidance, whereas a lower value for the last measure (CETR) suggests more tax avoidance. Using the Abadie and Imbens (2006) matching, we find that 4 of the tax avoidance measures are consistent with our expectation that tax avoidance increases significantly, relative to matched control firms, after a firm loses an analyst. These results are not only statistically but also economically significant. Specifically, after a broker closure or merger, BTD increases 2.3 percentage points over that of the control firms (significant at the 5% level), holding everything else constant.²² Treated firms' CETR drops by at least 0.9 percentage points relative to the control firms that do not experience a reduction in analyst coverage, which is 3.3% of the sample mean of CETR prior to broker exits. The direction of the DID estimate of SHELTER is correct, but it is insignificant at conventional significance levels, as shown in Panel C. This is not surprising, as it measures an extreme form of tax avoidance (e.g., Hanlon and Heitzman (2010), Kim et al. (2011)). We find that all of the main mean DID estimates are highly significant when we use propensity score matching. Note that even the DID estimate of SHELTER becomes significant at the 10% level. In Section IV.A, we show that SHELTER is positive and statistically significant for firms that already have a presence in tax havens, indicating that firms with subsidiaries in

²² Shevlin (2002) argues that one must be cautious when drawing inferences about the levels and trends in tax avoidance based on book-tax differences. The consistent results we achieve using alternative measures relax this concern.

tax havens more rapidly engage in tax sheltering following an increase in information asymmetry.

Based on the sample in Panel D of Table 2, we further rerun our DID estimation using a regression framework, and report our results in Panel E. POST denotes a dummy variable that is equal to 1 in the period after the broker exit, and 0 otherwise. TREAT is a dummy variable that indicates if a company is part of our treatment sample. We include firm fixed effects in our regressions to capture unobservable and time-invariant firm characteristics. We include industry-year fixed effects in all of our regressions to capture industry time trends. We further include broker-year fixed effects to capture all the time-variant broker-specific characteristics. In columns 6–10 of Panel E, we also control for our matching variables.²³ Standard errors are clustered at the firm level. We find that across all of the regression specifications, our previous DID results hold.²⁴

²³ In untabulated results, we find that the results are similar if we include all of the control variables as in the literature. We also find that the results do not change qualitatively if we cluster standard errors by firm and year (double clustering), or by broker and year. These results are available from the authors.

²⁴ In later analyses, we focus on our original DID matching estimator in model (1). Compared to a standard DID regression estimator, this strategy offers more flexible and non-linear functional forms. We can also use Abadie and Imbens (2006) matching to do non-parametric estimation. Furthermore, it could easily accommodate to different matching criteria. In addition, most of the studies using this broker merger and/or closure setting adopt the same estimation approach, such as Hong and Kacperczyk (2010), Derrien and Kecskes (2013), He and Tian (2013), and Chen et al. (2015), etc. Nevertheless, we make sure that our results are robust to the regression approach.

The significant effect of changes in analyst coverage due to brokerage exit is consistent with previous studies that use the same natural experiments of broker mergers and closures (e.g., Kelly and Ljungqvist (2012), Fong, Hong, Kacperczyk, and Kubik (2014)). Moreover, as we discuss in Section IV.B, we reestimate our results by partitioning the whole sample into subsamples of low or high analyst coverage before brokerage exit, and find that the increase in tax avoidance is largely driven by the subsample of firms with initial low analyst coverage, where the effect of an individual analyst is larger (firms lose about 20% of their analysts on average).²⁵

D. Robustness Test: Alternative Measures of Tax Avoidance and Alternative Matching Methods

We conduct a battery of robustness tests for our DID analysis. We first use 4 alternative measures of tax avoidance to check whether our results are robust. Among them, ETRDIFF measures the permanent portion of tax avoidance (Frank et al. (2009)), ETR is the rate that affects accounting earnings, and CFETR further removes the effect of earnings accruals. We redo the DID analysis using these measures. Appendix C reports the results. Panel A of Table C1 shows the results using Abadie and Imbens (2006) matching, whereas Panel B reports the results using propensity score matching. We find that across all of our additional measures of tax aggressiveness, all of the DID estimates are statistically

²⁵ In addition, we find that the broker exits strongly affect the information production incentives of the remaining analysts by reducing competition, resulting in reports with lower information content and more biased forecast estimates. These combined effects further amplify the effects of a drop in analyst coverage. These results are not tabulated and available from the authors.

significant, except ETR with propensity score matching.

We then check the robustness of our results by using alternative combinations of matching variables. The results are presented in Table 3. We focus on 4 major measures of tax avoidance (BTD, DDBTD, DTAX, and CETR), but the results on other measures are qualitatively similar. We begin with a simple matching that merely requires both treated and control firms to have valid information about measures of tax avoidance and analyst coverage. DID estimates of BTD, DDBTD, and DTAX are all significant and positive, whereas CETR is negative and statistically significant. We then match firms by pre-event BTD and analyst coverage. Firms with aggressive tax planning might differ from other firms in dimensions that are not fully captured by our matching variables, so we include a pre-event BTD level. We find that the results presented in the second row are similar to those reported above. We further add matching criteria step by step until we include all of the control variables as suggested in the literature (e.g., Chen et al. (2010)). We find that our results are robust to these combinations of matching variables. Indeed, of the 28 models presented in Table 3, only one DID estimate (DTAX), in row 6, is statistically insignificant at conventional levels.

[Table 3 about here]

IV. Further Explorations of Corporate Tax Avoidance

In this section, we examine the factors that affect the relationship between analyst coverage and tax avoidance, and how the effects on avoidance can be mitigated or exacerbated. We concentrate on firms' existing tax planning capacity (e.g., tax haven

presence), initial analyst coverage, industry type (consumer oriented or not), tax authority monitoring, and the number of peer firms.

A. Existing Tax Planning Capacity

The results reported above suggest that managers are likely to take immediate short-term opportunistic actions and avoid tax more aggressively when there is an increase in information asymmetry between firms and investors. This interpretation of the data is based on the underlying assumption that managers have the ability to change tax planning quickly. Previous studies (e.g., Desai and Dharmapala (2006), Chen et al. (2010), and Hanlon and Heitzman (2010)) have identified several techniques for tax avoidance such as investing in a municipal bond, transfer pricing, or using tax shelters. It is possible that many firms have unused existing tax planning capacity; for example, they may have complicated financial structures, own entities in tax havens, be familiar with transfer pricing practices, etc. When information asymmetry is low, firms may not engage in much aggressive tax planning. However, an increase in information asymmetry caused by drops in analyst coverage may provide a good opportunity for managers to incrementally use firms' available tax planning capacity. For instance, firms could effectively hide earnings using their complicated financial structure, or could shelter more earnings in their entities in tax havens. We directly test this hypothesis by dividing the sample into subsamples of firms with high and low tax planning capacity prior to the broker exit events. We use 2 distinct sets of measures of existing tax planning capacity: i) the number of segments and whether the firm is multinational; and ii) firms' presence in tax havens.

The sample is first partitioned into subsamples according to the number of segments in

a firm or whether the firm is a multinational corporation. Intuitively, firms with a greater number of business segments should have a complicated financial structure and be able to more rapidly engage in aggressive tax planning activities (e.g., Desai and Dharmapala (2006), Hanlon and Heitzman (2010)). We also look at whether the firm is multinational, as multinational firms might have more mechanisms for avoiding taxes such as shifting profits to low-tax foreign subsidiaries, shifting debt to high-tax jurisdictions, seeking offshore tax havens, and related-party transactions with foreign subsidiaries (e.g., Chen et al. (2010), Gravelle (2010)). The firms in the high number of segments subsample are in the top tercile for number of segments, and firms in the low number of segments subsample are in the bottom tercile in terms of number of segments. The data for the number of segments come from Compustat's Business Segment files. A firm is defined as multinational if it realizes a positive foreign income in a specific year. Panel A of Table 4 presents the results.

[Table 4 about here]

As shown in Panel A of Table 4, we find that for both of the 2 measures of tax planning capacity, the effect of the change in analyst coverage is more pronounced in the subset of firms that had higher tax planning capacity prior to the broker merger or closure events.

Next we look at one specific mechanism through which these aggressive tax avoidance activities are accomplished—tax haven presence prior to the broker exits. As subsidiary locations are given in the Exhibit 21 in firms' annual reports, we explore 3 measures of firms' presence in tax havens: Tax Haven, Haven Subsidiaries, and Haven Country Presence. Tax Haven is an indicator variable that equals 1 if a firm has at least one tax haven presence in a specific year, and 0 otherwise. Haven Subsidiaries is the total number of distinct tax

haven subsidiaries, and Haven Country Presence is the number of distinct tax haven countries in which at least one subsidiary is located. We collect this information as in Dyreng and Lindsey (2009). It is worth noting that analysts have access to this information and may have fully analyzed firm' sheltering behavior; therefore, firms with intensive coverage from analysts might not fully use their capacity in tax havens. With a reduction in analyst coverage, we expect that firms with tax haven presence, larger numbers of tax haven subsidiaries, or a presence in more tax haven countries will avoid tax more aggressively. Panel B of Table 4 presents the subsample results.

As expected, we find that our results are only significant in the subsample of firms with all three types of tax haven presence. The estimates in the subsample with tax haven presence, greater numbers of haven subsidiaries, or more haven countries are much larger as well. The large DID estimates for the probability of using tax shelters in the tax haven presence subsample is consistent with the estimation for the multinational firms subsample, supporting the observation that firms with existing sheltering capacity are more likely to take advantage of this capacity after an increase in information asymmetry.

B. Initial Analyst Coverage Before Broker Terminations

In this section, we look at the level of analyst coverage before broker terminations. To be consistent with our information hypothesis, an intuitive prediction is that a loss of 1 of 5 analysts should have more effect than the loss of 1 of 15 analysts (Hong and Kacperczyk (2010)). We first divide the whole sample into low and high initial analyst coverage subsamples, and expect that the effects will be more pronounced in the low initial analyst coverage firms. The initial analyst coverage subsample results are reported in Table 5.

[Table 5 about here]

We present the results for all 5 main measures of tax avoidance. In columns 1 and 2 of Table 5, we partition the sample using arbitrary “low” and “high” numbers of analysts following the firm before broker terminations: 5 and 15, respectively. Columns 3 and 4 divide the sample according to terciles sorted on initial analyst coverage. We further consider excessive analyst coverage in columns 5 and 6, to alleviate the concern that analyst coverage might capture the size effect or other characteristics of the firm. We calculate the excessive analyst coverage by the residuals from a regression of analyst coverage on firm size, lagged ROA, asset growth, external financing activities, cash flow volatility, and year dummies (Yu (2008)). As shown in Panel A, we find that our DID estimate is only statistically significant in the low analyst coverage subsample at conventional significance levels for 4 out of 5 measures of tax avoidance, for both sample division criteria. In regard to excessive analyst coverage, we find that our DID estimate is only significant in the low excessive analyst coverage subsample for all 5 measures of tax avoidance. These estimates are larger in magnitude than those in the whole sample estimations. These results confirm our hypothesis that the large effect of a reduction in analyst coverage is mainly driven by low initial analyst coverage firms.²⁶

²⁶ We further look at the summary statistics of the firms with low initial analyst coverage. One possible concern is that if they are loss-making firms, we would expect to learn little about tax avoidance. We find that the mean ROA of this subset of firms is 5.94%, indicating that they are in general generating positive earnings, alleviating our concern that they avoid taxes because of poor financial situations, rather than a higher degree of information asymmetry.

C. Firms in Consumer-Oriented Industries

Previous studies suggest that tax avoidance can create reputation concerns for firms in consumer-oriented industries where customer perception is important (e.g., Hanlon and Slemrod (2009), Graham et al. (2014)). Specifically, Graham et al. (2014) provide evidence suggesting that although firms benefit from avoiding taxes, if tax avoidance hurts a firm's overall reputation among customers it is more cautious in tax avoidance behavior. In this section, we directly test whether the significant effect of analyst coverage is stronger in consumer-oriented industries.

Specifically, we divide the sample into retail industries and other industries and redo our DID estimations. Retail industries include the firms with SIC codes ranging from 5200 to 5999. Table 6 presents the subsample analysis. We find that the DID estimates are more statistically significant and more economically pronounced in the subset of firms in retail industries. Out of our 5 measures of tax avoidance, 3 measures are only significant in retail industries. In terms of economic significance, the coefficients of 3 measures in the retail industries subsample approximately double the coefficients in the other industries subsample. Taken together, the results indicate that the strong effect of analyst coverage on tax avoidance is concentrated in consumer-oriented industries, which is consistent with our expectation that information asymmetry has more effect on the tax avoidance behavior of firms that value their reputation highly.

[Table 6 about here]

D. Tax Authority Monitoring and Enforcement

In this section, we examine how tax authority monitoring and enforcement influence the relationship between analyst coverage and tax avoidance. One reasonable prediction is that firms facing lower IRS monitoring would avoid tax more aggressively after a reduction of analyst coverage due to lower risks. Following Hanlon, Hoopes, and Shroff (2014), we define tax authority monitoring as the probability of an IRS audit measured by the ex-post realizations of actual face-to-face audits. Specifically, we capture managers' perception of audit probability, measured by the annual number of audits by the IRS standardized by the total number of returns in the previous fiscal year for a specific asset size group.²⁷ The data come from the Transactional Records Access Clearinghouse (TRAC).

We conduct subsample tests and report the results in Table 7. Consistent with our expectation, we find that the effect of a reduction in analyst coverage on tax avoidance only exists in the subsample of firms where IRS monitoring is low. These results further strengthen our findings by showing the interaction between IRS monitoring and analyst coverage.

[Table 7 about here]

E. Number of Peer Firms

In a survey of 169 CFOs, Dichev, Graham, Harvey, and Rajgopal (2013) find that peer-firm comparisons are one of the most important sources for detecting earnings

²⁷ Ideally this test would also use district variations in IRS monitoring, but the IRS stopped providing district level data in 2000, the year our sample starts. Therefore, we focus on variations in fiscal year and asset size.

management. Similarly, a reasonable prediction is that firms with many peer firms that disclose information might be less affected by a reduction in analyst coverage, as investors could use peer firms as an alternative information source about tax aggressiveness. We empirically test this using the methodology in Badertscher, Shroff, and White (2013). Specifically, we analyze subsamples categorized by the number of peers a firm has in its primary industry, based on 4-digit SIC codes. The results are presented in Table 8.

[Table 8 about here]

In Table 8, the sample is divided into subsamples according to the number of peer firms. We find supporting evidence that the effect of analyst coverage is indeed more pronounced in the subsample of firms with fewer peers. These results lend further support to our information story.

In sum, we show that analyst coverage reduces corporate tax avoidance, and the link between analyst coverage and tax avoidance further depends on existing tax planning capacity, initial level of analyst coverage, the number of peers, reputational concerns, and tax authority monitoring.

V. Additional Robustness Checks

Overall, the results confirm our hypothesis that analyst coverage reduces corporate tax avoidance. This hypothesis is strongly supported by our baseline regressions and DID analysis using our natural experiments. In this section, we provide additional robustness checks and a persistence test.

A. Persistence Test

In the preceding analyses, we directly test the effects of a decrease in analyst coverage by comparing our measures of tax avoidance from 1 year prior to the brokerage exit ($t-1$) to 1 year after the brokerage exit ($t+1$). It is not clear whether the increase in information asymmetry is permanent. However, even if the change in the information environment is not permanent, managers might still take advantage of this short period of increased information asymmetry. This is consistent with the findings in Section IV.B that firms with more unused tax planning capacity are more likely to aggressively avoid tax. The presence of this short-term opportunistic behavior is also supported by other studies using the same natural experiments of broker exits. For example, Derrien and Kecskes (2013) find that firms change investment, financing, and payout decisions after broker exits.

In further tests we examine the long-term effect of the broker exits. We find that most of our treatment firms regain their analyst coverage after 3 years. This is consistent with Derrien and Kecskes (2013), who find that the shocks to analyst coverage due to broker exits are one-time temporary decreases. We compare our tax avoidance variables 3 years ($t+3$ vs. $t-1$) and 5 years ($t+5$ vs. $t-1$) after and 1 year prior to the shock in our tests, and find no significant results. These findings suggest that managers observing a drop in analyst coverage caused by broker exits are likely to take more aggressive tax planning strategies immediately, to take full advantage of this short-window of opportunity and incrementally use their tax planning capacity.

B. Other Potential Mechanisms

We find that after a reduction in analyst coverage, firms avoid taxes more aggressively,

and the effect is driven by the subset of the firms that have low initial analyst coverage and a smaller number of peers prior to the broker exits. These findings are all consistent with our information asymmetry hypothesis. Balakrishnan, Billings, Kelly, and Ljungqvist (2014) find that some firms voluntarily disclose more information to shareholders in response to exogenous decreases in analyst coverage, but this pattern only exists for a very small proportion of firms (4.9% in their sample). We further test the robustness of our results by excluding firms that increase voluntary disclosure, and find our previous findings are maintained.²⁸

VI. Concluding Remarks

In this study, we examine the effect of information asymmetry, as measured by analyst coverage, on corporate tax avoidance. Using changes in analyst coverage caused by broker closures and mergers, we find that when a firm experiences a decrease in analyst coverage, it engages in more tax avoidance activities relative to similar firms that do not experience a reduction in analyst coverage. This suggests a strong negative effect of analyst coverage on tax avoidance. We further find that the effects are mainly driven by the firms with more existing tax planning capacity, smaller initial analyst coverage, and a smaller number of peer firms. Moreover, the effects are more pronounced in firms in consumer-oriented industries and firms facing lower levels of monitoring from tax authorities. Overall, our

²⁸ The data of management forecast come from the Company Issued Guidance section in the First Call Historical Database (FCHD) by Thompson Reuters. The results are not tabulated here but are available from the authors.

findings suggest that financial analysts help reduce the information asymmetry between firms and investors, and as a consequence reduce corporate tax avoidance.

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TABLE 1

Summary Statistics for the Full Sample prior to Matching

Table 1 presents descriptive statistics on measures of tax avoidance, analyst coverage, and firm characteristics for the full sample used in the paper prior to matching. Our sample consists of 23,475 firm-years from 5,401 publicly traded U.S. firms covering the 1999–2011 period. *BTD*, the total book-tax difference, is calculated by book income less taxable income scaled by lagged assets (*AT*). Book income is the pretax income (*PI*) in year *t*. *DDBTD* is Desai and Dharmapala's (2006) residual book-tax difference. *SHELTER* is the estimated probability that a firm uses a tax shelter based on Wilson's (2009) tax sheltering model. *DTAX* is based on Frank, Lynch, and Rego (2009). *CETR* is the cash effective tax rate based on Chen et al. (2010), calculated as cash taxes paid (*TXPD*) divided by pretax income (*PI*). *MPBTD* is Manzon and Plesko's (2002) book-tax difference, which is calculated by U.S. domestic book income less U.S. domestic taxable income less state tax income (*TXS*) less other tax income (*TXO*) less equity in earnings (*ESUB*) scaled by lagged assets (*AT*). *ETR* is the effective tax rate as calculated by Zimmerman (1983). *CFETR* is cash flow effective tax rate, calculated as cash taxes paid (*TXPD*) divided by operating cash flows (*OANCF*). $\ln(\text{COVERAGE})$ is the natural log of the total number of stock analysts following the firm during the year. Other variable definitions are given in Appendix A.

TABLE 1 (continued)

Variables	Mean	Std. Dev.	Q1	Median	Q3	N
<i>Measures of Tax Avoidance</i>						
BTD	-0.062	0.421	-0.053	0.008	0.046	23,475
DDBTD	0.023	0.294	-0.036	0.004	0.061	20,421
SHELTER	0.152	3.846	-0.934	0.739	2.346	18,572
DTAX	0.031	0.441	-0.019	0.025	0.111	18,853
CETR	0.270	0.211	0.116	0.256	0.361	14,836
MPBTD	-0.128	0.433	-0.095	-0.004	0.028	23,404
ETRDIF	-0.123	0.435	-0.076	0.002	0.020	23,343
ETR	0.267	0.255	0.045	0.227	0.388	16,395
CFETR	0.211	0.244	0.009	0.137	0.317	19,161
<i>Analyst Coverage</i>						
ln(COVERAGE)	1.421	0.943	0.693	1.447	2.150	23,475
<i>Firm Characteristics</i>						
Firm size (SIZE)	6.121	1.907	4.868	6.089	7.303	23,475
Tobin's Q (Q)	2.438	2.307	1.223	1.729	2.734	23,475
Tangibility (PPE)	0.443	0.369	0.156	0.333	0.641	23,475
Foreign income (FI)	0.008	0.030	0.000	0.000	0.005	23,475
ROA	0.025	0.321	0.016	0.114	0.175	23,475
Leverage (LEV)	0.166	0.216	0.000	0.085	0.265	23,475
NOL	0.662	0.473	0.000	1.000	1.000	23,475
Δ NOL	8.746	47.195	0.000	0.000	0.058	23,475
EQINC	3.216E-04	0.005	0.000	0.000	0.000	23,475
Intangibility (INTAN)	0.209	0.284	0.006	0.099	0.306	23,475

TABLE 2

DID Analysis of Tax Avoidance

Table 2 presents the main DID estimates for our measures of tax avoidance following broker closures or broker mergers. The sample consists of 1,415 treated firms that experienced a reduction in analyst coverage between 2000 and 2010 and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. Both groups of firms are publicly traded non-finance and non-utility firms. Panel A of Table 2 reports the summary statistics for the matched samples prior to broker terminations, based on Abadie and Imbens' (2006) bias-corrected average treated effect matching estimator. The matching variables include firm size (SIZE), Tobin's Q (Q), tangibility (PPE), foreign income (FI), analyst coverage (COV), Fama–French (1997) 48 industry, and fiscal year. Panel B reports the parallel trends in measures of tax avoidance 1 year and 2 years prior to the broker exits. Panel C reports the DID results using Abadie and Imbens' (2006) matching estimator. Panel D reports the DID results for tax avoidance using the nearest-neighbor logit propensity score matching estimator. SIZE is the natural log of the market value of equity ($CSHO \times PRCC_F$). Q is calculated as the market value of assets over book value of assets, $(AT - CEQ + CSHO \times PRCC_F)/AT$, and PPE is defined as property, plant, and equipment (PPEGT) divided by total assets. FI is calculated as foreign income (PIFO) divided by total assets (AT), and COV is the total number of stock analysts following the firm during the year. Other variable definitions are given in Appendix A. Panel E shows the DID results using a regression framework. Heteroskedasticity-consistent standard errors clustered at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2 (continued)

Panel A. Summary Statistics for Matched Samples prior to Broker Terminations

Variable	Treated Firms			Matched Control Firms			Difference in Means	<i>t-Stat.</i>
	Mean	Std. Dev.	Med.	Mean	Std. Dev.	Med.		
SIZE	7.476	1.806	7.395	7.446	1.782	7.312	0.031	[0.49]
Q	2.754	2.101	2.080	2.718	2.073	2.032	0.035	[0.49]
PPE	0.439	0.346	0.339	0.427	0.343	0.322	0.012	[1.01]
FI	0.012	0.030	0.000	0.014	0.031	0.000	-0.001	[-1.18]
COV	11.701	7.253	10.000	11.556	7.261	10.000	0.145	[-0.21]

Panel B. Parallel Trends in Tax Avoidance prior to Broker Terminations

Variable	Treated Firms			Matched Control Firms			Difference in Means	<i>t-Stat.</i>	Year
	Mean	Std. Dev.	Med.	Mean	Std. Dev.	Med.			
ΔBTD	0.024	0.991	-0.002	-0.009	0.177	-0.007	0.033	[1.09]	<i>t</i> -1
ΔDDBTD	0.039	1.316	0.000	-0.009	0.085	-0.005	0.048	[1.08]	<i>t</i> -1
ΔSHELTER	0.221	2.572	0.102	0.087	1.416	0.081	0.133	[1.41]	<i>t</i> -1
ΔDTAX	0.012	0.317	-0.003	0.015	0.138	0.009	-0.003	[-0.31]	<i>t</i> -1
ΔCETR	0.005	0.207	0.011	0.004	0.195	0.011	0.002	[0.18]	<i>t</i> -1
ΔBTD	0.057	0.549	0.000	0.051	0.427	0.004	0.007	[0.31]	<i>t</i> -2
ΔDDBTD	0.120	2.275	0.000	0.021	0.187	0.013	0.099	[1.19]	<i>t</i> -2
ΔSHELTER	0.181	2.370	0.125	0.260	1.803	0.109	-0.078	[-0.78]	<i>t</i> -2
ΔDTAX	0.008	0.170	0.000	-0.009	0.135	0.007	0.018	[-0.52]	<i>t</i> -2
ΔCETR	0.004	0.212	0.006	-0.002	0.231	-0.004	0.007	[0.63]	<i>t</i> -2

TABLE 2 (continued)

Variables	Average Treated Difference (year $t+1$ vs. $t-1$)	Average Control Difference (year $t+1$ vs. $t-1$)	DIDs: Treated versus Control
<i>Panel C. DID Results Using Abadie and Imbens' (2006) Matching Estimator</i>			
BTD	0.037** [0.015]	0.014 [0.012]	0.023** [0.010]
DDBTD	0.056*** [0.016]	0.025** [0.010]	0.031*** [0.009]
SHELTER	0.259*** [0.095]	0.141 [0.092]	0.118 [0.085]
DTAX	0.065*** [0.022]	0.010 [0.019]	0.055* [0.029]
CETR	-0.007 [0.010]	0.017* [0.009]	-0.025** [0.011]
<i>Panel D. DID Results Using Propensity Score Matching Estimator</i>			
BTD	0.033*** [0.009]	-0.018*** [0.005]	0.052*** [0.010]
DDBTD	0.048*** [0.008]	0.007 [0.004]	0.041*** [0.009]
SHELTER	0.164** [0.078]	-0.013 [0.046]	0.177* [0.091]
DTAX	-0.004 [0.009]	-0.028*** [0.006]	0.024** [0.011]
CETR	0.007** [0.003]	0.016*** [0.003]	-0.009* [0.005]

TABLE 2 (continued)

Panel E. DID Results Using a Regression Framework

	BTD	DDBTD	SHELTER	DTAX	CETR	BTD	DDBTD	SHELTER	DTAX	CETR
Variables	1	2	3	4	5	6	7	8	9	10
POST	-0.167** [0.083]	-0.013 [0.046]	-1.208** [0.572]	-0.041 [0.040]	0.022 [0.021]	-0.157* [0.085]	-0.006 [0.044]	-1.328 [0.814]	-0.046 [0.038]	0.022 [0.030]
POST × TREAT	0.073*** [0.023]	0.058*** [0.020]	0.270*** [0.095]	0.054** [0.025]	-0.015** [0.007]	0.068*** [0.022]	0.050*** [0.018]	0.456** [0.193]	0.057** [0.028]	-0.019* [0.010]
TREAT	-0.027* [0.015]	-0.021 [0.013]	-0.205* [0.106]	-0.015 [0.015]	0.008 [0.008]	-0.023 [0.015]	-0.018 [0.013]	-0.245** [0.117]	-0.016 [0.019]	0.008 [0.012]
Firm size (SIZE)						-0.013 [0.014]	-0.021* [0.012]	0.895*** [0.232]	-0.020* [0.011]	0.003 [0.009]
Tobin's Q (Q)						0.015 [0.010]	0.010 [0.009]	-0.098 [0.101]	0.024* [0.013]	-0.010* [0.006]
Tangibility (PPE)						0.135 [0.111]	0.103 [0.087]	0.223 [1.231]	-0.051 [0.161]	0.094* [0.053]
Foreign income (FI)						1.923*** [0.399]	0.403 [0.351]	21.523*** [3.493]	0.646 [0.562]	-0.428 [0.312]
Year × Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Broker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	5,660	4,736	4,800	4,880	4,880	5,660	4,736	4,780	4,880	4,880
Adj. R ²	0.371	0.379	0.701	0.226	0.767	0.383	0.385	0.726	0.229	0.770

TABLE 3

DID Analysis of Tax Avoidance: Alternative Matching Criteria

Table 3 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, using alternative matching criteria. The sample consists of 1,415 treated firms that experienced a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted for various combinations of the following matching variables: BTD, COV, SIZE, Q, PPE, FI, ROA, LEV, NOL, Δ NOL, INTAN, and EQINC. SIZE is the natural log of the market value of equity ($CSHO \times PRCC_F$). Q is calculated as the market value of assets over book value of assets, $(AT - CEQ + CSHO \times PRCC_F) / AT$, and PPE is defined as property, plant, and equipment (PPEGT) divided by total assets. FI is calculated as foreign income (PIFO) divided by total assets (AT), and COV is the total number of stock analysts following the firm during the year. ROA is return on assets, measured as operating income (PI - XI) divided by lagged assets. LEV is long-term leverage, defined as long-term debt (DLTT) divided by total assets (AT). NOL is a dummy variable coded as 1 if the loss carry forward (TLCF) is positive, and 0 otherwise. Δ NOL is defined as the change in loss carry forward, INTAN is calculated as intangible assets (INTAN) scaled by lagged assets, and EQINC is equity income in earnings (ESUB) divided by lagged assets. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 3 (continued)

	BTD	DDBTD	DTAX	CETR
Variables	1	2	3	4
Simple-matched	0.021** [0.009]	0.013* [0.008]	0.056*** [0.018]	-0.015** [0.007]
BTD/COV-matched	0.050*** [0.013]	0.024** [0.012]	0.033** [0.016]	-0.018** [0.008]
SIZE/COV-matched	0.032** [0.014]	0.033** [0.014]	0.135** [0.067]	-0.013* [0.007]
SIZE/Q/PPE/COV-matched	0.028** [0.014]	0.025* [0.014]	0.040*** [0.015]	-0.012* [0.007]
SIZE/Q /PPE/FI/ROA/LEV /COV-matched	0.026* [0.014]	0.031** [0.012]	0.028* [0.015]	-0.017** [0.007]
SIZE/Q /PPE/FI /ROA/LEV /NOL/ Δ NOL/COV-matched	0.034** [0.013]	0.029** [0.012]	0.055 [0.058]	-0.015** [0.007]
SIZE/Q /PPE/FI/ROA/LEV /NOL/ Δ NOL/INTAN/EQINC/ COV-matched	0.028** [0.014]	0.012*** [0.011]	0.012*** [0.014]	-0.012*** [0.007]

TABLE 4

DID Analysis of Tax Avoidance: Conditional on Existing Tax Planning Capacity

Table 4 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, conditional on firms' existing tax planning capacity. The sample consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms that are selected as the closest match, based on a set of firm characteristics, to the treated firms in the year before the broker terminations. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted. In Panel A of Table 4, the sample is partitioned into high and low tax planning capacity subsamples according to the number of segments in the firm or whether the firm is a multinational corporation. In Panel B, the sample is divided according to tax haven presence, the number of tax haven subsidiaries, and the number of distinct tax haven countries in which the firm has subsidiaries. A high number of segments indicates that a firm is in the top tercile of the sample, and low number of segments places the firm in the bottom tercile of the sample. The data for the number of segments come from Compustat's Business Segment files. A firm is defined as multinational if it realizes a positive foreign income. Tax Haven is an indicator variable that equals 1 if a firm has at least one tax haven presence in a specific year, and 0 otherwise. Haven Subsidiaries is the total number of distinct tax haven subsidiaries, and Haven Country Presence is the number of distinct tax haven countries in which at least one subsidiary is located. The data on tax havens come from Dyreng and Lindsey (2009). For Haven Subsidiaries and Haven Country Presence, the sample is divided according to terciles. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 4 (continued)

Panel A. Number of Segments and Multinational Firm

Variables	DIDs: Treated versus Control			
	No. of Segments		Multinational Firm	
	Low	High	No	Yes
	1	2	3	4
BTD	0.004 [0.011]	0.083*** [0.016]	0.007 [0.011]	0.072*** [0.014]
DDBTD	-0.005 [0.009]	0.071*** [0.015]	-0.007 [0.009]	0.065*** [0.013]
SHELTER	0.133 [0.105]	0.201 [0.140]	-0.148 [0.091]	0.336*** [0.128]
DTAX	0.015 [0.014]	0.035** [0.017]	0.005 [0.014]	0.033** [0.015]
CETR	-0.008 [0.008]	-0.011* [0.006]	-0.007 [0.008]	-0.011* [0.006]

Panel B. Tax Haven Presence

Variables	DIDs: Treated versus Control					
	Tax Haven		Haven Subsidiaries		Haven Country Presence	
	No	Yes	Low	High	Low	High
	1	2	3	4	5	6
BTD	0.041 [0.032]	0.055*** [0.018]	0.020 [0.013]	0.061*** [0.014]	0.022 [0.016]	0.061*** [0.014]
DDBTD	0.027 [0.018]	0.044*** [0.014]	0.008 [0.012]	0.053*** [0.012]	0.004 [0.014]	0.053*** [0.012]
SHELTER	-0.002 [0.123]	0.385*** [0.130]	0.027 [0.123]	0.196* [0.104]	-0.001 [0.140]	0.196* [0.104]
DTAX	0.045 [0.041]	0.167*** [0.050]	0.002 [0.017]	0.038*** [0.014]	-0.005 [0.019]	0.038*** [0.014]
CETR	-0.005 [0.005]	-0.013*** [0.001]	-0.012 [0.016]	-0.025** [0.013]	-0.004 [0.018]	-0.025** [0.013]

TABLE 5

DID Analysis of Tax Avoidance: Conditional on Initial Analyst Coverage

Table 5 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, conditional on initial analyst coverage. The sample consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted. The sample is divided into subsamples according to the initial analyst coverage. Columns 1 and 2 partition the sample using arbitrary “low” (5) and “high” (15) numbers of analysts following the firm before broker terminations. Column 1 consists of 870 firm-year observations, and column 2 consists of 646 firm-year observations. Columns 3 and 4 divide the sample according to the terciles sorted on initial analyst coverage, where the firms in the high initial analyst coverage subsample are those in the top tercile of the whole sample, and firms in the low initial analyst coverage subsample are in the bottom tercile of the whole sample. Column 3 consists of 930 firm-year observations, and column 4 consists of 948 firm-year observations. Columns 5 and 6 divide the sample using excessive analyst coverage instead of analyst coverage. Column 5 consists of 932 firm-year observations, and column 6 consists of 948 observations. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5 (continued)

DIDs: Treated versus Control

Variables	Analyst Coverage (COV)				Excessive Analyst Coverage	
	≤5	≥15	Low	High	Low	High
	1	2	3	4	5	6
BTD	0.118*** [0.028]	0.007 [0.014]	0.100*** [0.023]	0.005 [0.012]	0.095*** [0.018]	0.009 [0.010]
DDBTD	0.103*** [0.025]	0.008 [0.013]	0.069*** [0.021]	0.017* [0.010]	0.065*** [0.016]	0.016* [0.009]
SHELTER	0.278 [0.228]	0.229 [0.141]	0.153 [0.198]	0.102 [0.123]	0.267* [0.154]	0.089 [0.104]
DTAX	0.075*** [0.029]	0.028* [0.014]	0.083*** [0.025]	-0.006 [0.013]	0.062*** [0.019]	-0.011 [0.011]
CETR	-0.030*** [0.009]	-0.016* [0.009]	-0.017** [0.008]	-0.010 [0.008]	-0.013** [0.006]	-0.006 [0.007]

TABLE 6

DID Analysis of Tax Avoidance:

Consumer-Oriented Industries versus Other Industries

Table 6 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, conditional on retail industry versus other industries. The sample consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted. The sample is divided into subsamples according to whether the firms are in consumer-oriented industries. Column 1 consists of 430 firm-year observations and column 2 consists of 2,400 firm-year observations. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 6 (continued)

DIDs: Treated versus Control

Variables	Consumer-Oriented Industries	Other Industries
	1	2
BTD	0.055*** [0.012]	0.031** [0.012]
DDBTD	0.043*** [0.011]	0.029* [0.015]
SHELTER	0.182* [0.105]	0.151 [0.098]
DTAX	0.026** [0.013]	0.016 [0.015]
CETR	-0.040* [0.023]	-0.015 [0.011]

TABLE 7

DID Analysis of Tax Avoidance: Tax Authority Monitoring and Enforcement

Table 7 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, conditional on tax authority monitoring and enforcement. The sample consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted. The sample is divided into subsamples according to tax authority monitoring and enforcement. Following Hanlon, Hoopes, and Shroff (2014), tax authority monitoring is defined as the probability of an IRS audit measured by the ex-post realizations of actual face-to-face audits. The data come from the Transactional Records Access Clearinghouse (TRAC). The sample division is based on the median value of tax authority monitoring and enforcement, and the results are robust to a division based on terciles. Column 1 consists of 1,470 firm-year observations and column 2 consists of 1,360 firm-year observations. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 7 (continued)

DIDs: Treated versus Control

(tax authority monitoring and enforcement)

Variables	High	Low
	1	2
BTD	0.003 [0.010]	0.100*** [0.018]
DDBTD	-0.008 [0.008]	0.086*** [0.017]
SHELTER	0.002 [0.105]	0.326** [0.141]
DTAX	0.004 [0.010]	0.043** [0.018]
CETR	-0.015 [0.013]	-0.023* [0.014]

TABLE 8

DID Analysis of Tax Avoidance: Number of Peer Firms

Table 8 presents the DID estimates for our measures of tax avoidance following broker closures or broker mergers, conditional on the number of peer firms. The sample consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms in the year before the broker termination based on a set of firm characteristics. Both groups of firms are publicly traded non-finance and non-utility firms. Nearest-neighbor logit propensity score matching is adopted. The sample is divided into subsamples according to the number of peer firms. The sample division is based on median values of the numbers of peer firms, and the results are robust to a division based on terciles. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 8 (continued)

DIDs: Treated versus Control

Variables	(NUM_OF_PEERS)	
	High 1	Low 2
BTD	0.022*** [0.007]	0.076*** [0.018]
DDBTD	0.010 [0.008]	0.066*** [0.017]
SHELTER	-0.010 [0.149]	0.327*** [0.087]
DTAX	0.010 [0.012]	0.038** [0.017]
CETR	0.012 [0.014]	-0.054*** [0.014]

Appendix A. Variable Definitions

Appendix A presents the definitions and detailed calculations of the variables used in the paper.

Measures of Tax Avoidance

Book-tax difference (BTD): The total book-tax difference, which is calculated by book income less taxable income scaled by lagged assets (AT). Book income is pre-tax income (PI) in year t . Taxable income is calculated by summing the current federal tax expense (TXFED) and current foreign tax expense (TXFO) and dividing by the statutory tax rate and then subtracting the change in net operating loss carryforwards (TLCF) in year t . If the current federal tax expense is missing, the total current tax expense equals the total income taxes (TXT) less deferred taxes (TXDI), state income taxes (TXS), and other income taxes (TXO). We remove observations with total assets less than \$1 million and observations with negative taxable income (TXFED<0). Source: Compustat

Desai and Dharmapala's (2006) residual book-tax difference (DDBTD): Desai and Dharmapala's (2006) residual book-tax difference, which equals the residual from the following fixed effects regression: $BTD_{i,t} = \beta_1 TACC_{i,t} + \mu_i + \varepsilon_{i,t}$ where BTD is the total book-tax difference and TACC is the total accruals measured using the cash flow method per Hribar and Collins (2002). Both variables are scaled by lagged total assets. We remove observations with total assets less than \$1 million and observations with negative taxable income (TXFED<0). Source: Compustat

SHELTER: Estimated probability that a firm engages in a tax shelter based on Wilson's

(2009) tax sheltering model: $SHELTER = -4.86 + 5.20 \times BTD + 4.08 \times Discretionary$

$Accruals - 1.41 \times Leverage + 0.76 \times AT + 3.51 \times ROA + 1.72 \times Foreign\ Income + 2.43 \times R\&D$

where BTD is the total book-tax difference; Discretionary accruals is the absolute value of discretionary accruals from the modified cross-sectional Jones model;

Leverage is the long-term leverage, defined as long-term debt (DLTT) divided by

total assets (AT); AT is the total assets; ROA is the return on assets, measured as

operating income (PI - XI) divided by lagged assets; Foreign income is an indicator

variable equal to 1 for firm observations reporting foreign income (PIFO), and 0

otherwise; and R&D is R&D expense (XRD) divided by lagged total assets. Source:

Compustat

DTAX: A firm's residual from the following regression, estimated by industry and year:

$ETRDIFF_{it} = \beta_0 + \beta_1 INTANG_{it} + \beta_2 EQIN_{it} + \beta_3 MI_{it} + \beta_4 TXS_{it} + \beta_5 \Delta NOL_{it} + \beta_6 LAGETRDIFF_{it}$

$+ \varepsilon_i$, where ETRDIFF is calculated as $(PI - ((TXFED + TXFO)/STR)) - (TXDI/STR)$,

scaled by lagged assets (AT). PI is pretax book income; TXFED is the current federal

tax expense; TXFO is the current foreign tax expense; TXDI is the deferred tax

expense. INTANG is goodwill and other intangible assets (INTAN), scaled by lagged

assets (AT); EQIN is income (loss) reported under the equity method (ESUB), scaled

by lagged assets (AT); MI is income (loss) attributable to minority interest (MII),

scaled by lagged assets; TXS is current state tax expense, scaled by lagged assets;

ΔNOL is change in net operating loss carry forwards (TLCF), scaled by lagged assets;

LAGETRDIFF is ETRDIFF in the previous fiscal year. Based on Frank, Lynch, and

Rego (2009). Source: Compustat

Cash effective tax rate (CETR): Calculated as cash taxes paid (TXPD) divided by pretax income (PI). Based on Chen et al. (2010). CETR is set to missing when the denominator is 0 or negative. We winsorize CETR to the range [0, 1]. Source: Compustat

Manzon and Plesko (2002) book-tax difference (MPBTD): U.S. domestic book-tax difference, which is calculated by U.S. domestic book income less U.S. domestic taxable income less state tax income (TXS) less other tax income (TXO) less equity in earnings (ESUB) scaled by lagged assets (AT). U.S. domestic book income is domestic pretax income (PIDOM) in year t . U.S. domestic taxable income is calculated by the current federal tax expense (TXFED) divided by the statutory tax rate. We remove observations with total assets less than \$1 million and observations with negative taxable income (TXFED<0). Based on Manzon and Plesko (2002). Source: Compustat

ETR differential (ETRDIFF): Calculated as $(PI - ((TXFED + TXFO)/STR)) - (TXDI/STR)$, scaled by lagged assets (AT). PI is pretax book income; TXFED is the current federal tax expense; TXFO is the current foreign tax expense; TXDI is the deferred tax expense. Based on Frank, Lynch, and Rego (2009) and Kim et al. (2011). Source: Compustat

Effective tax rate (ETR): Calculated as total tax expense (TXT) less change in deferred tax (TXDI), divided by operating cash flows (OANCF). Based on Zimmerman (1983). ETR is set to missing when the denominator is 0 or negative. We winsorize ETR to the range [0, 1]. Source: Compustat

Cash flow effective tax rate (CFETR): Calculated as cash taxes paid (TXPD) divided by operating cash flows (OANCF). We winsorize CFETR to the range [0, 1]. Source: Compustat

Analyst Coverage

Coverage (COV): Total number of stock analysts following the firm during the year. Source: IBES

Firm Characteristics

Firm size (SIZE): Natural log of the market value of equity (CSHO × PRCC_F). Source: Compustat

Tobin's Q (Q): Market value of assets over book value of assets: $(AT - CEQ + CSHO \times PRCC_F)/AT$. Source: Compustat

Tangibility (PPE): Property, plant, and equipment (PPEGT) divided by total assets. Source: Compustat

Foreign income (FI): Foreign income (PIFO) divided by total assets. Source: Compustat

ROA: Return on assets, measured as operating income (PI - XI) divided by lagged assets. Source: Compustat

Leverage (LEV): Long-term leverage, defined as long-term debt (DLTT) divided by total assets (AT). Source: Compustat

NOL: Dummy variable coded as 1 if loss carry forward (TLCF) is positive. Source: Compustat

ΔNOL: Change in loss carry forward. Source: Compustat

Intangibility: Intangible assets (INTAN), scaled by lagged assets. Source: Compustat

EQINC: Equity income in earnings (ESUB) divided by lagged assets. Source: Compustat

Cash holdings (CASH): Cash and short-term investments (CHE) divided by total assets.

Source: Compustat

Cash flow (CF): Income before extraordinary items (IBC) divided by total assets (AT).

Source: Compustat

No. of segments: Total number of business segments in the firm. Source: Compustat's

Business Segment files

Multinational firm: A firm is defined as multinational if it realizes a positive foreign income. Source: Compustat

Tax haven: An indicator variable that equals 1 if a firm has at least 1 tax haven presence in a specific year, and 0 otherwise. Source: Dyreng and Lindsey (2009)

Haven subsidiaries: Total number of distinct tax haven subsidiaries that a firm has.

Source: Dyreng and Lindsey (2009)

Haven country presence: Total number of distinct tax haven countries in which at least one subsidiary of the firm is located. Source: Dyreng and Lindsey (2009)

Consumer-oriented industries: Industries with SIC codes from 5200 to 5999. Source:

Compustat

Tax authority monitoring and enforcement: Following Hanlon, Hoopes, and Shroff (2014), tax authority monitoring is defined as the probability of an IRS audit measured by the ex-post realizations of actual face-to-face audits. Source: Transactional Records Access Clearinghouse (TRAC)

NUM_OF_PEERS: Total number of peer firms for a particular firm operating in an industry based on 4-digit SIC codes. Source: Compustat

Appendix B. Broker Terminations

In Appendix B, we present the descriptive statistics for broker terminations, including the number of broker terminations and the number of affected firms in each year in our sample. Table B1 indicates that there is no obvious evidence of clustering in time and the exits are spread out evenly over the sample period.

TABLE B1

Descriptive Statistics for Broker Terminations

Table B1 presents the number of broker terminations and the number of affected firms in each year in our sample, for firms with valid information on the measures of corporate tax avoidance and matching variables in the DID analysis framework.

Year	No. of Broker Terminations	No. of Affected Firms
2000	8	233
2001	8	293
2002	4	227
2003	2	22
2004	2	41
2005	7	156
2006	3	68
2007	5	195
2008	3	122
2009	3	17
2010	2	41
Total	47	1,415

Appendix C. Alternative Measures of Tax Avoidance

In Appendix C, we show the DID estimates using alternative 4 measures of tax avoidance following broker closures or broker mergers. The sample in Table C1 consists of 1,415 treated firms that experience a reduction in analyst coverage in the 2000–2010 period, and the same number of control firms. Control firms are a subset of the non-treated firms selected as the closest match to the treated firms, based on a set of firm characteristics, in the year before the broker termination. The results indicate that our main results are quite robust.

TABLE C1

DID Analysis of Tax Avoidance: Alternative Measures of Tax Avoidance

Table C1 presents the DID analysis of tax avoidance using alternative measures of tax avoidance. Panel A reports the Difference-in-Differences results using Abadie and Imbens' (2006) matching. Panel B reports the DID results for tax avoidance using nearest-neighbor logit propensity score matching. The matching variables include firm size (SIZE), Tobin's Q (Q), tangibility (PPE), foreign income (FI), analyst coverage (COV), Fama–French (1997) 48 industry, and fiscal year. SIZE is the natural log of the market value of equity ($CSHO \times PRCC_F$). Q is calculated as the market value of assets over book value of assets, $(AT - CEQ + CSHO \times PRCC_F)/AT$, and PPE is defined as property, plant, and equipment (PPEGT) divided by total assets. FI is calculated as foreign income (PIFO) divided by total assets (AT), and COV is the total number of stock analysts following the firm during the year. Other variable definitions are given in Appendix A. Heteroskedasticity-consistent standard errors robust to clustering at the firm level are reported in square brackets below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE C1 (continued)

Variables	Average Treated Difference (year $t+1$ vs. $t-1$)	Average Control Difference (year $t+1$ vs. $t-1$)	DIDs: Treated versus Control
<i>Panel A. DID Results Using Abadie and Imbens' (2006) Matching</i>			
MPBTD	0.056** [0.022]	0.026* [0.014]	0.030** [0.011]
ETRDIF	0.055*** [0.020]	0.027* [0.015]	0.028*** [0.009]
ETR	-0.019*** [0.006]	-0.003 [0.007]	-0.016* [0.009]
CFETR	-0.048*** [0.008]	-0.033*** [0.008]	-0.015** [0.006]
<i>Panel B. DID Results Using Propensity Score Matching</i>			
MPBTD	0.056*** [0.008]	-0.008** [0.004]	0.064*** [0.009]
ETRDIF	0.058*** [0.009]	-0.010** [0.004]	0.067*** [0.010]
ETR	-0.011** [0.005]	-0.007 [0.007]	-0.004 [0.009]
CFETR	-0.034*** [0.007]	-0.014** [0.007]	-0.020** [0.010]