IPOs and Corporate Tax Planning*

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Abstract

Does going public affect the amount and type of a company's tax planning? Using a panel of U.S. corporate tax return data from 1994 to 2018, we compare tax avoidance for firms that completed an IPO relative to three groups: (i) firms that filed for an IPO but later withdrew and remained private, (ii) private firms that received private capital infusions, and (iii) public firms that received public capital infusions. Relative to all three groups, IPO-completing firms report higher levels of domestic tax planning after going public. These firms also engage in greater multinational tax planning, measured with an increased foreign haven presence and higher cross-border related party payments. The collective evidence demonstrates that an IPO is an unique inflection point when firms implement domestic tax planning and long-term foreign income shifting strategies. The paper provides evidence to bridge between prior work contrasting private and public firm tax planning, contributes to the nascent literature studying corporate tax implications of IPOs, and informs U.S. tax revenue estimates of corporate tax collections in light of the declining trend in IPO activity.

Keywords: Initial public offering, tax planning, multinational tax

JEL Codes: D12, E21, H24

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1 Introduction

An initial public offering (IPO) is a seminal event in the lifecycle of a firm, providing an influx of equity capital, expanding the ownership base, and subjecting the firm to public financial reporting requirements. Over 6,500 firms have gone public since 1990, raising approximately $1 trillion in outside capital (Lowry, Michaely and Volkova, 2017). A substantial academic literature examines IPOs, focusing on market underpricing, long-run underperformance, determinants of the going-public decision, and investment outcomes (reviewed by Ritter and Welch (2002) and Lowry, Michaely and Volkova (2017)). However, there is little evidence on the extent to which going public affects firms' tax obligations, even though the IPO is accompanied by operational, investment, and reporting shifts that provide opportunities to implement tax planning strategies. In this paper, we study the effect of IPO completion on firms' domestic and foreign tax avoidance.

Prior literature motivates competing predictions for the effect of an IPO on firm tax planning, with some work suggesting increased levels of tax avoidance.[1] For example, Badertscher, Katz and Rego (2013) show that highly concentrated ownership and control, such as that in pre-IPO private firms, is associated with higher levels of managerial risk aversion and - by extension - underinvestment in risky tax planning. Their finding suggests that a larger (and less risk-averse) post-IPO shareholder group should push the firm to pursue greater tax avoidance. Furthermore, Bird, Edwards and Ruchti (2018) document substantial peer effects in tax planning, implying that IPO-completing firms will converge to

[1] We use the terms "tax planning" and "tax avoidance" interchangeably throughout the paper, where we define both broadly as the reduction of explicit taxes (Hanlon and Heitzman, 2010). Wilde and Wilson (2018) define tax planning as "intentional efforts made to reduce corporate tax burdens and investment in tax avoidance."

higher levels of avoidance as exhibited by their public peers. The IPO event may also provide discrete opportunities to implement unique structures, such as "supercharging" tax benefits (Edwards, Hutchens and Rego, 2019). Finally, the IPO capital infusion may help firms overcome fixed costs of tax planning, particularly given that pre-IPO companies generally have limited resources to spend on the tax function.

However, prior literature also suggests that the IPO would be associated with *less* tax planning. The transition to a more disperse group of shareholders could introduce agency issues, manifesting in underinvestment in tax avoidance. The increased access to external capital afforded by the IPO could further exacerbate these agency issues, relaxing financial constraints that otherwise motivate firms to tax plan (Edwards, Schwab and Shevlin, 2016). In addition, perceived reputational costs associated with certain tax planning strategies might further constrain these newly-public firms' tax avoidance (Hanlon and Slemrod, 2009). Finally, many private firms report substantial tax losses, suggesting that IPO-completing firms may simply be insensitive to corporate taxes and thus exhibit no change in their tax avoidance. Thus, the relation between IPOs and corporate tax planning is unclear *ex ante*.

It is important to study the relation between IPOs and tax planning for (at least) two reasons. First, considerable academic work and policy discussions focus on corporate tax planning by large public firms, but there is little evidence to understand how, why, and when these firms implement sophisticated strategies at the heart of this debate. The literature largely paints a stark contrast between private firms, which are characterized by substantial tax losses, and sophisticated public multinationals who arbitrage tax rates and incentives across jurisdictions to minimize worldwide tax burdens. Documenting whether and to what extent the IPO is an inflection point in firms' tax planning is necessary to bridge the findings

from these two streams. Second, there has been a substantial decline in the number of U.S. IPOs in the past two decades, which has contributed to the diminishing number of public U.S. firms overall (Chemmanur and He, 2011). Changes in tax remittances as a consequence of going public are important to understand, both to inform U.S. tax revenue estimates of corporate collections in light of this declining trend and given the ongoing debate about the appropriate structure of the U.S. corporate tax system (Yellen, 2021; McBride and Durante, 2022).

Despite the importance of this research question and the centrality of IPOs in corporate finance research, prior work is largely silent on the relation between corporate taxes and IPO completion. The lack of prior evidence may be attributable to two factors. The first is an econometric challenge, as the decision to complete an IPO is endogenous. Indeed, a common critique of the IPO literature is that it is difficult to disentangle the effects of capital infusion from the managerial decision to go public, due to the lack of an appropriate control sample against which to measure effects. The second factor is a data challenge: there are few data sources on private firms, impeding researchers' ability to observe changes in firm performance from the pre- to post-IPO period.

To address the econometric challenges, we compare tax planning by firms that complete an IPO (the treatment group) to three different control groups. We first follow Bernstein (2015) and construct a control sample of firms that similarly filed for an IPO but ultimately withdrew and remained private (the "withdrawn" control sample). However, because these firms ultimately chose to forego the IPO and thus may differ in ways that affect our inferences, we construct two additional control samples of firms: privately held firms that raised equity of a comparable magnitude to IPO firms but did not go public (the "constant private"

control sample), and publicly traded firms that had an equity issuance of a comparable magnitude to IPO firms and remained public (the "constant public" control sample). The use of multiple control samples permits us to evaluate the robustness of any observed effects and estimate a range of magnitudes across groups. Furthermore, use of the private and public samples permits us to distinguish the IPO event from other external financing events. To further mitigate concerns about differences between IPO and control firms, we employ inverse probability weighting to ensure the firms are similar across a number of observable characteristics.

We address the data availability challenge by using a panel of confidential U.S. corporate tax returns from both public and private firms. Our data span 1994 through 2018, with a focus on the seven years surrounding each firm's IPO filing. These data permit us to construct measures of both domestic and foreign firm tax avoidance and to observe firms for several years prior to the IPO.[2] Furthermore, the data permit construction of the three control samples, improving upon the approach in recent literature that generally relies on withdrawn firms as a benchmark.

Our first empirical tests study whether domestic tax planning changes after the IPO. Following Mills (1998), we measure domestic tax planning using book-tax differences (BTDs), which captures the extent to which financial reporting and taxable incomes diverge. We employ a stacked cohort difference-in-difference design in which we compare IPO-completing firms in each sample year to withdrawing firms in the same sample year, thus holding constant the applicable U.S. corporate income tax rates and accompanying incentives. We observe a

[2]Prior literature generally studies worldwide tax planning using consolidated tax measures from publicly available financial statements. Use of the tax data permits us to separately examine domestic and foreign tax planning, enabling a more precise analysis of the tax strategies employed by firms.

large increase in BTDs in the years immediately following the IPO completion, consistent with increased tax planning. We also find consistent results when alternatively using the private and public control samples that similarly had large capital infusions, confirming that the effect is unique to the IPO decision. Additional tests use an alternative measure of tax planning, the domestic cash effective tax rate (Cash ETR), which is decreasing in tax avoidance. Although this measure cannot be calculated for a large proportion of the IPO sample with tax losses, the results support the same conclusion: firms report lower ETRs following the IPO, meaning an increased level of tax planning activity. The effects are not explained by "mechanical" tax changes around the IPO event, such as increased stock option deductions or statutory limitations on tax loss utilization triggered by the transaction.

Second, we study international tax planning. Foreign tax planning is of particular importance given the considerable academic and policy focus on firms' ability to lower their tax obligations through shifting both income and physical presence to lower-taxed jurisdictions. We first observe that IPO firms report a higher likelihood of owning foreign tax haven subsidiaries and own more foreign tax haven subsidiaries after going public. When comparing foreign haven use to the public firm sample, we observe strong peer effects: IPO firms' use of havens converges to that of their publicly traded peers by the third year after the IPO.

Next, we extend the analysis to explicitly test whether IPO firms' foreign structures facilitate cross-border income shifting. To do so, we use tax data on the amount, type, payor, and payee of related party transactions. We find that foreign subsidiaries of IPO-completing firms are much more likely to be engaged in "cost-sharing" arrangements in the years immediately following the IPO as compared to the withdrawn and private firm control

samples.[3] We next observe that foreign subsidiaries of IPO firms report large increases in the level of receipts from both the U.S. parent and other related domestic entities ("outbound" payments from the U.S.), whereas we observe smaller and weaker effects for the amount of payments *to* the U.S. ("inbound" payments). This reflects that the U.S. tax rate was generally higher than that in foreign jurisdictions during the sample period, providing an incentive for firms to shift income out of the U.S. Finally, we refine the measures of related party receipts and payments to include amounts most commonly associated with income-shifting: revenues from inter-company sales of intangibles, management service fees, royalty payments and license fees on intangible assets, and interest on related party loans. We again observe that amounts paid for these items to foreign subsidiaries increase, whereas there is no change in amounts remitted to the U.S. parent. Collectively, the evidence shows that IPO transactions motivate firms to engage in substantial and long-term international planning that facilitates tax-motivated income shifting.

This paper contributes to the prior literature studying companies' tax planning strategies. This literature focuses almost entirely on well-established public firms, despite the large number and aggregate activity of private firms in the U.S. economy (Lisowsky and Minnis, 2020). More recent work examines tax planning in private firms, documenting different tax planning strategies in these companies (Hoopes et al., 2020; Badertscher, Katz and Rego, 2013; Badertscher et al., 2019; Olbert and Severin, 2021). The key innovation of our paper is to study tax planning as firms transition from private to public status, thereby bridging between the findings of these other papers and the multitude of studies examining public

[3]Cost-sharing agreements are specific income shifting strategies that enable firms to source sales revenue to foreign jurisdictions. See additional detail in Section 2.

firms' tax avoidance. This evidence is particularly important as recent literature suggests that the nature of firms' tax planning changes during this transition (Badertscher et al., 2019). In so doing, we add to work that has primarily focused on IPO-related investor taxes (Guenther and Willenborg, 1999; Li, Lin and Robinson, 2016), as well as recent work studying the effects of the TCJA (Edwards and Hutchens, 2020) and the "supercharged" IPO tax strategy on corporate behavior (Edwards, Hutchens and Rego, 2019).

The remainder of the paper proceeds as follows. In Sections 2 and 3 we discuss the empirical methodology and data sources, respectively. We present empirical results from our analyses in Section 4. We conclude in Section 5.

2 Empirical strategy

2.1 Research Design

To test the relation between IPO completion and firm tax planning, we estimate the following ordinary least squares (OLS) equation:

$$Y_{i\tau} = \beta \cdot IPOCompleted_i \cdot Post_{c\tau} + \gamma_{c\tau} + \delta_i + \epsilon_{i\tau}. \tag{1}$$

$Y_{i\tau}$ is one of several tax planning measures for firm i in tax year τ, described further in Section 2.2. $IPOCompleted_i$ is an indicator variable equal to one for firms that complete an IPO transaction, and $Post_{c\tau}$ is an indicator variable equal to one if $\tau \geq t$. We estimate Equation (1) use a stacked-cohort difference-in-differences empirical strategy in which we compare firms that complete an IPO—the "treated" firms—with three sets of control firms; the sample construction is described in Section 3. This empirical strategy compares firms

in a fixed window around each IPO transaction to control firms in the same window and reduces concerns about biased effects that could arise in a staggered difference-in-differences approach (Baker, Larcker and Wang, 2021). Year t is the year of treatment for cohort c, which is defined as either the year of IPO filing (based on use of an IPO withdrawn firm sample) or the year of equity issuance (when using two alternative control samples). For each treatment year t, we include all treatment and control observations from $t-3$ to $t+3$, requiring firms to be observable for at least two years pre- and post-treatment ($t-2$ through $t+1$). We then stack the cohorts in event time to test the effects of IPO completion on corporate tax planning.

We include firm fixed effects δ_i in Equation (1) to control for time-invariant firm characteristics. Each IPO firm is included in only the one cohort associated with the year of IPO filing, and therefore, the firm fixed effects are equivalent to firm-by-cohort fixed effects. We also include year-by-cohort fixed effects $\gamma_{c\tau}$, which control for cohort-specific time series trends. The inclusion of the firm and year-by-cohort fixed effects subsumes the main effect of $IPOCompleted_i$ and $Post_{c\tau}$. The term $\epsilon_{i\tau}$ is an additive error term.

Our main coefficient of interest, β, measures the difference between treatment and control firms following an IPO, relative to any difference between these firms in the pre-IPO period. We do not include time-varying firm controls in the regression because these variables are also potentially affected by the firm IPO. Consequently, their inclusion would likely introduce a "bad controls" problem and bias our estimates (Angrist and Pischke, 2008; Roberts and Whited, 2013).[4] We cluster standard errors by industry using the first three digits of

[4]For completeness, we conduct additional analyses that demonstrate results are robust to the inclusion of time-varying control variables, such as the firm's age (*Ln(Age)*), total sales (*Ln(Sales)*), financial performance (*Income/Sales*), debt (*Debt/Assets*), and investment (*Fixed Assets/Total Assets*). Variables are defined in Appendix A.

self-reported NAICS codes. Clustering standard errors at the industry level corrects for unobserved error correlation within industries, which is particularly important given that IPO waves may be driven by industry-specific productivity shocks (Chemmanur, He and Nandy, 2010).[5]

Estimation of Eq. (1) measures an average treatment effect across all post-IPO years, relative to all pre-IPO years. We also estimate year-specific treatment effects using the following equation:

$$Y_{i\tau} = \alpha + \sum_{\tau=t-3}^{t+3} \beta_\tau \cdot IPOCompleted_i + \gamma'_{c\tau} + \delta'_i + \epsilon'_{i\tau}, \tag{2}$$

where t is the year of treatment for cohort c, $\gamma'_{c\tau}$ are year-by-cohort fixed effects, δ'_i are firm fixed effects, and $\epsilon'_{i\tau}$ is an additive error term. We omit β_{t-1} from the regression, and thus all treatment effects β_τ are measured relative to the difference between the treatment and control groups in the year prior to IPO completion.

2.2 Tax Planning Measures

Given our use of U.S. income tax data, we first construct three measures that capture *domestic* tax planning. *BTDs* is equal to the difference between pre-tax financial statement income ("book" income) and taxable income. We select this measure because Mills (1998) demonstrates that book-tax differences are associated with tax avoidance, thereby capturing the construct of interest. Furthermore, this measure can be calculated for all firms in the sample, unlike alternative measures commonly used in the literature that require firms to report positive income (ETRs).

[5]Results are also robust to clustering at the firm level.

We construct *BTDs* using two schedules from IRS Form 1120, U.S. Corporation Income Tax Return. For years prior to 2004 or for firms with less than $10 million of assets, we define book income as Net income (loss) per books (Form 1120: Schedule M-1, line 1) plus federal income tax per books (Form 1120: Schedule M-1, line 2). For firms with greater than $10 million of assets since 2004, book income is equal to Net income (loss) per income statement of includible corporations (Form 1120: Schedule M-3, Part I, line 11), plus U.S. current income tax expense (Form 1120: Schedule M-3, Part III, line 1) and U.S. deferred income tax expense (Form 1120: Schedule M-3, Part III, line 2). From book income, we subtract Pre-NOL Taxable Income, which is a firm's taxable income before taking into account its net operating loss deduction (NOL) and other special deductions (Form 1120, Line 28). We scale the difference between book income and taxable income by assets in year t-1.

We use two additional measures of domestic tax planning when estimating Eq. (1). *BTD Tax* is a supplemental, tax-based measure of book-tax differences from Mills (1998), equal to federal tax expense for books (from IRS Schedule M-1 or Schedule M-3), minus taxes paid as reported on the U.S. income tax return. The other measure is *Cash ETR*, which is equal to cash taxes paid as reported on Form 1120, scaled by book income. Although this measure cannot be calculated for the large proportion of our sample reporting tax losses, we use this measure because it is so prevalent in the related tax literature. *BTD* and *BTD Tax* (*Cash ETR*) are increasing (decreasing) in tax avoidance.

Prior work demonstrates that the multinational nature of a firm and its foreign haven presence are associated with greater tax planning (Rego, 2003; Dyreng and Lindsey, 2009). Unfortunately, the requisite tax forms filed by U.S. companies to report their foreign subsidiaries' activity do not include analogous foreign book-tax difference information. There-

fore, we construct several alternative measures to study foreign tax planning from the available information on IRS Forms 5471, Information Return of U.S. Persons with Respect to Certain Foreign Corporations ("CFCs"). These forms permit us to observe the existence and jurisdiction of U.S. firms' foreign subsidiaries, as well as financial statement information and intercompany transaction data.

We use these international tax data to measure (i) the ownership of foreign tax haven subsidiaries (Dyreng and Lindsey, 2009) and (ii) related party payments used for tax-motivated income shifting (De Simone et al., 2019). *Tax Haven (0/1)* is an indicator equal to one if a firm owns a foreign tax haven subsidiary, and *# Tax Havens*, is the count of a firm's total tax haven subsidiaries. A third measure is *# Dot Havens*, the number of subsidiaries in "small as a dot" tax haven countries in which firms locate primarily for tax reasons. We determine whether a country is a tax haven based on the list in Hoopes et al. (2020); see Appendix A.

We identify the jurisdiction of the entity using address information listed on Form 5471. While the Form 5471 data permit observation of a company's foreign tax presence, there are two limitations to using this information. First, the data only include a subset of the largest foreign subsidiaries through 2003, thereby limiting our analysis to years 2004 and later. Second, even since 2004, data are only available in the even years (2004, 2006, etc.). Despite this latter limitation, we identify haven presence in each year since 2004 given that firms report both beginning- and end-of-year assets on the Form 5471 balance sheet (Schedule F). That is, we identify the presence of a tax haven in a prior (odd) year if firms report beginning-of-year balances on Schedule F.[6]

[6]There are a limited number of observations for which we observe assets in a given CFC in years $t+1$ and $t-1$ (the previous SOI sample year for the 5471 data), but not in year t. In these instances, we interpolate the observations as having a foreign subsidiary or tax haven in year t as well.

We also construct several additional measures of foreign tax planning based on data reported on Form 5471 Schedule G ("Other Information") and Schedule M ("Transactions Between Controlled Foreign Corporation and Shareholders or Other Related Persons"). These measures are intended to capture tax-motivated income shifting following De Simone, Mills and Stomberg (2019). The first measure identifies whether a CFC was party to a cost-sharing agreement, which is a specific transfer pricing planning strategy in which a foreign subsidiary shares in costs used to develop intellectual property ("IP"). By sharing in the costs, the foreign subsidiary is entitled to a portion of the subsequent revenue generated from the IP. The associated income is then taxed in the (often lower-taxed) jurisdiction, as opposed to in the U.S. *Cost Sharing (0/1)* is an indicator equal to one if a firm has at least one subsidiary reporting participation in such an agreement on Form 5471 Schedule G.[7]

To directly study transactions between the U.S. parent and its foreign subsidiaries, we use data on the amount of intercompany payments reported on Form 5471, Schedule M. We construct two variables, *Total amounts received from U.S.* and *Total amounts paid to the U.S.*, which equal the natural logarithm of one plus the total amounts received from or paid to the U.S. parent and other domestic corporations, respectively. Because a vertically integrated firm could naturally have higher levels of intercompany payments, we also construct two additional, refined measures that include only those line items most closely related to

[7]The specific question we use to identify this strategy is (based on the 2012 form), "During the year, was the foreign corporation a participant in any cost sharing arrangement?" A 2011 Bloomberg article provides one public example of a cost-sharing arrangement used by Cisco. It states, "Beginning in 1995, the Netherlands subsdiary began paying for part of Cisco's ongoing research in the U.S. under a cost-sharing agreement, according to company records. Like other U.S. technology companies, Cisco qualifies for U.S. tax benefits by doing most of its research and development domestically. By paying for some of it, the overseas unit can remove a chunk of any subsequent profits from the U.S. and claim them offshore" (Drucker, 2011). Note that Cisco went public in 1990 (prior to our sample period); however, this example is illustrative of the type of internal tax-motivated agreements that have previously been understudied in the literature due to the lack of available data.

tax-motivated income shifting: intercompany sales of property rights such as patents and trademarks, platform contribution transaction payments, cost sharing transaction payments, managerial and other service-related compensation, royalties and license fees, interest income (expense), and insurance premiums. *Total income-shifting-related receipts from U.S.* and *Total income-shifting-related payments to U.S.* is equal to the natural logarithm of one plus the total amounts received from or paid to the U.S. and other domestic companies for these items, respectively. A fifth measure, *Total income-shifting-related receipts from U.S. and foreign*, also includes intercompany receipts from other related foreign entities. Use of these measures permits us to not only study whether total related party payments increase around the IPO, but if the amounts paid to foreign subsidiaries exhibit a different effect relative to the amounts paid to the U.S. parent. Such differential effects would be further suggestive of tax-motivated income shifting out of higher tax countries (like the U.S.) to lower-taxed foreign jurisdictions.

3 Data and Summary Statistics

3.1 Sample construction: IPO Filers

We construct the sample using data on firm IPOs from Thompson Financial, the SEC's EDGAR platform, and administrative tax records derived from U.S. corporate tax returns. Table 1 shows the sample selection steps for constructing the sample of IPO-completing firms, as well as the sample of firms that file but ultimately withdraw the IPO (the first control sample). The firms that withdraw their filing are perhaps the most natural comparison group

because they are at a similar life cycle stage and have similar incentives to go public. To the extent that the IPO *completion* decision is exogenous to tax planning outcomes, use of the withdrawn firms as a control group permits clean identification of the effect of an IPO on corporate tax planning.[8]

We draw the dataset of U.S. firms that complete or withdraw an IPO from the Thompson Financial's SDC Platinum New Issues database. We retain IPOs between 1996 and 2015, starting in 1996 because that was the first year in which firms were required to file with EDGAR ($n = 22,484$ events). Following standards in the literature (see, for example, Lowry, Michaely and Volkova (2017)), we exclude financial firms and utilities, as well as spin-offs, American depository receipts (ADRs), real estate investment trusts (REITs), closed-end funds, blank-check firms, and unit funds. We restrict the sample to only include issuances of common stock. As tax employer identification numbers (EINs) necessary for merging with the tax data are only available for a limited subset of firms in the SDC IPO sample, we scrape the EIN number from the IPO filing on the EDGAR platform when available. IPOs for which the EIN or the IPO filing date cannot be pulled from EDGAR are excluded. Approximately six percent of firms have multiple IPO events (i.e., they file for IPO, withdraw, and file again later). For these firms, we discard all but the earliest event. The final IPO sample includes 3,593 unique IPO events: 2,411 completed IPOs and 1,182 withdrawn IPOs.

Using firm EINs, we merge the IPO data with a stratified random sample of U.S. corporate tax returns covering tax years 1994 to 2018 so as to observe pre- and post-period observations for the sample of 1996-2015 IPO events. This stratified random sample is constructed,

[8]Bernstein (2015) finds that exogenous factors, such as economy-wide market conditions, influence IPO completion, suggesting that IPO completion may indeed be exogenous to tax planning.

cleaned, and edited each year by the Statistics of Income (SOI) division of the Internal Revenue Service. In total, we merge approximately 88% of the IPO firms to the SOI sample: 2,125 IPO-completing firms (21,985 firm-years) and 841 IPO-withdrawing firms (6,550 firm-years). From the merged sample, we drop observations that do not have C-corporation filing status, those with non-positive values for either gross receipts or total assets, and those missing industry information.[9] Because our research question relates to the effect of an IPO transaction on firm tax planning, we require all firms to be present in the sample for at least years $t-2$ through $t+1$ relative to the IPO filing in year. We drop observations outside of the $t-3$ to $t+3$ window to isolate the tax planning effects immediately surrounding the IPO. The final sample includes 5,683 observations for 900 firms, of which 75% complete the IPO.

3.2 Sample construction: Alternative Control Samples

While use of the withdrawn firms as a control sample follows prior literature, we acknowledge that there could be unobserved differences between IPO completing and withdrawing firms that may drive any observed tax planning result. To mitigate concerns about these unobserved differences, we also use two additional control samples composed of private and public firms of similar size that also receive external financing (but do not switch their private-public firm status). Not only does the use of these samples permit us to assess the sensitivity of effects, but it also mitigates concerns that the effects are attributable to *any* external capital infusion. That is, observing a relation between IPO completion and tax

[9] If industry information is missing or invalid, we attempt to alternatively identify industry affiliation using the firm's earliest-reported valid NAICS code in the tax data. Thus, we only drop those firms for which this industry classification procedure fails.

planning when using these alternative samples helps identify that the effects are specific to IPO transactions.

We follow similar procedures to construct the alternative control samples, as outlined in Table 1. The starting samples include all parent-level C corporations in the SOI sample from 2004 to 2018 (157,943 private firms and 10,771 public firms). We start in 2004 because we require information from U.S. tax form Schedule M-3 (introduced in 2004) to identify whether a firm is public or private.[10] As with the IPO filer sample, we require firms to have positive gross receipts and gross assets, and we exclude finance and utilities firms.

Next, we restrict the samples to include only those firms with equity issuances of comparable size to the equity issuances reported by the IPO firms in our sample.[11] We also drop any firms with assets larger than the IPO completing firms to ensure size comparability, as well as firms with a change in private-public status. Imposing the additional restriction related to industry affiliation and requiring the presence of pre- and post-period observations for each cohort results in a sample of 2,905 firms (18,588 firm-years) for the private control sample and 1,691 firms (11,014 firm-years) for the public control sample.

Figure 1 shows the number of completed and withdrawn IPOs by year of IPO filing in Panel a, the number of completed IPOs and privately held control firms by year of equity issuance in Panel b, and the number of completed IPOs and publicly held control firms in Panel c. The number of completed and withdrawn IPOs both peaked in 2000, prior to the 2001 recession. For the privately held control firms, there are around 300 firms with equity

[10]We also report results using the withdrawn firm sample over this shorter time period for comparison purposes; see Section 4.

[11]Specifically, we require that firms report an equity issuance at or above the 10th percentile of the level change in equity in our IPO sample, as well as the 10th percentile of the percentage change in equity in our sample. If a firm has more than one equity issuance of this size, we use the first equity issuance.

issuances of comparable magnitude to our IPO completing firms from 2006 to 2010. The count then dips slightly in 2011 to 2013 before increasing in 2014. In 2015, there are more than 350 privately held firms with large equity issuances in our sample. For the publicly held control firms, in contrast, there are more firms with large equity issuances prior to the Great Recession: over 250 in 2006 and 2007. After a dip in 2008 during the recession, the count rebounds to around 250 in 2009 before falling to approximately 100 to 150 for the remainder of the sample.

Figure 2 presents the trends in equity issuance (Panels a, b, and c) and equity financing (Panels d, e, and f) around the IPO for the three samples. *Equity issuance* is defined following Yagan (2015) as non-negative changes in equity from the prior to the current year (Column D of Form 1120: Schedule L, lines 22b and 23). *Equity financing* is defined as equity issuance as a share of total equity plus total debt (Column D of Form 1120: Schedule L, lines 16-21). For the withdrawn firm control group (Panels a and d), we present trends relative to t as the year of IPO filing, whereas for the private and public control groups (Panels b, c, e, and f), we show trends relative to t as the year of equity issuance.

Panels a and d suggest that most IPOs are completed in the same year of filing, with some additional IPO-related equity issuance occurring in $t + 1$. Panels b and e show that our private-firm control group has very similar magnitudes of equity issuance and equity financing relative to IPO firms. In contrast, panels c and f show that the dollar level of equity issuance in t is considerably larger for public firms than for IPO firms (around \$400 million compared to \$150 million), but that equity financing (the percentage increase in equity) is larger overall for the IPO firms than the public firms in the year of issuance.

3.3 Summary statistics

Table 2 displays summary statistics after inverse probability weighting (IPW) the treatment firms to each of the three control samples. IPW helps to mitigate concerns about differences in the samples by weighting based on observable firm characteristics. For each sample, we first predict the likelihood of IPO completion in the year prior to IPO filing (for the sample of withdrawn firm controls) or the year prior to equity issuance (for the samples of private and public firm controls) using a number of firm characteristics: firm age and size (*ln(Age)* and *ln(Assets)*), proxies for for firm investment opportunities and growth opportunities (*Sales Growth* and *Asset Growth*), financial performance (*pre-NOL Income/Sales*), asset tangibility (*Net PPE/Assets*), and leverage (*Debt/Assets*). All variables are defined in Appendix A. We then weight the completing and control firms according to the predicted values derived from the first stage estimation. To eliminate the influence of outliers, we winsorize variables at the five percent level following prior work that uses corporate tax data (Cohn, Mills and Towery, 2014; Zwick and Mahon, 2017; Yagan, 2015).[12]

Starting with the domestic tax planning variables, we observe that *BTD* is on average negative across the three samples, meaning that taxable income exceeds book income. The alternate measure *BTD Tax* has a mean and median of zero. The average domestic *Cash ETR* ranges from 19-20 percent across the three samples, whereas the median is lower: 17.2 percent, 11.8 percent, and 16.2 percent for the withdrawn IPO, private firm, and public firm samples, respectively.

Tax haven utilization is, unsurprisingly, the largest in the sample that includes public

[12]When alternatively winsorizing at the 1% level, we generally find larger effects - consistent with large outlier values affecting the point estimates. These results confirm the choice to more conservatively winsorize at the 5% level.

firm controls; in this sample, firms have 3.3 tax havens on average and report a 40.6 percent likelihood of tax haven use. For comparison, the withdrawn firm and private samples report 0.76 and 0.75 tax havens (27.4 and 25.4 percent incidence of haven ownership), respectively. The magnitudes of the income-shifting variables are comparatively quite small across all samples. Approximately 1.2 percent of observations in the withdrawn IPO firm sample have a cost-sharing agreement, compared with over two percent of observations in the private-firm sample and four percent in the public firm sample. The overall low magnitudes of total transfers and income-shifting payments are primarily attributable to the large number of firms that report zero values for these transactions.

We examine the similarity between the treatment and control samples in Table 3. Panel A presents a comparison of the IPO Completers and the control firms; Panels B and C present the comparison for the private and public firm control samples, respectively. Descriptive statistics in this table show that, after inverse probability weighting the three samples, the IPO completers and control firms are statistically similar across these firm characteristics. The private firms are approximately three years older than the IPO completing firms on average; while this difference is statistically different, the logged transformation used in the empirical tests is not. Section 4 discusses the similarity of pre-period tax planning trends based on graphical evidence comparing the IPO completing firms to each of the control firm samples.

4 Findings

4.1 Domestic Tax Planning

Table 4 presents results from estimating Eq. (1) using domestic tax planning measures. Panel A, Columns (1) through (4) include results using BTD as the dependent variable; Columns (5) through (8) report results using *BTD_tax*. Columns (1) and (5) report results for withdrawn firms for the full sample period (1994-2018), while Columns (2) and (6) report results for the withdrawn firms corresponding to the shorter sample of the private and public control firms group (2004-2018). Columns (3) and (7) report results for the private firm control sample; Columns (4) and (8) report results for the public firm sample.

Across seven of the eight columns, we find an increase in tax avoidance after the IPO. Results for BTD are consistently significant, with point estimates suggesting an increase in BTD of 2.1 to 4.8 percent after the IPO, depending on the control sample. Figure 3 plots year-specific treatment effects obtained from estimation of Equation (2); Panels a, b, and c correspond to the withdrawn firm, private firm, and public firm control samples, respectively. These graphs first confirm that the three samples generally exhibit parallel trends in BTD in the years preceding the IPO. The figure also shows the statistically significant increase in BTD after the IPO, consistent with the results presented in Table 4. Columns (5) through (7) confirm that we also observe statistically significant increases of 0.2-0.5 percent when using the alternative *BTD_tax* measure.

Table 4, Panel B presents results alternatively using domestic *Cash ETR* as the measure of tax planning. Recall that this measure is decreasing in tax avoidance, meaning that a negative coefficient implies an increase in tax avoidance. Although the effects are estimated

on a smaller sample given the large number of firms reporting tax losses, we observe results consistent with the inferences in Panel A. That is, *Cash ETR* declines after the IPO, with effects of 8.3 percent, 6.5 percent, and 3.9 percent relative to the withdrawn firms, private firms, and public firms, respectively. Figure 3 presents these effects graphically in Panels (d), (e), and (f). We observe ETR trends that are statistically similar between the treatment and control groups in the years prior to the IPO for the withdrawn IPO and private firm control samples. For these two samples, we observe a decline in *Cash ETR* for completed IPOs as compared to the control groups after the IPO. We also observe a similar decline for the public firm control sample, but we acknowledge that the pre-period trends are dissimilar and thus cannot conclude that the effects for this sample are attributable to the IPO.

We conduct several additional tests to better understand the causes of the observed changes in domestic tax planning. One possible explanation is that the results are driven by mechanical effects induced by the IPO event, such as a spike in stock option deductions attributable to the exercise of employee options. In untabulated tests, we re-estimate Eq. 1 after replacing *BTD* with the specific book-tax difference amount related to stock option exercise. While firms do indeed report an increased book-tax difference associated with this effect, the coefficient is only approximately 30 percent of the effect estimated with *BTD*. Thus, while the stock option deduction explains some of the change, it cannot explain the overall effect observed in Table 4.

We also consider whether the results are attributed to statutory restrictions imposed on the use of tax losses after large changes in ownership, such as the one that occurs in an IPO. Specifically, U.S. tax law imposes a statutory limitation that reduces the amount of

loss carryforwards a firm can use to offset post-IPO income.[13] While the *BTD* measure should be relatively unaffected by this measure because it is constructed using pre-NOL taxable income, we conduct two additional tests to further mitigate concerns about this mechanically driving results. First, we construct an indicator equal to one for those firms that appear to be affected by this limitation in the post-NOL period; that is, we identify those firms that claim lower levels of tax loss carryforwards post-IPO than they should otherwise be able to report. We find no statistically significant change in this incidence in the post-IPO period. Second, we drop the small number of firms for which the statutory limitation appears to bind. The effect of the IPO on tax avoidance slightly declines, but the effects remain statistically significant at the 1% level.[14]

In summary, the analysis presented in Table 4 shows that IPOs are associated with increased tax avoidance in the years immediately following the IPO event. We observe these effects across three different control samples, confirming that the effect is robust and is distinct to IPOs. Furthermore, observing the effect in both the long sample period from 1994 through 2018 for the withdrawn firm sample, as well as for the shorter period 2004 through 2018 for the public and private firm sample with requisite data, suggests that the results are not driven entirely by the "dot-com" IPOs of the early 2000's. Thus, the results suggest that the documented effects generalize to IPO events in the most recent preceding time period.

[13]Specifically, Internal Revenue Code Section 382 limits the ability of a firm to use its NOLs if the firm has had a greater than 50% ownership change over a three year rolling period. Briefly, the amount of the limitation is equal to the firm's market value at the time of the ownership change, times the federal long-term tax-exempt rate. See Erickson and Heitzman (2010) and Sikes, Tian, and Wilson (2014) for more discussion of this limitation.

[14]For example, the coefficient in the sample using withdrawn firms (private firms) as a control group declines from 0.048 to 0.039 (0.032 to 0.027).

4.2 Foreign Tax Planning

We first study firm foreign tax planning activity with a graphical analysis of trends in tax haven use around the IPO. Figure 4, Panels (a) and (b) show the average levels of *Havens(0/1)* and *#Havens* for firms that complete the IPO, as well as the private and public firm control groups in the years around equity issuance (year t). The top graph provides striking evidence that the IPO event is indeed an inflection point at which firms put in place foreign tax planning. At the beginning of the sample period – three years prior to the IPO – the probability of having a tax haven is similar between the private firm sample and the IPO completing sample. However, tax haven incidence begins to climb for IPO firms; it increases to 35 percent by the year of the IPO and converges to the level reported by public firms within three years of the IPO event. In contrast, the private firms exhibit only a slight increase in the probability of having a haven, remaining at a 15-20 percent level throughout the cohort window. Panel B exhibits a similar trend, with the number of havens tracking the trend exhibited by public firms.

While Figure 4 provides compelling evidence about changes in foreign planning around the IPO, we acknowledge that - unlike with the domestic planning measures - the IPO completing firms do not exhibit parallel trends for these particular foreign outcomes. Thus, when estimating Eq. 1 to study the foreign tax planning variables, we use only the withdrawn firm control sample for which similar trends are indeed observed in the pre-IPO period. Table 5, Panel A presents the empirical results for estimating the effect of IPO completion on *Havens (0/1)* in Column (1) and *#Havens* in Column (2). The probability that an IPO-completing firm has a haven subsidiary increases by 14.9 percentage points; we also

observe that the number of havens increases by 21.7 percent. Furthermore, estimation using #DotHavens in Column (3) shows an increase of 10.0 percent, further underscoring that the increased haven presence seems driven by tax motivations as opposed to simply growth and expansion of the IPO firm.

Figure 5 presents year-specific treatment effects estimated using Equation (2). Panel A presents the effects for *Haven (0/1)*, first showing that the IPO completing firms indeed exhibit similar trends as the withdrawn sample prior to the IPO event. After completion, the incidence of haven use diverges, with IPO completers reporting a much higher incidence. We see similar effects in Panel B, which plots the results for *#Havens*; specifically, we see similar levels in the pre-IPO period, with an increasing trend in the number of havens that is statistically different by year t+3.

Table 5, Panel B reports results for the measures that directly capture IPO firms' income shifting activities. In Column (1), we observe a positive and statistically significant effect for *Cost sharing agreement (0/1)*; the coefficient means that the likelihood of having a subsidiary participate in such arrangement increased by 3.8 percentage points.

In Column (2) and (3), we observe that the total amount of payments between the U.S. and foreign subsidiaries increased after the IPO; the coefficient of 1.093 in Column (2) means that *Total amounts received from U.S.* increased by over 100%. While we observe that *Total amounts paid to U.S.* also increased by 48.3 percent, we note that this coefficient is approximately half (and is slightly weaker) than the effect observed in Column (2). The difference in the size of coefficients is suggestive of greater incentive to report amounts in (potentially lower-taxed) foreign subsidiaries after the IPO.

Column (4) reports results for *Total income shifting receipts from U.S.*, which is the

refined measure of items paid to the foreign subsidiary that most likely relate to income shifting (sale of intangibles, royalty payments, licensing fees, etc.). We find that these payments also increase substantially, and that the result is similar if we also include related party payments from other foreign subsidiaries in Column (5).

Taken together, these results in Panels A and B of Table 5 suggest that foreign tax planning activity increases notably after a firm goes public.

5 Conclusion

This paper studies the extent to which domestic and foreign tax planning activity changes around U.S. IPOs. Using confidential corporate tax data for a large sample of firms that completed and withdrew IPOs between 1996 and 2015 (using data from 1994 through 2018) or issued equity as private or public firms between 2004 and 2015, we find that firms increase domestic tax planning activity after the IPO as well as foreign tax planning–specifically the utilization of tax havens and income shifting-related transactions.

There has been a striking decline in publicly listed firms in the United States in the past two decades, which has raised questions about the reasons for the decline and about the potential consequences of fewer public firms. Our results document the transition from private to public status and demonstrate how this transition is associated with changes in firm tax planning activity. We look forward to future work that continues to evaluate the tax implications of IPO transactions.

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Table A1: Variable Definitions

Variable	Source
Age	Tax year minus year of incorporation (Form 1120: box C)
Amts rec'd from U.S.	ln(Form 5471: Schedule M, line 12(b+c) + $1,000)
Amts paid to U.S.	ln(Form 5471: Schedule M, line 24(b+c) + $1,000)
Assets	Form 1120: Schedule L line 15(d)
Asset growth	Percentage change in *Assets* from the prior year to the current year
Book-tax differences (BTD)	[*Book income (loss) + Federal income tax expense - Pre-NOL taxable income (loss)*]/Prior year *Assets*
BTD tax	[*Federal income tax expense - Taxes Paid*]/Prior year Assets
Cash ETR	*Taxes Paid*/[*Book income (loss) + Federal income tax expense*]
CFCs (0/1)	Indicator variable equal to 1 if *# CFCs* > 0
# CFCs	Total number of controlled foreign corporations (CFCs) reported using Form 5471
Cost sharing agreement (0/1)	Indicator variable equal to 1 if firm answers "Yes" to Form 5471, Schedule G, Question 4 ("During the tax year, was the foreign corporation a participant in any cost-sharing arrangement?") for at least one CFC
Debt	Form 1120: Schedule L line 17(d) + line 20(d)
Debt/Assets	*Debt*/*Assets*
# Dot havens	Indicator variable equal to 1 if at least one CFC is located in one of the tax haven countries designated below in the definition of a tax haven with an asterisk.
Equity financing	*Equity Issuance*/[*Debt + Total paid-in capital*]
Equity issuance	Change in *Total paid-in capital* from the prior year minus the change in *Treasury stock* from the prior year.

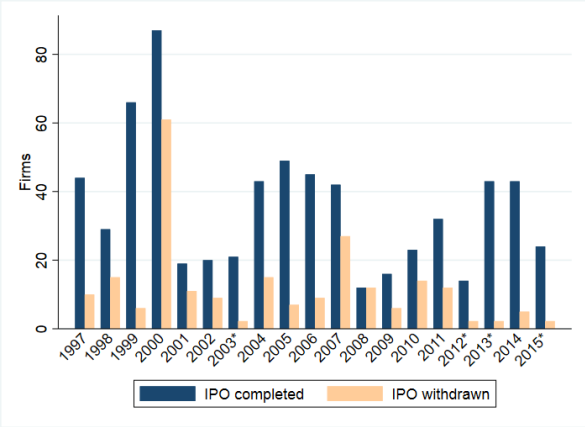
Table A1: Variable definitions (continued)

Variable	Source
Federal income tax expense	When a firm reports attaching a Schedule M-3 to the Form 1120 (Box A4): U.S. current income tax expense (Schedule M-3, Part III, line 1) U.S. deferred income tax expense (Schedule M-3, Part III, line 2)]. When a firm does not report attaching a Schedule M-3 or in the years the Schedule M-3 was not utilized: Federal income tax per books (Schedule M-1, line 2).
Income-shifting receipts from U.S.	ln(Form 5471: Schedule M, line 24 + $1,000)
Industry	First two digits of Form 1120: Schedule K, line 2a
*IPO Completed*Post*	Indicator variable equal to 1 for firms that complete an IPO, sourced from SDC Platinum and SEC filings
ln(Assets)	Natural log of *Assets*
ln(Sales)	Natural log of *Sales*
Net PPE	Form 1120: Schedule L line 10b(d)
Net PPE/Assets	*Net PPE*/*Assets*
Pre-NOL taxable income (loss)	Form 1120: line 28
Pre-NOL taxable income/Assets	*Pre-NOL taxable income*/Prior year *Assets*; when missing prior year *Assets*, use beginning-of-year assets from Form 1120: Schedule L line 15(b)
Pre-NOL taxable income/Sales	*Pre-NOL taxable income*/*Sales*
Pretax financial income	When a firm reports attaching a Schedule M-3 to the Form 1120 (Box A4): Net income (loss) per income statement of includible corporations (Schedule M-3, Part I, line 11). When a firm does not report attaching a Schedule M-3 or in the years the Schedule M-3 was not utilized: Net income (loss) per books (Schedule M-1, line 1).
Sales	Form 1120: line 1c
Sales growth	Percentage change in *Sales* from the prior year to the current year
Taxes Paid	Form 1120: Schedule L line 31
Tax havens (0/1)	Indicator variable equal to 1 if # *Tax havens* > 0

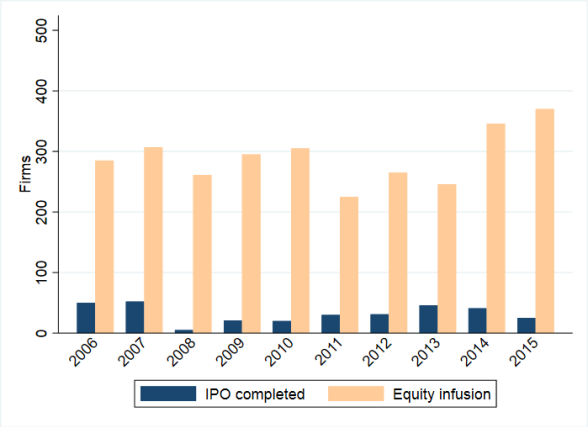
Table A1: Variable definitions (continued)

Variable	Source
# Tax havens	Total number of controlled foreign corporations (CFCs) reported on Form 5471 and located in one of the following countries: Andorra*; Antigua and Barbuda*; Aruba*; Bahamas*; Bahrain*; Barbados*; Belize*; Bermuda*; Botswana; British Virgin Islands; Brunei Darussalam; Cape Verde; Cayman Islands; Cook Islands; Costa Rica; Cyprus*; Dominica*; Gibraltar*; Grenada*; Guernsey and Alderney*; Hong Kong; Ireland; Isle of Man*; Latvia; Lebanon; Liberia; Liechtenstein*; Luxembourg*; Macau*; Maldives; Malta*; Marshall Islands; Mauritius*; Monaco; Monserrat; Nauru*; Netherlands Antilles*; Niue*; Palau; Panama; San Marino; Samoa; Seychelles; Singapore; St. Kitts and Nevis*; St. Lucia Island*; St. Vincent and the Grenadines*; Switzerland; U.S. Virgin Islands; Uruguay; Vanuatu*.
Total paid-in capital	Form 1120: Schedule L lines 22b(d) + 23(d)

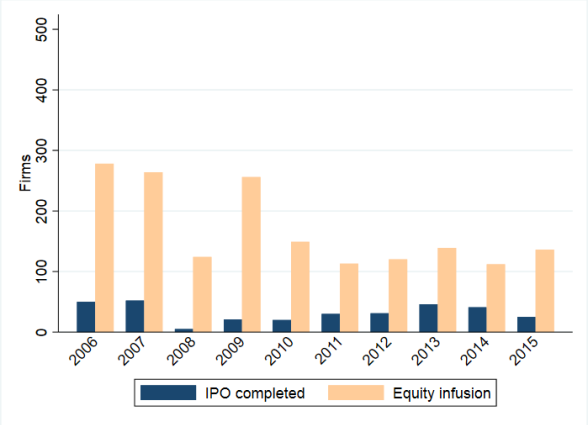
Figure 1: Firm counts



(a) Withdrawn IPO control group



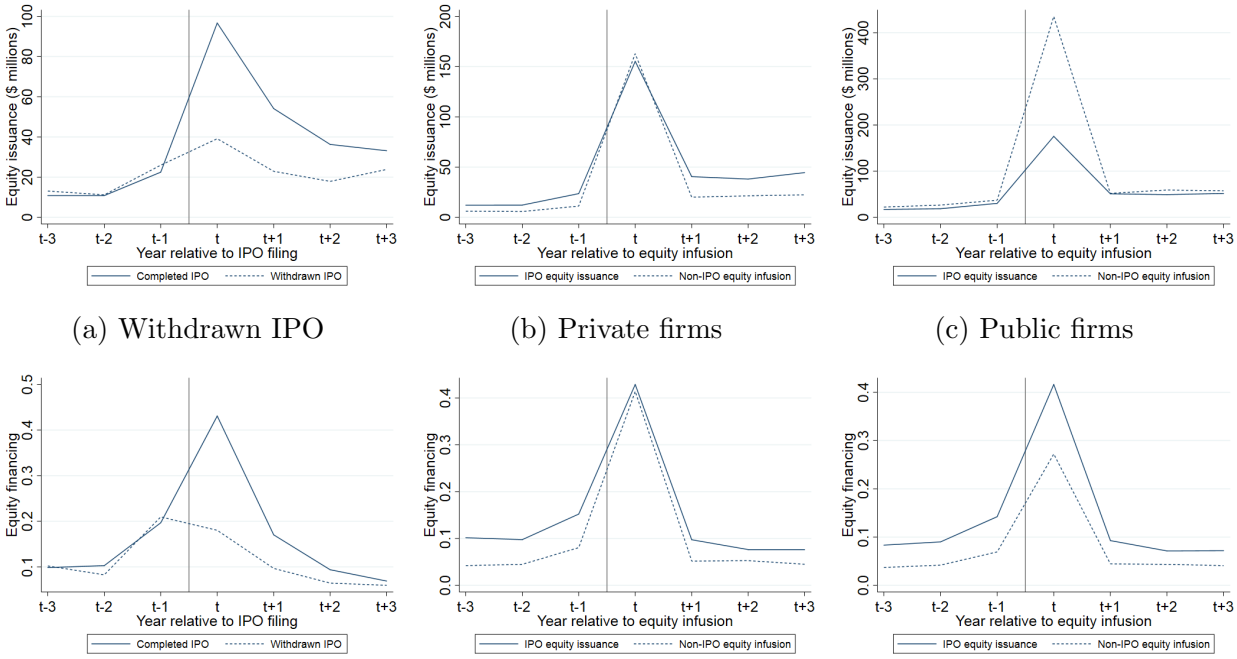
(b) Private firm control group



(c) Public firm control group

The figure shows counts of firms included in our sample. All three panels show IPO completers in navy (dark). In orange (light), Panels (a), (b), and (c) show the set of possible control groups of IPO withdrawers, private firms with equity infusions, and public firms with equity infusions, respectively. In Panel (a), to protect taxpayer privacy, we blur the counts of IPO withdrawers for the years marked by asterisks: 2003, 2012, 2013, and 2015. Specifically, we calculate the average IPO-withdrawer firm count pooling across all such years, and present this average value for each year. All panels were created by the authors using cleaned and edited data from the IRS.

Figure 2: Equity issuance and financing trends around IPO filing and equity infusion

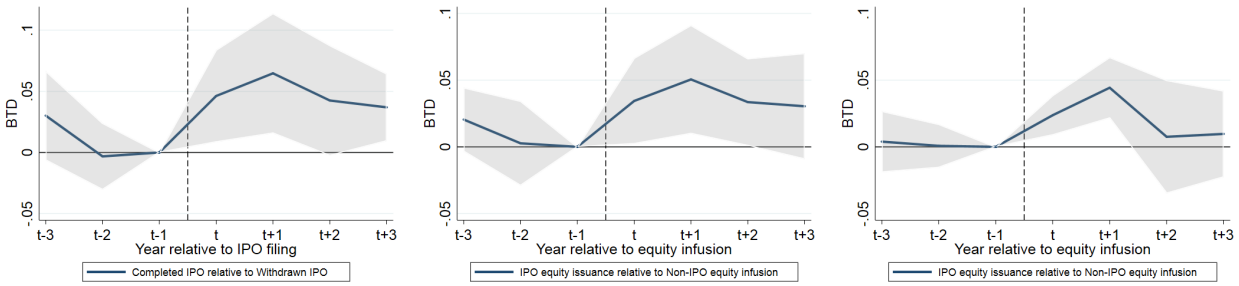


(a) Withdrawn IPO (b) Private firms (c) Public firms

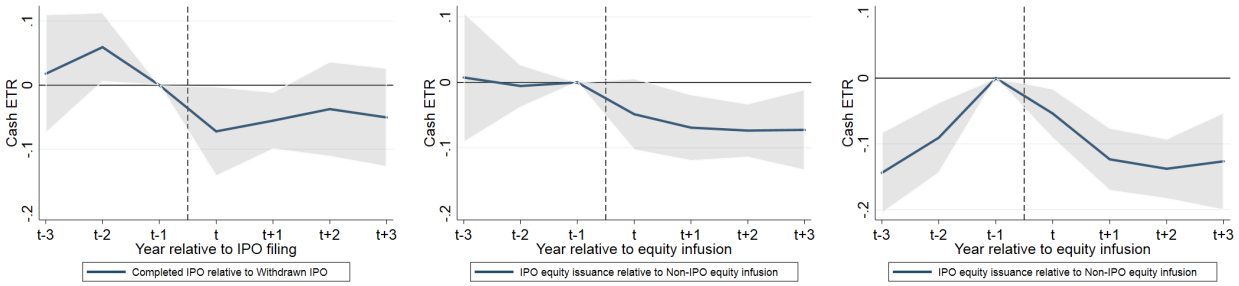
(d) Withdrawn IPO (e) Private firms (f) Public firms

The figure presents averages of equity issuance (Panels (a), (b), and (c)) and equity financing (Panels (d), (e), and (f)) around an IPO filing event or equity infusion event. The charts show amounts for firms that complete an IPO and for three sets of control firms: firms that filed for an IPO but later withdraw their IPO filing (Panels (a) and (d)), privately held firms that issued equity of comparable magnitudes to the IPO firms and remained private afterwards (Panels (b) and (e)), and publicly held firms that issued equity of comparable magnitudes and remained public (Panels (c) and (f)). Amounts for IPO completing firms are shown with a solid line; amounts for each control sample are shown with a dashed line. Year t is the year of IPO filing in Panels (a) and (d) and is the year of equity issuance in the other panels. All panels were created by the authors using cleaned and edited data from the IRS. Variables are defined in Appendix A and sample selection is explained in Section 3.

Figure 3: Tax planning around the IPO



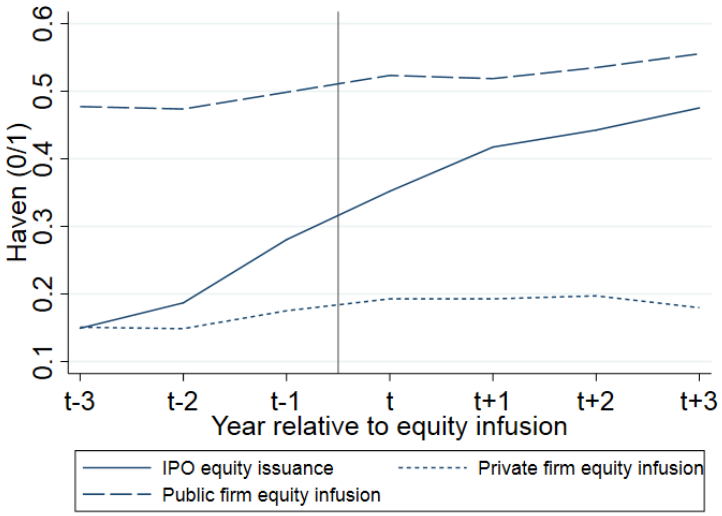
(a) Withdrawn IPO (b) Private firms (c) Public firms



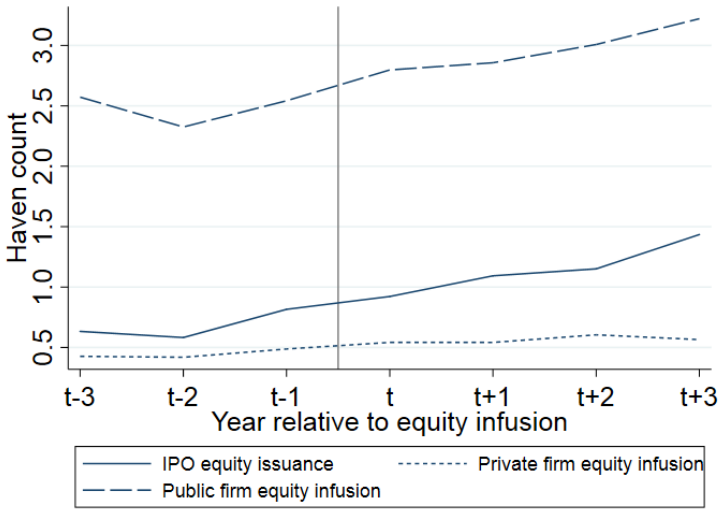
(d) Withdrawn IPO (e) Private firms (f) Public firms

This figure presents results from difference-in-differences regressions studying domestic Book-Tax Differences (Panels (a), (b), and (c)) and domestic Cash Effective Tax Rates (Panels (d), (e), and (f)) around the time of IPO filing. Firms that complete an IPO are compared to three sets of control firms: firms that filed for an IPO but later withdraw their IPO filing (Panels (a) and (d)), privately held firms that issued equity of comparable magnitudes to the IPO firms and remained private afterwards (Panels (b) and (e)), and publicly held firms that issued equity of comparable magnitudes and remained public (Panels (c) and (f)). Year t is the year of IPO filing in Panels (a) and (d) and is the year of equity issuance in the other panels. The chart plots year-specific treatment effect coefficients (relative to year $t-1$) and 95-percent confidence intervals from estimating Equation (2) for firms that complete an IPO and the relevant control sample; see Section 2. All panels were created by the authors using cleaned and edited data from the IRS. Variables are defined in Appendix A, and sample selection is explained in Section 3.

Figure 4: Tax haven trends around the IPO



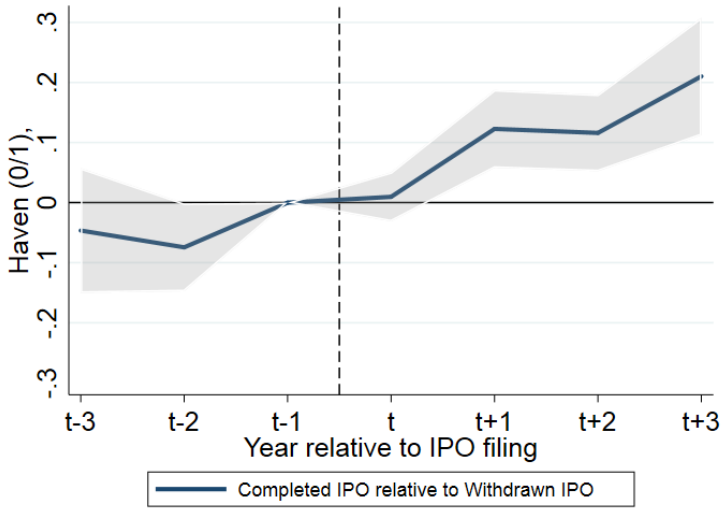
(a) Tax Haven CFC (0/1)



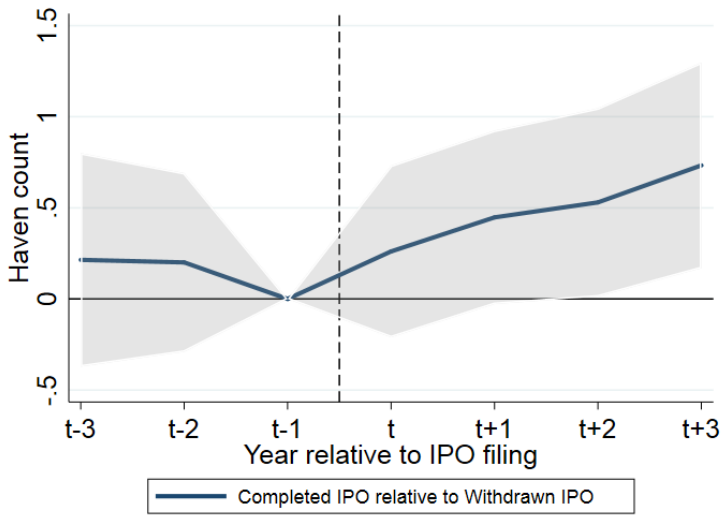
(b) # Tax Haven CFCs

The figure presents trends in foreign subsidiary activity for firms that complete an IPO as well as control firms that received an equity issuance around the time of the equity issuance. Panels (a) and (b) show the average likelihood of having a controlled foreign corporation ("CFC") in a tax haven, and the average number of CFCs in tax havens, respectively. Both panels depict outcomes of firms that complete an IPO (solid line) as well as the constant-private (short dashed line) and constant-public (long dashed line) control samples. Year t in each chart is the year of equity infusion. All panels were created by the authors using cleaned and edited data from the IRS. Variables are defined in Appendix A and sample selection is explained in Section 3.

Figure 5: Tax haven use around the IPO



(a) Tax Haven CFC (0/1)



(b) # Tax Haven CFCs

This figure presents results from difference-in-differences regressions studying Controlled Foreign Corporation (CFC) presence in tax havens around the time of IPO filing. Dependents variables used include an indicator for having a CFC in a tax haven (Panel (a)), and for the number of tax haven CFCs (Panel (b)). Firms that complete an IPO are compared to those that filed for an IPO but later withdraw their IPO filing. Year t is the year of IPO filing. The chart plots year-specific treatment effect coefficients (relative to year $t-1$) and 95-percent confidence intervals from estimating Equation (2); see Section 2. All panels were created by the authors using cleaned and edited data from the IRS. Variables are defined in Appendix A and sample selection is explained in Section 3.

Table 1: Sample criteria

Panel A: IPO selection

	IPO events	
	Events remaining	**% Completed**
U.S. IPOs from SDC Platinum, 1996 to 2015	22,484	0.27
Exclude finance/utilities industries	17,985	0.26
Exclude unit offers, closed-end funds, REITS, ADRs, LPs, blank check companies, and spin offs	5,975	0.60
Exclude issuance of noncommon stock	5,866	0.60
Observe EIN from SEC Edgar and observe IPO filing dat	3,800	0.67
Retain first IPO event per firm	3,593	0.67
Final sample of IPOs to merge with U.S. tax data	3,593	0.67

Panel B: Tax data for treatment and control samples

	Completed IPO Sample		Withdrawn IPO Sample		Private Firm Sample		Public Firm Sample	
Sample selection: Completed and withdrawn IPO samples	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining	Firms remaining	Firm-years	Firms remainin	Firm-years
Firms representing the IPO sample above	2,411		1,182					
Match to SOI tax data	2,125	21,985	841	6,550				
Drop IPO-withdrawer observations after a completed	2,125	21,985	838	5,731				
Require C-corporation status	2,107	21,603	815	5,519				
Require positive gross receipts and total assets	2,024	19,219	786	4,794				
Sample selection: Private and public firm samples								
All parent-level C corporations in SOI sample, 2004-2018					157,943	768,847	10,771	69,570
Require positive gross receipts and total assets					127,692	630,278	9,738	62,800
Exclude finance/utilities industries					104,894	502,381	7,492	47,695
Require equity issuance meeting our criteria					3,454	31,841	1,932	20,805
Drop observations if public-private status changed					(omitted)	30,571	(omitted)	20,002
Sample selection, all samples								
Require industry affiliation	1,990	19,177	765	4,765	2,905	30,526	1,691	19,995
Drop 1996 IPO cohort due to lack of withdrawers	1,914	18,521	765	4,765	2,905	30,526	1,691	19,995
Require firms present *t-2* to *t+1*	672	8,469	228	2,284	2,905	30,526	1,691	19,995
Retain observations in years *t-3* to *t+3*	672	4,317	228	1,366	2,905	18,588	1,691	11,014
Final samples	**672**	**4,317**	**228**	**1,366**	**2,905**	**18,588**	**1,691**	**11,014**

This table presents the selection steps for the treatment sample of completed IPOs and the three control samples: the withdrawn IPO sample, the private firm equity issuance sample, and the public firm equity issuance sample. Panel A presents the steps to identify U.S. IPOs for non-financial firms from 1996 to 2015 from SDC Platinum following Lowry et al. (2017); the firm-year observations correspond to the period 1994 through 2018 for these firms to permit observation of the pre- and post-IPO years. Panel B shows the number of distinct completed and withdrawn IPOs that merge with the U.S. corporate income tax data, as well as the number of private and public firms meeting the minimum threshold requirement for equity issuance in a given year to create the private and public firm control samples. Panel B also presents both the number of firms and firm-year observations after imposing requisite sample restrictions. Two counts are omitted to protext taxpayer privacy.

Table 2: Summary statistics

	Samples Used in Empirical Analyses								
	IPO Completing & Withdrawing Firms			*IPO Completing & Private Firms*			*IPO Completing & Public Firms*		
	No. Obs	**Mean**	**Median**	**No. Obs**	**Mean**	**Median**	**No. Obs**	**Mean**	**Median**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome Variables									
BTD	5,683	-0.068	-0.021	20,346	-0.032	-0.013	12,975	-0.016	0.000
BTD_tax	5,683	0.000	0.000	20,346	-0.001	0.000	12,975	0.001	0.000
Cash effective tax rate	2,006	0.204	0.172	8,252	0.194	0.118	6,954	0.190	0.162
CFC (0/1)	2,960	0.552	1.000	19,448	0.538	1.000	12,570	0.734	1.000
# CFCs	2,960	4.677	1.000	19,448	5.075	1.000	12,570	17.581	2.000
# Tax havens	2,960	0.766	0.000	19,448	0.745	0.000	12,570	3.341	0.000
Tax haven (0/1)	2,960	0.274	0.000	19,448	0.254	0.000	12,570	0.406	0.000
# Dot havens	2,960	0.208	0.000	19,448	0.229	0.000	12,570	1.197	0.000
Cost sharing agreement (0/1)	5,683	0.012	0.000	20,346	0.022	0.000	12,975	0.040	0.000
Total amounts received from U.S.	2,960	2.977	0.000	10,987	3.430	0.000	7,493	4.848	0.000
Total amounts paid to U.S.	2,960	2.604	0.000	10,987	3.562	0.000	7,493	6.320	0.000
Total income shifting receipts (from U.S.)	2,960	2.325	0.000	10,987	2.568	0.000	7,493	3.337	0.000
Total income shifting receipts (from U.S. & foreign)	2,960	2.643	0.000	10,987	3.029	0.000	7,493	4.485	0.000
Other Firm Characteristics									
Equity issuance ($Thousands)	5,683	30.952	5.209	20,346	41.998	6.064	12,975	85.855	10.786
Equity financing	5,683	0.147	0.040	20,307	0.121	0.026	12,965	0.088	0.020
Pretax financial income ($Thousands)	5,683	-8,483	-4,847	20,346	-3,623	-4,619	12,975	40,151	6,400
Federal income tax per books ($Thousands)	5,683	2,527	0.00	20,346	3,815	0.00	12,975	23,182	1,352
Pre-NOL taxable income ($Thousands)	5,683	-3,825	-3,544	20,346	1,822	-3,834	12,975	60,621	9,936
Taxes paid ($Thousands)	5,683	1,775	0.00	20,346	2,982	0.00	12,975	17,563	187.6
BTD, numerator ($Thousands)	5,683	-2,904	-1,046	20,346	-1,864	-1,466	12,975	879	-128.6
BTD_Tax, numerator ($Thousands)	5,683	361.479	0.000	20,346	-252.942	0.000	12,975	2,957	0.00
Total assets ($Thousands)	5,683	303,379	96,250	20,346	720,991	189,630	12,975	2,231,565	782,597
Asset growth	5,683	0.513	0.149	20,346	0.297	0.076	12,975	0.206	0.057
ln(Total assets)	5,683	11.553	11.475	20,346	12.294	12.153	12,975	13.553	13.570
Sales ($Thousands)	5,683	229,053	72,305	20,346	471,130	139,950	12,975	1,375,872	482,849
Sales growth	5,386	0.767	0.230	18,696	0.408	0.143	12,101	0.206	0.110
ln(Sales)	5,683	11.037	11.189	20,346	11.720	11.849	12,975	12.888	13.087
Net PPE/Assets	5,683	0.148	0.098	20,346	0.144	0.084	12,975	0.168	0.102
Debt/Assets	5,683	0.242	0.131	20,346	0.283	0.177	12,975	0.286	0.233
Pre-NOL taxable income/Lagged assets	5,683	-0.227	-0.042	20,346	-0.153	-0.019	12,975	-0.031	0.016
Pre-NOL taxable income/Sales	5,683	-1.135	-0.051	20,346	-0.597	-0.025	12,975	-0.252	0.023
Age	5,683	8.420	7.000	20,346	11.283	9.000	12,975	21.774	15.000
ln(Age)	5,683	2.062	2.079	20,346	2.249	2.303	12,975	2.827	2.773

This table presents summary statistics after inverse probability weighting the IPO completing firms to the control firms based on observable characteristics in year *t-1*. Columns (1), (2), and (3) present statistics for the sample of IPO completing firms and the withdrawn IPO firm control sample from 1994-2018 (for IPOs from 1996 to 2015). Columns (4), (5), and (6) present statistics for the sample of IPO completing firms and the private firm equity issuance control sample from 2004-2018 (for IPOs from 2006 to 2015). Columns (7), (8), and (9) present statistics for the sample of IPO completing firms and the public firm equity issuance control sample from 2004-2018 (for IPOs from 2006 to 2015). Variable descriptions are included in Appendix A. Data source is the Statistics of Income division of the Internal Revenue Service and authors' calculations. All percentile estimates are averages of the 10 observations around the percentile cutoff to preserve tax filing confidentiality.

Table 3: Pre-IPO filing firm characteristics, with IPW weights

	Completed IPO			Control firm			Diff. in means	
	No. Obs	Mean	Std. Dev	No. Obs	Mean	Std. Dev	Diff	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Control sample, withdrawn IPOs (1994-2018)								
Assets	672	224,324	394,437	228	227,681	382,144	-3,357	0.83
Asset growth	672	0.66	0.95	228	0.63	0.92	0.03	0.68
ln(Assets)	672	11.25	1.42	228	11.28	1.45	-0.03	0.68
Sales	672	176,609	297,450	228	210,325	331,347	-33,716	0.25
Sales growth	672	1.61	3.08	228	1.55	2.91	0.06	0.99
ln(Sales)	672	10.78	1.82	228	10.87	1.91	-0.09	0.33
Net PPE/Assets	672	0.16	0.14	228	0.15	0.13	0.01	0.37
Debt/Assets	672	0.27	0.29	228	0.26	0.27	0.01	0.95
Net income/Sales	672	-0.28	0.58	228	-0.29	0.60	0.02	0.54
Net income/Lagged assets	672	-1.10	2.64	228	-0.96	2.45	-0.13	0.38
Age	672	7.26	5.09	228	7.38	5.19	-0.12	0.95
ln(Age)	672	1.93	0.62	228	1.93	0.64	0.00	0.95
Panel B: Control sample, private firms (2004-2018)								
Assets	321	600,330	1,108,623	2,905	555,809	1,011,781	44,522	0.78
Asset growth	321	0.24	0.46	2,905	0.20	0.48	0.04	0.26
ln(Assets)	321	12.00	1.57	2,905	11.99	1.61	0.01	0.88
Sales	321	384,344	698,702	2,905	414,605	687,863	-30,261	0.69
Sales growth	321	0.63	1.33	2,905	0.72	1.60	-0.09	0.08
ln(Sales)	321	11.49	1.80	2,905	11.57	1.91	-0.07	0.79
Net PPE/Assets	321	0.15	0.15	2,905	0.16	0.17	-0.01	0.34
Debt/Assets	321	0.37	0.34	2,905	0.35	0.35	0.01	0.74
Net income/Lagged assets	321	-0.17	0.38	2,905	-0.17	0.40	0.00	0.90
Net income/Sales	321	-0.67	1.60	2,905	-0.62	1.66	-0.06	0.74
Age	321	8.89	6.87	2,905	11.47	10.72	-2.58	0.00
ln(Age)	321	2.10	0.61	2,905	2.17	0.86	-0.07	0.46
Panel C: Control sample, public firms (2004-2018)								
Assets	321	1,887,677	2,115,738	1,691	1,877,226	3,137,368	10,451	0.51
Asset growth	321	0.20	0.41	1,691	0.22	0.41	-0.02	0.75
ln(Assets)	321	13.42	1.71	1,691	13.22	1.64	0.20	0.90
Sales	321	1,303,390	1,614,595	1,691	1,211,548	2,042,667	91,841	0.70
Sales growth	321	0.35	0.53	1,691	0.31	0.62	0.04	0.20
ln(Sales)	321	12.95	1.86	1,691	12.57	1.98	0.38	0.48
Net PPE/Assets	321	0.19	0.19	1,691	0.15	0.16	0.04	0.36
Debt/Assets	321	0.37	0.30	1,691	0.25	0.25	0.12	0.22
Net income/Lagged assets	321	-0.01	0.22	1,691	-0.04	0.21	0.02	0.67
Net income/Sales	321	-0.16	0.77	1,691	-0.25	0.84	0.08	0.48
Age	321	22.59	18.74	1,691	18.48	14.99	4.12	0.62
ln(Age)	321	2.81	0.87	1,691	2.69	0.76	0.12	0.97

This table presents comparison of firm characteristics for the IPO completing firms relative to the three control samples after inverse probability weighting. Panel A presents characteristics in the year prior to IPO filing for firms that complete and IPO and the control set of firms that withdraw the IPO. Panel B presents characteristics in the year prior to IPO completion or equity issuance for the sample of IPO completing firms and the control set of private firms that issued equity. Panel C presents characteristics in the year prior to IPO completion or equity issuance for the sample of IPO completing firms and the control set of public firms that issued equity. Columns (1), (2), and (3) show the number of observations, the mean values, and the standard deviations for the completed IPO sample. Columns (4), (5), and (6) show the number of observations, the mean values, and the standard deviations for the control samples. Column (7) presents the difference in means, and Column (8) presents a within-cohort p-value for the difference between the two sample means. Variable descriptions are presented in Appendix A. Data source is the Statistics of Income division of the Internal Revenue Service and authors' calculations.

Table 4: Post-IPO tax planning

Panel A: Book-tax differences (BTDs)

Dependent var:	***BTDs***				***BTD_tax***			
	Withdrawn firms ('94-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)	Withdrawn firms ('94-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IPO Completed*Post	0.043***	0.048***	0.032***	0.021**	0.005***	0.003*	0.002**	0.001
	[3.762]	[4.173]	[3.021]	[2.064]	[4.311]	[1.738]	[2.397]	[0.903]
YearXCohort FEs	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Observations	5,683	2,480	20,346	12,975	5,683	2,480	20,346	12,975
R-squared	0.426	0.344	0.366	0.384	0.268	0.289	0.242	0.308

Panel B: Domestic cash effective tax rates (ETRs)

Dependent var:	***Cash effective tax rate***			
	Withdrawn firms ('94-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)
IPO Completed*Post	-0.083***	-0.125***	-0.065***	-0.039**
	[-3.437]	[-2.778]	[-5.088]	[-2.489]
YearXCohort FEs	+	+	+	+
Firm FEs	+	+	+	+
Observations	2,006	830	8,252	6,954
R-squared	0.567	0.573	0.569	0.592

This table presents results for the effect of completing an IPO on three measures of firm tax planning: domestic book-tax differences as a share of lagged assets following Mills (1998) (Panel A), a tax-based measure of book-tax differences as a share of lagged assets also from Mills (1998) (Panel A), and the domestic cash effective tax rate ("ETR" in Panel B). Both panels present OLS results from estimating Equation (1) for the sample of completed IPO firms and control firms after inverse probability weighting (IPW). Columns (1) and (5) present results for a control sample of firms that withdrew the IPO, using data from 1994-2018 (for IPOs from 1996-2015). Columns (2) and (6) present results for a control sample of firms that withdrew the IPO, using data from 2004-2018 (for IPOs from 2006-2015). Columns (3) and (7) present results for a control sample of private firms that completed an equity issuance, using data from 2004-2018 (for IPOs from 2006-2015). Columns (4) and (8) present results for a control sample of public firms that completed an equity issuance, using data from 2004-2018 (for IPOs from 2006-2015). Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of IPO transactions. Firms are only included in one cohort, and each cohort includes observations from t-3 to t+3 around the IPO filing date. Regressions include firm and year-by-cohort fixed effects. Standard errors are clustered at the IRS major industry level. T-statistics are reported in brackets. Variable descriptions are presented in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 5: Post-IPO foreign tax planning and activity

Panel A: Tax Haven Ownership

Dependent var:	***Havens (0/1)***	***#Havens***	***# Dot Havens***
	(1)	(2)	(3)
IPO Completed*Post	0.148***	0.217**	0.100**
	[5.032]	[2.211]	[2.435]
YearXCohort FEs	+	+	+
Firm FEs	+	+	+
Observations	2,394	2,394	2,394
R-squared	0.752	0.912	0.928

Panel B: Cross-border Related Party Payments

Dependent var:	***Cost sharing agreement (0/1)***	***Total amounts received from U.S.***	***Total amounts paid to U.S.***	***Total income shifting receipts from U.S.***	***Total income shifting receipts from U.S. and foreign***
	(1)	(2)	(3)	(4)	(5)
IPO Completed*Post	0.038***	1.093**	0.483*	1.097**	1.050**
	[6.145]	[2.307]	[1.787]	[2.450]	[2.226]
YearXCohort FEs	+	+	+	+	+
Firm FEs	+	+	+	+	+
Observations	2,480	2,394	2,394	2,394	2,394
R-squared	0.426	0.583	0.555	0.558	0.575

This table presents results for the effect of completing an IPO on firm foreign tax planning activity. Panel A presents results for tax haven ownership in Columns (1)-(3). Panel B presents results for cross-border related party agreements and payments commonly related to income shifting behavior. Both panels present OLS results from estimating Equation (1) for the sample of completed IPO firms and firms that filed but withdrew their IPO filing after inverse probability weighting (IPW), with data from 2004 to 2018 for IPOs filed between 2006 and 2016. Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of IPO or equity issuance. Firms are only included in one cohort, and each cohort includes observations from t-3 to t+3 around the IPO filing date. Regressions include firm and year-by-cohort fixed effects. Standard errors are clustered at the IRS major industry level. T-statistics are reported in brackets. Variable descriptions are presented in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.