IPOs and Corporate Tax Planning*

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Abstract

Does going public affect the amount and type of corporate tax planning? Using a panel of U.S. corporate tax return data from 1994 to 2018, we show that IPO completion is associated with the implementation of multinational income shifting strategies central to the current international tax policy debate. Specifically, firms (i) expand their foreign tax haven presence, (ii) enter into cross-border agreements that accompany intangible asset transfers to foreign subsidiaries, and (iii) increase their level of foreign related-party payments around the time that they go public. The effects are strongest among firms that switch to more sophisticated tax advisors in the years preceding the IPO. In contrast, we observe little domestic tax planning because large stock option deductions, which increase as a consequence of the IPO, provide large domestic tax shields. The paper contributes to the nascent tax literature studying IPOs by documenting the specific tax strategies enabling public firms to remain lightly taxed in the post-IPO period. Furthermore, the findings imply that U.S. tax policies targeted at early-stage innovative firms are critical for retaining domestically-developed IP – and the income earned on such assets – for the U.S. tax base.

Keywords: Initial public offering (IPO), tax planning, multinational tax

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1 Introduction

Multinational firms' low tax burdens have attracted substantial academic, policy, and press attention (e.g., Dyreng et al. 2017; Grinberg and Kysar, 2021; Drucker, 2021; Rubin and Walker, 2022). Current domestic and multilateral policy proposals address multinational tax planning by increasing tax rates and discouraging cross-border income shifting (OECD, 2021; Biden, 2022). However, critical questions remain unanswered about multinational firms and their tax planning. For example, what drives companies to initially implement tax planning strategies? At what point do companies invest in foreign tax structures? This paper finds that IPOs motivate firms to implement the global tax planning strategies central to the current international tax policy debate.

Understanding the relation between IPOs and tax planning is important for at least two reasons.[1] First, considerable academic work focuses on corporate tax planning by large public corporations (Doidge and Dyck, 2013; Cen et al., 2017; Dyreng, Lindsey and Thornock, 2013; Hanlon and Heitzman, 2010). However, aside from recent work examining non-corporate entities (Smith et al., 2019, 2022), there is less evidence about the tax strategies of smaller, innovative private firms such as those that file to go public. Some of this evidence shows that a substantial number of private corporations are insensitive to tax considerations due to large, reported tax losses (Cooper and Knittel, 2006, 2010), whereas sophisticated public multinationals arbitrage tax rates and incentives across jurisdictions to minimize worldwide tax burdens (Graham, Hanlon and Shevlin, 2010; Hanlon, Lester and Verdi, 2015; Faulkender, Hankins and Petersen, 2019). Documenting whether and to what extent the IPO is

[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).

the point at which firms alter their approach to tax planning helps to bridge between these findings. Second, current tax policy proposals target tax avoidance by established multinational companies.[2] Understanding the genesis of these tax strategies is informative for crafting the appropriate policy mechanisms to preserve the U.S. tax base.

Prior literature offers competing predictions for the effect of an IPO on firm tax planning, with some work suggesting increased levels of tax avoidance. First, the transition to public markets increases capital market scrutiny of corporate tax obligations. Badertscher et al. (2019) and Hoopes et al. (2020) show that private firms engage in certain types of "conforming" tax avoidance given that financial reporting and tax incentives are aligned in private firms. However, upon a transition to capital markets, these incentives diverge, affecting managers reporting and tax choices (Graham et al., 2014). These findings imply that IPO firms will increase or change their tax planning strategies to align with that of other public firms around the time that the firm goes public. Second, IPOs provide discrete opportunities to implement unique tax structures, such as "supercharging" tax benefits (Edwards, Hutchens and Rego, 2019). The cost of certain foreign strategies is based on the valuation of firm assets used in such structures; thus, firms have incentives to establish these structures in anticipation of the IPO, when a lower or wider range of valuations is plausible. The IPO valuation implications, as well as the increased capital market scrutiny, are reasons why firms may increase their tax planning around the IPO.

On the other hand, for many firms, an IPO may have little effect on tax planning. We show that the majority of private firms report substantial domestic tax losses in years prior

[2]For example, both the U.S. Corporate Minimum Tax and the OECD's Pillar 1 provisions are estimated to impact approximately 80 of the largest publicly-traded companies (EconPol, 2021; Rubin and Francis, 2022).

to the IPO. These firms may simply be insensitive to corporate taxes or unwilling to invest in corporate tax planning. In this case, we anticipate little to no change in firms' tax avoidance. Thus, the empirical relation between IPOs and corporate tax planning is unclear *ex ante*.

Despite the importance of this topic and the centrality of IPOs in corporate finance research (Ritter and Welch, 2002; Lowry, Michaely and Volkova, 2017), there is limited work on the relation between corporate taxes and IPO completion. The limited evidence is 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 tax planning between the pre- and post-IPO periods.

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 filed for an IPO but ultimately withdrew and remained private. These firms are a good control group because they are the most similar in their life cycle and their incentives to go public. However, because these firms ultimately chose to forego the IPO and thus could differ in ways correlated with tax planning, we construct two additional control samples of private and public firms. To align these samples with IPO firms, we require the public and private companies to be of a similar size and to also issue equity of a comparable magnitude. However, unlike the IPO firms, these companies do not change their private or public status during the sample period.

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 control groups permits us to distinguish the IPO event from other external financing events, thereby isolating the distinct impact of going public. For all comparisons between IPO and control firms, we employ inverse probability weighting on observable firm characteristics to further mitigate endogeneity concerns.

We address the data availability challenge by using a panel of confidential U.S. corporate tax returns of 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.[3] Furthermore, the data permit construction of the three control samples, improving upon the approach in recent literature that generally relies on only withdrawn firms as a benchmark.

Our first empirical analyses test whether domestic tax planning changes after the IPO. We examine book-tax differences (BTDs), which capture the extent to which financial reporting and taxable incomes diverge (Mills, 1998; Desai and Dharmapala, 2006; Chen et al., 2010). We also examine domestic cash effective tax rates (ETRs), which capture the amount of cash taxes paid per dollar of financial reporting income (Hanlon and Heitzman, 2010; Chyz et al., 2013; Hasan et al., 2014). BTDs (ETRs) are increasing (decreasing) in tax avoidance. We employ a stacked cohort difference-in-difference design in which we compare IPO-completing firms with each of the three control samples. We observe large increases in BTDs and decreases in ETRs in the years immediately following IPO completion relative to all three

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

groups. Although these effects are consistent with increased tax planning in the post-IPO period, additional tests reveal that the results are not due to explicit or intentional tax planning strategies. Instead, we find that the effects are driven by (i) increased earnings management activity, which increases financial reporting income but is largely reversed on firms' tax returns, and (ii) increased stock option deductions, which decrease taxable income. The stock option deductions are sufficiently large to minimize domestic tax obligations for several post-IPO years. Documenting these true factors driving the domestic tax planning effects is important for the growing IPO literature that draws inferences based on similar measures constructed from public data (e.g., Chyz et al., 2022; Chen et al., 2021).

We next study international tax planning, which is of particular importance given the considerable academic and policy focus on firms reducing their tax obligations through shifting both income and physical presence to lower-taxed jurisdictions. We examine three foreign tax planning outcomes. First, we show that, relative to IPO withdrawers, IPO completers are 57 percent more likely to own a subsidiary in a tax haven – and own a greater number of such subsidiaries – after going public. When separately comparing to public firms, we observe strong effects consistent with firms responding to capital market pressures for tax avoidance: IPO firms' likelihood of having a tax haven presence converges to that of the publicly traded companies by the third year after the IPO.

Next, we test whether IPO firms' foreign structures facilitate cross-border income shifting. We measure the incidence of a cost-sharing agreement, which is a specific income shifting strategy that enables firms to source sales revenue to lower-taxed foreign jurisdictions after moving U.S.-developed IP offshore (De Simone and Sansing, 2019). Because the U.S. charges an exit tax on the fair value of the assets at the time of the offshore transfer, firms have

incentives to strategically time this transfer prior to the IPO. We find sizeable IPO effects: the incidence of these agreements within IPO-completing firms more than doubles in the post-IPO period.

Third, we use foreign subsidiary tax data on the amount, type, payor, and payee of related-party transactions to study income shifting payments. We 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.). Because higher levels of cross-border payments could be attributable to post-IPO growth within a vertically integrated multinational firm, we conduct several additional tests to link these findings to explicit foreign tax planning. For example, we also measure changes in the amount of payments *to* the U.S. ("inbound" payments). Although we observe that these payments increased after the IPO as well, the economic magnitude is smaller than the outbound payments, mitigating concerns that all cross-border payments increased regardless of tax incentives. Additionally, we refine the measures of outbound 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 (Grubert, 1998, 2003; Faulkender, Hankins and Petersen, 2019). We find that the total outbound payments primarily relate to these income-shifting transactions, not to routine intercompany sales of goods and services. Finally, we expect that the foreign planning effects should be the greatest in high-intangibles firms because income attributable to intangible assets can be more easily shifted across borders (De Simone, Mills and Stomberg, 2019). Results are consistent with (weakly) larger effects in firms with greater R&D expenses pre-IPO.

To assess how firms accomplish this tax planning, we examine the use of professional tax advisors. This analysis is motivated by recent work documenting that tax advisors play an important role in firms' tax planning decisions (e.g., Zwick, 2021; Bustos et al., 2022). We find that the proportion of firms using a "sophisticated" tax advisor (defined as those signing more than 4,000 tax returns) increases substantially for IPO completers from three years prior to the IPO through the year of the IPO. We again observe converge with the publicly-traded firm control sample: IPO firms' use of sophisticated advisors increasees to the level of the public firms by year t-1, consistent with IPO firms responding to capital market pressures for tax planning. We then link this professionalization with the foreign planning effects, showing that these effects (i.e., having a tax haven subsidiary or participating in a cost-sharing agreement) are greatest among firms that switch to using a sophisticated tax advisor in the years leading up to IPO.

This paper contributes to the tax literature in finance, economics, and accounting studying the drivers of firm tax behavior. A key innovation of our paper is examining the transition from private to public status, thereby bridging between the extensive literature on public firms' tax avoidance (e.g., Dyreng et al., 2017) and tax considerations of private firms (Hoopes et al., 2020; Badertscher, Katz and Rego, 2013; Badertscher et al., 2019; Olbert and Severin, 2022). Specifically, we provide new evidence about the origins of foreign tax structures, which are inherently difficult to observe without access to the subsidiary tax data we use in this study. We show that the IPO motivates firms to substantially increase their foreign haven presence and cross-border transactions, thereby also contributing to the work studying multinationals' foreign tax planning activities (Bilicka, 2019; Dowd, Landefeld and Moore, 2017; Dharmapala, 2014; Bustos et al., 2022; Foley et al., 2007; Faulkender, Hankins

and Petersen, 2019; Faulkender and Smith, 2016; Hanlon, Lester and Verdi, 2015).

Second, we contribute to the literature studying the consequences of public listing (Asker, Farre-Mensa and Ljungqvist, 2015; Gilje and Taillard, 2016; Feldman et al., 2021; Michaely and Roberts, 2012; Gao, Harford and Li, 2013; Brav, 2009; Dobridge, Gilje and Whitten, 2022). We contribute in two ways. First, we broaden the identification strategy used in Bernstein (2015), which leverages withdrawn firms as a control sample, by also using equity-issuing private and public firms as control samples. Second, we contribute to the nascent literature examining the tax implications of IPOs. This literature uses relatively small samples of IPO firms to study how corporate taxes affect IPO pricing (Edwards and Hutchens, 2020), industry tax planning (Chyz et al., 2022), transaction structures (Edwards, Hutchens and Rego, 2019), and compensation planning (Dambra, Gustafson and Quinn, 2020). Not only do we examine a range of domestic and foreign tax outcomes for our sample firms, but we help explain the mechanism driving such effects by evaluating the use of professional advisors.

Finally, this paper provides policy-relevant insights. We show that sophisticated foreign planning begins relatively early in public firms' lifecycles. This finding means that policies targeting profit shifting will be most effective if they not only address large, well-established firms, but also young, growing, high-R&D firms. For example, incentives to retain intangible assets in the U.S. may be particularly effective in stemming the tide of foreign asset transfers that facilitate large amounts of income shifting. We look forward to future research that evaluates the effectiveness of recent incentives, such as the foreign-derived intangible income (FDII) provision in the 2017 Tax Cuts and Jobs Act.

2 Empirical strategy

2.1 Research Design

To test the relation between IPO completion and firm tax planning, we use a stacked-cohort difference-in-differences design in which we compare tax planning for firms that complete an IPO to three sets of control firms. For each IPO year, we construct a cohort of IPO-completing firms and control firms. We include all observations in a seven-year window centered around the IPO, requiring firms to be observable for at least two years pre- and post-IPO. We stack the cohorts in event time and estimate the following specification to measure the average effect of IPO completion on tax planning:

$$Y_{ij} = \beta \cdot IPOCompleted_i \cdot Post_{cj} + \gamma_{cj} + \delta_i + \epsilon_{ij}, \tag{1}$$

Y_{ij} is one of several tax planning measures for firm i in tax year j, described further in Section 2.2. $IPOCompleted_i$ is an indicator variable equal to one for firms that complete an IPO transaction, and $Post_{cj}$ is an indicator variable equal to one if $j \geq t$, where t is the year of IPO filing or completion for cohort c (depending on the control sample used, as discussed below). We include year-by-cohort fixed effects γ_{cj}, which control for cohort-specific time series trends. We also include firm fixed effects δ_i to control for time-invariant firm characteristics. Each IPO firm is included only once in the sample, and therefore, firm fixed effects are equivalent to firm-by-cohort fixed effects. The firm and year-by-cohort fixed effects subsume the main effects of $IPOCompleted_i$ and $Post_{cj}$. The term ϵ_{ij} is an additive error term.

Our coefficient of interest, β, measures the difference in tax planning between IPO firms

and control firms after an IPO, relative to any difference in the pre-period. We exclude time-varying firm controls from Equation (1) because these variables are likely also affected by the IPO and thus could introduce bias due to a "bad controls" problem (Angrist and Pischke, 2008; Roberts and Whited, 2013). We cluster standard errors by three-digit NAICS industry, reflecting that industry-specific productivity shocks contribute to IPO waves.[4]

To further study year-specific effects of IPO completion and also evaluate the parallel trends assumption, we estimate the following equation:

$$Y_{ij} = \alpha + \sum_{k=t-3, k\neq t-1}^{t+3} \beta_k \cdot I\{k = j\} \cdot IPOCompleted_i + \gamma'_{cj} + \delta'_i + \epsilon'_{ij}, \tag{2}$$

where t remains the year of IPO completion for cohort c, $I\{k = j\}$ is an indicator equal to one if $k = j$, γ'_{cj} are year-by-cohort fixed effects, δ'_i are firm fixed effects, and ϵ'_{ij} is an additive error term. We omit β_{t-1}, and thus all effects captured by β_k are measured relative to the difference between the IPO firms and control firms in the year prior to IPO completion.

2.2 Tax Planning Measures

We use several measures of tax planning. We first construct two measures of domestic tax planning from the prior literature. *BTD* is the difference between pre-tax financial statement income ("book" income) and taxable income, divided by beginning-of-year assets

[4]Section 4.4 presents two analyses for alternatively estimating Equation (1) including time-varying control variables and clustering at the firm level. Inferences are unchanged.

(Mills (1998); Desai and Dharmapala (2006); Chen et al. (2010); Hasan et al. (2014)).[5] We also use the firm's domestic effective tax rate, *Cash ETR*, which is equal to domestic cash taxes paid divided by domestic book income. Although this measure requires firms to report positive pre-tax income and thus cannot be calculated for the large number of loss firms in our sample, we use this measure because it is commonly used in prior work (e.g., Chen et al. (2010); Hanlon and Heitzman (2010); Chyz et al. (2013); Hasan et al. (2014)).

Prior literature shows that greater foreign presence is associated with greater tax planning (Rego (2003); Foley et al. (2007); Dharmapala, Foley and Forbes (2011); Hanlon, Lester and Verdi (2015); Faulkender and Smith (2016)). Therefore, we next construct several measures of foreign tax planning using 2004-2018 data on financial performance and intercompany transactions of foreign subsidiaries.[6] We construct three measures of tax haven presence. *Havens (0/1)* is an indicator variable equal to one if a firm owns at least one subsidiary in a foreign tax haven, and zero otherwise. *#Havens* is the count of tax haven subsidiaries; *#DotHavens* is a refined count capturing only those subsidiaries in "small as a dot" havens where firms locate primarily for tax reasons. We follow Hines and Rice (1994) and Dyreng and Lindsey (2009) to determine a county's haven status; see Appendix A.

[5]For firms required to file IRS Form Schedule M-3 with their tax return, we define book income as the sum of Net income (loss) per income statement of includible corporations, U.S. current income tax expense, and U.S. deferred income tax expense. However, Schedule M-3 was not introduced until 2004 and is only required for firms with at least $10 million in assets. Thus, for tax returns that do not include Schedule M-3, we define book income as the sum of Net income (loss) per books and federal income tax per books from Schedule M-1. Taxable income is measured before tax loss carryforwards and other special deductions. Appendix A provides the specific U.S. corporate income tax return line items used to construct all variables.

[6]We use data from Form 5471, Information Return of U.S. Persons with Respect to Certain Foreign Corporations, to construct these measures. We identify a foreign subsidiary's jurisdiction using address information listed on Form 5471. Because these data are only available for even years beginning in 2004, we limit the analysis to the post-2004 period and impute the presence of subsidiaries in the odd years based on whether the subsidiary reported positive beginning-of-year cash or other assets on its balance sheet (Schedule F) in the subsequent even year. For example, we assume a subsidiary was in existence in 2005 if the 2006 beginning balance sheet for that subsidiary had non-zero values. For firms that do not report beginning-of-year values, we impute the count of tax haven subsidiaries in a given odd year by taking the minimum of the counts in the prior and succeeding years.

We also use foreign subsidiary data to measure the firm's participation in tax-motivated income shifting structures. We first identify whether a foreign subsidiary was party to a cost sharing agreement, a specific income shifting strategy in which a foreign subsidiary shares in intellectual property (IP) development costs (De Simone and Sansing (2019)). Sharing in these costs entitles a foreign subsidiary to a portion of the subsequent IP-related revenue. Thus, by establishing a cost sharing agreement with a low-taxed foreign subsidiary, the U.S. parent effectively shifts income out of the U.S. to a low-taxed jurisdiction. *Cost sharing (0/1)* is an indicator equal to one if at least one foreign subsidiary participates in such an agreement during the year.[7,8]

Next, we construct measures of intercompany payments between the U.S. and its foreign subsidiaries. *Total amounts received from U.S.* and *Total amounts paid to U.S.* are equal to the natural logarithm of one plus the total amounts received from, or paid to, the U.S. parent and other domestic corporations, respectively (De Simone, Mills and Stomberg (2019)).[9] Because a vertically integrated firm could naturally have higher levels of intercompany payments, we also construct two refined measures that include only those line items most closely related to tax-motivated income shifting: intercompany sales of property rights

[7]The specific question we use to identify this strategy is (based on the 2012 Form 5471 Schedule G), "During the year, was the foreign corporation a participant in any cost sharing arrangement?"

[8]A 2011 Bloomberg article provides one public example of a cost sharing arrangement used by Cisco. It states, "Beginning in 1995, the Netherlands subsidiary 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). Though Cisco went public in 1990 (prior to our sample period), this example is illustrative of the type of internal tax-motivated agreements that have previously been understudied in the prior literature.

[9]These measures enable 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. Observing different effects would be suggestive of tax-motivated income shifting out of higher tax countries (like the U.S.) to lower-taxed foreign jurisdictions.

(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 (Collins and Shackelford, 1998; Grubert, 1998, 2003; Faulkender, Hankins and Petersen, 2019). *Total income-shifting receipts from U.S.* is equal to the natural logarithm of one plus the total amounts received from the U.S. parent and other related domestic entities for these items. An additional measure also captures related-party payments among foreign subsidiaries.

3 Data and Summary Statistics

3.1 Sample construction: IPO filers

We construct the sample of IPO filers using data from Thompson Financial, the SEC's EDGAR platform, and administrative U.S. corporate income tax records. IPO filers include IPO-completing firms, as well as firms that file but ultimately withdraw their IPO (the first control group). Table 1 shows the sample selection steps. We draw the dataset of U.S. IPO filers from Thompson Financial's SDC Platinum New Issues database. We retain IPOs between 1996 (the first year of requisite EDGAR IPO filing) and 2015 (22,484 events). Following standards in the IPO literature (e.g., Lowry, Michaely and Volkova, 2017), we exclude financial firms and utilities, as well as spin-offs, real estate investment trusts (REITs), American depository receipts (ADRs), closed-end funds, blank-check firms, and unit funds. We keep only common stock issuances, as well as IPO filings with requisite tax identifiers

(tax employee identification numbers or EINs) and filing date information.[10] For the six percent of withdrawing firms with multiple filings, we discard all but the first IPO filing. The final IPO sample includes 3,593 unique IPO events, of which 2,411 (1,182) are completed (withdrawn).

We use EINs to merge the IPO sample with a stratified random sample of U.S. corporate tax return data, retaining 83 percent of the IPO firms: 2,125 IPO-completing firms (21,985 firm-years) and 841 IPO-withdrawing firms (6,550 firm-years).[11] We retain observations for this merged sample from 1994 to 2018 so as to include pre- and post-period observations around the 1996-2015 IPO events. We discard observations that do not have C-corporation filing status, those with zero or negative values of gross receipts or total assets, and those missing industry information.[12] We drop the 1996 IPO cohort due to the lack of withdrawn firms after imposing the prior sample restrictions. To mitigate concerns about sample attrition within each cohort, we require all firms to be present in the sample for at least years $t-2$ through $t+1$ relative to the IPO filing.[13] 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 percent complete the IPO.

[10]EINs are necessary for merging with the tax data. Because these are unavailable for many firms in SDC, we scrape EINs from the IPO filing on EDGAR when available.

[11]This stratified random sample is constructed, cleaned, and edited each year by the Statistics of Income (SOI) division of the Internal Revenue Service; see also Zwick and Mahon (2017), Smith et al. (2022), and Feldman et al. (2021).

[12]If industry information is missing or invalid, we instead identify industry affiliation using the firm's earliest-reported valid NAICS code in the tax data. Thus, we only drop firms for which we never observe any industry information.

[13]Section 4.4 presents results relaxing this restriction. Inferences are unchanged.

3.2 Sample construction: Public & private control firms

To mitigate concerns that IPO-completing and -withdrawing firms differ on unobserved time-varying characteristics that drive tax planning, we use two additional control samples. These samples include similarly-sized private and public firms that also receive external financing but, unlike IPO firms, do not switch their private or public status. These samples permit us to assess the robustness of any observed IPO effect, and to difference out tax planning effects attributable to capital infusions.

We follow similar procedures to construct these two control groups, also outlined in Table 1. The starting samples include all unique parent-level C corporations in the tax data from 2004 to 2018 (157,943 private firms and 10,771 public firms).[14] As with the IPO sample, we exclude financial and utilities firms and require firms to have positive gross receipts and assets. To further align the samples, we retain only those firms with equity issuances of comparable size to the equity issuances reported by the sample IPO firms.[15] We drop a small number of firms with a change in private-public status; the count is omitted from Table 1 due to tax disclosure restrictions. Imposing additional restrictions related to industry affiliation and cohort presence results in a sample of 2,905 private control firms (18,588 firm-years) and 1,691 public control firms (11,014 firm-years).

Figure 1a shows the number of completed and withdrawn IPOs by year of filing. Figures 1b and 1c show the number of completed IPOs relative to private control firms and

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

[15]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 IPO sample. If a firm has more than one equity issuance of this size, we use the first equity issuance. As part of this step, we also drop firms with assets larger than the largest IPO-completing firm observation to ensure size comparability.

public control firms, respectively, by year of equity issuance. The number of completed and withdrawn IPOs peaks in 2000, prior to the 2001 recession. The sample includes approximately 300 private firms each year from 2006 to 2010; this number dips in 2011 to 2013 before increasing to over 350 firms in 2015. In contrast, the public firm sample includes the most observations in 2006 and 2007; after fluctuations around the Financial Crisis, the count stabilizes at 100-150 firms in the later years.

Figures 2a, 2b, and 2c present equity issuance trends for the three samples. *Equity Issuance* is defined following Yagan (2015) as the dollar value of non-negative changes in equity from the prior to the current year. Figures 2d, 2e, and 2f present trends for *Equity Financing*, which is defined as *Equity Issuance* scaled by the sum of total equity and debt. Panels a and d show that most IPOs are completed in the same year of filing (t), with some additional equity issuance in $t+1$. For the private and public control groups (Panels b, c, e, and f), we show trends relative to the year of equity issuance. The private firm control group exhibits similar patterns as the IPO firms for both the level and proportion of equity issued, whereas the absolute (relative) amount of *Equity Issuance* (*Equity Financing*) is larger (smaller) for public firms than for IPO firms.

3.3 Sample weighting

To further mitigate concerns about differences between the IPO and control samples, we inverse probability weight (IPW) each sample. Within each cohort, we first predict the likelihood of IPO completion in the year preceding the IPO filing/equity issuance. The covariates included in the probit model are firm age (*ln(Age)*), size (*ln(Total assets)*), firm

growth opportunities (*Asset growth*), financial performance (*Taxable income/Sales*), asset tangibility (*Net PPE/Assets*), and leverage (*Debt/Assets*); all variables are defined in Appendix A. We then weight the IPO and control firms using the inverses of the predicted likelihoods derived from the first-stage estimation. To eliminate the influence of outliers, we winsorize variables within each of the three samples at the five-percent level following prior work using corporate tax data (Cohn, Mills and Towery, 2014; Yagan, 2015; Zwick and Mahon, 2017).[16]

3.4 Summary statistics

Table 2 displays summary statistics for the three samples. Average *BTD* is negative in each sample, meaning that taxable income exceeds book income. Average domestic *Cash ETR* is approximately 20.0 percent, with median values of 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 public firm control sample; firms have 3.1 tax haven subsidiaries and report a 42.0 percent likelihood of tax haven use. For comparison, the withdrawn IPO firm and private samples report 0.79 and 0.77 tax havens (29.3 and 27.1 percent incidence of haven ownership), respectively. Approximately 1.2 to 4.0 percent of the sample participate in a cost-sharing agreement. The bottom portion of the table shows that firms increase their external *Equity Financing* by 8.9 to 14.7 percent. The sample including withdrawn firms has the smallest firms based on *Sales* and *ln(Total Assets)*, but it also reports the highest *Asset Growth*.

[16]Section 4.4 shows that inferences are unchanged if we exclude weights in estimation. Section 4.4 also presents results after winsorizing at the 1% level. We generally find larger effects, consistent with large outlier values affecting point estimates derived from Eq. (1).

Table 3 compares IPO firms to control firms in the year prior to IPO. Panel A presents a comparison of IPO completers and withdrawn firms. Panels B and C compare IPO firms to the private and public firm control samples, respectively; the number of IPO firms is smaller due to estimation on the shorter 2004-2018 sample period. IPO completers and control firms are statistically similar across almost all firm characteristics after weighting. The private control firms are 2.5 years older than the IPO completing firms on average; while this difference is statistically different, the logged transformation of age is not. The next section further demonstrates the similarity of IPO firms and control samples based on pre-period tax planning trends.

4 Results

4.1 Domestic Tax Planning

Table 4 presents results from estimating Eq. (1) using domestic tax planning measures. Columns (1) through (4) of Panel A include results using *BTD* as the dependent variable; Columns (5) through (8) report results using *Cash ETR*. 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 sample period of the private and public controls (2004-2018). Columns (3) and (7) ((4) and (8)) report results for the private (public) firm control sample.

BTD (*Cash ETR*) is increasing (decreasing) in tax avoidance; thus, across all eight columns, we observe results consistent with increased tax avoidance post-IPO. The coeffi-

cients in the first four columns mean that IPO completion is associated with an increase in *BTD* of 2.1 to 4.8 percentage points. Figure 3 plots year-specific treatment effects obtained from estimation of Eq. (2); Figures 3a, 3b, and 3c correspond to the withdrawn IPO firm (over the 1994 to 2018 period), private firm, and public firm control groups, respectively. These graphs first confirm that the three samples generally exhibit parallel trends in *BTD* in the years preceding the IPO. We also observe statistically significant increases in *BTD* after the IPO, consistent with the Table 4 results.

Columns (5) through (8) present results using domestic *Cash ETR* as the measure of tax avoidance. The effects are estimated on a smaller sample due to dropping loss firms. Despite this smaller sample, we observe results consistent with increased tax planning: *Cash ETR* declines by 3.9 to 12.5 percentage points after the IPO, equivalent to about a 15 to 50 percent decline.[17] Figures 3d and 3e graph the decline in *Cash ETR* for completed IPOs, consistent with the Table 4 results. We observe similar effects when using the public firm control sample, but we acknowledge that the pre-period trends are dissimilar and thus we cannot attribute the effects in this sample to the IPO necessarily.

The economic magnitudes imply large increases in tax planning. However, both *BTD* and *Cash ETR* capture *any* divergence between book and taxable income (Hanlon and Heitzman (2010)). Thus, to assess whether the effects relate to explicit tax planning strategies that companies implement around the IPO, we conduct several additional tests using the underlying tax data. We find that the results are largely attributable to two factors not considered "explicit" planning. First, we decompose *BTD* using the specific book-tax dif-

[17]The 15 (50) percent change is calculated for the public control sample (withdrawn firm sample over the 2004 to 2018 period) based on the pre-IPO untabulated average *Cash ETR* of 0.29 (0.23). All magnitudes are based on average values from the inverse probability weighted sample but are similar if calculated using unweighted descriptive statistics.

ference line items from the corporate income tax return and find that the effects are largely explained by increased stock option deductions; see Columns (1)-(3) of Panel B.[18] Second, we find that the increased difference between book and taxable income is also attributable to increased earnings management activity, which drives higher financial-reporting income but not taxable income; we report this analysis in Online Appendix B. Additional untabulated analyses reveal that no other particular book-tax difference exhibits similar effects as these items; furthermore, when we adjust *BTD* to exclude the effects of both stock compensation and earnings management, we observe insignificant effects for the public and private firm control samples and substantially diminished effects in the withdrawn IPO sample.

In summary, firms appear to engage in little explicit domestic tax planning after the IPO, likely because stock option deductions are sufficiently large to absorb U.S. taxable income for several post-IPO years. Documenting the true factors driving changes in domestic tax avoidance measures is important for the literature, particularly because concurrent work also uses these measures to study IPO-related tax planning effects (e.g., Chen et al., 2021; Chyz et al., 2022).

4.2 Foreign Tax Planning

We next test whether the IPO is associated with foreign tax planning activity. There are two factors unique to IPOs that motivate foreign tax planning: the requirement to disclose tax information via public financial reporting, and IPO valuation implications for firm intan-

[18]Specifically, we examine each line reconciling book income to taxable income on IRS Schedule M-3. Stock option deductions naturally increase after the IPO, as IPOs generally trigger stock option vesting. This in turn accelerates the exercise of options and the corresponding stock option deduction. Consequently, due to the difference in book and tax accounting for stock options (and not explicit tax planning activities), IPO firms will naturally report higher stock option deductions and thus larger *BTD* after the IPO.

gibles. First, public disclosure requirements should impact tax planning, likely motivating firms to engage in similar levels and types of avoidance as other public firms. Because firms have little reason to engage in additional domestic tax planning (in light of the large stock option deductions discussed above), firms may instead use foreign tax planning to converge to the level exhibited by public companies. Second, the IPO provides a clear, observable signal of firm value that impacts the cost of establishing foreign income-shifting structures. For example, certain income-shifting strategies entail transferring intangible assets from the U.S. to lower-taxed foreign jurisdictions, triggering a U.S. "exit tax" calculated on the intangible's fair market value.[19] Consequently, firms have incentives to shift intangibles offshore in years preceding the IPO (when the value is likely lower and less certain), to both minimize the exit tax and position the firm to use these strategies in the post-IPO period. This section documents evidence consistent with both factors.

We first provide a graphical analysis of trends in tax haven use around the IPO. Figures 4a and 4b plot the average levels of *Havens (0/1)* and *#Havens* for IPO firms as compared to the private and public firm control groups in the years around equity issuance (year t). The top graph provides striking evidence of increased foreign tax planning. At the beginning of the sample period – three years prior to the IPO – 17 percent of IPO completers own a tax haven subsidiary. This level is similar to that of the private firm sample. Tax haven ownership among IPO completers then climbs, increasing to 36 percent by the IPO filing year. The level converges near the level of public firms at 47 percent by $t + 3$. Panel B confirms that IPO firms also increase the number of haven subsidiaries around the IPO,

[19]Specifically, Internal Revenue Code Section 367 imposes a tax when U.S. companies transfer appreciated assets, such as internally developed intellectual property, to foreign subsidiaries. See technical discussions in Hicks and Sotos (2008), Fuller and Halpern (2012), and Murillo et al. (2016).

with trends tracking those of public firms. While descriptive, both figures show that IPO firms increase their haven use around the IPO to more closely resemble that of public firms.

Although Figure 4 provides strong evidence of changes in foreign planning around the IPO, these graphs also show that IPO completing firms do not exhibit parallel trends with the private or the public control samples for these outcomes. Thus, we use only the withdrawn IPO control sample for the next empirical tests. Figures 5a and 5b confirm that indeed the IPO completers and withdrawers exhibit similar pre-IPO trends for both *Haven (0/1)* and *#Havens*. Furthermore, across both figures, we observe increases by IPO completing firms in the post-IPO period. Table 5, Panel A presents empirical results corresponding to these figures, with results for *Havens (0/1)* in Column (1) and *#Havens* in Column (2). The probability that an IPO-completing firm has a haven subsidiary increases by 16.1 percentage points; based on the average haven incidence of 28.3 percent for IPO firms in the year prior to going public (untabulated), this is equivalent to a 57 percent increase. The number of haven and dot haven subsidiaries increases by 0.27 and 0.10, respectively, translating to a 34 percent and 41 percent increase compared to (untabulated) pre-IPO levels of 0.79 and 0.25, respectively. Observing statistically significant increases in dot havens underscores that the effects are largely attributable to tax motivations, not simply to growth and expansion of IPO firms.

Having established an increased tax haven presence, we next test the extent to which firms engage in cross-border income shifting within this entity structure. We first study whether the IPO is associated with increased use of cost-sharing agreements. The coefficient of 0.038 on *Cost sharing agreement (0/1)* in Panel B, Column (1) means that the likelihood of having a subsidiary participate in such an arrangement increases by 3.8 percentage points

after the IPO. Based on the average value of 3.1 percent in this sample in the year prior to the IPO (Table 3), this effect is roughly a 125 percent increase after the IPO.

Results presented in Columns (2) through (5) further show that these agreements indeed facilitate cross-border payments. We observe a coefficient of 2.179 in Column (2) for *Total amounts received from U.S.* While higher levels of cross-border payments could be attributable to post-IPO growth within a vertically integrated multinational firm, two additional results link this increase to tax-motivated income shifting. First, the *Total amounts paid to U.S.* also increased (see Column (3)), but the coefficient is 1.061. Although the coefficient on inbound payments is within the 90 percent confidence interval of the coefficient on the outbound payments (the confidence interval is 0.750 to 3.609), the greater economic magnitude for amounts paid from (rather than to) the U.S. is suggestive of shifting income into lower-taxed jurisdictions after the IPO. Second, when refining the measures to only include those line items most related to income shifting in Column (4) (i.e., the sale of intangibles, royalty payments, licensing fees, etc.), we observe a similar coefficient as in Column (2). This means that the increased payments in Column (2) primarily relate to income shifting transactions, not to routine intercompany sales of goods and services that are explicitly excluded in Column (4). In Column (5), we observe a similar coefficient even after augmenting the measure to include intercompany payments from other foreign subsidiaries, meaning that the primary effect in Column (4) is attributable to shifting out of the U.S.

In summary, Table 5 shows that IPOs are associated with (i) increased haven ownership, (ii) higher likelihood of income shifting agreements, and (iii) increased intangibles-related payments to foreign subsidiaries. The evidence is consistent with firms creating and increasingly using multinational tax planning structures around the time that they go public.

4.3 Heterogeneity in Effects

4.3.1 Domestic R&D-Intensive Firms

We expect that the foreign planning and income shifting effects documented above should be the greatest in high intangibles firms. This is because intangible assets generate "mobile" income that can be more easily shifted via intercompany and cross-border transfers of internally developed intangibles (Grubert, 2003; De Simone, Mills and Stomberg, 2019). Thus, in this section, we test whether we observe heterogeneity in the foreign planning effects based on companies pre-IPO research and development (R&D) spending.[20]

Specifically, we calculate firms' average R&D spending over the period t-3 to t-1 using R&D expenses reported on U.S. tax returns.[21] We then partition the sample based on the median level of pre-IPO domestic R&D expense and re-estimate Eq. (1). Table 6 Panels A and B report results for *Havens (0/1)* and *Cost sharing agreement (0/1)*, respectively. In both panels, the coefficients suggest a larger economic magnitude for high R&D firms in Column (1) as compared to low R&D firms in Column (2). For example, the incidence of havens increases by 21.4 percentage points among high-domestic-R&D firms, as compared to 13.3 percentage points among low-domestic-R&D firms. This difference is marginally significant (p=0.101). In Panel B, we similarly observe a 1.6 percentage point difference for *Cost sharing agreement (0/1)*, although this difference is not statistically significant (p=0.34).

Observing these patterns further supports the inference that the effects in Table 5 are

[20]Under U.S. accounting rules, internally developed intangibles are not recorded as assets but instead expensed as research and experimentation. Thus, the most appropriate way to test for this heterogeneity is to examine domestic R&D expense.

[21]We use information reported on Form 6765 for the U.S. Research and Experimentation Tax Credit.

attributable to tax planning and not simply to growth among vertically integrated firms (which would otherwise suggest no variation in the results). Furthermore, these results reveal the extent of worldwide tax planning for high-intangibles firms: firms develop intangibles in the U.S., often reducing or minimizing their domestic taxable income through the R&D credit, and then prior to the IPO valuation event, shift domestically-developed IP offshore to source subsequent income to the foreign jurisdiction. These effects suggest that firms may pay minimal tax—to either the U.S. or foreign jurisdictions—in the years surrounding the IPO.

4.3.2 Mechanism: Effects of Professionalization

We next examine the mechanism by which firms engage in increased foreign tax planning. Recent work studies the role of professional tax advisors in business tax decisions, including responsiveness to tax reform changes (Bustos et al., 2022), increased take-up for tax-based subsidies (Goodman, 2021), and tax avoidance activities (Battaglini et al., 2020). For example, Zwick (2021) shows that, among firms eligible to claim refunds for tax losses, those that have more sophisticated tax advisors are more likely to do so. We build on this recent literature and test whether greater tax professionalization affects firms' IPO-induced foreign tax planning.

We obtain preparer information using identifying information about the accounting firm that prepares and signs IPO firms' income tax returns. As tax preparer data for our sample are most complete since 2005, our analysis covers the period 2005 to 2018.[22] In a similar vein

[22]Unlike the other data used in the manuscript, tax preparer information is not included in the sample of cleaned and edited IRS data; rather, we obtain this information from the unedited population of tax returns. To construct this variable, we use preparer EIN data reported on the bottom of Form 1120, Page 1.

to Zwick (2021), we define a sophisticated tax preparer as a firm that prepares tax returns for more than 4,000 C corporations in any given tax year. During the 2005 to 2018 period, 53 percent of the IPO sample consists of tax returns prepared by a sophisticated preparer (Table 2).

Figure 6 provides descriptive evidence about the proportion of IPO firms that use professional advisors. Figure 6a shows the fraction of IPO completing firms relative to the public and private samples. We observe similar patterns as the earlier haven ownership graph (Figure 4a). That is, IPO firms' use of professional advisors is similar to private firms in year *t-3* (41 percent by IPO firms versus 36 percent by private firms). The proportion then increases at a faster rate than private firms, converging to the same level as public firms immediately preceding the IPO (year *t-1*) and then slightly exceeding that level by year *t+2* (53 percent for IPO firms versus 50 percent for public firms). In contrast to the IPO firms, public firms' use of sophisticated tax advisors remains constant throughout the window. Figure 6b compares the use of professional advisors for IPO completing firms relative to withdrawn firms. Withdrawn firms have similar levels and increases in professional advisor use in year *t-3* and *t-2*, but these firms then shift to less sophisticated advisors around the time of the withdrawal.

Table 6 links increased professionalization to increased foreign tax planning. We partition IPO firms based on whether the firm switched to a sophisticated tax preparer at any point between year *t-2* and *t* relative to the IPO. We then re-estimate Eq. 1 for *Haven (0/1)* and *Cost-Sharing (0/1)* and report results in Panel A and Panel B, respectively. As in Table 5, we use the withdrawn sample because of the similarity of pre-treatment foreign planning trends. We find evidence consistent with larger foreign planning effects among those firms that

switched to larger tax advisors; for example, the likelihood of haven ownership increased by 30.8 percentage points in Panel A, Column (3) as compared to 11.0 percentage points among those that did not switch (Column (4)). This 19.8 percentage point difference is statistically significant (p=0.00). Similarly, the likelihood of participating in a cost sharing program increased by 8.3 percentage points (Panel B, Column (3)) as compared to 2.9 percentage points among non-switchers (Column (4)), and the 5.4 percentage point difference is also statistically significant (p=0.04).

These results suggest that more sophisticated advisors facilitated the implementation of international tax structures.[23] It is not possible to determine whether a higher level of tax professionalization leads firms directly to increase tax planning activity or whether pre-IPO managers seek out more sophisticated tax preparation firms. However, the trends and observed heterogeneity imply greater investment in and salience of foreign tax planning around the IPO.

4.4 Additional Analyses

4.4.1 Robustness Tests

We perform several additional analyses to check the robustness of our results to alternative samples and specifications. Table 7 presents results for the 2004-2018 sample using withdrawn companies as control firms; results for domestic *BTD* are presented in Panel A and for foreign *Havens (0/1)* in Panel B.

Results are generally robust across a number of tests. For ease of comparison, we repeat

[23]In contrast, untabulated results show that we observe little variation in domestic tax planning based on professional advisors. This is consistent with the fact that increases in *BTD* are largely driven by stock option deductions (Table 4), which can be claimed with little external professional advice.

the baseline results from Table 4 and Table 6 in Column (1) of Panel A and Panel B, respectively. Column (2) presents results without inverse probability weighting, confirming that the results are not driven by this approach. For *BTD*, the baseline coefficient is unchanged; for *Havens (0/1)*, the coefficient is slightly smaller but remains statistically significant. Column (3) shows results clustering standard errors by firm rather than by industry, following Bernstein (2015). Standard errors increase, but results remain statistically significant. Column (4) reports results after estimating Eq. 1 including time-varying controls for *Debt/Assets*, *ln(Age)*, *NetPPE/Assets*, *ln(Sales)*, and *Pre-NOL taxable income/Sales*. These controls have little effect on our baseline results. In Column (5), we relax the requirement that a firm must have four years of observations to be included in the sample (from $t-2$ to $t+1$). We instead require a firm to only be present for two years (year $t-1$ and t), resulting in a 25 percent larger sample across both panels. Estimation on these larger samples yields a somewhat smaller estimated coefficient for *Havens (0/1)* but little change for *BTD*.

Columns (6) through (9) of Panel A present three additional tests for *BTD*. First, we report results after Winsorizing at the 1 percent level, rather than 5 percent as in the baseline. We observe a slightly larger coefficient, suggestive of outliers indeed influencing the point estimates.[24] Column (7) reports results from alternatively scaling *BTD* by sales to mitigate concerns that effects are driven by large increases in assets around the IPO. We observe a modest decrease in coefficient size and significance. Column (8) reports results after limiting the sample to only those firms that file the requisite tax forms to construct the foreign planning sample (Schedule M-3). This restriction has little effect on the coefficient, confirming that results are unaffected by sample construction. Finally, Column (9) reports results

[24]We omit this column from Panel B because we do not Winsorize indicator variables.

using an alternative tax-based measure of *BTD*, which is equal to the difference in financial reporting tax expense and actual cash taxes paid (Mills, 1998). Given that the measure is based on differences in *tax liabilities* (and not differences in book and tax *income*), we find the expected smaller coefficient. Observing that this coefficient is statistically significant confirms that the inferences extend to this alternative measure.

4.4.2 U.S. Statutory Limitations on NOL Use

One concern is whether the documented increase in *BTD* is attributable to statutory restrictions imposed on the use of tax losses after large changes in ownership. U.S. tax law imposes a statutory limitation that reduces the amount of loss carryforwards a firm can use to offset post-IPO income.[25] While *BTD* should be relatively unaffected by this limitation because the measure is constructed using pre-NOL taxable income, we conduct two additional tests to further mitigate concerns about this limitation mechanically driving results. First, we construct an indicator equal to one for those firms 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.[26] Columns (1) to (4) of Panel C presents results from regressing this indicator on $IPOCompleted * Post$ to test whether the incidence of the limitation changes after the IPO. We find no statistically significant change,

[25]Specifically, Internal Revenue Code Section 382 limits the ability of a firm to use its net operating losses (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), Erickson, Heitzman and Zhang (2013), and Sikes, Tian and Wilson (2014) for more discussion of this limitation. This U.S. tax limitation should have essentially no effect on firms' propensity to implement foreign planning.

[26]Specifically, the indicator variable is equal to one if the firm NOL deduction (Form 1120, line 29a) is less than the maximum allowable NOL deduction. The maximum allowable NOL deduction is defined as the minimum of pre-NOL taxable income (Form 1120, line 28) minus special deductions (Form 1120, line 29b) and the firm net operating loss stock (Form 1120: Schedule K, line 12).

meaning that the limitation does not substantially affect the firms included in our sample, at least within the three years following the IPO. Second, we drop the small number of firms for which the statutory limitation appears to bind in Columns (5) to (8). The effect of the IPO on *BTD* declines only slightly in magnitude and remains statistically significant at the 1% level across all four samples.

4.4.3 Instrumental Variables Analysis

Our empirical design uses three distinct samples of control firms to isolate the effect of the IPO on corporate tax planning. In additional tests, we employ the instrumental variables (IV) approach of Bernstein (2015), who uses the 2-month NASDAQ composite return as an instrument for IPO completion. Specifically we use this instrument and then adapt his approach to our setting by using the stacked cohort empirical design in the second stage. We confirm that this is a valid instrument for the sample period 1994 through 2018 (F-statistic of 18.5), which includes the sample period that overlaps with Bernstein's sample (1985-2003). We find consistent, although slightly weaker results for *BTD* in the second stage; the coefficient is 0.085 (t-statistic of 1.613). For the period with requisite foreign data from 2004 to 2018 (which does not overlap with Bernstein's sample period), we observe that the instrument in the first stage is weak; the F-statistic is 4.6, lower than the critical value of 8.96 (Larcker and Rusticus, 2010; Stock, Wright and Yogo, 2002). Consequently, this weak instruments problem precludes us from using the instrument for the most recent subsample with available foreign data. See Online Appendix A for the tabulated analyses.

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 IPO firms, we document three findings. First, the IPO motivates firms to set up and expand their international tax structure to facilitate post-IPO income shifting. Second, professional advisors play an important role in this foreign planning. Finally, firms do not appear to engage in domestic tax planning, as IPO-related stock option deductions appear to provide sufficient tax shields to absorb domestic taxable income. The foreign planning effects are consistent with firms converging to the level and types of tax avoidance exhibited by the public firms in the sample.

When and why firms implement multinational structures is an open question in the literature. We provide direct evidence showing that the IPO is an event that motivates such activity. Beyond informing the academic and policy work related to addressing firms' cross-border income shifting activity, we also provide evidence relevant for domestic revenue projections. 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 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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Appendix A: Variable definitions

The table below provides our variable definitions. All data are sourced from the IRS unless otherwise noted, and all dollar-denominated variables are measured in thousands.

Table A1: Variable definitions

Variable	Definition[a]
Age	Tax year minus year of incorporation (Form 1120: box C)
Assets	Form 1120: Schedule L line 15(d) (when missing prior year *Assets*, use beginning-of-year assets from Form 1120: Schedule L line 15(b) and when missing beginning-of-year assets, use current year assets)
Asset growth	Percentage change in *Assets* from the prior year to the current year
Book income (loss)	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).
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*]
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 controlled foreign corporation (CFC)
Debt	Form 1120: Schedule L line 17(d) + line 20(d)
Debt/Assets	*Debt*/*Assets*
Domestic book income	*Book income (loss)* + *Federal income tax expense*
Domestic taxable income	*Pre-NOL taxable income (loss)*
# Dot havens	Number of CFCs reported on Form 5471 and located in countries designated with asterisks in the list of tax havens later in this table.
Equity financing	*Equity issuance*/[*Debt* + *Total paid-in capital*]

[a]Line numbers reference the 2012 tax forms unless otherwise noted.

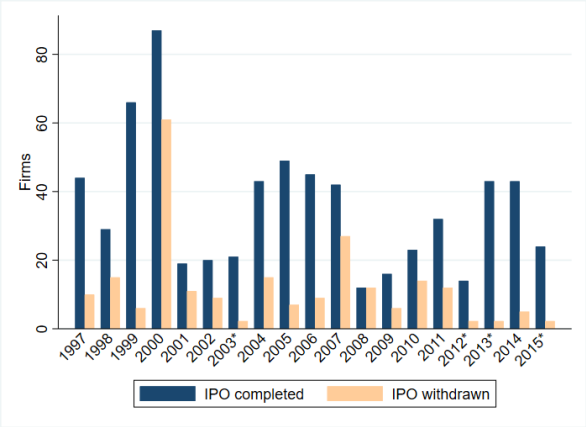
Table A1: Variable definitions (continued)

Variable	Definition
Equity issuance	Change in *Total paid-in capital* from the prior year minus the change in *Treasury stock* from the prior year.
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).
Foreign income items	(Schedule M-3, Part II, line 1 + line 2 + line 3 + line4 + line5 + line 10)
# Havens	Number of 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*; Jersey*; 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*. * indicates a "Dot" haven. List follows (Dyreng and Lindsey, 2009; Hines and Rice, 1994), adding Costa Rica as a "dot."
Havens (0/1)	Indicator variable equal to 1 if *# Havens* > 0
Industry	First two digits of Form 1120: Schedule K, line 2a
IPO Completed	Indicator variable equal to 1 for firms that complete an IPO, sourced from SDC Platinum and SEC filings
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))

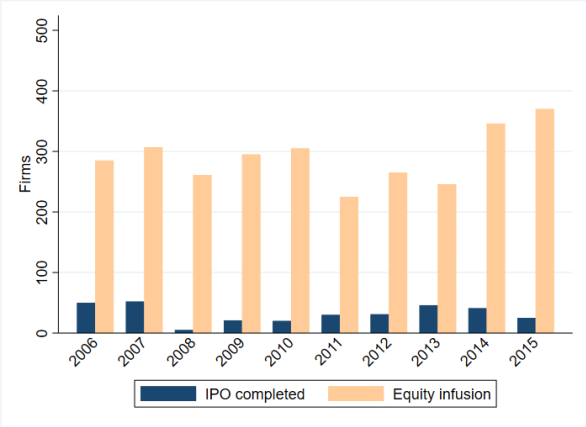
Table A1: Variable definitions (continued)

Variable	Definition
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).
R&D expense	Form 6765: maximum of line 9, line 53 and line 28
Sales	Form 1120: line 1c
Sales growth	Percentage change in *Sales* from the prior year to the current year
Stock option compensation	(Schedule M-3, Part III, line 9)
Taxes paid	Form 1120: Schedule L line 31
Total amounts received from U.S.	ln(Form 5471: Schedule M, line 12(b+c) + \$1,000)
Total amounts paid to U.S.	ln(Form 5471: Schedule M, line 24(b+c) + \$1,000)
Total income-shifting receipts from U.S.	ln(Form 5471: Schedule M, line 15(b+c) + line 16(b+c) + line 17(b+c) + line 18(b+c) + line 20(b+c) + \$1,000)
Total income-shifting receipts from U.S. and foreign	ln(Form 5471: Schedule M, line 15(b+c+d) + line 16(b+c+d) + line 17(b+c+d) + line 18(b+c+d) + line 20(b+c+d) + \$1,000)
Total paid-in capital	Form 1120: Schedule L lines 22b(d) + 23(d)

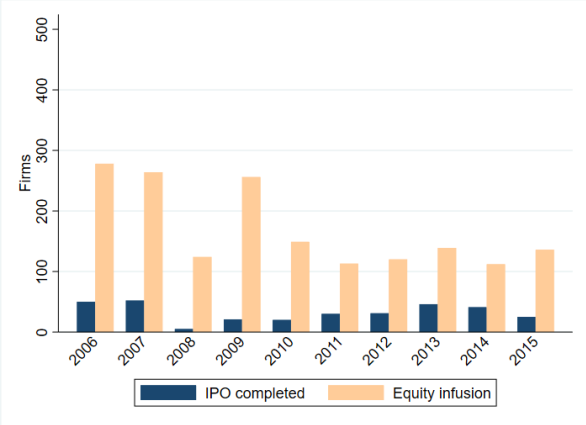
Figure 1: Firm counts



(a) Withdrawn IPO control group



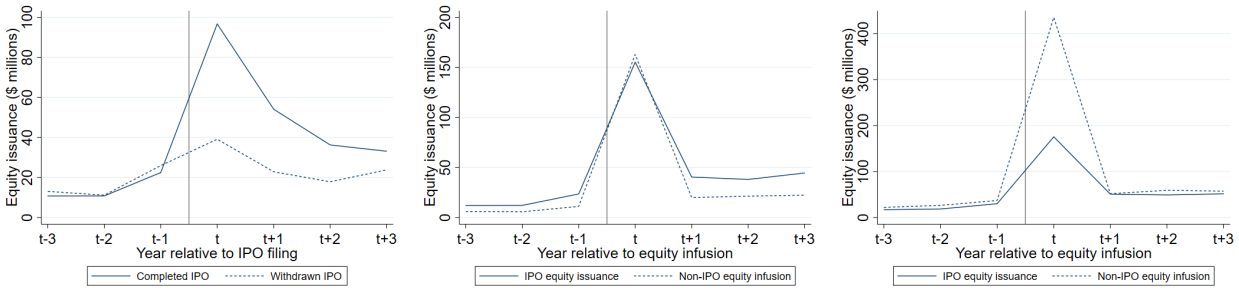
(b) Private firm control group



(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), Figures 1a, 1b, and 1c show the control groups of IPO withdrawers, private firms with equity infusions, and public firms with equity infusions, respectively. In 1a, 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 IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data.

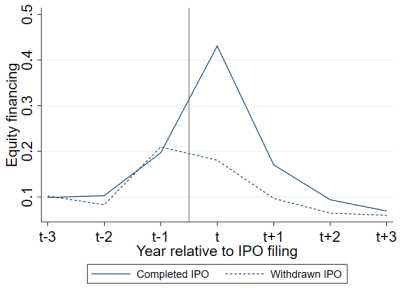
Figure 2: Equity issuance and financing trends around IPO filing and equity infusion



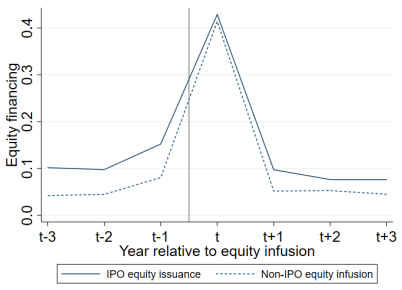
(a) Withdrawn IPO

(b) Private firms

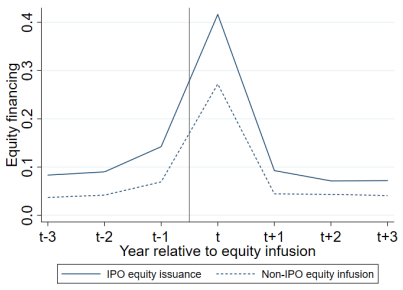
(c) Public firms



(d) Withdrawn IPO



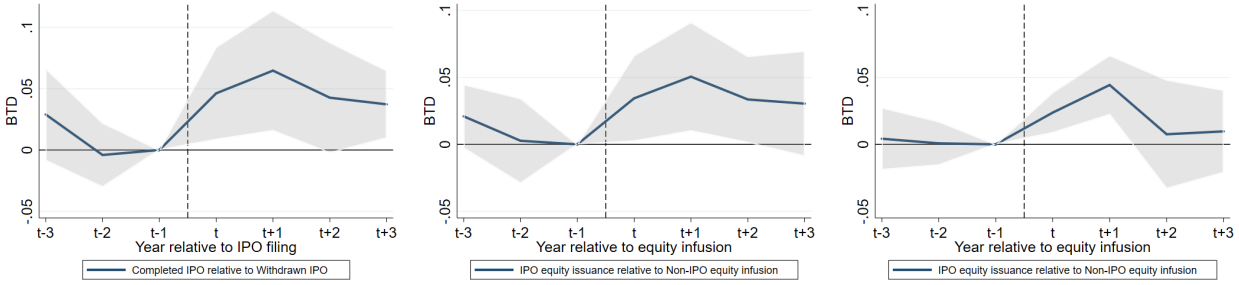
(e) Private firms



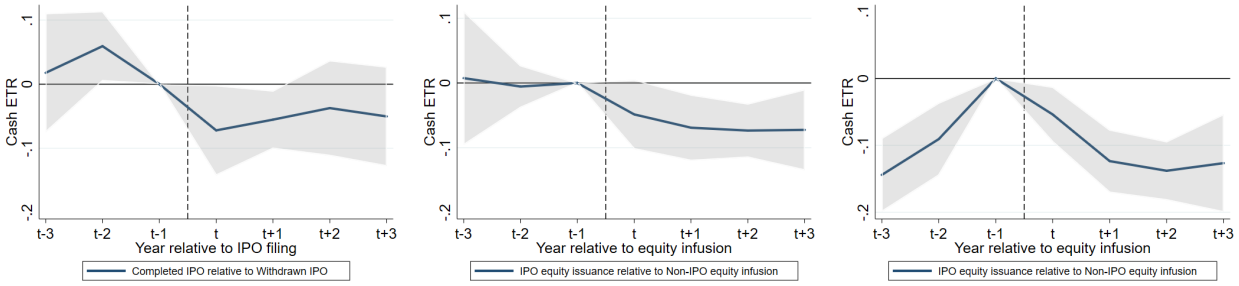
(f) Public firms

The figure presents averages of equity issuance (Figures 2a, 2b, and 2c) and equity financing (Figures 2d, 2e, and 2f) 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 (2a and 2d), privately held firms that issued equity of comparable magnitudes to the IPO firms and remained private afterwards (2b and 2e), and publicly held firms that issued equity of comparable magnitudes and remained public (2c and 2f). 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 2a and 2d and is the year of equity issuance in the other panels. All panels were created by the authors using IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data. Variables are defined in Appendix A; Section 3 describes the sample selection steps.

Figure 3: Domestic tax planning around the IPO



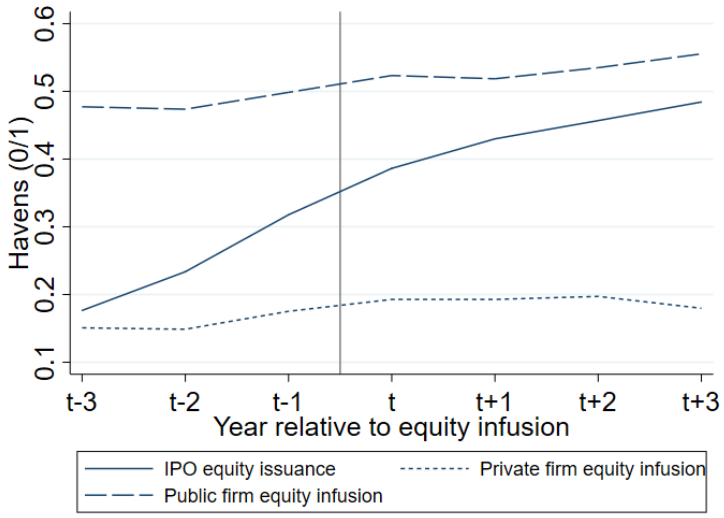
(a) Withdrawn IPO (b) Private firms (c) Public firms



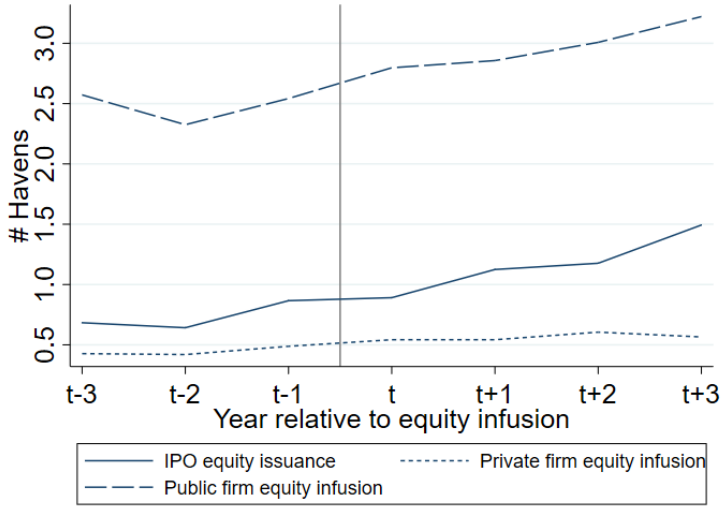
(d) Withdrawn IPO (e) Private firms (f) Public firms

This figure presents results from difference-in-differences regressions studying domestic Book-Tax Differences (Figures 3a, 3b, and 3c) and domestic Cash Effective Tax Rates (Figures 3d, 3e, and 3f) 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 (3a and3d), privately held firms that issued equity of comparable magnitudes to the IPO firms and remained private afterwards (3b and 3e), and publicly held firms that issued equity of comparable magnitudes and remained public (3c and 3f). Year t is the year of IPO filing in Panels 3a and 3d 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 IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data. Variables are defined in Appendix A; Section 3 describes the sample selection steps.

Figure 4: Tax haven trends around the IPO



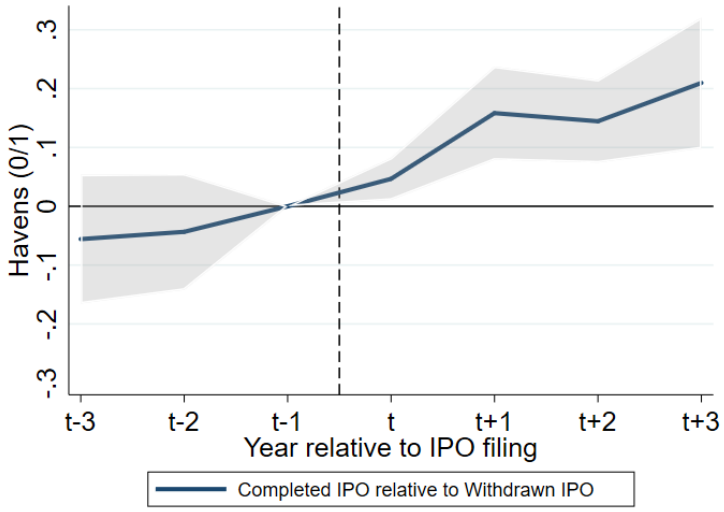
(a) Havens (0/1)



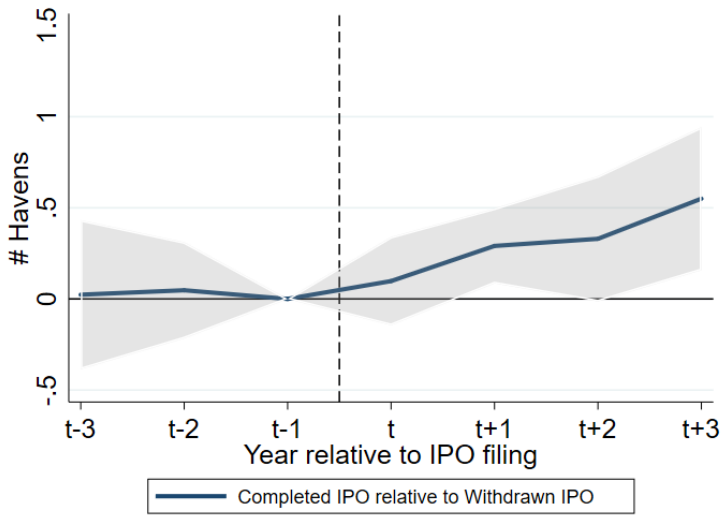
(b) # Havens

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. Figure 4a shows the average likelihood of having a controlled foreign corporation (CFC) in a tax haven; Figure 4b shows the average number of CFCs in tax havens. Both panels depict effects for 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 IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data. Variables are defined in Appendix A; Section 3 describes the sample selection steps.

Figure 5: Tax haven use around the IPO



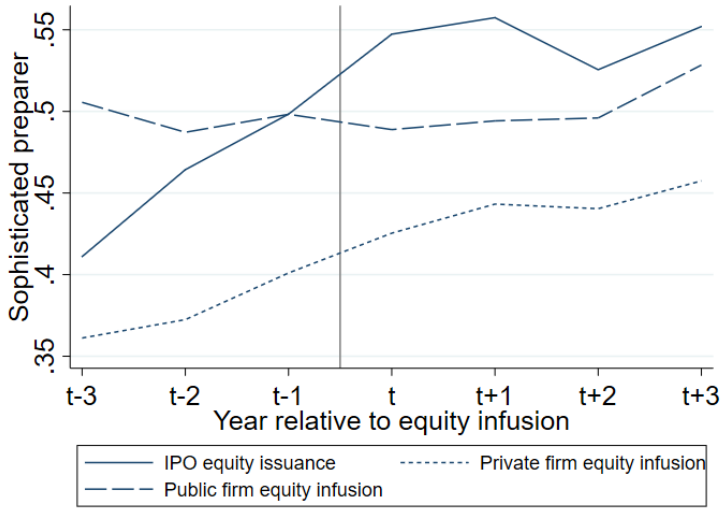
(a) Havens (0/1)



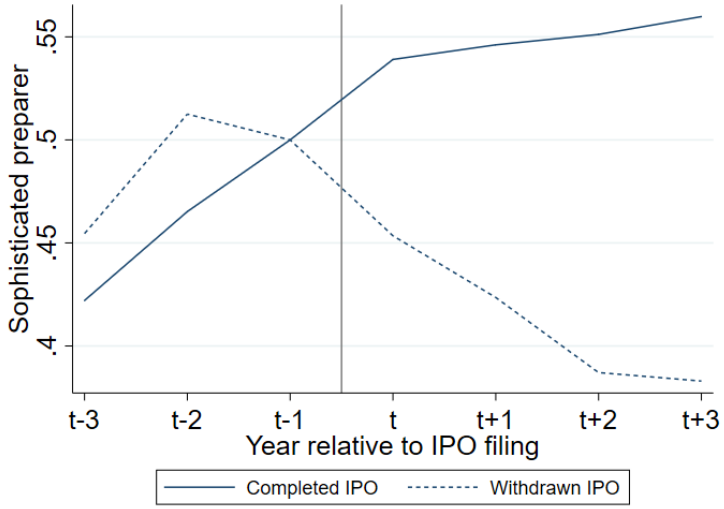
(b) # Havens

This figure presents results from difference-in-differences regressions studying controlled foreign corporation (CFC) presence in tax havens around the time of IPO filing. Dependent variables include an indicator for having a CFC in a tax haven (5a) and the number of tax haven CFCs (5b). 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 IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data. Variables are defined in Appendix A; Section 3 describes the sample selection steps.

Figure 6: Sophisticated preparer usage



(a) IPO completers compared to public and private firms



(b) IPO completers as compared to withdrawers

The figure presents trends in sophisticated preparer use for firms that complete an IPO as compared to control firms. In Figure 6a, the control firms include private and public firms that issued equity of comparable magnitudes to the IPO firms and remained private or public afterwards. In 6b, the control firms are IPO withdrawers. Year t is the year of equity issuance in 6a and is the year of filing in 6b. Both panels depict the average likelihood of using a sophisticated tax prepared, defined as an accounting firm that prepared over 4,000 tax returns during the year following Zwick (2021). Both panels were created by the authors using IPO data sourced from SDC Platinum, the SEC's Edgar platform, and IRS administrative tax data. Variables are defined in Appendix A; Section 3 describes the sample selection steps.

Table 1: Sample criteria

Panel A: IPO selection

	IPO events	
	Events remaining	Fraction completing IPO
U.S. IPOs from SDC Platinum, filing years 1996-2015	22,145	0.27
Exclude finance/utilities industries	17,735	0.25
Exclude unit offers, closed-end funds, REITS, ADRs,	5,846	0.60
LPs, blank check companies, and spin offs	5,741	0.60
Exclude issuance of noncommon stock	5,554	0.58
Observe EIN and IPO filing date from SEC Edgar	3,803	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	
Completed and withdrawn IPO samples	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining
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 obs. after completed IPO	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				
Private and public firm samples								
All 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 obs. if public/private status changed					(omitted)	30,571	(omitted)	20,002
All samples								
Require industry affiliation	1,990	19,177	765	4,765	2,905	30,526	1,691	19,995
Drop 1996 IPO cohort (no 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 sample, and the public firm sample. Panel A presents the steps to identify U.S. IPOs for non-financial firms from 1996 to 2015 from SDC Platinum following Lowry, Michaely and Volkova (2017). 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, as described in Section 3. Panel B also presents both the number of firms and firm-year observations after imposing requisite sample restrictions. Two counts are omitted to protect taxpayer privacy. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Table 2: Summary statistics

	Samples used in empirical analyses								
	IPO completing & withdrawing firms			IPO completing & private firms			IPO completing & public firms		
	Obs.	Mean	Median	Obs.	Mean	Median	Obs.	Mean	Median
Outcome variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
BTD	5,683	-0.068	-0.021	20,346	-0.032	-0.013	12,975	-0.016	0
Cash effective tax rate	2,006	0.204	0.172	8,252	0.194	0.118	6,954	0.190	0.162
Stock option compensation BTD	2,972	-0.005	0.000	19,606	-0.005	0.000	12,576	0.000	0.000
# Havens	2,960	0.788	0.000	19,448	0.771	0.000	12,570	3.142	0.000
Havens (0/1)	2,960	0.293	0.000	19,448	0.271	0.000	12,570	0.420	0.000
# Dot havens	2,960	0.217	0.000	19,448	0.232	0.000	12,570	1.110	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.	1,551	5.668	0.000	10,036	5.049	0.000	6,540	6.979	0.000
Total amounts paid to U.S.	1,551	4.966	0.000	10,036	5.246	0.000	6,540	9.095	13.546
Total income-shifting receipts									
from U.S.	1,551	4.450	0.000	10,036	3.786	0.000	6,540	4.808	0.000
from U.S. & foreign	1,551	5.056	0.000	10,036	4.465	0.000	6,540	6.460	0.000
Other firm characteristics									
Equity issuance	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	5,683	-8,483	-4,847	20,346	-3,623	-4,619	12,975	40,151	6,400
Federal income tax per books	5,683	2,527	0.000	20,346	3,815	0.000	12,975	23,182	1,352
Pre-NOL taxable income	5,683	-3,825	-3,544	20,346	1,822	-3,834	12,975	60,621	9,936
Taxes paid	5,683	1,775	0.000	20,346	2,982	0.000	12,975	17,563	188
BTD, numerator	5,683	-2,904	-1,046	20,346	-1,864	-1,466	12,975	879	-129
BTD tax, numerator	5,683	361	0.000	20,346	-253	0.000	12,975	2,957	0.000
Total assets	5,683	303,379	96,250	20,346	720,991	189,630	12,975	2,231,565	782,597
Asset growth	5,683	0.512	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	5,683	229,053	72,305	20,346	471,130	139,950	12,975	1,375,872	482,849
Sales growth	5,197	0.668	0.219	18,645	0.400	0.141	12,050	0.201	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/Assets$_{t-1}$	5,683	-0.227	-0.041	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
Uses a sophisticated tax preparer	2,524	0.533	1.000	16,061	0.505	1.000	8,609	0.554	1.000

The table presents unweighted counts as well as weighted means and medians for selected variables. The weights are inverse probability weights based on observable characteristics in year $t-1$. Columns (1), (2), and (3) present statistics for the sample of IPO completing and withdrawing firms from 1994-2018 (for IPOs filed from 1997 to 2015). Columns (4), (5), and (6) present statistics for the sample of IPO completing and private firms from 2004-2018 (for IPOs filed from 2006 to 2015). Columns (7), (8), and (9) present statistics for the sample of IPO completing and public firms from 2004-2018 (for IPOs filed from 2006 to 2015). Variables are defined in Appendix A. All dollar-denominated variables are reported in thousands. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations. All percentile estimates are averages of the ten 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	
	Obs.	Mean	Std. Dev	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	0.34
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
Pre-NOL taxable income/Assets$_{t-1}$	672	-0.28	0.58	228	-0.29	0.60	0.02	0.54
Pre-NOL taxable income/Sales	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
Pre-NOL taxable income/Assets$_{t-1}$	321	-0.17	0.38	2,905	-0.17	0.40	0.00	0.90
Pre-NOL taxable 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	0.50
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	0.89
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	0.47
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	0.21
Pre-NOL taxable income/Assets$_{t-1}$	321	-0.01	0.22	1,691	-0.04	0.21	0.02	0.68
Pre-NOL taxable 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	0.61
ln(Age)	321	2.81	0.87	1,691	2.69	0.76	0.12	0.97

The table compares firm characteristics for IPO-completing firms relative to the three control samples after inverse probability weighting. In Panels A, B, and C, the control samples are IPO-withdrawing firms, private firms, and public firms, respectively. Panel A describes firms in the year prior to IPO filing, while in Panels B and C it is the year prior to equity issuance. Columns (1), (2), and (3) show numbers of observations, mean values, and standard deviations, respectively, for the completed IPO sample. Columns (4), (5), and (6) show the same information for the control samples. Columns (7) and (8) give the difference in means and a within-cohort p-value for that difference. Variables are defined in Appendix A. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Table 4: Post-IPO domestic tax planning

Panel A: Book-tax differences (BTDs) and domestic cash effective tax rates (ETRs)

Dependent variable:	*BTDs*				*Cash ETRs*			
	Withdrawn IPO firms ('94-'18)	Withdrawn IPO firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)	Withdrawn IPO firms ('94-'18)	Withdrawn IPO firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IPO Completed × Post	0.044***	0.048***	0.031***	0.021**	-0.083***	-0.125***	-0.065***	-0.039**
	[3.868]	[4.057]	[3.021]	[2.158]	[-3.430]	[-2.777]	[-5.326]	[-2.246]
Year × Cohort FEs	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Observations	5,683	2,480	20,346	12,975	2,006	830	8,252	6,954
R-squared	0.427	0.345	0.366	0.384	0.567	0.573	0.569	0.592

Panel B: Decomposition of BTDs

Dependent variable:	*Stock option compensation*		
	Withdrawn IPO firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)
IPO Completed × Post	0.016***	0.003***	0.008***
	[4.525]	[3.588]	[6.253]
Year × Cohort FEs	+	+	+
Firm FEs	+	+	+
Observations	2,430	19,606	12,576
R-squared	0.417	0.540	0.423

This table presents regression results on firm domestic tax planning activity. Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of equity issuance or IPO filing. Panel A presents results for two measures of domestic tax planning: domestic book-tax differences as a share of lagged assets (Columns (1) to (4)), and the domestic cash effective tax rate (Columns (5) to (8)). Panel B presents results for stock-option-compensation book-tax differences as a share of lagged assets. In all regressions, IPO-completing firms are included and inverse probability weights are used. In Columns (1) and (5), withdrawn IPO firms are included, with data from 1994-2018. In Columns (2) and (6), the data are restricted to 2004-2018. In Columns (3) and (7), private firms are included, with data from 2004-2018. In Columns (4) and (8), public firms are included, with data from 2004-2018. Each firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+3$ around the equity issuance or IPO filing year. 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. Variables are defined in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Table 5: Post-IPO foreign tax planning and activity

Panel A: Haven ownership

Dependent variable:	*Havens (0/1)*	*# Havens*	*# Dot havens*
	(1)	(2)	(3)
IPO Completed × Post	0.161***	0.266**	0.101***
	[6.360]	[2.548]	[2.967]
Year × Cohort FEs	+	+	+
Firm FEs	+	+	+
Observations	2,394	2,394	2,394
R-squared	0.766	0.884	0.915

Panel B: Cross-border activity

Dependent variable:	*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. & Foreign*
	(1)	(2)	(3)	(4)	(5)
IPO Completed × Post	0.038***	2.179**	1.061**	2.320***	2.199**
	[6.165]	[2.508]	[2.453]	[2.768]	[2.558]
Year × Cohort FEs	+	+	+	+	+
Firm FEs	+	+	+	+	+
Observations	2,480	1,249	1,249	1,249	1,249
R-squared	0.426	0.801	0.793	0.787	0.806

This table presents regression results on firm foreign tax planning activity. Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of IPO filing. Panel A presents results for tax haven ownership. Panel B presents results for cross-border related-party agreements and payments. In all regressions, IPO-completing and IPO-withdrawing firms are included, with data from 2004 to 2018, and inverse probability weights are used. Each firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+3$ around the IPO filing year. 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. Variables are defined in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Table 6: Heterogeneity in foreign tax planning

Panel A: Haven ownership

Dependent variable:	Havens (0/1)			
Partition:	*High Domestic R&D*	*Low Domestic R&D*	*Switched to sophisticated preparer*	*Didn't switch to sophisticated preparer*
	(1)	(2)	(3)	(4)
IPO Completed × Post	0.214***	0.133***	0.308***	0.110**
	[5.168]	[4.322]	[5.853]	[2.347]
Year × Cohort FEs	+	+	+	+
Firm FEs	+	+	+	+
Observations	1,169	1,225	277	1,966
R-squared	0.761	0.783	0.839	0.759

Panel B: Cross-border activity

Dependent variable:	Cost sharing agreement (0/1)			
Partition:	*High Domestic R&D*	*Low Domestic R&D*	*Switched to sophisticated preparer*	*Didn't switch to sophisticated preparer*
	(1)	(2)	(3)	(4)
IPO Completed × Post	0.045***	0.029***	0.083***	0.029***
	[3.735]	[2.990]	[3.470]	[3.281]
Year × Cohort FEs	+	+	+	+
Firm FEs	+	+	+	+
Observations	1,224	1,256	288	2,033
R-squared	0.483	0.389	0.529	0.422

This table presents regression results on heterogeneity in firm foreign tax planning activity. Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of IPO filing. In Panels A and B, the dependent variables are indicators for owning a subsidiary in a tax haven and having a cost-sharing agreement in place with a foreign subsidiary, respectively. In all regressions, IPO-completing and IPO-withdrawing firms are included, with data from 2004 to 2018, and inverse probability weights are used. In each panel, Column (1) presents results for the subsample of firms with an above-median value of average domestic R&D expenditures in the pre-IPO-filing years (t-3 to t-1); Column (2) restricts to firms with a below-median value. Similarly, Column (3) presents results for the subsample of firms that switched to a sophisticated tax advisor in a year leading up to or including the IPO filing year (t-3 to t); Column (4) restricts to firms that did not. Each firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+3$ around the IPO filing year. 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. Variables are defined in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Table 7: Robustness Analysis

Panel A: Book-tax differences, withdrawn IPO firms control sample 2004-2018

Dependent variable:	*BTDs*								
	Baseline specification	Unweighted estimates	Clustering by firm	With controls	Require t-1 and t	Winsorize at 1%	Scale by sales	Limit to M-3 filers	Tax-based BTDs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IPO Completed × Post	0.048***	0.048***	0.048***	0.048***	0.048***	0.059**	0.034**	0.043***	0.003*
	[4.057]	[3.304]	[3.627]	[3.750]	[4.357]	[2.664]	[2.179]	[3.988]	[1.735]
Control variables	-	-	-	+	-	-	-	-	-
Year × Cohort Fes	+	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+	+
Observations	2,480	2,480	2,480	2,480	3,124	2,480	2,480	2,430	2,480
R-squared	0.345	0.352	0.345	0.353	0.392	0.285	0.452	0.348	0.289

Panel B: Haven presence, Withdrawn IPO firms control sample 2004-2018

Dependent variable:	*Havens* (0/1)				
	Baseline specification	Unweighted estimates	Clustering by firm	With controls	Require $t-1$ and t
	(1)	(2)	(3)	(4)	(5)
IPO Completed × Post	0.161***	0.118***	0.161***	0.146***	0.134***
	[6.360]	[3.895]	[4.322]	[5.329]	[6.802]
Control variables	-	-	-	+	-
Year × Cohort FEs	+	+	+	+	+
Firm FEs	+	+	+	+	+
Observations	2,394	2,394	2,394	2,394	3,020
R-squared	0.766	0.763	0.766	0.771	0.775

Table 7: Robustness Analysis (continued)

Panel C: Section 382 analyses

Sample:	Full sample				Dropping 382 limited firms			
Dependent variable:	*NOL deduction less than max allowable*				*BTDs*			
	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	0.012	0.004	0.018	0.025	0.040***	0.042***	0.027***	0.019*
	[0.761]	[0.149]	[1.187]	[1.275]	[3.437]	[3.465]	[2.693]	[1.913]
Year × Cohort Fes	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Observations	5,683	2,480	20,346	12,975	5,177	2,276	18,245	10,527
R-squared	0.541	0.588	0.568	0.640	0.427	0.349	0.367	0.395

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 an 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) of each panel show the number of observations, the mean values, and the standard deviations, respectively, for the completed IPO sample. Columns (4), (5), and (6) of each panel show the number of observations, the mean values, and the standard deviations, respectively, 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. Variables are defined in Appendix A. Data sources are SDC Platinum,the Statistics of Income division of the Internal Revenue Service and authors' calculations.

ONLINE APPENDIX

FOR ONLINE PUBLICATION

Online Appendix A: Alternative identification

This appendix discusses an alternative identification strategy to analyze post-IPO tax planning; specifically, we summarize results from using the instrumental variable (IV) from Bernstein (2015). As we describe in further detail below, we conclude that the Bernstein (2015) instrument – the two-month NASDAQ return – is not suitable for our setting, primarily because this approach suffers from a weak instrument problem for the more recent sample period used in testing our foreign tax planning outcomes. We begin by discussing the Bernstein (2015) strategy and then present results for this analysis.

Bernstein (2015) studies whether IPO completion affects firm innovation by comparing firms that complete an IPO with those that withdraw, instrumenting for the decision to complete an IPO with the NASDAQ composite two-month return following an IPO filing. The intuition for this instrument is that it reflects the sensitivity of managers to stock market changes during the IPO book-building process (Busaba, Benveniste and Guo, 2001; Dunbar and Foerster, 2008). As Bernstein (2015) notes, when market conditions worsen, firms may decide to withdraw an IPO instead of waiting for more favorable market conditions because of automatically expiring filing registrations. Furthermore, the decision to withdraw is influenced by the costs of waiting, which include prohibitions on disclosing new information to investors or banks and the inability to issue private placements. Consequently, the NASDAQ return should be positively associated with IPO completion: when market returns are higher, firms are more likely to complete their IPO.

We follow prior work and use this identification strategy on the sample of completed and withdrawn IPOs. The first-stage regression specification is as follows:

$$IPOCompleted_i \cdot Post_{cj} = \alpha \cdot NasdaqRet_i \cdot Post_{cj} + \gamma_{cj} + \delta_i + \epsilon_{ij}, \quad (3)$$

where the variable $NasdaqRet_i \cdot Post_{cj}$, equals the two-month NASDAQ return following firm i's IPO filing day, or is otherwise zero for years j prior to the IPO filing; γ_{cj} are fixed effects for cohort c in tax year j; δ_i are firm fixed effects (which subsume the firm-by-cohort fixed effects as each firm in our sample is only present in one cohort); and ϵ_{ij} is an additive error term.

In the second stage, we estimate a stacked-cohort difference-in-differences regression, as in Equation (1). In this specification, we instrument for $IPOCompleted_i \cdot Post_{cj}$ with the variable $NasdaqRet_i \cdot Post_{cj}$.[27]

For the IV strategy to be valid, the first stage estimation must demonstrate that the instrument is strong. Thus, we present results for the first-stage regressions in Table OA1, with results for the 1994-2016 sample period in column (1) and results for the 2005-2016 sample period in column (2). For both sample periods, the coefficient on the two-month NASDAQ return post-filing is statistically significant and has the expected positive sign. However, the F-statistic for the 1994-2016 sample is marginally weak, at 18.5. The F-statistic for the 2004-2018 sample is 4.6, which is much lower than the necessary critical value of 8.96

[27]Bernstein (2015) uses a cross-sectional regression specification in which the outcome variable in the second stage regression is the change in an average innovation metric (such as patent citations) over the five years after an IPO. The independent variable of interest is an indicator variable for whether a firm completed an IPO, and the regression includes a control variable for the average innovation metric over the three years preceding the IPO. We cannot perfectly extend this measure to our setting, however, because the corporate tax data have relatively few firm observations for three full years before and five full years after the IPO. For the 1996-2018 period, there are 342 total firms that meet this requirement (286 completed IPO firms and 56 withdrawn IPO firms); this number declines to 125 (105 and 20 completed and withdrawn IPO firms, respectively) for the 2004 to 2018 period. These numbers are very low as compared to our main sample of 900 firms described in Section 2 (672 completed IPO firms and 228 withdrawn IPO firms). Therefore, we adapt the Bernstein (2015) approach to use the most available data, resulting in estimation on an unbalanced panel.

(Larcker and Rusticus, 2010; Stock, Wright and Yogo, 2002). This latter value is particularly problematic given that the 2004-2018 sample is the period in which we test and find foreign tax planning effects for which we would ideally like to implement this alternative strategy. Consequently, this approach suffers from a weak instrument problem for our sample period of interest, mitigating our ability to draw inferences from the any second stage results.

Table OA1: First-stage results for IV estimation

Dependent var:	**IPO Completed × Post ('94-'18)** (1)	**IPO Completed × Post ('04-'18)** (2)
NASDAQ return × Post	0.665***	0.612**
	[4.299]	[2.138]
Post		
Year × Cohort FEs	+	+
Firm FEs	+	+
F-statistic	18.5	4.6
Observations	5,683	2,480
R-squared	0.757	0.759

This table presents results from a regression of *IPO Completed* interacted with *Post* on the *NASDAQ return* interacted with *Post*, where *IPO Completed* is an indicator variable equal to 1 if a firm completed an IPO, the *NASDAQ return* is the 2-month NASDAQ composite return post-IPO filing day, and *Post* is an indicator variable equal to 1 in the year of IPO filing and in the years after IPO filing for all firms in the sample. The regression specification is presented in Equation (3) in Online Appendix B2. The sample includes IPO completing firms and IPO withdrawing firms. Column (1) presents results for the sample period 1994-2018, which overlaps with Bernstein's (2015) sample period of 1985-2003. Column (2) presents results over the sample period 2004-2018, which corresponds to the sample period with the requisite foreign tax data. The regression specification is estimated on stacked cohorts, where each cohort corresponds to a calendar year of IPO filing. 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. Variables are defined in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.

Online Appendix B: BTDs and Earnings Management

Section 4.1 presents results showing that increases in *BTD* are largely driven by increased stock option deductions, which lower taxable income and increase the wedge between book and taxable income. In this section, we present additional evidence suggesting that a portion of the increased *BTD* effect is also attributable to post-IPO "earnings management" activity.

Prior work shows that managers of publicly traded firms manage earnings so that firms meet capital market benchmarks (see Dechow et al. 2010 for a review). Common strategies used to manage (and increase) earnings include adjusting financial statement accruals, which are financial accounting estimates of income and expenses that have not yet been earned or paid in cash. Earnings management incentives may be particularly strong around the IPO, as managers intend to both maximize the offering price at the time the firm goes public and meet subsequent earnings benchmarks to sustain their post-IPO stock price (Teoh, Wong and Rao, 1998; Teoh, Welch and Wong, 1998). However, because tax accounting methods are generally more "cash-based," tax law may require firms to reverse certain income-increasing accruals when calculating taxable income. If inflated financial statement income is "unwound" for tax purposes, this could drive larger *BTDs* in the post-IPO period.

To first assess earnings management activity, we decompose *BTD* into domestic book income and domestic taxable income. Like *BTD*, we scale by beginning-of-year assets. Results are reported in Table OB1, Panel A. Relative to all three control samples, both book and taxable incomes increase following an IPO. However, the coefficients for book income appear larger, suggesting a greater increase in book income relative to taxable income. For example, relative to IPO withdrawers, IPO completers see book income increase by 13.2 percentage

points after the IPO, whereas taxable income increases by a smaller, 8.8 percentage points. The effects are also statistically significant across all three samples for book income, whereas we only observe statistically significant increases in taxable income in Column (5).

These patterns alone do not permit a conclusion about earnings management; for example, increased book income could simply reflect higher sales and income of fast-growing IPO firms. Furthermore, the Table 4 results show that stock option deductions reduce post-IPO taxable income, thereby explaining different effects in Columns (4) to (6). Thus, we next conduct two additional tests. First, we partition the sample based on two financial reporting incentives: (i) whether the company was covered by an analyst post-IPO and (ii) whether the company reported high discretionary accruals. For both partitions, we then re-estimate Eq. (1) with *BTD* as the dependent variable, using the full set of control firms in all regressions.

Panel B reports results for the analyst partition. Firms with an analyst following are more scrutinized and thus should be more likely to manage their earnings. We measure analyst coverage using information from IBES; specifically, we identify firms with analysts based on non-missing values of *NUMEST* in year t to year $t + 3$ after the IPO. The results indicate that the increase in *BTD* is concentrated among firms with analyst coverage. For example, the coefficient in Column (1) means that IPO completers with analyst coverage have a 7.1 percentage point increase in *BTD*, whereas IPO completers lacking analyst coverage increase BTDs by only 1.3 percent of assets (and this increase is statistically insignificant in Column (4)). We observe similar patterns when comparing results in Column (2) to Column (5) for the private firm control sample, and when comparing Column (3) to Column (6) for the public firm sample.

Panel C reports results for the discretionary accruals partition. Discretionary accruals are

specific accounts that are particularly suited for earnings management activities. We merge the sample firms with Compustat data and estimate discretionary accruals based on the modified cross-sectional Jones (1991) model.[28] As in Panel B, we observe differences based on discretionary accruals, particularly among the public and private firm control groups. For example, we find an increase in *BTDs* of 4.8 percentage points for the high discretionary accrual group in Column (2) as compared to 1.8 percentage points in Column (5). Similarly, we observe an increase of 3.8 percentage points in Column (3) as compared to no effect in Column (6).

These tests document cross-sectional variation consistent with higher BTDs in firms with greater earnings management incentives. However, greater earnings will only contribute to *BTD* if the book earnings are unwound for tax purposes. Thus, in additional untabulated tests, we examine the specific book-tax difference line items to measure whether the income-increasing earnings management activity is excluded for tax purposes. We identify several line items on the book-tax difference reconciliation form (Schedule M-3) that capture differences in book income-increasing accounts and taxable income.[29] We observe variation consistent with earnings management activity. For example, we observe large *BTD* accrual adjustments by IPO firms relative to private firms, as private firms have no incentive to increase their earnings management behavior. We observe a weaker effect for withdrawn firms,

[28]The model is estimated annually by three-digit NAICS code; industries with fewer than ten observations are pooled together. Discretionary accruals are equal to the residual from estimation of the following model: $Totalaccruals_i/Bookassets_{i,t-1} = \alpha_1 \cdot 1/Bookassets_{t-1} + \alpha_2 \cdot 1[(Changeinrevenue - Changeintradereceivables)/Bookassets_{t-1} + \alpha_3 \cdot PPE/Bookassets_{t-1} + \varepsilon$. Total accruals are defined as income before extraordinary items (IBC), minus net cash flow from operating activities (OANCF) less adjusted to extraordinary items and discontinued operations (XIDOC). Book assets are defined as beginning-of-year total assets (AT). Change in revenue is defined as change in book sales (SALE). Change in trade receivables is defined as change in trade receivables (RECTR). PPE is defined as gross property, plant and equipment (PPEGT).

[29]These line items include deferred/unearned revenue adjustments, income from long-term contracts, bad debt expense, treatment of damages and awards, meals and entertainment expenses, and fines and penalties.

consistent with the fact that these firms likely also engaged in some earnings management activity prior to their withdrawal. Finally, we observe little difference with public firms that are subject to similarly high scrutiny around the time of their subsequent equity offering.

In summary, the evidence presented in this Online Appendix (for earnings management) and in Table 4 (for stock options) suggests that the increased BTD effect does not appear attributable to explicit tax planning. This decomposition analysis — which requires the detailed tax data — is critical for ensuring the correct inference with respect to domestic tax planning.

Table OB1: Analysis of BTDs

Panel A: Levels of domestic book income vs. domestic taxable income

Dependent var:	***Domestic book income***			***Domestic taxable income***		
	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)	(5)	(6)
IPO Completed × Post	0.132***	0.056***	0.030*	0.088***	0.015	0.009
	[3.766]	[2.773]	[1.916]	[2.760]	[1.052]	[0.876]
Year × Cohort FEs	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+
Observations	2,480	20,346	12,975	2,480	20,346	12,975
R-squared	0.662	0.721	0.711	0.645	0.710	0.689

Panel B: Heterogeneity based on analyst coverage

Dependent var:	***BTDs***					
Partitioning variable	***With analysts***			***No analysts***		
	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)	(5)	(6)
IPO Completed × Post	0.071**	0.156**	0.077*	0.012	0.033***	0.014
	[2.361]	[2.156]	[1.893]	[0.719]	[2.818]	[1.065]
Year × Cohort FEs	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+
Observations	1,548	19,412	12,041	1,488	19,189	11,818
R-squared	0.401	0.393	0.419	0.461	0.436	0.439

Panel C: Heterogeneity based on discretionary accruals

Dependent var:	***BTDs***					
Partitioning variable	***High discretionary accruals***			***Low discretionary accruals***		
	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)	Withdrawn firms ('04-'18)	Private firms ('04-'18)	Public firms ('04-'18)
	(1)	(2)	(3)	(4)	(5)	(6)
IPO Completed × Post	0.057***	0.048***	0.038***	0.043**	0.018	-0.025
	[2.889]	[3.217]	[5.701]	[2.520]	[0.962]	[-1.185]
Year × Cohort FEs	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+
Observations	1,019	18,759	11,388	1,030	18,774	11,403
R-squared	0.353	0.342	0.382	0.363	0.367	0.389

This table presents further analysis of BTDs along several dimensions. Panel A presents results for the effect of completing an IPO on the breakdown of BTDs into book income (Columns (1) to (3)) and taxable income (Columns (4) to (6), both measured as a share of lagged assets. Panel B presents results for heterogeneity

in estimates based on having analyst coverage in the post-IPO period or not having coverage (Columns (1) to (3) and Columns (4) to (6), respectively). Panel C presents results for heterogeneity in estimates based on having a high level of discretionary accruals post-IPO or having a low level of discretionary accruals post-IPO (Columns (7) to (9) and Columns (10) to (12), respectively). In each panel, Columns (1) and (4) show results for a control sample of firms that withdrew the IPO, with data from 2004-2018. Columns (2) and (5) in each panel show results for a control sample of private firms that completed an equity issuance, with data from 2004-2018. Columns (3) and (6) present results for a control sample of public firms that completed an equity issuance, with data from 2004-2018. All panels OLS results from estimating Equation (1) for the sample of completed IPO firms and control firms after inverse probability weighting (IPW). Equation (1) is estimated on stacked cohorts, where each cohort corresponds to a calendar year of equity issuance or IPO filing. Firms are only included in one cohort, and each cohort includes observations from $t-3$ to $t+3$ around the equity issuance or IPO filing year. 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. Variables are defined in Appendix A. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Data sources are SDC Platinum, the Statistics of Income division of the Internal Revenue Service, and authors' calculations.