IPOs and Foreign Tax Structures

Christine L. Dobridge,† Rebecca Lester,‡ and Andrew Whitten§

Abstract

Does going public affect the amount and type of corporate international tax planning? Using a panel of U.S. corporate tax return data from 2004 to 2018, we show that IPO completion is associated with the rapid implementation of multinational foreign tax structures. Specifically, within three years after filing for an IPO, firms (i) expand their presence in low-tax jurisdictions, (ii) enter into cross-border agreements that accompany intangible asset transfers to foreign subsidiaries, and (iii) increase their level of foreign related-party payments. Effects are strongest among firms with strong capital market pressure to hit post-IPO earnings targets, firms with high R&D spending pre-IPO, and firms with limited ability to use net operating loss carryforwards. We contribute to the nascent literature studying tax implications of IPOs by documenting the types and timing of specific tax strategies that enable public firms to remain lightly taxed in the post-IPO period.

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

JEL Codes: D12, E21, H24

I INTRODUCTION

Multinational firms' low tax burdens and international tax planning strategies have attracted substantial academic, policy, and press attention (Dyreng, Hanlon, Maydew, and Thornock 2017; Dyreng and Hanlon 2021; Grinberg and Kysar 2021; Drucker 2021). However, critical questions remain unanswered about these firms' international tax planning. For example, what drives companies to initially implement tax planning strategies? At what point in their life cycle do companies establish foreign tax structures, and how quickly do they implement these structures? This paper examines the type, amount, and timing of foreign planning. We find that completing an initial public offering (IPO) motivates firms to rapidly implement global tax planning strategies.

Understanding the relation between IPOs and international tax planning is important for at least two reasons.[1] First, considerable academic work focuses on the corporate tax planning of large public corporations (Wilson 2009; Hanlon and Heitzman 2010; Lisowsky 2010; De Simone, Klassen, and Seidman 2017; Wilde and Wilson 2018; Samarakoon and Organ 2025); however, there is less evidence on the tax strategies of smaller, innovative private firms, such as those that file to go public (Badertscher, Katz, and Rego 2013; Edwards, Hutchens, and Rego 2019; Lisowsky and Minnis 2020; Edwards and Hutchens 2024). There is some evidence that a substantial number of private corporations are insensitive to tax considerations due to large reported tax losses (Cooper and Knittel 2006; Cooper and Knittel 2010), whereas sophisticated public multinationals arbitrage tax rates and incentives across jurisdictions to reduce worldwide tax burdens (Hanlon, Lester, and Verdi 2015; Blouin and Krull 2018; De Simone and Olbert 2021). We connect these findings by documenting whether and to what extent the IPO is the point at which firms alter their approach to tax planning. Second, recent tax policy proposals have targeted tax avoidance by established multinational companies.[2] Understanding the genesis of these international tax strategies and the speed in which they are implemented is informative for crafting the appropriate policy mechanisms

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[1]We use the terms "tax planning" and "tax avoidance" interchangeably throughout the paper, where we define both broadly as the reduction of tax liabilities (Hanlon and Heitzman 2010; Wilde and Wilson 2018).

[2]For example, the OECD's Pillar 1 provisions are estimated to impact approximately 100 of the largest multinational companies (Delpeuch, Hanappi, Hugger, O'Reilly, and Whyman 2023).

to preserve the U.S. tax base.

Prior literature provides three reasons why the IPO should be associated with increased foreign tax planning. First, newly-public firms will face increased capital market scrutiny about their tax planning. Badertscher, Katz, Rego, and Wilson (2019) and Hoopes, Langetieg, Maydew, and Mullaney (2024) show that private firms engage in certain types of "conforming" tax avoidance given that their financial reporting and tax incentives are aligned. However, as firms transition to public markets, these incentives diverge, affecting managers' reporting and tax choices (Graham, Hanlon, Shevlin, and Shroff 2014). These findings imply that IPO firms will update their tax planning strategies to address these diverging reporting incentives. Second, the IPO provides opportunities to implement unique tax structures (Edwards, Hutchens, and Rego 2019), and certain characteristics of the U.S. tax system in place during our sample period prior to the Tax Cuts and Jobs Act (TCJA) specifically incentivized *foreign* tax planning (Foley, Hartzell, Titman, and Twite 2007; Graham, Hanlon, and Shevlin 2010; Blouin and Krull 2018). Third, because the costs of certain international tax strategies are based on the valuation of firm assets, firms have incentives to establish foreign structures around the IPO, when a lower or wider range of valuations is plausible.

Because this prior work provides clear predictions that an IPO should be associated with increased foreign tax planning, we focus on studying the *magnitude* and *timing* of such planning. Understanding the extent and pace at which firms implement these strategies is an open empirical question, particularly given that several factors may otherwise attenuate tax planning immediately after an IPO. For example, increased scrutiny by investors and the public could cause firms to shy away from establishing tax planning structures that have been a source of public consternation (Graham, Hanlon, Shevlin, and Shroff 2014; Gallemore, Maydew, and Thornock 2014). Second, going public results in more information being available to tax authorities, which may increase the likelihood of tax audit (Bozanic, Hoopes, Thornock, and Williams 2017). Third, even if firms engage in more tax planning, the planning may not relate to foreign operations given that these strategies can be costly and concentrated among more profitable companies (Lisowsky 2010). Finally, it is unclear whether firms will implement foreign structures within the relatively short three-year

post-IPO period that we study.

Despite the importance of this topic and the centrality of IPOs in prior research (Ritter and Welch 2002; Lowry, Michaely, and Volkova 2017), there is limited work quantifying the relation between corporate tax planning and IPO completion. The limited evidence is attributable to two factors. The first is an econometric challenge, as managers' decisions of whether and when to complete an IPO are endogenous. A common critique of the IPO literature is that it is difficult to address such endogeneity 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 challenge, we use a stacked-cohort difference-in-differences analysis in which we compare firms that complete an IPO (the treatment group) with firms that filed for an IPO but ultimately withdrew and remained private (the control group). IPO withdrawers are a natural control group to IPO completers because they are at a similar place in their life cycle, facing similar incentives to go public. Furthermore, evidence from prior work alleviates the concern that these firms are fundamentally different from IPO completers, as this literature documents a number of exogenous, market-wide factors that cause the IPO withdrawal decision (Busaba, Benveniste, and Guo 2001; Lian and Wang 2012; Bernstein 2015; Boeh and Dunbar 2021). We confirm that these two groups are similar on observable characteristics and exhibit parallel trends in foreign tax planning in the years leading up to the IPO filing. To further mitigate concerns about differences between the two groups and to better match the treatment and control firms on baseline characteristics, we employ inverse probability weighting (IPW) using pre-IPO asset growth, profitability, foreign presence, and foreign income growth, among other characteristics. In additional tests, we use alternative control samples of public firms and equity-issuing private firms, also matched on baseline characteristics using IPW weighting. The use of all three samples allows us to ensure the robustness of the results and generate a range of economic magnitudes.

We address the second empirical challenge—data availability—by using a panel of confidential U.S. corporate tax returns of public and private firms. Our data span 2004 through 2018, with a

focus on the seven years surrounding each firm's IPO filing. These data permit us to construct measures of foreign tax avoidance and to observe firms for several years prior to the IPO.[3]

We focus on three characteristics of foreign tax structures. We first study the strategy of locating corporate activities in jurisdictions with low statutory tax rates—so-called corporate "tax havens" (Hines and Rice 1994). We find that quickly after filing for the IPO—within three years—IPO completers are 9.0 to 12.5 percentage points more likely to own a subsidiary in a low-tax jurisdiction after going public. This corresponds to a 35 to 43 percent increase compared to pre-IPO ownership levels. The increase is particularly strong in so-called "small-as-a-dot" jurisdictions, where firms locate primarily for tax purposes as opposed to operational reasons.

Second, we test whether IPO firms set up structures that facilitate shifting income to foreign subsidiaries. In particular, we measure the incidence of a cost-sharing agreement—an arrangement in which a U.S. firm and its foreign subsidiary both contribute to the costs of developing intellectual property (IP) and both have rights to the income derived from it (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 that accompanies the establishment of such arrangement, firms may strategically time such transfers around the IPO. We find sizable IPO effects: The incidence of these agreements within IPO-completing firms *doubles* compared to IPO withdrawers.

Third, we use foreign-subsidiary measures of 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 receipts from both the U.S. parent and other related domestic entities ("outbound" payments from the U.S.). Moreover, we find that the increase in outbound payments to foreign subsidiaries is driven primarily by items 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; Grubert 2003; Faulkender, Hankins, and Petersen 2019).

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[3]Prior literature generally studies worldwide tax planning using consolidated tax measures from publicly available financial statements. The use of administrative U.S. tax data allows us to separate out foreign tax planning, enabling a more precise analysis of the tax strategies employed by firms.

To explore the mechanisms driving these effects and to further assess whether they are attributable to IPO-related tax planning, we conduct several cross-sectional tests. First, public capital market pressure (e.g., expecting firms to hit earnings targets) may contribute to the increased use of foreign tax structures because these structures are generally "non-conforming." This means that they reduce tax liabilities but with little negative impact on consolidated pre-tax financial reporting income. We therefore test whether the effects vary based on post-IPO analyst coverage (a proxy for capital market pressure) and confirm the effects are concentrated in firms covered by an analyst. Consistent with this explanation, we also find that results are concentrated in firms without a large equity holder, which otherwise would help insulate the firm from capital market pressures.

Second, we expect that high-intangibles firms will use foreign tax structures more because income from intangible assets is more easily shifted across borders (De Simone, Mills, and Stomberg 2019). We find that the results are indeed concentrated in firms with greater levels of domestic R&D spending as a proxy for innovative and high-intangibles firms.

Finally, we study the role of tax losses. Specifically, we examine whether results vary among firms likely subject to post-IPO limitations on net operating loss (NOL) deductions.[4] Prior to an IPO, a firm with large NOL carryforwards can use them to shield taxable income from tax liability. After an IPO, due to technical limitations on NOL use, an NOL firm could face a sharp change in incentives to implement foreign tax structures. In contrast, non-NOL firms would not experience this change in incentives. Thus, relative to non-NOL firms, we expect NOL firms to increase their tax planning more post-IPO. We find evidence in support of this prediction. Specifically, the effect of an IPO on opening a tax haven is 18.7 percentage points greater among firms that had a large NOL at the time of the IPO. This result is consistent with firms strategically implementing foreign tax structures to remain lightly taxed both domestically and internationally in the post-IPO period.

To assess the robustness of our results, we conduct a series of additional tests. We show that our results hold when using two alternative control samples—a public firm control sample that is

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[4]Specifically, Internal Revenue Code Section 382 limits a firm's ability to use an NOL carryfoward after a significant ownership change. An IPO could be considered an ownership change, thereby triggering the limitation. See Section V for technical details.

matched to the completed IPO sample using IPW weighting on measures of pre-IPO domestic and foreign growth trends, and an equity-issuing private firm control sample that controls for capital infusion effects, also matched using IPW weighting. The use of both samples reduces concerns that alternative explanations, such as firm foreign growth or external capital infusions, explain the observed effects. Additional analyses show that our results are also robust to different sample restrictions and estimation methodologies. Collectively, the evidence points to firms establishing and increasingly using foreign tax structures around the IPO.

This paper contributes to the literatures in finance, economics, and accounting that study the drivers of firm tax behavior. A key innovation of our paper is that it examines the transition of firms from private to public status, thereby bridging the extensive literature on public firms' tax avoidance (Dyreng, Hanlon, Maydew, and Thornock 2017) and prior work on the tax considerations of private firms (Badertscher, Katz, and Rego 2013; Badertscher, Katz, Rego, and Wilson 2019; Lisowsky and Minnis 2020; Olbert and Severin 2022; Hoopes, Langetieg, Maydew, and Mullaney 2024). Specifically, we provide new evidence about the origins of foreign tax structures, which are difficult to observe without access to the subsidiary tax data we use in this study. We find that the IPO motivates firms to substantially increase both their presence in low-tax jurisdictions and their cross-border transactions, contributing to the work documenting multinationals' foreign tax planning activities (Blouin and Krull 2009; Graham, Hanlon, and Shevlin 2010; De Simone and Olbert 2021; Bilicka, Guceri, and Organ 2024).

We also contribute to the literature that studies the consequences of public listing (e.g., Brav 2009; Michaely and Roberts 2012; Gao, Harford, and Li 2013; Bernstein 2015; Feldman et al. 2021). Some recent work considers the tax implications of IPOs, IPO pricing (Edwards and Hutchens 2024), industry tax planning (Chyz, Henry, Omer, and Wu 2022), transaction structures (Edwards, Hutchens, and Rego 2019), and compensation planning (Dambra, Gustafson, and Quinn 2020). We advance this literature by estimating the effects of IPOs on a range of foreign tax outcomes and explaining the mechanisms driving these effects. In particular, we evaluate the relationships between tax planning and pre-IPO R&D spending, tax loss restrictions, post-IPO analyst coverage,

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and post-IPO ownership concentration.

Our analyses provide policy-relevant insights. We provide evidence related to policies intended to motivate firms to retain investment and intellectual property in the United States, such as the Foreign-Derived Intangible Income (FDII) provision enacted as part of the TCJA in 2017. This provision underwent a few modifications as part of Public Law 119-21, which was enacted in 2025, to become the Foreign-Derived Deduction Eligible Income (FDDEI) provision.[5] The FDII/FDDEI provisions decrease the effective tax rate for intangible-related income derived abroad if the intangibles are owned domestically. If effective, the FDII/FDDEI provisions would change the incentives for firms, such as the IPO completers we study, to shift their IP offshore. We look forward to future research, once more recent administrative tax data are available, to evaluate whether these provisions have had such effects and altered the propensity of firms to establish and use foreign tax structures.

II MOTIVATION & PREDICTIONS

Prior literature establishes two facts that motivate our study. The first is the divergence between *financial* and *tax* reporting incentives for public firms. In general, firms prefer to report low levels of taxable income to minimize tax payments but high levels of book (financial reporting) income to appear more profitable to investors. When a firm has few other external reporting requirements— such as when the firm is private—then firms exhibit substantial "conformity" in book and tax reporting and focus primarily on cash tax savings (Graham, Hanlon, Shevlin, and Shroff 2014; Badertscher, Katz, Rego, and Wilson 2019; Hoopes, Langetieg, Maydew, and Mullaney 2024). However, once a company is public, capital market participants apply greater pressure on firms and managers to report high book income (Erickson, Hanlon, and Maydew 2004). Thus, public firms have greater incentives to use "non-conforming" tax strategies that reduce taxes paid but not

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[5]To counteract incentives for firms to move earnings and operations to lower-taxed countries, in addition to the FDII provision, the TCJA also included the Global Intangible Low-Tax Income (GILTI) and Base Erosion and Anti-Abuse Tax (BEAT) provisions. Public Law 119-21 enacted several changes to the GILTI (now the net CFC tested income provision), BEAT, and FDII (now FDDEI) provisions, all effective beginning in 2026. The broad intention of the changes to FDDEI is similar to that of FDII, with the 2025 changes expected to enhance the benefit of the provision (Hagerman and Napoli 2025).

book income (Badertscher, Katz, Rego, and Wilson 2019). Collectively, this literature implies that firms will alter the amount and type of tax planning they engage in around the IPO as the book and tax reporting incentives diverge.

Second, *foreign* tax planning plays a key role in public firms' tax avoidance strategies. Foreign planning allows firms to reduce their worldwide tax payments—without necessarily reducing financial statement income—by reporting income in lower-tax jurisdictions. Cross-border transactions are ultimately eliminated in financial reporting consolidation and thus have little impact on worldwide pre-tax foreign book income.[6] Foreign tax planning is especially relevant during our sample period (2004 to 2018) due to the relatively high U.S. statutory rate and the worldwide tax system with deferral (Foley, Hartzell, Titman, and Twite 2007; Graham, Hanlon, and Shevlin 2010; Dyreng and Hanlon 2021). Established multinational firms exhibited substantial foreign tax planning during this time, suggesting that younger, growing firms may initiate or ramp up foreign planning in anticipation of going public (Hanlon, Lester, and Verdi 2015; Edwards, Kravet, and Wilson 2016; De Simone, Klassen, and Seidman 2017).

Technical tax rules may further motivate foreign tax planning during the IPO window. Firms can lower tax burdens by partially or entirely transferring ownership of key assets to lower-tax foreign subsidiaries; however, the U.S. imposes exit taxes on these transfers based in part on the valuation of firm assets. Firms may therefore prefer to transfer assets to foreign subsidiaries around an IPO, when asset values are less certain and potentially lower than in the post-IPO period.[7]

Collectively, findings in the prior literature imply that we should observe a positive association between IPO completion and increased foreign tax planning. We confirm this relation in our data. However, the *magnitude* and *timing* of such tax planning effects are open empirical questions ex-ante. Thus, we focus on studying the extent to which, and how quickly, firms engage in such planning.

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[6]Furthermore, to the extent that firms report their foreign earnings as "permanently reinvested" in lower-tax jurisdictions before the TCJA, they could report *higher* book net income by reporting lower tax expense.

[7]Asset value uncertainty can lead to substantial litigation between firms and the IRS. For example, in 2022, AirBnB publicly disclosed a potential tax liability of $1.3 billion "relating to the valuation of our international intellectual property which was sold to a subsidiary in 2013" (AirBnB 2022, page 32).

While we expect firms to engage in increased foreign planning post-IPO, several factors could attenuate the magnitude or delay the timing of such effects. First, completing an IPO increases public scrutiny and the reputational costs of foreign tax planning, which could reduce a firm's propensity to establish foreign tax structures (Graham, Hanlon, Shevlin, and Shroff 2014; Gallemore, Maydew, and Thornock 2014). Second, the increased amount of information disclosed around the IPO is visible to tax authorities, who use public financial statement information for tax administration and enforcement purposes (Bozanic, Hoopes, Thornock, and Williams 2017). This may discourage firms from increasing tax avoidance.[8] Third, some firms may opt for domestic, rather than foreign, tax planning immediately around the IPO. Foreign strategies can be costly to implement, and prior work has documented foreign tax planning predominantly among larger and more profitable firms (e.g., Lisowsky 2010; Samarakoon and Organ 2025). Fourth, tax planning strategies take time to develop and implement, suggesting we might not observe strong effects within the three-year post-IPO period that we study.

III EMPIRICAL STRATEGY

Research Design

To assess the relation between IPO completion and foreign tax planning, we use a stacked-cohort difference-in-differences design that compares outcomes for IPO-completing versus IPO-withdrawing firms. Prior literature suggests that firms that file for an IPO but ultimately withdraw are a natural comparison group for IPO-completing firms, as these firms are at a similar phase in their life cycle and face similar incentives to go public (Bernstein 2015; Boeh and Dunbar 2021). We stack the cohorts in event time defined relative to the year of IPO filing (year t) and estimate the following specification:

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

In this equation, Y_{ij} is one of several foreign tax planning measures for firm i in tax year j, described further below under "Tax Planning Measures." $IPOCompleted_i$ is an indicator variable equal to

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[8]In survey evidence, Graham, Hanlon, Shevlin, and Shroff (2014) find that approximately 62 percent (70 percent) of firm respondents cite tax enforcement risk (reputational risk) as reasons for foregoing certain tax planning strategies.

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 for cohort c. We include year-by-cohort fixed effects γ_{cj}, which control for cohort-specific time effects, and firm fixed effects δ_i to control for time-invariant firm characteristics. Because each IPO firm is included only once in the sample, 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 average within-cohort difference in tax planning between IPO-completing firms and IPO-withdrawing firms after the IPO filing relative to any difference in the pre-period. We exclude time-varying control variables from Equation (1) because many firm-level controls would likely also be 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, allowing for industry-specific correlation in errors.

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

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

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}; thus all effects captured by β_k are measured relative to the difference between the IPO-completing and IPO-withdrawing firms in the year prior to IPO completion. In estimating Equations (1) and (2), we use sample weights described below in Section IV that help ensure the similarity of the IPO-completing and withdrawing firms on firm characteristics.

Tax Planning Measures

We study several measures of foreign tax planning (Y_{ij}) using data from IRS Form 5471, Information Return of U.S. Persons with Respect to Certain Foreign Corporations. We first construct four measures of presence in low-tax jurisdictions. *Havens (0/1)* is an indicator equal to one if a

firm owns at least one subsidiary in a low-tax jurisdiction.[9] *#Havens* is the count of subsidiaries in low-tax jurisdictions; *Dot havens (0/1)* and *#Dot havens* are a refined indicator and a refined count, respectively, capturing only those subsidiaries in "small-as-a-dot" jurisdictions where firms locate primarily for tax reasons. We follow Hines and Rice (1994) and Dyreng and Lindsey (2009) to determine a country's haven classification; see Appendix A.[10]

We also use Form 5471 data to detect tax-motivated income shifting. We first identify whether a foreign subsidiary was party to a cost-sharing agreement, in which the subsidiary shares IP development costs and is entitled to a portion of the subsequent IP-related revenue. At the time that the agreement is executed, the foreign subsidiary's "buy-in" payment should equal the future stream of profits that it expects to earn. However, firms may strategically time the agreement when valuations are uncertain (such as around an IPO) to justify a low buy-in payment, effectively shifting income out of the U.S. to a low-tax jurisdiction in future years (De Simone and Sansing 2019).[11] *Cost-sharing agreement (0/1)* is an indicator equal to one if at least one foreign subsidiary participates in such an agreement during the year; this variable is available beginning in 2008.

Next, we construct measures of intercompany payments between the U.S. parent and its foreign subsidiaries (De Simone, Mills, and Stomberg 2019). *Amounts received from U.S.* and *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 (measured in thousands of U.S. dollars). These measures permit us to study whether the amounts paid to foreign subsidiaries increase around the IPO relative to the amounts paid to the U.S. parent, consistent with tax-motivated

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[9]For brevity, we use the term "subsidiary" throughout the paper to refer to majority-owned foreign subsidiaries, also known as controlled foreign corporations (CFCs).

[10]Form 5471 data are available to us only for the even years of our sample window. In odd years, we impute the presence of subsidiaries based on whether the subsidiary reported positive beginning-of-year cash or other assets on its balance sheet (Schedule F) in the subsequent year. For example, we infer that a subsidiary existed in 2005 if its 2006 balance sheet reports nonzero beginning-of-year assets. For firms that do not report beginning-of-year values, we impute *#Havens* in a given odd year by taking the minimum of the counts in the prior and succeeding years. In general, our foreign subsidiary measures exclude partnerships and disregarded entities and therefore are lower-bound estimates for foreign tax presence.

[11]A 2011 Bloomberg article provides one example of a cost-sharing agreement 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).

income shifting.

Because a vertically integrated firm investing abroad could naturally have higher levels of intercompany payments, we also construct a refined measure that includes only those line items most closely related to income shifting: intercompany sales of property rights (such as patents and trademarks), platform contribution transaction payments, cost-sharing transaction payments, managerial and other service-related compensation, royalties and license fees, interest income (expense), and insurance premiums (Grubert 1998; Grubert 2003; Faulkender, Hankins, and Petersen 2019). *Income-shifting receipts from U.S.* is equal to the natural logarithm of one plus the total amounts received from the U.S. parent for these items (measured in thousands of U.S. dollars).[12]

IV   DATA AND SUMMARY STATISTICS

Sample Construction: IPO Filers

We construct the sample of IPO filers using data from Thompson Financial, the SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) platform, and administrative U.S. corporate income tax records. Table 1 shows the sample selection steps. We obtain data on all U.S. IPO filers between 1996 and 2016 from Thompson Financial's SDC Platinum New Issues database (22,484 events). Following standards in the IPO literature (Bernstein 2015; Lowry, Michaely, and Volkova 2017), we exclude financial firms, utilities, spin-offs, real estate investment trusts, American depository receipts, closed-end funds, blank-check firms, and unit funds.[13] We retain common stock issuances, as well as IPO filings with requisite tax identifiers and filing date information.[14] For the approximately 5 percent of firms with multiple filings, we discard all but the first IPO filing. We begin the sample with IPO filing year 2006 to align with our tax data, allowing for at least two pre-

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[12]For our income-shifting variables, cost-sharing transactions (payments or receipts) and insurance premiums paid appeared as separate line items beginning in 2008, and platform contribution transaction payments and receipts appeared beginning in 2012.

[13]We also explored analyzing so-called "de-SPAC" firms that go public by merging with a special purpose acquisition company (SPAC). We found, however, there were insufficient de-SPAC firm observations with requisite data during our sample period, which preceded the SPAC boom starting in 2020. Unfortunately, IRS data on foreign planning were not available at the time of writing to extend the sample period for purposes of studying SPACs.

[14]We use employer identification numbers ("EINs") as tax identifiers. Because EINs are often unavailable in SDC, we also scrape EINs from the EDGAR IPO filing.

IPO observations. We end the sample with IPO filing year 2015 because the 2016 data available to us are incomplete and have zero IPO withdrawers after applying our sample restrictions. The final IPO sample includes 1,220 unique IPO events, of which two-thirds are completed.

We use EINs to merge the IPO sample with a stratified random sample of 2004 to 2018 U.S. C corporation tax returns, retaining 78 percent of the IPO events: 670 IPO-completing firms (6,096 firm-years) and 287 IPO-withdrawing firms (2,055 firm-years).[15] These are the only years of tax data available to us with sufficient Form 5471 data covering the foreign activities of U.S. firms. We discard observations with non-positive values of gross receipts or total assets, as well as observations for withdrawing firms if they subsequently complete an IPO. To ensure our results are not driven by sample attrition, we require all firms to be present in the data (and to meet the restrictions above) for each year $t - 2$ through $t + 1$ relative to the year t of IPO filing. Finally, 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 2,417 observations for 379 firms, of which approximately 75 percent complete the IPO.

Figure 1a shows the distribution of sampled firms across cohorts, defined by the year of IPO filing. The number of IPO-completing firms is highest in the first cohort of 2006 filers. IPO withdrawal is highest among 2007 filers, coinciding with the start of the Great Recession. Figures 1b and 1c show trends in equity issuance and equity financing around IPO filing for completed and withdrawn IPOs, measured using the tax data. As expected, the figures show a large spike in equity issuance among IPO completers in year t, the year of IPO filing, validating the process to merge between the IPO and tax data.

Pre-IPO Sample Comparisons and Sample Weighting

Table 2 Panel A examines the ex-ante similarity of IPO-completing and IPO-withdrawing firms by reporting descriptive statistics on both groups of firms. Panel A presents average firm characteristics in year $t - 1$ for IPO completers (Column (1)) and withdrawers (Column (3)). Columns (5)

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[15]This stratified random sample, which always includes the largest U.S. firms, is constructed, cleaned, and edited each year by the IRS; see Zwick and Mahon (2017) and Feldman et al. (2021).

and (6) report the difference in means and corresponding p-value, respectively.

We find that the two types of firms resemble each other on many unweighted characteristics in the year prior to IPO filing. These similar features include (but are not limited to) asset growth, sales growth, foreign income growth, and all of our foreign tax planning outcome variables. We do not observe statistically significant differences on any of these dimensions. We only observe statistically significant differences for asset tangibility (*Net PPE/Assets*, statistically different at the 1 percent level), return on sales (*Pre-NOL taxable income/Sales*, different at the 5 percent level), and the probability of having a foreign subsidiary (*Foreign presence (0/1)*, different at the 10 percent level).

We further address concerns about underlying differences by applying inverse probability weighting (IPW) to the sample.[16] Specifically, we use a probit model to predict the likelihood of IPO completion based on $t - 1$ characteristics. We then weight the IPO completers (withdrawers) using the inverse of the predicted likelihood of IPO completion (withdrawal) derived from the first-stage estimation.

To control for foreign growth opportunities, our IPW predictors include *Foreign presence (0/1)* and the change in foreign income (*Foreign income change/Lag assets*). To control for firm growth and financial performance more generally, we also include *Asset growth* and return on sales (*Pre-NOL taxable income/Sales*). We further include measures of firm age (*ln(Age)*), size (*ln(Total assets)* and *ln(Sales)*), asset tangibility (*Net PPE/Assets*), and leverage (*Debt/Assets*).

Table 2 Panel B displays the estimated IPW first-stage coefficients. The coefficient for asset tangibility is statistically significant, consistent with the theory that firms with tangible assets are more easily valued and therefore less likely to withdraw due to investor mispricing (Busaba, Benveniste, and Guo 2001). The coefficient for foreign subsidiary presence is also statistically significant and positively correlated with IPO completion. On the other hand, the coefficient on foreign income growth is statistically insignificant and negative. Moreover, profitability as measured by *Pre-NOL taxable income/Sales* does not predict IPO completion, suggesting that poor

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[16]An alternative approach would be to entropy balance the samples, but the small size of some cohorts prohibits convergence and thus precludes this approach.

financial performance is not a strong determinant of IPO withdrawal in our sample. On the whole, most predictors of IPO completion are statistically insignificant, providing further support for use of IPO withdrawers as a control group.[17]

After applying IPW weights to the sample, we re-test differences across the two groups and present descriptive statistics in additional columns in Table 2, Panel A. Columns (2) and (4) present the IPW-weighted means for $t-1$ characteristics of IPO completers and withdrawers, respectively; Columns (7) and (8) report differences in means and p-values. The samples are no longer statistically different on any of the observed characteristics.

Summary Statistics

Table 3 displays summary statistics. To eliminate the influence of outliers on our results, we winsorize all nonbinary variables at the 5 percent level, following prior work using corporate tax data (Cohn, Mills, and Towery 2014; Yagan 2015; Zwick and Mahon 2017).[18] Approximately 29 percent of firm-year observations have a subsidiary in a low-tax jurisdiction, and 6 percent have a cost-sharing agreement. On average, firms report 0.87 entities in low-tax jurisdictions and 0.25 entities in "small-as-a-dot" or "dot" jurisdictions. The average (median) firm in our data has $464 million ($127 million) in assets and $294 million ($87 million) in sales. These firms are, unsurprisingly, young and growing rapidly. On average, they are around 9 years old. The average (median) firm experiences 39 percent (12 percent) annual growth in assets and 55 percent (21 percent) annual growth in sales. Both average and median taxable income amounts are negative, as measured by *Pre-NOL taxable income* as a share of lagged assets or sales; firms at the 75th percentile are profitable.

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[17]In Section V, we show that when we omit IPW weights in estimation, inferences are unchanged. These results are consistent with quasi-random, exogenous variation in market conditions playing an important role in the IPO completion decision (Busaba, Benveniste, and Guo 2001; Bernstein 2015). In a separate robustness test, we also find robust inferences when including the foreign tax planning outcome variables, measured in *t-1*, in the weighting process.

[18]Section V presents results after winsorizing at the 1 percent level. This generally leads to larger estimated effects, consistent with long right tails in the distributions of our nonbinary outcome variables.

V RESULTS

Foreign Tax Planning

Table 4 presents results from estimating Equation (1). As seen in Panel A, relative to IPO withdrawers, the probability that an IPO completer has a subsidiary in a low-tax jurisdiction (*Havens (0/1)*) increases by 12.5 percentage points following its IPO filing. Compared to IPO completers' average presence in these jurisdictions of 28.8 percent in the year prior to IPO filing (presented at the top of the table), this represents a 43 percent increase. To evaluate the parallel trends assumption in our difference-in-differences specification, Figure 2a presents results from estimating Equation (2) with *Havens (0/1)* as the dependent variable. The figure confirms that IPO completers and withdrawers exhibit similar pre-IPO trends in tax haven presence. We observe the increase in haven use by IPO-completing firms in the post-IPO period, corroborating the estimates from Table 4.

In Table 4 Column (2), we see that, relative to IPO withdrawers, IPO completers' number of subsidiaries in low-tax jurisdictions increases by 0.13 on average post-IPO, although the increase is not statistically significant. Figure 2b demonstrates that the relatively weaker effect is due to a delay—the number of havens appears to more meaningfully increase starting in year $t + 3$.

Columns (3) and (4) report results related to firm presence in "small-as-a dot" havens. We observe that the probability of having a subsidiary in a dot haven jurisdiction increases by 3.7 percentage points after the IPO; the number of dot haven subsidiaries increases by about 0.10. These two coefficients represent a 54 percent increase in the probability of having a dot haven subsidiary and a 42 percent increase in the number of dot haven subsidiaries among IPO completers, relative to year $t - 1$.

Having established that the IPO is associated with an increased presence in low-tax jurisdictions, we next test the extent to which firms engage in cross-border income shifting within this expanded foreign entity structure. We first study whether the IPO is associated with increased use of cost-sharing agreements with foreign subsidiaries. Column (1) of Panel B indicates that the likelihood of participating in such an arrangement increases by 7.5 percentage points after the IPO.

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This effect represents a *doubling* of the likelihood of such an agreement after the IPO, relative to year $t-1$.

Results presented in Panel B Columns (2) through (4) explore evidence of cross-border payments. We observe a positive coefficient of 1.72 in Column (2), which means that the *Amounts received from U.S.* by foreign subsidiaries increases after the IPO. 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 *Amounts paid to U.S.* also increased (see Column (3)), but the coefficient is 1.00. Although the coefficient on inbound payments is within the 90 percent confidence interval of the coefficient on the outbound payments (the confidence interval is approximately 0.1 to 3.4), the 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), we observe a similar coefficient as in Column (2), confirming strong effects for these particular types of cross-border payments.

In summary, Table 4 and Figure 2 quantify the extent to which IPOs are associated with (i) increased presence in low-tax jurisdictions, (ii) a higher likelihood of income-shifting agreements, and (iii) increased intangibles-related payments to foreign subsidiaries. Section V "Robustness Tests" presents a series of tests further demonstrating the robustness of results. The evidence is consistent with firms creating and increasingly using foreign tax structures within three years of going public.

Heterogeneity in Effects and Mechanisms

Analyst Coverage

We next study cross-sectional variation in the relation between IPO completion and foreign tax planning. We first focus on capital market pressures. We anticipate larger effects in those firms that experience a greater divergence between book and tax reporting incentives post-IPO (i.e., firms facing greater capital market pressure to report high book income). To test this, we examine heterogeneity in our outcomes based on post-IPO analyst coverage, assuming that firms followed

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by analysts have greater earnings and capital market pressure (Badertscher, Katz, Rego, and Wilson 2019). Using analyst data from I/B/E/S, we partition the sample of IPO completers based on whether the firm had analyst coverage in any of the years t through $t + 3$. We estimate Equation (1) separately for these two groups, using the full set of withdrawn IPO firms as the control sample each time.[19]

Table 5 Columns (1) and (2) present results for firms with and without post-IPO analyst coverage, respectively. Panel A shows results for *Havens (0/1)* as the outcome variable, while Panel B presents results for *Cost-sharing agreement (0/1)*. Following IPO completion, the likelihood of presence in low-tax jurisdictions increases by 12.6 percentage points for firms with analyst coverage, an effect of similar magnitude as in Table 4 for the overall sample. In contrast, the effect is smaller and not statistically significant for firms without post-IPO analyst coverage.[20] In Panel B, among firms with post-IPO analyst coverage, we see a statistically significant 7.7 percentage point increase in the likelihood of having a cost-sharing agreement after an IPO. We cannot run a similar regression for firms without analyst coverage, however, because none of the firms has a cost-sharing agreement during our sample period. Thus, the effect on cost-sharing agreements reported in Table 4 is driven purely by firms with analyst coverage. Overall, these results support our prediction: As expected, the increased use of foreign structures is greater among firms facing greater capital market pressures.

Ownership Concentration

To further test whether results vary based on capital market incentives, we also examine heterogeneity across firms with and without concentrated ownership post-IPO. Firms with higher ownership concentration would be more insulated from pressures to report high book income and thus may be less likely to engage in tax planning. We measure high inside ownership concentration based on

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[19]We cannot similarly partition the IPO withdrawers for this test because none have an analyst due to the fact they did not complete the IPO.

[20]Due to including the full sample of withdrawn IPOs as the control sample in both regressions, we are unable to test whether the coefficients in Columns (1) and (2) are statistically different, as the observations are not independent across the two subsamples.

whether the firm reports that it has a shareholder with either 20 percent direct ownership or 50 percent indirect ownership.[21] We present results in Columns (3) and (4) of Table 5 for firms without a large owner (low ownership concentration) and with a large owner (high ownership concentration) after the IPO, respectively.

As predicted, we observe that the increase in foreign tax planning is concentrated among firms with no large owner after the IPO (Column (3)), consistent with these firms facing more capital market incentives to implement foreign tax planning strategies. A firm with no large owner has a 17 percentage points greater probability of having a tax-haven subsidiary and an 11 percentage points greater probability of having a cost-sharing agreement after completing the IPO. These effects are statistically significant at 1 percent and 5 percent levels, respectively. In contrast, we observe no statistically significant effect on foreign tax planning structures for firms with a large owner after the IPO, and the magnitude of the coefficient appears considerably smaller. The difference in coefficients is not statistically significant, possibly due to the relatively small sample size in Column (4). Nonetheless, observing that the results appear concentrated in the group with no large owner provides further evidence that post-IPO capital market pressure contributes to the increase in foreign tax planning.

Domestic R&D-Intensive Firms

Our next cross-sectional prediction relates to whether foreign tax planning outcomes vary among IPO completers based on their R&D spending. We expect the effects to be greater in high-R&D firms because intangible assets generate "mobile" income that is more easily shifted across borders (Grubert 2003; De Simone, Mills, and Stomberg 2019; Bilicka, Devereux, and Guceri 2022). High-R&D firms also have stronger incentives to implement income-shifting structures around the IPO to reduce the U.S. exit tax when firms enter into cost-sharing arrangements.

Under U.S. accounting rules, internally developed intangibles are not recorded as assets but instead expensed as R&D costs. Thus, the most appropriate way to examine this cross-sectional

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[21]Specifically, we use whether a firm reports "Yes" to the Form 1120: Schedule K question 4a or 4b, which in summary, asks if any individual, estate, or corporate entity owned 20 percent or more of corporate voting power directly or 50 percent or more of voting power indirectly. We designate these firms as having a "large owner post-IPO."

variation is to examine domestic R&D expense as reported on firms' tax returns (also known as "research and experimentation" costs). We calculate firms' average domestic R&D spending reported on U.S. tax returns over the pre-IPO period from $t-3$ to $t-1$ using data from Form 6765 for the U.S. Research and Experimentation Tax Credit. We then partition the sample—both IPO completers and withdrawers—based on the median level of pre-IPO domestic R&D expense and re-estimate Equation (1). In both panels of Table 5, the coefficients indicate larger effects among high-R&D firms (Column (5)) as compared to low-R&D firms (Column (6)). The likelihood of presence in low-tax jurisdictions increases by 23.0 percentage points among high-R&D IPO firms but only by 6.5 percentage points among low-R&D IPO firms. This difference is statistically significant, with a p-value of 0.022, as reported in Panel A. In Panel B, we similarly find that the likelihood of having a cost-sharing agreement increases by 10.4 and 4.5 percentage points, respectively, among high- and low-R&D firms, although this difference is not statistically significant. Overall, the results point to firms developing intangibles domestically prior to shifting these assets offshore to source subsequent income to low-tax jurisdictions.

Tax Loss Firms

We conduct a final cross-sectional test based on whether a firm has a tax loss prior to the IPO. We focus on tax loss firms because the IPO is likely to have an even greater impact on these firms' incentives to tax plan relative to IPO firms with no tax losses. Prior to the IPO, NOL firms have tax losses that shield the company from paying tax. Therefore, NOL firms may have less incentive to engage in tax planning activity before the IPO.[22] However, after the IPO, these firms may be affected by Section 382 of the tax code, which limits the ability of a firm to use its NOL carryfowards if the firm has a greater than 50 percent ownership change. Thus, while both NOL and non-NOL firms have incentives to tax plan around the IPO, the NOL firms' incentives to tax plan may be even greater after the IPO because they can rely less on the tax loss to reduce their post-IPO tax liability.

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[22]Consistent with the intuition that NOL firms engage in less pre-IPO tax planning, we observe in untabulated results that in the year prior to IPO filing, firms with large NOLs have lower average values of five of the six tax planning outcomes than firms without large NOLs (and these differences are statistically significant).

To test whether tax planning behavior varies based on a firm's tax losses, we partition the sample based on whether the firm had a large NOL carryforward (greater than 10 percent of assets) in the year of IPO filing. In Column (7), the effect of completing an IPO on *Havens (0/1)* is positive and statistically significant for firms with large NOL carryforwards at the time of the IPO, with an estimated increase of 18.7 percentage points. In contrast, the effect is considerably smaller and insignificant for firms with no large NOL carryforwards (Column (8)). In Panel B, the increase in the probability of a having a post-IPO cost-sharing agreement is also greater among firms with large NOL carryforwards, but the difference is not as stark (7.8 percentage points and 6.4 percentage points, respectively, in Columns (7) and (8)). Overall, the evidence is consistent with our prediction of stronger tax planning effects among firms potentially affected by Section 382.

Additional Analyses

Alternative Explanation: Differences in Firm Quality

In this section, we further evaluate using withdrawn firms as a control sample in our empirical setting. As discussed above, IPO withdrawers are a natural control group for IPO completers given that both sets of firms are at similar points in their life cycles in deciding to raise public equity. However, since IPO withdrawal is not random, one concern is that withdrawn firms are fundamentally different, and such differences could potentially drive the observed effects in foreign tax planning instead of the IPO. For example, if these firms are of lower quality, or if they have lower growth prospects, then we could potentially misattribute the effects we observe to the IPO. We address this concern in four ways. First, in this subsection, we summarize relevant findings from the prior literature. Second, also in this subsection, we further examine the withdrawn firms along a number of dimensions to evaluate the quality of these companies. Third, we further show below that our results are robust to two alternative control samples of private and public firms to address concerns about firm growth. Finally, we look within the IPO completers to further address the growth explanation.

We first look to previous work studying drivers of IPO withdrawal. This work documents a

variety of contributing factors, of which firm quality is only one.[23] For example, some firms may be "dual tracking" an IPO with an M&A transaction, and they may ultimately withdraw the IPO if or when the M&A outcome is more favorable (Lian and Wang 2012). Furthermore, withdrawal is strongly correlated with indicators of market conditions (Busaba, Benveniste, and Guo 2001; Bernstein 2015), implying that exogenous variation plays an important role in the IPO withdrawal decision. A third factor contributing to IPO withdrawal includes intermediary characteristics, such as underwriter reputation and VC backing. The key takeaway is that many factors aside from firm quality or growth expectations contribute to the withdrawal decision, mitigating the concern that different firm-specific qualities of withdrawn companies drive the observed differences.

Second, to assess the quality of the IPO withdrawers in our sample, we examine post-withdrawal outcomes. Using IRS data has the advantage of allowing us to observe outcomes that most researchers cannot, such as post-withdrawal assets, sales, and equity issuance. Furthermore, using data from SDC Platinum and Capital IQ, we measure second-time IPOs, acquisition rates, and bankruptcies.

Table 6 Panel A presents outcomes in years $t + 1$ and $t + 3$ relative to year t of IPO filing for IPO withdrawers. In support of the view that withdrawn IPO firms are not poor quality firms overall, we find that these firms maintain asset and sales growth between 5 and 11 percent after withdrawing the IPO. Furthermore, these firms continue to secure equity financing, with approximately 65 percent obtaining financing within the first post-withdrawal year. While we note that approximately 7 percent of these firms file for bankruptcy, the same fraction also complete a second IPO.[24] Finally, about 20 percent of these firms are acquired by year *t+3*, suggesting these firms were attractive acquisitions. Our tests assume that—to the extent the withdrawn firms began the foreign tax planning process in preparation for the IPO—they likely intended to complete the IPO and had the same incentives for tax planning at that time. However, we acknowledge that for the 20 percent of firms that are eventually acquired, these firms may have potentially approached tax

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[23]See Busaba, Benveniste, and Guo (2001) and Boeh and Dunbar (2021) for a more detailed discussion of the theoretical relationship between a wide variety of firm characteristics and withdrawal probability.

[24]Boeh and Dunbar (2021) similarly report that 7 percent file for a second IPO, thereby validating our measurement and approach.

planning differently if they were also pursuing or considering M&A opportunities.[25]

Overall, the evidence indicates that IPO withdrawers are high-quality, growing companies, which supports using them as a control group in our setting.

Alternative Explanation: Firm Growth

Another alternative explanation for the observed increase in foreign presence of IPO firms relates to growth: While IPO withdrawers continue to grow post-withdrawal, IPO completers may have higher growth expectations, which could motivate them to implement foreign tax structures more rapidly. Notably, several of the preceding empirical analyses do not support this alternative explanation. For example, the IPW process balances the withdrawn firms and IPO-completing firms on pre-IPO foreign presence and foreign growth, as well as other measures of firm growth (asset growth, profitability, etc.), to ensure that the treatment and control firms appear similar on these dimensions. Second, it is unclear how growth alone would explain the observed increase in dot haven entities, as companies locate in those countries predominantly for tax savings purposes. Moreover, observing cross-sectional variation based on pre-IPO tax characteristics such as tax losses (Table 5) further reinforces the tax planning explanation.

Nonetheless, we further evaluate the growth explanation by constructing two additional control samples that are less subject to the growth concern. First, we construct a control sample of public firms. Public firms may have stronger growth expectations than withdrawn firms, and therefore using this alternative sample further "controls" for this firm characteristic. Second, we construct a control sample of private firms that have issued equity of comparable magnitude to IPO completers. Not only are these firms growing (based on the equity issuance), but these firms also receive a capital infusion like the IPO completers. Thus, use of this sample "controls" the effects of both growth and increased liquidity, thereby helping to further isolate the impact of going public.

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[25]In a further untabulated analysis, we compare other financial outcomes of withdrawn and completed IPOs in years *t+1* and *t+2* after year *t* of IPO filing. We find trends that suggest IPO completion is correlated with additional financial benefits for firms. For example, while the completed and withdrawn IPO firms were of similar sizes in terms of assets and sales prior to the IPO, the completed firms become (unsurprisingly) larger after the IPO. We also observe that in *t+1*, both the IPO-completing firms and the withdrawn firms report financial statement losses, but the magnitude of the loss is smaller for IPO completers than withdrawers.

As with the withdrawn firms, we match the public firms and the private firms to IPO-completing firms by IPW weighting on pre-IPO foreign growth and foreign presence, as well as other characteristics, including asset growth and sales growth. Appendix Table B1 presents sample selection and summary statistics for both samples. Table B2 provides a comparison of outcome variables and firm characteristics in the pre-IPO year for both samples compared to the IPO completing firms. The descriptive statistics confirm that IPO completers are similar to both groups of control firms in terms of foreign tax planning outcomes and other characteristics prior to the IPO. The table also provides first-stage IPW results for both samples.

Table 6, Panel B presents results from estimating Equation (1) using the public and private firm control samples. For the public firm control sample, we present results for *Havens (0/1)* in Column (1), *Dot havens (0/1)* in Column (2), *Cost-sharing agreement (0/1)* in Column (3), and *Income-shifting receipts* in Column (4). We present results for the same set of variables for the private firm control sample in Columns (5) to (8).

Across all eight columns, we observe statistically significant coefficients that show that the observed results are not driven by differences in the withdrawn firms. We note that the magnitudes are slightly smaller than those in Table 4, but still economically significant. For example, compared to public and private firms, IPO-completing firms have a 10.3 percentage point and 9.0 percentage point, respectively, higher probability of having a tax-haven subsidiary after the IPO. This compares to 12.5 percentage points in our main specification. Similarly, IPO-completing firms have a 4.9 and 2.3 percentage point higher probability of having a cost-sharing agreement after the IPO, compared to the public and private control samples. This compares to 7.5 percentage points in the main specification. Finding similar results using these alternative control samples further corroborates the choice of IPO withdrawers as an appropriate control group. Furthermore, these results increase confidence that the IPO itself is a primary driver of the observed foreign tax planning and provides a range of magnitudes to evaluate the overall effect size.

In our final analysis of the growth explanation, we study both the extensive and intensive margin effects of an IPO on the use of foreign subsidiaries (CFCs). If the IPO drives foreign planning, then

we would expect to see differential trends in growth for haven versus non-haven entities. Thus, we compare IPO firms' expansions into low-tax jurisdictions with expansions into other jurisdictions.

Figure 3a presents trends in IPO completers' likelihoods of having CFCs in low-tax and other jurisdictions around the IPO completion year t. For both measures, the likelihood is indexed to one in year $t - 3$ for ease of comparing the rates of change. Figure 3b presents trends in the intensive margin, studying the average number of CFCs in low-tax and other jurisdictions among IPO completers, again indexed to one in year $t - 3$.

Both panels of the figure are inconsistent with a growth explanation, which would predict that firms expand more into the better-developed markets of higher-tax countries rather than in low-taxed jurisdictions. Specifically, Figure 3a indicates that entry into low-tax jurisdictions around the IPO is in fact occurring much *faster* than entry into other areas. The likelihood of having a CFC in a low-tax jurisdiction increases by approximately 175 percent between $t - 3$ and $t + 3$, compared to approximately 25 percent in other jurisdictions. On the intensive margin, Figure 3b shows that from $t - 3$ to $t + 3$, the number of CFCs in both low-tax and other jurisdictions double, whereas growth would otherwise likely suggest higher expansion rates in non-haven locations. These figures, when considered with the robust results across three control samples, help further reinforce that the observed effects are attributable to the IPO.

The Use of Tax Professionals

We next assess whether use of professional tax advisors is related to the tax planning effects we observe. Recent work suggests that professional tax advisors play an important role in firm tax planning (Battaglini, Guiso, Lacava, and Patacchini 2020; Goodman 2021; Zwick 2021; Bustos, Pomeranz, Suárez Serrato, Vila-Belda, and Zucman 2022). Recognizing that using a sophisticated tax preparer is an endogenous firm decision with respect to implementing foreign tax planning strategies, we descriptively study whether tax preparers are associated with the outcomes we observe.

Using the tax data, we identify the accounting firm that prepares and signs each company's

income tax return.[26] Similarly 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. In the year prior to filing an IPO, about 50 percent of both the completed and withdrawn IPO samples use a sophisticated preparer.

In untabulated results, we find evidence consistent with larger foreign planning effects among those firms that switched to more sophisticated tax advisors in the years immediately preceding the IPO. For example, there is a 22.6 percentage point statistically significant difference in the likelihood of participating in a cost-sharing agreement among firms switching to more sophisticated preparers (27.7 vs. 5.1 percentage points).

We perform two additional untabulated analyses to better understand how the foreign planning effects relate to increased professionalization. First, if IPO firms hire new executives in the years prior to the IPO, and if the executives select different tax advisors, then the foreign planning effects may ultimately be attributable to the change in executives. However, we find that only 17 percent of IPO completers change executives, mitigating concerns that the observed professionalization effects are driven by underlying changes in management.[27] Second, we assess whether the association between foreign planning and tax preparers is instead attributable to venture capital (VC) ownership. However, when comparing VC-backed IPO completers relative to VC-backed IPO withdrawers (untabulated), we continue to find statistically and economically significant increases in the use of both subsidiaries in low-tax jurisdictions and cost-sharing agreements for IPO completers in the post-IPO period. Thus, neither changes in executives nor VC backing seem to explain the association between the use of professional tax advisors and foreign tax planning. While descriptive in nature, this analysis sheds further light on the potential role of tax advisors in IPO planning.

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[26]We obtain tax preparer information from the unedited population of tax returns, using preparer EIN data reported on the bottom of page 1 of Form 1120.

[27]We test this using data on the officers of the firm as reported on Form 1125-E. We identify top executive changes if any of the five highest-paid officers in year *t-2* is no longer an officer in year *t*. One caveat is that, because of measurement changes in the annual samples of tax data, we can only capture these changes for a small fraction of IPO-completing firms toward the beginning and end of our sample period.

Effective Tax Rates

In untabulated analyses, we find evidence consistent with increased post-IPO foreign tax planning leading to cash tax savings, as expected based on the tax-planning literature (Hanlon and Heitzman 2010). Specifically, when limiting the sample to those with positive pre-tax book income and no large NOLs, we find that IPO firms reduce their ETRs by approximately 8.8 percentage points in the subsequent two years as compared to the withdrawn firms. Furthermore, when calculating long-run cash effective tax rates and Henry and Sansing (2018) one-year cash tax differences among the IPO completers, we find that firms that open their first subsidiary in a tax haven around an IPO report reduced rates. These latter estimates are noisy, however, likely due in part to the relatively small sample size of firms with requisite data for these second ETR tests.

Robustness Tests

Finally, we perform several additional analyses to check the robustness of our results to alternative samples and specifications. Table 7 presents results for *Havens (0/1)* in Panel A and for *Cost-sharing agreement (0/1)* in Panel B.

Our results are generally robust across a number of tests. For ease of comparison, we repeat the baseline results from Table 4 in Column (1) of both panels. Column (2) presents results without IPW, confirming that the results are not driven by this technique. Column (3) shows results clustering standard errors by firm rather than by industry, following Bernstein (2015). Standard errors decrease for presence in low-tax jurisdictions and increase for cost-sharing agreements, but the results remain statistically significant in both cases. Column (4) reports results after estimating Equation (1) including time-varying controls for *Debt/Assets*, *ln(Age)*, *NetPPE/Assets*, *ln(Sales)*, and *Pre-NOL taxable income/Sales*, which slightly attenuates the results. In Column (5), we relax the requirement that firms must be observed four years (from $t-2$ to $t+1$) to be included in the sample, instead requiring only two years ($t-1$ and t). This results in approximately a 6 percent larger sample across both panels. Estimation on these larger samples yields a slightly larger coefficient for *Havens (0/1)* and a slightly smaller coefficient for *Cost-sharing agreement (0/1)*, but both

remain significant.

Column (6) presents results from alternative estimation.[28] Specifically, we implement the Callaway and Sant'Anna (2021) approach to estimating a robust staggered difference-in-differences method. This method is an alternative correction to potential bias in a staggered or stacked-cohort difference-in-differences design in which treatment effects may be heterogeneous (Baker, Larcker, and Wang 2022). We use the same data sample in the estimation as in the main specification. Results are similar to the main specification, with a slightly smaller coefficient for *Haven (0/1)* and a slightly larger coefficient for *Cost-sharing agreement (0/1)*. Observing statistically significant coefficients across all columns confirms the robustness of the main inferences.

VI   CONCLUSION

This paper studies the extent to which foreign tax planning activity changes around U.S. IPOs. Using confidential corporate tax data for IPO firms, we document two key findings. First, we quantify large effects of the IPO on tax planning. We measure the extent to which the IPO motivates firms to set up and expand their international tax structure to facilitate post-IPO tax strategies. Second, the effects are greater among firms with (i) increased pressure on financial reporting earnings, (ii) high levels of intangibles, and (iii) historical tax losses, suggesting that firms strategically implement these structures to prepare for post-IPO profitability. The results are robust to two additional control samples and do not appear to be due to alternative explanations.

When and why firms implement multinational structures is an open question in the literature. We provide direct evidence that the IPO is an event that motivates such activity. Beyond informing the academic work about firms' cross-border income-shifting activity, we also provide evidence relevant for policy. Specifically, the FDII and FDDEI policies were intended to motivate companies

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[28]We also considered an alternative estimation approach using an instrumental variables strategy in the vein of Bernstein (2015), instrumenting for IPO completion using NASDAQ stock market returns in the two months following IPO filing. This leverages the sensitivity of managers to stock market changes during the IPO book-building process (Busaba, Benveniste, and Guo 2001; Dunbar and Foerster 2008). However, in our sample period—which does not overlap with Bernstein's sample period of 1985 to 2003—we find a weak first-stage correlation between the NASDAQ return and IPO completion. The F-statistic for the first-stage regression in our sample is 4.6 (untabulated), which is much lower than the necessary critical value of about 9 (Stock, Wright, and Yogo 2002; Larcker and Rusticus 2010). This weak-instrument problem precludes us from using this strategy in our setting.

to retain domestically developed IP in the U.S. by taxing foreign income generated from such IP at lower rates. If effective, these provisions should reduce firms' incentives to set up low-tax operations and shift income offshore. We look forward to future work that evaluates whether the FDII/FDDEI provisions have had such an impact and that more generally further evaluates tax planning around IPO transactions.

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Figure 1: Firm counts, equity issuance, and equity financing
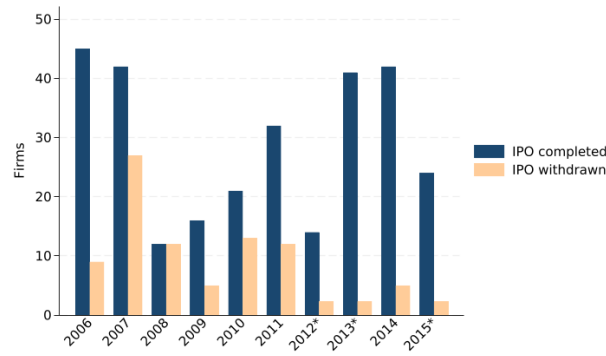
(a) Firm counts
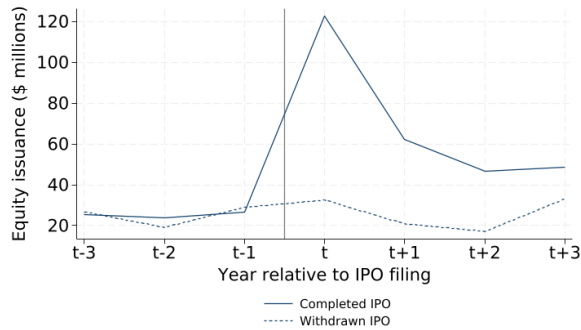
(b) Equity issuance
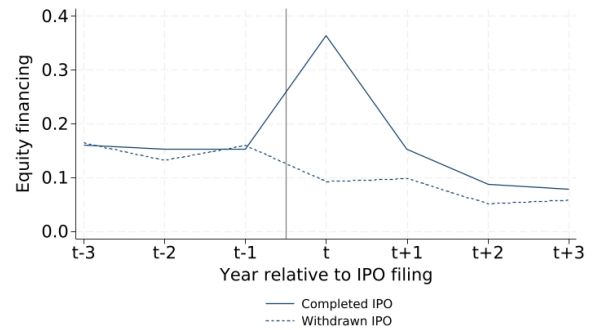
(c) Equity financing

Figure 1a shows counts of firms included in our sample, with IPO completers in dark navy and IPO withdrawers in light orange. To protect taxpayer privacy, we blur the counts of IPO withdrawers for the years marked by asterisks: 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. Figures 1b and 1c present unweighted averages of equity issuance and equity financing, respectively, around the IPO filing event (year t). In these panels, the solid lines indicate IPO completers and the dashed lines indicate IPO withdrawers. All panels were created by the authors using data sourced from SDC Platinum, the SEC's EDGAR platform, and the IRS. Variables are defined in Appendix A; Section IV "Sample Construction: IPO Filers" describes the sample selection steps.

Figure 2: Presence in low-tax jurisdictions around the IPO

(a) Havens (0/1)

(b) # Havens
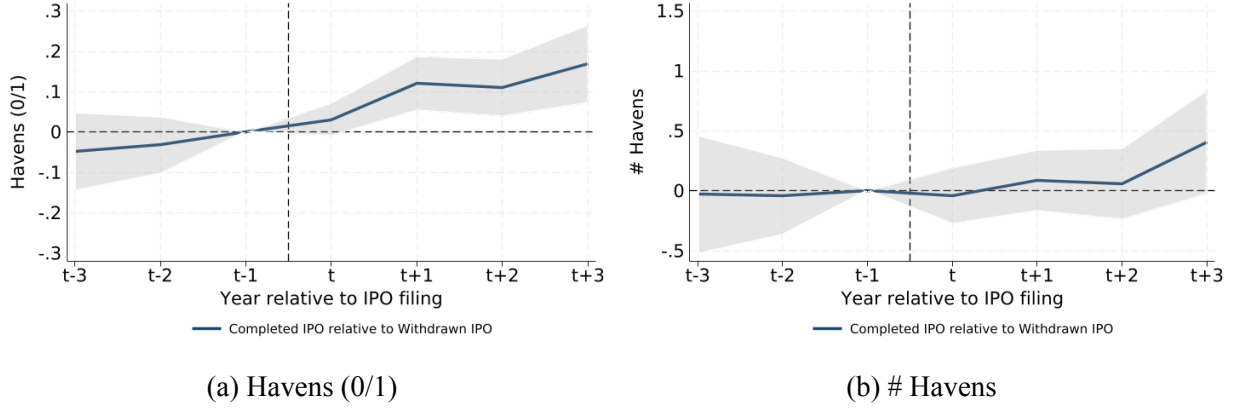
Figures 2a and 2b describe foreign subsidiary activity, comparing IPO-completing and IPO-withdrawing firms. The figures present results from difference-in-differences regressions estimating Equation (2). In Figure 2a the dependent variable is an indicator for having a controlled foreign corporation (CFC) in a low-tax jurisdiction, while in Figure 2b it is the number of such CFCs. Both panels plot year-specific treatment effect coefficients (relative to year $t-1$) and 95 percent confidence intervals, with year t denoting the year of IPO filing. All panels were created by the authors using data sourced from SDC Platinum, the SEC's EDGAR platform, and the IRS. Variables are defined in Appendix A; Section IV "Sample Construction: IPO Filers" describes the sample selection steps.

Figure 3: IPO completers' CFCs in low-tax vs. other jurisdictions

(a) *CFCs (0/1)*, normalized to $t-3$

(b) *# CFCs*, normalized to $t-3$
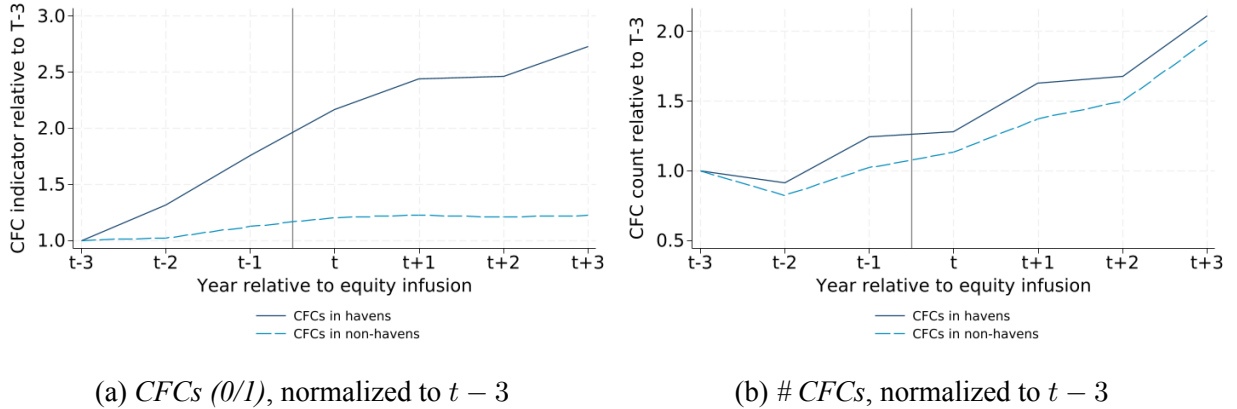
The figure describes IPO-completing firms' use of controlled foreign corporations (CFCs) in low-tax and other jurisdictions. Figure 3a depicts the average likelihood of IPO completers having a CFC in low-tax (solid line) and other (dashed line) jurisdictions, relative to their average likelihood in year $t-3$. Figure 3b depicts the average count of CFCs in low-tax (solid line) and other (dashed line) jurisdictions among IPO completers relative to their average count in year $t-3$. In both panels, year t is the year of equity infusion. Both panels were created by the authors using data sourced from SDC Platinum, the SEC's EDGAR platform, and the IRS. Variables are defined in Appendix A; Section IV "Sample Construction: IPO Filers" describes the sample selection steps.

Table 1: Sample selection criteria

Panel A: IPO selection

	IPO events	
	Events remaining	Fraction completing IPO
U.S. IPOs from SDC Platinum, filing years 1996 to 2016	22,484	0.27
Exclude finance/utilities industries	17,985	0.26
Exclude unit offers, closed-end funds, REITS, ADRs, LPs, blank check companies, and spin offs	5,975	0.60
Exclude issuance of noncommon stock	5,866	0.60
Observe EIN and IPO filing date from SEC Edgar	3,862	0.67
Retain first IPO event per firm	3,651	0.67
Retain IPOs with filing years 2006 to 2015	1,220	0.65
Final sample of IPOs to merge with U.S. tax data	1,220	0.65

Panel B: Sample selection in tax data

	Completed IPO sample		Withdrawn IPO sample	
	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining
Firms in the final IPO sample above	798		422	
Match to IRS 2004-2018 C corporation samples	670	6,096	287	2,055
Require positive gross receipts and total assets	607	5,071	270	1,725
Drop IPO-withdrawer obs. after subsequent completed IPO	607	5,071	[redacted]	1,570
Retain Form 1120 filers	[redacted]	5,017	262	1,520
Require firms present $t-2$ to $t+1$	289	3,125	90	757
Retain observations in years $t-3$ to $t+3$	289	1,876	90	541
Final samples	**289**	**1,876**	**90**	**541**

Note: This table presents selection steps for the treatment sample of completed IPOs and the control sample of withdrawn IPOs. Panel A presents the steps to identify IPOs from SDC Platinum following Lowry, Michaely, and Volkova (2017). Panel B shows the number of completed and withdrawn IPOs that merge with the U.S. corporate income tax data and presents the number of firms and firm-year observations after imposing requisite sample restrictions. See Section IV "Sample Construction: IPO Filers" for details. Two numbers are redacted to protect taxpayer privacy. Data sources are SDC Platinum, the SEC's EDGAR platform, and the Internal Revenue Service.

Table 2: Pre-IPO-filing firm characteristics and first-stage IPW results

Panel A: Pre-IPO-filing firm characteristics, with and without IPW weights

	IPO completing firms (n=289)		IPO withdrawing firms (n=90)		Difference in means, unweighted		Difference in means, IPW weights	
	Mean, unweighted	Mean, IPW weights	Mean, unweighted	Mean, IPW weights	Difference (3)-(1)	P-value	Difference (4)-(2)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcomes								
Havens (0/1)	0.30	0.29	0.23	0.25	-0.06	0.80	-0.04	0.94
Dot havens (0/1)	0.07	0.07	0.09	0.09	0.02	0.31	0.02	0.32
Cost sharing agreement (0/1)	0.08	0.07	0.05	0.04	-0.03	0.47	-0.03	0.33
Total income-shifting receipts from U.S.	5.09	4.96	4.55	4.62	-0.54	0.60	-0.33	0.69
Characteristics								
Assets	380,061	386,875	372,437	355,371	-7,624	1.00	-31,504	0.97
Asset growth	0.50	0.50	0.49	0.51	-0.01	0.81	0.01	0.64
ln(Assets)	11.73	11.73	11.76	11.70	0.03	0.91	-0.03	0.81
Sales	234,454	237,416	277,086	272,659	42,632	0.43	35,244	0.42
Sales growth	1.17	1.16	1.18	1.05	0.02	1.00	-0.11	0.57
ln(Sales)	11.23	11.19	11.11	11.16	-0.11	0.42	-0.04	0.88
Net PPE/Assets	0.14	0.13	0.11	0.13	-0.03	0.01	0.00	0.36
Debt/Assets	0.25	0.26	0.29	0.26	0.04	0.33	0.00	0.89
Pre-NOL txbl income/Lag assets	-0.19	-0.20	-0.19	-0.17	0.00	0.32	0.03	0.68
Pre-NOL txbl income/Sales	-0.93	-1.00	-1.25	-0.98	-0.31	0.05	0.02	0.48
Age	7.75	7.71	7.69	7.92	-0.06	0.97	0.21	0.50
ln(Age)	2.04	2.03	2.00	2.04	-0.03	0.87	0.01	0.43
Foreign income change/Lag assets	-0.03	0.00	0.12	-0.02	0.15	0.65	-0.02	0.64
Foreign presence (0/1)	0.67	0.65	0.58	0.65	-0.10	0.09	0.00	0.49

Panel B: IPW first-stage results (Dependent variable: IPO completed)

	(1)	(2)
ln(Age)	-0.004	[-0.030]
ln(Assets)	-0.053	[-0.552]
ln(Sales)	0.037	[0.387]
Asset growth	0.015	[0.134]
Net PPE/Assets	1.256**	[2.059]
Debt/Assets	-0.273	[-0.959]
Pre-NOL taxable income/Sales	0.007	[0.161]
Foreign income change/Lag assets	-0.044	[-0.935]
Foreign presence (0/1)	0.297*	[1.843]
Constant	0.670	[0.797]
Observations	379	
R-squared	0.0237	

Note: Panel A compares firm characteristics for IPO completers relative to withdrawers in the year prior to IPO filing ($t-1$). Columns (1) and (2) present unweighted and inverse-probability-weighted (IPW) means of characteristics for IPO completers, while Columns (3) and (4) present analogous means for IPO withdrawers. Columns (5) and (6) present the difference in unweighted means between the two samples and the p-value of this difference, while Columns (7) and (8) present analogous statistics after IPW weighting. Panel B presents the first-stage IPW probit regression results, predicting IPO completion using $t-1$ characteristics, as described in Section IV "Pre-IPO Sample Comparisons and Sample Weighting". Regression coefficients are presented in Column (1) and t-statistics are reported in brackets in Column (2). ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Variables are defined in Appendix A. Data sources are SDC Platinum, the Internal Revenue Service, and authors' calculations.

Table 3: Summary Statistics

	Obs.	Mean	Median	St. Dev	p25	p75
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome variables						
Havens (0/1)	2,417	0.29	0.00	0.45	0.00	1.00
# Havens	2,417	0.87	0.00	2.82	0.00	1.00
Dot Havens (0/1)	2,417	0.09	0.00	0.28	0.00	0.00
# Dot havens	2,417	0.25	0.00	1.25	0.00	0.00
Cost-sharing agreement (0/1)	896	0.06	0.00	0.25	0.00	0.00
Amounts received from U.S.	1,236	6.24	0.00	7.67	0.00	15.19
Amounts paid to U.S.	1,236	5.34	0.00	7.43	0.00	14.30
Income-shifting receipts from U.S.	1,236	5.00	0.00	7.17	0.00	14.05
Other firm characteristics						
Equity issuance ($Thousands)	2,273	40,215	8,935	74,656	1,432	40,825
Equity financing	2,273	0.14	0.06	0.18	0.01	0.22
Total assets ($Thousands)	2,417	464,274	127,211	883,414	46,345	381,260
Asset growth	2,417	0.39	0.12	0.80	-0.05	0.52
ln(Total assets)	2,417	11.90	11.75	1.47	10.74	12.85
Sales ($Thousands)	2,417	294,189	87,197	481,006	30,532	268,887
Sales growth	2,273	0.55	0.21	1.29	0.01	0.55
ln(Sales)	2,417	11.32	11.38	1.81	10.33	12.50
Net PPE/Assets	2,417	0.13	0.08	0.13	0.04	0.17
Debt/Assets	2,417	0.26	0.15	0.29	0.00	0.45
Pre-NOL txbl income/Lag assets	2,417	-0.19	-0.05	0.39	-0.34	0.05
Pre-NOL txbl income/Sales	2,417	-1.02	-0.06	2.74	-0.52	0.04
Age	2,417	8.77	8.00	5.09	5.00	11.00
ln(Age)	2,417	2.14	2.20	0.56	1.79	2.48
Foreign income change/Lag assets	2,191	-0.07	0.00	1.79	-0.06	0.11
Foreign presence (0/1)	2,417	0.66	1.00	0.47	0.00	1.00

Note: The table presents summary statistics after inverse probability weighting the sample as described in Section IV "Pre-IPO Sample Comparisons and Sample Weighting". The sample covers 2004 to 2018. Variables are defined in Appendix A. Data sources are SDC Platinum, the Internal Revenue Service, and authors' calculations. All percentile estimates are averages of the 10 observations around the percentile cutoff to preserve tax filing confidentiality.

Table 4: Post-IPO foreign tax planning and activity

Panel A: Presence in low-tax jurisdictions

Dependent var:	Havens (0/1)	#Havens	Dot havens (0/1)	#Dot havens
	(1)	**(2)**	**(3)**	**(4)**
Dep. Var. Mean (Completers, $t-1$)	0.288	0.839	0.068	0.232
IPO Completed*Post	0.125***	0.127	0.037*	0.097**
	[4.348]	[0.953]	[1.839]	[2.311]
YearXCohort FEs	+	+	+	+
Firm FEs	+	+	+	+
Observations	2,417	2,417	2,417	2,417
R-squared	0.754	0.883	0.771	0.917

Panel B: Cross-border activity

Dependent var:	Cost-sharing agreement (0/1)	Amounts received from U.S.	Amounts paid to U.S.	Income-shifting receipts from U.S.
	(1)	**(2)**	**(3)**	**(4)**
Dep. Var. Mean (Completers, t-1)	0.075	6.242	4.980	4.955
IPO Completed*Post	0.075***	1.721*	1.002*	1.744*
	[3.160]	[1.721]	[1.888]	[1.920]
YearXCohort FEs	+	+	+	+
Firm FEs	+	+	+	+
Observations	896	1,236	1,236	1,236
R-squared	0.770	0.820	0.805	0.805

Note: The table presents regression results on firm foreign tax planning activity. We estimate Equation (1) on stacked cohorts, with cohorts defined by IPO filing year. Panel A presents results for presence in low-tax jurisdictions. Panel B presents results for cross-border related-party agreements and payments. Our sample covers 2004 to 2018, and we use inverse probability weights in all regressions. 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, 5, and 10 percent significance, respectively. Data sources are SDC Platinum, the SEC's EDGAR Platform, the IRS, and authors' calculations.

Table 5: Heterogeneity in foreign tax planning

Panel A: Presence in low-tax jurisdictions

Dependent var:					*Havens (0/1)*			
	Had analyst post-IPO	No analyst post-IPO	No large owner post-IPO	Large owner post-IPO	High domestic R&D	Low domestic R&D	Large NOL in IPO filing year	No large NOL in IPO filing year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IPO Completed*Post	0.126***	0.056	0.170***	0.075	0.230***	0.065*	0.187***	0.024
	[4.460]	[0.389]	[4.967]	[1.540]	[3.914]	[1.826]	[5.601]	[0.507]
YearXCohort FEs	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Difference (p-value)			0.115		0.022		0.016	
Observations	2,384	567	1,626	791	1,218	1,199	1,601	816
R-squared	0.755	0.799	0.765	0.765	0.747	0.774	0.719	0.836

Panel B: Cost-sharing agreements

Dependent var:					*Cost-sharing agreement (0/1)*			
	Had analyst post-IPO	No analyst post-IPO	No large owner post-IPO	Large owner post-IPO	High domestic R&D	Low domestic R&D	Large NOL in IPO filing year	No large NOL in IPO filing year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IPO Completed*Post	0.077***	<	0.114**	0.041	0.104**	0.045**	0.078***	0.064*
	[3.127]	<	[2.544]	[1.215]	[2.472]	[2.166]	[3.371]	[1.820]
YearXCohort FEs	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Difference (p-value)			0.208		0.197		0.689	
Observations	887	NA	603	293	476	420	623	273
R-squared	0.770	NA	0.761	0.803	0.773	0.770	0.754	0.805

Note: The table presents regression results on heterogeneity in firm foreign tax planning activity. We estimate Equation (1) on stacked cohorts, with cohorts defined by IPO filing year, on various subsets of our data. In Panels A and B, the dependent variables are indicators for owning a subsidiary in a low-tax jurisdiction and having a cost-sharing agreement in place with a foreign subsidiary, respectively. Our sample covers 2004 to 2018, and we use inverse probability weights in all regressions. In each panel, Column (1) restricts to IPO completers that ever had an analyst in the post-IPO period; Column (2) restricts to IPO completers that did not. In these columns, all IPO withdrawers are included as well. Column (3) restricts to firms (IPO completers and withdrawers) that reported a large owner in any year $t+1$ to $t+3$ on Form 1120, Schedule K, Question 4 or 5; Column (4) restricts to firms that did not. Column (5) restricts to firms with an above-median value of average domestic R&D expenditures in years $t-3$ to $t-1$; Column (6) restricts to firms with a below-median R&D value. Column (7) restricts to firms that had a large NOL carryforward in year t (defined as an NOL carryforward greater than 10 percent of assets); Column (8) restricts to firms that did not. In Panel B Column (2), results are omitted (indicated by NA) because none of the sampled IPO-completing firms without analyst coverage have cost-sharing agreements in place during our sample years. 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, 5, and 10 percent significance, respectively. Data sources are SDC Platinum, the SEC's EDGAR Platform, the IRS, and authors' calculations.

Table 6: Additional validity analysis

Panel A: Post-IPO-filing outcomes for withdrawn IPO firms

	t+1	t+3
Asset growth	0.090	0.050
Sales growth	0.111	0.073
Mean equity financing	0.097	0.053
Cumulative equity financing	0.179	0.312
Equity issuance indicator (0/1)	0.649	0.582
Second IPO (0/1)	0.000	0.076
Bankruptcy (0/1)	0.000	0.071
Merger or Acquisition (0/1)	0.069	0.212
Observations in our IRS data sample	90	54

Panel B: Results using alternative control samples

	Public firm control sample				Private firm control sample			
	Havens (0/1) (1)	Dot havens (0/1) (2)	Cost-Sharing agreement (0/1) (3)	Income-shifting receipts from U.S. (4)	Havens (0/1) (5)	Dot havens (0/1) (6)	Cost-Sharing agreement (0/1) (7)	Income-shifting receipts from U.S. (8)
Dep. Var. Mean (Completers, $t-1$)	0.295	0.116	0.072	3.953	0.210	0.054	0.053	3.327
IPO Completed*Post	0.103***	0.032***	0.049**	0.665**	0.090***	0.042***	0.023**	0.510*
	[5.711]	[3.177]	[2.207]	[2.595]	[6.626]	[3.579]	[2.143]	[1.743]
YearXCohort FEs	+	+	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+	+	+
Observations	9,889	9,889	3,277	5,025	12,454	12,454	5,038	6,323
R-squared	0.776	0.779	0.786	0.783	0.743	0.730	0.730	0.753

Note: The table presents additional validity analyses of the empirical methodology. Panel A describes post-IPO-filing outcomes for the control group of withdrawn IPO firms included in the main sample, after IPW weighting as described in Section IV "Pre-IPO Sample Comparisons and Sample Weighting". Panel B presents regression results on firm foreign tax planning activity using two alternative control samples: public firms and private firms that issue equity of comparable magnitude to IPO firms. Construction of both samples is described in Appendix B. We estimate Equation (1) on stacked cohorts, with cohorts defined by IPO filing year. Columns (1) through (4) present results using the public firm control sample while Columns (5) to (8) use the private firm control sample. Dependent variables reflecting presence in low-tax jurisdictions and cross-border activity are described in Section III "Empirical strategy". Our sample covers 2004 to 2018, and we use inverse probability weights in all regressions. Each firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+3$ where year t is the year of equity issuance (IPO firms and private firms) or is selected randomly (public firms). 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, 5, and 10 percent significance, respectively. Data sources are SDC Platinum, the SEC's EDGAR Platform, the IRS, and authors' calculations.

Table 7: Robustness Analysis

Panel A: Presence in low-tax jurisdictions

Dependent var:	Havens (0/1)					
	Baseline specification	No IPW weights	Clustering by firm	With controls	Don't Require $t-2$ or $t+1$	CS specification
	(1)	(2)	(3)	(4)	(5)	(6)
IPO Completed*Post	0.125***	0.131***	0.125***	0.105***	0.140***	0.084***
	[4.348]	[4.472]	[3.481]	[3.274]	[4.620]	[2.96]
YearXCohort FEs	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+
Observations	2,417	2,417	2,417	2,417	2,590	2,417
R-squared	0.754	0.740	0.754	0.758	0.745	

Panel B: Cost-sharing agreements

Dependent var:	Cost-sharing agreement (0/1)					
	Baseline specification	No IPW weights	Clustering by firm	With controls	Don't Require $t-2$ or $t+1$	CS specification
	(1)	(2)	(3)	(4)	(5)	(6)
IPO Completed*Post	0.075***	0.072***	0.075***	0.054*	0.057***	0.097***
	[3.160]	[2.882]	[3.599]	[1.979]	[2.821]	[4.23]
YearXCohort FEs	+	+	+	+	+	+
Firm FEs	+	+	+	+	+	+
Observations	896	896	896	896	949	808
R-squared	0.770	0.753	0.77	0.775	0.764	

Note: The table presents robustness tests. Panels A and B present results with *Havens (0/1)* and *Cost-sharing agreement (0/1)* as the dependent variables, respectively. Column (1) repeats the baseline results from Table 4. The baseline specification estimates Equation (1) on stacked cohorts, with cohorts defined by IPO filing year. Our sample covers 2004 to 2018, and we use inverse probability weights. 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. The remaining columns report results using unweighted data (Column (2)), clustering by firm rather than industry (Column (3)), including firm-level control variables (Column (4)), using a larger sample that does not require firms to be present in the data in $t-2$ or $t+1$ (Column (5)), and using a staggered difference-in-difference regression methodology (Callaway and Sant'Anna 2021), denoted CS specification (Column (6)). All specifications are explained in detail in Section V "Robustness Tests". T-statistics are reported in brackets. Variables are defined in Appendix A. ***, **, and * indicate levels of 1, 5, and 10 percent significance, respectively. Data sources are SDC Platinum, the SEC's EDGAR Platform, the IRS, and authors' calculations.

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APPENDIX A: VARIABLE DEFINITIONS

The table below provides variable definitions. All data are sourced from the IRS unless otherwise noted, and all dollar-denominated variables are measured in thousands except for those of Form 5471, which are measured in dollars. Line numbers reference 2012 tax forms.

Table A1: Variable definitions

Variable	Definition
Age	Tax year minus year of incorporation (Form 1120: box C)
Amounts paid to U.S.	ln(Form 5471: Schedule M, line 24(b+c) + \$1)
Amounts received from U.S.	ln(Form 5471: Schedule M, line 12(b+c) + \$1)
Assets	Form 1120: Schedule L line 15(d)
Asset growth	$-1 + Assets_t / Assets_{t-1}$
Bankruptcy	Indicator variable equal to 1 if firm files for bankruptcy. Source: Capital IQ
# CFCs	Number of controlled foreign corporations (CFCs) reported using Form 5471
CFCs (0/1)	Indicator variable equal to 1 if *# CFCs* > 0
Cost-sharing agreement (0/1)	Indicator variable equal to 1 if firm answers "Yes" to Form 5471, Schedule G, Question 4 ("During the tax year, was the foreign corporation a participant in any cost-sharing arrangement?") for at least one CFC
Cumulative equity financing	(Sum of *Equity issuance* in years t through $t + k$)/($Debt_{t+k} + Equity_{t+k}$)
Debt	Form 1120: Schedule L line 17(d) + line 20(d)
# 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.
Dot havens (0/1)	Indicator variable equal to 1 if *# Dot havens* > 0
Equity financing	*Equity issuance*/[*Debt + Total paid-in capital*]
Equity issuance	Change in *Total paid-in capital* from the prior year minus the change in *Treasury stock* from the prior year. Coded as zero if this difference is negative.
Equity issuance (0/1)	Indicator variable equal to 1 if *Equity issuance* > 0
Foreign income	Form 1120: Schedule M-3 line 5(a) – line 5(b) + line 7a
Foreign income change/Lag assets	$100 \ast (Foreign\ income_t - Foreign\ income_{t-1})/Assets_{t-1}$
Foreign presence (0/1)	Indicator variable equal to 1 if a CFC is reported on Form 5471
Had analyst (0/1)	Indicator variable equal to 1 if a firm had analyst coverage in years t to $t + 3$ after IPO filing. Source: I/B/E/S
# 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;

Table A1: Variable definitions (continued)

Variable	Definition
	Samoa; Seychelles; Singapore; St. Kitts and Nevis*; St. Lucia Island*; St. Vincent and the Grenadines*; Switzerland; U.S. Virgin Islands; Uruguay; Vanuatu*. Asterisks indicate "dot" havens. This list follows Dyreng and Lindsey 2009 and Hines and Rice 1994, adding Costa Rica as a "dot."
Havens (0/1)	Indicator variable equal to 1 if $\# Havens > 0$
Income-shifting receipts from U.S.	ln(Form 5471: Schedule M, line 3(b+c) + line 4(b+c) + line 5(b+c) + line 6(b+c) + line 8(b+c) + 11(b+c) + 12(b+c) + \$1)
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
Merger or Acquisition (0/1)	Indicator variable equal to 1 if firm is the target of a merger or acqusition. Source: SDC Platinum.
Net PPE	Form 1120: Schedule L line 10b(d)
Pre-NOL taxable income	Form 1120: line 28
Pre-NOL taxable income/Lagged assets	*Pre-NOL taxable income*/$Assets_{t-1}$
Pre-NOL taxable income/Sales	*Pre-NOL taxable income*/*Sales*
R&D expense	Form 6765: maximum of line 9, line 53 and line 28
Sales	Form 1120: line 1c
Sales growth	$-1 + Sales_t/Sales_{t-1}$
Second IPO	Indicator variable equal to 1 if firm files for a second IPO after withdrawing an IPO in the sample. Source: SDC Platinum and SEC Edgar.
Taxable income	Form 1120: line 30
Total paid-in capital	Form 1120: Schedule L lines 22b(d) + 23(d)

APPENDIX B: ALTERNATIVE CONTROL GROUPS

Table B1 Panel A details the steps to construct the alternative public and private firm control samples analyzed in Section V "Alternative Explanation: Firm Growth". We start with all parent-level C corporations from 2004 to 2018 that file Form 1120, using stock ticker symbols reported on Form 1120 Schedule M-3, as well as data from Feldman et al. (2021), to identify public firms. We retain firms with positive gross receipts and assets, and we exclude finance and utilities firms. We require firms to be observed and to maintain a constant public or private status over at least one four-year period. We exclude firms above the 90th percentiles of the IPO-completing firm distributions of gross receipts, assets, or age.

For public firms, we randomly assign each firm to one IPO cohort (year t equity issuance) in which we observe the firm (and they meet our sample criteria) during years $t-2$ through $t+1$. These limitations result in a sample of 1,798 public firms (11,000 firm-years).

We impose an additional restriction on private firms, for better comparability with IPO firms. We require private firms to have equity issuances at or above the 10th percentile of the IPO-completing firm distribution, in terms of both level changes and percentage changes in equity. If a firm has more than one such equity issuance, we use the largest equity issuance. The year of equity issuance defines the cohort (year t). These limitations result in a sample of 19,357 private firms (133,777 firm-years).

Table B1 Panel B reports summary statistics for the alternative analysis samples. Relative to the private-firm control group, the table indicates that public firms are larger, older, and more likely to have CFCs in tax havens.

As with our main analyses, our alternative analyses using the public- and private-firm control groups employ IPW weights to mitigate any underlying differences between these firms and IPO completers. The IPW first-stage probit regression is estimated the same way as in Section IV "Pre-IPO Sample Comparisons and Sample Weighting", with results reported in Table B2 Panel B. Unlike our main analysis, which uses the IPO-withdrawer control group, here many coefficients

are statistically significant at conventional levels when using the public-firm (Column (1)) and private-firm (Column (2)) control groups. This suggests that the treatment and control firms differ meaningfully in unweighted baseline characteristics.

Table B2 Panel A formally compares IPO completers and the two alternative control groups in terms of $t-1$ characteristics. Columns (1) to (5) compare with public firms, while Columns (6) to (10) compare with private firms. After IPW weighting, as indicated in Columns (5) and (10), most observable characteristics—including the foreign tax planning outcome variables reported at the top of this panel—are statistically similar across the treatment and control groups. Only total assets and sales growth remain statistically different (at the 10 percent level) in the public-firm control sample and no characteristics are statistically different in the private-firm control sample. Thus, we view the IPW-weighting procedure as crucial when using the public- and private-firm control groups to study IPO effects.

Table B1: Sample information, public and private alternative control samples

Panel A: Sample selection: Private and public firm samples from IRS data

	Private firm sample		Public firm sample	
	Firms remaining	Firm-years remaining	Firms remaining	Firm-years remaining
	(1)	(2)	(3)	(4)
Form 1120 filers in SOI sample, 2004-2018	156,848	758,562	10,609	67,851
Require positive gross receipts and total assets	126,767	621,861	9,581	61,270
Exclude finance/utilities industries	104,347	497,925	7,363	46,574
Require at least one 4-year observation period	51,050	410,542	5,803	44,096
...with public or private status unchanged	49,620	407,959	4,265	40,890
Exclude firms larger and older than IPO firms	26,840	198,948	1,798	15,059
Require equity issuance meeting our criteria	19,357	138,473	1,798	15,059
Retain observations in years $t-3$ to $t+3$	19,357	133,777	1,798	11,000
Final samples	19,357	133,777	1,798	11,000

Panel B: Summary statistics: Public and private firm samples from IRS data

	IPO completing & public firms			IPO completing & private firms		
	Obs.	Mean	Median	Obs.	Mean	Median
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome variables						
Havens (0/1)	9,889	0.32	0.00	12,454	0.20	0.00
# Havens	9,889	1.00	0.00	12,454	0.47	0.00
Dot havens (0/1)	9,889	0.12	0.00	12,454	0.06	0.00
# Dot havens	9,889	0.31	0.00	12,454	0.12	0.00
Cost sharing agreement (0/1)	3,277	0.10	0.00	5,038	0.04	0.00
Total amounts received from U.S.	5,025	5.49	0.00	6,323	4.10	0.00
Total amounts paid to U.S.	5,025	5.56	0.00	6,323	3.71	0.00
Total income shifting receipts from U.S.	5,025	4.25	0.00	6,323	3.16	0.00
Other firm characteristics						
Equity issuance ($Thousands)	8,847	37,084	10,976	11,227	33,809	10,082
Equity financing	8,845	0.11	0.04	11,215	0.15	0.05
Total assets ($Thousands)	9,889	409,493	215,379	12,454	249,936	128,331
Asset growth	9,889	0.27	0.09	12,454	0.39	0.10
ln(Total assets)	9,889	12.27	12.28	12,454	11.72	11.76
Sales ($Thousands)	9,889	230,360	137,229	12,454	163,995	82,963
Sales growth	8,847	0.30	0.16	11,227	0.62	0.20
ln(Sales)	9,889	11.56	11.83	12,454	11.04	11.33
Net PPE/Assets	9,889	0.15	0.07	12,454	0.14	0.08
Debt/Assets	9,889	0.21	0.09	12,454	0.29	0.17
Pre-NOL txbl income/Lag assets	9,889	-0.08	-0.01	12,454	-0.23	-0.05
Pre-NOL txbl income/Sales	9,889	-0.53	-0.02	12,454	-1.13	-0.06
Age	9,889	9.87	10.00	12,454	7.13	7.00
ln(Age)	9,889	2.29	2.40	12,454	1.95	2.08
Foreign income change/Lag assets	8,777	0.06	0.00	11,157	-0.04	0.00
Foreign presence (0/1)	9,889	0.62	1.00	12,454	0.50	0.09

Note: This table describes two alternative control samples of firms: a sample of public firms and a sample of private firms that issue equity of a comparable value to equity issued by IPO completers. Panel A shows the sample selection steps from the U.S. corporate income tax data. Panel B presents summary statistics. All percentile estimates are averages of the 10 observations around the percentile cutoff to preserve tax filing confidentiality. Variables are defined in Appendix A. Data sources are SDC Platinum, the IRS, and authors' calculations.

Table B2: Pre-IPO firm characteristics and first-stage IPW results

Panel A: Pre-IPO firm characteristics, with and without IPW weights

	Completed IPOs and public firm control sample					Completed IPOs and private firm control sample				
	IPO completing firms (n=316)		Public firm sample (n=1,336)		P-value, difference in means, IPW weights	IPO completing firms (n=316)		Private firm sample (n=1,697)		P-value, difference in means, IPW weights
	Mean, unweighted	Mean, IPW weights	Mean, unweighted	Mean, IPW weights		Mean, unweighted	Mean, IPW weights	Mean, unweighted	Mean, IPW weights	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcomes										
Havens (0/1)	0.31	0.30	0.30	0.29	0.48	0.31	0.21	0.14	0.15	0.10
Dot havens (0/1)	0.09	0.12	0.11	0.11	0.98	0.09	0.05	0.04	0.04	0.39
Cost-sharing agreement (0/1)	0.09	0.07	0.05	0.05	0.58	0.09	0.05	0.01	0.02	0.11
Total income-shifting receipts from U.S.	4.50	3.95	4.03	3.94	0.85	4.41	3.33	2.12	2.36	0.21
Characteristics										
Assets	230,062	323,288	265,703	255,009	0.09	208,109	172,516	161,144	166,326	0.60
Asset growth	0.45	0.23	0.17	0.23	0.47	0.48	0.31	0.23	0.28	0.10
ln(Assets)	11.67	12.06	12.03	11.97	0.71	11.62	11.38	11.32	11.37	0.95
Sales	161,527	187,624	170,061	170,149	0.54	153,748	130,936	116,947	121,781	0.60
Sales growth	0.74	0.55	0.35	0.39	0.09	1.19	1.20	1.22	1.20	1.00
ln(Sales)	11.21	11.44	11.38	11.37	0.74	11.14	10.78	10.74	10.79	0.86
Net PPE/Assets	0.15	0.16	0.14	0.14	0.34	0.15	0.14	0.15	0.15	0.58
Debt/Assets	0.27	0.23	0.19	0.21	0.20	0.29	0.36	0.39	0.37	0.36
Pre-NOL txbl income/Lag assets	-0.14	-0.08	-0.06	-0.07	0.72	-0.21	-0.23	-0.30	-0.30	0.51
Pre-NOL txbl income/Sales	-0.66	-0.55	-0.46	-0.48	0.62	-0.99	-1.37	-1.31	-1.27	0.51
Age	7.40	8.80	9.10	8.77	0.84	7.24	6.05	5.65	5.90	0.46
ln(Age)	2.02	2.19	2.22	2.18	0.85	2.00	1.81	1.74	1.78	0.33
Foreign income change/Lag assets	0.11	0.11	0.12	0.11	0.61	0.03	-0.02	-0.05	-0.03	0.99
Foreign presence (0/1)	0.67	0.58	0.62	0.63	0.26	0.67	0.47	0.43	0.47	0.85

Panel B: IPW first-stage results (Dependent variable: IPO completed)

| | Public firm sample | | Private firm sample | |
	(1)	(2)	(3)	(4)
ln(Age)	-0.555***	[-6.923]	0.492***	[7.193]
ln(Assets)	-0.505***	[-8.486]	0.041	[0.831]
ln(Sales)	0.281***	[4.743]	0.121**	[2.439]
Asset growth	0.823***	[10.337]	0.425***	[7.022]
Net PPE/Assets	0.343	[1.301]	0.377	[1.564]
Debt/Assets	0.907***	[6.211]	-0.248**	[-2.384]
Pre-NOL txbl income/Sales	-0.166***	[-3.996]	-0.042**	[-2.131]
Foreign inc change/Lag assets	-0.005	[0.020]	0.049	[1.356]
Foreign presence (0/1)	0.442***	[5.042]	0.545***	[7.127]
Constant	2.252***	[4.143]	-4.175***	[-9.200]
Observations	1,652		2,013	
R-squared	0.156		0.115	

Note: Panel A compares firm characteristics for IPO completers (treated) relative to public and private firms (control) in year $t-1$. Year t is the year of equity issuance for IPO completers and private firms and is randomly assigned for public firms. Column (1) presents IPO completers' unweighted means of characteristics, while Column (2) presents means after inverse probability weighting (IPW). Columns (3) and (4) present unweighted and IPW-weighted means for public firms, and Column (5) reports the p-value of the difference between Columns (2) and (4). Columns (6) and (7) repeat Columns (1) and (2). Winsorizing and weighting relative to private firms instead of public firms. Columns (8) and (9) present unweighted and IPW-weighted means for private firms, and Column (10) reports the p-value of the difference between Columns (7) and (9). Panel B presents the first-stage IPW regression results for completing an IPO, using firm characteristics measured in $t-1$, and described in Section IV "Pre-IPO Sample Comparisons and Sample Weighting". Regression coefficients are presented in Column (1) and t-statistics are reported in brackets in Column (2). ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Variables are defined in Appendix A. Data sources are SDC Platinum, the IRS, and authors' calculations.

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