The Value of International Income Shifting to U.S. Multinational Firms

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ABSTRACT

We examine the value to U.S. multinational firms of shifting income from high- to low-tax jurisdictions. Such shifting is common and sizable, yet the consequences to shareholders are heretofore unknown. We use an identification strategy that relies on changes in country-level tax rates to identify exogenous changes in income-shifting incentives. We find that, on average, a one-percentage point increase in tax rate incentives to shift income is associated with a 0.5 percent discount on firm value. We also document that both income shifting between the U.S. and foreign jurisdictions and income shifting between different foreign jurisdictions result in firm value discounts. U.S.–foreign income shifting impairs firm value through myopic uses of income shifting, while foreign–foreign income shifting impairs firm value through managerial rent extraction, suggesting a fundamental difference in the nature and motivations of different types of income shifting.

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1. Introduction

Clausing (2016) estimates that the U.S. Treasury lost between $77 and $111 billion to corporate income shifting during 2012, consistent with a wealth transfer from the government to corporate shareholders. While conventional wisdom suggests that tax savings from income shifting benefit shareholders (e.g., Collins et al. 1998), the tactics used to shift taxable income are not costless. For example, income shifting can lead to a weak financial information environment (Chen et al. 2018) and ineffective management reward systems (Baldenius et al. 2004). Firms also incur significant costs when tax-motivated income shifting drives firms to relocate headquarters, move research and development (R&D) activities, change financing modes, or adjust intra-firm cash transfers. Later, when tax regimes or enforcement levels change, firms incur adjustment costs, unwinding costs, or tax penalties (Wilson 2009). Finally, Desai and Dharmapala (2006) and Desai et al. (2007) argue that agency costs reduce shareholder-level tax benefits obtained from income shifting. Overall, despite clear impairments to government coffers, we do not yet understand the net effects on shareholders of multinational income shifting.

We evaluate the net benefits or costs to shareholders of tax-motivated cross-border income shifting. To do this, we develop and validate a measure of income-shifting incentives, *ISI*, to proxy for tax-motivated cross-jurisdictional income shifting. *ISI* is conceptually similar to the *C* measure of Huizinga and Laeven (2008) but is adapted for use with publicly available U.S. data. This adaptation enables us to examine the firm value consequences of income shifting overall, and separately for income shifting between the U.S. company and foreign subsidiaries (U.S–foreign income shifting) and between different foreign subsidiaries (foreign–foreign income shifting). We also parse between exogenous and endogenous changes in income shifting using a decomposition technique from Lamont and Polk (2002). Because income shifting is correlated with numerous other variables, the ability to identify exogenous changes in income shifting helps ensure that income shifting drives our results, not correlated omitted variables. These aspects of our empirical methodology allow us to provide more varied and robust insights than previous empirical designs that evaluate income shifting.

We consider the financial effects of income shifting on shareholders by regressing firm value (i.e., Tobin's Q) on *ISI,* while extensively controlling for other factors that could affect Tobin's Q.[1] Across multiple specifications, we find consistent evidence that firm value is negatively associated with tax-motivated income shifting. On average, firm value decreases about 0.5 percent with a percentage point increase in *ISI*.

Given these results, we next examine why income shifting is negatively associated with firm value. We theorize that managers could cause this value impairment by focusing myopically on the short-term benefits of income-shifting at the expense of long-term firm value, or who use the opaque nature of the income-shifting setting to extract rents. Pressures from institutional and analyst oversight trigger managerial myopia (Bushee 1998; He and Tian 2013), but this oversight can also reduce managers' ability to extract rents (Desai and Dharmapala 2009b). We therefore examine how the association between *ISI* and firm value changes as corporate oversight increases. Overall, we find that income shifting is more negatively associated with firm value in the presence of greater corporate oversight/pressure, consistent with managers destroying long-term firm value through a myopic focus.

However, we find that corporate oversight influences the negative value effects of income shifting differently depending on whether income shifting is U.S.–foreign or foreign–foreign. Contextually, U.S.–foreign income shifting possesses two unique characteristics. First, outbound (inbound) U.S.–foreign income shifting, used in conjunction with increases (decreases) in income declared to be indefinitely reinvested (ASC 740-30-25-17; Krull 2004), enables income-increasing (income-decreasing) earnings management. Second, U.S. capital market participants are more likely to observe U.S.–foreign (vis-à-vis foreign–foreign) income shifting and understand the tax effects of income shifting between the U.S. and foreign jurisdictions due to their geographic proximity and familiarity with the U.S. tax and regulatory environment (Coval and Moskowitz 1999; Dvořák 2005). Combined, these capital market-related

[1] We use Tobin's Q to measure the value premium placed on firm assets by investors, consistent with numerous prior and concurrent studies (e.g., Desai and Dharmapala 2009b; Bryant-Kutcher et al. 2012; Iliev et al. 2014; Fauver et al. 2017; Fabisik et al. 2018). Our identification strategy and numerous controls address potential correlated omitted variables. We also examine alternative value measures in supplemental analyses.

incentives could motivate managers to myopically over-invest in U.S.–foreign income-shifting activities but limit rent extraction opportunities. Indeed, we find greater negative associations between U.S.–foreign income shifting and firm value when corporate oversight (and thus pressure to act myopically) is stronger, consistent with this theory. Conversely, since foreign–foreign income shifting is more difficult to monitor (Desai et al. 2007), managers may use this setting to extract rents related to the tax benefits from foreign–foreign income shifting. Evidence supports this view: Firm value is more negatively associated with foreign–foreign income shifting when corporate oversight is lower, suggesting that managers may use foreign–foreign income-shifting strategies to hide or facilitate rent extraction. The different sources of value impairment highlight the need to separately examine U.S.–foreign and foreign–foreign income shifting.

In supplemental analyses, we find that both U.S.–foreign and foreign–foreign income shifting are associated with overinvestment, consistent with the theory in De Simone et al. (2018), but only foreign–foreign income shifting is associated with higher executive compensation, further supporting that foreign–foreign income shifting can facilitate rent extraction (Blaylock 2016). We also find some evidence that income shifting is negatively associated with concurrent pre-tax returns and future post-tax returns on assets (ROAs), suggesting that the upfront costs to implementing income-shifting strategies can be economically significant. We further examine whether intangible assets affect the association between income shifting and firm value, because tax-motivated income-shifting strategies widely employ intangible assets due to their ease of relocation and because of the nebulous values assigned to them (Grubert 2003; Dischinger and Riedel 2011). We learn that internally created intangibles (e.g., R&D spending) increase the firm value discount associated with income shifting. Finally, robustness tests reveal that income shifting is also negatively associated with stock returns, and the negative association between *ISI* and firm value persists for up to three years into the future.

Our paper contributes to the income-shifting literature (e.g., Klassen et al. 1993; Collins et al. 1998) by showing that income shifting results in negative value consequences for shareholders due to agency costs that offset the tax benefits from income shifting. These results suggest that additional income-shifting restrictions, whether at the national or firm level, can protect shareholder wealth. Evidence that U.S.–

foreign and foreign–foreign income shifting each affect firm value in unique ways adds to this literature, showing that we must consider type and location of income shifting to understand its consequences. This new evidence could also alter the focus of the literature from documenting that income shifting exists and varies with cross-sectional traits to considering the consequences of income shifting for the firm and its stakeholder groups. In addition, we illustrate that income shifting offers another avenue for myopic behavior and managerial rent extraction to influence firm value, contributing to the literature on myopic managerial behavior (e.g., Bushee 1998) and managerial rent extraction (e.g., Desai and Dharmapala 2006, 2009a, 2009b). Finally, we develop a new empirical methodology that employs a firm-level measure of income-shifting with unique measurement advantages and an identification technique based on Lamont and Polk (2002). This approach should be useful to future researchers who seek a firm-level measure of income shifting that addresses endogeneity concerns.

Our study continues in Section 2 with a review of the literature on income shifting and the development of our hypotheses. Section 3 summarizes our data and empirical methodology, and Section 4 describes our empirical results. Section 5 concludes.

2. Background and hypothesis development

Income shifting

Tax authorities and academics have long been concerned with corporate cross-jurisdictional income shifting (OECD 2015a). Early income-shifting studies focused on measuring income shifting and the sensitivity of income shifting to corporate tax rate changes (Grubert and Mutti 1991; Harris 1993; Klassen et al. 1993; Grubert and Slemrod 1998; Collins et al.1998; Clausing 2009; Grubert 2012; Klassen and Laplante 2012b). These studies developed several techniques which have since been enhanced to identify income shifting and its determinants (Dyreng and Markle 2016).[2] These examinations note several determinants of the amount of income shifting, such as regulatory costs (Klassen and Laplante 2012b), reinvestment aims and earnings management objectives (Klassen and Laplante 2012a), local law

[2] A recent study (Markle et al. 2016) suggests that these income-shifting measures may be confounded with implicit taxes.

enforcement strength (Beuselinck et al. 2015), financial constraints (Dyreng and Markle 2016), internal information quality (McGuire et al. 2017), home country tax regime (Markle 2016), and the existence of loss subsidiaries (Hopland et al. 2018).

Little evidence documents the consequences of income shifting. This is likely due to inherent difficulties in measuring the test variable, income shifting, and the response variable, a consequence of income shifting. However, closely related studies examine how firms respond to differences in tax rates across jurisdictions. This literature finds evidence that tax rate differences drive certain corporate activities. Multinational firms are more likely to 1) locate their headquarters in low-tax jurisdictions (Clausing 2006; Barrios et al. 2012), 2) move R&D activities and intangible assets to jurisdictions with low tax rates or favorable treatment for intangible asset income (Grubert 2003; Dischinger and Riedel 2011; Griffith et al. 2014), 3) change the type and location of financing (Newberry 1998; Newberry and Dhaliwal 2001; Mills and Newberry 2004), and 4) alter the magnitude and characterization of intra-firm cash transfers in response to cross-jurisdictional tax rate differentials (Collins and Shackelford 1997; Grubert 1998). Studies report that, to shift income, firms adjust the prices at which they transfer goods and services within their affiliated groups (Grubert et al. 1993; Gramlich et al. 2004; Collins et al. 1997).

Concurrent papers examine the consequences of income shifting by developing new measures of income shifting. Chen et al. (2018) measure income shifting using regressions from prior literature (e.g., Collins et al. 1998; Klassen and Laplante 2012b) and find that income shifting is associated with a weak information environment, as proxied for by bid-ask spreads, insider trading profits, analyst forecast dispersion, and stock return volatility. De Simone et al. (2018) use regression to reflect an affiliate-specific association between tax rate differentials and profits, finding evidence that this measure is associated with greater affiliate-level investment. Saavedra and Williams (2017) measure the association between foreign sales and uncertain tax benefits (UTBs), and document that the extent of this association positively relates to higher borrowing costs and more restrictive debt covenants. Finally, De Simone et al. (2017) employ confidential IRS data to examine the determinants of income shifting. In the process, they develop an income-shifting likelihood score to estimate income shifting. Each of these papers attempts to measure

income shifting differently, but each measure has significant issues with estimation, data inputs, limited variation, and/or correlated omitted variables. We develop a new measure, *ISI*, which examines income-shifting consequences and avoids many of these issues, while adopting an identification strategy to ensure that exogenous variation in income shifting drives our results. See Appendix A and Figure A.1 for further detail about our measure, these alternate measures, and the relative strengths and weaknesses of each measure.

Tax avoidance valuation

Income shifting, while significant in its own right (Clausing 2016), is only one important component of a multinational firm's total tax-planning strategy. In general, tax avoidance appears to be positively associated with firm value (Desai and Dharmapala 2009b; Inger 2014) and may be negatively associated with the cost of equity capital (Goh et al. 2016). However, the positive effect of tax avoidance on the cost of equity likely depends on the risk involved, such as when tax policy risk is higher (Heitzman and Ogneva 2018). Unlike equity investors, lenders tend to view general tax avoidance negatively (Hasan et al. 2014).

However, the generally positive association between tax avoidance and firm value becomes insignificant or negative when corporate governance is poor (Desai and Dharmapala 2009b; Li et al. 2016), supporting the hypothesis that tax avoidance savings are subject to managerial rent extraction (Desai and Dharmapala 2006; Desai et al. 2007). To illustrate, Desai et al. (2007) provide a case study of how managers use income shifting to extract wealth from shareholders. Several studies have attempted to further explore the association between tax avoidance and rent extraction. Blaylock (2016) finds no evidence connecting tax avoidance and broad measures of rent extraction, and Seidman and Stomberg (2017) offer alternative explanations for the results found by Desai and Dharmapala (2006).[3] More recent papers identify links

[3] Specifically, Seidman and Stomberg (2017) suggest that the association between equity compensation and tax avoidance documented by Desai and Dharmapala (2006) does not reveal rent extraction. Instead, it shows that tax deductions associated with equity compensation reduce firms' demand for incremental tax avoidance. They further suggest that the governance cross-sectional tests in Desai and Dharmapala (2006) are highly sensitive to the governance measure used, with cross-sectional results not occurring when using more recently developed board-

between tax avoidance and direct measures of rent extraction, such as insider trading profitability and tunneling profits to related parties (Chung et al. 2018; Chan et al. 2016), or show that tax avoidance and managerial rent extraction are complementary when jurisdictions with weak investor protections generate tax avoidance (Atwood and Lewellen 2018).

Another line of research examines the valuation of specific types or contexts of tax avoidance, resulting in mixed evidence. For example, results are unclear as to whether a valuation premium exists for tax avoidance through 1) corporate inversions (Desai and Hines 2002; Cloyd et al. 2003), 2) tax shelters (Hanlon and Slemrod 2009; Wilson 2009; Gallemore et al. 2014), and/or 3) broad foreign tax avoidance (Bryant-Kutcher et al. 2012; Inger 2014). Prior literature also reports mixed evidence on whether investors positively or negatively value aggressive tax avoidance (Koester 2011; Heitzman and Ogneva 2018).

Recent research also examines the valuation of firms' multinationalism, foreign structures, and transfer pricing risk. Without focusing on or inferring the effects of taxes or tax rate differentials across countries, Creal et al. (2014) demonstrate that an equity value premium accompanies multinational firms. O'Donovan et al. (2018) present evidence that investors react negatively to the disclosure of entity structures that can facilitate income shifting (among other purposes). However, their study focuses on 488 firms implicated in the Panama Papers leak, and the negative reaction could be due to concerns about temporary reputational or regulatory penalties immediately related to the leak, rather than long-term value effects of income-shifting strategies. Finally, Mescall and Klassen (2018) report that takeover premia are smaller in industries with higher levels of intellectual property because these industries' arm's length transfer prices are more difficult to ascertain, and acquiring these firms imposes greater tax risks. Their evidence suggests that the risk associated with income shifting may result in smaller takeover premia, but they do not consider the general relation between income shifting in total (i.e., both the benefits and risks) and firm value.

Hypotheses development

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governance measures. We use well-established measures of external governance in our analyses to limit the relevance of these criticisms.

It is unclear ex ante whether tax-motivated income shifting will be positively or negatively associated with firm value. On one hand, conventional wisdom suggests that tax-motivated income shifting generates significant tax savings that can benefit firms and shareholders (e.g., Collins et al.1998), which is consistent with the positive association between general tax avoidance and firm value (Desai and Dharmapala 2009b; Inger 2014). Additionally, companies significantly change their operational, investing, and financing activities to achieve tax-motivated income shifting (e.g., Collins and Shackelford 1997; Mills and Newberry 2004; Clausing 2006). These responses would suggest that tax-motivated income shifting generates significant value for companies, as manager compensation is based on corporate performance and, thus, managers would not appear to have incentives to make such changes unless they increase firm value (Lambert and Larcker 1987; Bushman et al. 1996).

On the other hand, income-shifting strategies can be costly to implement and maintain, while the long-run benefits can be highly uncertain and subject to considerable risk (Towery 2017).[4] The inherent complexity in multinational income shifting could cause managers to overestimate the benefits and underestimate the costs and risks, leading them to believe that they would maximize firm value by shifting income. Even when income shifting results in short-term tax savings, managers may easily underestimate the long-term costs associated with income shifting.[5] For example, extra funds from taxes saved through income shifting can result in increased implicit taxes, a cost that managers would likely struggle to understand and plan for (Berger 1993; Jennings et al. 2012; Markle et al. 2016). Income shifting through tax-advantaged inter-company transfer prices can also create a setting in which tax planning and effective manager evaluation are at odds, with firm-wide managers forced to choose between saving taxes for

[4] For a few examples of the potential costs, Enron paid external advisors $87.6 million to aid in structuring abusive tax transactions (U.S. Congress Joint Committee on Taxation 2003), Wilson (2009) reports that interest and penalties on tax shelter transactions average 49 percent of abusive tax savings, and Dyreng et al. (2016) document that the release of a list of UK firms that had unreported tax haven subsidiaries led to negative three-day abnormal stock returns (i.e., a reputational penalty) of between 0.6 and 0.8 percent.

[5] These costs could include 1) non-tax costs of reorganizing and conducting suboptimal activities to receive beneficial tax treatment, 2) deferred taxes that may ultimately be paid, 3) planning and compliance costs, 4) the cost of paying penalties and/or interest to tax authorities if an income-shifting practice is disallowed, 5) the non-tax costs of unraveling a strategy if the practice is disallowed or if rules change so that the strategy no longer generates tax benefits, and 6) the potential impairment cost to the firm's reputation if negative publicity arises as a result of the income-shifting strategy.

shareholders or effectively evaluating division managers (Baldenius et al. 2004). Incentives encourage division managers to manipulate transfer prices to increase their compensation, perhaps in ways inconsistent with the company's tax-motivated income-shifting strategies. As Rego and Wilson (2012) report, equity risk incentives motivate managers to adopt riskier tax positions, and multinational income shifting may be one such result. Risks related to income shifting include potential costs of unwinding and direct penalties from policy changes or unexpected enforcement outcomes.

In addition, firms may not sufficiently compensate managers for long-term shareholder wealth creation. Short-term reductions in effective tax rates (ETRs) often produce rewards for managers (Robinson et al. 2010; Armstrong et al. 2012; Brown et al. 2016), but a myopic focus on short-term performance could motivate managers to underinvest in planning for the long-term profitability of income-shifting strategies. Similarly, they may overinvest in risky or unprofitable income-shifting strategies that produce short-term benefits but net long-term costs (Bushee 1998, 2001; He and Tian 2013). A myopic focus could also induce manager bias in estimates of long-term benefits and costs. Alternatively, managers may recognize the harm of income shifting to shareholders but opt to engage anyway. In such cases, managers may receive a net benefit from a period of low ETRs and not occupy the responsible position when long-term costs are ultimately incurred.[6] Prior research provides copious evidence that agency problems can lead managers to destroy firm value.[7]

Finally, managerial rent extraction may explain why managers shift income to save taxes. Tax avoidance through income shifting is particularly complex and opaque, and therefore particularly

[6] Although income shifting likely impairs executives' wealth if it negatively affects firm value, it is also possible that executives gain extra compensation and longer, more secure tenures (or better alternative employment opportunities) by engaging in income shifting. Firms frequently evaluate tax departments as profit centers focused on reducing GAAP ETRs, particularly when they have greater tax-planning opportunities (Robinson et al. 2010). Further, top managers may suffer impairments to their compensation and career outcomes when they do not engage in enough tax avoidance (Brown et al. 2016; Chyz and Gaertner 2018). Thus, the value of income shifting may be positive to a manager, even after considering possible negative effects on firm value.

[7] Agency theory (Jensen and Meckling 1976) expects that, in certain circumstances, managers will extract perquisites or shirk tasks at the expense of shareholder wealth. Empirical evidence supporting this theory includes: earnings management to increase earnings-based bonuses (Healy 1985) and avoid losses (Roychowdhury 2006), the backdating of compensatory stock options (Lie 2005), and insider trading on tax information (Chi et al. 2014; Chung et al. 2018). Evidence also exists that executive compensation contracts can themselves be the result of rent extraction, rather than optimal contracting devices (Abudy et al. 2017).

susceptible to managerial rent extraction (Desai et al. 2007; Armstrong et al. 2015).[8] In other words, managers could engage in income shifting that benefits the company but allows them to extract sufficient rents from the tax savings to ultimately decrease shareholder value. This finding is consistent with evidence from the broader tax-avoidance literature (Desai and Dharmapala 2006; Desai et al. 2007). Rent extraction can take multiple forms, including empire building (Hope and Thomas 2008) and tunneling profits through transactions with related parties (Chan et al. 2016).

Given these competing predictions as to whether income shifting will be positively or negatively associated with firm value, we offer the following null hypothesis:

HYPOTHESIS 1. *Tax-motivated income shifting is not associated with firm value.*

We do not simply examine the overall relation between tax-motivated income shifting and firm value but also why this relationship would exist. A positive association would imply that tax-motivated income shifting produces net tax savings in excess of the costs to implement and maintain income-shifting strategies. However, a negative association would be more difficult to interpret because it may be driven by a myopic focus on short-term benefits or managerial rent extraction. Prior research shows that external oversight in the form of greater institutional ownership (Bushee 1998; 2001) and analyst following (He and Tian 2013) tends to increase myopic investing behavior but reduce the extent to which managers can extract rents (Desai and Dharmapala 2009b; Li et al. 2017). Given these juxtaposing effects, we use measures of external oversight to differentiate whether any negative association between income shifting and firm value is attributable to myopic investment activity or managerial rent extraction. Given that we have no ex-ante

[8] Another way of thinking about this is that managers, in choosing an income-shifting strategy absent of myopic pressures or rent extraction, will attempt to maximize $p\times(E(b) - E(c))$, where p is the manager's stake in firm profits, b is the after-tax benefit of income shifting, c is the after-tax cost of income shifting, and E is the expectation operator. If managers maximize this, then the association between firm value and income shifting will be non-negative. However, if managers can extract rents from the company, then they instead attempt to maximize $p\times(E(b) - E(c)-r) + r$, where r is the amount managers extract. (We omit discovery costs and probabilities for simplicity but do not change inferences.) Managers may then choose income-shifting strategies where the value to shareholders $[(1-p)\times(E(b) - E(c)-r)]$ is negative if $(1-p)\times r \geq E(c) - E(b)$. Similarly, if a firm has long- and short-term benefits and costs, then the manager attempts to maximize $p\times(E(lb)+ E(sb) - E(lc) - E(sc))$, where l and s indicate long and short term, respectively. Myopia can have several effects on this function which would lead managers to choose income-shifting strategies with negative value to shareholders. For example, myopia may cause bias in managers' benefit and cost estimates, or may cause managers to have a payoff for short- and long-term net benefits that differs from the payoffs to shareholders for the same benefits.

expectation about which effect might dominate, we offer the following null hypothesis:

HYPOTHESIS 2. *Any association between tax-motivated income shifting and firm value does not vary with institutional investor and analyst oversight.*

We have no prior expectation about whether managerial myopia or rent extraction better explains an association between income shifting and firm value. However, we conjecture that the nature of income shifting, either U.S.–foreign or foreign–foreign, would likely change which effect dominates. When a potential tax-saving income-shifting strategy involves the United States, outside monitors such as analysts, institutional investors, the popular press, and the IRS are likely to be more aware of the nature of these strategies (Coval and Moskowitz 1999). Because of this attention, managers may be less inclined to extract rents from tax savings related to U.S.–foreign income shifting. However, U.S.–foreign (vis-à-vis foreign–foreign) income shifting may be more susceptible to myopic pressures. Further, shifting income from/to the United States will affect several tax accruals directly linked to benchmark-beating earnings management, such as foreign permanently reinvested earnings (PREs) and valuation allowances on U.S. net operating losses (NOLs) (Krull 2004; Frank and Rego 2006), with benchmark-beating benefitting managers in the short-term. Thus, myopic pressure to achieve certain earnings results, perhaps from diverse and active shareholder groups and high analyst following, may lead managers to engage in U.S.–foreign income shifting to help achieve short-term earnings targets at the expense of total firm value.

Earnings management can ultimately involve wealth transfers from shareholders (e.g., increasing income-based management bonuses) or creditors (i.e., avoiding debt covenant default by increasing income). However, when management avoids cash taxes, greater degrees of rent extraction may be possible in conjunction with foreign–foreign income shifting (Desai et al. 2007). Corporate stakeholders are likely to have more difficulty monitoring income shifting without a U.S. connection due to geographic distance, language barriers, variation in corporate laws and institutions across countries, and less applicability of the *Foreign Corrupt Practices Act,* along with less oversight by the U.S. Justice Department (U.S. Department of Justice and SEC 2012). Reduced oversight by market monitors and U.S. enforcement authorities leads us to expect that foreign–foreign income shifting may be more susceptible to managerial rent extraction

than U.S.–foreign income shifting, and that the negative effects of such rent extraction are likely to be lowest when managers have more powerful monitors. As such, we offer the following two hypotheses in alternative form:

HYPOTHESIS 3. *The association between U.S.–foreign income shifting and firm value is more negative (or less positive) with greater institutional investor and analyst oversight.*

HYPOTHESIS 4. *The association between foreign–foreign income shifting and firm value is less negative (or more positive) with greater institutional investor and analyst oversight.*

3. Empirical measures, data, and methodology

Measure of income shifting

We adapt the *C* measure of Huizinga and Laeven (2008) to the U.S. setting to measure income shifting. Appendix A describes the steps we take to adapt *C* and provides numerical examples. Given the different setting and that we measure income-shifting opportunities at the consolidated rather than affiliate level, we label our measure *ISI* and compute it as:

$$ISI = \frac{FS}{TS}\sum_{j=1}^{J}\sum_{i=1}^{I}\frac{x_j x_i}{\sum_{j=1}^{J}\sum_{i=1}^{I} x_j x_i} abs(t_j - t_i) + \frac{DS}{TS}\sum_{j=1}^{J}\frac{x_j}{\sum_{j=1}^{J} x_j} abs(t_j - t_{U.S.}) \quad (1)$$

where *FS* is sales attributable to foreign segments, *DS* is sales attributable to the United States, *TS* is consolidated sales, *t* denotes a jurisdiction's tax rate, *x* denotes the number of legal entities located in a jurisdiction, and both *i* and *j* denote jurisdictions. We design this firm-level weighted-average tax rate differential, *ISI*, to capture tax incentives to shift income across jurisdictions. The portion of the measure before the "+" in equation (1) captures all incentives to shift income between different foreign jurisdictions, while the part of the measure after the "+" in equation (1) captures incentives to shift income between the U.S. company and its foreign subsidiaries. Economically, the marginal dollar shifted guides the interpretation of *ISI*. That is, on average, if the firm were to shift one additional dollar of income across jurisdictions, the firm would receive *ISI* as the ETR reduction on that dollar of income. This single-period measure allows us to 1) enhance identification by controlling for firm fixed effects and other characteristics and 2) capture changes in income-shifting behavior across time in response to tax rate changes, which is

important given recent evidence that firms can adjust their tax positions relatively quickly to changes in the environment (Hoopes et al. 2012; Kim et al. 2017).

Two strengths of *ISI* become evident in the calculations above. First, by partitioning *ISI* at the "+" in equation (1), we can examine income shifting separately between U.S. and foreign jurisdictions, and between different foreign jurisdictions. Second, variation in *ISI* derives from three sources: 1) endogenous changes in firms' geographic footprints, 2) endogenous changes in the extent of firms' activities within each jurisdiction, and 3) exogenous changes in country-level corporate tax rates. While prior research finds that firms can endogenously affect the policies of U.S. states (Karpoff and Wittry 2018), it is considerably more difficult to affect national-level policies (Baumgertner et al. 2009; Godsell 2018). Additionally, the U.S. multinational firms in our sample are less likely to advocate for tax law changes in foreign countries because they can reduce their taxes by shifting income across jurisdictions, seeking state aid (Nesbitt et al. 2018), and taking advantage of tax holidays (Chow et al. 2018). This leads us to conclude that country-level tax rates are largely exogenous for the U.S.-based firms in our sample.

With these multiple sources of variation, we employ a decomposition technique developed by Lamont and Polk (2002) and used by Hann et al. (2013). We then test whether potentially endogenous sources of income-shifting variation, caused by firm actions, or exogenous variation in income-shifting incentives, caused by countries' tax rate changes, drive inferences based on *ISI*. Specifically, Lamont and Polk (2002) show that the change in a measure can be decomposed into endogenous (e.g., geographic structure) and exogenous (e.g., national tax rate differentials) sources as follows:

$$\begin{aligned} \Delta ISI_t = {} & f(structure_t, rates_t) - f(structure_{t-1}, rates_{t-1}) \\ & = [f(structure_t, rates_t) - f(structure_t, rates_{t-1})] \\ & + [f(structure_t, rates_{t-1}) - f(structure_{t-1}, rates_{t-1})] \\ & = \Delta ISI_{Exogenous} + \Delta ISI_{Endogenous} = \Delta ISIEX_t + \Delta ISIEN_t \end{aligned} \tag{2}$$

As a result, we can create a variable to capture the possibly endogenous sources (i.e., changes in firms' geographic footprints and the extent of activities within each jurisdiction) of variation in *ISI* (*ΔISIEN*) to

reflect variation in income-shifting incentives that may be correlated with other variables. We can also create a measure to identify exogenous variation that comes from country-level tax rate changes (*ΔISIEX*). The latter is unlikely to be correlated with omitted variables or simultaneously determined with firm value. Our design is unique in providing a method for identifying exogenous variation, which helps examine causal effects of income shifting in archival studies.[9]

While these two benefits (i.e., separate measures of U.S.–foreign and foreign–foreign income shifting and the ability to identify exogenous variation in income-shifting incentives) motivate our development and use of *ISI*, it is inherently a measure of income-shifting incentives rather than income-shifting activity. However, we use *ISI* to directly proxy for income-shifting activity, as incentives are a key determinant of tax-avoidance activity (Allingham and Sandmo 1972; Mills et al. 1998; Beardsley et al. 2017) and the assumption that incentives drive income-shifting activity underlie many alternative approaches to measuring income shifting (e.g., Collins et al. 1998; De Simone et al. 2017; Chen et al. 2018; De Simone et al. 2018). Nevertheless, to ensure that *ISI* acts as expected as a measure of income-shifting activity, we report detailed descriptive statistics of *ISI* in Appendix A and numerous validation tests of *ISI* in Appendix B. These analyses support that *ISI* can act as a proxy for income-shifting activity.

To obtain the data on entity locations used in *ISI*, we hand-collect subsidiary location data from Exhibit 21 subsidiary disclosures included in Form 10-K filings. The SEC mandates Exhibit 21 disclosures for all SEC filers, and these disclosures include both consolidated and non-consolidated subsidiaries under SEC Regulation S-K (Feng et al. 2009).[10] Prior literature (Dyreng and Lindsey 2009; Feng et al. 2009; Dyreng, et al. 2013) extensively uses these exhibits, though not to directly compute measures of income shifting. We match subsidiary locations to corporate statutory tax rates collected from the KPMG Corporate

[9] Chen et al. (2018) also develop an identification strategy related to income shifting. However, this strategy relies on a single narrow event and does not consider (and may exacerbate) key correlated omitted variable concerns as described in Appendix A (Botosan and Stanford 2005).

[10] SEC Regulation S-X defines control of a subsidiary for Exhibit 21 disclosures as "the possession, direct or indirect, of the power to direct or cause the direction of the management and policies of a person, whether through the ownership of voting shares, by contract, or otherwise." This definition of control is stricter than used for determining consolidation (e.g., FASB 2003), resulting in the inclusion of many non-consolidated entities in firms' Exhibit 21 disclosures (Feng et al. 2009; Demeré et al. 2017).

Tax Rate Survey, the OECD, and the websites of countries' tax authorities, and then compute tax rate differentials between each country pair that could have cross-jurisdictional intra-firm transactions.[11]

Gramlich and Whiteaker-Poe (2014) and Dyreng et al. (2018) document that in recent years some firms have changed how they interpret materiality for purposes of Exhibit 21 disclosures of subsidiaries and affiliates. Firms that adjust their materiality interpretation to significantly reduce the number of subsidiaries and affiliates reported tend to be engaged in more aggressive international tax avoidance transactions (Herbert et al. 2016) and have more media scrutiny and worse financial reporting quality (Dyreng et al. 2018). Additionally, firms are particularly likely to deem tax haven subsidiaries immaterial, and thus omit them from Exhibit 21 disclosures (Dyreng et al. 2018). While these represent data issues related to *ISI*, we use various control variables (including firm fixed effects) to try to minimize the effect on our results. Our identification strategy using the Lamont and Polk (2002) decomposition (see Appendix A) also controls for firm-specific correlated omitted variables, and thus should largely control for tax aggressiveness, media scrutiny, and financial reporting quality. Nevertheless, results using this measure may not externalize to firms with significant under-disclosure or with numerous hidden tax haven subsidiaries.[12]

We also consider using other income-shifting measures, some of which were developed concurrently with our study, but determine that these measures are less appropriate in examining firm value consequences. We discuss how *ISI* compares to these other income-shifting measures in Appendix A and

[11] This data is subject to two limitations. First, because we hand-collect the Exhibit 21 data, we do not have data for all countries. We focus on collecting the number of subsidiaries in countries that are OECD members, major tax havens, and are among the top-50 U.S. trading partners by exports or imports. Second, we cannot determine the statutory corporate tax rate for all country-years. We set the tax rate to missing when we cannot identify a tax rate.

[12] While these data issues represent a potential limitation of *ISI*, ours is not the only measure to face similar challenges. Bureau van Dijk data also suffer from significant missing data for subsidiaries in major economies such as the United States, China, or most tax haven locations. Bureau van Dijk coverage quality also appears to vary systematically across countries, and many financial statement numbers are not directly comparable across jurisdictions (OECD 2015b). To further address this concern, we follow Demeré et al. (2017) and define a significant drop in reporting subsidiaries as a firm-year with 50% or less of the number of countries with a subsidiary or special purpose vehicle-type entities as in the previous year, where the prior firm-year indicated ten or more countries or special purpose entities. Our results are robust to the exclusion of these firm-years and all subsequent firm-years after a firm experiences a significant Exhibit 21 reporting change. Interestingly, our results are also generally consistent among these dropped observations, suggesting that significant information about income shifting remains, even in Exhibit 21 disclosures that are likely subject to significant underreporting of subsidiaries.

Figure A.1; we also briefly summarize these inferences here. First, the approaches used by Collins et al. (1998) and Dyreng and Markle (2016) only examine potential determinants of income shifting, rather than the financial consequences we seek to assess in this paper. Second, to address this shortcoming, Chen et al. (2018) develop a firm-specific measure based on the Collins et al. (1998) methodology. However, the Chen et al. (2018) measure is a function of foreign sales growth and profitability by construction, which is problematic because sales growth and profitability are important determinants of firm value (Ohlson and Juettner-Nauroth 2005; Ohlson 2009), leading to a significant correlated omitted variable issue. Third, Saavedra and Williams (2017) use industry-year data to estimate a model of UTBs using foreign sales. They interpret the coefficient of foreign sales as an estimate of aggressive, but not overall, income shifting (i.e., where there is a less than 50 percent probability of winning upon audit); in contrast, our need is for an overall measure of income shifting. Their measure also suffers from data constraints that limit its ability to capture even aggressive income shifting accurately (Cazier et al. 2015; Beuselinck and Pierk 2017); also see Appendix B. Fourth, Huizinga and Laeven (2008) develop a theoretically driven income-shifting measure that serves as conceptual underpinning for our *ISI* measure. Unfortunately, Huizinga and Laeven (2008) calculate their measure at the subsidiary level and not at the consolidated level, making it inappropriate for consolidated-firm stock price evaluation. Fifth, De Simone et al. (2018) use Bureau van Dijk data to estimate income shifting for European parent companies, despite not having critical data for affiliates located in the United States, China, and most tax haven locations. In addition to the data limitation, similar to Chen et al. (2018), the De Simone et al. (2018) measure is a function of foreign sales growth and profitability, and these variables are important determinants of firm value. Sixth, De Simone et al. (2017) use confidential data from IRS Form 5471 to infer an income-shifting activity measure and then develop an "Outbound Score" as a function of regression coefficients that explain it. An obvious advantage of this approach is the use of confidential tax return data. On the other hand, several input variables in the Outbound Score are significant determinants of firm value, even without a connection to income shifting

(e.g., size, foreign sales growth, and return on foreign sales).[13] Controlling for determinants effectively removes them from the Outbound Score. Because the IRS data is confidential, we are unable to empirically ascertain the effects of this exclusion, which weakens its ability to effectively proxy for income shifting (see Appendix A for more detail).

Measure of firm value

We use Tobin's Q (*MTB*) to measure the value premium placed on firm assets by investors, consistent with prior and concurrent research (e.g., Desai and Dharmapala 2009b; Bryant-Kutcher et al. 2012; Iliev et al. 2014; Fauver et al. 2017; Fabisik et al. 2018). For this purpose, the market (book) values are measured by the sum of the market (book) value of equity and the book value of debt.[14] We measure *MTB* three months after fiscal year end to ensure that investors have sufficient time to incorporate information about changes in a firm's corporate tax rate and geographic footprint.

Alternately, we could use stock returns to measure firm value. However, using returns is particularly difficult in our setting, as it is unclear when investors learn about 1) changes in firm's geographic footprints that would affect income-shifting incentives and 2) changes in country tax rates that affect income-shifting incentives. For example, investors may react to changes in country tax rates (and their effect on income shifting) when the tax rate change is initially proposed, when it is enacted into law,

[13] A revised (2018) version of the De Simone et al. (2017) working paper modifies the model by adding new control variables that include our dependent variable, Tobin's Q, and removing the "aggressiveness" measure related to income shifting. We use the model provided in De Simone et al. (2017) to avoid the problem of including Tobin's Q as the dependent variable and as a component of the test variable. We note that the positive but insignificant coefficient on Tobin's Q in De Simone et al. (2018) does not necessarily imply a positive relation between Tobin's Q and income shifting because the test doesn't evaluate the shareholder value effect. Specifically, 1) the variables used to control variation in income shifting in De Simone et al. (2018) are not necessarily appropriate as controls for variation in Tobin's Q; 2) significant bias can occur in trying to interpret a reverse regression specification (Dietrich et al. 2007); 3) De Simone et al. (2017, 2018) order tax payments into ten bins to construct the dependent variable, and while this is appropriate for a dependent variable in an ordered logistic regression, use of the bin approach (vis-à-vis a continuous variable) loses important information, and 4) the coefficient of Tobin's Q in De Simone et al. (2018) is not statistically significant.

[14] In untabulated results, we remove goodwill from the book value of assets per Custódio (2014). While this adjusts for potential bias related to purchase accounting, it also introduces a potential confound with goodwill, so we control for goodwill in all these tests. These modifications do not change our inferences. However, this adjustment also introduces a significant amount of noise into our Tobin's Q, which causes adjusted R^2 values to plummet and even become negative in regressions with firm fixed effects. Given the questionable value of regressions with negative adjusted R^2 values and additional tests reported below to address mergers and acquisitions (M&A)-related bias, we use a traditional Tobin's Q value without a goodwill adjustment.

when the law goes into effect, when examining the most recent 10-K or 10-Q, or when the effects of income shifting are finally revealed in firm performance. The number of potential reaction events and their temporal distribution (perhaps over multiple years) implies that extremely long-run returns would be needed to capture the investor valuation effects of changes in country tax rates, which introduces significant noise into returns. Another issue arises when tax rate changes are enacted into law before the rates become effective, which means the correct empirical specification would be to regress past returns on future *ISI*; however, investor reactions may also change how firms shift income in the future, leading to simultaneity bias. Considering these issues, we focus on a common valuation measure (*MTB*) with greater stability and reduced simultaneity bias. However, we also examine stock returns in a supplemental analysis.

Empirical methodology

We employ the following regression model as a baseline in testing our hypotheses:

$$MTB_{i,t} = \alpha_{i,0} + \beta_1 ISI_{i,t} + \sum_{j=2}^{9} \beta_j CTRL_{j,i,t} + \psi_{0,t} + \epsilon_{i,t} \quad (3)$$

for each firm *i* and year *t*, where the variable of interest is *ISI* as defined above. A positive (negative) β_1 would imply that income shifting is positively (negatively) associated with firm value. In equation (3), ψ represents year fixed effects, while α alternately represents industry or firm fixed effects. To apply the Lamont and Polk (2002) decomposition, we adapt this model to a first-differences specification and split *ISI* into *ΔISIEN* and *ΔISIEX*, where we interpret the coefficients on these variables similar to β_1 with regard to *ISI*.

We test hypotheses 2 through 4 by adapting this model to include various measures of institutional ownership, analyst following, and managerial tendencies towards myopia, and interacting these variables with *ISI*. These measures include analyst following (*ANCOV*; He and Tian 2013), the percentage of institutional investor ownership (*IOWN*; Bushee 1998), the number of activist institutional owners (*ACTIVE*), the percentage of ownership held by long-term investors (*LTERM*), and the number of

institutional investors (*OWNERS*).[15] Here, the coefficient of interest is on the interaction between the oversight measure and *ISI*. A positive coefficient on the interaction would indicate that the oversight makes the association between income shifting and firm value more positive (or less negative), consistent with constraining rent extraction (Desai and Dharmapala 2009b). A negative interaction coefficient indicates that the oversight makes the association between income shifting and firm value more negative (or less positive), consistent with this oversight encouraging managers to overly focus on short-run effects when making income-shifting decisions (Bushee 1998, 2001; He and Tian 2013).

Equation (3) includes the vector, *CTRL*, of variables *j*, to control for factors that could potentially generate a spurious association between income shifting and firm value. Additional variables account for the number of countries in which a firm has operations (*CNTYS*) and the size of their foreign operations (*FORGN*) to control for a firm's foreign footprint and foreign diversification (Creal et al. 2014). We control for sales growth (*SGROW*) because a firm with a broader geographic footprint likely has different growth opportunities, which could affect firm value. Given that past literature links intangible assets to both income shifting (Collins and Shackelford 1997; Dischinger and Riedel 2011) and firm value (Sougiannis 1994), we also control for intangible asset intensity (*INTAN*), R&D activity (*RDE*), and whether a firm's R&D expense is missing (*RDEMIS*; Koh and Reeb 2015) to assure that we are not simply capturing a valuation effect of intangible assets.[16] We include control variables for several country-level features that might be correlated with tax rates and affect firm value because *ISI* derives variation from both the geographic location of company operations and tax rate differentials, and because tax rates may move in conjunction with other economic and legal changes in a country. Specifically, we control for the degree of corruption (*CORPT*), government effectiveness (*GEFF*), political stability (*POL*), regulatory quality (*REGQ*), rule of law (*LAW*),

[15] We measure long-term holdings (*LTERM*) as the percentage of stock held by an institutional investor for eight consecutive prior quarters. Changing *LTERM* to require that an institutional investor held a specific stock for twelve or twenty consecutive prior quarters increases the statistical significance of these results. All results are similar whether using ownership percentages or logged counts for each of these variables.

[16] Because income-shifting incentives and intangible assets may work together to influence firm value, we examine how the interaction of *ISI* with these measures affects firm value in supplemental analyses. In computing *RDE*, we set all missing values equal to zero; controlling for *RDEMIS* helps ensure that this design choice does not bias our results (Koh and Reeb 2015).

and freedom of expression (*VOICE*) in a country, as well as inflation (*INFL*), GDP per capita (*GDP*), and unemployment (*UNEM*), where all of these variables are weighted averages of the country-level factors that a firm faces.[17] We also include variables to control for size (*SIZE*) and leverage (*LEV*).[18] All variables are defined in Table 1, panel A.[19]

In additional specifications, we also include controls for three variables that may remove important variation from our results, yet also address significant potential sources of endogeneity. First, we control for the average foreign ETR (*FETR*) to ensure that our results are not simply capturing general foreign tax avoidance as examined in prior literature (Bryant-Kutcher et al. 2012). However, including *FETR* in our model may also remove some variation due to foreign–foreign income shifting, and thus understate the effects of income shifting.[20] Second, to address concerns that correlated non-income-shifting tax avoidance could drive the association between *ISI* and *MTB*, we control for the lagged three-year cash ETR (*LCETR*) (Dyreng et al. 2008). However, specifications that control for *LCETR* will understate our results if *ISI* has a persistent component that affects tax avoidance. Third, *ISI* contains some variation due to the extent and location of international operations, and thus could proxy for a firm's investment opportunity set. To ensure

[17] We weight all these country-level variables the same way we weight *ISI*. Specifically, we (1) weight U.S. statistics (e.g., rule of law or GDP per capita) and all foreign statistics based on the proportion of company sales in the U.S. and in foreign jurisdictions, respectively, and then we (2) weight each foreign statistic by the number of unique entities that a firm has in a given jurisdiction. While all results are similar if we instead use an average of non-U.S. country statistics (Dyreng et al. 2012), this specification has an advantage in that weighting these variables also helps control for endogenous variation in firms' geographic footprints, which is inherent in *ISI*. All country-level statistics exhibit some degree of variation across time.

[18] We could add a multitude of firm-level variables to this specification. For example, M&A could change firms' geographic footprints and firm values. While our results are robust to alternately adding a control variable for M&A activity (and many other firm-level variables) and using this variable to exclude firms from our analyses with M&A in a given year, we keep our model parsimonious since we are able to address correlated omitted firm-level variables using the Lamont and Polk (2002) decomposition. These decomposition tests suggest that endogeneity from any firm-level source (e.g., internal growth, M&A activities, geographic complexity, etc.) is not driving the Tobin's Q results.

[19] We winsorize all continuous variables by year at the 1st and 99th percentiles. Results are also robust to the use of robust regression to address outliers (Leone et al. 2017).

[20] We control for *FETR* to address concerns about *ISI* simply capturing foreign non-income-shifting tax avoidance. However, controlling for *FETR* also effectively controls for the *FTR* income-shifting incentive variable used by Collins et al. (1998) (i.e., the statutory U.S. tax rate minus *FETR*). Because the U.S. statutory tax rate remained constant at 35% during our sample period, this is the same as multiplying *FETR* by -1 and adding a constant, which will be absorbed by the intercept in our regression models. The insignificant coefficients on *FETR* in Table 2 suggest that *ISI* contains more information about income shifting than the Collins et al. (1998) *FTR* measure. In untabulated results, we remove *FETR* and *FORGN* from our analyses in case these controls are removing variation in income shifting that might affect our inferences. Results from these analyses are similar to or stronger than our tabulated analyses, supporting our inferences and that we are not "controlling away" positively-valued income shifting.

that the association with *MTB* is not due to *ISI* acting as a proxy of investment opportunities, we control for the investment opportunity set variable (*IOSF*) from Baber et al. (1996). Because one of the variables used in the Baber et al. (1996) factor analysis is the market-to-book value of assets, *IOSF* is highly positively correlated with *MTB*, and thus controlling for *IOSF* eliminates important variation in *MTB* that could be necessary in testing associations between *MTB* and other variables.

Finally, we include year fixed effects and either industry or firm fixed effects in equation (3) to control for time-invariant and firm-invariant unobservable effects, and cluster standard errors by firm and year (Gow et al. 2010). Including firm fixed effects also has the benefit of making our analyses a generalized differences-in-differences model (Wooldridge 2010; Roberts and Whited 2013), which allows us to more robustly capture treatment effects across changes in income shifting.[21] In some specifications, we also use first-differences per the Lamont and Polk (2002) decomposition described in Appendix A to better identify the causal effect of income shifting on firm value.

Data and sample selection

The sample period begins with 1997 (the first year the SEC required all firms to electronically file Form 10-K, including Exhibit 21) and ends with 2013.[22] We begin with the full population of Compustat firms over this period and eliminate firm-years that are financial or regulated utility firms (NAICS codes 52 and 22) because they face unique incentives and tax rules. We also remove firms with negative pre-tax income net of special items because these firms face different income-shifting incentives (Hopland et al.

[21] Difference-in-differences models typically control for constant effects across firms with a pre/post-treatment variable and control for time-constant effects with a treatment indicator variable. The coefficient on the interaction of the pre/post variable and the treatment variable represents the treatment effect. Here, year fixed effects control for effects that are constant across firms, without needing to specify a single treatment event, while firm fixed effects control for time constant effects within each firm. The treatment effect then is the coefficient on the firm- and time-variant variable *ISI* (Roberts and Whited 2013; Demeré et al. 2017).

[22] Having electronic files significantly aids hand-collection, which is why the sample begins in 1997. We end in 2013, given the need to impose an end to the hand-collection of data. In December 2017, Congress passed the *2017 Tax Cuts and Jobs Act* (KPMG 2017). This act is expected to reduce U.S.-outbound income shifting due to the decrease in U.S. corporate tax rates. However, U.S. firms' worldwide income shifting could increase because the act changes the United States from a worldwide to a territorial tax system (Atwood et al. 2012; Markle 2016). The proxy we develop, *ISI*, should prove useful for evaluating managerial responses to the regime change, as it can be used to examine the consequences of income shifting and offers a clear identification strategy (see Appendix A).

2018), which could introduce unnecessary noise into our analyses.[23] We remove firms without a U.S. incorporation code because we seek to focus on companies headquartered in the United States, as these companies will have to report their worldwide organizational structure in Exhibit 21 disclosures. Finally, we remove U.S. domestic-only firms, since they have no multinational operations that can engage in cross-jurisdictional income shifting.[24] This leaves a sample of 28,073 firm-year observations. Requiring an observation to have sufficient data to compute all control variables in equation (3) further reduces the sample to 22,467 firm-year observations.[25]

4. Results

Descriptive statistics and univariate results

Table 1, panel B reports distributional characteristics for the variables employed in our tests. The average (median) firm has a Tobin's Q (*MTB*) of about 2.6 (1.9), pre-tax ROA (*PTROA*) of about 9.9 (8.4) percent, post-tax ROA (*ROA*) of about 6.9 (6.0) percent, and opportunities to reduce its ETR on income that can be shifted by 5.1 (3.8) percentage points (*ISI*). Descriptive statistics are generally similar to those of recent literature (e.g., Robinson et al. 2010; Demeré et al. 2017).

Table 1, panel C reports Pearson correlation coefficients between *ISI*, *MTB*, *PTROA*, *ROA*, and all other variables. The Pearson correlation indicates that *ISI* is negatively associated with *MTB*, *PTROA*, and *ROA*, as are *DISI* and *FISI* (all $p < 0.05$). Although these correlations provide preliminary evidence that overall income shifting, and its U.S.–foreign and foreign–foreign components, are negatively associated with firm value and performance, we do not derive inferences from these correlations since these relations are univariate and thus correlated omitted variables could drive them. Variance inflation factors (VIFs) show no evidence of multicollinearity that would affect our results (i.e., untabulated VIFs are less than 5).

[23] In untabulated results, we remove this filter and rerun the analyses. In doing so, t-statistics decrease for most of the results, consistent with the removal of this filter increasing noise as a trade-off for the ability to externalize to a greater number of firms; however, all our tabulated results remain qualitatively similar.

[24] Our results are quantitatively similar if we do not remove U.S. domestic-only firms, likely due to the use of firm fixed effects in our analyses. Estimating on a subsample of U.S. multinationals also helps control for endogeneity related to firms' choice to expand their operations into foreign jurisdictions.

[25] We lose 4,119 observations due to missing quarterly data needed to compute *MTB* or the lack of a lagged value of sales to compute sales growth. The remaining 1,487 lost observations lack data for at least one control variable.

Income shifting and firm value (Hypothesis 1)

Table 2 reports the estimation of equation (3) as the primary test of Hypothesis 1. The basic specification in column (1) shows a negative association between *ISI* and Tobin's Q (*MTB*), while controlling for variables known or expected to affect *MTB* ($p < 0.01$). Thus, contrary to conventional wisdom, income shifting appears to be negatively associated with firm value. Column (2) introduces *FETR*, *LCETR*, and *IOSF* as additional control variables to address concerns that *ISI* is correlated with omitted tax avoidance characteristics or investment opportunity sets; *ISI*'s negative association with *MTB* remains negative ($p < 0.01$). In columns (3) and (4), we repeat the analyses in columns (1) and (2), respectively, except to replace industry fixed effects with firm fixed effects. The association between *ISI* and *MTB* remains negative in both columns ($p < 0.05$), suggesting that time-invariant firm characteristics do not drive the results.

In column (5), we use the Lamont and Polk (2002) decomposition to identify the effects of exogenous and endogenous changes in income-shifting incentives on changes in firm value. This analysis indicates that exogenous variation in *ISI*, as reflected in the negative coefficient for *ΔISIEX* ($p < 0.01$), drives the results in previous columns. Combined with the insignificant coefficient on *ΔISIEN*, these results suggest that the negative association between income shifting and firm value is largely robust to endogeneity concerns (i.e., correlated omitted firm characteristics or simultaneity does not affect it). Finally, in column (6), we add firm fixed effects on top of the Lamont and Polk (2002) decomposition to control for firm characteristics that might consistently drive changes in firm value or income shifting, finding that the coefficient on *ΔISIEX* remains significantly negative ($p < 0.10$).

In summary, the results in Table 2 support rejecting the null hypothesis of no effect of tax-motivated income shifting on firm value. After controlling for numerous variables and using a strong identification strategy, we consistently find that greater income shifting (*ISI*) is associated with lower firm value (*MTB*). This finding appears to be economically significant. For example, in columns (5) and (6), we find that increasing exogenous *ISI* by a 1% increment (e.g., from the mean of 5.1% to 6.1%) is associated with a

0.5% decrease in Tobin's Q at the mean.[26]

Cross-sectional analysis of corporate oversight's role (Hypothesis 2)

Given a negative association between income shifting and firm value, we now examine potential causes. Specifically, we investigate whether this negative association likely arises from managerial rent extraction (Desai and Dharmapala 2006, 2009b) or a myopic focus by managers (Bushee 1998, 2001; He and Tian 2013). Because prior research shows that external oversight in the form of greater institutional ownership (Bushee 1998, 2001) and analyst following (He and Tian 2013) increases myopic investing behavior but reduces the extent to which managers can extract rents (Desai and Dharmapala 2009b), we seek to identify the dominant explanation by examining how our Table 2 results vary with corporate oversight.

Table 3 reports the results of these analyses. Column (1) reveals that the negative association between *ISI* and *MTB* is greater for firms with stronger analyst following (i.e., greater *ANCOV*) because the coefficient on the *ISI*×*ANCOV* interaction is negative ($p < 0.01$). This result is inconsistent with analysts providing oversight that limits managerial rent extraction. However, it is consistent with analysts pressuring managers to meet short-term benchmarks, possibly leading managers to underinvest in long-term positive net-present-value projects when developing income-shifting strategies. This lack of foresight could lead firms to accept greater income-shifting risk and unsuccessful tax-related income-shifting outcomes, or to firms being subject to greater earnings management activities that reduce or eliminate income-shifting benefits, such as use of the PRE designation to manage earnings (e.g., Krull 2004).

In columns (2) to (5), we examine various measures of institutional owner oversight.[27] We fail to find significant cross-sectional variation in our primary result across firms with a greater percentage of institutional investors (*IOWN*) or percentage of ownership held by long-term investors (*LTERM*) in columns (2) and (5). However, in columns (3) and (4), we find that the negative association between *ISI* and *MTB* is

[26] In column (5), (-1.38/100)/(Mean *MTB* of 2.554) = -0.0054. In column (6), (-1.24/100)/(Mean *MTB* of 2.554) = -0.0049.

[27] The results are similar whether we use ownership percentages or logged counts for each of these variables.

greater for firms with a greater number of institutional investors (*OWNERS*; $p < 0.01$) and a greater number of activist institutional owners (*ACTIVE*; $p < 0.10$). These results are consistent with column (1) in suggesting that managerial myopia may drive our Hypothesis 1 results, since a greater number of owners is more likely to result in intra-shareholder conflicts that encourage managers to focus on the short term.[28]

Together, these results are generally consistent with managerial myopia driving the negative association between *ISI* and *MTB*, rather than managerial rent extraction. Thus, we cautiously reject the null hypothesis for Hypothesis 2. However, we recognize that these results may vary between different types of income shifting, a possibility that we examine next.

U.S.–foreign vs. foreign–foreign income shifting (Hypothesis 3 and Hypothesis 4)

An important advantage of the *ISI* measure is that we can partition it into incentives to shift income between U.S. entities and entities in foreign jurisdictions, and incentives to shift between entities located in different foreign jurisdictions. We do this by partitioning *ISI* at the "+" in its calculation, which produces measures of incentives to shift income between the U.S. and foreign jurisdictions (*DISI*) and between different foreign jurisdictions (*FISI*).[29] Decomposing *ISI* this way allows us to examine whether differences exist in how the two types of shifting affect firm value.

We first examine whether our Table 2 results are consistent between the two types of income shifting in Table 4, panel A. In columns (1) and (2), we repeat the analyses from columns (1) and (4) of Table 2, replacing *ISI* with U.S.–foreign income shifting (*DISI*).[30] Across both columns, we find negative and significant coefficients on *DISI* ($p < 0.01$), consistent with U.S.–foreign income shifting being negatively associated with firm value. In column (3), we use the Lamont and Polk (2002) decomposition on U.S.–foreign income shifting and find that both exogenous and endogenous variation in *DISI* drive the

[28] This result could also indicate rent extraction by majority owners from minority owners (Dann and DeAngelo 1983; Shleifer and Vishny 1997; La Porta et al. 2002); however, the *ANCOV* results likely refute this possibility. This result is also consistent with institutional owners pressuring managers to undertake risky investments (Goldman and Wang 2018) and with new institutional investors pressuring managers to quickly reduce taxes (Khan et al. 2017).

[29] We also do not weight U.S.–foreign tax rate differentials and foreign–foreign tax rate differentials based on the proportion of company sales in U.S. and foreign jurisdictions, respectively, to reduce some of the endogenous variation in these measures. For more detail, see Table 1, panel A.

[30] We focus on columns (1) and (4) of Table 2, as these columns have the least and most controls, respectively.

negative association with firm value, although exogenous variation has the greatest economic and statistical significance. Columns (4) and (5) repeat the analyses from columns (1) and (4) of Table 2, this time replacing *ISI* with foreign–foreign income shifting (*FISI*). We find that the coefficient on *FISI* is significantly negative when not controlling for additional variables and firm fixed effects ($p < 0.01$) but is just insignificant (but still negative) when including controls. Finally, when applying the Lamont and Polk (2002) decomposition to foreign–foreign income shifting in column (6), we see that exogenous variation in *FISI* drives the negative association with firm value, even with our full battery of controls. Thus, both *DISI* and *FISI* appear detrimental to firm value, consistent with our results for overall income shifting (*ISI*).

To test hypotheses 3 and 4, we now reexamine the results from Table 3 after partitioning *ISI* into *DISI* and *FISI*, and report the results of these analyses in Table 4, panel B. In column (1), we find that the negative association between *DISI* and *MTB* is greater among firms with greater analyst following ($p < 0.01$), while we identify a reduction in the negative association between *FISI* and *MTB* for firms with greater *ANCOV* ($p < 0.10$). These results are consistent with myopic managerial behavior driving the negative value effects of U.S.–foreign income shifting, and managerial rent extraction driving the negative value effects of foreign–foreign income shifting.

In columns (2) through (5), we find that the negative association between *DISI* and *MTB* is greater for firms with a greater institutional ownership percentage ($p < 0.05$), a greater number of institutional owners ($p < 0.01$), a greater number of activist investors ($p < 0.01$), and a greater long-term institutional ownership percentage ($p < 0.10$), consistent again with value destruction through managerial myopia. We also find a reduced negative association between *FISI* and *MTB* for firms with 1) a greater institutional ownership percentage ($p < 0.05$), 2) a greater number of institutional owners (insignificant), 3) a greater number of activist investors ($p < 0.05$), and 4) a greater long-term institutional ownership percentage ($p > 0.01$), consistent again with value destruction through managerial rent extraction.

In summary, these results suggest that the association between U.S.–foreign income shifting and firm value is more negative with greater oversight, consistent with pressure on managers that causes them

to act myopically and invest in income shifting in a value-destroying manner.[31] Additionally, these results suggest that the association between foreign–foreign income shifting and firm value is less negative with greater oversight, consistent with oversight limiting the ability of managers to extract rents from the firm. As such, these results support the alternative hypotheses 3 and 4. This evidence thus supports the view that both myopic investment behavior and managerial rent extraction can destroy firm value in the income-shifting setting (although managerial myopia may be the greatest concern per Table 3), and these mechanisms likely underlie the overall negative association between *ISI* and Tobin's Q.

Finally, these results suggest that it is important to consider both types of income shifting (i.e., U.S.–foreign and foreign–foreign) together and separately to obtain a complete picture of the consequences of income shifting. Because most other measures of income shifting focus only on U.S.-outbound income shifting (e.g., Chen et al. 2018; Dyreng and Markle 2016; De Simone et al. 2017), these results also highlight the value of decomposing tax-motivated income shifting into two separate components: U.S.–foreign and foreign–foreign income shifting.

Supplemental analyses

We conduct additional analyses to examine whether income shifting is associated with common measures of rent extraction, following Blaylock (2016). We start by examining whether income shifting is associated with overinvestment in Table 5, Panel A.[32] While *ISI* alone is not associated with overinvestment in column (1), this appears to be driven by endogenous variation, as the exogenous association between income shifting and overinvestment is significantly positive ($p < 0.01$) in column (2). When we split *ISI*

[31] In untabulated analyses, we explicitly examine a measure of managerial myopia, the extent to which a CEO's tenure exceeds the industry median tenure (Antia et al. 2010). This measure requires Execucomp data, which reduces our sample by approximately 33%. The interaction between *ISI* and industry-adjusted tenure is statistically insignificant, as is the interaction between *FISI* and industry-adjusted tenure. However, consistent with our inferences here, the interaction between *DISI* and industry-adjusted tenure is significantly negative ($p < 0.10$, one-tailed), suggesting CEO-driven myopia can increase the firm-value discount associated with U.S.–foreign income shifting.

[32] The dependent variable in these analyses is investment, rather than overinvestment. Consistent with recent evidence (Chen et al. 2018), we include normal determinants of investment as control variables in our regression, instead of running a first stage investment model and using the residual as a measure of overinvestment in a second-stage regression. As such, we can interpret our results as if overinvestment was the dependent variable (Chen et al. 2018). We use the natural log of investment rather than investment scaled by total assets because this offers superior measurement of investment (Demeré 2018).

into U.S.–foreign and foreign–foreign income-shifting components, we find that both are significantly associated with overinvestment, and this association is driven by exogenous variation (columns 3–6; all $p < 0.05$). While these results apparently indicate that both U.S.–foreign and foreign–foreign income shifting are associated with rent extraction, theory in De Simone et al. (2018) predicts that income shifting may be mechanically related to overinvestment. Thus, this analysis supports the De Simone et al. (2018) theoretical model despite failing to yield clear evidence pertaining to rent extraction in this setting.

To address the unclear rent extraction evidence, we next use another rent extraction measure from Blaylock (2016) and examine whether income shifting is associated with increased executive compensation. In column (1) of Table 5, Panel B, we find that *ISI* alone is not associated with executive compensation. However, when we decompose the exogenous and endogenous variation within *ISI*, we see that the association between exogenous income shifting variation and executive compensation is significantly positive ($p < 0.10$) in column (2), consistent with executives personally benefitting from income shifting. When we split *ISI* into the U.S.–foreign and foreign–foreign portions of income shifting, we find that U.S.–foreign income shifting is not associated with executive compensation. However, consistent with our inferences from Hypothesis 4, the association between exogenous foreign–foreign income shifting variation and executive compensation is significantly positive ($p < 0.01$) in column (6), supporting the view that foreign–foreign income shifting is susceptible to rent extraction (Desai et al. 2007).

We also examine the costs associated with income shifting by replacing *MTB* in equation (3) with the pre-tax ROAs (*PTROA*). While tax expenses (e.g., deferred taxes and tax assessments) will reduce post-tax (but not pre-tax) ROAs, many other expenses will impact pre-tax earnings (e.g., reorganization, planning, and reputation costs). Additionally, if several firms in the same industry increase their income shifting concurrently, the increased tax savings can result in the firms paying implicit taxes if they use the tax savings to increase output, and thus bid up input prices, or reduce output prices through increased supply (Markle et al. 2016). Results in Table 5, Panel C reveal that *ISI* is generally not associated with *PTROA*. However, when using the Lamont and Polk (2002) decomposition for foreign–foreign income shifting in column (6), we find that foreign–foreign income shifting is exogenously associated with reduced *PTROA*,

consistent with income-shifting strategies between non-U.S. countries resulting in significant up-front costs concentrated in pre-tax earnings. In untabulated analyses, we do not observe any association between income shifting and current *ROA*, which could indicate that immediate tax benefits offset up-front costs. However, this changes in future periods, as we find negative associations exist between current levels of *ISI* and both future *PTROA* and future *ROA* for the succeeding two years. Nevertheless, to the extent that firms restructure their multinational operations to take advantage of tax-motivated income-shifting incentives, the results do not rule out an exogenous association between *ISI* and ROAs.[33]

We further examine how our primary results vary with the extent of firms' investments in intangible assets, given the strong positive association between income shifting and intangible assets documented in prior literature (Collins and Shackelford 1997; Dischinger and Riedel 2011). Our first proxy for intangible asset investments is intangible assets from the balance sheet scaled by total assets (*INTAN*), and we split this further into goodwill (*GWILL*) and non-goodwill intangible assets (*OINTAN*). We also use firms' R&D intensity (*RDE*) and a binary variable for whether a firm is a member of an R&D-intensive industry (*HRDIND*; Chan et al. 2001).[34]

The results in Table 6, column (1) show that, while income shifting is detrimental to firm value, combining income shifting with more intangible assets can improve firm value (i.e., the interaction between *ISI* and *INTAN* is positive, $p < 0.05$). When we parse intangible assets into non-goodwill and goodwill components, however, the interactions between *OINTAN* and *ISI* in column (2) and between *ISI* and *GWILL* in column (3) are not significant.[35] These mixed results make it unclear whether and how income shifting interacts with intangible assets from the balance sheet to influence firm value. However, in column (4), we

[33] Income shifting can negatively affect firm value by reducing expectations of future cash flows or increasing the discount rates investors use with respect to the firm (or both). If investor expectations are accurate on average, the *ROA* and *PTROA* results suggest that income shifting is associated with reduced future cash flow expectations. In untabulated analyses, we also examine the association between income shifting and the implied cost of equity capital computed following Li and Mohanram (2014) with the moremata Stata function (Jann 2005). These results are directionally consistent with income shifting being associated with higher costs of equity capital but are statistically insignificant at conventional levels.

[34] We find very similar results when replacing *ISI* with *DISI* and *FISI* in this and subsequent supplemental analyses.

[35] Since increases in goodwill are likely due to new acquisitions, while decreases in goodwill are likely due to impairment write-downs or pre-2002 amortization, we separately examine the effects of increases and decreases in goodwill. Untabulated tests show no significant difference in our results between increases and decreases in goodwill.

find that the coefficient on the interaction of *ISI* and *RDE* is strongly negative ($p < 0.01$), which suggests that income shifting impairs firm value more (less) when R&D spending levels are high (low). Column (5) shows that income shifting impairs firm value more in high-R&D industries ($p < 0.01$). In total, these results suggest that income shifting negatively affects firm value more in the presence of internally developed intangible assets. On the surface, this result might seem unexpected, given the strong links in prior literature between income shifting and intangible assets. However, these results are consistent with previous governance-related results that show that managerial myopia appears to drive the negative association between income shifting and firm value, as prior literature has shown a strong sensitivity of investments in R&D to myopic investment behavior (Bushee 1998; He and Tian 2013).

Finally, while our earlier analyses focus on the association between income shifting and current Tobin's Q, we also examine whether our results persist when using long-run stock returns (subject to the caveats mentioned in Section 3) or into the following three years. In untabulated analyses, we fail to find a statistically significant effect of income shifting on long-run stock returns when all of our control variables are included. However, given that *FETR* is also a proxy for income-shifting incentives (Collins et al. 1998; Chen et al. 2018), we remove *FETR* from our control variables. Without controlling for *FETR*, we find that concurrent annual stock returns are negatively associated with income shifting, and particularly the exogenous changes in income shifting (i.e., *ISIEX*; $p < 0.1$), consistent with our primary results.[36] We also find that the negative association between income shifting and firm value persists into the future, with current *ISI* negatively associated with Tobin's Q one, two, and three years into the future ($p < 0.01$ in all years). These results suggest that *ISI* does not simply result in a temporary downward adjustment of firm value but has lasting negative value consequences.[37]

5. Conclusion

[36] This finding holds using annual stock returns calculated beginning either three or four months following the end of the prior fiscal year, and when using raw, market-adjusted, industry-size-adjusted, or Fama French (2015) 5-factor returns.

[37] These long-run tests only use industry and year fixed effects because firm fixed effects will remove most persistent variation. Nevertheless, we find that results for one year in the future are robust to firm fixed effects, but the results using firm fixed effects are statistically insignificant beyond one year.

This study examines the value to shareholders of tax-motivated income shifting. Because income shifting is difficult to detect and there are significant issues with current measures, we adapt a measure from prior literature (Huizinga and Laeven 2008) to our setting and pair it with an identification strategy from the finance literature (Lamont and Polk 2002) to generate well-identified inferences. We find evidence that increases in tax-motivated income shifting yield reductions in shareholder wealth—firm value is significantly lower when income shifting is greater. Our analyses reveal that exogenous changes in firm's income-shifting incentives drive this value impairment, making our finding robust to endogeneity concerns.

We next examine why income shifting is negatively associated with firm value. In general, we find evidence suggesting that the negative association is due to pressure on managers to focus on the short term. However, by separately examining income shifting between the U.S. and foreign jurisdictions, and shifting between different foreign jurisdictions, we further refine our inferences. Specifically, U.S.–foreign income shifting appears to reduce firm value by pressuring managers to engage in value-destroying myopic strategies. On the other hand, foreign–foreign income shifting appears to reduce firm value by enabling greater managerial rent extraction. These results also suggest that not all income shifting affects managers and shareholders the same, and it is likely that a value-maximizing board would need to employ different strategies to limit the negative value effects of different types of income shifting.

This paper contributes to the income-shifting literature by showing that income shifting, unlike tax avoidance more broadly, is negatively associated with firm value. Additionally, researchers can use the measure and identification strategy we developed in future studies to cleanly identify additional consequences of income shifting. Further evidence on other consequences of income shifting is needed, particularly given the significant concerns governments have about income shifting (OECD 2015a). Finally, this evidence should encourage policy makers (e.g., the OECD in their Base Erosion and Profit Shifting Project) to further restrict income shifting for the sake of, rather than at the expense of, equity investors.

Finally, we acknowledge that these results may be controversial. Conventional wisdom suggests that tax-motivated income shifting benefits shareholders (Collins et al. 1998) and prior evidence indicates that tax-motivated income shifting is widespread (Collins and Shackelford 1997; Klassen and Laplante

2012b; Clausing 2016). However, this is not the first evidence that certain manager decisions impair shareholder value. Prior research documents shareholder value destruction due to 1) real earnings management (Roychowdhury 2006; Cohen and Zarowin 2010; Kim and Sohn 2013), 2) benchmark beating (Graham et al. 2006), 3) backdating of compensatory stock options (Lie 2005), 4) insider trading (Bebchuk and Jolls 1999; Chi et al. 2014; Chung et al. 2018), 5) executive contracting (Abudy et al. 2017), and 6) corporate acquisitions (Hope and Thomas 2008; Harford et al. 2012). Neither is this the first paper to challenge conventional wisdom in this area.[38] At this juncture, our paper aims to advance the income-shifting literature by using currently-available data and empirical practices to document the effect of income shifting on firm value. We encourage further research in this area, either to support or to overturn our findings, as is often the case in the march of scientific progress.

[38] For example, Desai and Dharmapala (2006; 2009b) and Desai et al. (2007) challenged the conventional wisdom that tax avoidance is not associated with rent extraction, and as a result a link between tax avoidance and rent extraction became the new conventional wisdom (Hanlon and Heitzman 2010). Since then, the link between tax avoidance and rent extraction has itself been challenged (Blaylock 2016; Seidman and Stomberg 2017), and these challenges have in turn been challenged (Chan et al. 2016; Atwood and Lewellen 2018; Chung et al. 2018) by further evidence in support of the Desai and Dharmapala (2006) theory.

Appendix A

ISI *calculation details and comparison with other measures*

This appendix provides details about *ISI* in four sections. The first section details the construction of *ISI*, while the second section provides a numerical example to demonstrate the calculation of *ISI*. Next, the third section explains the need for developing *ISI* by discussing each of the alternative measures. Finally, the last section compares our primary Table 2 results using *ISI* with results using alternative measures.

Construction of ISI

We base the calculation of *ISI* on the *C* measure of income shifting developed by Huizinga and Laeven (2008) for use on affiliate-level data. According to Huizinga and Laeven (2008, p. 1169), *C* is "the product of two terms: $1/(1-t_i)$ and a weighted average of the tax rate differences." In other words, *C* is a subsidiary-specific constant multiplied by the sales-weighted average of tax rate differentials between a subsidiary and each of the other entities in the corporate structure, or

$$C_i = \frac{1}{1-t_i} \frac{\sum_{k\neq 1}^{K} \frac{S_k}{1-t_k}(t_i - t_k)}{\sum_{k=1}^{K} \frac{S_k}{1-t_k}} \tag{A1}$$

where *S* is the amount of sales attributable to a jurisdiction, *t* denotes a jurisdiction's tax rate, and both *i* and *k* indicate jurisdictions (the subsidiary's and other entities', respectively).

We modify *C* in six ways to examine income shifting within consolidated groups using U.S. data:

- First, we apply *C* to the consolidated entity level. To do this, we calculate the weighted average of *C* for each of a firm's subsidiaries and affiliates, including the parent company. Within-group weighting occurs by jurisdiction so that, for example, a firm with two subsidiaries located in Bermuda would have twice the weight applied to the tax rate differences between Bermuda and each of the other entity-jurisdictions of the affiliated group. Averaging across the entire corporate group allows us to examine the effect of income shifting on a consolidated-level outcome such as firm value.
- Second, we extend the *C* measure from the European to the U.S. setting by relying on SEC Form

10-K data rather than data from Bureau van Dijk. This is important for three reasons: 1) aside from subsidiary-level financial information, there is considerably more data available on U.S. firms than on European firms, so researchers can examine more empirical questions (e.g., Hypothesis 2 through Hypothesis 4 in this paper); 2) fundamental structural and cultural differences between European and U.S. firms make it unreasonable to extrapolate European-based results to the United States; and 3) use of Bureau van Dijk data imposes critical weaknesses on study findings since it is sporadic in coverage.[39]

- Third, we use firm-country pairs, rather than subsidiaries, as the base unit that we aggregate to the consolidated firm level. This avoids the canceling effect that would occur if each subsidiary is the base unit with inbound (outbound) income-shifting incentives mirrored by outbound (inbound) incentives of other subsidiaries, and *C* is a weighted average of each subsidiary's *C*. In other words, the weighted average of the *C* for each subsidiary would result in the inbound-shifting incentive of subsidiary B from subsidiary A canceling the outbound-shifting incentive of subsidiary A to subsidiary B.
- Fourth, we use absolute values, rather than signed values, of tax rate differentials. Similar to the third modification, we do this to avoid income-shifting incentives that cancel out each other. Without this modification, the order in which different jurisdictions entered the calculation could change the measure. For example, if Peru (30% tax rate), United Arab Emirates (UAE) (55% tax rate), and Latvia (15% tax rate) entered the calculation in that order, then a 25% inbound-shifting incentive into Peru from the UAE would be partially offset by the 15% outbound-shifting incentive from Peru to Latvia, as these tax rate differentials would have different signs. Using the absolute value produces the same outcome as allowing jurisdictions to enter the calculation by order of

[39] There are holes in Bureau van Dijk coverage of both companies and their subsidiaries, with subsidiary data likely to be missing for subsidiaries located in low-tax jurisdictions, where income shifting is more likely to occur, and for subsidiaries in jurisdictions with weak average corporate oversight, where managerial rent extraction is more likely to occur (OECD 2015b). Bureau van Dijk coverage quality also appears to vary systematically across countries, and many financial statement numbers are not directly comparable across jurisdictions (OECD 2015b).

highest to lowest tax rate, ensuring that our measure captures all income-shifting incentives.

- Fifth, we weight firm-country pairs by the number of entity-to-entity links that exist between the two jurisdictions, rather than by sales. For example, if a firm has two subsidiaries in France and three in Germany, then six links exist between the entity-jurisdictions of France and Germany. Data limitations drive this choice, as U.S. data provides neither subsidiary-level sales nor jurisdiction-specific sales.[40] Thus, the number of entities in a jurisdiction serves as a proxy for total profits in the jurisdiction (B_i in the Huizinga and Laeven (2008) model). This assumption makes intuitive sense, as a firm should have a greater presence, and a greater number of legal entities to provide limited liability for that presence, in jurisdictions with the greatest profit potential. However, we also test this assumption in untabulated analyses. In both univariate tests and multivariate tests that control for firm and year fixed effects, we find that the percentage of foreign-to-worldwide sales is positively associated with the number of countries in which a firm reports at least one subsidiary ($p < 0.01$). This supports our assertion that economic activity increases in relation to the subsidiary count. Further, remember that Bureau van Dijk does not report data for subsidiaries in major economies such as the United States and China or for most tax haven locations, which causes noise in the data and could cause systematic biases that produce erroneous findings or very limited inferences. Thus, some adaptation, such as using entity-by-entity links as we do, is necessary to capture most non-European subsidiaries and jurisdictions, even when using Bureau van Dijk data.
- Sixth, we assume that the number of entities in a jurisdiction, as a proxy for the profits in that

[40] Exhibit 21 only requires listing subsidiaries and their jurisdictions of incorporation. Thus, weighting each tax rate differential by the number of unique entity-to-entity links is the only feasible way to approximate economic activity in each Exhibit 21 jurisdiction, aside from equally weighting jurisdiction-to-jurisdiction links. The extent of income-generating economic activity between the subsidiaries of the firm in a given jurisdiction limits income-shifting activity. Our underlying assumption is that economic activity is better proxied by the number of entities than by the number of jurisdictions. For this reason, we do not simply weight each country-pair tax rate differential equally but rather use the best available data to attempt to incorporate the relative degree of economic activity into our measure (Huizinga and Laeven 2008). In untabulated analyses, we alternatively measure *ISI* by weighting each tax rate differential equally (i.e., assuming equally distributed activity across jurisdictions), and find qualitatively similar results to those reported.

jurisdiction, can proxy for after-tax profits, rather than simply pre-tax profits. Huizinga and Laeven (2008) represent profits in a pre-tax manner (B_i in their model), which then requires multiplying the sales weights and the overall C measure by $1/(1-t_i)$ to represent after-tax income-shifting incentives. We make this modification because a firm's presence in a jurisdiction should be associated with the after-tax, rather than pre-tax, profit potential in that jurisdiction. Aside from making intuitive sense, this modification also has the benefit of simplifying the calculation of our measure.

The firm-jurisdiction-level C analog we calculate is:

$$\frac{x_j x_i}{\sum_{j=1}^{J} x_j x_i} abs(t_j - t_i) \tag{A2}$$

where t denotes a jurisdiction's tax rate, x denotes the number of legal entities located in a jurisdiction, and both i and j denote subsidiary jurisdictions. In a second step, we aggregate these entity-jurisdiction-level measures into a single consolidated measure of income-shifting incentives, which we term *ISI* to differentiate from a subsidiary-level C measure. Using data on U.S. vs. non-U.S. sales from Compustat, we separately weight U.S.–foreign links by the proportion of U.S. sales and foreign–foreign links by the proportion of foreign sales.[41] This produces a firm level measure *ISI* as:

$$ISI = \frac{FS}{TS} \sum_{j=1}^{J} \sum_{i=1}^{I} \frac{x_j x_i}{\sum_{j=1}^{J} \sum_{i=1}^{I} x_j x_i} abs(t_j - t_i) + \frac{DS}{TS} \sum_{j=1}^{J} \frac{x_j}{\sum_{j=1}^{J} x_j} abs(t_j - t_{U.S.}) \tag{A3}$$

where FS is sales attributable to foreign segments, DS is sales attributable to the United States, TS is consolidated sales, t denotes a jurisdiction's tax rate, x denotes the number of legal entities located in a jurisdiction, and both i and j denote jurisdictions. We designed this firm-level weighted-average tax rate differential, *ISI*, to capture tax incentives to shift income across jurisdictions. Economically, the marginal

[41] We weight U.S.–foreign and foreign–foreign differentials based on the proportion of company sales in the U.S. and foreign jurisdictions, respectively, as reported in the Compustat Segments database because sales weighting is the best available proxy for economic activity (Huizinga and Laeven 2008). We do know the size of U.S. sales to specific foreign jurisdictions, so we weight U.S.–foreign links only by the number of entities in each foreign jurisdiction, rather than the number of entity-to-entity links.

dollar shifted guides the interpretation of *ISI*. That is, on average, if the firm were to shift one additional dollar of income across jurisdictions, the firm would receive *ISI* as the ETR reduction on that dollar of income. This measure is a single-period measure because this allows us to 1) enhance identification by controlling for firm fixed effects and other characteristics, and 2) capture changes in income-shifting behavior across time in response to tax rate changes, which is important given recent evidence that firms can adjust their tax positions relatively quickly to changes in the environment (Hoopes et al. 2012; Kim et al. 2017).

Numerical example to illustrate* ISI*, and a brief descriptive analysis

To illustrate the *ISI* measure, consider a hypothetical firm with 40 percent of its sales in the United States (which has a 35 percent corporate income tax rate during the sample period) and entities in the following countries:

Country	Number of Entities	Corporate Tax Rate
Latvia	2	15%
Peru	4	30%
UAE	2	55%

The first step in calculating *ISI* is to calculate the absolute tax rate differentials between each country pair, which are:

Country Pair	Absolute Tax Rate Differential
U.S.–Latvia	abs(35% - 15%) = 20%
U.S. –Peru	abs(35% - 30%) = 5%
U.S. –UAE	abs(35% - 55%) = 20%
Latvia–Peru	abs(15% - 30%) = 15%
Latvia–UAE	abs(15% - 55%) = 40%
Peru–UAE	abs(30% - 55%) = 25%

We weight U.S.–foreign tax rate differentials by the percentage of U.S. sales to total sales, and foreign–foreign tax rate differentials by the percentage of foreign sales to total sales. Finally, we weight the tax rate differentials by the number of unique entity-to-entity links constructed between two jurisdictions, as follows:

	(a)	(b)	(c)	(d)	a × b × (c/d)
Country Pair	**Absolute Tax Rate Differential**	**Sales Weight**	**Entity Links**	**Total Entity Links**	**Final Weight**

U.S. –Latvia	20%	40%	2	2 + 4 + 2 = 8	0.020
U.S. –Peru	5%	40%	4	2 + 4 + 2 = 8	0.010
U.S. –UAE	20%	40%	2	2 + 4 + 2 = 8	0.020
Latvia–Peru	15%	60%	2 × 4 = 8	8 + 4 + 8 = 20	0.036
Latvia–UAE	40%	60%	2 × 2 = 4	8 + 4 + 8 = 20	0.048
Peru–UAE	25%	60%	4 × 2 = 8	8 + 4 + 8 = 20	0.060
Sum					**0.194**

The sum of these final weights is our measure *ISI*, or in this example *ISI* = 0.194. This measure estimates that, on average, the tax incentive to shift income is $0.194 for each dollar of income shifted. However, because a firm cannot shift all its income, the impact on the firm's overall ETR will be smaller than 19.4 percentage points. Note that these amounts represent an extreme example for the purposes of illustration and are not representative of our sample of firms, as few firms have a large economic presence in countries with such extreme tax rate differentials (e.g., Latvia and UAE). For comparison, *ISI* is only 0.027 in an alternative example where 40% of sales are in the United States and the remainder occurs in two Japanese entities where the tax rate is 32% and in one entity where the tax rate is 30%. This illustrates potential benefits from shifting income between jurisdictions with smaller tax rate differentials.

We report additional detail on the temporal and industry distributions of *ISI* in Table A.1, in addition to the descriptive statistics shown in Table 1, panel B. In panel A of Table A.1, we see that *ISI* grows across time, consistent with declines in foreign statutory tax rates relative to the U.S. tax rate (Drake et al. 2018). This increase in income-shifting incentives mirrors the general temporal decline in ETRs reflected in this panel and documented by Dyreng et al. (2017). Panel B of Table A.1 indicates that *ISI* is high in industries with 1) severe corruption concerns, such as construction (Bowen et al. 2012), 2) significant intangible assets, such as information, and 3) previously documented evidence of widespread income shifting, such as mining (Fox et al. 2014).

The ability to separately identify U.S.–foreign and foreign–foreign shifting incentives, as well as shifting incentives that arise from exogenous variation in tax rates, are two important reasons that we develop and use this measure in our analyses. However, as discussed in Section 2, concurrent papers propose alternative measures to examine income-shifting consequences (De Simone et al. 2017; Chen et al.

2018; De Simone et al. 2018; Saavedra and Williams 2017). Figure A.1 summarizes the strengths and weaknesses of these measures, while Table A.2 illustrates how inferences change when using these alternative measures.

Why we constructed* ISI *(i.e., why existing measures don't fit our setting)

Figure A.1 tabulates the strengths and weaknesses of the various extant income-shifting measures. The Collins et al. (1998) and Dyreng and Markle (2016) measures have strong theoretical backgrounds and are excellent at measuring the determinants of income shifting, which enter into these regressions as cross-sectional variables. However, because these models identify income shifting through the association between a dependent and independent variable, they cannot directly examine the consequences of income shifting. Also problematic for our needs, Chen et al. (2018) implement the Collins et al. (1998) framework and use the coefficient of interest in a secondary regression to examine income-shifting consequences. However, this measure suffers from estimation issues, and foreign growth and profitability likely drives much of its variation. Because growth and profitability are significant first-order determinants of firm value (Ohlson and Juettner-Nauroth 2005; Ohlson 2009), even small biases related to these items could have large effects in examining firm value. To address this possible bias, Chen et al. (2018) identify the impact of income shifting on information asymmetry using the implementation of SFAS 131. However, this analysis continues to lend itself to a correlated omitted variable explanation, as Chen et al. (2018) find that firms that suspend geographic earnings disclosure following SFAS 131 drive their results, and these are likely to be the firms with the greatest foreign profitability (Botosan and Stanford 2005).

Next, De Simone et al. (2017) identify income-shifting firms using proprietary IRS Form 5471 data and examine the determinants of income shifting in a comprehensive manner. They suggest that coefficients from these regressions can produce prediction scores that identify income shifters when later examining consequences of income shifting. The resulting prediction scores are linear combinations of variables that are not direct proxies of income shifting and together explain less than two-thirds of the variation of a proxy for income shifting. Therefore, these scores should be relied upon with caution. Importantly, components of the prediction scores (i.e., significant regression coefficients in their model) include R&D, capital

expenditures, high technology industry membership, foreign percentage of total sales, lagged return on foreign sales, tax haven operations, lagged foreign tax rate, and firm size, leading to serious correlated omitted variable concerns. Unfortunately, the Frisch-Waugh-Lovell theorem (Davidson and MacKinnon 2003) shows that controlling for one or more of these variables effectively removes them from the De Simone et al. (2017) prediction score. As a result, there are limited options for a researcher to ensure that the likelihood score captures income shifting and one or more input variables is not driving it. One possibility is to remove several of the variables used in determining the first-stage De Simone et al. (2017) prediction score, which would further reduce its power to explain income shifting and harm the construct validity of the measure. Alternatively, the consequences model can exclude the overlapping variables, which would under-specify this model. This would result in significant endogeneity issues and thus harm the internal validity of the study.

In another measure, Saavedra and Williams (2017) model the effect of UTBs on foreign sales to produce an estimate of risky income shifting. This approach would seem reasonable if the aim is to measure only risky income shifting and if UTBs effectively measure risky tax behavior. In contrast with *ISI* and other measures, the Saavedra and Williams (2017) measure only seeks to capture income shifting that causes the firm to be in unsustainable tax positions. Thus, if the Saavedra and Williams (2017) measure worked well, it would not measure a construct that is comparable to the overall income-shifting incentive proxied by *ISI*. However, UTBs contain significant noise due to reporting incentives (Cazier et al. 2015), leading to even greater correlated omitted variable concerns with the Saavedra and Williams (2017) measure beyond those pertaining to the Chen et al. (2018) measure. Indeed, in our validation tests in Appendix B, we also find that reporting concerns appear to drive an insignificant or negative association between *ISI* and UTBs. This suggests that the association between foreign sales and UTBs does not reliably capture income shifting. Rather, such a measure may be capturing non-income-shifting foreign tax avoidance (Beuselinck and Pierk 2017), earnings management (Cazier et al. 2015), or U.S. domestic tax avoidance through a mechanical correlation between foreign and domestic sales or foreign and domestic tax avoidance (Drake et al. 2018).

Finally, Huizinga and Laeven (2008) compute a measure of income-shifting incentives using European data from Bureau van Dijk. We have described this measure above, as it theoretically underlies our *ISI* measure, and the reasoning for differences between *C* and *ISI*. De Simone et al. (2018) then regress logged ROAs on *C* and controls, using the coefficient on *C* as a measure to examine the consequences of income shifting. Similar to the Chen et al. (2018) measure, the De Simone et al. (2018) measure also suffers from estimation issues and correlated omitted variable concerns due to being a function of foreign profitability and geographic footprint.[42] Additionally, both measures suffer from limitations in the Bureau van Dijk data mentioned above.

Although we believe that the validation results in Appendix B and the econometric and theoretical advantages described above provide sufficient grounds for relying on *ISI* to measure income shifting, we next compare our firm value test results using some of these alternative measures.

Comparison of results across alternative measures

Tables A.2 and A.3 report how our Hypothesis 1 results vary across alternative measures. In Table A.2, we replace *ISI* in Table 2 with the De Simone et al. (2017) Outbound Score (*DMSO*).[43] We primarily focus on *DMSO* because De Simone et al. (2017) used confidential IRS data to develop it. Column (1) shows that the coefficient on *DMSO* is negative but insignificant. However, in column (2) (which includes controls for foreign and prior cash ETRs, as well as the firm's investment opportunity set), the *DMSO* coefficient is significantly negative ($p < 0.05$), consistent with our inferences from *ISI* that income shifting is associated with a firm value discount. Because adding these controls makes the *DMSO* coefficient

[42] For brevity, Figure A.1 simplifies the De Simone et al. (2018) measure. Technically, they estimate their measure for the entire sample, with sample-level coefficients on control variables, and firm-specific intercepts and coefficients on the Huizinga and Laeven (2008) *C* variable and a loss variable. The firm-specific coefficient on *C*, along with a firm-specific intercept, is roughly equivalent to running the regression by firm, which is where small sample biases arise (Nelson and Kim 1993; Kelley and Maxwell 2003). Because De Simone et al. (2018) estimate the coefficients of their control variables at the sample level, they only remove the sample-average effects of the control variables from the measure (and not the firm-year specific effects). To the extent that different firms, such as those in technology, manufacturing, and service, have very different profit-generating functions, some of the variation in first-stage control variables is likely to remain in the firm-specific coefficient on *C*.

[43] These analyses control for some of the inputs to the *DMSO* score, given that items such as growth, R&D, intangible assets, and foreign operations are significantly associated with firm value on their own (i.e., overall, without considering how they relate to income shifting), leading to significant correlated omitted variable bias concerns when not controlling for these variables (Sougiannis 1994; Lev and Sougiannis 1996; Ohlson 2009; Creal et al. 2014).

significant, these control variables are likely removing noise inherent in *DMSO*. Columns (3) and (4) repeat the analyses from the previous columns but also include firm fixed effects. In both columns, the coefficient on *DMSO* is negative but statistically insignificant. Because firm fixed effects remove variation in variables that are persistent over time, the insignificance of the *DMSO* coefficient in column (4) relative to column (2) could indicate that *DMSO* only captures long-run income-shifting strategies. However, we cannot rule out that there may be time-invariant correlated omitted variables that drive the results in the first two columns. We believe that these results support *ISI* as a more appropriate measure for assessing the shareholder effects of income shifting, given the lack of a clear identification strategy for *DMSO* and the significant correlated omitted variable concerns described above and evident in the variability of results in Table A.2.

In column (1) of Table A.3, we replicate the result from column (2) of Table 2 for reference. We then replace *ISI* with the De Simone et al. (2017) variables *DMSO* and *DMSA* in columns (2) and (3), respectively, and find similar results to *ISI*, consistent with Table A.2. However, when we use the Chen et al. (2018) or Saavedra and Williams (2017) measures in columns (4) and (5), respectively, we find opposing results. While this could suggest mixed findings, recall that these measures are functions of foreign profitability or sales, variables that are highly positively correlated with *MTB*.[44] Thus, these results likely illustrate the correlated omitted variable issues with these two variables.

Columns (6) and (7) of Table A.3 repeat columns (4) and (5) while including *ISI*. Remember that we eliminate the shared income-shifting variation between any two measures of income shifting in these regressions when interpreting the results. Nevertheless, we note that the coefficient on *ISI* remains significantly negative, incremental to *CHQW* and *SW*. At the same time, the coefficients on *CHQW* and *SW* remain significantly positive, even after removing all variation related to income-shifting incentives, as proxied by *ISI*. That these coefficients on these variables are relatively unaffected when controlling for a significant input to income shifting (Collins et al. 1998; De Simone et al. 2018) highlights the severity of

[44] The Pearson univariate correlation between foreign profitability and *MTB* is 0.145 ($p < 0.001$).

the correlated omitted variable bias that accompanies the *CHQW* and *SW* measures, and supports the need for an alternative measure of income shifting in this setting. Untabulated results using firm fixed effects are similar, although the coefficients on all four alternative income-shifting measures become statistically insignificant, regardless of whether the control variables include *ISI.*

Appendix B

ISI *measure validation*

Companies can use various means to accomplish tax-motivated income shifting, including relocation of assets, debts, and operations, as well as aggressive transfer pricing practices. As in prior research (e.g., Huizinga and Laeven 2008), *ISI* measures incentives to shift income through any of these means. However, because *ISI* only directly measures income-shifting incentives, we first assess the degree that *ISI* reflects actual income shifting. Our validation tests examine the extent that *ISI* reflects the following other measures that capture variation related to income shifting: 1) foreign profitability relative to worldwide profitability, 2) income-shifting scores calculated using private IRS data (De Simone et al. 2017), 3) Form 10-K search terms pertaining to foreign taxes and income shifting, 4) PRE, and 5) known financial statement measures of tax aggressiveness.[45] All variables are described in Table 1, panel A, and results of these validation tests are reported in Table B.1 of Appendix B.

Table B.1, panel A reports the association between *ISI* and 1) the ratio of foreign return on sales (ROS) to worldwide ROS (*FTOW*) in column (1), and 2) the difference between foreign and U.S. domestic ROS ratios (*ROSDIF*) in column (2), similar to the method used in prior studies to identify income-shifting determinants (e.g., Collins et al. 1998). In both columns, *ISI* is positively associated with these variables in a multivariate framework ($p < 0.01$), indicating strong association between *ISI* and existing income-shifting proxies.[46] Columns (3) and (4) of Table B.1, panel A demonstrate associations between *ISI* and the likelihood score variables in De Simone et al. (2017). These likelihood scores predict whether firms shift income out of the United States (*DMSO*) and whether firms aggressively shift income out of the United

[45] These variables cannot be used alone as measures of income shifting, as they either capture actual income shifting only through its relation to other constructs, such as foreign cash holdings or tax aggressiveness, or because they face significant limitations as measures of income shifting. Such limitations include endogeneity concerns, selection bias, high underreporting, and issues in trading off construct validity and internal validity.

[46] Given that *FTOW* is subject to extreme outliers even after winsorization, we use robust regression to examine the association between *FTOW* and *ISI*. Results using OLS regression are similar but statistically weaker, which suggests that outliers bias against finding an association between *FTOW* and *ISI*. All *p*-values are based on two-tailed t-tests unless otherwise indicated. Because multivariate models for many of these variables do not exist, we use controls typically associated with tax avoidance (Hanlon and Heitzman 2010).

States in excess of a firm's economic fundamentals (*DMSA*). *ISI* is positively associated with both likelihood scores ($p < 0.01$), consistent with income-shifting incentives being more prevalent among firms whose profile is similar to those that appear to engage in U.S.-outbound income shifting.[47]

We also examine how textual measures of foreign activity are associated with *ISI* because the text of a Form 10-K filing may provide clues regarding the extent of a firm's income-shifting activity. Columns (5) through (8) of Table B.1, panel A report the results of probit regressions of whether a firm indicates that it pays taxes abroad (*PFT*; column (5)), moves income (*MINC;* column (6)), faces favorable tax situations abroad (*FFT* column (7)), and actively engages in tax planning with respect to foreign operations (*TPFO*; column (8)) on *ISI*. Table 1, panel A defines the search terms used for each of these variables. In all columns, the coefficient on *ISI* is positive and statistically significant ($p < 0.01$), consistent with *ISI* reflecting income shifting and multinational tax-planning incentives and actions. Finally, in each column of Table B.1, panel A, we use a cluster-robust Vuong test to examine whether adding *ISI* to the model results in a significant incremental increase in explanatory power. We find that *ISI* increases the explanatory power of all models except the column (5) model that predicts whether a firm indicates that it pays taxes abroad.

Table B.1, panel B reports the results of using *ISI* in income-shifting regressions that follow Collins et al. (1998). We find positive and significant association between *ISI* and the foreign ROS after controlling for the worldwide ROS and, in column (2), other controls. This suggests that *ISI* is an important income-shifting determinant. We also use a cluster-robust Vuong test to determine whether *ISI* adds a significant amount of explanatory power to the Collins et al. (1998) regressions and find evidence that it does, as $p < 0.10$ in column (1) and $p < 0.05$ in column (2).

[47] We use these scores to help validate our *ISI* measure, but we recognize that they are computed as linear combinations of variables that do not directly measure income shifting. Thus, it is impossible to parse between the effects of the income-shifting prediction and the effects of other constructs pertaining to the underlying variables (e.g., size, leverage, profitability, foreign operations, and intangible assets). Thus, tests using these likelihood scores as independent variables have impaired internal validity. When we vary the score input variables we include in our empirical models or otherwise control for, we find wide variation in the sign and statistical significance of the coefficient on the De Simone et al. (2018) scores. This is consistent with the view that these scores also reflect constructs that are unrelated to income shifting (i.e., endogeneity exists related to correlated measurement error and correlated omitted variables).

In Table B.1, panel C, we examine the association between *ISI* and PRE, as firms are more likely to identify earnings as permanently reinvested when repatriation taxes are high due to the low tax rate initially incurred.[48] The first two regressions explain the natural log of PRE (*PREC*) and an indicator of 1 if a company reports any PRE (*PREB*), respectively; the coefficients of *ISI* are positive and significant ($p < 0.01$). Thus, firms with greater income-shifting incentives appear likelier to classify more foreign earnings as permanently reinvested to avoid losing income-shifting tax benefits when reporting GAAP earnings.[49]

Column (3) of Table B.1, panel C examines the association of income-shifting incentives with the Lisowsky (2010) tax sheltering score (*TSS*).[50] Prior literature using confidential U.S. IRS data documents that tax shelters are often associated with aggressive cross-jurisdictional income shifting (Lisowsky 2010), so if *ISI* measures tax-motivated income-shifting behavior, we would expect a positive association between *ISI* and *TSS*. We find the predicted positive and significant association between *ISI* and *TSS* in column (3).

Columns (4) to (6) examine the association of *ISI* and the natural log of UTBs (*LUTB*) because international income-shifting positions are among the most commonly reported UTBs (Towery 2017). Thus, we would expect a positive association between *ISI* and *LUTB* if UTBs accurately reflect tax

[48] Firms have greater financial reporting incentives to shift income out of the United States if they "permanently" reinvest that income abroad, and thus avoid recognizing tax expense on the income statement for the marginal U.S. portion of the tax (Graham et al. 2011). As such, PRE should be highly correlated with U.S.-outbound income shifting. PRE does not change in response to foreign–foreign income shifting; however, foreign–foreign income shifting does not increase GAAP earnings if both jurisdiction's tax rates are less than the U.S. tax rate and the income cannot be permanently reinvested, though some cash taxes can be saved. Thus, PRE should be somewhat correlated with incentives to engage in foreign–foreign income shifting, but PRE should not be correlated with U.S.-inbound income shifting. In untabulated analyses, we find that both U.S.–foreign income-shifting incentives (*DISI*) and foreign–foreign income-shifting incentives (*FISI*) are significantly associated with *PREC* and *PREB* ($p < 0.01$).

[49] Given the strong correlation between PRE and *ISI* in our validation tests, our results could reflect value consequences of PRE, rather than of income shifting. We address this concern by controlling for *PREC* in our primary analyses and find that doing so does not materially affect the coefficient on *ISI* or its significance. We do not control for *PREC* in the tabulated tests, as it results in substantial data attrition, in part because PRE data from Audit Analytics was not reliably available until 2007 and because there was significant noncompliance with PRE disclosure requirements during our sample period (Ayers et al. 2015).

[50] We do not use ETRs in the validation checks because we calculate our measure as the weighted-average tax rate differential within a firm, and thus, by construction, it mechanically associates with ETRs. For example, consider a firm with equal operations in jurisdiction A with a tax rate of 30 percent and B with a tax rate of 10 percent. If A increases its tax rate to 40 percent, then income-shifting incentives increase (i.e., the firm can now save 30 percent through income shifting), but unless all income from A is shifted to B, then the firm's tax rate also increases to reveal a mechanical positive correlation. If B follows up by decreasing its tax rate to 5 percent, then income-shifting incentives again increase (i.e., the firm can now save 35 percent through income shifting), but unless all income from B is shifted to A, the firm's tax rate will decrease to reveal a mechanical negative correlation.

uncertainty (Lisowsky et al. 2013). However, in column (4), we fail to find a statistically significant association between *ISI* and *LUTB*, consistent with prior evidence that UTB reserves are subject to considerable managerial discretion (Cazier et al. 2015). In column (5) [(6)], we partition firms into those with more (less) aggressive financial reporting practices based on Modified-Jones discretionary accruals that are above (below) the median. The results confirm the intuition that a weak association between *ISI* and *LUTB* is due to pressure to underreport the UTB liability and related tax expense (Cazier et al. 2015) and to limit IRS scrutiny (Mills et al. 2010; Graham et al. 2012; Bozanic et al. 2017). Specifically, the association for firms with more aggressive financial reporting is negative ($p < 0.10$), indicating that lower-quality UTB accruals decline with the amount of income shifting. This suggests that there is substantial under-reporting of income-shifting-related UTBs, even though income-shifting positions are among the most frequently reported UTBs (Towery 2017). Conversely, the association for firms with less aggressive financial reporting is positive but insignificant in column (6).

Overall, our tests support the view that *ISI* meaningfully reflects potential tax savings from shifting income across national borders.[51] We therefore rely on *ISI* to examine the association between firm value and income shifting.

[51] Per Table 1, panel C, this section's results are robust to using either Pearson or Spearman univariate correlations.

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TABLE 1: Panel A
Variable definitions

Variable	Definition
Primary income-shifting measure	
ISI	Income-shifting incentives, computed as: $$ISI = \frac{FS}{TS}\sum_{j=1}^{J}\sum_{i=1}^{I}\frac{x_j x_i}{\sum_{j=1}^{J}\sum_{i=1}^{I} x_j x_i} abs(t_j - t_i) + \frac{DS}{TS}\sum_{j=1}^{J}\frac{x_j}{\sum_{j=1}^{J} x_j} abs(t_j - t_{U.S.})$$ where *FS* is the amount of sales attributable to foreign segments, *DS* is the amount of sales attributable to the United States, *TS* is the total sales of the firm, *t* denotes a jurisdiction's tax rate, *x* denotes the number of legal entities located in a jurisdiction, and both *i* and *j* denote jurisdictions
Other income-shifting measures	
DISI	U.S.–foreign income shifting, computed as: $$DISI = \sum_{j=1}^{J}\frac{x_j}{\sum_{j=1}^{J} x_j} abs(t_j - t_{U.S.})$$ where *t* denotes a jurisdiction's tax rate, *x* denotes the number of legal entities located in a jurisdiction, and *j* denotes foreign jurisdictions
FISI	Foreign–foreign income shifting, computed as: $$FISI = \sum_{j=1}^{J}\sum_{i=1}^{I}\frac{x_j x_i}{\sum_{j=1}^{J}\sum_{i=1}^{I} x_j x_i} abs(t_j - t_i)$$ where *t* denotes a jurisdiction's tax rate, *x* denotes the number of legal entities located in a jurisdiction, and both *i* and *j* denote jurisdictions
ΔISIEX	ISI_t – *Endogenous* ISI_t, following Lamont and Polk (2002), where *Endogenous ISI* is computed the same as *ISI* above, but using prior period tax rates, current proportions of U.S. (or foreign) sales to total sales, and current Exhibit 21 subsidiary listings
ΔISIEN	*Endogenous* ISI_t – ISI_{t-1}, following Lamont and Polk (2002), where *Endogenous ISI* is computed the same as *ISI* above but using prior period tax rates, current proportions of U.S. (or foreign) sales to total sales, and current Exhibit 21 subsidiary listings
Dependent Variables	
MTB	Market-to-book ratio, computed as the sum of book debt and the market value of equity, divided by the sum of book debt and book equity. Measured at a three-month lag to the fiscal year to ensure investor access to the SEC Form 10-K.
INVEST	Investment, computed as the natural log of 1 plus total investment expenditures. Investment expenditures are capital expenditures plus R&D expense plus acquisition costs plus amortization and depreciation minus sales of property, following Richardson (2006).
EXCOMP	Executive compensation, computed as the natural log of CEO compensation from Execucomp. CEO compensation includes salary, bonus, restricted stock grants, stock option exercises, long-term incentive payouts, and other compensation.
PTROA	Pre-tax profitability, computed as pre-tax income divided by average total assets
ROA	Profitability, computed as pre-tax income less total tax expense, divided by average total assets

Interaction variables	
INTAN	Intangible assets, computed as intangible assets divided by total assets
OINTAN	Non-goodwill intangible assets, computed as intangible assets less goodwill, all divided by total assets
GWILL	Goodwill, computed as goodwill divided by total assets
RDE	R&D intensity, computed as R&D expense divided by lagged total assets (and set equal to 0 if R&D expense is missing)
HRDIND	Indicator variable equal to 1 if a firm is in a high-R&D industry, and 0 otherwise. High-R&D industries are NAICS 325, 334, 517, 518, and 541, following Chan et al. (2001).
ANCOV	Analyst coverage, computed as *ln*(A + 1), where A is the average number of analysts providing earnings forecasts at any point during the year
LTERM	Long-term institutional ownership, computed as the percentage of stock held by institutional owners for the prior eight consecutive quarters (Bushee 1998). The Thomson Reuters 13f database provides this data.
IOWN	Institutional ownership percentage, computed as the total percentage of stock held by institutional owners. The Thomson Reuters 13f database provides this data.
ACTIVE	The number of activist institutional owners, computed as *ln*(A + 1), where A is the number of institutional owners of a firm from the Thomson Reuters 13f database classified as activists per Cremers and Nair (2005)
OWNERS	The number of total institutional owners, computed as *ln*(O + 1), where O is the number of institutional owners of a firm from the Thomson Reuters 13f database
Control variables	
CNTYS	The number of countries in which a firm has Exhibit 21 subsidiaries
FORGN	Foreign activity, computed as pre-tax foreign income divided by lagged total assets (and set equal to 0 if foreign income is missing)
RDEMIS	Indicator variable equal to 1 if R&D expense is missing in Compustat, and 0 otherwise (Koh and Reeb 2015)
SIZE	Firm size, computed as the natural log of market capitalization
LEV	Leverage, computed as long-term debt divided by total assets
SGROW	Sales growth rate, computed as the difference in sales from the prior fiscal year, scaled by sales in the prior fiscal year
CRUPT	The weighted average country-level corruption that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level corruption as a percentile rank
GEFF	The weighted average country-level government effectiveness that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level government effectiveness as a percentile rank
POL	The weighted average country-level political stability that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level political stability as a percentile rank
REGQ	The weighted average country-level regulatory quality that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level regulatory quality as a percentile rank
LAW	The weighted average country-level rule of law that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level rule of law as a percentile rank

VOICE	The weighted average country-level freedom of expression that a firm faces, where the weights are the same as in computing *ISOP,* and the World Bank measures country-level freedom of expression as a percentile rank
INFL	The weighted average country-level inflation rate that a firm faces, where the weights are the same as in computing *ISOP,* and country-level inflation rates come from the World Bank
GDP	The weighted average country-level GDP per capita that a firm faces, where the weights are the same as in computing *ISOP,* and country-level GDP per capita comes from the World Bank
UNEM	The weighted average country-level unemployment rate that a firm faces, where the weights are the same as in computing *ISOP,* and country-level unemployment rates come from the World Bank
FETR	Foreign ETR, computed as the sum of current and deferred foreign taxes scaled by pre-tax foreign income
LCETR	Three-year prior cash ETR, computed as the three-year sum of cash taxes paid, divided by the three-year sum of pre-tax book income, less special items (summed over t-1, t-2, and t-3)
IOSF	Investment opportunity set, computed as described in Baber et al. (1996)

Other variables for validation tests

LUTB	Natural log of UTB reserves, computed as *ln*(UTB reserve + 1)
TSS	Tax shelter score, computed as the predicted value from Combined Model 2 of Table 4 from Lisowsky (2010)
FTOW	The ratio of foreign ROS to worldwide ROS, computed as (pre-tax foreign income divided by pre-tax income) divided by (1/R), where R is the ratio of foreign to worldwide sales
ROSDIF	The difference in the foreign and domestic ROS, computed as (pre-tax foreign income divided by foreign sales) less (pre-tax U.S. domestic income divided by U.S. sales)
DMSO	Outbound income-shifting score, computed as the predicted value from Model 1 of Table 4 from De Simone et al. (2017)
DMSA	Aggressive income-shifting score, computed as the predicted value from panel A of Table 6 from De Simone et al. (2017)
CHQW	The *SHIFT_AVE* measure of Chen et al. (2018), computed per their paper
SW	The *SHIFTER1* measure of Saavedra and Williams (2017), computed per their paper
PREC	Continuous PRE, computed as *ln*(P + 1) where P is PRE from Audit Analytics in thousands of U.S. dollars
PREB	Binary PRE, equal to 1 if Audit Analytics reports non-zero PRE, and 0 otherwise
PFT	Indicator variable equal to 1 if a firm's 10-K contains any of the terms "Foreign Tax*," "International Tax*," "Multinational Tax*," or "Multi*national Tax," and 0 otherwise. * is a wildcard character.
MINC	Indicator variable equal to 1 if a firm's 10-K contains any of the terms "Income *Alloc*," "*alloc* Income," "Income Shift*," "Shift* Income," or "Transfer Pric*," and 0 otherwise. * is a wildcard character.
FFT	Indicator variable equal to 1 if a firm's 10-K contains the term "Low* Tax* Rat*" along with "Foreign" or any of the terms "[good favor* benefic* prefer* low*] Tax* Jurisdiction," and 0 otherwise. * is a wildcard character and different searches alternately use words in brackets.

TPFO	Indicator variable equal to 1 if a firm's 10-K contains the terms "Tax Structur*" or "[Foreign International Multinational Multi*national] Tax Plan*," and 0 otherwise. * is a wildcard character and different searches alternately use words in brackets.
NOL	NOL, computed as tax-loss carryforward divided by lagged total assets (and set equal to 0 if NOL is missing)
ΔNOL	$NOL_t - NOL_{t-1}$
PPE	Capital intensity, computed as gross property, plant, and equipment, divided by total assets
CASH	Cash holdings, computed as cash and cash equivalents, divided by total assets
IFUND	Internal fund supply, computed as the sum of cash flows from operations and investing, divided by average total assets
CLTD	Debt renegotiation costs, computed as the current portion of long-term debt, divided by total assets
MTBA	Market-to-book ratio, computed as the sum of book debt and the market value of equity, divided by the sum of book debt and book equity. Measured at the end of the fiscal year.

TABLE 1: Panel B
Descriptive statistics

	N	Mean	Std. dev.	Median
ISI	22,467	0.051	0.056	0.038
MTB	22,467	2.554	2.537	1.860
PTROA	22,467	0.099	0.100	0.084
ROA	22,467	0.069	0.078	0.060
INVEST	22,270	4.602	1.857	4.566
EXCOMP	13,645	8.026	1.137	7.993
DISI	22,467	0.058	0.066	0.039
FISI	22,467	0.038	0.046	0.010
INTAN	22,467	0.189	0.184	0.134
OINTAN	22,467	0.059	0.093	0.020
GWILL	22,467	0.129	0.144	0.079
RDE	22,467	0.042	0.069	0.011
HRDIND	22,467	0.339	0.473	0.000
ANCOV	22,467	1.478	1.074	1.609
ACTIVE	19,026	1.755	0.845	2.048
OWNERS	19,026	4.556	1.233	4.747
LTERM	19,026	0.340	0.250	0.375
CNTYS	22,467	7.876	10.862	3.000
FORGN	22,467	0.028	0.043	0.010
RDEMIS	22,467	0.338	0.473	0.000
SIZE	22,467	6.691	1.975	6.702
LEV	22,467	0.169	0.175	0.134
SGROW	22,467	0.159	0.333	0.094
CRUPT	22,467	81.735	22.367	90.284
GEFF	22,467	83.529	22.553	91.244
POL	22,467	58.426	17.264	63.375
LAW	22,467	82.880	22.658	91.183
REGQ	22,467	84.175	22.898	92.290
VOICE	22,467	79.194	22.044	87.841
INFL	22,467	2.250	0.964	2.373
GDP	22,467	36,621	12,136	42,135
UNEM	22,467	5.645	1.902	6.012
FETR	16,386	0.273	0.228	0.257
LCETR	14,689	0.251	0.170	0.244
IOSF	19,606	-0.022	0.009	-0.024
LUTB	7,188	2.594	1.853	2.397
TSS	21,476	0.832	0.279	0.973
FTOW	11,565	1.107	1.527	0.946
ROSDIF	11,430	-0.004	0.313	0.003
DMSO	7,918	-0.868	0.573	-0.898
DMSA	7,918	-0.065	0.036	-0.065
PREC	8,961	6.023	6.164	6.531
PREB	22,467	0.202	0.402	0.000
PFT	22,467	0.326	0.469	0.000
MINC	22,467	0.110	0.313	0.000
FFT	22,467	0.063	0.243	0.000

(The table is continued on the next page.)

TABLE 1: Panel B (continued)

	N	Mean	Std. dev.	Median
TPFO	22,467	0.057	0.231	0.000
NOL	22,467	0.112	0.407	0.000
ΔNOL	18,990	-0.012	0.202	0.000
PPE	22,428	0.453	0.331	0.372
CASH	22,463	0.170	0.179	0.103
IFUND	22,457	0.018	0.142	0.034
CLTD	22,467	0.032	0.059	0.008
MTBA	22,467	2.570	2.399	1.862

Notes: All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Panel A.

TABLE 1: Panel C
Pearson correlations

	ISI	*MTB*	*PTROA*	*ROA*
ISI	1.000			
MTB	-0.038*	1.000		
PTROA	-0.042*	0.396*	1.000	
ROA	-0.029*	0.359*	0.926*	1.000
INVEST	0.275*	-0.008	-0.061*	-0.086*
EXCOMP	0.238*	0.113*	0.146*	0.143*
DISI	0.952*	-0.043*	-0.045*	-0.029*
FISI	0.866*	-0.020*	-0.034*	-0.023*
INTAN	0.154*	-0.122*	-0.151*	-0.158*
OINTAN	0.055*	-0.059*	-0.101*	-0.104*
GWILL	0.162*	-0.114*	-0.127*	-0.133*
RDE	0.026*	0.281*	0.044*	0.090*
HRDIND	0.056*	0.079*	0.024*	0.045*
ANCOV	0.288*	0.130*	0.086*	0.063*
ACTIVE	0.324*	0.089*	0.057*	0.033*
OWNERS	0.331*	0.134*	0.106*	0.078*
LTERM	0.380*	-0.044*	-0.042*	-0.048*
CNTYS	0.433*	0.003	-0.053*	-0.049*
FORGN	0.193*	0.121*	0.279*	0.282*
RDEMIS	-0.055*	-0.101*	-0.009	-0.038*
SIZE	0.291*	0.226*	0.156*	0.116*
LEV	-0.051*	-0.123*	-0.201*	-0.199*
SGROW	-0.074*	0.189*	0.182*	0.185*
CRUPT	0.147*	0.026*	0.013	0.012
GEFF	0.184*	0.024*	0.014*	0.015*
POL	0.238*	0.023*	0.009	0.007
LAW	0.157*	0.026*	0.014*	0.014*
REGQ	0.154*	0.026*	0.016*	0.015*
VOICE	0.112*	0.029*	0.010	0.007
INFL	0.122*	0.006	0.021*	0.016*
GDP	0.180*	0.013	0.008	0.013*
UNEM	0.334*	0.001	-0.018*	-0.006
FETR	-0.085*	-0.048*	-0.020*	-0.065*
LCETR	-0.048*	-0.039*	0.011	-0.040*
IOSF	-0.019*	0.561*	0.331*	0.338*
LUTB	0.189*	0.045*	0.037*	0.027*
TSS	0.200*	0.005	0.031*	-0.012
FTOW	0.025*	-0.087*	-0.070*	-0.054*
ROSDIF	0.027*	-0.112*	-0.245*	-0.237*
DMSO	0.045*	0.085*	0.060*	0.080*
DMSA	0.073*	-0.173*	-0.106*	-0.101*
PREC	0.264*	0.024*	-0.011	-0.014
PREB	0.425*	-0.027*	-0.037*	-0.024*
PFT	0.127*	-0.008	-0.082*	-0.059*
MINC	0.141*	0.006	-0.011	0.005
FFT	0.120*	0.019*	0.021*	0.026*

(The table is continued on the next page.)

TABLE 1: Panel C (continued)

	ISI	*MTB*	*PTROA*	*ROA*
TPFO	0.079*	0.008	-0.032*	-0.022*
NOL	-0.008	0.087*	-0.029*	0.063*
ΔNOL	0.011	-0.030*	-0.053*	-0.091*
PPE	-0.071*	-0.117*	-0.055*	-0.064*
CASH	0.059*	0.305*	0.215*	0.233*
IFUND	0.066*	0.115*	0.243*	0.229*
CLTD	-0.061*	-0.065*	-0.070*	-0.060*
MTBA	-0.047*	0.876*	0.437*	0.398*

Notes: This table reports Pearson correlations between variables. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Panel A. * denotes significance at a *p*-value of 0.05 (two-tailed).

TABLE 2
Firm value tests (Hypothesis 1)

	(1) *MTB*	(2) *MTB*	(3) *MTB*	(4) *MTB*	(5) *ΔMTB*	(6) *ΔMTB*
ISI	**-3.187*****	**-2.251*****	**-2.600*****	**-0.996****		
	(5.18)	**(3.31)**	**(4.06)**	**(2.07)**		
ΔISIEX					**-1.380*****	**-1.243***
					(3.13)	**(1.94)**
ΔISIEN					-0.440	-0.482
					(0.85)	(1.29)
CNTYS	-0.018***	-0.003	-0.015***	-0.006***		
	(5.18)	(0.97)	(4.03)	(2.58)		
FORGN	4.459***	3.734***	-1.247	1.772***		
	(4.70)	(5.20)	(1.01)	(3.19)		
RDE	8.566***	-7.711***	8.882***	-2.147***		
	(5.17)	(6.72)	(3.16)	(2.72)		
RDEMIS	-0.090	-0.174**	0.089	-0.032	0.011	-0.043
	(0.96)	(2.15)	(1.04)	(0.43)	(0.59)	(0.63)
INTAN	-2.340***	-1.914***	-4.120***	-2.902***		
	(8.55)	(10.26)	(9.64)	(12.99)		
SIZE	0.399***	0.207***	0.847***	0.262***		
	(9.38)	(11.47)	(7.69)	(6.31)		
LEV	-0.392	0.224	-0.215	-0.449*		
	(1.16)	(0.75)	(0.81)	(1.92)		
SGROW	0.996***	-0.701***	0.677***	-0.331***		
	(4.22)	(6.45)	(4.47)	(4.88)		
CRUPT	-0.018	-0.027**	-0.027	-0.023		
	(1.30)	(2.10)	(1.63)	(1.61)		
GEFF	-0.037*	0.016	0.016	0.026**		
	(1.73)	(0.83)	(1.28)	(2.19)		
POL	0.003	-0.004	0.005*	0.006		
	(0.81)	(1.18)	(1.74)	(1.52)		
LAW	0.008	-0.009	-0.024	0.012		
	(0.27)	(0.30)	(1.56)	(0.85)		
REGQ	0.023*	0.000	0.006	-0.030***		
	(1.71)	(0.01)	(0.56)	(2.72)		
VOICE	0.020*	0.024**	0.020*	0.016***		
	(1.95)	(2.17)	(1.87)	(2.96)		
INFL	0.089**	0.032	0.080***	0.027		
	(2.47)	(0.83)	(2.92)	(0.86)		
GDP	0.000	0.000	0.000	-0.000*		
	(1.38)	(1.20)	(0.43)	(1.91)		
UNEM	0.013	-0.013	0.035*	-0.023		
	(0.51)	(0.69)	(1.85)	(1.18)		
FETR		-0.125		0.004		
		(1.33)		(0.08)		
LCETR		0.383***		0.261**		
		(2.67)		(2.46)		
IOSF		170.081***		128.818***		
		(13.48)		(12.72)		

(The table is continued on the next page.)

TABLE 2 (continued)

	(1) *MTB*	(2) *MTB*	(3) *MTB*	(4) *MTB*	(5) *ΔMTB*	(6) *ΔMTB*
ΔCNTYS					-0.002	-0.002
					(1.02)	(0.96)
ΔFORGN					-0.420	-0.839***
					(1.29)	(2.67)
ΔRDE					-2.667***	-3.297***
					(3.78)	(4.02)
ΔINTAN					-2.475***	-2.430***
					(10.20)	(9.80)
ΔSIZE					0.286***	0.292***
					(2.68)	(2.79)
ΔLEV					-0.907***	-0.998***
					(5.21)	(6.07)
ΔSGROW					0.002	0.007
					(0.04)	(0.12)
ΔCRUPT					0.005	0.014**
					(0.00)	(2.46)
ΔGEFF					0.014*	0.012
					(1.79)	(1.54)
ΔPOL					-0.000	-0.000
					(0.09)	(0.01)
ΔLAW					-0.007	-0.003
					(0.42)	(0.16)
ΔREGQ					-0.019	-0.024**
					(1.62)	(2.02)
ΔVOICE					0.010	0.003
					(1.05)	(0.37)
ΔINFL					-0.011	0.001
					(0.33)	(0.06)
ΔGDP					-0.000*	-0.000
					(1.77)	(1.16)
ΔUNEM					-0.025***	-0.012*
					(3.67)	(1.67)
ΔFETR					0.051	0.081
					(0.76)	(1.12)
ΔLCETR					0.241***	0.382**
					(2.71)	(2.29)
ΔIOSF					111.313***	108.988***
					(7.25)	(6.52)
Intercept	-0.312	4.624***	-2.765***	4.014***	0.086***	0.119***
	(0.67)	(9.65)	(7.88)	(6.70)	(4.25)	(2.99)
Fixed Effects	I&Y	I&Y	F&Y	F&Y	I&Y	F&Y
Observations	22,467	10,413	22,154	10,064	6,601	6,345
Adjusted R^2	0.244	0.395	0.104	0.138	0.337	0.219

Notes: This table reports the estimates of equation (3). All specifications include either industry and year (I&Y) or firm and year (F&Y) fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 in models with firm fixed

effects (i.e., models 3 and 4) are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE 3
Cross-sectional analysis on governance and myopia variables

	(1) *MTB*	(2) *MTB*	(3) *MTB*	(4) *MTB*	(5) *MTB*
ISI	0.118	-2.807**	2.988	-0.023	-3.059***
	(0.14)	(2.45)	(1.64)	(0.02)	(2.91)
ANCOV	-0.086				
	(1.20)				
ISI×ANCOV	**-1.496*** **				
	(2.83)				
IOWN		-1.045***			
		(5.04)			
ISI×IOWN		**0.907**			
		(0.72)			
OWNERS			-0.312***		
			(3.45)		
ISI×OWNERS			**-1.076*** **		
			(2.61)		
ACTIVE				-0.428***	
				(4.67)	
ISI×ACTIVE				**-1.138***	
				(1.93)	
LTERM					-0.994***
					(4.25)
ISI×LTERM					**2.079**
					(1.28)
Intercept	-2.828***	-3.647***	-3.469***	-3.880***	-3.622***
	(8.05)	(9.01)	(8.48)	(9.54)	(8.96)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	22,154	18,688	18,688	18,688	18,688
Adjusted R^2	0.107	0.134	0.137	0.137	0.133

Notes: This table reports estimates of equation (3), including interactions between *ISI* and proxies for analyst and investor oversight. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 values are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE 4: Panel A
Firm value tests with U.S.–foreign vs. foreign–foreign *ISI*

	(1) *MTB*	(2) *MTB*	(3) *ΔMTB*	(4) *MTB*	(5) *MTB*	(6) *ΔMTB*
DISI	**-3.191*****	**-1.275*****				
	(4.88)	**(2.78)**				
FISI				**-3.127*****	**-0.640**	
				(5.43)	**(1.51)**	
ΔDISIEX			**-1.678****			
			(2.48)			
ΔDISIEN			-0.729*			
			(1.83)			
ΔFISIEX						**-0.965***
						(1.76)
ΔFISIEN						-0.101
						(0.27)
Intercept	-0.268	4.033***	0.136***	-0.358	4.021***	0.128***
	(0.58)	(6.74)	(3.31)	(0.80)	(6.69)	(2.96)
Fixed Effects	I&Y	F&Y	F&Y	I&Y	F&Y	F&Y
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FETR, *LCETR*, and *IOSF*	No	Yes	Yes	No	Yes	Yes
Observations	22,467	10,064	5,905	22,467	10,064	5,896
Adjusted R^2	0.245	0.138	0.226	0.244	0.137	0.237

Notes: This table reports the estimates of equation (3), where we partition *ISI* into incentives to shift income between the U.S. and foreign jurisdictions (*DISI*) and between different foreign jurisdictions (*FISI*). All specifications include either industry and year (I&Y) or firm and year (F&Y) fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 in models with firm fixed effects (i.e., models 2, 3, 5, and 6) are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE 4: Panel B
Split cross-sectional analysis on governance and myopia variables

	(1) *MTB*	(2) *MTB*	(3) *MTB*	(4) *MTB*	(5) *MTB*
DISI	1.097	0.372	3.645**	1.766*	-0.346
	(1.36)	(0.42)	(2.09)	(1.68)	(0.49)
FISI	-1.938	-2.361***	-1.783	-2.971***	-2.421***
	(1.61)	(2.73)	(1.04)	(2.97)	(3.46)
ANCOV	0.044				
	(0.92)				
DISI×ANCOV	**-1.380*** **				
	(3.59)				
FISI×ANCOV	**0.779***				
	(1.38)				
IOWN		-0.490***			
		(2.69)			
DISI×IOWN		**-2.195** **			
		(1.83)			
FISI×IOWN		**2.453** **			
		(1.84)			
OWNERS			-0.121***		
			(3.83)		
DISI×OWNERS			**-0.976*** **		
			(2.75)		
FISI×OWNERS			**0.267**		
			(0.68)		
ACTIVE				-0.183***	
				(3.46)	
DISI×ACTIVE				**-1.459*** **	
				(2.80)	
FISI×ACTIVE				**1.157** **	
				(1.98)	
LTERM					-0.449**
					(2.34)
DISI×LTERM					**-1.772***
					(1.35)
FISI×LTERM					**3.825*** **
					(2.53)
Intercept	-2.623***	-3.549***	-3.539***	-3.654***	-3.546***
	(7.74)	(9.81)	(9.59)	(10.20)	(9.85)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	14,414	12,702	12,702	12,702	12,702
Adjusted R^2	0.067	0.084	0.085	0.085	0.083

Notes: This table reports estimates of equation (3) with interactions between proxies for analyst and investor oversight and both *DISI* and *FISI* included. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses. One-tailed t-tests are used for all bolded coefficients given our directional predictions, and all other t-tests use two tails. *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels, respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 values are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE 5: Panel A
Supplemental analysis: Overinvestment

	(1) *INVEST*	(2) *ΔINVEST*	(3) *INVEST*	(4) *ΔINVEST*	(5) *INVEST*	(6) *ΔINVEST*
ISI	0.380					
	(1.56)					
ΔISIEX		1.057***				
		(3.40)				
ΔISIEN		-0.451**				
		(1.99)				
DISI			0.424**			
			(1.96)			
ΔDISIEX				0.984***		
				(3.57)		
ΔDISIEN				-0.223		
				(1.07)		
FISI					0.631**	
					(2.50)	
ΔFISIEX						1.017***
						(3.91)
ΔFISIEN						-0.348
						(1.46)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FETR, *LCETR*, and *IOSF*	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,472	6,616	10,472	6,164	10,472	6,158
Adjusted R^2	0.272	0.248	0.272	0.255	0.272	0.253

Notes: This table reports the estimates of equation (3), where the dependent variable is replaced with *INVEST*. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

TABLE 5: Panel B
Supplemental analysis: Executive Compensation

	(1) *EXCOMP*	(2) *EXCOMP*	(3) *EXCOMP*	(4) *EXCOMP*	(5) *EXCOMP*	(6) *EXCOMP*
ISI	0.305					
	(1.03)					
ΔISIEX		0.870*				
		(1.77)				
ΔISIEN		0.574**				
		(1.98)				
DISI			0.187			
			(0.86)			
ΔDISIEX				0.301		
				(0.59)		
ΔDISIEN				0.160		
				(0.75)		
FISI					0.137	
					(0.38)	
ΔFISIEX						1.056***
						(2.62)
ΔFISIEN						0.273
						(1.07)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FETR, *LCETR*, and *IOSF*	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,693	5,339	7,693	4,994	7,693	4,982
Adjusted R^2	0.130	0.012	0.130	0.009	0.130	0.012

Notes: This table reports the estimates of equation (3), where the dependent variable is replaced with *EXCOMP*. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

TABLE 5: Panel C
Supplemental analysis: Pre-Tax ROA

	(1) *PTROA*	(2) *ΔPTROA*	(3) *PTROA*	(4) *ΔPTROA*	(5) *PTROA*	(6) *ΔPTROA*
ISI	0.002					
	(0.07)					
ΔISIEX		-0.029				
		(0.85)				
ΔISIEN		0.074**				
		(2.11)				
DISI			0.002			
			(0.15)			
ΔDISIEX				-0.024		
				(0.90)		
ΔDISIEN				0.051		
				(1.46)		
FISI					-0.003	
					(0.11)	
ΔFISIEX						-0.060*
						(1.69)
ΔFISIEN						0.066**
						(2.20)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FETR, *LCETR*, and *IOSF*	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,488	6,631	10,488	6,177	10,488	6,171
Adjusted R^2	0.273	0.251	0.273	0.248	0.273	0.248

Notes: This table reports the estimates of equation (3), where the dependent variable is replaced with *PTROA*. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

TABLE 6
Cross-sectional analysis on intangible assets

	(1)	(2)	(3)	(4)	(5)
	MTB	*MTB*	*MTB*	*MTB*	*MTB*
ISI	-3.514***	-2.390***	-2.671***	-0.851	-1.304**
	(3.81)	(3.25)	(3.34)	(1.29)	(2.16)
INTAN	-4.741***				
	(7.51)				
ISI×INTAN	**5.149****				
	(2.36)				
OINTAN		-2.840***			
		(6.03)			
ISI×OINTAN		**-4.099**			
		(1.08)			
GWILL			-4.199***		
			(6.40)		
ISI×GWILL			**-0.221**		
			(0.10)		
RDE				11.305***	
				(3.43)	
ISI×RDE				**-51.747*****	
				(2.60)	
ISI×HRDIND					**-4.774*****
					(2.70)
Intercept	-2.202***	-2.381***	-2.559***	-3.048***	-2.550***
	(6.48)	(6.73)	(7.40)	(8.57)	(7.16)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	22,156	22,156	23,527	23,525	23,527
Adjusted R^2	0.085	0.048	0.083	0.088	0.057

Notes: This table reports the estimates of equation (3) with interactions between *ISI* and intangible asset proxies included. All specifications include firm and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 values are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE A.1: Panel A
Temporal distribution of *ISI*

Year	*ISI*	*FETR*	*GETR*	*CETR*
1997	0.018	0.320	0.315	0.275
1998	0.018	0.318	0.307	0.286
1999	0.016	0.335	0.324	0.280
2000	0.016	0.328	0.318	0.280
2001	0.014	0.312	0.293	0.288
2002	0.025	0.292	0.289	0.239
2003	0.028	0.291	0.279	0.219
2004	0.029	0.269	0.265	0.205
2005	0.058	0.273	0.280	0.241
2006	0.066	0.258	0.289	0.256
2007	0.070	0.272	0.283	0.268
2008	0.077	0.253	0.278	0.287
2009	0.076	0.244	0.275	0.273
2010	0.084	0.241	0.264	0.228
2011	0.088	0.240	0.257	0.221
2012	0.093	0.232	0.273	0.245
2013	0.099	0.259	0.262	0.261

Notes: This table reports the average of *ISI*, one-year foreign ETRs (*FETR*), one-year GAAP ETRs (*GETR*), and one-year cash ETRs (*CETR*) by year across our sample period.

TABLE A.1: Panel B
Temporal distribution of *ISI*

NAICS industry	*ISI*	*FETR*	*GETR*	*CETR*
23: Construction	0.060	0.266	0.299	0.257
51: Information	0.057	0.276	0.284	0.232
21: Mining, Oil, & Gas Extraction	0.056	0.343	0.325	0.238
33: Other Manufacturing	0.054	0.266	0.267	0.247
31: Food & Apparel Manufacturing	0.052	0.240	0.298	0.268
56: Administrative & Support Services	0.051	0.313	0.316	0.298
32: Wood & Petroleum Products Manufacturing	0.051	0.280	0.272	0.254
54: Professional Services	0.050	0.288	0.315	0.268
53: Real Estate	0.049	0.278	0.301	0.246
48: Transportation	0.048	0.234	0.269	0.196
99: Other	0.047	0.272	0.264	0.229
42: Wholesale Trade	0.046	0.269	0.323	0.302
61: Education	0.044	0.253	0.354	0.344
81: Other Services	0.038	0.315	0.278	0.237
44: Consumer Retail	0.037	0.285	0.344	0.297
49: Couriers & Warehousing	0.035	0.369	0.356	0.281
11: Agriculture, Forestry, & Fishing	0.034	0.250	0.221	0.204
72: Accommodation & Food Services	0.028	0.247	0.278	0.263
45: Miscellaneous Retail	0.028	0.238	0.333	0.305
71: Arts & Entertainment	0.027	0.211	0.297	0.260
62: Health Care	0.009	0.315	0.312	0.262

Notes: This table reports the average of *ISI*, one-year foreign ETRs (*FETR*), one-year GAAP ETRs (*GETR*), and one-year cash ETRs (*CETR*) by NAICS two-digit industry. Industries are ordered by highest to lowest *ISI*.

TABLE A.2
Firm value tests (Hypothesis 1) using the De Simone et al. (2017) measure

	(1)	(2)	(3)	(4)
	MTB	*MTB*	*MTB*	*MTB*
DMSO	**-0.089**	**-0.177****	**-0.013**	**-0.022**
	(1.23)	**(2.36)**	**(0.16)**	**(0.32)**
CNTYS	-0.011***	0.003	-0.014***	-0.004
	(3.01)	(1.18)	(2.58)	(1.21)
FORGN	6.294***	2.762***	0.690	0.627
	(6.34)	(4.15)	(0.82)	(1.17)
RDE	5.766***	-8.225***	3.507***	-1.592*
	(7.25)	(7.57)	(3.49)	(1.78)
RDEMIS	-0.097	-0.069	-0.085	-0.054
	(1.15)	(0.95)	(0.63)	(0.54)
INTAN	-1.501***	-1.932***	-3.575***	-3.126***
	(6.26)	(9.25)	(10.55)	(9.92)
SIZE	0.338***	0.178***	0.872***	0.312***
	(10.08)	(8.52)	(10.12)	(4.37)
LEV	-1.899***	-1.189***	-0.792***	-1.337***
	(5.60)	(4.87)	(2.83)	(6.60)
SGROW	0.751***	-0.567***	0.346***	-0.167***
	(3.55)	(5.34)	(3.42)	(2.84)
CRUPT	-0.033	-0.047*	-0.019	-0.019
	(1.12)	(1.69)	(0.97)	(1.38)
GEFF	0.005	0.054***	0.003	0.028*
	(0.15)	(2.60)	(0.09)	(1.77)
POL	-0.018***	-0.011**	-0.007	0.008
	(3.17)	(2.09)	(1.19)	(1.49)
LAW	0.022	0.005	0.034	0.001
	(0.85)	(0.23)	(1.31)	(0.08)
REGQ	0.015	-0.010	-0.023	-0.020
	(0.48)	(0.45)	(1.35)	(1.54)
VOICE	0.017*	0.020***	0.010	0.013
	(1.94)	(2.89)	(1.08)	(1.59)
INFL	0.099	0.010	0.069	0.031
	(1.55)	(0.18)	(1.55)	(0.84)
GDP	0.000	0.000**	-0.000	-0.000*
	(1.56)	(1.99)	(0.00)	(1.81)
UNEM	0.025	-0.002	-0.014	-0.010
	(0.89)	(0.10)	(0.57)	(0.53)
FETR		-0.029		-0.027
		(0.33)		(0.36)
LCETR		0.221		0.251**
		(1.48)		(2.55)
IOSF		177.734***		127.026***
		(13.16)		(10.24)
Intercept	-2.695***	3.274***	-3.271***	3.190***
	(4.01)	(5.11)	(3.19)	(3.36)

(The table is continued on the next page.)

TABLE A.2 (continued)

	(1)	(2)	(3)	(4)
	MTB	*MTB*	*MTB*	*MTB*
Fixed Effects	I&Y	I&Y	F&Y	F&Y
Observations	7,918	5,932	7,633	5,699
Adjusted R^2	0.311	0.549	0.143	0.339

Notes: This table reports the estimates of equation (3). All specifications include either industry and year (I&Y) or firm and year (F&Y) fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. The adjusted R^2 in models with firm fixed effects (i.e., models 3 and 4) are within firm, thus excluding the explanatory power of firm fixed effects. Variables are defined in Table 1, panel A.

TABLE A.3
Primary firm value tests (Hypothesis 1) with alternate income-shifting measures

	(1) *MTB*	(2) *MTB*	(3) *MTB*	(4) *MTB*	(5) *MTB*	(6) *MTB*	(7) *MTB*
ISI	-2.251***					-1.923**	-1.652**
	(3.31)					(2.33)	(2.20)
DMSO		-0.177**					
		(2.36)					
DMSA			-4.827***				
			(4.35)				
CHQW				0.149**		0.155**	
				(2.30)		(2.43)	
SW					5.024***		5.011***
					(9.22)		(13.77)
Intercept	4.624***	3.274***	2.836***	4.847***	4.088***	5.001***	4.089***
	(9.65)	(5.11)	(4.43)	(12.33)	(8.23)	(12.02)	(8.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FETR, *LCETR*, and *IOSF*	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,413	5,932	5,932	5,853	5,832	5,853	5,832
Adjusted R^2	0.395	0.549	0.553	0.535	0.401	0.536	0.402

Notes: This table reports the estimates of equation (3). All specifications include industry and year fixed effects, and use robust standard errors clustered by firm and year. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

TABLE B.1: Panel A
Validation tests of *ISI* using tangential income-shifting measures

	(1) *FTOW*	(2) *ROSDIF*	(3) *DMSO*	(4) *DMSA*	(5) *PFT*	(6) *MINC*	(7) *FFT*	(8) *TPFO*
ISI	**1.686*****	**0.712*****	**2.607*****	**0.181*****	**1.245*****	**0.627****	**1.556*****	**1.727*****
	(8.65)	**(3.80)**	**(8.53)**	**(8.78)**	**(5.51)**	**(2.22)**	**(4.58)**	**(5.10)**
SIZE	-0.009*	-0.009*	-0.055***	-0.001	-0.024***	0.058***	0.063***	0.001
	(1.73)	(1.96)	(5.63)	(1.53)	(3.43)	(6.11)	(5.72)	(0.07)
LEV	0.358***	0.005	-0.423***	-0.010**	0.256***	-0.016	-0.437***	0.155
	(6.39)	(0.13)	(5.27)	(1.98)	(3.66)	(0.17)	(3.65)	(1.39)
ROA	-3.840***	-1.986***	-0.099	-0.009	-1.123***	-0.677***	-0.607**	-0.648**
	(30.45)	(16.45)	(0.63)	(0.83)	(6.55)	(3.04)	(2.07)	(2.42)
RDE	-0.611***	-0.560***	1.729***	-0.166***	0.573**	0.717**	0.090	0.425
	(3.11)	(3.10)	(5.14)	(6.77)	(2.15)	(2.11)	(0.22)	(1.07)
RDEMIS	0.051***	-0.012	0.021	0.004*	0.013	-0.142***	-0.119***	-0.156***
	(2.67)	(1.04)	(0.64)	(1.88)	(0.46)	(3.75)	(2.68)	(3.49)
NOL	0.018	0.064***	-0.045	0.004	0.072**	-0.006	-0.335***	0.026
	(0.70)	(3.17)	(1.14)	(1.48)	(2.21)	(0.15)	(3.64)	(0.56)
ΔNOL	-0.032	0.032	-0.094**	-0.005	-0.094*	-0.079	-0.102	0.013
	(0.81)	(0.92)	(2.12)	(1.40)	(1.89)	(1.22)	(1.14)	(0.18)
FORGN	10.896***	3.932***	1.039***	0.044**	1.681***	2.502***	3.363***	1.601***
	(63.67)	(23.87)	(3.27)	(1.98)	(6.46)	(7.91)	(9.00)	(4.10)
INTAN	0.213***	0.082**	-0.531***	0.034***	0.156**	-0.420***	0.192	0.555***
	(3.97)	(2.36)	(5.61)	(5.36)	(1.96)	(3.96)	(1.55)	(4.51)
PPE	0.105***	0.025	-0.188***	-0.006	-0.128***	-0.152**	0.084	-0.226***
	(3.25)	(1.10)	(3.20)	(1.49)	(2.73)	(2.38)	(1.12)	(2.82)
CASH	-0.190***	-0.123**	0.146	-0.023***	0.077	-0.041	-0.273*	0.330**
	(3.00)	(2.42)	(1.30)	(2.71)	(0.87)	(0.34)	(1.90)	(2.39)
IFUND	-0.015	0.010	-0.013	-0.015***	-0.009	-0.057	-0.245*	-0.156
	(0.24)	(0.28)	(0.18)	(3.26)	(0.10)	(0.49)	(1.80)	(1.21)
CLTD	-0.311**	0.006	-0.097	-0.014	0.062	0.109	0.166	1.273***
	(2.27)	(0.10)	(0.50)	(1.15)	(0.33)	(0.42)	(0.55)	(4.71)
MTBA	-0.012**	0.005	-0.023*	-0.002**	0.002	-0.021**	-0.002	-0.011
	(2.01)	(1.02)	(1.93)	(2.38)	(0.27)	(2.13)	(0.16)	(0.94)

(The table is continued on the next page.)

TABLE B.1: Panel A (continued)

	(1) *FTOW*	(2) *ROSDIF*	(3) *DMSO*	(4) *DMSA*	(5) *PFT*	(6) *MINC*	(7) *FFT*	(8) *TPFO*
CNTYS	-0.002***	-0.001***	-0.002	-0.000***	0.006***	0.005***	0.005***	-0.001
	(2.86)	(2.87)	(1.62)	(4.18)	(5.15)	(3.57)	(2.99)	(0.44)
IOSF	-6.333***	-1.057	11.214***	0.366*	1.462	1.982	7.004**	4.858
	(3.48)	(0.57)	(3.20)	(1.70)	(0.65)	(0.68)	(1.98)	(1.47)
Intercept	0.699***	-0.008	-0.606***	-0.067***	0.179	-5.559	-1.727***	-1.301***
	(5.38)	(0.10)	(3.15)	(5.76)	(0.98)	(0.06)	(6.17)	(5.02)
Observations	9,553	9,455	6,876	6,876	17,469	17,399	17,399	17,416
Adjusted R^2	0.334	0.322	0.417	0.293	-	-	-	-
Pseudo R^2	-	-	-	-	0.042	0.088	0.079	0.067
Vuong Test *p*-value (*ISI*)	0.024	0.061	0.001	0.001	0.160	0.013	0.001	0.001

Notes: Panel A reports regressions of variables that tangentially capture income shifting on *ISI*, controls, and industry and year fixed effects. Robust regression is used in column (1) given the skewness of *FTOW*; otherwise, OLS is used in columns (2), (3), and (4), while probit regressions are used on the binary dependent variables in columns (5) through (8). We use robust standard errors clustered by firm. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. The Vuong test *p*-value reports the *p*-value of a cluster-robust Vuong test that contrasts the explanatory power of the reported model versus the same model with *ISI* omitted. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

TABLE B.1: Panel B
Validation tests of *ISI* using the Collins et al. (1998) framework

	(1) *FORROS*	(2) *FORROS*
ISI	0.170***	0.191***
	(3.87)	(3.47)
WWROS	0.553***	0.389***
	(26.18)	(14.34)
SIZE		0.006***
		(5.97)
LEV		0.019**
		(2.43)
ROA		-0.254***
		(8.56)
RDE		-0.084***
		(2.73)
RDEMIS		0.003
		(0.68)
NOL		0.007
		(1.50)
ΔNOL		-0.009***
		(2.86)
FORGN		1.454***
		(35.23)
INTAN		0.049***
		(4.81)
PPE		0.010*
		(1.91)
CASH		0.007
		(0.60)
IFUND		0.013
		(1.43)
CLTD		-0.008
		(0.44)
MTBA		-0.003***
		(3.14)
CNTYS		-0.001***
		(5.51)
IOSF		-0.034
		(0.13)
Intercept	0.024	-0.056**
	(1.36)	(2.21)
Observations	12,763	9,554
Adjusted R^2	0.292	0.632
Vuong Test *p*-value (*ISI*)	0.055	0.010

Notes: Panel B reports Collins et al. (1998) regressions, which include *ISI*, industry and year fixed effects, and controls in column (2). We use robust standard errors clustered by firm. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. The Vuong test *p*-value reports the *p*-value of a cluster-robust Vuong test that contrasts the explanatory power of the reported model versus the same model with *ISI* omitted. All continuous variables are winsorized by year at the 1st and 99th percentiles.

Variables are defined in Table 1, panel A.

TABLE B.1: Panel C
Validation tests of *ISI* using PRE and tax aggressiveness

	(1) *PREC*	(2) *PREB*	(3) *TSS*	(4) *LUTB*	(5) *LUTB*	(6) *LUTB*
ISI	**10.775*****	**2.983*****	**0.308*****	**-0.205**	**-0.927***	**0.464**
	(6.25)	**(10.98)**	**(4.49)**	**(0.55)**	**(1.90)**	**(1.05)**
SIZE	0.975***	0.146***	0.097***	0.841***	0.853***	0.816***
	(11.02)	(13.04)	(30.41)	(40.71)	(34.28)	(30.40)
LEV	0.457	-0.005	0.142***	1.168***	1.205***	1.220***
	(0.63)	(0.05)	(7.22)	(7.66)	(6.35)	(6.36)
ROA	-5.970***	-1.514***	-0.209***	-2.467***	-2.370***	-2.184***
	(4.74)	(5.97)	(4.57)	(8.17)	(4.02)	(6.32)
RDE	-2.163	-0.275	-0.136*	5.091***	4.919***	5.318***
	(0.80)	(0.63)	(1.70)	(7.75)	(5.73)	(5.94)
RDEMIS	-0.704**	-0.128***	0.004	-0.011	-0.038	0.035
	(2.15)	(3.10)	(0.44)	(0.16)	(0.47)	(0.42)
NOL	-0.333	-0.228***	-0.045***	0.279***	0.221**	0.292***
	(1.61)	(4.11)	(4.21)	(3.78)	(2.08)	(3.78)
ΔNOL	0.355	0.153*	0.039***	-0.006	-0.040	-0.010
	(1.48)	(1.71)	(3.41)	(0.09)	(0.41)	(0.11)
FORGN	19.266***	3.402***	0.401***	2.346***	1.873***	3.129***
	(7.20)	(9.35)	(5.12)	(4.14)	(2.75)	(3.94)
INTAN	-1.802**	-0.268**	0.093***	-1.113***	-1.420***	-0.918***
	(2.05)	(2.32)	(3.39)	(5.93)	(6.46)	(3.78)
PPE	-0.924*	-0.183**	0.028*	-0.534***	-0.548***	-0.511***
	(1.68)	(2.53)	(1.67)	(4.63)	(3.69)	(3.77)
CASH	-3.170***	-0.616***	0.056*	-0.332	-0.402	-0.384
	(3.31)	(4.36)	(1.88)	(1.64)	(1.55)	(1.58)
IFUND	0.630	0.156	-0.010	0.370**	0.490**	0.011
	(1.07)	(1.12)	(0.63)	(2.17)	(2.32)	(0.04)
CLTD	1.885	0.228	-0.270***	1.017**	0.755	1.803***
	(0.90)	(0.76)	(4.46)	(1.98)	(1.21)	(3.52)
MTBA	-0.056	0.010	-0.012***	-0.063**	-0.070*	-0.043
	(0.74)	(0.90)	(5.29)	(2.39)	(1.73)	(1.61)
CNTYS	0.052***	0.007***	-0.001***	0.013***	0.011***	0.016***
	(3.63)	(4.30)	(3.52)	(4.73)	(3.20)	(5.12)
IOSF	-42.996**	-6.899**	-1.712***	-42.404***	-39.302***	-46.677***
	(2.33)	(1.98)	(2.82)	(7.02)	(5.17)	(5.74)
Intercept	3.257	-1.051***	0.105	-4.655***	-4.720***	-4.369***
	(1.48)	(3.03)	(1.45)	(7.75)	(7.17)	(9.32)
Sample	All	All	All	All	High DA	Low DA
Observations	7,727	10,025	16,881	6,217	2,984	2,984
Adjusted R^2	0.268	-	0.517	0.693	0.716	0.676
Pseudo R^2	-	0.305	-	-	-	-

(The table is continued on the next page.)

TABLE 10: Panel C (continued)

Notes: Panel C reports regressions of PRE and tax aggressiveness variables on *ISI*, controls, and industry and year fixed effects. OLS is used in all columns except column (2), which uses a probit regression for the binary dependent variable in column (2). All columns are run on all available observations, except columns (5) and (6), which partition the Panel C column (4) results into observations with high discretionary accruals, column (5), and low discretionary accruals, column (6), where discretionary accruals are calculated using the Modified Jones model run by corporate life-cycle and year (Dechow et al. 1995; Chang and Li 2015). Robust standard errors clustered by firm are used. Absolute t statistics are reported in parentheses (all based on two-tailed t-tests). *, **, and *** denote statistical significance at the $p < 0.10$, 0.05, and 0.01 levels (two-tailed), respectively. All continuous variables are winsorized by year at the 1st and 99th percentiles. Variables are defined in Table 1, panel A.

Figure A.1 Income-shifting measure strengths and weaknesses

Measure	Calculation	Strengths	Weaknesses
Collins et al. (1998)	The coefficient on a tax incentive variable (e.g., the difference between the average foreign tax rate and the U.S. statutory rate) in a regression with foreign income divided by foreign sales as the dependent variable. Or, more specifically, β_2 in the regression: $FINC/FSALE = \beta_0 + \beta_1 WWINC/WWSALE + \beta_2 RATEDIF$	●Theoretically derived ●Easy to estimate	●Because the measure already specifies both independent and dependent variables, it can only examine income-shifting determinants, which are incorporated through interaction with the tax incentive variable ●Only captures net inbound or outbound income shifting
Dyreng and Markle (2016)	The relative coefficients in regressions of 1) foreign income on foreign sales and U.S. domestic sales and 2) U.S. domestic income on U.S. domestic sales and foreign sales. Estimated using structural equation modeling.	●Theoretically derived ●Can measure both inbound and outbound income shifting ●More flexible than Collins et al. (1998)	●Because the measure already specifies both independent and dependent variables, it can only examine income-shifting determinants, which are incorporated through interaction with the tax incentive variable
De Simone et al. (2017)	The linear combination of income-shifting-related variables based on a model that predicts outbound income shifting per the Form 5471.	●Derived using proprietary IRS data ●Can examine income-shifting consequences ●Has both firm and year variation	●Because the input variables are not specific to income shifting (e.g., R&D, foreign sales, size, capital investment, etc.), the risk of correlated omitted variables is high. For example, regressions of a dependent variable on *DMSO* could result in non-income-shifting portions of R&D, size, etc. driving coefficients. ●As a linear combination of variables, controlling for an input variable effectively removes it from the measure, thus breaking the ability of the measure to capture income shifting (i.e., by reducing the predictive ability of the measure in the first stage prediction model) ●Useful only for U.S.-outbound income shifting

(The table is continued on the next page.)

Figure A.1 (continued)

Measure	Calculation	Strengths	Weaknesses
Chen et al. (2018)	Similar to Collins et al. (1998), as modified by Klassen and Laplante (2012b), except the regression is essentially run by firm, and the firm-specific β_2 coefficient is then used as an independent variable in any other regression.	●Follows Collins et al. (1998) ●Can examine income-shifting consequences	●Lacks good temporal variation, so cannot be used in differences or with firm fixed effects ●Is inherently a function of profitability and foreign growth by construction (i.e., struggles with correlated omitted variables) ●First-stage regression is very small sample (i.e., six or more observations) and thus subject to small-sample biases (Nelson and Kim 1993)
Saavedra and Williams (2017)	The coefficient on foreign sales in a regression with UTBs (or the current increase in UTBs) as the dependent variable. Or, more specifically, β_1 in the regression: $$UTB = \beta_0 + \beta_1\, Foreign\ Sales$$ where the regression is run by industry year and the β_1 coefficient is then used as an independent variable in any other regression.	●Can examine income-shifting consequences ●Has both industry and year variation	●Variation at the industry, rather than firm, level ●Is inherently a function of foreign growth and footprint by construction (i.e., struggles with correlated omitted variables) ●Is inherently a function of UTBs by construction, which are noisy and may contain various tax avoidance, earnings management, and reporting quality confounds (i.e., struggles with correlated omitted variables) ●Assumes that the association between UTBs and foreign operations is solely due to transfer pricing, rather than the presence of non-income-shifting foreign UTBs (Beuselinck and Pierk 2017) or U.S. domestic tax avoidance through a mechanical correlation between foreign and domestic sales and foreign and domestic tax avoidance (Drake et al. 2018)
Huizinga and Laeven (2008)	The product of $\frac{1}{1-\tau}$, where τ is the tax rate on a foreign affiliate, and a weighted average of the tax rate differences that a foreign affiliate faces with other foreign affiliates. Specifically: $$C_i = \frac{1}{1-\tau_i}\frac{\sum_{k\neq 1}^{K}\frac{Sales_k}{1-\tau_k}(\tau_i-\tau_k)}{\sum_{k=1}^{K}\frac{Sales_k}{1-\tau_k}}$$	●Theoretically derived ●Can examine income-shifting consequences of firm subsidiaries, but not of firms	●Subject to Bureau van Dijk data limitations, such as lacking data for subsidiaries in major economies (i.e., the United States and China) or for most tax haven locations ●Measure can only be calculated for European firms ●Measure is at the affiliate level, rather than the consolidated firm level

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Figure A.1 (continued)

Measure	Calculation	Strengths	Weaknesses
De Simone et al. (2018)	The coefficient on Huizinga and Laeven's (2008) measure in a regression with logged ROAs as the dependent variable. Or, more specifically, β_1 in the regression: $\ln(ROA + 1) = \beta_0 + \beta_1 H\&L + \beta_j X$ where the regression is essentially run by firm-year across firm subsidiaries and the β_1 coefficient is then used as an independent variable in any other regression.	●Follows Huizinga and Laeven (2008) ●Can examine income-shifting consequences	●Is inherently a function of foreign subsidiary growth and geographic footprint by construction (i.e., struggles with correlated omitted variables), although some additional controls in the X vector may help to an unknown degree ●First-stage regression is based on very small samples (i.e., two or more observations) as it relates to the coefficient of interest, and thus the measure is subject to small-sample biases (Nelson and Kim 1993) ●Subject to Bureau van Dijk data limitations, such as lacking data for subsidiaries in major economies (i.e., the United States and China) or for most tax haven locations ●Measure can only be calculated for European firms
Demeré and Gramlich (2018)	See above in text.	●Follows Huizinga and Laeven (2008) ●Can examine income-shifting consequences ●Comes along with a strong identification technique to address correlated omitted variable concerns ●Follows Huizinga and Laeven (2008), but can be used at the consolidated firm level ●Can be used along with other rich U.S. data	●Subject to Exhibit 21 data limitations, such as missing data for some tax haven subsidiaries and a greater likelihood of missing data for firms with low accounting and reporting quality (Dyreng et al. 2018), although the Lamont and Polk (2002) identification strategy addresses issues with accounting and reporting quality