Endogeneity and the Economic Consequences of Tax Avoidance

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Abstract

Academic research investigating the economic consequences of tax avoidance is almost always interested in the consequences of intentional, deliberate actions undertaken to reduce taxes relative to income. Therefore, it is crucial that such research distinguishes between intentional and incidental tax avoidance, since failure to do so can create endogeneity concerns and lead to incomplete and incorrect economic inferences. In this paper, we first develop a framework that conceptually defines and distinguishes between intentional and incidental tax avoidance. We highlight that the endogeneity problem arises because intentional tax avoidance is not directly observable. We consider two approaches to mitigating endogeneity concerns and apply these approaches by reexamining two influential studies that investigate the economic consequences of tax avoidance. We show how controlling for past accounting losses eliminates the effect of tax avoidance on credit spreads (Hasan et al. 2014) and how using an instrumental variables approach changes the sign of the relation between tax sheltering and stock price crash risk (Kim et al. 2011). Overall, our paper punctuates the importance of both (1) conceptually distinguishing between incidental and intentional tax avoidance and (2) econometrically addressing the challenges that arise when empirical differentiation between incidental and intentional tax avoidance is important to the research question.

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1 | INTRODUCTION

In this paper, we define intentional and incidental tax avoidance conceptually. We then investigate how failure to adequately and accurately distinguish between these components of tax avoidance may result in endogeneity that alters the findings and economic inferences in empirical research that examines the relation between intentional tax avoidance and economic outcomes. The notion that tax avoidance is a continuum spanning everything from benign tax positions to illegal tax evasion is frequently adopted in the literature (Hanlon and Heitzman 2010). Despite this established convention, researchers examining the economic consequences of tax avoidance are almost always interested in the subset of tax avoidance that is deliberate and purposeful, commonly referred to in the literature as "tax planning," "tax aggressiveness," or "tax sheltering." We refer to this subset of tax avoidance that arises from real or accounting responses to tax incentives as *intentional* tax avoidance. We refer to the other subset of tax avoidance that would have occurred even in the absence of tax incentives as *incidental* tax avoidance.[1]

An important stream of tax literature examines the association between tax avoidance and a variety of economic outcomes, such as a higher cost of capital (e.g, Hasan et al. 2014; Cook et al. 2017) or stock price crash risk (e.g., Kim et al. 2011).[2] This literature largely focuses on the tradeoff between the costs incurred from *intentionally* arranging business affairs to save taxes, and the benefits derived from lower tax payments. In this paper, we assert this research is almost always interested in the economic consequences of intentional tax avoidance. To bolster this assertion, note that the consequences of incidental tax avoidance are largely uninteresting as these same consequences or outcomes would be expected to occur even in the absence of tax incentives

[1] We conceptually define these two mutually exclusive types of tax avoidance in more detail in Section 2.
[2] To gauge the scope of the issues we discuss in this paper, we reviewed articles published in high quality (Financial Times 50 or sectional level) accounting and finance journals for 2006-2022 and identified at least 32 articles that use tax avoidance proxies such as effective tax rates to investigate the effect of tax avoidance on some economic outcome.

(because they arise from incidental sources unrelated to tax incentives). It is the tension surrounding the tradeoff between tax savings from intentional tax avoidance and the potential accompanying costs stemming from the strategic or deliberate actions taken to avoid taxes and detection that make an empirical investigation both compelling and meaningful.

A challenge faced by researchers examining the consequences of tax avoidance is that intentional tax avoidance is not directly observable. For example, recent research suggests that effective tax rates (ETRs), which are commonly used empirical proxies for tax avoidance, seem to capture intentional tax avoidance with significant error (e.g., Drake et al. 2020; Christensen et al. 2022; Schwab et al. 2022). However, these studies do not investigate the endogeneity concerns that may arise when ETRs or other empirical measures are used as noisy proxies for intentional tax avoidance, nor do they explore the extent to which the associated measurement error may alter the inferences that can be drawn from research that examines the effect of tax avoidance on various economic outcomes.

In our paper, we first define intentional and incidental tax avoidance conceptually as mutually exclusive constructs using a mathematical framework. However, no empirical proxy in the literature perfectly distinguishes between these constructs of interest, meaning that empirical proxies for tax avoidance likely measure intentional tax avoidance with error. The construct validity concerns that arise when there is a mismatch between the theoretical construct of intentional tax avoidance and the empirical proxy are a form of measurement error that researchers must thoughtfully address to avoid drawing incomplete or incorrect economic inferences about the consequences of tax avoidance. As a result, in this manuscript, we are focused on research investigating the consequences of tax avoidance; thus, research investigating the determinants of tax avoidance is beyond the scope of our paper.

To help researchers navigate potential endogeneity concerns, we discuss and demonstrate two techniques that researchers might employ when studying the consequences of intentional tax avoidance. First, researchers could include control variables that capture variation in incidental tax avoidance, such that the remaining variation in the tax avoidance proxy arises from intentional sources or from incidental variation that is uncorrelated with the outcome of interest (and thus uncorrelated with a regression's error term).[3] It is evident that myriad economic activities can generate incidental tax avoidance, and including control variables that capture all incidental tax avoidance would be impractical, if not impossible. Overcoming the endogeneity problem does not require control variables that capture all incidental tax avoidance. Accurate inferences can be made if sufficient controls are included so that any remaining variation in the tax avoidance proxy related to incidental avoidance is uncorrelated with the outcome of interest (Armstrong et al. 2022). Thus, this approach is successful to the extent researchers correctly identify all relevant correlated variables so the empirical tax avoidance proxies reliably measure the underlying conceptual constructs while also being careful not to "over control" for factors that could also contain elements of intentional tax avoidance.

Second, researchers could use "quasi-experimental" methods (e.g., instrumental variables, difference-in-differences, and regression discontinuity) to isolate the variation in tax avoidance proxies that arises from intentional tax avoidance, allowing the researcher to draw inferences about the effect of intentional avoidance on some economic outcome that is uncontaminated by incidental tax avoidance. Importantly, this approach is only successful insofar as researchers can

[3] Prior research often implicitly employs this first approach, as evidenced by the variety of control variables intended to capture incidental forms of tax avoidance that are generally included in the research design. For example, researchers commonly include empirical proxies for capital expenditures to capture the incidental tax avoidance that arises when investing in the ordinary course of business, and research and development expense is often included to control for the effect of tax credits, and so forth. However, it is worth emphasizing that a certain magnitude of capital expenditures and research and development expense may reflect incidental tax avoidance while a higher magnitude may also incorporate some intentional tax avoidance.

identify a valid instrument, shock, or stylized setting in which a researcher can exploit exogenous (as-if random) variation in intentional avoidance to identify economic effects.

To illustrate how endogeneity can affect results, we revisit two influential studies that examine the economic consequences of tax avoidance. First, we reexamine Hasan et al. (2014), who show that the cost of private debt is increasing in tax avoidance. The authors argue that the actions taken by the borrower to reduce taxes increase complexity, decrease transparency, exacerbate agency conflicts, and/or increase audit risk, and lenders respond to this increased risk by increasing loan spreads. In the empirical execution of the study, Hasan et al. (2014) use Cash ETR as a proxy for intentional tax avoidance. However, Cash ETR likely captures elements of both incidental and intentional tax avoidance, meaning that variation in Cash ETR arising through incidental factors may drive the relation with credit spreads as opposed to the variation arising from the intentional actions taken to avoid taxes. This seems particularly likely given that recent poor performance, which can manifest as incidental tax avoidance in Cash ETR (Schwab et al., 2022), should affect a borrower's cost of debt because lenders consider past performance when pricing the loan (Jiang 2008; Easton et al. 2009).

We show that after controlling for recent accounting losses the relation between Cash ETR and credit spreads documented in Hasan et al. (2014) disappears, while at the same time, we find that recent accounting losses are positively related to credit spreads.[4] Our findings show that the negative relation between Cash ETR and credit spreads likely reflects past performance, suggesting incidental avoidance rather than intentional avoidance. This implies that Cash ETR

[4] Note that we are not merely interested in controlling for tax losses. Instead, we are concerned with the possibility that past performance can affect both Cash ETR (through tax loss carryforwards) and credit spreads (through banks use of prior performance when pricing the loan). Thus, we control for recent accounting losses to remove the component of Cash ETR that reflects incidental tax reductions due to past losses and is also plausibly associated with the outcome variable of interest through past performance.

may be statistically significant in Hasan et al. (2014) because it is highly correlated with prior firm performance, which is also correlated with credit spreads, the dependent variable. Including recent losses in the model allows the coefficient estimate on Cash ETR to capture the effect of *intentional* tax avoidance that is orthogonal to losses on credit spreads.[5]

Second, we reexamine Kim et al. (2011), who show that tax sheltering is positively associated with stock price crash risk. The authors argue that the opacity created in the act of tax sheltering enables managers to hide bad news from the market. However, at some point, it becomes prohibitively costly or no longer possible to withhold bad news, and the release of the accumulated bad news results in a stock price crash. Kim et al. (2011) use an out-of-sample prediction model developed by Wilson (2009) as the primary empirical proxy for tax sheltering. However, because this proxy is a linear combination of factors that could also capture incidental tax avoidance, it is possible that economic factors associated with incidental tax avoidance drive the observed relation.

Because the tax sheltering measure selected by Kim et al. (2011) is a linear combination of several firm attributes, numerous possible constructs could be correlated with both sheltering and stock price crash risk, making it difficult to identify a sufficient set of control variables that would appropriately mitigate endogeneity concerns. Thus, we employ an instrumental variable approach, to isolate intentional avoidance from incidental avoidance. We propose the existence of significant operations in tax haven countries as a valid instrument for tax sheltering because it is likely to be correlated with tax sheltering, while only being correlated with stock price crash risk through the supposed complexity and opacity associated with tax sheltering activities or through other

[5] We do *not* claim that controlling for past losses accounts for *all* incidental variation in Cash ETR. Instead, we assert that the incidental variation remaining in Cash ETR after controlling for past losses is uncorrelated with the error term in this setting, meaning that any remaining measurement error acts as an uncorrelated omitted variable, and thus is not a threat to bias the coefficients of interest. We discuss the use of control variables for mitigating endogeneity concerns in more detail in Section 5.

variables that we include as controls in the model. To mitigate the econometric concerns surrounding a firm choosing its subsidiary locations, we follow a technique employed by Faulkender and Smith (2016) and Dyreng et al. (2022). First, we use tax haven locations from each firm's first year in our data and fix these locations for all subsequent years for the firm. Thus, tax haven locations are effectively pre-determined and when matched with exogenous variation allow for an effective exposure instrument (Bartik 1991; Breuer 2022; Duguay 2022). To derive plausible exogenous variation, we use the signing of tax information exchange agreements between the U.S. and certain tax haven countries (Bennedsen and Zeume 2018), which we argue should reduce or eliminate the effectiveness of those tax haven countries for sheltering activities, thereby altering a firm's exposure to tax haven countries.

When implementing a two-stage least squares procedure with tax haven exposure as the instrument, we find that the relation between tax sheltering and stock price crash risk is negative, which is the opposite of the relation shown by Kim et al. (2011). Our finding is consistent with research that struggles to find a positive relation between tax sheltering and agency problems or opacity (e.g., Blaylock 2015; Lewellen 2022).

We contribute to the literature on tax avoidance in meaningful ways. First, we create a mathematical framework in which we conceptually define tax avoidance and separate it into two distinct, mutually exclusive components, which we categorize as incidental and intentional. Although others have used these terms to describe different aspects of tax avoidance, our study is the first to define them mathematically, bringing precision and rigor to this research.

Second, our work reveals a gap between the theoretical constructs of intentional and incidental tax avoidance and empirical proxies. Because research examining the economic consequences of tax avoidance focuses primarily on intentional tax avoidance, distinguishing between these two constructs is essential. We highlight how a mismatch between the conceptual construct and empirical proxy affects construct validity and can lead to inaccurate or incomplete economic inferences.

Third, we illustrate through the reexamination of two studies two approaches that are designed to mitigate the endogeneity concerns that arise out of the mismatch between the construct and the empirical proxy. In the first study – Hasan et al. (2014) – we demonstrate that controlling for correlated omitted variables can mitigate endogeneity concerns and alter inferences. In the second study – Kim et al. (2011) – we illustrate that an instrumental variables approach can be used to isolate the effects of intentional avoidance, again resulting in altered inferences relative to the original study. Thus, we show that carefully designed and implemented empirical research is crucial to obtaining meaningful outcomes.

2 | BACKGROUND and THEORY

2.1 | Intentional and Incidental Tax Avoidance

The concept of tax avoidance is only meaningful in the presence of a counterfactual or benchmark (Guenther et al. 2021). How much tax would have been paid if tax had not been avoided is the unobservable counterfactual. Out of empirical convenience, researchers have often defined tax avoidance in terms of observable proxies. For example, Dyreng et al. (2008) define tax avoidance as "the ability to pay a low amount of cash income taxes … relative to corporate pre-tax earnings." This definition implicitly uses pretax income as a proxy for the unobservable counterfactual. Hanlon and Heitzman (2010) define tax avoidance broadly as "the reduction of explicit taxes" implying a comparison to an unobservable benchmark where explicit taxes are not reduced. While empirically convenient, these definitions fall short when examining research questions beyond the scope of the empirical proxy (e.g., studying conforming tax avoidance, separating tax avoidance into components).[6]

To overcome these weaknesses, we define tax avoidance both conceptually and mathematically in the absence of empirical proxies. First, we define the benchmark as the tax that would have been paid if a firm operated in a world with no taxes and then taxes were retroactively applied to output that was generated in the absence of tax incentives. To illustrate this benchmark, consider a representative profit-maximizing firm that invests capital (K) and labor (L) to generate profit (π). The profit generating function can be represented as:

$$\pi = F(K, L) - \rho K - \omega L \tag{1}$$

where output is defined by $F(K, L)$, ρ is the cost of capital, and ω is the cost of labor. The firm will choose the optimal capital (K^*) and labor (L^*) such that profit is maximized (π^*).[7] This first-best output of the firm forms the basis of the counterfactual that we use to define total tax avoidance. The benchmark is $F(K^*, L^*)\tau$, where τ is the statutory corporate tax rate. Note that this benchmark requires retroactive application of the tax because if the tax had been known ex-ante, the firm's optimal capital and labor investment might have been altered.

With the benchmark defined, tax avoidance can also be defined. Total tax avoidance is anything that reduces tax payments relative to the benchmark $F(K^*, L^*)\tau$.[8] Now suppose a tax τ is introduced on output, $F(K, L)\tau$, while tax deductions are granted for the costs of inputs, $\tau(\rho K + \omega L)$, and the tax is known ex-ante. After-tax profit will now be defined by the following:

$$\pi_{aftertax} = (1 - \tau)[F(K, L) - \rho K - \omega L]. \tag{2}$$

[6] See Blouin (2014) for additional discussion about the shortcomings of empirical proxies for tax avoidance.
[7] The model represented in Eq. (1) captures economic output and does not feature an accounting system. Although, prior research indicates that accounting systems capture economic output with some error (e.g., Sloan 1996; Dechow and Dichev 2002; Richardson et al. 2005), for interpretive purposes one can think of output [$F(K, L)$] as being similar to accounting revenue and profit [π] as being similar to accounting net income.
[8] In other words, total tax avoidance is the difference between the observed tax burden and a counterfactual tax burden in which tax is imposed retroactively on firm output (i.e., revenues) without deductions.

The firm will choose the optimal capital (K^*) and labor (L^*) such that profit is maximized ($\pi^*_{aftertax}$). Because the firm receives a full tax deduction for the economic costs of capital and labor, only profits generated over and above the required rate of return are taxed. This type of tax, also known as a pure profits tax (Stiglitz 1976), or a tax on economic rents, does not distort economic behavior because the firm can generate maximum after-tax profit by choosing the same capital and labor in a world with no taxes.. Nevertheless, tax is "avoided" because tax payments, which are equal to $\tau[F(K^*, L^*) - \rho K^* - \omega L^*]$, are lower than the benchmark $F(K^*, L^*)\tau$. Because the firm did not alter its behavior to achieve these tax savings, we label this form of tax avoidance as "incidental." That is, incidental tax avoidance is a reduction in tax payments relative to the benchmark $F(K^*, L^*)\tau$ that arises without any intentional action on the part of the firm and is equal to $(\rho K^* + \omega L^*)\tau$. Thus, tax deductions granted by governments to firms on the economic costs of capital and labor investments at the first-best level are defined as incidental tax avoidance.

The tax system represented in Eq (2) above is not representative of real-world tax systems because real-world tax systems rarely grant deductions exactly equal to the economic costs of inputs. For example, certain types of compensation are not deductible (e.g., §162(m) executive compensation) and tax deductions for capital investment may not reflect the true economic cost of that capital. Indeed, the deduction granted for investment in capital or labor could be more or less than the true economic cost.

To capture the imperfections in real-world tax systems, suppose the government does not give deductions exactly equal to the economic costs of capital and/or labor. In this scenario, the after-tax profit will now be given by the following:

$$\pi_{aftertax} = (1-\tau)[F(K, L) - \eta\rho K - \theta\omega L] - (1-\eta)\rho K - (1-\theta)\omega L \tag{3}$$

The firm will choose the optimal capital (K^+) and labor (L^+) such that profit is maximized ($\pi^+_{aftertax}$). Optimal capital (K^+) and optimal labor (L^+) need not be equal to the optimal capital (K^*) and labor (L^*) that are derived from Eq. (2). Indeed, distortions arise in capital and labor because the government is subsidizing or penalizing one type of economic input investment over another as long as ρ and/or ω is not equal to one.[9] Tax is avoided because tax payments, which are equal to $\tau[F(K^+, L^+) - \rho K^+ - \omega L^+]$ are lower than the benchmark $F(K^*, L^*)\tau$. That is, total tax avoidance is given by:

$$F(K^*, L^*)\tau - \tau[F(K^+, L^+) - \rho K^+ - \omega L^+]. \tag{4}$$

Because the firm has "intentionally" altered its behavior to address the tax preferences offered or penalties imposed by the government, tax avoidance now has two components. Rearranging terms, the two components are given by:

Incidental: $\tau\eta\rho K^* + \tau\theta\omega L^*$, (5)

Intentional: $\tau[F(K^*, L^*) - F(K^+, L^+) + \eta\rho(K^+ - K^*) + \theta\omega(L^+ - L^*)]$. (6)

Intuitively, incidental tax avoidance captures the difference in the tax burden relative to the counterfactual that arises because the government grants tax deductions associated with the costs of investments as if the investment had been made at the first-best level. Alternatively, intentional tax avoidance captures differences in the tax burden relative to the counterfactual that arises from (1) any additional deductions the government grants because the levels and types of investment changed, and (2) changes in the level of output due to changes in levels and types of investment.

Applying our framework to a real transaction, let us consider the case where a firm accelerates the purchase of an asset in order to claim bonus depreciation or §179 immediate

[9] We describe the parameters (η) and (θ) as capturing governmental subsidization or penalization of certain investment activities by altering the deductibility of one investment relative to another. One can also think of these parameters as capturing timing differences in permitted deductions under the tax system.

expensing. In this case the value of the deductions that are accelerated from purchasing and placing the asset into service early relative to the value of the deductions that would have been claimed if the purchase had not been accelerated is intentional avoidance. The value of the deductions that would have been claimed if the asset had been purchased on a regular timeline is considered incidental avoidance.

Thus far, we have not considered how accounting choices may affect the tax burden (for example, via income shifting using transfer pricing techniques). To incorporate this possibility, we can modify Eq. (3) resulting in the following:

$$\pi_{aftertax} = (1 - (\tau - A))[F(K, L) - \eta\rho K - \theta\omega L] - (1 - \eta)\rho K - (1 - \theta)\omega L - c(A) \quad (7)$$

where A represents the accounting technology employed to reduce the tax bill, and $c(A)$ captures the cost of implementing the tax avoiding technology. The firm will now select the optimal capital ($K^{\ddagger}$), labor ($L^{\ddagger}$), and avoidance ($A^{\ddagger}$) such that profit is maximized ($\pi^{\ddagger}_{aftertax}$). Total tax avoidance will now be given by:

$$\tau[F(K^{*}, L^{*})] - (\tau - A^{\ddagger})[F(K^{\ddagger}, L^{\ddagger}) - \rho K^{\ddagger} - \omega L^{\ddagger}] - c(A^{\ddagger}) \quad (8)$$

Although Eq. (8) considers a firm's investment in accounting technology to reduce its tax burden, we can still define tax avoidance as comprising two components:

Incidental: $\tau\eta\rho K^{\ddagger} + \tau\theta\omega L^{\ddagger}$ (9)

Intentional: $\tau[F(K^{*}, L^{*}) - F(K^{\ddagger}, L^{\ddagger})] + [A^{\ddagger}F(K^{\ddagger}, L^{\ddagger}) - \eta\rho K^{*} - \theta\omega L^{*}] + \tau\eta\rho K^{*} + \tau\theta\omega L^{*} - (\tau - A^{\ddagger})\eta\rho(K^{\ddagger} - K^{*}) - (\tau - A^{\ddagger})\theta\omega(L^{\ddagger} - L^{*}) - c(A^{\ddagger})$ (10)

From Eq. (9) we see that incidental tax avoidance continues to capture any differences in the tax burden relative to the counterfactual that arises because the government grants tax deductions associated with the costs of investments as if the investment had been made at the first-best level. Intentional tax avoidance represented in Eq. (10) includes differences in the tax burden relative to the counterfactual that arises from tax savings due to accounting technology, in addition to (1) any additional deductions the government grants because the levels and types of investment changed, and (2) changes in the level of output due to changes in levels and types of investment. Note that relative to Eq. (6), the levels and types of investment may also be affected by the tax avoidance technology.

Our model can be applied to any empirical situation to understand whether that situation gives rise to incidental tax avoidance, intentional tax avoidance, or both. The key distinguishing characteristic between incidental and intentional tax avoidance is whether the tax savings arose because of managerial action (either real or accounting) that would have been taken in the absence of tax incentives. If the tax savings arise from action that would have taken place in the absence of tax incentives, the savings are incidental. If the tax savings arise from actions that were influenced by tax incentives, then the savings are intentional (See Appendix 1 in the Online Appendix for additional details). Naturally, many actions will be influenced at the margin, and thus, actions may have components of both incidental and intentional tax savings.

Relating our model to empirical research, researchers investigating the consequences of tax avoidance are almost always interested in tax avoidance that arises from firms' (or managers') intentional actions, whether real (arising through choices of K or L) or accounting (arising through the choice of A) in nature, aimed at reducing the tax burden. However, a perplexing problem arises because it is unlikely any empirical proxy perfectly captures intentional tax avoidance, as we have defined it, or perfectly distinguishes intentional avoidance from incidental avoidance. Moreover, the conceptual notion of first-best investment and output is unobservable in the data. This inherently results in a mismatch between the construct of intentional tax avoidance and its empirical proxies, which creates construct validity concerns that affect researchers' ability to draw accurate economic inferences about the consequences of intentional tax avoidance.

2.2 | Endogeneity Concerns when Examining the Consequences of Tax Avoidance

In this study, we focus on research investigating the consequences of tax avoidance for two reasons.[10] First, research investigating the consequences of tax avoidance is more likely to be focused exclusively on *intentional* tax avoidance relative to research on the determinants of tax avoidance.[11] Second, the endogeneity concerns that arise from not distinguishing between incidental and intentional tax avoidance are likely more problematic when examining the consequences of tax avoidance than when examining the determinants of tax avoidance.[12]

Research has investigated numerous consequences of tax avoidance such as its effect on firm value (Desai and Dharmapala 2009), credit spreads (Hasan et al. 2014), and executive reputations (Gallemore et al. 2014).[13] This research often draws motivation from theory that suggests the complex corporate structures that often facilitate tax avoidance may create opacity, information asymmetry (e.g., Balakrishnan et al. 2019), and other agency problems that may increase firm risk, such as information risk, and even, in some cases, enable managers to

[10] For an overview about the determinants of tax avoidance see Wilde and Wilson (2018); Belnap et al. (2023).

[11] The effects of incidental tax avoidance are largely uninteresting since these same outcomes would be expected to occur in the absence of a tax system. Alternatively, we believe it is reasonable, for researchers to be interested in the determinants of both incidental and intentional tax avoidance (or be indifferent to distinguishing between the determinants of each respective form of tax avoidance). For example, researchers may be interested in how economic conditions, or a given policy are related to effective tax rates (a commonly used proxy for tax avoidance) but are not particularly concerned about *how* a firm arrives at a given effective tax rate.

[12] This is because when examining the consequences of tax avoidance, the tax avoidance proxy appears as an explanatory (or independent) variable in a regression model. To the extent there are differences between the conceptual construct and an empirical proxy, which is likely the case for most tax avoidance research, this will result in measurement error (Jennings et al. 2024). From an econometric standpoint, measurement error, which is a form of endogeneity can lead to biased inferences when correlated with observed or unobserved explanatory variables (Roberts and Whited 2013). Importantly, as Wooldridge (2010, pg. 73) notes, "… traditionally, measurement error in an explanatory variable has been considered a much more important problem than measurement error in the response variable."

[13] Consistent with most tax avoidance research, we focus on non-conforming tax avoidance, which is tax avoidance that does not reduce pretax financial statement earnings. Researchers have also studied conforming tax avoidance (e.g., Badertscher et al. 2019), that is, tax avoidance that reduces both taxes paid and pretax financial accounting income, which is beyond the scope of this study.

expropriate wealth from the firm (Desai and Dharmapala 2006; Desai et al. 2007).[14] The notion that tax avoidance is associated with opacity or risk is inherently connected to intentional tax avoidance because a firm lacks the incentive to conceal or avoid detection from actions associated with incidental tax avoidance since such actions do not alter outcomes relative to what would exist in the absence of taxes. It is important to note that even if managers do not deliberately seek to conceal tax avoidance strategies, the structures frequently required to implement successful tax avoidance strategies in a world with complex tax laws can nevertheless result in opacity (Balakrishnan et al. 2019). If structures causing opacity are created in response to tax incentives, the opacity reflects intentional avoidance. If the opacity would have surfaced absent tax incentives, then it is not caused by intentional avoidance, and such a setting would likely be uninteresting to a tax researcher. Thus, from a conceptual perspective, we assert that research investigating the consequences of tax avoidance is almost always interested in the consequences of intentional tax avoidance, and researchers should strive to empirically distinguish between intentional and incidental tax avoidance.

A common approach for distinguishing between intentional and incidental tax avoidance is the use of control variables. For example, researchers investigating the consequences of tax avoidance often control for factors that are correlated with incidental avoidance, such as R&D expenditures or firm leverage, likely to try and better identify and separate the effects of intentional

[14] Expropriation could take many forms such as pecuniary benefits for managers (e.g., TYCO or Enron), empire building through suboptimal acquisitions (Hanlon et al. 2015; Edwards et al. 2016), and the increased ability to manipulate firm performance (Dyreng et al. 2012; Durnev et al. 2017), which might lead to higher executive compensation (Core et al. 1999; Black et al. 2014), managerial shirking (Bertrand and Mullainathan 2003), and other opportunistic actions. Not all research investigating the consequences of tax avoidance relies on that notion that tax avoidance engenders managerial opportunism. For example, Donelson et al. (2022) investigate the effect of tax avoidance on tax litigation risk. In doing so they suggest that tax avoidance may create opacity which increases tax litigation risk. However, their theoretical motivations do not entertain the notion that the relation between tax avoidance and increased tax litigation risk is due to agency problems or managerial opportunism that arises because of the opacity created in connection with tax avoidance activities.

avoidance from those of incidental avoidance.[15] The objective of including controls is to account (or control) for incidental factors that may be correlated with both the tax avoidance proxy and the dependent variable. This is because any incidental factor that is unaccounted for in the model and is correlated with both the tax avoidance proxy (e.g., Cash ETR) and the dependent variable, will result in the error term being correlated with the independent variables, which is the conceptual definition of endogeneity (See Appendix 2 in the Online Appendix for more details). Endogeneity leads to biased, inconsistent coefficient estimates. However, if the incidental factors that are related to the outcome variable are adequately controlled, the coefficient on the tax avoidance proxy will capture only variation in the outcome variable associated with intentional tax avoidance, and any variation associated with incidental tax avoidance will be uncorrelated with the error term.[16]

Which control variables are relevant and necessary for the aforementioned approach to be successful depends on the underlying research question. Controls are effective at mitigating bias associated with endogeneity if 1) any remaining omitted (or uncontrolled) incidental factors are uncorrelated with the included regressors, and 2) the control variables adequately measure the underlying economic constructs for which the researcher is trying to control.

Quasi-experimental methods (e.g., instrumental variables, difference-in-differences, and regression discontinuity) represent a second approach that researchers can employ to try and isolate

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[15] Notably, many of the same factors that researchers may often associate with incidental factors, such as R&D expenditures or leverage, may also capture intentional tax avoidance depending on the managerial intent that gave rise to values observed by the researcher. For example, any changes that a firm makes to the timing or magnitude of R&D expenditures in response to tax incentives that in turn affect the firm's taxes relative to a benchmark would qualify as intentional tax avoidance within our conceptual framework.

[16] Researchers may also seek to identify a variable that captures only intentional avoidance. For example, sheltering measures (e.g., Wilson 2009; Lisowsky 2010) and DTAX (Rego and Wilson 2012) are intended, conceptually, to capture more aggressive forms of tax avoidance (i.e., intentional avoidance). However, our evidence suggests these proxies likely still capture components of incidental avoidance (see Table 3). Furthermore, researchers using these proxies still typically control for constructs that may be correlated with incidental avoidance, suggesting an understanding that these proxies are not entirely effective at isolating intentional avoidance. Thus, we do not consider this a separate strategy from isolating intentional avoidance through the use of control variables.

the effects of intentional avoidance from that of incidental avoidance. These methods require a researcher to identify a valid instrument, shock, or stylized setting that allows a researcher to exploit exogenous (as-if random) variation in intentional avoidance to identify its effect on some economic outcome. For example, to implement an instrumental variable approach, a researcher must identify an instrument that is correlated with intentional tax avoidance yet is only correlated with the outcome measure through intentional tax avoidance.

In subsequent sections, we show how these two approaches can be applied to settings where researchers are interested in the economic consequences of intentional tax avoidance while available proxies of intentional tax avoidance are plausibly contaminated by incidental tax avoidance. To do this, we select settings from two published papers and demonstrate that some conclusions drawn are questionable due to endogeneity concerns. The purpose of this exercise is not to overturn the entirety of results documented in the papers, but to urge caution in future research. In our first example, we reexamine the main finding from Hasan et al. (2014), who show that credit spreads from private loans are increasing in the level of tax avoidance where the proxy for tax avoidance is Cash ETR. In the second example, we reexamine the primary findings from Kim et al. (2011), who show that tax sheltering affects stock price crash risk, where the proxy for tax sheltering is the shelter score following Wilson (2009). In both settings, we show that the economic inferences drawn in prior research are sensitive to endogeneity concerns.

3 | MITIGATING ENDOGENEITY VIA CONTROL VARIABLES: REEXAMINATION OF Hasan et al. (2014)

In many cases, researchers can identify variables that capture incidental tax avoidance and may also correlate with the economic outcome of interest. This is why studies investigating the consequences of tax avoidance include control variables (such as R&D expenditures, leverage, firm size, etc.) that are designed to capture incidental avoidance that arises through ordinary business operations and is not tax motivated.[17] To illustrate how failure to control for incidental factors that are correlated with both the economic outcome of interest and the tax avoidance proxy used can affect research inferences, we reexamine the main result from Hasan et al. (2014), who show that tax avoidance is associated with higher credit spreads in private loans.[18]

Hasan et al. (2014) propose that tax avoidance can lead to higher credit spreads because tax avoidance may be associated with increased opacity, which could in turn exacerbate agency problems. These agency concerns prompt creditors to respond by price protecting or increasing the price charged on newly issued loans. For a set of loans from 1985 to 2009, the authors show that tax avoidance is associated with larger credit spreads. The authors interpret their findings as evidence that lenders charge higher interest rates to compensate for the increased risk exposure associated with tax avoidance (Desai and Dharmapala 2006).

In Hasan et al. (2014), the authors are conceptually interested in the effect of *intentional* tax avoidance on credit spreads. However, because one way the authors empirically capture tax avoidance is by using Cash ETR, it is also plausible that the association between Cash ETR and credit spreads is driven by *incidental* avoidance, particularly prior losses, since past performance affects loan pricing (Jiang 2008; Easton et al. 2009) and past performance is shown to affect

[17] It is worth reiterating that these commonly used variables to control for incidental tax avoidance may very well include intentional tax avoidance actions taken by managers. This means control variables could reduce a researcher's ability to detect an effect of intentional tax avoidance. Therefore, control variables must be thoughtfully considered for each research question instead of merely occupying a spot on the go-to list of control variables from prior research.

[18] Hasan et al. (2014) are not alone in proposing and documenting evidence consistent with a positive relation between tax avoidance (or tax rate volatility) and cost of debt (e.g., Saavedra 2019; Shevlin et al. 2020). To be clear, we do not seek to suggest that such a relation does not exist. Our primary objective is to illustrate that for research investigating the consequences of tax avoidance, endogeneity concerns can arise when there is a disconnect between the conceptual construct and a researcher's empirical proxy (Jennings et al. 2024). For our purposes, Hasan et al. (2014) is an illustrative example where this disconnect is present and compounded by the existence of a correlated omitted variable.

effective tax rate measures (Drake et al. 2020; Christensen et al. 2022; Schwab et al. 2022).[19] Therefore, to investigate the effect of intentional tax avoidance on credit spreads when using Cash ETRs to proxy for avoidance, it is vital to control for incidental forms of avoidance, such as prior losses, that are likely correlated with Cash ETRs and the outcome of interest (i.e., credit spreads).

To illustrate the potential underlying issue, we compose a sample of firm-year observations from 1989 to 2020 with positive pretax earnings for the current year since most studies that use ETR measures discard observations with current pretax losses. Table 1 shows our sample selection procedure. We begin the sample in 1989 because the statement of cash flows, which reports taxes paid, became mandatory for fiscal years beginning after July 15, 1988. After excluding observations with negative pretax income or missing information about taxes paid, we retain 65,907 unique firm-year observations representing 7,824 unique firms.

In Table 2 Panel A (Panel B) we report summary information about the relation between *Cash ETRs* (*GAAP ETRs*) and the frequency of losses a firm has incurred in the previous five years. We find that the mean and median value for both *Cash ETR* and *GAAP ETR* decrease monotonically with the frequency of losses incurred. This aligns with research suggesting variation in ETRs is strongly correlated with past performance (Christensen et al. 2022; Schwab et al. 2022). Figure 1 shows that this relation between ETRs and prior performance is persistent throughout the period from 1989 to 2020. In Table 3, we show that tax avoidance proxies other than effective tax rate measures, such as book-tax differences, also exhibit a monotonic relation with prior losses, suggesting the phenomenon documented in Table 2 is not unique to effective tax rate measures.

[19] Measurement error associated with effective tax rate measures (a common proxy for tax avoidance) may arise from numerator effects or denominator effects (e.g., Edwards et al. 2021; Guenther et al. 2021), or both. For example, Edwards et al. (2021) suggest that growth in pretax income, the typical denominator for effective tax rate measures, can cause effective tax rates to decrease which some may interpret as "tax avoidance", yet Edwards et al. (2021) suggest the decline may be attributable to an increase in pretax income."

Evidence that ETRs are correlated with the frequency of prior losses suggests ETRs capture some element of incidental avoidance since it is unlikely that publicly traded firms purposefully and persistently operate at a loss merely to avoid taxes.[20] Thus, carefully controlling for incidental tax avoidance forces, such as prior losses, is crucial to drawing correct inferences, especially when the incidental portion of ETRs may be correlated with the economic outcome of interest, as is likely the case with losses and the cost of borrowing.

Next, we replicate the primary result from Table 2, Column 3 of Hasan et al. (2014) and then extend their analysis by controlling for recent losses in the past five years.[21] We report these results in Table 4. In Column 1 we report our replication of the primary result in Hasan et al. (2014) without control variables.[22] The coefficient on *Cash ETR* is −0.304 and statistically significant ($p<0.01$), suggesting credit spreads are negatively (positively) related to *Cash ETR* (tax avoidance).[23] Column 2 reports our replication of the main result from Hasan et al. (2014) when including controls for borrower and loan characteristics. The coefficient on *Cash ETR* is −0.098 and statistically significant ($p<0.05$). A coefficient estimate of −0.098 suggests that a one percentage point increase (decrease) in tax avoidance (*Cash ETR*) is associated with a 0.098 percent increase in loan spreads.[24] Consistent with Hasan et al. (2014), we remove observations with *current-year* losses due to challenges with measuring effective tax rates for loss observations.

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[20] It is well known that firms will strategically reduce income temporarily for tax purposes (e.g., Maydew 1997), but we are unaware of any research that suggests firms strategically report losses over periods of many consecutive years.
[21] We use the term "replicate" to describe our reexamination of Hasan et al. (2014) and Kim et al. (2011) because in most cases we seek to adopt the same design, model and sample period as the original authors (Hail et al. 2020).
[22] Although Hasan et al. (2014) report that their sample period is 1985-2009, Cash ETR is not calculatable for most firms until the cash flow statement became mandatory beginning in 1988.
[23] In Hasan et al. (2014) the authors multiply Cash ETR by negative one. For interpretive purposes, we do not adjust the sign of Cash ETR. Therefore, the signs for the coefficient estimates on Cash ETR will differ between our results and those reported in Hasan et al. (2014).
[24] In untabulated results, we find that the average credit spread is 156 basis points. Thus, a one standard deviation change in Cash ETR (23 percentage points) is associated with an approximately 3.5 basis point change in credit spreads (3.51=0.0098*156*23). Hasan et al. (2014) report a 4.87 basis-point economic significance of their primary results, which is similar to the magnitude of results from our replication.

However, a remaining concern is that a firm's current *Cash ETR* is affected by *prior* losses, which likely are also associated with credit spreads, thereby affecting the inferences one can draw about the true relation between intentional tax avoidance and credit spreads.

To determine the effect of past losses on the relation between intentional tax avoidance and credit spreads, we regress credit spreads on *Cash ETR* when controlling for past losses by including *Loss5*, an indicator variable equal to one if a firm incurred a pretax loss in any of the previous five years in the regression model and report the results in Column 3.[25] After controlling for recent losses, the coefficient on *Cash ETR* remains negative (−0.034) yet is statistically insignificant ($p>0.1$), indicating that we fail to find a statistically significant relationship between intentional tax avoidance and credit spreads. Alternatively, the coefficient on *Loss5* is 0.153 and statistically significant ($p<0.01$), suggesting that the incidence of a recent prior loss is associated with a 16.5 percent increase in credit spreads, all else equal.[26] In Column 4 we replace *Loss5* with *Loss5%*, which is the percentage of years in the previous five years that a firm incurred a pretax loss, and find results consistent with those in Column 3.

To provide more direct evidence that the inclusion of past losses *changes* economic inferences from the original model, we examine the change in the coefficient on *Cash ETR* after including past losses. We find that coefficient estimates on *Cash ETR* in Columns 3 and 4 are statistically and economically different from the coefficient estimate on *Cash ETR* in Column 2 ($p<0.01$). In particular, the coefficient on *Cash ETR* decreases from -0.098 in Column 2 to -0.034 and -0.031 in Columns 3 and 4 respectively, indicating a reduction in the economic magnitude by

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[25] We focus on the five-year period following Drake et al. (2020), who provide evidence that loss history over the previous five years is related to ETRs.
[26] The 16.5 percent increase is computed as $exp(0.153) - 1 = 0.1653$. In untabulated results, we find the average credit spread is 156 basis points, therefore, the incidence of a loss in the previous five years is associated with a 25 basis points increase in credit spreads.

between 65 and 68 percent in addition to the coefficient on *Cash ETR* becoming insignificant in Columns 3 and 4.[27] Thus, based on the results in Column 2, concluding that intentional tax avoidance creates opacity which results in higher credit spreads seems inaccurate, as the underlying relation between prior losses and *Cash ETR* appears to drive this result.[28]

4 | MITIGATING ENDOGENEITY VIA AN INSTRUMENTAL VARIABLES APPROACH

In certain instances, it may not be possible for a researcher to identify all potential correlated omitted variable(s) when investigating the effect of intentional tax avoidance on an economic outcome. This is because it may not be clear what the correlated omitted variable (or variables) are, or it may not be possible to adequately measure the correlated omitted construct(s) with available data. Thus, an alternative method for distinguishing the effects of intentional avoidance from incidental avoidance is to use quasi-experimental methods to identify exogenous variation in intentional tax avoidance (Armstrong et al. 2022). To implement such an approach, a researcher seeks to isolate variation associated with intentional avoidance from variation associated with incidental avoidance, thereby allowing a researcher to cleanly identify the effect of intentional avoidance on an economic outcome. While instrumental variables represent a specific case of these quasi-experimental methods, the overarching conceptual objective is similar across methods – identifying as-if random variation in the explanatory variable.

[27] We do not find a significant increase in the R-squared of the model after including Loss5 or Loss5%. This suggests that other variables already included in the model largely capture variation from this omitted variable, which helps clarify that the problem is a *correlated* omitted variable rather than model misspecification due to an omitted variable.
[28] In untabulated results, we replace our measures for recent losses (Loss5 and Loss5%) with indicators for negative retained earnings (e.g., Christensen et al. 2019) and high predicted NOL carryforward (e.g., Heitzman and Lester 2021). When including these alternative measures, we continue to observe a negative and statistically significant coefficient ($p<0.01$) on Cash ETR, suggesting that, relative to these alternative measures, recent losses better capture aspects of incidental avoidance and related performance factors.

4.1 | Illustration of an Instrumental Variables Approach to Mitigating Endogeneity Concerns

To implement an instrumental variable approach in a research setting investigating the consequences of tax avoidance, a researcher must identify a variable that is correlated with intentional tax avoidance, while only theoretically correlated with the economic outcome of interest through intentional tax avoidance. Identifying such a variable is often not straightforward, which may explain why this approach is infrequently used in the tax avoidance literature.[29]

To illustrate how an instrumental variables approach may be used to separate the effects of intentional avoidance from incidental avoidance on an economic outcome, we reexamine Kim et al. (2011), who show that, for a sample of firms from 1995 to 2008, tax sheltering, a form of intentional tax avoidance, is positively associated with stock price crash risk. Kim et al. (2011) argue that intentional tax avoidance creates complex, opaque structures that foster agency problems such as managerial opportunism or resource diversion (Desai and Dharmapala 2006, 2009). The opacity enables managers to conceal bad news; however, the accumulation of bad news eventually reaches a "tipping point" where the bad news can no longer be concealed and the release of the accumulated bad news causes a stock price crash.

While tax avoidance activities can span a continuum from investing in municipal bonds to aggressiveness, sheltering, or outright tax evasion (Hanlon and Heitzman (2010), Kim et al. (2011) argue that their conceptual underpinnings align more closely to the sheltering end of the continuum. Because of this, Kim et al. (2011) suggest that the *SHELTER* measure from Wilson (2009) is "the most suitable measure for our research question" (pg. 640). Thus, when re-examining the results from Kim et al. (2011), we focus exclusively on the *SHELTER* measure.

[29] One notable paper that does use this approach is Chen et al. (2018), who use a simultaneous system of equations with a two-stage least squares technique to investigate the effect of income shifting, a particular form of tax avoidance, on information asymmetry.

The *SHELTER* measure is an out-of-sample prediction model developed by Wilson (2009). This prediction model is a linear combination of the following factors: leverage, total assets (size), the occurrence of foreign income, R&D expense, discretionary accruals, return on assets, and book-tax differences. Wilson (2009) developed the measure by demonstrating that the prediction model was correlated with known occurrences of sheltering activity, suggesting the measure is associated with intentional tax avoidance activities. However, the documented relation between the SHELTER measure and sheltering activity does not mean that the *SHELTER* measure is uncorrelated with incidental tax avoidance. Thus, to the extent that *SHELTER* captures incidental avoidance in addition to intentional avoidance, it could render the inferences from research investigating the effect of tax sheltering on economic outcomes incorrect or incomplete. This is particularly salient with the findings of Kim et al. (2011) since the economic outcome of interest – stock price crash risk – is shown to be correlated with many firm characteristics (See Table 2 from Kim et al. 2011). Thus, the positive relation between tax sheltering and stock price crash risk may be driven by factors incidental to tax sheltering.

To reexamine Kim et al. (2011), we use an instrumental variable approach to isolate the intentional avoidance component of the *SHELTER* measure from any incidental components. Econometrically, a valid instrument must satisfy several conditions. First, the instrument must be correlated with the endogenous variable which is commonly referred to as the relevance condition. In this context, the instrument must correlate with tax sheltering. Second, the instrument must affect the outcome only through the endogenous variable. This is often referred to as the exclusion condition and is essential for the instrument to be considered valid. In our current setting, this means the instrument must affect stock price crash risk only through sheltering. The final condition is that the effect of the instrument on the outcome is unconfounded, or uncorrelated with the error term. This condition is sometimes referred to as the exchangeability or independence condition.

In our reexamination of Kim et al. (2011), we propose the extent to which a firm operates in tax haven countries as a valid instrument for intentional tax avoidance in the context of Kim et al. (2011) because tax havens are a plausible channel through which a firm may avoid taxes, yet simultaneously increase opacity that may be used for rent extraction purposes (Bennedsen and Zeume 2018; Atwood and Lewellen 2019). This combination of a potential effective tax avoidance structure that simultaneously creates opacity is at the core of the theoretical motivation in Kim et al. (2011). Therefore, we argue that tax haven existence is a suitable instrument.

To measure a firm's exposure to tax haven countries, we extract information about a firm's significant operations in tax haven countries from Exhibit 21 filings. To mitigate concerns about the location of material operations in tax havens being a choice, we follow an approach similar to the one used by Faulkender and Smith (2016) and Dyreng et al. (2022). Specifically, we use tax haven countries from each firm's first year in our data and fix these locations for all subsequent years. Doing this means that tax haven locations are effectively predetermined. We then match our fixed tax haven locations with information about tax information exchange agreements (TIEAs) that are signed between the U.S. and tax haven countries (Bennedsen and Zeume 2018).[30] These TIEAs are signed in a staggered manner over multiple years, are plausibly exogenous to individual firms, and have been shown to induce real effects (Bennedsen and Zeume (2018).

In our setting, we argue that once a TIEA has been signed between the U.S. and a particular tax haven country (for example, the Bahamas), a firm's ability (or effectiveness) to engage in

[30] By matching predetermined tax haven locations with plausibly exogenous variation from TIEAs we effectively create an exposure instrument (Bartik 1991; Breuer 2022).

sheltering activities in that tax haven country is diminished.[31] Consequentially, these TIEAs reduce a firm's ability to shift profits to tax haven countries. Accordingly, for our instrument – *NonTIEAHavenCountries* – we count only the "fixed" number of tax haven countries in which a firm reports significant operations that are *not* subject to a TIEA. Thus, although the tax haven locations are fixed at the firm level, the countries subject to TIEAs vary over time allowing for within-firm variation in tax haven exposure across the sample period.

4.2 | Results from Reexamining Kim et al. (2011)

For our reinvestigation of Kim et al. (2011), we first reexamine the main finding that tax sheltering is positively related to future stock price crash risk. These results are shown in Table 5. Column 1 reports our replication of the results reported in Table 4, Panel A, Column 1 from Kim et al. (2011). Because the initial stock price crash risk measure used by Kim et al. (2011) – Crash – is a binary variable, we follow Kim et al. (2011) and estimate the results reported in Column 1 using a logistic model. However, because the estimation of a two-stage least squares regression requires a linear model, we also report results using a linear probability model in Column 2.[32] In both columns, the coefficient estimate on *SHELTER* is positive and statistically significant ($p<0.01$). These findings suggest that sheltering activities are positively associated with future stock price crash risk, which is the primary inference drawn from Kim et al. (2011).

Because our instrument for sheltering – *NonTIEAHavenCountrie*s – requires information about the firm's significant subsidiary locations, our instrumental variables approach is limited to

[31] According to prior research (Desai and Dharmapala 2006; 2009, and others), tax avoidance structures, such as tax shelters, are potentially a formidable rent extraction tool because they can be viewed as a "good" investment, yet also can engender the opacity necessary for managerial rent extraction. If a tax sheltering scheme were to be rendered ineffective, for example, due to a TIEA, a firm (or manager) that continues to "use" an ineffective tax shelter will undoubtedly draw attention from outside monitors (board of directors, lenders, auditors, etc.). Thus, for a tax shelter to be able to mask rent extraction activities, presumably the tax shelter must exhibit some level of effectiveness.

[32] We do not report results when using a probit model due to well-known incidental parameter problem which can arise when estimating a probit model with fixed effects (Lancaster 2000).

firms with non-missing Exhibit 21 data. Thus, in Column 3 (Column 4) we replicate the primary result from Kim et al. (2011) using a logistic (linear probability) model for only those firms with non-missing Exhibit 21 data. In both columns, the coefficient on *SHELTER* remains positive and statistically significant ($p<0.01$), suggesting the positive association between sheltering and future stock price crash risk persists for the subset of firms with non-missing Exhibit 21 data.

The initial stock price crash risk measure used by Kim et al. (2011) is an indicator variable for whether a firm experiences one or more crash weeks, where a crash week is defined as a week in which the firm-specific weekly return is 3.2 standard deviations or more below the mean firm-specific weekly return over the entire fiscal year. A secondary stock price crash risk measure used by Kim et al. (2011) is *NCSKEW*, a continuous measure, which is defined as the negative conditional return skewness (Chen et al. 2001). In Table 6, we report our replication of the primary results from Kim et al. (2011) when using *NCSKEW* as the measure for stock price crash risk. Column 1 reports the results for the full sample. The coefficient on *SHELTER* is 0.042 and is statistically significant at the 10% level. Column 2 reports the results for the subsample of firms with non-missing Exhibit 21 data. The coefficient on *SHELTER* in Column 2 is 0.06 and statistically significant at the 5% level.

In Table 7, we report the two-stage least squares regression results when using Crash as the stock price crash measure. In the first stage, the coefficient estimate on our instrument is 0.013 and statistically significant ($p<0.01$), suggesting the instrument is a significant determinant of *SHELTER*.[33] The Cragg-Donald F-statistics for our instrument is 70.74, which is well above the critical values suggested by Stock and Yogo (2005), and rejects that our instrument is weak. In the second stage, we find that the coefficient on *SHELTER* flips sign and becomes *negative* and

[33] In untabulated results, we find that the Pearson correlation coefficient for the endogenous regressor (SHELTER) and our instrument (NonTIEAHavenCountries) is 0.3141 ($p<0.01$).

statistically significant ($p<0.05$).[34]

To provide confidence that the use of the IV approach changes inferences from the original specification, we verify that the two-stage least squares coefficient estimate on *SHELTER* differs significantly from the coefficient estimate on *SHELTER* in the linear probability model reported in Table 5, Column 4 (Hausman p-value = 0.01). Thus, we show that when using an instrumental variable approach the relation between *SHELTER* and Crash now becomes negative, suggesting that sheltering activity is negatively associated with future stock price crash. A possible explanation for this finding is that sheltering engenders diversification benefits that actually reduce stock price crash risk, and it is the incidental factors associated with the *SHELTER* measure that leads to the positive relation between sheltering and crash risk documented in Kim et al. (2011).

In Table 8, we report the two-stage least squares regression results when using *NCSKEW* as the stock price crash measure. The first-stage results are identical to the first-stage results reported in Table 7. In the second stage, we find that the coefficient on *SHELTER* is once again negative and statistically significant ($p<0.01$), suggesting that *SHELTER* is negatively associated with future stock price crash risk when using an alternative measure for crash risk. The two-stage least squares coefficient estimate on *SHELTER* differs significantly from the coefficient estimate on SHELTER in the OLS model reported in Table 6, Column 2 (Hausman p-value <0.01).

Overall, our instrumental variables approach suggests that when isolating the intentional component of *SHELTER* from the incidental component, we fail to find a positive relation between tax sheltering and stock price crash risk. In fact, we find that the relation may be negative.[35] A

[34] The coefficient estimate on *SHELTER* in Table 7 Column 2 (the second-stage results) is -0.271. This coefficient estimate suggests that moving from the 25^{th} percentile of *SHELTER* to the 75^{th} percentile of *SHELTER* (which is associated with an approximately 0.65 change in *SHELTER*) is associated with a 17% decrease in $Crash_t$.

[35] Our failure to find evidence that tax sheltering is associated with future stock price crash risk is consistent with other research that fails to find empirical evidence of a relation between tax avoidance and managerial agency costs (e.g., Blaylock 2015). However, we do not seek to claim that our results disprove the existence of relation between tax

caveat to our instrumental variables approach, like any instrumental variables approach, is that the exclusion condition is empirically unverifiable. Thus, despite our best efforts at identifying a valid instrument, it could be the case that our instrument does not solely relate to crash risk through sheltering activities, which would be a violation of the exclusion condition.

5 | IMPLICATIONS FOR RESEARCHERS

Our results show that insufficiently distinguishing between intentional and incidental avoidance could result in endogeneity concerns and flawed inferences in research that examines the economic consequences of tax avoidance. Notably, in other settings it is possible that mitigating these endogeneity concerns could strengthen previously weak or insignificant findings, or reveal new results previously masked by measurement error.

When designing a study, researchers should begin by identifying the conceptual construct of interest, and then determine the empirical proxies that best capture that construct (e.g., Guenther et al. 2021). In many cases, no empirical proxy perfectly captures a researcher's conceptual construct. This disconnect between one's conceptual construct and empirical proxy is a form of measurement error (Jennings et al. 2024) and creates construct validity concerns (Trochim and Donnelly 2001). Measurement error, like all forms of endogeneity, gives rise to internal validity concerns because it impedes a researcher's ability to draw meaningful economic inferences (Trochim and Donnelly 2001). To dampen these internal validity concerns, researchers should take appropriate remedial actions, which will vary depending on the research setting.

For situations where the researcher can identify incidental factors that are correlated with both the outcome of interest and the imperfect tax avoidance proxy (i.e., correlated omitted variables), such as prior losses in the case of Hasan et al. (2014), inclusion of these factors in the

avoidance and agency costs as there are a number of prior studies that do find evidence consistent with this proposed relation (e.g., Desai and Dharmapala 2006; Desai et al. 2007).

empirical model may be sufficient.[36] Nevertheless, we urge caution with regard to the risks of "over-controlling" for seemingly incidental factors (R&D, intangible intensity, etc.) because some factors may contain elements of both incidental and intentional avoidance.[37] Thus, controlling for one type of avoidance may inadvertently control for the other type, resulting in the proverbial "throwing the baby out with the bath water" (Whited et al. 2022). When deciding whether to control for a particular factor, researchers should carefully consider the probability and weigh the consequences of Type 1 (rejecting the null when the null is true) and Type 2 (failing to reject the null when the alternative is true) errors. For example, in the case of Hasan et al. (2014), controlling for prior losses undoubtedly captures incidental factors because it is unlikely firms intentionally report financial losses repeatedly over many years simply to avoid taxes. However, since the intentions behind the actions leading to a loss are unobservable, prior losses may also reflect some aspect of intentional avoidance. Yet, given the clear conceptual relation between prior losses and credit spreads (the outcome in Hasan et al. (2014)), the inclusion of prior losses in the model is likely warranted to mitigate concerns about a Type I error, which seems more probable and consequential than a Type II error that may arise from controlling for prior losses.

Second, in settings where it may be difficult to identify correlated omitted variables or appropriately measure omitted constructs with available data, quasi-experimental methods such as an instrumental variables approach can be used to effectively isolate intentional tax avoidance from incidental avoidance. To implement an instrumental variable approach, researchers must understand their research question and empirical setting to identify a valid instrument that meets

[36] The intent behind this approach is to identify and control for factors that are correlated with both the undesired variation in the imperfect tax avoidance proxy – specifically, the variation responsible for the disconnect from the conceptual construct – and the outcome of interest.
[37] Diller et al. (2021) demonstrate an interesting case in which firms' inconsistent transfer pricing reporting across jurisdictions arises due to inconsistent standards (and the application of those standards) across jurisdictions. To some jurisdictions, firms' inconsistent reporting may be viewed as intentional avoidance; however, it's possible these inconsistencies arise solely due to the inconsistent standards, thus representing incidental avoidance.

the relevance and exclusion criteria. Appropriate instruments are often context specific and should be directly related to the type of intentional avoidance that is relevant for the setting. For example, variation in the effectiveness of tax haven subsidiaries, which is our instrument in our reexamination of Kim et al. (2011), may not be an appropriate instrument in other settings, such as for a sample comprised solely of domestic-only firms.

6 | CONCLUSION

In this paper, we define intentional and incidental tax avoidance and discuss why distinguishing between them is vital, particularly for research investigating the consequences of tax avoidance. From an econometric perspective, we articulate how failure to distinguish between these tax avoidance components when examining the consequences of tax avoidance can result in endogeneity, leading to biased estimates and incomplete economic inferences. We revisit the findings from two published studies – Hasan et al. (2014) and Kim et al. (2011). These studies document adverse outcomes associated with intentional tax avoidance. We demonstrate that once we control for incidental factors related to past losses (Hasan et al. 2014) or isolate intentional tax avoidance using an instrumental variables approach (Kim et al. 2011), the inferences from the studies no longer persist.

Overall, our discussion and findings suggest researchers examining the consequences of tax avoidance should be aware that their focus is almost always on the effects of intentional tax avoidance, thus requiring them to carefully distinguish between intentional and incidental avoidance in their empirical approach so that meaningful, reliable economic inferences can be drawn.

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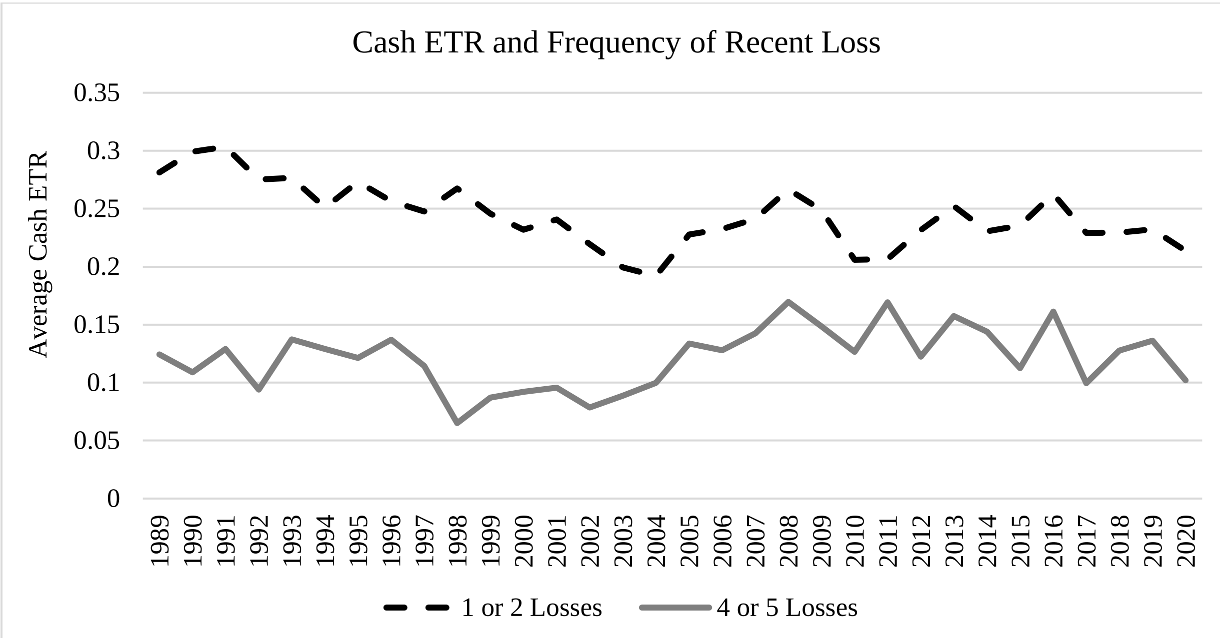
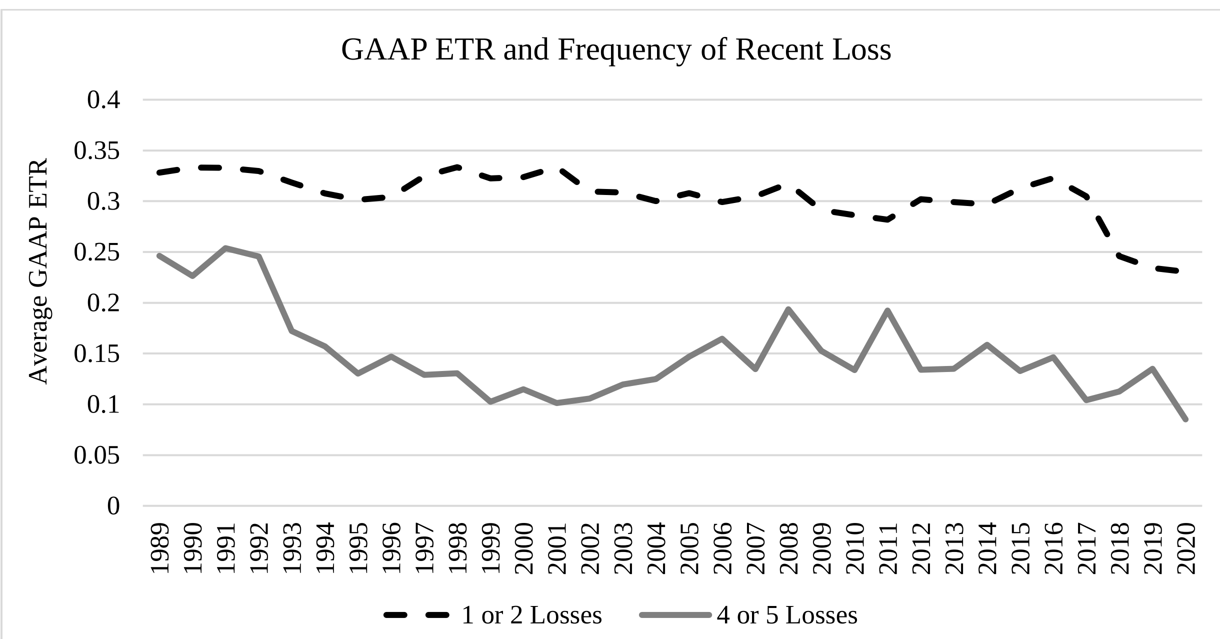
Figure 1 Frequency of Prior Losses and ETRs (1989-2020)

Panel A – Cash ETRs



Panel B – GAAP ETRs



Notes: This figure reports the average Cash ETR (Panel A) or average GAAP ETR (Panel B) by year for observations with 1 or 2 pretax accounting losses in the previous five years relative to observations with 4 or 5 pretax accounting losses in the previous five years.

Table 1 Sample Selection for Comparing ETRs and Prior Losses

Criteria	Observations	Firms
Non-financial, non-utility U.S. corporations with non-missing assets and sales (1989-2020)	171,337	17,154
Less: Observations with negative pretax income	91,612	11,774
Less: Observations with missing information about taxes paid	85,402	10,726
Less: Observations with missing pretax income for the previous five years	65,907	7,824

This table reports the sample selection criteria for our sample.

Table 2 Losses and Effective Tax Rates (ETRs)

Panel A: Cash ETRs

			Cash ETR	
Losses in Previous 5 Years	N	% of N	Mean	Median
0	37,364	56.7%	31.2%	29.6%
1	11,885	18.0%	26.3%	21.0%
2	7,431	11.3%	21.7%	12.3%
3	4,737	7.2%	16.7%	5.7%
4	2,946	4.5%	12.6%	3.0%
5	1,544	2.3%	10.9%	1.5%
	65,907	100.0%	26.9%	24.5%

Panel B: GAAP ETRs

			GAAP ETR	
Losses in Previous 5 Years	N	% of N	Mean	Median
0	37,364	56.7%	28.1%	30.6%
1	11,885	18.0%	24.0%	25.1%
2	7,431	11.3%	20.2%	16.6%
3	4,737	7.2%	15.9%	7.0%
4	2,946	4.5%	13.2%	3.3%
5	1,544	2.3%	12.0%	1.6%
	65,907	100.0%	24.5%	26.8%

Notes: This table compares the average and median effective tax rates for profit firm-years based on the number of losses (i.e., pretax income is negative) the firm incurred in the previous 5 years. Panel A reports results for Cash effective tax rates (Cash ETR), where Cash ETR is are computed as cash taxes paid (TXPD) scaled by pretax income (PI) and are winsorized to be between 0 and 1. Panel B reports results for GAAP effective tax rates, which are computed as corporate tax expense (TXT) scaled by pretax income (PI) and are winsorized to be between 0 and 1.

Table 3 Losses and Alternative Tax Avoidance Proxies

Panel A: Book-Tax Differences (BTDs)

			Total BTD / AT_{t-1}	
Losses in Previous 5 Years	N	% of N	Mean	Median
0	36,038	56.5%	0.026	0.019
1	11,558	18.1%	0.029	0.018
2	7,244	11.4%	0.037	0.019
3	4,617	7.2%	0.049	0.022
4	2,846	4.5%	0.056	0.022
5	1,485	2.3%	0.092	0.029
	63,788	100.0%	0.032	0.019

Panel B: DTAX (Frank et al. 2009)

			DTAX	
Losses in Previous 5 Years	N	% of N	Mean	Median
0	29,244	56.2%	-0.055	0.011
1	9,521	18.3%	-0.045	0.016
2	5,956	11.4%	-0.032	0.024
3	3,813	7.3%	-0.022	0.037
4	2,313	4.4%	0.020	0.066
5	1,194	2.3%	0.225	0.140
	52,041	100.0%	-0.038	0.016

Notes: This table compares the number of losses (i.e., pretax income is negative) a firm incurred in the previous 5 years to two additional tax avoidance proxies. Panel A reports results when the tax avoidance proxy is book-tax differences (BTD) scaled by lagged total assets (AT). Panel B reports results when the tax avoidance proxy is DTAX from Frank et al. (2009). All variables are defined in Appendix A.

Table 4 Replication of Results from Hasan et al. (2014)

	DepVar = Credit Spread			
Cash ETR_{t-1}	-0.304***	-0.098**	-0.034	-0.031
	(-6.297)	*(-2.442)*	*(-0.892)*	*(-0.809)*
$LNAT_{t-1}$		-0.083***	-0.069***	-0.069***
		(-6.413)	*(-5.668)*	*(-5.541)*
LEV_{t-1}		0.409***	0.389***	0.386***
		(14.470)	*(13.820)*	*(13.590)*
$Tangibility_{t-1}$		-0.110**	-0.085*	-0.081*
		(-2.490)	*(-1.952)*	*(-1.828)*
Cash $Holding_{t-1}$		-0.003	-0.055	-0.058
		(-0.047)	*(-0.848)*	*(-0.891)*
ROA_{t-1}		-0.931***	-0.851***	-0.873***
		(-8.418)	*(-7.807)*	*(-7.944)*
MB_{t-1}		-0.004**	-0.004***	-0.004**
		(-2.236)	*(-2.589)*	*(-2.544)*
Sales $Growth_{t-1}$		0.060**	0.056*	0.056*
		(2.040)	*(1.904)*	*(1.906)*
Earnings $Volatility_{t-1}$		0.011	-0.000	0.001
		(1.239)	*(-0.002)*	*(0.145)*
$Z\text{-}Score_{t-1}$		-0.067***	-0.053***	-0.050***
		(-6.954)	*(-5.454)*	*(-4.919)*
Log(Loan $size_t$)		-0.166***	-0.166***	-0.166***
		(-13.910)	*(-13.890)*	*(-13.930)*
Log(Loan $Maturity_t$)		-0.008	-0.007	-0.007
		(-0.453)	*(-0.415)*	*(-0.425)*
Syndication		0.045*	0.045*	0.045*
		(1.818)	*(1.826)*	*(1.852)*
Loss5			0.153***	
			(9.046)	
Loss5 %				0.289***
				(7.646)
Observations	12,458	12,410	12,410	12,410
Adjusted R^2	0.542	0.661	0.666	0.664
Test of Coefficients (P-Value) vs. Column 2			0.000	0.000

Fixed Effects	Rating, Loan Type, Loan Purpose, Industry and Year	Rating, Loan Type, Loan Purpose, Industry and Year	Rating, Loan Type, Loan Purpose, Industry and Year	Rating, Loan Type, Loan Purpose, Industry and Year

Notes: This table reports the results of regressing credit spreads from private loans on lagged cash effective tax rates (Cash ETR) and a set of lagged controls for credit facilities starting between 1989 and 2008. The selected control variables are based on those used in Hasan (2014). Standard errors are clustered at the firm level. All variables are defined in Appendix A. *,**, *** denote statistical significance at the 10%, 5%, or 1% level using a two-tailed t-test.

Table 5 Replication of Results from Kim et al. (2011) using Crash

	Dependent Variable = ***Crash*** $_t$			
	1	2	3	4
	Logit	LPM	Logit	LPM
SHELTER$_{t-1}$	**0.152*** **	**0.026*** **	**0.198*** **	**0.037*** **
	(3.271)	***(3.480)***	***(3.025)***	***(3.742)***
DTURN$_{t-1}$	0.017*	0.003	0.010	0.002
	(1.734)	*(1.682)*	*(1.060)*	*(1.133)*
NCSKEW$_{t-1}$	0.026	0.005*	0.019	0.004
	(1.449)	*(1.958)*	*(0.901)*	*(1.155)*
SIGMA$_{t-1}$	10.580***	1.101***	11.570***	1.441***
	(4.887)	*(4.144)*	*(3.918)*	*(4.292)*
RET$_{t-1}$	0.952***	0.086***	0.997***	0.113***
	(4.476)	*(4.098)*	*(3.059)*	*(3.516)*
MVE$_{t-1}$	0.070***	0.010***	0.061***	0.009***
	(5.087)	*(4.871)*	*(4.109)*	*(4.026)*
MB$_{t-1}$	0.000	0.000	0.000	0.000
	(0.635)	*(0.547)*	*(1.622)*	*(1.369)*
LEVERAGE$_{t-1}$	-0.177**	-0.028**	-0.158*	-0.028**
	(-2.182)	*(-2.482)*	*(-1.876)*	*(-2.235)*
ROA$_{t-1}$	0.178***	0.013***	0.276**	0.015*
	(3.083)	*(3.965)*	*(2.284)*	*(1.990)*
ACCM$_{t-1}$	-0.045	-0.008	-0.015	-0.006
	(-1.084)	*(-1.494)*	*(-0.508)*	*(-1.382)*
Observations	37,820	37,820	25,571	25,571
Sample	Full	Full	Non-Missing Exhibit 21 Data	Non-Missing Exhibit 21 Data
Pseudo R^2 OR Adjusted R^2	0.015	0.014	0.014	0.012
Fixed Effects	Year	Year	Year	Year

Notes: This table reports the results of regressing stock price crash risk (Crash) on lagged SHELTER (Wilson 2009) and a set of lagged control variables for 1995-2008. Control variables are based on those used in Kim et al. (2011). Columns 1 and 3 (2 and 4) report results when using a logistic (linear probability) model. Columns 1 and 2 report results using the full sample for 1995-2008. Columns 3 and 4 report results for subset of firms with non-missing subsidiary information reported in Exhibit 21 filings. Standard errors are clustered at the firm and year level. All variables are defined in Appendix A. *,**, *** denote statistical significance at the 10%, 5%, or 1% level using a two-tailed t-test.

Table 6 Replication of Results from Kim et al. (2011) using NCSKEW

	Dependent Var = ***NCSKEW*** $_t$	
	1	2
SHELTER$_{t-1}$	**0.042***	**0.060****
	(1.932)	***(2.941)***
DTURN$_{t-1}$	0.008**	0.005
	(2.323)	*(1.293)*
NCSKEW$_{t-1}$	0.012*	0.006
	(2.130)	*(0.752)*
SIGMA$_{t-1}$	5.246***	6.104***
	(6.351)	*(7.507)*
RET$_{t-1}$	0.351***	0.428***
	(4.717)	*(4.656)*
MVE$_{t-1}$	0.072***	0.066***
	(10.090)	*(7.756)*
MB$_{t-1}$	-0.000	-0.000
	(-1.496)	*(-0.336)*
LEVERAGE$_{t-1}$	-0.093***	-0.071*
	(-3.533)	*(-2.075)*
ROA$_{t-1}$	-0.001	0.002
	(-0.057)	*(0.046)*
ACCM$_{t-1}$	-0.021	-0.015
	(-1.464)	*(-0.901)*
Observations	37,818	25,571
Sample	Full	Non-Missing Exhibit 21 Data
Adjusted R^2	0.015	0.015
Fixed Effects	Year	Year

Notes: This table reports the results of regressing stock price crash risk (NCSKEW) on lagged SHELTER (Wilson 2009) and a set of lagged control variables for 1995-2008. Control variables are based on those used in Kim et al. (2011). Columns 1 reports results using the full sample for 1995-2008. Columns 2 reports results for subset of firms with non-missing subsidiary information reported in Exhibit 21 filings. Standard errors are clustered at the firm and year level. All variables are defined in Appendix A. *,**, *** denote statistical significance at the 10%, 5%, or 1% level using a two-tailed t-test.

Table 7 Instrumental Variable Approach for Kim et al. (2011) using Crash

	1st Stage	2nd Stage
	SHELTER $_{t-1}$	*Crash* $_t$
Variable of Interest		
$SHELTER_{t-1}$		-0.271**
		(-2.122)
Instrument		
$NonTIEAHavenCountries_{t-1}$	0.013***	
	(8.411)	
Control Variables		
$DTURN_{t-1}$	0.005***	0.003*
	(4.229)	*(1.893)*
$NCSKEW_{t-1}$	0.017***	0.009**
	(8.433)	*(2.340)*
$SIGMA_{t-1}$	-2.866***	0.549
	(-14.390)	*(1.264)*
RET_{t-1}	-0.136***	0.070**
	(-8.343)	*(2.401)*
MVE_{t-1}	0.096***	0.040***
	(61.150)	*(3.087)*
MB_{t-1}	0.000	0.000
	(1.307)	*(1.170)*
$LEVERAGE_{t-1}$	-0.069***	-0.050***
	(-5.274)	*(-3.154)*
ROA_{t-1}	0.063**	0.034**
	(2.270)	*(2.224)*
$ACCM_{t-1}$	-0.079***	-0.031**
	(-4.628)	*(-2.257)*
Observations	25,571	25,571
Adjusted R^2	0.525	
First Stage F-Statistic		***70.74***
Hausman P-Value		***0.01***
Fixed Effects	Year	Year

Notes: This table reports the results of a two-stage least squares regression of Crash on SHELTER where NonTIEAHavenCountries is the instrumental variable. The sample includes all firm-year observations for 1995-2008 (the same sample period used in Kim et al. (2011)) with non-missing subsidiary information reported in Exhibit 21 filings. Control variables are based on those used in Kim et al. (2011). Columns 1 (Column 2) reports the first-stage (second-stage) results. Standard errors are clustered at the firm and year level. All variables are defined in Appendix A. *,**, *** denote statistical significance at the 10%, 5%, or 1% level using a two-tailed t-test.

Table 8 Instrumental Variable Approach for Kim et al. (2011) using NCSKEW

	1st Stage	2nd Stage
	SHELTER $_{t-1}$	*NCSKEW $_t$*
Variable of Interest		
$SHELTER_{t-1}$		-1.255***
		(-4.558)
Instrument		
$NonTIEAHavenCountries_{t-1}$	0.013***	
	(8.411)	
Control Variables		
$DTURN_{t-1}$	0.005***	0.012***
	(4.229)	*(3.146)*
$NCSKEW_{t-1}$	0.017***	0.028***
	(8.433)	*(3.156)*
$SIGMA_{t-1}$	-2.866***	2.291**
	(-14.390)	*(2.339)*
RET_{t-1}	-0.136***	0.242***
	(-8.343)	*(3.614)*
MVE_{t-1}	0.096***	0.199***
	(61.150)	*(6.990)*
MB_{t-1}	0.000	0.000
	(1.307)	*(0.139)*
$LEVERAGE_{t-1}$	-0.069***	-0.165***
	(-5.274)	*(-4.248)*
ROA_{t-1}	0.063**	0.083
	(2.270)	*(1.339)*
$ACCM_{t-1}$	-0.079***	-0.120***
	(-4.628)	*(-3.085)*
Observations	25,571	25,571
Adjusted R^2	0.525	
First Stage F-Statistic		***70.74***
Hausman P-Value		***0.00***
Fixed Effects	Year	Year

Notes: This table reports the results of a two-stage least squares regression of NCSKEW on SHELTER where NonTIEAHavenCountries is the instrumental variable. The sample includes all firm-year observations for 1995-2008 (the same sample period used in Kim et al. (2011)) with non-missing subsidiary information reported in Exhibit 21 filings. Control variables are based on those used in Kim et al. (2011). Columns 1 (Column 2) reports the first-stage (second-stage) results. Standard errors are clustered at the firm and year level. All variables are defined in Appendix A. *,**, *** denote statistical significance at the 10%, 5%, or 1% level using a two-tailed t-test.

Appendix A Variable Definitions

Variable	Description	Source
	Dependent Variables	
Crash	An indicator variable equal to one if a firm-year experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly returns for the fiscal year.	CRSP
Credit Spread	The natural log of one plus the amount the borrower pays in basis points over the London Interbank Borrowing Rate (LIBOR) or LIBOR equivalent. This variable is titled ALLINDRAWN in the Dealscan Database.	Dealscan
NCSKEW	The negative skewness of firm-specific weekly returns throughout a fiscal year (Chen et al. 2001)	CRSP
	Other Variables	
ACCM	The moving sum of the absolute value of discretionary accruals for the previous three years, estimated using the modified Jones model (See Kim et al. (2011) for more details)	Compustat
BTD	The calculation for total book-tax differences is $PI - (\left[\frac{U.S.CTE+Foreign\ CTE}{U.S\ Statutory\ Rate}\right] - [NOL_t - NOL_{t-1}])$ where PI is pretax book income, U.S. CTE is current federal tax expense (TXFED), or if TXFED is missing then U.S. CTE is total tax expense (TXT) less deferred taxes (TXDI), state taxes (TXS), and other income taxes (TXO). Foreign CTE is the current foreign tax expense (TXFO). NOL is net operating loss carryforwards (TLCF).	Compustat
Cash ETR	Taxes paid (TXPD) scaled by pretax income (PI)	Compustat
Cash Holding	Cash and marketable securities (CHE) scaled by lagged total assets (AT)	Compustat
DTAX	The residual from the following regression estimated by year at the two-digit SIC Code level: $PERM_{i,t} = \alpha + \beta_1 INTANG_{i,t} + \beta_2 UNCON_{i,t} + \beta_3 MI_{i,t} + \beta_4 CSTE_{i,t} + \beta_5 \Delta NOL_{i,t} + \beta_6 PERM_{i,t-1} + \epsilon_{i,t}$, where $PERM_{i,t} = BI_{i,t}\left[\frac{CFRE_{i,t}+CFOR_{i,t}}{STR_{i,t}}\right] - (\frac{DTE_{i,t}}{STR_{i,t}})$. BI is pretax book income (PI), CFTE is current federal tax expense (TXFED), CFO is current foreign tax expense (TXFO), DTE is deferred tax expense (TXDI), STR is the statutory tax rate, INTANG is goodwill and other intangibles (INTAN), UNCON is income (or loss) reported under the equity method (ESUB), MI is income (or loss) attributable to minority interest (MII), CSTE is current state income tax expense (TXS) and ΔNOL is the change in net operating loss carryforwards (TLCF)	Compustat
DTURN	The difference in average monthly share turnover in the current year from the prior year. Monthly share turnover is the monthly trading volume divided by the number of shares outstanding	CRSP
Earnings Volatility	The standard deviation of earnings (NI) in the previous five years	Compustat
LNAT	The natural log of 1 + total assets (AT)	Compustat
LEV or LEVERAGE	The sum of long-term debt (DLTT) and the current portion of long-term debt (DLC) scaled by lagged total assets (AT)	Compustat

Variable	Description	Source
Loan Maturity	Number of months to maturity of a loan facility obtained in a given year	Dealscan
Loan Size	The total amount of the loan facility (in $millions) obtained by a firm in a given year	Dealscan
Loss5	An indicator variable equal to one if a firm incurred negative pretax income (PI) in any of the previous five years, and zero otherwise	Compustat
Loss5%	The proportion of years in the previous five years for which a firm incurred a loss	Compustat
MB	The market value of equity scaled by the book value of equity (CEQ). The market value of equity is calculated as the product of current shares outstanding (CSHO) and price per share at the fiscal year end (PRCC_C)	Compustat
MVE	The product of current shares outstanding (CSHO) and price per share at the fiscal year end (PRCC_C)	Compustat
NonTIEAHavenCountries	A count of the number of haven countries in which a firm reports material subsidiaries in its Exhibit 21 filings. Following Faulkender and Smith (2016) and Dyreng et al. (2022) we use data only from a firm's first year in our data. We allow the number of haven countries to vary within a firm by counting only those haven countries in which a firm reports material operations that are not subject to a tax information exchange agreement with the U.S. (Bennedsen and Zeume 2018).	Exhibit 21
RET	The average firm-specific weekly return for a fiscal year multiplied by 100	CRSP
ROA (Kim)	Income before extraordinary items (IBC) scaled by Total Assets (AT)	Compustat
ROA (Hasan)	Operating income before depreciation (OIBDP) scaled by lagged total assets (AT)	Compustat
Sales Growth	Percentage change is sales (SALE) from two years prior to the loan to the year immediately before the loans	Compustat
SHELTER	Estimated sheltering probability based on Wilson's (2009) tax sheltering model: SHELTER=-4.86 + 5.2*BTD + 4.08*|DAP| -4.41*Leverage + 0.76*AT + 3.51*ROA + 1.72*Foreign Income + 2.43*R&D BTD is the total book-tax difference, which is computed as (PI-((TXFED+TXFO)/0.35-ΔTLCF)) where TXFED is current federal tax expense, TXFO is current foreign tax expense, TLCF is net operating loss carryforwards and PI is pretax income. DAP is the absolute value of discretionary accruals from the performance-adjusted modified cross-sectional Jones model (Kothari et al. 2005). Leverage is long-term debt (DLTT) scaled by total assets (AT). ROA is pretax income (PI) scaled by total assets (AT). Foreign Income is an indicator variable equal to one for firms that report foreign income (PIFO), and zero otherwise. R&D is research and development expense (XRD).	Compustat
SIGMA	The standard deviation of firm-specific weekly returns for a fiscal year	CRSP
Syndication	An indicator variable equal to one if the loan is syndicated, and zero otherwise	Dealscan
Tangibility	Net property, plant, and equipment (PPENT) scaled by lagged total assets (AT)	Compustat

Variable	Description	Source
Z-Score	Modified Altman (1968) Z-Score from Graham et al. (2008) computed as follows: $Z - score = (1.2 * WCAP + 1.4 * RE + 3.3 * PI + 0.999 * SALE)/AT$, where WCAP is working capital (current assets less current liabilities plus the current portion of long-term debt), RE is retained earnings, and PI is pretax income, SALE is total sales, and AT is total assets. This modified Z-score does not include the ratio of market value of equity to book value of total debt because a similar term, the market-to-book ratio (MB), is included in regressions as a separate control variable.	Compustat