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Authors
Shevlin, T
Thornock, J
Williams, B

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An Examination of Firms' Responses to Tax Forgiveness

Terry Shevlin, University of California - Irvine, tshevlin@uci.edu
Jacob Thornock, Brigham Young University, thornock@byu.edu
Braden Williams, University of Texas at Austin, brady.williams@mccombs.utexas.edu

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Abstract: This study uses state tax amnesties to examine how firms respond to forgiveness—particularly repeated forgiveness—by a taxing authority. We posit that tax forgiveness programs alter taxpayer perceptions of the probability of detection by enforcers or the probability of future forgiveness programs, either of which could affect future tax aggressiveness. We find that firms headquartered in an amnesty-granting state increase state income tax aggressiveness following the first instance of tax amnesty, relative to control firms in other states. Moreover, we find evidence that tax aggressiveness incrementally increases with each additional repetition of a tax amnesty. Finally, we find that the effect of amnesties on tax aggressiveness is more prominent for small firms, which face less tax authority scrutiny and for which the tax aggressiveness measures are less confounded. Our findings suggest that repeated programs of tax forgiveness have increasingly negative implications for corporate tax collections.

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1. Introduction

Corporate taxpayers have clear incentives to legally minimize their tax liabilities to the extent that doing so maximizes after-tax returns (Scholes et al. 2014; Slemrod 2004). Conversely, tax authorities have a clear mandate to ensure that taxpayers meet their tax obligations and enforce tax rules in place to achieve that mandate. These conflicting incentives create an inherent tension between taxpayers and tax authorities wherein both parties use various tactics to achieve their aims. On the one side, corporations reduce taxes by use of tax shelters, offshore income shifting, and a variety of other tax reduction mechanisms. Tax authorities, on the other side, will often undertake measures to increase tax revenues by increasing detection efforts or enforcement of tax statutes.[1] One particular tactic used by tax authorities is to offer a temporary grace period during which taxpayers can remit unpaid or overdue taxes for a reduced penalty or no penalty at all, which we generically label "tax forgiveness."[2] Tax forgiveness programs are often billed as a *one-time chance* for a fresh start—the last opportunity for the taxpayer to clear the slate. However, many jurisdictions have offered tax forgiveness multiple times, undercutting their claim that it is truly a one-time chance.

The objective of this study is to learn how firms respond to tax forgiveness programs offered by state tax authorities, especially when those programs are repeated. Specifically, we examine changes in corporate state effective tax rates by firms that are headquartered in states that grant the amnesty programs. Because the purpose of many state tax amnesty programs is to enroll and collect overdue taxes from new, unknown, noncompliant taxpayers, prior research has

[1] We consider tax authorities to be the joint team of those responsible for creating tax law (e.g., legislative bodies) and those responsible for enforcing tax laws (e.g., revenue agencies or other tax collection agencies).
[2] Recent uses of this tactic have occurred at the individual taxpayer level (e.g., forgiveness programs offered to individual taxpayers accused of using Swiss bank accounts to avoid U.S. taxes) and at the corporate taxpayer level (e.g., various state tax amnesty programs).

focused on the effect of these "nexus" amnesties on new enrollees (e.g., Fisher, Goddeeris, and Young 1989; Christian, Gupta, and Young 2002) or aggregate tax revenues (e.g., Alm and Beck 1993; Luitel and Sobel 2007). In contrast, by focusing on firms headquartered in the amnesty-granting state, we examine the effect of tax forgiveness on existing, known taxpayers. We hypothesize that offering to forgive noncompliant taxpayers can affect existing taxpayers' future effective tax rates for two related reasons. First, such forgiveness programs may reduce expectations of the tax authority detecting aggressive tax positions, because the very need for such a program may reveal a shortfall in the existing tax detection processes. That is, taxpayers might view the forgiveness program as a signal of weak tax enforcement, as suggested in Baer and LaBorgne (2008). If the expected probability of detection decreases, the rational taxpayer will likely become less compliant (Allingham and Sandmo 1972). Second, the first instance of tax forgiveness plausibly increases the taxpayer's *expectation* of another future tax forgiveness program. That is, an expectation of a future amnesty reduces the expected interest and penalty on any admitted underpayment, again reducing the expected cost of aggressive tax planning. Based on these arguments, we predict that tax forgiveness programs, especially repeated programs, will lead to increased corporate tax aggressiveness—our primary objective in this paper is to test these predictions. [3]

As an empirical setting, we employ income tax amnesty programs offered by U.S. states over the past four decades. Although each state amnesty program has unique attributes, there are

[3] We use the term "tax aggressiveness" to reflect increased non-compliance among taxpayers who are already in the tax system. It captures the aggressive activities and strategies that a firm may undertake in order to reduce their explicit tax burden, which includes interpreting state tax rules more aggressively and/or also engaging in more tax aggressive tax planning to avoid state taxes (such as becoming more aggressive in shifting income out of high tax states into lower tax states). The inverse of this concept is analogous to the term "tax compliance" used in the economics literature. We acknowledge at the outset that empirical measurement of tax aggressiveness is challenging, especially in separating tax aggressiveness from more benign tax avoidance. We provide an important set of limitations and robustness tests in the conclusion of this section and in the Online Appendix.

three common features associated with state-level corporate income tax amnesties that make them suitable for empirically testing our research question. First, the amnesties occur in different states at different times, which aids in the empirical identification of a tax response by corporations. Second, amnesties are often coupled with threats of increased enforcement and increased penalties post-amnesty, which could decrease future tax aggressiveness (Mikesell and Ross 2012), providing tension to our main predictions.[4] Third, many states have offered multiple instances of tax amnesty, which allows us to assess the effects of repeated tax forgiveness. During our sample period, 39 states offered a corporate income tax amnesty program at least once; of these 39 states, 30 states have repeated a similar program at least once and several states have offered up to five different amnesties.[5]

An important feature of our empirical analyses is that we examine both large and small publicly-traded corporations (hereafter "firms"). These firms are important economic actors in the economy—they have both the incentives to compete on tax strategies and the legal expertise to effectively implement tax avoiding strategies. How they react to state tax amnesties is an important research question that has not been previously studied.[6] We employ the firm's state effective tax rate (*STATE ETR*) as a measure of the firm's tax burden—lower values of *STATE ETR* are associated with higher tax aggressiveness. We use a difference-in-differences research design, in which amnesty firms are matched to non-amnesty firms in a control sample to mitigate

[4] For example, Mikesell and Ross (2012) note that recent state tax amnesties have been accompanied by increased fines and penalties (many states, including Maryland, New Jersey, Virginia and California), potential publication of a list of delinquent taxpayers (New Jersey), stricter enforcement (Indiana), increased jail time (Mississippi) and major tax reform (Ohio).

[5] For states that have multiple amnesties, the average (median) elapsed time between amnesties is 10 (9) years.

[6] Boyd (2011) examines how required disclosure influences firms' tendency to participate in voluntary state tax amnesty programs. In contrast, we focus on firms' reactions to these programs being offered in terms of their state tax planning.

the influence of unobserved, contemporaneous effects.[7] Thus, we test whether firms headquartered in amnesty-granting states exhibit greater increases in tax aggressiveness following the initial and subsequent amnesties than firms headquartered in non-amnesty-granting states during the same time period. The empirical model also includes firm-level controls, such as the firm's federal effective tax rate, sales revenues, R&D expense, leverage, and capital intensity to account for firm-level drivers of legal tax avoidance. Finally, the empirical model includes time-varying state statutory tax rates to account for subsequent changes in the state tax regime, a measure of the headquarter state's business friendliness index to account for the incentives and opportunities firms have to engage in non-aggressive tax avoidance, and headquarter-state fixed effects to account for state heterogeneity (e.g., red state versus blue state).

Overall, the evidence is consistent with firms headquartered in amnesty states increasing their tax aggressiveness following an amnesty. The average drop in *STATE ETR* following an initial amnesty is in the range of 0.44 to 0.99 percentage points, which is an 8 to 18 percent decrease from the average *STATE ETR* for amnesty firms. These results are robust to the inclusion of important additional control variables and fixed effects, as well as to several alternate research design choices (such as alternative control groups and matching criteria).

In addition, we find evidence consistent with higher levels of tax amnesty repetition (i.e., when a state offers its second, third, fourth or fifth amnesty) being positively associated with levels of state tax aggressiveness. Whereas the first amnesty is associated with an average drop in *STATE ETR* of 0.64 percentage points in this test, the second amnesty is associated with a drop of 0.69 percentage points and the third (and subsequent) amnesties are associated with a

[7] This research design is subject to both strengths and weaknesses, which are discussed in detail in Section 5 and the Online Appendix.

drop of 1.25 percentage points. Overall, our findings lead us to conclude that tax forgiveness events are associated with increased tax aggressiveness, and that tax aggressiveness is increasing in the number of forgiveness repetitions.

To understand the economic magnitude of these effects, we compare them to the actual revenues recovered for the average amnesty, which Mikesell and Ross (2012) report to be approximately $100M. While this number may sound large, researchers have noted that there may subsequent loss of revenues from compliant taxpayers who sense unfairness or a sense of low enforcement capacity by the tax authority (e.g., Baer and Le Borgne 2008). Our estimates suggest that among small public firms with headquarters in amnesty granting states where we find the increased aggressiveness is most concentrated, corporate tax collections across future years decreased by $28M for the average amnesty.[8] By contrast, relative to the 2011 single-year average state corporate tax receipts of $804 million, the multiyear future revenue losses are small. Overall, these numbers suggest that future noncompliance has an economically meaningful impact on the amounts collected in tax forgiveness programs, but compared to total corporate receipts, the effect is relatively small.

We perform a variety of tests to mitigate empirical concerns and rule out alternate explanations. First, we find the empirical results are primarily concentrated within small firms. This finding is consistent with the notion that large firms are less likely to respond to an amnesty as they are already under more constant scrutiny and face high detection risk. This test also helps address the measurement issues that arise from using *STATE ETR*, which is a blended rate representing the overall state tax burden for all states where the company does business, and could thus mute the effect of an amnesty in a single state. Second, we find that our results

[8] The details supporting this estimate are discussed at the end of Section 4.

continue to hold in short-term tests (i.e., two years either side of the effective date of the amnesty), although statistical significance is somewhat lower in these tests. Third, we employ two additional databases to identify corporate headquarter locations since 1993, and find evidence suggesting that our results are not due to headquarter relocations. Finally, we find that our results continue to hold after tightening or altering the matching procedure, removing unique controls firms (such as those in zero-tax states) or employing different sample criteria.

Even with the battery of robustness tests and alternate samples, there are some empirical limitations that remain. First, our measure of tax aggressiveness, *STATE ETR*, will inevitably capture some non-aggressive strategies. Second, *STATE ETR* is a blended rate that will capture tax strategies across the multiple states where the firm has operations. Third, tax amnesties do not occur at random, but are conducted by states based on political and economic factors. For each of these concerns, we employ multiple robustness tests.[9] However, we cannot rule these concern out entirely, and thus we encourage readers to interpret all results within the scope of these limitations.

This study demonstrates potential firm-level consequences faced by offering tax forgiveness programs, which would be useful for state tax policy makers interested in understanding the consequences of policy choices. Although the economics literature has examined the effects of amnesties on aggregate tax revenues and individual taxpayer compliance (i.e., from the perspective of the government or the individual taxpayer), to our knowledge, this is the first study that examines the effects of tax forgiveness programs on tax aggressiveness from the perspective of the firm.

[9] The Online Appendix contains a detailed discussion of the empirical limitations we face, as well as many of the robustness tests we employ to address them.

Examining firms is important because, relative to most individual taxpayers, they are more capable of employing strategic tax planning, more geographically disperse, and more sensitive to competitive pressures.[10] There are many state tax planning strategies that firms could take during the sample period, including the Delaware holding company (Dyreng, Lindsey and Thornock 2013), captive real estate investment trusts (see the Walmart example reported by Drucker 2007), profit sharing with special purpose entities (Demere, Donohoe and Lisowsky 2015), internal debt allocation, and abusive use of management companies (Barnwell 2009). Thus, public firms who use these schemes differ from the typical taxpayer affected by tax amnesties, as the typical taxpayer is an unknown individual, unregistered small business or larger corporations that "come into the fold" for the first time. This distinction also separates our research from prior research on tax amnesties which primarily focuses on the fiscal implications of amnesties for states or compliance implications for individuals.

The rest of the paper is organized as follows: Section 2 develops hypotheses; Section 3 discusses the data and empirical design; Section 4 presents the primary empirical results; Section 5 presents alternate and robustness tests; and Section 6 concludes.

2. Background and Hypotheses Development

2.1 Background

Tax amnesties have received broad usage across countries and states. Tax amnesty programs are generally conducted by states in order to achieve two objectives (Baer and Le Borgne 2008). First, tax amnesties are designed to generate short-term revenue gains for local governments and are often enacted during periods of macroeconomic downturns and tightened

[10] Barnwell (2009) provides a history of state tax planning strategies over the past three decades and details a number of available strategies.

budgets.[11] Second, tax amnesties are designed to provide a longer-term benefit by adding new taxpayers to the system, thereby broadening the tax base for current and future tax years. Baer and Le Borgne (2008) summarize the literature on tax amnesties, including a full discussion of the types of amnesties, the frequency of amnesties, and the potential costs and benefits of amnesties.

As a case study of income tax amnesties, we provide some basic details on the business tax amnesty conducted by the state of Pennsylvania in 2010 that allowed errant taxpayers, including both individuals and corporations, to remit outstanding taxes without paying penalties and with minimal interest.[12] The program provided amnesty for more than 12,000 businesses of various sizes, which each paid an average of $20,000 in back taxes and interest. The amnesty for these corporations generated $171 million dollars in corporate *income* tax revenues for Pennsylvania, including interest payments of about $20 million dollars. Amnesty revenues from all *other* types of taxes (e.g., sales and use, employer withholding) generated the remaining $83 million of the $254 million total revenues. Moreover, the program broadened the tax base by registering over 1,500 previously unregistered businesses, which brought in more than $171 million and will continue to yield medium-term benefits going forward.

The post-program report provided by the state of Pennsylvania provides some insights that are relevant to our study. First, nearly half of the new revenues generated by the amnesty program came from *out-of-state businesses* with operations in the state of Pennsylvania, with nearly every state in the U.S represented among the list of states with amnesty participants. This

[11] This can present an empirical challenge because the effect of an amnesty can be confounded by unknown macroeconomic and political effects—we discuss this challenge in the Online Appendix.

[12] http://www.revenue.pa.gov/GeneralTaxInformation/News%20and%20Statistics/Documents/2010_final_amnesty_report.pdf

is consistent with the intuition that some larger corporations with some operations within a given state like Pennsylvania might not be filing a state tax return because they might mistakenly believe that they do not have nexus within the state. However, because we only have data on the *STATE ETR* for the firm across all states and not ETRs by state for each firm, we cannot assess firm's responses along this nexus dimension. Second, while about 500 participants paid more than half of the total amnesty collections ($153 million of a total of $254 million), the majority of back taxes were small. This insight lends support for our tests of small firms in Section 5. Finally, the report indicates that the explicit costs of administering the amnesty program, about $12.6 million, were a small fraction of the gross revenues of over $250 million across all tax types. However, there may be hidden costs of these programs that are more difficult to ascertain. For example, Baer and Le Borgne (2008) point out that an amnesty's success should be compared against the "eventual reduction in taxpayer compliance resulting from the loss of credibility of the tax administration…" (p. 2). In the next section, we consider the economic implications of the amnesty for other taxpayers in the jurisdiction, whose reaction to the program could lead to decreased effective tax rates (i.e., increased tax aggressiveness).

2.2 Tax aggressiveness for the first instance of tax forgiveness

Explicit tax reduction ranges from the unquestionably legal (e.g., investing in municipal bonds) to fraudulent (e.g., tax evasion). The economics of more aggressive forms of tax avoidance are rooted in Becker's (1968) seminal model of the economics of crime. Drawing upon that theory, Allingham and Sandmo (1972) model tax non-compliance as a risky economic decision, in which an individual taxpayer chooses the level of taxable income to report to tax authorities in the face of risky tax penalties if caught underreporting that income. These theories have motivated a large body of subsequent research on tax compliance in which the taxpayer is an amoral, rational decision-maker who decides whether or not to underreport income (and therefore cheat on taxes) based on the maximum utility derived from retaining the unpaid tax dollars after factoring in the likelihood of getting caught and the associated penalties (see Slemrod 2004 for a review).[13]

When applied to the setting of a tax forgiveness event, the intuition of the traditional cost-benefit model of tax compliance suggests that taxpayers will become more tax aggressive to the extent that tax forgiveness programs are perceived to indicate a decline in the likelihood of getting caught. For instance, tax forgiveness events can themselves be an indication of poor enforcement as they reveal the fact that a non-trivial number of taxpayers have been non-compliant. Moreover, amnesties are often driven by political and economic incentives to raise revenues quickly. However, the tax administrator is not necessarily part of the decision process, but ends up bearing the compliance and enforcement burden of the amnesty with the same level of resources, which can in turn lower their detection capacity. These arguments are arguably more salient for repeated tax forgiveness programs, which can have two additional effects on future tax aggressiveness: first, by changing taxpayers' perceptions of the credibility of tax authorities' claims and second, by altering taxpayers' expectations for future tax forgiveness events (Baer and Le Borgne 2008). Indeed, our discussions with tax administrators associated with state tax amnesties reveal that a tax amnesty "changed the dialogue" as taxpayers repeatedly sought and lobbied for additional amnesty.

H1: There is an increase in corporate tax aggressiveness following a tax forgiveness event.

[13] The early tax compliance models primarily address risk averse individuals, but see Mills and Sansing (2000), Mills, Robinson and Sansing (2010) and De Simone, Sansing and Seidman (2013) for applications of tax compliance models to firms that incorporate both tax evasion and financial reporting incentives. None of these single-period models consider amnesty programs or repeated game incentives.

On the other hand, to the extent that tax forgiveness events give the perception of increased tax enforcement, the rational taxpayer facing an economic cost/benefit decision regarding tax compliance could *decrease* their level of tax aggressiveness (Alm and Beck 1991). From a practical perspective, to the extent that firms reveal or become more transparent in their current tax strategies as a result of the tax amnesty, this can lead to decreased tax aggressiveness because their options for tax planning techniques have been diminished. Tax forgiveness events can give the perception of increased enforcement because they are often bundled with increased future enforcement and/or penalties, such as increased fines and penalties, stricter enforcement, new enforcement technology, increased jail time and major tax reforms (Mikesell and Ross 2012). Mikesell (1986) and Alm and Beck (1991) note that these accompanying programs could potentially make tax non-compliance much more costly. Alm et al. (1990) use a controlled experiment to disentangle these two effects (in an individual rather than corporate tax setting) and find that the average level of individual taxpayer compliance declines (that is, tax aggressiveness increases) after an authority offers tax forgiveness, and increases after an authority increases enforcement efforts. Moreover, they find that when a forgiveness event is accompanied by increased enforcement, compliance increases by more than an identical increase in enforcement alone. Other studies, however, have produced mixed evidence with regards to the implication that forgiveness bundled with increased enforcement can significantly boost compliance (e.g., Mikesell 1986; Alm and Beck 1993).

2.3 Tax Aggressiveness for Repeated Instances of Tax Forgiveness

We distinguish our study from prior research in two ways—we examine the effects of tax forgiveness programs (1) *from the perspective of the firm (2) when tax forgiveness events are repeated*. In our judgment, no paper has examined these two things in tandem. Much of the prior research has focused primarily on individual taxpayers; however, we expect that firms will react differently than individuals to tax forgiveness events. Firms operate in a competitive environment and are generally more sophisticated than individuals and therefore more able to employ strategic tax planning to reduce taxes paid. Moreover, while the average individual taxpayer is likely risk averse in tax planning, firms will be relatively more risk neutral in tax planning because firms compensate managers with equity incentives to offset individual risk aversion (Rego and Wilson 2012).

On the other hand, firms are also subject to reputational concerns that may inhibit their willingness to aggressively tax plan. Moreover, many firms operate in multiple jurisdictions, which may reduce their responses to a single jurisdiction's tax amnesty.

In addition, the typical study examines the revenue and compliance effect of state tax forgiveness programs without distinguishing between single and repeated amnesties (e.g., Mikesell and Ross 2012). This comment is not meant to disparage these studies, because understanding the revenue and policy implications of a given amnesty for individual taxpayers is very important. Instead, we highlight this to point out that different taxpayers and different iterations of amnesties may actually yield different responses and have different implications.

Although the literature is replete with studies of single tax forgiveness events, very little empirical research has used archival data to examine the effects of repeated tax forgiveness. An exception is Luitel and Sobel (2007), who use a panel of state quarterly tax revenue data to show that repeated tax forgiveness leads to less revenue collection in the actual amnesty period and magnifies revenue losses in the long-run. As noted in Mikesell and Ross (2012), an issue with Luitel and Sobel (2007) is that it is unclear whether repeated amnesties are the result or the cause of lower tax revenues. This issue highlights a weakness of using broad state level measures and simple cross-sectional tests, as opposed to examining the question at the firm level using staggered amnesties in a difference-in-difference design, which allows for a more clear identification of an amnesty effect.

When a tax forgiveness event is repeated, we posit there are three potential effects on a firm's level of future tax aggressiveness that are unique relative to the effects of a single forgiveness event. First, the tax authority can lose credibility in the eyes of taxpayers because they are again repeating a program that are often alleged to be "one-time only." Second, and relatedly, the perceived likelihood of the taxing authority subsequently offering additional amnesties can also increase. Third, in a competitive economy, firms that were not avoiding taxes may feel they are at an increasing competitive disadvantage to tax avoiding firms as the number of forgiveness events increases, which in turn can affect their level of tax aggressiveness.

While repeated amnesties could include provisions that increase enforcement resources or penalties for overly aggressive tax positions, these enforcement effects are likely to be tempered by the loss of credibility of the tax authority, changes in perceived competitiveness, and the perceived probability of future forgiveness. Accordingly, we posit the following directional hypothesis:

H2: Firms respond to repeated state tax forgiveness events with increasing levels of tax aggressiveness.

We now describe the sample and research design implemented to test these hypotheses.

3. Sample, descriptive statistics, and empirical design

3.1 Sample

Our objective is to assess firms' changes in tax aggressiveness subsequent to income tax forgiveness. We begin with a comprehensive list of state tax amnesties maintained by the

Federation of Tax Administrators (FTA), which contains details on the types of delinquent taxes included in each amnesty program.[14] We exclude amnesties that applied exclusively to types of taxation other than corporate income taxes, such as sales taxes or individual income taxes. Between 1980 and 2012, states offered 99 amnesties related to corporate income taxes. In that time period, 39 states offered at least one corporate income tax amnesty and 30 states offered multiple corporate income tax amnesties. The Online Appendix details the criteria we use to determine which amnesties are included in our amnesty listing and lists these actual amnesties by state and the year in which the amnesty period began.

We expect that firms will respond to tax amnesties by adjusting their level of tax aggressiveness. To capture the firm's average level of state tax aggressiveness, we use the annual state effective tax rate (*STATE ETR*) for each firm-year using Compustat data for all firms headquartered in the United States since 1978.[15] We compute *STATE ETR* as state tax expense (TXS) divided by the firm's pretax domestic income (PIDOM), or pretax income (PI) if detail on domestic and foreign income is not provided.[16] Lower *STATE ETR*s proxy for higher state tax aggressiveness and higher *STATE ETR*s proxy for lower state tax aggressiveness. However, measuring tax aggressiveness is challenging because firms do not publicly disclose their "tax aggressiveness"—they simply report their tax expense. Therefore, the empirical models include controls to pick up the incentives for legitimate and normal tax avoidance. For example, we include two time-varying state-level measures—the state statutory tax rate and the state's business friendliness index—which capture state-specific incentives and opportunities for normal

[14] This listing of amnesties has also been used in other studies (e.g., Mikesell and Ross 2012).
[15] Using the firm's headquarter *STATE ETR* can be problematic because firms often operate and pay taxes in multiple states, not just the headquarter state—we further discuss this issue and present alternate approaches in Section 5 and the Online Appendix.
[16] Compustat variable pneumonics are listed in all caps in the parentheses, while computed variables are always shown in italics.

tax avoidance. In addition, we include several firm-level variables that capture the firm's general incentives for tax avoidance, including their federal effective tax rate, firm size, leverage and other firm attributes. By including these tax avoidance variables in the empirical estimation, the remaining variation in state effective tax rates is more likely to be driven by tax aggressiveness.

As summarized in Table 2, we impose the following data requirements for a firm-year observation to be included in our sample. First, we remove financial firm-years from the sample, as they often face differing regulatory and tax issues than other firms. Second, we require complete data in Compustat for all test variables (listed in Appendix 1). Third, we remove all firm-year observations that have a pre-tax loss to aid in the interpretation of *STATE ETR*, consistent with other research using state effective tax rates (e.g., Gupta and Mills 2002). Finally, we exclude remaining firm-year observations that have state and federal effective tax rates that fall outside of the range [0, 1].[17] Other than *STATE ETR*, we winsorize all continuous variables at the 1% level and the 99% level. There are 77,342 firm-years that meet these criteria.

We employ a matched control sample of firms that are headquartered in a few states that do not offer a corporate tax amnesty during our sample period. To create the matched sample, we first classify each of the 77,342 firm-year observations into ranked deciles based on market value of equity (*MVE*) pooled across all years. We then split the full sample into two subsamples. The first subsample is composed of 63,095 observations headquartered in states that offered at least one corporate income tax amnesty during our sample period. The second subsample is composed of 14,247 observations headquartered in states that did not offer a corporate income tax amnesty during the sample period. For each observation in the amnesty

[17] Observations outside this range represent extreme tax rates not driven by conscious tax planning; rather, these observations are likely artifacts of differences in book-tax accounting, operational changes (e.g., restructuring or goodwill charges) or a small denominator effect.

subsample, we look for a matching observation in the non-amnesty subsample in the same year, industry (using the Fama & French 17 industry classification), and size decile. If multiple potential matches are found within those criteria, we choose the firm-year observation with the closest return on assets (*ROA*) to serve as the matched observation. We match "with replacement" and allow firms from the non-amnesty subsample to be matched with multiple observations from the amnesty subsample. We find 56,497 suitable matches, which gives us a "treatment group" of 56,497 observations, and a corresponding "control group" that also has 56,497 observations.[18]

3.2 Descriptive statistics

We present summary descriptive statistics in Table 3, Panel A for both amnesty firm and matched control firm observations. The median *STATE ETR* in the amnesty observations is 4.7%, while that for the control observations is 3.4%, and the difference in medians is statistically significant.[19] That difference, however, is likely driven in part by the large difference between the *statutory tax rates* of amnesty versus non-amnesty states; many of the non-amnesty states do not collect income taxes from firms, and thus control firms (while still paying state income taxes in other states in which they have operations) have a median

[18] By allowing a single control firm to serve as a match for multiple treatment firms, we may be susceptible to undue influence by the repeated control firms. However, by allowing the firms in the control group to serve as matches for multiple treatment firms, we greatly increase the number of usable observations in our sample. We also note that our fixed effects estimation described below should alleviate the effects of industries or states that have a disproportionate number of observations in the control group. We cluster standard errors by firm to reflect the multiple occurrences of the same control firms.

[19] In Panel A of Table 3, we assess statistical significance by median regression of the test variable on an amnesty indicator and clustering by firm to account for serial correlation.

headquarter statutory rate, *HQ STATE RATE*, of 0%.[20] In robustness tests reported in the Online Appendix, we consider a control sample of firm-years that excludes those headquartered in zero-tax states. Importantly, we find that the two groups exhibit no statistical difference in the median federal effective tax rate (*FEDERAL ETR*), which we interpret to imply that the two groups are not different in federal tax avoidance in general (i.e., the federal tax burden is similar for both treatment and control observations). We acknowledge, however, that finding an adequate control group is challenging because there are only a handful of states that have not offered an amnesty; in all our primary analyses, we include state fixed effects and several time-varying state-level controls to account for these empirical concerns. In addition, we provide test results using two alternative control matching procedures in the Online Appendix.

Figure 1 presents the average *STATE ETR* and *HQ STATE RATE* over the sample period for the treatment and control firms. The figure shows that for amnesty states, there is an increasing spread between the statutory tax rate and the average *STATE ETR*. For non-amnesty states, the figure shows that the statutory tax rate and average *STATE ETR* move in tandem throughout the sample period. The figure also shows that average state statutory tax rates for amnesty states stayed quite stable throughout the sample period, suggesting that the effect is not driven by changes in legal tax rates. Overall, the figure provides visual evidence that firms in amnesty states appear to exhibit a relative decline in their *STATE ETR*, while these firms face a relatively flat statutory tax rate over the sample period.

3.3 Empirical design

[20] There are several concerns related to the difference in *STATE ETR* between the treatment and control samples. First, there is the concern that treatment firms face higher statutory state rates and therefore have much more incentive for tax aggressiveness. Second, and relatedly, treatment firms also have much “room” for tax aggressiveness, in that their opportunities for tax aggressiveness are likely greater. We attempt to account for these concerns with a difference-in-differences research design, which accounts for temporal changes in *STATE ETR*, and with state-level controls (including the state statutory tax rate and headquarter-state fixed effects) and the firm’s *FEDERAL ETR*, which account for the firm’s tax aggressiveness incentives and activity.

We use two variations of a difference-in-differences design to test our hypotheses. The first variation is specified as follows:

$$STATE\ ETR_{it} = \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 POST\ FIRST\ AMNESTY_{it} + \alpha_3 AMNESTY\ STATE_i * POST\ FIRST\ AMNESTY_{it} + \gamma CONTROLS_{it} + \varepsilon_{it}. \quad (1)$$

In equation (1), *AMNESTY STATE* is an indicator variable that equals one for firms with headquarters in a state that at some point implemented an amnesty on corporate taxes, and zero otherwise.[21] *POST FIRST AMNESTY* is an indicator variable that equals one for all firm-year observations (both treatment and control firms) in the year of and years following a tax amnesty, and zero otherwise.[22] We estimate equation (1) including a vector of controls (*CONTROLS*) that includes sales revenues, leverage, market capitalization, R&D expense, capital intensity, time-varying headquarter state statutory tax rates, and the federal effective tax rate, all of which are as defined in Appendix 1.[23] The model includes year and industry fixed effects, as well as headquarter state fixed effects where indicated.[24] Standard errors are clustered by firm and year.

The coefficient, α_3, in equation (1) addresses our first hypothesis (H1). $\alpha_3 < 0$ suggests that *STATE ETRs* decrease in the treatment group following an amnesty program, consistent with

[21] Note that a given firm's headquarters can change over time, but Compustat's identification of headquarter state is static because it is based on the most recent location. We consider the effect of this measurement error in the Online Appendix.
[22] For a given firm in the treatment group, *POST FIRST AMNESTY* is set equal to one for all years after the firm's headquarter state grants their initial amnesty program. For a given firm in the control group, *POST FIRST AMNESTY* is artificially set to one when the corresponding matched firm in the treatment group is also set to one.
[23] The headquarter-state statutory tax rates account for over-time variation in the state's statutory tax rate for corporations. However, one shortcoming of statutory tax rates is that they do not account for changes in tax base or in tax credits. *BUSINESS FRIENDLINESS* rankings are largely a function of tax incentives and should help control for changes to the tax base. Also, because of limited data in the early part of our sample, we omit control variables that proxy for several determinants of tax avoidance studied in prior literature (e.g., incentive compensation (Rego and Wilson 2012), managerial style (Dyreng, Hanlon, Maydew 2010), etc.). To the extent that these omitted and unavailable control variables are not systematically correlated with states' amnesty schedules, our results should be unbiased.
[24] When the model includes state fixed effects, we drop *AMNESTY STATE* from the estimation because it does not vary over time and is therefore perfectly collinear with the state fixed effects.

the notion that, on average, firms are more tax aggressive after the first instance of tax forgiveness.

To test our second hypothesis of how firms respond to repeated forgiveness, we extend equation (1) by including an additional indicator variable (*POST REPEAT AMNESTY*) for repeated amnesties as follows:

$$\begin{aligned} STATE\ ETR_{it} = {} & \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 POST\ FIRST\ AMNESTY_{it} \\ & + \alpha_3 POST\ REPEAT\ AMNESTY_{it} + \alpha_4 AMNESTY\ STATE_i * POST\ FIRST\ AMNESTY_{it} \\ & + \alpha_5 AMNESTY\ STATE_i * POST\ REPEAT\ AMENSTY_{it} + \gamma CONTROLS_{it} + \varepsilon_{it}. \qquad (2) \end{aligned}$$

In the treatment group, *POST REPEAT AMNESTY* is an indicator variable set equal to one for all firm-years after the state in which the firm is headquartered grants a second amnesty (including firm-years after any more additional amnesties). In the control group, observations take on the value for *POST REPEAT AMNESTY* shared by their matched observation in the treatment group. The Online Appendix illustrates the way the timing variables are operationalized, and includes both an example and a timeline of each of the timing measures. H2 predicts $\alpha_5 < 0$, i.e., that firms are more tax aggressive when they operate in a tax environment that *repeatedly* forgives taxpayers.

We also provide an alternative design for testing H2 in which the constructs of forgiveness and repeated forgiveness are operationalized through indicators grouped by the n^{th} amnesty. *N1* is an indicator variable set equal to one for the group of firm-year observations that are headquartered in states that have previously offered one (and only one) corporate income tax amnesty at the time of the observation. *N2* is similarly equal to one for the groups of firm-year observations that are headquartered in states that have previously offered two (and only two) corporate income tax amnesties at the time of observation. *N3+* is set equal to one for all firm-year observations that have previously been offered at least three amnesties by the state in which the firm has its headquarters. The estimation includes these indicators as follows:

$$STATE\ ETR_{it} = \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 N1_{it} + \alpha_3 N2_{it} + \alpha_4 N3+_{it} + \alpha_5 AMNESTY\ STATE_i * N1_{it} + \alpha_6 AMNESTY\ STATE_i * N2_{it} + \alpha_7 AMNESTY\ STATE_i * N3+_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \quad (3)$$

In estimating equation (3), we employ the same set of control variables, fixed effects, clustering and winsorization techniques as in equation (2).

H2 predicts that α_6 and α_7 are negative and that $0 > \alpha_5 > \alpha_6 > \alpha_7$. This pattern in the coefficients would suggest that relative to firms in the control group, firms that are offered the chance to participate in multiple tax forgiveness events are progressively more tax aggressive. In summary, the empirical models are designed to account for factors that influence both tax aggressiveness by firms and the incidence of tax amnesties by states. The difference-in-differences research design includes a main effect for amnesty states that captures the primary differences for that state. The models also include year fixed effects to account for economic booms and busts, pro-cyclicality and general trends in amnesties; state fixed effects to account for time-invariant political factors (red vs. blue state) and regional effects; and time-varying measures of the statutory tax rate and the state's business friendliness to account for changes in the tax climate. Moreover, the amnesties are staggered across both states and time, which should account for elements unique to a particular state or time period. These design choices are intended to minimize concerns about unobserved or unmeasured variation in tax aggressiveness or the non-random nature of tax amnesties; we consider many robustness tests in Section 5 and the Online Appendix, where we also detail the study's empirical limitations.

4. Empirical results

4.1 Regression results on primary tests

Models 1 and 2 of Table 4 show the results of estimating equation (1) as a test of our first hypothesis—that an initial tax amnesty is associated with an increase in tax aggressiveness.

Models 1 and 2 report the parameter estimates of estimating equation (1) without and with state fixed effects, respectively. In both models, we find a negative and significant coefficient on *AMNESTY STATE *POST FIRST AMNESTY*, which is consistent with firms becoming more tax aggressive following an initial amnesty. Specifically, we find that in Model 2, α_3 is -0.0072 and significant at the one percent level, which suggests that after controlling for the other known determinants of state tax avoidance and relative to the firms in the control group, after firms are offered the chance to participate in a tax forgiveness program by the state in which they are headquartered, their state effective tax rate decreases by 0.72 percentage points. For the amnesty sample, the average *STATE ETR* is 5.5%, so that the regression point estimate implies a 13% decrease in the *STATE ETR*.

Models 3 and 4 of Table 4 show the results of estimating equation (2), which tests H2. We find a negative and significant coefficient on *AMNESTY STATE *POST REPEAT AMNESTY*. Specifically, when we include state fixed effects, we find that α_5 is -0.0026, which is significant at the ten percent level. The estimated coefficient on α_3 (*AMNESTY STATE *POST FIRST AMNESTY*) remains statistically negative with a value of -0.0064, which is consistent with our evidence on H1 from equation (1). The two coefficients imply that, relative to firms in the control group, firms in the treatment group have *STATE ETR*s that are 0.64 percentage points lower than in the pre-period following a single amnesty, while following a second amnesty, *STATE ETR*s decrease by another 0.26 percentage points. The results are robust to the inclusion of important controls, including market capitalization, sales, federal effective tax rate, leverage, R&D, capital intensity, state statutory tax rates, as well as state fixed effects.

Table 5 shows the results of estimating equation (3). Consistent with the evidence on H1 and H2 from Table 4, the estimated coefficient on the interactions between *AMNESTY STATE* and *N1*, *N2*, and *N3+* are all significantly less than zero. Specifically, in Model 3, which includes state fixed effects, α_5 is -0.006, α_6 is -0.007, and α_7 is -0.013, each of which is significantly below zero at the one percent level (one-tailed). We also observe that the coefficient on each iterative amnesty variable is more negative than the previous one—i.e., the coefficients follow the pattern: $\alpha_5 > \alpha_6 > \alpha_7$—which is consistent with firms increasing tax aggressiveness with each additional amnesty. From a statistical perspective, we see that the difference between the first and second amnesty is insignificant (i.e., a test of $\alpha_5 = \alpha_6$ yields an F-stat of 0.17, with a p-value of 0.67) and the difference between the second and third amnesty is statistically significant (i.e., a test of $\alpha_6 = \alpha 7$ yields an F-stat of 6.33, with a p-value of 0.01, one-tailed).

In order to examine the economic magnitude of these estimates, we calculate the average decrease in state tax collections as follows: the coefficient estimates of *AMNESTY STATE*N1*, *AMNESTY STATE*N2*, and *AMNESTY STATE*N3+* presented in Column 1 of Table 6, Panel B are multiplied by total pretax domestic income \$119 billion, \$40 billion, and \$51 billion respectively, which represent the aggregate total pretax domestic income of small firms (i.e., firms with total assets less than the sample median by year) that have headquarters in amnesty states across all years where the state authority has previously offered one, two, or three or more amnesties, respectively. The sum of these three products suggests a total decrease in small firm tax collections across all amnesties in the sample of approximately \$2.7 billion. There are 99 amnesties in our sample, which suggests that the average amnesty is associated with an approximate \$28 million multi-year decrease in future tax collections. This quantification of economic magnitude is subject to several assumptions. To the extent that any of the intermediate assumptions in this calculation are inaccurate, the final estimate will also be affected.

In summary, our results of tests of H1 show that *STATE ETRs* are lower following the first amnesty. Moreover, the results are consistent with H2, in that *STATE ETRs* become progressively lower with each additional amnesty offered by a state. Overall, the results are consistent with tax forgiveness programs being positively associated with tax aggressiveness, and that association becomes more positive with repetitions of forgiveness.

5. Robustness tests

5.1 Small Firms and STATE ETRs

Prior research has shown that large firms are audited much more regularly by federal tax authorities (e.g., Hoopes, Mescall and Pittman 2012). In fact, the largest firms are constantly under federal audit under the IRS's Coordinated Industry Case program (Ayers, Seidman and Towery 2015). The same also likely holds for state tax authorities, who are likely well aware of the largest and most prominent corporate taxpayers in the state. Because these firms are constantly scrutinized, they are less likely to respond to tax amnesties with increased tax aggressiveness. Based on this economic rationale, we predict that an amnesty effect will be more prominent for smaller firms than for larger firms.

To test this notion, we replicate the tests in Tables 4 and 5, but partition the sample into two groups: small firms and large firms. Specifically, we define small firms (large firms) as those firm-years that fall below (above) the yearly sample median of total assets. We then estimate equation (2) for each partition of the sample using the fixed-effects specification presented in in Table 4, Model 4. The results of this test are presented in Table 6, Panel A. The results show strong evidence that the negative association between amnesties and *STATE ETRs* is much more pronounced for small firms. In particular, the coefficients on *AMNESTY STATE*POST FIRST AMNESTY* and *AMNESTY STATE*POST REPEAT AMNESTY* are both negative and significant for the group of small firms, whereas neither of these coefficients is significant in the group of large firms. Moreover, in cross-equation tests (for which we use seemingly unrelated estimation), the coefficients for small firms are statistically greater than those for large firms.

In Panel B of Table 6, we present the results of estimation equation (3) for each of the size-based partitions of the sample, using the fixed-effects specification, as in Table 5, Model 3. Here again, we find consistent evidence that the amnesty effect on tax aggressiveness appears to be concentrated in small firms. The coefficients on the interaction terms (α_5, α_6 and α_7) are negative and significant for the group of small firms, but are insignificant (at the five percent level) for the large firms. In cross-equation tests of coefficient equality, we find that the primary coefficients are much more negative, in both statistical and economic terms, for the group of small firms than they are for the group of large firms. Finally, we find more clear evidence of the pattern that increased repetitions of amnesties are associated with increased tax aggressiveness for the group of small firms—this pattern is not present in the group of large firms. Overall, the cross-sectional tests for different size firms provide more evidence to suggest that amnesties are associated with increased tax aggressiveness, especially for small firms.

These small-firm tests provide another empirical benefit related to the measurement of tax aggressiveness with *STATE ETR*. The *STATE ETR* is actually a blended rate of the state tax expense for the firm across all state and sub-national jurisdictions where the firm has operations, which gives rise to several concerns with the measurement of *STATE ETR* for our study. First, the effect of a given amnesty, which occurs in a single state, will be muted for firms with operations in many states. Second, to the extent that an amnesty occurs in a non-headquarters state, but has an effect on the firm's tax aggressiveness, our test will not be able to detect such an effect. Finally, as noted above, changes in state taxation that are unrelated to amnesties (such as changes in apportionment, combined versus unitary reporting, investment tax credits, R&D credits, etc.) will also affect the *STATE ETR*.[25]

By partitioning the sample into small and large firms, we are isolating the set of firms (i.e., the small firms) for whom this problem is a lesser concern. The economic intuition is that the more concentrated a firm's activity is within its headquarter state, the more likely that the *STATE ETR* is representative of the firm's overall state tax burden in its headquarter state. In untabulated descriptive statistics, we find that these small firms operate in less than half the states as the large firms and have just one fourth the number of subsidiaries that large firms have. Thus, for small firms, the *STATE ETR* is more likely to represent the tax burden in the state where the firm is headquartered. The fact that the results reported in Table 6 are so much stronger for small firms bears this point out—the results are strongest both where firms face less scrutiny and where measurement error in *STATE ETR* is less of an issue.[26]

5.2 Short-window Event Study Around Amnesties

[25] While we cannot control for every change in a state tax regime that can affect the tax base (e.g., apportionment, unitary filing, throwback rules, etc.), it is unlikely that any single correlated omitted factor is driving our results because of the repeated nature of the amnesties and the non-repeated nature of the regime changes. In other words, a state can offer amnesty multiple times, but a state can only switch to unitary filing once. Because we see incremental results from each amnesty, it is unlikely that an omitted characteristic of the state tax system is driving our results.

[26] In addition to the small versus large firm tests, we attempt to overcome this issue with three untabulated tests based on three different sample partitions to approximate the concentration of the firm's operations using location information as reported in the firm's 10-K, as in Dyreng et al. (2013). The three partitions are: firms with high vs low proportion of subsidiaries in the headquarter state; firms with high versus low total number of subsidiaries; firm with high versus low number of states where they operate. We expect that a tax forgiveness program in a particular state will be more salient for firms that have highly concentrated operations in the headquarter state. We find that most of our results continue to persist, though with lower statistical significance, in the "high concentration" subsamples while the results do not persist in the "low concentration" subsamples. These results are untabulated (but available upon request) because these data are only available after 1995 and only for a subset of firms, which removes a substantial portion of the primary sample (i.e., our sample size decreases by more than 70%).

In the primary analyses, we examine firms' responses to amnesty programs in all periods following the amnesty. An issue with this research design choice, however, is that substantial time can pass following an amnesty, which could diminish its impact on the firm's choice to increase tax aggressiveness. For example, California had only one amnesty and it occurred in the mid-1980s. It is quite unlikely that that particular amnesty program continues to have an effect on firm's tax decisions 10 or 20 years later. To address this concern, we re-estimate our primary analyses using a short-window event study design, in which the *STATE ETRs* of amnesty firms are compared to those for non-amnesty firms in a five-year window around the amnesty event (+/- two years prior around the amnesty year). We choose this window somewhat arbitrarily, assuming the two-year "pre" period will serve as a baseline for the subsequent two-year "post" period. This allows us to view the relative change in *STATE ETRs* in the short period before and after the amnesty.

The results of this alternative test are presented in Table 7, with a separate column for each iteration of amnesty (Model 1 for the window around a first amnesty, and so on). Panel A reports results for the full sample, while Panel B reports the results for the subsample of small firms. Overall, we find evidence consistent with H1 using a short-window research design for both the full sample and the subsample of small firms. Specifically, in Model 1, the coefficient on *POST*AMNESTY STATE* is negative (-0.0025) and significant at the five percent level (one-tailed).

In this design, we find that the evidence is weaker for H2. For example, in Model 3, which examines the response to the third amnesty, the coefficient is negative, but only marginally significant ($p < 0.10$, one-tailed), while the coefficient for the second amnesty is not significantly different from zero. Panel B shows fairly similar results, with statistical significance slightly improving for the small firms in this subsample. A limitation of this type of short-window analysis, however, is that tax planning often takes years to implement and adjustment costs are substantial—hence, a short-window test misses what a longer-window analysis, such as that in Table 3, might capture.

5.3 Additional Robustness Tests

In the Online Appendix, we describe and tabulate additional robustness tests that help rule out some specific concerns that could potentially influence the inferences of this study, including that: firms can move headquarters during the sample period (Heider and Ljungqvist 2015 and Engelberg, Ozoguz and Wang 2013); the nature of the firm is changing over time (Fama and French 2001); trends in tax avoidance are changing over time, (Dyreng, Hanlon, Maydew and Thornock 2016); *STATE ETR* must be interpreted differently following FIN 48 (Gupta, Mills, and Towery 2014), etc.); control observations are drawn from a small subset of states and are matched "with replacement"; state laws are systematically different in the latter part of the sample period; and finally, that control observations often come from states with no income tax for corporations. Our results are generally robust in tests designed to address these concerns.

6. Conclusion

This paper examines the effect of tax forgiveness and repeated tax forgiveness on corporate tax aggressiveness. We hypothesize that a single tax forgiveness event will be associated with an increase in the level of corporate tax aggressiveness. Tax forgiveness can increase tax aggressiveness to the extent that it changes rational taxpayers' assessment of the likelihood of getting caught in non-tax compliance or that it changes taxpayers' expectation for future amnesties. To the extent, however, that the program contains an element of increased enforcement, which Mikesell and Ross (2012) note is common place with tax amnesties or to the extent that taxpayers are unaware of the tax forgiveness programs, corporate tax aggressiveness could remain unchanged. Across multiple tests, we find evidence of increased tax aggressiveness. Given that finding, we predict that greater repititions of tax forgiveness will be associated with increasing levels of corporate tax aggressiveness. Our results also bear out this prediction. We believe our study has implications for policy, in that it shows evidence of a cost to tax amnesty programs.

To a degree, our results also speak to the potential effects of an additional tax repatriation holiday, a topic of recent debate in the U.S. (e.g., Sloan 2011; Citizens for Tax Justice 2013; Rauf 2012). For example, some lawmakers are currently considering a tax holiday on corporate offshore earnings that is billed as a "one-time tax break," despite the fact that Congress allowed a corporate tax holiday in the mid-2000s (Stephenson and Temple-West 2014; Blouin and Krull 2009). Although tax amnesties and tax holidays differ along several important dimensions, at a simplified level, they are similar in that both offer an element of tax forgiveness. To the extent that firms anticipate that another forgiveness event may occur, they could become more tax aggressive and recognize more income overseas to reduce U.S. taxes. Indeed, articles in the business press report that U.S. firms are heavily lobbying for a tax holiday (Rubin and Drucker 2011) and make the conjecture that firms are sitting on cash "awaiting a repatriation tax holiday" (Fleischer 2012). Our evidence, albeit in a very different setting, is consistent with such a potential response by U.S. firms to another tax holiday.[27]

[27] However, we emphasize the existence of several clear differences between a tax holiday and a tax amnesty, such as possible differences in taxpayers' level of presumed tax avoidance, differences in financial reporting incentives, and differences in the tax complexity between state and international taxation. Hence, we acknowledge substantial differences in the tax holiday and tax amnesty settings and urge caution in over-interpreting our results in the international setting.

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Appendix 1
Definition and source of variables

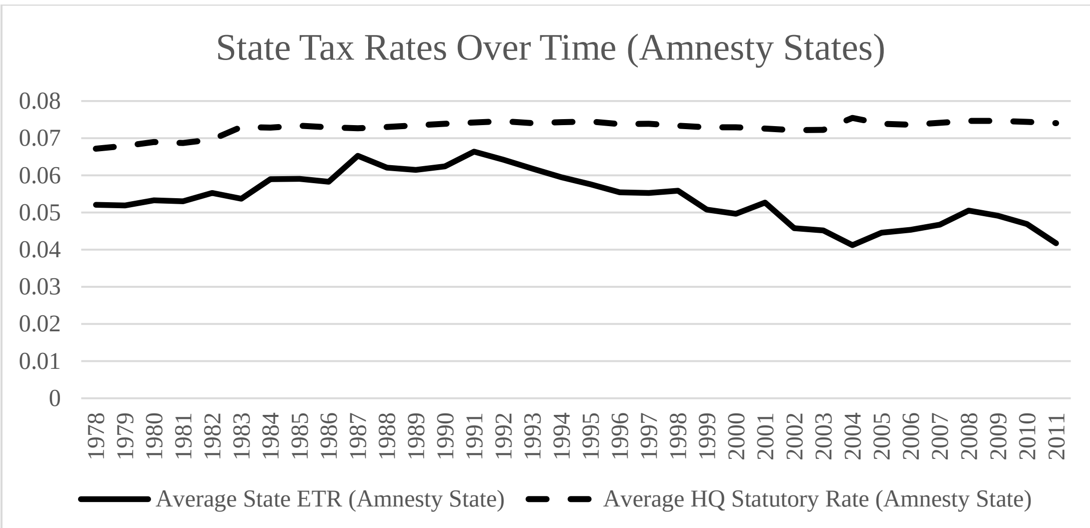
Variable	Description	Calculation	Source
STATE ETR	State effective tax rate, calculated as state tax expense divided by pre-tax income	=TXS/PIDOM; TXS/PI if PIDOM is missing.	Compustat: TXS, PIDOM, PI
AMNESTY STATE	An indicator if the HQ state offered an amnesty at any point in the sample	=1 if the firm's HQ state offered any corporate income tax amnesty program between 1981-2012, and 0 otherwise	Mikesell & Ross (2012); Federation of Tax Administrators
POST FIRST AMNESTY	Post-period indicator	=1 if the HQ state has previously offered at least one amnesty at the time of observation, and 0 otherwise.	Mikesell & Ross (2012); Federation of Tax Administrators
POST REPEAT AMNESTY	Post-period indicator	=1 if the HQ state has previously offered at least two amnesties at the time of observation, and 0 otherwise.	Mikesell & Ross (2012); Federation of Tax Administrators
N1	Amnesty count indicator	=1 if HQ state has previously offered one (and only one) amnesty at the time of observation, and 0 otherwise.	Mikesell & Ross (2012); Federation of Tax Administrators
N2	Amnesty count indicator	=1 if HQ state has previously offered two (and only two) amnesties at the time of observation, and 0 otherwise.	Mikesell & Ross (2012); Federation of Tax Administrators
N3+	Amnesty count indicator	=1 if HQ state has previously offered three or more amnesties at the time of observation, and 0 otherwise.	Mikesell & Ross (2012); Federation of Tax Administrators
SALES	Total revenue (logged for regressions)	=ln(REVT)	Compustat: REVT
LEVERAGE	Total liabilities divided by total assets	=LT/AT	Compustat: LT, AT
SIZE	The equity market capitalization.	=CSHO*PRCC_F	Compustat: CSCHO, PRCC_F
R&D EXPENSE	The amount spent on research and development, scaled by total revenue.	=XRD/REVT; if XRD is missing then 0.	Compustat: XRD, REVT
CAPITAL INTENSITY	The amount property, plant, and equipment, scaled by total assets.	=PPEGT/AT	Compustat: PPEGT, AT
HQ STATE RATE	The statutory corporate income tax rate the firm's headquarter state.	=top statutory corporate income tax rate in headquarter state	CCH manuals
FEDERAL ETR	Federal effective tax rate, calculated as federal tax expense divided by pre-tax income	=TXFED/PIDOM; or TXFED/PI if PIDOM is missing	Compustat: TXFED, PIDOM, PI
BUSINESS FRIENDLINESS	An annual rank of the business friendliness of a given state relative to the 49 other states.	=Rank value of 1 (least business friendly) to 50 (most business friendly)	Tax Foundation's Annual State Business Tax Climate Index

*Compustat data item mnemonics are all presented in capital letters.

Figure 1
Time Trend in Tax Rates for Amnesty and Non-Amnesty States

This figure presents plots of average state tax rates over time. Panel A (B) plots the average state effective tax rate over time for sample firms that have headquarters in a state that offered (did not offer) a state income tax amnesty program during our sample period. Both panels also plot the average statutory rate across these states during the sample period. The average statutory rate plotted in Panel B includes states with no corporate income tax.

Panel A: State income tax rates over time (Amnesty states)



Panel B: State income tax rates over time (Non-amnesty states)

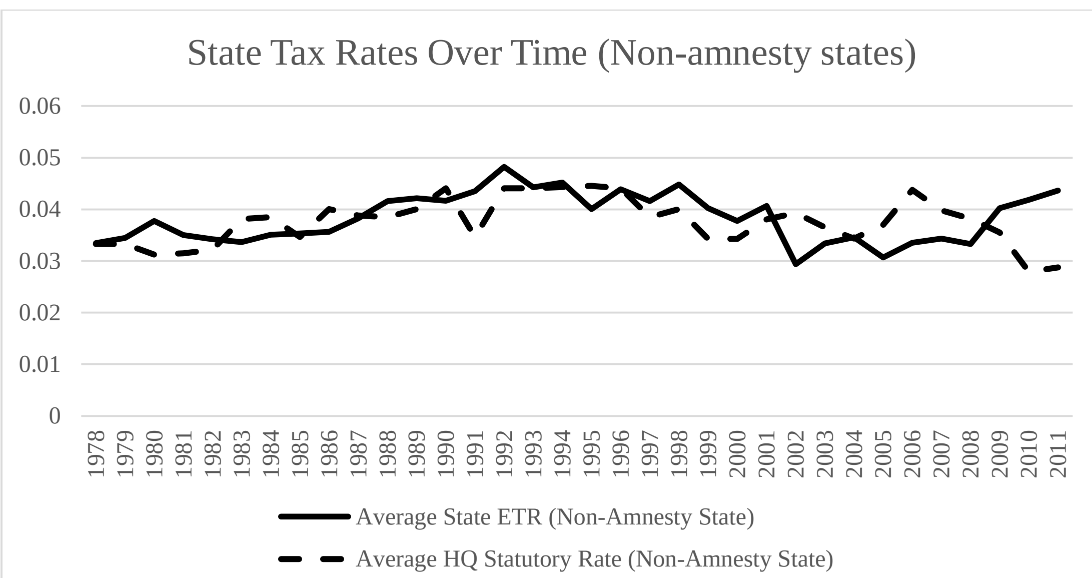


Table 1
Amnesty Selection

This table presents the sample selection of amnesties by reconciling our sample of amnesties that cover corporate income taxes with the Mikesell and Ross (2012) sample of amnesties that cover all tax types.

Number of amnesties from Mikesell & Ross (2012)		117
Less: all amnesties in states with no income tax		
	Nevada (2002, 2008, 2010)	-3
	South Dakota (1999)	-1
		-4
Less: amnesties for taxes on bases other than corporate income		
	Arizona Individual Income Tax (2002)	-1
	California Sales Tax (2005)	-1
	Florida Intangibles Tax (1987)	-1
	Idaho Individual Income Tax (1983)	-1
	Ohio Commercial Activity Tax (2002, 2006, 2012)	-3
	Texas Franchise & Gross Receipts Tax (1984, 2004, 2007)	-3
	Washington Business & Occupation Tax (2011)	-1
		-11
Less: amnesties in non-state jurisdictions		
	District of Columbia (1987, 1995, 2010)	-3
		-3
Number of amnesties in sample		99

Table 2
Sample Selection

This table details the criteria applied to select the firm-year observations for our sample. We begin our sample period three years before the first amnesty.

Initial sample of U.S. firm-year observations (1978 - 2012).		256,081
LESS: Financial firms-years (6000s)	(45,747)	210,334
LESS: Non-meaningful observations	(80,207)	80,207
Total Assets <=0		
MVE <=0		
Negative book income		
State or federal ETR <0		
State or federal ETR>1		
LESS: Observations missing state information	(2,339)	77,868
LESS: Observations missing other controls	(526)	77,342
Detail		
Firms HQ in amnesty-granting state		63,095
Firms HQ in non-amnesty-granting state		14,247
Total Firm-Years in Full Sample		77,342
Matched Sample		
Firms HQ in amnesty states (Treatment)		56,497
Firms HQ in non-amnesty states (Control)		56,497
Total Firms in Matched Sample		112,994

Table 3
Descriptive Statistics and Correlations

This table presents descriptive statistics and correlations for the variables used in our analyses. In Panel A, we present univariate summary statistics for firms in our sample, splitting on whether the firm has its corporate headquarters in a state that sponsored a corporate income tax amnesty program anytime during our sample window (Amnesty Sample) or not (Matched Sample). The sample consists of 56,497 firm-year observations from each group over the time period 1978-2012. In Panel B, we present Pearson (above) and Spearman (below) pairwise correlations. All variables are defined in Appendix 1. Continuous variables are winsorized at the 1% and 99% levels. In Panel A, ** indicates a statistically significant difference (at the 5% level) in the medians between the two samples.

Panel A: Descriptive Statistics

	AMNESTY SAMPLE					MATCHED SAMPLE			
VARIABLE	**N**	**Mean**	**Median**		**Std. Dev.**	**N**	**Mean**	**Median**	**Std. Dev.**
STATE ETR	56,497	0.055	0.047	**	0.058	56,497	0.039	0.034	0.041
FEDERAL ETR	56,497	0.237	0.264		0.162	56,497	0.241	0.267	0.161
HQ STATE RATE	56,497	0.078	0.088	**	0.022	56,497	0.032	0.000	0.039
REVENUE	56,497	1,598	165		8,111	56,497	1,680	193	6,891
ROA	56,497	0.115	0.097		0.083	56,497	0.108	0.094	0.070
LEVERAGE	56,497	0.475	0.481	**	0.214	56,497	0.492	0.499	0.200
SIZE	56,497	2,002	136		12,787	56,497	2,334	137	14,841
R&D EXPENSE	56,497	0.027	0.000		0.050	56,497	0.017	0.000	0.040
CAPITAL INTENSITY	56,497	0.527	0.450	**	0.361	56,497	0.568	0.507	0.355
BUS. FRIENDLINESS	56,497	16.4	13.0	**	12.6	56,497	36.5	44.0	12.6

Panel B: Correlation Table

	VARIABLE	1	2	3	4	5	6	7	8	9	10
1	*STATE ETR*		0.30	0.21	-0.03	-0.05	0.00	-0.03	-0.03	-0.10	-0.19
2	*FEDERAL ETR*	0.44		0.04	0.03	0.19	-0.14	0.04	-0.04	-0.13	-0.02
3	*HQ STATE RATE*	0.28	0.04		-0.02	0.04	-0.02	-0.03	0.06	-0.05	-0.77
4	*REVENUE*	0.08	0.16	-0.03		0.01	0.11	0.62	-0.01	0.04	-0.02
5	*ROA*	0.08	0.28	0.04	0.03		-0.31	0.07	0.08	-0.13	-0.05
6	*LEVERAGE*	-0.04	-0.15	-0.04	0.30	-0.36		0.03	-0.30	0.20	0.06
7	*SIZE*	0.02	0.12	-0.04	0.87	0.16	0.07		0.08	0.00	-0.02
8	*R&D EXPENSE*	-0.04	0.00	0.11	-0.10	0.14	-0.30	0.06		-0.26	-0.14
9	*CAPITAL INTENSITY*	-0.12	-0.12	-0.05	0.17	-0.10	0.22	0.09	-0.24		0.10
10	*BUSINESS FRIENDLINESS*	-0.24	-0.02	-0.77	0.01	-0.03	0.06	-0.01	-0.17	0.10	

Table 4
The Effect of Tax Amnesties on Subsequent Tax Aggressiveness

This table presents the results of estimating equations (1) and (2):

$$STATE\ ETR_{it} = \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 POST\ FIRST\ AMNESTY_{it} + \alpha_3 AMNESTY\ STATE * POST\ FIRST\ AMNESTY_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \quad (1)$$

$$STATE\ ETR_{it} = \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 POST\ FIRST\ AMNESTY_{it} + \alpha_3 AMNESTY\ STATE * POST\ FIRST\ AMNESTY_{it} + \alpha_4 POST\ REPEAT\ AMNESTY_{it} + \alpha_5 AMNESTY\ STATE * POST\ REPEAT\ AMNESTY_{it} + \gamma CONTROLS_{it} + \varepsilon_{it} \quad (2)$$

where *STATE ETR* is the ratio of state tax expense to pretax income; *AMNESTY STATE* is an indicator variable set equal to one if the firm has headquarters in a state that sponsors a corporate income tax amnesty anytime during the sample period; *POST FIRST AMNESTY* is an indicator variable set equal to one if the state in which the firm has headquarters has previously granted a single corporate income tax amnesty as of the time of the observation); and *POST REPEAT AMNESTY* is an indicator variable set equal to one if the state in which the firm has headquarters has previously granted multiple corporate tax amnesties. The model also includes interactions of *AMNESTY STATE* with *POST FIRST AMNESTY* and *POST REPEAT AMNESTY*, as well as controls for sales revenues, leverage, market capitalization, R&D expense, capital intensity, state statutory tax rate of the headquarter state, and the federal effective tax rate, all of which are as defined in Appendix 1. The model includes year and industry fixed effects, as well as state fixed effects where indicated. In Models 2 and 4, *AMNESTY STATE* is not included in the estimation because it is perfectly collinear with the headquarter-state fixed effects. Standard errors are clustered by firm and year. The sample period covers the years 1978 to 2011. Firm-year observations in the treatment group are matched to control firm-year observations that are headquartered in states that have never sponsored an amnesty program, and the values of *POST FIRST AMNESTY* and *POST REPEAT AMNESTY* for control firms are determined by the matched observation from the treatment group. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively, using one-tailed p-values where we make a directional prediction and two-tailed p-values otherwise.

Coef	Variable	Pred.	Model 1	Model 2	Model 3	Model 4
α_1	*AMNESTY STATE*	+/-	0.0099***	(omitted)	0.0099***	(omitted)
			(7.78)		(7.73)	
α_2	*POST FIRST AMNESTY*	+/-	0.0032***	0.0026***	0.0025***	0.0020**
			(3.63)	(3.08)	(3.00)	(2.55)
α_3	*AMNESTY STATE*POST FIRST AMNESTY*	-	-0.0050***	-0.0072***	-0.0041***	-0.0064***
			(-3.70)	(-5.26)	(-3.14)	(-5.02)
α_4	*POST REPEAT AMNESTY*	+/-			0.0033***	0.0029***
					(4.15)	(3.96)
α_5	*AMNESTY STATE*POST REPEAT AMNESTY*	-			-0.0027**	-0.0026*
					(-1.80)	(-1.62)
	SALES		0.0035***	0.0036***	0.0035***	0.0036***
			(6.84)	(7.10)	(6.84)	(7.11)
	LEVERAGE		0.0103***	0.0106***	0.0102***	0.0105***
			(4.72)	(4.84)	(4.70)	(4.83)
	MARKET VALUE OF EQUITY		-0.0048***	-0.0049***	-0.0048***	-0.0049***
			(-10.37)	(-10.74)	(-10.37)	(-10.75)
	RESEARCH AND DEVELOPMENT		-0.0140	-0.0264***	-0.0136	-0.0262***
			(-1.39)	(-2.63)	(-1.34)	(-2.60)

(continued on next page)

Table 4 (Continued)
The Effect of Tax Amnesties on Subsequent Tax Aggressiveness

CAPITAL INTENSITY	-0.0059***	-0.0047***	-0.0059***	-0.0047***
	(-4.17)	(-3.42)	(-4.21)	(-3.45)
STATUTORY RATE	0.1379***	0.1598***	0.1391***	0.1618***
	(6.19)	(5.50)	(6.23)	(5.61)
FEDERAL EFFECTIVE TAX RATE	0.0947***	0.0958***	0.0947***	0.0958***
	(17.35)	(17.53)	(17.34)	(17.52)
BUSINESS FRIENDLINESS	-0.0002***	0.0001	-0.0002***	0.0001
	(-5.97)	(1.33)	(-5.95)	(1.31)
CONTROLS	YES	YES	YES	YES
STATE FIXED EFFECTS	NO	YES	NO	YES
INDUSTRY & YEAR FIXED EFFECTS	YES	YES	YES	YES
CLUSTERS:	FIRM & YEAR	FIRM & YEAR	FIRM & YEAR	FIRM & YEAR
OBSERVATIONS	112,994	112,994	112,994	112,994
ADJUSTED RSQ	0.166	0.178	0.167	0.178

Table 5
The Effect of Repeated Tax Amnesties on Subsequent Tax Aggressiveness

This table presents the results of estimating equation (3):

$$STATE\ ETR_{it} = \alpha_{FE} + \alpha_1 AMNESTY\ STATE_i + \alpha_2 N1_{it} + \alpha_3 N2_{it} + \alpha_4 N3+_{it} \\ +\alpha_5 AMNESTY\ STATE * N1_{it} + \alpha_6 AMNESTY\ STATE * N2_{it} + \alpha_7 AMNESTY\ STATE * N3+_{it} \\ +\gamma CONTROLS_{it} + \varepsilon_{it},$$

where *STATE ETR* is the ratio of state tax expense to pretax income; *AMNESTY STATE* is an indicator variable set equal to one if the firm has headquarters in a state that sponsors a corporate income tax amnesty anytime during the sample period; and *N1*, *N2*, and *N3+* are indicator variables corresponding to the count of the number of times a state has previously offered corporate income tax amnesties as of the time of observation. The model also includes interactions of *AMNESTY STATE* and *N1*, *N2*, and *N3+*, as well as controls for sales revenues, leverage, market capitalization, R&D expense, capital intensity, state statutory tax rates, and the federal effective tax rate, all of which are as defined in Appendix 1. The model includes year and industry fixed effects, as well as state fixed effects where indicated. Standard errors are clustered by firm and year. The sample period covers the years 1978 to 2011. Firms in the treatment group are matched to control firms that are headquartered in states that have never sponsored an amnesty program, and the values of *N1*, *N2,* and *N3+* for control firms are determined by the matched observation from the treatment group. In Model 3, *AMNESTY STATE* is not included in the estimation because it is perfectly collinear with the headquarter-state fixed effects. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively, using one-tailed p-values where we make a directional prediction and two-tailed p-values otherwise.

Coef.	**Variable**	*Pred.*	**Model 1**	**Model 2**	**Model 3**
α_1	*AMNESTY STATE*	+/-	0.0189*** (15.75)	0.0097*** (7.61)	(omitted)
α_2	*N1*	+/-	0.0022** (2.55)	0.0026*** (3.04)	0.0020** (2.56)
α_3	*N2*	+/-	0.0044*** (4.40)	0.0051*** (5.20)	0.0042*** (4.50)
α_4	*N3+*	+/-	0.0061*** (3.92)	0.0072*** (3.82)	0.0057*** (3.27)
α_5	*AMNESTY STATE*N1*	-	-0.0027* (-1.81)	-0.0037*** (-2.83)	-0.0060*** (-4.68)
α_6	*AMNESTY STATE*N2*	-	-0.0061*** (-3.45)	-0.0052*** (-3.46)	-0.0069*** (-4.13)
α_7	*AMNESTY STATE*N3+*	-	-0.0080*** (-3.20)	-0.0092*** (-3.31)	-0.0125*** (-4.44)
	SALES			0.0035*** (6.84)	0.0036*** (7.11)
	LEVERAGE			0.0102*** (4.68)	0.0105*** (4.80)
	MARKET VALUE OF EQUITY			-0.0048*** (-10.39)	-0.0048*** (-10.77)
	RESEARCH AND DEVELOPMENT			-0.0138 (-1.37)	-0.0265*** (-2.63)
	CAPITAL INTENSITY			-0.0059*** (-4.20)	-0.0047*** (-3.44)
	STATUTORY RATE			0.1394*** (6.22)	0.1654*** (5.50)
	FEDERAL EFFECTIVE TAX RATE			0.0947*** (17.32)	0.0958*** (17.51)

	(1)	(2)	(3)
BUSINESS FRIENDLINESS		-0.0002***	0.0001
		(-5.98)	(1.21)
STATE FIXED EFFECTS	NO	NO	YES
INDUSTRY & YEAR FIXED EFFECTS	YES	YES	YES
CLUSTERS:	FIRM &YEAR	FIRM &YEAR	FIRM &YEAR
OBSERVATIONS	112,994	112,994	112,994
ADJUSTED RSQ	0.057	0.167	0.178

Tests of Coefficient Equality (Model 3)	*F-Test*	*p*-value
$\alpha_5 = \alpha_6$	0.17	0.67
$\alpha_6 = \alpha_7$	6.33	0.01
$\alpha_5 = \alpha_7$	5.51	0.01

Table 6
The Differential Effect of Tax Amnesties on Subsequent Tax Aggressiveness for Small versus Large Firms

This table presents the results of estimating equations (2) and (3) for partitions of the sample based on firm size. Specifically, in Panel A (Panel B), we present the results of a replication of the model in Table 4, Model 4 (Table 5, Model 3), estimated separately for small firms and large firms. Small firms (large firms) are defined as those firm-years that fall below (above) the annual sample median of annual total assets. The models include all controls, as well as industry and state fixed effects as in the previously estimated models. Standard errors are clustered by firm and year. The sample period covers the years 1978 to 2011. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively, using one-tailed p-values where we make a directional prediction and two-tailed p-values otherwise. Because all models include full fixed effects, *AMNESTY STATE* is omitted because it is perfectly collinear with the headquarter-state fixed effects. For cross-equation tests of coefficient equality, we employ seemingly unrelated estimation and report the corresponding Wald χ^2 statistics and p-values.

Panel A: Replication of Table 4, Model 4, Estimated Separately for Small versus Large firms

			Small firms	*Large Firms*	*Model 1 = Model 2*	
Coef.	**Variable**	*Pred.*	**Model 1**	**Model 2**	$\chi 2$ Stat.	*p*-value
α_1	*AMNESTY STATE*	+/-	(omitted)	(omitted)		
α_2	*POST FIRST AMNESTY*	+/-	0.0034***	0.0012	4.32	0.04**
			(3.33)	(1.06)		
α_3	*AMNESTY STATE*POST FIRST AMNESTY*	-	-0.0104***	-0.0024	5.53	0.02**
			(-5.78)	(-1.46)		
α_4	*POST REPEAT AMNESTY*	+/-	0.0040***	0.0018**	36.86	0.00***
			(3.25)	(2.34)		
α_5	*AMNESTY STATE*POST REPEAT AMNESTY*	-	-0.0047**	-0.0009	4.97	0.03**
			(-1.89)	(-0.50)		
	SALES		0.0048***	0.0019***		
			(6.04)	(3.25)		
	LEVERAGE		0.0075**	0.0179***		
			(2.50)	(5.21)		
	MARKET VALUE OF EQUITY		-0.0051***	-0.0042***		
			(-7.24)	(-8.77)		
	RESEARCH AND DEVELOPMENT		-0.0441***	0.0091		
			(-4.31)	(0.47)		
	CAPITAL INTENSITY		-0.0063***	-0.0028*		
			(-3.11)	(-1.71)		
	STATUTORY RATE		0.1791***	0.1257***		
			(3.60)	(4.83)		
	FEDERAL EFFECTIVE TAX RATE		0.0935***	0.0978***		
			(14.56)	(18.36)		
	BUSINESS FRIENDLINESS		0.0001	0.0001		
			(0.94)	(0.83)		
	CONTROLS		YES	YES		
	STATE FIXED EFFECTS		NO	YES		
	INDUSTRY & YEAR FIXED EFFECTS		YES	YES		
	CLUSTERS:		FIRM & YEAR	FIRM & YEAR		
	OBSERVATIONS		56,491	56,503		
	ADJUSTED RSQ		0.172	0.206		

Table 6 (Continued)
The Differential Effect of Repeated Tax Amnesties on Subsequent Tax Aggressiveness for Small versus Large Firms

Panel B: Replication of Table 5, Model 3, Estimated Separately for Small versus Large firms

Coef.	Variable	Pred.	*Small firms* Model 1	*Large Firms* Model 2	*Model 1 = Model 2* χ2 Stat.	*p*-value
α_1	*AMNESTY STATE*	+/-	(omitted)	(omitted)		
α_2	*N1*	+/-	0.0033***	0.0012		
			(3.18)	(1.08)		
α_3	*N2*	+/-	0.0065***	0.0024**		
			(4.83)	(2.08)		
α_4	*N3+*	+/-	0.0076**	0.0045***		
			(2.55)	(3.01)		
α_5	*AMNESTY STATE*N1*	-	-0.0104***	-0.0024	37.00	0.00***
			(-5.81)	(-1.46)		
α_6	*AMNESTY STATE*N2*	-	-0.0115***	-0.0024	21.82	0.00***
			(-4.87)	(-1.33)		
α_7	*AMNESTY STATE*N3+*	-	-0.0206***	-0.0046	47.95	0.00***
			(-4.89)	(-1.53)		
	SALES		0.0048***	0.0019***		
			(6.04)	(3.25)		
	LEVERAGE		0.0075**	0.0178***		
			(2.50)	(5.20)		
	MARKET VALUE OF EQUITY		-0.0051***	-0.0042***		
			(-7.25)	(-8.78)		
	RESEARCH AND DEVELOPMENT		-0.0440***	0.0092		
			(-4.30)	(0.47)		
	CAPITAL INTENSITY		-0.0064***	-0.0028*		
			(-3.12)	(-1.71)		
	STATUTORY RATE		0.1785***	0.1281***		
			(3.48)	(4.79)		
	FEDERAL EFFECTIVE TAX RATE		0.0934***	0.0978***		
			(14.54)	(18.35)		
	BUSINESS FRIENDLINESS		0.0001	0.0001		
			(0.87)	(0.80)		
	STATE FIXED EFFECTS		YES	YES		
	INDUSTRY & YEAR FIXED EFFECTS		YES	YES		
	CLUSTERS:		FIRM &YEAR	FIRM &YEAR		
	OBSERVATIONS		56,491	56,503		
	ADJUSTED RSQ		0.173	0.206		
	F-tests of Coefficient Equality		*p*-value	*p*-value		
	$\alpha_5 = \alpha_6$		0.56	0.96		
	$\alpha_6 = \alpha_7$		0.01	0.38		
	$\alpha_5 = \alpha_7$		0.01	0.46		

Table 7
Additional Test: The Short Window Effect of Tax Amnesties on Tax Aggressiveness

This table presents the results of an event study using a five year window around the amnesty programs. For firms that have headquarters in a state that grants an amnesty in the sample period, we include two years of data prior to the amnesty program as a baseline pre-event period. We also include data from the year of the amnesty and the two following years as a *POST* period. Each observation from a firm headquartered in an amnesty granting state (*AMNESTY STATE*) is matched to an observation for a similar firm with headquarters in a state that did not grant an amnesty during the period. Model 1 is estimated using only observations from the window surrounding the first instance of an amnesty. Model 2 uses observations from the window surrounding a second instance of amnesty. Model 3 aggregates observations surrounding a third, fourth, or fifth instance of amnesty. Model 4 includes data from all occurrences. Panel A presents the results of these tests on the full sample of firms-years, as identified in Table 2. Panel B presents the results of similar tests on a subsample of small firms, which are defined as those firm-years that fall below the annual sample median of annual total assets. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent level, respectively, using one-tailed p-values where we make a directional prediction and two-tailed p-values otherwise. .

Panel A: Short Window Effect (All firms)

		Amnesty #1	Amnesty #2	Amnesties #3+	All amnesties
Variable	*Pred.*	**Model 1**	**Model 2**	**Model 3**	**Model 4**
POST	+/-	0.0011	0.0025**	0.0011	0.0013
		(1.16)	(2.06)	(0.60)	(1.27)
*POST*AMNESTY STATE*	-	-0.0025**	0.0001	-0.0038*	-0.0019
		(-1.74)	(0.08)	(-1.29)	(-1.22)
CONTROLS		YES	YES	YES	YES
STATE FIXED EFFECTS		YES	YES	YES	YES
INDUSTRY & YEAR FIXED EFFECTS		YES	YES	YES	YES
CLUSTERS:		FIRM &YEAR	FIRM &YEAR	FIRM &YEAR	FIRM &YEAR
OBSERVATIONS		17,020	10,850	11,056	38,926
ADJUSTED RSQ		0.194	0.184	0.162	0.173

Panel B: Short Window Effect (Small firms)

		Amnesty #1	Amnesty #2	Amnesties #3+	All amnesties
Variable	*Pred.*	**Model 1**	**Model 2**	**Model 3**	**Model 4**
POST	+/-	0.0018	0.0013	0.0035	0.0016
		(1.50)	(0.79)	(1.24)	(1.16)
*POST*AMNESTY STATE*	-	-0.0049**	0.0014	-0.0070*	-0.0031*
		(-2.13)	(0.62)	(-1.34)	(-1.28)
CONTROLS		YES	YES	YES	YES
STATE FIXED EFFECTS		YES	YES	YES	YES
INDUSTRY & YEAR FIXED EFFECTS		YES	YES	YES	YES
CLUSTERS:		FIRM &YEAR	FIRM &YEAR	FIRM &YEAR	FIRM &YEAR
OBSERVATIONS		8,515	5,432	5,545	19,493
ADJUSTED RSQ		0.207	0.174	0.169	0.174