# I Love, I Love, I Love My Calendar Firm: Estimating the Impact of a Regime Change in Mandatory Tax Disclosure

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PRELIMINARY: DO NOT CITE WITHOUT AUTHOR'S PERMISSION

## ABSTRACT

We identify an underlying tax aggressiveness for US Corporations using a recent change in tax disclosure (2011 Redesign of IRS Form 3800). Our empirical strategy exploits novel variation in exposure created by exogenous differences in tax years across firms. We find that firms report a stock of carry-forward General Business Credits that is 23% larger, and this response reflects an underlying tax-aggressiveness of firms. Further, we find that private firms increase their tax aggressiveness by 14% more than public firms. In this way, the increased disclosure in 2011 cost between $1.4 and $1.8 billion in corporate receipts.

**Keywords :**

**JEL Classification :**

To effectively administer tax systems, tax authorities must be able to measure the tax base accurately. In the absence of truthful information revelation through third-party reporting and audit, taxpayers would minimize their tax burden through misreporting (Allingham and Sandmo, 1972; Becker, 1968). Noncompliance by taxpayers increases the cost of raising revenues and shifts the tax burden to taxpayers with highly-visible income, e.g., wage earners (Slemrod, forthcoming). With this in mind, the IRS mandates information disclosure across a myriad of forms and worksheets. However, information disclosure must strike a balance: increased disclosure decreases asymmetric information, increases oversight, and ultimately increases compliance rates and tax revenues, but this does not come without compliance costs to the taxpayer.

Despite the importance of information disclosure for tax collection, little is known about how firms respond to these incentives. There are several reasons for this gap in our knowledge. First, public disclosure, rather than private disclosure, has been the focus of most of the research. This research typically investigates the impact of public financial disclosure required by the Securities Act of 1933 and the Securities Exchange Act of 1934 for publicly traded firms (Greenstone et al., 2006; La Porta et al., 1999, 1997, 2002; Stigler, 1964; Jarrell, 1981; Robbins and Werner, 1964; Friend and Herman, 1964). This literature has found that disclosure provides pertinent information to investors (for a review see Kothari, 2001), affects real behavior through public pressure (Dyreng et al., 2016), and can impose costs by revealing information to competitors (Hayes and Lundholm, 1996; Bernard, 2016). Second, data on tax disclosure is not easily accessible to researchers. The notable exceptions are the few cases of public tax disclosure, though, in these cases, public disclosure does not seem to affect evasion (Hasegawa et al., 2013; Bø et al., 2015; Hoopes et al., 2018; Boynton and Mills, 2004). Because most tax disclosure is costly to taxpayers, research on the impact of private disclosure is needed to understand its effect on compliance and firm behavior more fully.

We leverage a change in disclosure requirements made by the IRS in 2011 to present new evidence on the effects of private tax disclosure on firm behavior. In particular, the IRS redesigned Form 3800, which is the information disclosure supporting the General Business Credit. In 2010, firms earned $28 billion worth of general business credits and firms held a stock of $43 billion in accumulated unused general business credits. The redesign required firms to disaggregate unused general business credits or carry-forward credits.

The redesigned Form 3800 provides a compelling setting for evaluating the consequences of mandatory private disclosure for at least four reasons. First, the redesign provides exogenous variation: it was not mandated by legislation, it was not previously announced, and it was released in draft-form midway through the relevant tax year. Second, not all firms were subject to the new disclosure requirements in the same year due to a unique institutional detail that results in a staggering exposure. This quasi-natural experiment, combined with a compelling control group, allows for an empirical estimate of the impact of disclosure requirements on firm behavior that avoids confounding factors.

Third, the change in the general business credit reporting regime is a change to disclosure that affects both public and private firms. The effects of disclosure on public and private firms have not been widely studied because (1) private firms are not typically subject to required public disclosure that would be easily observable to researchers (2) disclosure to the IRS, which affects both public and private firms, is unobserved in publicly available data. Notable exceptions include mandated disclosure that reconciles book and tax income (Schedule M-3 Boynton and Mills (2004); Hope et al. (2013)) and disclosure of income tax uncertainty (FIN 48, Robinson et al. (2015); Towery (2017); Gupta et al. (2014)). We overcome these limitations by analyzing the effects of tax disclosure using data from the IRS, which allows us to observe both public and private firms.

Fourth, general business credits are an essential policy tool and a large tax expenditure. The United States administers many social programs through the corporate sector, and general business credits play a prominent role in this implementation. Examples include credits for the provision of low-income housing and paid family leave. General business credits are also used extensively to correct for externalities and market underprovision as Pigouvian subsidies. Most prominent among these is the investment tax credit for research and experimentation. Finally, the federal government leverages temporary credits as a fiscal stimulus to smooth the business cycle during periods of economic downturn.

We begin by investigating whether firms respond to the most direct effect of newly disaggregated tax disclosure by adjusting reported information. Specifically: how do firms respond to the redesigned Form 3800 in 2011? A naive model suggests that this change should have no impact on reported carry-forward credits; after all, carry-forward credits reflect lagged activity and therefore should not be affected by contemporaneous changes in reporting requirements. Contrary to this hypothesis, our firm-level difference-in-differences estimation reveals that firms reported 23% *more* carry-forward credits in the year that they were required to disaggregate their carry-forward credits.

Because lagged activity determines carry-forward credits, any increase in carry-forward general business credits must reflect a reporting response. To evaluate this apparent over-reporting of carry-forward credits, we investigate the underlying tax aggressiveness of firms. Tax aggressiveness is relevant for the optimal design of tax policy and has therefore been studied in many different contexts (Dyreng et al., 2008; Hanlon and Slemrod, 2009; Desai and Dharmapala, 2009).[1] Previous research is based on several imperfect measures of tax aggressiveness (Hanlon and Heitzman, 2010). We advance this literature by providing a credible

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[1]A large literature on corporate tax avoidance finds evidence of different strategies (see for example Graham and Tucker, 2006; Rego, 2003; Engel et al., 1999; Mills, 1998; Matsunaga et al., 1992; Frank et al., 2009; Chen et al., 2010).

measure based on the difference between reported carry-forward credits and projected carry-forward credits in firm-level administrative tax data. Between 2001 and 2016, we find that 75% of firms were tax aggressive at least once, and these firms over-reported carry-forward credits by $20,000. We also find that the change in disclosure requirements increased the propensity of firms to be tax aggressive by nine percentage points, and these firms increased their over-reporting by 14%.

Finally, we investigate whether public firms are more or less tax aggressive than private firms due to differences in agency costs, public disclosure rules, and capital structure. We find that 91% of public firms and 67% of private firms are ever tax aggressive. In response to an increase in disclosure requirements, we find private firms are more responsive than public firms—increasing tax aggressiveness by 14% more than public firms.

Taken together, our results suggest that firms are tax-aggressive, so there are benefits of increased oversight through increased tax disclosure. The transition, however, may be costly as it induces additional over-reporting local to the change in reporting requirements. In the case of the 2011 redesign of form 3800, increased disclosure cost between $1.4 and $1.8 billion in corporate tax receipts.

# 1. General Business Credits and SOI Data

The U.S. tax system provides for a suite of business tax credits to encourage certain business activities. The government uses these credits to achieve policy goals such as social policy, Pigouvian subsidies, and fiscal stimulus. Firms document their qualifying activity across a series of IRS forms, and each earned credit is reported separately on IRS Form 3800. This level of disaggregated detail allows the IRS to observe and monitor current-year activity.

In 2010, firms engaged in $28 billion worth of qualifying business activity.[2] The four largest credits account for 72% of total general business credits: $8.5 billion was in research and experimentation, $7.2 billion in low-income housing, $1.5 billion was for qualified investment, and $2.8 billion was for the development of renewable electricity. General business credits are used to offset positive tax liability. All told, firms offset $15 billion in positive tax liability with the general business credit in 2010, which was roughly 7% of nearly $223 billion total corporate tax receipts.

To use a general business credit, a firm must be in a taxable position. In addition, the use of general business credits faces several frictions including the Net Operating Loss (NOL) deduction, the Foreign Tax Credit (FTC), and, before 2018, the Alternative Minimum Tax (AMT). Carlson and Metcalf (2008) estimates the sizable impact of these frictions on the ability of firms to use credits. In general, unused business tax credits may be carried back one year and carried forward up to twenty years. If a firm carries a positive stock of credits into the current tax year, this stock of carry-forward credits must be fully exhausted before using current-year earned credits, following a first-in-first-out method.

In addition to $28 billion in earned business tax credit, firms carried in $43 billion in unused general business credits from prior tax years. However, unlike earned business tax credits, firms were not required to disaggregate reported carry-forward credits across the various business tax credits before 2011. Instead, they reported an aggregate carry-forward general business credit. In 2010, for example, firms had a total of $71 billion in tentative general business credits available for use, which are the sum of carry-forward and earned credits. By comparison, just $15 billion, or 21% of total available credits, were used to offset positive tax liability.

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[2] These figures are documented in annual IRS Statistics of Income reports.

## 1.1. 3800 Form Change: A Quasi-natural Experimental Setting

In 2011, the IRS redesigned Form 3800. As part of the form redesign, firms were newly required to disaggregate carry-forward general business credits to the same level of detail as earned credits. Before 2011, it was practically impossible to track and audit stocks of specific credits used and unused because of complicated staking order of use and FIFO accounting. This newly disaggregated reporting provides greater transparency to the IRS in facilitating audits of tax returns.

The IRS released a draft of the redesigned Form 3800 in July 2011. Before release, there was no indication of an impending form redesign: the new form was not a legislative requirement, there were no new business tax credits introduced in the 2011 tax year, and there were no public notifications of the upcoming change in typical federal reporting outlets.[3] For this reason, the 2011 form redesign marks an exogenous regime switch in the reporting requirements for the carry-forward general business credits in 2011.

Firms report credits carried forward from previous years and earned this year on Form 3800. Based on this information, Form 3800 directs firms to calculate allowable claimed credits, taking into account all use-frictions. By definition, the stock of carry-forward credits in year $t$ is based on activity in year $t-1$,

$$\text{Carry-Forward}_t = \text{Carry-Forward}_{t-1} + \text{Earned}_{t-1} - \text{Claimed}_{t-1}. \qquad (1)$$

This accounting identity makes clear that lagged activity determines the stock of unused credits carried into any given tax year. Further, this suggests that unused general business credits

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[3]For example, the IRS typically announces pending form changes through the Federal Registrar. On July 13th, 2010, the IRS notified the public that Form 3800 would be updated to account for newly available and expiring business tax credits. The next mention of modifications to IRS Form 3800 (OMB Number: 1545-0895) was not until November 22nd, 2013 to accommodate changes due to the Small Business Jobs Act.

carried into the current tax year should be perfectly projected based on lagged reported information.[4]

The predictability of Equation 1 and the timing of the form redesign implies that reported carry-forward credits should be unaffected by the newly disaggregated reporting requirements in 2011. If instead there is a change in reported carry-forward credits due to the form redesign, it must reflect a reporting response on the part of firms. We will investigate this hypothesis by analyzing firm-level tax data.

## 1.2. IRS Firm-Level Data

To examine the impact of newly desegregated reporting requirements on firm behavior, we utilize the firm-level corporate tax data that underlies the annual aggregate statistics of income. The SOI corporate data are a stratified random sample of all Corporations, including detailed data reported on Form 3800.[5] From these data, we create a panel of firms data from 2001 through 2016.

Table 1 provides summary statistics for the years unaffected by the form redesign, 2001–2010 and 2012–2016, which we define as the control years. 2011 Summary Statistics are reported in Table A.1. Panel A describes general firm characteristics, Panel B describes general business credit activity, and Panels C and D provide the distribution of firms by size and industry, respectively.

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[4]This statement must be true based on the full set of administrative tax data. The SOI data, however, does not update information from amended returns. For this reason, these data may not be able to predict carry-forward credits across tax years perfectly.

[5]We exclude sub-chapter S Corporations because general business credits are claimed on individual owner's tax returns, rather than at the entity level. The SOI sample is representative of the population of C corporation based on sample weights.

In Column (1) of Table 1 we report summary statistics for the population of C corporations. During this period, firms held, on average, $1.6 million in assets, earned $17 thousand in taxable income, and paid $56 thousand in corporate income tax. 46% of firms were in loss. Firms earned $4 thousand in general business credits, carried-forward a stock of $7 thousand in unused general business credits, and offset $3 thousand in tax liability with the general business credit. Averages for general business credits are somewhat misleading, however, because general business credit activity is highly concentrated.

Roughly 6% of firms (23% of the SOI sample due to stratification) engage in activity that qualifies for a business tax credit. To this end, we focus our analysis on the subsample of firms that claim a general business credit at least once between 2001 and 2016. These data contain a panel of roughly 26,754 firms and 1,036,361 million firm-years from 2001–2016 (197,967 weighted firms and 28,830,700 million weighted firm-years).

Column (2) of Table 1 presents descriptive statistics for our estimation sample. These firms are, on average, six times larger when measured by average taxable income and tax liability, and about half as likely to be in loss. These firms earn $57 thousand in general business credits, carry forward $67 thousand in unused general business credits, and claim $47 thousand in general business credits. Firms in our sample are more heavily concentrated among the largest firms, reported in Panel C. Despite the upward tilt in the size distribution of our sample; it is distributed across industries in roughly the same proportion as the full population of firms, reported in Panel D.

# 2. Calendar Year Firms and Empirical Identification

There are many reasons why the stock of carry-forward credits change from year to year. Identifying the effect of the form change, therefore, requires variation beyond time variation. We leverage a unique institutional detail underlying the SOI corporate data to provide this. Specifically, firms are not subject to the same rigid definition of a tax year as individuals for the purposes of reporting taxable income. Instead, firms are granted flexibility to report income based on either a calendar year or a fiscal year.[6] We will exploit this variation to identify treated and control firms in 2011.

## 2.1. Institutional Details: Calendar-Year and Noncalendar-Year Firms

Roughly 60% of firms report taxable income based on the calendar year. Because the IRS reports aggregate income as a calendar year concept, the business data must be aligned so that it is centered on the calendar year. Therefore, business data is joined together in an SOI year so that the majority of the tax year is contained within the appropriate calendar year. In this way, 2011 aggregate income reflects income earned based on tax years ending between July 31, 2011–June 30, 2012.

While the SOI year is defined based on the *end* of the tax year, relevant tax forms are based on the *beginning* of the tax year. This definition implies that there is a subset of firms included in aggregate income in year $y$ that fills out a tax form released for year $y - 1$. This variation in tax forms within a year results in heterogeneous exposure to the redesigned Form 3800 in 2011. Specifically, noncalendar year firms (tax years ending July–November) are subject to

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[6]Firms make this decision within the first year of incorporation, and thereafter can only adjust their tax year with the approval of the IRS.

the old reporting regime on the 2010 Form 3800. In contrast, calendar year firms (tax years ending December–June) face the new disaggregated reporting regime on the 2011 Form 3800.

## 2.2. Empirical Specification

We exploit variation in tax years to identify the effect of the disaggregated tax disclosure based on a difference-in-differences empirical specification. To begin, we estimate the impact of the form redesign on two outcomes: (1) the log of carry-forward credits and (2) the change in carry-forward credits. Calendar-year firms serve as our treatment group, and noncalendar-year firms serve as our control group. The policy effect is given by the coefficient $\beta_1$ on the interaction of indicator variables identifying calendar-year firms and 2011. We use OLS to estimate the parameters in the equation

$$
\begin{aligned}
log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) \\
+ \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) \\
+ \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t},
\end{aligned}
\tag{2}
$$

where an observation is a firm-year for firm $i$ in year $t$.

We run three specifications based on different dimensions of variation. First, we report the raw difference-in-differences specification. Second, we use within-firm variation by adding firm fixed effects $\lambda_i$ and firm-level controls including indicator variables for a series of use-frictions (whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, whether a firm was ever publicly traded, industry fixed effects, and size fixed effects. Finally, we use within-year variation by including year fixed effects $\lambda_t$ and year

by calendar-year firm fixed effects $\eta_{i,t}$. Errors are clustered at the firm-level to model serial correlation in the error terms due to the panel nature of the data. Finally, we run all three specifications based on two different sets of control years: 2001–2011 and 2001–2016. The first set of years uses the ten years before 2011 as a control and the second set adds the five years after 2011 as additional control years because the reporting regime for 2012-2016 was unchanged.

These specifications remove any omitted variable bias or confounded selection of being a calendar-year firm. As usual in these models, our estimates capture the causal effect in the absence of omitted variables, causing a differential effect between calendar-year and noncalendar-year firms in 2011.

Following Bertrand et al. (2004), we account for auto-correlated errors in several ways. First, we cluster our standard errors at the firm level. Second, we use the within-firm transformation to calculate standard errors. Finally, figure A.3 collapses the time variation into a before and after period.

## 2.3. Identification

Identification in the differences-in-differences model requires that longitudinal changes in the control group serve as an appropriate counterfactual for the treatment group absent the policy change. This is commonly referred to as a "parallel trends" assumption. Evidence of non-parallel dynamics or compositional differences across these two groups in the years before and after the form redesign would pose a threat to the validity of our empirical strategy. To investigate these potential threats to identification, we compare observable characteristics between the treatment and control firms in Table 1 and differences in outcome pretends in Figure 1. The similarity between these two groups of firms based on both observable charac-

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teristics and pretrends provides evidence in support of differences-in-differences identification assumption.

Table 1 provides descriptive statistics for treatment and control firms in the control years: column (3) for control firms with tax years ending July–November (non-calendar year firms) and filling out tax forms from SOI Year $t-1$, and column (4) for treatment firms with tax years ending December–June (calendar year firms) and filling out tax forms from the current SOI Year. These descriptive statistics highlight the similarity between calendar-year and noncalendar-year filers across the majority of observable characteristics, with the exception being that calendar-year filers are larger when measured by total assets. In addition, the mix of industries between these two groups is slightly different: calendar-year filers are less likely to be in Wholesale, Retail, and Services, and more likely to be in Insurance.

We formally test the assumption of parallel trends between the treatment and control group in the control years by estimating the following specification via OLS

$$\log(CF) = \beta_0 + \beta_1 TREAT + \sum_{t=2001}^{2016} \gamma_t \mathbb{1}(t) + \sum_{t=2001}^{2016} \theta_t \mathbb{1}(t) \times TREAT + \nu \tag{3}$$

$\widehat{\theta}_t$ identify differences in trends between the treatment and the control group. We fail to reject the null hypothesis that the trends are the same for all control years with a p-value of 0.42. This hypothesis test provides statistical support of the parallel trends assumption. This evidence provides support that omitted variables are not deferentially affecting calendar-year and noncalendar-year firms (Kahn-Lang and Lang, 2018).

Panel (b) of Figure 1 displays $\widehat{\theta}_t$. During the control years, the difference in trend is near zero in all years and never statistically significant. In 2011, there is a noticeable increase that is statistically significant at the 1 percent level. To formally investigate the response of firms to changes in disclosure, we examine the estimates of equation (2) in the following section.

# 3. The Effect of Disclosure on Carry-Forward Credits

By leveraging heterogeneity in tax years in a differences-in-differences estimation strategy, we reject the null hypothesis that there is no impact of the form redesign on carry-forward credits, as is suggested by a naive model, at the 1% level. Instead, we find that firms reported *more* carry-forward credits due to newly disaggregated disclosure requirements. Moreover, by exploiting the accounting identity in equation (1), we find that this increase must reflect a reporting response. These results are reported in Table 2.

In panel A, we examine the impact of the regime change in reporting requirements on log carry-forward credits, and in panel B, we examine the effects on carry-forward credits based on lagged credit activity. Columns 1–3 reports the estimates using the years 2001–2010 as the control years and 2011 as the treated year for calendar-year firms. The estimates are similar when we use 2012–2016 as additional control years, reported in columns 4–6.

## 3.1. Impact of Form Redesign: Carry-Forward Credits

Panel A of Table 2 reports that the change in disclosure requirements increased 2011 reported carry-forward credits by 20% to 26%. These estimates are based on the specification given in equation (2) with log carry-forward credits as the dependent variable.[7] The estimated impact of the reporting regime change is robust to (1) a wide set of control variables and (2) the expansion of control years to include 2012–2016.

The baseline difference-in-differences estimate reported in columns 1 and 4 suggests that calendar-year firms carried forward 23.1% and 20.4% more credits under the new reporting

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[7]Because tax data is highly skewed, the error terms are more likely to be log-normal than normal. For this reason, we estimate the log specification. Data is shifted up to ensure the inclusion of the full sample. Appendix B provides the mathematical support for this adjustment.

regime, relative to noncalendar-year firms. In Columns 2 and 5, we exploit within-firm variation by estimating a firm fixed effects panel model and including firm-level time-varying controls. Finally, in Columns 3 and 6, we exploit within-year variation by including year and year by calendar-year firm fixed effects. This specification is the most robust to the necessary identification assumptions for differences-in-differences models. Results across these specifications are highly robust: carry-forward credits increased by 22.6%-26.2% for firms facing the redesigned Form 3800. Finally, our results are robust to the use of the level of carry-forward credits as a dependent variable, as seen in Table A.2

The robustness of our estimates to the inclusion of additional control variables and additional control years supports the identification requirement that noncalendar-year firms serve as an appropriate control group, which allows us to identify counterfactual carry-forward credits absent the reporting regime change. All estimates are precisely estimated with p-values around or below 0.01.[8]

To put these numbers in context, consider the cost in terms of tax revenue from an increase in carry-forward credits of 20 to 26 percent. Table 1 reports that the average calendar-year firm in our estimation sample carries forward $71,000 worth of credits during the control years (column 4, panel B). A 20 to 26 percent increase in carry-forward credits, therefore, amounts to an increase of $14-$18 thousand in credits per firm. For 103,309 firms in 2011 that claimed a GBC, our estimates suggest that the change in reporting requirements caused carry-forward credits to increase by an amount that could offset between $1.4 billion and $1.9 billion in tax revenues.

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[8]To account for potential inference complications with the difference-in-differences approach from serial correlation, we follow Bertrand et al. (2004) and report estimates using within-year variation (columns 3 and 6), which corrects for serially correlated error terms due to the presence of unobserved firm-specific effects. Standard errors in columns 1, 3, 4, and 6 are clustered by firm for similar reasons.

## 3.2. Impact of Form Redesign: Accounting Identity

To further investigate the effects of the form change on reported carry-forward credits, we directly model the accounting identity in equation (1). Specifically, we estimate the change in carry-forward credits in based on the following differences-in-differences specification,[9]

$$\Delta\text{Carry-Forward Credits}_t = \beta_0 + \beta_1 \text{Earned}_{t-1} + \beta_2 \text{Used}_{t-1}$$
$$+ \beta_3 \text{Calendar-Year Firm} \times \mathbb{1}(2011)$$
$$+ \beta_4 \text{Calendar-Year Firm} + \beta_5 \mathbb{1}(2011)$$
$$+ \gamma X_{i,t} + \lambda_i + \lambda_t + \varepsilon_{i,t}, \tag{4}$$

as a function of the earned and claimed credits in period $t-1$, as well as the interaction of the indicator variables for calendar-year firm and year 2011, and potential controls including firm, year, and calendar-firm by year fixed effects and firm level controls. The accounting identity in equation (1) implies that $\beta_1 = 1$, $\beta_2 = -1$, and all other coefficients should be zero absent reporting responses on the part of the firm.

Panel B of Table 2 reports that calendar-year firms increased their carry-forward credits in 2011 by between \$6.0 and \$8.4 thousand, relative to noncalendar year firms. This increase is beyond what is explained by lagged earned and claimed credits. Moreover, the coefficients on earned credits are between 0.330 and 0.446, and the coefficients on claimed credits are between -0.370 and -0.360. These precision of these estimates are such that we can reject a null hypothesis that these estimates are 1 and -1 with a high level of confidence. This evidence is consistent with the hypothesis that firms are over-reporting carry-forward credits in response to the newly disaggregated reporting requirements.

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[9] $\Delta\text{Carry-Forward Credits}_t = \text{Carry-Forward Credits}_t - \text{Carry-Forward Credits}_{t-1}$.

Even though the stock of carry-forward credits should be entirely determined by lagged activity, the reported stock increases in response to a contemporaneous change in the disclosure requirements. Moreover, the accounting identity in equation (1) does not adequately describe firm behavior. To better model this puzzling firm behavior, we add a tax aggressiveness term to the accounting identity.

## 4. Tax Aggressiveness

The implementation of newly disaggregated reporting provides a unique opportunity to identify evidence of tax aggressiveness empirically. While there is no universally accepted definition of tax aggressiveness, it is generally described as occupying one extreme of a continuum of tax planning strategies, opposite responses like municipal bond investments (Hanlon and Heitzman, 2010). Tax planning strategies are of inherent interest to policy makers, tax administrators, and researchers alike, and in particular, those that are described as tax aggressive.

Because tax aggressiveness is a conceptual notion, the empirical literature employs a variety of measures. Most common among these are measures based on effective tax rates or book-to-tax differences (Plesko, 2000; Manzon and Plesko, 2002; Desai, 2002; Yin, 2003; Hanlon and Shevlin, 2005; Hanlon et al., 2005; Mills and Newberry, 2001a; Plesko, 2002, 2004). Except in the case of Unrecognized Tax Benefits (UTB), it is rare to find a direct measure that reflects tax avoidance. Through general business credit reporting, we provide a direct measure using firm-level tax data and an accounting identity. In this way, we answer the call by Shackelford and Shevlin (2001) and Hanlon and Heitzman (2010) for more research and different measures of tax aggressiveness.

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## 4.1. Measuring Tax Aggressiveness

Our measure of tax aggressiveness is the difference between reported and projected carry-forward general business credits in a given tax year,

$$\text{Tax Aggressive}_t = \text{Carry-Forward}_t - \text{Carry-Forward}_{t-1} - \text{Earned}_{t-1} + \text{Claimed}_{t-1}. \quad (5)$$

In a world of zero tax aggressiveness, we should expect that tax aggressiveness is never positive. A negative value of tax aggressiveness likely reflects the expiration or general loss of unused carry-forward credits. This explanation would be consistent with empirical evidence of expiration among other tax assets; for example, Cooper and Knittel (2006) estimate that 25-30% of Net Operating Losses are never used. On the other hand, a positive value of tax aggressiveness must mean that firms are carrying more general business credits forward than can be explained by lagged activity. This can only be a sign of tax aggressiveness.

## 4.2. Impact of Form Redesign: Tax Aggressiveness

To test whether tax aggressiveness changes in response to changes in disclosure, we estimate the difference-in-differences specification in equation (2). We focus on two measures of tax aggressiveness as dependent variables; the log of tax aggressiveness and an indicator variable equal to one if tax aggressiveness is positive. We use the log of tax aggressiveness because our measure of tax aggressiveness is highly skewed.[10] These estimates are reported in Table 3.

Panel A of Table 3 reports that calendar-year firms increased their tax aggressiveness by between 13.4% and 15.4% due to the regime change in reporting requirements, relative to

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[10]Because tax aggressiveness can take on negative values, we shift the distribution of tax aggressiveness by the 5th percentile of its distribution and correct for this shift using a Taylor-series approximation, shown in B.

noncalendar-year firms. The difference-in-differences estimates reported in columns 1 and 4 imply that tax aggressiveness increased by 13.4% using data from 2001–2011, and 13.8% using data from 2001–2016. The estimates using within-firm variation imply an increase in tax aggressiveness of 14.5% and 15.4%, reported in columns 2 and 5. When using within-year variation, we estimate that calendar-year firms increased tax aggressiveness by 14.8% in 2011, relative to noncalendar-year firms. These estimates are relatively precisely estimated with p-values below 0.05.

68% of firms were tax aggressive at least once during the control years of our sample. The number of firms that are tax aggressive also increases in response to the form change. Panel B of Table 3 reports estimates where the dependent variable is an indicator variable equal to one if tax aggressiveness is positive and zero otherwise. These estimates suggest that firms were between 8.1 and 9.7 percentage points more likely to be tax aggressive in response to the new reporting requirements, relative to noncalendar-year firms. The estimates are similar when we use the difference-in-differences specification (columns 1 and 4), within-firm variation (columns 2 and 5), and within-year variation (columns 3 and 6). These estimates suggest that the form change had an extensive margin response that encouraged more firms to be tax aggressive.

The pervasiveness of tax aggressiveness in the context of general business credits suggests that improved information disclosure can have large benefits for tax collectors. Our evidence also shows that changes in disclosure can provide opportunities for additional tax aggressiveness. To better understand this tax aggressiveness response, we consider whether public and private firms differ in their response in the following section.

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## 4.3. Tax Aggressiveness of Public and Private Firms

Using our measure of tax aggressiveness, we can compare public and private firms to determine if they reacted differently to the reporting change. Theoretically, it is an open question whether public firms are more or less aggressive than private firms. Private firms have different agency costs (due to different ownership structures) and public disclosure rules that may allow them to be more tax aggressive (Cloyd et al., 1996; Beatty and Harris, 1999; Mikhail, 1999; Mills and Newberry, 2001b; Hanlon et al., 2007). Private firms may be less tax aggressive than public firms; however, due to different capital structures that make them less elastic (Myers and Majluf, 1984; Fama and Jensen, 1985; Stulz, 1990; Campello et al., 2010, 2011). At the same time, empirical studies of private firms are severely restricted by data availability, despite representing a substantial portion of the U.S. economy. [11]

To investigate the differences in tax aggressiveness of public and private firms, Panels (e) and (f) of Figure 1 and Table 4 reports estimates of tax aggressiveness separately for these two sets of firms. Panels (e) and (d) show that the difference in trend is near zero in all control years and only once statistically different from zero at the 5 percent level. Columns 1–3 in Table 4 replicate the first three columns in Table 3 restricting the set of firms to public firms. Columns 4–6 repeat these specifications for private firms.[12] We report the log of tax aggressiveness in Panel A and the indicator of whether a firm is tax aggressive in Panel B.

As Panels (e) and (f) of Figure 1 and Table 4 show, tax aggressiveness is starkly different across public and private firms. Panels (e) and (f) show that in 2011, there is a 9 percentage point increase in the number of *private* firms that are tax aggressive, in contrast, there is no

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[11]The notable exception is Coles et al. (2018), that find evidence that private firms are tax aggressive—but they are unable to compare to public firms.

[12]The estimates in Table 4 use the years 2001–2011. The estimates are similar when we use the 2001–2016 sample and those estimates are shown in Table A.3

corresponding increase for public firms. These findings are replicated across all specifications in Table 4.

The first three columns of Table 4 report that public firms did not increase their tax aggressiveness, and firms were not more likely to behave tax-aggressively in response to the reporting change. Specifically, the coefficients are negative, small, and imprecisely estimated. In contrast, columns 4–6 show that private firms increased tax aggressiveness by 14%, and firms were 9 percentage points more likely to be tax aggressive in response to the reporting change. These coefficients are positive, large, and relatively precisely estimated.

Taken together, these estimates show that the increase in tax aggressive observed in the full sample is driven by private firms. Private firms are not only tax aggressive, but they respond to a regime change in reporting requirements by behaving in a more tax aggressive manner than public firms. It is important to note that these estimates do not indicate that public firms are not tax aggressive on average. In fact, we find that 91% of public firms are ever tax aggressive compared to 67% of private firms. Instead, these estimates indicate that public firms do not respond to newly disaggregated reporting requirements by adjusting tax aggressive behavior in 2011. There are many reasons why public firms may be less elastic to a form change. For example, many public firms have in-house auditors, which may have meant carry forward credits were better tracked before the form redesign. These estimates, however, do provide empirical evidence that private firms are sophisticated tax planners.

# 5. Discussion of Results

We find that when firms are exposed to newly disaggregated tax reporting requirements for carry-forward general business credits, they respond by reporting more carry-forward cred-

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its. This runs counter to a naive hypothesis that firms should not respond to this change in disclosure environment based on a fundamental tax accounting identity. This observed response leads to a direct measure of tax aggressiveness, and we find that firms are more tax aggressiveness in 2011 when first exposed to the form redesign.

Given these findings, we further explore two potential complications to our measure of tax aggressiveness. First, it may be that firms choose not to file a Form 3800 in a year that they experience a loss, knowing that they will be unable to claim a credit. This would be consistent with evidence that some firms do not file Form 1116 to claim Foreign Tax Credits in loss years (Robison and Nutter, 1994). However, discretionary filing on the part of firms is a form of tax aggressiveness; Form 3800 is not an optional tax disclosure for firms. To the extent that firms engage in this behavior, our measure accurately captures this. Second, our measure of tax aggressiveness may be influenced by merger decisions. In particular, the data is such that our measure captures instances where an acquiring firm subsumes general business credits from a target as part of the transaction. Our differences-in-differences specification nets out these concerns as long as these behaviors are independent of tax aggressiveness and do not differentially affect calendar year firms relative to noncalendar year firms in 2011. The similarity of calendar- and noncalendar-year firms based on observable characteristics and evidence of parallel trends is inconsistent with such a differential effect. Said differently, the differences-in-differences empirical strategy is robust to these concerns.

Next, we consider whether more detailed disclosure requirements improved taxpayer alertness. In particular, the disaggregated information required in 2011 may serve as a catalyst or motivation for more careful accounting on the part of a firm. However, the stacking order and first-in-first-out use of general business credits already necessitate that firms keep a detailed accounting of disaggregated carry-forward credits. If firms were not conducting this accounting with precision during the relative opaque disclosure regime, improved awareness due to

the form redesign is itself a revelation of an underlying tax aggressiveness on the part of the firm. In this way, if firms are more alert due to the redesigned form in 2011, this is a behavior on the continuum tax planning strategies that is captured by our measure of tax aggressive.

# 6. Conclusion

In this paper, we investigate whether and how firms respond to newly disaggregated disclosure requirements. Our empirical methodology exploits an exogenous tax form redesign and an institutional detail for business tax reporting. Specifically, we study the impact of the redesign of Form 3800, which increased the required tax disclosure for the purposes of claiming the general business credit. We find that firms are responsive to changes in this reporting regime change, and this response is driven by tax aggressiveness. These credits are an important policy tool; in 2010, firms offset $XX billion in positive tax liability with the general business credit.

We find evidence that increased oversight through disclosure may be beneficial because firms are tax aggressive. Specifically, firms subject to the disclosure change reported 13% more carry-forward credits in 2011 than firms not subject to the change—and this constitutes tax aggressiveness. The transition to more disclosure, however, was costly. Our estimates suggest the increase in reported carry-forward credits could offset between $1.4 and $1.8 billion in corporate tax receipts.

We introduce a new measure of tax aggressiveness, defined as the difference between actual and projected carry-forward general business credits, based on an accounting identity. In the absence of tax aggressiveness, this firm-level measure should never be positive. Positive tax aggressiveness represents an increase in reported carry-forward credits that cannot be ex-

plained by lagged carry-forward credits, earned credits, and claimed credits. This measure, therefore, captures a pure reporting response.

Finally, we investigate the role of agency costs by comparing the response of public and private firms. In response to the increased disclosure, we find that private firms increased their tax aggressiveness by 14%, while public firms had no change. Increased disclosure also encouraged a 9 percentage point increase in the number of private firms that were tax aggressive; public firms, again, had no change. These estimates suggest that looser public financial reporting requirements and different agency costs that private firms have may allow them to be more tax aggressive.

The set of results in this paper suggest there is more research needed on (1) private disclosure, (2) general business credits, (3) tax aggressiveness, and (4) comparisons between public and private firms. To determine the efficient level of private disclosure to the IRS, more evidence is needed on its effects on firm behavior.

How firms respond to increased disclosure is necessary to determine the efficient level of private disclosure the IRS should mandate. Our evidence suggests that changes in private disclosure can have large effects, but more evidence is needed, especially on different pieces of information. In addition, given the magnitude of the cost of general business credits, more evidence is needed on their effectiveness. Finally, new measures and evidence on tax aggressiveness and differences in tax aggressiveness between public and private firms are necessary to understand the benefits and costs of firms organizing as public and private. Our paper also highlights a new source of variation between fiscal- and calendar-year firms that future researchers can exploit to answer a host of new questions.

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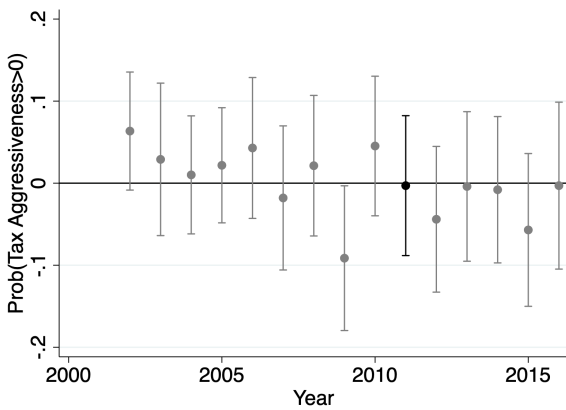**Figure 1.** Differences-in-Differences: Identification Assumptions 2001 - 2016
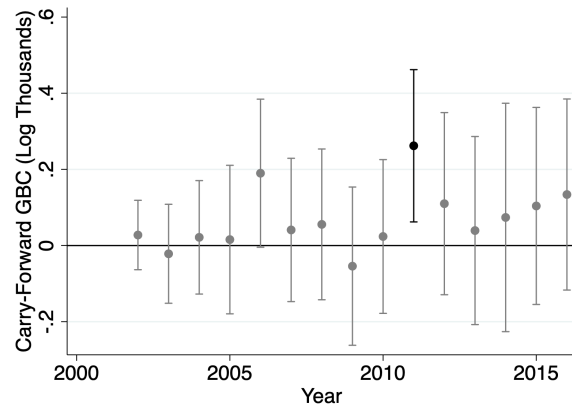
(a) Distribution Tax Aggressiveness

(b) Carry-Forward Credits
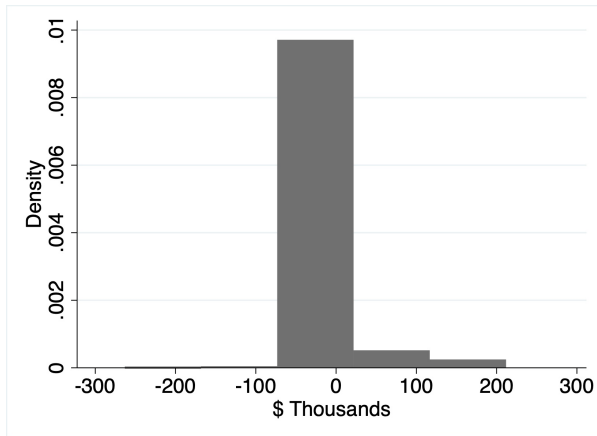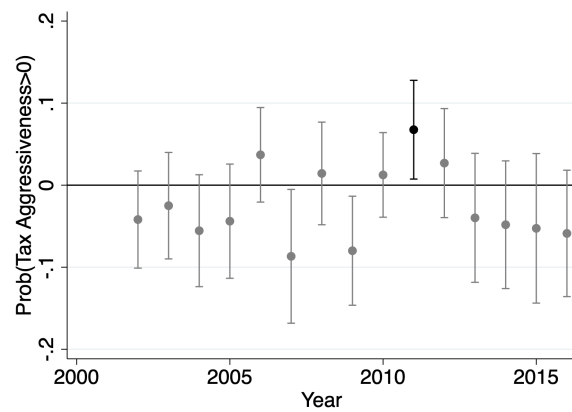


(c) Tax Aggressiveness

(d) 𝟙(Tax Aggressive Firm)
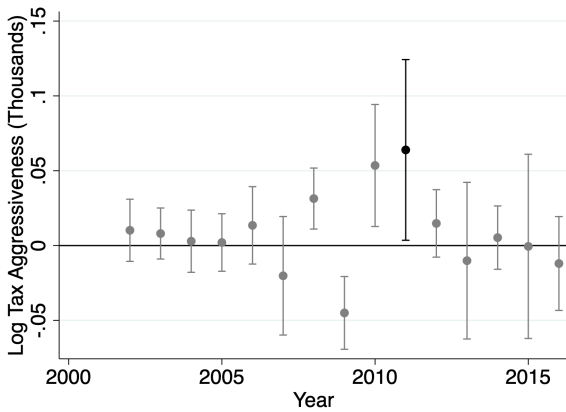


(e) Public Firms: 𝟙(Tax Aggressive Firm)

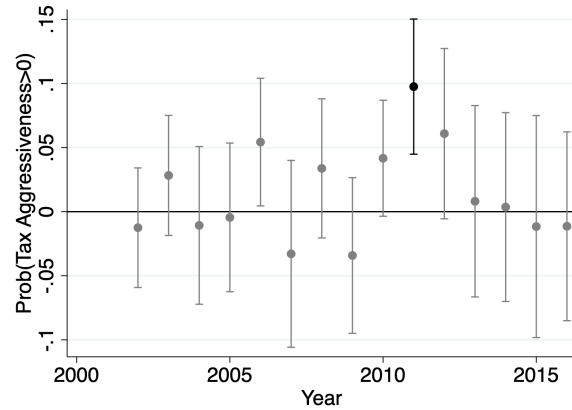(f) Private Firms: 𝟙(Tax Aggressive Firm)



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**Table 1**

**Summary Statistics: IRS Corporate Statistics of Income (SOI): Control Years**

Table details mean values based on Form 1120 and Form 3800 for SOI Years 2000-2016. We detail relevant forms for Tax Year 2015. Taxable Income: Form 1120 Line 30. Total Assets: Form 1120 Schudle L 19d. Total Taxes Paid: Form 1120 Line 31. General Business Credits (GBCs) Claimed: Form 1120 Schedule J Line 5c. GBCs Earned: Form 3800 Part III Line 6, Box A and B. GBCs Carried Forward: Form 3800 Part III Line 6, Box C and F. Control firms in 2011 are those with a Tax Year ending July 1 2011 - Nov 30 2011. Treated firms in 2011 are those with a Tax Year ending Dec 1 2011 - June 31 2012. Both treatment and control firms are included in the 2011 SOI file. $-Weighted Distribution is weighted by Gross Receipts.

| | | | GBC Subsample | |
| --- | --- | --- | --- | --- |
| | | | Control | Treatment |
| | Full Population | Full Sample | Tax Year End July–Nov | Tax Year End Dec–June |
| | (1) | (2) | (3) | (4) |
| *Panel A: Firm Characteristics ($ Thousands or Percents)* | | | | |
| Gross Receipts | 1,711 | 7,343 | 7,346 | 7,343 |
| Taxable Income | 56 | 410 | 331 | 424 |
| Taxes Paid | 17 | 128 | 101 | 133 |
| Total Assets | 1,625 | 8,707 | 6,497 | 9,094 |
| In Loss (%) | 46 | 25 | 22 | 26 |
| Public(%) | 0.4 | 3 | 2 | 3 |
| *Panel B: GBC Activity ($ Thousands or Percents)* | | | | |
| Claimed | 3 | 47 | 34 | 49 |
| Earned | 4 | 57 | 40 | 59 |
| Carry-Forward | 7 | 67 | 43 | 71 |
| *Panel C: $-Weighted Distribution of Firms by Total Income (%)* | | | | |
| < $1 million | 4 | 1 | 1 | 1 |
| $1 million–$10 million | 8 | 2 | 4 | 2 |
| $10 million–$25 million | 3 | 1 | 2 | 1 |
| $25 million–$100 million | 6 | 3 | 5 | 3 |
| $100 million–$1 billion | 17 | 16 | 22 | 16 |
| > $100 billion | 61 | 77 | 66 | 78 |
| *Panel D: $-Weighted Distribution of Firms by Industry (%)* | | | | |
| Construction | 3 | 1 | 3 | 1 |
| Manufacturing | 34 | 40 | 39 | 40 |
| Wholesale/Retail | 28 | 26 | 36 | 25 |
| Transportation | 3 | 2 | 0 | 3 |
| Information | 5 | 5 | 5 | 5 |
| Finance | 3 | 3 | 3 | 3 |
| Insurance | 9 | 10 | 0 | 11 |
| Services | 9 | 7 | 9 | 6 |
| Other | 6 | 6 | 4 | 6 |
| N | 1,385,780 | 222,995 | 28,618 | 194,377 |
| Weighted N | 27,187,024 | 1,539,536 | 229,043 | 1,310,492 |

## Table 2
## Impact of 2011 Form 3800 Redesign on Carry-Forward GBCs

This table reports the difference-in-differences specification of the form:

$$log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) + \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) + \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t}$$

for firm $i$ in year $t$. We report the coefficient of interest: $\beta_1$. Panel A and B evaluate different dependent variables. Controls include indicator variables for whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, and whether a firm was ever publicly traded. Controls also include firm size and industry fixed effects. Standard errors in Columns 1,2,4, and 6 are clustered at the firm level to account for auto-correlated errors within firm over time. Standard errors in Columns 3 and 5 account for auto-correlated errors based on the within-firm transformation.

| | Control Years: 2001 - 2011 | | | Control Years: 2001 - 2016 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: Log Carry-Forward General Business Credits** | | | | | | |
| 2011× Calendar-year Firm | 0.231 | 0.249 | 0.262 | 0.204 | 0.226 | 0.262 |
| | (0.0814) | (0.0192) | (0.102) | (0.0734) | (0.0228) | (0.102) |
| Weighted N | 1,159,501 | 1,159,501 | 1,159,501 | 1,642,844 | 1,642,844 | 1,642,844 |
| **Panel B: Carry-Forward General Business Credits ($ Thousands)** | | | | | | |
| 2011× Calendar-year Firm | 8.38 | 7.66 | 6.10 | 7.98 | 8.29 | 6.02 |
| | (1.94) | (3.79) | (2.31) | (1.96) | (3.55) | (2.30) |
| Earned GBC$_{t-1}$ | 0.437 | 0.330 | 0.437 | 0.446 | 0.374 | 0.446 |
| | (0.00852) | (0.00255) | (0.00852) | (0.00644) | (0.00178) | (0.00643) |
| Claimed GBC$_{t-1}$ | -0.370 | -0.367 | -0.370 | -0.364 | -0.360 | -0.364 |
| | (0.00812) | (0.00211) | (0.00812) | (0.00599) | (0.00156) | (0.00599) |
| Weighted N | 970,856 | 970,856 | 970,856 | 1,425,177 | 1,425,177 | 1,425,177 |
| Controls | | ✓ | | | ✓ | |
| Firm FE | | ✓ | | | ✓ | |
| Year FE | | | ✓ | | | ✓ |
| Year FE × Calendar Year Firm FE | | | ✓ | | | ✓ |

**Table 3**
**2011 Form 3800 Redesign: Evidence of Tax Aggressiveness**

This table reports the difference-in-differences specification of the form:

$$log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) + \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) + \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t}$$

for firm $i$ in year $t$. We report the coefficient of interest: $\beta_1$. Panel A and B evaluate different dependent variables. Controls include indicator variables for whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, and whether a firm was ever publicly traded. Controls also include firm size and industry fixed effects. Standard errors in Columns 1,2,4, and 6 are clustered at the firm level to account for auto-correlated errors within firm over time. Standard errors in Columns 3 and 5 account for auto-correlated errors based on the within-firm transformation.

| | Control Years: 2001 - 2010 | | | Control Years: 2001 - 2016 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Log(Tax Aggressiveness)* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | 0.134 | 0.145 | 0.148 | 0.138 | 0.154 | 0.148 |
| | (0.0660) | (0.0170) | (0.0711) | (0.0665) | (0.0174) | (0.0711) |
| | | | | | | |
| Weighted-N | 1,076,383 | 1,076,383 | 1,076,383 | 1,159,501 | 1,159,501 | 1,159,501 |
| *Panel B: $\mathbb{1}(Tax\ Aggressive\ Firm)$* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | 0.0885 | 0.0937 | 0.0969 | 0.0860 | 0.0818 | 0.0969 |
| | (0.0199) | (0.00662) | (0.0262) | (0.0195) | (0.00740) | (0.0262) |
| | | | | | | |
| Weighted N | 1,539,810 | 1,539,810 | 1,539,810 | 1,642,844 | 1,642,844 | 1,642,844 |
| Controls | | ✓ | | | ✓ | |
| Firm FE | | ✓ | | | ✓ | |
| Year FE | | | ✓ | | | ✓ |
| Year FE × Calendar Year Firm FE | | | ✓ | | | ✓ |

**Table 4**

**2011 Form 3800 Redesign: Heterogeneity by Ownership Type**

This table reports the difference-in-differences specification of the form:

$$log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) + \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) + \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t}$$

for firm $i$ in year $t$. Results are reported for two subsamples: firms that were public traded between 2001 and 2016 and those that were never publicly traded between 2001 and 2016. We report the coefficient of interest: $\beta_1$. Panel A and B evaluate different dependent variables. Controls include indicator variables for whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, and whether a firm was ever publicly traded. Controls also include firm size and industry fixed effects. Standard errors in Columns 1,2,4, and 6 are clustered at the firm level to account for auto-correlated errors within firm over time. Standard errors in Columns 3 and 5 account for auto-correlated errors based on the within-firm transformation.

| | Control Years: 2001 - 2010 | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Public | | | Private | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Log(Tax Aggressiveness)* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | -0.127 | -0.030 | -0.154 | 0.136 | 0.142 | 0.144 |
| | (0.167) | (0.159) | (0.211) | (0.0672) | (0.0160) | (0.0723) |
| | | | | | | |
| Weighted N | 32,589 | 32,589 | 32,589 | 1,043,793 | 1,043,793 | 1,043,793 |
| *Panel B: $\mathbb{1}(Tax\ Aggressive\ Firm)$* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | -0.0212 | -6.98e-05 | -0.0034 | 0.0908 | 0.0954 | 0.0976 |
| | (0.0279) | (0.0266) | (0.0435) | (0.0203) | (0.00736) | (0.0269) |
| | | | | | | |
| Weighted-N | 37,811 | 37,811 | 37,811 | 1,121,689 | 1,121,689 | 1,121,689 |
| Controls | | ✓ | | | ✓ | |
| Firm FE | | ✓ | | | ✓ | |
| Year FE | | | ✓ | | | ✓ |
| Year FE × Calendar Year Firm FE | | | ✓ | | | ✓ |

# A. Appendix: Tables and Figures

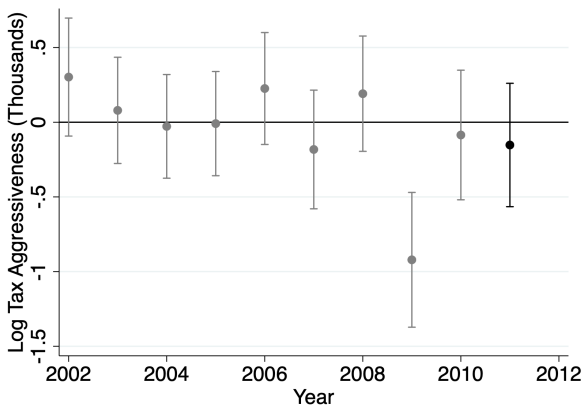**Figure A.1.** Differences-in-Differences: Identification

(a) Public Firms: Carry-Forward Pre-Trends
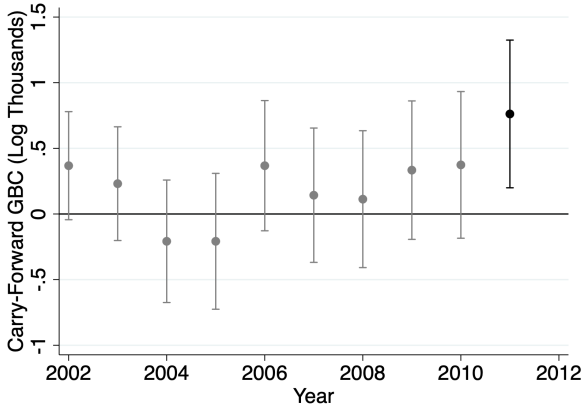
(b) Private Firms: Carry-Forward Pre-Trends
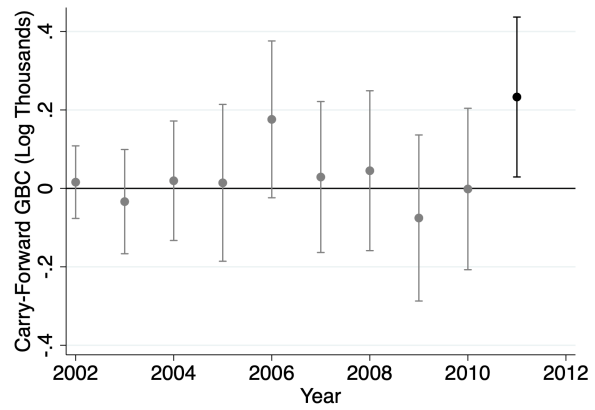
(c) Public: Log(Tax Aggressiveness)
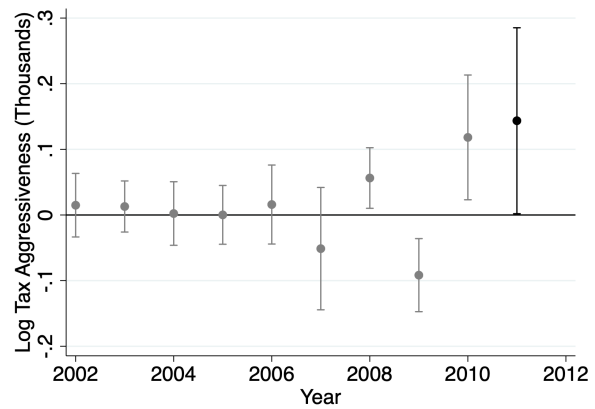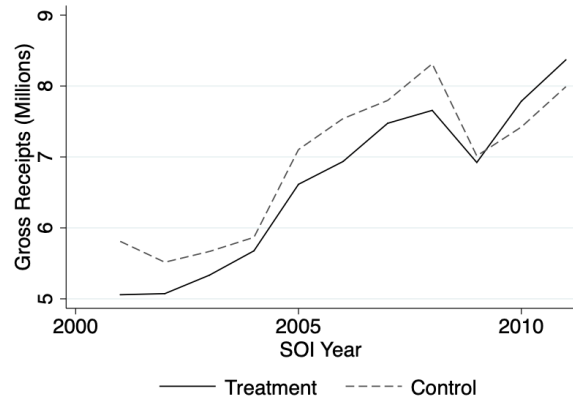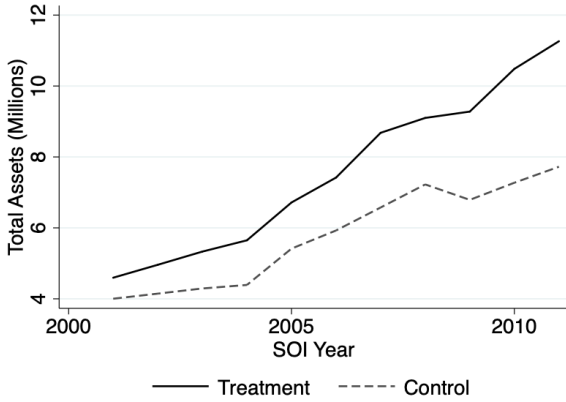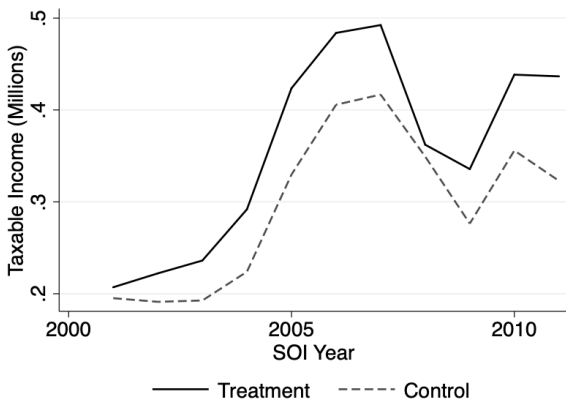
(d) Private: Log(Tax Aggressiveness)

**Figure A.2.** Differences-in-Differences: Parallel Trends

(a) Total Assets

(b) Gross Receipts



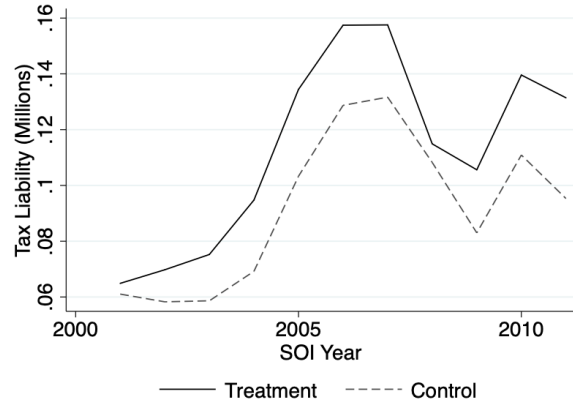(c) Taxable Income

(d) Taxes Paid

**Figure A.3.** Differences-in-Differences: Collapsed Analysis

(a) Log Carry-Forward Credits
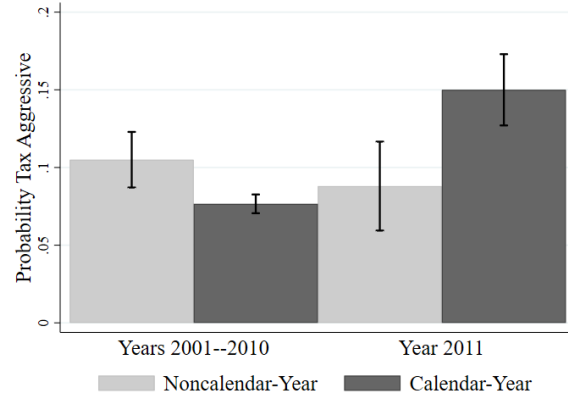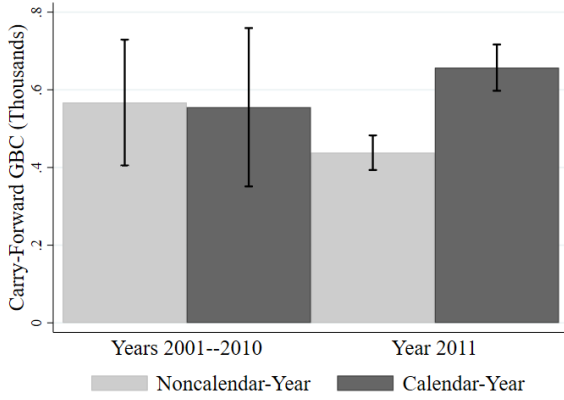
(b) Probability Tax Aggressive

## Table A.1
## Summary Statistics: IRS Corporate Statistics of Income (SOI): Treatment Year

Table details mean values based on Form 1120 and Form 3800 for SOI Years 2000-2016. We detail relevant forms for Tax Year 2015. Taxable Income: Form 1120 Line 30. Total Assets: Form 1120 Schudle L 19d. Total Taxes Paid: Form 1120 Line 31. General Business Credits (GBCs) Claimed: Form 1120 Schedule J Line 5c. GBCs Earned: Form 3800 Part III Line 6, Box A and B. GBCs Carried Forward: Form 3800 Part III Line 6, Box C and F. Control firms in 2011 are those with a Tax Year ending July 1 2011 - Nov 30 2011. Treated firms in 2011 are those with a Tax Year ending Dec 1 2011 - June 31 2012. Both treatment and control firms are included in the 2011 SOI file. $-Weighted Distribution is weighted by Gross Receipts.

|  |  | GBC Subsample | | |
|---|---|---|---|---|
|  |  |  | Control | Treatment |
|  | Full Population | Full Sample | Tax Year End July–Nov | Tax Year End Dec–June |
|  | (1) | (2) | (3) | (4) |
| *Panel A: Firm Characteristics ($ Thousands or Percents)* | | | | |
| Gross Receipts | 1,843 | 8,316 | 7,991 | 8,370 |
| Taxable Income | 57 | 420 | 323 | 437 |
| Taxes Paid | 17 | 126 | 95 | 131 |
| Total Assets | 1,962 | 10,758 | 7,724 | 11,262 |
| Loss Share | 46 | 28 | 23 | 29 |
| Public | 0.0 | 3 | 2 | 3 |
| *Panel B: GBC Activity ($ Thousands or Percents)* | | | | |
| Claimed | 3 | 40 | 31 | 41 |
| Earned | 4 | 51 | 44 | 52 |
| Carry-Forward | 9 | 90 | 45 | 97 |
| N | 1,385,780 | 222,995 | 28,618 | 194,377 |
| Weighted N | 27,187,024 | 1,539,536 | 229,043 | 1,310,492 |

## Table A.2
## Impact of 2011 Form 3800 Redesign on Carry-Forward GBCs

This table reports the difference-in-differences specification of the form:

$$log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) + \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) + \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t}$$

for firm $i$ in year $t$. We report the coefficient of interest: $\beta_1$. Controls include indicator variables for whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, and whether a firm was ever publicly traded. Controls also include firm size and industry fixed effects. Standard errors in Columns 1,2,4, and 6 are clustered at the firm level to account for auto-correlated errors within firm over time. Standard errors in Columns 3 and 5 account for auto-correlated errors based on the within-firm transformation.

| | Control Years: 2001 - 2010 | | | Control Years: 2001 - 2016 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Carry-Forward GBC ($ Thousands)* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | 33.8 | 25.0 | 43.3 | 24.6 | 18.0 | 43.3 |
| | (5.57) | (8.73) | (7.01) | (4.72) | (9.62) | (7.01) |
| | | | | | | |
| Weighted N | 1,159,501 | 1,159,501 | 1,159,501 | 1,642,844 | 1,642,844 | 1,642,844 |
| Controls | | ✓ | | | ✓ | |
| Firm FE | | ✓ | | | ✓ | |
| SOI Year FE | | | ✓ | | | ✓ |
| SOI Year × CY Firm | | | ✓ | | | ✓ |

**Table A.3**

**2011 Form 3800 Redesign: Heterogeneity by Ownership Type**

This table reports the difference-in-differences specification of the form:

$$log(Y_{i,t}) = \beta_0 + \beta_1 \text{Calendar-Year Firm} \times \mathbb{1}(2011) + \beta_2 \text{Calendar-Year Firm} + \beta_3 \mathbb{1}(2011) + \gamma X_{i,t} + \lambda_i + \lambda_t + \eta_{i,t} + \varepsilon_{i,t}$$

for firm $i$ in year $t$. We report the coefficient of interest: $\beta_1$. Panel A and B evaluate different dependent variables. Controls include indicator variables for whether a firm carried a stock of Net Operating Losses, whether a firm was limited by the Alternative Minimum Tax, and whether a firm used a Foreign Tax Credit), whether a firm was taxable, and whether a firm was ever publicly traded. Controls also include firm size and industry fixed effects. Standard errors in Columns 1,2,4, and 6 are clustered at the firm level to account for auto-correlated errors within firm over time. Standard errors in Columns 3 and 5 account for auto-correlated errors based on the within-firm transformation.

| | Control Years: 2001 - 2016 | | | | | |
| --- | --- | --- | --- | --- | --- | --- |
| | Public | | | Private | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Log(Tax Aggressiveness)* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | -0.0873 | 0.00300 | -0.154 | 0.139 | 0.148 | 0.144 |
| | (0.163) | (0.165) | (0.211) | (0.068) | (0.016) | (0.072) |
| | | | | | | |
| Weighted-N | 46,452 | 46,452 | 46,452 | 1,493,357 | 1,493,357 | 1,493,357 |
| *Panel B: $\mathbb{1}(Tax\ Aggressive\ Firm)$* | | | | | | |
| | | | | | | |
| 2011× Calendar Year Firm | -0.0112 | 0.00479 | -0.0034 | 0.0879 | 0.0832 | 0.0976 |
| | (0.0272) | (0.0270) | (0.0435) | (0.0200) | (0.00823) | (0.0269) |
| | | | | | | |
| Weighted-N | 52,987 | 52,987 | 52,987 | 1,589,856 | 1,589,856 | 1,589,856 |
| Controls | | ✓ | | | ✓ | |
| Firm FE | | ✓ | | | ✓ | |
| Year FE | | | ✓ | | | ✓ |
| Year FE × Calendar Year Firm FE | | | ✓ | | | ✓ |

# B. Appendix: Empirical

## B.1. A Technical Note on Log Transformations

We transform our tax aggressiveness measure to account for zero and negative values. Specifically, we shift the distribution of the dependent variable tax aggressiveness, denoted $y$ by the 5th percentile, denoted $c$, to ensure we do not lose important variation. This transformation is not innocuous because it changes the interpretation of the coefficients. To recover the coefficient of interest, we apply a standard correction using a Taylor series approximation. In particular, the difference-in-differences coefficient is $\hat{\beta}_3 = E[\frac{1}{y+c} \frac{\partial y}{\partial \text{Calendar-year Firm} \partial \mathbb{1}(2011)}]$ and the true coefficient is given by $\beta_3^T = E[\frac{1}{y} \frac{\partial y}{\partial \text{Calendar-year Firm} \partial \mathbb{1}(2011)}]$. Given the constant and the estimated coefficient, the true coefficient can be recovered.

$$\hat{\beta}_3 = E\left[\frac{1}{y+c} \frac{\partial y}{\partial \text{Calendar-year Firm} \partial \mathbb{1}(2011)}\right]$$

$$= E\left[\frac{y}{y+c} \frac{1}{y} \frac{\partial y}{\partial \text{Calendar-year Firm} \partial \mathbb{1}(2011)}\right]$$

$$= E\left[\frac{y}{y+c} \beta_3^T\right]$$

$$\approx \left(\frac{\bar{y}+c}{\bar{y}+2c} + \frac{-c^2}{(\bar{y}+2c)^2} + \frac{-c^3}{(\bar{y}+2c)^3} \cdots\right) \beta_3^T$$

$$= \left[\frac{\bar{y}+2c}{\bar{y}+2c} - \frac{c}{\bar{y}+2c}\left(1 + \frac{c}{\bar{y}+2c} + \frac{c^2}{(\bar{y}+2c)^2} + \frac{c^3}{(\bar{y}+2c)^3} \cdots\right)\right] \beta_3^T$$

$$\approx \left[1 - \frac{c}{\bar{y}+2c} \frac{1}{1-\frac{c}{\bar{y}+2c}}\right] \beta_3^T$$

$$= \frac{\bar{y}}{\bar{y}+c} \beta_3^T$$

Therefore, $\beta_3^T = \frac{\bar{y}+c}{\bar{y}} \hat{\beta}_{DD}$.