Firm Responses to Book Income Alternative Minimum Taxes *

Jordan Richmond, September 13, 2022

Abstract

Despite interest from policy-makers in book income alternative minimum taxes (AMTs), research exploring their implications is scarce. In this paper, I use an event study approach to study firm responses to the AMT book income adjustment in 1987. I find no evidence that firms avoid the tax, and no evidence of significant real production or investment responses. I estimate an elasticity of book income of -0.03 $[-0.63,0.56]$. This estimate is lower than previous elasticity estimates because I account for mean reversion. The null results indicate that firms face strong, non-tax incentives to report high book incomes.

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"You cannot go to a mill where somebody is making $20,000 a year and attempt to explain to them why a major American corporation can have over $1 billion in profits and pay no taxes." – Senator Bob Packwood on alternative minimum taxes[1]

I Introduction

In 2017, Amazon reported $5.6 billion in profit but paid $0 in taxes (Gardner, 2020). Profitable firms like Amazon can owe small tax bills because the tax code includes deductions and credits meant to incentivize productive economic behavior, and substantial use of these incentives can eliminate all tax liabilities. Over the last forty years, U.S. tax policy-makers have attempted to eliminate the divergence between firms' incomes and tax liabilities by imposing alternative minimum taxes (AMTs). AMTs assign a lower tax rate to a broader tax base that excludes many deductions and credits, implicitly limiting economic incentives in an effort to raise revenue from profitable firms.

Recently, policy-makers have expressed interest in an AMT based on *book income*, the income firms report on their financial statements. This interest culminated in the Inflation Reduction Act of 2022 which implements a corporate minimum tax based on book income in the United States. Book income AMTs are appealing because book income provides a broad tax base, suggesting the tax could effectively raise revenue from profitable firms that pay few taxes. However, a book income AMT's capacity to raise revenue may be limited by tax avoidance because firms have substantial discretion to determine their own book incomes (Manzon and Plesko, 2002), and broadening the tax base could lead firms to make inefficient changes to their production and investment policies (Diamond and Mirrlees, 1971).

In this paper, I estimate firm responses to a book income AMT by exploiting variation in the minimum tax rate on book income introduced by the alternative minimum tax book income adjustment in 1987 (AMTBIA87), the only previous U.S. AMT that has ever incorporated book income into the tax base.[2] I estimate firm responses using an event study framework that compares firms more likely to be subject to the AMT on book income (treatment) to firms less likely to face the tax (control), dividing firms into treatment and control groups based on their average effective tax rates over 1984-1986.

[1]U.S. Senate Finance Committee Hearing. May 3, 1995. "Alternative Minimum Tax".

[2]This policy has also been referred to as the tax on Business Untaxed Reported Profits (BURP).

Using Compustat data, I find no evidence that firms avoid AMTBIA87. AMTBIA87 imposes a 10 percent tax rate on *book tax differences*, the excess of book income over taxable income. To avoid the tax, firms would have to shrink book tax differences by altering revenue or expense items that count differently under the book and tax systems. Using book tax differences as an outcome variable in an event study framework, my preferred point estimates imply that in the first three years after AMTBIA87 the elasticity of book income with respect to the net of tax rate is -0.03 with a 95% confidence interval of -0.63 to 0.56. In the fourth through sixth years after AMTBIA87 the elasticity of book income is -0.24 with a 95% confidence interval of -0.69 to 0.22.

Previous studies of firm tax base responses to AMTBIA87 find large avoidance responses with book income elasticity point estimates ranging from 1.4 to 2.1 (Dhaliwal and Wang, 1992; Manzon, 1992; Dharmapala, 2020). My estimates reject elasticities of this magnitude because I control for mean reversion. In my event study framework, mean reversion impacts estimates of tax base responses to AMTBIA87 because effective tax rates are mechanically and negatively related to book tax differences. Therefore, expected increases in the low, pre-reform effective tax rates of treatment firms lead to book tax difference declines that stem from the treatment definition and do not represent avoidance responses to AMTBIA87. To distinguish between mean reversion and avoidance, I use a placebo-in-time approach that measures mean reversion using event study estimates of tax base responses to treatment definitions in pre-reform years, and identifies avoidance responses as the difference between firm responses to AMTBIA87 and firm responses to the treatment definition in pre-reform years.[3] Failing to correct for mean reversion yields an elasticity estimate of 1.65, directly in line with the previous literature, while using the placebo-in-time approach with alternative specifications, treatment definitions, outcome constructions, and across different subsamples consistently yields close to zero elasticities.

The placebo-in-time approach relies on an assumption that the effective tax rate time series process, and its impact on book tax differences, remains stable over time. I validate this assumption in three ways. First, I show that event study estimates of book tax

[3] Coombs, Dube, Jahnke, Kluender, Naidu and Stepner (2021) use a similar approach to study the impacts of unemployment insurance withdrawal during the COVID19 pandemic.

difference responses to the treatment definition are stable over a period of years spanning 1977-1989. Second, I characterize a time series process for effective tax rates and use a minimum distance procedure to estimate the parameters governing this process. Using the estimated parameters in simulations, I show that reasonable parameter deviations would introduce minimal bias into placebo-in-time estimates. Third, I use effective tax rate moments in the data to argue that any possible bias introduced by a changing effective tax rate process would only push placebo-in-time estimates towards finding an avoidance response to AMTBIA87, strengthening the argument that previous estimates of book tax difference responses to AMTBIA87 are too large.

The lack of avoidance responses to AMTBIA87 cannot be attributed to a lack of tax salience. Placebo-in-time estimates using tax liability as an outcome suggest firms facing AMTBIA87 saw their tax liabilities increase by an average of 0.29% of lagged assets over 1987-1989. This estimate increases to 0.67% of lagged assets when I exclude multinationals and loss firms that could reduce tax liabilities with foreign tax credits and net operating losses, but I still estimate null avoidance responses in this restricted sample. I also provide evidence that the lack of avoidance response is unlikely to be driven by AMT credits, delayed burden from AMTBIA87 due to the difference between firm fiscal years and tax years, or financing constraints.

I use a static, partial equilibrium model to show that avoidance responses to AMTBIA87 are governed by the relative strength of the tax incentive to report lower book income and non-tax incentives to report high book income. Existing research suggests that firms and their managers face strong incentives to report high book incomes (Burgstahler and Dichev, 1997; Graham, Harvey and Rajgopal, 2005; Terry, 2017), to the extent that they are even willing to pay additional taxes to justify reporting fraudulently high earnings (Erickson, Hanlon and Maydew, 2004). The model predicts that we might only observe avoidance responses to AMTBIA87 among firms with weaker non-tax incentives to report high book income. To test this implication of the model, I estimate placebo-in-time specifications of firm avoidance responses restricting to firms with less incentive-based compensation, missing salient earnings thresholds by large margins, and followed by fewer analysts. These estimates are in line with the core prediction of the model, providing suggestive evidence that firms with weaker incentives to report high earnings exhibit larger avoidance responses.

While the placebo-in-time approach addresses the mean reversion that impacts estimates of book tax difference responses to AMTBIA87, it does not address two other shortcomings of using book tax differences as an outcome. First, because taxable income is constructed from financial statement data, I may understate book tax differences for firms paying minimum taxes. Second, book tax differences capture both tax and accounting responses to AMTBIA87, and do not allow me to isolate each response. To address these issues, I also measure firm avoidance responses to AMTBIA87 using discretionary accruals, a common proxy for earnings management (Dechow, Sloan and Sweeney, 1995). Discretionary accruals are not mechanically related to effective tax rates, are not measured with error for firms paying minimum taxes, and isolate firm accounting responses to AMTBIA87. Book income elasticities based on earnings management responses that highlight accounting-specific manipulation of book income yield similar null results as the placebo-in-time estimates.

AMTBIA87 persisted for three years, but was replaced by the adjusted current earnings adjustment (ACEA90) in 1990. ACEA90 levied a 15% marginal tax rate on a base meant to be as broad as book income, but constructed using tax principles and therefore not precisely the same as the AMTBIA87 base. Event study estimates suggest there was no book tax difference or discretionary accrual response to the policy transition. This evidence eases concerns that my null results are driven by differential effects of economic conditions at a specific point in time for treatment relative to control firms.

Additional event study estimates show that firms are unlikely to respond to a book income AMT by modifying their production or investment policies. Using sales, costs of inputs, investment, debt, and employment as outcomes, I fail to reject the null hypothesis of zero response to AMTBIA87 in any year from 1987-1992 for all five outcomes. Complementary instrumental variables analysis suggests that increases in total tax liability stemming from AMTBIA87 have no detectable impact on firm sales, costs of inputs, investment, or debt.

To evaluate the implications of the firm responses that I estimate for contemporary policies, I develop a ten-year revenue score for the book income AMT included in the Biden administration's 2020 tax plan. I estimate that, if firms respond to a modern book income AMT as they did to AMTBIA87, this proposed book income AMT would raise $337 billion over a decade. Using larger elasticity estimates from earlier work on firm responses to AMTBIA87 understates projected revenue by 18%. Close to one-third of the revenue comes from the ten firms facing the largest tax liability increases, which include Hewlett Packard, Berkshire Hathaway and Delta. However, Amazon only faces the 42nd largest tax liability increase because foreign tax credits and losses reduce their book income AMT liability. These results suggest that many firms, not just Amazon, have diverging incomes and tax liabilities that would be targeted by a book income AMT, and that narrowing the tax base may leave leeway for profitable firms to mitigate increases in tax liability.

This paper contributes to a substantial literature that uses financial statement or tax data to estimate tax base and earnings management responses to AMTBIA87 (Gramlich, 1991; Dhaliwal and Wang, 1992; Boynton, Dobbins and Plesko, 1992; Manzon, 1992; Wang, 1994; Choi, Gramlich and Thomas, 2001). In contrast to most previous work, I estimate null avoidance responses to AMTBIA87, and show these differences arise from a failure to account accurately for mean reversion. In addition, I build on this previous work by estimating production and investment responses to AMTBIA87.

This paper also contributes to a large body of research that examines the ability of minimum taxes and other government interventions to reduce corporate tax evasion and avoidance (Mosberger, 2016; Alejos, 2018; Almunia and Lopez-Rodriguez, 2018; Lobel, Scot and Zuniga, 2020; Bachas and Soto, 2021) and the welfare impacts of broadening the tax base (Diamond and Mirrlees, 1971; Best, Brockmeyer, Kleven, Spinnewijn and Waseem, 2015). One major contribution of this literature is to show that broad-based taxes can be welfare enhancing if they offset production distortions with increased revenue through reduced evasion or avoidance. The empirical evidence in this paper suggests that under a tax on book income, the tension between firm's desire to report high earnings to investors and report low earnings to minimize tax liability can help limit evasion or avoidance.

II Tax Policy Details

AMTBIA87 is one piece of the Tax Reform Act of 1986 (TRA86), which made a number of changes to the corporate tax system. Overall, the reform broadened the tax base while lowering the tax rate. TRA86 reduced statutory corporate tax rates from 46% to 40% in 1987 and 34% in 1988, while repealing the investment tax credit, reducing the generosity of depreciation schedules, and broadening the corporate minimum tax base.

Minimum taxes assign a lower tax rate to a broader tax base. AMTBIA87 implements a minimum tax based on book income, the income firm's report on their financial statements. AMTBIA87 requires firms to pay a 20% minimum tax on a tax base that includes taxable income (TI), some tax preferences and adjustments (TPA) allowed as tax deductions for the normal corporate tax, and a book income adjustment (BIA) equal to half the difference between book income and the sum of taxable income and preferences and adjustments.[4]

In equations,

$$\begin{aligned} BIA &= \max\{0.5\Big(BI - (TI + TPA)\Big), 0\}, \\ AMT &= \max\Big\{0.2\Big(TI + TPA + BIA\Big) - \tau TI, 0\Big\}. \end{aligned} \tag{1}$$

In short, AMTBIA87 imposes a 10% marginal tax rate on book income in excess of taxable income plus preferences and adjustments for any firms subject to the AMT.

AMTBIA87 increases taxes for firms with book incomes much larger than their taxable incomes. Writing tax preferences and adjustments as a fraction of the difference between book and taxable income $f(BI - TI)$ and rewriting equation (1), firms face positive AMT liability if $BI > \frac{\tau - 0.1 + 0.1f}{0.1 + 0.1f} TI$. If we assume that tax preferences and adjustments account for all of book tax differences ($f = 1$), then firms must pay the AMT in 1987 if $BI > 2TI$ because $\tau = 0.4$. On the other hand, if we assume that tax preferences and adjustments account for a negligible fraction of book tax differences ($f \to 0$), then firms must pay the AMT in 1987 if $BI > 3TI$.

Effective tax rates distinguish between firms that do and do not face AMTBIA87. Defining effective tax rates as the ratio of tax expense to book income $\frac{\tau TI}{BI}$, firms with the largest ratios of book to taxable income have the lowest effective tax rates and face the minimum tax. Projecting back before the implementation of the policy to 1986 when $\tau = 0.46$, firms

[4]Depreciation of property placed in service after 1986 and depletion account for 88% of tax preferences and adjustments from 1987-1989 (Gill and Treubert, 1992). Other preferences and adjustments include long-term contracts entered into after 1986, intangible drilling costs and passive activity losses.

with book income more than twice as large as taxable income will have ETRs $< 23\%$, while firms with book income more than three times as large as taxable income will have ETRs $< 15\%$. Aggregate data released by the IRS suggests that, averaging across 1987-1989, $f = 0.39$ (Gill and Treubert, 1992), but I do not have firm-level data on specific tax preferences and adjustments. Therefore, I use ETR cutoffs corresponding to $f = 0, 0.39$ and 1 to split firms into treatment and control groups in section IV.

Congress voted to adopt AMTBIA87 during 1986, implying that firms were aware of the policy while filing their 1986 financial statements. AMTBIA87 went into effect the next year, in 1987. The window between the 1986 vote and 1987 implementation provided an opportunity for firms to respond to AMTBIA87 through advanced accounting planning.

During the legislative debate over TRA86, Congress considered multiple AMT reforms. Congress was unsure whether to implement AMTBIA87 or the adjusted current earnings adjustment (ACEA90), which aimed to construct a measure of income as broad as book income using tax principles (Redmond Soneff, 1986). In the final version of TRA86, Congress chose to implement AMTBIA87 from 1987 to 1989 and replace it with ACEA90 in 1990, but also commissioned a Treasury study due before the 1990 switch to explore the impacts of both AMT policies (Redmond Soneff, 1986). While this congressional hedging likely caused some uncertainty about whether AMTBIA87 would be replaced with ACEA90, the policy switch occurred in 1990 as originally specified.

ACEA90 imposed a 20% tax on three-quarters of the difference between a corporation's adjusted current earnings (ACE) and the sum of their taxable income and tax preferences and adjustments. In equations,

$$\begin{aligned} ACEA &= 0.75\Big(ACE - (TI + TPA)\Big), \\ AMT &= \max\{0.2\Big(TI + TPA + ACEA\Big) - \tau TI, 0\}. \end{aligned} \tag{2}$$

ACE attempted to construct a measure of income as broad as book income using tax principles by eliminating additional deductions to broaden the base (Janiga, 1988).[5] Finally,

[5]For example, ACE includes depreciation that is allowed as a deduction for taxable income purposes. ACE also includes forms of income excluded from taxable income like interest on tax-exempt bonds and income on annuity contracts. Janiga (1988) provides additional details.

both AMTBIA87 and ACEA90 generated minimum tax credits that could be used to reduce normal tax liability down to minimum tax liability in future years.[6]

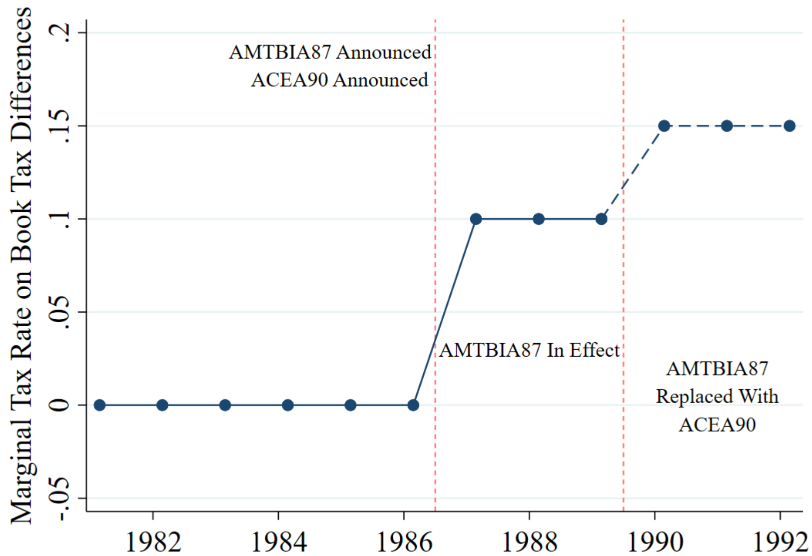


Figure 1: Marginal Tax Rates on Book Tax Differences

Notes: This figure presents the evolution of marginal tax rates on book tax differences due to the alternative minimum tax book income adjustment (AMTBIA87) and the adjusted current earnings adjustment (ACEA90). The dashed line after 1989 represents the change in the tax base from book tax differences to the excess of adjusted current earnings over taxable income plus tax preferences and adjustments, assuming that adjusted current earnigns are equivalent to book income. Tax rates assume proper controls for tax preferences and adjustments.

After controlling for TPA, AMTBIA87 imposed a marginal tax on book tax differences from 1987-1989. Starting in 1990, ACEA90 imposed a marginal tax on a different quantity than book tax differences, but was meant to achieve a similar purpose. In the rest of the paper I treat ACEA90 as a tax on the same base as AMTBIA87, but provide separate estimates for the 1987-1989 and 1990-1992 periods to ensure the changing tax base does not impact estimates of firm responses to AMTBIA87 during the years the policy was in effect.

I summarize variation in the marginal tax rate on book tax differences over time in Figure 1, assuming ACEA90 applies to the same base as AMTBIA87. There was no tax on book tax differences before 1987. From 1987 to 1989, AMTBIA87 imposed a 10% marginal tax

[6]AMT credits are awarded for income and expense items that cause temporary differences between adjusted taxable income and taxable income over time like depreciation, but not exclusion items that cause permanent differences over time like exclusions for small business stock gains.

rate on book tax differences. Starting in 1990, the replacement of AMTBIA87 with ACEA90 increased the marginal tax rate on book tax differences to 15%.

III Data

To evaluate how firms respond to AMTBIA87, I construct a balanced panel of Compustat firms, restricting to firms with non-missing total accruals and positive, non-missing assets, sales, and pretax income that are incorporated in the United States and appear in the data every year from 1981 to 1992. I end the baseline panel in 1992 because the Omnibus Budget Reconciliation Act of 1993 changed the ACEA90 tax base.[7] However, to examine earlier and later years, I also use balanced panels spanning 1974-1986 and 1984-1995. Both are constructed with the same restrictions as the 1981-1992 panel.

Table 1 provides summary statistics for a 1985 cross section of the 1981-1992 balanced panel, with all variables rescaled into 2018 dollars.[8] I winsorize all outcome variables at the 1st and 99th percentile to minimize the role of outliers in the results and scale all outcomes by lagged assets to account for skew in the firm size distribution. Means exceed medians for most variables across the whole sample, reflecting the skewed firm size distribution. The 1981-1992 panel only includes 11% of all firms in Compustat in 1985, but these firms hold 20% of all assets and take in 31% of all revenues. While these firms are not representative of the economy as a whole, they do represent the large firms targeted by AMTs.

The key variable to categorize firms into treatment and control groups is the effective tax rate, because firms with lower effective tax rates are more likely to face AMTBIA87. I measure effective tax rates as tax liability divided by book income. Following Collins and Shackleford (2004), I define tax liability as total income taxes minus deferred income taxes minus other taxes.[9]

I use the tax base, book tax differences (BTDs), as a summary measure of firm's tax avoidance responses to AMTBIA87. I measure book tax differences as the difference between

[7]OBRA eliminated the adjusted current earnings depreciation adjustment for property placed in service after 1993, effectively narrowing ACE by allowing depreciation deductions.

[8]I inflate to 2018 dollars using the GDP price deflator from NIPA table 1.1.9, "Implicit Price Deflators for Gross Domestic Product" from the BEA.

[9]Results are similar when subtracting state tax expense from tax liability.

Table 1: Summary Statistics for 1985 Cross Section of Estimation Sample

	Observations	Mean	SD	P10	Median	P90
Lagged Assets	845	2854	5919	52	608	7365
Book Income	845	0.15	0.10	0.06	0.12	0.28
Taxable Income	845	0.11	0.10	0.01	0.09	0.25
Book Tax Differences	845	0.04	0.04	−0.01	0.03	0.08
Discretionary Accruals	845	0.00	0.06	−0.06	0.00	0.07
Effective Tax Rate	845	0.31	0.18	0.06	0.35	0.48
Sales	845	1.43	1.06	0.43	1.27	2.53
Costs of Goods Sold	845	0.97	0.85	0.23	0.80	1.86
Investment	840	0.23	0.17	0.07	0.19	0.46
Debt	844	0.27	0.10	0.14	0.29	0.39
Depreciation	845	0.05	0.03	0.02	0.04	0.08
Depletion	845	0.01	0.02	0.00	0.00	0.04
Employment	819	12	24	0	3	31

Notes: This table reports summary statistics for a 1985 cross section from the 1981-1992 balanced panel of firms used to estimate firm responses to the alternative minimum tax book income adjustment. Statistics are expressed as a share of lagged assets, except for counts, the effective tax rate, employment (thousands), investment (capital expenditure per dollar of lagged net property plant and equipment) and lagged assets (millions USD).

book income and taxable income. Book income is a broader income measure than taxable income, illustrated by the fact that book tax differences are positive for 85% of firms in 1985. Book and taxable income differ because tax and GAAP rules for realizing income and expense items differ. These differences can be either temporary or permanent. Temporary BTDs arise from income and expense items that count for both tax and book incomes, but that are realized at different times, while permanent BTDs arise from income or expense items that count for either tax or book income, but not both.[10] Any firm attempting to avoid AMTBIA87 would have to manipulate income or expense items to shrink either permanent or temporary book tax differences.

Book income is reported directly on firm's financial statements, but taxable income is not. To construct taxable income, I divide tax liability by the marginal tax rate (Manzon and Plesko, 2002). While book tax differences provide a useful measure of tax base responses

[10]Estimates of temporary book tax differences can be constructed as deferred tax expense divided by the marginal tax rate and estimates of permanent book tax differences can be constructed as the difference between total and temporary book tax differences (Poterba, Rao and Seidman, 2011). Unfortunately, comprehensive data on individual book tax difference components is not available (Raedy, Seidman and Shackelford, 2011).

to AMTBIA87, they also present three problems. First, the binned scatter plot in Figure 2 shows that book tax differences are mechanically related to the effective tax rate.[11] An OLS regression of book tax differences scaled by lagged assets on effective tax rates in the 1981-1992 balanced panel yields a coefficient of -0.18. Second, scaling tax expense by the marginal tax rate will overstate taxable income and understate book tax differences when part of firm's current tax liability comes from minimum taxes. This shrinks the book tax differences of firms paying minimum tax, potentially biasing my event study estimates towards finding avoidance responses because treatment firms are more likely to pay minimum tax. Third, book tax differences capture both book and tax responses with no ability to distinguish between the two.

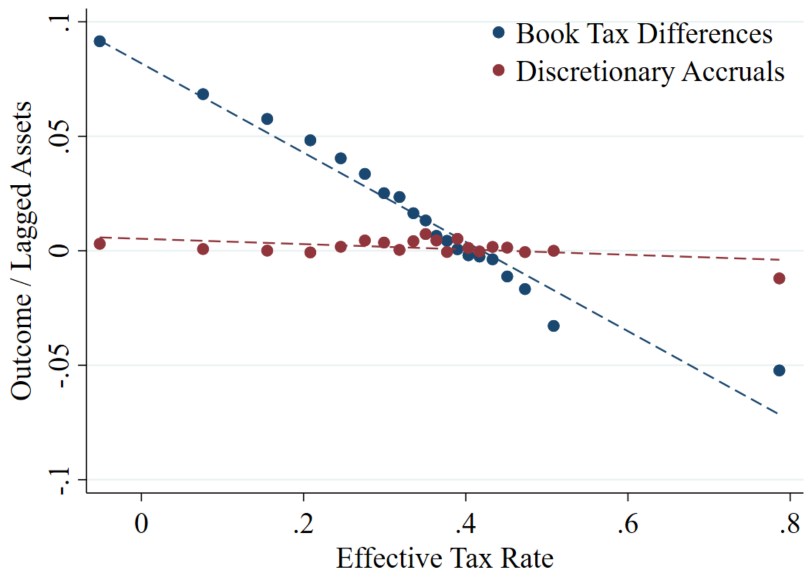


Figure 2: Relationships Between Avoidance Outcomes and Effective Tax Rates

Notes: This figure presents a binned scatter plot of the relationship between two outcomes, book tax differences and discretionary accruals, and effective tax rates. The binned scatter plot uses all firms and years in the 1981-1992 balanced panel. The dashed lines are linear fits of the points in each series.

One way I address these concerns is by using discretionary accruals, a common proxy for earnings management, as a second measure of firm's tax avoidance responses to AMT-

[11] To see the mechanical relationship mathematically, note that I construct taxable income as $\widehat{TI} = \frac{\text{Current Tax Expense}}{\text{Marginal Tax Rate}}$. Book income is reported directly on firms financial statements. Then $BTD = BI - \widehat{TI}$ and $ETR = \text{Current Tax Expense}/BI$. Therefore, an increase in current tax expense or a decrease in BI both lead to an increase in ETR and a decrease in BTD.

BIA87 (Healy, 1985; Jones, 1991; Boynton, Dobbins and Plesko, 1992; Dechow, Sloan and Sweeney, 1995). Discretionary accruals measure the components of earnings not explained by cash flows and not predicted by economic conditions by residualizing accruals on revenues and capital stocks. Managers have a great deal of discretion to manipulate these earnings (Bergstresser and Philippon, 2006). I closely follow Dechow, Sloan and Sweeney (1995) to construct discretionary accruals, and describe this procedure in Appendix A. Discretionary accruals help alleviate concerns with using book tax differences as an outcome because discretionary accruals are not mechanically related to effective tax rates (see Figure 2), are not understated for firms paying minimum tax, and isolate accounting responses to AMTBIA87. The standard deviation of discretionary accruals in the sample is 6% of lagged assets.

The key outcomes to measure production and investment responses are sales, costs of goods sold, investment, debt and employment. I define investment as capital expenditure per dollar of lagged net property plant and equipment (Cummins, Hasset and Hubbard, 1994; Desai and Goolsbee, 2004; Edgerton, 2010; Ohrn, 2018) and debt as total liabilities per dollar of lagged assets (Edgerton, 2010; Ohrn, 2018). Some firms in the sample are missing information required to construct the investment, employment, and debt variables.[12] I use depletion as a control in most regressions and impute missing depletion data with zeros, but results are not sensitive to eliminating this control variable.

I supplement the Compustat data with Execucomp data to explore whether incentive-based compensation mitigates downwards earnings manipulation in response to AMTBIA87 because managers with incentive-based compensation face stronger incentives to report high earnings that keep stock prices high. I measure incentive-based compensation using the value of all regular and restricted stock option grants to executives as a fraction of total compensation (stock options, salary, and bonus), summing across all firm managers (Desai and Dharmapala, 2006). The executive compensation data has limited coverage and is only available for 56% of the sample.[13] Among sample firms present in the Execucomp data, the

[12] I linearly interpolate capital expenditures between non-missing firm-year observations to increase coverage of the investment variable, but results are near identical without the interpolation.

[13] The executive compensation data also does not begin until 1992, the last year of my panel. Hall and Liebman (1998) document rapidly rising rates of stock-based compensation among the managers of large public companies throughout the 1980s and 1990s, so firms with low stock-based compensation by 1992-1994 seem unlikely to have used it in earlier years, but I cannot rule this out. As one robustness check, I measure incentive-based compensation averaging over 1992-1994 and using only 1992 and find similar results.

average share of compensation that is incentive-based is 17.7%.

I also supplement the Compustat data with IBES data to explore whether analyst coverage mitigates downwards earnings manipulation in response to AMTBIA87 (Yu, 2008). I measure analyst coverage as the mean number of analysts covering a firm across 1981-1992. Firms in the sample are covered by an average of 3.3 analysts.

IV Tax Avoidance Responses

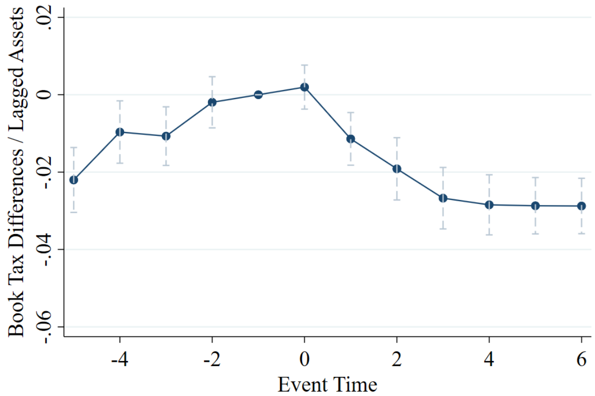
To study whether firms avoid AMTBIA87 by reducing their book tax differences (BTDs), I use an event study framework to compare the BTD responses of treatment firms with average effective tax rates (ETRs) over 1984-1986 $<$ 23% to control firms with average ETRs $\geq$ 23%. Averaging ETRs over three years captures firms with persistently low ETRs. I choose the 23% cutoff, derived in section II, to conservatively exclude any firm from the control group that could face positive tax liability from AMTBIA87.

I estimate

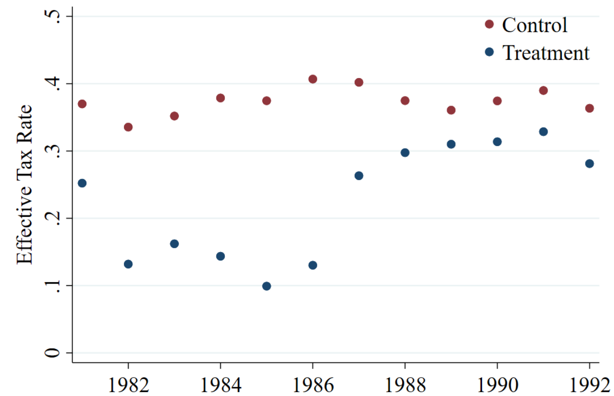
$$Y_{ie} = \sum_{e=-5, e \neq -1}^{6} \Big(\beta_e \cdot Treat_{ie} \Big) + \rho X_{ie} + \delta_e + \gamma_i + \varepsilon_{ie}, \tag{3}$$

where $Treat_i$ is a dummy = 1 if average ETRs over years 1984-1986 $<$ 23%, $Treat_{ie}$ is the interaction of $Treat_i$ with event time dummies, and the last year of the treatment definition, in this case 1986, is event time zero. I plot estimates of the event study coefficients in Figure 3, panel (a). These estimates appear to suggest there are large negative BTD responses to AMTBIA87 for treatment relative to control firms.

However, this treatment definition leads to some expected mean reversion because the treatment group is selected to have low ETRs in specific years. In a time series model of the ETR process with mean zero shocks, this treatment assignment will select firms with negative shocks in the years used to split firms into treatment and control groups, suggesting that on average the ETRs of the treatment firms will increase in time periods after treatment assignment independent of any policy change. Figure 3, panel (b) plots average ETRs among treatment and control firms in the 1981-1992 panel. Treatment firm's ETRs are low in the



(a) Event Study



(b) Average Effective Tax Rates

Figure 3: Event Study Estimates with Mean Reversion

Notes: This figure plots event study estimates of book tax difference responses to AMTBIA87 and the effective tax rate mean reversion pattern that may drive these estimates. Panel (a) plots point estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs and clustering standard errors at the firm level. Panel (b) plots average effective tax rates in each year for the treatment and control groups.

years of the treatment definition and rise in the years after the treatment definition. This increase in ETRs among treatment firms will lead to BTD declines like the one in panel (a) because, as shown in Figure 2, ETRs are mechanically related to BTDs. Therefore, a key challenge in this empirical set up is to differentiate between BTD responses to AMTBIA87 and BTD changes caused by mean reversion stemming from the treatment definition.

To test for the presence of mean reversion in this empirical setting, I re-estimate equation (3) dividing firms into treatment and control groups based on ETRs in earlier years. Each alternative treatment series in Figure 4, panel (a) plots estimates of β_e from equation (3) using the 1974-1986 balanced panel, and a treatment definition based on ETRs in the years indicated by the series labels. Each series using earlier years to split firms into treatment and control shows the evolution of BTDs in response to the treatment definition in the absence of AMTBIA87 using only pre-reform years. Therefore, each alternative series provides a test for the presence of mean reversion. If there was no mean reversion associated with the treatment definition, we would expect each estimate of β_e in each alternative treatment series to equal zero. Instead, the estimates show sharp declines in BTDs and reject zero in every event time greater than zero, clearly demonstrating mean reversion stemming from the treatment definition.

An intuitive way to estimate book tax difference responses to AMTBIA87 while controlling for mean reversion is to subtract firm responses to placebo treatments before the policy is implemented from firm responses to the policy. I implement this placebo-in-time approach by subtracting average firm responses to treatment definitions in pre-AMTBIA87 years from the firm responses to AMTBIA87 displayed in Figure 3, panel (a).

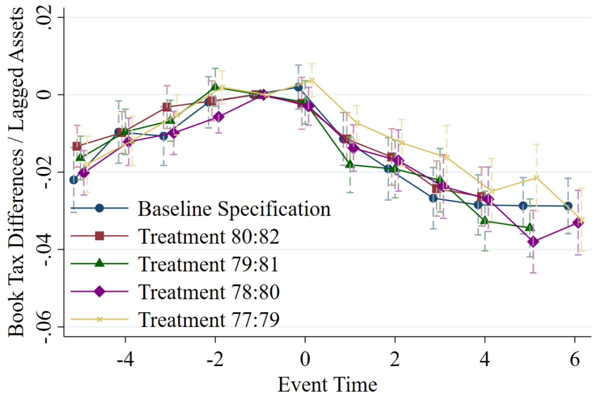
I use a stacked event study specification to estimate average firm responses to treatment definitions in years before the policy. To construct the sample for this regression, I take the 1981-1992 balanced panel, define $Treat_{id}$ as a dummy = 1 if average ETR over three year period $d \in D < 23\%$, and stack copies of the data, one for each alternative treatment definition d. Then, I estimate

$$Y_{ied} = \sum_{e=-5, e\neq -1}^{6} \Big(\eta_e \cdot Treat_{ied}\Big) + \psi Treat_{id} + \rho X_{ied} + \delta_e + \gamma_i + \varepsilon_{ied}, \tag{4}$$

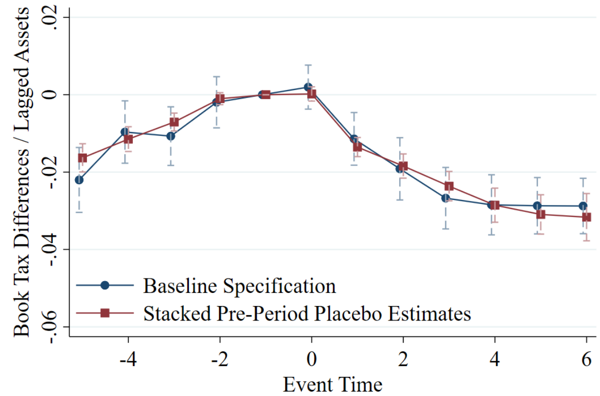
on the stacked data, using a large set of placebo treatments $D = \{(77-79), (78-80), (79-81), (80-82), (81-83), (82-84), (83-85)\}$ and restricting to years before 1987 to avoid any bias from the implementation of AMTBIA87. The stacked versions of the data with treatment definitions starting in 1980 and earlier use the 1974-1986 balanced panel.

The estimand of interest is $\beta_e - \eta_e$ (from equations (3) and (4) respectively), the BTD response to AMTBIA87 in excess of average BTD responses to placebo treatment definitions based on years not directly before the implementation of AMTBIA87. All estimates of equations (3) and (4) include depreciation and depletion as controls to flexibly account for tax preferences and adjustments that are not part of the AMTBIA87 base because depreciation of property placed in service after 1986 and depletion account for 88% of tax preferences and adjustments from 1987-1989 (Gill and Treubert, 1992).

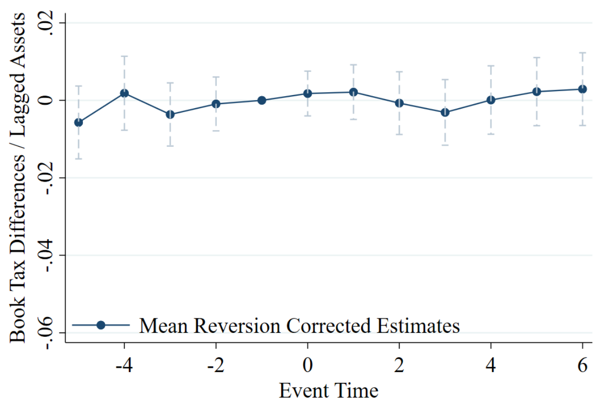
I plot my baseline estimates of β_e alongside estimates of η_e from the stacked event study in Figure 4, panel (b) and the difference $\beta_e - \eta_e$ in panel (c). Estimates from the baseline and placebo series track each other closely, suggesting that the perceived BTD responses in the baseline series are due entirely to mean reversion and do not represent tax avoidance responses to AMTBIA87. The mean reversion corrected estimates in panel (c) are all close



(a) Year-by-Year Placebos



(b) Averaged Placebos



(c) Mean Reversion Corrected Estimates

Figure 4: Placebo-in-Time Estimates

Notes: This figure plots placebo-in-time estimates of tax avoidance responses to AMTBIA87. Panel (a) plots point estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs in the baseline specification series. The earlier series plot β_e estimates using the 1974-1986 balanced panel splitting into treatment and control groups based on earlier years specified in the series labels. Panel (b) plots point estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs in the baseline specification series, and estimates of η_e from equation (4) splitting data from a 1974-1986 balanced panel and the 1981-1992 balanced panel into treatment and control groups based on ETRs from three year time periods spanning 1977-1985 in the stacked pre-period placebo series. Panel (c) plots estimates of $\beta_e - \eta_e$, the difference between the baseline specification and stacked pre-period placebo series in panel (b) with confidence intervals constructed from non-parametrically bootstrapped standard errors using 300 iterations. All standard errors are clustered at the firm level.

to zero and reject avoidance in excess of roughly 1% of lagged assets in every year after AMTBIA87. There is no evidence of firms shifting income into 1986 to avoid the tax, or of a BTD avoidance response to the transition from AMTBIA87 to ACEA90.

IV.A Validating the Placebo-in-Time Approach

The placebo-in-time approach comparing estimates of equations (3) and (4) relies on an assumption that the time series process of ETRs, and its impact on BTDs, does not change because of the implementation of AMTBIA87. This assumption is similar to the common parallel trends assumption underlying difference-in-differences designs. The key distinction is that while difference-in-differences designs assume the outcomes of treatment and control groups would have evolved similarly in the absence of a policy, the placebo-in-time approach assumes that the outcome response to placebo treatment definitions in pre-reform years is the same as the outcome response to treatment definition when the policy is implemented, but in the absence of the policy. If prior-year responses to placebo treatment definitions underestimate mean reversion, this would bias placebo-in-time estimates towards finding larger avoidance responses, while overestimates of mean reversion would bias placebo-in-time estimates towards finding smaller responses.[14]

In this section, I take three approaches to validating the placebo-in-time approach. First, I show visual evidence that the pattern of book tax difference responses to treatment definitions is stable over a wide range of years before and after AMTBIA87 is implemented. Second, I characterize a time series process for effective tax rates and use a minimum distance procedure to estimate the parameters governing this process. Applying the estimated parameters in simulations, I show that significant deviations from the estimated parameters would introduce minimal bias into placebo-in-time estimates. Third, I use raw mean and variance moments of the effective tax rate distribution to argue that any changes to the ETR time series process that might bias placebo-in-time estimates should result in larger estimated avoidance responses. Despite this, I still estimate null responses, strengthening the argument that firms facing AMTBIA87 did not shrink their BTDs to avoid the tax.

Visual Placebo-In-Time Evidence: Appendix Figure D.1 plots book tax difference

[14]If prior-year responses to placebo treatment definitions understate the amount of mean reversion, they understate the book tax difference decline in response to the treatment definition. Therefore, placebo-in-time estimates will subtract a negative number that's magnitude is too small and be biased towards finding larger BTD declines among treatment firms.

responses to a wide range of placebo treatment definitions based on years before and after AMTBIA87 is implemented. While these series exhibit some variation across treatment definitions, they also track the baseline specification, suggesting that the mean reversion of ETRs and its impact on BTDs due to the treatment definition remained stable through the period before and after the implementation of AMTBIA87.

Minimum Distance Estimation of Time Series Process: To quantify how changes to parameters driving effective tax rate variation might bias placebo-in-time estimates, I also estimate the parameters governing the ETR time series process. Suppose ETRs follow

$$\begin{aligned} ETR_{it} &= ETR_i + u_{it} + e_{it}, \\ u_{it} &= \alpha u_{it-1} + \varepsilon_{it}, \end{aligned} \tag{5}$$

where we assume $\varepsilon_{it} \sim \mathcal{N}(0, \nu_t)$ and $e_{it} \sim \mathcal{N}(0, \sigma_e^2)$ are serially uncorrelated and uncorrelated with each other. Treatment firms have $\frac{1}{3}\sum_{e=-2}^{0} ETR_{ie} < 0.23$, and mean reversion occurs because $\mathbb{E}[ETR_{i1}|\frac{1}{3}\sum_{e=-2}^{0} ETR_{ie} < 0.23] > ETR_{i0}$ as long as $ETR_i > 0.23$. As shown in Figure 3, panel (b), average firm ETRs are well above 0.23. Therefore, the time series model clarifies that treatment firm's ETRs are low in the years of the treatment definition because of negative shocks and rise in the years after the treatment definition as shocks change over time.

I use classical minimum distance to estimate the parameters governing the ETR time series process shocks to understand whether changes to this process are biasing my placebo-in-time estimates. The estimation procedure fits the model in equation (5) to the elements of the effective tax rate covariance matrix. Formally, I estimate

$$\min_{\theta} [\hat{m} - m(\theta)]'[\hat{m} - m(\theta)], \tag{6}$$

where $\theta = \{\alpha, \{\nu_t\}_{t=81}^{89}, \sigma_e^2\}$, $m(\theta)$ is a vector of the elements of the ETR covariance matrix, and $\hat{m}$ are the estimates of ETR covariances and autocovariances in the data. I use data from 1981-1989 to focus on the time period before the implementation of AMTBIA87 and to eliminate variation that could be caused by ACEA90. I discuss the estimation in detail in Appendix B.

The key parameters that I estimate are the variance of shocks ν_t, and the persistence of shocks α. Table D.1 displays all parameter estimates with standard errors. I estimate that the shock variances $\{\nu_t\}_{t=81}^{89}$ all take on values between 0.016 and 0.022 with standard errors <= 0.002. While the shocks remain relatively stable over time, they are statistically distinguishable from one another. A Wald test rejects the joint null hypothesis that $\nu_t = \nu_{t+1} \forall t \in [1981, 1988]$ (Wald statistic = 115.3). I estimate that α is 0.22 with a 95% confidence interval spanning [0.2,0.24] and do not allow α to vary over time in estimation.

Changes in these parameters could bias placebo-in-time estimates. If the persistence of shocks α increases or the variance of shocks ν_t decreases when AMTBIA87 is implemented, we would expect less abrupt mean reversion among treatment firms, prior-year responses to placebo treatment definitions would overstate the amount of mean reversion, and placebo-in-time estimates would be biased towards finding smaller avoidance responses.[15]

To evaluate possible bias quantitatively, I simulate nine period panels of effective tax rates using the estimated parameters in Table D.1 while varying α or ν_t. Appendix Figure D.2, panel (a) plots the evolution of effective tax rates for treatment and control firms in three simulated panels, splitting firms into treatment and control groups using average ETRs in the middle three time periods in the simulations and setting the sixth period of each panel to be event time zero. The baseline panel uses $\alpha = 0.22$ in every time period. The low persistence panel uses $\alpha = 0.00$ after event time zero, while the high persistence panel uses $\alpha = 0.45$ after event time zero. The baseline series track ETR patterns in the data displayed in Figure 3, panel (b) reasonably closely. Figure D.2, panel (c) plots the difference in average ETRs for treatment and control normalizing event time zero to zero. As expected, the low persistence series exhibits slightly larger mean reversion, while the high persistence series exhibits slightly less mean reversion, but the magnitudes of these differences are small.

The high persistence series ETRs increase by 2.81% less than the baseline series in event time one, suggesting that even if α doubled to 0.45 when AMTBIA87 was implemented, we would only expect firm responses to prior-year treatment definitions to overestimate

[15]Suppose the variance of ETR shocks decreases when AMTBIA87 is implemented. Then past shocks that determine treatment status play a larger role in future ETRs than they would have otherwise, implying less mean reversion for treatment firms back to permanent levels than there was in previous years.

ETR mean reversion by 2.81%. The coefficient in an OLS regression of BTDs on ETRs in the 1981-1992 balanced sample is -0.18, suggesting mismeasuring mean reversion would bias placebo-in-time estimates downwards by 0.51% of lagged assets.[16] In this extreme scenario where persistence doubles to well outside its estimated 95% confidene interval when AMTBIA87 is implemented, bias corrected post-reform estimates in Figure 4 would be close to -0.5% of lagged assets, and confidence intervals would still not reject zero.

Figure D.2, panel (b) plots the evolution of effective tax rates for treatment and control firms in simulated panels with different ν values in event times one through three. The baseline panel uses the estimated values of ν in Table D.1, while the high variance panel uses $\{\nu_e\}_{e=1}^{3} = 0.028$ and the low variance panel uses $\{\nu_e\}_{e=1}^{3} = 0.010$, adding or subtracting the range of estimated ν from the highest and lowest estimated values. As expected, there is more mean reversion in the high variance scenario than the low variance scenario because as the variance of current shocks increases, past shocks play a relatively smaller role.

Figure D.2, panel (d) plots the difference in average ETRs for treatment and control in the different variance panels. The high variance series only mean reverts by 2.57% more than the baseline series in event time one. This suggests that if the variance of ETR shocks rose well above the 95% confidence interval for any of my estimates for three consecutive years after AMTBIA87 was implemented I would only overestimate ETR mean reversion by 2.57% and only bias BTD placebo-in-time estimates towards finding smaller avoidance responses by 0.46% of lagged assets.

Raw Effective Tax Rate Moment Evidence: To complement the minimum distance estimates described above, I also explore effective tax rate mean, variance and autocovariance moments in the data. Appendix Figure D.3, panel (a) displays average ETRs in the 1981-1992 and 1974-1986 balanced panels, while panel (b) displays three year running ETR variances (to approximate the variance during a treatment definition) and autocovariances.

Effective tax rate variances increase from the late 1970s to early 1980s and stay stable afterwards. Autocovariances remain relatively stable but decline slightly throughout the 1980s. Mean ETRs increase in the early 1980s. In contrast to the simulated variance

[16] As discussed later, OLS regressions of changes in BTDs on changes in ETRs among treatment firms for whom mean reversion is salient yields a smaller coefficient of -0.12 suggesting the bias is 0.34% of lagged assets rather than 0.51%.

and persistence increases discussed above, each of these changes would lead BTD responses to prior year placebo treatment definitions to overstate the amount of mean reversion we would expect when AMTBIA87 is implemented in the absence of the policy, and would therefore bias placebo-in-time estimates towards finding larger avoidance responses. I include a detailed discussion of changes in ETR moments in Appendix C.

To summarize, simulations based on minimum distance estimates of parameters governing the ETR process suggest limited bias from significant changes in parameters, while ETR moments from the data suggest that if bias exists, it would only push placebo-in-time estimates towards finding larger avoidance responses to AMTBIA87. As discussed in the next section, my avoidance estimates are smaller than previous estimates in the literature, despite any potential bias stemming from changing ETR moments, and any bias arising from understating BTDs for firms paying minimum tax. This only strengthens the argument that previous estimates of BTD responses to AMTBIA87 are too large.

One remaining concern with the above analysis is that it examines possible changes in the ETR time series process but assumes the relationship between ETRs and BTDs remains fixed before and after AMTBIA87. To test this assertion for treatment firms that exhibit significant average ETR changes, I estimate distributed lag regressions using a stacked dataset analogous to the one used for the stacked event study above using treatment definitions $d \in \{(81-83),(82-84),(83-85),(84-86),(85-87),(86-88),(87-89)\}$, restrict to event times between negative one and one, and using treatment firms estimate

$$\begin{aligned} \Delta BTD_{ied} &= \beta_0 \Delta ETR_{ied} + \beta_1 \Delta ETR_{ied} \times Post_{ied} \\ &\quad + \beta_2 \Delta ETR_{ie-1d} + \beta_3 \Delta ETR_{ie-1d} \times Post_{ied} + \phi_d + \delta_e + \varepsilon_{ied}, \end{aligned} \tag{7}$$

where $Post_{ied}$ is an indicator for years after 1986 for firm i in event time e and treatment d. β_0 and β_2 quantify how BTDs change in response to current and lagged ETR changes, while β_1 and β_3 capture whether that impact changes after the implementation of AMTBIA87. Appendix Table D.2 displays coefficients from an OLS regression of equation (7).[17] I cannot

[17] Appendix Table D.2 also displays coefficients from an instrumental variables regression of ΔBTD_{ied} on ΔETR_{ied} following the form of equation (7), but dropping ΔETR_{ie-1d} to maintain the sample and instrumenting for ΔETR_{ied} and $\Delta ETR_{ied} \times Post_{ied}$ with ETR_{ie-2d} and $ETR_{ie-2d} \times Post_{ied}$ to address

reject a zero coefficient for the interaction of ETR with the *Post* dummy, suggesting the relationship between ETRs and BTDs remains the same before and after the implementation of AMTBIA87. Furthermore, the OLS coefficient on ΔETR is -0.12, suggesting using the -0.18 coefficient from a univariate regression of BTDs on ETRs to scale changes in the ETR time series process into BTD impacts may overstate bias by one third.

IV.B Robustness and Scaling

The effective tax rate below which firms must pay minimum tax, the cutoff I use to define treatment and control groups, depends on tax preferences and adjustments which I do not directly observe in the data. The 23% cutoff used in Figure 4 conservatively assumes that all of book tax differences are tax preferences and adjustments, ensuring that no firm in the control group whose ratio of tax expense to book income remains the same will face the minimum tax. However, in practice, tax preferences and adjustments account for an average of only 39% of book tax differences across 1987-1989 (Gill and Treubert, 1992). Therefore, different effective tax rate cutoffs for the treatment control split may be reasonable.

To explore whether results are sensitive to treatment definition, I test four alternative definitions. The first three definitions split firms into treatment and control groups based on average effective tax rate cutoffs at 15% (TPA are 0% of BTDs), 19% (TPA are 39% of BTDs) and 26%. The last excludes firms with average ETRs between 15-23%, defining treatment as average ETR below 15% and control as average ETR above 23%.

Appendix Figure D.4 plots baseline estimates and stacked pre-period placebo estimates analogous to Figure 4, panel (b) for each treatment definition. Results are not sensitive to alternative treatment definitions. In all cases, baseline estimates track pre-period placebo estimates, and there is no evidence of larger book tax difference declines in response to AMT-BIA87 than the book tax difference declines in response to placebo treatments using any of the four alternative treatment definitions. Appendix Figure D.5 plots mean reversion corrected estimates analogous to Figure 4, panel (c). The vast majority of the post-AMTBIA87 point estimates cannot reject zero response to any of the treatment definitions. Given the stability of the basic results across treatment definitions, I use the conservative 23% ETR

any potential concerns about serial correlation in ETRs biasing the OLS estimates. Results are similar.

cutoff throughout the rest of the paper.

For comparability to other studies, I take the baseline and stacked event study estimates and rescale them into an elasticity of book income with respect to the net of tax rate

$$\varepsilon_e^{BI} = \Big(\frac{\beta_e}{\overline{BI}_\beta} - \frac{\eta_e}{\overline{BI}_\eta}\Big) \cdot \frac{1-\tau}{\Delta(1-\tau)_e}, \tag{8}$$

where $\overline{BI}_\beta, \overline{BI}_\eta$ are average book incomes in the pre-period of the samples used to estimate equations (3) and (4) respectively. I nonparametrically bootstrap standard errors to account for sampling uncertainty in the event study and average book income estimates, clustering at the firm level.

Figure 5 plots elasticity estimates pooling event time coefficients over $e = 1-3$ and $e = 4-6$ by replacing yearly post-period dummies and their treatment interactions in equations (3) and (4) with dummies for event time $e = 1-3$ and $e = 4-6$ and their treatment interactions. The baseline elasticity estimate over 1987-1989 is -0.03 with a 95% confidence interval from -0.63 to 0.56, rejecting elasticity point estimates of the magnitude previously estimated in the literature based off book tax difference responses to AMTBIA87, aggregated in Dharmapala (2020) and represented in the figure by the horizontal dashed black lines.[18]

The elasticity estimates in this paper diverge from previous point estimates in the literature because of the mean reversion correction, not because of different controls, samples or measurement of outcomes. The "no mean reversion" series in Figure 5 plots elasticity estimates based on equation (8) without subtracting η_e. These estimates fall within the range of existing point estimates in the literature.[19] On the other hand, subtracting state

[18]As discussed in Dharmapala (2020), early studies of firm responses to AMTBIA87 "were written at a time when ... it was common to focus on the statistical significance of estimated coefficients rather than on the implications of the magnitudes of these coefficients." Therefore, early studies were not focused on reporting elasticities or their confidence intervals. To understand the magnitude of previously estimated firm responses, Dharmapala (2020) scales the point estimates reported in Dhaliwal and Wang (1992) and Manzon (1992) into elasticities using summary statistic calculations based on a different Compustat panel construction. Dharmapala (2020) does not report confidence intervals on these elasticities, likely because of an inability to estimate covariances between the scaling data and data used in the original estimates. Nevertheless, the point estimates that Dharmapala (2020) scales into elasticities are reported to reject zero, suggesting my estimates do not lie within the confidence intervals of existing estimates in the literature.

[19]Previous authors were aware of potential mean reversion in similar quasi-experimental set-ups, e.g. the discussion in Dhaliwal and Wang (1992) section 4. However, they adjust for possible mean reversion

taxes out of tax expense when constructing BTDs, adding asset tercile time trends (defined in event time negative one), adding two digit SIC industry time trends, excluding finance and utility firms, including years after 1986 in the estimate of equation (4), and excluding depreciation and depletion controls all do not materially impact my elasticity estimates.[20]

These robustness checks rule out a number of concerns. The lack of estimate variation when measuring taxes differently suggests that mismeasurement of tax expense does not drive my results. The fact that the elasticity estimates do not change with the inclusion of asset tercile or industry time trends suggests that if other TRA86 policy changes are biasing my results, this bias cannot be driven by industry or firm size-specific impacts of those policy changes. The lack of estimate variation when excluding finance and utility firms suggests that rate of return regulation and different profit and reporting incentives faced by these firms do not drive my results. The lack of estimate variation when including post-1986 years in the estimation of equation (4) suggests the elasticity estimates are not sensitive to bias in the placebo-in-time counterfactual potentially introduced by AMTBIA87, nor the specific choice of placebo. The lack of estimate variation when excluding depreciation and depletion controls from regressions suggests controls for tax preferences and adjustments are not driving the null estimates and neither is any potential bias introduced by time-varying controls.

Furthermore, I find little heterogeneity in avoidance responses across firm sizes or industries. Appendix Figure D.6, panels (a) and (b) display BTD responses scaled by pre-period standard deviations of the outcome and largely cannot reject the null of zero response across asset terciles or the four industries with the most firms in the sample: manufacturing, trade, transportation, and utilities. Appendix Figure D.7 also shows that placebo-in-time estimates of firm avoidance responses remain similar when scaling outcomes by average assets in the pre-reform period rather than lagged assets, especially over 1987-1989.

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using only a single placebo year. Given the idiosyncratic year-to-year sampling variation in the evolution of treatment relative to control firm's BTDs displayed in Figure 4, panel (a), I view averaging over a number of placebo series as more appropriate. Even if I selected a single placebo treatment definition the vast majority of available years to choose from would yield close to null elasticity estimates.

[20]To standardize SIC codes within firms I use the mode SIC code within firms across years, breaking ties with the smaller SIC code. I impute two digit SIC codes manually based on financial statement information for firms missing an SIC code in every year.

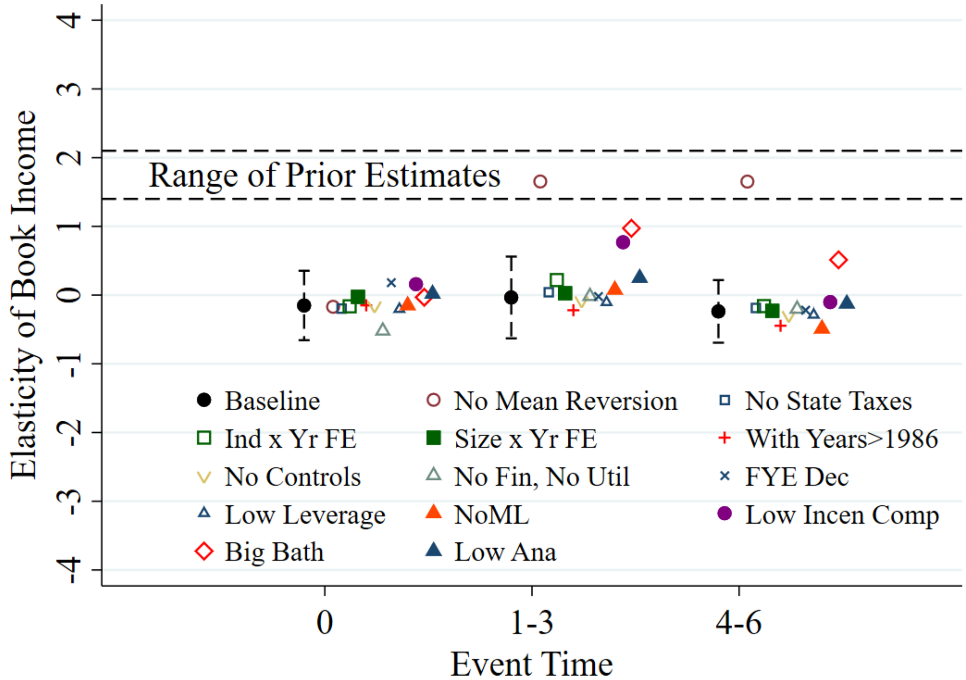


Figure 5: Placebo-in-Time Book Tax Differences Elasticity Estimates

Notes: This figure plots tax avoidance responses to AMTBIA87, scaling estimates of β_e from equation (3) and η_e from equation (4) into elasticities of book income with respect to the net of tax rate following equation (8). Confidence intervals are constructed from nonparametrically bootstrapped standard errors using 300 iterations and clustering at the firm level. The dashed horizontal lines represent the range of point estimates of firm responses to AMTBIA87 from previous studies aggregated in Dharmapala (2020).

IV.C Explaining the Lack of Avoidance Responses

Why might firms not exhibit large book tax difference responses to AMTBIA87? In this section, I rule out concerns that the tax increase was not salient, that firms could reduce their tax liabilities with foreign tax credits and net operating losses, that firms did not care about AMTBIA87 liabilities because they generated AMT credits, that firms with early fiscal-year ends did not fully face AMTBIA87 in 1987, and that financing constraints push firms to report high earnings even in the presence of the tax.

To confirm the salience of the tax increase, I plot placebo-in-time estimates of firm tax liabilities in Appendix Figure D.8 and mean reversion corrected estimates of firm tax liabilities in Appendix Figure D.9. Panel (a) in both figures plots estimates using the whole sample. While individual yearly estimates do not reject zero, they suggest that firms facing AMTBIA87 saw their tax liabilities increase modestly by an average of 0.29% of lagged assets over 1987-1989.

Placebo-in-time tax liability estimates and avoidance elasticity estimates excluding multinational and loss firms confirm that tax liability increases were larger for firms that could not reduce their AMTBIA87 tax liability with foreign tax credits and net operating loss deductions (Boynton, Dobbins and Plesko, 1992), and that these firms still did not avoid AMTBIA87. Panel (b) in Appendix Figures D.8 and D.9 plots tax liability estimates dropping multinationals (firms with non-missing pretax foreign income or foreign tax expense at any event time before zero) and loss firms (firms with positive tax loss carryforwards at event time zero). Among non-multinational, non-loss firms, tax liability rose by an average of 0.67% of lagged assets over 1987-1989. Furthermore, the "NoML" series in Figure 5 plots elasticity estimates excluding multinational and loss firms. These elasticity estimates are very close to the baseline estimates. This evidence implies that I do not find null elasticities only because firms are reducing AMTBIA87 liability with foreign tax credits and losses, that subsidiary aggregation differences between book and tax systems are not driving my estimates, and that even among firms facing larger tax liability increases there is no avoidance response to AMTBIA87.

To confirm that the absence of tax avoidance cannot be explained by AMTBIA87 liability generating AMT credits that reduce future liabilities, I exploit the fact that firms receive AMT credits for taxes paid on temporary BTDs but not permanent BTDs. If the lack of avoidance was driven by AMT credits, we would expect firms to shrink permanent BTDs that do not generate AMT credits to avoid the tax. However, Appendix Figure D.10 plots placebo-in-time estimates using permanent BTDs and finds permanent BTDs that do not generate AMT credits appear to increase by 0.53% of lagged assets over 1987-1989.

The income firms with early fiscal-year ends reported in 1987 may not have been fully subject to AMTBIA87 because some of it was accrued during the 1986 tax year. To confirm these timing concerns do not drive the lack of avoidance responses, the "FYE Dec" series in Figure 5 plots elasticity estimates restricting to firms with fiscal years ends in December, after the announcement of TRA86 and AMTBIA87. These estimates are very similar to the baseline results, appearing to rule out that the lack of avoidance stems from firms not facing the tax.[21]

Firms with high leverage may face financial constraints and therefore face particularly strong incentives to keep reported earnings high to lower the price of external finance or avoid triggering debt covenant violations (Baker, Stein and Wurgler, 2003; Defond and Jiambalvo, 1994). The low leverage series in Figure 5 plots elasticity estimates restricting to firms with below median leverage. The estimates are very close to the baseline estimates. While leverage is only a rough proxy for financing constraints or potential debt covenant violations, this evidence suggests that financing constraints do not explain the null elasticity estimates.

To rationalize the lack of avoidance responses to AMTBIA87, I specify a static, partial equilibrium model of firm tax evasion and earnings manipulation decisions in the presence of corporate taxes, in which firms face incentives to maximize after-tax profits and stock prices.[22] Firms choose output y with convex costs $c(y)$. Some fraction of firm costs μ_t are deductible for tax purposes so that taxable income is $y - \mu_t c(y)$, and some fraction μ_b are deductible for book purposes so book income is $y - \mu_b c(y)$. Firms can evade or avoid taxes by misreporting tax costs $\hat{c}_t \neq c(y)$ at a convex cost of misreporting $g(\hat{c}_t - c(y))$, and can manipulate book income by misreporting book costs $\hat{c}_b \neq c(y)$ at a convex cost of misreporting $h(\hat{c}_b - c(y))$. Firms seek to maximize after-tax profits subject to misreporting costs while keeping stock prices high. Firm earnings manipulation impacts stock prices via $s(\hat{c}_b - c(y))$ where I assume $s'() < 0$ so that firms want to manipulate their earnings upwards to keep stock prices high. Firms pay taxes $T(y, \hat{c}_t, \hat{c}_b)$ that can depend on reported taxable or book income.

[21] Previous studies have found BTD increases in 1986 suggestive of firms shifting BTDs from 1987 into 1986, a result that may also be biased by including firms with different fiscal-year ends. However, I find no increase in BTD in 1986 when restricting to firms with fiscal year-ends in December. If I define treatment and control groups based only on 1986 ETRs I observe a spike in BTDs in 1986, but this spike stems from the treatment definition and is comparable to spikes from placebo treatment definitions based on earlier single years.

[22] For models that consider dynamic earnings misreporting incentives, see Shackleford, Slemrod and Sallee (2011), Terry, Whited and Zakolyukina (2021) who focus on earnings manipulation impacting firm user cost of capital, and Zakolyukina (2018) who focuses on understanding how frequently firms misreport their earnings.

Table 2: Firm Incentives Under Different Tax Systems

FOC	(1) Book Income	(2) Taxable Income
$c'(y)$	$1-\tau_{E,b}$	$1-\tau_{E,t}$
$g'(\hat{c}_t - c(y))$	0	$\tau_t \mu_t$
$h'(\hat{c}_b - c(y))$	$s'(\hat{c}_b - c_b(y)) + \tau_b \mu_b$	$s'(\hat{c}_b - c_b(y))$

Notes: This table presents first order conditions of the firm problem in equation (9). Column 1 uses the book income tax function. Column 2 uses the taxable income tax function. $\tau_{E,b} \equiv \tau_t \frac{1-\mu_b}{1-\tau_b \mu_b}$, and $\tau_{E,t} \equiv \tau_t \frac{1-\mu_t}{1-\tau_t \mu_t}$.

The firm solves

$$\max_{y,\hat{c}_t,\hat{c}_b} \; y - c(y) - T(y, \hat{c}_t, \hat{c}_b) - g(\hat{c}_t - c(y)) - h(\hat{c}_b - c(y)) + s(\hat{c}_b - c(y)). \tag{9}$$

I consider two different tax functions,

$$\text{Tax on Taxable Income: } T(y, \hat{c}_t, \hat{c}_b) = \tau_t(y - \mu_t \hat{c}_t)$$

$$\text{Tax on Book Income: } T(y, \hat{c}_t, \hat{c}_b) = \tau_b(y - \mu_b \hat{c}_b)$$

The first order conditions, which I display in Table 2, determine the level of output, tax evasion and earnings manipulation at the firm optimum. Columns 1 and 2 display how the first order conditions vary with the chosen tax function. Firms choose output to set marginal costs $c'(y)$ equal to $1 - \tau_E \equiv 1 - \tau\frac{1-\mu}{1-\tau\mu}$, the effective net of tax rate that varies with the statutory tax rate and base. A pure profit tax with $\mu = 1$ is therefore production efficient, while tax systems with larger effective tax rates result in production inefficiency. When moving from a tax on taxable income to a tax on book income, firm's marginal benefit of reporting lower book costs changes from $s'()$ to $s'() + \tau_b \mu_b$, as stock benefits from reporting higher earnings are offset by additional taxes.

This stylized model motivates quasi-experimental setups that search for evidence of downward earnings manipulation in response to AMTBIA87 by comparing firms facing AMTBIA87 whose marginal cost of over-reporting earnings increases to firms that do not. However, the magnitude of this earnings manipulation response depends on the relative magnitudes of the stock benefit and tax incentives, as well as the shape of the cost misreporting and stock benefit functions. Figure 6 plots an example assuming $h()$ is quadratic and $s()$ is linear to clarify this intuition. Moving from a tax on taxable income to a tax on book income shifts the marginal benefit function from $s'(\hat{c}_b - c(y))$ to $s'(\hat{c}_b - c(y)) + \tau_b\mu_b$, moving optimal earnings misreporting from $\hat{c}_b^* - c(y^*)$ to $\hat{c}_b' - c(y')$. However, the magnitude of this shift will be small if the magnitude of the tax incentive to report lower book income introduced by a tax on book income is small relative to stock incentives to report high book income.

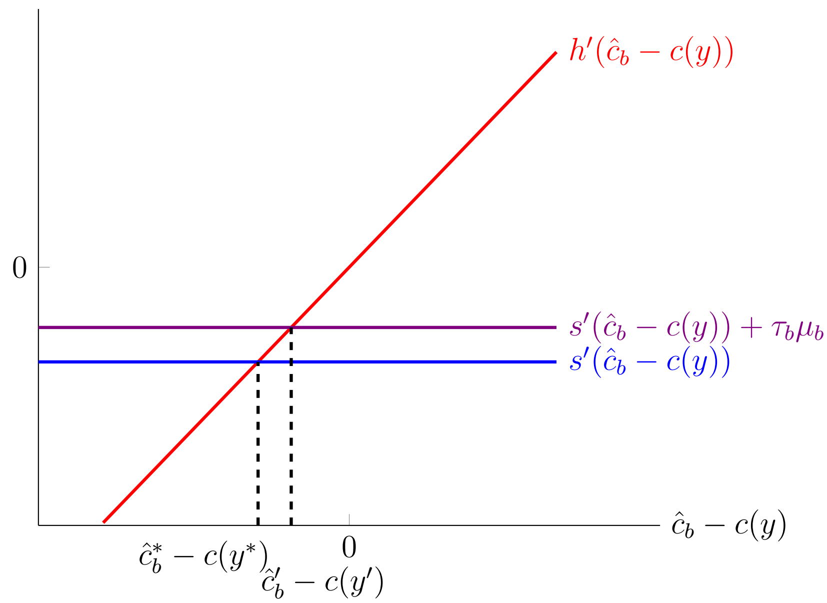


Figure 6: Marginal Firm Decisions

Notes: This figure plots book misreporting cost and stock benefit functions from equation (9) under a tax on taxable income and a tax on book income assuming $h()$ is quadratic and $s()$ is linear. The shift in firm book cost misreporting at the optimum when transitioning from a tax on taxable income to a tax on book income is denoted by $\hat{c}_b^* - c(y^*) - (\hat{c}_b' - c(y'))$, and is determined by the slope of the misreporting cost function $h()$, and the relative strengths of the stock-based incentive to report higher book income $s()$ and the tax incentive to report lower book income $\tau_b\mu_b$.

A large literature in finance suggests that firm incentives to report high book incomes are very strong. Graham, Harvey and Rajgopal (2005) survey firm managers and find they fixate on reporting increasing earnings, positive earnings, and earnings that beat analyst targets. Empirical research documents bunching in the firm earnings distribution at these cutoffs (Burgstahler and Dichev, 1997; Terry, 2017). In addition, Erickson, Hanlon and Maydew (2004) find that firms appear willing to pay extra taxes in order to justify reporting fraudulently high earnings.[23]

However, firm incentives to report high book incomes are not universal across firms, suggesting that we should expect to observe larger avoidance responses to AMTBIA87 among firms with weaker incentives to report high earnings. I focus on three types of firms with weaker incentives to report high earnings: firms with less incentive-based compensation (Bergstresser and Philippon, 2006), firms missing earnings targets by large margins, and firms covered by fewer analysts (Yu, 2008).

To focus on firms with less incentive-based compensation, the "Low Incen Comp" series in Figure 5 plots elasticity estimates restricting to firms present in Execucomp, but excluding firms with managers whose compensation is more than 20% incentive-based in 1992 in an attempt to eliminate firms where managers face the strongest incentives to report high earnings. The elasticity point estimate over 1987-1989 for the low incentive-based compensation series is 0.77, suggesting that firms with managers lacking incentives to keep earnings high managed their earnings downwards to avoid AMTBIA87. These results are robust to a variety of cutoff fractions used to determine which firms have low incentive-based compensation, and regardless of whether I measure incentive-based compensation in 1992 or averaging over 1992-1994. I plot elasticity point estimates varying the cutoff fraction and measure of incentive-based compensation in Appendix Figure D.11, panel (c). The lower estimates spanning 1990-1992 may suggest that ACE was not a close analogue to book income.

Firms taking "Big Baths" that are missing earnings benchmarks by large margins also may face weaker incentives to report high book incomes. To focus on these firms, the "Big Bath" series in Figure 5 plots elasticity estimates restricting to firms where the difference between 1987 and 1986 book incomes is less than -0.5% of assets. The elasticity point estimate over 1987-1989 for the big bath series is 0.97, providing suggestive evidence that firms taking big baths shrunk their BTDs to avoid AMTBIA87.

Finally, to focus on firms that are covered by fewer analysts, the "Low Ana" series in Figure 5 plots elasticity estimates restricting to firms with fewer than an average of 3 analysts

[23] One concern with taxes on book income is that attempts to avoid the tax will distort the information content of earnings with negative consequences for investors. The model makes clear that even if firms do distort their earnings to avoid a tax on book income, this distortion is likely to improve the accuracy of earnings by offsetting many stock-based incentives firms have to report high earnings.

covering them over 1981-1992. This series provides little support for analyst coverage playing a role in firm responses to AMTBIA87, as the elasticity estimates are close to zero.

Unfortunately, these subsample analyses lack power and the point estimates cannot reject zero. Mechanically, these tests use fewer observations by restricting to only a fraction of the sample. In addition, Execucomp data is only available for 56% of the sample, further limiting the power of the incentive-based compensation test.[24] I plot elasticity estimates with confidence intervals for the baseline series, Execucomp sample, the low incentive-based compensation series, the big bath series, the low analyst coverage series, and the non-multinational non-loss series in Appendix Figure D.11, panel (a). Despite the limited power, these tests provide suggestive evidence that firms with less incentive to report high earnings had larger avoidance responses to AMTBIA87.

IV.D Earnings Management Responses

The book tax difference results above are important because they provide a summary measure of tax avoidance responses to AMTBIA87. However, using book tax differences as an outcome introduces mean reversion because BTDs are mechanically related to effective tax rates, the outcome may be biased by the construction of taxable income in the data, and BTDs capture both accounting and tax responses to AMTBIA87.[25] While the placebo-in-time approach is designed to overcome mean reversion issues, it does not overcome these other challenges.

To avoid mean reversion issues, measurement error in taxable income, and to focus specifically on the accounting manipulation aspect of firm's potential avoidance behavior, I study how discretionary accruals (DAs), a common proxy for earnings management, change in response to AMTBIA87. As shown in Figure 2, DAs are not mechanically related to ETRs and therefore should not exhibit mean reversion, suggesting I can use the basic event study

[24]Furthermore, the Execucomp data does not begin until 1992. However, Hall and Liebman (1998) show that levels of incentive-based compensation rose rapidly during the 1980s and 1990s so it seems reasonable to assume firms with low incentive-based compensation in 1992 also had low incentive-based compensation in previous years.

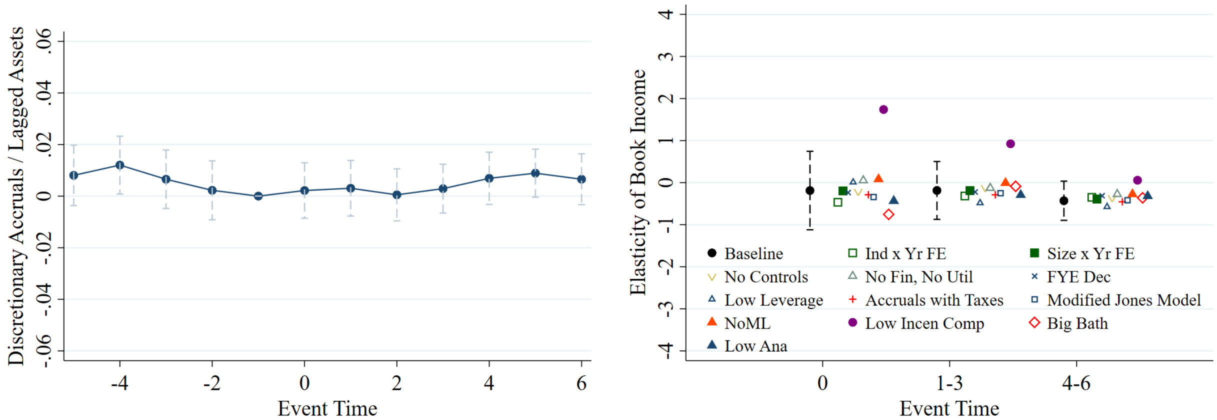
[25]Another potential concern with using BTDs as an outcome is that TRA86 cut the corporate tax rate, providing an incentive to shift taxable income from 1986 into 1987, which would reduce BTDs. However, this would only bias estimates in my event study framework if there is differential shifting of taxable income between treatment and control groups, something I observe no evidence of in Appendix Figure D.8.

framework in equation (3) to estimate DA responses to AMTBIA87. Appendix Figure D.12 plots baseline estimates of equation (3) alongside stacked pre-period placebo estimates of equation (4) using DAs as an outcome and confirms this intuition. When placebo treatments are defined using years before 1984-1986, DA responses to placebo treatments for treatment relative to control firms cannot reject zero at every event time.

I plot estimates of β_e from equation (3) in Figure 7, panel (a). In panel (b), I rescale earnings management estimates into elasticities of book income with respect to the net of tax rate using equation (8) without the mean reversion correction. The baseline elasticity estimate over 1987-1989 is -0.19 with a 95% confidence interval from -0.87 to 0.5. These estimates reject downwards earnings management of more than -0.58% of lagged assets, or -0.06% of lagged assets per 1% change in the tax rate. The standard deviation of DA in the entire sample is 6% of lagged assets. Therefore, these estimates reject downwards earnings management with enough precision to rule out earnings manipulation responses to AMTBIA87 that are an order of magnitude smaller than the standard deviation of DAs in the data.

The baseline null elasticity estimates in Figure 7, panel (b) do not change significantly under a similar set of robustness tests as those in Figure 5. Adding asset tercile or industry time trends, excluding controls, and excluding financial and utility firms does not change these estimates. In addition, the null result holds under different measurement of DAs. The "Accruals with Taxes" estimates use a DA construction that includes changes in taxes paid in the measurement of total accruals, more closely resembling the definition used in Boynton, Dobbins and Plesko (1992). The "Modified Jones Model" estimates use a DA construction that focuses on changes in credit sales as discussed in Appendix A and outlined in Dechow, Sloan and Sweeney (1995). Furthermore, Appendix Figure D.6, panels (c) and (d) display DA responses scaled by pre-period standard deviations of the outcome and cannot reject the null of zero response across asset terciles or the manufacturing, trade, transportation and utilities industries.

Revisiting the hypotheses from above that some firms may face stronger incentives to manage earnings under AMTBIA87, I also estimate elasticities in Figure 7, panel (b) excluding high leverage firms, firms with fiscal year ends before December, multinationals and



(a) Event Study

(b) Elasticity Estimates

Figure 7: Discretionary Accrual Responses

Notes: This figure plots discretionary accrual responses to AMTBIA87. Panel (a) plots estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs. Panel (b) scales the β_e estimates in panel (a) into an elasticity of book income with respect to the net of tax rate following equation (8) without the mean reversion correction $\varepsilon_e^{BI} = (\frac{\beta_e}{BI_\beta}) \cdot \frac{1-\tau}{\Delta(1-\tau)_e}$. Confidence intervals are calculated from standard errors clustered at the firm level.

firms with losses, firms with high incentive-based compensation, firms without large earnings declines, and firms covered by many analysts. I find little evidence of more downwards earnings management when dropping firms with early fiscal year ends, multinationals and loss firms, high leverage firms, firms not taking big baths, or firms covered by many analysts, but do find some suggestive evidence that firms with low incentive-based compensation manage their earnings downwards. While the low incentive-based compensation estimates have the largest positive magnitudes, none of the estimates can reject zero.

IV.E Responses to ACEA90

Do firms respond to the transition from AMTBIA87 to ACEA90? All of the above analysis suggests that the answer is no. I observe no differential book tax difference or discretionary accrual decline for firms more likely to face the minimum tax in 1990 when the minimum tax base changes and the minimum tax rate increases to 15%. However, all of this analysis uses treatment and control splits from 1984-1986. To complement these results, I also use a balanced panel spanning 1984-1995 and split firms into treatment and control groups using an average effective tax rate cutoff of 17% over the years 1987-1989, directly before the transition to ACEA90.

Appendix Figure D.13, panel (a) repeats the exercise in Figure 4, panel (a) and plots estimates of β_e from equation 3, using treatment definitions over different years close to the implementation of ACEA90, where the baseline specification now uses effective tax rates from 1987-1989. Book tax difference responses to this treatment definition in a number of years close to the implementation of ACEA90 are stable, suggesting there are no book tax difference responses to the transition from AMTBIA87 to ACEA90 and the observed declines are due to mean reversion.

Appendix Figure D.13, panel (b) repeats the exercise in Figure 7, panel (a) and plots estimates of β_e from equation 3, using a treatment definition over 1987-1989 with an average effective tax rate cutoff of 17%. Estimates of discretionary accrual responses to the transition to ACEA90 cannot reject zero in any post-transition year, again suggesting there was no firm response to the transition to ACEA90.

These additional estimates also alleviate concerns that the firm responses I estimate to AMTBIA87 are driven by differential effects of economic conditions at a specific point in time for treatment relative to control firms. Even three years later and using a different effective tax rate cutoff in the treatment definition I confirm the null avoidance results I estimate to AMTBIA87.

V Production and Investment Responses

AMTBIA87 may impact firm's production and investment behavior by broadening the tax base and curbing deductions meant to incentivize these behaviors. To test whether firms exhibit real production and investment responses to AMTBIA87, I estimate firm sales, costs of goods sold, investment, debt and employment responses to AMTBIA87 using the basic event study framework in equation (3), utilizing the 1981-1992 balanced panel but excluding finance and utility firms to avoid balance sheet differences and conflicting incentives from rate of return regulations. None of the estimates can reject the null of zero in any of the post-1986 years across all five outcomes, suggesting that firms did not exhibit significant production and investment responses to AMTBIA87. The sales and COGS estimates in panel (a) suggest that firms did not modify their production in response to AMTBIA87 because there are no clear changes in firm revenues or costs of inputs.

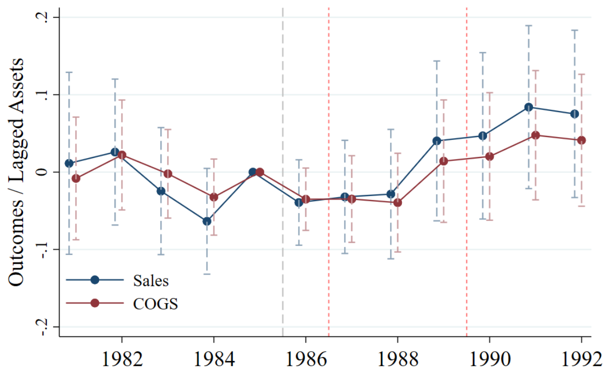
Firms also do not appear to make economically meaningful changes to their investment, debt, or employment in response to AMTBIA87. In panel (b), I reject decreases in investment in 1989 of more than −0.48% of lagged assets for every 1% change in the tax rate. In panel (c), I estimate debt responses in 1989 of −0.02% of lagged assets with a 95% confidence interval from −0.24 to 0.20. In panel (d) I estimate log employment responses in 1989 of −0.01 with a 95% confidence from −0.11 to 0.08. These confidence intervals rule out other estimates of firm responses to tax policy changes in the literature by a wide margin. For example, Ohrn (2018) estimates that firms decrease debt by 5.3% of total assets and increase investment by 4.7% for every 1% reduction in the tax rate due to the Domestic Production Activities Deduction, while Zwick and Mahon (2017) estimate that investment increases 2.89% for every 1% decrease in the net of tax rate due to bonus depreciation changes.

To complement the event study production and investment responses presented in Figure 8, I also estimate the impact of expected total tax liability on outcomes, using expected AMTBIA87 liability as an instrument for total tax liability and estimating

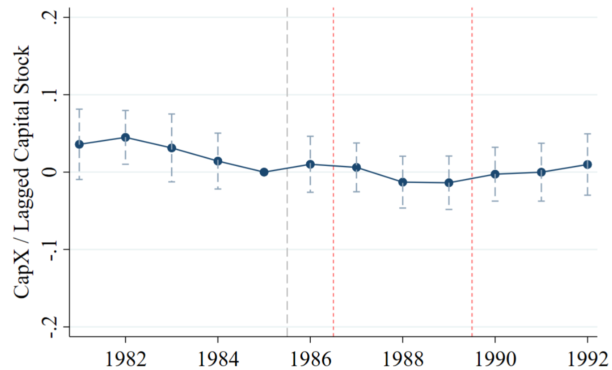
$$Y_{it} = \phi TaxLiab_i + \delta_t + \gamma_s + \varepsilon_{it}, \tag{10}$$

where $TaxLiab_i$ is a firm's expected tax liability based on 1987 policy and 1986 status, Y_{it} are outcomes, δ_t are year fixed effects, and γ_s are industry (SIC2) fixed effects. I instrument for $TaxLiab_i$ with $AMTBIA_i$, a firm's expected AMTBIA87 liability based on 1986 status calculated as 10% of BTD if the firm is in the treatment group and zero otherwise. I estimate this regression using all treatment and control firms in the 1981-1992 balanced panels, restricting to years 1987-1992 and excluding the finance and utilities industries.

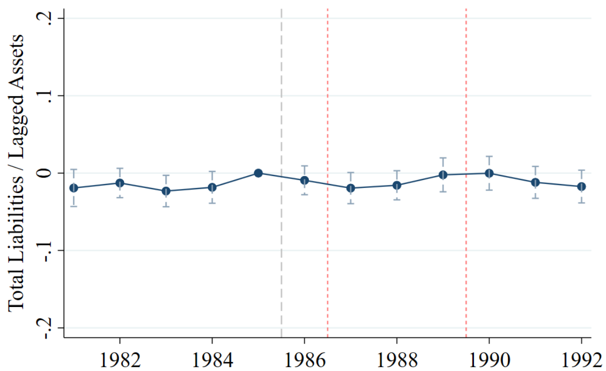
The two stage least squares estimates identify the causal effect of additional tax liability on outcomes under the assumption that expected AMTBIA87 liability impacts outcomes only through changes in tax liability. The instrument is relevant because expected AMT-BIA87 liability is mechanically related to expected total tax liability, and unlikely to violate



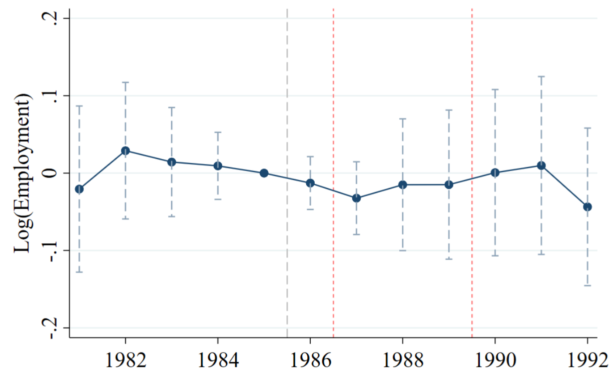
(a) Sales and COGS



(b) Investment



(c) Debt



(d) Employment

Figure 8: Real Outcome Responses

Notes: This figure plots real outcome responses to AMTBIA87. Each panel plots estimates of β_e from equation (3) estimated on the 1981-1992 balanced panel, excluding finance and utility firms (SIC codes 4000-4899 and 4900-4999). 95% confidence intervals are constructed from standard errors clustered at the firm level. Panel (a) uses sales and costs of goods sold as outcomes. Panel (b) uses investment as an outcome, defined as capital expenditures per dollar of lagged net PPE. Panel (c) uses debt as an outcome, defined as total liabilities per dollar of lagged assets. Panel (d) uses log employment as an outcome. Full variable definitions are given in section III.

exclusion unless firms respond to AMTBIA87 for reasons unrelated to tax liability changes.[26]

Estimates of the predicted tax liability coefficient ϕ are particularly useful because they can be interpreted as the impact of tax liability on outcomes, but are identified using only variation in expected AMTBIA87 liability, abstracting from other TRA86 changes. In addi-

[26]For example, firms might face higher administrative burdens due to AMTBIA87 with significant costs that crowd out investment. However, this seems unlikely because firms already were required to calculate book and taxable income for their taxes and financial statements before TRA86.

tion, constructing the instrument from BTDs eliminates concerns that event study controls for tax preferences and adjustments do not rid my estimates of bias from mismeasuring the tax base if tax base error is independent across firms.

Table 3: Production and Investment Instrumental Variable Estimates

Coefficient	(1) Sales	(2) COGS	(3) Investment	(4) Debt	(5) Employment
Predicted Liability Effect	−0.001	−0.001	−0.000	0.000	0.012
	(0.001)	(0.001)	(0.000)	(0.000)	(0.006)
Observations	3636	3636	3614	3636	3523
Clusters	606	606	603	606	598
F Stat	3.97	3.97	3.79	3.97	3.85
LM Stat	2.83	2.83	2.76	2.83	2.80

Notes: This table reports instrumental variable regression coefficients from equation (10) across five outcome variables: sales, costs of goods sold, investment, debt and employment. The predicted liability effect is the ϕ coefficient on predicted tax liability in equation (10), while the first stage coefficient is the coefficient on the expected AMTBIA87 liability instrument in the first stage. Standard errors are in parentheses and clustered at the firm level. The sample includes all firm-years from 1987-1992 not in the finance and utilities industries.

Table 3 displays instrumental variable regression results using sales, COGS and debt scaled by lagged assets, investment scaled by lagged capital stock, and log employment as outcomes. None of the predicted liability effect coefficients reject the null hypothesis of zero except for employment. The estimates in column 1 suggest that for every $1 million increase in expected AMT tax liability, sales decrease by 0.1% of lagged assets.

The null production and investment responses to AMTBIA87 that I estimate are consistent with the predictions of the model presented above, where firm output decisions are determined by the effective tax rate (see Table 2). AMTBIA87 applies a low rate to a broader base, leading to a small change in the effective tax rate and small, if any, changes in output.

VI Revenue Scores

To understand the implications of firm tax avoidance responses to AMTBIA87 for contemporary policy, I develop a revenue score of a recent Biden administration proposal to implement a book income AMT (Li, Watson and Lajoie, 2020).[27] The proposed Biden book income AMT would institute a 15% minimum tax on book income. The minimum tax would only apply to firms with at least $100 million in annual income. In addition, firms calculating minimum tax liability would still be allowed to claim deductions for loss carryforwards and foreign taxes.[28] To score the proposed Biden book income AMT, I simulate the evolution of firm book incomes over a ten-year period in a 2018 cross section of Compustat firms, incorporating possible firm tax avoidance responses to the policy and applying the proposed book income AMT to the simulated data to estimate revenue. I explain the details of my scoring methodology in Appendix D.

This scoring methodology yields a range of estimates that depend on chosen values of the book income elasticity. I construct four scenarios that vary elasticity assumptions to explore how these assumptions impact revenue scores. Scenario 1, in line with the estimates in section IV, assumes zero responses to a book minimum tax. Scenario 2 makes moderate elasticity assumptions close to the upper edge of the confidence intervals of the elasticity estimates in section IV. Scenarios 3 and 4 make higher elasticity assumptions, where the assumptions in scenario 3 are in line with previous estimates of book income elasticities and the assumed elasticities in scenario 4 are even larger.

I summarize the revenue raised by the proposed book income AMT in each simulation scenario in Table 4, panel A. Column 1 displays aggregate revenue scores from each scenario. Column 2 displays the revenue raised by the firms facing the ten largest tax liability increases in each simulation. Columns 3-6 display the revenue raised from firms in the utilities, manufacturing, finance and insurance, and transportation and warehousing sectors respectively. In my preferred Scenario 1, the proposed book income AMT raises $337 billion over a decade. This is simply a mechanical tax calculation. In the more conservative scenario 2, avoidance responses to the proposed Biden book income AMT reduce revenue by 12%. Scenario 3 shows that assuming elasticities in line with previous estimates in the literature reduces

[27] I do not develop a revenue score for the corporate minimum tax included in the Inflation Reduction Act of 2022 because it allows firms to deduct bonus depreciation from the tax base which I do not observe on financial statements. My revenue score suggests narrowing the tax base may allow firms to avoid the AMT in the same way they avoid paying taxes under the standard corporate tax system.

[28] Historically, when firms have paid an AMT, they have also generated AMT credits which could be used against normal tax liability in future years. I assume the proposal would include AMT credits, and that 30% of AMT revenue is returned to firms via credits.

estimated revenue by 18%.

Table 4: 10 Year Revenue Scores of the Biden Book Income AMT

Panel A: Baseline Scenarios	(1) Revenue	(2) Top 10	(3) Util	(4) Manf	(5) Fin	(6) Tran
S1: $\varepsilon_t = \{0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0\}$	337	86	82	77	45	37
S2: $\varepsilon_t = \{0.0, 0.5, 0.5, 1.0, 1.0, 1.0, 1.0, 1.0, 1.0, 1.0\}$	296	78	73	66	40	32
S3: $\varepsilon_t = \{0.5, 0.5, 1.0, 1.0, 1.0, 1.5, 1.5, 1.5, 2.0, 2.0\}$	275	73	69	61	38	30
S4: $\varepsilon_t = \{1.0, 2.0, 3.5, 4.0, 4.5, 5.0, 5.0, 5.0, 5.0, 5.0\}$	169	52	43	32	29	17
Panel B: No FTC Scenarios	Revenue	Top 10	Util	Manf	Fin	Tran
S1: $\varepsilon_t = \{0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0, 0.0\}$	416	93	83	87	82	39
S2: $\varepsilon_t = \{0.0, 0.5, 0.5, 1.0, 1.0, 1.0, 1.0, 1.0, 1.0, 1.0\}$	362	85	74	74	71	34
S3: $\varepsilon_t = \{0.5, 0.5, 1.0, 1.0, 1.0, 1.5, 1.5, 1.5, 2.0, 2.0\}$	334	80	69	67	65	32
S4: $\varepsilon_t = \{1.0, 2.0, 3.5, 4.0, 4.5, 5.0, 5.0, 5.0, 5.0, 5.0\}$	197	58	43	34	41	18

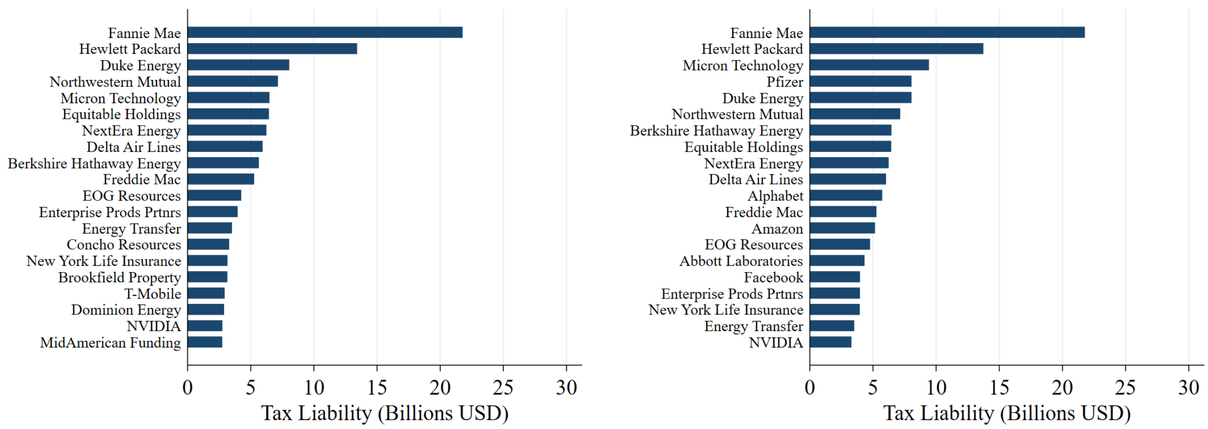
Notes: This table reports ten year revenue scores of the proposed Biden book income AMT across a range of assumptions for ε_t, the elasticity of book income with respect to the net of tax rate at time horizon t. Panel (a) displays revenue estimates for the basic policy described in the text, assuming 30% of AMT liability is recovered via AMT credits. Panel (b) displays revenue estimates for the same policy except that firms are not able to use foreign tax credits to reduce their AMT liabilities. Column 1 displays the total revenue estimate. Column 2 displays the revenue raised from the ten firms contributing the most revenue. Columns 3-6 display the total revenue coming from the four most affected industries across simulations, Utilities (NAICS2=22), Manufacturing (NAICS2=31-33), Finance and Insurance (NAICS2=52) and Transportation and Warehousing (NAICS2=48-49) respectively. Revenue scores are in billions of USD.

Column 2 of Table 4 shows that across scenarios, between 26% to 31% of the revenue raised by the proposed book income AMT comes from the firms with the ten largest tax liability increases due to the policy.[29] Columns 3-6 of Table 4 show that, across revenue simulations, most of the revenue raised by the proposed Biden book income AMT would come from the utilities, manufacturing, finance and insurance and transportation sectors.

Figure 9, panel (a) identifies which firms face the largest tax liability increases from the proposed book income AMT by plotting the tax revenue raised in my preferred simulation from the twenty firms facing the largest changes. The firms facing the very largest tax liability increases include Hewlett Packard, Fannie Mae, Berkshire Hathaway Energy and Delta Airlines.[30] One firm noticeably absent from the top twenty is Amazon.

[29]This share increases in the scenarios incorporating larger avoidance responses to the policy.

[30]Fannie May and Freddie Mac are government sponsored enterprises (GSEs). While GSEs are exempt from state and local taxes, they are not exempt from federal taxes.



(a) Biden Book Income AMT

(b) Without Foreign Tax Credits

Figure 9: 20 Largest Tax Liability Increases Over a Decade

Notes: This figure plots the tax liability of the firms facing the 20 largest increases in tax liability as a result of the proposed book income AMT in my preferred simulation, Scenario 1. Panel (a) displays tax liability increases for the baseline proposed policy. Panel (b) displays tax liability increases for a modified policy that does not allow firms to use foreign tax credits to reduce minimum tax liability.

Amazon faces a $1.7 billion increase in tax liability in my preferred ten-year revenue score, the 42$^{\text{nd}}$ largest among firms in the sample. Reassuringly, in my simulations the book income AMT does appear to accomplish its stated aim to increase the tax liabilities of profitable firms like Amazon that pay very few taxes. However, the book income AMT captures significantly more revenue from a number of other firms who are either more profitable, pay less taxes, or both. Therefore, while criticism that Amazon is highly profitable but pays few taxes is accurate, that criticism can also be levied at many other firms, some of whom are even more extreme examples of diverging profitability and tax liability.[31]

Amazon's tax liability under the proposed book income AMT is mitigated by substantial tax loss carryforwards and foreign tax credits the firm has accumulated. Generally, allowing deductions to substantially narrow a book income AMT base may allow firms to avoid the AMT in the same way they avoid paying taxes under the standard corporate tax system. To explore the type of AMT that would preserve a wider base, I also run revenue simulations for a modified version of the proposed book income AMT that does not allow firms to reduce

[31] Amazon's book income is not changed by avoidance assumptions I make in revenue simulations because Amazon would not pay the book income AMT based on their 2018 financial statements. If I applied avoidance estimates to the book income of Amazon, the firm would contribute even less revenue.

their minimum tax liability with foreign tax credits.[32] Figure 9, panel (b) plots the twenty largest tax liability increases in response to this modified book income AMT, using my preferred elasticity estimates from Scenario 1. After excluding FTCs, Amazon faces a $5.2 billion increase in tax liability, the 13th largest among all firms. Table 4, panel B displays aggregate revenue estimates for the simulation without foreign tax credits. This policy would raise $416 billion over a decade with similar levels of revenue concentration among the ten largest contributors and across industries as the proposed policy with a narrower base.

VII Conclusion

In this paper, I estimate firm responses to an AMT on book income. Within an event study framework, placebo-in-time estimates suggest that firms do not manipulate their earnings to avoid AMTBIA87. My avoidance estimates diverge from previous estimates of book income elasticities because I account for mean reversion, not because of different sample construction, variable construction, or controls. I develop a static partial equilibrium model that rationalizes the lack of avoidance responses to AMTBIA87 by showing avoidance depends on the relative magnitudes of tax and non-tax incentives to report high book incomes. Therefore, firms facing weaker incentives to report high earnings should exhibit larger avoidance responses to AMTBIA87. Empirical tests of avoidance among firms with lower incentive-based compensation and firms missing earnings benchmarks provide suggestive evidence that supports this core model prediction. I also find little evidence that AMTBIA87 distorts firm production or investment decisions.

The purpose of AMTs is to bolster public perceptions of tax code fairness by ensuring all firms with substantial income pay taxes. To evaluate the implications of the tax avoidance I estimate in response to AMTBIA87 for contemporary policy, I develop revenue scores of a proposed book income AMT. In my preferred simulation, the book income AMT would raise $337 billion in revenue over a decade. These revenue scores suggest that a book income AMT would raise substantial revenue from firms with high income and low tax liability, but

[32]This policy would impose double taxation on earnings of foreign subsidiaries if implemented in conjunction with a country-by-country minimum tax, but in the absence of a country-by-country minimum tax would serve as a reasonable backstop in an attempt to capture additional tax revenue from profitable firms.

that firms would still have some scope to escape larger tax payments because of deductions and credits allowed in the book income tax base.

The results in this paper suggest that taxes on book income may be non-distortionary and raise substantial revenue because firms face non-tax incentives to report high book incomes. However, it is also important to consider how firm responses to book income AMTs could depend on the regulatory environment (Terry, Whited and Zakolyukina, 2021) and the salience of book income versus non-GAAP income measures for investors, firms and managers. Furthermore, a tax on book income could lead to a politicization of the accounting standards setting process (Shaviro, 2020), which could allow special interests to limit the breadth of a book income tax base and continue to allow firms to report high incomes while paying few taxes. Policymakers should carefully consider these factors in addition to the evidence in this paper as they write regulations to implement the book income AMT in the Inflation Reduction Act of 2022 and consider future reforms.

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A Constructing Discretionary Accruals

Following Dechow, Sloan and Sweeney (1995), I measure total accruals as the change in current assets less change in current liabilities less change in cash plus change in long term debt in current liabilities minus depreciation, all scaled by lagged assets. Total accruals are income for which cash has not yet been exchanged. I then model total accruals as a function of economic conditions (Jones, 1991),

$$\frac{TA_{i,t}}{A_{it-1}} = \sum_{j=1}^{J} \beta_{1j} \frac{1}{A_{it-1}} + \beta_{2j} \frac{\Delta Sales_{it}}{A_{it-1}} + \beta_{3j} \frac{GPPE_{it}}{A_{it-1}} + \psi_j + \varepsilon_{it}, \tag{A.1}$$

where TA_{it} are total accruals and $GPPE_{it}$ is gross property plants and equipment for firm i in 2 digit SIC industry j in year t. I estimate (A.1) using data from 1981-1985 in the period before which there should be any earnings management from AMTBIA87, then predict non-discretionary accruals $\widehat{\frac{NDA_{i,t}}{A_{it-1}}}$ using the regression coefficients over the whole 1981-1992 sample. Discretionary accruals are measured as $\frac{TA_{i,t}}{A_{it-1}} - \widehat{\frac{NDA_{i,t}}{A_{it-1}}}$.

I also explore two alternative measures of discretionary accruals. First, I add changes in taxes payable to the measure of total accruals to more closely match the definition used in Boynton, Dobbins and Plesko (1992). Second, I use a "modified Jones model" as in Dechow, Sloan and Sweeney (1995); Bergstresser and Philippon (2006); Yu (2008), running the regression $\frac{TA_{i,t}}{A_{it-1}} = \beta_1 \frac{1}{A_{it-1}} + \beta_2 \frac{\Delta Sales_{it}}{A_{it-1}} + \beta_3 \frac{GPPE_{it}}{A_{it-1}} + \psi_j + \varepsilon_{it}$ and predicting non-discretionary accruals using $\hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3$ but applying $\hat{\beta}_2$ to $\Delta Sales_{it} - \Delta Receivables_{it}$ instead of $\Delta Sales_{it}$ to focus on changes in credit sales. Neither alternative measure significantly changes results. Hribar and Collins (2002) point out that using these discussed balance sheet approaches to measure accruals may lead to mismeasurement for firms with M&A activities, discontinued operations or significant foreign currency accounts. Unfortunately, I am unable to use the solution proposed in Hribar and Collins (2002) because it relies on statement of cash flow data that does not exist before 1988.

B Minimum Distance Procedure

In this section I describe the procedure I use to estimate the parameters governing the ETR time series process in equation (5).

Consider the AR(1) model of the ETR time series process

$$ETR_{it} = ETR_i + x_{it}\beta + u_{it} + e_{it}$$
$$u_{it} = \alpha u_{it-1} + \varepsilon_{it},$$

where $e_{it} \sim \mathcal{N}(0, \sigma_e^2)$, and $\varepsilon_{it} \sim \mathcal{N}(0, \nu_t)$.

I estimate the parameters governing this process by fitting a model to the vector of elements of the covariance matrix C of the ETR residuals r_{it} from an OLS regression of ETR_{it} on covariates x_{it}. The covariates are depreciation and depletion scaled by lagged assets, and C can be written as

$$C = \begin{pmatrix} Var(r_{i81}) & & & & \\ Cov(r_{i82}, r_{i81}) & Var(r_{i82}) & & & \\ Cov(r_{i83}, r_{i81}) & Cov(r_{i83}, r_{i82}) & Var(r_{i83}) & & \\ \vdots & \vdots & \vdots & \ddots & \\ Cov(r_{i89}, r_{81}) & Cov(r_{i89}, r_{82}) & Cov(r_{i89}, r_{83}) & & Var(r_{i89}) \end{pmatrix}.$$

I can write the residuals as

$$r_{it} = ETR_i + \alpha^t u_{i0} + \alpha^{t-1}\varepsilon_{i1} + ... + \alpha\varepsilon_{it-1} + \varepsilon_{it} + e_{it}, \tag{B.1}$$

Which implies that I can write closed form expressions for each element of the covariance matrix C in terms of model parameters,

$$Var(r_{it}) = \sigma_E^2 + \alpha^{2t}\sigma_{u0}^2 + \nu_t + \alpha^2\nu_{t-1} + ... + \alpha^{2(t-1)}\nu_1 + \sigma_e^2, \tag{B.2}$$

$$Cov(r_{it}, r_{is}) = \sigma_E^2 + \alpha^{s+t}\sigma_{u0}^2 + \alpha^{t-s}v_s + \alpha^{t-s+2}v_{s-1} + \ldots + \alpha^{s+t-2}v_1 \quad (s < t), \tag{B.3}$$

where $\sigma_E^2 = Var(ETR_i)$ and $\sigma_{u0}^2 = Var(u_{i0})$.

I use the k elements of the residual covariance matrix C as moments to estimate the parameters θ governing the ETR process. I calibrate σ_E^2, the variance of the permanent component of ETRs, as 0.009, the variance of average residualized firm ETRs across the 1981-1992 balanced panel. I also calibrate the initial condition variance σ_{u0}^2 as 0.02, and restrict the variance of e to be stable over time, so that $Var(e_{i81}) = Var(e_{it}) = \sigma_e^2$.[33] These restrictions and calibrations focus estimation on the persistence of shocks α and the variance of shocks ν_t.

I estimate the model using data from 1981-1989 to include years before and after the implementation of AMTBIA87 without additional policy variation introduced by ACEA90. These nine years of data imply 11 parameters in θ and 45 moments in moment vector $m(\theta)$ (one for each element of C).

$$m(\theta) = \begin{pmatrix} Var(r_{81}) \\ \vdots \\ Var(r_{89}) \\ Cov(r_{82}, r_{81}) \\ Cov(r_{83}, r_{81}) \\ \vdots \\ Cov(r_{89}, r_{88}) \end{pmatrix}, \quad \theta = \begin{pmatrix} \alpha \\ \{\nu_t\}_{t=81}^{89} \\ \sigma_e^2 \end{pmatrix}$$

Minimum Distance Estimation and Inference I use a classical minimum distance estimator to find the parameters that minimize the distance between the empirical and model moments,

$$\min_{\theta \in \Theta} [\hat{m} - m(\theta)]'[\hat{m} - m(\theta)],$$

where $\hat{m}$ is the empirical estimate of the moments from the data, calculated as the variance and covariance of effective tax rates residualized on depreciation and depletion in each year from 1981-1989, and $m(\theta)$ are the moments expressed as functions of model parameters

[33] Results are qualitatively very similar under different calibrations of σ_{u0}^2.

according to equations (B.2) and (B.3).

Under regularity conditions, the vector of estimates of the moments will have a standard normal distribution $\sqrt{N}(\hat{m} - m) \rightarrow \mathcal{N}(0, V)$, and the estimated parameters will follow $\sqrt{N}(\hat{\theta} - \theta) \rightarrow \mathcal{N}(0, \Delta)$, where $\Delta = (F'F)^{-1}F'VF(F'F)^{-1}$ and $F = \frac{\partial m(\theta)}{\partial \theta}$. I estimate $\hat{F}$ as $F(\hat{\theta})$ and $\hat{V}$ as the second moments of the moment vector.

C Effective Tax Rate Moments

This appendix provides a heuristic discussion of possible biases in placebo-in-time estimates stemming from changes in effective tax rate moments. Appendix Figure D.3, panel (a) displays average ETRs in the 1981-1992 and 1974-1986 balanced panels, while panel (b) displays three year running ETR variances and autocovariances.

The variance and autocovariance moments in Figure D.3, panel (b) confirm the intuition suggested by the minimum distance estimates described in section IV.A. ETR variances are relatively stable throughout the 1980s, suggesting little bias in placebo-in-time estimates from changing shock variances.[34] While ETR variances are lower in the late 1970s, having lower ETR shock variances during treatment definition years should lower the amount of mean reversion measured in response to placebo treatment definitions because the low ETRs of treatment firms during treatment definition years will be higher, biasing placebo-in-time estimates towards finding larger avoidance responses to AMTBIA87. ETR autocovariances remain relatively stable but decline slightly throughout the 1980s. The relative stability of autocovariances suggest there is no drastic change in shock persistence, implying the magnitude of any bias induced by changes in persistence will be small. The slight autocovariance decline once again suggests that any bias from changing ETR shock persistence should push placebo-in-time estimates towards finding an avoidance response.

The mean effective tax rates plotted in Figure D.3, panel (a) allow us to roughly bound bias in placebo-in-time estimates that could be introduced by a change in permanent effective tax rates ETR_i. If AMTBIA87 or TRA86 changed ETR_i, it would change the level firms

[34] Using ETRs residualized on the depreciation and depletion covariates used in the baseline event study regressions, tests of equal variance before and after AMTBIA87 in the 1981-1992 balanced panel fail to reject the null hypothesis (p-value = 0.24).

mean revert towards. Average ETRs remain remarkably stable from 1986-1992, suggesting that the implementation of TRA86 and AMTBIA87 did not increase permanent effective tax rates ETR_i. However, there is a 3.19% increase in average ETRs from 1985 to 1986. Suppose that this increase reflected a permanent increase in ETRs, was concentrated entirely among treatment firms, and coincided with the implementation of AMTBIA87. In this scenario, we would expect prior-year responses to placebo treatment definitions to understate ETR mean reversion by 3.19%, biasing placebo-in-time estimates upwards by roughly -0.57% of lagged assets. Post-reform point estimates in Figure 4, panel (c) with bias corrections would be approximately 0.5% of lagged assets instead of zero, but they would still not reject zero.

D Revenue Scoring Methodology

To develop a revenue score of the Biden book income AMT I simulate the evolution of a 2018 cross section of firms' book incomes over the scoring time frame while incorporating avoidance responses to the policy. To select a cross section of firms in 2018, I restrict the Compustat fundamentals annual data to firms with positive, non-missing assets, sales, and pretax income that are incorporated in the U.S. and exist in the data in both 2017 and 2018.[35] I display summary stats for this sample of firms in Table D.3. Relative to the historical sample, firms are significantly larger in 2018 but exhibit the same type of skew with means of most variables exceeding medians by a large amount.

In the 2018 cross section of firms, I construct measures of total tax liability, eligible carryforwards for net operating loss deductions, the tax amount potentially due because of the book income AMT, and new tax due under the book income AMT. I measure applicable tax loss carryforwards as the minimum of Compustat pretax income and tax loss carryforwards. I calculate potential tax due because of the book income AMT as 15% of the difference between Compustat pretax income and applicable tax carryforwards, all less foreign taxes. Finally, I calculate the firm's new tax liability as the maximum of the firm's old tax liability or the potential tax due because of the book income AMT, only applying the AMT if the

[35] I include all firms present in Compustat, including firms with partnership structures like Enterprise Production Partners LP and Energy Transfer LP, which are among the firms displayed in Figure 9.

firm has over $100 million in EBITD.

To ensure my construction of tax status in the Compustat data is consistent with tax data, I compare aggregates of tax variables available in SOI line item reports to aggregate proxies in the Compustat data in Figure D.14 for available years spanning 2008-2015. While there are differences in aggregates in Compustat and the SOI line item reports, the magnitudes reasonably track each other across years.

Building on the 2018 cross section, I construct a panel by simulating ten years forward for each firm, taking into account possible avoidance responses to the proposed Biden book income AMT. To facilitate a direct mapping from the event study estimates of avoidance in section IV into the simulated data, I use CBO's 2018 ten-year GDP forecast as a proxy for book income growth per year for all firms, inflating book income (as well as EBITD and all other tax variables) by the CBO projected growth rate, and calculating book income as the sum of projected book income and a possible avoidance response to the policy.[36] Summing across the new tax liabilities in the first year of the simulation with no behavioral responses yields a one year mechanical tax revenue estimate of $30 billion.

To incorporate firm avoidance responses into the book income projection, I define book income for each firm in the simulation as the sum of projected mechanical book income, and a possible avoidance response to the proposed Biden book income AMT,

$$BI_t = BI_t^{mech} + \varepsilon_t \cdot BI_t^{mech} \cdot \frac{\Delta(1-\tau)}{1-\tau} \cdot \mathbb{1}(T=1), \tag{D.1}$$

where BI_t^{mech} is projected book income over the ten-year window applying only CBO GDP forecasts to 2018 book income, ε_t is the elasticity of book income with respect to the net of tax rate over time horizon t that I estimate in section IV, and $\frac{\Delta(1-\tau)}{1-\tau} = \frac{0.85-1}{1} = -0.15$ is the change in the net of tax rate after the introduction of the proposed Biden book income AMT 15% marginal tax on book income.

[36] To account reasonably for firm losses, I calculate the share of firms with positive losses in 2018 and calculate the ratio of those firm's losses to their pretax income. In each subsequent simulation year I randomly select a fraction of firms that matches the share with positive losses in 2018, and within this sample subtract the fraction of pretax income that was removed via applicable losses in the 2018 calculation. In unreported results, I find that revenue estimates are similar when I instead calculate the observed fraction of tax loss carryforwards over book income in 2018, and reduce projected book income for every firm by that same fraction in each subsequent simulated year.

I capture avoidance responses to the proposed Biden book income AMT with $\varepsilon_t \cdot BI_t^{mech} \cdot \frac{\Delta(1-\tau)}{1-\tau} \cdot \mathbb{1}(T=1)$. The first terms $\varepsilon_t \cdot BI_t^{mech} \cdot \frac{\Delta(1-\tau)}{1-\tau}$ unwind the elasticity into a change in book income for each firm. $\mathbb{1}(T=1)$ is an indicator for firms with over \$100 million in EBITD in 2018 that would pay the proposed Biden book income AMT in 2018. This ensures that I only apply avoidance responses in the revenue simulation to a group of firms analogous to the treatment group in the event study analysis in section IV.

After projecting book incomes, I calculate firms additional tax liability as the excess of their projected book income AMT tax liability over their projected tax liability under the normal corporate tax system. Book income AMT liabilities are reduced by foreign tax credits and net operating losses. This methodology calculates a revenue score for the proposed book income AMT holding all other tax policies fixed, though it can be adjusted to incorporate other changes like modifications to the corporate tax rate or treatment of losses and foreign tax credits.

Table D.1: Classical Minimum Distance Parameter Estimates

Parameter	Point Estimate	Standard Error
α	0.224	0.012
ν_{81}	0.016	0.001
ν_{82}	0.022	0.001
ν_{83}	0.017	0.001
ν_{84}	0.017	0.001
ν_{85}	0.019	0.001
ν_{86}	0.022	0.001
ν_{87}	0.017	0.001
ν_{88}	0.020	0.001
ν_{89}	0.019	0.002
σ_e^2	0.001	0.001

Notes: This table reports classical minimum distance estimates of the parameters governing the effective tax rate process in equation (5). The estimation procedure is described in detail in Appendix B.

Table D.2: Distributed Lag Regression Estimates

Variable	OLS (1)	IV (2)
ΔETR_t	-0.12	-0.15
	(0.01)	(0.04)
$\Delta ETR_t \times Post$	-0.01	-0.05
	(0.01)	(0.08)
ΔETR_{t-1}	-0.00	
	(0.01)	
$\Delta ETR_{t-1} \times Post$	-0.00	
	(0.01)	
Observations	1261	1261
Clusters	343	343
F Stat		3.16
LM Stat		5.43

Notes: This table reports OLS and instrumental variable regression coefficients from equation (7). The estimation sample is all treatment firms in a stacked data set with treatment definitions based on ETRs in 1981-1983, 1982-1984, 1983-1985, 1984-1986, 1985-1987, 1986-1988 and 1987-1989. The regression restricts to event times negative one through one. Standard errors are in parentheses and clustered at the firm level.

Table D.3: Summary Statistics for Revenue Simulation Sample

	Observations	Mean	P10	Median	P90
Lagged Assets	2689	12255	173	2000	24995
Book Income	2689	0.08	0.01	0.05	0.18
Taxable Income	2689	0.06	0.00	0.02	0.17
Book Tax Differences	2689	0.02	−0.04	0.00	0.08
Sales	2689	0.76	0.05	0.52	1.87
Costs of Goods Sold	2689	0.48	0.01	0.23	1.34
Investment	2436	0.25	0.06	0.18	0.50
Debt	2686	0.69	0.28	0.69	1.03
Depreciation	2689	0.03	0.00	0.02	0.06
Depletion	2689	0.00	0.00	0.00	0.00
Employment	2490	13	0	2	30

Notes: This table reports summary statistics for the sample of firms used in revenue simulations. Statistics are expressed as a share of lagged assets, except for counts, employment (thousands), investment (capital expenditure per dollar of lagged net property plant and equipment) and lagged assets (millions USD).

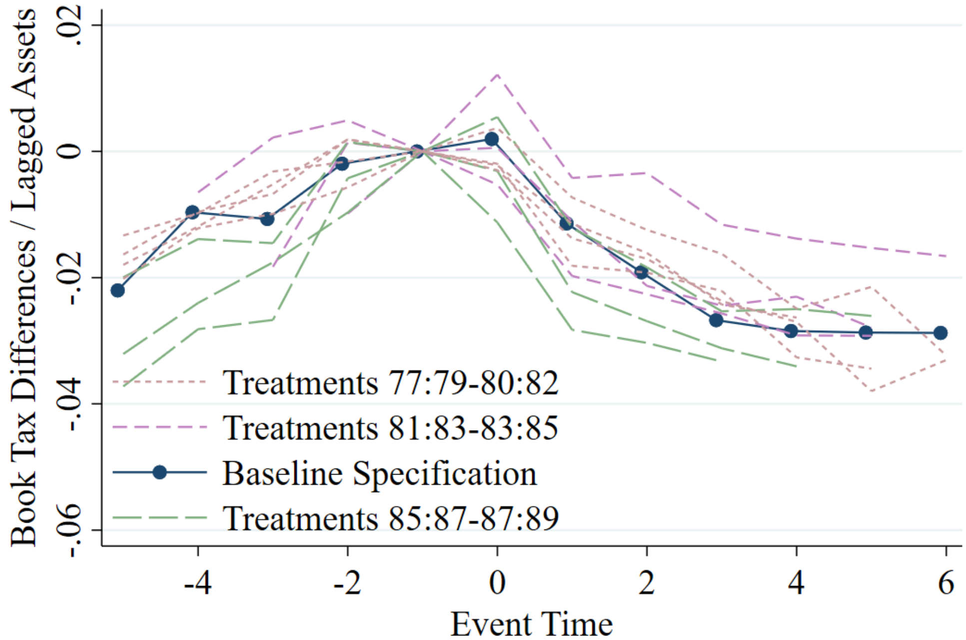
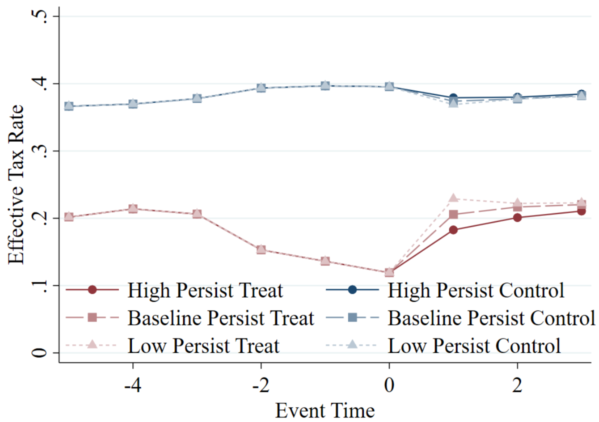
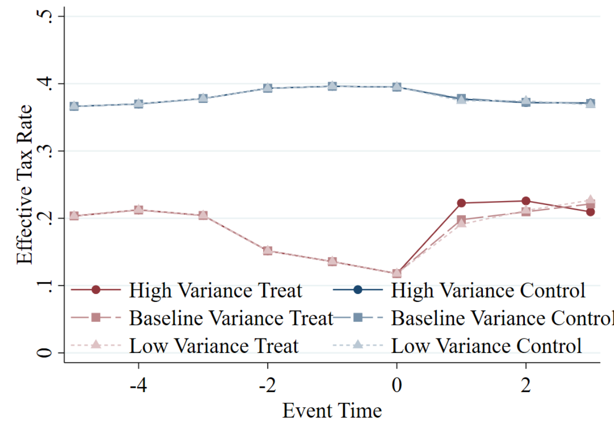


Figure D.1: Book Tax Difference Responses to Pre and Post-Reform Placebo Treatments

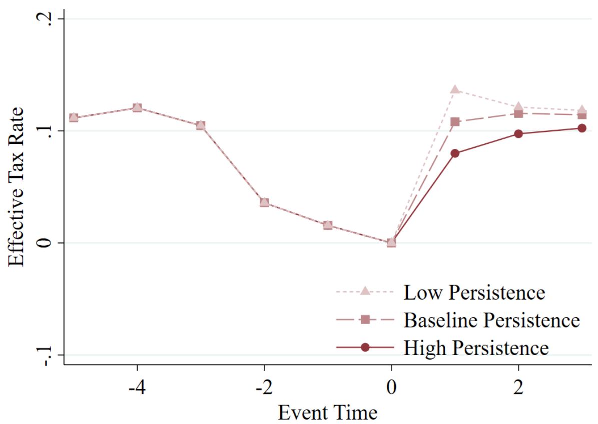
Notes: This figure plots placebo-in-time estimates of tax avoidance responses to AMTBIA87. Each series plots point estimates of β_e from equation (3). The baseline specification series splits the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs. The other series plot β_e estimates using treatment definitions based on effective tax rates during different years. Treatment definitions starting in 1980 and earlier use the 1974-1986 balanced panel.



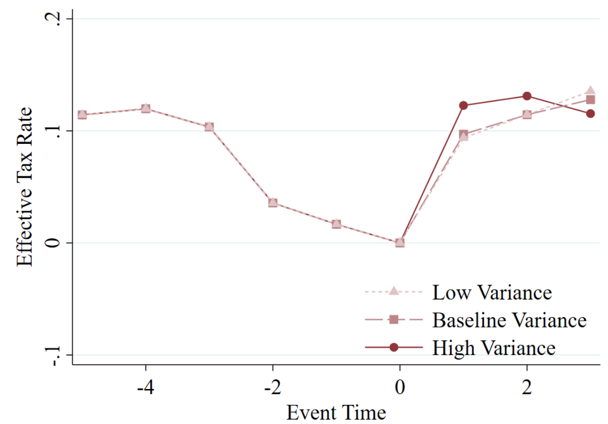
(a) Simulated ETRs Under Different α



(b) Simulated ETRs Under Different ν



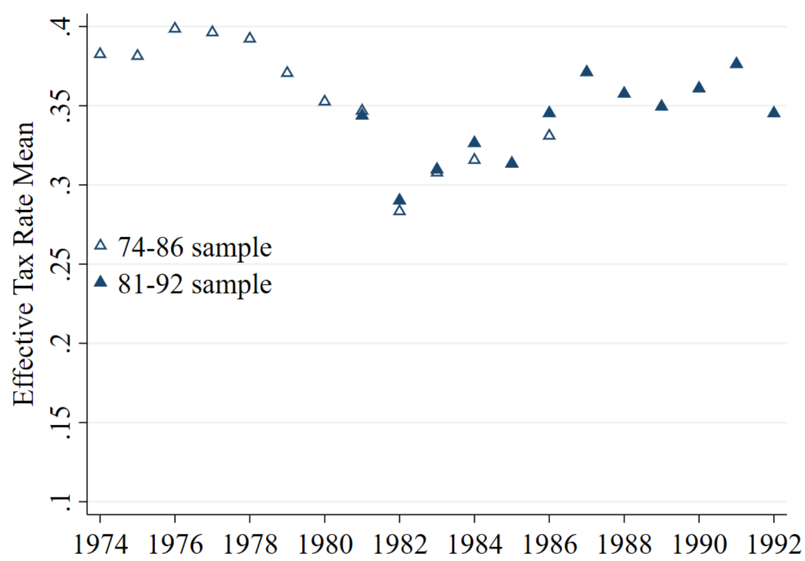
(c) Difference in ETRs Under Different α



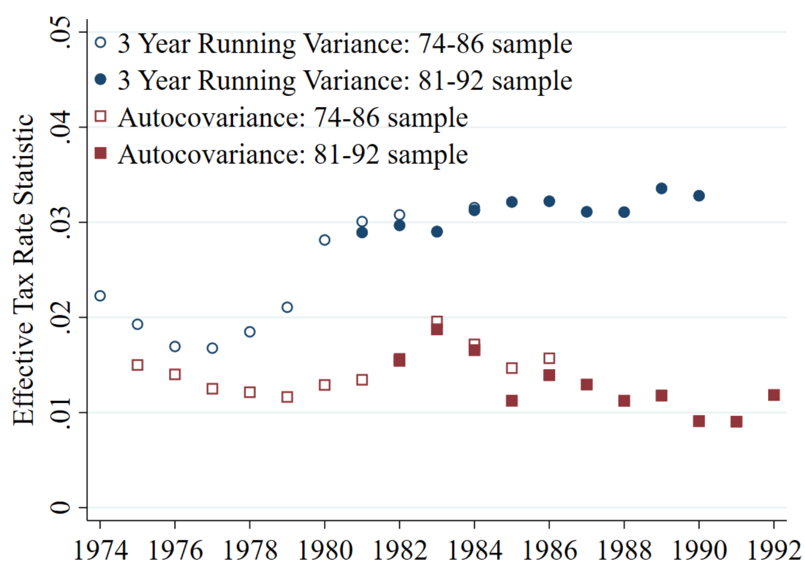
(d) Difference in ETRs Under Different ν

Figure D.2: Simulating Mean Reversion With Different Parameter Values

Notes: This figure plots simulated effective tax rates under different assumptions for α, the persistence of effective tax rate shocks and ν, the variance of effective tax rate shocks. Panel (a) shows average ETRs for treatment and control firms in a nine period simulated panel of ETRs using the time series model in equation (5) and the estimated parameters from the minimum distance procedure displayed in Table D.1. The baseline series is made from simulated data using all estimated parameters. The high persistence series uses all the same parameters except for $\alpha = 0.45$ in event times 1-3, and the low persistence series uses all the same parameters except for $\alpha = 0.00$ in event times 1-3. Firms are put in the treatment group if their average ETRs are below 23% in event times -2-0. Panel (b) also shows average ETRs for treatment and control firms, but the high variance series uses $\{\nu_e\}_{e=-2}^{0} = 0.028$ and the low persistence series uses $\{\nu_e\}_{e=-2}^{0} = 0.010$. Panels (c) and (d) show the difference in treatment and control ETRs for panels (a) and (b), normalizing the difference in event time 0 to 0.



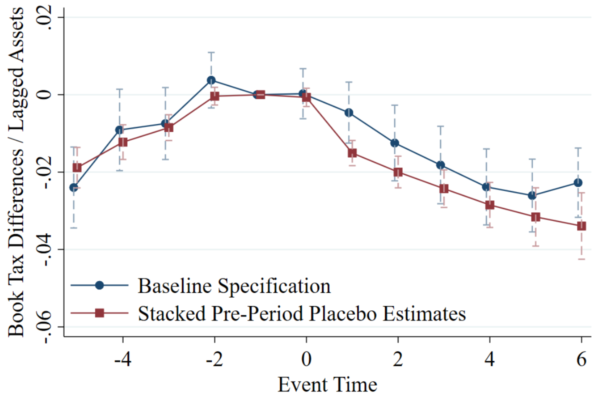
(a) Effective Tax Rate Means



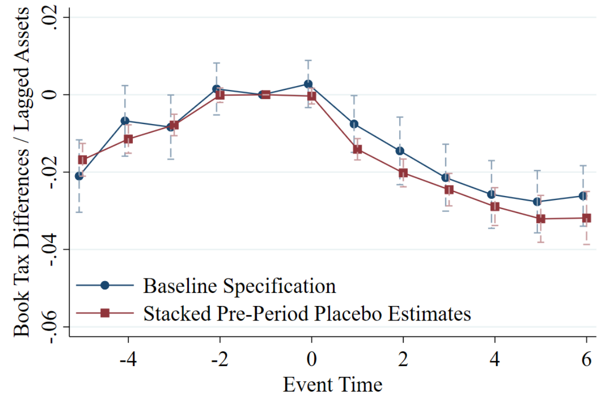
(b) Effective Tax Rate Variances

Figure D.3: Effective Tax Rate Moments

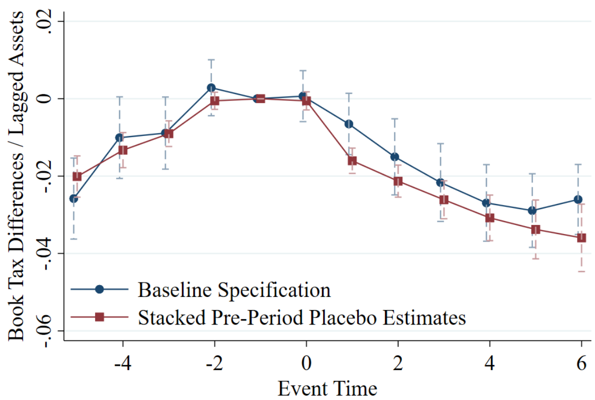
Notes: This figure plots effective tax rate statistics in the 1981-1992 and 1974-1986 balanced panels by year. Panel (a) plots the mean effective tax rate, while panel (b) plots three year running variances and autocovariances of the effective tax rate. The three year running variance in any given year reports the variance of effective tax rates in that year and the two following years.



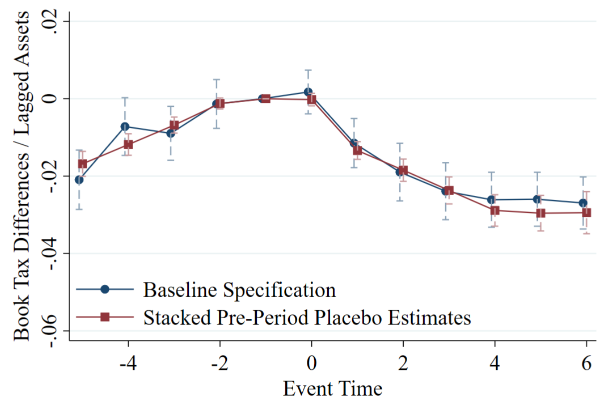
(a) 15% Effective Tax Rate Cutoff



(b) 19% Effective Tax Rate Cutoff



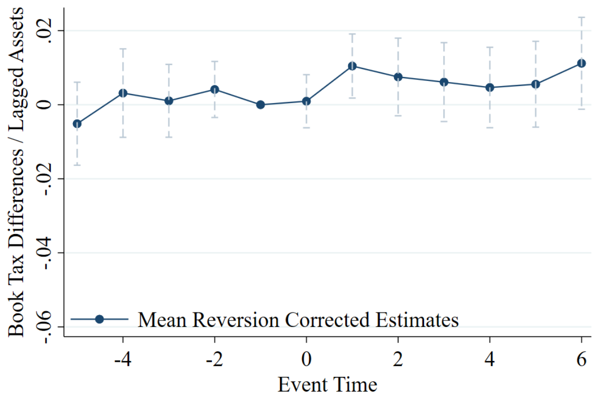
(c) Treatment <15%, Control > 23%



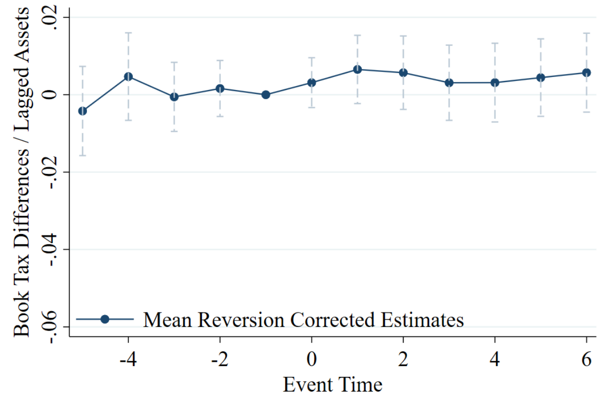
(d) 26% Effective Tax Rate Cutoff

Figure D.4: Placebo-in-Time Estimates With Alternative Treatment Definitions

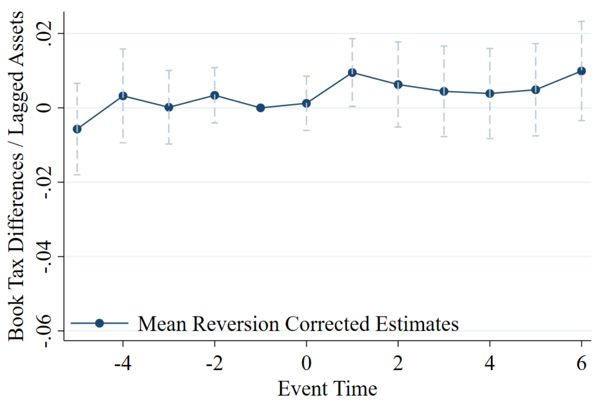
Notes: This figure plots placebo-in-time estimates of tax avoidance responses to AMTBIA87. Each panel replicates the estimates in Figure 4, panel (b), but with a different treatment definition. Panel (a) uses a 15% average effective tax rate cutoff for the treatment and control groups. Panel (b) uses a 19% effective tax rate cutoff. Panel (c) assigns treatment status to firms with average effective tax rates below 15%, control status to firms with average effective tax rates above 23%, and excludes firms between. Panel (d) uses a 26% average effective tax rate cutoff for the treatment and control groups.



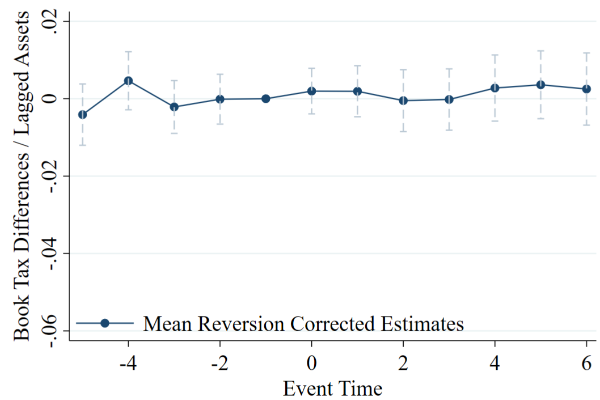
(a) 15% Effective Tax Rate Cutoff



(b) 19% Effective Tax Rate Cutoff



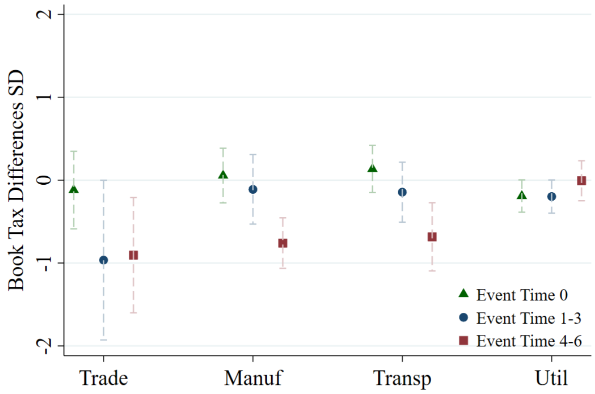
(c) Treatment <15%, Control > 23%



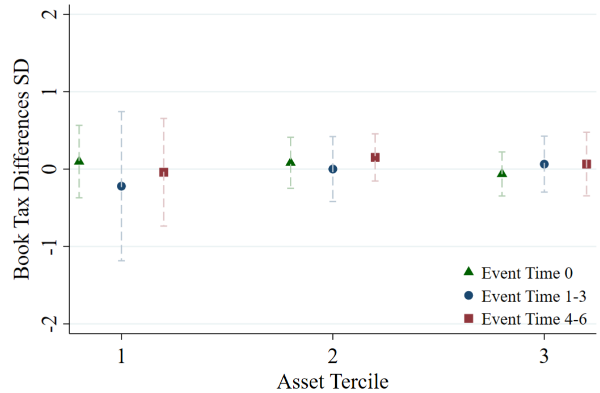
(d) 26% Effective Tax Rate Cutoff

Figure D.5: Mean Reversion Corrected Estimates With Alternative Treatment Definitions

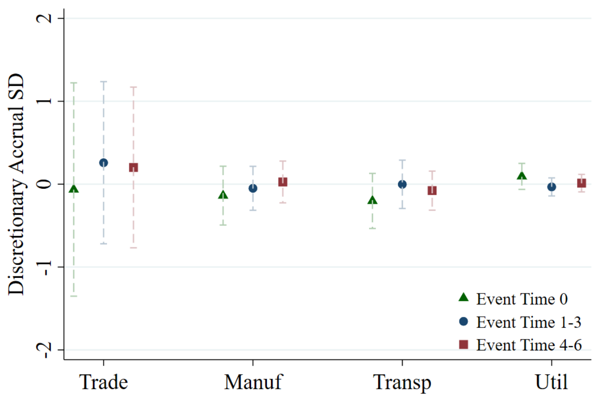
Notes: This figure plots placebo-in-time estimates of tax avoidance responses to AMTBIA87. Each panel replicates the estimates in Figure 4, panel (c), but with a different treatment definition. Panel (a) uses a 15% average effective tax rate cutoff for the treatment and control groups. Panel (b) uses a 19% effective tax rate cutoff. Panel (c) assigns treatment status to firms with average effective tax rates below 15%, control status to firms with average effective tax rates above 23%, and excludes firms between. Panel (d) uses a 26% average effective tax rate cutoff for the treatment and control groups.



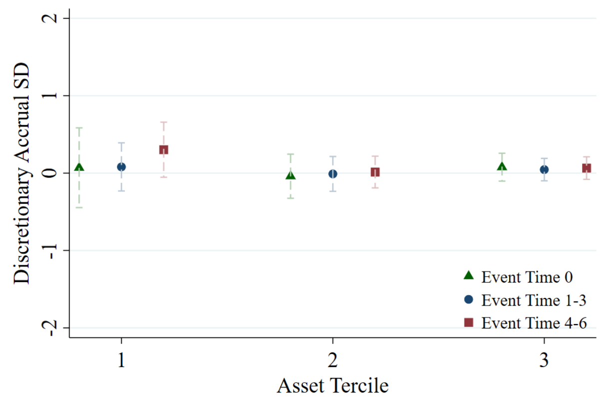
(a) BTD Industry Heterogeneity



(b) BTD Size Heterogeneity



(c) DA Industry Heterogeneity



(d) DA Size Heterogeneity

Figure D.6: Avoidance Response Heterogeneity

Notes: This figure plots tax avoidance responses to AMTBIA87 for industry and size subgroups, scaling each outcome by its standard deviation in the pre-reform period. Panels (a) and (b) use book tax differences scaled by lagged assets as the outcome, estimate β_e and η_e from equations (3) and (4), and plot the difference between these two estimates scaled by the standard deviation of the outcome in the pre-reform period in the baseline panel data set. Confidence intervals are constructed from bootstrapped standard errors clustered at the firm level with 300 iterations. Panel (a) plots estimates separately across industry subgroups, while panel (b) plots estimates separately across 1985 asset tercile subgroups. Panels (c) and (d) display estimates of β_e from equation (3) using discretionary accruals scaled by lagged assets as an outcome, scaling estimates of β_e by the pre-period standard deviation of the outcome. Confidence intervals are constructed from standard errors clustered at the firm level. Panel (c) plots estimates across industries, and panel (d) plots estimates across 1985 asset terciles. Industries include manufacturing (SIC codes 2000-3999), utilities (SIC codes 4900-4999), finance and insurance (SIC codes 4000-4899) and trade (SIC codes 5200-5999).

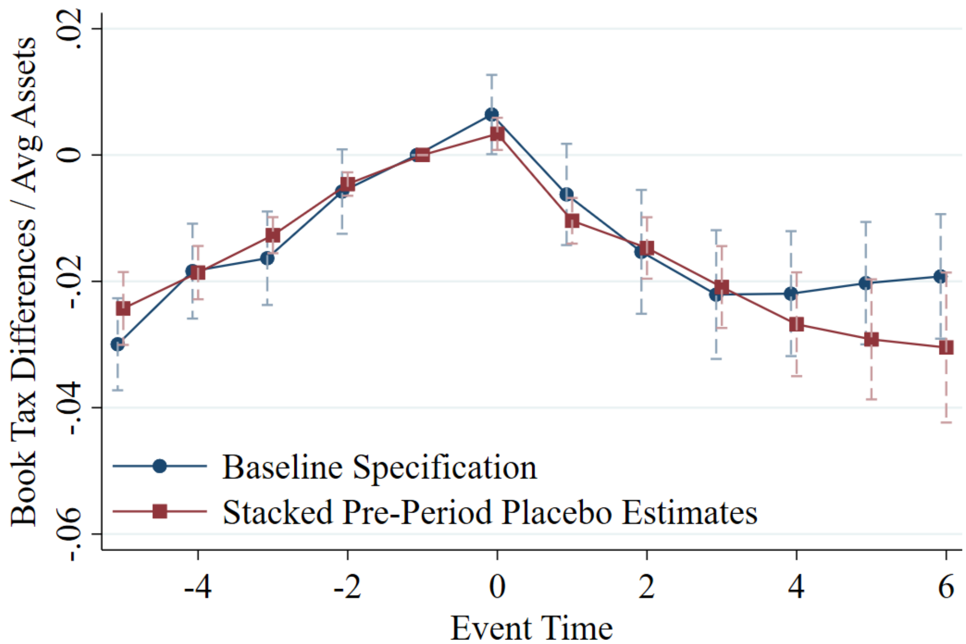
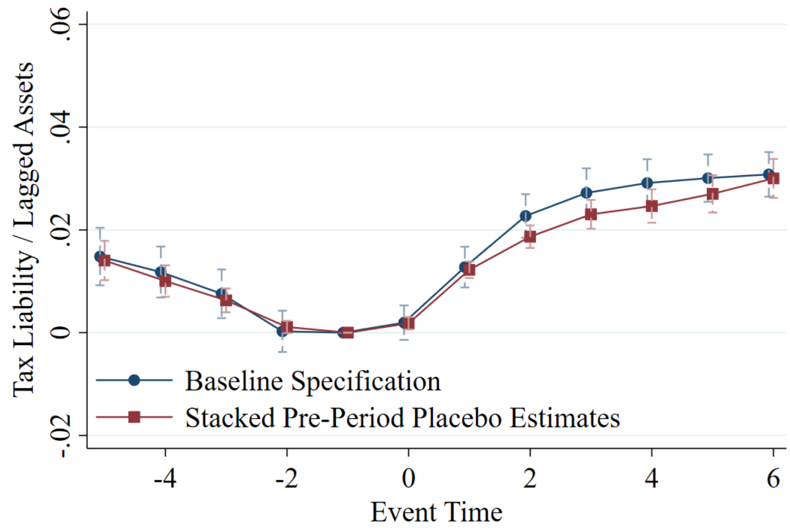
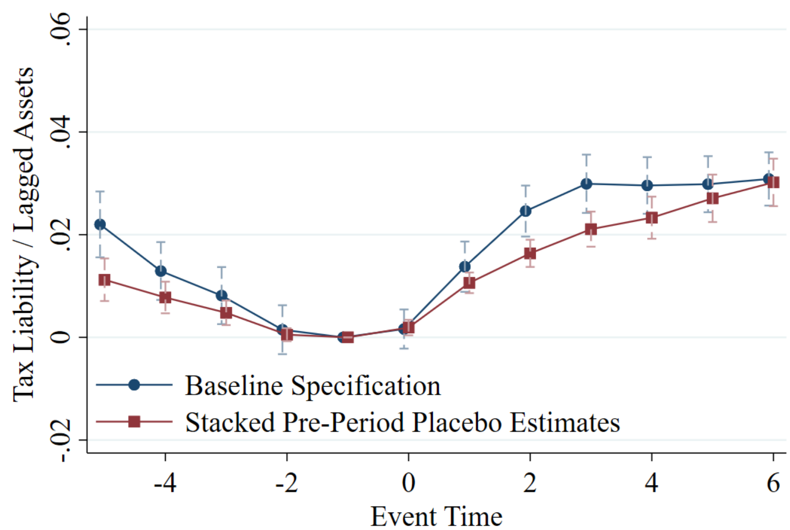


Figure D.7: Placebo-in-Time Book Tax Difference Estimates Scaled by Average Assets

Notes: This figure plots book tax difference responses to AMTBIA87 scaling the outcome by average pre-period assets. The baseline specification series plots estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs. The stacked pre-period placebo series plots estimates of η_e from equation (4) splitting data from the 1974-1986 and 1981-1992 balanced panels into treatment and control groups based on ETRs from three year time periods spanning 1977-1985, as in Figure 4, panel (b). Confidence intervals are calculated from standard errors clustered at the firm level.



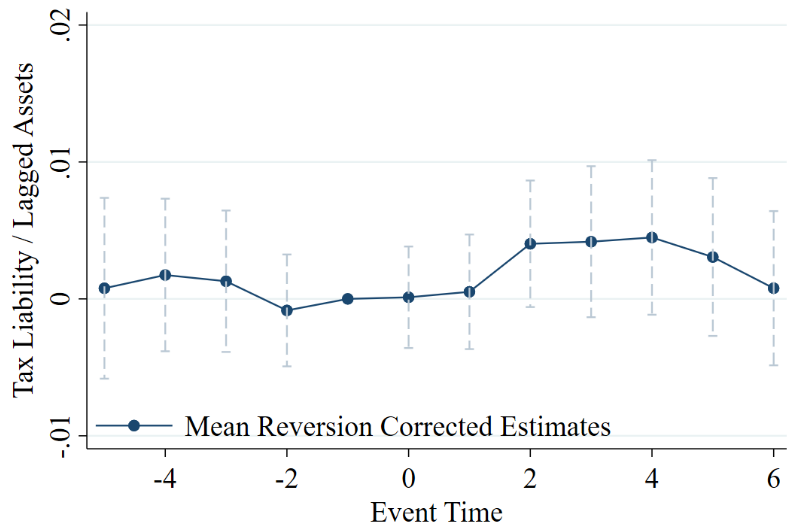
(a) Full Sample



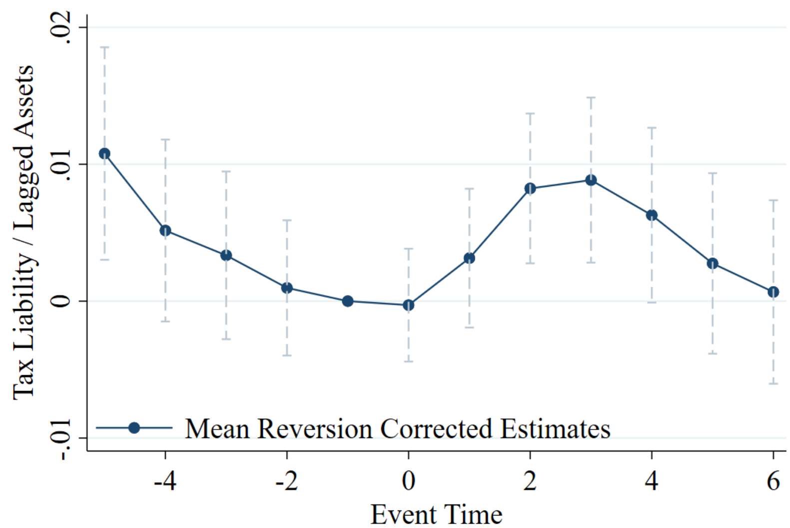
(b) No Multinationals or Loss Firms

Figure D.8: Placebo-in-Time Tax Liability Estimates

Notes: This figure plots tax liability responses to AMTBIA87. Panel (a) plots point estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs in the baseline specification series. Panel (a) also plots estimates of η_e from equation (4) in the stacked pre-period placebo series, splitting data from the 1974-1986 and 1981-1992 balanced panels into treatment and control groups based on ETRs from three year time periods spanning 1977-1985, as in Figure 4, panel (b). Confidence intervals are calculated from standard errors clustered at the firm level. Panel (b) is identical to panel (a) but excludes all firms that are multinationals (firms with non-missing pretax foreign income or foreign tax expense at any event time before zero) or that have losses (firms with positive tax loss carryforwards at event time zero) from the estimation sample.



(a) Full Sample



(b) No Multinationals or Loss Firms

Figure D.9: Mean Reversion Corrected Tax Liability Estimates

Notes: This figure plots placebo-in-time estimates of tax liability responses to AMTBIA87. Each panel replicates the estimates in Figure 4, panel (c) using tax liability scaled by lagged assets as an outcome. Panel (a) displays estimates using all firms while panel (b) excludes multinationals and loss firms.

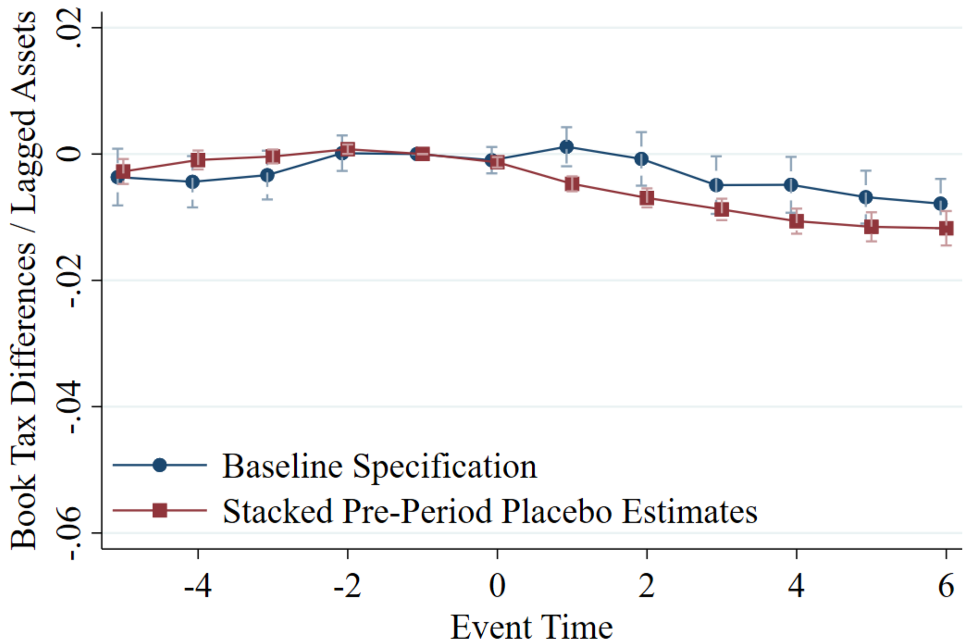
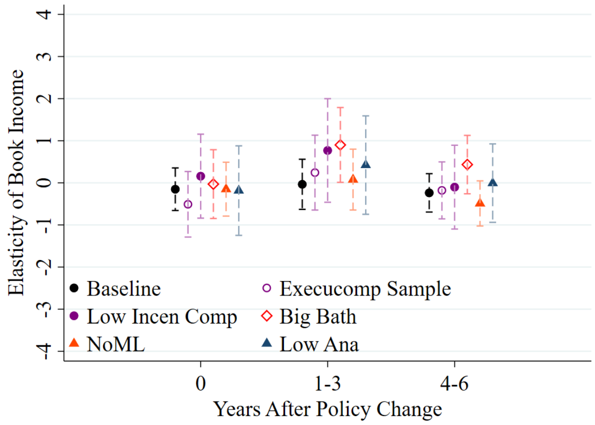
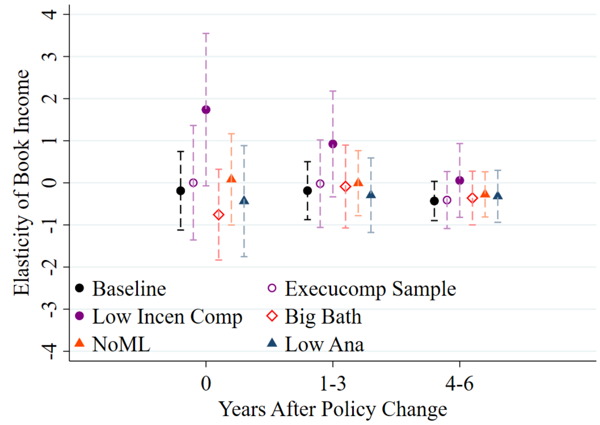


Figure D.10: Placebo-in-Time Permanent Book Tax Difference Estimates

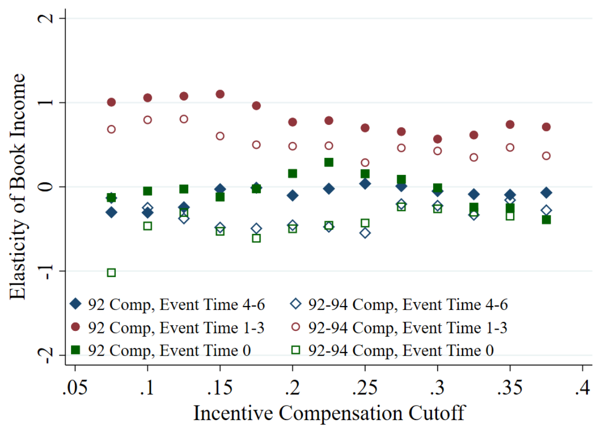
Notes: This figure plots permanent book tax difference responses to AMTBIA87, constructed as book tax differences minus deferred tax assets divided by the statutory tax rate. The baseline specification series plots point estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs. The stacked pre-period placebo series plots estimates of η_e from equation (4), splitting data from the 1974-1986 and 1981-1992 balanced panels into treatment and control groups based on ETRs from three year time periods spanning 1977-1985, as in Figure 4, panel (b). Confidence intervals are calculated from standard errors clustered at the firm level.



(a) BTD Subsample Elasticity Estimates



(b) DA Subsample Elasticity Estimates



(c) BTD Elasticity Estimates By Incentive-Based Compensation Fraction

Figure D.11: Subsample Elasticity Estimates

Notes: This figure plots tax avoidance responses to AMTBIA87 among firm subsamples facing the strongest incentives to avoid the tax. Elasticities in panel (a) are calculated with an identical procedure to the one described in Figure 5, and 95% confidence intervals are constructed from nonparametrically bootstrapped standard errors clustered at the firm level. Elasticities in panel (b) are calculated with an identical procedure to the one described in Figure 7, and 95% confidence intervals are constructed from standard errors clustered at the firm level. Panel (c) plots point estimates of elasticities restricting the sample to firms present in the Execucomp data and with incentive-based compensation below the cutoff on the x-axis. Filled markers plot point estimates using cutoffs based on incentive-based compensation in 1992, while hollow markers plot point estimates using cutoffs based on average incentive-based compensation from 1992-1994.

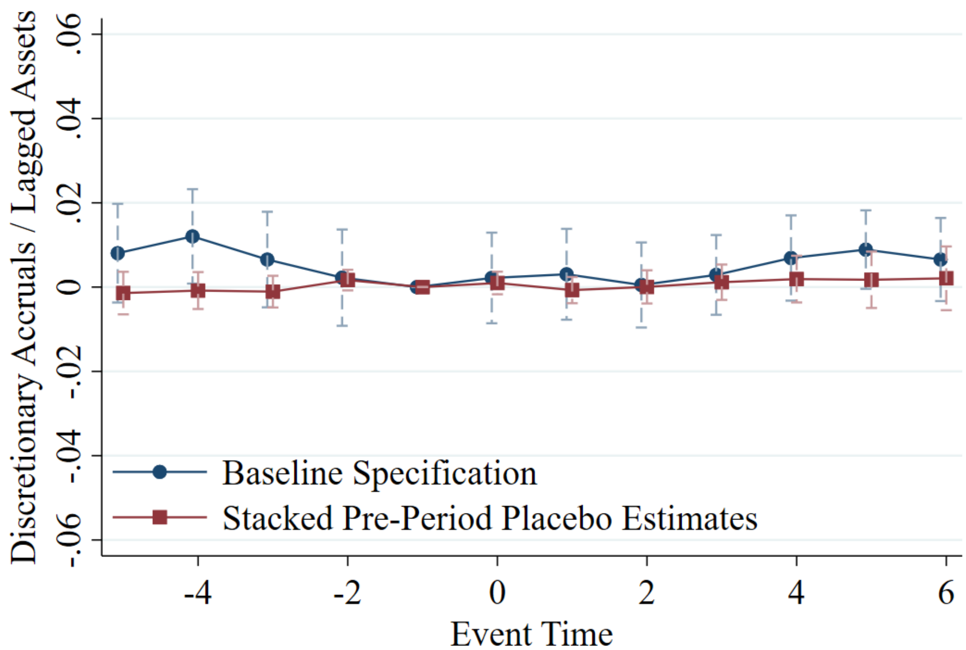
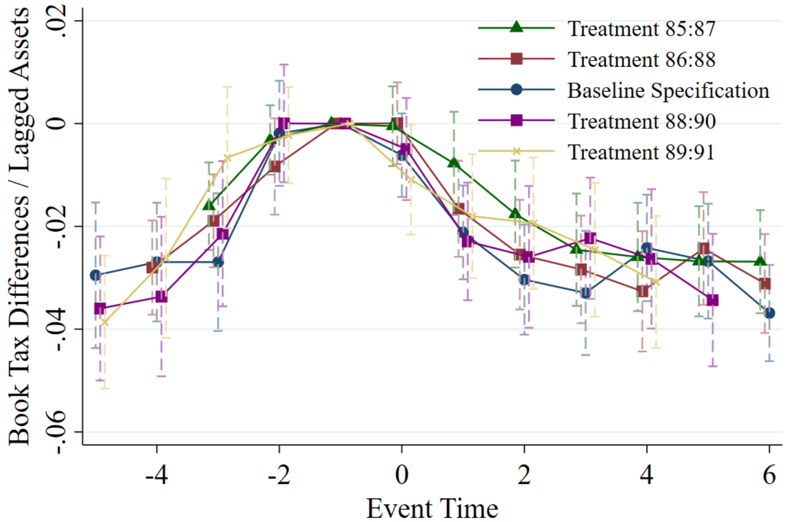
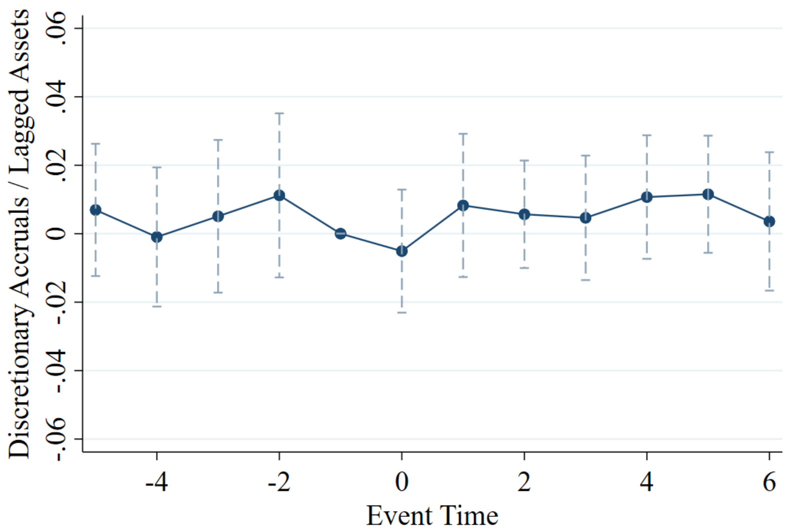


Figure D.12: Discretionary Accrual Mean Reversion

Notes: This figure plots discretionary accrual responses to AMTBIA87. The baseline specification series plots estimates of β_e from equation (3). The stacked pre-period placebo series plots estimates of η_e from equation (4), splitting data from the 1974-1986 and 1981-1992 balanced panels into treatment and control groups based on ETRs from three year time periods spanning 1977-1985, as in Figure 4, panel (b). Standard errors are clustered at the firm level.



(a) Book Tax Differences



(b) Discretionary Accruals

Figure D.13: Firm Responses To ACEA90

Notes: This figure plots firm responses to the transition from AMTBIA87 to ACEA90 when the book income component of the minimum tax base was replaced with adjusted current earnings and the marginal tax rate on this quantity was raised from 10 to 15%. Both panels in this figure use a balanced panel spanning 1984-1995, and split firms into treatment and control groups using a 17% effective tax rate cutoff. Panel (a) plots estimates of β_e from equation (3) using book tax differences as an outcome. The treatment definition for each series is based on effective tax rates in the years specified in the labels, while the baseline specification uses 1987-1989. Panel (b) plots estimates of β_e from equation (3) using discretionary accruals as an outcome. Confidence intervals are calculated from standard errors clustered at the firm level.

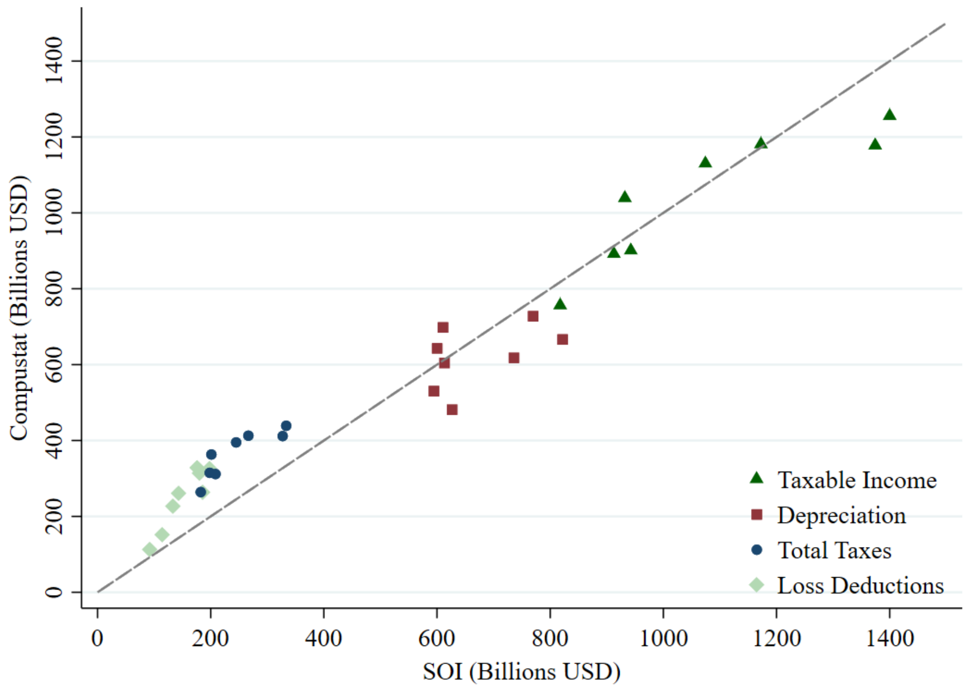


Figure D.14: Comparison of SOI Line Item Estimates to Compustat Aggregates 2008-2015

Notes: This figure compares aggregate sums of taxable income, depreciation, total taxes and net operating loss deductions in Statistics of Income line item reports and Compustat for years 2008 through 2015. The Compustat sample contains all firms with positive, non-missing assets, sales and pretax income that are incorporated in the U.S. in each year. Exact values for depreciation are available from both data sources. I construct a measure of total taxes in Compustat as total income taxes minus deferred income taxes minus other taxes, and my measure of taxable income in Compustat is total taxes divided by the marginal tax rate. To construct a measure of net operating loss deductions in Compustat, I take the minimum of Compustat tax loss carryfowards and pretax income.