Firm Responses to Book Income Alternative Minimum Taxes *

Jordan Richmond, January 31, 2023

Abstract

This paper studies how firms respond to book income alternative minimum taxes (AMTs) by examining the AMT book income adjustment in 1987. Using Compustat data and an event study approach, I find no evidence that firms avoid the tax, and no evidence of significant real production or investment responses. Firm tax base responses imply an elasticity of book income of -0.03 $[-0.63, 0.56]$, smaller than previous estimates because I correct 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]

1 Introduction

In 2017, Amazon reported $5.6 billion in profit but paid no U.S. federal income tax (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 taxes. Over the last forty years, U.S. tax policy-makers have attempted to eliminate the divergence between firms' incomes and taxes 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 book income AMT 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 because firms have substantial discretion to determine their own book incomes (Manzon and Plesko, 2002), and broadening the tax base could push firms to reduce their production and investment (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 of tax base responses to 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 their 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, my preferred event study 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.

Why might firms not avoid AMTBIA87? To explain the lack of avoidance responses, I develop a static, partial equilibrium model of firm taxes that allows for misreporting. The model microfounds an intuitive idea from the finance literature that firms and their managers face strong non-tax incentives to report high book incomes to investors. Existing empirical research supports this hypothesis (Burgstahler and Dichev, 1997; Graham, Harvey and Rajgopal, 2005; Terry, 2017), to the extent that firms even appear willing to pay additional taxes to justify reporting fraudulently high earnings (Erickson, Hanlon and Maydew, 2004). The model predicts larger avoidance responses to AMTBIA87 among firms with weaker non-tax incentives to report high book income. To test this, I estimate firm tax base responses restricting to firms with less incentive-based compensation and firms missing salient earnings thresholds by large margins. 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.

A host of robustness checks suggest alternative explanations for the null tax base responses are unlikely. One concern is that the treatment definition does not properly identify firms facing tax increases from the AMT. However, event study estimates using current tax expense as an outcome suggest treatment firms saw their taxes increase by an average of 0.29% of lagged assets relative to control firms over 1987-1989. Another concern highlighted by previous research is that many firms were able to mitigate the tax with net operating losses and foreign tax credits (Boynton, Dobbins and Plesko, 1992; Manzon, 1992). As expected, I find larger average tax increases of 0.67% of lagged assets when I exclude multinationals and loss firms that could use credits and deductions to reduce the tax. However, I still estimate null tax base responses in this restricted sample. The null results also appear unlikely to be driven by AMT credits, and hold across different treatment definitions, outcome constructions, controls, industries, firm sizes, and samples.

One shortcoming of using accounting data is that I measure the taxable income component of firm's book tax differences with some error (Manzon and Plesko, 2002). Particularly, I am likely to understate book tax differences for firms paying minimum taxes. This may bias event study estimates of tax base responses towards finding an avoidance response, strengthening the argument that firms do not exhibit large avoidance responses to AMTBIA87.

However, to avoid the specific measurement error concerns associated with book tax differences, I also perform robustness checks measuring firm avoidance responses to AMTBIA87 using discretionary accruals, a common proxy for earnings management capturing manipulable components of earnings not explained by cash flows or economic conditions (Dechow, Sloan and Sweeney, 1995; Bergstresser and Philippon, 2006). Estimates of book income elasticities based on event study estimates of discretionary accrual responses to AMTBIA87 are also close to zero, hold across a similar set of robustness checks to the tax base estimates, and are larger for firms with weaker incentives to report high book incomes. Despite discretionary accruals not capturing the entire tax base, and serious limitations of their ability to proxy for earnings management (Ball, 2013), the similarity of my discretionary accrual estimates to the tax base estimates strengthens the case that firms did not avoid AMTBIA87.

Additional event study estimates show that firms did not respond to AMTBIA87 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 expense 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 $337 billion over a decade. Using larger elasticity estimates from earlier work on firm responses to AMTBIA87 understates projected revenue by 18%. Close to one-third of the revenue comes from the ten firms facing the largest tax increases, which include Hewlett Packard, Berkshire Hathaway and Delta. However, Amazon only faces the 42$^{\text{nd}}$ largest tax increase because foreign tax credits and losses reduce their tax. 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 tax increases.

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 closely related work, Dhaliwal and Wang (1992) find large tax base responses to AMTBIA87 comparing Compustat firms with 1986 effective tax rates less than 23% (treatment) to firms with 1986 effective tax rates greater than 23% (control). Rescaling these estimates, Dharmapala (2020) finds they imply a book income elasticity point estimate of 1.7 that rejects 0. I carefully document that my estimates reject elasticities of this magnitude because I control for mean reversion.

Mean reversion impacts estimates of tax base responses to AMTBIA87 in my event study framework 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 Dhaliwal and Wang (1992) estimates, while using the placebo-in-time approach with alternative specifications, treatment definitions, outcome constructions, and across different subsamples consistently yields close to zero elasticities.

[3] Coombs, Dube, Jahnke, Kluender, Naidu and Stepner (2021) and Derenoncourt, Noelke and Weil (2021) use similar approaches to study the impacts of unemployment insurance withdrawal during the COVID19 pandemic and spillover effects of voluntary employer minimum wages.

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 four ways. First, I show that event study estimates of book tax difference responses to treatment definitions based on different years are stable over a significant time period before AMTBIA87. Second, I use a minimum distance procedure to estimate the parameters governing a time series process for effective tax rates, and show in simulations 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 tax rate process would only push placebo-in-time estimates towards finding an avoidance response to AMTBIA87, strengthening the argument that firms did not avoid AMTBIA87. Fourth, I run distributed lag regressions of changes in effective tax rates on changes in book tax differences that show the relationship between the two does not change around the implementation of AMTBIA87.

Other early research finds evidence of significant firm responses to AMTBIA87 using discretionary accruals as a proxy for earnings management (Manzon, 1992; Boynton, Dobbins and Plesko, 1992; Wang, 1994). While my null earnings management results hold across alternative specifications, treatment definitions, outcome constructions and across subsamples, two possible explanations for the differences between my estimates and previous work remain. First, discretionary accrual proxies may identify earnings management where none occurred because of bias introduced by using residuals as dependent variables or the influence of peer firm choices (Chen, Hribar and Melessa, 2018; Christodoulou, Ma and Vasnev, 2018; Jackson, 2018). Second, Manzon (1992), Boynton, Dobbins and Plesko (1992), and Wang (1994) all use ex-post treatment definitions that select firms paying AMT in administrative tax data or listing an AMT payment in their financial statement tax footnote after AMTBIA87 was implemented. These treatment definitions exclude firms that avoid the tax altogether, and I am unable to replicate them because the necessary data is not available.[4]

This paper makes five contributions to the literature on AMTBIA87 to bolster our understanding of how firms respond to book income AMTs. First, this paper delivers estimates

[4]Ex-post treatment definitions rely on administrative tax data or financial statement tax footnote mentions of minimum tax payment, but I have been unable to access either resource because administrative tax data access is restricted and the tax footnotes come from a time period before SEC EDGAR.

of tax base responses to AMTBIA87 correcting for mean reversion, and shows previous estimates of tax base responses were too large. Second, this paper develops a model explaining the lack of avoidance responses and develops corroborating empirical evidence that only firms with fewer incentives to report high book incomes appear to avoid the tax. Third, this paper confirms that the null avoidance estimates hold when focusing on earnings management rather than the entire tax base. Fourth, this paper estimates production and investment responses to AMTBIA87. And finally, this paper develops revenue scores of a contemporary book income AMT proposal to understand the implications of new and old estimates of firm responses for tax revenue.

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 taxes can help limit evasion or avoidance.

Finally, economists and tax policymakers often use tax base elasticities to assess the deadweight loss from taxation (Feldstein, 1999; Chetty, 2009; Dharmapala, 2020). Modern estimates of the elasticity of corporate taxable income span from 0.13-0.2 (Gruber and Rauh, 2007; Dwenger and Steiner, 2012; Devereux, Liu and Loretz, 2014), within the confidence intervals I estimate in this paper. Therefore, while my elasticity estimates do not clearly distinguish whether taxing book income or taxable income is less distortionary, they do suggest that book income taxes should not be discarded from the policy toolkit because they are more distortionary.

2 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.[5]

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 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 $BI > 3TI$.

[5]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.

Effective tax rates distinguish between firms that do and do not face AMTBIA87. Defining effective tax rates as the ratio of current tax expense to pre-tax 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 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 $< 15\%$. This 23% cutoff is used as the treatment definition in Dhaliwal and Wang (1992). 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, while I use the 23% ETR cutoff as my primary treatment definition to conservatively exclude any firm from the control group that may pay minimum tax, I also show my results are robust to treatment definitions based on different ETR cutoffs corresponding to $f = 0, 0.39$ and 1.

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,

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

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).[6] Finally, both AMTBIA87 and ACEA90 generated minimum tax credits that could be used to reduce normal tax down to minimum tax in future years.[7]

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 tax a similarly broad base constructed from tax principles. 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 rate on book tax differences. Starting in 1990, the replacement of AMTBIA87 with ACEA90 increased the marginal tax rate to 15%.

3 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 book income that are incorporated in the United States and appear in the data every year from 1981 to 1992. These restrictions identify firms with positive revenues that are not missing key outcome variables and the variables necessary to construct the

[6]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.

[7]From 1987-1989 AMT credits were 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. Starting in 1990, the Omnibus Budget Reconciliation Act of 1989 changed the law so that AMT credits were awarded for income and expense items causing both temporary and permanent differences.

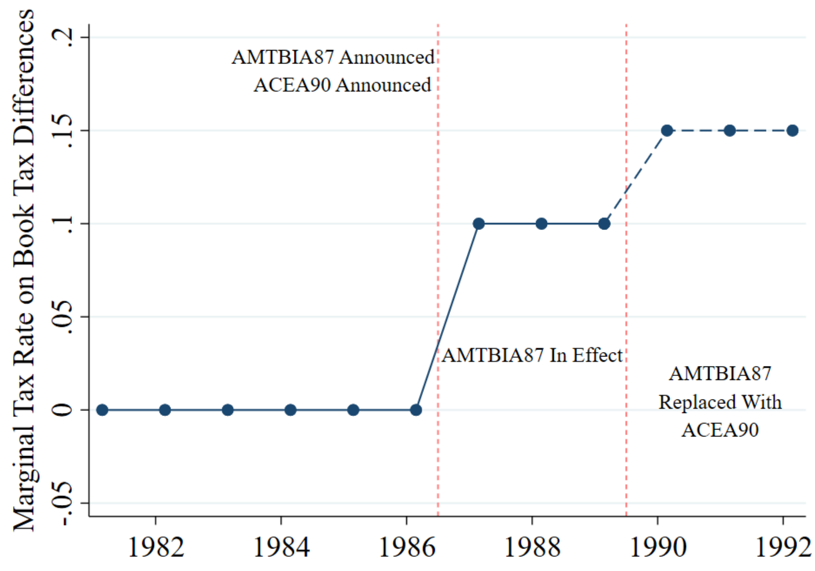


Figure 1: Marginal Tax Rates on Book Tax Differences

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.

treatment definition. I end the baseline panel in 1992 because the Omnibus Budget Reconciliation Act of 1993 changed the ACEA90 tax base.[8] 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.

The 1981-1992 panel includes 845 firms and 10140 firm years. These firms account for 11% of all firms in Compustat in 1985, but 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, and constitute a broader sample than those explored in previous work on AMTBIA87 (Gramlich, 1991; Dhaliwal and Wang, 1992; Manzon, 1992; Boynton, Dobbins and Plesko, 1992; Wang, 1994). To contextualize my data in the literature, I list the sample choices of other papers exploring AMTBIA87 in Table D.1. Other papers

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

use sample sizes ranging from $N = 56$ to $N = 414$.[9]

Table 1 provides summary statistics for a 1985 cross section of the 1981-1992 balanced panel, with all variables rescaled into 2018 dollars.[10] 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.

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).

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 current tax expense divided by pretax book income. Following Collins and Shackleford (2004), I define current tax expense as total income taxes minus deferred income taxes minus other taxes.[11]

[9] To expand my sampling frame even further, I also develop unbalanced panels restricting to firms with positive and non-missing assets, sales, and pretax income, and requiring only that specific outcomes of interest are non-missing.

[10] 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.

[11] Results are similar when subtracting state tax expense.

I measure firm tax base responses to AMTBIA87 using book tax differences, the difference between pretax 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.[12] 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 current tax expense by the statutory tax rate (Manzon and Plesko, 2002). The difference between pretax book income and this construction of taxable income provides a key measure of the tax base, but this measure also faces two limitations. First, the binned scatter plot in Figure 2 shows that book tax differences are mechanically related to the effective tax rate.[13] 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 current tax expense by the statutory tax rate will overstate taxable income and understate book tax differences when part of firm's current tax expense 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.[14]

This potential bias calls for the development of an alternative outcome measuring firm

[12]Estimates of temporary book tax differences can be constructed as deferred tax expense divided by the statutory 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).

[13]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.

[14]An additional issue inherent in using financial statement data to measure tax quantities is that the the level of aggregation among affiliated entities differs between consolidated group tax returns and financial statements. Manzon and Plesko (2002) provide additional details.

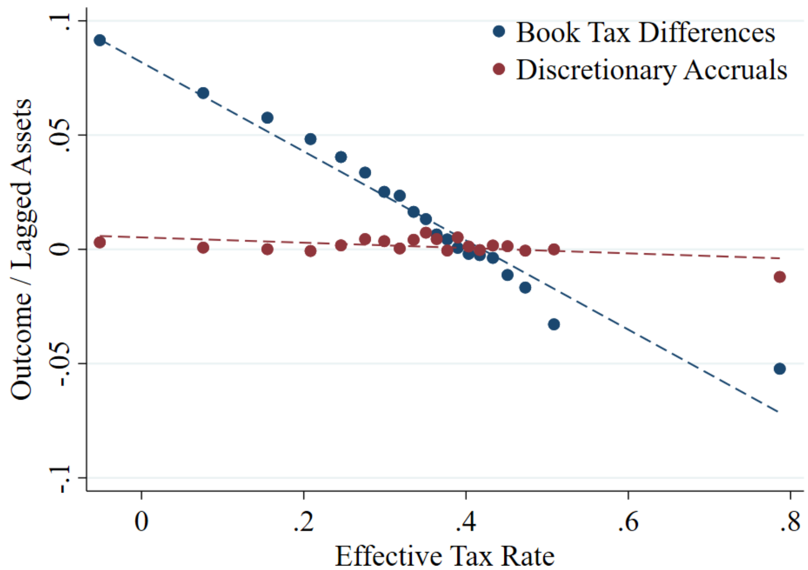


Figure 2: Relationships Between Avoidance Outcomes and Effective Tax Rates

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.

avoidance responses to AMTBIA87 that does not suffer from measurement error in taxable income. Following existing literature (Healy, 1985; Jones, 1991; Boynton, Dobbins and Plesko, 1992; Wang, 1994; Dechow, Sloan and Sweeney, 1995), I use discretionary accruals, a common proxy for earnings management, as a second measure of firm's avoidance responses to AMTBIA87. Discretionary accruals focus on income that managers have a great deal of discretion to manipulate (Bergstresser and Philippon, 2006), measuring the components of earnings not explained by cash flows and not predicted by economic conditions by residualizing accruals on revenues and capital stocks. I closely follow Dechow, Sloan and Sweeney (1995) to construct discretionary accruals, and describe this procedure in Appendix A.

While discretionary accruals are not mechanically related to effective tax rates (see Figure 2) and do not rely on dividing current tax expense by the statutory tax rate to measure taxable income, they also suffer from some issues. Discretionary accruals can identify earnings management where none occurred due to the influence of peer firms (Jackson, 2018) and can lead to biased event study estimates stemming from the use of residuals as dependent variables in regressions (Chen, Hribar and Melessa, 2018; Christodoulou, Ma and Vasnev, 2018). In addition, they may miss some firm responses to AMTBIA87 because they do not capture the entire tax base. Therefore, I use the book tax differences tax base as my primary measure of aggregate firm responses to AMTBIA87, and view discretionary accrual results as complementary robustness checks. 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.[15] 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 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.[16] Among sample firms present in the Execucomp data, the average share of compensation that is incentive-based is 17.7%.

[15] 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.

[16] 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.

4 Tax Base Responses

To study whether firms avoid AMTBIA87, I use an event study framework to compare the responses of treatment firms with average effective tax rates (ETRs) over 1984-1986 < 23% to control firms with average ETRs ≥ 23%. I choose the 23% cutoff, derived in section 2 and following Dhaliwal and Wang (1992), to conservatively exclude any firm from the control group that could face taxes from AMTBIA87. I average ETRs over three years to capture firms with persistently low ETRs, rather than firms with idiosyncratic one-time events that lead to low ETRs.[17]

Formally, I estimate

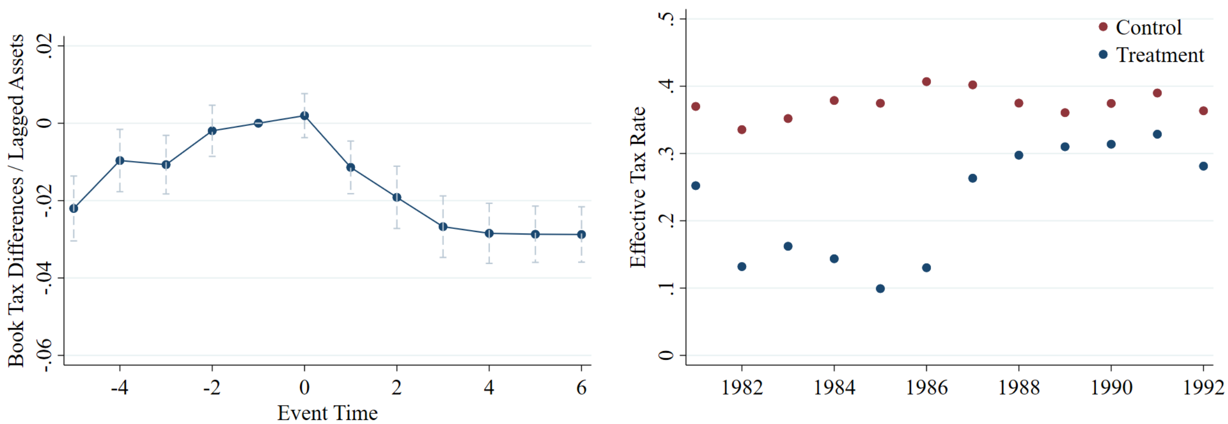
$$Y_{ie} = \sum_{e=-5, e\neq -1}^{6} \Big(\beta_e \cdot Treat_{ie}\Big) + \rho X_{ie} + \delta_e + \gamma_i + \varepsilon_{ie}, \tag{3}$$

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.

Using book tax differences as an outcome and the balanced 1981-1992 sample, I plot estimates of β_e from equation (3) 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 effective tax rates (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. Treatment firm's ETRs are low in the 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

[17] Appendix Figure D.12 shows that I obtain similar results when using a single year treatment definition.



(a) Event Study

(b) Average Effective Tax Rates

Figure 3: Tax Base 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.

challenge to overcome to measure aggregate tax base responses 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, 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 aggregate tax base 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 average over many placebo treatment definitions rather than only using the most recent pre-reform treatment definition because using the 1983-1985 treatment definition would only yield one year of counterfactual firm responses to treatment definitions in the absence of the policy.

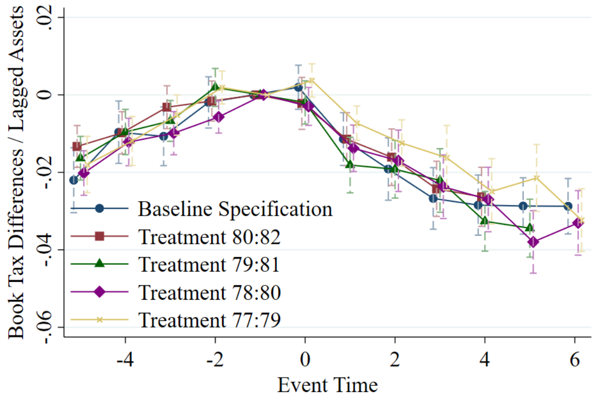
I use a stacked event study specification to estimate average firm responses to treatment definitions in years before the policy. To construct the sample for this regression, I take the 1981-1992 balanced panel, define $Treat_{id}$ as a dummy = 1 if average ETR over three year period $d \in D < 23\%$,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

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

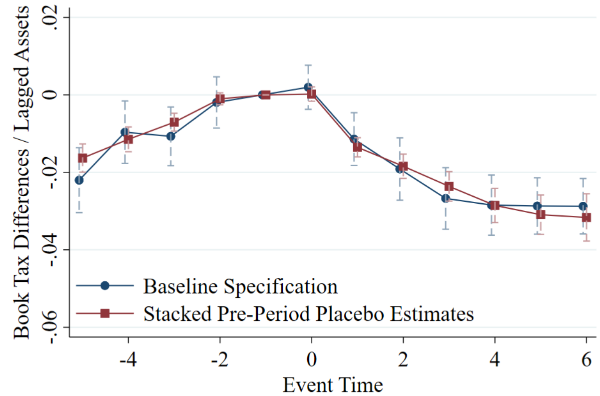
on the stacked data, using a large set of placebo treatments $D = \{(77-79), (78-80), (79-81), (80-82), (81-83), (82-84), (83-85)\}$ and restricting to years before 1987 to avoid any bias from the implementation of AMTBIA87. Without this restriction, the counterfactual would include 1987 responses to the 1983-1985 treatment definition in the placebo estimates, violating the requirement that the placebo estimates capture firm responses to treatment definitions *in the absence* of the policy.[18] 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 earlier years. 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

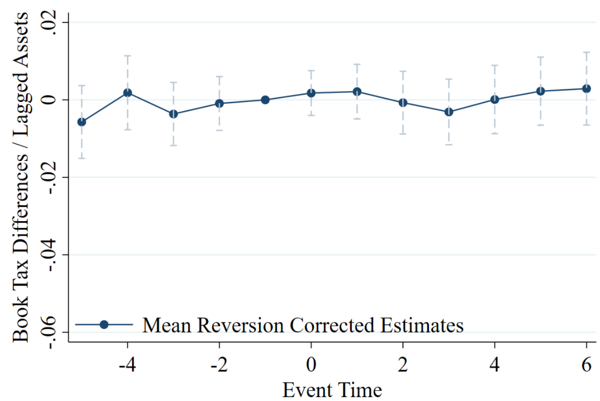
[18] In Appendix Figure B.1, I show results are robust to only using placebo treatments closer to policy implementation. In Appendix Figure D.4, panel (a), I show results are robust to including post-1986 years in the estimation of η_e. Both robustness checks suggest the time series process for effective tax rates and its impact on book tax differences is stable over a range of years around the implementation of AMTBIA87. I present more evidence confirming this intuition in Appendix B.



(a) Year-by-Year Placebos



(b) Averaged Placebos



(c) Mean Reversion Corrected Estimates

Figure 4: Placebo-in-Time Tax Base Estimates

Notes: This figure plots placebo-in-time estimates of tax base 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 nonparametrically bootstrapped standard errors using 300 iterations. All standard errors are clustered at the firm level.

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 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.

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.[19]

I validate the placebo-in-time approach in four ways, each of which I discuss thoroughly in Appendix B. First, I show visual evidence that the pattern of book tax difference responses to treatment definitions is stable over different sets of years before 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. The estimated parameters appear stable over time, and applying them in simulations, I show that significant deviations 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

[19]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.

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. Fourth, I estimate distributed lag regressions of changes in BTDs on changes in ETRs that indicate the influence of ETR mean reversion on BTDs does not change around the implementation of AMTBIA87. Taken together, this evidence suggests my placebo-in-time approach provides valid estimates of tax base responses to ATMBIA87.

4.1 Economic Magnitudes

Taking the baseline and stacked event study estimates from equations (3) and (4), I scale my estimates of book tax difference responses to AMTBIA87 into elasticities of book income with respect to the net of tax rate following,

$$\varepsilon_e^{BI} = \Big(\frac{\beta_e}{\overline{BI}_\beta} - \frac{\eta_e}{\overline{BI}_\eta}\Big) \cdot \frac{1-\tau}{\Delta(1-\tau)_e}, \tag{5}$$

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. Rather than calculate elasticities in each year, I focus on three year average elasticities over 1987-1989 while AMTBIA87 was implemented and over 1990-1992 when AMTBIA87 was replaced by ACEA90. To obtain three year averages I replace the post-reform event time dummies and their interactions in equations (3) and (4) with indicators for the first three and fourth through sixth years after treatment definitions and their interactions with the treatment dummy.

Rescaling book tax differences estimates, I obtain an elasticity estimate from 1987-1989 of -0.03 with a 95% confidence interval of -0.63 to 0.56. These estimates reject declines in the tax base of more than -0.81% of lagged assets, or -0.08% of lagged assets per 1% change in the tax rate, both significantly smaller than the standard deviation of BTD in the entire sample, which is 4% of lagged assets. I obtain a similar elasticity estimate of -0.24 with a 95% confidence interval of -0.67 to 0.19 from 1990-1992.

4.2 Explaining the Lack of Avoidance Responses

On average, why might firms not exhibit large tax base responses to AMTBIA87? One simple explanation is that firms have a strong preference to report high book incomes to investors. To rationalize the lack of avoidance responses to AMTBIA87, I specify a static, partial equilibrium model of firm earnings manipulation decisions in the presence of corporate taxes, in which firms face incentives to maximize after-tax profits and stock prices.[20]

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 (with $\mu_b < \mu_c$) are deductible for book purposes so book income is $y - \mu_b c(y)$. Firms can reduce 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. The firm solves

$$\max_{y,\hat{c}_t,\hat{c}_b} \; y - c(y) - T(y, \hat{c}_t, \hat{c}_b) - g(\hat{c}_t - c(y)) - h(\hat{c}_b - c(y)) + s(\hat{c}_b - c(y)). \tag{6}$$

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, and tax 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

[20] 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 (6). 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 $\tau_{E,t} \equiv \tau_t \frac{1-\mu_t}{1-\tau_t\mu_t}$.

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 $s'() < 0$ to $s'() + \tau_b\mu_b$ which can be less negative or positive, 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 5 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 $\hat{c}_b' - c(y')$.[21] 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.

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

[21] One concern with taxes on book income is that attempts to avoid the tax will distort the information content of earnings, leading to negative consequences for investors (Hanlon, Laplant and Shevlin, 2005). 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.

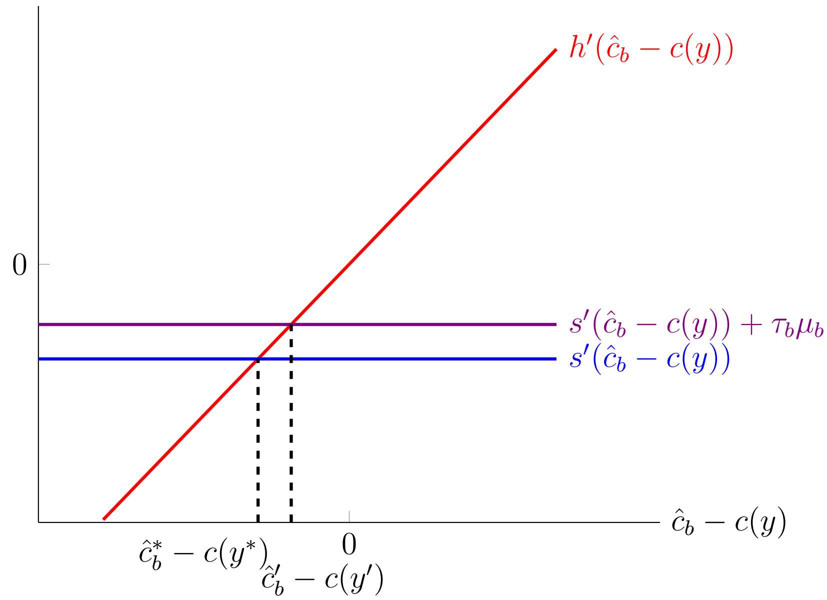


Figure 5: Marginal Firm Decisions

Notes: This figure plots book misreporting cost and stock benefit functions from equation (6) 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 $\tau_b \mu_b$.

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.

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. To test for more avoidance behavior among firms with weaker incentives to report high book incomes, I reestimate tax base book income elasticities focusing on firms with less incentive-based compensation (Bergstresser and Philippon, 2006) and firms taking "big baths" that are missing earnings benchmarks by large margins. To focus on firms with less incentive-based compensation, I restrict to firms present in Execucomp, but exclude firms with managers whose compensation is more than

20% incentive-based in 1992 to eliminate firms where managers face the strongest incentives to report high earnings. To focus on firms taking big baths, I restrict to firms where the difference between 1987 and 1986 book income is less than -0.5% of assets.

I display baseline and restricted sample tax base elasticity estimates for the 1987-1989 period while AMTBIA87 is in effect in Table 3. Unfortunately, these subsample analyses have less power and some of 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.[22]

Table 3: Tax Base Elasticities Varying Incentives to Report High Book Income

	(1) Baseline	(2) Execucomp	(3) Low Incentive-Based Comp	(4) Big Bath
ε^{BI}	-0.03	0.24	0.77	0.90
	(0.30)	(0.43)	(0.60)	(0.46)
Base Observations	10140	5148	3192	3744
Base Clusters	845	429	266	312
Placebo Observations	66638	33098	20076	37128
Placebo Clusters	1237	552	330	704

Notes: This table reports elasticity estimates varying firm incentives to report high book incomes. All elasticity estimates are constructed by rescaling placebo-in-time estimates of book tax difference responses to AMTBIA87 following equation (5). The baseline estimates in Column 1 use all available data from the 1981-1992 balanced panel, and both the 1981-1992 and 1974-1986 balanced panels to construct placebos as described in the text. The Execucomp estimates in column 2 restrict to only firms present in Execucomp. The low incentive-based compensation estimates in column 3 restrict to firms present in Execucomp with managers whose compensation is $\leq$ 20% incentive-based in 1992, and the big bath estimates in column 4 restrict to firms where the difference between their 1987 and 1986 book income is $<$ 0.5% of assets. The base observation and cluster numbers correspond to the the baseline 1981-1992 sample, while the placebo observation and cluster numbers correspond to the stacked data used in the pre-period placebo estimation described in equation (4). Elasticity standard errors are reported in parantheses and clustered at the firm level, calculated via bootstrap with 300 iterations.

However, in contrast to the baseline estimates, the big bath tax base elasticity estimate is 0.90 with a 95% confidence interval spanning [-0.004,1.805], while the lower incentive-based compensation elasticity is 0.77 with a 95% confidence interval spanning [-0.41,1.95].

[22]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.

Both estimates provide suggestive evidence that firms with weaker incentives to report high book incomes to their investors shrink the tax base to avoid AMTBIA87. While the lower incentive-based compensation elasticity estimate is not close to rejecting zero, it is not driven by the specific measure of incentive-based compensation, or the specific cutoff fraction I use to denote low incentive-based compensation. Appendix Figure D.1 plots point estimates for tax base elasticities using 1992 executive compensation and an average of 1992-1994 executive compensation across a wide range of cutoff fractions. The point estimates are consistently positive and close to 1. Furthermore, the higher point estimates are not explained by restricting to the firms present in Execucomp but not cutting by incentive-based compensation. Elasticity estimates in Table 3 using the sample of firms present in Execucomp have lower point estimates.

4.3 Robustness

I include a host of robustness checks in the appendix. One concern with the baseline null results is that using treatment firms with average ETRs below 23% from 1984-1986 may not accurately identify firms facing AMTBIA87. This could occur for two reasons. First, the effective tax rate below which firms must pay minimum tax depends on tax preferences and adjustments which I do not directly observe in the data. The 23% cutoff conservatively assumes that all of book tax differences are tax preferences and adjustments, ensuring that no firm in the control group whose ratio of current 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.2 plots mean reversion corrected estimates of tax base responses to AMTBIA87 analogous to Figure

4, panel (c), but varying treatment definitions. Results are not sensitive to alternative treatment definitions. The vast majority of the post-AMTBIA87 point estimates cannot reject zero response to any of the treatment definitions.

Second, the mean reversion patterns documented above suggest that low effective tax rate treatment firms will end up with higher effective tax rates in the years after the treatment definition (in the absence of the policy), and therefore may not be subject to the minimum tax. To test whether the minimum tax increases the tax expense of treatment firms in excess of the increase we would expect from mean reversion stemming from the treatment definition, I plot mean reversion corrected estimates using current tax expense as an outcome in Appendix Figure D.3.[23] Panel (a) plots estimates using the whole sample, suggesting that firms facing AMTBIA87 saw their taxes increase modestly by an average of 0.29% of lagged assets over 1987-1989.

Previous research has highlighted the use of foreign tax credits and net operating losses as tax shields that could reduce tax owed due to AMTBIA87 (Boynton, Dobbins and Plesko, 1992; Manzon, 1992). Appendix Figure D.3, panel (b) plots mean reversion corrected current tax expense estimates dropping multinational firms (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). Current tax expense rose by an average of 0.67% of lagged assets over 1987-1989 in this restricted sample. In summary, treatment firms face tax increases from AMTBIA87 over and above those we would expect due to mean reversion, and the tax increases are larger among firms with fewer tax shields, suggesting the treatment definition identifies firms facing increased taxes from AMTBIA87.[24]

I also show that the baseline null results are robust to different outcome constructions, different estimation samples, the inclusion of time-varying controls, and industry or size-specific time trends. Appendix Figure D.4 plots tax base elasticity estimates at event times

[23]The mean reversion correction is necessary because current tax expense is mechanically related to effective tax rates, the ratio of current tax expense to book income.

[24]Some of the prior research on AMTBIA87 summarized in Appendix Table D.1 tries to identify firms facing AMTBIA87 by looking for mentions of minimum taxes in firm financial statement tax footnotes in 1987 (Manzon, 1992; Wang, 1994), or verifying payment of minimum tax in tax records (Boynton, Dobbins and Plesko, 1992). I have been unable to access financial statement tax footnotes for the relevant period in a systematic way because it predates SEC EDGAR, and am unable to perform the latter check without tax data.

0, 1-3 and 4-6 in panel (a). The figure includes the baseline estimates with confidence intervals, as well as point estimates from a variety of alternative specifications.

I obtain similar null elasticity estimates when dropping time-varying controls for depreciation and depletion and when including 1985 asset tercile or two digit SIC industry time trends.[25] I also obtain similar null elasticity estimates when dropping finance and utility firms from the sample, when restricting to firms with fiscal year-ends in December to eliminate firms that may not face AMTBIA87 on all of their 1987 income, and when dropping multinational and loss firms that can use foreign tax credits and losses as tax shields to limit the impact of AMTBIA87. Even among firms facing larger tax increases from AMTBIA87 (see Appendix Figure D.3, panel (b)), I still find no evidence of tax base responses.

Furthermore, I find little heterogeneity in avoidance responses across firm sizes or industries. Appendix Figure D.5, panels (a) and (b) display tax base responses to AMTBIA87 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. The combination of the robustness of the null results to industry and size-specific time trends and the lack of heterogeneity across industry and firms sizes 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.

I find similar null elasticity estimates under an alternative construction of taxable income accounting for state taxes. I construct the taxable income component of book tax differences by dividing current tax expense (total income taxes minus deferred income taxes minus other taxes) by the statutory tax rate. One concern with this measure is that it can include state tax expense, but I show in Appendix Figure D.4, panel (a) that I obtain similar results when subtracting state taxes from current tax expense, suggesting measurement error in federal tax expense is not driving my results.

My tax base results do not appear to be driven by the specific choice of placebo treatment used to estimate equation (4). Appendix Figure B.1 shows placebo-in-time estimates would be very similar using only placebo treatment definitions spanning 1977-1981 or 1980-1984,

[25] 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.

while Appendix Figure D.4, panel (a) shows tax base elasticity estimates do not change when including post-1986 years when estimating counterfactual firm responses to placebo treatment definitions.

The baseline null results also do not appear to be driven by AMT credits from AMTBIA87 that reduce future taxes. From 1987-1989 firms received 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.6 plots placebo-in-time estimates using permanent BTDs and finds no evidence of firms shrinking their permanent BTDs to avoid AMTBIA87.

Furthermore, the results hold in even broader samples than the baseline balanced panels I use in most of my analysis. Appendix Figure D.7, panel (a) plots placebo-in-time estimates of tax base responses using larger unbalanced panels. The data underlying the figure includes all firms with positive and non-missing sales, assets and pretax income and the variables necessary to construct book tax differences and treatment status, resulting in 14078 firm years and 1237 unique firms in the 1981-1992 book tax differences panel. There is no evidence of avoidance behavior when using this broader sample.

I also find that firms do not appear to respond to the transition from AMTBIA87 to ACEA90. While all of the above analysis suggests no avoidance responses, it is based on 1984-1986 treatment definitions. 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.8, panel (a) plots tax base responses to the transition, repeating the exercise in Figure 4, panel (a) and plotting 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. I find no avoidance responses, suggesting firms did not respond to the transition to ACEA90, and alleviating 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 variation in the minimum tax rate.

5 Earnings Management Responses

As discussed in section 3, the book tax difference results above may be biased towards finding avoidance responses to AMTBIA87 due to the construction of taxable income from current tax expense. While this potential bias only strengthens the argument that, on average, firms did not avoid AMTBIA87, it also calls for additional robustness checks. To explore firm responses to AMTBIA87 using an outcome not impacted by the construction of taxable income, I follow a broad literature in economics, finance and accounting, as well as previous work on AMTBIA87 that does not focus on the tax base (Boynton, Dobbins and Plesko, 1992; Wang, 1994), and use discretionary accruals (DAs) as a proxy for earnings management. 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 (see Appendix A), assuming that deviations from predicted accruals are evidence of accounting manipulation.

While DAs do not suffer from bias stemming from my measure of taxable income, they also come with significant problems that are well documented in the accounting literature. Jackson (2018) shows that firms can end up with large DAs without taking any action because of peer firm choices. Chen, Hribar and Melessa (2018) and Christodoulou, Ma and Vasnev (2018) show that regression estimates using DAs as an outcome can be attenuated, biased, and wrong-signed. Furthermore, DAs do not capture the entire tax base and therefore may miss some firm responses to AMTBIA87. For these reasons, I present discretionary accrual estimates as a complementary robustness check to the tax base estimates above, but only use them to build confidence in the baseline results.

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 framework in equation (3) to estimate DA responses to AMTBIA87. Appendix Figure D.9 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) using discretionary accruals as an outcome in Figure 6. In line with my estimates of aggregate tax base responses, I also find zero earnings management responses to ATMBIA87.

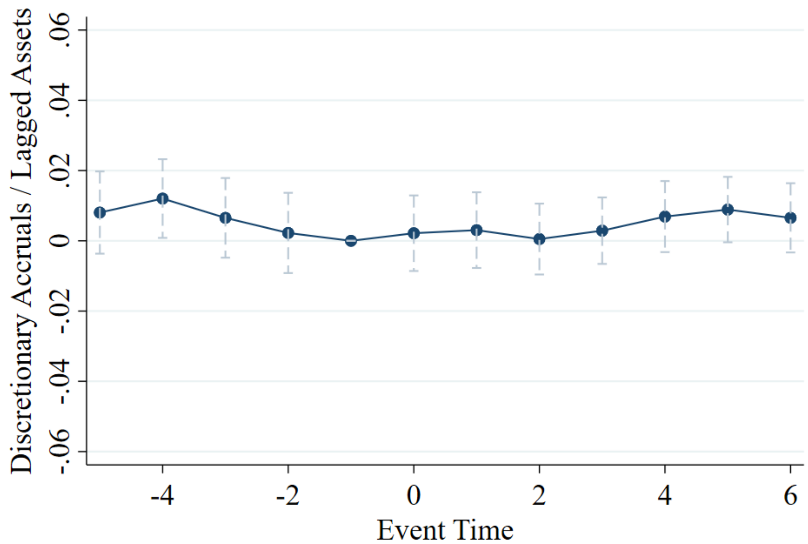


Figure 6: Earnings Management Event Study Estimates

Notes: This figure plots estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs and using discretionary accruals as the outcome. Confidence intervals are calculated from standard errors clustered at the firm level.

Following equation (5), but dropping the η_e correction for mean reversion, I rescale earnings management event study estimates into elasticities of book income and find similar results to the tax base estimates above. I estimate an elasticity of -0.19 over 1987-1989 with a 95% confidence interval from -0.87 to 0.50. 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 DAs 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. From 1990-1992, I estimate an elasticity of -0.43 with a 95% confidence interval from -0.90 to 0.04.

Similar to the tax base estimates, there is some evidence that the earnings management elasticity estimates are larger for firms with fewer incentives to report high book incomes. Appendix Table D.2 displays baseline and restricted sample earnings management elasticity estimates for the 1987-1989 period while AMTBIA87 is in effect. The low incentive-based compensation earnings management elasticity point estimate is 0.92, suggesting that firms with managers lacking incentives to keep earnings high acted to avoid AMTBIA87, although neither the incentive-based compensation or big bath estimates reject 0.

The earnings management estimates are also robust to a similar set of checks as those discussed above for the tax base estimates. Appendix Figure D.10 plots event study estimates of earnings management responses to AMTBIA87 analogous to Figure 6, but varying the effective tax rate cutoffs used for treatment definitions. The results are not sensitive to the choice of treatment definition.

There is little evidence of earnings management response heterogeneity across firm sizes or industries. Appendix Figure D.5, panels (c) and (d) display earnings management responses to AMTBIA87 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.

I also obtain null earnings management results under different constructions of discretionary accruals. Discretionary accruals are the components of total accruals not predicted by changes in sales and gross PPE (see Appendix A for details), and 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, as in Bergstresser and Philippon (2006). However, Boynton, Dobbins and Plesko (1992) include changes in taxes payable in their total accrual definition. I show in Appendix Figure D.4, panel (b) that I obtain similar results when including changes in taxes payable in total accruals. Previous earnings management research also explores modified Jones models to account for managerial discretion over credit sales by predicting accruals using only cash sales changes and not credit sales changes (Dechow, Sloan and Sweeney, 1995; Bergstresser and Philippon, 2006; Yu, 2008), but I also obtain similar results using a modified Jones model to construct discretionary accruals.

Furthermore, the earnings management results hold in even broader samples than the baseline balanced panels I use in most of my analysis. Appendix Figure D.7, panel (b) plots event study estimates of earnings management responses using larger unbalanced panels. The data underlying the figure includes all firms with positive and non-missing sales, assets and pretax income and the variables necessary to construct discretionary accruals and treatment status, resulting in 12077 firm years and 1125 unique firms. There is no evidence of avoidance behavior in this broader sample.

Finally, Appendix Figure D.8, panel (b) plots earnings management responses to the transition from AMTBIA87 to ACEA90, plotting estimates of β_e from equation (3), and using a treatment definition over 1987-1989 with an average effective tax rate cutoff of 17%. I find no evidence of earnings management responses to the transition.

Despite the potential issues with using discretionary accruals as an outcome, the consistency of the discretionary accrual results with the tax base results builds confidence that firms did not have large avoidance responses to AMTBIA87.

6 Reconciling with Previous Research

6.1 Summarizing Previous Research

Previous research explores firm tax base or earnings management responses to AMTBIA87 using a variety of samples, outcome variables, and methods to identify firms paying minimum tax (Gramlich, 1991; Dhaliwal and Wang, 1992; Boynton, Dobbins and Plesko, 1992; Manzon, 1992; Wang, 1994). I summarize each study's approach in Appendix Table D.1.

These previous studies have three key differences. First, they use different samples, all smaller than the sample of firms used in this paper. The largest sample has 414 firms (Boynton, Dobbins and Plesko, 1992), while the smallest sample has 56 firms (Wang, 1994). Where possible to verify, it appears that the smaller samples used in previous work are not necessarily representative of firms in the broader data used in this paper. For example, firms in the Boynton, Dobbins and Plesko (1992) sample average \$2.5 billion in 1987 assets and book income equal to 9.4% of assets, and firms in the Manzon (1992) sample average book income equal to 7% of assets. Meanwhile, in 1987, firms in the 1981-1992 balanced panel in this paper average $1.9 billion in assets (in 1987 dollars) and book income equal to 13.8% of lagged assets. These sample selection differences could drive different results if, for example, firms with less book income as a fraction of assets avoided AMTBIA87 more.

Second, previous papers choose different outcome variables to measure firm responses. Dhaliwal and Wang (1992) explore tax base responses, while the other studies explore various accrual measures, some of which account for changes in economic conditions as the discretionary accrual measure in this paper does.

Third, previous studies use different methods to identify firms paying minimum tax, relying on pre-reform tax status (Gramlich, 1991; Dhaliwal and Wang, 1992), post-reform mention of AMT in financial statement tax footnotes (Manzon, 1992; Wang, 1994), or post-reform payment of AMT in administrative tax data (Boynton, Dobbins and Plesko, 1992). The first type of treatment definition identifies firms before AMTBIA87 that are likely to face the tax. The latter two treatment definitions identify firms that pay the minimum tax, but exclude firms that manipulate earnings to avoid the tax altogether. Unfortunately, I cannot pursue either of the latter two treatment definitions because of data limitations[26], but I verify in Appendix Figure D.3 that my treatment definition identifies firms facing increasing taxes, and using an ex-ante treatment definition avoids excluding firms avoiding the tax altogether.

Despite these myriad different choices, four of the five previous papers find evidence that firms report increased earnings in 1986, and all five find evidence of firms reporting lower earnings after 1986. Dharmapala (2020) rescales tax base point estimates from Dhaliwal and Wang (1992) and finds they imply an elasticity of 1.7 and rescales discretionary depreciation point estimates from Manzon (1992) and finds they imply a range of elasticity estimates from 1.4 - 2.1.[27]

[26]I do not have access to the administrative tax data for this project, and have been unable to systematically analyze financial statement tax footnotes digitally because this period was before SEC EDGAR.

[27]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

Some previous studies also explore heterogeneous responses and find evidence of avoidance among specific subgroups of firms, though the results do not paint a consistent picture. For example, both Boynton, Dobbins and Plesko (1992) and Manzon (1992) find larger avoidance responses to AMTBIA87 among firms with few foreign tax credits and losses that can act as tax shields, but Boynton, Dobbins and Plesko (1992) find more avoidance among smaller firms while Manzon (1992) finds more avoidance among larger firms.

Given the large number of different empirical choices made in prior studies, it is not surprising that the conclusions of this early literature have proven controversial. Choi, Gramlich and Thomas (2001) questions the results of these earlier papers, criticizing their choice of outcome variables, scaling, and treatment definitions, while a rebuttal suggests the original papers address the critique's concerns (Dhaliwal, 2001).

6.2 Explaining Differences with Previous Research

Dhaliwal and Wang (1992) (DW) is the one previous paper that estimates tax base responses to AMTBIA87. I carefully document that differences between the tax base results in this paper and DW stem from correcting for mean reversion, and are not explained by other empirical choices. Figure 7 shows mean reversion corrected tax base elasticity estimates based on equation (5) alongside tax base elasticity estimates without the mean reversion correction subtracting η_e. The uncorrected estimates are almost exactly identical to the Dhaliwal and Wang (1992) estimates.[28]

Readers may be concerned that three differences between the approach in this paper and DW other than the mean reversion correction could explain our different results. First, DW use a smaller sample of 360 firms that excludes finance and utility firms and firms with early fiscal year-ends. Second, DW scale book tax differences by book income instead of assets, and third DW divide firms into treatment and control groups based on their 1986 ETRs,

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 point estimates do not lie within the confidence intervals of existing estimates in the literature.

[28]Dhaliwal and Wang (1992) split firms into treatment and control groups using an effective tax rate cutoff of 23% based only on 1986 effective tax rates. They were aware that their treatment definition could induce some mean reversion and attempted to control for it in section 4 of their paper. However, the evidence in this paper suggests their adjustments did not properly control for mean reversion.

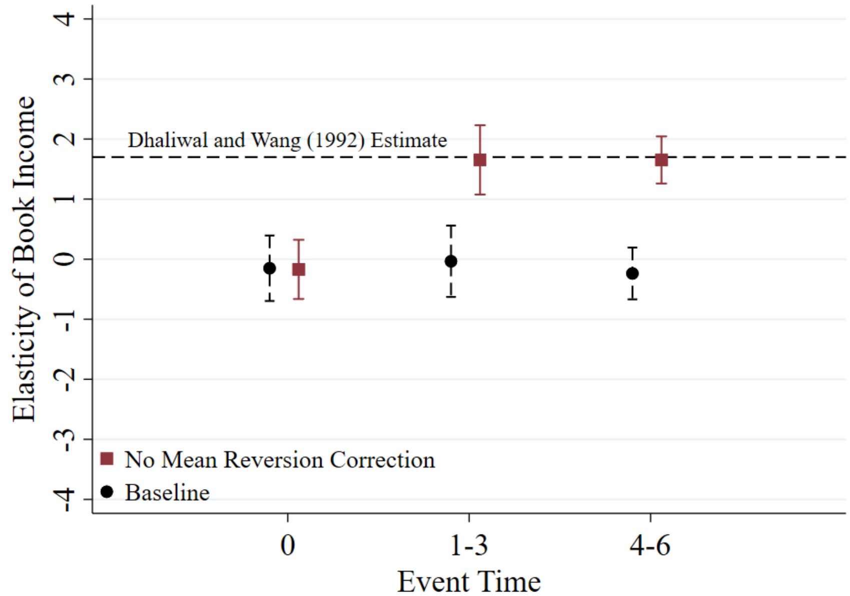


Figure 7: Tax Base Elasticity Estimates With and Without Mean Reversion Correction

Notes: This figure plots elasticities of book income with respect to the net of tax rate constructed from mean reversion corrected tax base estimate following equation (5), with and without the mean reversion correction subtracting η_e. The dashed black line represents the rescaling of Dhaliwal and Wang (1992) tax base estimates from Dharmapala (2020). Standard errors are clustered at the firm level and constructed via 300 bootstrap iterations for the baseline estimates. The no mean reversion correction series standard errors come directly from event study estimates.

rather than averaging over three years. However, none of these differences appear to drive the null results like the mean reversion correction. I continue to find null tax base elasticity estimates when varying the sample, when scaling book tax differences by book income, and when using a single year treatment definition.

Appendix Figures D.2, D.4, and D.5 show that I obtain similar null results across different treatment definitions, across industry and firm size subsamples, for firms with fewer tax shields, using different outcome measures, and specifically excluding finance and utility firms and firms with fiscal year-ends not in December. Appendix Figure D.11 plots estimates of β_e in equation (3) and η_e in equation (4) scaling book tax differences by book income instead of assets. The two series track each other closely.

Using only a single year for the treatment definition appears to capture firms that have idiosyncratically low ETRs (and high BTDs) in 1986 that do not persist over time, and not correcting for the dynamics driven by this treatment definition could lead to the conclusion that firms increased income in 1986 and decreased income after 1986 to avoid AMTBIA87. Appendix Figure D.12, panel (a) plots tax base responses to a single year treatment definition, dividing firms into treatment and control groups based on only their 1986 ETRs (rather than averaging over 1984-1986). The tax base estimates of β_e from equation (3) in panel (a) suggest a spike in book tax differences in 1986 followed by a decline in the post-reform period, in-line with the findings of previous studies that firms shifted income into 1986 and shrunk income in the post-reform period to avoid AMTBIA87. However, the estimates of η_e from equation (4) track these dynamics closely, suggesting they are driven by the treatment definition and do not represent firm responses to the policy.[29]

Other previous papers estimate earnings management responses to AMTBIA87 using different accrual-based proxies for earnings management. Gramlich (1991) uses changes in total accruals, assuming that any changes in accruals represent earnings management. Manzon (1992), Boynton, Dobbins and Plesko (1992) and Wang (1994) improve on this assumption by proxying for earnings management with discretionary accruals[30] that cannot be explained by changes in economic conditions. I follow these later studies by using discretionary accruals to measure earnings management. In contrast to these papers, I find null avoidance responses across treatment definitions, outcome constructions, samples, and industry and size subgroups, and suggestive evidence of earnings management among firms with lower incentives to report high book incomes (see Appendix Figures D.4, D.10, D.12 and Appendix Table D.2). This analysis suggests differences are not driven in particular by sample selection or by the choice of outcome variable.

Two potential explanations for the difference between previous discretionary accrual estimates and the estimates in this paper remain. First, Manzon (1992), Boynton, Dobbins and Plesko (1992) and Wang (1994) all use financial statement tax footnotes or administrative tax data to identify treatment firms ex-post that pay the minimum tax. These ex-post treatment definitions eliminate firms that avoid the tax altogether, and could potentially bias estimates of earnings management upwards if they miss many firms close to the cutoff that

[29] I observe no 1986 spike in BTDs when using a treatment definition averaging over three years of effective tax rates (see Figure 4).

[30] Or discretionary depreciation in the case of Manzon (1992), just using depreciation rather than total accruals as the left hand side variable in a Jones model.

would only have to make very small changes to book income to avoid the tax altogether.[31] Unfortunately, I cannot replicate the tax data treatment definition without access to the administrative data for this project, and I have been unable to replicate the tax footnote measure because the footnotes come from before SEC EDGAR was established.[32]

Second, finding significant discretionary accrual responses to AMTBIA87 does not necessarily provide evidence of earnings management and could stem from doing inference on residuals. Chen, Hribar and Melessa (2018), Christodoulou, Ma and Vasnev (2018) and Jackson (2018) all show that discretionary accrual proxies may identify earnings management where none occurred. Firms may be assigned large discretionary accrual values due to the behavior of other firms in their industries, and using residuals as a dependent variable can lead to substantial bias. In particular, Christodoulou, Ma and Vasnev (2018) show that even in known instances of income-increasing or income-decreasing manipulation from SEC Accounting and Auditing Enforcement Action cases, discretionary accruals often classify firms with residuals of different magnitudes and signs than the known manipulation.

Given the distinct limitations of discretionary accruals and the lack of data to exactly replicate previous study's treatment definitions, it is difficult to rule out the possibility that previous estimates of earnings management responses to AMTBIA87 are spurious. I use discretionary accruals to both relate to previous attempts to measure firm responses to AMTBIA87 and to develop a measure of firm avoidance responses that is not impacted by measurement error in taxable income, but due to limitations of the outcome I only view these results as a robustness check for the clearer tax base results.

Ultimately, data constraints limit my ability to fully reconcile every result in this paper with previous work. Nevertheless, the contribution of this paper is to reexamine firm responses to AMTBIA87 while making transparent and defensible choices to determine my outcome variables, treatment and control groups, and sample.

I use book tax differences to measure the entire tax base subject to AMTBIA87. I divide firms into treatment and control groups based on their average effective tax rate over 1984-

[31]Of course, this bias could also work in the opposite direction if the firms who avoid the tax altogether make very large changes to their book incomes.

[32]I have been unable to access the financial statement tax footnotes systematically despite an extensive search with the help of a data librarian at Princeton.

1986 to target firms with persistently lower tax rates that should face AMTBIA87, and show this treatment definition properly identifies firms facing increased taxes. I choose a balanced panel of Compustat firms with enough non-missing information to construct the outcome variables and assign treatment status, leading to a broader sample than is used in previous work.

Contingent on these outcome variables, treatment and control groups, and sample decisions, I find zero tax base responses to AMTBIA87. I develop a model to formalize the hypothesis that these null responses can be explained by non-tax incentives for firms to report high book incomes. Consistent with the model, I find suggestive evidence of tax base responses only among firms with weaker incentives to report higher book incomes. Furthermore, I show previous estimates of tax base responses to AMTBIA87 are too high, and that I also estimate null earnings management responses to AMTBIA87 when using discretionary accruals as an outcome.

7 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 (Diamond and Mirrlees, 1971). 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 tax expense on outcomes, using expected AMTBIA87 tax expense as an instrument for tax expense and estimating

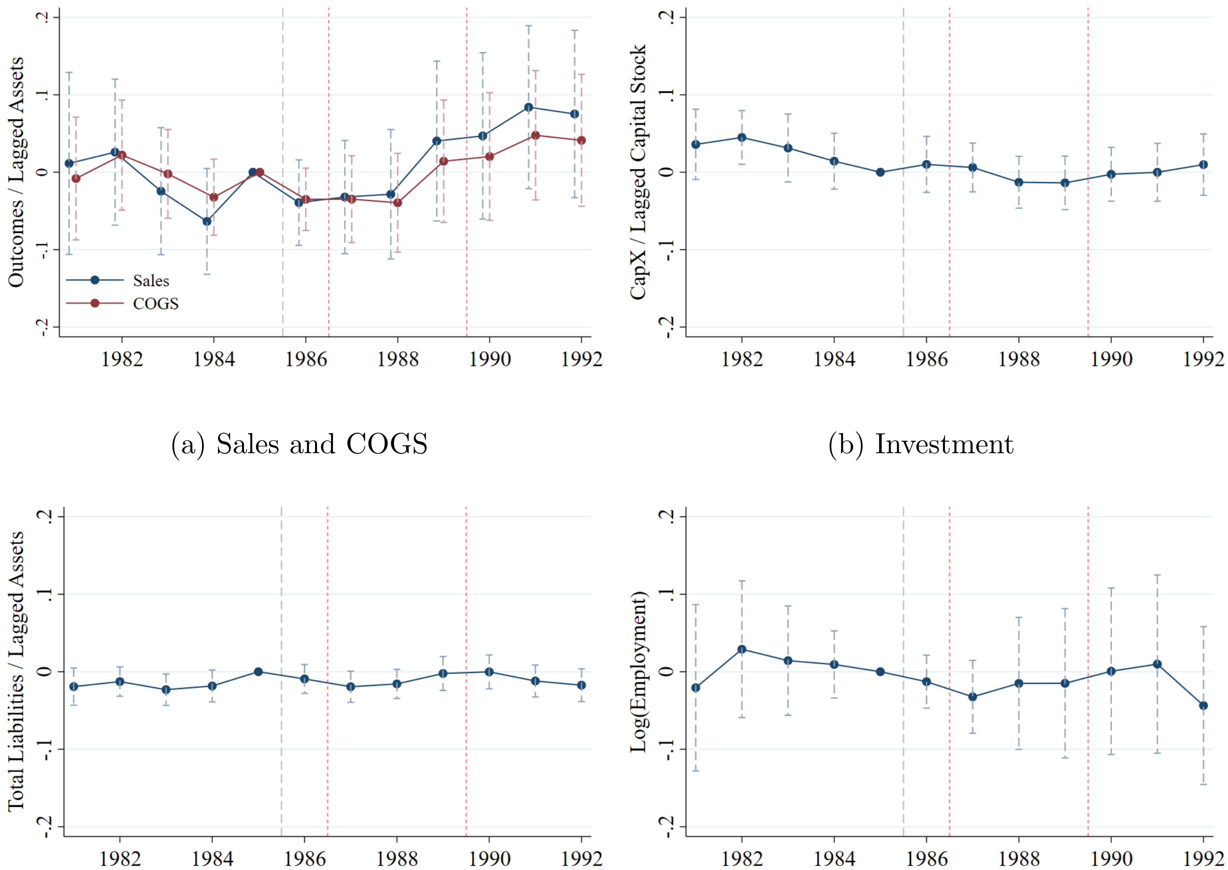
$$Y_{it} = \phi TaxExp_i + \delta_t + \gamma_s + \varepsilon_{it}, \tag{7}$$

where $TaxExp_i$ is a firm's expected tax expense 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 $TaxExp_i$ with $AMTBIA_i$, a firm's expected AMTBIA87 tax expense 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 expense on outcomes under the assumption that expected AMTBIA87 tax expense impacts outcomes only through changes in tax expense. The instrument is relevant because expected AMTBIA87 tax expense is mechanically related to expected tax expense, and unlikely to violate exclusion unless firms respond to AMTBIA87 for reasons unrelated to tax expense changes.[33]

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

[33]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.



(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 3.

addition, 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 4 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 tax expense effect coefficients reject the null hypothesis of

Table 4: 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 (7) 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 (7), 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.

zero except for employment. The estimates in column 1 suggest that for every \$1 million increase in expected AMT tax expense, 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.

So far I have shown evidence that firms do not avoid AMTBIA87 and that the tax does not induce large production or investment distortions. One might be tempted to conclude that these results imply a social planner could set a tax rate on book income very high to raise revenue without distortions. However, this is not necessarily true for two reasons. First, there may be non-linearities in firm responses as a function of the tax rate (Dowd, Landefeld and Moore, 2017). Second, even if aggressively raising the tax rate on book income did not induce avoidance or production and investment distortions, the model in Figure 5 implies another externality. As the tax rate on book income increases, earnings underreporting will become more prevalent. While some level of earnings underreporting should correct existing overreporting, a very high tax rate on book income could lead to widespread earnings underreporting and distort the information content of earnings, leading to inefficient outcomes in investment markets (Hanlon, Laplant and Shevlin, 2005).

8 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).[34] 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 would still be allowed to claim deductions for loss carryforwards and foreign taxes.[35] 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 C.

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 4, assumes zero responses to a book minimum tax. Scenario 2 makes moderate elasticity assumptions that exceed the upper edges of the confidence intervals of the elasticity estimates in section 4. 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 5, 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 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

[34] 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.

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

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 estimated revenue by 18%.

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

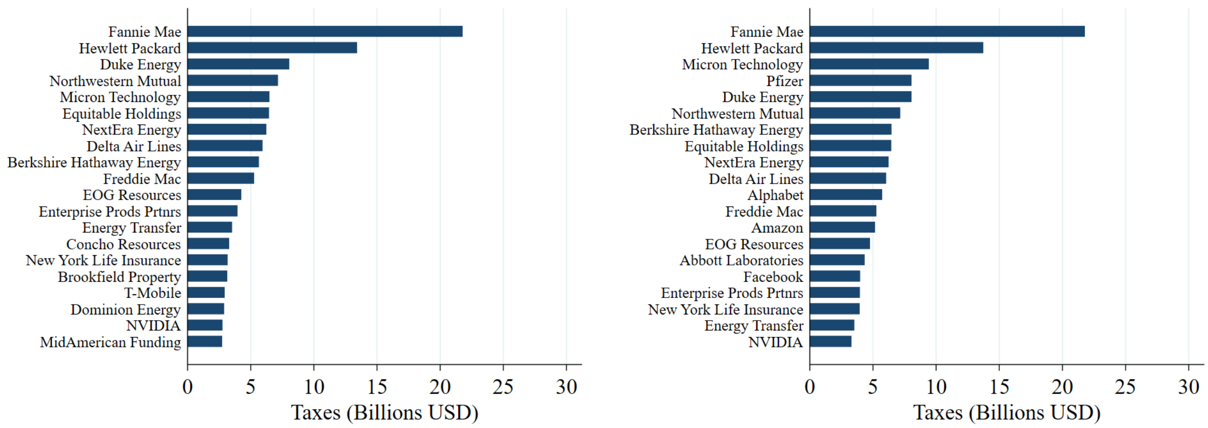
Notes: This table reports ten year revenue scores of the proposed Biden book income AMT across a range of assumptions for ε_t^{BI}, the elasticity of book income with respect to the net of tax rate at time horizon t. Panel (a) displays revenue estimates for the basic policy described in the text, assuming 30% of AMT 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. 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 5 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 increases due to the policy.[36] Columns 3-6 of Table 5 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 increases from the proposed book income AMT by plotting the tax revenue raised in my preferred simulation from the

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

twenty firms facing the largest changes. The firms facing the very largest tax increases include Hewlett Packard, Fannie Mae, Berkshire Hathaway Energy and Delta Airlines.[37] One firm noticeably absent from the top twenty is Amazon.



(a) Biden Book Income AMT

(b) Without Foreign Tax Credits

Figure 9: 20 Largest Tax Increases Over a Decade

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

Amazon faces a $1.7 billion increase in taxes in my preferred ten-year revenue score, the 42[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 taxes of profitable firms like Amazon that pay few taxes. However, the book income AMT captures significantly more revenue from a number of other firms who are either more profitable, pay fewer 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.[38]

Amazon's taxes under the proposed book income AMT are mitigated by substantial tax loss carryforwards and foreign tax credits the firm has accumulated. Generally, allowing

[37] 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.

[38] 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.

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 their minimum tax with foreign tax credits.[39] Figure 9, panel (b) plots the twenty largest tax increases in response to this modified book income AMT, using my preferred elasticity estimates from Scenario 1. After excluding FTCs, Amazon faces a $5.2 billion increase in taxes, the 13$^{\text{th}}$ largest among all firms. Table 5, 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.

9 Conclusion

In this paper, I estimate firm responses to an AMT on book income. I find zero average tax base responses to AMTBIA87, and show previous estimates of tax base responses are too high because of mean reversion. I develop a model to formalize the non-tax incentives firms face to report high book incomes to investors that mitigate avoidance, and find suggestive evidence of avoidance responses only among firms with weaker incentives to report higher book incomes. Focusing on subgroups highlighted in previous work, I find null avoidance responses among firms with fewer loss and foreign tax credit tax shields, and among industry and firm size subgroups. Further analysis suggests the baseline null results hold in even broader unbalanced samples, that these null results are not driven by AMT credits, and that there are on average zero earnings management responses to AMTBIA87. Consistent with AMTBIA87 inducing a small change in firm's effective tax rates, 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

[39]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 could potentially serve as a backstop in an attempt to capture additional tax revenue from profitable firms.

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 incomes and low taxes, but 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 yield important lessons for policymakers as they attempt to implement the book income AMT included in the Inflation Reduction Act of 2022 and consider similar taxes in the future. First, while the precision of elasticity estimates limits our ability to distinguish between whether a tax on taxable or book income is more distortionary, the null estimates in this paper also suggest we should not discard taxes on book income from the policy toolkit because they are too distortionary. Instead, taxes on book income may be non-distortionary and raise substantial revenue because firms face non-tax incentives to report high book incomes. Second, we should expect some avoidance responses to a book income tax among firms with weaker incentives to report high book incomes. Third, the revenue raised by a book income AMT will critically depend on the breadth of the tax base.

Almost forty years after AMTBIA87, questions still remain about the efficacy of a tax on book income. While the evidence in this paper contributes to our understanding of firm responses to book income AMTs, it does not speak to every important issue facing policymakers today. Large corporations and earnings reporting both function somewhat differently today than they did during the 1980s. Incentive-based compensation is more frequent today (Desai, 2003), and both the composition of book tax differences (Gaertner, Laplante and Lynch, 2016) and the regulatory environment dictating information disclosure (Terry, Whited and Zakolyukina, 2021) have changed. While the results in this paper suggest the rise in incentive-based compensation should limit avoidance responses to a modern book income AMT, variation from AMTBIA87 does not allow us to study how firm responses might differ based on the regulatory environment or the composition of book tax differences. Furthermore, implementing 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.

Ultimately, we only possess incomplete historical evidence to guide our implementation of an AMT on book income. Policymakers should carefully consider the lessons from the empirical work in this paper, as well as how the corporate and tax environments have changed over time as they write regulations to implement the book income AMT in the Inflation Reduction Act of 2022 and consider future reforms. In parallel, researchers should carefully evaluate firm responses to the new book income AMT to continue to add to our understanding of the impacts of taxes on book income.

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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. One concern with the non-modified Jones model is that it assumes no managerial discretion is exercised over revenues. The modified Jones model assumes that all changes in credit sales are evidence of earnings management, building on logic that it is easier to manage earnings related to sales on credit than sales for which cash is immediately received (Dechow, Sloan and Sweeney, 1995).

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.

As described in the main text, while discretionary accruals are often used as a proxy for earnings management, they may not perform well (Ball, 2013; Jackson, 2018; Chen, Hribar and Melessa, 2018; Christodoulou, Ma and Vasnev, 2018). Therefore, rather than view estimates using the discretionary accruals outcome as primary results, I view them only as useful robustness checks that help confirm the results established by tax base estimates.

B Validating the Placebo-In-Time Approach

In this appendix I present four pieces of evidence to validate the placebo-in-time approach, which depends on an assumption that the time series process of ETRs and its impact on BTDs remains stable over time. These include: i) visual evidence of the stability of the impact of ETR mean reversion on BTDs over time, ii) minimum distance estimates of ETR time series process parameters and simulations suggesting deviations from those parameters will not cause substantial bias in placebo-in-time estimates, iii) heuristic discussion of raw mean and variance moments of the ETR distribution suggesting any changes to the ETR time series process should result in larger estimated avoidance responses, and iv) distributed lag regressions of changes in BTDs on changes in ETRs that indicate the influence of ETR mean reversion on BTDs does not change around the implementation of AMTBIA87

B.1 Visual Placebo-In-Time Evidence

Appendix Figure B.1 plots book tax difference responses to different placebo treatment definitions based on years well before AMTBIA87 is implemented (1977-1981) and based on years closer to the implementation of AMTBIA87 (1980-1984). I choose the placebo treatment years to preserve some post-reform period for each set of treatments before AMTBIA87 is implemented. While these series exhibit some variation, they also closely track the baseline specification, suggesting that the mean reversion of ETRs and its impact on BTDs due to the treatment definition remained stable over time.

B.2 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

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

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.

Minimum Distance Estimation and Inference:

I use classical minimum distance to estimate the parameters governing the ETR time series process shocks. I estimate the parameters governing this process by fitting the model in equation (B.1) 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

$$r_{it} = ETR_i + \alpha^t u_{i0} + \alpha^{t-1}\varepsilon_{i1} + ... + \alpha\varepsilon_{it-1} + \varepsilon_{it} + e_{it}, \tag{B.2}$$

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

$$Var(r_{it}) = \sigma_E^2 + \alpha^{2t}\sigma_{u0}^2 + \nu_t + \alpha^2\nu_{t-1} + ... + \alpha^{2(t-1)}\nu_1 + \sigma_e^2, \tag{B.3}$$

$$Cov(r_{it}, r_{is}) = \sigma_E^2 + \alpha^{s+t}\sigma_{u0}^2 + \alpha^{t-s}v_s + \alpha^{t-s+2}v_{s-1} + \ldots + \alpha^{s+t-2}v_1 \quad (s < t), \tag{B.4}$$

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

I use the elements of the residual covariance matrix C as moments to estimate the parameters θ governing the ETR process. 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 parameter vector θ and 45 moments in moment vector $m(\theta)$ (one for each element of C).

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

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 according to equations (B.3) and (B.4).

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.

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 $Var(e_{i81}) = Var(e_{it}) = \sigma_e^2$.[40] These restrictions and calibrations focus estimation on the key parameters, namely the persistence of shocks α and the variance of shocks ν_t.

Table B.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 [0.20,0.24] and do not allow α to vary over time in estimation.

Bias From Parameter Changes:

Changes in the key parameters ν_t and α 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.[41]

To evaluate possible bias quantitatively, I simulate nine period panels of effective tax rates

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

[41] 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.

using the estimated parameters in Table B.1 while varying α or ν_t. Appendix Figure B.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 B.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 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.[42] 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 B.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 B.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 B.2, panel (d) plots the difference in average ETRs for treatment and control in

[42] 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%.

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.

B.3 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 B.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.

The variance and autocovariance moments in Figure B.3, panel (b) confirm the intuition suggested by the minimum distance estimates described above. ETR variances are relatively stable throughout the 1980s, suggesting little bias in placebo-in-time estimates from changing shock variances.[43] 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 B.3, panel (a) allow us to roughly bound

[43] 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).

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 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.

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. My avoidance estimates are smaller than previous estimates in the literature, despite any potential bias stemming from changing ETR moments. This only strengthens the argument that previous estimates of BTD responses to AMTBIA87 are too large.

B.4 Distributed Lag Regressions

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 in equation (4) 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

$$\begin{aligned} \Delta BTD_{ied} &= \beta_0 \Delta ETR_{ied} + \beta_1 \Delta ETR_{ied} \times Post_{ied} \\ &+ \beta_2 \Delta ETR_{ie-1d} + \beta_3 \Delta ETR_{ie-1d} \times Post_{ied} + \phi_d + \delta_e + \varepsilon_{ied}, \end{aligned} \tag{B.5}$$

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 B.2 displays coefficients from an OLS regression of equation (B.5).[44] I cannot 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.

Table B.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 (B.1). The estimation procedure is described in detail in Appendix B.2.

[44] Appendix Table B.2 also displays coefficients from an instrumental variables regression of ΔBTD_{ied} on ΔETR_{ied} following the form of equation (B.5), 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 any potential concerns about serial correlation in ETRs biasing the OLS estimates. Results are similar.

Table B.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 (B.5). 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.

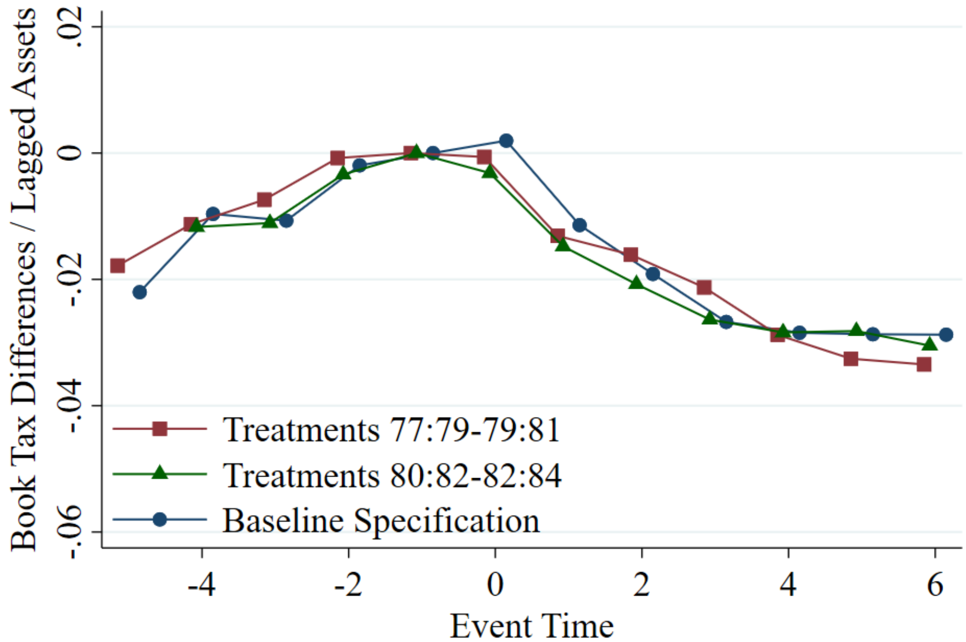
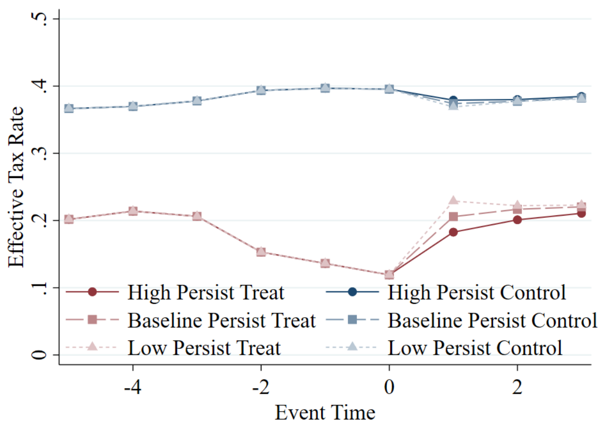
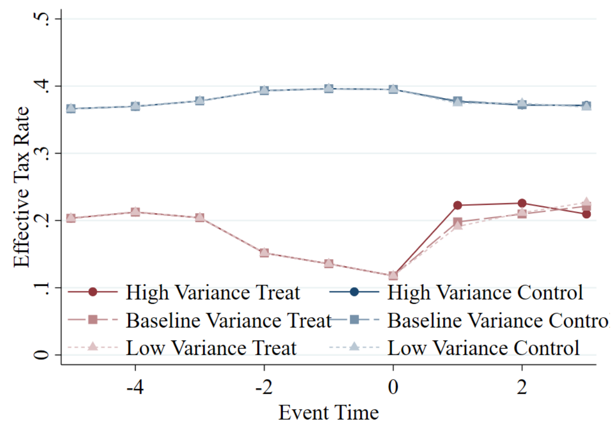


Figure B.1: Book Tax Difference Responses to Different Placebo Treatments

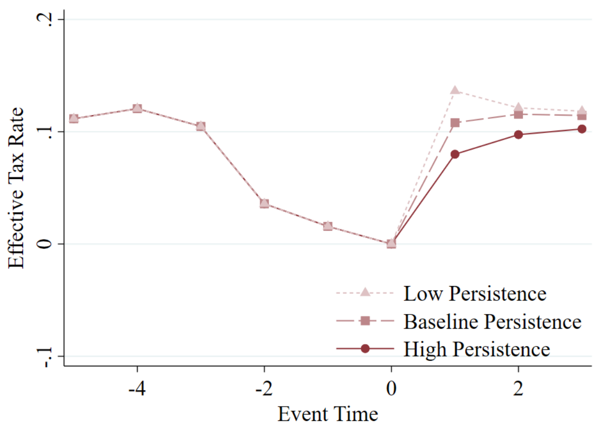
Notes: This figure plots placebo-in-time estimates of tax base responses to AMTBIA87. The baseline series plots point estimates of β_e from equation (3), splitting the 1981-1992 balanced panel into treatment and control groups based on 1984-1986 ETRs. The other series plot η_e estimates using treatment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



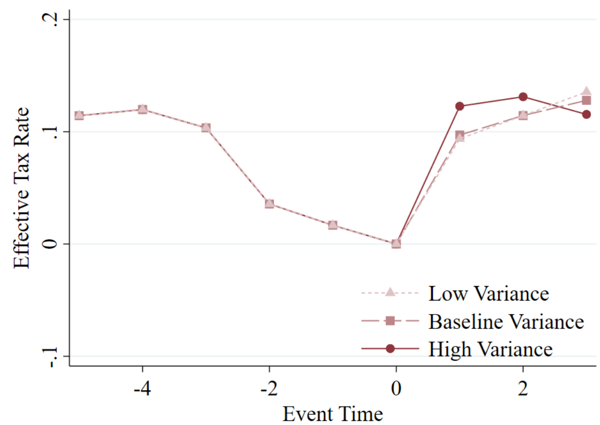
(a) Simulated ETRs Under Different α



(b) Simulated ETRs Under Different ν



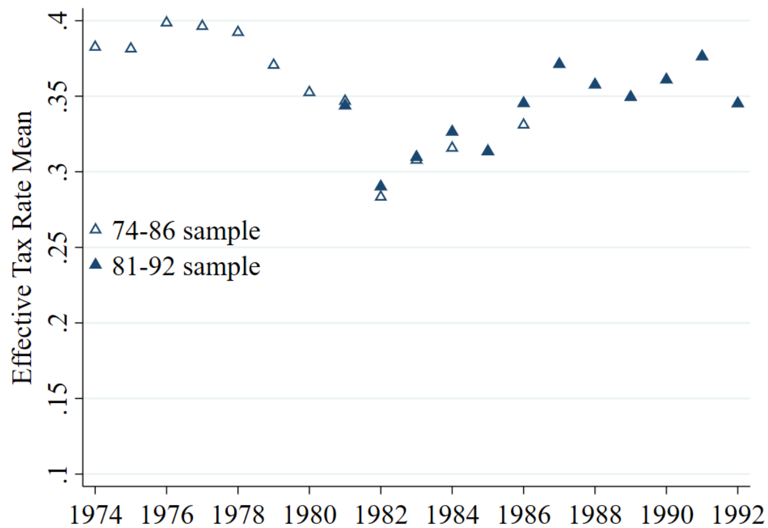
(c) Difference in ETRs Under Different α



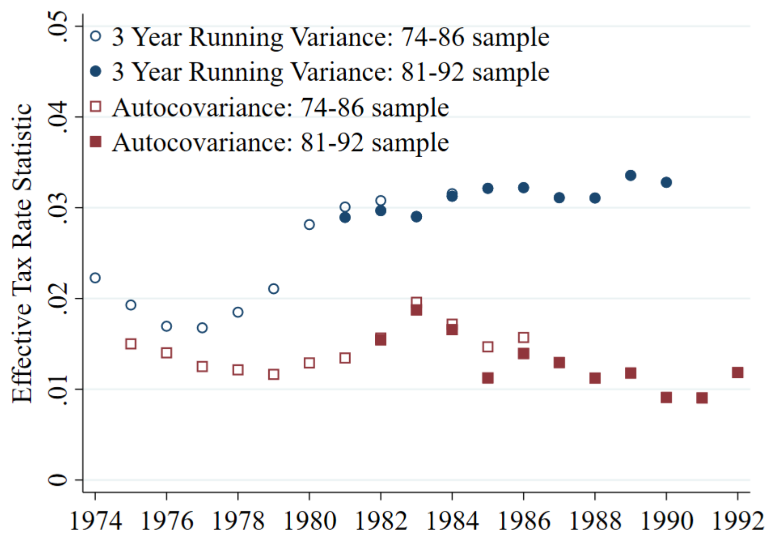
(d) Difference in ETRs Under Different ν

Figure B.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 (B.1) and the estimated parameters from the minimum distance procedure displayed in Table B.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 B.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.

C Revenue Scoring Methodology

To develop a revenue score of the Biden book income AMT proposal 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.[45] I display summary stats for this sample of firms in Appendix Table C.1. 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 current tax expense, 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 as the maximum of the firm's old tax or the potential tax due because of the book income AMT, only applying the AMT if the 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 Appendix Figure C.1 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 4 into the simulated data, I use CBO's 2018 ten-year GDP forecast as a proxy for

[45]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.

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.[46] Summing across the additional tax due 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,

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

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 4, 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.

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 4.

After projecting book incomes, I calculate firm's additional tax as the excess of their projected book income AMT over their projected tax under the normal corporate tax system. Book income AMT is reduced by foreign tax credits and net operating losses. This methodology calculates a revenue score for the proposed book income AMT holding all other

[46] 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.

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 C.1: 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).

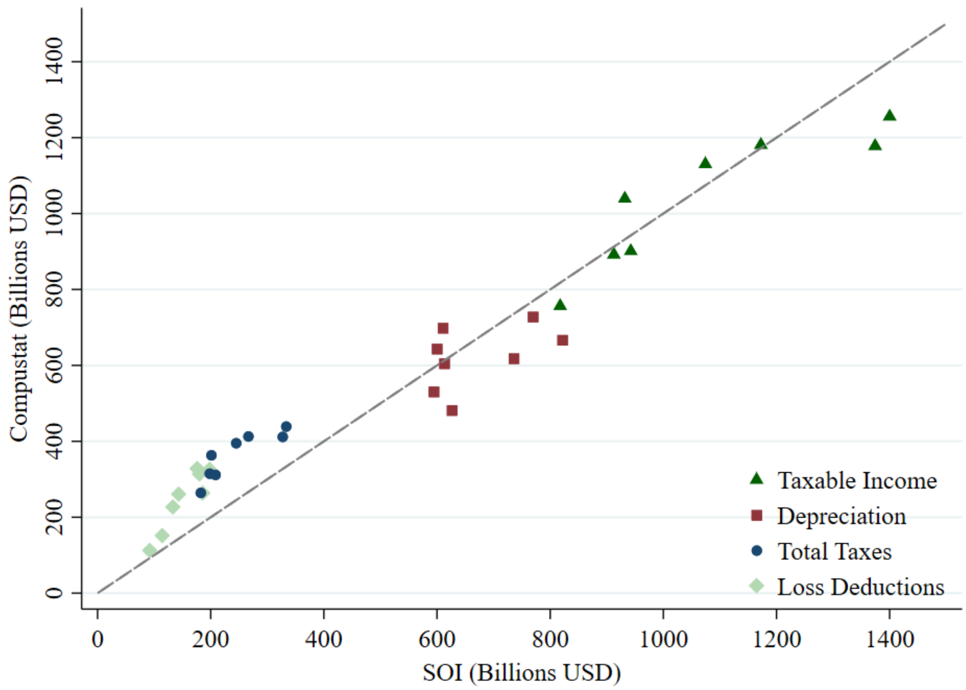


Figure C.1: Comparison of SOI Line Item Estimates to Compustat Aggregates 2008-2015

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 tax expense minus deferred tax expense minus other tax expense, and my measure of taxable income in Compustat is current tax expense divided by the statutory 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.

D Additional Tables and Figures

Table D.1: Previous AMTBIA87 Research

Panel A: Paper Information

Paper	Data	Treatment	Outcome	Firm Response 1986	Firm Response 1987-1989
Gramlich (1991)	Compustat	low $\frac{\text{taxes paid}}{\text{tax expense}}$	Δ Total Accruals	+	–
Dhaliwal and Wang (1992)	Compustat	1986 $ETR < 23\%$	$\Delta \frac{BTD}{BI}$	+	–
Manzon (1992)	Compustat	N/A	Discretionary Depreciation	+	–
Boynton et al. (1992)	Compustat & Tax	Pay 1987 AMT	Discretionary Accruals	0	–
Wang (1994)	Compustat	Pay 1987 AMT to Ind $\times$ Size match	Discretionary Accruals	+	–

Panel B: Samples

Gramlich (1991)	$N = 351 > 0$ BI 1984-1986, top and bottom $\frac{TP}{TE}$ quartiles, not missing outcome
Dhaliwal and Wang (1992)	$N = 360 \geq 0$ BI 1985-1987, drop fin and util, drop early fiscal year end, unconsolidated fin subsidiaries
Manzon (1992)	$N = 151$ listing AMT payment in 1987 tax footnote, drop fin, not missing outcome
Boynton et al. (1992)	$N = 414$ manf and transp, not missing assets, sales, liabilities, gross PPE, depreciation
Wang (1994)	$N = 56 > 0$ BI, AMT payment and accrued revenue/expenses in footnote, drop fin and util

Notes: This table compiles information on previous studies exploring firm responses to AMTBIA87. Panel A lists the data, treatment definition (if the paper compares outcomes between a treatment and control group), primary outcome variable, and the direction of estimated manipulation responses in each paper. Panel B lists information on the samples used for analysis in each paper. The five papers referenced in the table are Gramlich (1991); Dhaliwal and Wang (1992); Manzon (1992); Boynton, Dobbins and Plesko (1992); Wang (1994).

Table D.2: Earnings Management Elasticities Varying Incentives to Report High Book Income

	(1) Baseline	(2) Execucomp	(3) Low Incentive-Based Comp	(4) Big Bath
ε^{BI}	-0.19	-0.02	0.92	-0.09
	(0.35)	(0.53)	(0.64)	(0.50)
Base Observations	10140	5148	3192	3744
Base Clusters	845	429	266	312

Notes: This table reports earnings management elasticity estimates varying firm incentives to report high book incomes. All elasticity estimates are constructed by rescaling event study estimates of discretionary accrual responses to AMTBIA87 following equation (5). The baseline estimates in Column 1 use all available data from the 1981-1992 balanced panel. The Execucomp estimates in column 2 restrict to only firms present in Execucomp. The low incentive-based compensation estimates in column 3 restrict to firms present in Execucomp with managers whose compensation is $\leq 20\%$ incentive-based in 1992, and the big bath estimates in column 4 restrict to firms where the difference between their 1987 and 1986 book income is $< 0.5\%$ of assets. The base observation and cluster numbers correspond to the 1981-1992 balanced sample. Elasticity standard errors are reported in parantheses and calculated directly from event study estimates.

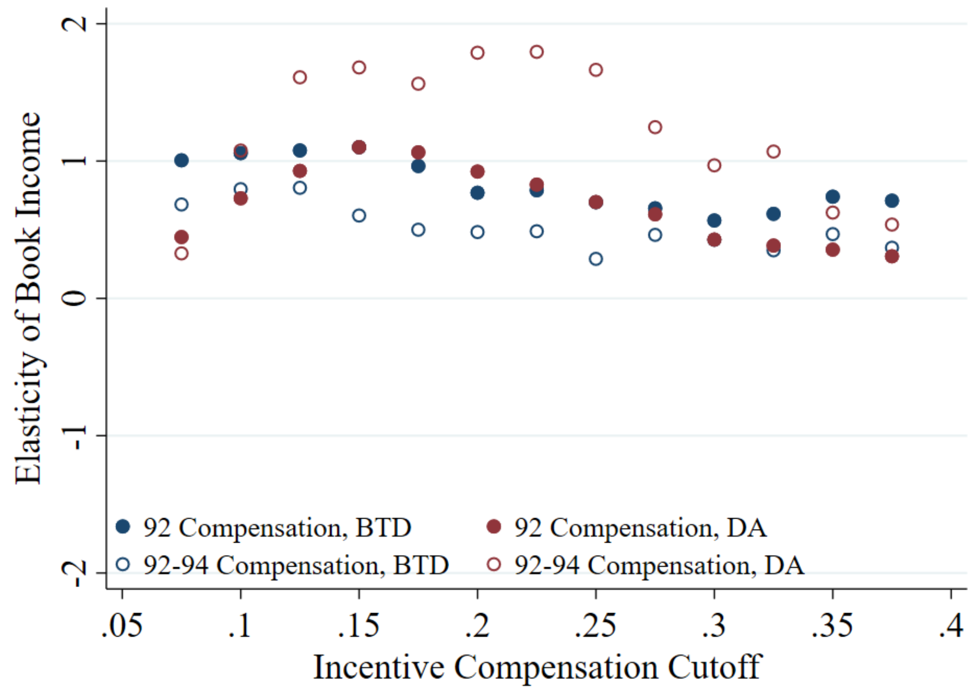
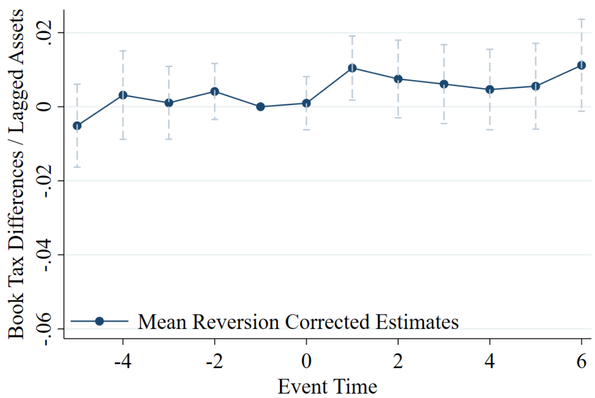
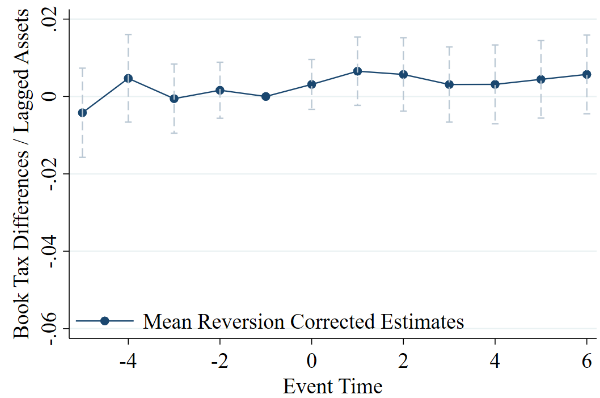


Figure D.1: Avoidance Responses by Incentive-Based Compensation Fraction

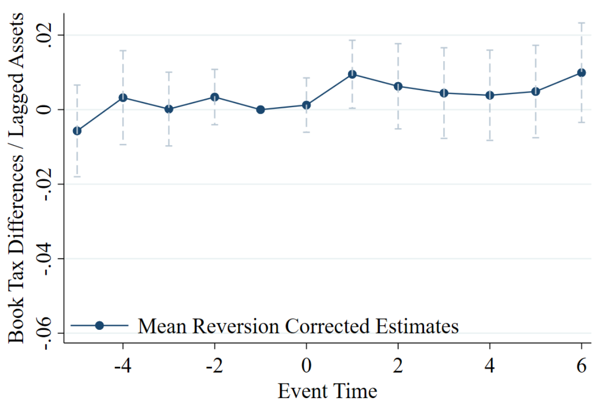
Notes: This figure plots point estimates of tax base and earnings management responses to AMT-BIA87. The point estimates in the book tax differences (BTD) series represent the difference between estimates of β_e from equation (3) and η_e from equation (4), while the point estimates in the discretionary accrual (DA) series represent estimates of β_e from equation (3). Each series splits firms into treatment and control groups based on 1984-1986 ETRs and restricts 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.



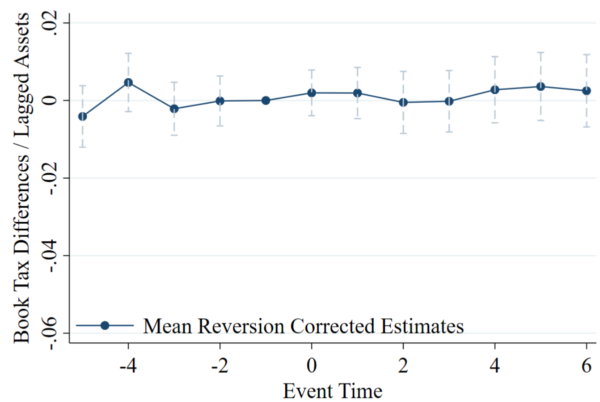
(a) 15% Effective Tax Rate Cutoff



(b) 19% Effective Tax Rate Cutoff



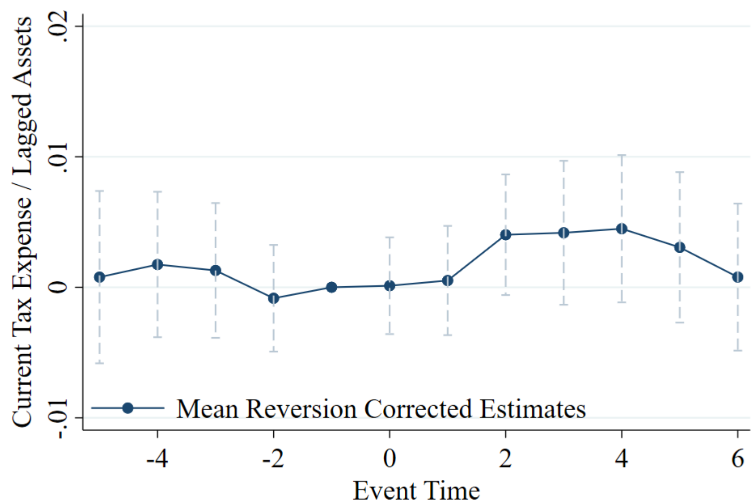
(c) Treatment <15%, Control > 23%



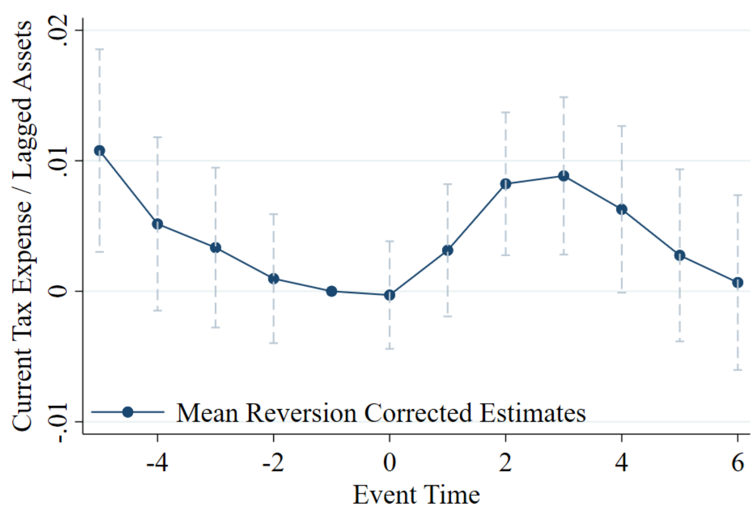
(d) 26% Effective Tax Rate Cutoff

Figure D.2: Mean Reversion Corrected Tax Base Estimates With Alternative Treatments

Notes: This figure plots mean reversion corrected estimates of tax base 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. Confidence intervals are constructed from non-parametrically bootstrapped standard errors clustered at the firm level using 300 iterations.



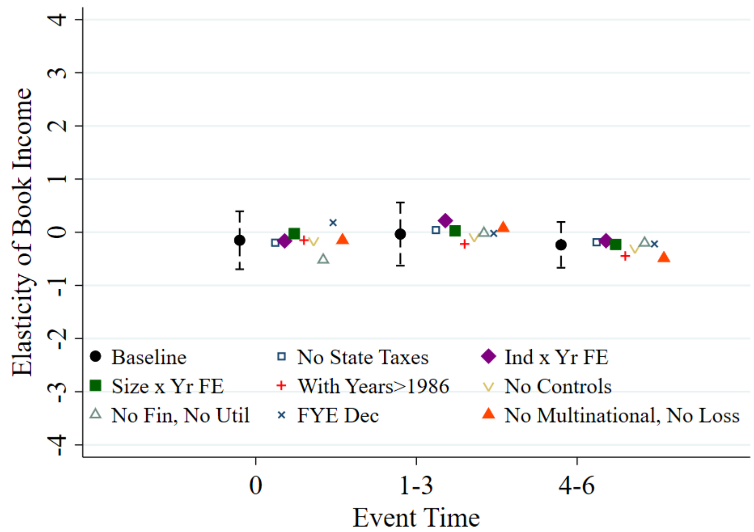
(a) Full Sample



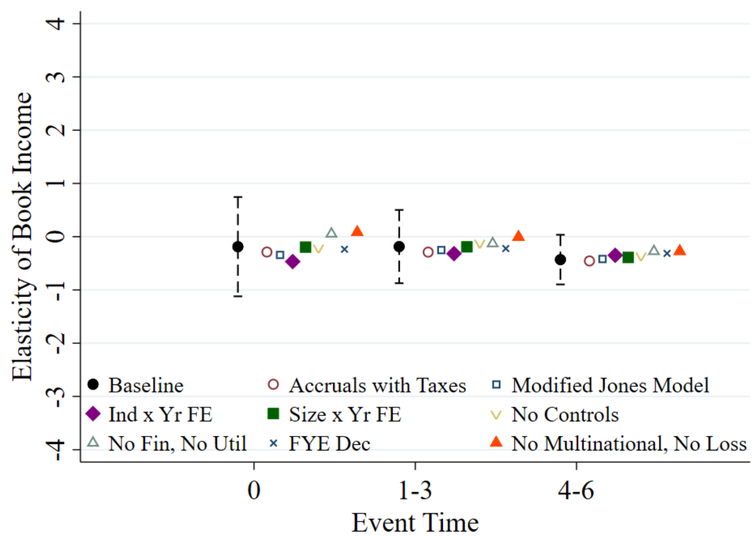
(b) No Multinationals or Loss Firms

Figure D.3: Mean Reversion Corrected Tax Expense Estimates

Notes: This figure plots mean reversion corrected estimates of current tax expense responses to AMTBIA87. Each panel replicates the estimates in Figure 4, panel (c) using current tax expense scaled by lagged assets as an outcome. Panel (a) displays estimates using all firms while panel (b) excludes multinationals and loss firms. Confidence intervals are constructed from non-parametrically bootstrapped standard errors clustered at the firm level using 300 iterations.



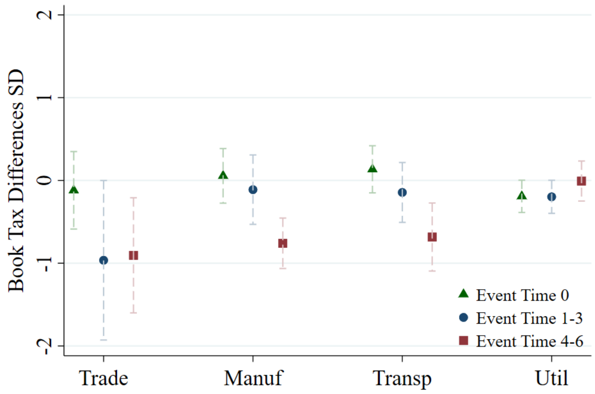
(a) Tax Base Elasticity Estimates



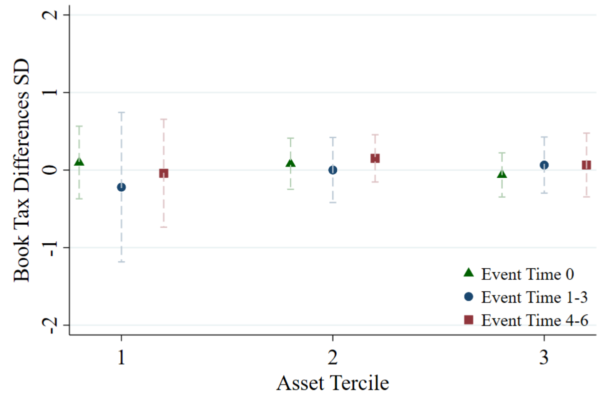
(b) Earnings Management Elasticity Estimates

Figure D.4: Elasticity Robustness

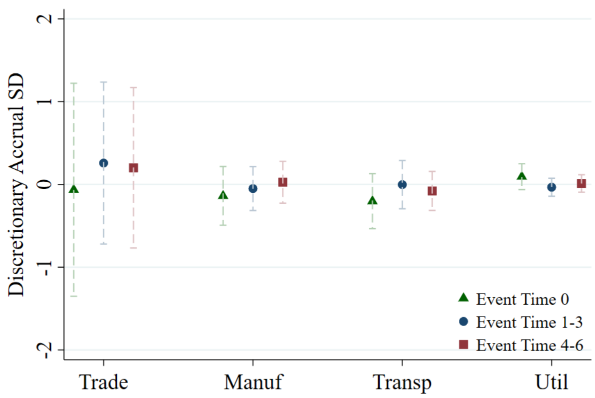
Notes: This figure plots elasticities of book income with respect to the net of tax rate using different controls, samples, and outcome constructions. Panel (a) reports elasticities constructed from mean reversion corrected tax base estimates following equation (5), while panel (b) reports elasticities constructed from event study estimates of earnings management following equation (5) without the mean reversion correction. The confidence intervals on the baseline estimates in panel (a) are constructed from nonparametrically bootstrapped standard errors using 300 iterations and clustering at the firm level, while the confidence intervals on the baseline estimate in panel (b) are clustered at the firm level.



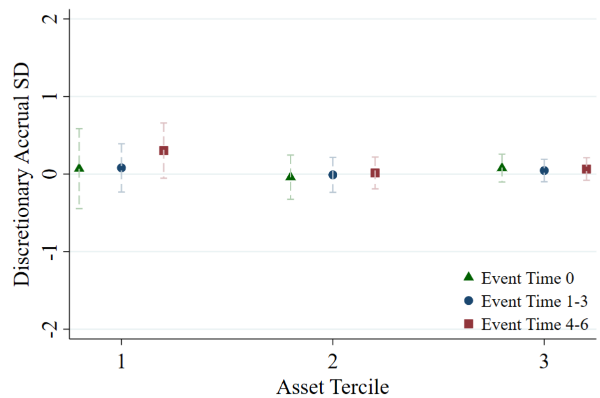
(a) BTD Industry Heterogeneity



(b) BTD Size Heterogeneity



(c) DA Industry Heterogeneity



(d) DA Size Heterogeneity

Figure D.5: 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 terciles. 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), transportation (SIC codes 4000-4899), utilities (SIC codes 4900-4999) and trade (SIC codes 5200-5999).

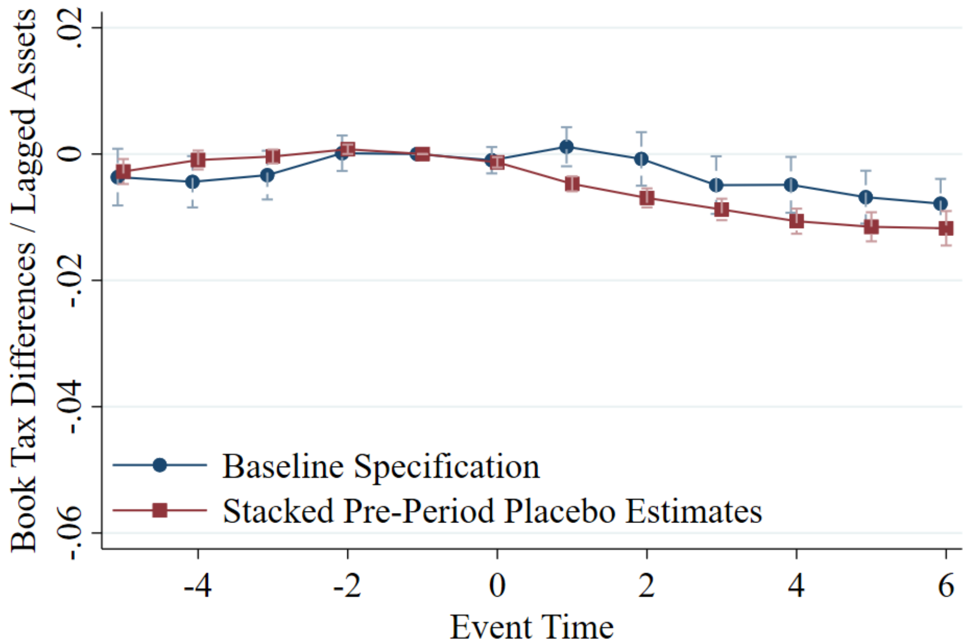
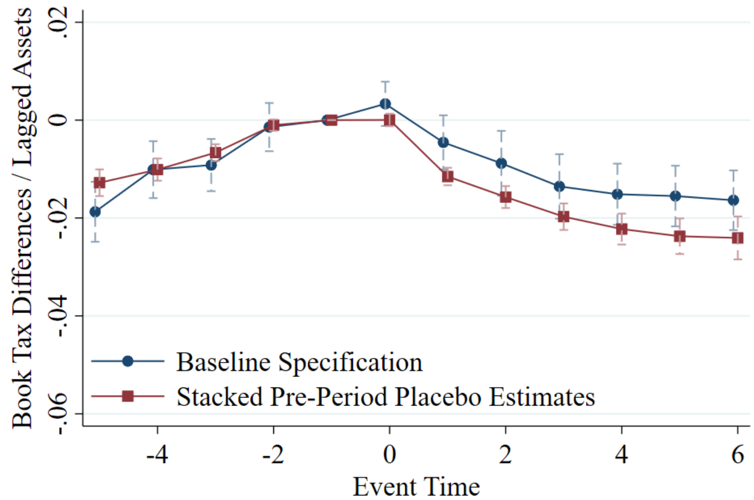
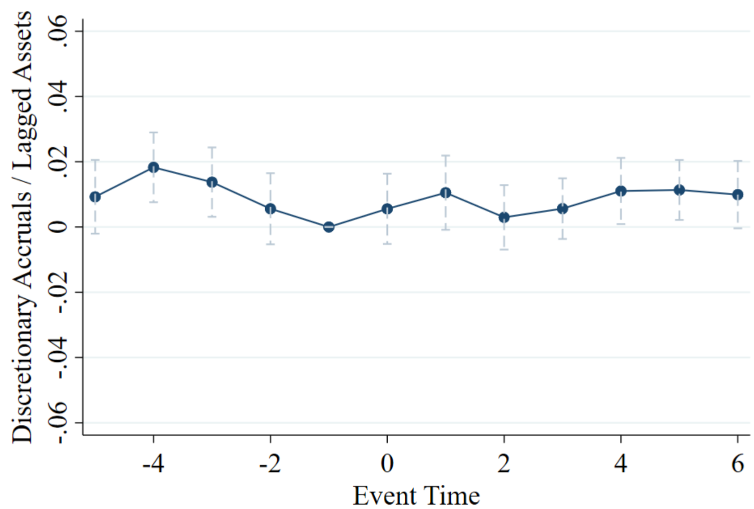


Figure D.6: Placebo-in-Time Permanent Book Tax Difference Estimates

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.



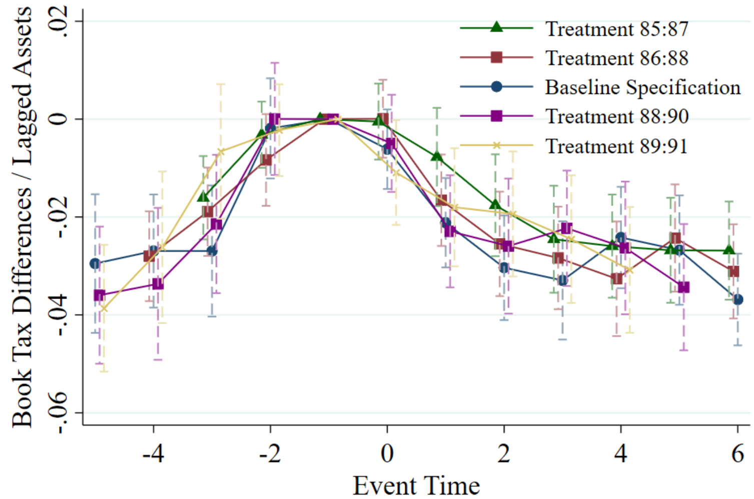
(a) Tax Base Responses



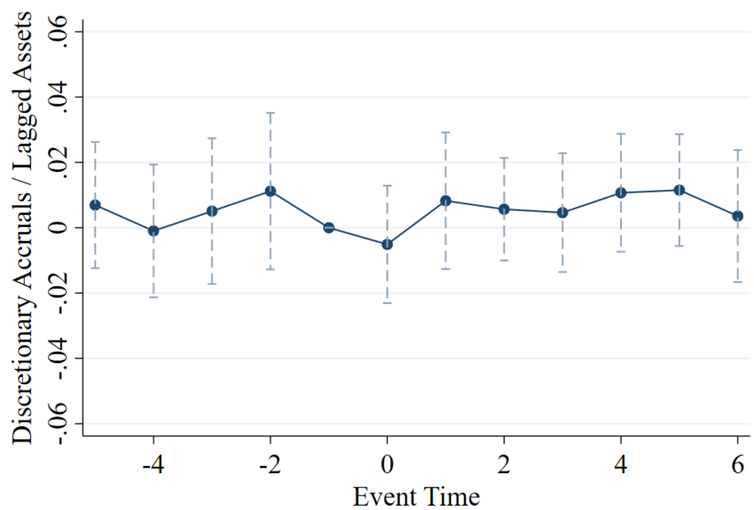
(b) Earnings Management Responses

Figure D.7: Firm Responses To AMTBIA87 in Unbalanced Panels

Notes: This figure plots tax base and earnings management responses to AMTBIA87 using larger unbalanced panels. Panel (a) plots estimates of β_e from equation (3) splitting the 1981-1992 unbalanced book tax differences 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 unbalanced panel and the 1981-1992 unbalanced 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 (b) plots estimates of β_e from equation (3) splitting the 1981-1992 unbalanced discretionary accrual panel into treatment and control groups based on 1984-1986 ETRs and using discretionary accruals as the outcome. Confidence intervals are calculated from standard errors clustered at the firm level.



(a) Tax Base Responses



(b) Earnings Management Responses

Figure D.8: 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.

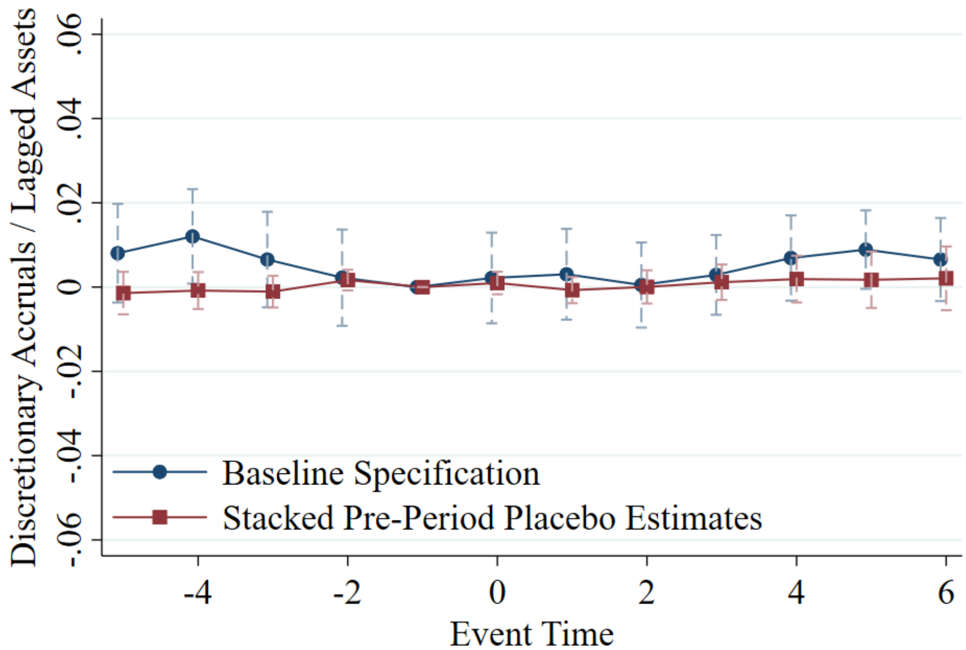
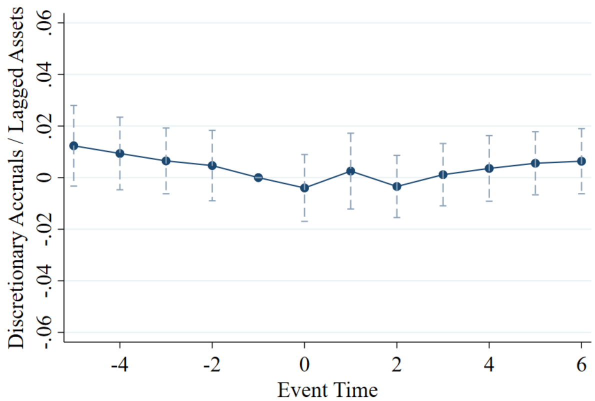
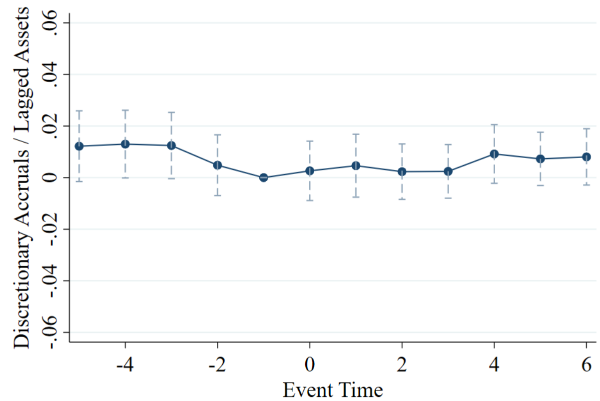


Figure D.9: Discretionary Accrual Mean Reversion

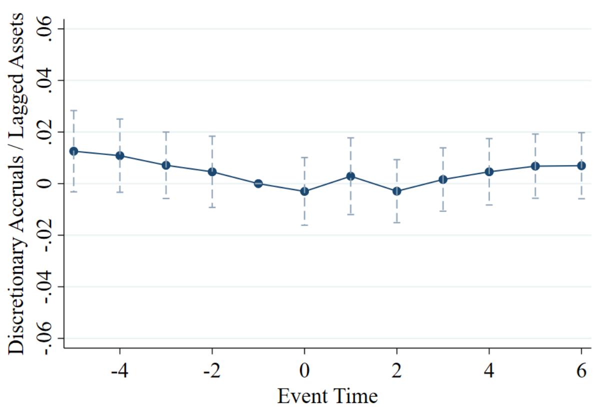
Notes: This figure plots discretionary accrual responses to AMTBIA87. The baseline specification series plots estimates of β_e from equation (3). The stacked pre-period placebo series plots estimates of η_e from equation (4), splitting data from the 1974-1986 and 1981-1992 balanced panels into treatment 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.



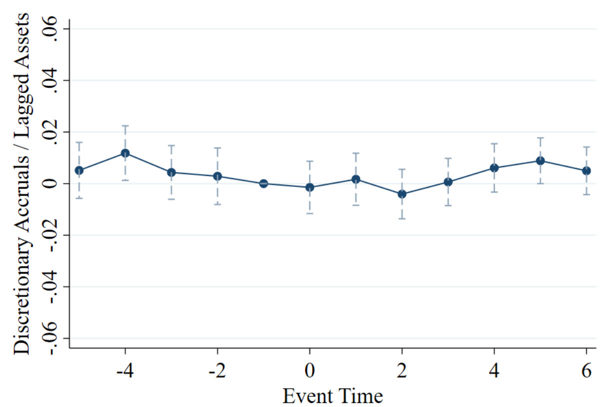
(a) 15% Effective Tax Rate Cutoff



(b) 19% Effective Tax Rate Cutoff



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



(d) 26% Effective Tax Rate Cutoff

Figure D.10: Earnings Management Event Study Estimates With Alternative Treatments

Notes: This figure plots event study estimates of earnings management responses to AMTBIA87. Each panel replicates the estimates in Figure 6, 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. Confidence intervals are constructed from standard errors clustered at the firm level.

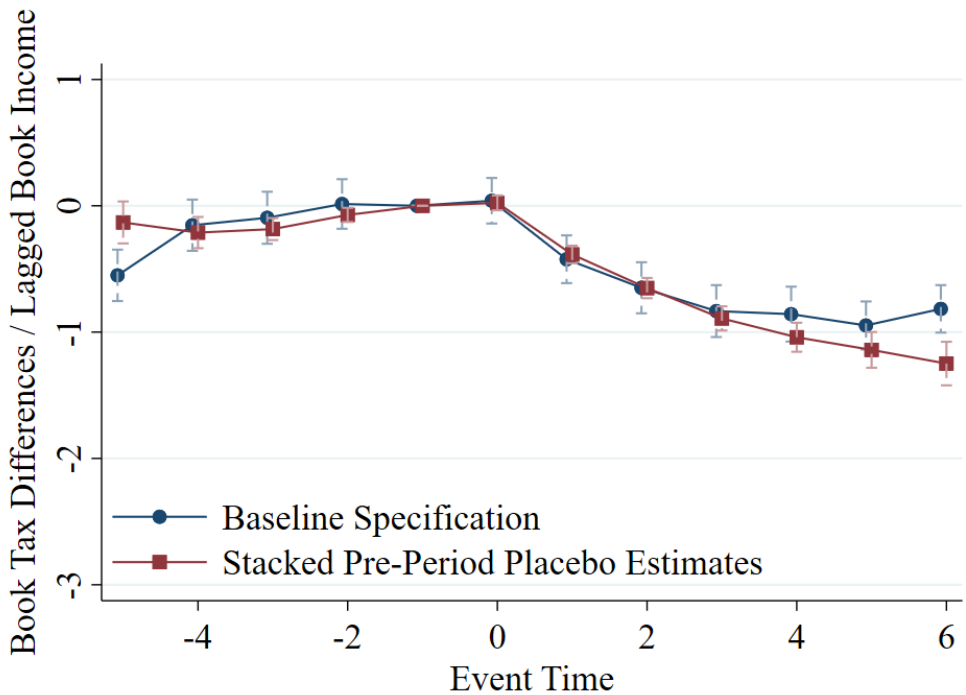
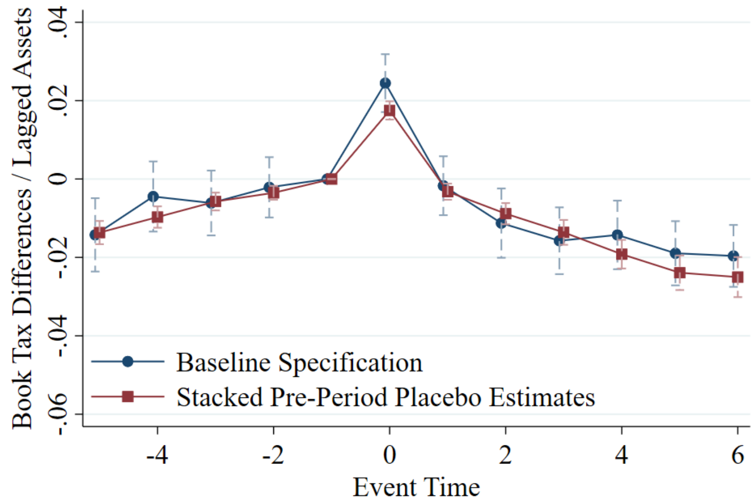
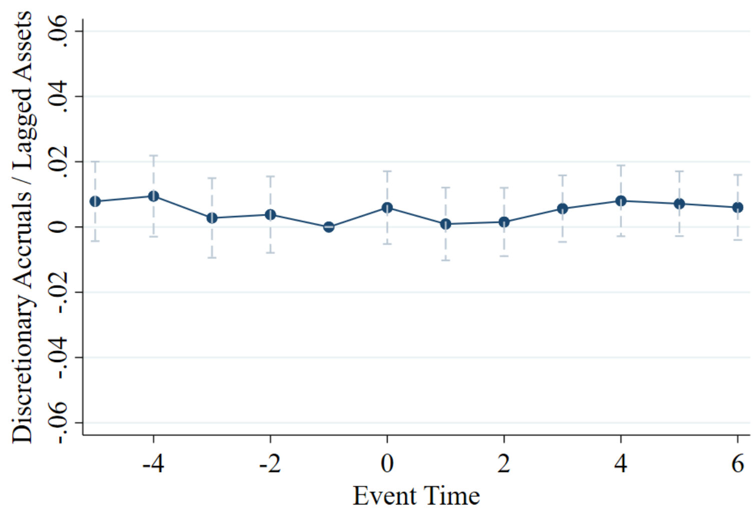


Figure D.11: Placebo-in-Time Tax Base Estimates Scaled by Book Income

Notes: This figure plots book tax difference responses to AMTBIA87 scaling the outcome by lagged book income. 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) Tax Base Responses



(b) Earnings Management Responses

Figure D.12: Avoidance Responses to Single Year Treatment Definition

Notes: This figure plots firm responses to AMTBIA87 while specifying that treatment firms have 1986 effective tax rates below 23% and control firms have 1986 effective tax rates above 23%, rather than averaging over three years. The baseline specification series in panel (a) plots estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 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). Panel (b) plots estimates of β_e from equation (3) splitting the 1981-1992 balanced panel into treatment and control groups based on 1986 ETRs. Confidence intervals are calculated from standard errors clustered at the firm level.