MARGINAL TAX RATES AND INCOME:
NEW TIME SERIES EVIDENCE

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ABSTRACT

Using new narrative measures of exogenous variation in marginal tax rates associated with postwar tax reforms in the US, this study estimates short run elasticities of taxable income of around 1.2 based on time series from 1946 to 2012. Elasticities are larger in the top 1% of the income distribution but are also positive and statistically significant for other income groups. Previous time series studies of tax returns data have found little evidence for income responses to taxes outside the top of the income distribution. The different results in this study arise because of additional efforts to account for dynamics, expectations and especially the endogeneity of tax policy decisions. Marginal rate cuts lead to increases in real GDP and declines in unemployment. This study also presents evidence that the responses are to marginal rather than average tax rates. Counterfactual tax cuts targeting the top 1% alone have positive effects on economic activity and incomes outside of the top 1% but increase inequality in pre-tax incomes. The data and methodology in this study do not permit any conclusions about the impact of tax rate changes targeting lower income taxpayers alone.

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1 Introduction

To what extent do marginal tax rates matter for individual decisions to work and invest? The answer is essential for public policy and its role in shaping economic growth. The empirical literature studying US tax returns, surveyed in Saez, Slemrod and Giertz (2012), concludes that reported pre-tax incomes react only modestly to marginal tax rates and attributes evidence of larger responses for top incomes to tax avoidance rather than real economic effects. In contrast, many macro studies find that indicators of real activity such as GDP, investment and employment respond significantly to changes in taxes, e.g. Romer and Romer (2010), Barro and Redlick (2011) or Mertens and Ravn (2013). The macro evidence for real economic effects of taxes should also be apparent in market incomes reported on tax returns.

This study contributes time series evidence on the aggregate responses to marginal tax rates by combining existing macro methodologies with the reported income measures of Piketty and Saez (2003) and newly constructed series on average marginal tax rates for the 1946-2012 period. Existing time series estimates of the elasticity of taxable income (ETI) with respect to net-of-tax rates (one minus the marginal tax rate) are close to zero in the aggregate. As a contribution to the ETI literature, I show that adopting specifications that address central concerns related to timing, expectations and in particular the endogeneity of tax policy leads to statistically significant short run elasticities centered around a value of 1.2 for all taxpayers. At the core of the identification strategy are new measures of the impact of a number of federal tax reforms on average marginal tax rates. The selection of tax reforms is based on Romer and Romer's (2009) narrative account of postwar US tax policy, focusing on individual income and payroll tax changes implemented within a year of their legislation to avoid anticipation effects. The ETI estimates are obtained by using these measures as proxies for exogenous tax rate innovations in structural vector autoregressions as in Mertens and Ravn (2013), or as instruments for tax rates in univariate regressions as in Barro and Redlick (2011). This paper also contributes to the macro literature by developing the narrative identification approach for marginal rather than average tax rate shocks and by analyzing responses along the income distribution.

The regression results indicate that incomes in the top 1% of the income distribution display the strongest

short run response to tax rates, which is consistent with the notion that high income tax payers display more avoidance behavior. However, contrary to prior time series studies of tax return data, I also find statistically significant elasticities for lower income groups and narrower wage income measures. Moreover, marginal rate cuts lead to increases in real GDP and declines in the unemployment rate that are broadly consistent with existing macro results. Simple calculations suggest aggregate hours elasticities of 0.37 on the intensive margin and 0.41 on the extensive margin, which is within the range of the quasi-experimental labor supply evidence surveyed in Chetty, Guren, Manoli and Weber (2013).

This study also conducts a novel test to determine whether real economic activity responds primarily to marginal or average tax rates. Combining measures of the impacts of the Romer and Romer (2009) reforms on both, I estimate the consequences of counterfactual tax experiments leading to an innovation in marginal tax rates but not in average tax rates and vice versa. Marginal rate changes lead to very similar income responses regardless of the change in the average tax rate. There is however no evidence for any effect on incomes when average tax rates decline but marginal rates do not. The transmission of US federal tax policy therefore seems to operate through incentive effects rather than disposable income and demand stimulus.

Finally, this study analyzes the impact of a counterfactual tax reform cutting marginal tax rates only for the top 1% in the income distribution. The associated short run taxable income elasticity for the top 1% is estimated to be around 1.5. A top marginal rate cut raises real GDP, lowers aggregate unemployment and also has a measurable positive effect on incomes outside of the top 1%. Nevertheless, marginal rate cuts targeting top incomes lead to greater income inequality. These results have implications for the interpretation of the observed correlation between top marginal tax rates and top income shares, see Piketty, Saez and Stantcheva (2014). Causal explanations based on avoidance or rent-seeking alone cannot explain the finding that top marginal rate cuts have real economic effects and spill over to lower income groups. Using the methodology in this paper, the nature of the postwar variation in marginal tax rates unfortunately does not permit any conclusions about the impact of tax reforms changing tax rates for lower income groups only.

2 Income and Average Marginal Tax Rates

2.1 Motivating Framework

The empirical methodology in this paper does not rely on any specific theory or functional form assumptions. A minimal theoretical framework is however useful to highlight issues of aggregation and to provide a starting point for discussing the empirical results. One such framework capturing one of the most basic potential distortionary effects of taxation is the standard labor supply model.

Suppose there is a unit measure of agents indexed by $i \in [0,1]$ with quasilinear utility,

$$c_{it} - \frac{hx_{it}}{1+1/\varepsilon}(h_{it}/h)^{1+1/\varepsilon} \; ; \; h, \varepsilon \geq 0 \;, \tag{1}$$

where c_{it} and h_{it} denote consumption and hours worked, x_{it} is an exogenous preference shifter and h is a constant. The parameter $\varepsilon \geq 0$ is the (Frisch) labor supply elasticity. The budget constraint is $c_{it} \leq e_{it} - T(e_{it}) + f_{it}$ where $e_{it} = w_{it}h_{it}$ is wage income, $T(\cdot)$ are taxes due, and f_{it} is other income which for simplicity is untaxed. Utility maximization yields the labor supply function,

$$h_{it} = h\left((1-T'(e_{it}))w_{it}/x_{it}\right)^{\varepsilon} \;, \tag{2}$$

where $T'(\cdot)$ is agent i's marginal tax rate. The labor supply elasticity ε determines the distortionary effect of marginal tax rates on incentives to work.

Consider a simple time invariant tax schedule of the type originally proposed by Feldstein (1969):

$$T(e_{it}) \; = \; e_{it} - (1-\tau_t)\frac{(e_{it}/\bar{e}_t)^{1-\gamma}}{1-\gamma}\bar{e}_t \;, \; 0 \leq \gamma < 1 \tag{3}$$

where $\bar{e}_t = \left(\int_0^1 e_{it}^{1-\gamma}di\right)^{1/(1-\gamma)}$ is an aggregate of taxable income and $\tau_t = 1 - \int_0^1 (e_{it}/\bar{e}_t)(1-T'(e_{it}))di$ is the economy-wide average marginal tax rate, henceforth abbreviated by AMTR. The AMTR is a weighted average of individual marginal tax rates with weights given by income shares. The parameter γ measures

the progressivity in the tax system: When $\gamma = 0$ all agents face the same marginal tax rate τ_t, when $\gamma > 0$ the tax system is progressive. The marginal net-of-tax rate for agent i is $1 - T'(e_{it}) = (1 - \tau_t)(e_{it}/\bar{e}_t)^{-\gamma}$.

Substituting into (2) and aggregating over all agents in any subset $S \subseteq [0,1]$ implies that aggregated wage income is $e_t^s = h(1 - \tau_t^s)^\varepsilon (w_t^s)^{1+\varepsilon} (x_t^s)^{-\varepsilon} z_t^s$ where $w_t^s = \int_S w_{it} di$ is the average hourly wage for agents in S, $x_t^s = \int_S x_{it} di$ and z_t^s depends only on higher order moments of the cross-sectional distribution of (w_{it}, x_{it}) over S.[1] Taking logs and first differencing the expression for e_t^s yields

$$\Delta \ln(e_t^s) \quad = \quad \varepsilon \Delta \ln(1 - \tau_t^s) + r_t^s \tag{4}$$

where $\tau_t^s = 1 - \int_S (e_{it}/\bar{e}_t^s (1 - T'(e_{it})) di)$ is the AMTR for all agents in S and the residual term $r_t^s = (1 + \varepsilon)\Delta \ln(w_t^s) - \varepsilon \Delta \ln(x_t^s) + \Delta \ln(z_t^s)$ is stationary. When the aggregation is over all agents and $S = [0,1]$, then $\tau_t^s = \tau_t$ and $e_t^s = e_t$ is total taxable income.

Equation (4) suggests a potentially viable strategy to estimate the labor supply elasticity ε and therefore measure the distortionary effects of taxes: regress taxable income growth on changes in log net-of-tax rates. When using aggregated data, the relevant tax measure in such a regression is the AMTR, or the income weighted average of the individual marginal tax rates faced by the agents included in the aggregate. An immediate problem with this strategy is reverse causality. In a progressive tax system, marginal tax rates depend on income. However, occasional reforms mean that tax schedules are not time invariant. A large number of quasi-experimental studies in public finance exploit tax reforms to obtain independent variation in marginal tax rates and focus on pre-tax incomes reported on tax returns as the outcome variable.

Even if tax experiments provide independent variation in marginal tax rates, empirical estimates of the elasticity of taxable income are in practice not narrowly interpretable as Frisch labor supply elasticities. Simple extensions of the labor supply model above to include income effects, extensive margin responses or human capital accumulation all invalidate this interpretation, see Keane (2011), Keane and Rogerson (2012) or Blundell et al. (2013). In addition, taxable income in the US also includes profits, rents and realized capital

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[1]More precisely, $z_t^s = \left(\int_S \left((w_{it}/w_t^s)^{1+\varepsilon} (x_{it}/x_t^s)^{-\varepsilon} \right)^{\frac{1}{1+\varepsilon\gamma}} di \right) \left(\int_S \left((w_{it}/w_t^s)^{1+\varepsilon} (x_{it}/x_t^s)^{-\varepsilon} \right)^{\frac{1-\gamma}{1+\varepsilon\gamma}} di \right)^{\varepsilon\gamma/(1-\gamma)}$.

gains such that tax rates also impact on savings and investment decisions. There is also tax evasion and there are many avoidance opportunities arising from deductions and exemptions and the ability to time income across tax years. Elasticities of taxable income, or ETIs, will therefore capture a range of labor supply and investment incentive effects as well as a variety of other behavioral responses that result from the incentives to minimize the burden of taxation. They may also incorporate endogenous effects on wages and other prices because of general equilibrium or other effects. ETIs have nevertheless received much attention as useful summary measures of the distortionary effects of taxes. They are generally informative about the efficiency and revenue implications of tax policy changes and can in some cases be used as a sufficient statistic for optimal tax analyis.[2]

2.2 Existing Evidence on Income Responses to Taxes

The public finance literature has obtained quasi-experimental ETI estimates using tax return data in a variety of ways. The analysis of the 1981 reform by Lindsey (1987) used cross-sectional data and counterfactual income simulations to document very large elasticities centered around 1.6. To better control for confounding factors, panel data studies of the 1986 reform starting with Feldstein (1995) exploited heterogeneity in marginal tax rate changes to establish treatment and control groups and make difference-in-difference comparisons. The combined evidence from the 1980s reforms in Lindsey (1987), Feldstein (1995), Auten and Carroll (1995, 1999) and others pointed to large short run ETIs in a range of 0.7 to over 3.0, although broadening the sample of taxpayers, the definition of income or the set of controls yields estimates towards the lower end of that range. Subsequent event studies of reforms in the 1990s, such as Sammartino and Weiner (1997), Carroll (1998) or Giertz (2010), instead obtained lower values of close to 0 up to 0.5. This confirmed growing suspicions that the estimates for the 1980s were largely artifacts of insufficient controlling and/or of certain attributes of these reforms leading to highly transitory effects, see Slemrod (1995, 1996).

Diff-in-diff case studies offer no definitive answer because there are many other determinants of relative income changes that are hard to control for and because it must be assumed either that there is no tax change for the control group or otherwise that the ETIs are identical for both groups.[3] To overcome some of these

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[2]See for instance Feldstein (1999), Saez (2001), Chetty (2009), Diamond and Saez (2011) or Badel and Huggett (2015).

[3]Blundell et al. (1998), Slemrod (1998), Triest (1998), Goolsbee (1999) and Saez et al. (2012) discuss the empirical issues.

difficulties, one strategy is to assume ETIs are roughly time-invariant and cumulate evidence from a number of tax reforms.[4] Empirical models that under reasonable assumptions restrict unobservable confounding influences on income growth to have zero mean allow for averaging out those influences over time. For instance, Gruber and Saez (2002) use a long panel dataset from 1979 to 1990 to exploit richer variation in tax rates during that period and find an elasticity of income (before deductions and exemptions) of close to zero in the sample of all tax returns. Starting with Feenberg and Poterba (1993), most studies adopting a broader time perspective however use more aggregated data that is available for different and/or longer sample periods. By gathering evidence from multiple reforms, such studies have further confirmed the view that the reported income responses observed for the 1980s reforms were anomalies. In time series regressions of top income shares on net-of-tax rates over the 1950-1990 period, Slemrod (1996) for instance finds that the elasticity drops considerably when the last five years containing the 1986 reform are excluded. Goolsbee (1999) uses aggregate data to obtain separate short run diff-in-diff elasticities for seven reforms between 1920 and 1975 and finds that the largest estimate is far below those for the 1980s reforms. In aggregate time series regressions, Saez (2004) and Piketty, Saez and Stantcheva (2014) find small and statistically insignificant elasticities for all tax units and moderate but significant elasticities for top incomes. Collecting diff-in-diff evidence from reforms during the interwar period, Romer and Romer (2014) find elasticities for top incomes around 0.20. In their survey of the available evidence, Saez, Slemrod and Giertz (2012) acknowledge there are no truly convincing estimates of long run ETIs but conclude that the best available estimates are in a range of 0.12 to 0.40. There is much evidence for larger short run ETIs for high income tax payers which they attribute mostly to better access to avoidance opportunities. Beyond that, Saez et al. (2012) argue, there is no compelling evidence for any real economic responses to marginal tax rates.

The conclusions from the ETI literature are at odds with the recent macro empirical literature that exploits policy reforms as quasi-experiments to identify the effects of taxes on aggregate real economic variables such as GDP, unemployment or investment. Starting with Romer and Romer (2010), 'narrative approach'

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[4]Another strategy is to look for features in the tax code that generate differential tax rates for narrower but more similar groups of taxpayers. Unfortunately the results may not be more broadly representative and, while the case for identification may become more convincing, the identifying variation in tax rates is typically smaller. Taxpayers may not be aware of the minute details of the tax code and/or have insufficient incentive to respond to small changes, see Chetty (2012). The findings may therefore be less relevant for larger changes in marginal tax rates.

studies have consistently estimated substantial short and medium run effects of taxes on economic activity. For instance, Romer and Romer (2010) find that a policy-induced increase in federal tax liabilities of 1% of GDP lowers GDP by 3% and investment by 9% after two years. Mertens and Ravn (2013) find that a one percentage point decrease in the personal average tax rate raises GDP by 1.5% and lowers the unemployment rate by 0.5 percentage points within a year. Also using tax reforms for identification, Cloyne (2013) and Hayo and Uhl (2014) find remarkably similar results for the UK and Germany, while Leigh, Pescatori and Guajardo (2014) find large contractionary effects of tax based fiscal consolidations in OECD countries. The lack of evidence for real substitution effects in the ETI literature is also puzzling in light of a closely related labor supply literature that uses tax experiments and hours or employment as outcome variables. Based on their reading of the recent evidence, Chetty, Guren, Manoli and Weber (2013) view elasticities of aggregate hours of 0.5 for a permanent tax change and 0.75 for a transitory tax change as realistic. As broader measures of the behavioral response, ETIs should be at least as large as these labor supply elasticities.

The conflicting evidence on the real effects of taxes between the ETI and macro literatures cannot be easily resolved by any of the explanations for the gap between micro and macro labor supply elasticities, since the public finance evidence includes analyses of aggregate time series.[5] One potential explanation is that most of the macro studies focus on average rather than marginal tax rates. Many reforms impact differently on both and any aggregate demand effects due to changes in disposable income may feature more prominently in the macro estimates. Using the Romer and Romer (2010) tax policy measure as an instrument, Barro and Redlick (2011) however find that a one percentage point cut in the AMTR raises per capita GDP by around 0.5% in the following year. This estimate is statistically significant and amounts to a short run GDP elasticity to the net-of-tax rate of 0.36, which should be considered a lower bound for the ETI. By comparing results from specifications with average tax rates, Barro and Redlick (2011) also tentatively conclude that the response is mainly to marginal rather than average tax rates. The objective of this paper is to further investigate the main claims of both the ETI and macro literatures on the real effects of tax changes and expose the sources of the disagreement. To include more historical variation in tax rates, I employ newly extended time series on postwar AMTRs that are discussed next.

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[5] See Keane and Rogerson (2012) and Chetty et al. (2013) for the debate on micro and macro labor supply elasticities.

2.3 Average Marginal Tax Rates 1946-2012: Description and Stylized Facts

Figure 1 depicts estimates of annual US average marginal tax rates from 1946 to 2012 for the aggregate economy and within different income brackets. The series combine federal individual income tax rates and contribution rates under the Old-Age, Survivors and Disability Insurance and Medicare Hospital Insurance programs. The tax rates and income rankings reflect the population of potential tax units, defined as all married men and singles aged 20 or more. The upper panel of Figure 1 shows two AMTR measures for all tax units that differ primarily in the income concept used for weighting. The first measure is based on a broad concept of labor income used by Barro and Redlick (2011) that includes wages, self-employment, partnership, and S-corporation income. The other aggregate series as well as the series for top and bottom tax units in the lower panel of Figure 1 use an income concept from Piketty and Saez (2003) that includes non-labor income but excludes capital gains and government transfers. The percentiles are for the distribution of the Piketty and Saez (2003) definition of income across potential tax units.[6]

Figure 2 shows the income tax component. The first series for all tax units updates the measure of Barro and Redlick (2011) to include observations post 2006. The series based on the Piketty and Saez (2003) income concept extend those of Saez (2004) by almost 30 years using data from the IRS Statistics of Income. The social security tax rates in Figure 3 are constructed from data published by the SSA as well as individual IRS tax returns. The series for all tax units are updates of Barro and Sahasakul (1986). The series for top and bottom tax units are entirely new. Appendix A provides full details on the construction of the tax rates. One limitation of the series is that social security benefits depend partially on earnings. In principle, marginal changes in benefits should be netted out to obtain the tax component. In practice the inclusion of social security has no major implications for the results in this paper. Another limitation is that the series do not include state-level taxes. The amount of short run variation in aggregated state-level marginal tax rates is very small, see Barro and Redlick (2011), such that this omission is unlikely to be important.

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[6]Piketty and Saez (2003, 2007) provide a detailed description of the income data, which for most years is based on public use files containing around 100,000 returns. In the postwar period, the top 1% income share was about 11% after the war, declined to 8% in the 1960s and 1970s and has gradually risen since to about 19% in 2012. The top 10% share was about 1/3 after the war and has risen since the late 1970s to about 48% in 2012.

The tax rates for all tax units in Figure 1 display an upward trend starting at around 20% right after WWII and rising to over 35% at the beginning of the 1980s. The main source of this trend is the gradual expansion of social security contributions from less than 1% in 1946 to around 9% since the early 1990s, see Figure 3. The upward trajectory accelerates in the 1970s because of rapid increases in income tax rates primarily due to high inflation and bracket creep. In the 1980s, the continuing rise in social security rates is largely offset by decreases in income tax rates. The income tax component appears stationary over the postwar period and is typically in the 20%-25% range. The tax rates by income in the lower panel of Figure 1 show a substantial decline in progressivity after 1980. This decline is mostly the result of reforms in the 1980s but also partly due to the growing importance of the regressive social security tax, which taxes individual earnings above a statutory ceiling at a zero marginal rate before 1994 and only at the lower hospital insurance rate afterwards.

In the short run, the tax rate series in Figure 1 display substantial variation that is predominantly driven by income taxes. The larger annual fluctuations in income tax rates reflect well known legislative changes.[7] Because brackets and ceilings are imperfectly indexed, AMTRs also vary automatically with nominal income levels even in the absence of legislative changes.[8] Changes in the social security rates are less important for the year-to-year variability in overall rates.[9] To provide more insight into the sources of annual variation in tax rates, Figure 4 depicts estimates of the impact of policy driven statutory changes in overall tax rates (upper panel) as well as in the income tax and social security rates individually (lower left and right panels). The estimated statutory component in year t is calculated as the difference between a counterfactual average marginal tax rate, calculated using the year $t-1$ income distribution and year t rates and brackets deflated

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[7]The most significant adjustments include the rate reductions in 1948 following the end of WWII, the tax increases in the 1950s during the Korean War; the 1964 Kennedy tax cuts; the 1968-1970 surcharge during the Vietnam War; the 1980s Reagan tax cuts and in particular the 1986 Tax Reform Act; the early 1990s Bush and Clinton tax increases; and the W. Bush tax cuts in the early 2000s.

[8]Annual inflation adjustments to income tax brackets began only in 1985 and to date there is no real income indexation. De facto inflation adjustments started in 1985 although automatic indexing to the CPI did not begin until 1987. Some components of the tax code, such as the alternative minimum tax, have not been automatically indexed to inflation even after 1987. The American Taxpayer Relief Act of 2012 starts automatic indexation of the alternative minimum tax in 2013. All indexation occurs with significant delay and is applied roughly uniformly across the income distribution.

[9]Social security contributions depend on taxable maxima that have been automatically indexed to national average wage growth starting in 1975. The many statutory changes to social security contribution rates and/or taxable earnings prior to the early 1990s are all permanent and gradual increases that are comparatively smaller in size. The most noticeable changes result from the Great Society initiatives under Johnson including the introduction of Medicare in 1966, the 1972, 1977 and 1983 amendments of Social Security and the expansion of the Medicare tax in the early 1990s. The only reduction is the temporary cut in contribution rates under Obama in 2011 and 2012.

by any automatic adjustments between $t-1$ and t, and the actual year $t-1$ average marginal tax rate. The difference between actual and policy induced annual changes in tax rates thus captures the effect on AMTRs of the change in the income distribution relative to the previous year. This is of course only an 'effect' in a purely accounting sense and should not be given a causal interpretation.

Table 1 quantifies some key characteristics of the combined AMTR series in Figure 1 and the sources of variation. The first eight columns provide first and second order properties of the tax rate levels and of changes in the net-of-tax rates by income group. The last three columns in Table 1 contain the contribution of statutory changes to the overall variation in annual net-of-tax rate changes. These are measured by the R^2 coefficient of regressions of net-of-tax rate changes on the statutory changes estimated for each income group separately as described above, i.e. by constructing a counterfactual tax rate that keeps the income distribution fixed and adjusts for automatic indexation.

Table 1 reveals a number of important features of the tax rate series. First, there is substantial variation in postwar AMTRs, most of which is driven by policy changes. The raw standard deviation of annual changes in net-of-tax rates for all tax units is 1.8 to 2.0 percent. More than 85% of the variation is explained by statutory changes. Second, the federal income tax is the dominant source of fluctuations in income-weighted tax rates. Three quarters of the variation in net-of-tax rates for all tax units is explained by legislative changes to income taxes, whereas statutory changes in social security taxes account for 14% to 17%. Third, there is considerable heterogeneity in tax rate variability across income groups. Annual percentage changes in net-of-tax rates are considerably more volatile for top incomes than for lower incomes, explaining 80% or more of the total variation. Not surprisingly, statutory changes in social security taxes contribute very little to the variation in top tax rates. For the bottom 90% and 99% groups, statutory social security changes on the other hand explain up to one fifth, resp. a quarter of the variation whereas statutory income tax changes account for up to 62%, resp. 53%. Fourth, the AMTRs remain very highly correlated across large sections of the income distribution. The lowest correlation, between the top 1% and bottom 90%, is 0.70. The income specific AMTRs are all highly correlated with either of the series for all tax units: even the top 1% AMTR has a correlation of over 0.80 with the aggregate for all tax units. Finally, the two AMTR measures for all tax

units are very highly correlated and none of the results below are very sensitive to which measure is chosen.

The initial analysis of the tax rates highlights some of the advantages and challenges of using aggregate time series to identify tax elasticities. The full postwar history of federal tax legislation clearly offers a rich amount of potential identifying variation and includes many large increases and decreases in tax rates. Policy-induced fluctuations in tax rates are especially large at the top of the income distribution. A longer time series perspective can therefore be particulary revealing about the behavioral responses of high income taxpayers in a way that is not too dependent on any particular reform. At the same time, the dominant role of the income tax in the variability of income-weighted tax rates means that any results are likely to be representative only for the middle and higher income classes. Many low income households have no federal income tax liabilities and variation in social security contributions is more limited. The large cross-correlations of tax rates among income groups also point to a potentially important role for general equilibrium effects in shaping the income response to tax rates. The vast majority of federal tax reforms are aggregate events that may influence the wage distribution, monetary policy and interest rates, or other fiscal policy instruments such as government spending and corporate and other taxes. In reality, the tax transmission mechanism is complex and ETI estimates based on aggregate series do not lead directly to any strong conclusions about micro-level elasticities. On the other hand, macro elasticities that incorporate all these effects provide a more complete measure of the ultimate distortionary effects of marginal tax rates that is at least as useful for evaluating tax policies in practice.

3 Preliminary Elasticity Estimates From Univariate Regressions

Before moving on to the main analysis, it is useful to first consider some preliminary regressions for contrast. The preliminary results will also establish that the broader coverage of the income weighted tax rate series alone does not change the key conclusions of existing studies that use similar aggregate data. Saez (2004), Saez et al. (2012) and Piketty et al. (2014) estimate aggregate elasticities in time series regressions of income (before deductions and exemptions) or top income shares on net-of-tax rates and polynomials of time. Using AMTR series covering 1960-2000 and including linear and quadratic trends, Saez (2004) finds an elasticity for all tax units of 0.20 that is not statistically significant. Separate regressions by income group result in

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a highly significant value of 0.50 for the top 1% and zero for the bottom 99%. Using the top 1% income share instead of the level and adding a cubic time trend, Saez et al. (2012) obtain a highly significant top 1% elasticity of 0.58 in the 1960-2006 sample. Piketty et al. (2014) use series for top statutory rates from 1913 to 2008 and obtain highly significant top 1% ETIs of 0.27 and 0.30 in the level and share regressions with a linear trend. Using the 1946-2012 AMTR series and the same specifications, I obtain a tightly estimated top 1% elasticity close to 0.60 in the level and share regressions and lower insignificant values in the others.[10] As in Saez (2004), instrumenting with statutory changes to avoid endogeneity related to tax progressivity has little effect on the results. Static regressions with basic time controls therefore produce results in line with a zero or small overall response and a moderately large response at the top. The latter remains outside of the range obtained in the short run for the 1980s reforms. Unfortunately, there are two broad reasons why these regressions may not yield reliable estimates of the causal effect of tax rates on reported income. The first reason is the failure to account for the dynamics of tax rates and the timing of the behavioral response. The second reason is the endogeneity of tax policy decisions.

If tax rate changes are permanent, the elasticity in level regressions measures the eventual long run response and should be insensitive to timing. If tax rates changes are instead transitory, than the timing of the income response becomes very important. In reality, many tax reforms affect tax rates only temporarily by including sunset provisions or because of subsequent reforms in the opposite direction. In the extreme case where tax rates are uncorrelated over time, the regressions will detect no effects if for instance the income response occurs entirely in years before or after the tax change. Any measurable response is likely to be partially delayed in practice, which can lead to a downward bias in the elasticity estimate. One reason is that statutory tax changes occur throughout the year before filing such that the full income response may not be observed until the first year following the change. In addition, tax rates may also impact on investment and other dynamic decisions with lagged effects on reported incomes. There are also good reasons to believe that income responses partially lead tax rate changes. Many statutory tax rate changes are phased in gradually

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[10]In the 1946-2012 sample, the Saez (2004) level regressions yield values of 0.30 for all tax units, 0.61 for the top 1% and 0.37 for the bottom 99%. Only the top 1% estimate is statistically significant. The top 1% share regression of Saez et al. (2012) yields a highly significant value of 0.59 in the full sample. As in the original papers, I used AMTRs for the federal income tax only.

over multiple years or are implemented with long delays.[11] In response to future changes in marginal tax rates, forward looking agents have incentives to allocate income generating activities optimally across time. There is indeed substantial empirical evidence for such anticipatory effects to taxes.[12] The sign of the bias due to tax foresight is ex ante ambiguous and depends on the relative strength of intertemporal subsitution versus income effects, among other things. Regardless, the complicated intertemporal linkages between tax rates and incomes cast doubts on the results from the static regressions.

The other major concern is that instrumenting with statutory changes alone does not address the endogeneity of tax policy itself. Legislative reforms have a variety of motivations that are hardly independent of other influences on incomes or income shares. Tax policy has been actively used for macroeconomic stabilization and has systematically responded to temporary changes in military spending or other budgetary needs.[13] Given a relatively broad consensus for expansionary effects of government spending, see Ramey (2011b), both channels make tax rates procyclical and induce a downward bias in the elasticity estimates.[14] Bracket creep also remains an important source of reverse causality. Three of the largest rounds of statutory income tax rate cuts (part of reforms in 1964, 1981 and 2001) each followed periods of substantial bracket creep and effectively restored tax rates to historical averages.[15] Sectoral shifts, demographic trends, trade policies or changing political preferences are among the many other possible factors that simultaneously shape the income distribution and tax policy decisions. Static regressions with time polynomials are unlikely to control for even the most important of all of these confounding factors.

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[11]This is the case for instance for the marginal rate changes under the Revenue Act of 1964, the Economic Recovery Tax Act of 1981, the 1986 Tax Reform Act or the Economic Growth and Tax Relief Reconciliation Act of 2001. Most adjustments to social security contribution rates have been implemented with multi-year lags.

[12]Kueng (2014) finds evidence in municipal bond yields that financial markets forecast federal tax rates remarkably well. The public finance literature documents anticipatory effects for a number of reforms, see Saez, Slemrod and Giertz (2012). For theory and evidence of real anticipatory effects, see Yang (2005), House and Shapiro (2006), McGrattan (2012), Mertens and Ravn (2012) and Leeper, Walker and Yang (2013).

[13]The Revenue Acts of 1950 and 1951 increased taxes to finance the war efforts in Korea. The Revenue and Expenditure Control Act of 1968 imposed a temporary 10 percent surcharge to prevent the economy from overheating and finance the escalation of the Vietnam war. The Tax Relief Reconciliation Act of 2001 introduced a new 10% low income tax bracket to cushion the economic slowdown. The vast majority of increases in social security rates fund benefit expansions. The temporary cut in contribution rates under Obama in 2011 and 2012 was motivated by the continued weakness in the US economy. See Pechman (1987) or Romer and Romer (2009) for historical background and more examples.

[14]Parker and Vissing-Jørgensen (2010) document the procyclicality of top income shares. Ceteris paribus, procyclical tax rates then lead to downward bias for higher incomes also in income share regressions.

[15]Figures 2 and 4 (lower left panels) show that, while there were no major statutory income tax increases in the 1970s, high inflation and bracket creep caused AMTRs to rise by 6 to 8 percentage points. The 1955-1963 period as well as the mid to late 1990s also saw no significant legislative changes but rising tax rates due to (mostly real) increases in incomes.

To address some of these issues, I follow Slemrod (1996) and adopt specifications that include dynamic terms as well as a number of control variables. The reported income measures are in constant 2010 dollars per tax unit and are the same as in Piketty and Saez (2003, 2007). These measures include all sources of market income before deductions and exemptions but exclude realized capital gains and government transfers. The tax rates are those in Figure 1 and include federal payroll taxes. Table 2 shows results for regressions of changes in log income on changes in the log net-of-tax rate of income group j, as suggested by equation (4):

$$\Delta \ln(income_t^j) = \beta \Delta \ln(1 - AMTR_t^j) + [\text{controls}] + u_t \ , \ \text{and} \tag{5}$$

$$\ln(income_{t+1}^j) - \ln(income_{t-1}^j) = \gamma \Delta \ln(1 - AMTR_t^j) + [\text{controls}] + v_t. \tag{6}$$

where Δ denotes the annual difference. By using differences instead of levels, these regressions aim for short rather than long run elasticities. The first equation includes annual reported income and tax rate changes, whereas the second equation uses two year income growth as the regressand. To the extent a tax change persists into the subsequent year, the second regression potentially produces a more meaningful short run estimate by measuring the income response after the first full year following a tax change, see also Barro and Redlick (2011). I focus on income levels rather than income shares primarily because of the high correlations of tax rates among the income groups and all of the prior evidence that elasticities vary with income. Another reason not to use income share regressions is the potential for spill-over effects of tax rate changes for one group on incomes of the others.

Panel A first presents results for the regressions in (5) and (6) without including any additional controls. This yields short run elasticities that range from 0.55 for the top 1% percent to -0.49 for the bottom 90% in the same year of the tax change, and elasticities of 0.84 for the top 1% to -0.52 for the bottom 90% in the following year. As before, only for top incomes is there evidence for positive elasticities that are statistically significant at conventional confidence levels. The ETIs outside the top 1% and for all tax units are generally not significantly different from zero at either horizon. For the bottom 90% and 99% the same year estimates are significantly negative, suggesting that tax rate increases lead to higher income growth in those groups.

To mitigate concerns about timing and endogeneity, Panel B includes two lags of income and net-of-tax rates of group j as well as a large number of other lagged macroeconomic variables as controls.[16] The predetermined variables are assumed to jointly contain information about the relevant history of events before time t that determine income and tax rates from time t onwards. These past events include tax rate changes, announcements of future tax rate changes, cyclical and other fiscal shocks or any other relevant causal factors that continue to influence current and future income and tax rates. Panel C instruments with the statutory changes in Figure 4 to further correct for any contemporaneous influence on income that also has an effect on tax rates because of progressivity. The results in Table 2 show that adding controls and instrumenting with statutory changes each raises the point estimates relative to the simple OLS estimates. The subsequent year top 1% elasticity increases to just above one in both panels B and C and instrumentation results in some evidence for a significant effect also in the top 5 to 1%. The point estimates for the bottom 99% and 90% become positive or only mildly negative but remain insignificant. The first stage F-statistics are large in all cases, which is not surprising given that changes in AMTRs are predominantly due to statutory changes.

One conclusion from Table 2 is that switching to a short run specification and including richer controls raises the top 1% ETI from 0.6 to around 1. Despite being based on the entire postwar sample, this value is now more firmly in the range of short run responses associated with the 1980s reforms, which contradicts the view that these reforms were large anomalies. At the same time, the main conclusions of Saez (2004), Saez et al. (2012) and Piketty et al. (2014) remain intact. Moving outside of the top 1% or 5%, the elasticities drop off sharply and are generally insignificant. Based on the results in Table 2, the evidence for a sizeable response outside the top 1% or in the aggregate appears weak or nonexistent. The relatively large short run elasticities for the top 1% also do not contradict more modest long run responses. As Slemrod (1995, 1996) has documented for the 1986 reform, much of the short run response may be due to transitory timing and avoidance effects rather than changes in real economic activity.

The key argument in this paper is that none of the reported income regressions in Table 2 or the existing

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[16]To make a clear comparison, the control set is identical as in the vector autoregressions of Section 4 and includes two annual lags of real GDP, the unemployment rate, inflation, the federal funds rate, government spending, the change in government debt held by the public and the real stock market price as well as dummies for 1949 and 2008.

literature fully resolves the endogeneity of tax policy. If any of the contemporaneous influences on income, such as cyclical or budgetary shocks, also systematically influences tax policy, reverse causality remains a concern. As the next section shows, addressing this concern has important consequences for the results.

4 Dynamic SVAR Estimates of the Income Response to Marginal Tax Rates

This section presents ETI estimates from structural vector autoregressive models (SVARs). Introduced by Sims (1980), SVARs are flexible dynamic models for interrelated multiple time series that first isolate unpredictable variation in policy and outcome variables and subsequently sort out the contemporaneous causal relationships by imposing identifying restrictions. In macroeconomics, SVAR models have been influential for evaluating the effects of monetary and fiscal policy interventions as well as other aggregate shocks.[17] The main strategy for measuring exogenous changes to marginal tax rates in this paper is based on the narrative analysis of Romer and Romer (2010) and the identifying restrictions proposed by Mertens and Ravn (2013, 2014) and Stock and Watson (2008, 2012). The methodology combines the quasi-experimental approach with traditional SVAR analysis and differs from univariate regressions in several ways. First, it emphasizes the need for including a sufficiently rich set of lagged macroeconomic controls to isolate unanticipated variation in tax rates and reported income. Second, an exogenous component of the unpredicted variation in tax rates is identified using a selection of policy reforms that are not or less likely to be driven by other contemporaneous events, such as recessions or wars, and that are not obviously anticipated because they were legislated in previous years. Third, the model includes a variety of other endogenous variables in a dynamic system, which enables the estimation of the full dynamic income effects and allows for general feedback mechanisms. The model also identifies the expected future trajectory of tax rates, which is important for interpreting ETI estimates. Finally, by including GDP and the unemployment rate as endogenous variables, the SVAR model reveals whether reported income effects are also associated with important changes in real economic activity.

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[17]See Ramey (2015) for a recent survey.

4.1 Structural VAR Methodology

Consider a general representation of the dynamics of total aggregate income

$$\ln(income_t) = d_{1t} + A_1(L)v_{t-1} + \zeta_e v_t^o + \eta v_t^\tau \qquad (7)$$

where d_{1t} captures all deterministic terms, $A_1(L)$ is a lag polynomial of potentially infinite order and $v_t = [v_t^\tau, v_t^{o\prime}]^\prime$ is a vector that contains structural shocks with $E[v_t] = 0$, $E[v_t v_t^\prime] = \Sigma_v$ is a diagonal matrix and $E[v_t v_{t-j}^\prime] = 0$ for $j \neq 0$. The vector of shocks consists of exogenous innovations in tax rates v_t^τ as well as all other impulses v_t^o to income dynamics. The parameter η measures the contemporaneous impact of an unanticipated change in taxes on income. Let X_t be a vector of control variables and consider

$$\ln(1 - AMTR_t) = d_{2t} + A_2(L)v_{t-1} + \xi_e \ln(income_t) + \xi_x X_t + v_t^\tau, \qquad (8)$$

$$X_t = d_{3t} + A_3(L)v_{t-1} + \zeta_x v_t^o + \theta v_t^\tau, \qquad (9)$$

where d_{2t}, d_{3t} capture deterministic terms and $A_2(L), A_3(L)$ are infinite order lag polynomials. The first equation specifies the behavior of the log net-of-tax rate for all tax units as a function of (i) the entire history of shocks; (ii) a contemporaneous tax rate shock v_t^τ; and (iii) additional variables X_t. The parameters ξ_e and ξ_x capture any contemporaneous feedback from income levels or any element of X_t on tax rates. The second equation describes the dynamics of X_t with θ measuring the short run impact of tax shocks on X_t. Together, equations (7)-(9) provide a representation of all the variables as functions of histories of unobserved i.i.d. random variables, one of which is an aggregate shock to marginal tax rates. Since the system allows for all possible dynamic causal effects, essentially any linear dynamic economic model yields a representation of this general form.

Identifying exogenous innovations to tax rates, i.e. the structural shock v_t^τ, requires some assumptions. The first key assumption is that there exists a finite order vector autoregressive (VAR) representation of the joint dynamic behavior of $\ln(income_t), \ln(1 - AMTR_t)$ and X_t. This requires that there are (at least) as many shocks as endogenous variables, $dim(X_t) = dim(v_t^o) - 1$, and that a finite number of lags of the en-

dogenous variables contains (approximately) the same information as the entire history of shocks. The VAR representation is given by

$$
\begin{bmatrix}
\ln(1 - AMTR_t) \\
\ln(income_t) \\
X_t
\end{bmatrix}
= d_t + B(L)
\begin{bmatrix}
\ln(1 - AMTR_{t-1}) \\
\ln(income_{t-1}) \\
X_{t-1}
\end{bmatrix}
+
\begin{bmatrix}
u_t^{AMTR} \\
u_t^{income} \\
u_t^x
\end{bmatrix},
\tag{10}
$$

where d_t contains deterministic terms, $B(L)$ is a lag polynomial of finite order $p-1$ and p is the lag length. If the set of endogenous variables is informationally sufficient, then the reduced form residuals u_t^{AMTR}, u_t^{income} and u_t^x are related to the structural shocks v_t^τ and v_t^o by

$$
\begin{aligned}
u_t^{AMTR} &= v_t^\tau + \xi_e u_t^{income} + \xi_x u_t^x \\
u_t^{income} &= \eta v_t^\tau + \zeta_e v_t^o \\
u_t^x &= \theta v_t^\tau + \zeta_x v_t^o
\end{aligned}
\tag{11}
$$

The validity of (11) is in practice determined by the selection of variables included in X_t and the lag length p, both of which determine the span of the conditioning information set. An appropriate choice of X_t and p ensures that the VAR residuals correspond to unpredictable variation in the variables and therefore that all anticipated changes in marginal tax rates are controlled for.

The VAR residuals u_t^{AMTR}, u_t^{income} and u_t^x are straightforward to estimate by OLS, but more assumptions are needed to identify the exogenous innovation to tax rates v_t^τ. The identification strategy follows exactly Mertens and Ravn (2013, 2014) and Stock and Watson (2008, 2012) and relies on the availability of a proxy measure m_t for the latent structural tax shock v_t^τ that satisfies the identifying assumptions

$$
E[m_t v_t^\tau] \neq 0 ,
\tag{12}
$$

$$
E[m_t v_t^o] = 0 .
\tag{13}
$$

18

The first condition states that m_t is contemporaneously correlated with the shock to marginal tax rates. The second condition requires m_t to be contemporaneously uncorrelated with all other structural shocks. When these conditions hold, the variable m_t can be used as an instrument to obtain η, θ, ξ_e and ξ_x and v_t^τ as follows:

1. Regress u_t^{income} and u_t^x on u_t^τ using m_t as instruments. The residuals in these regressions are n_t^e and n_t^x.

2. Regress u_t^{AMTR} on u_t^x and u_t^{income} using n_t^e and n_t^x as instruments, which yields unbiased estimates of ξ_e and ξ_x. The residual in this regression is the estimate of v_t^τ.

3. Regress u_t^{income} and u_t^x on v_t^τ to obtain estimates of η and θ.

Once the contemporaneous impact of an unexpected exogenous tax shock is obtained, the effects in subsequent years can be traced out using (10). The resulting impulse response functions measure the expected dynamic adjustment of all the endogenous variables to the initial shock to marginal tax rates.

4.2 New Measures of Tax Rate Shocks and VAR Specification

The key part of the analysis is the construction of the proxy m_t for exogenous unanticipated changes in average marginal tax rates. This variable ideally has the highest possible correlation with the true tax rate surprises faced by economic agents and cannot be correlated with other contemporaneous macroeconomic influences. To obtain a variable that optimally meets these requirements, I use new measures of the AMTR impact of a selection of historical changes to income tax rates and/or social security contributions.

The first important step in constructing m_t is to collect instances of variation in tax rates that can reasonably be considered to be contemporaneously exogenous. Using a variety of historical sources, Romer and Romer (2009) conduct an extensive narrative analysis of all major postwar federal tax reforms. They propose a classification according to the primary motivation for the reforms into four main categories: responding to a current or planned change in government spending, offsetting other cyclical influences, addressing an inherited budget deficit, and attempting to increase long-run growth. The last two categories aim specifically at isolating tax policy changes that are not systematically related to other concurrent macroeconomic events.[18]

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[18]Romer and Romer (2010) use the liability impact of tax reforms falling in these categories to identify tax multipliers. Barro and Redlick (2011) and Mertens and Ravn (2013, 2014) exploit the same classification for identifying the effects of tax policy.

I adopt the same classification and focus on tax changes induced by all reforms affecting personal taxes that Romer and Romer (2009) classify as motivated by long-run considerations or as arising from inherited deficit concerns. All policy interventions classified as spending driven or business cycle related are omitted. In practice, this means that for instance the temporary wartime income tax hikes, the 2001 income tax cut and the increases in social security rates funding benefit expansions are excluded.[19]

The second step in the construction of m_t is to obtain measures that are highly correlated with the true surprise innovations to personal tax rates. Many of the reforms are implemented with a delay or have gradual multi-year phase-ins. To avoid policy variation with no or little element of surprise, I exclude all tax changes induced by reforms that were legislated at least one year before becoming effective. This means for instance that most rate cuts under the 1981 Economic Recovery Tax Act, which despite its name Romer and Romer (2009) view as mostly ideologically motivated, are not included in m_t. After the elimination of tax changes with delayed implementation, the selection procedure yields a total of 15 tax reforms between 1946 and 2012 with significant and immediate impact on personal tax liabilities. Appendix A provides a list as well as a description of the main provisions in each of these reforms.

The precise impact of the selected tax reforms is measured by scoring the AMTR impact of the legislative change relative to pre-existing law. The scoring proceeds in a similar way as the calculation of the statutory component of annual changes in AMTRs shown in Figure 4. However, instead of comparing to the previous year AMTR, now the change is measured relative to the tax code that would have prevailed under prior law, i.e. in the absence of the legislative change. More precisely, the estimated impact in year t of a given selected tax reform is the difference between a first counterfactual tax rate, calculated using the year $t-1$ income distribution and the current law rates and brackets deflated by any automatic adjustments between $t-1$ and t, and a second counterfactual tax rate based on the year $t-1$ income distribution and the prior law rates and brackets. The latter are obtained from official government publications sourced in appendix A. After scoring the tax reforms in this manner, eight out of the selected 15 tax reforms lead to a measurable change in AMTRs. The scores are shown in Table 3 and reflect key provisions of many of

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[19]The temporary Obama payroll tax cuts postdate Romer and Romer's (2009) analysis but are excluded for being primarily motivated by the continuing weakness in the US economy following the 2007-2009 recession.

the more important reforms, such as the tax cuts of 1948 and 1964, the Tax Reform Act of 1986, the Bush-Clinton tax increases as well as the acceleration in 2003 of earlier tax cuts. The proxy m_t for exogenous unanticipated changes in aggregate average marginal tax rates are the scores in the years of the tax reforms shown in the first column of Table 3, and zeros in all other years.

Several features of the proxy for unanticipated AMTR changes merit further discussion. First, the number of observations is small. The eight reforms listed in Table 3 all include direct changes to the basic income tax rate schedules. The other seven selected reforms contain only provisions altering tax credits, deductions or coverage, which affect tax liabilities but do not have any direct AMTR impact, or at least not one that is easily picked up by the static scoring method. Appendix B performs an analysis with alternative proxies based on the tax liability impact of all 15 reforms and also verifies the sensitivity to the inclusion of particular reforms such as those in the 1980s. What is important is that the eight benchmark reforms still capture a large amount of variation in marginal tax rates. Virtually all of this variation stems from federal income tax changes. Most changes to social security rates are excluded because they fund benefit expansions and/or have long implementation lags. Table 3 also provides the scores for individual income groups. With only one minor exception, the reforms change AMTRs in the same direction for all income groups, but there is also some heterogeneity across reforms in the relationship between income and the size of the change. In particular, the tax changes are usually much larger for higher income taxpayers. There are six cuts in tax rates, three under Democratic and three under Republican presidents.[20] There are two tax increases, one under a Democratic and the other under a Republican presidency. There is therefore no obvious relation with presidential party affiliation. Reforms lowering income tax rates are generally more frequent, which is not surprising given the lack of indexation in the tax code. Finally, the often lengthy political and legislative processes preceding tax reforms mean that the eventual marginal tax rate changes were certainly to some extent anticipated prior to their enactment. This fact does not violate the identifying assumptions since only contemporaneously exogeneity with respect to other macroeconomic shocks is required. As long as there is sufficient randomness in the timing and/or size of the changes, the proxies remain useful measures that are correlated with the underlying surprise changes. The relevance of the proxies will be confirmed later

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[20] Although the 1948 reform was passed after a Truman veto.

through formal tests. Unfortunately, the contemporaneous exogeneity of the series in Table 3 is not testable since there are no overidentifying restrictions.

In addition to the log net-of-tax rate and log income levels, the VAR model includes a fixed set of controls X_t: *Log real GDP per tax unit*, the *unemployment rate*, the log *real stock market index*, *inflation* and the *federal funds rate*. These variables generally capture business cycle conditions, interactions with monetary policy as well as the effects of bracket creep. The VAR also includes log *real government spending per tax unit* (purchases and net transfers) and the log change in *real federal government debt per tax unit*. These variables are included to capture interactions with other current and past fiscal policies, in particular since tax changes are often motivated by concerns about government deficits.[21] The (recent) history of these variables as well as income and tax rates are among the most likely to contain the relevant information to isolate the unanticipated short run innovations in tax rates and income.[22] Appendix B verifies robustness to the inclusion of a variety of additional control variables.

The benchmark VAR includes two lags ($p = 2$) of the nine endogenous variables for an effective sample covering 1948-2012 and includes dummy variables for 1949 as well as 2008 as additional regressors. Standard lag selection criteria recommend one to three lags. However, inspection of the residuals indicates a minimum of two lags is required to eliminate evidence of residual autocorrelation. Appendix B provides more discussion and presents results for alternative lag length choices. The inclusion of the 1949 and 2008 dummies, both recession years, is not innocuous. The first and last few years in the sample are periods of relative macroeconomic turbulence and unusual policy variation associated with the end of WWII and the 2007-2009 financial crisis. As a result, variations in the start and end points of the sample leads to some instability in the VAR coefficients and impulse responses. Rather than dropping these periods from the sample, as is common practice, the dummy approach yields results that are more stable across subsamples while preserving the major 1948 tax reform as a source of identifying variation.[23] The sensitivity to the sample

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[21] Appendix A provides precise variable definitions and sources.

[22] Based on these variables, the VAR model is for instance quite successful in capturing many of the known pre-announced tax rate changes. Results are available on request.

[23] Romer and Romer (2010) and Barro and Redlick (2011) report the sensitivity to inclusion of the 1948 tax reform and use samples starting in 1950 for their main analysis. I found the results to be much more sensitivity to a dummy for the 1949 recession than including the 1948 reform. Appendix B provides more discussion. Mertens and Ravn (2013) also focus on the 1950-2006

choice and dummies is discussed further below and in Appendix B.

4.3   The Response of Aggregate Income to Marginal Tax Rates

The first set of results is based on a VAR that includes aggregate reported income and the Barro and Redlick (2011) aggregate net-of-tax rate. Figure 5 depicts impulse responses to a one percent increase in the aggregate net-of-tax rate identified using the proxy for aggregate tax rate surprises in the first column of Table 3. The figure also displays 90% and 95% bootstrapped confidence intervals for a horizon up to 6 years.[24] The income responses are on a scale directly comparable to the coefficients in the regressions of Section 3.

Figure 5 (top left panel) shows that an unanticipated decrease in taxes has transitory effects on the average marginal tax rate. The initial decrease in the tax rate persists almost perfectly into the second year. From the third year onwards the tax rate gradually reverts to the level expected prior to the shock. Although statutory changes in federal tax rates are usually legislated as permanent, the VAR estimates imply that in expectation policy shocks are fully reversed by sunsets, subsequent reforms or bracket creep after five or six years. The estimated dynamic adjustment of tax rates has two important implications for the interpretation of the results. First, since the tax rate decrease persists almost perfectly into the second year, the second year income response provides a plausible estimate of the short run ETI associated with a full year of lower tax rates. Second, the transitory nature of changes in tax rates implies a potentially important role for timing and intertemporal substitution effects.

Reported income per tax unit (bottom right panel) reacts positively to the unanticipated decrease in the AMTR. Income rises on average by 0.71% in the year of the tax cut and by 1.37% in the following year. Both estimates are significant at the 1% level and contrast sharply with the low and insignificant estimates for the aggregate elasticities in the univariate regressions in the first column of Table 2. The income response

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sample. Saez (2004) and Saez et al. (2012) use data for 1960-2000 and 1960-2006 respectively.

[24]The intervals are computed using a recursive wild bootstrap using 10,000 replications, see Gonçalves and Kilian (2004). Define $Y_t = [\ln(income_t), \ln(1 - AMTR_t), X_t']'$ and $u_t = [u_t^{AMTR}, u_t^{income}, u_t^{x\prime}]'$. Bootstrap draws Y_t^b are generated recursively using $\hat{B}(L)$ and $\hat{u}_t e_t^b$, where $\hat{B}(L)$ and $\hat{u}_t$ denote the VAR estimates and e_t^b is the realization of a random variable taking on values of -1 or 1 with probability 0.5. I also generate a draw for $m_t^b = m_t e_t^b$, re-estimate the VAR for Y_t^b and apply the identifying restrictions. The percentile intervals are for the resulting distribution of impulse response coefficients. This procedure requires symmetric distributions for the VAR residuals and m_t but is robust to conditional heteroscedasticity.

remains significant at the 5% level for four years and peaks at almost 1.50% in the third year. From the fourth year onwards, incomes gradually decrease to levels expected prior to the shock, although the effects appear more persistent than the decline in the AMTR. A cut in the marginal tax rate also leads to a significant increase in real GDP per capita (top right panel) and a persistent and significant decline in the unemployment rate (bottom right panel). Real GDP rises by 0.44 percent on impact and by 0.78 percent in the third year. The unemployment rate falls by 0.23 percentage points on impact and by 0.39 percentage points in the third year. Similar to the response of income reported on tax returns, the output and unemployment responses are hump-shaped and more persistent than the change in tax rates. The responses of GDP and unemployment indicate that the positive response of income reported to tax authorities has important real effects.

The first column in Panel A of Table 4 presents the elasticity estimates and the associated confidence intervals as obtained from the first two SVAR impulse response coefficients. The second column in Panel A shows that replacing the Barro and Redlick (2011) AMTR with the Piketty and Saez (2003) AMTR yields very similar results. The first two columns in Panels B and C in Table 4 report results from additional SVARs in which total income is replaced by real wage income per tax unit and, respectively, real non-wage income per tax unit. The data on the composition of income is taken from Piketty and Saez (2003, 2007). Non-wage income includes entrepreneurial income (S-corporation, partnership and sole proprietorship profits), rents, dividends and interest. Table 4 shows that both aggregate wage and non-wage income respond significantly to a cut in marginal tax rates. Based on the following year impact, the estimated elasticity of wage income is 0.90, which is significant at the 5% level. To the extent wage income is less prone to avoidance and timing effects, which certainly may be questioned for taxpayers with the highest incomes, this is consistent with a real economic response. The elasticity of non-wage income is 3.51 and is significant at the 1% level. Reported non-wage income therefore appears to be especially sensitive to the level of marginal tax rates. This may be due to real substitution effects, but potentially also to the fact that these sources of income are generally easier to time or conceal.

The benchmark SVAR model yields a short run ETI estimate for all tax units of 1.37, as measured by the second year impulse response coefficient, with a 95% confidence range of 0.50 to 2.33. Appendix B

conducts a number of checks to assess the robustness to various specification and sample choices. The ETI estimates are not very sensitive to the inclusion of any particular tax act in the proxy, including the larger ones such as the 1948, 1964 or 1980s reforms. The inclusion of the dummies is more consequential: the point estimate declines to 1.15 when the 2008 dummy is omitted and to 0.96 when the 1949 dummy is dropped, although both estimates are highly significant. Restricting the sample to 1950-2012, 1950-2006 or 1960-2000 on the other hand raises the ETIs to 1.41, 1.50 and 1.40 respectively. In all these cases, the estimates remain highly significant. Using the AMTR series based on the Piketty and Saez (2003) income concept or the series that only capture the federal income tax component also yields somewhat larger ETI estimates. Appendix B also documents similar results for two alternative proxies based on official estimates of the tax liability impact of the full set of 15 tax reforms. One source of concern is that the selected tax reforms are systematically correlated with other policy changes. There is little historical or empirical evidence of correlation with spending changes, see Romer and Romer (2010) or Mertens and Ravn (2013), but changes in personal tax rates occasionally coincide with changes to corporate taxes in the same direction. An extended model that controls for simultaneous changes in corporate taxes using the methodology of Mertens and Ravn (2013) results in a similar ETI estimate of 1.35. Various additions to the set of control variables also have no major impact and all point estimates exceed one and remain significant at least at the 5% level.

4.4 The Response to Marginal Tax Rates at Different Income Levels

According to the SVAR model with data covering all tax units, reported income and real GDP rise significantly following cuts in marginal tax rates and unemployment falls. I adopt two different approaches to also provide more disaggregated evidence on the sensitivity of income to marginal tax rates. The left panel of Figure 6 shows dynamic ETI estimates obtained as the cumulated effect implied by regressions of annual log income growth on the contemporaneous value and lags of the aggregate tax shock v_t^τ identified in the SVAR in Section 4.3. This approach identifies elasticities associated with an unanticipated aggregate tax rate change, measured by the exogenous innovation to the AMTR series for all tax units. The elasticities reflect the income response when the marginal rate cuts are distributed across income groups according to the average distribution over the selected reforms. To express the dynamic response in terms of ETIs, the estimates are rescaled by the coefficient in a regression of the group specific net-of-tax rates on v_t^τ.

The other way of assessing how ETIs differ across income groups is based on estimating a separate SVAR for each income group. This results in the estimates shown in the right panel of Figure 6. The methodology in this case is the same as in the previous section, but the net-of-tax rate and income series for all tax units are replaced with the corresponding series for each income group. The impulse responses are identified using the income specific proxies shown in Table 3. This approach identifies ETIs associated with unanticipated changes in group specific tax rates. However, given the high correlation between tax rate changes implemented by the reforms, the resulting estimates will again also reflect effects from correlated tax rate changes for the other income groups. The distribution of those cuts across income groups however will now differ for every estimate and the associated change in the AMTR for all tax units may also very in size.

It is important to keep in mind that the ETIs under both approaches incorporate general equilibrium effects generated by the fact that marginal rates change for a larger fraction of tax payers than just the group considered.[25] If the correlation across tax rates were perfect, both approaches would be equivalent. In practice the correlation is imperfect, but the results are nevertheless similar. Alternatively, it is possible to include the disaggregated series within the same model and identify income group specific tax shocks in isolation. This avenue is pursued later in Section 5.2. Another strategy would be to study income shares or income differences between groups. Such a strategy however does not produce meaningful results if indeed the ETIs vary with income or if there are spill over effects, evidence for which I will present later in Section 5.2.

In both panels of Figure 6, the ETIs are positive at all of the horizons considered. The income responses are very similarly hump-shaped across income groups, generally peaking in the third year at values ranging from around 0.8 for the top 10-5% bracket up to 1.5 (right) and 1.75 (left) for the top 1% bracket. In the left panel the estimates start declining from the fourth year onwards, whereas in the right panel the effects appear somewhat more persistent. The top 1% elasticities are consistently the highest, but in sharp contrast to the results of the univariate regressions in Table 2, the elasticities are now also large for all other income groups. Panel A in Table 4 reports the first two coefficients of the impulse responses in the right panel of Figure 6,

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[25]Of course the same caveats of interpretation apply to the univariate regressions in Table 2.

corresponding to the same and following year tax elasticities, together with 95% bootstrap intervals. The top 1% elasticities are highly statistically significant. The following year estimate is 1.35 compared to 1.07 in the earlier regression instrumenting with all statutory tax changes. A major difference with the earlier results is that the SVAR-identified elasticities are large and statistically significant also at income levels outside of the top 1%. The following year elasticities for the top 5-1% and top 10-5% are 0.91 and 0.79 compared to 0.22 and 0.03 in Panel C of Table 2. Similarly, the bottom 90% elasticity rises from 0.36 to 0.98. Note that in Table 4 the ETIs by income group are all below and therefore seemingly inconsistent with the estimate for all tax units. This however is because of the differences in the distribution and overall size of the tax cuts across income groups. The estimates in the left panel of Figure 5 instead do aggregate consistently because the distribution of the cuts in this case is fixed. Taken together, both approaches provide clear evidence for responses to tax rates that extend well beyond the highest incomes alone. If tax payers outside the top 1% indeed have fewer avoidance opportunities then more broad-based responses of income taxes are indicative of real economic effects.

Panels B and C in Table 4 report SVAR evidence on the elasticities of wage and non-wage income for each group. Interestingly, the elasticities of wage income are the largest for the top 1% and bottom 90% brackets and in both cases equal 0.90. The bottom 90% wage income response is probably the least likely to be due to avoidance and is consistent with the impact on unemployment. The wage income elasticities are substantially smaller and insignificant for the brackets in between. For every income group, the elasticity of non-wage income is much larger. However, they are the largest in all brackets within the top 10%. The elasticity of non-wage income is generally statistically significant for all income groups except for the bottom 90%. There thus appears to be also some heterogeneity across income groups in the type of income that is sensitive to tax rates.

4.5   Discussion and Comparison with Earlier Evidence

The evidence for large ETIs and real economic responses to taxes is not an artefact of the vector autoregressive modeling approach per se. Instead, the main reason for the discrepancy with the time series evidence from public finance is the selection of tax reforms by Romer and Romer (2010) to address the endogeneity

of tax policy. To make this unambiguous, Table 5 reports results from additional instrumental variable regressions of income growth on net-of-tax rate changes. Each regression includes exactly the same controls as in Table 2 and also as in the income equations of the SVAR models in Table 4. The only difference with the regressions in Table 2 is the instrument for tax rates. Panel A of Table 5 presents the estimates when the instrument for each income group is the corresponding series shown in Table 3 with zero values in years without tax reforms. Panel B instead reports elasticities when the instrument is the aggregate tax shock v_t^τ identified in the SVAR of Section 4.3 with the data for all tax units.

By construction, the 'same year' 2SLS estimates in Panel A of Table 5 implicitly involve the same regressions as those underlying the SVAR impact coefficients in Table 4. The same year point estimates are therefore identical. The 'following year' 2SLS estimates now result from direct projections instead of inversions of the autoregressive coefficients in the VAR models. The following year SVAR and 2SLS estimators are both consistent but not identical in finite samples. The direct projection is less efficient but on the other hand permits the use of straightforward asymptotic inference formulas.[26] Table 5 reports traditional 95% Newey West confidence intervals whereas the SVAR confidence intervals were based on bootstrap methods. Table 5 also reports the first stage F-statistics on the excluded instrument. These are particularly insightful in Panel A because they are also the F-statistics that implicitly test the relevance of the proxies as instruments for tax rates in the SVARs of Table 4.

The 2SLS estimates in Table 5 confirm the large ETIs and establish the importance of accounting for the endogeneity of tax policy. In both panels, the 'following year' 2SLS elasticity estimates for the top 1%, 5% and 10% are essentially identical to the SVAR estimates and are highly significant. The ETIs for all tax units and for the bottom 90%-99% are slightly below those of the SVARs but are nonetheless much higher than after simply instrumenting with statutory changes. Based on the less efficient direct projections and standard asymptotic tests the evidence for statistical significance in the bottom groups is somewhat weaker. The ETI estimates for all tax units are between 1.07 and 1.24 and remain highly significant. The first stage F-statistics

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[26]The regressions in (6) effectively implement Jorda's (2005) local projections approach. Under the same assumptions, this alternative method for obtaining impulses response coefficients is asymptotically equivalent to SVAR impulse responses but can be somewhat more robust to model misspecification.

are large in all cases such that the marginal tax rate instruments are highly relevant.

From Tables 2 and 5 it is clear that each of the steps undertaken to control for confounding factors has the largest effects for the lower income groups. For the bottom 90%, adding the predetermined variables raises the elasticity from -0.52 to zero. Instrumenting with all statutory changes increases the ETI to 0.36, while instrumenting with the exogenous tax rate changes brings the estimate up to about 0.80. The same steps raise the top 1% elasticity by a smaller amount, from 0.84 into the 1.30-1.40 range. The likely explanation is simply that the fraction of endogenous variation in tax rates is decreasing in income. Federal income tax policy interventions, in practice the dominant source of useful identifying variation, explain a larger fraction of the tax rate variation at the top, see Table 1, and the same is true for the exogenous measures in Table 3. Measured by the R^2 coefficient, the proxies in Table 3 explain 48% of the variation in all statutory changes for the top 1% but this decreases with income to 25% for the bottom 90%. This indicates that cyclical and spending motivated policy changes are relatively more important for lower incomes. Another contributing factor is that top marginal rates are much more insulated from bracket creep. Consistent with the existing evidence, the ETIs do remain larger for top incomes. Besides possible heterogeneity in real substitution behavior, one plausible explanation is greater access to avoidance opportunities. Another possibility that the smaller tax rate changes for lower incomes do not always incur sufficient utility losses to induce changes in behavior, see Chetty (2012).

The size of the real economic responses estimated in the SVAR models is in a range consistent with many previous findings in the macro and labor supply literatures. The GDP impact for all tax units in Section 4.3 is roughly consistent with those in Romer and Romer (2010) and Mertens and Ravn (2013) after rescaling from net-of-marginal rates to the average rate measures used in these papers. The GDP response is about twice as large as the one found by Barro and Redlick (2011), but this may be due to any of several differences in specification or because their estimate omits the same year growth impact. Following Barro and Redlick (2011) and assuming a labor elasticity of output of one, the second year GDP response suggests a value for the elasticity of aggregate hours of 0.78. Evaluated at the mean unemployment rate in the sample, the unemployment response suggest an extensive labor supply elasticity of 0.41. Taking the difference yields an

intensive labor supply elasticity of 0.37. Based on the recent summary of the quasi-experimental evidence on the labor supply response to taxes, Chetty, Guren, Manoli and Weber (2013) consider values of 0.3 to 0.5 for the intensive margin and a value of 0.25 for the extensive margin as plausible. The SVAR responses are therefore consistent with the labor supply evidence on the intensive margin elasticity, while the extensive elasticity is perhaps moderately higher.

Taking a simple average of the benchmark SVAR and IV estimates of the (following year) ETIs and both tax rate measures yields a value of 1.2 for all tax units. The estimates from numerous variations in specification reported in Appendix B also average to a similar value. There obviously remains considerable uncertainty associated with all of the estimates, as the confidence intervals still cover a relatively wide range of values. This is unavoidable given the sample size and the extent of the postwar variation in marginal tax rates that can plausibly be classified as exogenous. The estimates should be interpreted as short-run macro elasticities associated with a persistent but transitory change in average marginal tax rates that incorporate general equilibrium effects as well as intertemporal substitution effects.

5 Additional Evidence for Real Responses to Marginal Tax Rates

The evidence for real economic effects of taxes remains open to several interpretations. Innovations in marginal tax rates simultaneously induce changes in after tax incomes. The stimulative effects on income and employment may therefore operate primarily through demand-side multiplier effects rather than direct effects on individual incentives. The disaggregated ETI estimates show significant responses across income groups, but these incorporate the effects of correlated tax rate changes among a large fraction of tax payers. It is therefore not clear whether for instance a tax reform targeting top incomes alone would also have important real economic effects. This section presents additional evidence for more specific counterfactual tax experiments to answer these questions.

5.1 Average versus Marginal Tax Rates

The Romer and Romer (2010) classification of postwar tax changes yields, after eliminating those with delayed implementation, a total of 15 plausible instances of tax policy 'shocks'. The results so far are based

on a subset of 8 of those instances corresponding to tax reforms with a direct impact on AMTRs through changes in statutory rate schedules. The other 7 tax policy changes had nontrivial effects on average tax rates but did not alter the basic marginal rate schedules relative to prior law. Through modifications to tax credits, exemptions, deductions or coverage, the impact on average marginal tax rates of these policy changes is either zero or smaller and more indirect. This section extends the SVAR model to make use of these additional policy shocks for identification. In addition to the proxy for shocks to the AMTR for all tax units used in Section 4.3, the identification strategy relies on an additional proxy for shocks to average tax rates that allows separating the causal effects of average and marginal tax rate shocks. The results present insights into the importance of traditional multiplier effects for explaining the income response to tax rate changes.

The VAR model is identical to Section 4.3 but now also includes the (log) average tax rate as an additional endogenous variable, defined as total revenue and contributions as a ratio of the Piketty and Saez (2003) measure of aggregate market income.[27] Consider a vector of two correlated exogenous innovations to the AMTR and the average tax rate, $\bar{v}_t^\tau = [v_t^{AMTR}, v_t^{ATR}]'$ with $E[\bar{v}_t^\tau] = 0$, $E[\bar{v}_t^\tau \bar{v}_t^{\tau\prime}] = \Sigma_\tau$ nonsingular but not necessarily diagonal, and $E[\bar{v}_t^\tau \bar{v}_{t-j}^\tau] = 0$ for $j \neq 0$. The assumption is thus that exogenous changes in tax policy instruments have imperfectly correlated impacts on average and marginal rates. Also, let $\bar{u}_t^\tau = [u_t^{AMTR}, u_t^{ATR}]'$ denote the reduced form VAR residuals associated with the AMTR and average tax rate series. Analogous to (11), the VAR residuals are assumed to be related to the structural shocks by

$$
\begin{aligned}
\bar{u}_t^\tau &= \bar{v}_t^\tau + \xi_e u_t^{income} + \xi_x u_t^x \\
u_t^{income} &= \eta \bar{v}_t^\tau + \zeta_e v_t^o \\
u_t^x &= \theta \bar{v}_t^\tau + \zeta_x v_t^o \ .
\end{aligned}
\tag{14}
$$

The idea is to exploit the historical variation in the impact on average and marginal rates across tax reforms to isolate the effects of average and marginal tax rate changes. The additional proxy for average tax rates required for identification is an annual version of the quarterly narrative measures of legislative changes in federal individual income and payroll taxes described in Mertens and Ravn (2013). Similar to Romer and

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[27]See appendix A for the precise definition.

Romer (2010), innovations to average tax rates are measured by dividing the ex ante estimated impact of the selected tax reforms on tax liabilities by (previous year) total income. The tax liability impact estimates are obtained from official contemporaneous government sources such as the Joint Committee on Taxation or the Congressional Budget Office. Appendix A provides the numbers and source documents.

Denoting the vector of marginal and average tax rate proxies by $\bar{m}_t$, the identifying assumptions are

$$E[\bar{m}_t \bar{v}_t^{\tau\prime}] = \Phi , \tag{15}$$

$$E[\bar{m}_t v_t^{o\prime}] = 0 . \tag{16}$$

where Φ is a unknown nonsingular 2×2 matrix. As before, the second condition imposes contemporaneous exogeneity of the tax policy changes and is motivated by Romer and Romer (2009)'s narrative analysis. The first condition states that the two measures of shocks to average and marginal rates are contemporaneously correlated with the true exogenous surprise innovations. Naturally, both must also be allowed to be mutually correlated such that Φ is generally not diagonal. As such, conditions (15)-(16) do not suffice to identify v_t^{AMTR} and v_t^{ATR} separately. However, they are sufficient to identify the impulse responses to any linear combination of shocks to marginal and average tax rates. The exogenous variation in tax rates can be recovered as follows:

1. Regress u_t^{income} and u_t^x on $\bar{u}_t^{\tau}$ using $\bar{m}_t$ as instruments. The residuals in these regressions are n_t^e and n_t^x.

2. Regress $\bar{u}_t^{\tau}$ on u_t^x and u_t^{income} using n_t^e and n_t^x as instruments. The residuals in these regressions are $\bar{n}_t^{\tau}$.

These auxiliary regressions isolate $\bar{n}_t^{\tau}$, which is a vector containing the correlated exogenous innovations to the average and marginal rates. The covariance of $\bar{n}_t^{\tau}$ is an estimate of Σ_τ and any decomposition $\Sigma_\tau = DD'$ yields a set of impact coefficients to two independent linear combinations of exogenous shocks to average and marginal tax rates. The contemporaneous impact of these combinations of tax rate shocks on all the other variables can be obtained by regressing u_t^{income} and u_t^x on $D^{-1}n_t^{\tau}$. The dynamic responses of all the variables can subsequently be traced out using the estimated VAR system.

Two particulary meaningful decompositions of the exogenous variation in average and marginal tax rates

are the upper and lower triangular (Choleski) factorizations of Σ_τ. Ordering the marginal tax rate first, the first column of the upper triangular factorization yields the response to an unanticipated counterfactual tax reform that affects marginal rates but has no impact on statutory average tax rates. The associated income response is therefore due to a change in marginal rates since any direct effect on average tax rates and disposable income is restricted to be zero on impact. The Tax Reform Act of 1986, with large marginal rate reductions accompanied by various revenue enhancing provisions, is probably the closest historical equivalent of this type of tax experiment. The second column of the lower triangular factorization corresponds to a counterfactual tax reform that changes average tax rates but leaves statutory marginal rates unchanged. The associated income response is likely to be predominantly generated by changes in disposable income and multiplier effects rather than direct incentive effects, since at least on impact the statutory effect on marginal tax rates is imposed to be zero. Many of the postwar tax policy interventions are of this type.

Figure 7 depicts the dynamic responses to the counterfactual tax experiments. For a clear comparison, Panel A first reports the case of marginal tax rate shock that does allow for impact on the statutory average tax rate.[28] Panel A establishes that including the average tax rate in the VAR system and using all of the exogenous reforms does not substantively alter the earlier results. The income response to an increase in the net-of-tax rate of one percent is very similar in size to Figure 5 and remains significant. Including the exogenous reforms without direct marginal rate provisions does not lead to tighter estimates and, partly because of the larger system, the confidence bands are slightly wider than in the benchmark model. Not surprisingly, the average tax rate declines following a marginal rate cut.

Panel B in Figure 7 depicts the response to a marginal tax rate cut of the same size but now without any statutory change in the average tax rate. The associated response of income is very similar to panel A as well as the benchmark estimates and is highly significant. The average tax rate still declines as a result of the estimated net feedback from incomes and the variables in X_t, but the decline is much smaller and is in contrast to panel A not statistically significant. Panel C shows the response to a one percent average tax rate cut imposing no statutory impact on the AMTR. Even after allowing for indirect endogenous feedback,

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[28]The impulse response in this case is based on the first column of the lower triangular decomposition of Σ_τ.

the response of the AMTR remains close to zero throughout. The main finding is that, in sharp contrast to the results for marginal tax rate changes after controlling for average tax rates, there is no evidence that income responds strongly to average tax rate changes once marginal rate changes are controlled for. The point estimates are in fact slightly negative, although they are not statistically significant at any horizon.

The findings in panel B and C of Figure 7 indicate that the large tax elasticities of income are not explained by strong multiplier effects due to changes in after tax income. The results are instead much more consistent with theories built on direct incentive effects and forward looking behavior. Based on a comparison of the regression coefficients on AMTR changes and the Romer and Romer (2010) series, Barro and Redlick (2011) arrive at the same conclusion. The methodology above, which is the same used by Mertens and Ravn (2013) to separate the impacts of personal and corporate taxes, is in principle better suited for isolating the causal effects of average and marginal tax rates. A few qualifications are however in order. The confidence bands of the income response in Panel C are fairly large and do not permit ruling out sizeable positive effects of average tax rate changes. Also, the causal effects are in practice identified mostly by variation in federal income taxes. Because many lower income households face no federal income tax liabilities, the results may not be that informative about policies that more strongly affect disposable incomes of those lower in the distribution. Nonetheless, the results in Figure 7 clearly emphasize the importance of marginal tax rates in explaining the evidence for the real economic effects of tax reforms.

5.2 The Effects of Cutting Top Marginal Tax Rates

Many of the postwar tax reforms have made particularly large changes in top marginal tax rates. This section exploits the additional idiosyncratic variation in US top rates to estimate the effects of a hypothetical tax reform that only alters marginal tax rates for the top 1%. The associated responses thus differ from those reported in Sections 4.3 and 4.4, which captured the effects of tax changes affecting a large fraction of tax payers simultaneously. Isolating the causal effects of more targeted marginal rate changes is potentially informative about general equilibrium and spill-over effects. It is for instance less likely that top marginal rate changes directly generate a large demand stimulus. Focusing on top rate changes in isolation also fits into recent policy discussion on optimal tax rates at the top, see e.g. Diamond and Saez (2011), and provides

insight into possible theoretical explanations for the correlation between top marginal tax rates and top 1% income shares documented by Piketty et al. (2014) for the US and other OECD countries.

The key empirical challenge is to control for simultaneous exogenous changes in tax rates for the bottom 99%, while at the same time preserving all endogenous feedback that arises because of changes in relative incomes, bracket creep, etc. This challenge is addressed in exactly the same fashion as in the previous section isolating the separate effects of average and marginal tax rates. The results are derived from a VAR model identical to Section 4.3 but with two modifications. First, the aggregate AMTR and income series are replaced with the corresponding separate series for the top 1% and bottom 99%. Second, following Saez (2004) and Saez et al. (2012) I include linear and quadratic time trends to capture longer run trends in income inequality.[29] The identification strategy relies on the joint use of the proxies for shocks to the top 1% and bottom 99% AMTR series, as given in Table 3, and a Choleski factorization of the covariance of exogenous innovations to both tax rate series.

Figure 7 displays the dynamic response to a targeted one percent rise in the net-of-tax rate of the top 1%. The upper left panel shows that the top marginal rate shock causes a persistent but transitory decrease in the top 1% tax rate. The increase is more persistent than in the case of an aggregate tax shock, which not only appears consistent with postwar policy decisions regarding top rates but may also partly reflects that bracket creep is less relevant at the top of the income distribution. The upper right panel shows that after allowing for endogenous feedback the response of the bottom 99% AMTR remains close to zero at all horizons. This justifies interpreting the results as originating exclusively with top marginal rate changes.

The panels in the second row of Figure 7 provide the responses of average incomes of both groups. The left panel shows that the targeted tax cut leads to a significant increase in top 1% income, which rises by 1.51% on impact and by 1.57% and in the following year, after which there is a gradual decline. The responses for the first two years are significant at the 5% level and the third year response is significant at the 10% level. The second year response is similar and even slightly larger than those estimated earlier in

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[29]Appendix B shows that including the same time trends in the VAR model of Section 4.3 does not have any important effects.

Section 4.4, although the difference is certainly not statistically significant. Short run ETIs for top incomes as large as 1.5 are similar to those initially found by Lindsey (1987) and Feldstein (1995) for the 1980s reforms, which made the largest changes to top statutory rates. Romer and Romer (2014) also focus on top rate changes and control for tax rate changes for lower income groups by focusing on the interwar period, during which only households with the highest incomes effectively paid income taxes. In diff-in-diff regressions using thin slices at the top of the income distribution, Romer and Romer (2014) find much smaller short run elasticities of around 0.2. Whether this is due to differences between the pre- and postwar federal tax codes, the diff-in-diff strategy, the persistence of the tax rate changes, data quality or other factors is not clear.

According to the right panel in Figure 7, a cut in top rates has a positive influence on lower incomes. Average incomes of the bottom 99% rise by 0.23 percent on impact and by up to 0.44 percent in the following year. The positive spill over effect is significant at the 5% level in the second year. Nonetheless, the top marginal rate cut unambiguously leads to greater inequality in pre-tax income. The results are therefore consistent with the correlation between top statutory rates and top income shares observed in the US and internationally. Piketty, Saez and Stantcheva (2014) propose a causal explanation for this correlation beyond classical incentive or avoidance effects: lower top marginal tax rates lead to more aggressive rent-seeking efforts by agents with the highest incomes. Avoidance or rent-seeking channels alone would imply zero or negative spill overs from a top rate cut onto incomes outside the top. The positive impact on lower incomes does not invalidate these channels but implies a dominant role for classical incentive effects. At least at this level of aggregation, the finding of spill over effects also raises concerns with the interpretation of ETI estimates obtained using income shares or income differences.

The bottom row of Figure 7 presents the response of GDP and the unemployment rate. The cut in top 1% tax rates leads to a increase in real GDP of 0.26% on impact and of 0.30% in the following year. Although the GDP response is only marginally statistically significant, it nonetheless suggests that the positive top income response does not result exclusively from avoidance or rent-seeking behavior. The finding that a top marginal rate cut leads to increases in aggregate economic activity is corroborated by the unemployment response in the bottom right panel of Figure 7. The unemployment rate decreases by 11 and 17 basis points

in the first two years and both of these estimates are significant at conventional levels. The positive spill over effects on lower incomes may therefore be to an important extent driven by net job creation.

The identification of the responses to top marginal rate cuts in Figure 7 relies crucially separate instances of large changes in top rates but relatively smaller changes in AMTRs for other tax units. The effects of the reverse experiment of a cut to marginal tax rates for the bottom 99% (or 90%) but not for the top 1% (or 10%) are extremely imprecisely estimated by the model. Unfortunately, there does not seem to be enough identifying variation in the data for such an experiment and this paper is silent about its possible effects.

6 Concluding Remarks

Using a narrative identification strategy to obtain measures of exogenous variation in marginal tax rates, this study finds significant and broad based effects on reported income. This is consistent with recent macro studies detecting substantial effects of tax changes on real economic variables in the US and other countries using similar identification approaches. However, it conflicts with existing evidence in the public finance literature that uses aggregate time series constructed from tax returns. The difference can be explained by the efforts to resolve the endogeneity of marginal tax rates due to tax policy being responsive to spending and the business cycle, bracket creep, anticipation effects, etc. One may certainly question whether these efforts truly circumvent all endogeneity problems. However, at the very least this paper demonstrates the important consequences of taking elementary steps to mitigate the influence of confounding factors. As in any time series study, it is necessary to assume that the economic environment at some level is stable across the sample. It is therefore always risky to draw strong conclusions about the impact of particular historical or contemplated future tax reforms. With these caveats in mind, the findings are relevant for assessing the role of income taxation for macroeconomic stabilization and the impact of austerity programs, for understanding the empirical relationship between income taxes and inequality, and for optimal tax policy. Although disagreement about magnitudes remains, the results also add more indirectly to the growing evidence that hours and employment decisions are influenced by taxes, see e.g. Blundell, Duncan and Meghir (1998), Keane (2011), Chetty (2012) and Chetty et al. (2013).

There are several ways for future research to verify and extend the analysis. The identification of exogenous variation in marginal tax rates ultimately relies on a limited number of postwar tax reforms in the US. Tax returns data and narrative datasets become increasingly available for other countries, e.g. Piketty et al. (2014), Cloyne (2013), Hayo and Uhl (2014) and Leigh et al. (2014), which allows for replication of the results. Second, measuring the long run effects of marginal tax rate changes in US data alone is extremely challenging. However, separating the effects of temporary and permanent tax changes at least for top incomes may be feasible by incorporating prewar data. Third, the empirical models in this paper are linear. There may be important nonlinearities in the relationship between marginal tax rates and economic activity, both in the short and long run, see Auerbach and Gorodnichenko (2012) or Jaimovich and Rebelo (2014). Finally, the results in this paper are based on reduced form models and should be combined with realistic structural models to gain greater insight into the tax transmission mechanism. I leave these and other extensions for future work.

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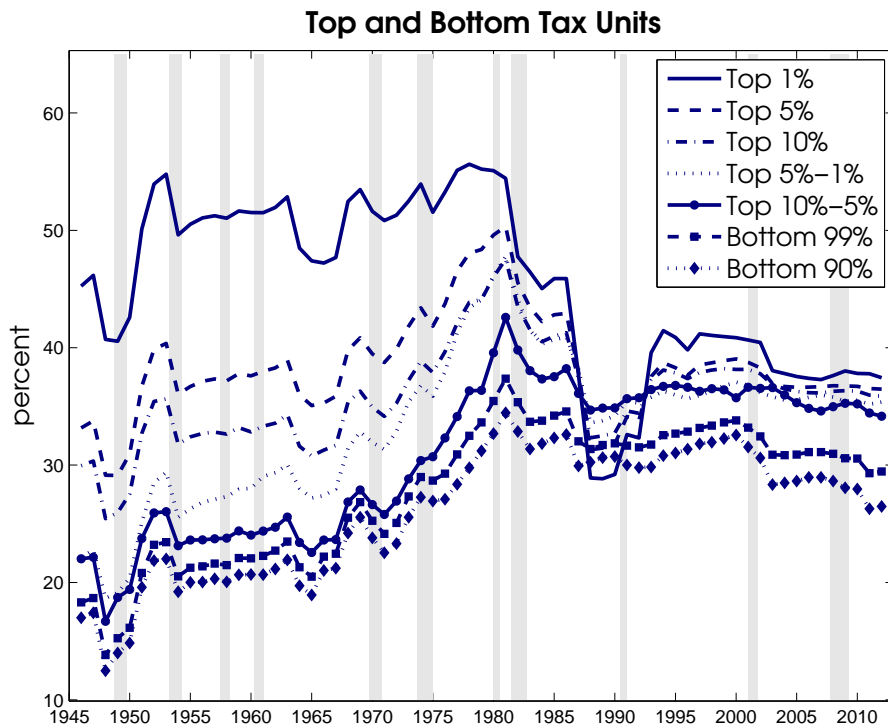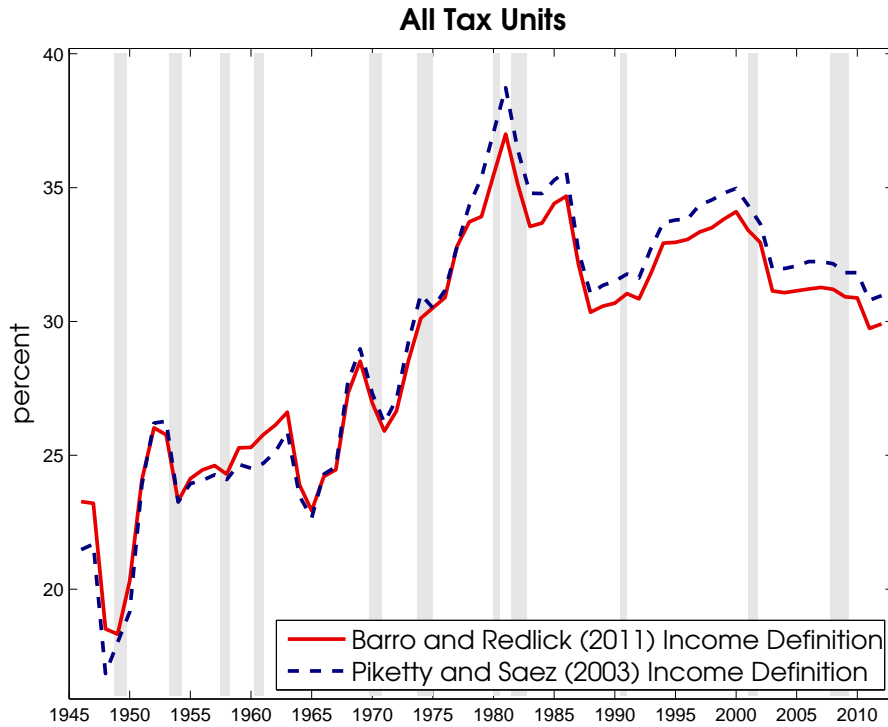
Figure 1 Average Marginal Tax Rates 1946-2012: Individual Income and Social Security.

The series are income weighted averages of marginal federal individual income tax rates and social security (OASDI and HI) contribution rates. Top and bottom tax percentiles are based on the distribution of income over potential tax units as in Piketty and Saez (2003). Sources and data construction are detailed in the appendix. Shaded areas denote NBER-dated recessions.

All Tax Units, Individual Income Tax

Top and Bottom Tax Units, Individual Income Tax
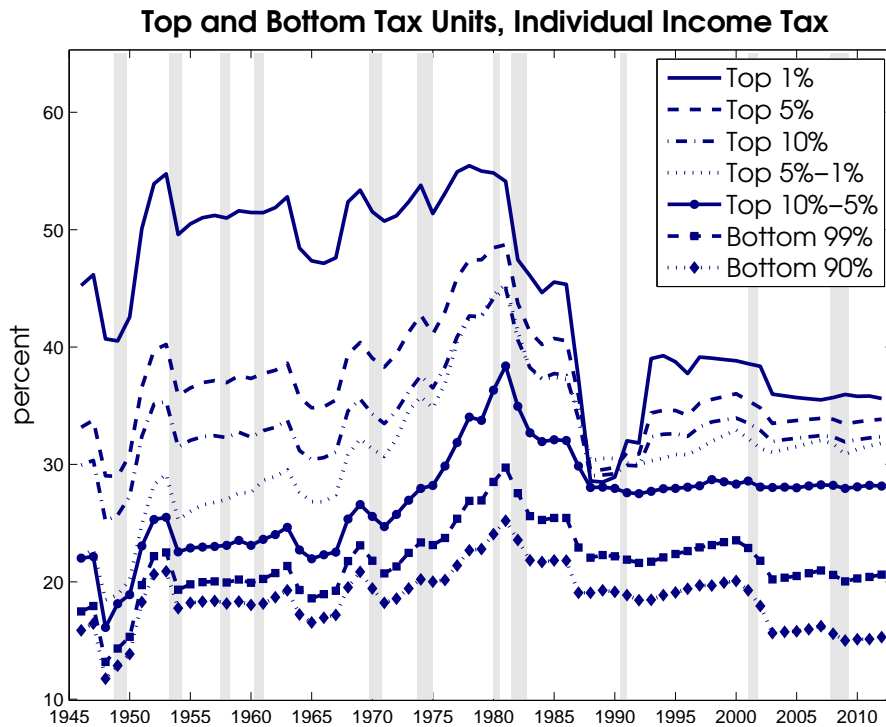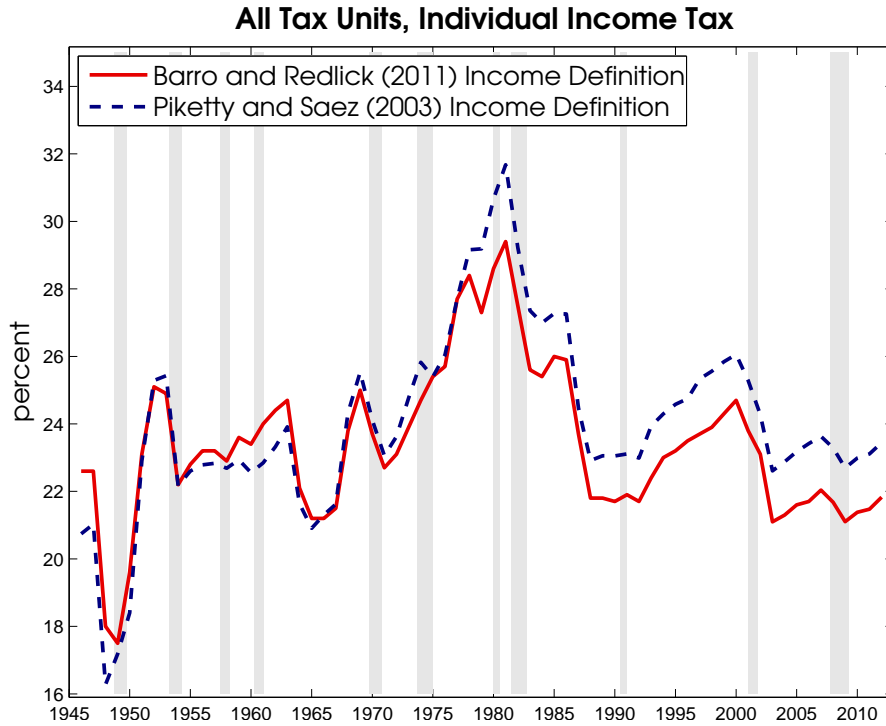
Figure 2 Average Marginal Individual Income Tax Rates 1946-2012.

The series are income weighted averages of marginal federal individual income tax rates. Top and bottom tax percentiles are based on the distribution of income over potential tax units as in Piketty and Saez (2003). Sources and data construction are detailed in the appendix. Shaded areas denote NBER-dated recessions.

All Tax Units, Social Security

Top and Bottom Tax Units, Social Security
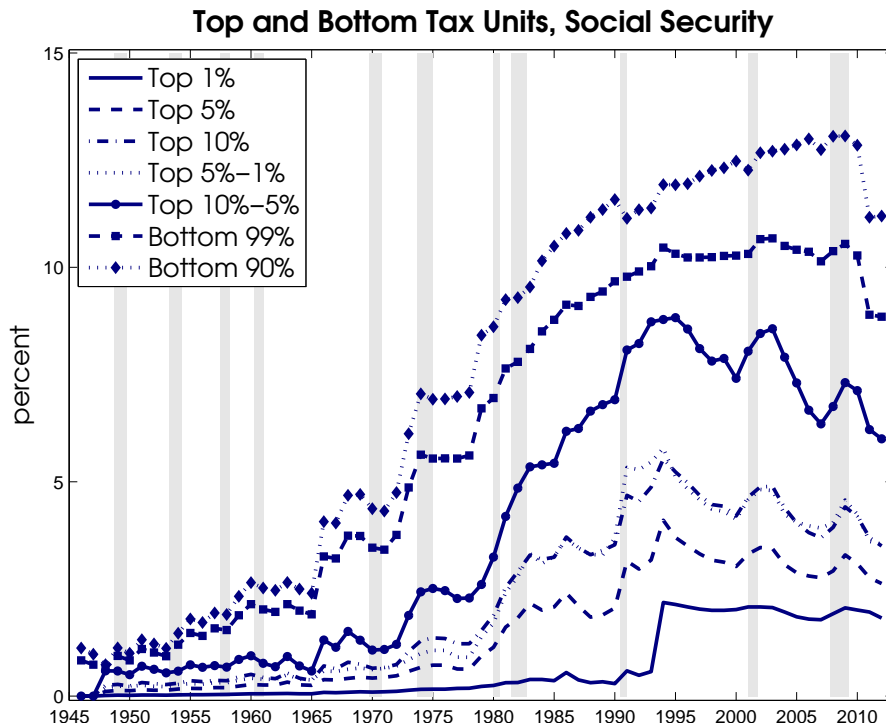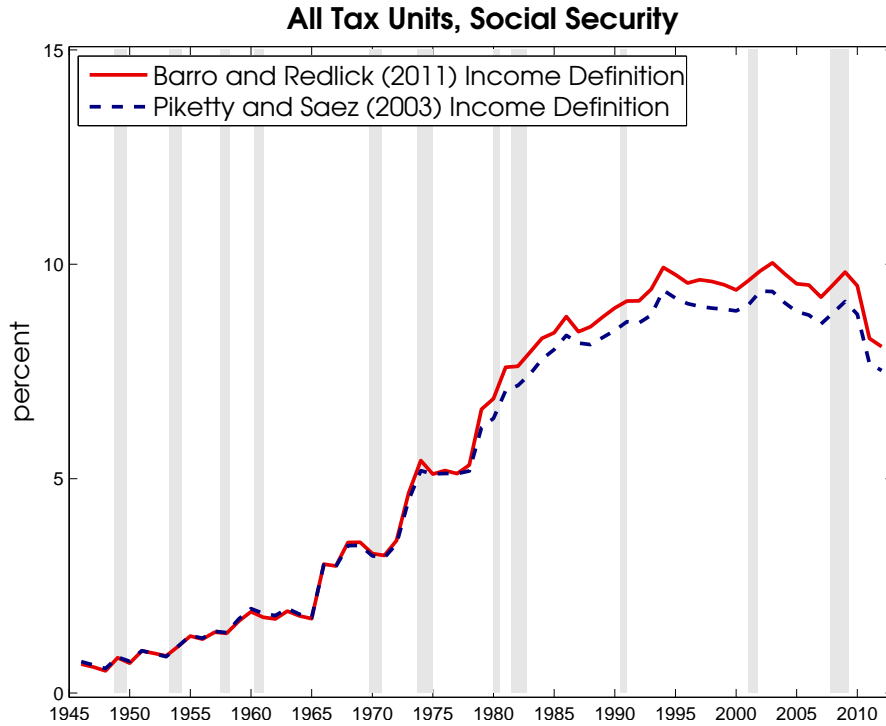
Figure 3 Average Marginal Social Security Tax Rates 1946-2012.

The series are income weighted averages of marginal social security (OASDI and HI) contribution rates. Top and bottom tax percentiles are based on the distribution of income over potential tax units as in Piketty and Saez (2003). Sources and data construction are detailed in the appendix. Shaded areas denote NBER-dated recessions.

All Tax Units

All Tax Units, Individual Income Tax

All Tax Units, Social Security
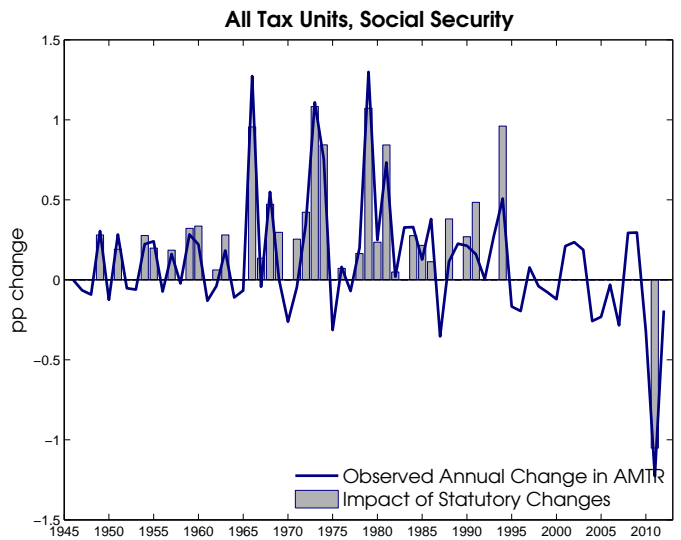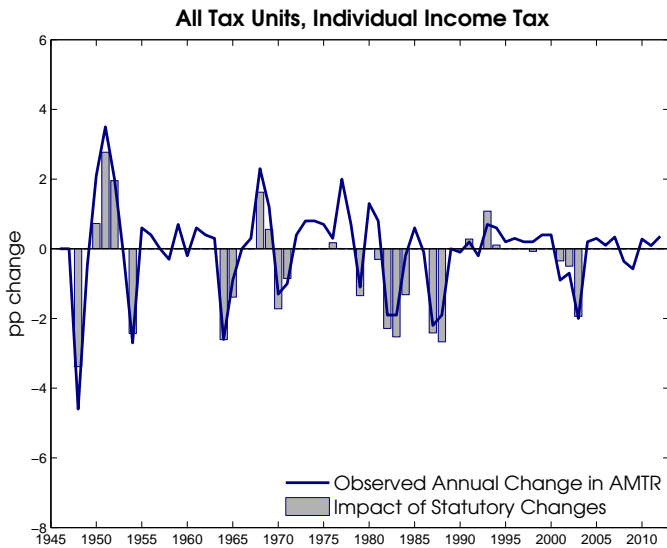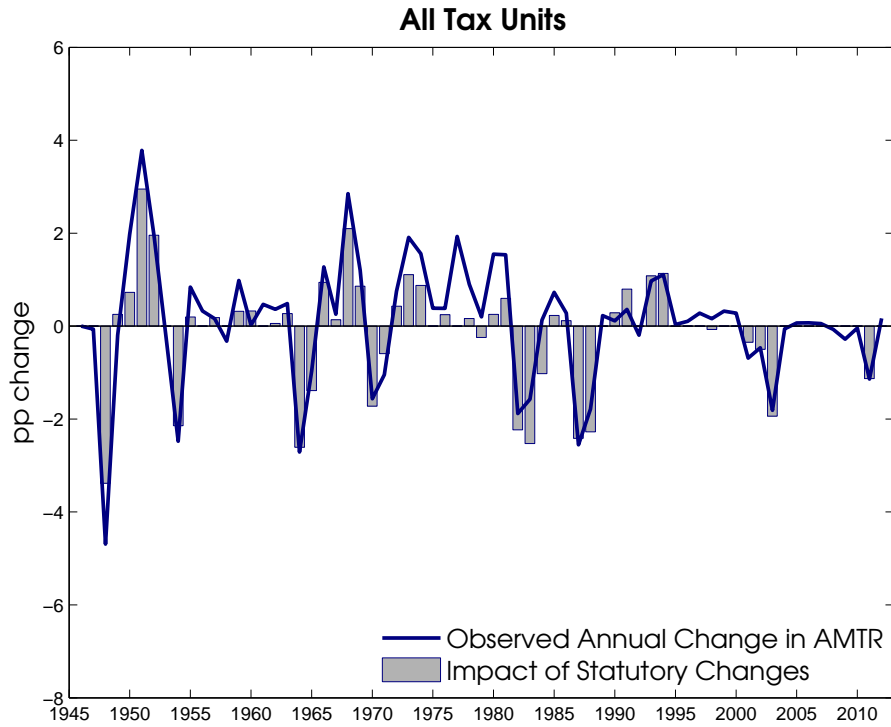
Figure 4 Estimates of the Impact of Statutory Changes on Average Marginal Tax Rates.

Observed annual change in the Barro and Redlick (2011) income concept AMTR for all tax units. The impact of statutory changes in year t is the difference between a counterfactual AMTR constructed using the year $t-1$ income distribution and year t rates and brackets deflated by any automatic adjustments between $t-1$ and t, and the observed year $t-1$ average marginal tax rate.
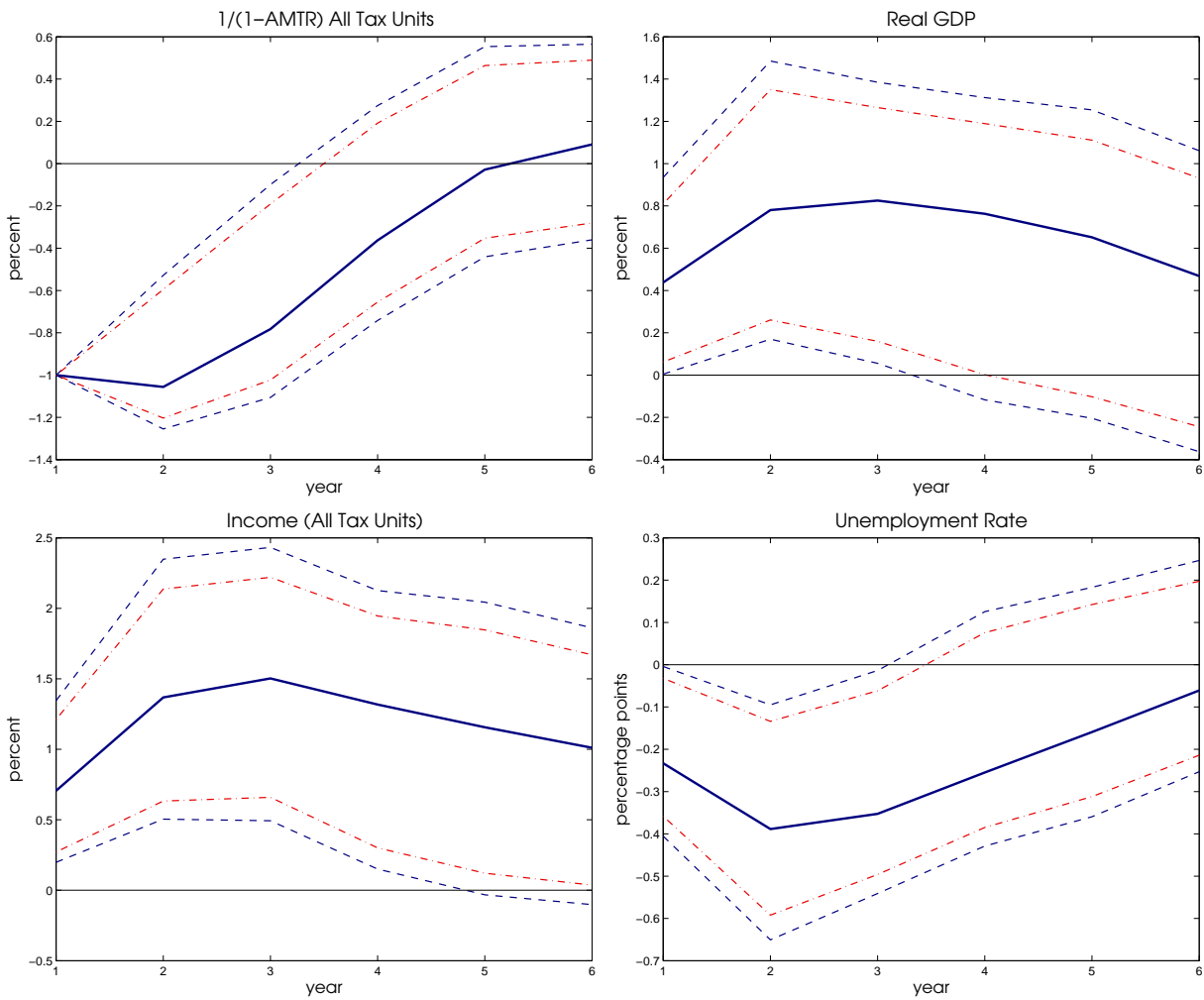
Figure 5 Aggregate Dynamic Responses to a One Percent Increase in the Marginal Net-of-Tax Rate.

Income is aggregate real income per tax unit from Piketty and Saez (2003) and excludes realized capital gains and government transfers. The upper left panel plots the response of the inverse net-of-tax rate to clarify that the shock induces an AMTR decrease. Broken lines are 90% and 95% confidence bands based on 10,000 wild bootstrap replications.
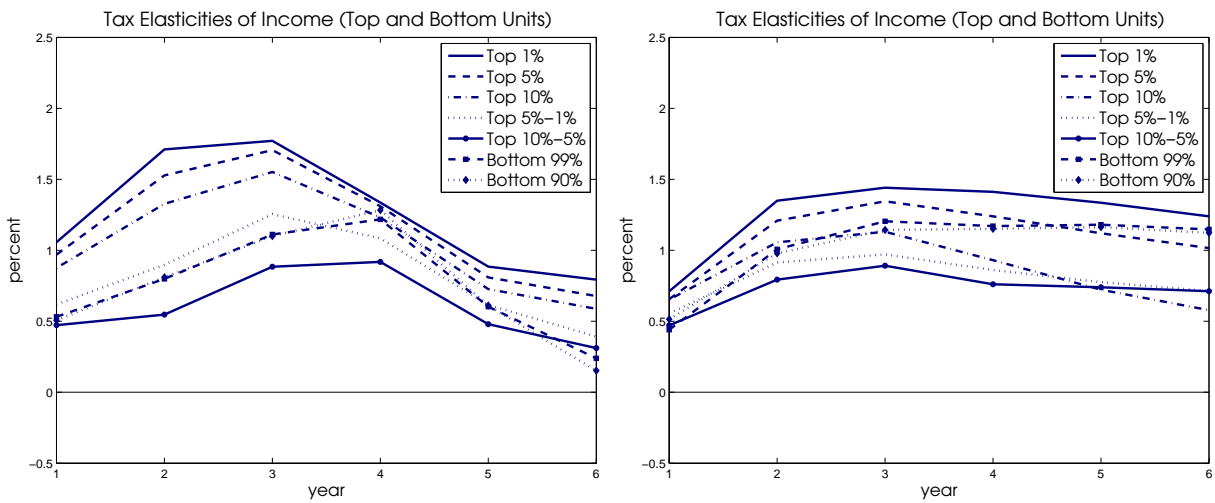
Figure 6 Dynamic Estimates of Tax Elasticities Across Income Groups.

Left panel: estimates based on projections onto the aggregate AMTR shocks identified in the SVAR model with aggregate series. *Right panel*: estimates from separate SVAR models each including the income group specific AMTR and income series and using the income group specific proxies for identification. The income measures are from Piketty and Saez (2003) and exclude realized capital gains and government transfers.

A. Cut in Marginal Tax Rate Allowing Impact on Statutory Average Tax Rate
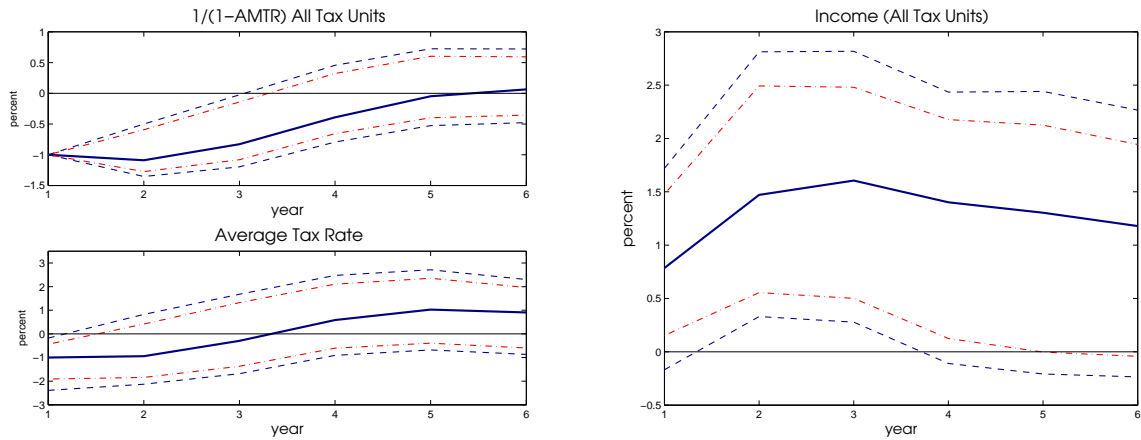
B. Cut in Marginal Tax Rate Without Impact on Statutory Average Tax Rate
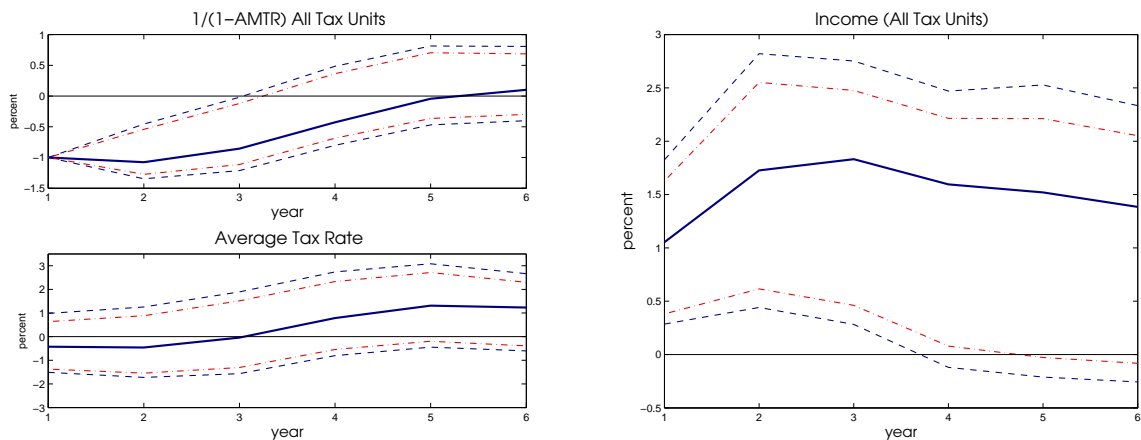
C. Cut in Average Tax Rate Without Impact on Statutory Marginal Tax Rate
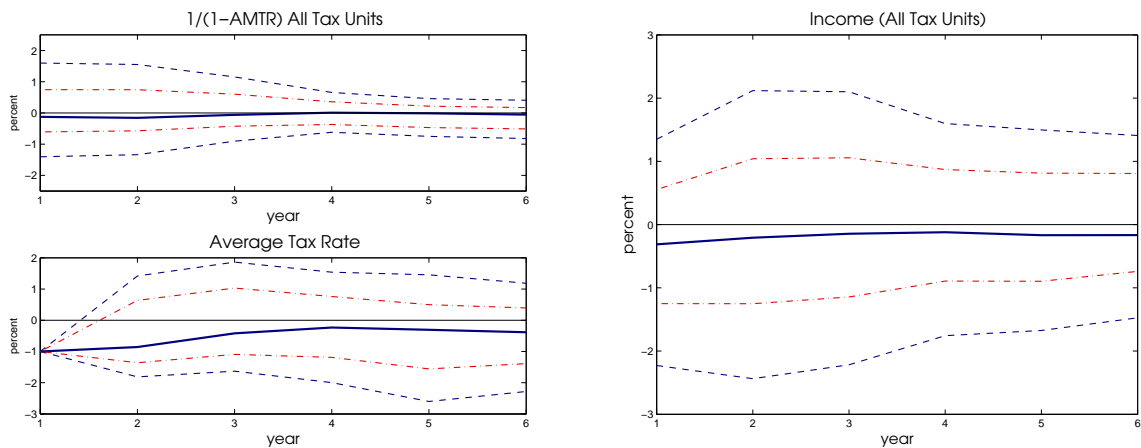
Figure 7 Aggregate Dynamic Responses to Combinations of Shocks to Average and Marginal Rates.

Panels A and B show dynamic responses to a one percent increase in the marginal net-of-tax rate. Each of the upper left subpanels shows the response of the inverse net-of-tax rate to clarify that the shock induces an AMTR decrease. Panel C shows the response to a one percent decline in the average tax rate. The income measures are from Piketty and Saez (2003) and exclude realized capital gains and government transfers. Broken lines are 90% and 95% confidence bands based on 10,000 wild bootstrap replications.

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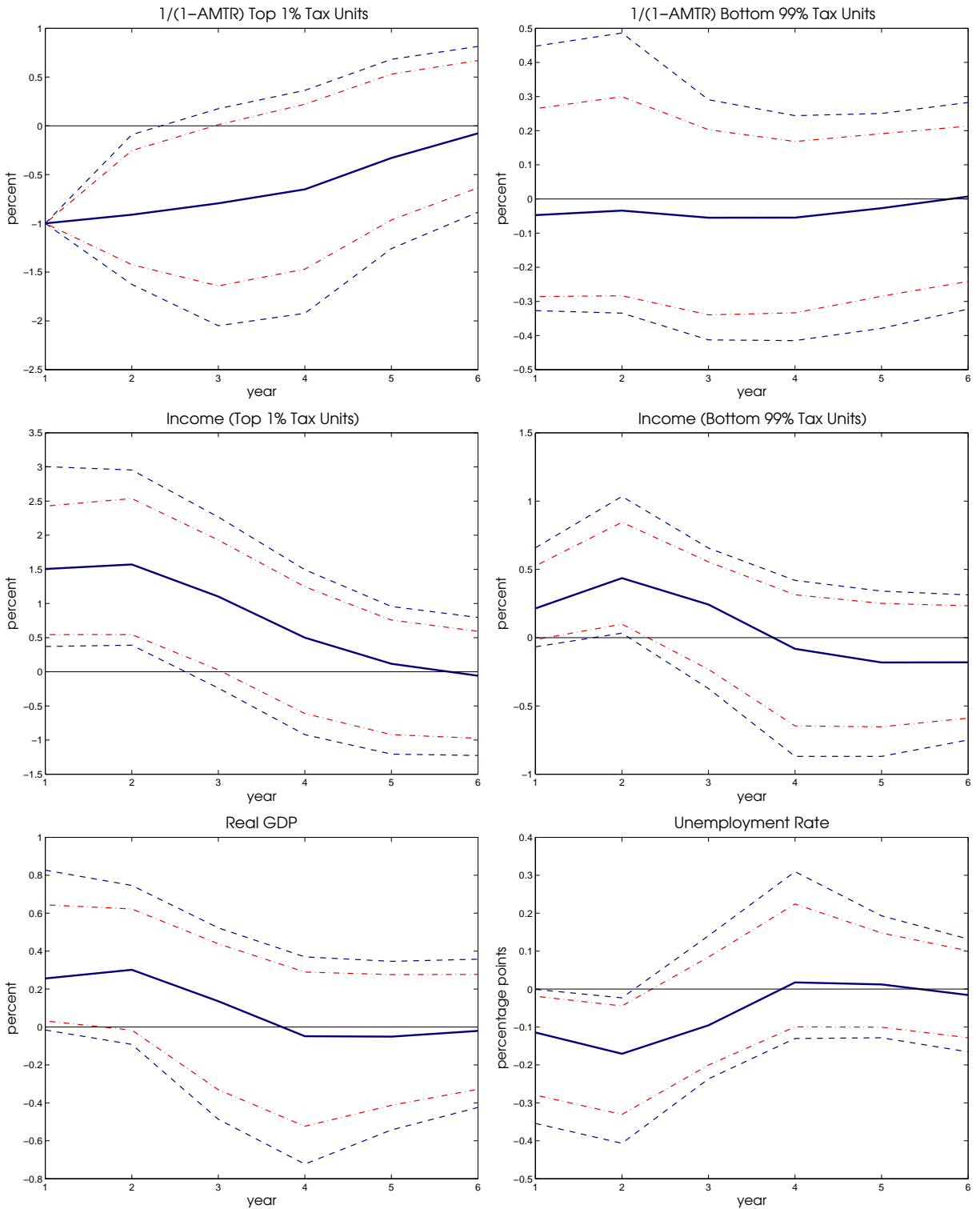
Figure 8 Dynamic Responses to a One Percent Increase in the Top 1% Marginal Net-of-Tax Rate.

The first row plots the responses of the inverse marginal net-of-tax rates for ease of interpretation. The income measures are from Piketty and Saez (2003) and exclude realized capital gains and government transfers. Broken lines are 90% and 95% confidence bands based on 10,000 wild bootstrap replications.

Table 1 Average Marginal Tax Rates 1946-2012: Descriptive Statistics

		AMTR × 100		Δln(1 − AMTR) × 100									
						Correlation with					% of total variation due to statutory changes in		
		Mean	*St. Dev.*	*St. Dev.*	[2]	[3]	[5]	[8]	[9]	*Inc Tax*	*SS Tax*	*Both*
[1]	All (BR 2011)	29.11	4.38	1.83	0.97	0.82	0.91	0.96	0.93	0.76	0.14	0.87
[2]	All (PS 2007)	29.50	5.05	1.99	1.00	0.84	0.93	0.99	0.96	0.74	0.17	0.86
[3]	Top 1%	45.20	7.34	4.86	0.84	1.00	0.94	0.77	0.70	0.86	0.01	0.87
[4]	Top 5%	38.38	4.42	3.29	0.90	0.97	0.99	0.83	0.75	0.82	0.02	0.83
[5]	Top 10%	35.89	4.30	2.78	0.93	0.94	1.00	0.88	0.79	0.79	0.03	0.81
[6]	Top 5-1%	33.43	6.29	2.54	0.90	0.85	0.96	0.87	0.78	0.67	0.03	0.70
[7]	Top 10-5%	30.94	6.39	2.12	0.91	0.74	0.90	0.91	0.83	0.55	0.09	0.61
[8]	Bottom 99%	27.80	5.74	1.87	0.99	0.77	0.88	1.00	0.98	0.62	0.20	0.80
[9]	Bottom 90%	26.03	5.46	1.86	0.96	0.70	0.79	0.98	1.00	0.53	0.24	0.77

Notes: Descriptive statistics for combined federal income and social security tax rates. AMTR is the tax rate in percent and $\Delta \ln(1 - \text{AMTR})$ is the annual log change in the net-of-tax rate. Rows [1]-[2] are national averages using Barro and Redlick (2011), resp. Piketty and Saez (2003) income concepts. Rows [3] to [9] are averages within the specified brackets using the income measures of Piketty and Saez (2003). The last three columns report the R^2 coefficient of regressions of $\Delta \ln(1 - \text{AMTR})$ on the estimated impact of statutory changes to income taxes, social security taxes, or both, on the overall tax rates of the specified income bracket.

Table 2 Preliminary Univariate Regressions of Income on Net-of-Tax Rates

	All Tax Units		Top 1%	Top 5%	Top 10%	Top 5-1%	Top 10-5%	Btm. 99%	Btm. 90%
	BR 2011	PS 2003							
A. Ordinary Least Squares, Sample: 1947-2012									
Same year	-0.23	-0.21	0.55^{**}	0.37	0.28	0.02	-0.00	-0.35^{**}	-0.49^{***}
	$(-0.62, 0.16)$	$(-0.51, 0.09)$	$(0.02, 1.07)$	$(-0.12, 0.86)$	$(-0.13, 0.70)$	$(-0.23, 0.26)$	$(-0.21, 0.20)$	$(-0.65, -0.06)$	$(-0.82, -0.16)$
Following year	-0.09	-0.09	0.84^{***}	0.62^{**}	0.49^{**}	0.21	0.09	-0.31	-0.52
	$(-0.77, 0.58)$	$(-0.68, 0.50)$	$(0.24, 1.44)$	$(0.13, 1.11)$	$(0.05, 0.92)$	$(-0.06, 0.48)$	$(-0.30, 0.49)$	$(-0.96, 0.33)$	$(-1.24, 0.20)$
B. Ordinary Least Squares with Controls, Sample: 1948-2012									
Same year	-0.05	-0.07	0.61^{***}	0.48^{***}	0.40^{***}	0.01	-0.03	-0.19	-0.28
	$(-0.43, 0.33)$	$(-0.36, 0.22)$	$(0.31, 0.91)$	$(0.25, 0.70)$	$(0.20, 0.60)$	$(-0.16, 0.18)$	$(-0.15, 0.10)$	$(-0.52, 0.13)$	$(-0.69, 0.14)$
Following year	0.09	0.19	1.02^{***}	0.80^{***}	0.64^{***}	0.19^{**}	0.15	0.07	0.00
	$(-0.44, 0.63)$	$(-0.29, 0.67)$	$(0.70, 1.33)$	$(0.50, 1.10)$	$(0.38, 0.90)$	$(0.04, 0.34)$	$(-0.11, 0.41)$	$(-0.38, 0.52)$	$(-0.56, 0.57)$
C. 2SLS with Controls and Statutory Tax Changes as Instrument, Sample: 1948-2012									
Same year	0.04	0.08	0.64^{***}	0.48^{***}	0.39^{***}	0.13	-0.09	-0.10	-0.09
	$(-0.33, 0.42)$	$(-0.26, 0.43)$	$(0.33, 0.95)$	$(0.24, 0.73)$	$(0.14, 0.63)$	$(-0.08, 0.34)$	$(-0.37, 0.20)$	$(-0.53, 0.33)$	$(-0.65, 0.46)$
Following year	0.33	0.40^{*}	1.07^{***}	0.75^{***}	0.57^{***}	0.22^{*}	0.03	0.20	0.36
	$(-0.11, 0.77)$	$(-0.05, 0.86)$	$(0.70, 1.43)$	$(0.49, 1.01)$	$(0.33, 0.80)$	$(-0.02, 0.46)$	$(-0.37, 0.42)$	$(-0.41, 0.82)$	$(-0.38, 1.10)$
1st Stage F	307.68	149.49	124.82	232.03	150.80	72.87	50.84	190.72	160.56

Notes: Same year estimates are based on regressing $\Delta \ln(income_t^j)$ on $\Delta \ln(1 - AMTR_t^j)$ and following year estimates are based on regressing $\ln(income_{t+1}^j) - \ln(income_{t-1}^j)$ on $\Delta \ln(1 - AMTR_t^j)$ for every income group j. The regressions in Panels B and C include two lags of $\ln(income_t^j)$ and $\ln(1 - AMTR_t^j)$ as well as two lags of GDP, unemployment rate, government spending, change in federal debt, inflation, real stock prices and the federal funds rate and dummies for 1949 and 2008. Results in the first column are based on the AMTR series for all tax units using the income definition of Barro and Redlick (2011). The other series are based on the income definition of Piketty and Saez (2003). Newey-West 95% intervals with 8 lags in parentheses. Asterisks denote 10%, 5% or 1% significance.

Table 3 Estimated AMTR Impact of Selected Tax Reforms (Percentage Points)

	In year	All Tax Units	Top 1%	Top 5%	Top 10%	Top 5-1%	Top 10-5%	Btm. 99%	Btm. 90%
Revenue Act of 1948	1948	−3.38	−9.14	−7.07	−6.07	−5.24	−3.68	−2.67	−2.06
Revenue Act of 1964	1964	−2.61	−6.47	−4.39	−3.66	−3.05	−2.30	−2.26	−2.11
Revenue Act of 1978	1979	−1.35	−0.76	−0.96	−1.36	−1.09	−2.06	−1.40	−1.34
Economic Recovery Tax Act 1981	1981	−0.31	−0.77	−0.66	−0.58	−0.58	−0.46	−0.26	−0.17
Tax Reform Act of 1986	1987	−2.41	−10.15	−6.52	−5.31	−4.05	−3.03	−1.64	−0.89
Omnibus Budget Reconciliation Act of 1990	1991	0.79	2.70	1.86	1.63	1.09	1.09	0.48	0.22
Omnibus Budget Reconciliation Act of 1993	1993	1.08	7.43	3.45	2.45	−0.28	0.13	0.09	0.17
Jobs and Growth Tax Relief Reconciliation Act of 2003	2003	−1.95	−3.30	−2.68	−2.50	−2.07	−2.03	−1.71	−1.54

Notes: The numbers are the difference between a first counterfactual AMTR, calculated using the year $t-1$ income distribution and the new statutory rates and brackets deflated by any automatic adjustments between $t-1$ and t, and a second counterfactual AMTR based on the year $t-1$ income distribution and rates and brackets under prior law for year t.

Table 4 Structural VAR Estimates of Short Run Tax Elasticities, Sample: 1948-2012

	All Tax Units BR 2011	PS 2003	Top 1%	Top 5%	Top 10%	Top 5-1%	Top 10-5%	Btm. 99%	Btm. 90%
A. Total Market Income									
Same year	0.71***	0.75***	0.71***	0.66**	0.65**	0.56*	0.47**	0.44	0.51*
	(0.19, 1.33)	(0.25, 1.46)	(0.29, 1.04)	(0.13, 1.12)	(0.18, 1.25)	(−0.05, 2.25)	(0.01, 1.25)	(−0.08, 1.10)	(−0.04, 1.34)
Following year	1.37***	1.45***	1.35***	1.21**	1.06**	0.91*	0.79**	1.01**	0.98**
	(0.50, 2.33)	(0.61, 2.53)	(0.53, 1.89)	(0.30, 1.97)	(0.10, 1.86)	(−0.01, 2.73)	(0.09, 1.76)	(0.16, 2.02)	(0.03, 2.12)
B. Wage Income									
Same year	0.49**	0.50**	0.65**	0.37	0.40	0.00	0.52	0.34	0.45*
	(0.03, 1.14)	(0.05, 1.20)	(0.11, 0.95)	(−0.40, 0.69)	(−0.20, 0.85)	(−0.99, 1.17)	(−0.21, 1.49)	(−0.17, 1.12)	(−0.09, 1.56)
Following year	0.90**	0.97**	0.90**	0.65	0.47	0.21	0.65	0.81*	0.90*
	(0.09, 1.94)	(0.18, 2.10)	(0.05, 1.41)	(−0.31, 1.18)	(−0.51, 1.13)	(−1.01, 1.44)	(−0.28, 1.47)	(−0.04, 2.04)	(−0.04, 2.47)
C. Other Income									
Same year	1.87***	1.99***	0.85***	1.32***	1.43***	2.06**	1.50	2.00**	1.95
	(0.57, 3.89)	(0.73, 4.27)	(0.48, 1.50)	(0.82, 3.01)	(0.88, 3.53)	(0.63, 8.69)	(−1.03, 7.78)	(0.09, 4.24)	(−1.30, 3.96)
Following year	3.51***	3.66***	1.86***	2.50***	2.77***	2.89**	2.44*	2.97**	1.86
	(1.41, 6.07)	(1.51, 6.55)	(1.05, 2.58)	(1.36, 4.33)	(1.53, 5.19)	(0.71, 9.74)	(−0.36, 8.42)	(0.20, 5.86)	(−2.11, 4.76)

Notes: Each of the same and following year estimates are based on a separate SVAR system that includes the income group specific AMTR and income series and using the income group specific proxies for identification. The income measures are from Piketty and Saez (2003) and exclude realized capital gains and government transfers. Results in the first column are based on the AMTR series for all tax units using the income definition of Barro and Redlick (2011). The other series are based on the income definition of Piketty and Saez (2003). In parentheses are 95% confidence intervals based on 10,000 wild bootstrap replications. Asterisks denote 10%, 5% or 1% significance.

Table 5 IV Estimates of Short Run Tax Elasticities of Total Market Income, Sample: 1948-2012

	All Tax Units		Top 1%	Top 5%	Top 10%	Top 5-1%	Top 10-5%	Btm. 99%	Btm. 90%
	BR 2011	PS 2003							
A. 2SLS with Controls and Selected Statutory Tax Changes as Instrument									
Same year	0.71***	0.75***	0.71***	0.66***	0.65***	0.56**	0.47***	0.44*	0.51
	(0.29, 1.13)	(0.30, 1.19)	(0.30, 1.12)	(0.23, 1.08)	(0.23, 1.07)	(0.12, 0.99)	(0.18, 0.76)	(−0.06, 0.94)	(−0.18, 1.21)
Following year	1.19***	1.24***	1.37***	1.24***	1.03***	0.85**	0.50	0.73*	0.79
	(0.45, 1.93)	(0.57, 1.91)	(0.60, 2.15)	(0.66, 1.83)	(0.50, 1.56)	(0.20, 1.50)	(−0.11, 1.12)	(−0.14, 1.61)	(−0.38, 1.96)
1st Stage F	229.25	62.24	51.30	33.38	34.43	17.88	14.07	29.91	16.90
B. 2SLS with Controls and Aggregate SVAR Tax Shock as Instrument									
Same year	0.71***	0.71**	0.69***	0.64***	0.60***	0.61***	0.47***	0.51*	0.49
	(0.20, 1.21)	(0.17, 1.24)	(0.19, 1.19)	(0.22, 1.07)	(0.20, 0.99)	(0.21, 1.02)	(0.15, 0.78)	(−0.08, 1.09)	(−0.24, 1.21)
Following year	1.07***	1.08***	1.31***	1.13***	0.93***	0.85***	0.53**	0.80**	0.83*
	(0.41, 1.74)	(0.42, 1.74)	(0.60, 2.02)	(0.66, 1.60)	(0.55, 1.32)	(0.24, 1.46)	(0.07, 0.99)	(0.03, 1.57)	(−0.10, 1.77)
1st Stage F	100.56	53.68	60.97	74.01	72.77	18.80	54.39	53.00	45.99

Notes: The income measures are from Piketty and Saez (2003) and exclude realized capital gains and government transfers. Same year estimates are based on regressing $\Delta \ln(income_t^j)$ on $\Delta \ln(1 - AMTR_t^j)$ and following year estimates are based on regressing $\ln(income_{t+1}^j) - \ln(income_{t-1}^j)$ on $\Delta \ln(1 - AMTR_t^j)$ for every income group j. All regressions include the same controls as the VARs, i.e. two lags of $\ln(income_t^j)$ and $\ln(1 - AMTR_t^j)$ as well as two lags of GDP, unemployment rate, government spending, change in federal debt, inflation, real stock prices and the federal funds rate and dummies for 1949 and 2008. Results in the first column are based on the AMTR series for all tax units using the income definition of Barro and Redlick (2011). The other series are based on the income definition of Piketty and Saez (2003). Newey-West 95% intervals with 8 lags in parentheses. Asterisks denote 10%, 5% or 1% significance.

A Data Appendix

A.1 Average Marginal Tax Rates

This section details the construction of the average marginal tax rates (AMTR) measures for 1946-2012. The series are the sum of the Average Marginal Individual Income Tax Rate (AMIITR) and Average Marginal Payroll Tax Rate (AMPTR):

$$\text{AMTR} = \text{AMIITR} + \text{AMPTR}$$

Matlab code as well as spreadsheets with all the underlying data are available at `https://mertens.economics.cornell.edu/research.htm`.

Average Marginal Individual Income Tax Rate (AMIITR)

The analysis is based on two AMIITR series that differ in income concepts. The first aggregate series simply extends the measure of Barro and Redlick (2011) from 2006 to 2012. The other series (aggregate as well as for different income groups) extend the measures provided by Saez (2004) to include the following years: 1946-1959, 1961/1963/1965 and 2001-2012. From 1966 onwards, the AMIITR series are based on a large sample of tax returns and the NBER's TAXSIM program to calculate the marginal tax rate for each return. In case of Barro and Redlick (2011), the income weights are based on a concept of labor income that includes wages as well as self-employment, partnership and S-corporation income. Saez (2004) uses a broader income concept based on adjusted gross income (AGI) before adjustments but excluding government transfers and capital gains. His series reflects different assumptions on the income of non-filers and also includes TAXSIM-based observations for 1960, 1962 and 1964. Unfortunately, TAXSIM is not consistently available prior to 1966. The series are therefore extended based on data in the annual Statistics of Income (SOI) from the IRS, available at `http://www.irs.gov/uac/SOI-Tax-Stats-Archive`, using a methodology analogous to Barro and Sahasakul (1983) and using adjusted gross income for weighting.

The SOI contain tables with information on the number of returns, total AGI, and taxable income for different ranges of AGI per return. In most years, these data are available separately for each filing status (married filing jointly/separately, single person, head of household or surviving spouse). For each year and filing status, I fit a probability distribution function $D(y)$ for adjusted gross income per return y,

$$D(y) = \sum_{i=1}^{n} w(i) \int_{b(i)}^{\min\{y,b(i+1)\}} f_i(x)dx \ ,$$

$$f_i(x) = \begin{cases} \text{Beta}(a(i),1) & \text{if } m(i) \geq (b(i)+b(i+1))/2 \text{ and } i < n \\ \text{Beta}(1,a(i))/(2+c) & \text{if } (b(i)+b(i+1))/(2+c) \leq m(i) < (b(i)+b(i+1))/2 \text{ and } i < n \\ \text{BoundPar}(a(i)) & \text{if } m(i) < (b(i)+b(i+1))/(2+c) \text{ or } i = n \end{cases}$$

where n is the total number of brackets, $b(i)$ is the bracket floor and $b(n+1) = \infty$, $w(i)$ is the fraction of returns in bracket i and $m(i)$ is the mean AGI within bracket i. $D(y)$ approximates the AGI distributions by piecewise combinations of Beta (power function) distributions switching to (Bounded) Pareto distributions in the right tail. For each bracket the parameter $a(i)$ is set to match $m(i)$. Many brackets have $a(i) \approx 1$ such that the distribution is locally approximately uniform. The scalar c determines the location of the switch from a positively-skewed Beta to a Pareto distribution and is set to 0.25. Computing floors on various percentiles for all returns with positive AGI yields numbers that with few exceptions are well within 1% of those reported for 1986-2009 by the IRS. The percentiles used for the calculations of tax rates are for all potential tax units as defined by Piketty and Saez (2003, 2007), see Section A.2. Nonfilers' AGI is assumed to equal

20% of average reported AGI per return.

Method 1 for computing AMIITRs is based on SOI tables that for each filing status report the total AGI and number of returns for which a given statutory rate is the highest marginal rate. The distributions $D(y)$ are used to interpolate for each filing status the total AGI taxed at each statutory rate applicable to returns exceeding the percentile floor. This method only considers returns with a regular tax rate as the highest marginal rate, which comprise the vast majority of returns, and does not reflect that certain types of income have a lower marginal rate.[30] Nonfilers and untaxed returns carry a zero marginal rate. **Method 2** for computing AMIITRs uses the data on taxable income in combination with the statutory tax rates and brackets, including surcharges and reductions, to calculate the marginal rate for each AGI level and filing status. The AMIITRs are subsequently computed using numerical integration based on the distributions $D(y)$. This method is again an approximation because all taxable income is assumed to be taxed at the regular rates.

The SOI statistics are not reported consistently over time and missing observations reflect absent or inadequate data. For instance, the tables listing statutory rates and AGI taxed which are required for Method 1 are only available for since 1961-1973 with 1974 and 1978 missing. AGI distributions disaggregated by filing status are to varying degrees incomplete for 1979-2002. In the overlapping years, both methods yield AMIITRs that are very highly correlated with the original series of Saez (2004) and Barro and Redlick (2011). The missing values in their series are interpolated by OLS regressions on the Method 1 series when available, and else on the Method 2 series. This method was used to obtain the overall AMIITR and the AMIITRs for the top 1%, 5% and 10%. The remaining AMIITRs are calculated residually using the income shares in Piketty and Saez (2007).

Average Marginal Payroll Tax Rate (AMPTR)

The marginal payroll tax series capture the taxation of labor and self employment earnings under the federal Old-Age, Survivors and Disability Insurance (OASDI) and Hospital Insurance (HI) programs of Social Security. The tax rates are calculated as

$$AMPTR = w_1 \frac{s_f + s_w}{1 + s_f} + w_2 s_e$$

where s_f, s_w and s_e are the contribution rates paid by firms, workers and the self employed respectively, and w_1 and w_2 are the total taxable earnings of those with earnings below the annual maximum taxable as a ratio of total income. The contribution rates and taxable ceilings are available from the Social Security Annual Statistical Supplement (http://www.ssa.gov/policy/docs/statcomps/supplement/). The calculation of the aggregate series follows Barro and Sahasakul (1986) and employs data reported by the SSA on the number of workers and self employed with maximum earnings as well as total taxable earnings to calculate total taxable earnings of covered workers with earnings below the maximum. For OASDI, this information is available from the SS Annual Statistical Supplement for the entire sample. For HI, the information is from the Supplement since 1997, whereas earlier data is based on various issues of the Annual Report of the Board of Trustees of the Federal Hospital Insurance Trust Fund. The series is similar but different from the aggregate payroll tax series calculated by Barro and Redlick (2011) who use instead earnings data from IRS tax returns to estimate the weights. For the aggregate series, I prefer the SSA measures of taxable earnings because they take into account changes in coverage of both programs over time and do not require any im-

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[30]Method 1 omits returns for which the capital gains rate is the highest marginal rate and returns with alternative tax computations.

putations on the division of earnings between joint filers. The series is constructed for both the Barro and Redlick (2011) and Piketty and Saez (2003) income concepts.

The AMPTR series for the different income groups must be calculated using weights obtained from earnings reported on IRS tax returns within the income group. For the available years (1962, 1964 and 1966-2008), following Barro and Redlick (2011) I use the US Individual Income Tax Public Use Sample available at the NBER. For most years (1974 and 1979-2008), these files include an earnings split between spouses filing jointly which with a few exceptions is based on imputations rather than direct data. For the other years (1962, 1964, 1966-1973 and 1975-1978) the earnings split on joint returns is imputed by interpolating between two counterfactual tax rates that assume earnings splits that maximize, resp. minimize the payroll tax rate. The interpolation assumes that the true tax rate is a weighted average of these lower and upper bounds with weights that are the same is in the closest subsequent year for which the earnings split is available. For years where the micro data is not available, I use more aggregated data from the IRS SOI on the number of returns with earnings and the total amount of earnings within the top 1%, 5% and 10% income percentiles. Given the longer run trends in female labor force participation and the number of two-earner joint filers, I pursue different strategies for imputing the earnings split before 1966 and after 2008. For 1946-1961, 1963 and 1965, I make the following assumptions about joint filers in the top 1%, 5% and 10% percentiles that report earnings: (a) the proportion of dual earners is the same as in the aggregate (b) primary earnings on returns in the top 10% are above the maximum taxable amount (c) secondary earnings are log normally distributed with means of resp. 230%, 140% and 115% of average wages and a standard deviation of 25% and (d) there are no self employment earnings below the maximum amount. These assumptions seem roughly plausible when compared to data for adjacent years. Moreover, given the relatively low ceilings and contribution rates prior to 1966, even relatively large perturbations of these assumptions have little impact on the resulting tax rates for the top income tax units for that period. The assumptions are applied to 1946-1976 and the resulting series are merged with the series based on micro data using OLS regressions for the overlapping years. For 2009-2012, the tax rate series for top income units are based on weights estimated by the predicted values from regressions on wage and self employment income and the maximum ceiling amounts for 1993-2008. These regressions have a very good fit over that period. The above methodology was used to obtain the AMPTRs for the top 1%, 5% and 10%. The remaining AMPTRs are calculated residually using the income shares in Piketty and Saez (2007).

A.2   Other Time Series

The market and wage income series are from updates of the data provided in Piketty and Saez (2003, 2007). **Potential Tax Units** is all married men and singles aged 20 or over, obtained from Piketty and Saez (2003); **Real GDP per tax unit** is NIPA 1.1.3 line 1 divided by potential tax units; **Inflation** is the log change in the Bureau of Labor Statistics' CPI Research Series Using Current Methods (CPI-U-RS), obtained from Piketty and Saez (2003). The **Federal Funds Rate** after 1953 is the annual average effective federal funds rate from the Board of Governors. I use the secondary 3 month Tbill rate for 1946-1949 and observations from Romer and Romer (2010) for 1950-1953. **Government Debt per Tax Unit** is federal debt held by the public, measured by Table L.106 line 19 (federal government, liabilities, credit market instruments) in the US Financial Accounts (release Z.1 of the Federal Reserve Board), divided by the CPI-U-RS and potential tax units. **Government Spending per Tax Unit** is the sum of federal government purchases, net interest rate expenditures and net transfers (NIPA 3.2 line 46 less lines 3,4,7,10 and 11 plus NIPA 3.12U line 25), divided by the CPI-U-RS and potential tax units. The **Real Stock Price** is the S&P composite index from updates of Shiller (2000), divided by the CPI-U-RS. The **Unemployment Rate** and **Female Participation Rate** are for the civilian noninstitutional population aged 16 or more are from the Bureau of Labor Statistics.

The **Average Tax Rate** is the sum of federal personal current taxes and contributions for social insurance (NIPA 3.2 line 3 plus NIPA 3.7 lines 3 and 21) divided by total market income from Piketty and Saez (2003). The **Average Capital Gains per Tax Unit** is from Piketty and Saez (2003). **Ramey News** is the measure of news about defense spending (annual totals) constructed by Ramey (2011a). The Moody's **Corporate BAA Rate** is from the H.15 release of the Federal Reserve Board. **Education** is the percent of people 25 years or over which completed 4 years of college or more from the Current Population Survey (Table A.2 historical tables on educational attainment). Missing values prior to 1964 are linearly interpolated. The **Top 10% Income Share** is in logs and based on data from Piketty and Saez (2003).**Wage inequality** is the log difference between average wage income of the top 10% and bottom 90%. The **Gini Coefficient** for families is from the Current Population Survey (series F4 of historical tables on income inequality). The average **Corporate Tax Rate** is federal taxes on corporate income excluding Federal Reserve banks (NIPA Table 3.2 line 9) divided by corporate profits (NIPA Table 1.12 line 13 less Federal Reserve Bank Profits in NIPA Tables 6.16 B-C-D). The narrative **Corporate Tax Changes** series is from Mertens and Ravn (2013).

A.3   Narrative Measures of the Tax Liability Impact of Tax Reforms

Table A provides the data on the tax liability impact of the selected tax reforms. The series for the total tax liability impact that is used as the additional proxy in Section 5.1 is the sum of the four columns in the table as a percentage of total income in the previous year. In the robustness section, column [22] of Table B uses the same series, whereas column [21] of Table B is the sum of columns [1] and [3] in Table A as a percentage of total income in the previous year. In each case the series have zero values an all other years.

Background and sources:

1. **<u>Revenue Act of 1948</u>** Signed: 4/2/48; The 1948 act reduced marginal tax rates on individual income for all taxpayers, with the percentage reduction in rates being largest for low-income taxpayers. It also increased the personal exemption and the standard deduction amounts and permitted income splitting by married couples. A Senate report (No. 1013 March 1948, Table XIV) contains the Joint Committee of Internal Revenue Taxation estimate of the impact on calendar 1949 income tax liabilities of $4.6 billion, of which $1.8 billion is directly attributed to the rate reductions. I add the effects of income splitting ($0.6 billion) to obtain the total effect of direct marginal rate provisions. The same Senate report also contains the prior law rate schedule.

2. **<u>Internal Revenue Code of 1954</u>** Signed: 8/16/54; This law was a comprehensive reform of the individual income tax system: it combined the 3 percent normal tax and the reduced surtax into a single comprehensive rate schedule, permitted three new tax credits (retirement income, dividends and tax exempt interest), introduced new concepts of taxable income and adjusted gross income, altered or introduced tax deductions (medical expenses, dependent care) and changed filing requirements. (SOI 1954, page 8 -9). In 1954 marginal tax rate decreased as legislated under the Revenue Act of 1951. These changes are treated as fully anticipated (see existing law schedules in Joint Committee on the Economic Report, 1952, Federal Tax Changes and Estimated Revenue Loss under Present Law). The 1954 Treasury Annual Report (p. 44) provides an estimate of the structural changes of the income tax and states the bill reduced taxes on individuals by $0.8 billion in fiscal year 1955. I use the full amount for calendar 1954 classified as non marginal tax rate provisions.

3. **<u>Revenue Act of 1964</u>** Signed: 2/26/64; The Revenue act of 1964 substantially reduced statutory marginal tax rates across the board. It also changed the adjustments made to gross income (excluding sick pay,

allowing higher dividend exclusion), created a new deduction (employee moving expenses), introduced income averaging and the minimum standard deduction and made various other changes (SOI 1964). Taxes on individual were reduced in two stages (1964 and 1965). The 1965 Economic Report (p.65) reports the effect on individual income tax liabilities of the first round of cuts, made retroactive to January 1964, as $6.7 billion in calendar 1964. I use similar numbers from the Joint Committee on Internal Revenue Taxation in its report on the 1964 Revenue Bill (February 1964, p.2-3). This report estimates a total individual income tax revenue impact for calendar 1964 of -$6.1 billion with an impact of the basic rate changes of -$6.3 billion. A 1982 Report to the Subcommittee on Monetary and Fiscal Policy ('The Mellon and Kennedy Tax Cuts: A Review and Analysis'), reports the existing law schedule prior to the 1964 Act which is used to calculate the impact on the average marginal tax rates.

4. **Revenue Act of 1971** Signed 12/10/71; The Revenue Act of 1971 provided tax relief primarily for lower income individuals by increasing the personal exemption as well as the minimum standard deduction. The act also instated the Job Development Credit and made changes to depreciation guidelines, which also had implications for individual income tax liabilities. These provisions were made in part retroactive to 1971. The 1973 Budget, the Congressional Records and the Report of the Joint Committee on Taxation (JCT) all provide estimates of the impact on individual tax liabilities that are roughly consistent. The JCT report contains an estimate of the calendar 1971 impact of $2 billion for the provisions affecting individuals (December 1972).

5. **Tax Reform Act of 1976** Signed 10/4/76; The law contained various changes to the individual income tax code, including an increase in the 'additional tax for tax preferences' (minimum tax), a new child care credit, an increase in the general tax credit and various measures to close loopholes (see SOI 1976, p iv.). The 1977 Economic Report contains at an annualized $2.4 billion total increase in revenues for 1976. The 1978 Budget (p. 60) and the JCT report (October 1976 Appendix A) provide numbers for the separate individual income tax effects of the bill for fiscal 1977 that are very much consistent with the aggregate numbers for 1977 in the Economic Report. The 1978 Budget breaks down the revenue effects of the reform for fiscal 1977. Tax shelter provisions and tax simplification measures canceling each other out with an impact of $0.4 billion and -$0.4 billion for fiscal 1977, respectively (1978 Budget p. 60). The net effect is zero, therefore only the figures provided for the increase in the minimum tax rate for individuals ($1.1 billion in fiscal 1977), out of total increase in liabilities of $1.6 billion, is included. The same proportions are used to deduce the effect for calendar 1976, i.e. $(1.1/1.6) \times 2.4$ or $1.65 billion increase in individual tax liabilities due to the expansion of the minimum tax.

6. **Tax Reduction and Simplification Act of 1977** Signed 5/23/77; The Tax Reduction and Simplification Act of 1977 established the "zero bracket amount" which was included in the definition of taxable income, a simplified single deduction amount based-on marital status and a new jobs credit. The Act also extended several temporary provisions of the Tax Reform Act of 1976 (see SOI 1977 p. vi.). The 1978 Economic Report (p.52) provides estimates for individual tax liabilities effects in calendar 1977 of -$3.3 billion for calendar 1977. I subtract another $2.1 billion in individual income tax revenues to account for the withholding effect on individual taxpayers (see 1979 Budget (p. 50) and Romer and Romer (2009)). The Act changed the tax tables to reflect the change in the definition of taxable income, but this did not affect the rate structure. The entire amount is therefore classified as being due to 'other changes'.

7. **Revenue Act of 1978** Signed 11/6/78; The Revenue Act of 1978 lowered individual tax rates. It

widened and reduced the number of brackets, increased the personal exemption and the zero bracket amount, expanded the earned income tax credit and made several other changes (see SOI 1979 p. viii, CBO 1998 Projecting Federal Tax Revenues and the Effect of Changes in the Law, p.11). The 1979 Economic Report (p.93) describes the effect of the bill as a $14.1 billion cut in personal taxes and a $0.7 increase in outlays for the earned income tax credit in calendar 1979. The 1980 Budget (p. 60-62) reports a similar total impact of $ 14.5 billion in calendar 1979 and $0.7 billion increase in outlays for the EITC, of which $ 10.4 billion is due to the rate and bracket changes. I adopt these latter numbers. The JCT report on the 1978 Act contains very similar estimates and also provides the marginal rate schedule under prior law (March 1979, p.42).

8. **Economic Recovery Tax Act of 1981** Signed 8/13/81; The Economic Recovery Tax Act of 1981 consisted for the main part of permanent, across-the-board reductions in marginal tax rates in several stages and also instituted the indexing of the bracket structure. Effective in 1981 were changes to the minimum tax, the alternative tax and several other changes to the tax code (see SOI 1981 p 6, CBO 1998 Projecting Federal Tax Revenues and the Effect of Changes in the Law, p.14). The 1983 Budget (p.4-9 and 4.10) provides the decomposition of the decline in tax liabilities for 1981 and puts the reduction in individual income tax liabilities at a total of $4.0 billion for calendar 1981. The ERTA report of the JCT (December 1981, Table V-4 in Appendix) estimates an impact on personal tax liabilities in calendar 1981 of $ 4.1 billion of which $3.6 billion is due the rate cuts effective in 1981 and also describes the rate schedule under prior law. The Act had little or no direct implications for employment taxes.

9. **Tax Equity and Fiscal Responsibility Act of 1982** Signed 9/3/82; The act repealed the add-on minimum tax, added several new tax preferences to the minimum tax, restructured the treatment of itemized deductions in the minimum tax, established a flat rate of 20 percent for the minimum tax, and increased the minimum tax exemption, and made several other changes. The CBO provides an estimated impact on individual income tax liabilities of $5 billion for fiscal 1983 (CBO 1998 Projecting Federal Tax Revenues and the Effect of Changes in Tax Law p.18-19). The JCT Report (December 1982) indicate a total effect of 3.1 billion for calendar 1983, and another 0.7 billion in each of calendar 1982 and 1983 due the expansion of the taxation of unemployment benefits, The report assigns $0.7 billion as due to the expansion of the alternative minimum tax for calendar 1983. The prior law rate schedule for 1983 is the one legislated under ERTA 1981. The Act also increased the FUTA wage base and rate and expanded the Medicare tax to federal government employees. According to the JCT report these measures raised employment tax liabilities by $3.0 billion in calendar 1983, of which $0.8 billion is due the increased Medicare coverage.

10. **Deficit Reduction Act of 1984** Signed 7/18/84; The Deficit Reduction Act of 1984 postponed or repealed several revenue reducing provisions scheduled to take effect after 1984 (e.g. the net interest exclusion, made changes to thresholds for income averaging and a large number of minor provisions that raised revenues from corporate and individual taxpayers (SOI 1984 p. 3 and CBO 1998 Projecting Federal Tax Revenues and the Effect of Changes in Tax Law p. 16). Calendar year numbers are not available. The 1986 and 1987 Budgets contain fiscal year revenue impact projections of DEFRA on individual taxes, which where $0.7 billion for 1984 and $5.6 billion for 1985. I estimate a calendar 1984 year impact of $0.7 + $5.6 \times 0.25 = $2.1 billion and classify the entire amount as due to non MTR provisions. The prior law rate schedule for 1984 is the one legislated under ERTA 1981. The Act had little or no direct implications for employment taxes and the higher social security rates that became effective in 1984 were part of legislation in previous years.

11. **Tax Reform Act of 1986** Signed 10/22/86 ; The Tax Reform Act of 1986 significantly reduced individual income tax liabilities, broadened the individual tax base (eliminating the itemized deductions for state sales taxes paid and expanding the alternative minimum tax) and was the first complete revision of the Internal Revenue Code since 1954 (CBO, Projecting Federal Tax Revenues and the Effect of Changes in Tax Law p. 21). The revenue effects of the tax change in 1987 are generally hard to discern, see Romer and Romer (2009). The CBO (Projecting Federal Tax Revenues and the Effect of Changes in Tax Law p. 25) estimates a total projected reduction of $15 billion in individual income tax revenues for fiscal 1987, which is similar to a reduction of $14.0 billion for fiscal 1987 after adding the provision affecting individuals in the JCT report (May 1987 Appendix Tables) on TRA 1986. The JCT estimates the revenue impact of the rate reductions alone to be $16.9 billion for fiscal 1987. The Act provided for rate reductions in two stages, with 1987 as a transitional tax year. I estimate the calendar 1987 revenue impact of the rate reductions as $-16.9 \times 1.25 = $-\$21.1$ billion and the combined effect of the other provisions as $(-14.0 + 16.9) \times 1.25 = \$3.7$ billion. The prior law schedule for 1987 is described in the JCT report on TRA 1986 and corresponds to the tax schedule for 1986 under the inflation adjustment to the tax brackets using the IRS method. The implications of TRA 1986 for payroll tax liabilities are very small ($-\$0.1$ billion).

12. **Omnibus Budget Reconciliation Act of 1987** Signed 12/22/87; The act made some minor changes to tax credits and deductions. The Conference Report (100th Congress, 1st Session, House of Representatives Report No. 100-495, 12/21/87) gives a detailed breakdown of the revenue effects for fiscal years 1988 and 1989. Summing the 1988 FY impact and one quarter of the 1989 FY impact yields an estimated change of $-\$0.2$ billion in income tax liabilities for calendar 1988. The act also increased employment taxes by expanding the social security wage base and increasing the FUTA rate. Based on numbers in the Conference Report these measures increased payroll tax liabilities by $1.7 billion in calendar 1988 of which $0.5 billion is due to expansions of the FICA wage base.

13. **Omnibus Budget Reconciliation Act of 1990** Signed 11/5/90; The Omnibus Budget Reconciliation Act of 1990 increased personal income taxes for upper-income taxpayers by three provisions: a higher top tax rate, a revised phaseout of personal exemptions, and a limit on itemized deductions. It also imposed a new statutory rate of 31 percent on certain income of high-income taxpayers and replaced a set of provisions enacted in TRA-86 that had created an implicit 33 percent statutory tax rate over a limited range but that had resulted in a top marginal rate of 28 percent for the highest-income taxpayers. (CBO, Projecting Federal Tax Revenues and the Effect of Changes in Tax Law p. 27,31). The JCT Report (October 1990) provides detailed estimates of the fiscal year impacts of all provisions. The total impact of $2.9 billion for calendar 1991 is obtained by summing the FY 1991 amounts and one quarter of the FY 1992 amounts for provisions affecting individual income taxes. Based on the JCT report, the basic, minimum and maximum rate changes account for $1.3 billion. The prior law tax schedule is the 1990 schedule adjusted for inflation using the IRS method. The Act also affected employment taxes by instituting a large increase in the Medicare tax earnings cap, expanding the coverage of social security to State and Local employees not participating in a public employee retirement system and by extending the higher FUTA rate. Based on the JCT report, these measures increased payroll tax liabilities by $5.3 billion, of which $4.2 billion is due to higher FICA taxes.

14. **Omnibus Budget Reconciliation Act of 1993** Signed 8/10/93; The Omnibus Reconciliation Act of 1993 increased income tax rates, mostly for higher earners. The JCT report on OBRA 1993 (August 1993) provides a by-component breakdown of the revenue implications for fiscal 1994 onward, including details on components retroactive to 1/1/93. The revenue effects for the 1993 part of the tax reform

are mostly entirely due to the increase in individual tax rates. I adopt the JCT estimate of $15.4 billion for the full fiscal 1994 as the estimated impact of the rate changes for calendar 1993. The prior law tax schedule is the 1992 schedule adjusted for inflation using the IRS method. The act also eliminated the earnings cap for the Medicare tax (Hospital Insurance). The removal of the cap ($2.8 billion in 2004) was only effective in 1994 and is therefore not included.

15. **Jobs and Growth Tax Relief Reconciliation Act of 2003** Signed 8/10/03; The Jobs and Growth Tax Relief Reconciliation Act of 2003 included an expansion of the 10 percent tax bracket, an increase in exemptions for the minimum tax, a reduction in rates on married couples, an increase in the child credit, as well as lower a reduction in taxes on dividends and capital gains. The JCT report (May 2003) provides detailed estimated of the fiscal year impacts of all provisions. The total impact of -$63.5 billion for calendar 2003 is obtained by summing the FY 1993 amounts and one quarter of the FY 1994 amounts for provisions affecting individual income taxes. Based on the JCT report, the rate reductions account for a decrease of $43.0 billion in liabilities. The prior law tax schedule is the one legislated for 2003 by the 2001 Economic Growth and Tax Relief Reconciliation Act of 2001 after inflation adjustments using the IRS method.

Table A Estimated Tax Liability Impact of Selected Tax Reforms

		Federal Individual Income Tax		Social Security Tax	
	Due to	Changes in rate schedule	Other changes	OASDI-HI changes	Other changes
		[1]	[2]	[3]	[4]
	In year				
[1] Revenue Act of 1948	1948	−2.4	−2.2	0	0
[2] Internal Revenue Code of 1954	1954	0	-0.8	0	0
[3] Revenue Act of 1964	1964	−6.3	0.2	0	0
[4] Revenue Act of 1971	1971	0	−2.0	0	0
[5] Tax Reform Act of 1976	1976	1.65	0	0	0
[6] Tax Reduction and Simplification Act of 1977	1977	0	−5.4	0	0
[7] Revenue Act of 1978	1979	−10.4	−4.8	0	0
[8] Economic Recovery Tax Act 1981	1981	−3.6	0.5	0	0
[9] Tax Equity and Fiscal	1982	0	0.7	0	0
Responsibility Act of 1982	1983	0.7	3.1	0.8	2.2
[10] Deficit Reduction Act of 1984	1984	0	2.1	0	0
[11] Tax Reform Act of 1986	1987	−21.1	3.7	0	−0.1
[12] Omnibus Budget Reconciliation Act of 1987	1988	0	−0.2	0.5	1.2
[13] Omnibus Budget Reconciliation Act of 1990	1991	1.3	1.5	4.2	1.1
[14] Omnibus Budget Reconciliation Act of 1993	1993	15.4	0	0	0
[15] Jobs and Growth Tax Relief Reconciliation Act of 2003	2003	−43.0	−20.5	0	0

Billions of current dollars. Column [1] are numbers due to changes in rate schedules of the individual income tax only. Column [2] provides the impact of other provisions affecting individual income tax liabilities.

B Robustness Analysis

Table B summarizes results for a number of robustness checks. Each column present the first two impulse response coefficients of average market income associated with a one percent increase to the aggregate net-of-tax rate. These are derived from SVARs identical to the benchmark model described in Section 4.3 but with the specified modification.

Sensitivity to Selection of Tax Reforms. Previous studies have found significant variation in the income responses across tax reforms. This variation could driven by insufficient controlling for coincident but unrelated changes in the determinants of income or by the specific attributes of each reform. By substantially increasing corporate relative to individual tax rates, the 1986 Tax Reform Act provided for instance particularly large incentives for income shifting, see Slemrod (1995, 1996). Given the relatively low number of tax changes used for identification, the results are therefore potentially sensitive to the inclusion of particular reforms. Columns [1] to [9] presents the SVAR results using alternative proxies for exogenous tax shocks that each omit the specified tax reforms using a proxy based on replacing the corresponding observations in Table 3 by zero. This generally has the expected effect of widening the confidence bands somewhat, in particular when the large 1964 Kennedy tax reform is dropped. However, all of the point estimates remain statistically significant and close in size to the benchmark model. Omitting the 1986 reform or both of the 1980 reforms leads to moderately larger ETI estimates.

Sensitivity to SVAR Specification and Sample Choice. SVAR results are sometimes sensitive to lag length, the inclusion of time trends or sample modifications. Without further steps to reduce the dimension of the estimation problem, preserving a reasonably rich set of endogenous controls in the VAR system dictates a choice of lag length of either one, two or three. For the 9 variables benchmark system, the Schwarz, Hannan-Quinn and Akaike criteria disagree and recommend respectively one, two and three lags. Residual tests reveal clear evidence for residual autocorrelation when only one lag is used, in particular for the AMTR equation. Such evidence disappears with two or three lags. Columns [10] and [11] report results when instead of the benchmark choice of two lags, one or three lags are included. Including just one lag leads to considerably lower estimates but is almost surely inadequate for obtaining plausible tax rate innovations. Including three lags of all the endogenous variables leads to point estimates that are very similar to the benchmark but with wider confidence intervals. The wider confidence intervals are partly due to the larger number of parameters, but also because the use of three lags excludes 1948 from the sample, which is one of the tax reform years used for identification.

Columns [12] and [13] assess the consequences of dropping the dummies for 1949 and 2008. As discussed in the main text, this lowers the point estimates for the following year ETIs from 1.37 to 0.96 and 1.15 respectively, although these estimates remain significant at the 5% level. The large tax cuts under the 1948 Tax Reform Act and the brief deflationary 1949 recession were both somewhat unusual. Major elements in the recession were the backlogs built up during the war and monetary tightening, see Caplan (1956). The 1948 tax reform was also atypical because it introduced the split-income method for joint returns, which lowered tax rates for primary earners but raised them for secondary earners. The results are much more sensitive to the 1949 dummy than to including the 1948 reform among the identifying variation. Restricting the sample to exclude the 1940s (column [15]) raises the estimate to 1.41. In the 1950-2006 sample, which is the one used in the main analysis of Barro and Redlick (2011) and in Romer and Romer (2010) and Mertens and Ravn (2013), the estimated ETI is rises to 1.50 (column [16]). In the 1960-2000 sample studied by Saez (2004) and Saez et al. (2012), the estimate equals 1.40 which is very close to the benchmark (column [16]). The confidence bands are considerably wider in this case because of the smaller sample and the omission of

the 1948 and 2003 reforms in the identification. As in Saez (2004) and Saez et al. (2012), column [16] adds a linear and quadratic time trend and in column [18] the model includes an additional cubic term. This leads to moderately lower but nevertheless highly significant ETI estimates of 1.19 and 1.17.

Sensitivity to Alternative Tax Rate Measures and Proxies.   Column [19] in Panel C shows that the alternative AMTR measure based on the Piketty and Saez (2003) income concept leads to slightly larger ETI estimates at both horizons. Columns [20] and [21] replace the AMTR series with the series capturing only the federal individual income tax component, as shown in the upper panel of Figure 2. For both income concepts, omitting the social security component leads to moderately higher point estimates. Note that the implied level decrease in the marginal tax rate is slightly larger in this case. The higher estimates could therefore mean that the tax semi-elasticity of income is more stable than the elasticity. Overall, the inclusion of payroll taxes has no major impact on the findings of this paper. As discussed in the main text, almost all of the identifying variation in postwar US tax rates originates with the federal income tax.

Columns [22] and [23] show results based on alternative proxies for exogenous variation in marginal tax rates based on official estimates of the tax liability impact of the broader selection of 15 tax reforms. In column [22] the proxy is based on tax liability impact estimates associated with specific provisions of the reforms that made direct changes to the basic rate schedules. These numbers are obtained from a number of official government sources and are described in detail in appendix A. The resulting elasticities are very similar to the benchmark. Column [23] uses estimates of the tax liability impact of all the provisions of the 15 tax reforms, including those with no or only indirect implications for marginal rates. This series is very similar to the narrative variables used by Romer and Romer (2010) and Mertens and Ravn (2013) as measures for shocks to average rates. As a measure of shocks to marginal rates it is less accurate, especially in case of reforms without provisions directly affecting marginal rates. This measure yields an ETI of 1.25 that is significant at the 5% level, which is similar in size to the benchmark but is also less precise.

Sensitivity to the Inclusion of Additional Control Variables.   The estimates in Panel D of Table B are all derived from SVARs that add an additional endogenous variable relative to the benchmark nine variable system. Column [24] adds the average realized capital gains reported on income tax returns, which are very responsive to the timing of tax changes and may therefore have additional predictive power for marginal tax rates. Column [25] adds Ramey's (2011b) measure of news about future changes in defense spending. Because marginal tax rates have been systematically adjusted in reaction to changes in defense spending, this variable may also contain useful information for predicting future marginal tax rates. Following Slemrod (1996) and Barro and Redlick (2011), column [25] includes corporate bond rates as a measure of credit conditions and as a determinant of taxable interest. Columns [27] and [28] add educational attainment, measured by the fraction of college-educated adults, and the female labor participation rate to capture longer run trends relevant for income and tax rate dynamics. Similarly, columns [29] to [31] include measures of income inequality and earnings dispersion to help explain lower frequency movements in income and tax rates. While these additional control variables inevitably lead to variation in the elasticity estimates, none of them has any major impact. In all cases, the point estimates exceed unity and are significant at the 5% level. The last column in Table B presents results derived from a model that controls for the fact that corporate tax shocks may be correlated with AMTR innovations. The methodology is identical to Mertens and Ravn (2013) and relies on an annual version of the quarterly narrative series for corporate tax changes described in that paper. The VAR specification also includes the average corporate income tax rate as an additional endogenous variable. The estimates in column [32] measure the income response to a one percent increase in the net-of-tax rate while restricting the innovation to the corporate tax rate to zero. This yields very similar results as the benchmark model.

Table B Robustness of SVAR Estimates of Short Run Tax Elasticities of Aggregate Income

A. Omitting Tax Reforms

	[1] RA 1948	[2] RA 1964	[3] RA 1978	[4] ERTA 1981	[5] TRA 1986	[6] ERTA 1981 +TRA 1986	[7] OBRA 1990	[8] OBRA 1993	[9] JGTRRA 2003
Same year	0.69***	0.73*	0.68**	0.68***	0.92***	0.88***	0.64**	0.55**	0.84***
	(0.25, 1.25)	(−0.03, 1.89)	(0.16, 1.33)	(0.18, 1.30)	(0.25, 2.19)	(0.24, 2.02)	(0.12, 1.25)	(0.03, 1.11)	(0.30, 1.67)
Following year	1.39***	1.31**	1.31***	1.34***	1.61***	1.56***	1.31***	1.16***	1.61***
	(0.56, 2.26)	(0.18, 2.90)	(0.41, 2.27)	(0.50, 2.31)	(0.56, 3.31)	(0.56, 3.07)	(0.45, 2.26)	(0.29, 2.07)	(0.73, 2.86)

B. Alternative Specifications and Samples

	[10] One lag in VAR	[11] Three lags in VAR	[12] No 1949 dummy	[13] No 2008 dummy	[14] 1950-2012 Sample	[15] 1950-2006 Sample	[16] 1960-2000 Sample	[17] Quadr. trend	[18] Cubic trend
Same year	0.48**	0.57*	0.76**	0.62**	0.69***	0.73**	0.73	0.62***	0.61**
	(0.06, 1.02)	(−0.08, 1.38)	(0.01, 1.24)	(0.08, 1.29)	(0.20, 1.30)	(0.12, 1.60)	(−0.24, 2.33)	(0.17, 1.26)	(0.14, 1.20)
Following year	0.51	1.26**	0.96**	1.15**	1.41***	1.50**	1.40**	1.19***	1.17**
	(−0.11, 1.14)	(0.18, 2.56)	(0.04, 1.76)	(0.26, 2.18)	(0.44, 2.34)	(0.44, 2.74)	(0.09, 3.49)	(0.50, 2.12)	(0.42, 2.02)

C. Alternative Tax Rate Measures and Proxies

	[19] AMTR PS 2003	[20] AMIITR BR 2011	[21] AMIITR PS 2003	[22] Marg. Tax Liab. Proxy	[23] Total Tax Liab. Proxy
Same year	0.75***	0.81***	0.85***	0.70***	0.55
	(0.25, 1.46)	(0.22, 1.72)	(0.32, 1.76)	(0.18, 1.38)	(−0.16, 1.46)
Following year	1.45***	1.52***	1.66***	1.41***	1.25**
	(0.61, 2.53)	(0.54, 2.98)	(0.76, 3.13)	(0.52, 2.47)	(0.26, 2.71)

D. Additional Controls

	[24] Capital Gains	[25] Ramey News	[26] Corp. Baa rate	[27] Educ- ation	[28] Female Part. Rate	[29] Top 10% Share	[30] Gini Coeff.	[31] Wage Ineq.	[32] Corp. Tax
Same year	0.43*	0.61**	0.79***	0.59**	0.70**	0.59**	0.66**	0.54**	0.63*
	(−0.01, 1.23)	(0.07, 1.17)	(0.19, 1.52)	(0.09, 1.30)	(0.12, 1.47)	(0.09, 1.12)	(0.10, 1.39)	(0.03, 1.21)	(−0.10, 1.45)
Following year	1.06***	1.19***	1.47***	1.04**	1.44***	1.11***	1.30**	1.07***	1.35**
	(0.31, 2.22)	(0.36, 2.14)	(0.45, 2.52)	(0.20, 2.13)	(0.47, 2.79)	(0.25, 1.91)	(0.30, 2.40)	(0.24, 2.13)	(0.27, 2.52)

Notes: Elasticities are the first two SVAR impulse response coefficients of average market income to a one percent increase to the aggregate net-of-tax rate. In parentheses are 95% confidence intervals based on 10,000 wild bootstrap replications. Asterisks denote 10%, 5% or 1% significance. PS 2003 and BR 2011 refer to the AMTR series using the income definition of Barro and Redlick (2011) and Piketty and Saez (2003) respectively. AMIITR refers to the tax rate series for the federal individual income tax only. The additional variables in the bottom panel are described in appendix A.