NBER WORKING PAPER SERIES

TAX CUTS FOR WHOM? HETEROGENEOUS EFFECTS OF INCOME TAX CHANGES ON GROWTH AND EMPLOYMENT

Owen M. Zidar

Working Paper 21035
http://www.nber.org/papers/w21035

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2015

I am grateful to Alan Auerbach, Dominick Bartelme, Alex Bartik, Marianne Bertrand, David Card, Gabe Chodorow-Reich, Austan Goolsbee, Greg Kaplan, Ben Keys, Pat Kline, Attila Lindner, Zachary Liscow, Neale Mahoney, Atif Mian, John Mondragon, Enrico Moretti, Matt Notowidigdo, Christina Romer, David Romer, Jesse Rothstein, Emmanuel Saez, Jim Sallee, Andrew Samwick, Amir Sufi, Laura Tyson, Johannes Wieland, Dan Wilson, Danny Yagan, and Eric Zwick for helpful comments and Dan Feenberg for generous help with TAXSIM. I am especially thankful to Amir Sufi as well as Marianne Bertrand and Adair Morse for generously sharing data with me. This project grew out of an undergraduate research project that I worked on with Dan Cohen, and I am grateful to him and Jim Feyrer for input on the paper at its inception. Prab Upadrashta and Stephen Lamb provided excellent research assistance. This work is supported by the Kathryn and Grant Swick Faculty Research Fund at the University of Chicago Booth School of Business. The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

Tax Cuts For Whom? Heterogeneous Effects of Income Tax Changes on Growth and Employment
Owen M. Zidar
NBER Working Paper No. 21035
March 2015
JEL No. E32,E62,H2,H20,H31,N12

ABSTRACT

This paper investigates how tax changes for different income groups affect aggregate economic activity. I construct a measure of who received (or paid for) tax changes in the postwar period using tax return data from NBER's TAXSIM. I aggregate each tax change by income group and state. Variation in the income distribution across U.S. states and federal tax changes generate variation in regional tax shocks that I exploit to test for heterogeneous effects. I find that the positive relationship between tax cuts and employment growth is largely driven by tax cuts for lower-income groups and that the effect of tax cuts for the top 10% on employment growth is small.

Owen M. Zidar
University of Chicago
Booth School of Business
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
owen.zidar@chicagobooth.edu

Changes to income tax policy in the U.S. have varied substantially in the postwar period. In the early 1980s and 2000s, the largest tax cuts as a share of income went to top income taxpayers. In the early 1990s, top income earners faced tax increases while taxpayers with low to moderate incomes received tax cuts. This paper investigates how the composition of these tax changes affects subsequent economic activity. The possibility that the impact of tax changes depends not only on how large the changes are, but also on how they are distributed has important implications for understanding macroeconomic activity, designing countercyclical policy, and assessing the consequences of many redistributive policies.

The main contribution of this paper is to use new data and a novel source of variation to quantify the importance of the distribution of tax changes for their overall impact on economic activity. I find that the positive relationship between tax cuts and employment growth is largely driven by tax cuts for lower-income groups and that the effect of tax cuts for the top 10% on employment growth is small.

Establishing this result – that tax cuts that go to high income taxpayers generate less growth than similarly-sized tax cuts for low and moderate income taxpayers – requires overcoming three empirical difficulties: endogeneity, simultaneity, and observability. First, many tax changes happen in response to current or expected economic conditions. Second, tax changes for low and high income taxpayers often occur at the same time. Third, the number of data points and tax changes in the postwar period is limited.

This paper uses variation in the regional impact of national tax shocks to overcome these empirical difficulties. Federal income tax changes have heterogeneous regional impacts due to variation in the income distribution across U.S. states. For instance, Connecticut, whose share of top income taxpayers is nearly twice that of the typical state, faced relatively larger shocks to high income earners after the Omnibus Budget Reconciliation Act of 1993, which raised top income tax rates. I focus on a subset of federal tax changes that are not related to the current state of the economy according to the classification approach of Romer and Romer (2010).[1] The interaction of (1) regional heterogeneity and (2) exogenous federal tax changes produces plausibly exogenous regional tax shocks, differently-sized shocks for different income groups, and more data on the economic consequences of tax changes.

I use individual tax return data from NBER's TAXSIM to quantify these tax shocks. For

[1] They use the historical record (such as congressional records, economic reports and presidential speeches) to identify tax changes that were taken for more exogenous reasons such as pursing long run growth or deficit reduction. Doing so reinforces my ability to overcome endogeneity concerns.

each tax change, I construct a measure of who received (or paid for) the tax change. The measure of the tax change is based on three things for every individual return: income and deductions in the year prior to an exogenous tax change, the old tax schedule, and the new tax schedule. For example, consider a taxpayer in 1992 whose income was $180,000. Based on her 1992 income and deductions, she would have paid $50,500 in taxes according to the old 1992 tax rate schedule and $54,000 according to the new 1993 tax rate schedule. My measure assigns her a $3,500 tax increase for 1993. I use the prior year tax data to avoid conflating behavioral responses and measured changes in tax liabilities. After calculating mechanical tax changes for each individual taxpayer, I then aggregate these tax changes for each taxpayer in a state in a given income group, such as the bottom 90% and top 10% of national AGI respectively.

With these year-state-income group level tax shock measures, I investigate how responsive employment growth is to tax shocks for different income groups. The empirical analysis has three components: (1) evidence of heterogeneous effects, (2) research design validation, and (3) mechanisms and national results.

I begin by presenting non-parametric plots of state employment growth and tax changes for different income groups: the top 10%, the top 50%, the bottom 90%, and the bottom 50%. If tax cuts for high income earners generate substantial economic activity, then states with a large share of high-income taxpayers should grow faster following a tax cut for high income earners. Similar logic applies for tax changes that affect other portions of the income distribution. I find that subsequent state employment growth is substantially more responsive to tax shocks for lower income groups than to equally-sized tax shocks for top earners. In particular, a 1% of state GDP tax cut for the bottom 90% results in roughly 5.1 percentage points of employment growth over a two-year period. The corresponding estimate for the top 10% is 0.1 percentage points and is statistically insignificant.

These heterogeneous effects motivate analyzing the function $\beta(g)$, which maps an income group g to the reduced-form effect of a tax change for that group on state employment growth. I estimate a second order approximation of the $\beta(g)$ function using tax changes for each income decile. My estimate of the $\beta(g)$ function resembles marginal propensity to consume (MPC) as a function of income. The result that the impact of tax changes over the income distribution resembles MPC suggests demand-side forces are important determinants of the stimulative effect of tax policy.[2]

[2] See Section 3.4 for a discussion of these issues. It is worth highlighting that this emphasis on the *demand-side*

The second part of the empirical analysis provides evidence that supports assumptions underlying these results. The research design compares employment growth in states like Connecticut to employment growth in states with less exposure to high income shocks to identify the impact of high income tax changes.[3] The validity of this comparison relies on three key assumptions: (1) outcomes from less exposed states provide a reasonable counterfactual, (2) targeted tax shocks are unrelated to targeted spending shocks, and (3) state tax shocks are exogenous. I provide three pieces of evidence supporting the first assumption. Event studies show no systematic relationship in the path of log employment preceding the tax shocks for bottom and top income groups. In addition, specifications that allow for flexible trends produce similar results. Placebo tests that use outcomes from prior years before the event show that the nature of these comparisons does not mechanically produce my results. In terms of the second assumption, I primarily adopt a control function approach. I show that controlling for changes in per capita transfer payments, which may be correlated with progressive tax changes, does not materially affect the estimates in a variety of specifications. I also split the sample into states that have below and above median per capita transfer payments. This approach reveals that my estimates are not coming from the portion of the sample that has high transfer payments. It is likely that high transfer payments are correlated with other forms of progressive spending, so this split sample test provides further evidence that unobserved progressive spending changes are not driving my results. Finally, since I control for state and year fixed effects, the third assumption maintains that federal policymakers are not systematically setting tax policy to respond to *idiosyncratic state shocks*. Relying on variation from federal tax changes that Romer and Romer (2010) classify as exogenous makes it less likely policymakers are responding to idiosyncratic state shocks since the Romer and Romer (2010) changes are due to concerns about long-run aggregate growth and inherited budget deficits. To support the exogeneity assumption by income group, I show that these federal tax shocks for each income group pass the Favero and Giavazzi (2012) orthogonality test, which amounts to showing that the raw series of tax shocks by group are similar to these series after partialling out macro aggregates.

The third part of empirical analysis focuses on national results that aggregate tax changes by year. Since there are only roughly sixty years in the postwar period, these estimates are inherently imprecise. However, they are strikingly consistent with the regional results – I find

differs from the typical focus on labor *supply* responses to tax changes.

[3] Similar logic applies for federal tax changes that affect other portions of the income distribution.

that the relationship between upper income tax changes and growth is negligible to small in magnitude and substantially weaker than equivalently sized tax changes for lower-income groups. My national estimates from specifications that separate those with top 10% incomes and those with bottom 90% incomes suggest that almost all of the stimulative effect of exogenous tax cuts is due to tax cuts for the bottom 90%. In particular, a 1% of GDP tax increase reduces two-year employment growth by 1.5 (se=1.1) percentage points. The corresponding point estimate for the top 10% is -0.2 (se=1.0) percentage points. This similarity between the state and national estimates suggests that tax cuts for high-income taxpayers are small and have limited spillovers on employment growth in other states. If there were large effects, they would show up somewhere. Finally, in terms of mechanisms, I provide evidence on consumption, investment, and labor force participation. Differential consumption responses help explain why a dollar of tax cuts for the top 10% produces less growth than those for the bottom 90%. Investment responses are also stronger following tax cuts for the bottom 90%, suggesting that the effects of additional economic growth tend to exceed the effects from income changes among those who are more likely to save. Aggregate impacts on labor force participation are also largest for tax shocks for the bottom half of the income distribution, suggesting that differential labor supply responses contribute to the observed difference in the impacts on economic activity. Overall, tax cuts for the bottom 90% tend to result in more output, employment, consumption, and investment growth than equivalently sized tax cuts for the top 10% over a business cycle frequency.

To my knowledge, this is the first paper to quantify the importance of heterogeneity in terms of the aggregate effects of fiscal policy. Many theoretical papers support the notion that heterogeneity matters in the context of fiscal policy. Monacelli and Perotti (2011) use an incomplete markets model with borrowing constraints to show that lump sum redistribution from savers to borrowers is expansionary when nominal prices are sticky. The main intuition is that while both borrowers and savers optimize inter-temporally, redistribution to borrowers *also* relaxes their borrowing constraint and results in a level of consumption that exceeds the amount that savers reduced their consumption. This higher level of aggregate consumption raises output and employment. Similarly, Heathcote (2005) finds that temporary tax cuts can have large real effects in simulated models with heterogeneous agents and incomplete markets. Gali et al. (2007) show that macro models with some cash-on-hand agents and sticky prices do a better job explaining observed aggregate consumption patterns than representative-agent models. More recently, Farhi and Werning (2013) show that regional transfers and exogenous stimulus payments can result in multipliers that are substantially larger than 1.

This paper also builds on the empirical literature on multipliers surveyed by Ramey (2011). In particular, the empirical approach in this paper resembles that of Nakamura and Steinsson (2014), but for taxes (with heterogeneity) rather than government spending.[4] This regional approach complements the approach of Mertens and Ravn (2013a) who investigate differences for personal income and corporate taxes as well as Mertens (2013) for top income groups using a time series approach with national data on tax *rates*. Constructing a new measure of tax *changes* based on micro tax return data also contributes to this literature because measurement error can partly explain large differences in the estimated effects of fiscal policy (Mertens and Ravn, 2013b).

The empirical literature on consumption and tax responses provides evidence of mechanisms that could generate heterogeneous aggregate effects. Numerous studies provide evidence that lower-income households tend to have higher marginal propensities to consume (Parker, 1999; Dynan et al., 2001; McCarthy, 1995; Jappelli and Pistaferri, 2010).[5] Aaronson et al. (2012) show large micro consumption responses (especially on durable goods) following minimum wage increases. More broadly, Chetty et al. (forthcoming) estimate that approximately 85% of individuals are rule-of-thumb spenders. Saez and Zucman (2014) also show total savings among the bottom 90% is roughly zero and has been flat since the 1980s. Micro evidence also suggests that the costs of raising taxes on top income taxpayers in terms of labor supply and other margins may be limited (Saez et al., 2012; Romer and Romer, 2012) and largely reflect shifting in the timing or form of income (Goolsbee, 2000; Auerbach and Siegel, 2000).

1 Econometric Model

This section describes three approaches to measuring the relationship between changes in taxes for different groups to subsequent economic activity. The first two are at the state level and the third is at the national level. First, I look at the relationship between (i) two-year changes

[4] See Suárez Serrato and Wingender (2011) for a paper estimating how high and low-skilled workers respond to different types of government spending shocks. Chodorow-Reich et al. (2012) and Hausman (2013) use similar methods to analyze two important fiscal policy episodes – Medicaid payments to states in the Great Recession and payments to veterans in 1936, respectively. Important contributions also include Clemens and Miran (2012); Shoag (2010); Wilson (2011).

[5] Note that not all papers, e.g., Shapiro and Slemrod (1995), find significant differences in spending responses as a function of income.

in taxes by income group and (ii) two-year changes in economic activity. Second, I look at the dynamic relationship between tax changes for these groups and economic activity. Finally, I look at national relationships using a specification that is similar to that of Romer and Romer (2010), but decomposed by income group. The national approach, while inherently noisy due to limited data, supplements the state results by (1) quantifying aggregate effects and (2) testing different mechanisms using data that are available only at the national level.

1.1 Baseline Specification

In a given state s and year t, the outcome $y_{s,t}$ is decomposed into a state component α_s, a time component δ_t, the effects of tax shocks $T^g_{s,t}$ for income group g, an index of time-varying state-characteristics $\mathbf{X}'_{s,t}\mathbf{\Lambda}$, and a residual component $\varepsilon_{s,t}$:

$$y_{s,t} = \alpha_s + \delta_t + \sum_g \beta(g) T^g_{s,t} + \mathbf{X}'_{s,t}\mathbf{\Lambda} + \varepsilon_{s,t} \tag{1}$$

where the function $\beta(g)$, which maps an income group g into the reduced-form effect of a tax shock for that group, is the key object of interest. Tax shocks are expressed as a share of state GDP to facilitate comparisons over time. I use three different ways to divide income groups: (i) $g \in \{\text{Bottom 90}, \text{Top 10}\}$, (ii) $g \in \{\text{Bottom 50}, \text{ Top 50}\}$, and (iii) $g \in \{\text{Decile 1}, \text{Decile 2}, ..., \text{Decile 10}\}$. The first grouping is parsimonious and results in similarly-sized tax shocks in dollar terms for the two groups. The second directly highlights the bottom half of the income distribution. The third grouping requires estimating more parameters, but can show effects across the income distribution more flexibly. To fix ideas, consider the first grouping, which gives the following estimating equation:

$$y_{s,t} = \alpha_s + \delta_t + \beta^{B90} T^{B90}_{s,t} + \beta^{T10} T^{T10}_{s,t} + \mathbf{X}'_{s,t}\mathbf{\Lambda} + \varepsilon_{s,t}$$

where $T^{B90}_{s,t}$ is an exogenous tax shock as a share of state GDP for taxpayers who are in the bottom 90% of AGI nationally and $T^{T10}_{s,t}$ is defined analogously.

1.2 Two-Year Effects of Tax Changes for Different Income Groups

Using two-year changes parsimoniously shows the relationship between growth and tax changes for different income groups. When the outcome $y_{s,t}$ is a two-year growth rate, two-year tax shocks are defined analogously, and $\mathbf{X}'_{s,t}\mathbf{\Lambda}$=0, I obtain the specification used by Nakamura and Steinsson (2014) for tax shocks (by income group) rather than for government spending shocks:

$$\frac{Y_{s,t} - Y_{s,t-2}}{Y_{s,t-2}} = \alpha_s + \delta_t + \beta^{B90}\left(\frac{Tax^{B90}_{s,t} - Tax^{B90}_{s,t-2}}{Y_{s,t-2}}\right) + \beta^{T10}\left(\frac{Tax^{T10}_{s,t} - Tax^{T10}_{s,t-2}}{Y_{s,t-2}}\right) + \varepsilon_{s,t}. \quad (2)$$

In this case, the year-fixed effects δ_t absorb aggregate macroeconomic shocks and the state-fixed effects effectively control for different state trends in the outcome.

For OLS to identify the parameters of interest, tax shocks need to be exogenous conditional on fixed effects and controls, i.e., $\mathbb{E}(\varepsilon_{s,t}|\alpha_s, \delta_t, \Delta T^{B90}_{s,t}, \Delta T^{T10}_{s,t}) = 0$ where $\Delta T^g_{s,t} \equiv \left(\frac{Tax^g_{s,t} - Tax^g_{s,t-2}}{Y_{s,t-2}}\right)$. Intuitively, this identifying assumption is that national tax shocks, which Romer and Romer (2010) define as exogenous, are not disproportionately favoring states that are doing poorly relative to how fast they normally grow. In addition, an important aspect of this identifying assumption is that other policy changes, such as progressive spending policy, do not tend to occur at the same time as progressive tax policy. If these policies systematically occur at the same time and both increase growth, then β^{B90} would reflect both the true effect of tax changes for the bottom 90% and the effects of spending policies and result in upwardly-biased estimates. I use a control function approach to address this concern. Ideally, I could use a series of exogenous spending changes by income group, but to my knowledge no such series exists. Absent this ideal data, I focus on the practical concern of coincident progressive spending policy and include controls for government transfer income as a share of GDP. Using this control function approach and identifying these parameters enables me to test $\beta^{B90} = \beta^{T10}$ to determine if there are heterogeneous effects of tax changes by income group.[6]

In addition to estimating the effect for these two different groups, I characterize the effect of tax changes β as a function of the income group g. A flexible second order approximation of the $\beta(g)$ function is $\beta(g) = \theta_0 + \theta_1 g + \theta_2 g^2$. This function maps an income group into the effect on growth from a tax change over the last two years for that income group. Rewriting the main estimating equation 1 and plugging in the flexible approximation for the β function yields the following specification:

$$\Delta Y_{s,t} = \beta_1 \Delta T^1_{s,t} + \beta_2 \Delta T^2_{s,t} + ... + \beta_{10} \Delta T^{10}_{s,t} + u_{s,t}$$

$$\Delta Y_{s,t} = \underbrace{(\theta_0 + \theta_1 + \theta_2)}_{=\beta_1} \Delta T^1_{s,t} + \underbrace{(\theta_0 + \theta_1 2 + \theta_2 2^2)}_{=\beta_2} \Delta T^2_{s,t} + ... + u_{s,t}$$

$$\Delta Y_{s,t} = \theta_0 \left(\sum_{g=1}^{10} \Delta T^g_{s,t}\right) + \theta_1 \left(\sum_{g=1}^{10} g \times \Delta T^g_{s,t}\right) + \theta_2 \left(\sum_{g=1}^{10} g^2 \times \Delta T^g_{s,t}\right) + u_{s,t} \quad (3)$$

[6] I also use analogous specifications using $g \in \{\text{Bottom 50, Top 50}\}$.

where $\Delta Y_{s,t} \equiv \frac{Y_{s,t}-Y_{s,t-2}}{Y_{s,t-2}}$ is outcome growth, $\Delta T^g_{s,t} \equiv \left(\frac{Tax^g_{s,t}-Tax^g_{s,t-2}}{Y_{s,t-2}}\right)$ is an analogously defined tax change for income decile g, and $u_{s,t}$ is a residual that includes state and year fixed effects. Thus, I can use a simple regression of outcome growth on sums of simple functions involving tax changes across income groups to recover estimates of θ_0, θ_1, and θ_2, which will show how the effects of tax changes vary across the income distribution. These estimates will also enable me to test for differences across groups using the $\beta(g)$ function.

1.3 Dynamic Effects of Tax Changes for Different Income Groups

The second approach for understanding the effects of tax changes for different groups is to analyze annual effects during a window of time around the event.

$$y_{s,t+h} = a_s + d_t + \sum_g b^{g,h} T^g_{s,t} + \mathbf{X}'_{s,t}\tilde{\mathbf{\Lambda}} + e_{s,t+h} \tag{4}$$

where $h \in \{-3, -2, ..., 3, 4\}$ is the horizon, $y_{s,t+h}$ is log employment in year $t+h$, $b^{g,h}$ is the reduced-form effect of a tax change as a share of GDP for group g in year t for the specification with horizon h. For example, the effect two years after a tax change when $g \in \{\text{Bottom } 90, \text{Top } 10\}$ without controls is:

$$y_{s,t+2} = a_s + d_t + b^{B90,2} T^{B90,2}_{s,t} + b^{T10,2} T^{T10,2}_{s,t} + e_{s,t+2}$$

This specification directly projects tax changes onto log employment two years later. This approach has several advantages. First, direct projections do not impose dynamic restrictions as noted by Jorda (2005); Stock and Watson (2007); Auerbach and Gorodnichenko (2013). Second, this approach allows me to estimate average outcomes before tax shocks to determine if tax shocks for different groups occur soon after unusually good or bad economic times. This determination helps reinforce the validity of the identifying assumption in the two-year change analysis. Third, the direct projections approach allows me to build on the traditional VAR approach of constructing shocks to government spending. Including a control for government transfers as a share of GDP in time t as well as a lag of log employment, i.e., $\mathbf{X}_{s,t} = \begin{bmatrix} G_{s,t} & y_{s,t+h-1} \end{bmatrix}'$, is equivalent to controlling for an innovation in government transfer spending $\tilde{G}_{s,t}$. This innovation is comparable to what would be included as a shock in a standard VAR with variables: Y, T, G. The ability to control for innovations in government transfers strengthens the control function approach. Finally, the direct projection approach also shows how the effects of tax changes vary over time and can potentially reveal anticipatory effects, which may vary by income group.

For OLS to identify the parameters of interest for each time horizon h, tax shocks need to be exogenous conditional on fixed effects and controls, i.e., $\mathbb{E}(e_{s,t+h}|a_s, d_t, \mathbf{X}_{s,t}) = 0$.[7] The identifying assumption is similar to that of the prior section, although here the outcome is in logs rather than in log differences. As a result, the state- and year-fixed effects adjust for level differences rather than state and year trends. The horizon is also shifted h years, so the specification with $h = 3$, for instance, relates innovations in government transfers three years ago to the error term this year.

1.4 National Effects of Tax Changes for Different Income Groups

The third approach is to look at the effects of tax changes for different groups at the national level. Since there are only sixty years in the postwar period (1950-2010), these estimates are inherently imprecise. However, they can reveal how the state-level effects aggregate to the national level and can account for the possibility that investment effects from top earners show up outside of the state of residence. I use a specification that is similar to the Romer and Romer (2010) specification, but decomposed by income group. They decompose annual outcome growth into terms related to the effect of tax changes as a share of output and a residual:

$$\Delta Y_t = \sum_{m=\underline{m}}^{\overline{m}} \left(\gamma_{B90,m} \Delta Tax_{t-m}^{B90} + \gamma_{T10,m} \Delta Tax_{t-m}^{T10} + \mathbf{X}'_{t-m} \mathbf{\Gamma}_m \right) + \nu_t, \tag{5}$$

where $\Delta Y_t \equiv \ln Y_t - \ln Y_{t-1}$, $\gamma_{B90,m}$ and $\gamma_{T10,m}$ are the effects of changes in taxes as a share of GDP at lag m. The identifying assumption here is the same as Romer and Romer (2010) plus the additional assumption that progressive spending does not confound the tax shocks. I adopt a control function approach and include transfers as a share of GDP, which assumes that conditional on this measure of progressive spending, the tax shocks for both income groups are exogenous. In addition, I also include a control for non-personal income tax changes, such as corporate tax changes, which is defined by subtracting my measure from the measure of exogenous tax changes from Romer and Romer (2010). I include non-income tax changes to ensure that other tax changes which may coincide with tax changes for different income groups are not driving the results.[8]

[7] Here, $\mathbf{X}_{s,t}$ includes the tax changes for each group.

[8] Note that this control is not necessary for the state-level analysis because of year-fixed effects.

2 Data

2.1 Tax Data

This section describes how I construct a national time-series of tax changes by income group from 1950-2011. The following section then shows how this national series is distributed across U.S. states.

2.1.1 National Tax Changes by Income Group

I use tax measures from NBER when possible and rely on the Statistics of Income (SOI) tables to calculate changes before 1960. See Appendix A for a description of how I calculate the four pre-NBER tax changes, which affected tax liabilities in 1948, 1950, 1954 and 1960.[9]

To calculate tax changes occurring after 1960, I use NBER's Tax Simulator TAXSIM, which is a program that calculates individual tax liabilities for every annual tax schedule since 1960 and stores a large sample of actual tax returns. I construct my measure of tax changes by comparing each individual's income and payroll tax liabilities in the year preceding an exogenous tax change to what their tax liabilities would have been if the new tax schedule had been applied. For instance, consider the 1993 Omnibus Budget Reconciliation Act, which raised rates on high-income taxpayers by adding new brackets in 1993 according to the schedule in Table 1. For every taxpayer, my measure subtracts how much he paid in 1992 from how much he would have paid in 1992 if the 1993 tax schedule had been in place. When calculating tax liabilities, TAXSIM takes into account every individuals' deductions and credits and their treatment under both the 1992 and 1993 tax schedules.[10] Panel A of Figure 1 plots the results for 1993.[11] Many individuals with adjusted gross income above $100,000, and especially those with adjusted gross income exceeding $150,000, faced a roughly thousand-dollar tax increase based on this measure.

After calculating a change in tax liability for each taxpayer, I collapse the data by averaging it for every income percentile of AGI. Panel B of Figure 1 shows the results for four recent, prominent tax changes. Based on this measure of tax changes, 1993 taxpayers below median

[9] This approach is similar to that of Barrow and Redlick (2011), who focus on *marginal rate* changes rather than *tax liability* changes.

[10] Note that this method avoids bracket creep issues in the period before the great moderation since the hypothetical tax schedule applies to the old tax form data. Since inflation has been low during the Great Moderation, measurement error induced by this approach (due to inflation indexing) is quite small in magnitude. Also, it is not obviously correct to weight old tax data by CPI since median income growth has stagnated. As such, adjusting for the mild inflation of the Great Moderation may exacerbate measurement error rather than reduce it.

[11] Note that the 1993 results are based on the sample of 1992 tax returns and the 1992 and 1993 tax schedules.

AGI received a modest tax cut of less than one percent of AGI and only the highest-income taxpayers faced higher taxes. A similar pattern emerges in 1991 under George H.W. Bush. In contrast, high-income taxpayers received the largest cuts in 1982 and 2003 under Reagan and Bush, respectively. Finally, to compute total changes in income and payroll taxes, I add each percentile's tax changes to form the bottom 90% and top 10% groups.

As a robustness check, I compare my measure to the Romer and Romer (2010) total tax change measure. They are quite similar. Summing my measure of tax changes across all income percentiles for each year yields similar results, as shown in Figure B in the Appendix. Total revenue figures are divided by nominal GDP in order to facilitate comparisons across years. Note that differences between my aggregate measure and the Romer and Romer (2010) measure are partially due to exogenous tax changes that did not affect income or payroll taxes, such as corporate income tax changes, and are defined accordingly: $\Delta Tax_{NONINC} \equiv \Delta Tax_{ROMER} - \Delta Tax_{INCOME}$.[12]

Exogenous tax changes occurred in thirty-one years of the postwar period.[13] In exogenous years, the average income and payroll tax change was -0.16% of GDP, or roughly $25 billion in 2011 dollars. It was -0.075% overall in the entire sample. On average, in exogenous years in which the top 10% taxpayers did not see a tax increase, the size of the tax cut for the bottom 90% and the top 10% was roughly the same size. In exogenous years in which the top 10% did see tax increases, the size of the tax increase as a share of output was an order of magnitude larger for the top 10% than for the bottom 90%. On average, tax changes have been negative for both groups, meaning that tax cuts as a share of output tend to be larger than tax increases as a share of output.

Figure 2 shows how income and payroll taxes have changed by AGI quintile since 1960. There are a few notable features. First, tax changes for different income groups often happen simultaneously. Based on Frisch and Waugh (1933) logic, a tax change that provides atypical changes to a given income group will influence estimates more strongly than proportionate tax changes. The Frisch Waugh regression figure, Appendix Figure A3, shows this point explicitly

[12]Note that ΔTax_{INCOME} includes both income and payroll tax changes; the subscript is abbreviated for brevity. Also note that their tax change measure is at a quarterly frequency, so I simply sum their measure to construct an annualized version.

[13]Exogenous is defined as a year in which Romer and Romer (2010) show a nonzero tax change where more than half the revenue was from an exogenous change. Stricter definitions of exogenous, i.e., ways to categorize years in which there were both exogenous and endogenous changes occurring in that year, produced very similar results.

– years like 2003 provided disproportionately larger tax cuts to the top 10% given the size of the tax change for the bottom 90%. Second, the magnitudes of tax changes for the top 10% are larger in share of output terms since their income share is large and has been increasing. Third, tax increases have been rare since the 1980s, especially on the bottom four quintiles. Fourth, the earlier tax increases on the bottom 90% mostly came through payroll tax increases before 1980.

2.1.2 State Tax Changes by Income Group

National tax changes have disparate impacts across regions of the United States due to substantial variation in the income distribution across states. Figure 3 shows the average share of taxpayers who have incomes in the top 10% nationally from 1981-2007. Based on this measure, a taxpayer in Connecticut is roughly three times more likely to be in the top 10% than a taxpayer in Maine.

Similar to the national changes, I define state tax shocks as a share of state GDP, i.e., $T^g_{s,t} \equiv \frac{\text{Tax Liability Change}^g_{s,t}}{GDP_{s,t}}$, where Tax Liability Change is the sum of mechanical changes in tax liability for all the residents in state s and group g in year t. Note that the income groups are defined on a national basis, so top 10% means a taxpayer's adjusted gross income is in the top 10% of national taxpayers (as opposed to a measure relative to others in their state).

I am able to aggregate by state since TAXSIM has a variable indicating the state of residence for nearly all tax returns. However, taxpayers with AGI above \$200,000 in nominal dollars have the state identifier removed in the IRS data.[14] This data limitation causes the first measure of tax changes to be approximated within TAXSIM for very high incomes at the state level.

Due to the \$200,000 censoring, I have to extrapolate part of the state shares for the top income group. I determine the total number of income earners whose incomes exceed the \$200,000 cutoff every year and allocate them according to extrapolated state shares for that year. I assume that each state's share of the total number of U.S. income earners just below the cutoff (from \$150,000 to \$200,000) is the same as its share of national income earners whose incomes exceed \$200,000. Very little extrapolation is required in the early years, in which more than 99% of incomes fall below the censoring cutoff. In 2010, more than 95% of income earners

[14] In 1975, the first year with state data available, the price level was roughly 25% of the 2010 level, so this cutoff amounts to roughly \$800,000 of AGI. Put another way, \$200,000 was between the 99.9% and 99.99% income cutoff in the 1975 AGI distribution. In 2010, an AGI of \$200,000 is still well above the 95^{th} income percentile (the cutoff is roughly \$150,000).

still earned less than $200,000.

2.2 Non-Tax Data

2.2.1 Non-tax Data at the State Level

The main outcome for the state analysis is employment growth. Employment is more precisely measured at the state level than GDP and the two are closely related (albeit indirectly) via Okun's law. I use employees on nonfarm payrolls from BLS as my measure of employment (e.g., Fred TXNAN). State unemployment data are also from BLS. Since 1980, state employment has grown 1.66% each year on average as shown in Table 2. Employment growth has increased by as much as almost 10% in Nevada in 1994 and has fallen by roughly 7% in Wyoming in 1983. It is somewhat volatile – one standard deviation in state employment growth is 2.1. When employment growth fell by 7% in Wyoming in 1983, unemployment averaged 17.45 on the year. The state unemployment rate is roughly 5.8% on average.

I use state data from BEA on government transfers and state tax receipts as well as population data from BEA. For government transfers, I use state level current transfer receipts of individuals from the government from BEA Table CA35. I also use personal current taxes, which account for federal, state, and local taxes, from BEA SA50. Government transfers per capita in 2007 averaged $5,500 in 2007 dollars and ranged from $4,500 to $6,500 from the 10^th to the 90^{th} percentile, respectively.

2.2.2 Non-tax Data at the National Level

The aggregate civilian employment data come from the Bureau of Labor Statistics and the other aggregate macroeconomic outcome variables come from the BEA. In particular, real GDP, consumption, investment, and government data are the chain-type quantity indexes from the Bureau of Economic Analysis' National Income and Product Accounts Table 1.1.3; the nominal GDP data come from the National Income and Product Accounts Table 1.1.5.

Table 3 presents the summary statistics of the national dataset. In the postwar period, annual growth in employment averaged 1.5%. Real GDP growth was roughly twice as large on average and varies considerably - a one standard deviation decrease more than offsets a typical year of real GDP growth. Consumption is less volatile in terms of annual growth, although this is not the case for durable goods consumption growth. Investment growth is highly volatile. Average investment growth is 5.1%, but one standard deviation covers a range from -10.9 to

21.1. Residential investment growth has been even more volatile.

I also use macroeconomic data on government transfers and unemployment. For government transfers, I use government social benefits to persons from line 17 of NIPA Table 2.1. Government transfers as a share of output have been increasing over the postwar period as described by Chetty and Finkelstein (2013). The unemployment rate data are from the BLS (Fred UNRATE). I use annual averages of monthly data from this series.

3 Results

This section provides results on the effects of tax changes for different income groups on economic activity. Section 3.1 provides evidence on the two-year effects of tax changes for different groups and across the income distribution. Section 3.2 shows dynamic results and Section 3.3 highlights supplemental national evidence. Section 3.4 discusses the estimates and relates them to existing evidence. Finally, Section 3.5 briefly describes additional support for the validity of the estimates, placebo tests, and robustness tests.

Figure 4 helps fix ideas and preview the results for different income groups. Panel A shows that there is a small to negligible relationship between tax changes for the top 10% and employment growth over a two-year period. While the level of employment in 1984 was substantially higher than it was in 1982 (thus high on the y-axis) and cumulatively sizable tax cuts were given in 1984, 1983, and 1982 (thus to the left on the x-axis), other periods such as the early to mid-1990s had employment growth despite tax increases on taxpayers in the top 10% of AGI. Large tax cuts for high-income taxpayers in the early 2000s were followed by low levels of employment growth. However, this simple plot of the data obscures the true relationship between tax changes for the top 10% and employment growth because tax changes for the top 10% tend to move together with bottom tax changes as shown in Figure 2.

Panel B shows a stronger relationship between employment growth and tax changes for the bottom 90%, particularly after 1950. As mentioned above, since tax changes for the top 10% are often correlated with tax changes for the bottom 90%, the apparent slight relationship between tax changes for the top 10% and output growth could result from tax changes for the bottom 90% that have a stimulative effect and occur at the same time. Thus, one should look at the regression results, which provide estimates for the effects of tax changes for the top 10% while holding tax changes for the bottom 90% constant (see Appendix Figure A4 for a graphical depiction). The same applies for the effects for the bottom 90%.

These national results are suggestive and inherently imprecise, but they help preview the two main results of this paper: (1) the positive relationship between tax cuts and employment growth is largely driven by tax cuts for lower-income groups, and (2) the effects of tax cuts for the top 10% on employment growth is small.

3.1 Two-Year Effects of Tax Changes for Different Income Groups

I begin by presenting non-parametric plots of state employment growth and tax changes for different income groups: the top 10%, the top 50%, the bottom 90%, and the bottom 50% in Figure 5. It plots mean employment growth by bins of residualized tax changes as a share of state GDP for different income groups. The tax changes are residualized in the sense that they are the residual variation in tax changes for the group after controlling for tax changes for the other group. For instance, in Panel A, the residual tax change as a share of GDP for the top 10% shows the relationship between employment growth over a two year period and the portion of tax changes for the top 10% that is orthogonal to tax changes for the bottom 90% in that state. The four panels show that the relationship between tax changes and employment growth over a two-year period is stronger for tax changes for lower income groups. In particular, the bivariate relationship between two-year state employment growth and orthogonalized tax changes for the top 10%, top 50%, the bottom 90%, and the bottom 50% is characterized by the following slopes: -0.1, -0.4, -5.1, and -9.5, respectively. A 1% of state GDP tax change for the bottom 90% reduces the level of state employment by 5.1% cumulatively over a two-year period.

Table 4 shows that these effects across groups are statistically different and not confounded by changes in progressive spending. Column (1) shows that a 1% of GDP increase in taxes in a given state reduces employment growth over a two-year period in that state by 2.6 percentage points when the tax increase is paid for by taxpayers in the bottom 90% and by 0.2 percentage points when the tax increase is paid for by taxpayers in the top 10% on average.[15] The bottom of the table shows that these effects are statistically different. We can reject the test that the effects are the same; the estimated difference is roughly 2.4 percentage points. Column (2) shows that these estimates are very similar when controlling for changes in per capita government transfers.

[15] These effects are slightly lower than the binned scatter plots because they control for lagged state employment growth.

Columns (3)-(4) and (5)-(6) provide analogous results for different income groupings.

As shown in equation 3, we can use these two year changes to characterize the effects across the income distribution more flexibly. Figure 6 shows the results of the second order approximation of the reduced-form effect of tax changes by income decile. It shows that the largest effects of tax cuts in absolute value come from the lowest income deciles and that the effects of tax cuts for the top income decile are small. The point estimates are noisy for the lowest income decile, but the $\hat{\theta}$'s are precise enough to indicate that low-to-moderate income earners, i.e., deciles two through six, have statistically and economically larger effects relative to the top decile. The particular estimates are $\hat{\theta}_0 = -10.3(6.09)$, $\hat{\theta}_1 = 1.4(1.97)$ and $\hat{\theta}_2 = -0.4(0.14)$. The difference between the implied effect from the $\hat{\beta}(g)$ function and the implied effect for the top decile, i.e., $\hat{\beta}(10)$, is -7.2** (2.9), -6.07***(1.9), -4.9***(1.4), -3.9***(1.4), and -2.9**(1.5), for the second through sixth deciles, respectively.[16]

3.2 Dynamic Effects of Tax Changes for Different Income Groups

These estimates hinge on the validity of the identifying assumption that lightly treated states provide a valid counterfactual for the heavily treated states. This section presents evidence to support this assumption and analyze the dynamic effects of tax changes for different groups on economic activity.

Figure 7 shows the evolution of mean log employment in a seven-year window around a tax change event. It shows that the years preceding tax shocks for the top 10% and the bottom 90% are similar and not abnormally good or bad. There is a sharp decline in log employment when taxes change for the bottom 90%, but no detectable effect following tax changes for the top 10%. On average, employment falls by about 5% and is rises slightly to roughly 3% four years after. This graph provides no evidence that tax changes for high-income earners materially impact economic activity over a business cycle frequently.[17] Appendix Figure A5 compares these effects with those for the bottom 50% and shows that the effects for the bottom half of the income distribution are roughly twice as large, and also show no detectable pretends.

Table 5 and Table 6 provide the point estimates for each year of the seven-year window for two sets of results: a specification with the bottom 90% (controlling for changes for the top

[16]Note that Columns (5)-(6) of Table 4 present less parametric evidence on the impacts of tax changes for the bottom 30, middle 40, and top 30 that is consistent with the results from the second order approximation.

[17]While it is possible that the effects show up further into the future, detecting such effects is inherently difficult. See Romer and Romer (2012) for some historical evidence on longer-term effects.

10%) and another with the bottom 50% (controlling for changes for the top 50%). Column (1) of Table 5 presents results shown in the Figure 7, which come from a specification with state- and year-fixed effects and a lag of log state employment. Column (2) shows similar results that control for government transfers as a share of GDP in the state. Columns (3)-(4) provide different specifications to address the concern that lagged employment is not a sufficient statistic for pre-existing trends. In particular, Column (3) shows similar results when flexibly allowing for year and state trends by including a state $\times$ year fixed effect. Column (4) combines the controls in the prior two specifications.

Figure 8 illustrates these results graphically. The results are remarkably stable across these specifications, suggesting that prior economic conditions, differential trends, and targeted spending are not driving the results. Importantly, including these trends effectively partials out some of the government transfer variable, resulting in an orthogonal portion of government transfers that is commonly used as an "innovation" or "shock" to government transfer spending in the VAR literature. Controlling for these "innovations" is an imperfect way to make sure that changes in the composition of government spending is not confounding my estimates.

The bottom portions of Table 5 and Table 6 present results for differences in the effects for different time horizons h in this seven-year window. They both show that the patterns described above are statistically and economically different across low and high income groups.

3.3 National Effects of Tax Changes for Different Income Groups

Table 7 provides the main national results for employment growth. Under the exogeneity assumption of Romer and Romer (2010), the specification in column 1, which does not include any controls or lags of the dependent variable, is a simply moving average representation that will produce valid point estimates and standard errors after the latter are corrected for serial correlation. It shows that exogenous income and payroll tax increases on the bottom 90% depress annual employment more strongly than increases on the top 10%. The point estimates, which are statistically significant only for the bottom 90%, show that employment falls by roughly 5 percentage points cumulatively following a 1% of GDP tax increase for the bottom 90%. The corresponding point estimate for the top 10% which is smaller in magnitude actually has the wrong sign but is statistically no different than zero. Note that a 1% of GDP tax change is much larger than the size of the historical average tax change for either group. Column 2 shows that including a few lags in an autoregressive specification does not change the estimates very much.

To ensure that progressive spending policies, which may tend to occur in periods of progressive tax changes, are not driving the results, column 3 includes different controls for government spending. Column 3 controls for changes in government transfers as a share of output. Ideally, as mentioned previously, I could include only exogenous innovations in the government transfers series, but no comparable narrative record of government transfers or spending shocks by income group is easily available. However, these ideal innovations are arguably pretty close to what is actually included based on a Frisch and Waugh (1933) interpretation. Since the specifications in column 3 also include lagged employment growth terms, the inclusion of the government spending and expenditure variables uses variation from the portion of government spending and expenditure that is orthogonal to prior employment growth (in column 3). Interestingly, these results correspond with the baseline estimates from the Romer and Romer estimates of 1.4 from Table A2. One can think of their estimate as a weighted average between 2.3 and 0, roughly suggesting that the stimulative effects of tax cuts on employment largely result from tax cuts for the bottom 90%.

Figure A7, which shows national results for consumption and investment growth, helps reveal the mechanisms through which tax changes for different groups have different effects.[18] Consumption appears to decrease more following a 1% of GDP tax increase on the bottom 90% compared to an equivalently sized tax increase for the top 10%, but estimates are noisy, so the point estimates for the bottom 90% do not fall substantially below the confidence interval for the top group. Since consumption is less volatile than output growth, and since it comprises a substantial portion of output growth, small differences in consumption impacts can have large macroeconomic consequences. The graph in the first row and second column shows a stronger relative response in durable consumption growth following a tax increase on the bottom 90%, which is consistent with other literature on how people respond to tax rebates (Parker et al., 2013). The investment results show a similar pattern (as do the residential investment growth results in the third row). Together, these graphs seem consistent with the idea that some lower- and middle-income households use a portion of their tax cuts to make larger purchases, which can boost output and employment. Overall, this figure provides imprecise but suggestive evidence that differential consumption and investment responses are a key part of the story regarding why a dollar of tax cuts for the bottom 90% leads to more economic activity than a dollar of tax cuts for the top 10%.

[18] The impulse responses are based on the moving average specification without controls.

3.4 Discussion of Results

The results in sections 3.1, 3.2, and 3.3 show large employment following tax changes affecting lower-income taxpayers. This section provides an interpretation for these results and relates them to existing evidence.

The employment results are reduced-form estimates that reflect changes in both the supply and demand for labor following a tax change. The consumption results, particularly for durable consumption, as well as the MPC-likeness of Figure 6, suggest a substantial role for differential changes in demand across the income distribution. However, differential labor supply responses across the income distribution could also contribute to the observed reduced-form impacts. To provide evidence on supply responses, I construct analogous results to Figures 5 and 6 with labor force participation as the outcome. The non-parametric results for labor force participation in Appendix Figure A8 closely resemble the corresponding results for employment in Figure 5.[19] Interestingly, the effects for labor force participation are more concentrated among those in the bottom half of the income distribution than those for employment (see in Appendix Figure A9 and Figure 6). Nevertheless, labor force participation decisions are equilibrium outcomes that also reflect both supply and demand forces, so these supplemental results do not reveal the relative importance of supply and demand changes.[20]

Quantitatively, the reduced-form results in this paper are large, but within a range that is consistent with existing evidence. Aaronson et al. (2012) show that household spending increases by roughly \$700 per quarter following a \$250 per quarter income increase due to minimum wage increases. This $\frac{700}{250} \approx 3X$ impact on spending among low-income earners comes from a small number of households that make large durable purchases following the income shock. Similar spending behavior following tax shocks for lower-income earners could generate sizable impacts on economic activity. The modest average size of the tax changes for low-income earners (as shown in Figure 2) could actually contribute to a larger impact on spending (Kaplan and Violante, 2014a).[21] Overall, differential impacts on the product market likely result in differential impacts on the labor market.

In terms of the impacts on employment, my estimates are within a range that is consistent

[19] Results for employment-to-population ratios are also provided in Appendix Figure A10 and A11.

[20] Decomposing these forces requires a more structural approach that is beyond the scope of this paper, but is an interesting area for future research.

[21] Kaplan and Violante (2014b) point out that if a tax cut for hand-to-mouth agents were sufficiently large, it could materially loosen liquidity constraints and result in some households deciding to save some of it.

with existing cross-sectional evidence surveyed by Ramey (2011). In particular, the 5% estimate for the increase in state employment from a 1% of GDP tax cut for the bottom 90% translates to roughly $21,500 per job.[22] My estimates for the impact of tax cuts for the top 10% on employment are statistically and economically indistinguishable from zero, so the corresponding cost-per-job estimate is much higher. Therefore, given my estimates by income group, the overall impact of a tax cut of 1% of GDP that goes half to the bottom 90% and half to the top 10% will have roughly a $40,000 cost-per-job. These cost-per-job estimates are consistent with those reported in Ramey (2011): $25,000 in Wilson (2011), roughly $28,600 in Chodorow-Reich et al. (2012), $30,000 in Suárez Serrato and Wingender (2011), and $35,000 in Shoag (2010).[23]

3.5 Threats to Validity, Placebo Tests, and Robustness

This section briefly discusses supplemental evidence that supports the assumptions underlying my results. It then briefly describes placebo and robustness tests.

There are three key threats to the validity of the estimates: endogenous tax changes, concomitant progressive government spending changes, and prior economic conditions and differential trends. First, I assess the concern that the composition of tax shocks may be endogenous by appealing to an orthogonality test used by Favero and Giavazzi (2012). This test compares the federal tax change series before and after partialling out macro aggregates. Appendix Figure A2 shows that the raw tax shock series and the orthogonalized tax shock series are very similar for each income group, supporting the compositional exogeneity assumption.[24] Second, in terms of progressive government spending, I supplement the main control function results by presenting results for two types of states - those with above- and below-median per capita transfer payments. My preferred measure of government transfers adjusts for economic conditions by partialling out state GDP and employment rates from per capita government transfers.[25] The

[22] Using 2011 numbers, the cost of a 1% of GDP tax cut is roughly $150 billion and a 5% increase in employment on a base of 140 million is 7 million. Therefore, the cost-per-job is $\frac{\$150{,}000M}{7M} = \$21,429$.

[23] Note that Wilson (2011) and Chodorow-Reich et al. (2012) focus on effects during a recession, which likely results in lower cost-per-job estimates. Figure 9 shows how my estimates differ in periods with below- and above-median state unemployment.

[24] More generally, tax changes could be endogenous by income group, year, and state. I address concerns with respect to the timing and location of tax changes by using only tax changes Romer and Romer (2010) classify as exogenous and by exploiting regional variation in the income distribution.

[25] I first regress log per capita government transfers on (a) log state GDP in year t, $t-1$, and year $t-2$ as well as (b) the state unemployment rate in year t, $t-1$, and year $t-2$. I then take the residual per capita government transfer measure as the object of interest and generate an annual series of the median value of residual per capita government transfers. Finally, I define an state-year indicator for whether or not the state is above or below the median that year and split the sample on that basis.

goal of this adjustment is to focus on states with generous per capita transfers that are not due to cyclical economic conditions, i.e., some states have high transfer support even in good times. Appendix Figure A6 shows that both low- and high-transfer states have economically and statistically significant relationships between tax changes for the bottom 90% and employment growth. Importantly, the point estimate for the states with lower progressive government spending, i.e., those that are below the median in adjusted per capita government transfers, is higher than the point estimate for the high progressive spending states. This split-sample result indicates that the results do not rely on estimates from high-transfer states. Moreover, this split-sample result provides evidence that unobserved progressive spending changes are not driving my main results. Third, I use a placebo test to supplement the event study and regression evidence on robustness to prior economic conditions and differential trends. The placebo test replaces the actual employment outcomes that start in 1980 with each states's employment starting in 1970 and then reruns the main state specification to show that nothing mechanical drives the results. Appendix Table A1 shows that shifting the outcomes results in insignificant estimates, indicating that the nature of the exercise does not generate spurious results.

To address potential concerns about serial correlation (Bertrand et al., 2004), I implement a permutation test similar to the one run by Chetty et al. (2009). I first randomly select a "placebo triplet" that consists of a state-year-income group tax shock.[26] I then re-estimate the baseline specification using 500 different random number generator seeds and plot the point estimate $\hat{\beta}^{B90}$ and the bottom-top difference in point estimate $\hat{\beta}^{B90} - \hat{\beta}^{T10}$ for each of the placebos. The results in Figure 10 show that the actual point estimates from Column (1) of Table 4, which are depicted by the red line, exceed the vast majority of placebo estimates that result from randomly assigning another state's tax shocks (for both the top and bottom income group) to another state within that year. Panel A shows that 20 of 500 placeable estimates or 4% exceed my actual estimate. Similarly, Panel B shows that 26 out of 500, or 5.2% of the estimated difference between the bottom 90% and top 10% coefficients exceed my actual estimate from Column (1) of Table 4.

Finally, I also run robustness checks for various concerns regarding anticipation effects, al-

[26] The randomly selected shock is from another state in that year. Note that the units of the tax shocks are shares of state GDP, which make them interchangeable. In addition, there are a few states that are missing tax shocks due to their small size and the sample size limitations within TAXSIM data. In the few cases in which the placebo triplet selects a missing value, I replace the shock with a draw from a normal distribution with the same mean and standard deviation as the actual shocks in that year.

ternate ranking schemes based on family income, and temporary versus permanent tax changes. Mertens and Ravn (2012) favor using only unanticipated tax changes, but there is some disagreement about whether or not this distinction matters for the size of estimated multipliers (Perotti, 2012). Using the Mertens and Ravn (2012) classification of unanticipated tax changes, I show results in Appendix Table A3 using only unanticipated tax changes. The point estimates, which are based on a smaller number of unanticipated tax changes, are broadly consistent with the results for all exogenous tax changes.[27] Results are also quite similar if tax units are ranked not by income but by income adjusted for differences in household size, i.e., $\frac{AGI}{\sqrt{1+exemptions}}$, which is an adjustment the CBO has used.[28] Finally, regarding the concern that temporary and permanent tax changes may have different effects, the vast majority of these tax changes are classified as permanent by Mertens and Ravn (2012).[29]

4 Conclusion

This paper quantifies the importance of the distribution of tax changes for their overall impact on economic activity. The results are important for characterizing central equity-efficiency tradeoffs in tax policy, for informing structural models with heterogeneous agents, and for predicting the consequences of redistributive policies such as tax increases on high-income earners.

I construct a new data series of tax changes by income group from tax return data. I use this series and variation from the income distribution across states and federal tax shocks to estimate the effects of tax changes for different groups. I show substantial heterogeneity in the effects of fiscal policy. In particular, I find that the stimulative effects of income tax cuts are largely driven by tax cuts for the bottom 90% and that the empirical link between employment growth and tax changes for upper-income earners is weak to negligible over a business cycle frequency. These effects hold at both the state and federal levels, and are not confounded by changes in progressive spending, state trends, or prior economic conditions. The effects are larger in states with high unemployment rates and seem to come from increased durable consumption, investment, and labor force participation.

Extending the analysis to study medium- and longer-term effects of tax changes, such as new

[27] At the national level, the effects are slightly amplified - there are large negative effects from tax changes for the bottom 90% and effects that are slightly larger in the other direction for the top 10%.

[28] These results are not reported, but are available upon request.

[29] The main exception is the Jobs and Growth Tax Relief Reconciliation Act of 2003, which is still almost entirely in effect ten years later. Therefore, there is little room for distinction as most of these tax changes are considered permanent.

firm creation or patent activity, is a good topic for future research. Finally, the estimates in this paper come from modest changes in tax rates that have been executed in the post-war period; using these estimates to evaluate the likely impacts of substantial tax changes on high-income earners requires extrapolation beyond the observed variation in the data.

References

Aaronson, Daniel, Sumit Agarwal, and Eric French, "The Spending and Debt Response to Minimum Wage Hikes," *American Economic Review*, 2012, *102* (7), 3111–39.

Auerbach, Alan and Jonathan Siegel, "Capital-Gains Realizations of the Rich and Sophisticated Program," *American Economic Review*, 2000, *90* (2), 276–282.

_ and Yuriy Gorodnichenko, *Measuring the Output Responses to Fiscal Policy*, University of Chicago Press, 2013.

Barrow, Robert J. and Charles J. Redlick, "Macroeconomic Effects from Government Purchases and Taxes," *The Quarterly Journal of Economics*, 2011, *126*, 51–102.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-In-Differences Estimates?," *The Quarterly Journal of Economics*, 2004, *119* (1), 249–275.

Chetty, Raj, Adam Looney, and Kory Kroft, "Salience and Taxation: Theory and Evidence," *American Economic Review*, September 2009, *99* (4), 1145–77.

_ and Amy Finkelstein, *Handbook of Public Economics*, Amsterdam: North-Holland, 2013.

_ , John N. Friedman, Søren Leth-Petersen, Torben Heien Nielsen, and Tore Olsen, "Active vs. Passive Decisions and Crowd-Out in Retirement Savings Accounts: Evidence from Denmark," *Quarterly Journal of Economics*, forthcoming.

Chodorow-Reich, Gabriel, Laura Feiveson, Zachary Liscow, and Gui Woolston, "Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act," *American Economic Journal: Applied Economics*, 2012, *4* (3), 118–145.

Clemens, Jeffrey and Stephen Miran, "Fiscal Policy Multipliers on Subnational Government Spending," *American Economic Journal: Economic Policy*, September 2012, *4* (2), 46–68.

Dynan, Karen, Jonathan Skinner, and Stephen Zeldes, "Do the Rich Save More?," *Journal of Political Economy*, 2001, *112* (2), 397–444.

Farhi, Emmanuel and Ivan Werning, "Fiscal Multipliers: Liquidity Traps and Currency Unions Fiscal Multipliers: Liquidity Traps and Currency Unions Fiscal Multipliers: Liquidity Traps and Currency Unions," Technical Report, Harvard and MIT October 2013.

Favero, Carlo and Francesco Giavazzi, "Measuring Tax Multipliers. The Narrative Method in Fiscal VARs.," *American Economic Journal: Economic Policy*, 2012, *4* (2), 69–94.

Frisch, Ragnar and Frederick V. Waugh, "Partial Time Regressions as Compared with Individual Trends," *Econometrica*, 1933, *1* (4), 387–401.

Gali, Jordi, J. David Lopez-Salido, and Javier Valles, "Understanding the Effects of Government Spending on Consumption," *Journal of the European Economic Association*, 2007, *5* (1), 227–270.

Goolsbee, Austan, "What Happens When You Tax the Rich? Evidence from Executive Compensation," *Journal of Political Economy*, 2000, *108* (2), 352–378.

Hausman, Josh, "Fiscal Policy and Economic Recovery: The Case of the 1936 Veterans' Bonus," Technical Report, University of Michigan 2013.

Heathcote, Jonathan, "Fiscal Policy with Heterogenous Agents and Incomplete Markets," *Review of Economic Studies*, 2005, *72*, 161–188.

Jappelli, Tullio and Luigi Pistaferri, "The Consumption Response to Income Changes," *American Review of Economics*, 2010, *2*, 479–506.

Jorda, Oscar, "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 2005, *95* (1), 161–182.

Kaplan, Greg and Giovanni L. Violante, "A Model of the Consumption Response to Fiscal Stimulus Payments," *Econometrica*, 2014, *82* (4), 1199–1239.

_ **and** _ , "A Tale of Two Stimulus Payments: 2001 versus 2008," *American Economic Review*, 2014, *104* (5), 116–21.

McCarthy, Jonathan, "Imperfect Insurance and Differing Propensities to Consume Across Households," *Journal of Monetary Economics*, 1995, *36* (2), 301–327.

Mertens, Karel, "Marginal Tax Rates and Income: New Time Series Evidence," *NBER working paper 19171*, June 2013.

_ **and Morten O. Ravn**, "Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated U.S. Tax Policy Shocks," *American Economic Journal: Economic Policy*, 2012, *4* (2), 145–181.

_ **and** _ , "The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States," *American Economic Review*, 2013, *103* (4), 1212–47.

_ **and** _ , "A reconciliation of SVAR and narrative estimates of tax multipliers," *Journal of Monetary Economics*, 2013.

Monacelli, Tommaso and Roberto Perotti, "Tax Cuts, Redistribution, and Borrowing Constraints," Technical Report, Bocconi University May 2011.

Nakamura, Emi and Jon Steinsson, "Fiscal Stimulus in a Monetary Union: Evidence from US Regions," *American Economic Review*, 2014, *104* (3), 753–92.

Parker, Jonathan, "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes," *American Economic Review*, 1999, *89*, 959–73.

Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson, and Robert McClelland, "Consumer Spending and the Economic Stimulus Payments of 2008," *American Economic Review*, 2013, *103* (6), 2530–53.

Perotti, Roberto, "The Effects of Tax Shocks on Output: Not So Large, but Not Small Either," *American Economic Journal: Economic Policy*, 2012, *4* (2), 214–37.

Ramey, Valerie A., "Can Government Purchases Stimulate the Economy?," *Journal of Economic Literature*, 2011, *49* (3), 673–85.

Romer, Christina and David Romer, "The Incentive Effects of Marginal Tax Rates: Evidence from the Interwar Era," Technical Report, U.C. Berkeley 2012.

Romer, Christina D. and David H. Romer, "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks," *American Economic Review*, 2010, *100* (3), 763–801.

Saez, Emmanuel and Gabriel Zucman, "Wealth Inequality in the United States Since 1913: Evidence from Capitalized Income Tax Data," Technical Report 20625, National Bureau of Economic Research October 2014.

_ , Joel Slemrod, and Seth H. Giertz, "The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review," *Journal of Economic Literature*, 2012, *50* (1), 3–50.

Shapiro, Matthew D. and Joel Slemrod, "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding," *American Economic Review*, March 1995, *85* (1), 274–283.

Shoag, Daniel, "The Impact of Government Spending Shocks: Evidence on the Multiplier from State Pension Plan Returns," Working Paper, Harvard University 2010.

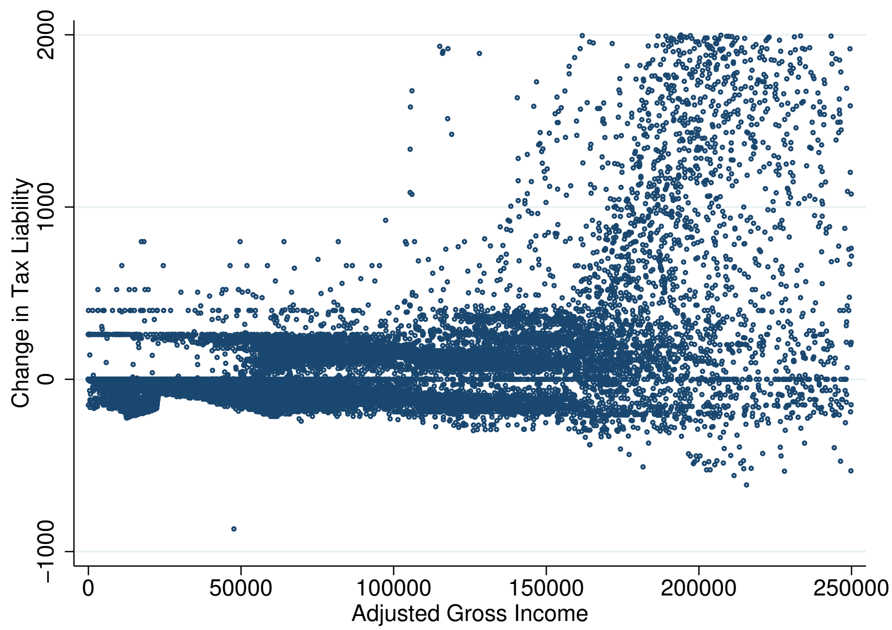
Stock, James and Mark Watson, "Why has U.S. Inflation Become Harder to Forcast?," *Journal of Money, Banking and Credit*, 2007, *39* (1), 3–33.

Suárez Serrato, Juan Carlos and Philippe Wingender, "Estimating Local Fiscal Multipliers," Working Paper, U.C. Berkeley 2011.

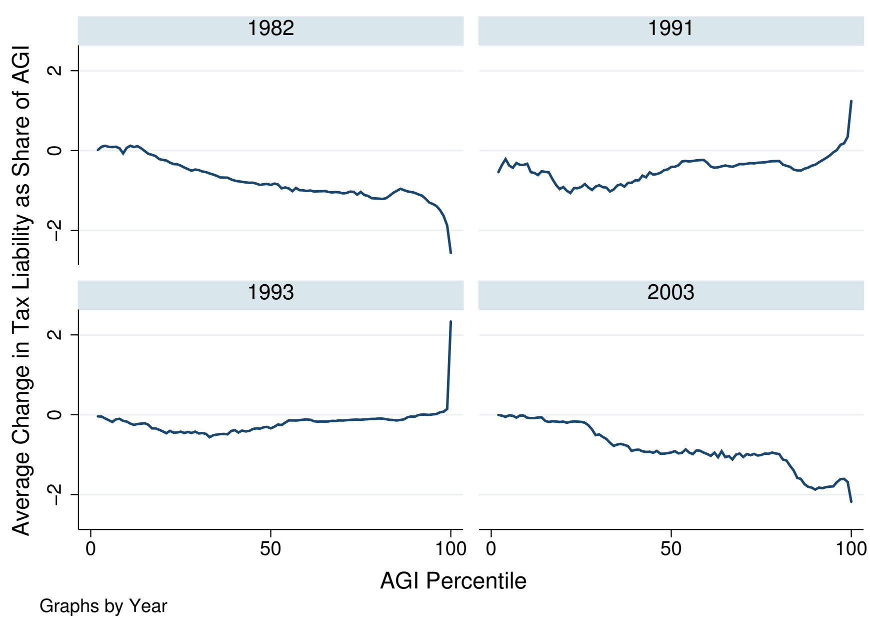
Wilson, Daniel J., "Fiscal Spending Jobs Multipliers: Evidence from the 2009 American Recovery and Reinvestment Act," Working Paper 2010-17, Federal Reserve Bank of San Francisco 2011.

Figure 1: Constructing A Measure of Tax Changes by Income Group

A. Tax Change Calculation for Each Tax Return: 1993 Example

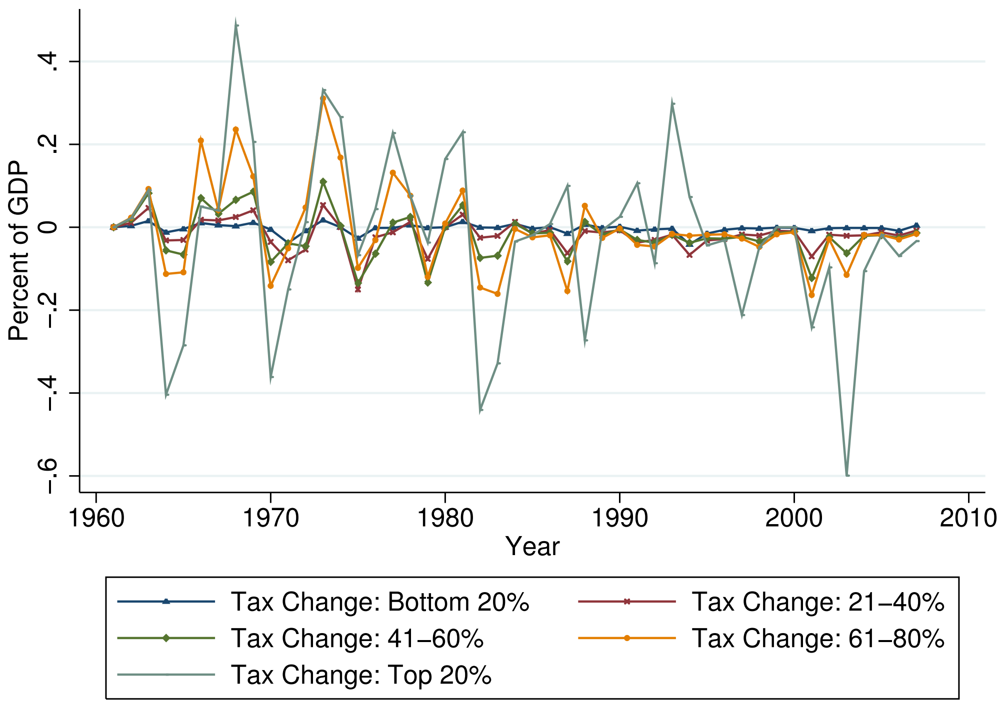


B. Selected Historical Tax Changes for Each AGI Percentile



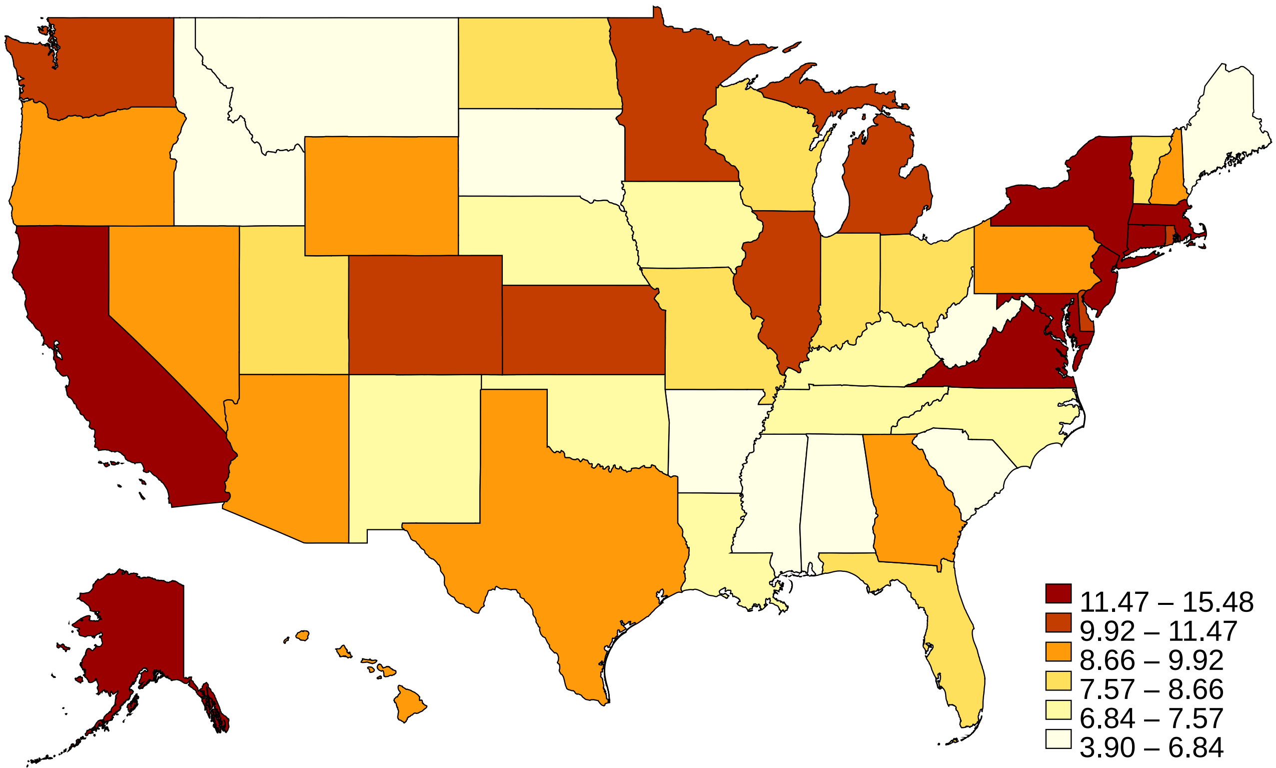
Notes: This figure displays the mechanical change in income and payroll tax liability for each tax return in TAXSIM from tax schedule changes in 1993 by AGI in Panel A and a summary version in Panel B, which shows mean tax changes as a share of AGI for every income group, for 1993 and for three other prominent years. For display purposes, Panel A shows results for tax changes for $0 < AGI < 250K$ and $|\Delta Tax| < 2,000$. Panel B does not show results for the smallest AGI percentile (since the smallest income group result is amplified by a small denominator).

Figure 2: Federal Income and Payroll Tax Changes by AGI Quintile since 1960



Notes: This figure displays changes in individual income and payroll tax liabilities by income quintile as a share of GDP from 1960 to 2007. Tax returns from TAXSIM are available from 1960-2007 and are used to construct a tax change measure. The period from 2008-2011 has no exogenous tax changes so those years are coded as zero exogenous change for each AGI quintile throughout the paper. Both exogenous and endogenous tax changes are shown in the figure.

Figure 3: Share of Taxpayers in Top 10% Nationally

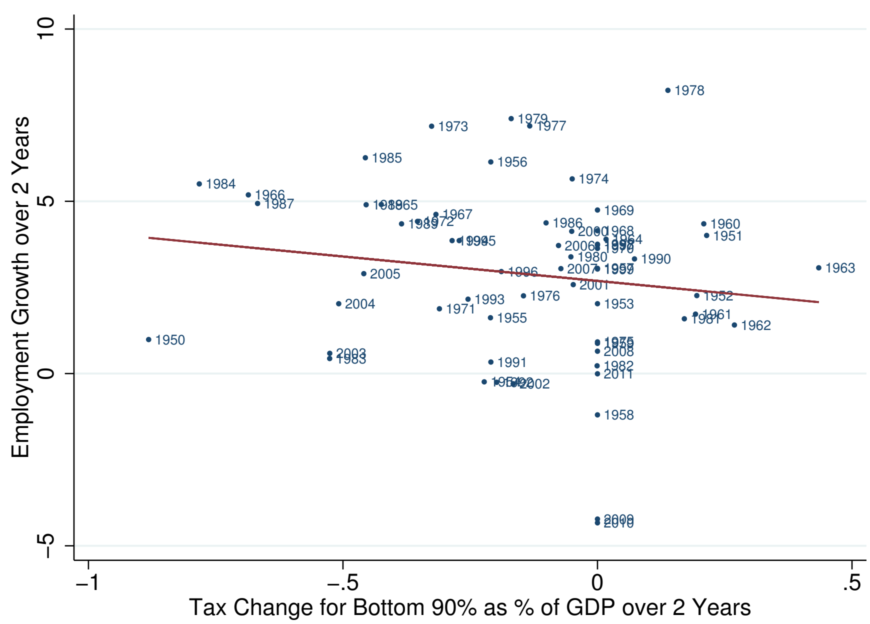


Notes: This figure shows that there is substantial geographic variation in the location of households in the top income decile. For instance, 12.4% percent of households filing from Virginia are in the top 10% of AGI nationally on average from 1980-2007. The data plotted are the average shares of households filing from a given state for the years 1980-2007 who are in the top 10% nationally in that year.

Figure 4: National Employment Growth and Federal Income Tax Changes

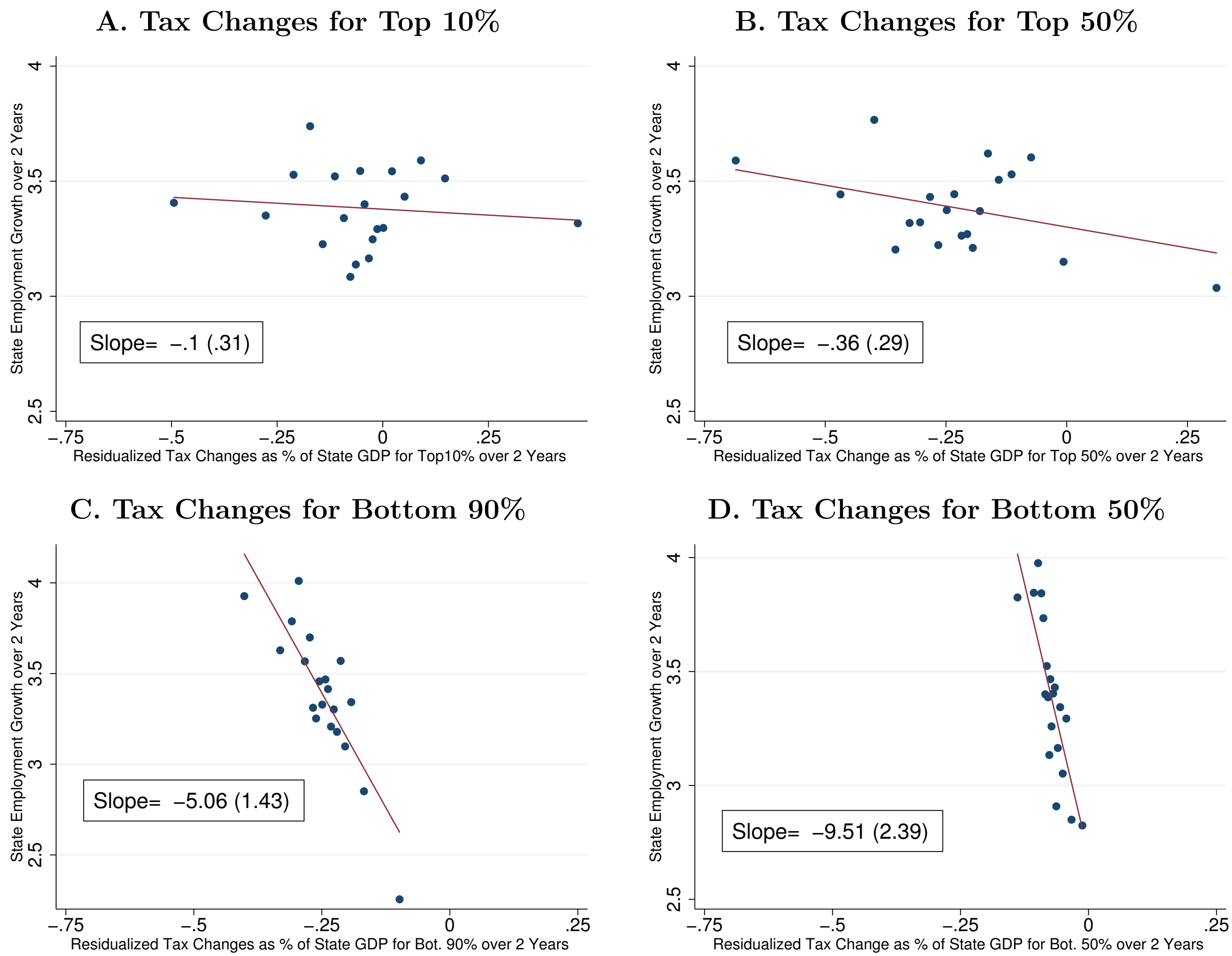
A. Tax Changes for Top 10%

B. Tax Changes for Bottom 90%



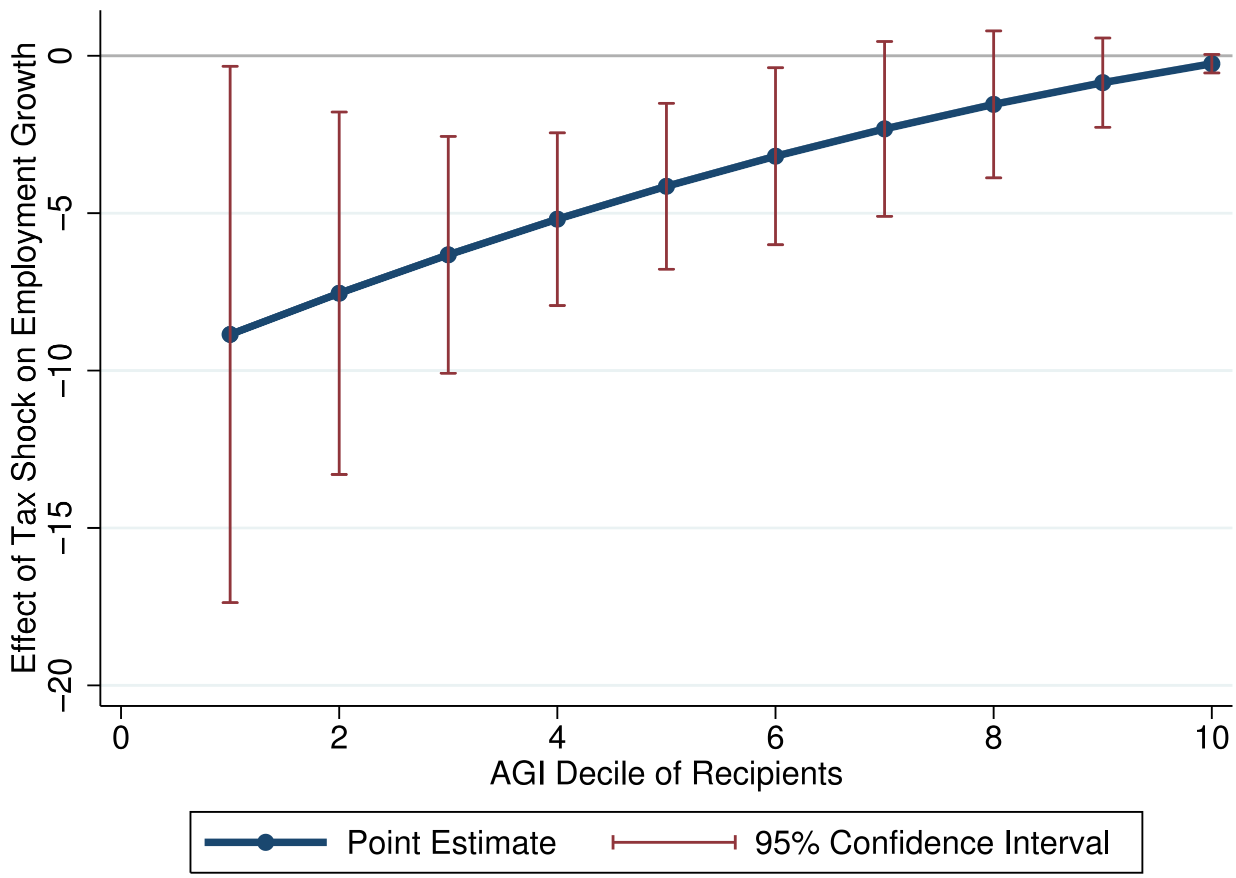
Notes: This figure plots two-year employment growth and the sum of income and payroll tax changes as a share of GDP during the prior two years for those with AGI in the top 10% in Panel A and for those with AGI in the bottom 90% in Panel B. The figure also plots the predicted value of two-year employment growth from a simple bivariate regression. Only tax changes that Romer and Romer (2010) classify as exogenous are considered non-zero tax changes. Following Romer and Romer (2010), the data start in 1950. See Appendix Figure A4 for a version that partials out the tax change for the other group and shows a starker difference between the effects.

Figure 5: State Employment Growth and Federal Income Tax Shocks by Income Group



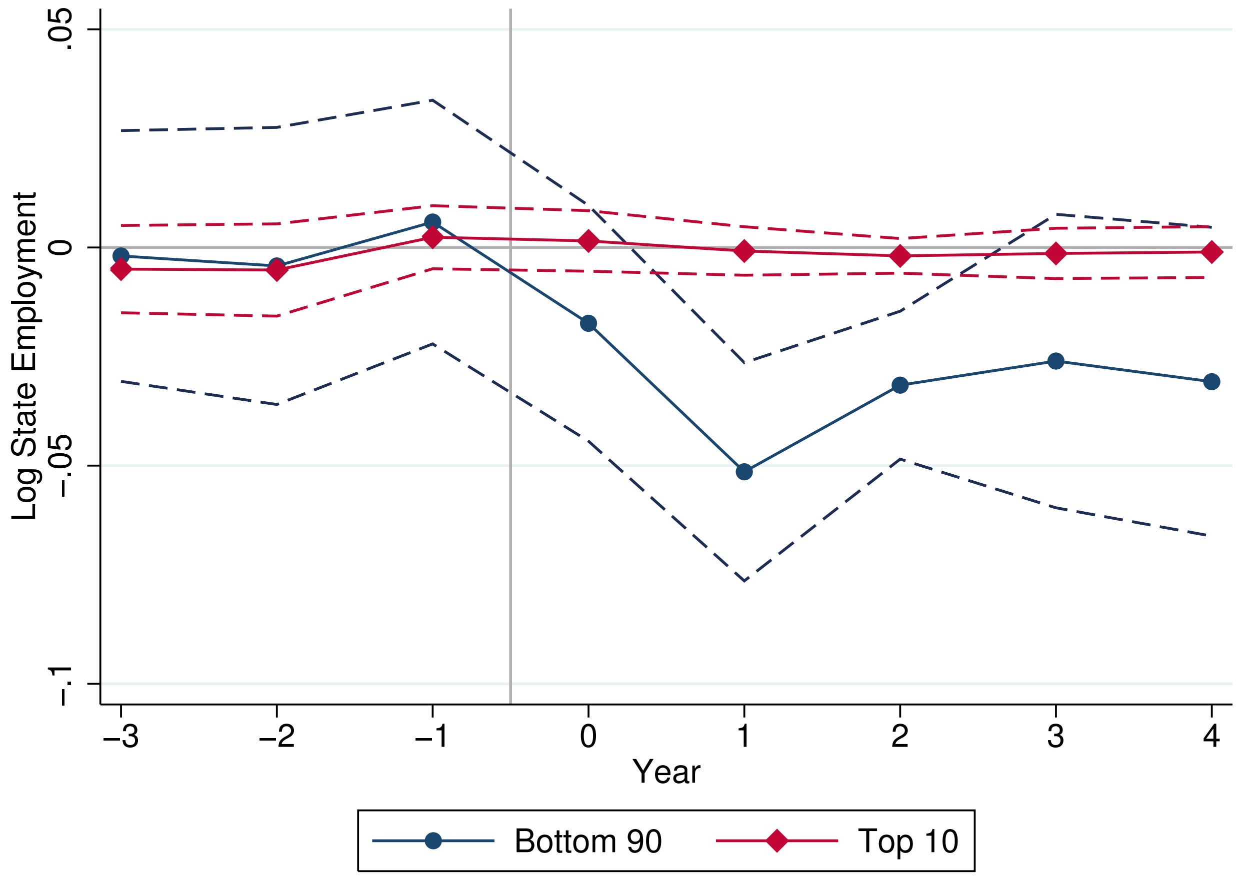
Notes: This figure plots mean state employment growth by bins of orthogonalized tax changes as a share of GDP during the prior two years for those with AGI in the top 10% nationally in Panel A and for those with AGI in the bottom 90% nationally in Panel B. The tax changes are orthogonalized by controlling for tax changes for the other group as well as state- and year-fixed effects. The figure also plots the predicted value of state employment growth from a simple bivariate regression. Slopes and robust standard errors clustered by state are reported. Only tax changes that Romer and Romer (2010) classify as exogenous are considered non-zero tax changes. Due to availability of state identifiers in TAXSIM, the data start in 1980.

Figure 6: Aggregate Effects of Individual Tax Changes Across the Income Distribution



Notes: This graph shows how the effect on state employment growth of a 1% of GDP increase in taxes varies by the AGI decile of the taxpayer who pays for it. In particular, it shows $\hat{\theta}_0 + \hat{\theta}_1 g + \hat{\theta}_2 g^2$, which is the estimated second order approximation of the $\beta(g)$ function that maps an income decile into the estimated effect on annual state employment growth from an exogenous income and payroll tax change over the prior two years for that decile. The point estimates are noisy for the lowest income decile, but the $\hat{\theta}$'s are precise enough to indicate that low-to-moderate income earners, i.e., deciles two through six, have statistically and economically larger effects relative to the top decile. The particular estimates are $\hat{\theta}_0 = -10.3(6.09)$, $\hat{\theta}_1 = 1.4(1.97)$, and $\hat{\theta}_2 = -0.4(0.14)$. The difference between the implied effect from the $\hat{\beta}(g)$ function and the implied effect for the top decile, i.e., $\hat{\beta}(10)$, is -7.2** (2.9), -6.07***(1.9), -4.9***(1.4), -3.9***(1.4), and -2.9**(1.5), for the second through sixth deciles, respectively. Standard errors are robust and clustered by state. Note that Columns (5)-(6) of Table 4 present less parametric evidence on the impacts of tax changes for the bottom 30, middle 40, and top 30 that is consistent with the results from the second order approximation

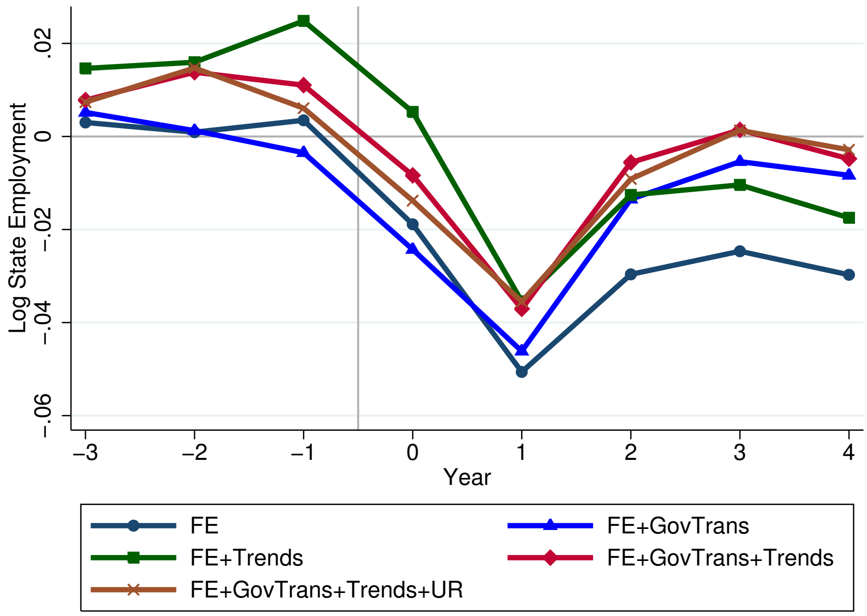
Figure 7: Event Study of Federal Tax Shock on State Employment by Income Group



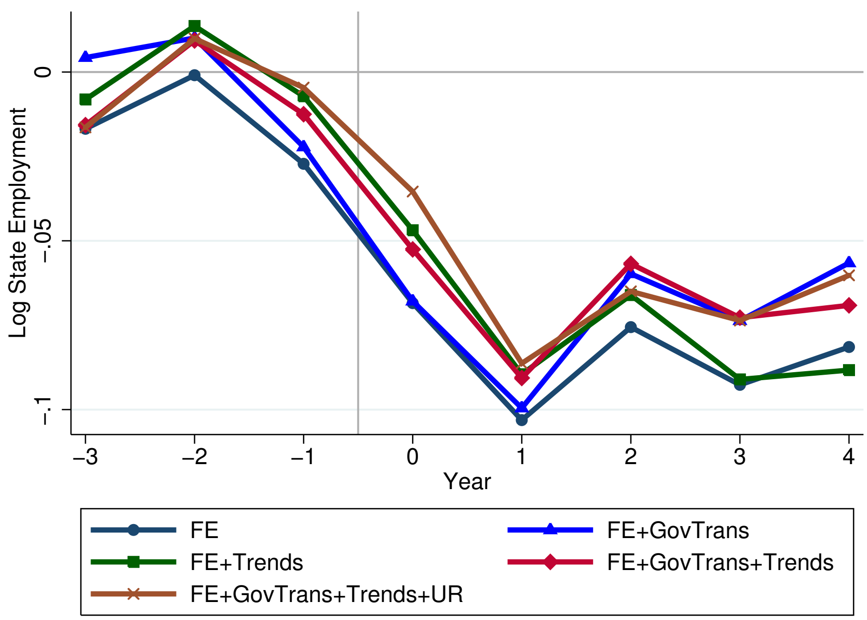
Notes: This figure shows an event study of a 1% of GDP tax increase on state employment for those with AGI in the bottom 90% nationally in blue and for those with AGI in the top 10% nationally in red. See Section 3.2 for more details and Table 5 for point estimates for the bottom 90% group. Due to availability of state identifiers in TAXSIM, the data start in 1980. Standard errors are robust and clustered by state and 95% confidence intervals are shown.

Figure 8: Event Study of Federal Tax Shock on State Employment Growth by Specification

A. Effect of Tax Shock for Bottom 90%



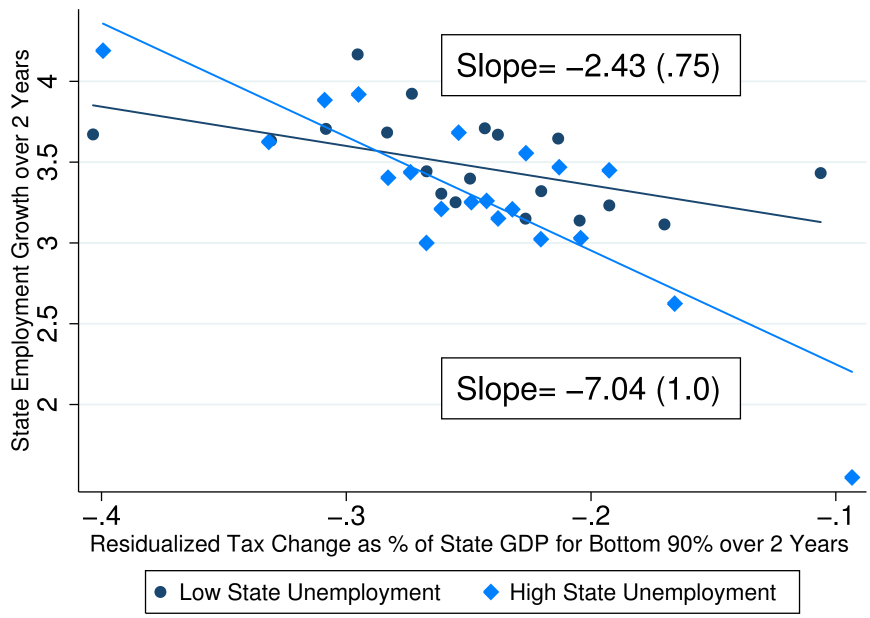
B. Effect of Tax Shock for Bottom 50%



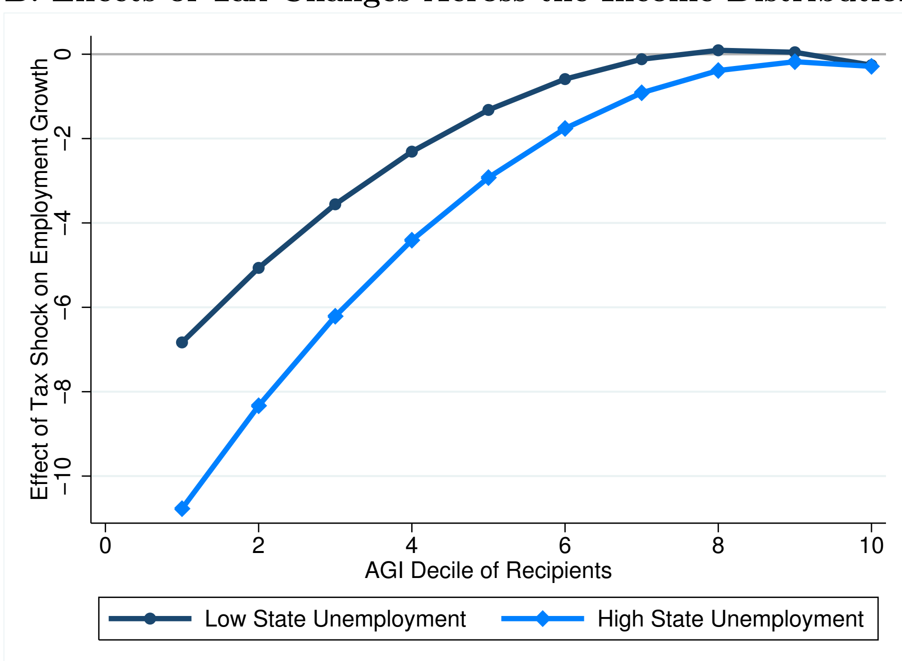
Notes: This figure shows point estimates of a tax change of 1% of GDP in year t on log state employment in year $t+h$. Panel A shows the effects of tax changes for taxpayers in the bottom 90% of national AGI (controlling for tax changes for the top 10%) and Panel B shows the effects from a separate set of specifications for the bottom 50% (controlling for tax changes for the top 50%). Each line represents different specifications that correspond with the estimates in Table 5 and Table 6. In brief, FE means state- and year-fixed effects, trends means state-$\times$ year-fixed effects, GovTrans means controlling for government transfers as a share of state GDP in year t, and UR means controlling for the state unemployment rate in year t. See section 3.2 for more detail. Note that Appendix Figure A5 shows standard errors for the main specification for both Panel A and B and that the confidence interval for $h = -1$ includes zero for both.

Figure 9: Effects of Tax Shocks on State Employment Growth by State Unemployment Rate

A. Effect of Tax Changes for Bottom 90%



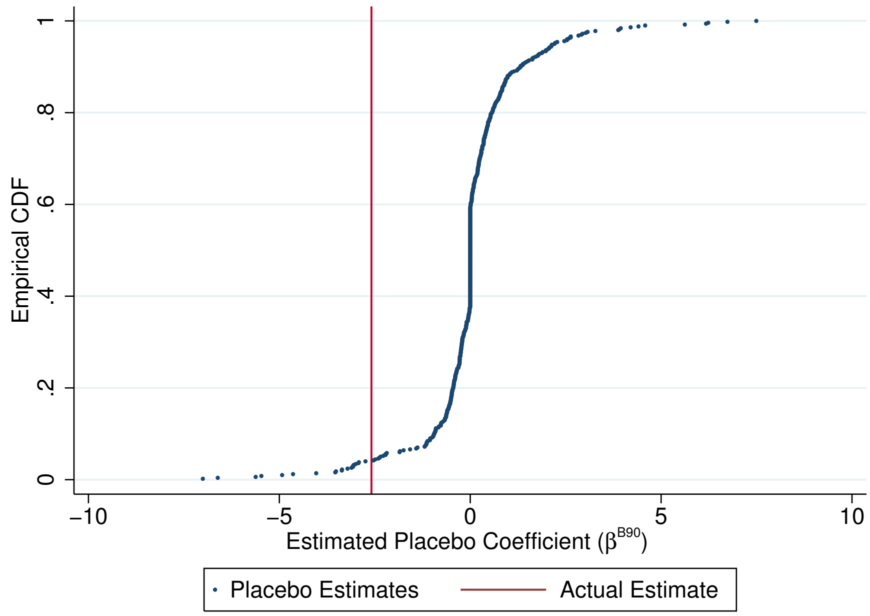
B. Effects of Tax Changes Across the Income Distribution



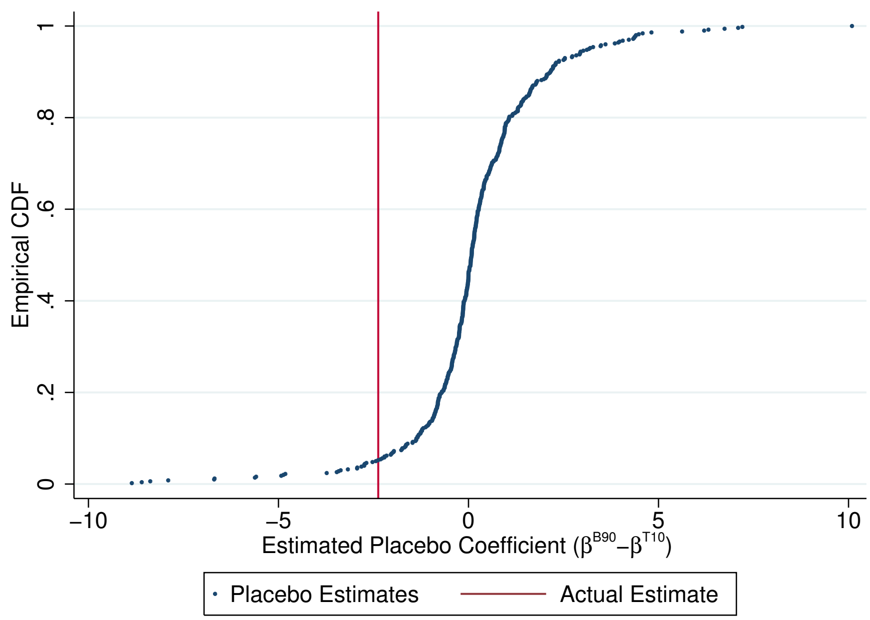
Notes: This figure shows figures that are similar to Panel A of Figure 5 and Figure 6 for two subsamples: state-year observations with above- and below-median state unemployment. See notes of those figures and section 3.5 for more detail.

Figure 10: Non-Parametric Permutation Tests of Tax Shocks on State Employment Growth

A. Effect of Tax Changes for Bottom 90%



B. Difference in Effects of Tax Changes: Bottom 90%- Top 10%



Notes: These graphs provide non-parametric evidence from Chetty et al. (2009) permutation tests. They show that my actual point estimates from Column (1) of Table 4, which are depicted by the red line, exceed the vast majority of placebo estimates that result from randomly assigning another state's tax shocks (for both the top and bottom income group) to another state within that year. Panel A shows that 20 of 500 placeable estimates, or 4%, exceed my actual estimate. Similarly, Panel B shows that 26 out of 500, or 5.2%, of the estimated difference between the bottom 90% and top 10% coefficients exceed my actual estimate from Column (1) of Table 4. See section 3.5 for additional discussion.

Table 1: Example of Tax Schedule Change in 1993

1992 Schedule			1993 Schedule		
Tax Rate	Bracket Min	Bracket Max	Marginal Tax Rate	Bracket Min	Bracket Max
15%	$ 0	$35,800	15%	$ 0	$36,900
28%	$35,800	$86,500	28%	$36,900	$89,150
31%	$86,500	-	31%	$89,150	$140,000
			36%	$140,000	$250,000
			39.6%	$250,000	-

Notes: This table shows the tax schedule in 1992 and 1993 for married taxpayers filing jointly. Extra top brackets were added in 1993. These new brackets mechanically increased tax liabilities for higher-income taxpayers as shown in Figure 1. Tax schedule data are from the Tax Foundation.

Table 2: State Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Year	1994	7.792	1981	2007	1350
Log Employment	7.25	1.02	5.21	9.63	1350
$GovTrans_{PERCAP}$	2991.4	1337.5	834.5	7243.5	1350
$PersTaxes_{PERCAP}$	2665.1	1242.1	744.6	9935.1	1350
State Unemployment Rate	5.74	2.028	2.242	17.45	1350
$T_{s,t}^{B90}$	-0.075	0.177	-0.818	0.449	1349
$T_{s,t}^{T10}$	-0.016	0.192	-1.427	1.636	1350
$T_{s,t}^{B50}$	-0.022	0.048	-0.32	0.132	1349
$T_{s,t}^{T50}$	-0.129	0.277	-1.772	1.594	1350
$T_{s,t}^{B30}$	-0.007	0.023	-0.167	0.052	1350
$T_{s,t}^{m40}$	-0.035	0.072	-0.405	0.192	1349
$T_{s,t}^{T30}$	-0.093	0.245	-1.682	1.617	1350
$\Delta lnEmp_{t,t-2}$	3.295	3.795	-12.22	17.084	1350
$\Delta T_{s,t}^{B90}$	-0.248	0.233	-1.048	0.168	1297
$\Delta T_{s,t}^{T10}$	-0.052	0.335	-1.687	2.161	1300
$\Delta T_{s,t}^{B50}$	-0.072	0.071	-0.43	0.037	1297
$\Delta T_{s,t}^{T50}$	-0.228	0.457	-2.173	2.025	1300
$\Delta T_{s,t}^{B30}$	-0.022	0.037	-0.203	0.039	1300
$\Delta T_{s,t}^{M40}$	-0.136	0.092	-0.527	0.043	1297
$\Delta T_{s,t}^{T30}$	-0.164	0.414	-2.053	2.086	1300
$\Delta GovTrans_{PERCAP}$	0.113	0.057	-0.413	0.436	1250

Table 3: National Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Year	1978	19.485	1945	2011	67
$\Delta \ln Emp_t$	1.409	1.536	-3.773	4.382	63
$\Delta \ln GDP_t$	2.803	3.02	-11.589	8.384	67
$\Delta Tax_{ROMER,t}$	-0.097	0.469	-1.858	0.858	67
$\Delta Tax_{Bottom90,t}$	-0.047	0.172	-0.955	0.282	67
$\Delta Tax_{Top10,t}$	-0.028	0.139	-0.501	0.308	67
$\Delta Tax_{NONINC,t}$	-0.022	0.294	-0.924	0.634	67
$\Delta \ln Consumption_t$	3.437	2.099	-1.964	11.722	67
$\Delta \ln Durables_t$	6.036	9.199	-8.689	59.149	67
$\Delta \ln Nondurables_t$	2.561	1.782	-2.463	8.633	67
$\Delta \ln Investment_t$	5.123	16.01	-28.542	94.144	67
$\Delta \ln ResidentialInv_t$	4.221	21.783	-27.344	143.427	67
Transfers to GDP_t	8.247	3.441	2.287	15.428	67
Unemployment_t	5.78	1.63	2.9	9.70	64

Notes: The ΔTax variables are percent of Nominal GDP (i.e., $100 \times \frac{\text{Tax Liability Change}_t}{GDP_t}$).

Table 4: Effect of Tax Change By Income Group on State Employment Growth

State Employment Growth	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta T^{Bottom90}_{s,t}$	-2.6**	-2.7***				
	(1.0)	(0.9)				
$\Delta T^{Top10}_{s,t}$	-0.2	-0.2				
	(0.2)	(0.1)				
$\Delta T^{Bottom50}_{s,t}$			-7.1***	-8.2***		
			(1.9)	(1.8)		
$\Delta T^{Top50}_{s,t}$			-0.3*	-0.3**		
			(0.2)	(0.1)		
$\Delta T^{Bottom30}_{s,t}$					-5.0	-6.7**
					(3.0)	(2.6)
$\Delta T^{Middle40}_{s,t}$					-5.1**	-5.1***
					(2.1)	(1.9)
$\Delta T^{Top30}_{s,t}$					-0.3*	-0.3**
					(0.2)	(0.1)
Control for $GovTrans_{PERCAP,s,t}$	N	Y	N	Y	N	Y
Observations	1,247	1,247	1,247	1,247	1,247	1,247
R-squared	0.914	0.921	0.915	0.922	0.915	0.922
Bottom - Top:	-2.37**	-2.47**	-6.82***	-7.86***	-4.73	-6.35**
	(1.02)	(0.95)	(1.90)	(1.84)	(3.03)	(2.61)

Notes: This table shows the effects on state employment growth of tax changes by income group of national income and payroll tax changes that Romer and Romer (2010) classify as exogenous. Both state employment growth and tax changes are defined as changes over two years, i.e., $\frac{E_{s,t}-E_{s,t-2}}{E_{s,t-2}}$ and $\frac{\Delta TaxLiability^g_{s,t}+\Delta TaxLiability^g_{s,t-1}+\Delta TaxLiability^g_{s,t-2}}{GDP_{s,t-2}}$ where g is the income group. All columns include state- and year-fixed effects. Column (1) and (2) show effects for tax changes for bottom 90% and top 10%, (3) and (4) show bottom 50% and top 50%, and (5) and (6) show effects by income group quintile. Even numbered columns control for growth in per capita government transfers to account for the concern that progressive tax policy may coincide with progressive spending policy. Bottom - Top shows the estimate of $\Delta T^{Bottom} - \Delta T^{Top}$. Each test indicates statistically and economically significant differences between income groups. The data are annual and begin in 1980. Tax return data from NBER TAXSIM enable me to determine the state from which taxes were filed and thus disaggregate national tax changes by state and income group. All results are weighted by state population. Robust standard errors clustered by state are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of Tax Change By Income Group on State Employment

$\ln Emp_{s,t+h}$	$T^{g}_{s,t}$	(1)	(2)	(3)	(4)
$h=-3$	$T^{Bottom90}_{s,t}$	-0.002	0.000	0.009	0.001
		(0.015)	(0.015)	(0.014)	(0.012)
$h=-2$	$T^{Bottom90}_{s,t}$	-0.004	-0.004	0.010	0.007
		(0.016)	(0.016)	(0.016)	(0.013)
$h=-1$	$T^{Bottom90}_{s,t}$	0.006	-0.001	0.026	0.011
		(0.014)	(0.013)	(0.017)	(0.014)
$h=0$	$T^{Bottom90}_{s,t}$	-0.017	-0.023	0.006	-0.008
		(0.014)	(0.014)	(0.013)	(0.013)
$h=1$	$T^{Bottom90}_{s,t}$	-0.051***	-0.047***	-0.037***	-0.039***
		(0.013)	(0.012)	(0.011)	(0.010)
$h=2$	$T^{Bottom90}_{s,t}$	-0.032***	-0.016*	-0.016*	-0.008
		(0.009)	(0.008)	(0.008)	(0.010)
$h=3$	$T^{Bottom90}_{s,t}$	-0.026	-0.007	-0.013	0.000
		(0.017)	(0.014)	(0.015)	(0.015)
$h=4$	$T^{Bottom90}_{s,t}$	-0.031*	-0.010	-0.020	-0.006
		(0.018)	(0.014)	(0.017)	(0.016)
$h=-3$	$T^{Top10}_{s,t}$	-0.005	-0.005	-0.006	-0.006
		(0.005)	(0.004)	(0.005)	(0.005)
$h=-2$	$T^{Top10}_{s,t}$	-0.005	-0.005	-0.006	-0.007
		(0.005)	(0.005)	(0.006)	(0.005)
$h=-1$	$T^{Top10}_{s,t}$	0.002	0.002	0.001	0.000
		(0.004)	(0.003)	(0.004)	(0.004)
$h=0$	$T^{Top10}_{s,t}$	0.001	0.001	0.001	0.001
		(0.004)	(0.004)	(0.004)	(0.004)
$h=1$	$T^{Top10}_{s,t}$	-0.001	-0.001	-0.002	-0.002
		(0.003)	(0.003)	(0.003)	(0.003)
$h=2$	$T^{Top10}_{s,t}$	-0.002	-0.002	-0.003	-0.003
		(0.002)	(0.002)	(0.002)	(0.002)
$h=3$	$T^{Top10}_{s,t}$	-0.001	-0.002	-0.002	-0.002
		(0.003)	(0.003)	(0.003)	(0.003)
$h=4$	$T^{Top10}_{s,t}$	-0.001	-0.002	-0.002	-0.001
		(0.003)	(0.003)	(0.003)	(0.003)
Control for $\Delta GovTrans_{PERCAP,s,t}$		N	Y	N	Y
$h=-3$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	0.003	0.005	0.015	0.008
		(0.016)	(0.015)	(0.015)	(0.013)
$h=-2$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	0.001	0.001	0.016	0.014
		(0.017)	(0.017)	(0.018)	(0.015)
$h=-1$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	0.003	-0.003	0.025	0.011
		(0.015)	(0.014)	(0.017)	(0.015)
$h=0$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	-0.019	-0.024	0.005	-0.008
		(0.014)	(0.015)	(0.013)	(0.013)
$h=1$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	-0.051***	-0.046***	-0.035***	-0.037***
		(0.012)	(0.011)	(0.010)	(0.010)
$h=2$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	-0.030***	-0.013	-0.013	-0.006
		(0.009)	(0.008)	(0.008)	(0.010)
$h=3$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	-0.025	-0.005	-0.010	0.001
		(0.018)	(0.014)	(0.015)	(0.015)
$h=4$	$\beta^{Bottom}_{t+h}-\beta^{Top}_{t+h}$	-0.030	-0.008	-0.017	-0.005
		(0.019)	(0.014)	(0.017)	(0.016)

NOTES: See Section 3.2. *** if $p < 0.01$; ** if $p < 0.05$; * if $p < 0.10$.

Table 6: Effect of Tax Change By Income Group on State Employment

$\ln Emp_{s,t+h}$	$T^g_{s,t}$	(1)	(2)	(3)	(4)
$h=-3$	$T^{Bottom50}_{s,t}$	-0.021	0.000	-0.012	-0.021
		(0.033)	(0.035)	(0.036)	(0.034)
$h=-2$	$T^{Bottom50}_{s,t}$	-0.006	0.005	0.009	0.004
		(0.030)	(0.028)	(0.030)	(0.022)
$h=-1$	$T^{Bottom50}_{s,t}$	-0.024	-0.020	-0.003	-0.010
		(0.021)	(0.017)	(0.025)	(0.015)
$h=0$	$T^{Bottom50}_{s,t}$	-0.068***	-0.068***	-0.044	-0.052***
		(0.023)	(0.023)	(0.028)	(0.022)
$h=1$	$T^{Bottom50}_{s,t}$	-0.108***	-0.104***	-0.094***	-0.095***
		(0.024)	(0.024)	(0.025)	(0.024)
$h=2$	$T^{Bottom50}_{s,t}$	-0.080***	-0.062***	-0.069***	-0.059***
		(0.023)	(0.019)	(0.021)	(0.019)
$h=3$	$T^{Bottom50}_{s,t}$	-0.095***	-0.074***	-0.092***	-0.072***
		(0.031)	(0.023)	(0.028)	(0.021)
$h=4$	$T^{Bottom50}_{s,t}$	-0.084***	-0.058***	-0.090***	-0.069***
		(0.029)	(0.020)	(0.028)	(0.020)
$h=-3$	$T^{Top50}_{s,t}$	-0.004	-0.005	-0.004	-0.005
		(0.005)	(0.004)	(0.004)	(0.004)
$h=-2$	$T^{Top50}_{s,t}$	-0.005	-0.005	-0.005	-0.005
		(0.005)	(0.005)	(0.005)	(0.005)
$h=-1$	$T^{Top50}_{s,t}$	0.004	0.003	0.004	0.002
		(0.004)	(0.003)	(0.004)	(0.003)
$h=0$	$T^{Top50}_{s,t}$	0.001	0.000	0.003	0.001
		(0.003)	(0.003)	(0.003)	(0.003)
$h=1$	$T^{Top50}_{s,t}$	-0.005*	-0.005*	-0.004	-0.004
		(0.003)	(0.003)	(0.003)	(0.003)
$h=2$	$T^{Top50}_{s,t}$	-0.004***	-0.003	-0.003*	-0.002
		(0.002)	(0.002)	(0.002)	(0.002)
$h=3$	$T^{Top50}_{s,t}$	-0.002	0.000	-0.001	0.001
		(0.003)	(0.003)	(0.003)	(0.003)
$h=4$	$T^{Top50}_{s,t}$	-0.003	-0.001	-0.002	0.000
		(0.003)	(0.003)	(0.004)	(0.004)
Control for $\Delta GovTrans_{PERCAP,s,t}$		N	Y	N	Y
$h=-3$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.017	0.004	-0.008	-0.016
		(0.033)	(0.035)	(0.035)	(0.033)
$h=-2$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.001	0.010	0.014	0.009
		(0.031)	(0.028)	(0.031)	(0.022)
$h=-1$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.027	-0.022	-0.007	-0.013
		(0.020)	(0.016)	(0.023)	(0.014)
$h=0$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.068***	-0.068***	-0.047*	-0.053***
		(0.023)	(0.022)	(0.027)	(0.021)
$h=1$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.103***	-0.100***	-0.090***	-0.091***
		(0.024)	(0.024)	(0.024)	(0.024)
$h=2$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.076***	-0.060***	-0.066***	-0.057***
		(0.023)	(0.019)	(0.021)	(0.020)
$h=3$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.093***	-0.074***	-0.091***	-0.073***
		(0.031)	(0.023)	(0.028)	(0.021)
$h=4$	$\beta^{Bottom}_{t+h} - \beta^{Top}_{t+h}$	-0.081***	-0.057***	-0.088***	-0.069***
		(0.028)	(0.020)	(0.027)	(0.019)

NOTES: See Section 3.2. *** if $p < 0.01$; ** if $p < 0.05$; * if $p < 0.10$.

Table 7: National Effects of Tax Changes By Income Group on Employment Growth

National Employment Growth	(1)	(2)	(3)
ΔTax_t^{B90}	-0.6	-0.5	-0.6
	(1.0)	(1.1)	(0.8)
ΔTax_{t-1}^{B90}	-2.4**	-2.5**	-2.3**
	(1.1)	(1.0)	(0.9)
ΔTax_{t-2}^{B90}	-2.1**	-1.4*	-1.2
	(1.0)	(0.8)	(0.9)
ΔTax_t^{T10}	2.2	2.0	1.5
	(1.5)	(1.7)	(1.1)
ΔTax_{t-1}^{T10}	0.3	-0.4	-0.0
	(1.5)	(1.8)	(1.2)
ΔTax_{t-2}^{T10}	-0.8	-0.4	-0.3
	(0.8)	(0.6)	(0.5)
Constant	1.2***	0.9***	1.2**
	(0.3)	(0.3)	(0.6)
Control for $\Delta Tax_{NONINC,t}$ and lags	Y	Y	Y
Control for lagged Employment Growth	N	Y	Y
Control for Transfers to GDP_t and lags	N	N	Y
Observations	61	61	61
R-squared		0.258	0.706
Bottom90 Tax Change: $\beta_t + \beta_{t-1} + \beta_{t-2}$	-5.12**	-4.34**	-4.01*
	(2.14)	(1.74)	(1.95)
Top10 Tax Change: $\beta_t + \beta_{t-1} + \beta_{t-2}$	1.69	1.17	1.18
	(2.66)	(3.15)	(2.07)
Bottom - Top:	-6.81*	-5.51	-5.19
	(4.03)	(4.28)	(3.31)

Notes: This table shows the effects by income group of income and payroll tax changes that Romer and Romer (2010) classify as exogenous on annual U.S. employment growth. Column (1) uses a simple moving average specification for employment growth and controls for a measure of exogenous non-income taxes. Column (2) uses an autoregressive specification with three lags. Column (3) controls for transfers as a share of GDP and lags to account for the concern that progressive tax policy may coincide with progressive spending policy. Following Romer and Romer (2010), data begin in 1950 (although lags reflect data from prior years). Newey West standard errors with lag of 2 in parentheses are shown in Column (1). I allow for for serial correlation by including $Growth_{E,t-k}$ for $k \in (2,3)$ in regressions. Robust standard errors are in parentheses for Column (2) and (3). *** p<0.01, ** p<0.05, * p<0.1.

Appendices for Online Publication

A Data

Only four exogenous changes, affecting tax liabilities in 1948, 1950, 1954, and 1960, took place in a time preceding the coverage of TAXSIM. For each of these changes, I manually calculated tax changes by income group using the SOI data.[30] The SOI reports provide data on the number of taxable returns and the amount of taxable income for groups created by size of adjusted gross income. With many AGI brackets, one can form a rough idea how taxes changed across the income distribution. For each income bracket, I created a representative taxpayer by dividing the amount of taxable income by the number of taxable returns. I then calculated this representative person's change in payroll or federal income tax liability using his income in the year prior to the tax change, the old schedule and the new schedule. Data from the tax schedule were from (2) and (3) for the income tax changes and from (5) for the payroll rate and base changes. For instance, in 1960, any representative taxpayer whose earnings were below the payroll tax base of $4,800 had to pay 1% of their income extra since rates increased from 5% to 6%. Note that Barro and Sahasakul (1983) used a somewhat similar approach to calculate average marginal rates.

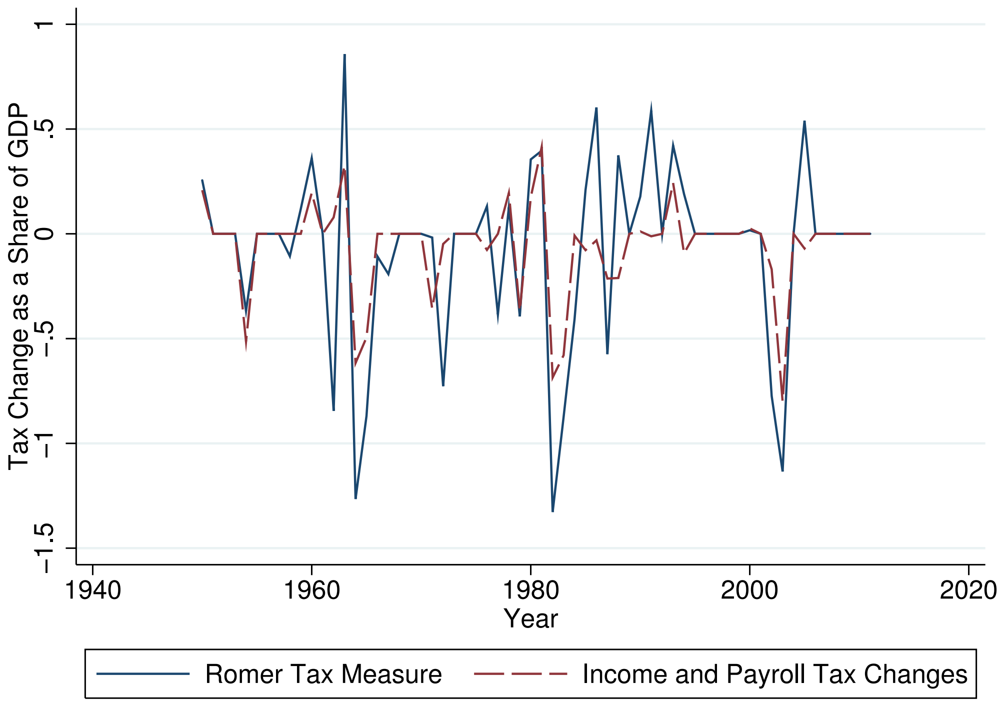
In general, the following sources were helpful for constructing these tax change measures: (1) the Brooking Institution's "Individual Income Tax Brackets, 1945-2010," (2) the Tax Foundation's "U.S. Federal Individual Income Tax Rates History, 1913-2010," (3) the Internal Revenue Service's annual individual income tax return reports, and (4) the Tax Policy Center's Historical Payroll Tax Rates report.[31]

[30] The 1948 change was from the Revenue Act of 1948, the 1950 change was from the 1947 Social Security Amendment, the 1954 change was from the 1950 Social Security Amendment and the Internal Revenue Code of 1954, and the 1960 change was from the 1958 Social Security Amendment (Romer & Romer (2009)).

[31] Note that the Tax Policy Center data on the payroll base and rates come from the following two Social Security Administration sites: http://www.ssa.gov/OACT/COLA/cbb.html and http://www.ssa.gov/OACT/ProgData/taxRates.html.

B Empirics

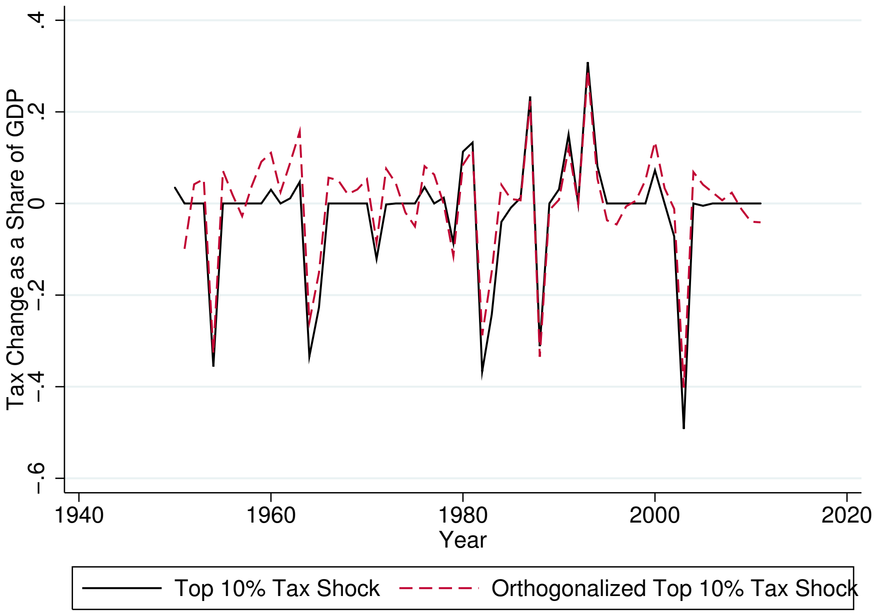
Figure A1: Comparison of Aggregate Tax Changes with Romer & Romer Changes



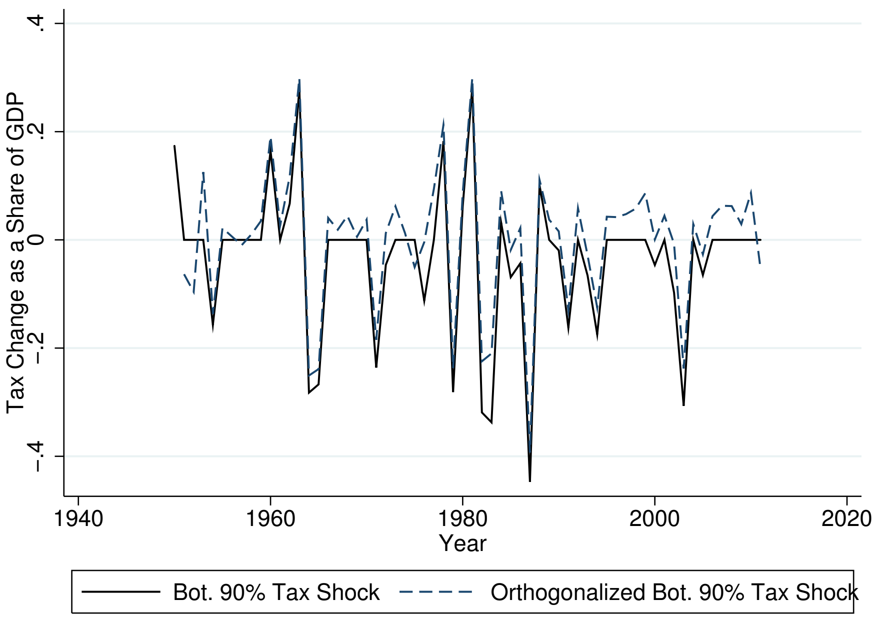
Notes: This figure shows two postwar time series of tax changes: (1) the sum of all income and payroll tax changes that Romer and Romer (2010) classify as exogenous and (2) the exogenous tax change measures of Romer and Romer (2010). Both series are as a share of GDP. Some of the Romer and Romer (2010) tax changes affect corporate taxes and other revenue sources, but the two series track each other fairly closely.

Figure A2: Favero and Giavazzi Orthogonality Test for Both Tax Change Series

A. Tax Changes for Top 10%



B. Tax Changes for Bottom 90%



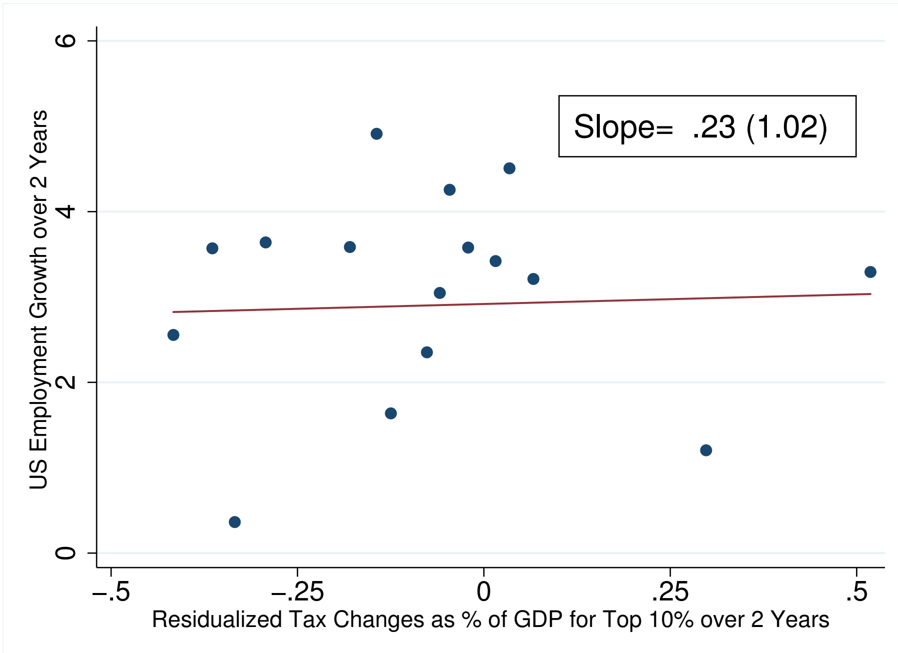
Notes: This figure plots the raw time series of federal income and payroll tax changes as a share of GDP as well as an orthogonalized time series of the residual of the tax change measure after partialling out lagged macro aggregates, which are annual log changes in employment, inflation, government transfers as a share of GDP, and federal debt as a share of GDP. The graphs show that the orthogonalized version is quite similar to the raw time series, suggesting that these federal tax shock series for the top 10% and bottom 90% both pass the Favero and Giavazzi (2012) orthogonality test. See data section for sources and section 3.5 for additional discussion.

Figure A3: Frisch Waugh Regression: Tax Changes for Top versus Bottom

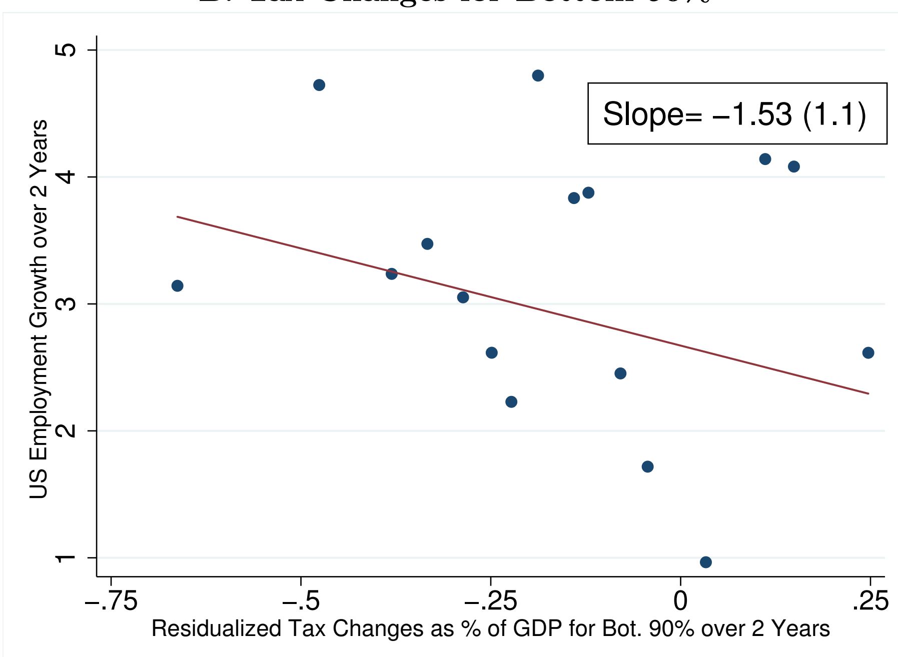
Notes: This figure plots exogenous tax changes for those with AGI in top 10% by those for the bottom 90%. Both tax changes are as a share of output. The figure also plots the predicted value of exogenous tax changes for those in the top 10% from a simple bivariate regression on exogenous tax changes for those with AGI in the bottom 90%. Years that fall below the best fit line had tax changes that went disproportionately to the top 10% (given the magnitude of tax changes for the bottom 90% as a share of output).

Figure A4: U.S. Employment Growth and Orthogonalized Federal Income Tax Changes

A. Tax Changes for Top 10%

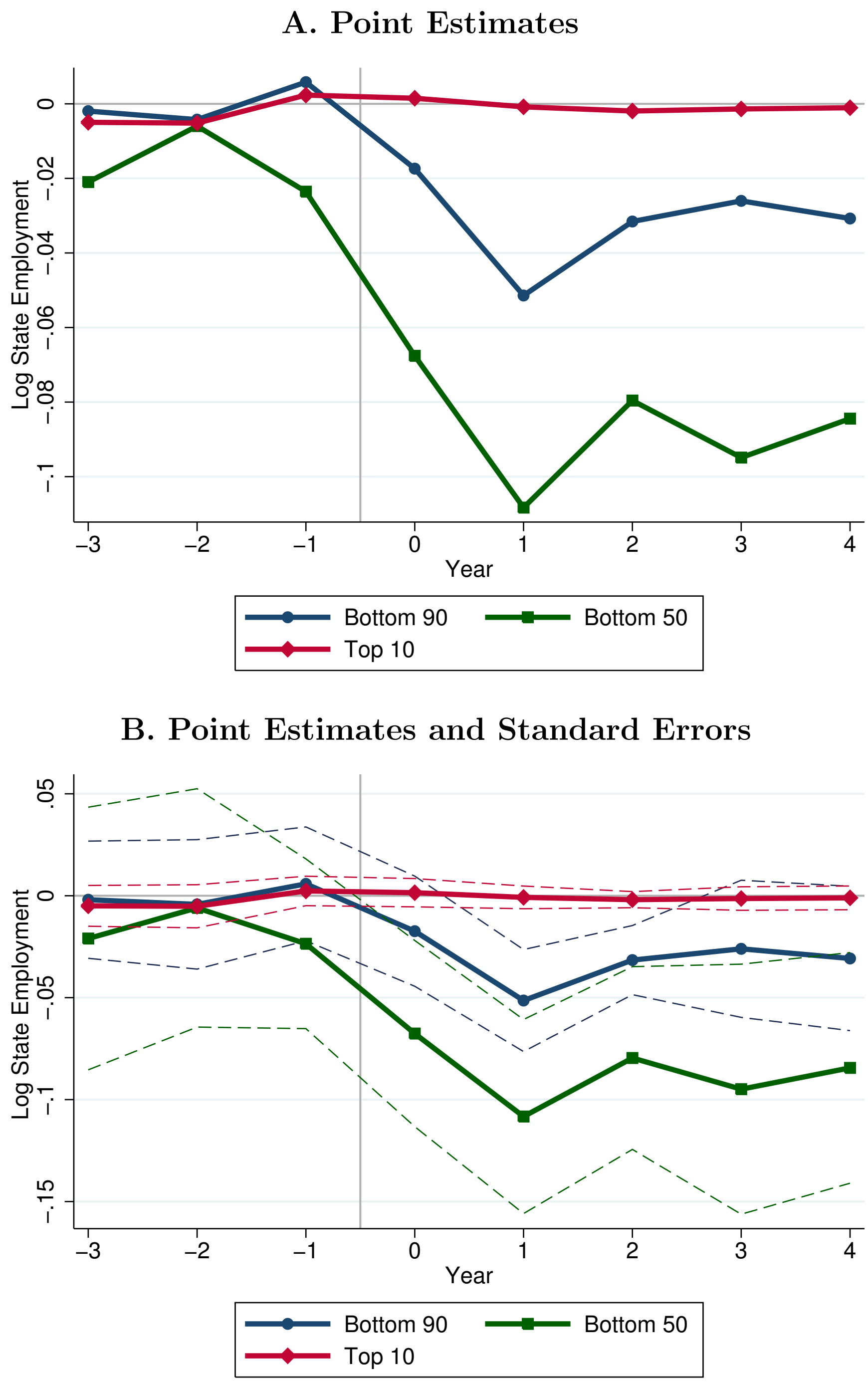


B. Tax Changes for Bottom 90%



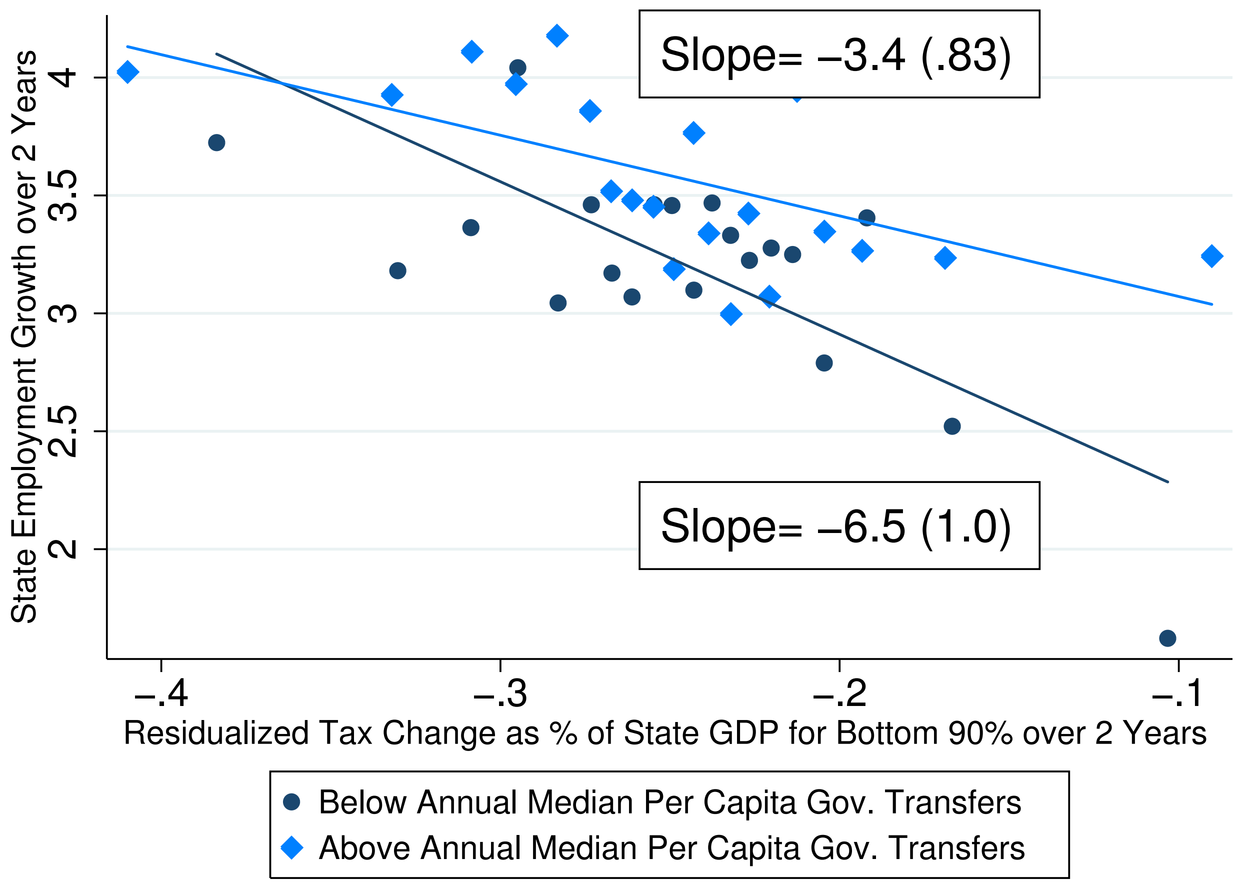
Notes: This figure plots average two year employment growth for bins of orthogonalized sum of income and payroll tax changes as a share of GDP during the prior two years for those with AGI in the Top 10% in Panel A and for those with AGI in the Bottom 90% in Panel B. The figure also plots the predicted value of two year employment growth from a simple bivariate regression. The tax changes are orthogonalized by controlling for tax changes for the other group. Only tax changes that Romer and Romer (2010) classify as exogenous are considered non-zero tax changes. Following Romer and Romer (2010), the data start in 1950. See Figure 5 for a similar figure for state-level outcomes.

Figure A5: Event Study of Federal Tax Shock on State Employment Growth by Income Group



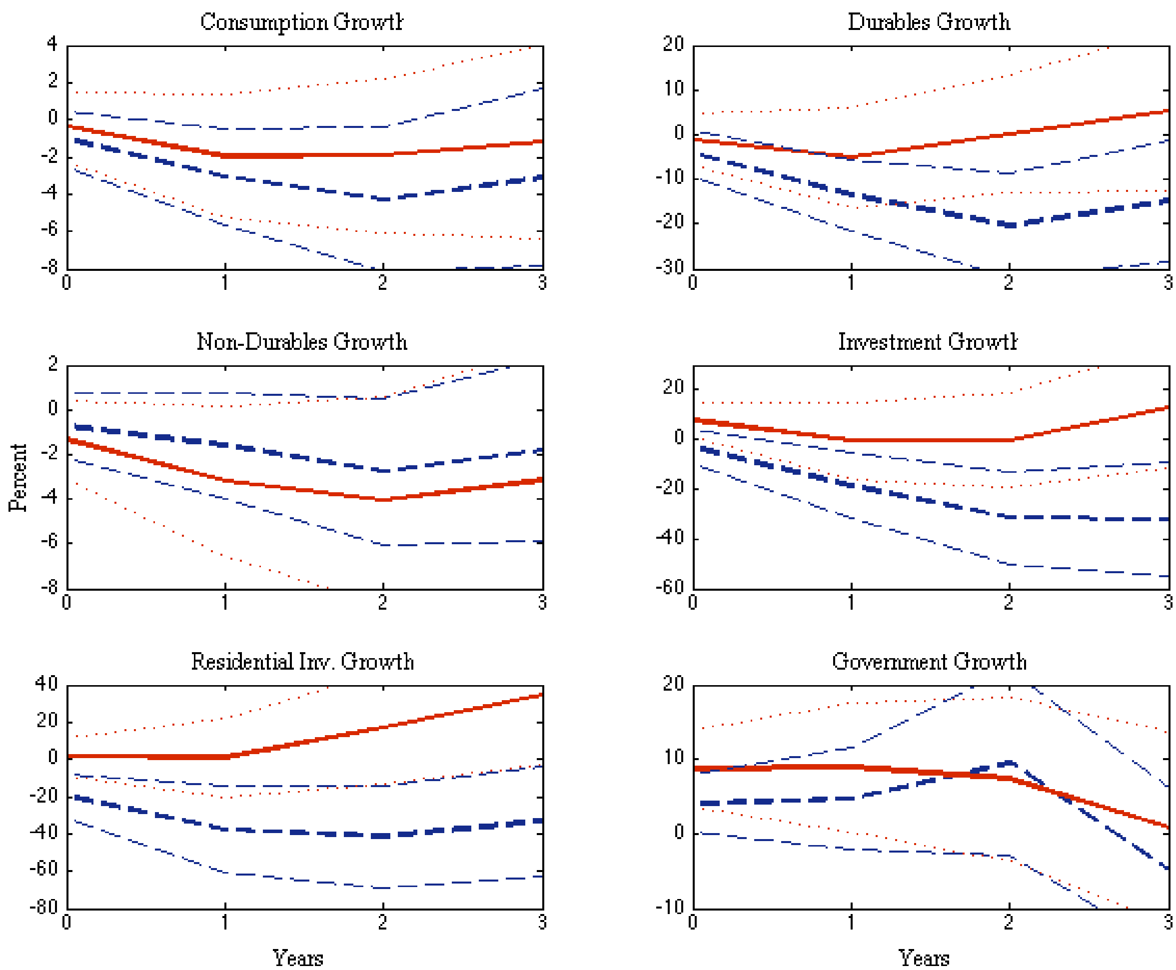
Notes: This figure shows an event study of a 1% of GDP tax increase on state employment for those with AGI in the bottom 90% nationally in blue and for those with AGI in the top 10% nationally in red as in Figure 7. However, this figure also shows the effects for a similar specification with effects for the bottom 50% and the top 50% (not shown). See Section 3.2 for more details and Table 5 and Table 6 for point estimates for the bottom 90% and bottom 50% groups, respectively. Due to availability of state identifiers in TAXSIM, the data start in 1980. Standard errors are robust and clustered by state and 95% confidence intervals are shown.

Figure A6: Effects of Tax Shocks on State Employment Growth by Government Transfer Group



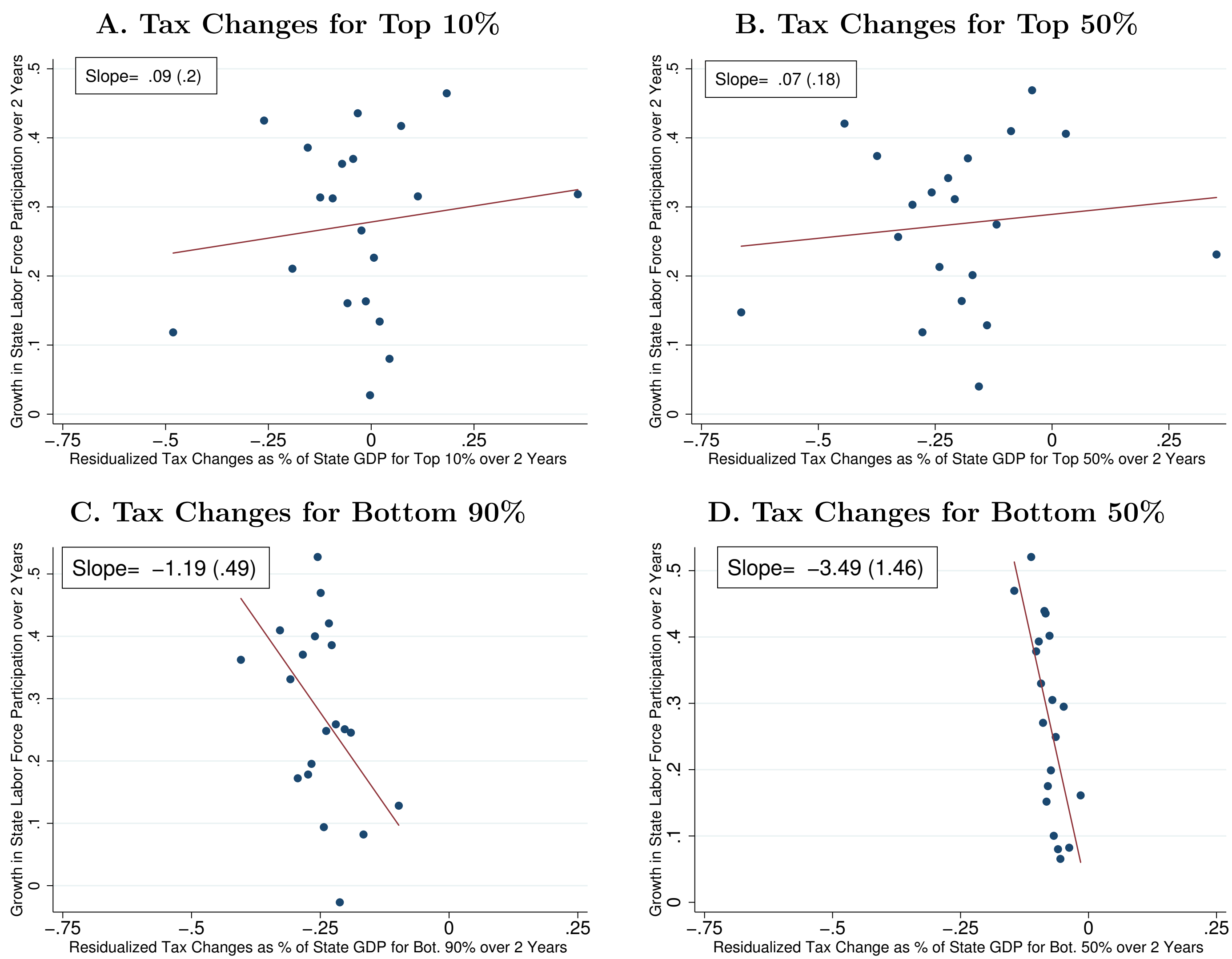
Notes: This figure is similar those in Figure 5, but split into two groups: above and below median per capita government transfers (relative to economic conditions). In particular, I first regress log per capita government transfers on (a) log state GDP in years t, $t-1$, and $t-2$ as well as (b) the state unemployment rate in years t, $t-1$, and $t-2$. I then take the residual per capita government transfer measure as the object of interest and generate an annual series of the median value of residual per capita government transfers. Finally, I define an state-year indicator for whether or not the state is above or below the median that year and split the sample on that basis. The motivation of this exercise is that high transfer payments are correlated with other forms of progressive spending, so this split sample test provides further evidence that unobserved progressive spending changes are not driving my results.

Figure A7: National Impulse Responses: Macroeconomic Aggregates



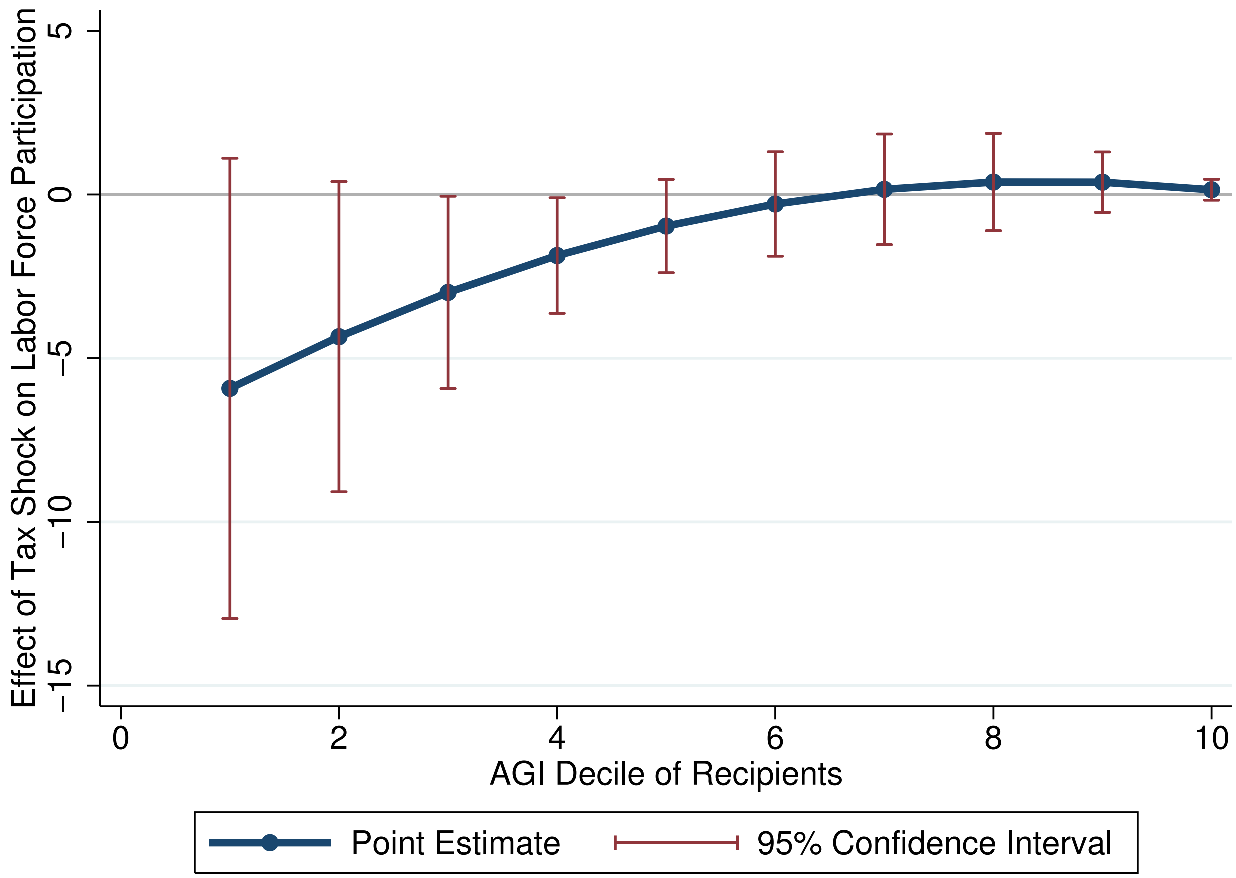
Notes: These six graphs show impulse responses of macroeconomic aggregates to exogenous tax changes of 1% of GDP for the bottom 90% (dashed, blue) and top 10% (solid, red) respectively. Each uses a simple moving average specification (see the details in column 1 of Table 7). One standard error bans are shown. Standard errors are calculated using Monte Carlo simulations of 10,000 draws from respective estimated point estimate vector and covariance matrix.

Figure A8: Labor Force Participation Growth and Federal Income Tax Shocks by Income Group



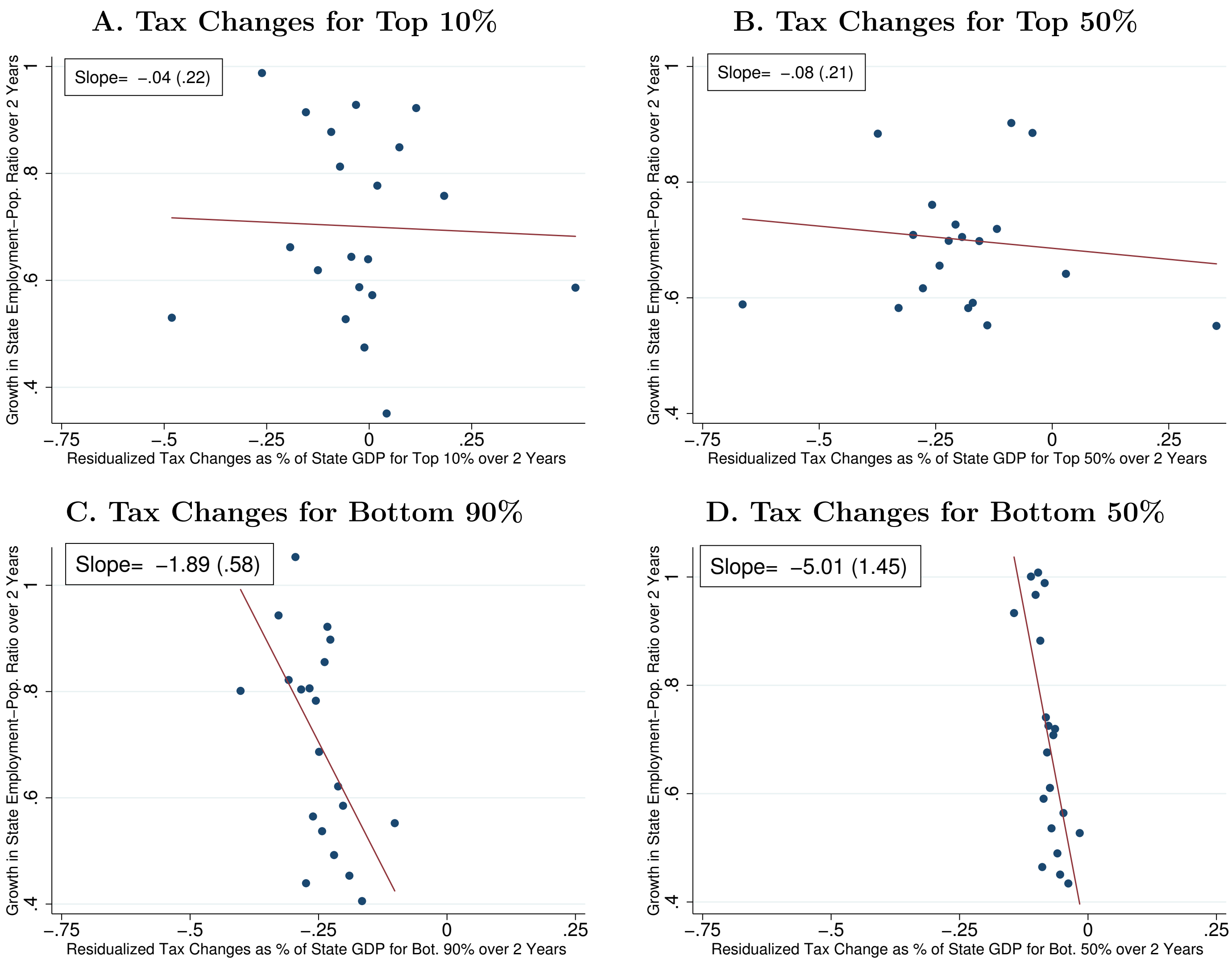
Notes: This figure plots mean labor force participation growth by bins of orthogonalized tax changes as a share of GDP during the prior two years for those with AGI in the top 10% nationally in Panel A and for those with AGI in the bottom 90% nationally in Panel B. The tax changes are orthogonalized by controlling for tax changes for the other group as well as state- and year-fixed effects. The figure also plots the predicted value of labor force participation growth from a simple bivariate regression. Slopes and robust standard errors clustered by state are reported. Only tax changes that Romer and Romer (2010) classify as exogenous are considered non-zero tax changes. Due to availability of state identifiers in TAXSIM, the data start in 1980. Labor force participation data are from BLS Local Area Unemployment Statistics, which are available at `http://www.bls.gov/lau/rdscnp16.htm#data`.

Figure A9: Aggregate Effects of Individual Tax Changes Across the Income Distribution



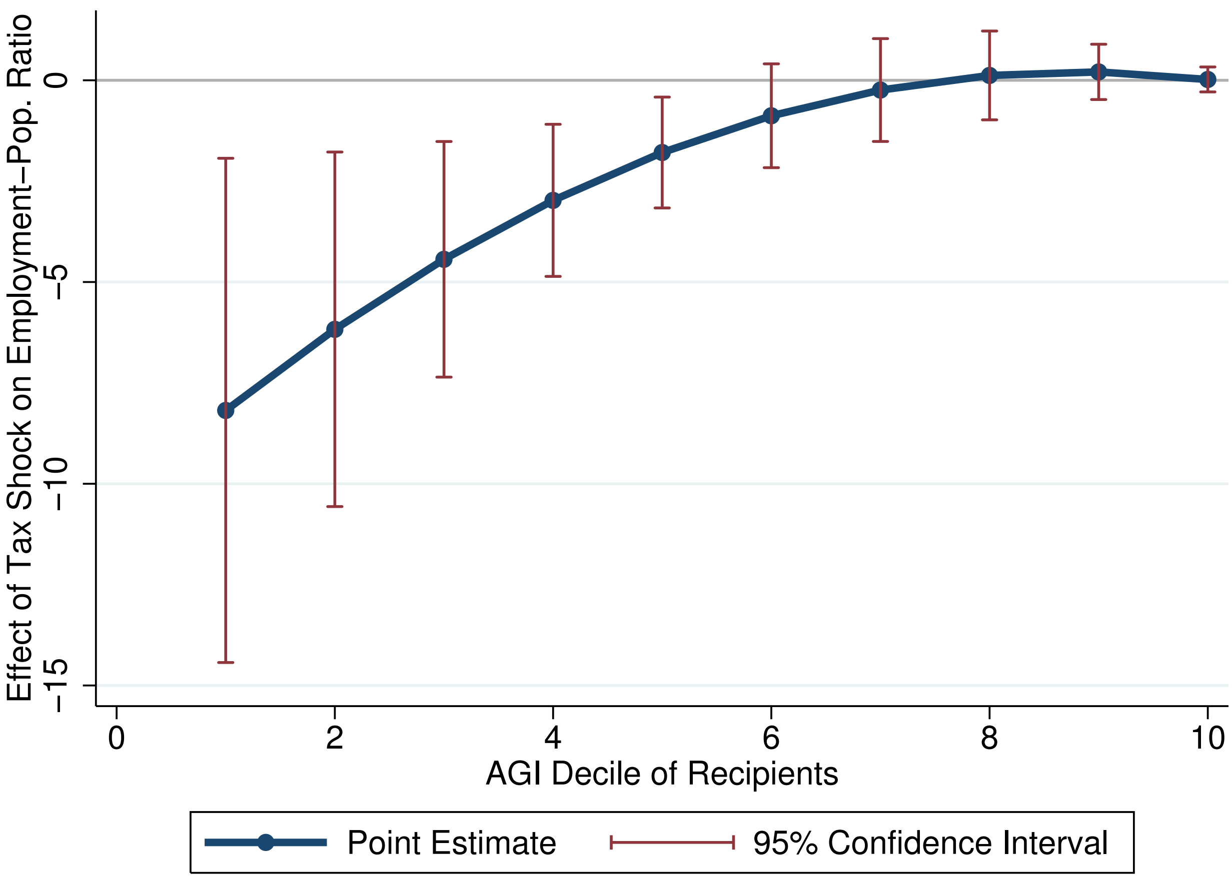
Notes: This graph shows how the effect on the labor force participation rate of a 1% of GDP increase in taxes varies by the AGI decile of the taxpayer who pays for it. In particular, it shows $\hat{\theta}_0 + \hat{\theta}_1 g + \hat{\theta}_2 g^2$, which is the estimated second order approximation of the $\beta(g)$ function that maps an income decile into the estimated effect on annual labor force participation growth from an exogenous income and payroll tax change over the last two years for that decile. Labor force participation data are from BLS Local Area Unemployment Statistics, which are available at `http://www.bls.gov/lau/rdscnp16.htm#data`.

Figure A10: Employment-to-Population Growth and Federal Income Tax Shocks by Income Group



Notes: This figure plots mean growth in the employment-to-population ratio by bins of orthogonalized tax changes as a share of GDP during the prior two years for those with AGI in the top 10% nationally in Panel A and for those with AGI in the bottom 90% nationally in Panel B. The tax changes are orthogonalized by controlling for tax changes for the other group as well as state- and year-fixed effects. The figure also plots the predicted value of employment-to-population rate growth from a simple bivariate regression. Slopes and robust standard errors clustered by state are reported. Only tax changes that Romer and Romer (2010) classify as exogenous are considered non-zero tax changes. Due to availability of state identifiers in TAXSIM, the data start in 1980. Employment-to-population data are from BLS Local Area Unemployment Statistics, which are available at http://www.bls.gov/lau/

Figure A11: Aggregate Effects of Individual Tax Changes Across the Income Distribution



Notes: This graph shows how the effect on the employment-to-population ratio of a 1% of GDP increase in taxes varies by the AGI decile of the taxpayer who pays for it. In particular, it shows $\hat{\theta}_0 + \hat{\theta}_1 g + \hat{\theta}_2 g^2$, which is the estimated second order approximation of the $\beta(g)$ function that maps an income decile into the estimated effect on annual employment-to-population growth from an exogenous income and payroll tax change over the last two years for that decile. Employment-to-population data are from BLS Local Area Unemployment Statistics, which are available at http://www.bls.gov/lau/rdscnp16.htm#data.

Table A1: PLACEBO Effects of Tax Change By Income Group on State Employment Growth

State Employment Growth	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta T_{s,t}^{Bottom90}$	-0.3	-0.6				
	(1.1)	(1.1)				
$\Delta T_{s,t}^{Top10}$	0.1	0.1				
	(0.3)	(0.3)				
$\Delta T_{s,t}^{Bottom50}$			1.0	1.3		
			(1.6)	(1.7)		
$\Delta T_{s,t}^{Top50}$			0.0	0.0		
			(0.3)	(0.2)		
$\Delta T_{s,t}^{Bottom30}$					3.7	4.8*
					(2.5)	(2.6)
$\Delta T_{s,t}^{Middle40}$					-1.3	-1.9
					(1.9)	(1.7)
$\Delta T_{s,t}^{Top30}$					0.0	0.0
					(0.3)	(0.3)
Control for $GovTrans_{PERCAP,s,t}$	N	Y	N	Y	N	Y
Observations	1,097	1,097	1,097	1,097	1,097	1,097
R-squared	0.878	0.881	0.878	0.881	0.878	0.881
Bottom - Top:	-0.43	-0.70	1.01	1.30	3.64	4.74
	(1.27)	(1.23)	(1.70)	(1.81)	(2.54)	(2.72)

Notes: This table shows the PLACEBO effects on state employment growth of tax changes by income group of national income and payroll tax changes that Romer and Romer (2010) classify as exogenous. It runs the same procedure as in Table 4, but with employment outcomes that start in 1970 rather than 1980. For instance, the first observation for Alabama with tax shocks from 1980 uses Alabama employment outcomes from 1970 rather than 1980. See notes of Table 4 for additional detail.

Table A2: Effects of Romer Tax Changes on Output & Employment Growth

Dependent Variable	$Growth_Y$		$Growth_E$	
	(1)	(2)	(3)	(4)
$\Delta Tax_{ROMER,t}$	-0.4	-0.4	-0.0	-0.1
	(0.8)	(0.8)	(0.4)	(0.4)
$\Delta Tax_{ROMER,t-1}$	-1.4***	-1.4**	-0.6**	-0.6*
	(0.5)	(0.5)	(0.3)	(0.3)
$\Delta Tax_{ROMER,t-2}$	-0.6	-0.4	-0.8***	-0.6**
	(0.5)	(0.5)	(0.3)	(0.3)
Constant	2.9***	2.5***	1.3***	1.1***
	(0.3)	(0.7)	(0.2)	(0.3)
Control for $Growth_Y$ lags	N	Y	N	N
Control for $Growth_E$ lags	N	N	N	Y
Observations	61	61	61	61
R-squared		0.123		0.171
Romer Tax Change: $\beta_t + \beta_{t-1} + \beta_{t-2}$	-2.403**	-2.173**	-1.408**	-1.265**
t-stat	-2.395	-2.453	-2.257	-2.520
p-val	0.0199	0.0174	0.0278	0.0147

Notes: This table shows the effects of exogenous tax change measures of Romer and Romer (2010) on growth in output Y and employment E. These estimates provide a baseline for subsequent estimates for different income groups. Column (1) uses a simple moving average specification for output growth and Column (2) uses an autoregressive specification with two lags. Columns (3) and (4) are similar for employment growth. Following Romer and Romer (2010), data begin in 1950 (although lags reflect data from prior years). Note that this regression is at an annual rather than quarterly frequency as in their paper. Newey West standard errors with lag of 2 in parentheses in Column (1) & (3). I allow for serial correlation by including $Growth_{E,t-k}$ or $Growth_{Y,t-k}$ for $k \in (1,2)$ in regressions Columns (2) & (4). Robust standard errors in parentheses for Column (2) and (4). *** p<0.01, ** p<0.05, * p<0.1.

Table A3: National Effects of Unanticipated Tax Changes By Income Group on Emp. Growth

National Employment Growth	(1)	(2)	(3)
ΔTax_t^{B90}	-2.6	-2.1	-2.5
	(3.1)	(3.1)	(1.7)
ΔTax_{t-1}^{B90}	-1.1	-1.2	-3.5**
	(1.5)	(1.4)	(1.5)
ΔTax_{t-2}^{B90}	-1.7	-0.9	-1.9
	(1.2)	(1.1)	(1.3)
ΔTax_t^{T10}	1.2	1.1	1.4
	(2.8)	(2.6)	(1.5)
ΔTax_{t-1}^{T10}	-2.6	-3.5	-0.5
	(1.8)	(2.3)	(1.8)
ΔTax_{t-2}^{T10}	-1.1	-0.3	-0.9
	(1.4)	(1.2)	(1.4)
Constant	1.2***	1.0***	0.9*
	(0.3)	(0.3)	(0.6)
Control for $\Delta Tax_{NONINC,t}$ and lags	Y	Y	Y
Control for lagged Employment Growth	N	Y	Y
Control for Transfers to GDP$_t$ and lags	N	N	Y
Observations	61	61	61
R-squared		0.217	0.728
Bottom90 Tax Change: $\beta_t + \beta_{t-1} + \beta_{t-2}$	-5.46	-4.19	-7.92
	(4.50)	(4.02)	(3.61)
Top10 Tax Change: $\beta_t + \beta_{t-1} + \beta_{t-2}$	-2.47	-2.73	-0.041
	(3.83)	(3.74)	(2.50)
Bottom - Top:	-2.99	-1.46	-7.88
	(7.57)	(7.01)	(4.88)

Notes: This table shows the effects by income group of income and payroll tax changes that Romer and Romer (2010) classify as exogenous and the Mertens and Ravn (2012) classify as unanticipated on annual U.S. employment growth. Column (1) uses a simple moving average specification for employment growth and controls for a measure of exogenous non-income and payroll taxes. Column (2) uses an autoregressive specification with three lags. Column (3) controls for transfers as a share of GDP and lags to account for the concern that progressive tax policy may coincide with progressive spending policy. Following Romer and Romer (2010), data begin in 1950 (although lags reflect data from prior years). Newey West standard errors with lag of 2 in parentheses in Column (1). I allow for for serial correlation by including $Growth_{E,t-k}$ for $k \in (2, 3)$ in regressions. Robust standard errors in parentheses for Column (2) and (3). *** p<0.01, ** p<0.05, * p<0.1.