Tax Cuts for Whom? Heterogeneous Effects of Income Tax Changes on Growth and Employment

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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. Variation in the income distribution across US 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 percent on employment growth is small.

> There are two ideas of government. There are those who believe that if you just legislate to make the well-to-do prosperous, that their prosperity will leak through on those below. The Democratic idea has been that if you legislate to make the masses prosperous their prosperity will find its way up and through every class that rests upon it. (William Jennings Bryan, July 1896)

The consequences of changing tax policy for different groups are fiercely debated. Some policy makers maintain that tax changes for high-income earners "trickle down" and are the most effective way to affect prosperity. They argue that higher marginal tax rates for top-income taxpayers

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lead to large distortions in labor supply, investment, and hiring, so tax cuts for top-income taxpayers most effectively increase aggregate economic activity. Others, however, contend the opposite. They argue that lower-income groups have higher marginal propensities to consume and disincentives to work from means-tested benefits, so tax cuts for lower-income groups generate sizable consumption and labor supply responses and, thereby, more overall activity. Do tax changes for high-income earners "trickle down"? Would these effects be larger if the tax changes were less targeted at the top?

Variation in income tax policy in the United States can help us answer these questions and inform the debate on "trickle-down" versus "bottom-up" economics. 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 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 tax cuts that go to high-income taxpayers generate less growth than similarly sized tax cuts for low- and moderate-income taxpayers. In fact, the positive relationship between tax cuts and employment growth is largely driven by tax cuts for lower-income groups and the effect of tax cuts for the top 10 percent on employment growth is small.

Establishing this result requires overcoming three empirical difficulties. 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, so separately identifying the effects of low- and high-income tax cuts is difficult. Third, the number of data points and tax changes in the postwar period is limited.

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This paper uses variation in the regional impact of national tax shocks to overcome these empirical difficulties. Variation in the income distribution across US states leads to heterogeneous regional impacts of federal income tax changes. 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 the NBER's TAXSIM to quantify these tax shocks. For 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. On the basis of 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's tax data to avoid conflating behavioral responses and measured changes in tax liabilities. I then aggregate these mechanical tax changes for each taxpayer in a state by income group, such as the bottom 90 percent and top 10 percent of national adjusted gross income (AGI), respectively.

With these year-state-income-group-level tax shock measures, I investigate how responsive employment growth and economic activity are to tax shocks for different income groups. I estimate the dynamic effects of tax changes for different groups using event studies, distributed lag models, and more parsimonious 2-year changes. Since federal tax changes differ in their progressivity, the tax shock from a given federal tax change differs regionally on the basis of each location's income distribution. These regional differences in tax shocks enable me to identify the effects of tax shocks for both low- and high-income groups. For example, I identify the impact of high-income tax changes by comparing the responsive-

[1] Romer and Romer (2010) use the historical record (such as congressional records, economic reports, and presidential speeches) to identify tax changes that were implemented for more exogenous reasons such as pursuing long-run growth or deficit reduction. Doing so reinforces my ability to overcome endogeneity concerns. Online app. table A1 lists each tax change and how it is classified.

ness of employment growth in states like Connecticut to responsiveness in states with less exposure to high-income shocks. The empirical analysis has three components: (1) evidence of heterogeneous effects, (2) research design validation, (3) and mechanisms and discussion.

First, I find that state employment growth and economic activity are substantially more responsive to tax shocks for lower-income groups than to equally sized tax shocks for top earners. In particular, a 1 percent of state GDP tax cut for the bottom 90 percent results in roughly 3.4 percentage points of employment growth over a 2-year period. The corresponding estimate for the top 10 percent is 0.2 percentage points and is statistically insignificant. Other measures of state economic activity, such as state GDP, payrolls, and net earnings, respond similarly, in that they are very responsive to tax changes for the bottom 90 percent and unresponsive to tax changes for the top 10 percent.

Second, I provide several pieces of evidence to support the validity of these estimates. I build and use new state-level microsimulation models of social insurance programs (Aid to Families with Dependent Children [AFDC], Temporary Assistance for Needy Families [TANF], the Supplemental Nutrition Assistance Program [SNAP], Supplemental Security Income [SSI], and Medicaid) to show that the impacts of tax changes for lower-income groups do not reflect policy changes in social insurance programs. Event study evidence shows that tax shocks are not disproportionately favoring states that are doing poorly relative to how fast they normally grow. Similarly, differential state cyclicality as well as contemporaneous oil price shocks, interest rate shocks, or regional trends are not driving the results.

Third, in terms of mechanisms, I show how tax changes for different groups affect labor market outcomes and consumption. Tax changes for the bottom 90 percent have a much greater impact on both the extensive margin and intensive margin of labor supply than tax changes for the top 10 percent. Specifically, a 1 percent of state GDP tax increase for the bottom 90 percent lowers labor force participation rates by 3.5 percentage points and hours by roughly 2 percent. Tax changes of the same size for the top 10 percent have no detectable impact on these margins. State-level consumption also shows larger impacts for bottom 90 percent tax changes. These estimates on labor market outcomes and consumption are reduced-form effects on equilibrium outcomes that reflect changes in both supply and demand. I find that real wages increase after tax changes for lower-income groups. While the estimates are imprecise, they suggest that labor supply responses are an important mechanism for the results.

The empirical literature on these mechanisms—consumption and labor supply—is consistent with the possibility of heterogeneous aggregate effects of tax changes. One strand of evidence relates to heterogeneous consumption responses.[2] Many studies provide evidence that lower-income households tend to have higher marginal propensities to consume (McCarthy 1995; Parker 1999; Dynan, Skinner, and Zeldes 2004; Johnson, Parker, and Souleles 2006; Jappelli and Pistaferri 2010; Parker et al. 2013).[3] A second strand of evidence relates to tax policy and labor supply responses of different income groups. On the extensive margin for lower-income groups, Eissa and Liebman (1996) and Meyer and Rosenbaum (2001) show that the Earned Income Tax Credit has increased labor force participation.[4] For high-income earners, there is some evidence that the costs of raising taxes on top-income taxpayers in terms of labor supply and other margins may be limited (Saez, Slemrod, and Giertz 2012; Romer and Romer 2014) and largely reflect shifting in the timing or form of income (Auerbach and Siegel 2000; Goolsbee 2000). By focusing on the overall impacts of tax changes for different groups, this paper not only incorporates the effects of heterogeneous consumption responses but also provides evidence on the heterogeneous effects of supply-side policies that often do not assess the efficacy of tax changes for low-versus high-income groups.

The estimates in this paper build on the regional multiplier literature, which was recently 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.[5]

[2] Many macro papers, which often have consumption responses as a key channel, also 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 intertemporally, redistribution to borrowers also relaxes their borrowing constraint and results in a level of consumption that exceeds the amount by which 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. Galí, López-Salido, and Vallés (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.

[3] Note that not all papers (e.g., Shapiro and Slemrod 1995) find significant differences in spending responses as a function of income. More broadly, Chetty et al. (2014) estimate that approximately 85 percent of individuals are rule-of-thumb spenders. Saez and Zucman (2016) also show that total savings among the bottom 90 percent is roughly zero and has been flat since the 1980s.

[4] While evidence based on bunching (Heckman 1983; Saez 2010) suggests that intensive margin responses are small, other work, such as Kline and Tartari (2016), provides evidence that tax policy changes can lead to nontrivial intensive margin responses among low-income groups. Kosar and Moffitt (2016) provide evidence on the cumulative marginal tax rates of low-income households.

[5] 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 (2016) 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 Shoag (2010), Clemens and Miran (2012), and Wilson (2012).

This regional approach complements the approach of Mertens and Ravn (2013), 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 changes in tax liabilities 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 2014). In addition, the regional approach provides more power and variation in tax shocks for different groups, which enables me to separate and identify their effects on economic activity.

I. Data on Tax Changes and Economic Activity

A. Tax Data

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

1. National Tax Changes by Income Group

I use tax measures from NBER when possible and rely on the Statistics of Income tables to calculate changes before 1960.[6] 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 a 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. For every taxpayer, my measure subtracts how much she paid in 1992 from how much she would have paid in 1992 if the 1993 tax schedule had been in place.[7] When calculating tax liabilities, TAXSIM takes into account each individual's deductions and credits and their treatment under both the 1992 and 1993 tax schedules, resulting in a highly detailed measure of the mechanical, policy-induced change in tax liability at the individual tax return level.[8] After calculating a change in tax liability for each tax-

[6] See app. A.1.1 for a description of how I calculate the four pre-NBER tax changes, which affected tax liabilities in 1948, 1950, 1954, and 1960. This approach is similar to that of Barro and Redlick (2011), who focus on marginal rate changes rather than tax liability changes.

[7] See app. A.1.2 for more detail on the 1993 example tax change calculation.

[8] 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

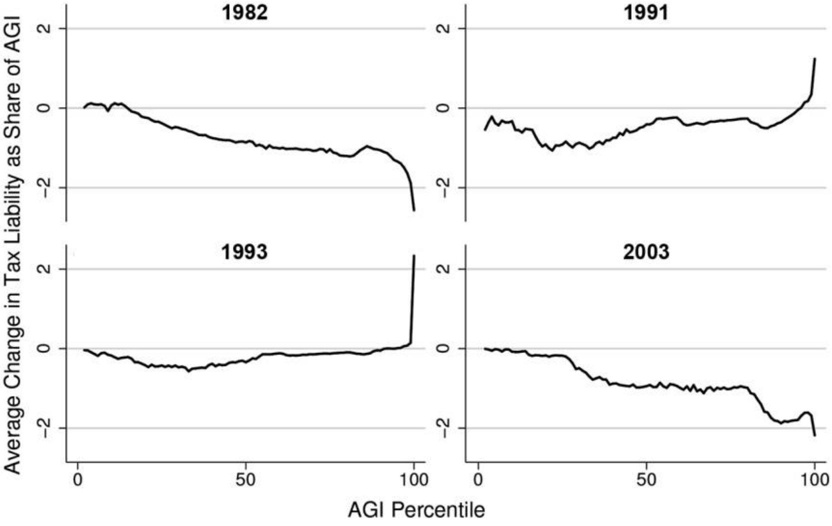

FIG. 1.—Selected historical tax changes for each adjusted gross income (AGI) percentile. This figure displays the average mechanical change in income and payroll tax liability for each tax return in TAXSIM from tax schedule changes as a share of AGI by AGI percentile for 1993 and for three other prominent years. For display purposes, it does not show results for the smallest AGI percentile (since the smallest income group result is amplified by a small denominator).

payer, I collapse the data by averaging them for every income percentile of AGI.

Figure 1 shows the results for four recent, prominent tax changes. On the basis of this measure of tax changes, 1993 taxpayers below median AGI received a modest tax cut of less than 1 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.

To compute total changes in income and payroll taxes in a given year, I multiply the average change in liability for each percentile by the number of returns in that percentile and then sum up each percentile's aggregate tax changes to obtain total tax changes for the bottom 90 percent and top 10 percent groups. I define tax shocks as a share of GDP, that is, $T_t^g \equiv \text{Tax Liability Change}_t^g/\text{GDP}_t$, where Tax Liability Change_t^g is the sum of mechanical changes in tax liability for those in income group $g \in \{\text{Bottom 90, Top 10}\}$ in year t. As a robustness check, I com-

(due to inflation indexing) is quite small in magnitude. Also, it is not obviously correct to weight old tax data by the Consumer Price Index (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.

pare my measure, that is, the sum of tax changes for the bottom 90 percent and top 10 percent, to the Romer and Romer (2010) total tax change measure. They are quite similar.[9] Differences between my aggregate measure and their measure are partially due to tax changes that did not affect income or payroll taxes, such as corporate income tax changes, and are defined accordingly: $T_{\text{NONINC}} \equiv T_{\text{ROMER}} - \Sigma_g T_t^g$.

Exogenous tax changes occurred in 31 years of the postwar period.[10] In exogenous years, the average income and payroll tax change was -0.16 percent of GDP, or roughly \$25 billion in 2011 dollars. It was -0.075 percent overall in the entire sample. On average, in exogenous years in which the top 10 percent taxpayers did not see a tax increase, the size of the tax cut for the bottom 90 percent and the top 10 percent was roughly the same. In exogenous years in which the top 10 percent 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 percent than for the bottom 90 percent. 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.

Panel A of 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.[11] Second, the magnitudes of tax changes for the top 10 percent 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 percent mostly came through payroll tax increases before 1980.

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. Panel B of figure 2 shows the average share of taxpayers who have incomes

[9] Figure A7 plots both series by year. The Romer tax change measure is at a quarterly frequency, so I sum their measure to construct an annualized version.

[10] Exogenous is defined as a year in which Romer and Romer (2010) show a nonzero tax change in which 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. For nonexogenous years, the tax change measure is set to zero. Table A1 lists exogenous tax changes used in this paper.

[11] On the basis of the logic of Frisch and Waugh (1933), a tax change that provides atypical changes to a given income group will influence estimates more strongly than proportionate tax changes. Figure A9 shows this point explicitly: years like 2003 provided disproportionately larger tax cuts to the top 10 percent given the size of the tax change for the bottom 90 percent.

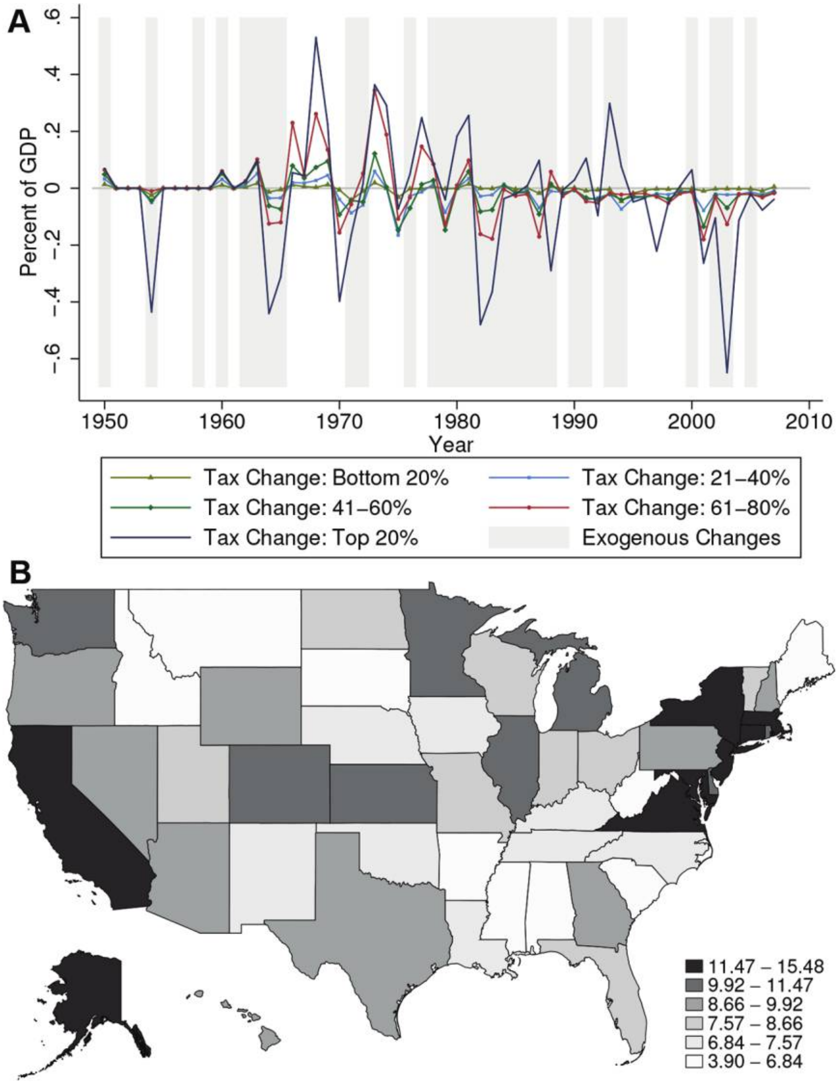

FIG. 2.—Federal tax changes by income group and heterogeneous high-income shares. *A*, Federal income and payroll tax changes by AGI quintile. *B*, Share of high-income taxpayers. This figure shows that there is both time-series and cross-sectional variation in tax changes by income group. Panel A displays changes in individual income and payroll tax liabilities by income quintile as a share of GDP from 1950 to 2007. Tax returns from TAXSIM are used to construct a tax change measure. The period 2008–11 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 (table A9 shows how each tax change is classified). Panel B 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 percent of AGI nationally, on average, from 1980 to 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 percent nationally in that year.

in the top 10 percent nationally for 1980–2007. On the basis of this measure, a taxpayer in Connecticut is roughly three times more likely to be in the top 10 percent than a taxpayer in Maine.

Similarly to the national changes, I define state tax shocks as a share of state GDP, that is, $T^g_{s,t} \equiv \text{Tax Liability Change}^g_{s,t}/\text{GDP}_{s,t}$, 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 percent means that a taxpayer's AGI is in the top 10 percent 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 Internal Revenue Service data.[12] This data limitation causes the first measure of tax changes to be approximated within TAXSIM for very high incomes at the state level.[13]

B. *Nontax Data*

1. Nontax Data at the State Level

The main measures of economic activity are employment and income. I use two measures of employment: the employment-to-population ratio and the number of people employed.[14] I also use two measures of state income: state GDP and net earnings. Net earnings (which is state personal income less personal government transfers and dividends, interest, and rents) provides a measure of income that nets out components that are less related to regional tax shocks.

A limitation of the income measures, however, is that they are in nominal terms, and converting them into real terms is difficult because state-level price indexes are imperfect. My preferred state price index is $P^{\text{ACCRA}}_{s,t}$, which is the average price index from the American Chamber of Com-

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

[13] Because of 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 US 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 percent of incomes fall below the censoring cutoff. In 2010, more than 95 percent of income earners still earned less than $200,000.

[14] I use the Current Population Survey (CPS) to construct employment-to-population ratios, the Bureau of Labor Statistics (BLS) for employment, and the Bureau of Economic Analysis (BEA) for GDP at the state level.

merce Researchers Association on cost of living in a state-year. It has been used in the local labor markets literature (e.g., Moretti 2013) to construct regional price indexes and is available for the full panel of states since 1980. I supplement this price index with $P_{s,t}^{\text{Moretti}}$, which follows the approach from Moretti (2013) to create a local price index based on state house prices and national CPI.[15]

To better understand mechanisms, I also analyze several labor market outcomes from the CPS at the state level: labor force participation, hours, wages, and real wages.[16] I focus on labor force participation to analyze extensive margin responses and on hours among full-time employed residents aged 25–60 to isolate intensive margin responses. Wages are wage income divided by hours among full-time workers. Finally, to remove the influence of compositional changes of labor market participants on average wages, I also construct composition-constant wages.[17] Appendix A.2 provides additional detail on variable sources and definitions. Real wages and real composition-constant wages are these nominal series divided by a price index, which is $P_{s,t}^{\text{ACCRA}}$ unless otherwise specified.

There are two main sets of controls. First, I include controls on oil prices and real interest rates from Nakamura and Steinsson (2014). Second, I use controls for contemporaneous policy and spending changes. I construct microsimulation models to measure social insurance policy changes in an analogous way to my tax shocks.[18] Specifically, I develop a state-specific, formula-driven mechanical change in spending for AFDC, TANF, SNAP, SSI, and Medicaid. I then divide each mechanical spending change by state GDP. To supplement these controls, I also control directly for several other policy parameters that are enumerated in data appendix A.3.

2. Nontax Data at the National Level

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 BEA's National Income and

[15] Moretti (2013) uses a local price index based on rental payments and national CPI, but rental payments are available only in 1980, 1990, and the 2000s, so I use state house prices from the Federal Housing Finance Agency in place of rental payments. Since house prices are asset prices that are forward looking, I prefer the $P_{s,t}^{\text{ACCRA}}$ measure but show results using $P_{s,t}^{\text{Moretti}}$ as well as $P_{s,t}^{\text{BLS}}$, which is a price index based on BLS city price indexes but is available only for roughly 20 cities. See data app. A.2 for details.

[16] I also provide supplemental evidence on payrolls, which are from the County Business Patterns, as well as employment rates. The employment rate is the share of people in the labor force who are employed.

[17] I follow the approach of Busso, Gregory, and Kline (2013) and Suárez Serrato and Zidar (2016) to construct composition-constant wages.

[18] Appendix C provides more detail on these microsimulation models.

Product Accounts table 1.1.3; the nominal GDP data come from the National Income and Product Accounts table 1.1.5.

II. Econometric Methods

This section describes how I estimate the relationship between changes in taxes for different groups and subsequent economic activity. First, I fit distributed lag models and direct projections to look at the dynamic relationship between (i) tax changes by income group and (ii) subsequent changes in economic activity at the state level. I then consider a more parsimonious specification that estimates the relationship between (i) 2-year changes in taxes by income group and (ii) 2-year changes in economic activity. Second, I study these relationships at the national level using a specification that is similar to that of Romer and Romer (2010) but has tax changes that are decomposed by income group. The national approach, while inherently noisy and suggestive because of limited data, supplements the state results by quantifying aggregate effects.

A. *State-Level Effects of Tax Changes for Different Income Groups*

1. Distributed Lag Model of Tax Changes for Different Income Groups

In a given state s and year t, changes in the outcome $y_{s,t}$ between years $t-1$ and t are decomposed into a state component μ_s, a time component δ_t, the effects of current and lagged tax shocks $T^g_{s,t}$ for income group g, an index of time-varying state characteristics $\mathbf{X}'_{s,t}\boldsymbol{\Lambda}$, and a residual component $\varepsilon_{s,t}$:

$$y_{s,t} - y_{s,t-1} = \sum_g \left(\sum_{m=\underline{m}}^{\bar{m}} \beta^{g,m} T^g_{s,t-m} \right) + \mathbf{X}'_{s,t}\boldsymbol{\Lambda} + \mu_s + \delta_t + \varepsilon_{s,t}, \tag{1}$$

where $g \in \{\text{Bottom 90, Top 10}\}$ indexes the income groups and the time index m for the lags of tax changes ranges from $\underline{m} = 0$ to $\overline{m} = 2$ in the baseline specification.[19] The term $T^{B90}_{s,t}$ is an exogenous tax shock as a share of state GDP for taxpayers who are in the bottom 90 percent of AGI nationally, and $T^{T10}_{s,t}$ is defined analogously. Tax shocks are expressed as a share of state GDP to facilitate comparisons over time.

For ordinary least squares to identify the parameters of interest, tax shocks need to be exogenous conditional on fixed effects and controls, that is, $\mathbb{E}(\varepsilon_{s,t} | T^{B90}_{s,t}, T^{T10}_{s,t}, \mathbf{X}_{s,t}, \mu_s, \delta_t) = 0$. Intuitively, this identifying assumption is that national tax shocks, which Romer and Romer (2010) define as

[19] Similar results with different lead and lag structures are also presented in the appendix.

exogenous, are not disproportionately favoring states that are doing poorly relative to how fast they normally grow. The validity of comparing outcomes of states with different income distributions relies on three key assumptions: (1) state tax shocks are exogenous, (2) targeted tax shocks are unrelated to targeted spending shocks, and (3) outcomes from less exposed states provide a reasonable counterfactual in the absence of the tax shock.

Since I control for state and year fixed effects in equation (1), the first assumption maintains that federal policy makers 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 policy makers are responding to idiosyncratic state shocks since the Romer and Romer changes are due to concerns about long-run aggregate growth and inherited budget deficits.[20]

Even if state tax shocks are exogenous, they may occur at the same time as other progressive policy changes. If progressive tax and spending 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 percent and the effects of spending policies, resulting in upwardly biased estimates. To address this concern, I directly control for government transfer payments as well as specific policy parameters. I first control for a comprehensive measure of total government spending on transfer programs, but this amount of spending responds to economic conditions. To isolate changes in policy parameters from changes in economic conditions, my preferred approach is to control for mechanical policy-induced changes in social insurance program spending. I include the mechanical policy-induced spending changes of several key transfer programs in the vector of controls $\mathbf{X}_{s,t}$ in the baseline specification and then present estimates that control for additional policy parameters in robustness specifications.

I provide several pieces of evidence to support the third assumption that outcomes from less exposed states provide a reasonable counterfactual in the absence of the tax shock. I consider the possibility that states that disproportionately benefit from a given tax change may be generally more cyclical. I do so by replacing year fixed effects δ_t in equation (1) with $\delta_{q(s),t}$, where $\delta_{q(s),t}$ is each state's cyclicality-quintile-specific year fixed effect. The function $q(s): \{\text{AL}, \text{AK}, \ldots, \text{WY}\} \rightarrow \{1, \ldots, 5\}$ gives the quintile of the state's sensitivity to national changes in economic conditions. I present a few ways to measure how cyclically sensitive each state is, but the baseline approach follows the β-differencing approach of Blanchard

[20] 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 partialing out macro aggregates.

and Katz (1992), which regresses changes in state economic activity on national changes in economic activity to estimate the state's average responsiveness to national shocks.[21] The resulting group-by-year fixed effect $\delta_{q(s),t}$ measures common year shocks in the 10 states with similar levels of cyclicality. Additionally, I consider regional trends as well as other controls used in the regional multiplier literature (e.g., state-specific trends and state-specific interest rate and oil price sensitivity). I provide further support for the third assumption by examining the path of economic activity preceding tax shocks for bottom- and top-income groups.

2. Direct Projections of Tax Changes for Different Income Groups

To examine how the path of economic activity evolves before and after tax shocks for bottom- and top-income groups, I run a series of direct projection regressions for different horizons $h \in \{-4, -3, \ldots, 5\}$:

$$y_{s,t+h} - y_{s,t-1} = \alpha_h^{B90}\left(T_{s,t}^{B90}\right) + \alpha_h^{T10}\left(T_{s,t}^{T10}\right) + \mathbf{X}'_{s,t}\boldsymbol{\Lambda}_h + \mu_{s,h} + \delta_{t,h} + \varepsilon_{s,t,h}, \quad (2)$$

where s and t index state and year, $y_{s,t+h} - y_{s,t-1}$ is a measure of growth in economic activity at horizon h, and $\mu_{s,h}$ and $\delta_{t,h}$ are horizon-specific state and year fixed effects.[22] The path of economic activity around the tax shocks for bottom- and top-income groups is described by the sequences of coefficients $\{\alpha_h^{B90}\}_{h=-4}^{h=5}$ and $\{\alpha_h^{T10}\}_{h=-4}^{h=5}$, which quantify the impacts of these shocks on economic activity over different horizons. As noted by Jorda (2005), Stock and Watson (2007), and Auerbach and Gorodnichenko (2013), using direct projections of tax shocks on outcomes is attractive because it does not impose dynamic restrictions on the estimates at different horizons. I use these specifications to estimate average outcomes before tax shocks to determine if tax shocks for different groups occur soon after unusually good or bad economic times. 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.

[21] See app. B.1 for details. I also show results using deciles instead of quintiles and using quintiles of each state's standard deviation in real GDP per capita, $\sigma_{s,1963\text{–}79}$, in the years preceding the sample period 1980–2007.

[22] In the baseline specification, I use cyclicality-quintile year fixed effects described in the prior section, i.e., $\delta_{q(s),t}$ formed using the β-differencing approach of Blanchard and Katz (1992), which are indexed by the horizon, i.e., $\delta_{q(s),t,h}$. I also include the mechanical policy-induced spending changes of several key transfer programs in the vector of controls $\mathbf{X}_{s,t}$ in the baseline specification as well. Specifically, the five distinct policy controls are the mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a percentage of state GDP.

3. Two-Year Effects of Tax Changes for Different Income Groups

While the direct projection specifications are useful for examining how economic activity evolves around a tax change, I fit more parsimonious models that use 2-year changes to show the cumulative effects of tax changes on employment and income for different income groups.[23] The 2-year specification follows a specification similar to that of Nakamura and Steinsson (2014), but for tax shocks (by income group) rather than for government spending shocks:

$$\frac{Y_{s,t} - Y_{s,t-2}}{Y_{s,t-2}} = b^{B90}\left(\sum_{m=0}^{2} T_{s,t-m}^{B90}\right) + b^{T10}\left(\sum_{m=0}^{2} T_{s,t-m}^{T10}\right) + \mathbf{X}_{s,t}'\boldsymbol{\Lambda} + a_s + d_t + e_{s,t}. \quad (3)$$

In this case, the year fixed effects d_t absorb common aggregate macroeconomic shocks and the state fixed effects effectively control for different state trends in the outcome. An advantage of this specification is that the average effects of tax changes are captured by one parameter for each income group (rather than a parameter for each lag of each income group). I use $d_{q(s),t}$ instead of d_t in the baseline specification (where $d_{q(s),t}$ is each state's cyclicality-quintile-specific year fixed effect) and also control for mechanical policy-induced spending changes.

B. *National Effects of Tax Changes for Different Income Groups*

I also fit specifications similar to equation (1) at the national level:

$$y_t - y_{t-1} = \sum_{m=\underline{m}}^{\overline{m}} \left(\gamma^{B90,m} T_{t-m}^{B90} + \gamma^{T10,m} T_{t-m}^{T10} + \mathbf{X}_{t-m}'\boldsymbol{\Gamma}_m\right) + \nu_t, \quad (4)$$

where $\gamma^{B90,m}$ and $\gamma^{T10,m}$ are the effects of changes in taxes as a share of GDP at lag m and the time index m for the lags of tax changes ranges from $\underline{m} = 0$ to $\overline{m} = 2$ in the baseline specification. The term T_t^{B90} is an exogenous tax shock as a share of national GDP for taxpayers who are in the bottom 90 percent of AGI nationally, and T_t^{T10} is defined analogously. The term $\mathbf{X}_t = [T_{\text{NONINC},t}]$ includes non–income tax and non–payroll tax changes that Romer and Romer (2010) classify as exogenous (e.g., corporate tax changes). One way to interpret equation (4) is that it decomposes the Romer and Romer exogenous tax change measure into three mutu-

[23] Note that each of the elements of the tax shock is normalized by the initial level of state GDP (i.e., $Y_{s,t-2}$). There is nothing special about 2-year changes per se other than that this duration is somewhat standard in this literature (e.g., Nakamura and Steinsson 2014).

ally exclusive and collectively exhaustive components: T_t^{B90}, T_t^{T10}, and the nonincome and nonpayroll portion, that is, $T_{\mathrm{NONINC},t}$.

III. Effect of Tax Changes for Different Income Groups

This section provides results on the effects of tax changes for different income groups on economic activity. Section III.A provides evidence on the effects of tax changes for different groups on employment and income growth. Section III.B provides results for mechanisms and highlights supplemental national results. Section III.C discusses the estimates and relates them to existing evidence. Finally, Section III.D briefly describes additional support for the validity of the estimates and robustness tests.

A. *Impacts on State Economic Activity*

Figure 3 shows the evolution of the state employment-to-population ratio and state employment relative to the year before a tax change for different income groups. Panel A shows that the employment-to-population ratio exhibits little trend prior to tax changes and then gradually falls in the years following a tax change for the bottom 90 percent. Specifically, the estimates for the impact of tax changes in year h for the bottom 90 percent, $\hat{\alpha}_h^{B90}$ from equation (2), and those for the top 10 percent, $\hat{\alpha}_h^{T10}$, are shown in blue and red, respectively. The employment-to-population ratio is roughly 4 percentage points lower 3 years after a 1 percent of state GDP tax change for the bottom 90 percent relative to the employment-to-population ratio the year before the tax change (i.e., $\hat{\alpha}_3^{B90} \approx 4$). After 4 years, on average, the ratio improves slightly to be roughly 3 percentage points below the level prior to the tax change. Panel B shows similar patterns for state employment. State employment tends to be 2 percent lower in the year after the tax change for the bottom 90 percent, falls to 4 percent 2 years after the change, and then recovers somewhat to be roughly 2 percent lower 4 years after the tax change. Tax changes for the top 10 percent, in contrast, have no detectable impact on the state employment-to-population ratio and state employment in the 8-year window around tax changes.

Figure 4 shows the evolution of the state income and prices. Panel A shows that nominal state GDP sharply declines following tax changes for the bottom 90 percent and is roughly 8 percent lower than the year before the tax change. These declines are very large.[24] However, panel B shows that prices also fall by roughly 6 percent. This price decline estimate is noisy but indicates that the GDP declines are smaller in real terms. Panels C

[24] I discuss the magnitudes and relate them to existing literature in Sec. III.C.

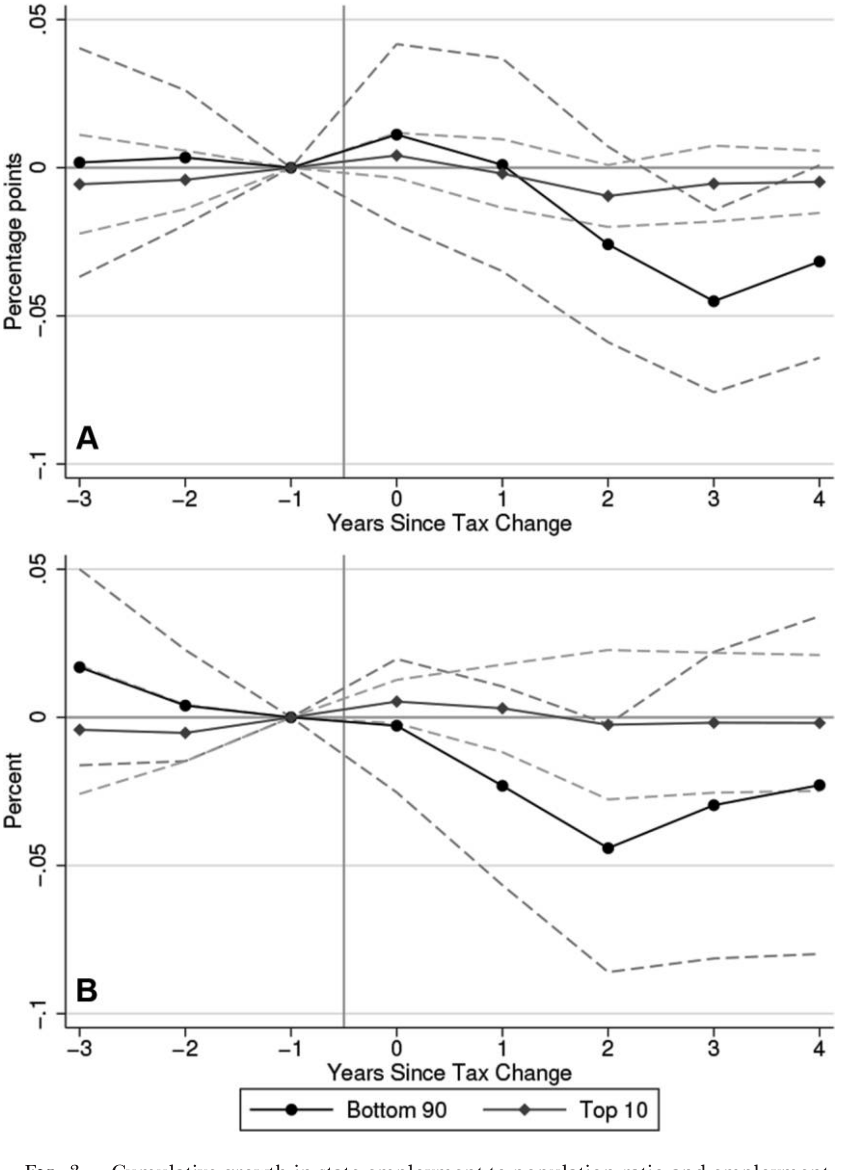

FIG. 3.—Cumulative growth in state employment-to-population ratio and employment. *A*, Employment-to-population ratio. *B*, Employment. This figure shows event studies of a 1 percent of GDP tax increase on the state employment-to-population ratio and employment for those with AGI in the bottom 90 percent nationally and for those with AGI in the top 10 percent nationally. Specifically, the figure plots the estimates from the baseline specification of equation (2) for the impact of tax changes in year h for the bottom 90 percent, $\hat{\alpha}_h^{B90}$, and the top 10 percent, $\hat{\alpha}_h^{T10}$. The baseline specification includes controls for mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a percentage of state GDP, as well as cyclicality-quintile year fixed effects. See Section II for details. Standard errors are robust and are clustered by state; 95 percent confidence intervals are shown as dashed lines. The sample period is 1980–2007.

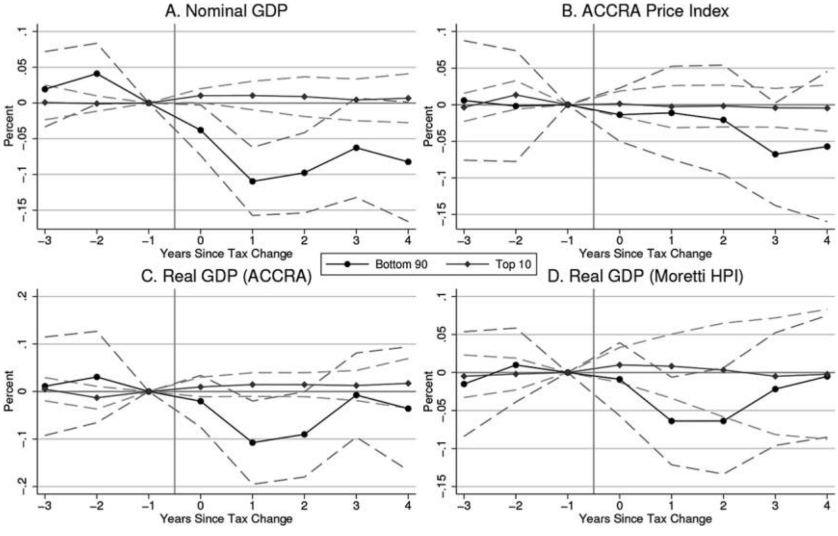

FIG. 4.—Cumulative growth in state economic activity. This figure shows event studies of a 1 percent of GDP tax increase on outcomes for those with AGI in the bottom 90 percent nationally in blue and for those with AGI in the top 10 percent nationally in red. These outcomes are (*A*) nominal state GDP, (*B*) the ACCRA state price index $P_{s,t}^{\mathrm{ACCRA}}$, (*C*) real state GDP using $P_{s,t}^{\mathrm{ACCRA}}$, and (*D*) real state GDP using $P_{s,t}^{\mathrm{Moretti}}$. Specifically, the figure plots the estimates from the baseline specification of equation (2) for the impact of tax changes in year h for the bottom 90 percent, $\hat{\alpha}_h^{B90}$, and the top 10 percent, $\hat{\alpha}_h^{T10}$. The baseline specification includes controls for mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a percentage of state GDP, as well as cyclicality-quintile year fixed effects. See Section II for details. Standard errors are robust and are clustered by state; 95 percent confidence intervals are shown as dashed lines. The sample period is 1980–2007.

and D show results for real GDP using the ACCRA price index $P_{s,t}^{\mathrm{ACCRA}}$ and a home price–based index $P_{s,t}^{\mathrm{HPI}}$. The real series show smaller impacts, especially 3 and 4 years after the tax changes for the bottom 90 percent. In terms of estimates from tax changes for the top 10 percent, estimates for both measures of income in nominal and real terms provide no evidence that tax changes for high-income earners materially affect economic activity over a business cycle frequency.[25]

Table 1 presents the main regression estimates of state employment and income. Panel A shows estimates of the distributed lag specification using equation (1) as well as the sum of effects $\Sigma_{m=0}^{2}\beta^{g,m}$ of tax changes for each group $g \in \{\text{Bottom 90, Top 10}\}$. Panel B shows estimates from the more parsimonious 2-year change specification using equation (3). For

[25] While it is possible that the effects show up further into the future, detecting such effects is inherently difficult. See Romer and Romer (2014) for some historical evidence on longer-term effects.

each panel, the baseline specification is a rich set of controls: mechanical policy changes in spending as a share of state GDP on social insurance programs (AFDC, TANF, SNAP, SSI, and Medicaid) as well as state and cyclicality-quintile by year fixed effects. Employment declines roughly 3.5 percent in both specifications following a tax change of 1 percent of state GDP for the bottom 90 percent, and top tax changes have no impact in either specification. Panel B also reports the p-value for the test that $b^{B90} = b^{T10}$, that is, that the impacts on 2-year employment growth from tax changes for both groups are equal. This test is rejected with 94 percent confidence in column 1. The employment-to-population ratio also shows similar patterns but is less precise over a 2-year window relative to 3 and 4 years after the tax change as shown in figure 3. The next three columns show estimates for nominal and real state GDP. The impacts are very large for the bottom 90 percent and not for the top 10 percent. Although the point estimates for state GDP are less stable and range from 5.3 percent to 9.2 percent, the qualitative pattern of nearly all responsiveness from lower-income groups and small impacts from top groups is very robust.[26] Each specification rejects the null hypothesis of equal impacts from tax changes for the bottom 90 percent and top 10 percent with more than 99 percent confidence.

B. Mechanisms

The results in Section III.A show large employment and income declines after tax changes affecting lower-income taxpayers. These employment and income results are reduced-form estimates that reflect changes in both the supply of and demand for labor following a tax change. This section discusses impacts on labor market outcomes and on consumption, the relative importance of supply and demand changes at the state level, and effects on aggregate investment.

Figure 5 shows the impacts of tax changes for different groups on extensive and intensive labor market responses, real wages, and consumption. On the extensive margin, panel A shows that labor force participation rates decline roughly 3 percentage points 3 and 4 years after a tax change for the bottom 90 percent. On the intensive margin, hours of workers who work at least 48 weeks decline by roughly 2 percent soon after the tax change but return to the levels before the tax change.[27] Panel C shows that real wages increase following tax changes for the

[26] Tables A8 and A9 show robustness tests for nominal state GDP. Tables A10 and A11 show robustness tests for real state GDP.

[27] Results are similar for hours of workers who work, on average, at least 35 hours per week and at least 48 weeks per year.

TABLE 1

State-Level Effects of Tax Changes for Different Income Groups

	Employment (1)	Employment/ Population (2)	Nominal GDP (3)	Real GDP (ACCRA) (4)	Real GDP (Moretti) (5)
	A. Distributed Lag Model of Tax Changes for Different Income Groups				
B90	.33	.97	−2.05	−.29	.86
	(1.22)	(1.59)	(2.18)	(2.83)	(2.79)
B90 1-year lag	−1.87**	−1.23	−7.39***	−8.40***	−5.96***
	(.89)	(1.34)	(1.34)	(2.85)	(1.87)
B90 2-year lag	−1.98	−2.48*	.25	2.11	−.17
	(1.25)	(1.35)	(1.44)	(2.11)	(2.05)
T10	.52	.47	1.05*	.91	1.00
	(.45)	(.40)	(.58)	(.99)	(1.08)
T10 1-year lag	−.24	−.66	.10	.88	−.11
	(.60)	(.41)	(.70)	(.61)	(1.25)
T10 2-year lag	−.61	−.80	−.12	−.12	−.41
B90 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−3.52	−2.74	−9.19***	−6.59	−5.27
	(2.28)	(1.68)	(3.40)	(4.97)	(4.44)
T10 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−.33	−.99	1.03	1.67	.48
	(1.57)	(.65)	(1.56)	(1.33)	(3.41)
Bottom − top	−3.19	−1.74	−10.22**	−8.26	−5.75
	(3.14)	(1.87)	(3.96)	(5.10)	(4.57)

			B. 2-Year Changes		
Bottom 90	−3.42**	−1.53	−11.98***	−9.70***	−7.00**
	(1.54)	(.96)	(2.21)	(3.46)	(2.88)
Top 10	.22	−.24	.75	1.44*	.56
	(.90)	(.33)	(.96)	(.85)	(2.17)
p-value (bottom 90 = top 10)	.06	.23	.00	.00	.03

NOTE.—Panel A presents estimates of the effects of tax changes for different income groups $g \in \{\text{Bottom 90, Top 10}\}$ at different lags m from the following specification:

$$y_{s,t} - y_{s,t-1} = \alpha_s + \delta_{q(s),t} + \sum_g \left(\sum_{m=0}^{2} \beta^{g,m} T^g_{s,t-m} \right) + \mathbf{X}'_{s,t}\mathbf{\Lambda} + \varepsilon_{s,t},$$

where $y_{s,t}$ is the log outcome (for all outcomes other than the employment-to-population ratio, which is measured in percentage terms), α_s is a state fixed effect, $\delta_{q(s),t}$ is each state's cyclicality-quintile-specific year fixed effect where $q(s):\{\text{AL, AK, ..., WY}\} \to \{1, \dots, 5\}$ is the quintile of the state's sensitivity to national changes in economic conditions (see Sec. II.A.1 or app. B.1 for details), $T^{B90}_{s,t}$ is an exogenous tax shock as a share of state GDP for taxpayers who are in the bottom 90 percent of AGI nationally, $T^{T10}_{s,t}$ is defined analogously, and $\mathbf{X}_{s,t}$ includes controls for mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a percentage of state GDP with the same lag structure as the tax changes (i.e., current values, 1-year lags, and 2-year lags). Similarly, panel B presents estimates of the effects of tax changes for the bottom 90 percent, b^{B90}, and the top 10 percent, b^{T10}, from the following specification:

$$\frac{Y_{s,t} - Y_{s,t-2}}{Y_{s,t-2}} = a_s + d_{q(s),t} + b^{B90}\left(\sum_{m=0}^{2} T^{B90}_{s,t-m} \right) + b^{T10}\left(\sum_{m=0}^{2} T^{T10}_{s,t-m} \right) + \mathbf{X}'_{s,t}\mathbf{\Lambda} + e_{s,t},$$

where $Y_{s,t}$ is the level of the outcome, $(Y_{s,t} - Y_{s,t-2})/Y_{s,t-2}$ is 2-year growth in the outcome, a_s is a state fixed effect, $d_{q(s),t}$ is each state's cyclicality-quintile-specific year fixed effect, $\Sigma^2_{m=0} T^g_{s,t-m}$ is the change in taxes over the last 2 years, and $\mathbf{X}_{s,t}$ includes controls for mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a share of state GDP defined analogously as the tax shocks. For the employment-to-population ratio, the outcome is the simple difference, i.e., $Y_{s,t} - Y_{s,t-2}$. The p-values for the null hypothesis that the effects of tax changes for the bottom 90 percent and top 10 percent are the same, i.e., $b^{B90} = b^{T10}$, are presented in the last row of panel B. Standard errors are clustered by state. Data are at the state-year level from 1980–2007. See app. A.2 for data definitions and sources.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

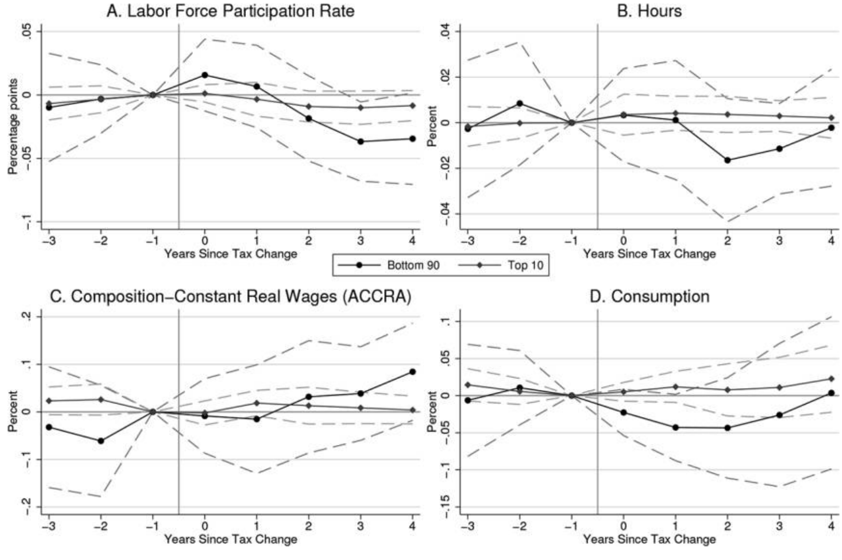

FIG. 5.—Cumulative growth in state labor market outcomes and consumption. This figure shows event studies of a 1 percent of GDP tax increase on outcomes for those with AGI in the bottom 90 percent nationally and for those with AGI in the top 10 percent nationally. These outcomes are (*A*) labor force participation rate (in percentage points), (*B*) mean hours worked among those who have worked at least 48 weeks in the past year, (*C*) real composition-constant average wages using $P_{s,t}^{\mathrm{ACCRA}}$, and (*D*) state consumption. Specifically, the figure plots the estimates from the baseline specification of equation (2) for the impact of tax changes in year h for the bottom 90 percent, $\hat{\alpha}_h^{B90}$, and the top 10 percent, $\hat{\alpha}_h^{T10}$. The baseline specification includes controls for mechanical changes in AFDC, TANF, SNAP, SSI, and Medicaid spending as a percentage of state GDP, as well as cyclicality-quintile year fixed effects. See Section II for details. Standard errors are robust and are clustered by state; 95 percent confidence intervals are shown as dashed lines. The sample period is 1980–2007 for panels A–C. However, state consumption is available only since 1997, so the sample period for panel D is 1997–2007.

bottom 90 percent.[28] These real wage results, though imprecise, reveal the relative importance of supply and demand changes in the labor market. The increase in real wages suggests that supply-side responses are important and may exceed demand-side responses to tax changes for the bottom 90 percent.

In terms of aggregate mechanisms, table 2 shows national results for real GDP and its components. Real GDP decreases 3.8 percent following tax changes for the bottom 90 percent and decreases 1.1 percent following

[28] Nominal wages tend to be flat but then increase following tax changes for the bottom 90 percent. Panel C uses the ACCRA price index $P_{s,t}^{\mathrm{ACCRA}}$ as a deflator and adjusts wages holding constant the composition of workers, which indicates that the real wage increases are reflecting actual increases rather than compositional shifts in labor supply. Results using other deflators and raw average wages are similar and are presented in fig. A14.

TABLE 2
National Effects of Tax Changes for Different Income Groups

	GDP	Investment	Residential Investment	Consumption	Durable Consumption	Nondurable Consumption
B90	.66	−1.16	−15.01	−.53	−2.62	−.20
	(1.61)	(7.68)	(11.33)	(1.41)	(5.55)	(1.41)
B90 1-year lag	−3.24	−15.00*	−16.54	−1.87	−8.64	−.86
	(2.17)	(8.54)	(14.49)	(1.69)	(5.83)	(1.41)
B90 2-year lag	−1.21	−11.15	1.50	−.41	−4.92	−.52
	(1.99)	(8.41)	(13.51)	(1.82)	(6.75)	(1.42)
T10	1.78	6.89	2.47	−.24	−.41	−1.25
	(2.61)	(8.10)	(12.27)	(1.95)	(6.40)	(1.69)
T10 1-year lag	−2.13	−7.77	−4.53	−2.28	−6.31	−2.45
	(2.54)	(11.39)	(12.84)	(2.23)	(8.73)	(2.06)
T10 2-year lag	−.76	1.24	11.83	−.66	3.76	−1.40
	(1.26)	(5.34)	(10.08)	(1.25)	(5.41)	(1.41)
B90 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−3.78	−27.30*	−30.04	−2.80	−16.19	−1.58
	(3.33)	(15.55)	(19.40)	(2.74)	(10.98)	(2.72)
T10 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−1.12	.36	9.76	−3.18	−2.96	−5.11
	(4.64)	(18.84)	(24.93)	(4.12)	(15.45)	(4.23)
Bottom − top	−2.67	−27.66	−39.80	.38	−13.23	3.53
	(6.64)	(29.24)	(33.39)	(5.77)	(22.45)	(6.18)

Note.—This table presents estimates of the effects of tax changes for different groups $g \in \{\text{Bottom 90, Top 10}\}$ at different lags m at the national level from the following specification:

$$y_t - y_{t-1} = \sum_{m=\underline{m}}^{\bar{m}} \left(\gamma^{B90,m} T_{t-m}^{B90} + \gamma^{T10,m} T_{t-m}^{T10} + \mathbf{X}_{t-m}' \mathbf{\Gamma}_m\right) + \nu_t,$$

where y_t is the log outcome, T_t^{B90} is an exogenous tax shock as a share of national GDP for taxpayers in the bottom 90 percent of AGI nationally, and T_t^{B90} is defined analogously. The vector $\mathbf{X}_t = [T_{\text{NONINC},t}]$ includes non–income and non–payroll tax changes that Romer and Romer (2010) classify as exogenous. Robust standard errors are reported in parentheses. The sample period is 1950–2007. See app. A.2 for data sources.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

tax changes for the top 10 percent. These point estimates are noisy—the standard error for the top 10 percent estimate is 4.6 percent at the national level—but could be consistent with impacts of tax changes from the top 10 percent that spill over to other states. That said, the impacts on the top 10 percent are statistically indistinguishable from zero and 2.7 percentage points lower than the aggregate estimate for the bottom 90 percent. The components of GDP are also noisy.[29] Other than the impacts on investment, which are much more responsive to tax changes for the bottom 90 percent and are weakly significant statistically, there is not enough variation in the time series to pin down heterogeneous effects on macro aggregates.[30] The investment responses and the overall real GDP point estimates, however, suggest that the effects of additional economic growth from tax changes for the bottom 90 percent tend to exceed the effects from income changes among those who are more likely to save.

C. *Discussion of Results*

Quantitatively, the main reduced-form results in this paper are large, but within a range that is consistent with existing cross-sectional evidence. In particular, the 3.4 percent estimate for the increase in state employment from a 1 percent of GDP tax cut for the bottom 90 percent translates to roughly \$31,500 per job.[31] These cost-per-job estimates are consistent with those reported in Ramey (2011): \$25,000 in Wilson (2012), roughly \$28,600 in Chodorow-Reich et al. (2012), \$30,000 in Suárez Serrato and Wingender (2011), and \$35,000 in Shoag (2010).[32] My estimates for the impact of tax cuts for the top 10 percent 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 percent of GDP that goes

[29] Given the limited number of tax change events in the postwar period, the possibility of coincidental trends in income inequality, e.g., suggests caution when interpreting the national results and provides another reason why evidence from the state-level analysis, especially when the analysis accounts for regional trends, may be more informative.

[30] The consumption results are somewhat mixed. Although durable good consumption is much more responsive to bottom 90 percent tax changes, the nondurable consumption estimates work in the opposite direction, leading to similar overall consumption impacts. The similarity in consumption impacts is inconsistent with the literature on marginal propensities to consume (MPCs) and the state-level results in fig. 4, which show much larger responses from the bottom 90 percent on consumption.

[31] Using 2011 numbers, the cost of a 1 percent of GDP tax cut is roughly \$150 billion and a 3.4 percent increase in employment on a base of \$140 million is \$4.76 million. Therefore, the cost per job is \$150,000 M/4.76 M = \$31,513.

[32] Note that Chodorow-Reich et al. (2012) and Wilson (2012) focus on effects during a recession, which likely results in lower cost-per-job estimates. There are also estimates of smaller multipliers (e.g., Clemens and Miran 2012). See Chodorow-Reich (2017) for a recent survey.

half to the bottom 90 percent and half to the top 10 percent will have roughly a \$63,000 cost per job.

The estimates for impacts on real income, however, are larger than those of most papers in this literature.[33] First, the variation that I am exploiting could potentially yield stronger effects than prior studies. Second, the confidence intervals are large, so one cannot rule out smaller effects. Third, in terms of point estimates, the average output multiplier in a recent survey by Chodorow-Reich (2017) is 2.1, though some studies estimate sizable cumulative output multipliers (e.g., Leduc and Wilson [2015] estimate a cumulative multiplier of 6.6). The estimated impact on real income from the bottom 90 percent depends on the specification but is roughly 7.[34] The impact from the top 10 percent is roughly 0, so the overall multiplier on real income, computed as the average of the group-specific multipliers, is roughly 3.5. It is important to emphasize that these estimates are regional multipliers, which can differ from national multipliers to the extent that time fixed effects absorb general equilibrium forces (e.g., countercyclical monetary policy).[35] Since state GDP, particularly in real terms, is measured with error,[36] my preferred interpretation of these results is that the point estimates for real income are more variable and thus less reliable than the employment estimates, but impacts on both outcomes provide robust evidence that economic activity is substantially more responsive to tax changes for the bottom 90 percent than to those for the top 10 percent. Okun's law suggests that employment and GDP are closely related, so putting emphasis on the better measured of the two seems advantageous.

In terms of mechanisms and the relative importance of consumption and labor supply responses, rationalizing the large responses in economic activity through consumption responses alone is not persuasive. First, the traditional multiplier of $\text{MPC}/(1 - \text{MPC})$ would require marginal propensities to consume that are larger than most MPCs estimated in the lit-

[33] Nakamura and Steinsson (2014), e.g., find output multipliers from government spending of 1.32 to 4.79 in their table 3 and roughly similar estimates for output multipliers in real terms.

[34] See, e.g., fig. 4, panels C and D, or the real income estimates in table 1 or tables A6 and A7. Other measures of income, e.g., total personal income from the CPS, increase by roughly 5 percent as shown in fig. A21, but these estimates are noisy.

[35] Although regional multipliers are generally believed to be larger than national multipliers, the relative size of regional and national multipliers is an active area of research (Chodorow-Reich 2017). It is also worth noting that common national shocks like countercyclical monetary policy are not likely to be fully absorbed by time fixed effects given regional heterogeneity and the possibility of heterogeneous impacts of monetary policy changes.

[36] The BEA relies on measures from a range of sources when computing state GDP, many of which are from the economic census. The economic census is compiled every 5 years, and in nonbenchmark years, state GDP estimates involve "interpolation and extrapolation techniques using indicator series that mirror the movement in the GDP by state component being estimated." See http://www.bea.gov/sites/default/files/methodologies/0417_GDP_by_State_Methodology.pdf.

erature (e.g., Johnson et al. 2006; Parker et al. 2013). Second, in terms of heterogeneous MPCs by income group, the initial impact on consumption could be sizable;[37] but the subsequent rounds do not feed back exclusively to lower-income groups, so the MPCs in subsequent rounds are not the MPCs of lower-income consumers, but economywide average MPCs. Third, to the extent that some of the initial spending is on durable goods, which are often traded, the impacts from increased consumption may not be especially concentrated in the states where tax change recipients live (other than through spillovers to the consumption of complementary nontradables). Substantial labor supply responses, therefore, are likely an important mechanism, which is consistent with the evidence presented on labor force participation, hours, and real wages.

One may find these results surprising from the perspective of the theoretical literature. Although the employment estimates are comparable to those in the empirical literature on regional multipliers, it may be somewhat surprising from the perspective of the theoretical literature that tax cuts for lower-income earners are more effective than government spending.[38] Farhi and Werning (2016), however, show that externally financed regional multipliers with redistribution and non-Ricardian agents can be larger than traditional multipliers. Additionally, other channels, such as extensive margin labor supply responses with heterogeneous agents, are often not incorporated and can affect conclusions about multipliers.

The results may also be surprising in terms of Ricardian equivalence. Ricardian agents will increase expenditures on the basis of the annuity value of the tax change, which may be zero if they expect to finance the tax change in the future.[39] However, there are a few reasons why Ricardian equivalence may fail, especially when considering tax changes for lower-income groups in a spatial setting. First, agents may consider tax changes a transfer (i) if the tax change is financed contemporaneously by other agents (from other locations or from other income groups) or (ii) if they expect others to pay for it in the future. Second, agents may be liquidity

[37] Aaronson, Agarwal, and French (2012) show that household spending increases by roughly \$700 per quarter following a \$250 per quarter income increase due to minimum wage increases. This $700/250 \approx 3X$ 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.

[38] MPC estimates are typically smaller than one, and the traditional government spending multiplier is $1/(1-\text{MPC})$, so the traditional tax multiplier is smaller than the traditional government spending multiplier; i.e., $\text{MPC} < 1$ implies that

$$\frac{\text{MPC}}{1-\text{MPC}} < \frac{1}{1-\text{MPC}}.$$

[39] This discussion of Ricardian equivalence draws from the discussion of Ricardian equivalence and regional multipliers in Chodorow-Reich (2017).

constrained. Third, agents may be myopic. These considerations may also help explain why there are different impacts for different income groups.

Finally, these estimates have implications for the budgetary consequences of tax reforms and dynamic scoring debates. At the national level, we can use estimates from table 2 to compute back-of-the-envelope calculations for three reforms: a tax cut of 1 percent of GDP on the top 10 percent, a 1 percent of GDP income tax cut, and a 1 percent of GDP payroll tax cut. These calculations require a few inputs: GDP, federal tax revenue as a share of GDP, the share of income and payroll tax liabilities paid by a group, and the cumulative effects of tax cuts on GDP for different groups. Table 3 lists the calibrated values and sources for each.

The first policy, which cuts income taxes by 1 percent of GDP for the top 10 percent, has a mechanical budget impact of −\$195 billion. However, on the basis of the cumulative effect estimates $\hat{\beta}^{T10} = 0.0112$, this tax reform would increase the level of GDP by \$218 billion, 17 percent of which (or \$37 billion) is additional federal tax revenue; so on net, the effect on the budget is −\$162 billion.[40] An across-the-board income tax cut, and especially an across-the-board payroll tax cut, are more favorable in terms of budgetary impacts because of higher growth from tax changes for the bottom 90 percent.[41] Since the estimates of the cumulative effects are noisy at the national level, these back-of-the-envelope calculations are rough estimates; we cannot reject the null of zero dynamic effects of these reforms at the national level.[42] Moreover, extrapolation of linear effects from small tax changes should be done cautiously.

[40] Specifically, the fiscal impact $\Delta\text{Rev} \approx -162\text{B}$ is the difference between the mechanical budget effects, $\Delta\text{Rev}^M = -.01 \times \text{GDP} = -195\text{B}$, and the dynamic effects due to changes in economic growth,

$$\Delta\text{Rev}^D = \hat{\beta}^{T10} \times \text{GDP} \times s^R = .0112 \times 19{,}500\text{B} \times .17 \approx 37\text{B}.$$

[41] The fiscal impact of a 1 percent of GDP income tax cut is the difference between $\Delta\text{Rev}^M = -195\text{B}$ and

$$\begin{aligned}\Delta\text{Rev}^D &= \left(s^{T10}_{\text{Income}} \cdot \hat{\beta}^{T10} \times \text{GDP} + s^{B90}_{\text{Income}} \cdot \hat{\beta}^{T90} \times \text{GDP}\right) \times s^R \\ &= (.55 \times .0112 \times 19{,}500\text{B} + .45 \times .0378 \times 19{,}500\text{B}) \times .17 \\ &\approx 76\text{B},\end{aligned}$$

which amounts to −\$118 billion. Similarly, for the payroll cut,

$$\begin{aligned}\Delta\text{Rev}^D &= \left(s^{T10}_{\text{Payroll}} \cdot \hat{\beta}^{T10} \times \text{GDP} + s^{B90}_{\text{Payroll}} \cdot \hat{\beta}^{T90} \times \text{GDP}\right) \times s^R \\ &= (.30 \times .0112 \times 19{,}500\text{B} + .70 \times .0378 \times 19{,}500\text{B}) \times .17 \\ &\approx 99\text{B},\end{aligned}$$

resulting in a net budget impact of −\$96 billion. See table 3 for a list of sources for each parameter.

[42] At the state level, factor mobility across states can lead to larger budget impacts. See sec. VII of Suárez Serrato and Zidar (2016) for additional analysis.

TABLE 3
CALIBRATION PARAMETERS USED IN THE TAX REFORM BUDGET ANALYSIS

Parameter	Value	Source
Prereform GDP ($T)	19.5	BEA: 2017 Q3
Federal receipts as a share of GDP: s_R	.17	FRED: 1950–2007
Top 10% income tax liability share: s^{T10}_{Income}	.55	TAXSIM: 2011
Bottom 90% income tax liability share: s^{B90}_{Income}	.45	TAXSIM: 2011
Top 10% payroll tax liability share: s^{T10}_{Payroll}	.30	TAXSIM: 2011
Bottom 90% payroll tax liability share: s^{B90}_{Payroll}	.70	TAXSIM: 2011
Cumulative effect of tax change for B90: $\hat{\beta}^{B90}$	−.0378	Table 2
Cumulative effect of tax change for T10: $\hat{\beta}^{T10}$	−.0112	Table 2

NOTE.—This table shows the values and sources for calibrated parameters for the analysis of the budgetary consequences of a few payroll and individual income tax reforms in Sec. III.C.

D. *Threats to Validity and Robustness*

There are three key threats to the validity of the estimates: endogenous tax changes, prior economic conditions and differential trends, and concomitant progressive government spending changes. 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 partialing out macro aggregates. Figure A8 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.[43]

Tables 4 and 5 present distributed lag estimates for a wide range of robustness tests to address the second and third concerns, respectively. Table 4 shows impacts of tax changes on state employment growth.[44] The first five columns present different ways to account for state-specific cyclicality: column 1 presents the baseline specification with cyclicality-quintile by year fixed effects, column 2 year effects, column 3 cyclicality-quintile by year fixed effects in which the quintiles are defined on the basis of the standard deviation in state GDP per capita, column 4 cyclicality-decile by year fixed effects, and column cyclicality-quintile by year fixed effects that group states using only the years before the sample (i.e., before 1980). The next five columns show controls for state-specific sensitivity to other shocks and trends: column 6 controls for oil price interacted with state dummies, column 7 controls for real interest rate interacted with state dummies, columns 8 and 9 add region fixed effects to columns 6 and 7, and column 10 includes state-specific trends. The specific point es-

[43] 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.

[44] Tables A8 and A9 show results for nominal state GDP. Tables A10 and A11 show results for real state GDP.

timates for the impact on employment growth from tax changes for the bottom 90 percent depend on the specification, are almost always significant statistically, and tend to be within a 1 percentage point range of the baseline estimates. Similar patterns emerge in table 5, which shows results for a wide range of policy parameters and controls for government spending. Panel B of both tables shows the same controls using the 2-year change specification for additional measures of economic activity and show similar patterns. For example, table 5 shows that 2-year employment growth following a tax change for the bottom 90 percent ranges from 3.2 percent to 3.6 percent across 11 different policy controls. Overall, the general patterns are quite robust. Almost all the impact on economic activity from tax changes comes from tax changes from the bottom 90 percent.

IV. Conclusion

This paper quantifies the importance of the distribution of tax changes for their overall impact on economic activity. 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 find that the stimulative effects of income tax cuts are largely driven by tax cuts for the bottom 90 percent and that the empirical link between employment growth and tax changes for the top 10 percent is weak to negligible over a business cycle frequency. These effects are not confounded by changes in progressive spending, state trends, or prior economic conditions. The effects seem to come from labor supply responses as well as increased consumption and investment.

These results are important for characterizing central equity-efficiency trade-offs in tax policy. If policy makers aim to increase economic activity in the short to medium run, this paper strongly suggests that tax cuts for top-income earners will be less effective than tax cuts for lower-income earners. While it is possible that tax cuts for top-income earners have sizable long-run impacts through different channels such as human capital investment, firm creation, or innovation,[45] much more compelling evidence on these channels is needed to support top-income tax cuts on efficiency grounds, especially given the magnitude of resources devoted to these tax policy changes. Overall, the results not only suggest some skepticism for "trickle-down" economics but also provide evidence that supply-side tax policies should do more to consider the relative efficacy of tax cuts targeted lower in the income distribution. Finally, as a note of caution, the estimates in this paper come from modest changes in tax rates that

[45] Extending the analysis to study medium- and longer-term effects of tax changes, such as new firm creation or patent activity, is a good topic for future research.

TABLE 4

State-Level Effects of Tax Changes by Income Group on Employment: Cyclical Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. Distributed Lag Model of Tax Changes for Different Income Groups											
$T^{B90}_{s,t}$	.33	1.01	.40	.36	1.22	.89	.40	1.10	.44	−.17	−.33	−.50
	(1.22)	(.87)	(.72)	(1.26)	(.93)	(.90)	(.78)	(1.10)	(1.03)	(1.23)	(1.42)	(1.29)
$T^{B90}_{s,t-1}$	−1.87**	−2.07**	−1.71**	−1.91**	−1.41*	−2.14**	−2.27***	−1.98**	−2.37***	−2.22**	−2.41***	−2.49**
	(.89)	(.78)	(.67)	(.78)	(.76)	(.81)	(.76)	(.82)	(.65)	(1.00)	(.70)	(1.00)
$T^{B90}_{s,t-2}$	−1.98	−1.17	−.86	−2.24*	−.85	−1.32	−1.84*	−1.19	−1.86*	−2.80*	−2.84*	−2.58*
	(1.25)	(1.10)	(.87)	(1.19)	(1.22)	(1.18)	(1.03)	(1.33)	(.93)	(1.43)	(1.42)	(1.35)
$T^{T10}_{s,t}$	.52	.33	.22	.32	.32	.29	.32	.33	.29	.56	.55	.48
	(.45)	(.32)	(.32)	(.36)	(.28)	(.32)	(.32)	(.36)	(.36)	(.46)	(.44)	(.48)
$T^{T10}_{s,t-1}$	−.24	−.43	−.19	−.56	−.30	−.49	−.55	−.38	−.55	−.28	−.26	−.29
	(.60)	(.44)	(.46)	(.50)	(.36)	(.46)	(.46)	(.39)	(.43)	(.65)	(.55)	(.63)
$T^{T10}_{s,t-2}$	−.61	−.66	−.59	−.92	−.62	−.74	−.83*	−.58	−.82*	−.74	−.69	−.67
	(.67)	(.46)	(.56)	(.56)	(.42)	(.47)	(.49)	(.36)	(.42)	(.72)	(.62)	(.69)
B90 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−3.52	−2.23	−2.17	−3.80**	−1.04	−2.57	−3.71**	−2.06	−3.79**	−5.20**	−5.58**	−5.57**
	(2.28)	(2.07)	(1.81)	(1.87)	(2.11)	(2.22)	(1.71)	(2.60)	(1.66)	(2.52)	(2.05)	(2.73)
T10 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−.33	−.76	−.56	−1.17	−.60	−.94	−1.06	−.62	−1.08	−.46	−.40	−.48
	(1.57)	(1.07)	(1.17)	(1.21)	(.85)	(1.08)	(1.10)	(.81)	(.96)	(1.67)	(1.34)	(1.65)
Bottom − top	−3.19	−1.47	−1.61	−2.63	−.44	−1.63	−2.64	−1.43	−2.71	−4.74	−5.18*	−5.10
	(3.14)	(2.13)	(2.43)	(2.36)	(2.06)	(2.26)	(2.17)	(2.73)	(2.02)	(3.48)	(2.78)	(3.63)

	B. 2-Year Changes											
Bottom 90	−3.42**	−2.58*	−2.58*	−3.58***	−1.55	−2.68*	−3.45***	−2.45	−3.37**	−4.25**	−4.44**	−4.69**
	(1.54)	(1.31)	(1.33)	(1.28)	(1.35)	(1.38)	(1.16)	(1.77)	(1.35)	(1.71)	(1.57)	(1.80)
Top 10	.22	−.00	−.05	−.24	−.01	−.02	−.11	.10	−.14	.31	.24	.15
	(.90)	(.65)	(.61)	(.66)	(.49)	(.63)	(.59)	(.54)	(.57)	(.88)	(.72)	(.93)
p-value (bottom 90 = top 10)	.06	.04	.12	.02	.22	.04	.01	.20	.06	.03	.04	.03
Controls:												
1. Baseline cyclicality	Yes	No	No	No	No	No	No	No	No	Yes	Yes	Yes
2. Year	No	Yes	No	No	No	Yes	Yes	Yes	Yes	No	No	No
3. σ_{GDPPC} cyclicality	No	No	Yes	No	No	No	No	No	No	No	No	No
4. Alternate β-diff. control #1	No	No	No	Yes	No	No	No	No	No	No	No	No
5. Alternate β-diff. control #2	No	No	No	No	Yes	No	No	No	No	No	No	No
6. Oil price × state	No	No	No	No	No	Yes	No	No	No	Yes	No	No
7. Real interest rate × state	No	No	No	No	No	No	Yes	No	No	Yes	No	No
8. Oil price × state + region	Yes	No	No	No	No	No	No	Yes	No	No	Yes	No
9. Real interest rate × state + region	No	No	No	No	No	No	No	No	Yes	No	Yes	No
10. State trends	No	No	No	No	No	No	No	No	No	No	No	Yes

Note.—Each specification is the same as col. 1 in table 1 other than the controls. Cols.1–5 present different ways to account for state-specific cyclicality (see Sec. II.A.1 or app. B.1 for details): (1) baseline specification with cyclicality-quintile by year fixed effects, (2) year fixed effects, (3) cyclicality-quintile by year fixed effects in which the quintiles are defined on the basis of the standard deviation in state GDP per capita, (4) cyclicality-decile by year fixed effects, and (5) cyclicality-quintile by year fixed effects that group states using only the years before the sample (i.e., before 1980). Cols. 6–10 show controls for state-specific sensitivity to other shocks and trends: (6) controls for oil price interacted with state dummies, (7) controls for real interest rate interacted with state dummies, (8) and (9) add region fixed effects to (6) and (7), and (10) includes state-specific trends. Standard errors are clustered by state in all specifications other than (8) and (9), which are clustered by region. The sample period is 1980–2007. See app. A.2 for data definitions and sources.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE 5

State-Level Effects of Tax Changes by Income Group on Employment: Policy Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. Distributed Lag Model of Tax Changes for Different Income Groups											
$T^{B90}_{s,t}$	.26	.26	.32	−.11	.21	.18	.18	.50	.36	.09	.36	−.32
	(1.13)	(1.19)	(1.22)	(1.19)	(1.14)	(1.16)	(1.14)	(1.16)	(1.22)	(1.15)	(1.22)	(1.03)
$T^{B90}_{s,t-1}$	−2.01**	−1.86**	−1.83**	−1.58	−1.67*	−1.86**	−1.90**	−1.69*	−1.85**	−2.46**	−1.85**	−1.86**
	(.83)	(.88)	(.91)	(.96)	(.92)	(.87)	(.87)	(.86)	(.90)	(.94)	(.89)	(.84)
$T^{B90}_{s,t-2}$	−1.92	−1.99	−1.93	−1.39	−1.96	−1.87	−2.07	−1.86	−2.00	−2.29**	−1.95	−1.67
	(1.16)	(1.25)	(1.29)	(1.31)	(1.34)	(1.16)	(1.27)	(1.32)	(1.25)	(1.07)	(1.24)	(1.12)
$T^{T10}_{s,t}$	.43	.52	.53	.47	.54	.67*	.51	.49	.53	.48	.54	.44
	(.46)	(.45)	(.44)	(.45)	(.48)	(.38)	(.47)	(.41)	(.45)	(.39)	(.44)	(.36)
$T^{T10}_{s,t-1}$	−.21	−.25	−.24	−.25	−.19	−.13	−.27	−.22	−.23	−.21	−.25	−.14
	(.56)	(.60)	(.60)	(.58)	(.58)	(.50)	(.62)	(.55)	(.60)	(.50)	(.60)	(.40)
$T^{T10}_{s,t-2}$	−.52	−.62	−.62	−.63	−.54	−.59	−.64	−.57	−.60	−.69	−.63	−.55
	(.61)	(.67)	(.68)	(.66)	(.64)	(.56)	(.68)	(.63)	(.67)	(.56)	(.67)	(.45)
B90 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−3.67*	−3.59	−3.44	−3.08	−3.42	−3.54	−3.79*	−3.05	−3.49	−4.66**	−3.45	−3.86*
	(2.09)	(2.25)	(2.32)	(2.40)	(2.38)	(2.17)	(2.21)	(2.23)	(2.30)	(2.12)	(2.29)	(1.98)
T10 sum: $\beta_t + \beta_{t-1} + \beta_{t-2}$	−.30	−.35	−.33	−.41	−.20	−.05	−.41	−.31	−.31	−.42	−.33	−.25
	(1.47)	(1.58)	(1.57)	(1.55)	(1.57)	(1.27)	(1.63)	(1.44)	(1.57)	(1.29)	(1.57)	(1.05)
Bottom − top	−3.37	−3.23	−3.11	−2.67	−3.22	−3.49	−3.38	−2.74	−3.18	−4.24	−3.12	−3.60
	(2.93)	(3.14)	(3.17)	(3.19)	(3.23)	(2.81)	(3.16)	(2.96)	(3.16)	(2.89)	(3.13)	(2.58)

	B. 2-Year Changes											
Bottom 90	−3.18**	−3.35**	−3.42**	−3.42**	−3.62**	−3.42**	−3.48**	−3.37**	−3.43**	−3.47**	−3.42**	−3.64**
	(1.52)	(1.51)	(1.54)	(1.53)	(1.53)	(1.53)	(1.48)	(1.53)	(1.53)	(1.52)	(1.54)	(1.45)
Top 10	.28	.23	.23	.18	.25	.22	.21	.17	.21	.19	.22	.19
	(.82)	(.88)	(.90)	(.88)	(.87)	(.90)	(.90)	(.83)	(.89)	(.87)	(.90)	(.81)
p-value (bottom 90 = top 10)	.07	.06	.06	.06	.04	.06	.05	.06	.06	.06	.06	.04
Controls:												
1. Government transfers per capita	Yes	No	No	No	No	No	No	No	No	No	No	No
2. Federal IG spending per capita	No	Yes	No	No	No	No	No	No	No	No	No	No
3. Minimum wage	No	No	Yes	No	No	No	No	No	No	No	No	Yes
4. OASDI	No	No	No	Yes	No	No	No	No	No	No	No	Yes
5. SSI	No	No	No	No	Yes	No	No	No	No	No	No	Yes
6. Max SNAP benefits	No	No	No	No	No	Yes	No	No	No	No	No	Yes
7. Medicaid benefits	No	No	No	No	No	No	Yes	No	No	No	No	Yes
8. AFDC + TANF benefits	No	No	No	No	No	No	No	Yes	No	No	No	Yes
9. Mechanical change in AFDC and TANF	No	No	No	No	No	No	No	No	Yes	No	No	Yes
10. Mechanical change in SNAP and SSI	No	No	No	No	No	No	No	No	No	Yes	No	Yes
11. Mechanical change in Medicaid	No	No	No	No	No	No	No	No	No	No	Yes	Yes

Note.—Each specification is the same as col. 1 in table 1 with additional controls that have the same lag structure as the tax changes. Cols. 1 and 2 control for total state transfers per capita (i.e., intergovernmental grants) and total federal transfers to a state per capita, respectively. Col. 3 controls for the minimum wage. Cols. 4–11 control for the following as a share of state GDP: OASDI payments, SSI payments, SNAP benefits (assuming max allotment per recipient), Medicaid vendor payments, AFDC and TANF payments, mechanical changes in AFDC and TANF spending, mechanical changes in SNAP and SSI spending, and mechanical changes in Medicaid spending. See apps. A.2, A.3, and C for more details on these controls and on the microsimulation model–based mechanical changes. Standard errors are clustered by state. The sample period is 1980–2007.

* $p < .1$.
** $p < .05$.
*** $p < .01$.

have been executed in the postwar period; using these estimates to evaluate the likely impacts of large tax changes on high-income earners requires extrapolation beyond the observed variation in the data.

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