

**THE EFFECTS OF MARGINAL TAX RATES:
EVIDENCE FROM THE INTERWAR ERA**

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ABSTRACT

This paper uses the interwar period in the United States as a laboratory for investigating the incentive effects of changes in marginal income tax rates. Marginal rates changed frequently and drastically in the 1920s and 1930s, and the changes varied greatly across income groups at the top of the income distribution. We examine the effect of these changes on taxable income using time-series/cross-section analysis of data on income and taxes by small slices of the income distribution. We find that the elasticity of taxable income to changes in the log after-tax share (one minus the marginal rate) is positive but very small (approximately 0.2) and precisely estimated (a t -statistic over 6). The estimate is highly robust. We also examine the time-series response of available indicators of investment and entrepreneurial activity to changes in marginal rates. We find little evidence of an important relationship, suggesting that the long-run productivity effects of changes in marginal rates may also be small.

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I. INTRODUCTION

A central concern of tax policy is the incentive effects of marginal tax rates. Do high marginal rates reduce labor supply? Do they give rise to income shielding? Do marginal rates affect productive investment and entrepreneurial activity? The answers to these questions are crucial for understanding how tax changes are likely to affect tax revenues and economic growth.

Many studies have looked at these possible effects of marginal rates using data from the postwar United States. One problem with using this period as the testing ground is that the variation in tax rates is relatively small. For example, the Reagan tax cuts, which are commonly acknowledged to be the most significant in the postwar era, only reduced top marginal rates on personal income by twenty percentage points. Because the identifying variation is relatively small, the effects of tax changes are often measured imprecisely.

The interwar United States has been greatly underused as a laboratory for analyzing the effects of marginal tax rates. Marginal rates moved frequently and dramatically in the period between the two World Wars. The top marginal rate at the end of World War I was 77 percent; by 1929 it had been reduced to 24 percent; by 1936 it had been raised to 79 percent, and in 1940 it was 86.9 percent. Furthermore, the tax changes did not just move the tax schedule up and down uniformly. For example, some acts mainly changed rates at very high income levels, while others were across-the-board changes. As a result, there was both tremendous time-series and tremendous cross-section variation in rates. This paper seeks to use this extreme variation to provide new estimates of the incentive effects of marginal rates.

One key feature of the interwar tax system is that the personal income tax fell almost entirely on the wealthy. As discussed below, the top 2/10^{ths} of 1 percent of the income distribution paid roughly 95 percent of the income tax. The vast majority of Americans paid no income tax at all. For this reason, we focus our analysis on the behavior of the very top of the income distribution.

While individual panel data, such as are used in most postwar studies, do not exist for the

interwar era, the Bureau of Internal Revenue (the precursor to the IRS) provided detailed data on reported income, deductions, and other variables for different income ranges for this period. These data can be used to calculate the policy-induced changes in marginal rates and the changes in reported taxable income by slices of the upper part of the income distribution.

To analyze the responsiveness of income to changes in marginal rates, we estimate time-series/cross-section regressions of changes in real taxable income on policy-induced changes in the after-tax share. We find that changes in marginal rates have a significant effect on reported taxable income. However, the effects are modest. The estimated elasticity of income with respect to the change in the log after-tax share (that is, 1 minus the marginal rate) is 0.2; this is lower than what most comparable postwar studies have found, particularly for high-income taxpayers. Because of the extreme variation in marginal rates in the interwar era, both over time and across slices of the income distribution, the interwar estimates are more precise than most postwar estimates. The results suggest that short-run supply-side or income-shielding effects of marginal rate changes, while clearly present, were of limited economic significance.

This finding is robust to a wide variety of specifications and checks. Eliminating outliers, considering the possibility of lagged responses, and allowing for different trends in income inequality at the top of the distribution by decade all have little effect on the estimates. Restricting the analysis to the shorter sample 1923–1932, a period well away from both world wars and when there were large changes in rates but no significant changes to the structure of the tax code, increases the estimated elasticity moderately (to 0.38) but provides no evidence of a large effect.

To look for longer-run effects of marginal rate changes, we consider time-series evidence on the response of investment and entrepreneurship. Policymakers in the 1920s, especially President Coolidge and his Secretary of the Treasury Andrew Mellon, felt that a key effect of high marginal tax rates was to skew investment funds away from productive activities and toward tax-free state and municipal bonds. In their view, high tax rates distorted behavior in a way that could reduce economic growth over a long horizon. Such long-run effects would be difficult to find in our time-series/cross-section analysis, which

looks at the response of taxable income in the few years immediately following a tax change.

We test for the presence of these more long-run effects by examining the response of a number of high-frequency indicators of productive investment activity and business formation to the aggregate policy-induced change in the log after-tax share. To abstract from the obvious influence of other macroeconomic developments, we look not at the simple change in these variables, but at the change controlling for the past behavior of the series and, in some specifications, the path of overall output. This allows us to at least partially separate the effect of marginal rates on investment from the usual response of investment to business cycle conditions. Nevertheless, because these tests focus on time-series evidence, the results are inherently more speculative than the time-series/cross-section results about the effects of marginal rates on taxable income.

We find no evidence that the large swings in marginal rates in the interwar era had a significant impact on investment in new machinery or commercial and industrial construction, but some evidence that they impacted the number of business incorporations. This suggests that the modest, fairly immediate effects of marginal rate changes on income we identify from the time-series/cross-section analysis may be the bulk of the supply-side effects.

Our paper builds on a large literature that examines the response of taxable income to tax rates using postwar data. Key contributions include Lindsey (1987), Feldstein (1995), Auten and Carroll (1999), Goolsbee (2000), Moffitt and Wilhelm (2000), Gruber and Saez (2002), Kopczuk (2005), and Giertz (2007). As described by Saez, Slemrod, and Giertz (forthcoming), the studies with the most credible identification generally find elasticities of taxable income between 0.1 and 0.4 for all taxpayers, and between 0.5 and 0.8 for high-income taxpayers. We discuss possible reasons for our somewhat lower estimates below.

Only a few papers consider the incentive effects of interwar tax changes. The one that is closest to ours methodologically is that by Goolsbee (1999). He examines the behavior of taxable income in selected years spanning three of the interwar tax changes (as well as several postwar changes). He concludes that the episodes suggest very different responses to changes in rates. Our analysis differs from

Goolsbee's in considering all years and all personal income tax changes in the interwar period, and in pooling the observations to see if the overall elasticity can be estimated with more precision. We also go beyond his analysis by computing marginal tax rates more accurately and by considering the responses of various indicators of investment.

Brownlee (2000) and Smiley and Keehn (1995) also examine interwar tax changes. Brownlee provides a careful historical analysis of some of the political economy of the tax changes and of policymakers' beliefs about their impact on incentives, but does not analyze their effects. Smiley and Keehn also provide some historical background, and examine the relationship over the period from World War I to 1929 between marginal rates and the number of taxpayers falling in various categories of taxable income. They find a significant negative relationship between marginal rates and the number of returns. Their regressions, however, do not control for time fixed effects and are estimated in levels. Thus, they may be confounded by the large swings in output and the price level over their sample. And, because of their focus on numbers of returns, it is difficult to translate their results into estimates of the elasticity of taxable income.

Our analysis of the effects of interwar tax changes is organized as follows. Section II discusses the nature and history of interwar tax changes, the available tax data, and our estimates of policy-induced changes in marginal rates by slice of the income distribution. It also discusses the possible incentive effects of changes in marginal rates in this period. Section III presents our time-series/cross-section estimates of the response of reported income to policy-induced changes in marginal rates. Section IV reports our findings on the time-series relationship between changes in marginal rates and the relative strength of entrepreneurial activity and productive investment. Section V presents our conclusions.

II. INTERWAR TAX CHANGES

The federal personal income tax was established by the Revenue Act of 1913, following ratification of the Sixteenth Amendment. Legislation changing the income tax was passed, on average, about every other year in the interwar period. Table 1 lists all acts affecting personal income taxes in the

period 1919–1941. It shows the estimated revenue effects of each act, the impact on the top marginal rate, and gives a brief description of the key changes contained in the act.¹

A. Impact on Aggregate Demand

The revenue estimates shown in Table 1 suggest that most interwar tax changes had small effects on revenue—often just a few tenths of a percent of GDP. One reason for this is that tax rates were low or zero for most households. As a result, even fairly large changes in rates translated into modest effects on revenue. This point is well illustrated by the famous tax cuts in 1924 and 1926 (the Revenue Acts of 1924 and 1926), sometimes referred to as the “Mellon tax cuts” after the Treasury Secretary who championed them. Though these changes cut rates dramatically, their revenue effects, even without any correction for possible incentive effects, were expected to be small.

Obviously, the revenue impact of some tax changes in the interwar period was more substantial. The tax cut immediately after World War I (the Revenue Act of 1921) was of moderate size, and the tax increases leading up to World War II (particularly the Revenue Act of 1941) were large. The 1941 increase was particularly large precisely because it greatly increased the fraction of households required to pay taxes. The tax increase passed near the nadir of the Great Depression (the Revenue Act of 1932) was surprisingly large, measuring close to 2 percent of GDP. However, over half of the revenue increase came from a new excise tax on all manufactured goods rather than from changes to the personal and corporate income tax.

Another crucial fact about interwar tax changes is that they were usually balanced-budget changes. That is, tax changes and spending changes typically went in the same direction and were similar in magnitude. As a result, the impact on the overall budget deficit or surplus was even smaller than the estimated effect on tax revenue.

The best evidence for this link between taxes and spending comes from the narrative record. If one reads what policymakers said about why they were taking various tax actions, it is clear that the

¹ Romer and Romer (2011) provide a narrative account of each piece of tax legislation, including the motivation for the act, the revenue estimates, and the nature of the tax changes.

behavior of spending and the state of the budget were crucial determinants, particularly in the pre-Roosevelt era. President Coolidge, for example, often spoke of reducing spending *so* taxes could be reduced. He said in his Address at the Meeting of the Business Organization of the Government in June 1924: “this fight for economy had but one purpose, that its benefits would accrue to the whole people through reduction in taxes.”² Both spending and taxes did indeed drop during the Coolidge years.

The implication of these two key facts—that the revenue effects of interwar tax changes were typically small, and that tax changes were usually accompanied by spending changes in the same direction—is that interwar tax changes are unlikely to have had much effect on aggregate demand. The budget surplus simply did not move much in response to tax changes. Thus, to the extent that tax changes mattered, it was probably not through effects on disposable income and spending. Hence, we focus on the incentive effects of changes in marginal rates.

B. Estimating Marginal Rates

While interwar tax changes had fairly small effects on revenues and the budget deficit, they had large effects on marginal tax rates. Legislation led to dramatic variation in marginal rates both over time and across different parts of the top of the income distribution. Before discussing those changes, it is necessary to briefly describe how we estimate the marginal rates faced by the various groups at the top of the income distribution in this period. The details of our calculations are presented in the Appendix.

Data. Our figures for the marginal rates faced by different percentile groups are derived from the data reported in the *Statistics of Income* (U.S. Bureau of Internal Revenue, various years). The key income concept in the *Statistics of Income* is what the Bureau called “net income.” With a few minor differences, net income corresponds to taxable income. The *Statistics of Income* divides taxpayers into various ranges of net income, such as \$90,000–\$100,000, \$100,000–\$150, 000, and so on. For each income category, there are data on the number of returns, income of various types, deductions, and other variables. Figure 1 reproduces a table from the 1933 *Statistics of Income* to illustrate the nature of the

² Presidential speeches are available from Woolley and Peters. The Coolidge quotation is from 6/30/24, p. 2.

interwar data. The yearly volumes also provide detailed descriptions of the tax code and the rates that applied at different levels of income.

Procedures. From these data by income ranges, it is relatively straightforward to calculate marginal rates by slices of the income distribution. We use data from Piketty and Saez (2001, Table A0) on the number of tax units each year. This determines how many returns we need for each percentile group.

Most of the variation in marginal rates occurred at annual incomes above \$20,000. In addition, the exclusion of some items from taxation at low levels of income makes it harder to estimate marginal rates at lower income levels. These considerations lead us to focus on the top 0.05 percent of the income distribution. Specifically, the number of returns we consider in a year is 0.05 percent of the number of tax units in the United States in that year. Since there were about 50 million tax units in the United States in this period, we consider about 25,000 returns per year. The net income cutoff for being in this group ranged from \$25,400 (in 1933) to \$75,100 (in 1928).

A percentile group in a given year typically spans a number of the income categories in the *Statistics of Income*, and it generally includes partial categories at its upper and lower ends. We therefore need to make an assumption about the distribution of taxpayers within each income category. Following the literature, we assume that the taxpayers' income follows a Pareto distribution. We estimate the Pareto parameter using the number of taxpayers across income categories, and then use it to allocate taxpayers within the categories.

We then use the tax tables to estimate the marginal rate at each level of income in a percentile group. We calculate the average marginal rate for the group by weighting the component rates by income.

One important complication in these calculations involves the treatment of capital gains, which varied greatly over the period. We therefore exclude capital gains from our definition of income, and focus on the relationship between taxable income exclusive of capital gains and losses and marginal rates on non-capital-gains income. Excluding capital gains is standard in studies of tax responsiveness, both

because they often reflect the timing of realizations rather than current income, and because they are often taxed differently than other types of income (Saez, Slemrod, and Giertz, forthcoming). As a further check that our results are not driven by the behavior of capital gains, at times we focus on the shorter sample period 1923–1932, over which the treatment of capital gains was unchanged.

Policy-Induced Changes. Because the interwar tax system was highly progressive, marginal rates changed not only because of legislated changes, but also because of economic growth, inflation and deflation, behavioral responses to legislated tax changes, and other forces. To determine the effects of changes in marginal rates, it is therefore important to separate the changes resulting from legislation from those arising endogenously from economic developments. Specifically, for each legislated change in taxes, we calculate the policy-induced change in the marginal rate of a given percentile group as the change that would have occurred if its income had not changed.

When the tax code was changed retroactively (as sometimes occurred in this period), we focus on the rates that were in effect at the time individuals were earning income, not on the rates that were applied ex post. The Appendix explains the details of our calculations of taxable incomes excluding capital gains, marginal rates, and policy-induced changes in marginal rates.

C. Interwar Changes in Marginal Rates

To give a sense of the time-series variation in marginal rates over the interwar period, Figure 2 shows the top marginal rate in each year. The figure reveals large, serially correlated changes. The top rate was extremely high (close to 80 percent) coming out of World War I. It was reduced by more than two-thirds in a series of tax actions in the 1920s, most notably the Revenue Acts of 1921, 1924, and 1926. It was then raised dramatically by the Hoover-era Revenue Act of 1932. The Roosevelt administration increased it further in the Revenue Act of 1935, which was aimed primarily at the very rich, and again through a series of broad-based tax increases on the eve of World War II.

While the top marginal rate is a potentially interesting number, the marginal rate faced at different points in the income distribution is clearly more important. Also, for calculating elasticities, it is helpful

to look not at the simple change in the marginal rate, but at the change in the log after-tax share (one minus the marginal rate). Figure 3 shows our estimates of the policy-induced changes in the log after-tax share for the top 10 two-hundredths of 1 percent of the income distribution. The results are in the change in logs (approximately 0.01 times the percent change in the after-tax share). A positive value corresponds to a tax cut; a negative value to a tax increase.

These numbers show that changes in marginal rates and thus after-tax shares, while correlated across income groups, were highly variable. Some laws, such as the Revenue Act of 1924, lowered rates and raised after-tax shares on all slices of the income distribution fairly uniformly. Other laws, such as the Revenue Act of 1935, raised rates and lowered after-tax shares much more for top income groups than for lower income groups. And laws such as the Revenue Act of 1941 raised rates and lowered after-tax shares dramatically on slices of the income distribution below the very top, but made almost no changes to the top marginal rate. This variation in changes in marginal rates across income groups will be central to our identification strategy for estimating the incentive effects of tax rate changes.

The fact that there is so much variation in marginal rates in the interwar era has two important implications. First, it means that the effects of the inherent imprecision of trying to construct data on average marginal rates of different percentile groups from the *Statistics of Income* are likely to be modest. The signal provided by changes in statutory marginal rates that frequently exceeded 10 percentage points in a year is likely to swamp the noise introduced by the imperfections in our data construction.

Second, and more important, it means that this period has the potential to provide important evidence about the effects of changes in marginal rates on taxable income. Most obviously, there is a great deal of identifying variation. More subtly, that variation consists mainly of large, salient changes. Chetty (2011) stresses that responses to small or obscure features of the tax code, especially at short horizons, may be driven largely by adjustment costs or lack of attention rather than by deeper properties of preferences. As Saez, Slemrod, and Giertz (forthcoming) observe, many postwar studies focus on such features; cases of credible identification using large changes are rare.

D. Changes in Other Tax Features

The description of each tax law given in Romer and Romer (2011) gives a sense of some of the other changes in the tax system in the interwar era. For example, Social Security taxes were imposed by the Social Security Act passed in 1935. The initial tax rate was 1 percent on the first \$3000 of earnings received by workers and on the first \$3000 of wages paid by employers. The taxes took effect in 1937. The \$3000 cut-off level was below the income of the part of the income distribution we analyze in this paper. As a result, Social Security taxes did not affect the marginal rates of the taxpayers in our sample.

The descriptions also show that the interwar period was a time of significant changes in the corporate income tax. The most extreme changes involved various excess profits taxes, which were removed after World War I, reintroduced in the National Industrial Recovery Act of 1933, and greatly increased in the run-up to World War II. There was also a substantial undistributed profits tax introduced in 1936 and gradually eliminated shortly thereafter. The regular corporate income tax was changed frequently in the interwar era, but usually within a very narrow range. Between 1922 and 1933, the corporate rate varied between 11 and 13³/₄ percent.

One type of corporation came in for quite extreme tax changes in the mid-1930s. A personal holding company was a corporation set up to hold the assets of an individual or a family; the individual or family then held stock in the corporation. Income was largely retained by the corporation, which paid the much lower corporate tax rate, rather than distributed to the shareholders, who would have paid the much higher personal income tax rate. The Revenue Acts of 1934, 1936, and 1937 greatly increased the tax rates on such corporations.

Changes in the corporate income tax, for the most part, should not greatly affect the sensitivity of personal income to personal income tax rates. The one exception might be the changing treatment of personal holding companies, which reduced one avenue of income shielding. The fact that changes in the corporate tax were minimal from 1923 to 1932 provides another reason for examining the robustness of the results to focusing on this shorter sample period.

E. Possible Incentive Effects

Before we turn to the statistical analysis, it is useful to discuss the possible incentive effects of interwar tax changes. What do the features of the interwar economy and the interwar tax system suggest are the likely effects of tax changes on behavior? In thinking about these likely effects, we consider what policymakers at the time though were the most important linkages.

Labor Supply. A key feature of the interwar tax system was that income taxes were paid almost entirely by the rich. Figure 4 shows the fraction of total federal personal income taxes paid by households at the top of the income distribution. Specifically, it shows the percent paid by the top 10 two-hundredths of the top 1 percent, where the amounts are cumulated as we move down the income distribution. It shows that between 25 and 40 percent of federal personal income taxes were paid by households in the top 1/200th of 1 percent of the income distribution. Roughly 60 percent were paid by households in the top 1/20th of 1 percent of the income distribution. And, though not shown in Figure 4, approximately 95 percent were paid by the top 2/10^{ths} of 1 percent of the income distribution.

The fact that income taxes were paid almost entirely by the very rich is important for thinking about the likely incentive effects. The income of the rich in this period did not come primarily from wages and salaries, but rather from dividends, interest, and business or partnership profits. The *Statistics of Income* (Table 7) for each year breaks down total income for nominal income categories into a number of components. From this data, along with our calculations of the income cutoffs for various slices of the top of the income distribution, it is possible to calculate the fraction of total income accounted for by income from different sources in the interwar period.

For the top 1/200th of 1 percent of the income distribution, wages and salaries accounted for only about 10 percent of total income less capital gains, while dividends and interest income accounted for about 70 percent. Business and partnership profits accounted for another 10 percent of income. For the top 10 two-hundredths of 1 percent of the income distribution combined (the top 1/20th of 1 percent), wages and salaries accounted for 15 to 20 percent of total income less capital gains, while dividends and interest accounted for roughly 60 percent. Business and partnership income accounted for another 10 to

15 percent. For both these slices of the income distribution, the importance of wage and salary income rose slightly over time.

That wage and salary income was a fairly small fraction of total income for the groups that paid most of the federal income tax in the interwar era suggests that the labor supply effects of marginal rates were of limited importance. If marginal tax rates mattered in this period, it was probably not by encouraging people to enter or exit the labor force or to work more or fewer hours. Simply too few of the relevant taxpayers appear to have been conventional wage and salary workers.

This argument that the labor supply effects of marginal rates were probably of limited importance is consistent with their noticeable absence from policymakers' discussions of incentive effects. In Romer and Romer (2011), we document that policymakers discussed the incentive effects of marginal rates extensively in the interwar era. But, we find little discussion of rates affecting decisions to work and labor effort in the narrative record. The closest we have found to this argument is a discussion in the 1921 *Treasury Annual Report* that talked of high marginal rates "drying up ... the activities of individuals in trade operations" (p. 16). This near absence of discussion is in stark contrast to the postwar narrative record, where such labor supply effects were discussed frequently and thought to be central (see Romer and Romer, 2009).

Income Shielding and Investment. The effects of marginal rates that were more likely to be important in the interwar era are those related to income shielding, both legal and illegal. When most income is derived from investments, changes in marginal rates create incentives to change the amount of income received and the form in which wealth is held. In this way, changes in marginal rates can have a large impact on reported income and government revenues. Whether they have effects on efficiency and long-run growth depends on whether the shielding activities require substantial wasted effort or lead to important distortions.

Policymakers in the interwar era believed that income shielding was prevalent and quite responsive to marginal tax rates. One of the clearest statements of this belief comes from the 1921 *Treasury Annual Report*. It stated:

Experience teaches us that means of avoiding taxes which are regarded as excessive or unreasonable will always be found, and it would be useless to attempt to catalogue them, for new methods will constantly be developed so long as the tax rates continue so high that persons having money for investment find it unprofitable to continue their investments in productive industry (p. 15).

The method of income shielding emphasized most by Coolidge and Mellon was placing wealth in tax-exempt securities such as municipal bonds. In a letter to the chairman of the House Ways and Means Committee in 1923, Mellon stated:

Taxpayers subject to the higher rates can not afford, for example, to invest in American railroads or industries or embark upon new enterprises in the face of taxes that will tax 50 per cent or more of any return that may be realized. These taxpayers are withdrawing their capital from productive business and investing it instead in tax-exempt securities and adopting other lawful methods of avoiding the realization of taxable income (1923 *Treasury Annual Report*, p. 8).

The Coolidge administration believed this diversion of investment funds was significant enough that it was impeding employment and long-run growth. It not only called for a drastic decline in marginal rates to decrease this incentive, but also repeatedly asked for a constitutional amendment taking away the right of states and municipalities to issue such securities.

Like their counterparts in the 1920s, policymakers in the 1930s were also very concerned about income shielding. A special subcommittee of the Ways and Means Committee established in June 1933 was charged with “studying tax avoidance and the means of preventing such avoidance” (“Revenue Bill of 1934,” 73rd Congress, 2^d Session, House of Representatives Report No. 704, 2/12/34, p. 1). Among the methods the committee found most worrisome was the creation of personal holding companies described above. Roosevelt in 1936 suggested that the problem was broader. He argued that many corporations were retaining earnings rather than paying dividends as a way to help shareholders avoid paying the personal income surtax. Unlike Coolidge, who pushed for lower marginal rates as a way to lessen efforts at income shielding, Roosevelt and the Democratic Congress responded by raising rates on personal holding companies and placing a special tax on undistributed corporate profits. Nevertheless, their concern about tax avoidance suggests they believed that such incentive effects were substantial.

While most of policymakers’ discussions focused on legal income shielding and tax avoidance, it

is important to note that tax enforcement was relatively lax in the interwar era. There was no tax withholding, so self-reporting was key. The only enforcement tool was auditing, which was done relatively little (though occasionally with great fanfare, as in the case of the audit of Mellon in 1933). As a result, there is reason to expect more illegal hiding of income in the interwar era than in the postwar era. This would be a factor that could tend to make reported income particularly sensitive to changes in marginal rates.

Finally, because business formation and entrepreneurial activity have the potential to generate large increases in income over time, changes in marginal rates could affect the behavior of people not currently paying the income tax. A low-earning entrepreneur may consider what will happen if an investment is successful enough to lift him or her into the tax-paying brackets. The entrepreneur may not see much potential reward from starting a new business when marginal tax rates are high, but more when marginal rates are lower. Thus, changes in rates could affect behavior not reflected in the income statistics of current taxpayers. We examine this issue in Section IV.

III. TIME-SERIES/CROSS-SECTION ESTIMATES OF THE IMPACT OF CHANGES IN MARGINAL RATES ON REPORTED TAXABLE INCOME

The first way that we investigate the impact of changes in marginal rates is to use the detailed time-series/cross-section data from the *Statistics of Income*. As described in the previous section, we use the data in the *Statistics of Income* to measure taxable income by percentile group and the corresponding policy-induced changes in marginal rates. We have compiled these data for the 10 top 1/200ths of 1 percent of the income distribution for the period 1919 to 1941.³ Because interwar tax changes typically affected the percentile groups differently, we can test to see if the responses of reported income differ systematically. Because we have many years of data, we can use the numerous tax actions to derive more precise estimates of the impact of changes in marginal rates.

³ We calculate marginal rates by percentile group beginning in 1918. The policy-induced changes are therefore available starting in 1919.

A. Specification

Our baseline specification is:

$$(1) \quad \Delta \ln Y_{it} = \alpha_i + \beta_t + \sum_{j=0}^J \gamma_j \Delta \ln(1 - \tau)_{i,t-j}^{PI} + \varepsilon_{it},$$

where $\Delta \ln Y_{it}$ is the change in the log of real reported taxable income of group i in year t (exclusive of capital gains), and $\Delta \ln(1 - \tau)_{i,t-j}^{PI}$ is the policy-induced change in the log of the marginal after-tax share of group i in year $t - j$. We regress the percent change in reported taxable income of a group on the change in the log of the after-tax share of income of that group. The estimated coefficient is therefore the elasticity of taxable income with respect to the share of income kept by the taxpayer. Economic theory implies a positive coefficient: a decline in the marginal tax rate (which raises the after-tax share) raises taxable income.

We include a full set of group and time dummy variables. The group dummies (the α_i 's) capture differences in trend growth of the income of the various percentile groups. The time dummies (the β_t 's) capture year effects. The interwar period was a time of extreme movements in output, stock price swings, and wartime mobilization and demobilization. Such time-specific macroeconomic shocks could obscure the incentive effects of changes in marginal rates if time dummies were not included. We also consider specifications that exclude one or both sets of dummies and that replace the time dummies with GDP growth.

In the simplest regressions, we only consider the contemporaneous relationship between income and the after-tax share. However, it is surely possible that it takes additional time for taxpayers to adjust their behavior even to the large tax changes in this period. We therefore consider specifications including 1 and 2 lags of the tax policy variable.

Our baseline specification estimates the relationship between income and policy-induced tax changes using OLS. An obvious alternative to this “reduced-form” specification is to use an instrumental variables approach. In this specification, we regress the change in income (by year and percentile group) on the change in the actual after-tax share, instrumenting with the policy-induced portion.

In the baseline specification, we estimate the relationship over the full sample period 1919 to 1941. In the robustness section, we consider a range of alternative samples.

One important alternative sample period is 1923–1932, when there were no major changes in capital gains or corporate income taxes. Our underlying data are grouped by taxpayers' net incomes including capital gains. As a result, our estimate of, for example, the non-capital-gains income of the top 1/200th of 1 percent of the population ranked by net income will understate the non-capital-gains income of the top 1/200th of 1 percent of the population ranked by non-capital-gains income. Piketty and Saez (2001) find that this effect is non-trivial for the top 1/100th of the income distribution. However, one would expect—and Piketty and Saez's results suggest—that the effect would not change substantially in years when the tax treatment of capital gains is stable. Thus, although this effect is unlikely to have a large impact on our estimates even over the full sample, it is especially unlikely to cause bias over the 1923–1932 sample.

Likewise, some of the response of taxable income to marginal rates likely reflects shifting between personal and corporate forms of income, which acts to mitigate the welfare costs of the response (Gordon and Slemrod, 2000). Focusing on a period when corporate rates barely changed helps to isolate the effects of the personal tax.⁴

B. Baseline Results

Table 2 shows the results of the baseline specification and some key econometric permutations. The coefficient(s) of interest are those on the contemporaneous and lagged values of the after-tax share. When we include lags of the tax variable, we focus on the sum of the coefficients on the contemporaneous and lagged values, along with the associated standard error.

A striking feature of the results is their uniformity. The estimated impact of a rise in the after-tax share is consistently positive, small, and precisely estimated. In the simplest specification, which

⁴ Even in this case, however, some of the response of taxable personal income to changes in personal tax rates likely takes the form of shifts between personal and corporate income (and, similarly, between non-capital-gains and capital-gains income). Thus, our estimates will tend to overstate the loss of total revenue from shrinkage of the tax base in response to increases in marginal personal income tax rates (Slemrod, 1998).

includes only the contemporaneous value (line 1), the coefficient estimate is 0.19 with a t -statistic over 6. This estimated elasticity implies that a rise the after-tax share of 1 percent (say from 50.0 to 50.5 percent) raises reported taxable income by just under $2/10^{\text{th}}$ s of a percent.

The results are virtually identical when one uses IV in place of OLS. In all the specifications we consider, the actual after-tax share moves almost one-for-one with the policy-induced change and the effect is overwhelmingly significant. In the first stage of the IV variant of the baseline specification, for example, the coefficient on the policy-induced change in the log after-tax share is 0.998 with a t -statistic of 28. As a result, using IV (line 2) barely changes the estimated elasticity and increases the standard error only slightly. Because the interpretation of the IV specification is somewhat complicated, in subsequent robustness checks we focus on the reduced-form OLS specification.

Including lags of the tax variable (lines 3 and 4) increases the sum of the coefficients slightly—from 0.19 with no lags to 0.27 with 2 lags. The standard error on the sum of the coefficients rises slightly, but the sum is still highly statistically significant ($t = 4.7$). The individual coefficient estimates on the first and second lags, however, are not significantly different from zero.

As shown in line 5, leaving out the group dummy variables has almost no impact on the estimates. On the other hand, leaving out the time dummies (line 6) matters substantially. As discussed above, we would expect the results might be affected by other aggregate shocks when the year dummies are excluded, and they are. Replacing the year dummies with the growth rate of GDP (line 7) yields results similar to those when the year dummies are omitted. This suggests that the time dummies are capturing more than just the systematic response of taxable income to GDP growth. For example, they may capture surges in patriotism around the run-up to World War II or different responsiveness of reported income to output in the 1920s than in the 1930s.

C. Robustness

The previous section showed the key results and investigated their robustness to differences in the econometric specification. In this section, we investigate their robustness to different specifications of the

tax variable and the group dummy variables. We also consider a wide range of alternative samples.

Specification of the Tax Variable. As described in the Appendix, some interwar tax changes were retroactive to the previous year. Because people cannot change their behavior retroactively, our baseline tax policy variable reflects the change in the year it was passed, not the year it took effect. However, because tax returns were not filed until the subsequent year (and after the retroactive tax changes had been passed), fraudulent income reporting could respond to the retroactive changes. Individuals could also have responded to the retroactive changes if the changes were widely anticipated. For these reasons, we consider an alternative specification of the tax variable that dates changes when they took effect, even if they were passed the next year. We try specifications that replace our baseline tax variable with this alternative and that include both the baseline variable and the difference between the alternative series and the baseline.

The results are shown in the lines 2 and 3 of Table 3. For simplicity, in evaluating robustness we only report the baseline specification, which includes no lags and is estimated using OLS. The results of this specification for our baseline tax variable are repeated in line 1 of Table 3.

Replacing the baseline tax variable with the perfect-foresight alternative (line 2) has only a moderate impact on the results. The estimated response of taxable income to tax changes falls from 0.19 to 0.12 and is less overwhelmingly significant. The most likely explanation for this pattern is that the perfect-foresight variable is a less accurate measure of the incentives that taxpayers were responding to.

As noted above, taxpayers could respond to retroactive changes by altering the amount of their income that they reported. To test for this possibility, we add the difference between the two tax variables to the baseline regression. In this specification (line 3), the coefficient on the baseline tax variable is virtually identical to before and the coefficient on the difference is negative, near zero, and far from statistically significant. This suggests that fraudulent income reporting and anticipation effects in response to retroactive tax changes may have been relatively unimportant in the interwar period.

Decade-Varying Group Dummies. Another robustness concern involves secular changes in income inequality and taxes. The 1920s were a period of rising income inequality and falling marginal

rates, while the 1930s were a period of declining inequality and rising marginal rates. It is possible that with only group dummies for the whole period, the regression interprets this correlation as a behavioral response to marginal rates. To check for this possibility, we specify two sets of group dummies—one for the period through 1929 and one for the period beginning in 1930.

The results of this exercise are shown in line 4. Letting the average growth rate of reported income by percentile groups be different in the 1920s and 1930s has no impact on the results. The estimated elasticity of income to the after-tax share is still 0.19 with a t -statistic of 6.

Another way that we test for the same effect is to interact the group dummies with real GDP growth. It is possible that the top income groups do particularly well in good times and particularly badly in bad times. As shown in line 5, letting the group dummies vary with real GDP has little impact on the results.

A related robustness check just excludes the very top percentile group (the top 1/200th of 1 percent of the income distribution) from the estimation. This exercise tests whether the results are being driven only by this one group whose relative income moved substantially in the interwar period. Line 6 of Table 3 shows that the results are very similar to the baseline when this group is eliminated.

Sample Period. We consider the robustness of the results to a wide range of sample periods. The baseline sample is the full period from 1919 to 1941. This period already excludes the most extreme wartime changes. But to avoid the impacts of demobilization in 1919 and mobilization in 1940 and 1941, we consider the non-war sample 1920 to 1939. In addition, as discussed above, we consider the period from 1923 to 1932, which was relatively free of changes in capital gains and corporate taxes. Finally, the Great Depression was such an enormous shock that we consider both a pre-Depression sample (1919 to 1929) and a Depression sample (1930 to 1941).

Lines 7 to 10 of Table 3 report the results for the various sample periods. In general, the alternative sample periods lead to estimates that are somewhat higher than for the full sample, but still modest. In each of the non-war, stable corporate and capital gains tax, pre-Depression, and Depression samples, the coefficient on the after-tax share is small, positive, and precisely estimated. The largest

estimate is for the period where other aspects of the tax system were relatively stable, where the estimated elasticity is 0.38.⁵

Figure 5 shows the scatter plot of the contemporaneous percentage change in income and the change in the log after-tax share, after partialing out the impact of the group and time dummies. That is, both the x and y variables have been regressed on the two sets of dummies, and what is plotted are the residuals against one another. This graph shows that there are some extreme observations—particularly for the very top 1/200th of 1 percent of the income distribution. But, the scatter also illustrates why the results are quite robust to excluding the very top group and to different sample periods. The extreme observations are all cases of large tax changes and relatively small income changes, so excluding one or two with a different sample is unlikely to change the results. Also, the extreme observations are generally in line with the mass of the observations. As a result, excluding the extremes (for example, by excluding the very top 1/200th of 1 percent) has little impact on the estimates.

D. Discussion

This analysis shows that the results of the time-series/cross-section analysis are exceedingly robust to econometric specification, tax measure, treatment of year and group fixed effects, and sample. Across all specifications we consider, changes in marginal tax rates (and hence in the after-tax share) have a precisely estimated but small impact on reported taxable income.

One way to quantify just how small the estimated elasticity is is to note what it implies about the revenue-maximizing tax rate. If the tax rate is constant and the elasticity of taxable income with respect to the after-tax share is constant across income levels, tax revenue is maximized when the tax rate equals 1 divided by 1 plus the elasticity of taxable income. Thus, an elasticity of 0.19 implies that the revenue-maximizing tax rate is $1/(1+0.19)$, or 84 percent. Even an elasticity of 0.38, which is the highest we find in our robustness checks, implies that the revenue-maximizing rate is 73 percent.

⁵ For this sample, in contrast to the full sample, introducing lags reduces the overall effect somewhat. With either 1 or 2 lags, the sum of the coefficients on the tax variables is 0.28, and still highly statistically significant. As before, the coefficients on the lags are not significant.

The estimated elasticity of taxable income to the after-tax share obviously includes a variety of effects. It captures incentives to adjust labor effort and hours, to take actions to legitimately shield income from taxes, and to risk prosecution by falsely reporting income. The time-series/cross-section results suggest that the sum of all of these effects was small in the interwar period.

An elasticity of taxable income of 0.2 is toward the low end of postwar estimates for all taxpayers, and substantially below postwar estimates for high-income taxpayers. There are several factors that could contribute to this finding.

One obvious possibility is that taxpayers were less sophisticated in the interwar era than they are today. However, it would be a mistake to think of taxpayers in this period as naïve. The taxpayers we focus on were very wealthy; and with marginal tax rates often well over 50 percent, the stakes were high. Policymakers and taxpayers were very aware of strategies for tax avoidance and tax minimization (Brownlee, 2000; Romer and Romer, 2011). Thus, we are skeptical that changes in sophistication are the main source of the difference over time.

Two other ways the interwar and postwar periods differ are in the composition of taxpayers' income and the enforcement regime. Compared with their modern counterparts, high-income taxpayers in the interwar period had higher capital income and lower wage and salary income. And the enforcement regime relied almost exclusively on self-reporting and the possibility of audits and, potentially, prosecution. But it is not clear that these differences help to explain the different estimates. The usual finding from the postwar period is that individuals in occupations where incomes come primarily from wages and salaries are less responsive to marginal rates than individuals in other occupations (Auten and Carroll, 1999). And, one might expect that greater reliance on self-reporting would lead to greater responsiveness of reported income to incentives.

One factor that may explain the findings is the greater simplicity of the interwar tax system. Theoretically, one would expect a smaller responsiveness when there are fewer margins to adjust on (Slemrod and Kopczuk, 2002). Empirically, a consistent finding of studies of the postwar period is that a broader base and a simpler tax system lead to a lower elasticity of taxable income. Non-itemizers are less

responsive than itemizers, total income is less responsive than taxable income, low-income taxpayers are less responsive than high-income taxpayers, and responsiveness was lower after the Tax Reform Act of 1986 than before (Gruber and Saez, 2002; Kopczuk, 2005; Giertz, 2007). The interwar tax system was quite simple, with a broad base, few deductions, and instructions that took up only a few pages. These features likely contribute to the lower responsiveness of reported taxable income to marginal tax rates.

The final possibility is the most prosaic, but still potentially important: perhaps the true difference between the eras is smaller than the difference in point estimates. The postwar estimates, in particular, have non-trivial standard errors. The well known study by Gruber and Saez (2002), for example, finds an elasticity of 0.40 with a standard error of 0.11 for all taxpayers, and 0.57 with a standard error of 0.30 for high-income taxpayers. Thus, an elasticity of 0.2 is within the confidence interval for both estimates, and there is little evidence against the hypothesis that the elasticity at high incomes is, say, 0.4 or 0.5 rather than 0.57. Simple sampling error may therefore be important to the difference in the estimates between the two eras.

IV. TIME-SERIES EVIDENCE ON THE EFFECT OF MARGINAL RATES ON INVESTMENT

The previous section found that the very large movements in marginal rates in the interwar period did not have a large short-run impact on the behavior of the taxpayers directly affected. Many concerns about the effects of marginal tax rates, however, involve more than taxpayers' short-run responses through such channels as labor supply, income-shielding, and tax evasion. Instead, they focus on the possibility that high marginal rates discourage investment, innovation, and entrepreneurship, and so slow long-run growth.

Our findings about the response of taxable income do not rule out the possibility of such effects. For example, suppose a small component of overall investment, such as machinery investment, is particularly important for long-run growth (as argued by DeLong and Summers, 1991). In this case, a change in marginal rates that changed investment behavior might have little impact on the income of the wealthy in the short-run, but a substantial effect over time for the entire economy. Similarly, as discussed

in Section II, changes in marginal rates could affect the entrepreneurial behavior of individuals not currently paying taxes. If a cut in marginal rates increased business formations by the less wealthy, this would not be apparent in the short-run income response at the upper end of the income distribution, but could again have a large impact on growth.

This section therefore investigates the responses of available interwar series on investment and entrepreneurial activity to policy-induced changes in marginal rates at the top of the income distribution. We examine whether variables such as machinery investment and business incorporations responded systematically to changes in marginal rates.

This exercise is inherently more tentative than the examination of the response of taxable income in Section III. Because we do not have data on investment and entrepreneurship by income level, we can only exploit the time-series variation in marginal rates. Thus we lose a considerable part of our identifying variation. More importantly, the fact that we are forced to rely on the time-series variation means that the effects we are interested in may be confounded by the enormous shocks affecting the economy over this period. In addition, because the investments and entrepreneurial decisions that drive growth are hard to isolate, we do not have high-quality measures of the activities that we are interested in. Thus, our results need to be interpreted with caution.

A. Series Analyzed

Changes in marginal tax rates were frequent in the interwar period. It is therefore useful to consider high-frequency indicators of productive investment and entrepreneurial activity. While the number and quality of such indicators is obviously much more limited for the interwar period than for today, there are some potentially useful measures.

Investment and Entrepreneurial Activity. We focus on three indicators of business investment and entrepreneurial activity. The first is the Federal Reserve index of industrial production for

machinery.⁶ This series is available starting in 1923:1. The main types of machinery included are electrical machinery, foundry and machine-shop products, engines, and agricultural implements. This series measures the production of these types of investment goods rather than actual investment, but the two are likely to be highly correlated. The machinery series for this period is derived almost entirely from man-hour data collected by the Bureau of Labor Statistics. Since productivity is generally procyclical, the use of man-hours means that cyclical moments are likely to be understated. But there is no reason to think that the series is inconsistent over the interwar period.

The second series we consider is the value of construction contracts for commercial and industrial buildings. These data are from Lipsey and Preston (1966, pp. 88-90), and are based on information compiled on a relatively consistent basis by the F. W. Dodge Company. They are available throughout the interwar period. The series covers mainly the eastern half of the country and shows commitments to start work within about 60 days. Lipsey and Preston report that various tests suggest that the data cover most non-residential construction and are reasonably accurate. We deflate the series by the Consumer Price Index to convert it from nominal to real.⁷ The construction contracts series is a leading indicator of investment in commercial and industrial buildings. As such, it is a high-frequency indicator of this type of fixed investment.

The third series we consider is an index of business incorporations from Evans (1948, Table 38, pp. 80-81). This series is based on detailed data culled from various states. The particular group of states included in the index varies over our sample, but data for New York and Delaware are included for most of the period. Evans is careful to construct substantial periods of overlap between series using different sets of states and to splice the series together appropriately. Nevertheless, the changing sample could

⁶ This series was first described and presented in an article in the *Federal Reserve Bulletin* ("A New Federal Reserve Index of Industrial Production," Vol. 26, August 1940, pp. 753-769). Data through 1941:12 were collected from later issues of the *Federal Reserve Bulletin*. We use the seasonally adjusted version of the series.

⁷ The number of states covered by the Dodge series rises from 27 before 1923 to 36 in 1923 and 1924 (9 southern states were added) to 37 starting in 1925 (Texas was added). We take the measure with the widest coverage and join the series using a ratio splice in the latest year of overlap. We use the seasonally adjusted version of the series. Because the movements in the nominal series are so large relative to movements in prices, the specifics of how we deflate it are unlikely to be important. The specific series we use is the Consumer Price Index for all urban consumers (series CUUR000SA0, CUUS0000SA0, downloaded 1/19/2011). Only the not seasonally adjusted series is available for this period. However, seasonal movements in the series are small.

affect trend behavior if states had substantially different trends, for example because of state-specific legislation. The resulting index is a measure of one type of business formation.⁸

Interest-Rate Ratio. As discussed in Section II, interwar policymakers were convinced that high marginal tax rates skewed investment greatly toward local public investment and away from private investment. To test this idea, we examine the ratio of the interest rate on municipal bonds to the rate on AAA corporate bonds. This series is not a measure of investment, but an indicator of the extent to which the tax system distorted investment incentives. These data are available for our full sample period.⁹

Aggregate Tax Variables. Since we are focusing only on the time-series dimension of the data, we need a measure of the overall policy-induced change in the log after-tax share for the upper end of the income distribution. We calculate this series by aggregating the changes for the different percentile groups in our earlier analysis, weighting them by income. That is, consistent with the time-series/cross-section analysis, we calculate the marginal after-tax share of the top 1/20th of 1 percent of the income distribution for their year $t - 1$ income under both the year $t - 1$ tax code and the year t tax code. The difference between the two measures is the policy-induced change in year t .

We use a slightly different tax measure when we are considering the ratio of the interest rates on tax-free and taxable bonds. If the bonds are otherwise comparable, one would expect the ratio of the two interest rates to equal 1 minus the marginal tax rate of the marginal investor. In analyzing this series, we therefore focus on the policy-induced change in 1 minus the average marginal rate of wealthy taxpayers (rather than in the log of 1 minus their marginal rate).

Because our investment indicators are monthly, we need the tax series at a monthly frequency as well. For tax changes that were passed before they took effect, we date the changes as occurring when

⁸ Evans presents two indexes, one covering the period through 1925 and the other beginning in 1924. Following the procedure he uses in other cases, we splice the two series together using their annual averages in 1924. The series is seasonally adjusted. Note that because the series is a measure of incorporations, it captures not only the formation of new businesses but also shifts from unincorporated to incorporated businesses. As a result, using it may lead to underestimates of the impact of the personal income tax on overall business formation.

⁹ The data on both interest rates are from the U.S. Board of Governors of the Federal Reserve System (1943, Table 128, pp. 468-471). Interest rate data are generally thought to be accurate for the interwar period. The markets were thick and information on rates was widely published.

they went into effect (which is always in January of the year). When a change was retroactive, we date the changes as occurring when the legislation was passed. That is, we ignore the retroactive component and compare the new rate with the rate under the previous legislation.

B. The Behavior of the Investment Series

Given the exceptional nature of our sample period, it is helpful to start by simply looking at the data. Figure 6 presents graphs of each investment series and the aggregate tax variable. Our tax series begins in 1919; all of the investment series are available by then except for machinery production, which begins in 1923.

One fact apparent from the figure is just how extreme the tax changes were in the interwar period. The largest changes moved the after-tax share by 20 percent or more. The positive values in the 1920s correspond to the large cuts in marginal rates in this decade. The negative values in the 1930s and early 1940s correspond to the large increases in marginal rates, particularly in 1932 and in the mobilization leading up to World War II.

A second fact apparent from the figure is that there were huge swings in the investment series that almost certainly reflect the effects of the large macroeconomic shocks of this period and of additional forces other than marginal tax rates. Machinery production, for example, skyrocketed in the late-1920s boom, fell by over a factor of three in the Depression, and returned almost to its pre-Depression peak in 1937 before going through an additional gyration in the years leading up to World War II. The construction and incorporations series also exhibit tremendous movements over extended periods, and those movements differ substantially from those of machinery production.

Investment and Entrepreneurial Activity. Now consider the behavior of each series and its relation to changes in taxes in more detail. Panel (a) shows the behavior of the production of machinery beginning in 1923. It suggests no clear link between tax rates and this component of investment. Machinery production grew strongly after the 1924 tax cut, but was largely flat after the larger 1926 cut. It was likewise relatively flat immediately after the very large 1932 tax increase, before surging in the

early stages of the recovery from the Depression. Not surprisingly, it also surged during the mobilization for World War II despite two substantial tax increases.

Panel (b) shows the behavior of commercial and industrial construction contracts beginning in 1919. This graph also suggests that fluctuations in construction were driven largely by factors other than taxes. Construction more than quadrupled after the large tax increase in 1919 to pay for World War I. After plummeting in the recession of 1920–1921, it rose over the 1920s, but choppily and in a way that was not clearly related to the three tax cuts in this period. Industrial construction changed little following the 1932 tax increase, and rose temporarily after the 1935 increase. And like machinery production, it rose strongly after the tax increases leading up to World War II.

Panel (c) shows the behavior of incorporations from 1919. This graph shows potentially more of a relationship between investment and legislated changes in taxes. Incorporations largely rose over the 1920s, when the after-tax share was rising, and fell over the 1930s, when it was falling. Moreover, there were noticeable surges in incorporations after each of the tax cuts in the 1920s, and noticeable declines after the tax increases in 1940 and 1941. However, there was a dramatic surge after the 1919 tax increase that goes strongly against this correlation, and little change following the tax increases of 1932 and 1935. Nevertheless, incorporations appear to be the one investment series that may suggest an impact of marginal rates.

Interest-Rate Ratio. Finally, Figure 7 shows the ratio of the interest rate on municipal bonds to the rate on AAA corporate bonds. In keeping with what one would expect about the determinants of this ratio, we look at the after-tax share rather than its log, and at the level of the series rather than only at policy-induced changes. Thus the tax series shown is simply 1 minus the average marginal rate of the top 1/20th of 1 percent of the income distribution. In addition, for simplicity the figure uses the actual tax rates paid, without adjustment for the fact that in some cases the rates were set retroactively. (Using the rates that people would have thought were in effect at the time would yield a very similar picture.)

Figure 7 shows that to the degree the two series are related, the relationship is extremely muted. In the 1920s, the after-tax share was rising sharply while the interest-rate ratio was at most creeping

upward. In the 1930s, as the after-tax share was falling, the interest-rate ratio was falling as well, but less than one-for-one. And following the 1932 tax increase, the ratio surged rather than fell. The simple picture therefore suggests that it will be difficult to find Mellon's hypothesized relationship.

C. Statistical Tests

We now turn to more formal examination of the relationship between legislated changes in marginal rates and our investment indicators. We begin by describing our specifications and sample periods. We then present the baseline findings, and finally discuss robustness.

Specifications and Samples. Our baseline specification is a two-variable vector autoregression (VAR) with the investment measure of interest and the policy-induced change in the log after-tax share of the top 1/20th of 1 percent of the income distribution. The investment measures are entered in log levels, and the VAR includes 24 lags. The tax variable is ordered first, so that investment can potentially respond to taxes within the month but taxes cannot respond to investment.

This specification asks how investment behaves relative to its usual behavior following a legislated change in taxes that is not predictable based on the past behavior of investment and of changes in taxes. Throughout, we find that the tax changes are essentially unpredictable; for example, the adjusted R^2 of the tax equation in the VAR is almost always negative. Thus the VAR in effect summarizes how investment behaves relative to its usual behavior following a legislated change in taxes.

To help address the fact that there were enormous macroeconomic fluctuations in this era, we consider two variations on this specification. The first includes the overall index of industrial production (in logs) as another variable in the VAR. This controls for the effects of movements in overall economic activity prior to the tax changes. The second and larger variation includes the contemporaneous value and twelve lags of industrial production but treats them as exogenous. Thus, this specification asks how investment behaves in the wake of tax changes given the path of overall economic activity following the changes. The specification is reasonable if the effects of the tax changes on the overall economy are small, which is plausible in light of their small impact on aggregate demand discussed in Section II and

the small short-run response of high-income taxpayers found in Section III. Under this assumption, this specification can help address the possibility that the effects of the tax changes might be swamped by the large cyclical fluctuations of this period.

When we consider interest rates, the variables are slightly different. The two variables in our basic VAR are the ratio of the municipal bond interest rate to the AAA corporate bond interest rate and the policy-induced change in 1 minus the average marginal rate at the top of the income distribution. As described above, if the taxpayers we consider are the ones relevant to the determination of the relative interest rates, one would expect the interest-rate ratio to move one-for-one with the tax variable.

Our basic specification uses data back to 1919:1 when they are available; the end date is 1941:12. Since the VAR includes 24 lags, this means that for business construction, incorporations, and the interest-rate ratio, the sample period is 1921:1–1941:12. Because the machinery data only begin in 1923, for that VAR the basic sample period is 1925:1–1941:12.

As with our analysis of the responses of taxable income, we consider the effects of limiting the sample to the period when there were few changes in capital-gains or corporate taxes (so the 1921:1 starting dates are changed to 1923:1, and the end date is 1932:12). We also examine the effects of limiting it to the period before the Depression (so the ending date is 1929:8, the month of the NBER business cycle peak), and of excluding the tax increases leading up to World War II (so the end date is 1939:12). We do not consider the pre-Depression sample for the machinery VAR, since that sample would consist of only 56 observations.

As we have described, when taxes were changed retroactively, we date the changes as occurring when they were passed. We test the robustness of our results to two alternative timing assumptions. First, as in Section II, we consider the perfect-foresight variant of our series. This variant dates retroactive changes as occurring when they took effect rather than at the later dates when they were passed, even though individuals would not have had full information about them at the time. Second, because of adjustment costs, investment may start to respond to tax changes before they take effect. We therefore consider a variant that dates *all* changes as occurring when the legislation was passed, even if it

did not change tax rates immediately.

Baseline Results. Figure 8 shows the impulse response functions of machinery investment, business construction, and incorporations to a 1-percentage-point innovation to the measure of policy-induced changes in the after-tax share. The figure also shows the 1-standard-error confidence bands.

The results largely echo the patterns suggested by the simple plots in Figure 6. For machinery investment and commercial and industrial construction, there is no evidence that increases in marginal rates lower investment. For machinery, the estimated impact of an increase in the after-tax share is positive, small, and insignificant for a few months, then consistently negative. A 1 percent increase in the after-tax share is followed by a fall in machinery investment of about 1½ percent. From 12 to 21 months after the change, the *t*-statistic exceeds 2; at other horizons, the null hypothesis of no effect cannot be rejected.

For the construction measure, there is no evidence of any relationship. The point estimates are negative and insignificant at short horizons, and positive and highly insignificant at long horizons.

For business incorporations, in contrast, there is evidence of a positive effect of tax reductions. For the first year, the estimated effect of a 1 percent rise in the after-tax share is generally positive, but irregular and almost always insignificant. After about a year, however, the estimated impact rises rapidly to almost 1 percent and is highly significant, with the *t*-statistic often over 3.

Figure 9 shows the estimated response of the interest-rate ratio to a fall in marginal rates of 1 percentage point. The response is at best modest. The hypothesis that it is one-for-one is overwhelmingly rejected at all horizons, and at medium and long horizons the estimates are negative rather than positive. The estimates are positive only for the first eight months, and even then they are small and never significantly different from zero.

Robustness. The baseline findings are very robust. Because of the potential confounding effects of the World War II build-up, one particularly important robustness check is to drop 1940 and 1941 from the sample. (Note that because of the inclusion of lags in the VAR, the observations immediately after the end of World War I are already excluded.) This omission changes the response of machinery

investment to a rise in the after-tax share from negative and sometimes significant to irregular and highly insignificant. The response of construction remains small and far from significant, but it changes from irregular to generally positive. The omission weakens the estimated response of incorporations slightly: the estimated maximum effect falls from 1.00 to 0.80, and there is now only one month where the t -statistic exceeds 3. Finally, there is little impact on the response of the interest-rate ratio.

The findings for business incorporations and interest rates are extremely robust to all of the other variations in specification, sample, and measurement of the tax variable described above. The largest changes in the estimates occur when retroactive tax changes are dated when they took effect rather than at the later date when they were passed. For incorporations, the maximum response is again 0.80 rather than 1.00, and the maximum t -statistic falls to 2.4. For the ratio of tax-free to taxable interest rates, the short-run response changes from insignificantly positive to significantly negative (that is, it is in the opposite direction from what one would expect), but the response at longer horizons is little changed.

For both machinery production and commercial and industrial construction, we never find a significant positive response to increases in the after-tax share. For machinery production, however, the result that the impact is significantly negative at some horizons is not robust: the estimated effects are generally negative but insignificant when we include industrial production as another variable in the VAR, when we treat industrial production as exogenous, and when date the retroactive changes when they took effect. For commercial and industrial construction, where the estimated effect has no consistent sign in the baseline specification, there are some variations where it is generally negative and some where it is generally positive. But there are none where it is significantly positive (and one—the 1923–1932 sample—where it is significantly negative).¹⁰

As noted above, taxes were generally falling and incorporations generally rising in the 1920s, and taxes were generally rising and incorporations generally falling in the 1930s. If this pattern were driving the results for incorporations, there would be reason to be concerned that other secular changes might be

¹⁰ Using the variant of our tax measure that dates all policy changes when they were passed, regardless of when they took effect, has little impact for any of the investment or interest rate VARs.

responsible for the correlation. To check for this possibility, we add a dummy variable equal to 1 beginning in 1929:9. This addition reduces the estimated impact by over a third and cuts its statistical significance somewhat. But the effect remains substantial and significant: the peak effect is 0.63, and the maximum t -statistic is 2.9. Thus, although the decadal swings in taxes and incorporations are contributing to the estimates, they are not their main source.

It is also important to note that because of the 24 lags in the VAR, the regressions do not include 1919, which is a time when incorporations and tax changes moved strongly in opposite directions. It is likely that if 1919 entered the estimation, the positive impact of a rise in the after-tax share on incorporations would be substantially reduced.

D. Discussion

The finding that is most straightforward to interpret concerns the relative interest rates on tax-free and taxable bonds. The natural interpretation is twofold. The first part concerns the anomalous behavior of the interest-rate ratio following the 1932 tax increase: the obvious possibility is that this pattern is not a consequence of the tax change, but related to the extreme financial stress of this period, the threats of defaults by municipal governments, and the full-blown financial crisis of early 1933. The second part concerns the remainder of the sample. What appears to be occurring is that the taxpayers at the top of the income distribution (who, as described in Section II, paid the vast majority of taxes) were inframarginal, and that the relative interest rates were determined by taxpayers who faced marginal rates that were much lower and moved much less over this period. As a result, the interest-rate ratio moved in the direction one would expect on the basis of the changes in the marginal rates faced by high-income taxpayers, but the movements were muted. This finding is similar in spirit to the findings of the time-series/cross-section analysis of taxable income: taxes were distortionary, but the distortions were small.

The findings for the other measures are somewhat harder to interpret. Taken at face value, they suggest no important effect of tax cuts on machinery investment and commercial and industrial construction, with hints of a possible perverse effect. And they suggest a positive effect of tax cuts on

business formation.

As noted at the start of this section, however, the results of this analysis need to be interpreted cautiously. The data are imperfect and not necessarily fully consistent over time (especially in the case of incorporations), and during this period the economy was subject to a series of enormous shocks with far-reaching consequences. But the result about business incorporations is suggestive. If it holds up under further scrutiny—either of the interwar period or of other settings—it could mean that, despite the apparent lack of strong effects on taxable income and investment, marginal tax rates may have substantial effects on long-run economic performance.

V. CONCLUSION

Determining the incentive effects of marginal tax rates is important for welfare and public policy. This paper shows that the interwar United States provides an excellent laboratory for investigating this issue. Changes in marginal rates were frequent, often dramatic, and very heterogeneous across income groups. In addition, income taxes were paid almost entirely by the wealthy, and changes in the overall level of taxes were generally modest relative to the scale of the economy and accompanied by changes in government spending similar in direction and size to the changes in taxes. As a result, the main channel through which changes in taxes are likely to have affected economic performance is through their impact on incentives.

We use this laboratory to examine the incentive effects of marginal rates in two ways. The first is through time-series/cross-section regressions estimating the responsiveness of taxable income to marginal rates. The use of time-series/cross-section data allows us to control for potential sources of differential trends in income across sub-groups of taxpayers, and more importantly, for aggregate shocks affecting taxpayers' overall taxable income.

The estimates have four important features. First, consistent with what one would expect given the tremendous identifying variation, they are very precise. Second, they show that taxes are indeed distortionary: the null hypothesis of no effect is overwhelmingly rejected. Third, they indicate that the

distortions are small. Our baseline estimate of the elasticity of taxable income with respect to the after-tax share (that is, one minus the marginal tax rate) is approximately 0.2. This is considerably smaller than the findings of postwar studies (though generally within their confidence intervals). If this elasticity characterized the entire population, the constant tax rate that maximized revenue would be over 80 percent. Finally, the estimates are extremely robust.

Our second approach is more speculative. We ask how the time-series variation in overall marginal rates is related to indicators of investment and entrepreneurial activity that could be important to long-run growth. We find clear evidence that one major concern of interwar policymakers was of little importance: the changes in marginal rates in this era had at most small effects on the relative interest rates on municipal and corporate bonds, and so caused only small distortions in the incentives for the composition of investment on this dimension. We find no evidence that high marginal rates reduced machinery investment or business construction, but suggestive evidence that they reduced business formation. This last result, which clearly deserves further study, identifies one possible channel through which high marginal rates might have had important consequences.

The obvious disadvantage of the interwar period for studying the effects of marginal rates is that the economic environment was very different from today's. Thus, one important question raised by our findings is whether changes over the past three-quarters of a century are likely to have substantially increased the distortionary effects of high marginal rates. Another is whether there are features of the interwar tax system—most obviously, its comparative simplicity—that contributed to its relatively low distortionary effects and that could help guide changes in the tax system today.

Table 1
Interwar Tax Legislation

Act (Date Enacted)	Revenue Estimate	Change in Top Marginal Rate (Percentage Points)	Nature of Tax Change
Revenue Act of 1918 (2/24/19)	+\$1,608 million +2.05% of GDP	+10 (1918) -4 (1919)	Raised normal tax rates in 1918 and then lowered partially in 1919; raised surtax rates; introduced war-profits tax
Revenue Act of 1921 (11/23/21)	-\$835 million -1.14% of GDP	-15	Reduced surtax rates; changed treatment of capital gains
Revenue Act of 1924 (6/2/24)	-\$341 million -0.39% of GDP	-14.5 (1923) +2.5 (1924)	Reduced both normal and surtax rates by roughly 25 percent
Revenue Act of 1926 (2/26/26)	-\$326 million -0.34% of GDP	-21	Reduced surtax rates roughly in half; large increase in personal exemption
Revenue Act of 1928 (5/29/28)	-\$233 million -0.24% of GDP	0	Increased earned-income credit; primarily changed corporate income tax
Joint Resolution No. 133 (12/16/29)	-\$160 million -0.15% of GDP	-1 (1929) +1 (1930)	Temporarily reduced the normal personal income tax and the corporate income tax by 1 point
Revenue Act of 1932 (6/6/32)	+\$1,121 million +1.91% of GDP	+38	Raised normal and surtax rates; surtax rates doubled at most income levels; raised corporate income tax and excise taxes
Revenue Act of 1934 (5/10/34)	+\$258 million +0.39% of GDP	0	Rearranged normal and surtax rates; changed treatment of capital gains; closed loopholes
Revenue Act of 1935 (8/30/35)	+\$270 million +0.37% of GDP	+16	Raised surtax rates on incomes over \$50,000; raised estate tax; established graduated corporate income tax
Revenue Act of 1936 (6/22/36)	+\$620 million +0.74% of GDP	0	No change in personal tax rates; subjected dividends to normal tax; large change in corporate tax, including graduated tax on undistributed profits

Table 1 (Continued)
Interwar Tax Legislation

Act (Date Enacted)	Revenue Estimate	Change in Top Marginal Rate (Percentage Points)	Nature of Tax Change
Revenue Act of 1937 (8/26/37)	Trivial	0	Raised surtax on undistributed net income of personal holding companies; closed loopholes
Revenue Act of 1938 (5/28/38)	Trivial	0	Changed treatment of capital gains so tax depended on how long asset was held; largely eliminated undistributed profits tax; made other fundamental changes in corporate income tax
Revenue Act of 1939 (6/29/39)	Trivial	0	Extended a number of existing excise taxes; made revenue-neutral changes to corporate income tax
Revenue Act of 1940 (6/25/40)	+\$1,004 million +0.99% of GDP	+7.9	Lowered personal exemption; raised surtax rates on incomes between \$6,000 and \$100,000; temporary “defense tax” equal to 10 percent of all regular taxes
2 nd Revenue Act of 1940 (10/8/40)	+\$700 million +0.69% of GDP	0	Raised corporate income tax rates; introduced new graduated excess profits tax on corporations
Revenue Act of 1941 (9/20/41)	+\$3,500 million +2.76% of GDP	-5.9	Raised surtax rates dramatically except at very top; subjected all income levels to surtax; reduced personal exemption

Table 2
Time-Series/Cross-Section Results

	Estimation Method	Lags Included	Control Variables	Elasticity of Taxable Income with Respect to After-Tax Share ^a
(1)	OLS	None	Year, group dummies	0.189 (0.031)
(2)	IV	None	Year, group dummies	0.189 (0.033)
(3)	OLS	1	Year, group dummies	0.312 (0.048)
(4)	OLS	2	Year, group dummies	0.272 (0.057)
(5)	OLS	None	Year dummies	0.191 (0.031)
(6)	OLS	None	Group dummies	0.090 (0.067)
(7)	OLS	None	Group dummies, real GDP growth	0.067 (0.047)

Note: Standard errors are in parentheses.

^aWhen lags of the tax variable are included (rows 3 and 4), coefficients and standard errors are for sum of coefficients.

Table 3
Robustness of Time-Series/Cross-Section Results

	Specification	Elasticity of Taxable Income with Respect to After-Tax Share
(1)	Baseline	0.189 (0.031)
(2)	Using tax rates applied retroactively, rather than tax rates in effect when income was earned	0.120 (0.038)
(3)	Using baseline tax measure, but including difference between rate applied retro- actively and baseline measure ^a	0.174 (0.037)
(4)	Include separate group dummies by decade	0.191 (0.032)
(5)	Include group dummies interacted with real GDP growth	0.168 (0.035)
(6)	Exclude top 1/200 th of 1 percent of income distribution	0.160 (0.027)
(7)	Exclude war years (1919 and 1940–1941)	0.288 (0.039)
(8)	Restrict sample to period of stable capital gains and corporate taxes (1923–1932)	0.378 (0.037)
(9)	Pre-Depression sample (1919–1929)	0.198 (0.044)
(10)	Depression sample (1930– 1941)	0.186 (0.046)

Notes: All regressions are estimated by OLS and include group and year dummies, and are estimated with no lags. Standard errors are in parentheses.

^aThe coefficient on difference is -0.034 , with a standard error of 0.047 .

Figure 1
Sample Table from the *Statistics of Income, 1933*

TABLE 3.—Individual returns for 1933 by net income classes, showing simple and cumulative distribution of number of returns, net income and tax, and percentages
[For text defining certain items and describing methods of tabulating and estimating data, see pp. 1-5]

Net income classes (Thousands of dollars)	Returns					
	Simple distribution		Cumulative distribution from highest income class		Cumulative distribution from lowest income class	
	Number	Percent of total	Number	Percent of total	Number	Percent of total
Under 1 (est.)	397,676	10.68	3,723,558	100.00	397,676	10.68
1-2 (est.)	1,480,717	39.77	3,325,882	89.32	1,878,393	50.45
2-3 (est.)	914,198	24.55	1,845,165	49.55	2,792,591	75.00
3-4 (est.)	436,081	11.71	930,967	25.00	3,228,672	86.71
4-5 (est.)	162,994	4.38	494,886	13.29	3,391,666	91.09
5-6	92,808	2.49	331,892	8.91	3,434,474	93.58
6-7	55,802	1.49	239,084	6.42	3,540,076	95.07
7-8	38,855	.99	183,482	4.93	3,576,931	96.06
8-9	25,334	.68	146,627	3.94	3,602,265	96.74
9-10	19,155	.52	121,293	3.26	3,621,420	97.26
10-11	14,082	.38	102,138	2.74	3,635,502	97.64
11-12	11,091	.30	88,056	2.36	3,646,593	97.94
12-13	8,661	.23	76,965	2.06	3,655,254	98.17
13-14	7,199	.19	68,304	1.83	3,662,453	98.36
14-15	6,000	.16	61,105	1.64	3,668,453	98.52
15-20	18,281	.49	55,105	1.48	3,686,734	99.01
20-25	10,329	.28	36,824	.99	3,697,063	99.29
25-30	6,663	.18	26,495	.71	3,703,726	99.47
30-40	7,594	.20	19,832	.53	3,711,320	99.67
40-50	4,166	.112	12,238	.330	3,715,486	99.782
50-60	2,434	.065	8,072	.218	3,717,920	99.847
60-70	1,551	.042	5,638	.153	3,719,471	99.889
70-80	917	.025	4,087	.111	3,720,388	99.914
80-90	652	.018	3,170	.086	3,721,040	99.932
90-100	467	.0126	2,518	.068	3,721,507	99.9446
100-150	1,084	.0291	2,051	.0554	3,722,591	99.9737
150-200	406	.0109	967	.0263	3,722,997	99.9846
200-250	188	.0051	561	.0154	3,723,185	99.9897
250-300	101	.0027	373	.0103	3,723,286	99.9924
300-400	86	.0023	272	.0076	3,723,372	99.9947
400-500	55	.0015	186	.0053	3,723,427	99.9962
500-750	56	.0015	131	.0038	3,723,483	99.9977
750-1,000	25	.0007	75	.0023	3,723,508	99.9984
1,000-1,500	32	.0009	50	.0016	3,723,540	99.9993
1,500-2,000	7	.0002	18	.0005	3,723,547	99.9995
2,000-3,000	8	.0002	11	.0003	3,723,555	99.9997
3,000-4,000	1	.0001	3	.0003	3,723,556	99.9998
4,000-5,000	1	.0001	2	.0002	3,723,557	99.9999
5,000 and over	1	.0001	1	.0001	3,723,558	100.00
Total	3,723,558	100.00				

TABLE 3.—Individual returns for 1933 by net income classes, showing simple and cumulative distribution of number of returns, net income and tax, and percentages—Continued

Net income classes (Thousands of dollars)	Net income					
	Simple distribution		Cumulative distribution from highest income class		Cumulative distribution from lowest income class	
	Amount	Percent of total	Amount	Percent of total	Amount	Percent of total
Under 1 (est.)	\$264,784,042	2.41	\$11,008,637,754	100.00	\$264,784,042	2.41
1-2 (est.)	2,093,291,611	19.01	10,743,853,712	97.59	2,358,075,653	21.42
2-3 (est.)	2,295,586,215	20.85	8,650,562,101	78.58	4,653,661,868	42.27
3-4 (est.)	1,484,562,953	13.49	6,354,975,886	57.73	6,138,224,821	55.70
4-5 (est.)	722,895,165	6.57	4,870,412,933	44.24	6,861,119,986	62.33
5-6	506,900,778	4.61	4,147,517,768	37.67	7,368,020,764	66.94
6-7	359,438,117	3.27	3,640,616,990	33.05	7,727,458,881	70.21
7-8	275,126,577	2.50	3,281,178,873	29.79	8,002,585,458	72.71
8-9	214,704,486	1.95	3,006,052,296	27.29	8,217,289,944	74.66
9-10	181,705,488	1.65	2,791,347,810	25.34	8,398,995,432	76.31
10-11	147,564,774	1.34	2,609,642,322	23.69	8,546,560,206	77.65
11-12	127,355,343	1.16	2,462,077,548	22.35	8,673,915,549	78.81
12-13	108,116,073	.98	2,334,722,205	21.19	8,782,031,622	79.79
13-14	97,090,472	.88	2,226,606,132	20.21	8,879,122,094	80.67
14-15	86,898,377	.79	2,129,515,660	19.33	8,966,020,471	81.46
15-20	314,864,071	2.86	2,042,617,283	18.54	9,280,884,542	84.32
20-25	230,196,680	2.09	1,727,753,212	15.68	9,511,081,222	86.41
25-30	182,207,780	1.68	1,497,556,532	13.59	9,693,289,002	88.07
30-40	262,358,726	2.38	1,315,348,752	11.93	9,955,647,728	90.45
40-50	185,438,623	1.68	1,052,990,026	9.55	10,141,086,351	92.13
50-60	132,772,640	1.21	867,551,403	7.87	10,273,858,991	93.34
60-70	100,343,290	.91	734,778,763	6.66	10,374,202,281	94.25
70-80	68,446,005	.62	634,435,473	5.75	10,442,648,286	94.87
80-90	55,295,324	.50	565,989,468	5.13	10,497,943,610	95.37
90-100	44,191,960	.40	510,694,144	4.63	10,542,135,570	95.77
100-150	129,158,784	1.17	466,502,184	4.23	10,671,294,354	96.94
150-200	69,759,240	.63	337,343,400	3.06	10,741,053,594	97.57
200-250	42,081,396	.38	267,584,100	2.43	10,783,134,990	97.95
250-300	27,374,302	.25	225,502,764	2.05	10,810,509,292	98.20
300-400	30,099,524	.27	198,128,462	1.80	10,840,608,816	98.47
400-500	24,471,169	.22	168,028,938	1.53	10,865,079,985	98.69
500-750	34,919,569	.32	143,557,769	1.31	10,899,999,554	99.01
750-1,000	21,780,911	.20	108,638,200	.99	10,921,780,465	99.21
1,000-1,500	38,202,638	.35	86,857,289	.79	10,959,983,103	99.56
1,500-2,000	(1)	(1)	(1)	(1)	(1)	(1)
2,000-3,000	20,182,979	.18	48,654,651	.44	10,980,166,082	99.74
3,000-4,000	(1)	(1)	(1)	(1)	(1)	(1)
4,000-5,000	(1)	(1)	(1)	(1)	(1)	(1)
5,000 and over	(1)	(1)	(1)	(1)	(1)	(1)
Classes grouped together	28,471,672	.26	28,471,672	.26	11,008,637,754	100.00
Total	11,008,637,754	100.00				

Figure 2
Top Marginal Tax Rate

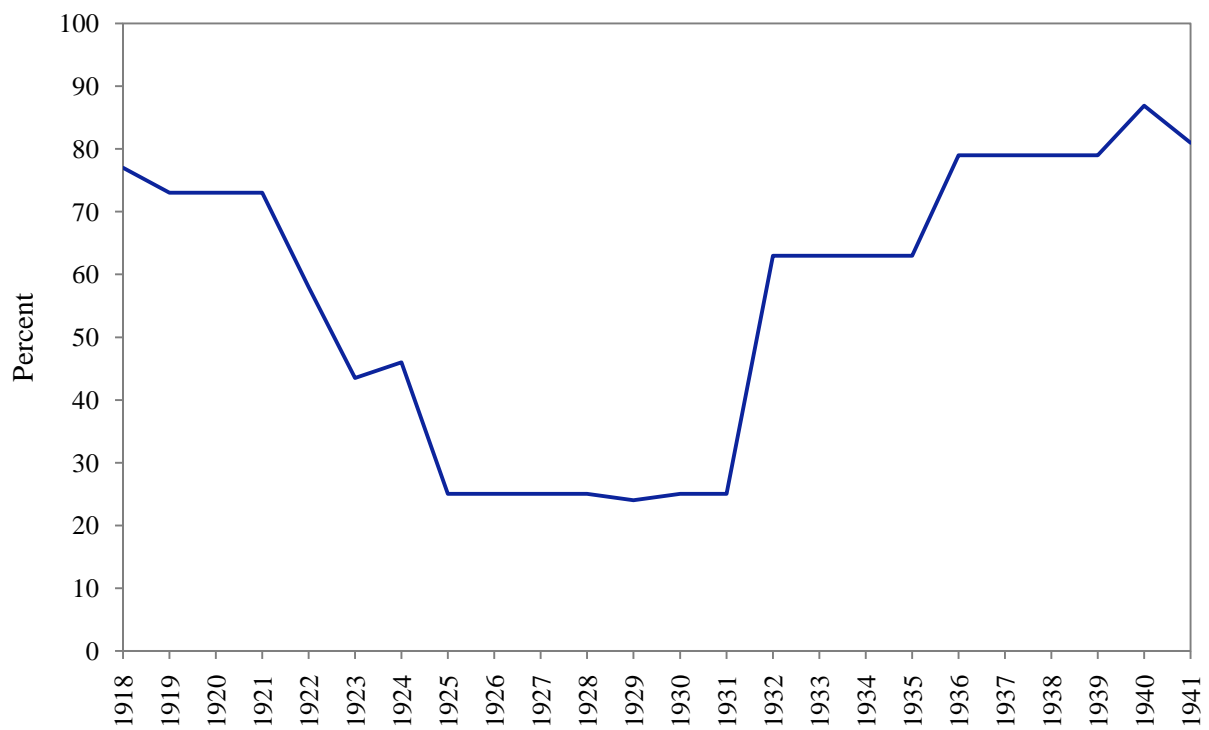
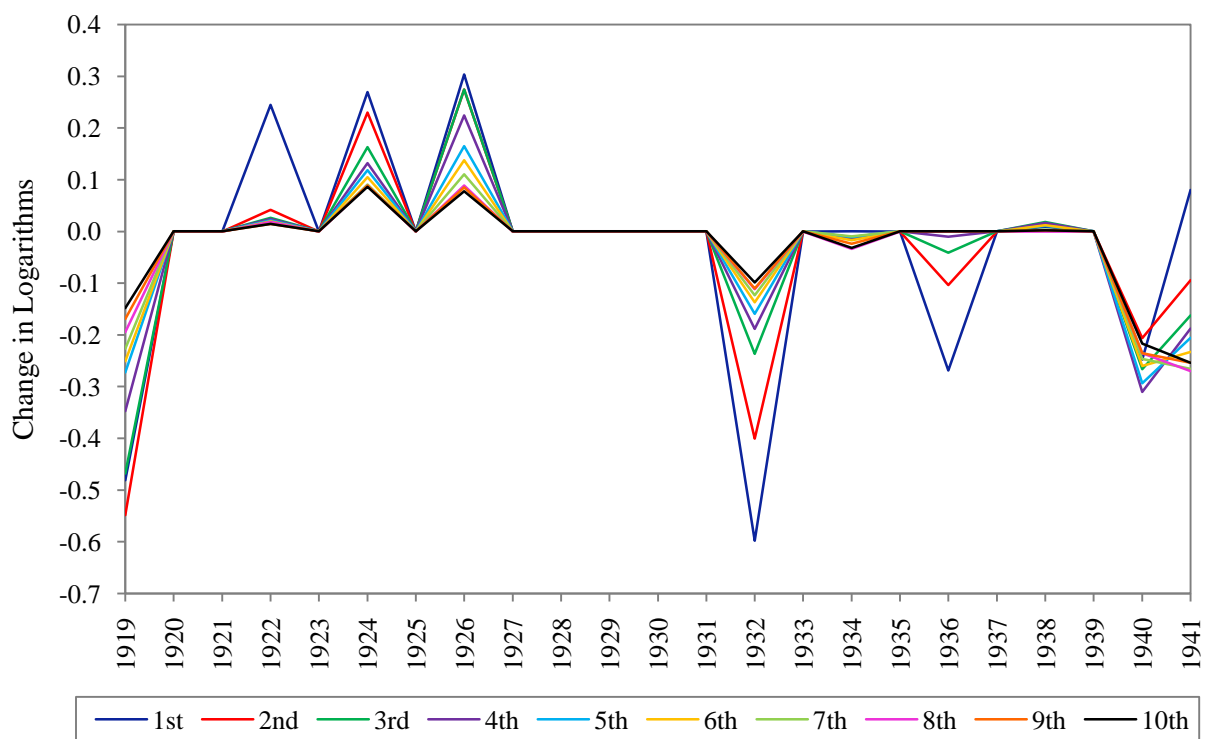


Figure 3
Policy-Induced Change in the Log After-Tax Share for Different Percentile Groups



Note: Each line represents the change for a given tenth of the top 1/20th of 1 percent of the income distribution.

Figure 4
 Percent of Total Income Tax Paid by Tenths of the Top 1/20th of 1 Percent of
 the Income Distribution (Cumulative)

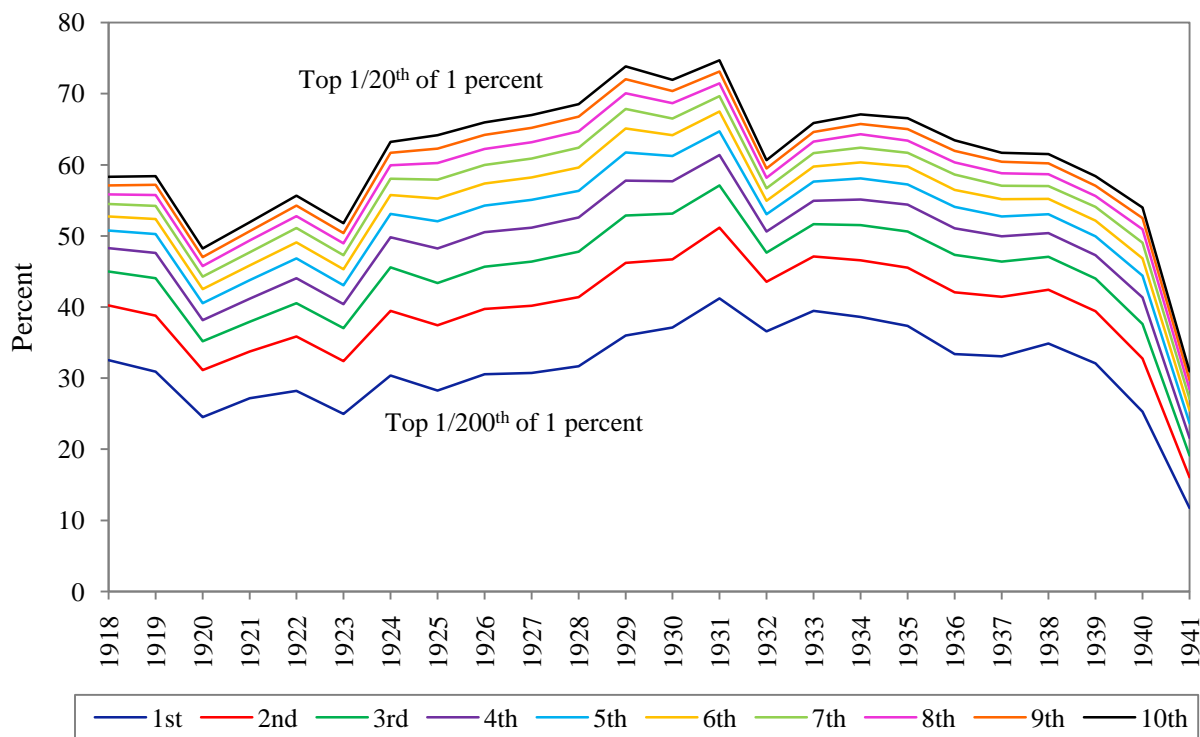
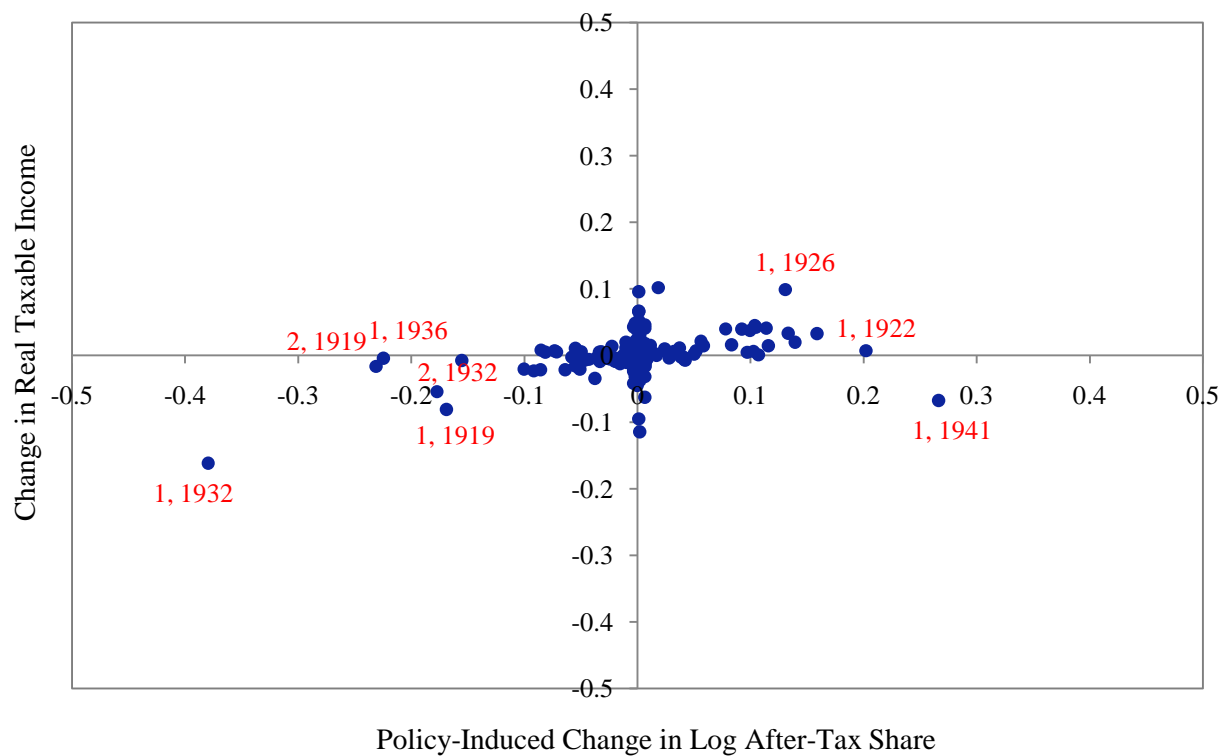


Figure 5
Scatter Plot of Change in Log After-tax Share and Change in Real Taxable Income



Notes: Both variables are expressed as residuals from a regression on the year and group dummy variables. The labels on particular observations report the tenth of the top 1/20th of 1 percent of the income distribution (1 to 10) and which year (1919 to 1941) the observation corresponds to.

Figure 6
Investment and Policy-Induced Changes in the Log After-Tax Share

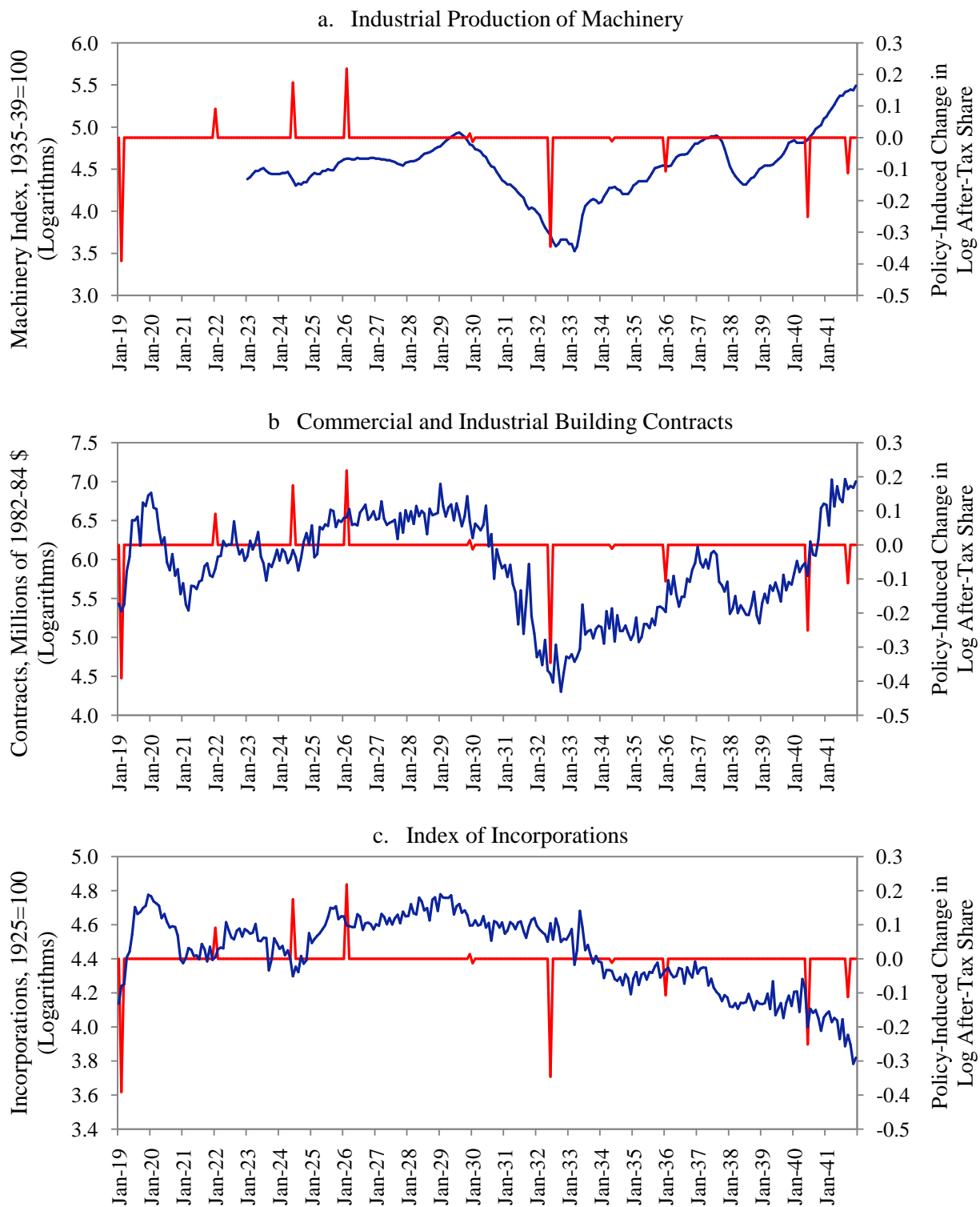


Figure 7
Ratio of Municipal Bond Rate to AAA Rate and After-Tax Share

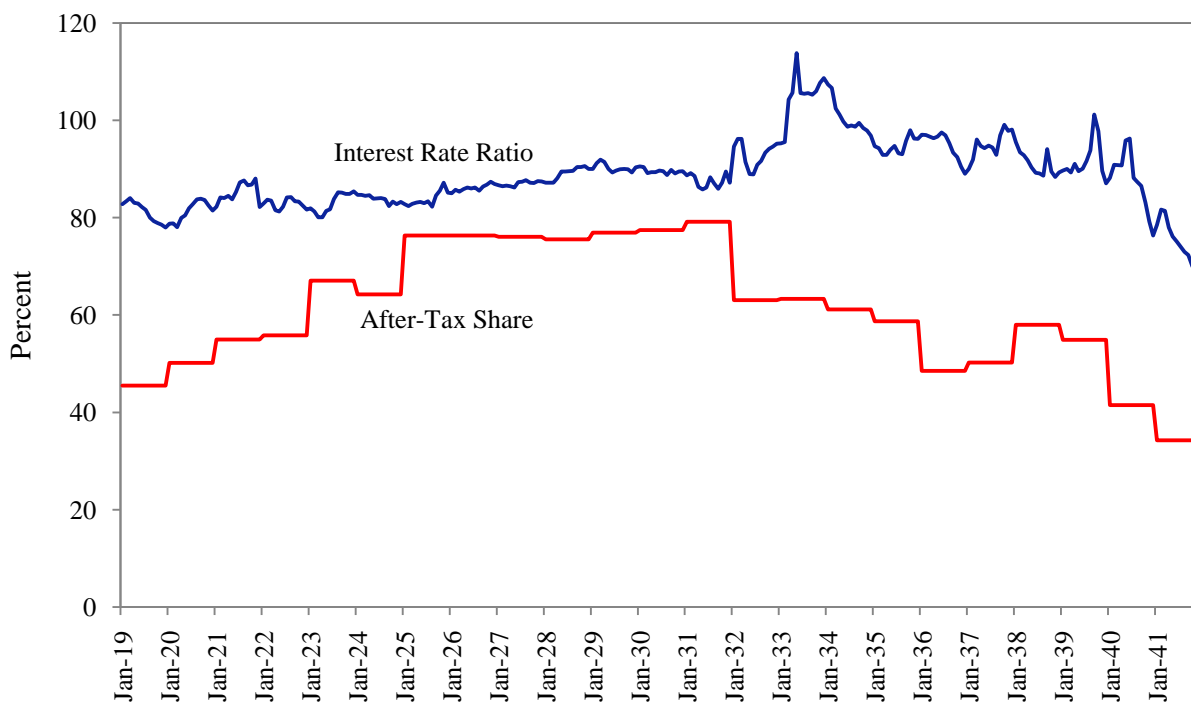


Figure 8
Impulse Response of Investment to a 1 Percent Rise in the Log After-Tax Share

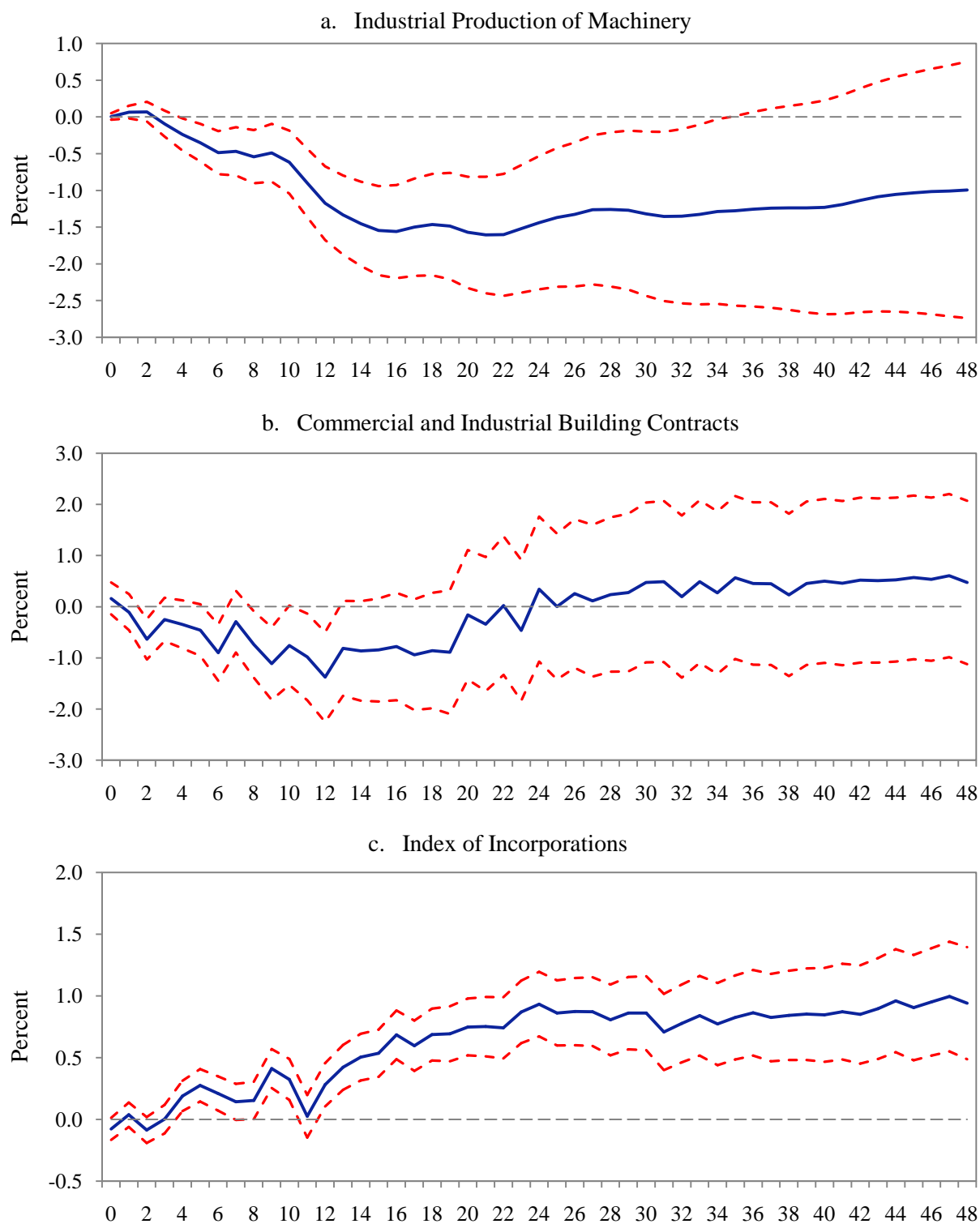
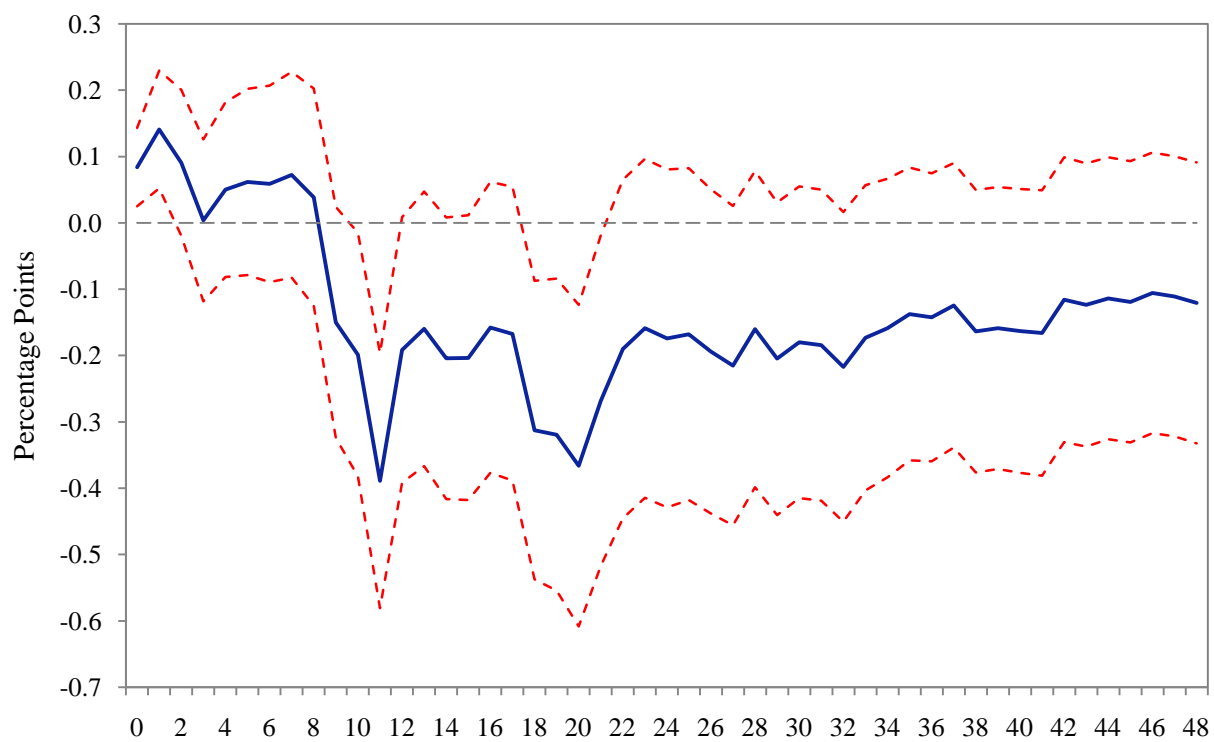


Figure 9
Impulse Response of the Interest Rate Ratio to a 1 Percentage Point
Rise in the After-Tax Share



APPENDIX

DETAILS OF COMPUTATIONS OF CHANGES IN TAXABLE INCOME AND CHANGES IN MARGINAL RATES

This appendix describes how we construct data for taxable income exclusive of capital gains and losses, marginal rates on non-capital-gains income, and policy-induced changes in marginal rates from the data in the *Statistics of Income*.

A. Overview

Income. Obtaining the income data we need for taxpayers in a given range of net income involves two steps. First, and most important, we need to remove capital gains and losses. Second, because we do not want to include income changes that resulted from changes in how taxable income was defined, we need to correct for changes in the definition of taxable income. Throughout our sample period, taxable non-capital-gains income was very similar to net income excluding capital gains and losses, and the definition of the non-capital-gains components of net income did not change. We therefore use net income excluding capital gains and losses—which we refer to as “ordinary taxable income”—as our income measure throughout.

Marginal Rates. To find the marginal rate on non-capital gains income faced by households in a given range of net income, we need to exclude any portion of their net income that was either untaxed or taxed separately. For example, in most of the 1920s capital gains income was taxed at a separate rate, and in much of the 1930s a portion of capital gains income was excluded from taxable income. We can then find the marginal rate that applied to the relevant level of taxable income.

The personal income tax in the interwar era had two components: a “normal” tax and a “surtax.” Normal tax rates were low, typically on the order of 4 percent, relatively stable, and only slightly graduated. Surtax rates, in contrast, were often very high, volatile, and extremely progressive. In all of our analysis, we look at the combined effects of the two components to measure marginal rates.

In cases where legislation changed the tax code retroactively, our baseline measure uses the definition of taxable income and tax rates that were in effect during the year, not the rates that were applied ex post. However, we also compute marginal rates using the ex post rates.

Policy-Induced Changes. A key input into our analysis is the change in marginal rates that was the result of policy (rather than of economic developments changing households’ incomes, and thus moving them into different tax brackets). To find the policy-induced change in marginal rates in year t , we compute marginal rates on year $t - 1$ income using the definition of taxable income and the tax rates that were in effect in year t and compare them with the marginal rates implied by the definition of taxable income and the tax rates that were in effect during year $t - 1$.

Interpolation. The income categories in the *Statistics of Income* do not correspond exactly to the groups we want to use in our statistical work. For example, our top percentile group in some year might include the filers in all income categories over \$200,000 plus a certain number of filers in the \$150,000–\$200,000 category. To estimate the total ordinary taxable income of the top percentile group, we would therefore need to estimate the division of the taxable income in the \$150,000–\$200,000 group between the taxpayers who are in the top percentile group and those who are not. Similarly, everyone in the \$90,000–\$100,000 range in some year might have been in our second percentile group, but the marginal

rate might have changed within this range. To estimate the average marginal rate faced by the second percentile group, we would therefore need to estimate the fraction of the overall income of the \$90,000–\$100,000 that was taxed at each relevant marginal rate.

The *Statistics of Income* reports the number of households in each range of net income. We follow the standard practice of modeling high incomes as following a Pareto distribution. We fit a Pareto distribution to the ranges of net income at the top of the income distribution for each year, and assume that incomes within each range follow this distribution. This allows us to find the total ordinary taxable income of each percentile group and group’s weighted average log after-tax share.

The remainder of this appendix describes the specifics of how we construct the figures we need for our analysis.

B. Capital Gains Corrections

The *Statistics of Income* reports some data on capital gains by income category throughout our sample period. But because both the tax code and the data on capital gains and losses in the *Statistics of Income* changed over our sample, our procedure for subtracting capital gains (and adding capital losses) from the reported figures for overall net income is slightly different in different years.

1918–1921. We estimate ordinary taxable income by subtracting “Profits from sales of real estate, stock, bonds, etc.” from net income. The *Statistics of Income* for these years does not report data on the net capital losses of taxpayers who had net losses. As a result, although taxpayers could deduct these losses in computing ordinary net income, we are unable to add them back into the net income figures. Thus, our estimates for this period correspond to ordinary taxable income minus net capital losses. In later years when data on net capital losses are available, they are only about 5 percent of net income for high-income taxpayers. Other studies of tax responsiveness also neglect net capital losses (for example, Gruber and Saez, 2002).

1922–1923. The *Statistics of Income* breaks capital gains into short-term and long-term. We subtract both from reported net income. As with 1918–1921, we are unable to add net capital losses back into the net income figures.

1924–1925. We again subtract both short-term and long-term capital gains from net income. However, the resulting concept is slightly different than in earlier years. Starting in 1924, long-term net capital losses could no longer be claimed as a deduction in computing net income, but instead could be claimed as a 12½ percent tax credit. Thus, when we subtract capital gains from net income, the result is ordinary net income less net short-term capital losses (rather than ordinary taxable income less all net capital losses), which is closer to what we want conceptually.

This change means that there is a conceptual discontinuity in our income measure from 1923 to 1924. To prevent it from affecting our results, when we compute the percentage change in income from 1923 to 1924, we use our 1924 income figures minus eight times the 12½ percent tax credit for long-term capital losses. As a result, we are finding the change in a consistent series (ordinary taxable income minus all capital losses).

1926–1933. Starting in 1926, the *Statistics of Income* includes data on net short-term capital losses (which continued to be deductible in computing net income). We therefore subtract both short-term and long-term capital gains from net income as before, and add short-term capital losses. The resulting measure corresponds to taxable income excluding all capital gains and losses. This change

again introduces a discontinuity in our measure. To prevent it from affecting our results, when we compute the percentage change in income from 1925 to 1926, we do not add short-term capital losses to the 1926 income figures.

1934–1937. We subtract “net capital gain” from net income and add “net capital loss.” As with our figures for 1926–1933, the resulting figures correspond to ordinary taxable income.

1938. Beginning in 1938, some assets were no longer classified as capital assets, and gains and losses on them were treated differently than other capital gains and losses. However, data on these gains and losses are reported in the *Statistics of Income*. We therefore subtract gains on all assets from net income and add losses on all assets (other than short-term losses on assets classified as capital assets, which could not be deducted in computing net income). Again, the resulting figures correspond to ordinary taxable income.

1939–1941. Starting in 1939, short-term losses on assets classified as capital assets from the previous year could be carried forward and deducted against the current year’s capital gains. Since these losses are subtracted in the computation of net income, we add them back in. The remainder of the calculation of ordinary net income is the same as for 1938.

C. Actual and Policy-Induced Changes in Marginal Tax Rates

Knowing a household’s capital gains income and its net income exclusive of capital gains is almost, but not quite, enough to know what its tax liability was, and hence the marginal rate it faced on non-capital-gains income. A household’s computation of both its normal tax and its surtax began with its net income (sometimes, as described above, excluding some or all of capital gains), which equaled gross income less deductions. However, the steps from net income to tax due were slightly different for the two taxes.

For the normal tax, there were several items other than capital gains that received special treatment. A personal exemption and a credit for dependents were subtracted from net income; until 1936, dividends were excluded; and from 1934 to 1941, 10 percent of the first \$14,000 of earned income was also subtracted. Finally, from 1924 to 1931, the normal tax on earned income was reduced by a credit of 25 percent of the normal tax the taxpayer would have had to pay if his or her unearned income was zero. The amount of earned income eligible for the credit varied between \$10,000 and \$30,000. Fortunately for our purposes, however, normal tax rates were low, and the maximum marginal normal rate was reached at relatively low levels of income. We therefore neglect these complications and assume that all taxpayers at the income levels we are considering paid the top marginal normal rate.¹¹

For the surtax, the computation of the tax was simpler. The relevant taxable income was either non-capital-gains income (in the years when capital gains were taxed separately) or non-capital-gains income plus the taxable portion of capital-gains income (in years when some or all of capital gains were taxed with other income).

There were only two minor complications with the surtax. First, starting in 1934, the personal exemption and credit for dependents were subtracted from net income for purposes of the surtax as well as for the normal tax. We have figures on personal exemptions and credits by income range for each

¹¹ Note that by assuming that taxpayers paid the top marginal normal rate, we are implicitly finding the marginal rate on non-capital gains, non-dividend income. Computing the weighted average marginal rate on all non-capital gains income would yield slightly lower marginal rates before 1936.

year, so we can subtract these from income before finding marginal rates. For high-income taxpayers, the deductions were small relative to income, and so the effects of this adjustment are minor.

Second, from 1924 to 1931, the same 25 percent tax credit on earned income up to some limit that applied to the normal tax also applied to the surtax. Because most high-income taxpayers were beyond the limit, and because the credit reduced marginal rates by no more than a few percentage points for the others, we neglect this complication.

When legislation changed only tax rates and not the computation of taxable income, finding the policy-induced change in marginal rates at a given level of taxable income is straightforward: the policy-induced change is just the change in the marginal rate at that level of income. When legislation changed how taxable income was computed from year $t - 1$ to year t , the situation is slightly more complicated. Consider a household with a given level of taxable income in year $t - 1$. We need to estimate what the household's taxable income would have been using the year t definition, and then find what the marginal rate would have been at that level. We discuss each case where the definition of taxable income changed in turn.

1921 to 1922. Beginning in 1922, the normal and surtax rates applied to income excluding capital gains, and capital gains were taxed separately. Thus, to know what the relevant taxable income of a 1921 taxpayer would have been under 1922 rules, we should subtract long-term capital gains from the taxpayer's 1921 income. Unfortunately, the 1921 *Statistics of Income* does not separate long-term and short-term capital gains. We therefore subtract all capital gains, times the proportion of all capital gains in 1922 for the relevant income group that were long-term. Because capital gains were only a few percent of income in 1921, the effects of this correction are small.

1923 to 1924. Beginning in 1924, long-term capital losses could no longer be deducted from taxable income, but instead resulted in a separate tax credit. The taxable income of a 1923 household under 1924 law therefore equaled its 1923 taxable income plus any long-term capital losses. Since we do not have data on 1923 capital losses, we assume that long-term capital losses as a share of net income for each income group were the same in 1923 as in 1924. We then add the resulting estimates of long-term capital losses in 1923 to the reported 1923 incomes for each group to obtain an estimate of what their taxable income would have been under 1924 law. The effects of this correction are small.

1933 to 1934. There were two changes to how taxable income was calculated in 1934. First, the treatment of long-term capital gains and losses was changed. Rather than being taxed separately, a portion of these gains and losses was included in taxable income, with the fraction varying by the holding period. In addition, the deduction for capital losses (net of any gains) was capped at \$2000. In 1933, short-term capital losses and short-term gains (both of which were included in the computation of income subject to the surtax) were similar in magnitude, and long-term losses were much larger than long-term gains. In 1934, reported capital gains income and deductions for losses were similar in magnitude. That is, in both years capital gains and losses on net had little impact on income subject to the surtax. We therefore make no adjustment for the change in the treatment of gains and losses.

Second, starting in 1934 the personal exemption and credit for dependents could be deducted from income subject to the surtax. In finding the taxable incomes for the purposes of the surtax that 1933 taxpayers would have had under 1934 law, we therefore subtract their exemptions and dependent credits.

1937 to 1938. Starting in 1938, capital gains and losses on assets held more than 18 months were again taxed at a separate rate. Gains on assets held less than 18 months, however, were now entirely included in taxable income, and none of current-year net losses could be deducted in computing net income. The 1937 *Statistics of Income* does not separate capital gains income by holding period. The

1938 *Statistics of Income*, however, separates it according to whether the holding period was more or less than 18 months. To approximate the effect of the change on the relevant taxable income a 1937 taxpayer would have had under the 1938 code, we assume that this division for a given income range was the same in 1937 as in 1938. We assume that half of the long-term capital gains were already excluded in 1937 (the actual fraction varied from 20 to 70 percent depending on the holding period), and that none of the short-term gain was excluded. Since all long-term gains were taxed separately in 1938, this allows us to estimate how much lower a taxpayer's relevant taxable income would have been under the 1938 rules. We also add back in net losses, since these were no longer deductible.

1939 to 1940. The Revenue Act of 1940 lowered all personal exemptions by 20 percent. To find the taxable incomes that 1939 taxpayers would have had under 1940 law, we therefore add back in 20 percent of their personal exemptions. The effects of this adjustment are minor.

1940 to 1941. In 1941, personal exemptions were reduced by an additional 25 percent for joint filers and 6 percent for other taxpayers. 57 percent of the value of all personal exemptions in 1940 was claimed by joint filers (1940 *Statistics of Income*, p, 121). To find the taxable incomes that 1940 taxpayers would have had under 1941 rules, we therefore add back in 17 percent of their personal exemptions. The effects of this adjustment are again minor.

D. Retroactive Changes

If a change to the tax code was enacted at the end of the year or after the end of the year, our baseline measure of tax rates uses the rates that were in effect during the year, not the rates that were applied ex post. For the one case where a change was enacted after mid-year but well before year-end, we try both approaches.

There are five cases of retroactive changes enacted after mid-year. Tax bills enacted in 1919, 1924, and 1926 changed taxes for the previous year; a Congressional resolution enacted on December 16, 1929 changed 1929 taxes; and the Revenue Act of 1941, enacted on September 20, 1941, changed 1941 taxes. Our baseline measures of marginal rates and policy-induced changes in marginal rates use the tax code in effect during 1918, 1923, 1925, and 1929, and ignore the retroactive changes. The 1941 change, however, was in effect for a non-trivial part of the year, and taxpayers likely knew before the bill was passed that tax rates would be raised. Our baseline measure therefore uses the rates specified by the 1941 act in computing marginal rates. However, we also consider the effects of coding this as no change in rates in 1941. The treatment of 1941 has no important effect on our results. In addition, as described in the text, we also consider series for marginal rates that use the rates that were applied ex post in all cases.

E. Interpolation and Aggregation to Construct Data for Percentile Groups

To construct figures for different percentile groups rather than for the income ranges in the *Statistics of Income*, we often need estimates of the breakdown of income within a given income range. For example, if some but not all of the filers in the \$150,000–\$200,000 range were in the top percentile group in some year, we need to estimate the fraction of the ordinary taxable income of the filers in that range that went to the filers in the top percentile group. Similarly, suppose all households in the \$90,000–\$100,000 range were in the second percentile group in some year, but the marginal rate changed within this range. Then we need to estimate the fraction of the income of this group accruing to households facing each marginal rate.

The highest income ranges in the *Statistics of Income* usually have fewer than a hundred

households. And, the other income ranges that are relevant to our analysis are generally narrow, such as \$90,000–\$100,000. As a result, our estimates are not sensitive to the details of our interpolation procedure. The specific approach we use is to fit a Pareto distribution for each year to the income categories at or above the category that includes the return at the 99.95th percentile of the income distribution in that year. Because the treatment of the very top income categories in the *Statistics of Income* varies over time, we aggregate the taxpayers with incomes over \$1.5 million into a single category. This group always includes less than 1/1000th of 1 percent of the income distribution.

The Pareto distribution function is

$$(A1) \quad F(Y) = 1 - \left(\frac{k}{Y}\right)^\theta \quad \text{for } Y \geq k.$$

Let L_i and H_i denote the bottom and top of income category i , and assume that $L_i > k$. The probability that a return falls in category i is

$$(A2) \quad P_i = \left(\frac{k}{L_i}\right)^\theta - \left(\frac{k}{H_i}\right)^\theta.$$

Thus, the likelihood function is

$$(A3) \quad L = \left(\prod_{i=1}^M P_i^{N_i}\right) \frac{N!}{N_1! N_2! \dots N_M!},$$

where M is the number of income categories, N_i is the number of returns in category i , and N is the total number of returns in the sample we are considering. The log likelihood function is therefore

$$(A4) \quad \ln L = K + \sum_i^M N_i \ln P_i,$$

where $K \equiv \ln(N!) - \sum_{i=1}^M \ln(N_i!)$. Note that K does not depend on the parameters of the distribution.

We estimate the model by maximum likelihood for each year. The number of income categories in the sample varies from 13 to 18. The estimates of θ (which is the parameter relevant to the interpolation) range from 1.41 in 1929 to 2.01 in 1920. These estimates are similar to other estimates for income distributions. The estimates are extremely precise: the standard error for the estimate of θ is always less than 0.001.

We then use the Pareto parameters to construct the data that we need on ordinary taxable incomes by percentile group. Suppose, for example, that 40 percent of the filers in the \$150,000–\$200,000 range are in the top percentile group in some year, and that the estimate of θ for that year is 1.5. Then the assumption that incomes follow a Pareto distribution implies that 43.5 percent of the income of the filers in this range went to those in the top percentile group.

Similarly, we use the Pareto parameters to estimate each percentile group's income-weighted average log after-tax share and the policy-induced change in a group's income-weighted average log after-tax share.¹² For example, consider again a case where all households in the \$90,000–\$100,000

¹² To see why the change in the log of the taxable income of a group should be related to the change in the group's income-weighted log after-tax share, suppose the taxable income of the household at percentile i of the income distribution in year t is given by $\ln y_{it} = \alpha_i + \beta_t + \gamma \ln S_{it} + \varepsilon_{it}$, where S_{it} is the household's after-tax share. Then $\ln y_{i,t+1} - \ln y_{it} = \tilde{\beta}_{t+1} + \gamma(\ln S_{i,t+1} - \ln S_{it}) + \tilde{\varepsilon}_{i,t+1}$ (where $\tilde{\beta}_{t+1} \equiv \beta_{t+1} - \beta_t$, $\tilde{\varepsilon}_{i,t+1} \equiv \varepsilon_{i,t+1} - \varepsilon_{it}$). This

range are in the second percentile group. Suppose that 5 percent of the income of the filers in this range was either untaxed or taxed separately, that one marginal rate applied to \$80,000–\$90,000 and a higher one to \$90,000–\$100,000, and that the Pareto parameter for the year is 1.5. Then our assumptions imply that 49.3 percent of the ordinary taxable income of the filers in this range was taxed at the lower marginal rate and 50.7 percent was taxed at the higher rate. This would be one part of the overall weighted average for this percentile group. Similarly, to find the policy-induced change from one year to the next, we find the marginal rate at each level of income under each year’s tax code, weight using the first year’s income distribution, and find the difference.

The calculations described in this appendix are clearly not exact. Most importantly, we assume that quantities that we need to subtract from net income, such as capital gains, are a constant proportion of income within each income range. As Barro and Sahasakul (1983) observe in a different context, the aggregates will be reasonably accurate either if the quantities we need to subtract from net income do not vary greatly as a share of income among members of the group or if the log after-tax share is approximately linear in taxable income over the relevant range. In our case, because the adjustments involve only a moderate fraction of net income, and because the log after-tax share fell fairly steadily with income, the approximation error is likely to be small. And because the actual changes in marginal rates in this period were so large, even moderate errors would have little impact on our estimates.

As a check on our calculations, we have computed the implications of our assumptions for the amount of taxes paid by the households in selected income ranges for certain years. We find that the calculations match actual taxes paid quite well, sometimes remarkably so.¹³

in turn implies $y_{i,t+1} - y_{it} \cong y_{it}[\tilde{\beta}_{t+1} + \gamma(\ln S_{i,t+1} - \ln S_{it}) + \tilde{\varepsilon}_{i,t+1}]$. Thus, summing over members of the percentile group being considered, and letting Y_t be the total taxable income of the group, we have:

$$\begin{aligned} \frac{\sum_i (y_{i,t+1} - y_{it})}{Y_t} &\cong \sum_i \left(\frac{y_{it}}{Y_t} \right) y_{it} [\tilde{\beta}_{t+1} + \gamma(\ln S_{i,t+1} - \ln S_{it}) + \tilde{\varepsilon}_{i,t+1}] \\ &= \gamma \sum_i \frac{y_{it}}{Y_t} (\ln S_{i,t+1} - \ln S_{it}) + v_{t+1}, \end{aligned}$$

where $v_{t+1} \equiv \tilde{\beta}_{t+1} + \sum_i \left(\frac{y_{it}}{Y_t} \right) \tilde{\varepsilon}_{i,t+1}$. This in turn implies the posited relationship:

$$\ln Y_{t+1} - \ln Y_t \cong \gamma \left(\sum_i \frac{y_{it}}{Y_t} \ln S_{i,t+1} - \sum_i \frac{y_{it}}{Y_t} \ln S_{it} \right) + v_{t+1}.$$

¹³ The largest discrepancies we have found involve the normal tax before 1936. The discrepancies appear to stem from the fact that dividends were exempt from the normal tax until 1936, and some high-income households had sufficiently high deductions and low non-dividend income that excluding only a portion of their dividend income was enough to reduce their normal tax liability to zero. Thus, our estimates appear to overstate average marginal rates for these years. But, since the marginal normal tax rate was low and most high-income households paid some normal tax, the errors appear small.

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