

The Incentive Effects of Marginal Tax Rates: Evidence from the Interwar Era[†]

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This paper uses the interwar United States as a laboratory for investigating the incentive effects of marginal income tax rates. We examine the impact of the large changes in rates in this period on taxable income using time-series/cross-section analysis of data by small slices of the income distribution. We find that the effect operated in the expected direction but was economically small, and that it is precisely estimated and highly robust. We also find suggestive time-series evidence of a positive impact of marginal rate cuts on business formation, but no evidence of an important effect on other indicators of investment. (JEL D31, H24, H31, M13, N42)

A central concern of tax policy is the incentive effects of marginal income tax rates. Do high marginal rates give rise to income shielding? Do they reduce labor supply? 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 the effects of marginal rates using data from the postwar United States. The variation in tax rates in this period, however, is relatively small. As a result, the effects of tax changes are often measured imprecisely.

In contrast, marginal tax rates moved frequently and dramatically in the United States in the period between the two world wars. The top marginal income tax rate at the end of World War I was 77 percent; by 1929 it had been reduced to 24 percent; by 1936 it was back up to 79 percent. Furthermore, the 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 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. The top two-tenths of 1 percent of the income distribution paid roughly 95 percent of the individual income tax. The vast majority of

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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 income distribution.

To analyze the responsiveness of high incomes to changes in marginal rates, we estimate time-series/cross-section regressions of changes in real taxable income by slices of the income distribution on policy-induced changes in the after-tax share (that is, 1 minus the marginal rate). We find that changes in marginal rates have a statistically significant effect on reported taxable income. However, the effects are modest. The estimated elasticity of income with respect to the after-tax share is about 0.2, which 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, the interwar estimates are more precise than most postwar estimates. The results suggest that short-run income-shielding or labor-supply effects of marginal rate changes, while clearly present, were of limited economic significance.

This finding is very robust. Eliminating outliers, clustering standard errors, allowing for differential trends across income groups, and considering the possibility of lagged responses 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. More generally, we find that the estimated elasticity is remarkably stable across time. We also find that different types of income, such as wages and salaries, capital income, and entrepreneurial income, respond similarly to changes in marginal rates.

Our time-series/cross-section analysis inherently focuses on the short-run effects of changes in tax rates. But even if rate changes have little impact on reported income in the near term, they could nevertheless affect longer-run growth through physical- and human-capital investment, innovation, career choices, risk-taking, and other channels. Definitively addressing the importance of these channels in the interwar era is not possible. But to shed some light on them, we consider time-series evidence on the response of a number of indicators of investment activity and business formation to the aggregate policy-induced change in the after-tax share of high-income earners. Because this analysis uses only the time-series variation in marginal rates, it is difficult to separate the effects of tax changes from the large cyclical movements in investment over this period. As a result, this part of our study is inherently more speculative than the time-series/cross-section analysis.

The time-series data provide no evidence that the large swings in the after-tax share in the interwar era had a significant impact on investment in new machinery or commercial and industrial construction, but some evidence that increases in the share had a positive effect on 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 results concern an environment distant from today's. Technology, culture, and politics all differed greatly from those of modern economies, and the era we study witnessed the Great Depression and global upheaval. More mundanely, the structure and complexity of the tax code, the tax enforcement regime, and the sources of income of wealthy taxpayers differed considerably from their modern counterparts. Thus, our findings cannot be applied blindly to today's economy.

Nonetheless, our findings may have implications for modern environments. Most obviously, they show that a high responsiveness of the taxable income of the wealthy to marginal tax rates is not inevitable. More importantly, they raise the question of whether there are features of the interwar tax system that contributed to its relatively low distortionary effects and that could help guide changes to today's tax system. Preliminary examination of the sources of our low estimated responsiveness casts doubt on the hypotheses that it resulted from taxpayer naïveté or from the composition of taxpayers' income. Instead, it suggests that the simplicity of the tax system may have played a significant role, and leaves open the issue of the role of the enforcement regime.

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). Only a few papers consider the incentive effects of interwar tax changes. The one that is closest to ours methodologically is 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), and concludes that the episodes suggest very different responses to changes in rates. Our analysis differs from his 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 more fully accounting for the specifics of the tax code in computing marginal tax rates and by considering the responses of various indicators of investment. We discuss the stability of the estimates over time and the relationship between our approach and Goolsbee's (1999) in more detail below.¹

The remainder of the paper is organized as follows. Section I discusses the interwar tax system, the nature and history of interwar tax changes, and our estimates of policy-induced changes in marginal rates by slice of the income distribution. Section II presents our basic time-series/cross-section estimates of the response of reported income to policy-induced changes in marginal rates. Section III investigates the robustness of our results, and analyzes the stability of the estimates across time and across different types of income. Section IV reports our findings on the time-series relationship between changes in marginal rates and productive investment and entrepreneurial activity. Section V presents our conclusions.

¹Brownlee (2000) and Smiley and Keehn (1995) also examine interwar tax changes. Brownlee analyzes the political economy of the changes and policymakers' beliefs about their impact on incentives. 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 ranges of taxable income, finding a significant negative relationship. 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.

TABLE 1—SIGNIFICANT INTERWAR TAX LEGISLATION

Act (date enacted)	Revenue estimate	Change in top marginal rate, percentage points	Nature of tax change
Revenue Act of 1918 (February 24, 1919)	+\$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 (November 23, 1921)	-\$835 million -1.14% of GDP	-15	Reduced surtax rates; changed treatment of capital gains
Revenue Act of 1924 (June 2, 1924)	-\$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 (February 26, 1926)	-\$326 million -0.34% of GDP	-21	Cut surtax rates roughly in half; large increase in personal exemption
Revenue Act of 1928 (May 29, 1928)	-\$233 million -0.24% of GDP	0	Increased earned-income credit; reduced corporate income tax rate slightly
Joint Resolution No. 133 (December 16, 1929)	-\$160 million -0.15% of GDP	-1 (1929) +1 (1930)	Temporarily reduced the normal personal income tax and the corporate income tax by 1 percentage point
Revenue Act of 1932 (June 6, 1932)	+\$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
National Industrial Recovery Act (June 16, 1933)	+\$154 million +0.27% of GDP	0	Introduced or increased taxes on capital, excess profits, dividends, and gasoline; the taxes ended when Prohibition ended (December 5, 1933)
Revenue Act of 1934 (May 10, 1934)	+\$258 million +0.39% of GDP	0	Rearranged normal and surtax rates; changed treatment of capital gains; closed loopholes
Social Security Act (August 14, 1935)	+\$909 million +1.24% of GDP	0	Created employee and employer taxes on wages up to \$3000, and unemployment insurance tax on employer payrolls
Revenue Act of 1935 (August 30, 1935)	+\$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 (June 22, 1936)	+\$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
Revenue Act of 1937 (August 26, 1937)	Trivial	0	Raised surtax on undistributed net income of personal holding companies; closed loopholes
Revenue Act of 1938 (May 28, 1938)	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 (June 29, 1939)	Trivial	0	Extended a number of existing excise taxes; made revenue-neutral changes to corporate income tax
Revenue Act of 1940 (June 25, 1940)	+\$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
2nd Revenue Act of 1940 (October 8, 1940)	+\$700 million +0.69% of GDP	0	Raised corporate income tax rates; introduced new graduated excess profits tax on corporations
Revenue Act of 1941 (September 20, 1941)	+\$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

I. Interwar Income 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, as well as other significant tax actions. It shows the size of each act (measured using contemporary policymakers' estimates of its revenue effects), its impact on the top marginal rate, and a brief description of its key tax features. A companion background paper (Romer and Romer 2012) provides 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. That paper is included in the online Appendix (www.aeaweb.org/aej/pol/app/0603/pol2012-0353_app.pdf).

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. The act with the largest revenue impact in our sample was the Revenue Act of 1941, which was particularly large precisely because it greatly increased the fraction of households required to pay taxes.

In addition, interwar tax changes were usually balanced-budget. As discussed in Romer and Romer (2012), a prime determinant of interwar tax actions, particularly in the pre-Roosevelt era, was actual and anticipated changes in spending. As a result, the impact on the overall budget deficit or surplus was often smaller than the estimated revenue effect.

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. Thus, to the extent that tax changes mattered, it was probably not through effects on disposable income and spending. Hence, we focus on their incentive effects.

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. 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. The details of our calculations are presented in the online Appendix.

Most of the variation in marginal rates occurred at incomes above \$20,000. In addition, the fact that some items were excluded 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 of the income distribution. Specifically, we consider

the returns filed by the top 0.05 percent of households (or, more precisely, tax units) in each year. Since there were about 50 million tax units in the United States in a typical year in this period, we consider about 25,000 returns per year.² The net income cutoff for being in this group ranged from \$25,400 (in 1933) to \$75,100 (in 1928).

Our interest is in the cross-section and time-series variation in marginal rates across different subgroups of this part of the income distribution. We therefore divide this group into deciles; that is, we look at ten groups, each of which represents one-two-hundredth of 1 percent of the income distribution. We refer to these subgroups as percentile groups.

Data.—Our figures for the marginal rates faced by different percentile groups are derived from the data reported in the *Statistics of Income* (US 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 range, 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 data. The yearly volumes also provide detailed descriptions of the tax code, including the marginal tax rate that applied at different levels of income.

Procedures.—A percentile group in a given year typically spans a number of the income ranges in the *Statistics of Income*, and it generally includes partial ranges at its upper and lower ends. For example, our top percentile group might include all filers in the income ranges over \$200,000 plus a certain number of filers in the \$150,000–\$200,000 range. To estimate the total taxable income of the group, we therefore need to estimate the division of the taxable income in the \$150,000–\$200,000 range between the taxpayers who are in the top 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 varied within this income range. To estimate the average marginal rate faced by the second percentile group, we therefore need to estimate the fraction of the overall income in the \$90,000–\$100,000 range that was taxed at each marginal rate.

The highest income ranges in the *Statistics of Income* usually have fewer than 100 households, and the other income ranges that are relevant to our analysis are generally narrow. As a result, the details of how we estimate the distribution of income across the taxpayers in each income range have virtually no impact on our estimates.

As described in the online Appendix, the specific way that we deal with these issues is by making the standard assumption that taxpayers’ incomes follow a Pareto distribution. We estimate the Pareto parameter separately for each year using the numbers of taxpayers in the different income ranges at the top of the distribution in that year. Using the Pareto parameter, we allocate the taxpayers within the ranges,

²The data on the number of tax units by year are from Piketty and Saez (2001, table A0). Only about five million of the 50 million tax units filed personal income tax returns in the interwar era.

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	939,967	25.00	3,228,672	86.71
4-5 (est.)	162,994	4.38	494,888	13.29	3,391,666	91.09
5-6	92,808	2.49	331,892	8.91	3,434,474	93.58
6-7	55,602	1.49	239,084	6.42	3,540,076	95.07
7-8	36,855	.99	188,482	5.05	3,576,931	96.06
8-9	25,334	.68	145,627	3.94	3,632,265	97.04
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	73,965	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	35,824	.99	3,697,063	99.29
25-30	6,663	.18	25,495	.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	99.9845
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	.0007	3,723,547	99.9995
2,000-3,000	8	.0002	11	.0005	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				

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.)	2,295,556,215	20.85	8,650,562,104	78.58	4,658,661,868	42.27
3-4 (est.)	1,484,552,953	13.49	6,354,975,883	57.73	6,138,224,821	55.70
4-5 (est.)	722,895,165	6.57	4,870,412,933	44.24	6,861,119,886	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	3,640,616,993	33.06	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	3,006,052,293	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	2,462,077,543	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	9,280,884,432	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	1.66	1,497,556,532	13.59	9,693,289,002	88.07
30-40	262,353,726	2.38	1,315,348,752	11.93	9,955,647,728	90.45
40-50	185,438,623	1.68	1,052,990,023	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

FIGURE 1. SAMPLE TABLE FROM THE *Statistics of Income*, 1933

(Continued)

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
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	10,741,063,594	97.57
200-250.....	42,081,396	.38	267,584,160	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	10,865,679,985	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 ¹	28,471,672	.26	28,471,672	.26	11,008,637,754	100.00
Total.....	11,008,637,754	100.00				

Net income classes (Thousands of dollars)	Tax					
	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.).....	\$97,155	0.03	\$374,120,479	100.00	\$97,155	0.03
1-2 (est.).....	10,344,707	2.77	374,023,314	99.97	10,441,862	2.80
2-3 (est.).....	7,700,920	2.06	363,678,637	97.20	18,151,782	4.86
3-4 (est.).....	9,421,818	2.52	355,968,687	95.14	27,573,600	7.38
4-5 (est.).....	8,975,493	2.40	346,546,809	92.62	36,549,093	9.78
5-6.....	8,449,038	2.26	337,571,376	90.22	44,098,131	12.04
6-7.....	7,389,587	1.98	329,122,338	87.96	52,387,718	14.02
7-8.....	6,851,807	1.83	321,732,751	85.98	59,239,525	15.85
8-9.....	6,330,372	1.69	314,880,944	84.15	65,569,897	17.54
9-10.....	6,056,545	1.62	308,550,572	82.46	71,626,440	19.16
10-11.....	5,406,929	1.45	302,494,029	80.84	77,053,360	20.61
11-12.....	5,121,157	1.37	297,087,109	79.39	82,154,517	21.98
12-13.....	4,693,116	1.25	291,965,952	78.02	86,847,633	23.23
13-14.....	4,438,710	1.19	287,272,836	76.77	91,286,343	24.42
14-15.....	4,225,474	1.13	282,834,126	75.58	95,511,817	25.55
15-20.....	16,033,106	4.45	278,608,652	74.45	112,144,923	30.00
20-25.....	14,458,054	3.86	261,975,546	70.00	126,602,977	33.86
25-30.....	12,603,898	3.37	247,517,492	66.14	139,206,875	37.23
30-40.....	21,536,528	5.75	234,913,594	62.77	160,743,403	42.98
40-50.....	18,214,650	4.86	213,377,096	57.02	178,958,053	47.84
50-60.....	15,198,790	4.06	195,162,416	52.16	194,156,843	51.90
60-70.....	13,165,687	3.51	179,963,626	48.10	207,322,530	55.41
70-80.....	10,526,967	2.81	166,797,989	44.59	217,849,497	58.22
80-90.....	9,753,656	2.61	156,270,972	41.78	227,603,133	60.83
90-100.....	8,846,153	2.36	146,517,336	39.17	236,449,286	63.19
100-150.....	30,369,138	8.12	137,671,183	36.81	266,818,424	71.31
150-200.....	19,354,705	5.18	107,302,045	28.69	286,173,129	76.49
200-250.....	12,682,320	3.39	87,947,340	23.51	298,855,449	79.88
250-300.....	8,375,028	2.24	75,285,020	20.12	307,280,477	82.12
300-400.....	9,642,709	2.58	66,889,992	17.88	316,873,186	84.70
400-500.....	8,267,419	2.21	57,247,283	15.30	325,140,605	86.91
500-750.....	13,294,295	3.55	48,979,864	13.09	338,434,900	90.46
750-1,000.....	7,926,568	2.12	35,685,569	9.54	346,361,468	92.58
1,000-1,500.....	12,772,060	3.41	27,759,001	7.42	359,133,528	95.99
1,500-2,000.....	(1)	(1)	(1)	(1)	(1)	(1)
2,000-3,000.....	7,354,770	1.97	14,986,941	4.01	366,488,298	97.96
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 ¹	7,632,171	2.04	7,632,171	2.04	374,120,469	100.00
Total.....	374,120,469	100.00				

¹ Classes grouped to conceal identity of taxpayer.

FIGURE 1. SAMPLE TABLE FROM THE *Statistics of Income, 1933* (Continued)

and so estimate both the division of income between percentile groups when the boundary between groups occurs within a range and the division of income between parts of the range when the marginal range changes over the range.³

We find the marginal rate at each income level from the tax code. Then, using the information on the number and income of taxpayers in each income range and the interpolation, we calculate the income-weighted average marginal rate of the percentile group.

One important complication in these calculations involves capital gains, whose tax treatment varied greatly over the interwar period. To address this issue, we exclude capital gains from our definition of income, and focus on the relationship between taxable noncapital-gains income and marginal rates on that income. Capital gains averaged about 15 percent of total income in the interwar period. 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 2012). An additional advantage of focusing on noncapital-gains income is that, as described in the online Appendix, it allows us to employ a measure of taxable income whose definition does not change over our sample.

Policy-Induced Changes.—Because the interwar tax system was highly progressive, marginal rates changed not only because of legislated changes, but also because economic growth, inflation or deflation, and other nonpolicy forces affected which brackets taxpayers were in. 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. To calculate the policy-induced change in the marginal rate of a given percentile group, we find the marginal rate in the current year and in the previous year, both at the previous year's level of income, and then take the difference. 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 online Appendix describes the data available in the *Statistics of Income* more fully, and explains the details of our calculations of taxable incomes excluding capital gains, marginal rates, and policy-induced changes in marginal rates.

³As a concrete example, consider the top percentile group in the first year we consider, which is 1918. Since there were 40,451,000 tax units in 1918, our top group—the top one-two-hundredth of 1 percent—is 2,023 households. The 1918 *Statistics of Income* reports that there were 1,275 taxpayers with incomes above \$200,000, with total net income of \$557.39 million. In addition, there were 866 taxpayers with incomes between \$150,000 and \$200,000, with total net income of \$148.74 million. Thus, the only uncertainty about the income of the top percentile group is how much of the \$148.74 million was earned by the top 748 members of the \$150,000–\$200,000 group. Since they were the highest earning members of the group, their share must have been at least 748/866, which corresponds to an income of \$128.48 million. And since each member of the group earned at least \$150,000, the highest possible earnings of the top 748 members is \$148.7 million minus $118 \times \$150,000$, or \$131.04 million. Thus, the total net income of the top group must have been between \$685.87 million (\$557.39 million + \$128.48 million) and \$688.43 million (\$557.39 million + \$131.04 million). If we make the additional assumption that the density of taxpayers was declining over the range from \$150,000 to \$200,000—a minimal assumption about high incomes—the lower bound rises to \$688.08 million. The resulting range is so narrow that the exact procedure we use to choose a number within the range is unimportant; the specific value that results from our Pareto procedure is \$688.15 million. Most percentile groups in most years exhibit similar patterns (although the gap between the lower and upper bounds averages slightly less than 1 percent, rather than less than 0.1 percent as in this case).

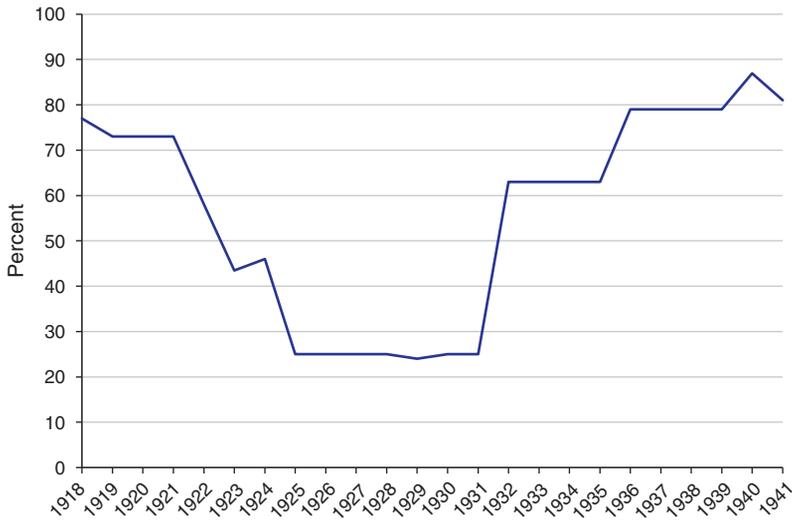


FIGURE 2. TOP MARGINAL TAX RATE

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 shows 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 an interesting number, the marginal rate at different points in the income distribution is more important for our analysis. Also, for calculating elasticities, it is helpful to look not at the change in the marginal rate, but at the change in the log after-tax share. Figure 3 shows our estimates of the policy-induced changes in the log after-tax share for the top ten one-two-hundredths of 1 percent of the income distribution. The results are in changes 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.

The figure shows that changes in after-tax shares, while correlated across 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 top of the income distribution fairly uniformly. Others, such as the Revenue Act of 1935, raised rates and lowered after-tax shares much more for the very top groups than for others. And 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 change to the top marginal rate. This variation across income groups is central to our identification strategy for estimating the incentive effects of tax rate changes.

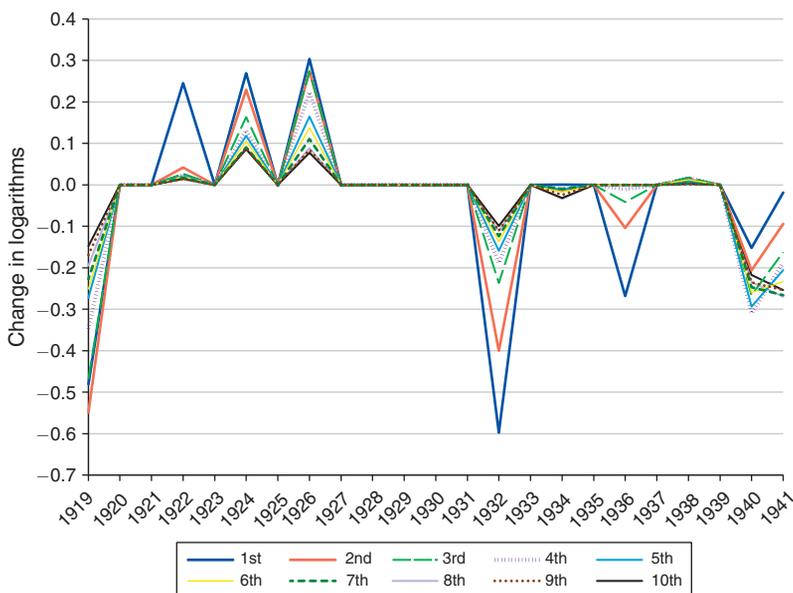


FIGURE 3. POLICY-INDUCED CHANGE IN THE LOG AFTER-TAX SHARE FOR DIFFERENT PERCENTILE GROUPS

Note: Each line represents the change for a given tenth of the top one-twentieth of 1 percent of the income distribution.

The fact that there is so much variation in this 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 valuable evidence about the effects of changes in marginal rates. Most obviously, there is a great deal of identifying variation. More subtly, that variation consists mainly of large, salient changes. Chetty (2012) 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 more fundamental features of preferences. As Saez, Slemrod, and Giertz (2012) observe, many postwar studies focus on such subtle features, and cases of credible identification using large changes are rare.

Finally, note that we are tracking slices of the income distribution rather than individuals. That is, we measure the taxable income of, for example, the top one-two-hundredth of 1 percent of the income distribution in each year, even though the identities of the taxpayers in this group changed from year to year. This approach of using repeated cross sections has an advantage and a disadvantage. The advantage is that it avoids the need to model mean reversion in income at the individual level. After reviewing the various complications in panel analysis of tax responsiveness, Saez, Slemrod, and Giertz (2012, 29) conclude that such “repeated cross-section analysis ... may be a more robust and transparent approach.”

The disadvantage arises from the fact that in the underlying data, taxpayers are grouped by their total net incomes rather than by their noncapital-gains incomes. As a result, changes in capital gains income affect the allocation of taxpayers to different groups, and so can affect the estimated growth rate of noncapital-gains income (Slemrod 1996).⁴ Piketty and Saez (2001, appendix A.2.iv) show that in the postwar period, the measurement error in the growth rate of noncapital-gains income resulting from this problem is relatively small, despite very large changes in the share of capital gains in overall incomes.⁵ Nonetheless, concern about these effects is one reason for being particularly interested in the 1923–1932 sample, when there were no changes in capital gains taxes.

D. Other Features of the Interwar Income Tax

In analyzing the effect of changes in marginal tax rates, several other features of the interwar tax system are relevant.

Distribution and Composition.—Figure 4 shows the degree to which income taxes in the interwar era were paid almost entirely by the rich. Specifically, it shows the fraction of total federal personal income taxes paid by the top ten one-two-hundredths of 1 percent of households, where the amounts are cumulated as we move down the income distribution. Between 25 and 40 percent of personal income taxes were paid by the top one-two-hundredth of 1 percent of the income distribution. Roughly 60 percent were paid by the top one-twentieth of 1 percent. And, though not shown in Figure 4, approximately 95 percent were paid by the top two-tenths of 1 percent.

The *Statistics of Income* breaks down gross income for the various income ranges into a number of components. These data show that for the top one-two-hundredth of 1 percent of the income distribution, wages and salaries accounted for only about 10 percent of total income less capital gains on average over the period 1919–1941; business and partnership income, which partly reflects the labor input of business owners, made up another 15 percent. Dividends, interest, and rental income accounted for about 75 percent. For the top ten one-two-hundredths of 1 percent of the income distribution combined (the top one-twentieth of 1 percent), wages and salaries represented about 20 percent of total income less capital gains, and business and partnership income made up another 15 to 20 percent. Dividends, interest, and rental income accounted for roughly 65 percent.

For both the top one-two-hundredth and the top one-twentieth of 1 percent, the importance of wage and salary income rose slightly over the interwar era, but remained far below postwar levels even in 1941. For comparison, Piketty and Saez (2003, table III) report that in 1998, wages and salaries accounted for 45 percent of the noncapital-gains

⁴To see this, consider an extreme example: suppose all taxpayers stop earning any capital gains, with no changes in their noncapital-gains income. After the change, the top group will consist of the taxpayers with the highest noncapital-gains income. Thus, unless the taxpayers with the highest total incomes before the change were also those with the highest noncapital-gains incomes, the measured noncapital-gains income of the top group would rise even though there had been no true change in any taxpayer's noncapital-gains income.

⁵Also, note that since the growth rate of noncapital-gains income is the dependent variable in our empirical work, measurement error in this variable will not bias our estimates.

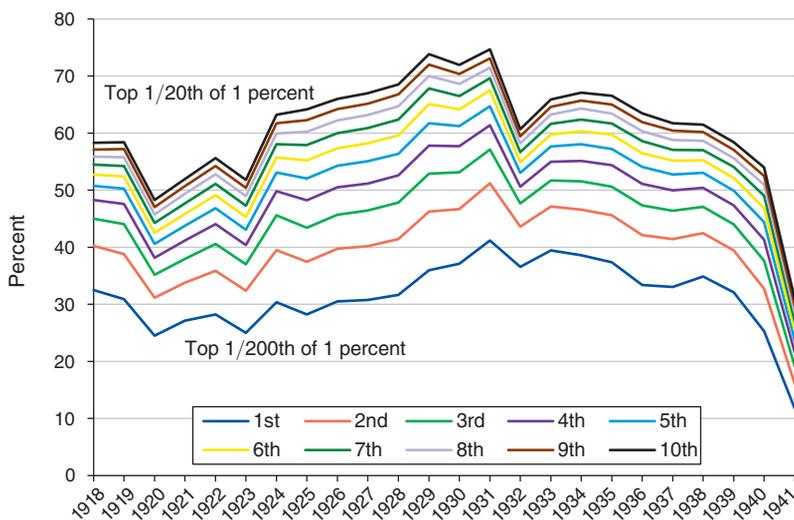


FIGURE 4. PERCENT OF TOTAL INCOME TAX PAID BY TENTHS OF THE TOP ONE-TWENTIETH OF 1 PERCENT OF THE INCOME DISTRIBUTION (*Cumulative*)

income of the top one-one-hundredth of 1 percent of the income distribution, business and partnership income made up 33 percent, and capital income 22 percent.

The Corporate Income Tax.—As described in Romer and Romer (2012), 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 1923 and 1932, the corporate rate varied between 11 and 13.75 percent.

One type of corporation was subject to large 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 tax rates on personal holding companies.

Margins for Changing Taxable Income.—The interwar tax system was comparatively simple. The entire income tax law was straightforward enough that it was largely rewritten with each revenue act. In general, there was a broad base and relatively few deductions. One useful indicator of this simplicity, particularly in the 1920s, is that almost all the relevant income tax schedules and instructions (for both individuals and corporations) could be reproduced in the *Statistics of Income* each year. In 1928, they took up just seven double-sided sheets of paper.

As a result, there were fewer margins for legal income shielding in the interwar tax code than today.

Nonetheless, there were ways in which interwar taxpayers could change their taxable income in response to changes in tax rates. That labor income did not dominate the incomes of the taxpayers who paid most of the income tax suggests that the conventional channels of entry and exit from the labor force and changes in hours of work were probably not the main margins of adjustment. Likely more important, entrepreneurs could potentially shift their compensation between taxable income and untaxed fringe benefits and perquisites, and among personal income, corporate income, and capital gains; wealthholders could potentially shift between high- and low-dividend stocks, taxable and tax-free bonds, and personal and corporate income; wage earners may have had some scope to shift their compensation between taxable income and fringe benefits; and all taxpayers could potentially reduce their taxable income through legal deductions and illegal tax evasion.

Consistent with these observations, the labor supply effects of marginal rates were noticeably absent from interwar policymakers' discussions of incentive effects of taxes. In Romer and Romer (2012), we document that interwar policymakers discussed the incentive effects of marginal rates extensively. But, we find little mention of rates affecting decisions to work and labor effort. This lack of emphasis is in stark contrast to the postwar narrative record, where such labor supply effects were discussed frequently and thought to be central (Romer and Romer 2009).

Nevertheless, interwar policymakers believed that income shielding was prevalent and quite responsive to marginal tax rates. The method of income shielding emphasized most by Calvin Coolidge and his Treasury Secretary, Andrew 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 (US Department of the Treasury 1923, 8):

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.

The Coolidge administration not only called for a drastic reduction in marginal rates to decrease these incentives, but also repeatedly asked for a constitutional amendment taking away the right of states and municipalities to issue such securities.

In the 1930s, the main type of income shielding that policymakers emphasized was the blurring of the line between individual and corporate income. A special subcommittee of the Ways and Means Committee established in June 1933 highlighted the use of personal holding companies described above ("Revenue Bill of 1934," 73rd Congress, 2d Session, House of Representatives Report No. 704, February 12, 1934). Franklin Roosevelt in 1936 suggested that the problem was broader, arguing that many corporations were retaining earnings rather than paying dividends as a way to help shareholders avoid paying the personal income tax. Unlike

Coolidge, who pushed for lower marginal rates 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.

Both the changing treatment of personal holding companies and the undistributed profits tax in the mid-1930s could have reduced the potential for income shielding, and so affected the sensitivity of personal income to marginal rates. 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.

Enforcement.—The enforcement regime was quite different in the interwar period than it is today. Income tax withholding did not begin until 1943. Instead, enforcement was based mainly on reporting and auditing. Payments that exceeded some threshold had to be reported to the Bureau of Internal Revenue. For example, Section 256 of the Revenue Act of 1918 required that “all individuals, corporations, and partnerships . . . , making payment of interest, rent, salaries, wages, . . . , of \$1,000 or more in any taxable year, . . . shall render a true and accurate return to the Commissioner [of Internal Revenue], . . . setting forth the amount . . . and the name and address of the recipient.”

The importance of auditing is shown by the data provided by the Bureau of Internal Revenue in the *Annual Report of the Secretary of the Treasury* on both total income tax receipts and the revenue resulting from audits and field investigations. For the 1920s and 1930s, these additional audit revenues were on the order of 10 to 20 percent of total income tax receipts. The comparable number from the middle years of the postwar era is just 2 to 3 percent.⁶

Because the absence of withholding and the greater importance of auditing operate in opposite directions, it is not clear whether the ability to illegally shield income from taxation was higher or lower in the interwar era. Quite possibly, it was not on net dramatically different from today.

II. Basic Time-Series/Cross-Section Estimates of the Impact of Marginal Rates on Reported Taxable Income

We investigate the impact of changes in marginal rates using 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 incomes and the policy-induced changes in marginal rates for the top ten one-two-hundredths of 1 percent of the income distribution for the period 1919 to 1941.

⁶To give a specific example, consider the data in the *Annual Report of the Secretary of the Treasury* for 1936. Income tax collections were \$1,412,938,303.89 (page 136). Additional revenue derived from audits by the Income Tax Unit was \$222,099,314.64 (page 140); another \$8,547,790 came from the Accounts and Collections Unit, which audited the simpler 1040A forms (page 138). The ratio of the additional revenue to total receipts was 16.3 percent. Similar data in the 1979 *Annual Report* show that the ratio of additional revenue from audits (\$6.2 billion, page 171) to total income tax receipts (\$322.9 billion, page 165) was 1.9 percent in 1979. The numbers are for both the individual and corporate income tax, because audit revenues were not reported separately for individuals in the interwar era.

A. Specification

Our basic specification is

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

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 $t - j$.⁷ That is, we regress the percent change in reported taxable income of a group on the percent change in the after-tax share 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 reported taxable income.

We include a full set of group and time dummy variables. The group dummies (the α_i s) capture differences in trend income growth of the various percentile groups. The time dummies (the β_t s) capture year effects. In the simplest regressions, we only consider the contemporaneous relationship between income and the after-tax share. However, we also consider specifications including one and two lags of the tax policy variable, and in some cases, one lead.

Our basic specification estimates the relationship between income and policy-induced tax changes using OLS. Because the interwar US tax system was progressive, one potential difficulty with this approach is that if policy-induced increases in the after-tax share raise taxable income, they would push taxpayers into higher tax brackets. This would reduce their incentives to increase their incomes in response to the policy change. As a result, the tax elasticity estimated by an OLS regression of income on the policy-induced change might be biased downward. This difficulty can be addressed by using an IV procedure, where the change in income is regressed on the actual change in the after-tax share, instrumenting with the policy-induced change.

Finally, in the baseline specification, we estimate the relationship over the full sample period 1919 to 1941.⁸ In Section III, we consider a range of alternative samples.

B. Interwar Tax Changes versus the Ideal Experiment

For the elasticity we wish to estimate, the ideal experiment would be a one-time, unanticipated, permanent change in taxes enacted on January 1 of some year, with the different impact of the policy change on the after-tax shares of different groups not made in response to other forces affecting income differentially across groups.

⁷ Recall that we measure the policy-induced change in the after-tax share in year t as the change in the after-tax share from year $t - 1$ to year t if income were at its year $t - 1$ level in both years. An alternative is to compute the change with income at its year t level in both years. Using this alternative has virtually no effect on the results.

⁸ We calculate marginal rates by percentile group beginning in 1918. The policy-induced changes are therefore available starting in 1919.

As with all changes in taxes, the interwar changes do not correspond exactly to that ideal. Here we discuss the main potential differences between the actual changes and that ideal, and the issues they raise. They fall into four groups.

Timing.—The changes in taxes were not enacted on exactly January 1 and were not completely unanticipated. To the extent that tax changes were enacted after January 1 and were made retroactive (and were not anticipated on January 1), their impact on reported income could have been delayed. And to the extent a change was anticipated before taking effect on January 1, it could have affected taxable income before the change. That effect could operate in either direction. If taxpayers face adjustment costs, anticipation of a tax cut could cause them to start to raise labor supply and reduce income shielding, and so raise current taxable income; if they can substitute intertemporally, an anticipated tax cut could reduce current taxable income. Note, however, that neither anticipation effects nor delayed responses should affect the total impact of a permanent change.

In fact, the actual timing of interwar tax changes did not differ dramatically from the January 1 ideal. All but two of the major tax changes in our sample period were enacted in February or June. The exceptions are the 1922 tax cut, which was enacted in November 1921, and the 1941 increase, which was enacted in September 1941. As described in Romer and Romer (2012), the major tax changes were generally under active consideration for about six months before they were enacted.⁹ Thus, it does not appear to be a poor approximation to assume that taxpayers became aware of the tax changes around January 1.

Our main strategy for dealing with potential timing effects is to experiment with including a lead and up to two lags of the tax changes. Examining the sum of the coefficients then provides an estimate of the total impact of a change. In addition, we consider different timing assumptions. Our baseline specification assumes that in cases where a law retroactively changed taxes for the previous year, behavior in that previous year was based on the tax code in effect at the time, not the tax code applied ex post. But we also consider specifications where our measure of tax changes is based on the tax rates in effect ex post. And, because the 1941 change was enacted later in the year than any of the others on our sample, we consider both the possibility that behavior in that year was based on the rates imposed by the 1941 law and that behavior was based on the rates under the previous year's tax code. None of these variations has an important impact on the results.

Permanence.—Given the frequency of tax legislation in the interwar era, the actions clearly do not correspond to the ideal experiment of a permanent, one-time change. However, other features of the tax debates of the 1920s and 1930s suggest that this deviation is unlikely to have greatly impacted taxpayer responsiveness.

First, the analysis in Romer and Romer (2012) suggests that taxpayers probably viewed most of the changes in marginal rates as likely to be long-lived. The only change that was explicitly temporary was the across-the-board cut of one percentage

⁹The biggest exception is that the Coolidge administration began advocating the tax cuts that were ultimately enacted in the Revenue Act of 1926 even before the passage of the Revenue Act of 1924.

point in marginal rates applied to 1929 incomes. Many of the other changes, notably the cuts in the 1920s under Harding and Coolidge and the increases in the 1930s under Roosevelt, built on previous changes and were elements of broader economic policies unlikely to be reversed without a major political change. And given the looming conflict, it is unlikely that the tax increases on the eve of World War II were expected to be reversed in the near term. Thus, there is little reason to fear that taxpayers might have ignored the large tax changes in this era on the grounds that they would be transitory.

Likewise, the narrative evidence shows that debates about taxes were ongoing. In both the 1920s and the 1930s, tax actions were soon followed by calls both for more changes in the same direction and for repeal. As a result, taxpayers were unlikely to have thought that they should postpone responding to the changes because there would soon be decisions that would permanently stabilize the tax system.¹⁰

Endogenous Legislation.—The type of anticipatory or endogenous legislative behavior that would threaten our identification strategy would involve policymakers choosing the differences in tax changes across groups on the basis of information about other factors that would differentially affect the groups' income, or other factors leading to both differential tax changes and differential income changes. We do not find any hint of such behavior in the historical record of the factors leading to the tax changes. That record strongly suggests that tax changes and their distribution across groups were motivated by changes in military needs and other broad developments, general views about fairness, and beliefs about the importance of incentive effects, not by short-term forces differentially affecting different groups at the top of the income distribution.

Likewise, we know of no evidence that the changes in government spending that were often associated with the tax changes differentially affected groups at the top of the income distribution (much less that they did so in a way that was correlated with the differences in the tax changes across groups). The spending changes generally involved the broad contours of the budget, not narrow programs likely to have effects concentrated on a subset of the wealthy.

Heterogeneity.—Finally, heterogeneity across the percentile groups we are considering has the potential to bias our estimates. In particular, suppose that responsiveness is increasing with income. A typical tax change in our sample moved the after-tax share of all the percentile groups in the same direction, but moved the after-tax share of the wealthiest taxpayers by more than the after-tax shares of the other groups. For concreteness, consider a tax cut of that form. In that situation, there will be some widening of the income distribution not from the fact that the highest-income taxpayers received the largest cuts, but from the fact that taxes are lower on average for all taxpayers, and the highest-income taxpayers are the most

¹⁰The historical record also provides no evidence that the degree of uncertainty about future tax changes was systematically related to the direction of recent tax changes. That is, although uncertainty about future tax changes is among the factors influencing the residual in our equation, the history of this period does not suggest that it is correlated with our right-hand side variable.

responsive to this change. In this situation, the estimates from (1) could be larger than the elasticity of any of the percentile groups.

This challenge is a general one facing this literature (Saez, Slemrod, and Giertz 2012, 25–26). There are three reasons that it does not appear to be unusually problematic for our application. First, we are dealing with a very narrow slice of the income distribution, and so substantial heterogeneity appears unlikely. Second, one possible source of heterogeneous responses arises from the fact that the composition of income varies somewhat across the percentile groups that we consider; but, as described below, we find no evidence that the responsiveness of different types of income to tax changes differed substantially in this period. Third, and perhaps most important, the likely direction of any potential bias through this channel would be to overstate tax responsiveness; thus this possibility cannot account for our findings of low responsiveness.

C. Results

Table 2 shows the results of the baseline specification and some key permutations. The coefficient of interest is that on the after-tax share. When we include lags of the tax variable, we report the sum of the coefficients on the contemporaneous and lagged values, along with the associated standard error.

The estimated impact of a rise in the after-tax share is consistently positive, small, and precisely estimated. In the simplest specification, which includes only the contemporaneous value (line 1), the coefficient estimate is 0.21 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 over two-tenths of a percent.

The results are virtually identical when we use the IV procedure described above rather than OLS (line 2). This specification regresses the change in income on the change in the actual after-tax share, instrumenting with the policy-induced change. In the first stage, the coefficient on the policy-induced change is 0.995 with a *t*-statistic of 28. This result is consistent with the finding that the responsiveness of income to the after-tax share is small. In that situation, taxpayers are unlikely to be pushed into other tax brackets by their response to a policy change. Thus there is little endogeneity in the after-tax share, and so the bias from using OLS is small. As a result, using IV barely changes the estimated elasticity and increases the standard error only slightly. Because the reduced-form OLS approach and IV always yield extremely similar results, in the tables that follow we only report the OLS estimates.

Including lags of the tax variable (lines 3 and 4) increases the sum of the coefficients slightly—from 0.21 with no lags to 0.27 with two lags. The standard error on the sum of the coefficients rises modestly, but the sum is still highly statistically significant ($t = 4.8$). 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: the point estimate falls in half and the standard error doubles.

TABLE 2—BASIC TIME-SERIES/CROSS-SECTION RESULTS

	Estimation method	Lags included	Control variables	Elasticity of taxable income with respect to after-tax share	Observations
(1)	OLS	None	Year, group dummies	0.207 (0.031)	230
(2)	IV	None	Year, group dummies	0.208 (0.034)	230
(3)	OLS	1	Year, group dummies	0.316 ^a (0.048)	220
(4)	OLS	2	Year, group dummies	0.270 ^a (0.056)	210
(5)	OLS	None	Year dummies	0.209 (0.031)	230
(6)	OLS	None	Group dummies	0.093 (0.067)	230

Notes: The dependent variable is the change in the log of real taxable income. The table reports the estimated coefficient on the policy-induced change in the log after-tax share. As described in the text, the equations are estimated using the top 0.05 percent of the income distribution, subdivided into ten groups of equal size. In lines 1–2 and 5–6, the sample period is 1919 (that is, the changes in income from 1918 to 1919) to 1941. In lines 3 and 4, which include lags, the sample periods begin in 1920 and 1921, respectively. Standard errors are in parentheses.

^aThe coefficient estimate and standard error are for the sum of the coefficients.

Figure 5 presents the scatter plot corresponding to the baseline regression (line 1 of Table 2): it shows the relationship between the contemporaneous percentage change in income and the change in the log after-tax share, after partialing out the group and time dummies. The figure shows that there are some extreme observations, particularly for the very top one-two-hundredth of 1 percent of the income distribution. But it also illustrates why there is a clear but modest relationship. The extreme observations are generally in line with the mass of the observations. There are almost no observations far toward the upper left or lower right of the diagram. Many observations lie near a line with a small positive slope, and most of the remaining observations show either little change in income or little change in tax rates (or both). All of this suggests that the results are likely to be highly robust.

III. Robustness and Extensions of the Time-Series/Cross-Section Analysis

In this section, we consider systematically the robustness of the results along a wide range of dimensions. We also extend the analysis to consider the stability of the estimates across the interwar period and across different types of income.

A. Robustness

To make comparisons to the previous results straightforward, we focus on the baseline specification, which includes no lags and is estimated using OLS over the full sample. The estimated elasticity from this specification for our preferred tax variable is repeated in line 1 of Table 3.

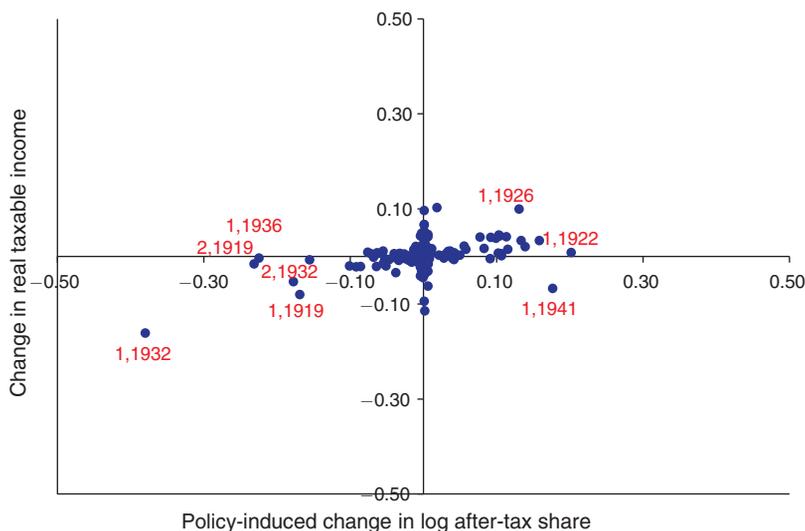


FIGURE 5. SCATTER PLOT OF CHANGE IN LOG AFTER-TAX SHARE AND CHANGE IN REAL TAXABLE INCOME

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 one-twentieth of 1 percent of the income distribution (1 to 10) and the year (1919 to 1941) the observation corresponds to.

Specification of the Tax Variable.—As described above, 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 a tax change was passed, not the year it took effect. However, individuals could have responded to the retroactive changes if they were anticipated. In addition, because tax returns were not filed until the subsequent year (after the retroactive changes had been passed), fraudulent income reporting could respond to the retroactive changes. For these reasons, we consider an alternative specification that dates changes when they took effect, even if they were passed the next year.

Line 2 of Table 3 shows that replacing the baseline tax variable with the perfect foresight alternative has only a moderate impact on the results. The estimated response of taxable income to tax changes falls from 0.21 to 0.14 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.¹¹

For all tax changes, retroactive or not, it is possible that taxpayers responded to anticipations of future actions. As discussed above, a prospective reduction in

¹¹The difference between the two tax variables (the one computed using the rates actually applied and the one computed using the rates in effect during the year) measures the retroactive component of tax changes. To test for the possibility that taxpayers responded to retroactive changes by altering the amount of their income they reported, we add the difference between the two tax variables to the baseline regression. In this specification, 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 in response to retroactive tax changes may have been relatively unimportant.

TABLE 3—ROBUSTNESS OF TIME-SERIES/CROSS-SECTION RESULTS

	Specification	Elasticity of taxable income with respect to after-tax share	Observations
(1)	Baseline: OLS, no lags, full sample (line 1 of Table 2)	0.207 (0.031)	230
(2)	Using perfect foresight tax variable in place of baseline measure	0.139 (0.038)	230
(3)	Include both a lead and the current value of baseline tax measure	0.160 ^a (0.046)	220
(4)	Include separate group dummies by decade	0.210 (0.033)	230
(5)	Include lagged income growth	0.224 (0.037)	220
(6)	Exclude top one-two-hundredth of 1 percent of income distribution	0.160 (0.027)	207
(7)	Weighted least squares, standard errors clustered by year	0.167 (0.050)	230
(8)	Nonwar sample (1920–1939)	0.288 (0.039)	200
(9)	Pre-Depression sample (1919–1929)	0.198 (0.044)	110
(10)	Depression sample (1930–1941)	0.220 (0.047)	120
(11)	Period of stable capital gains and corporate taxes (1923–1932)	0.378 (0.037)	100
(12)	Exclude war years, large post-WWI change in capital gains taxes (1923–1939)	0.332 (0.037)	170

Notes: See notes to Table 2 for a description of the basic specification. All regressions are estimated by OLS including group and year dummies, with no lags of the tax variable. The sample period is 1919–1941 unless otherwise noted, or where leads or lags of variables are included. Standard errors are in parentheses.

^aThe coefficient estimate and standard error are for the sum of the coefficients (lead and contemporaneous). The coefficient on the lead is -0.093 , with a standard error of 0.035 .

tax rates could either increase or decrease reported income. To test for anticipation effects, we follow Slemrod (1996) and include a lead of our baseline tax variable along with the contemporaneous value. The results (line 3) point in the direction of modest intertemporal substitution effects. The coefficient on the first lead of the tax variable is negative, small, and statistically different from zero ($t = 2.7$). The sum of the coefficients on the current value and the lead is little changed from the baseline specification and is still highly significant ($t = 3.5$).

Differential Trends.—A natural concern is that the effects of tax changes could be confounded by different trends in income across groups. The inclusion of the group dummies allows for the possibility of different average rates of income growth over the full sample, but it does not allow for more complicated differential trends. We address this concern in several ways.

First, one obvious possibility is different patterns across decades. The 1920s were a period of rising income inequality and falling marginal rates, while the 1930s exhibited the opposite pattern. With only group dummies for the whole period, the regression

might interpret this correlation as a behavioral response to marginal rates. To address this possibility, we include two sets of group dummies—one for the period through 1929 and one for the period beginning in 1930. Line 4 of Table 3 shows that this specification change has essentially no impact on either the point estimate or the standard error. That is, very little of our identifying variation is coming from the fact that tax rates were generally falling in the 1920s and generally rising in the 1930s.

Second, differences across groups could occur because the incomes of the top groups are particularly sensitive to the economy as a whole or to asset markets. We therefore add interactions between the group dummies and real GDP growth, or between the dummies and the real return on the stock market. Neither approach has a large impact on the results: the estimated elasticity is 0.19 ($t = 5.4$) when we include the interactions with GDP growth, and 0.21 ($t = 6.5$) when we include the interactions with the stock market.

Third, if our results reflected different trends across groups, changes in taxable income might precede tax changes. But as noted above, when we include a lead of our tax variable, it enters negatively rather than positively.

Finally, if different groups have different trends, changes in income will tend to be serially correlated. Including lagged income growth could capture such a pattern. But when lagged income growth is added to the regression, its coefficient is small and far from significant. Line 5 of the table shows that as a result, the coefficient and standard error on our tax variable are almost unchanged.

Non-i.i.d. Residuals.—There are two reasons to fear that the regression residuals may not be i.i.d. First, as Figure 5 shows, they appear to be larger for the top percentile group. Second, there may be correlation in the behavior of groups with similar incomes in a given year. For example, the bottom two income groups that we consider may behave similarly for reasons unrelated to any tax changes that they face.

A simple way to test whether the larger residuals for the top percentile group are driving the results is to just exclude the very top percentile group (the top one-two-hundredth of 1 percent of the income distribution) from the estimation. Line 6 shows that the results are quite similar to the baseline in this case.

To address the issue of non-i.i.d. residuals more generally, we modify our estimation in two ways. First, we use weighted least squares (technically, feasible generalized least squares). We find the variances of the residuals by percentile group from the OLS estimates, and use those to weight the observations. As one would expect from Figure 5, the variance of the residuals is much larger for the top percentile group than for the others, and moderately larger for the second group than for any of the remaining ones. Second, we cluster by year in computing the standard errors. This accounts for any remaining heteroskedasticity and for arbitrary correlation among the observations for each year.¹²

Line 7 of Table 3 shows the results. Again, the basic messages are unchanged. The point estimate is reduced slightly, and the robust standard errors are moderately larger than the conventional ones. But the estimated responsiveness of taxable

¹²Clustering by group rather than by year *reduces* the standard errors sharply.

income to the after-tax share remains highly significant ($t = 3.3$), and large effects are decisively rejected. Indeed, the upper end of the two-standard-error confidence interval is essentially the same as in the baseline regression.

Sample Period.—Finally, we consider robustness to a wide range of sample periods. The baseline sample is the full period 1919–1941. 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 nonwar sample 1920–1939. Likewise, the Great Depression was such an enormous shock that we consider both a pre-Depression (1919–1929) and a Depression (1930–1941) sample.

A particularly important alternative sample period is 1923–1932, when there were no major changes in capital-gains or corporate taxes. As discussed above, changes in capital-gains taxes could introduce measurement error in our estimates of the growth of noncapital-gains income. Likewise, some of the response of taxable income to marginal rates likely reflects shifting between personal and corporate forms of income (Gordon and Slemrod 2000). Focusing on a period when corporate rates barely changed helps to isolate the effects of the personal tax.¹³ A variation on this sample period is 1923–1939; this excludes the years associated with the wars and the largest change in the tax treatment of capital gains, which occurred in 1922 when capital gains changed from being treated as ordinary income to being taxed separately at a much lower rate.

Lines 8 to 12 of Table 3 report the results for the various periods. In general, the alternative samples lead to estimates that are somewhat higher than for the full sample, but still modest. In each of the alternative periods, 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 (line 11), where the estimated elasticity is 0.38 ($t = 10.2$).¹⁴

B. Stability of the Estimated Elasticity over the Interwar Period

We can go beyond examining robustness to specific choices of the sample period, and ask more generally whether the estimate varies in important ways over time. A simple way to obtain evidence on this issue is to reestimate our baseline regression allowing the coefficient on the tax variable to be different in each year. That is, we estimate,

$$(2) \quad \Delta \ln Y_{it} = \alpha_i + \beta_t + \gamma_t \Delta \ln(1 - \tau)_{it}^{PI} + \varepsilon_{it},$$

where the variables are defined as in equation (1).

¹³Even 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 noncapital-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 personal income tax rates (Slemrod 1998). The evidence in Goolsbee (1998) suggests, however, that shifts between personal and corporate income in this period may have been small.

¹⁴For this sample, in contrast to the full sample, introducing lags reduces the overall effect somewhat. With either one or two lags, the sum of the coefficients on the tax variables is 0.28, and still highly statistically significant.

In this specification, the coefficient on the tax changes in a given year reflects the effects of both the tax changes and any other forces tending to compress or widen the top of the income distribution in that year. For example, if there was a tax increase that particularly raised taxes on the wealthiest taxpayers in a year when other factors were reducing their relative incomes, the coefficient on that year's tax changes will overstate the impact of the tax changes. Since there were surely other forces compressing or widening the income distribution in various years, one would not expect the coefficient on the tax changes to be stable over time even if the effects of tax changes were constant. It is precisely because other factors likely affected the income distribution from year to year that our basic approach pools the data across years, and that we take various steps to deal with the possibility of different trends across groups.

When we allow the coefficient to vary over time, the estimated relationship is nevertheless quite stable. Figure 6 shows the estimated tax elasticity for each year, along with the two-standard-error ranges. The baseline elasticity estimate of 0.21 is rejected for only two years. One is 1941, when the estimated elasticity is -0.25 (with a standard error of 0.10). The source of this result is that the Revenue Act of 1941 raised taxes relatively little on the most wealthy, yet their incomes fell relative to those of the other groups. In light of the sharp compression of the income distribution in the early 1940s (see Piketty and Saez 2003), this is not surprising. The other is 1932, when a tax increase that fell especially on the most wealthy occurred in the same year as a downturn that especially harmed those taxpayers. For this year, the coefficient on the change in the after-tax share is 0.42 (with a standard error of 0.05). For the other years with tax changes, the coefficient is usually either quite close to the baseline estimate (for example, 1919, 1924, and 1926), or the tax changes are so small that the coefficient cannot be estimated with any useful degree of precision (particularly 1934 and 1938).

More generally, the hypothesis that the effects of tax changes are time-varying predicts that if one imposes a constant elasticity, the regression residuals will be on average larger when tax changes are larger. To investigate this possibility, we regress the squared residuals from the baseline specification on the group and time dummies and the square of our tax variable. The coefficient on the squared tax variable is positive, but quantitatively very small and far from statistically significant. Thus, this test yields no evidence of important time variation.¹⁵

Goolsbee (1999) performs a related exercise, estimating the elasticity of taxable income using four-year changes over three periods in the interwar era: 1922–1926, 1931–1935, and 1934–1938. He finds large variation in the elasticity across the three periods. When we redo his estimation, our estimates are quite different from his.

The sharpest difference between Goolsbee's results and ours is for 1934–1938, where he finds an estimate that is large and negative while we obtain one that is large and positive. The difference in this period appears to stem largely from the fact that

¹⁵To better understand the motivation for this test, suppose (neglecting the group and year dummies for expositional simplicity) that the true relationship is time-varying, so that $\Delta \ln Y_{it} = (\gamma + v_t)X_{it} + \varepsilon_{it}$, where $X_{it} \equiv \Delta \ln(1 - \tau)_{it}^{PI}$, γ is the average effect, and v and ε are uncorrelated. Then $E[(\Delta \ln Y_{it} - \gamma X_{it})^2] = \sigma_v^2 X_{it}^2 + \sigma_\varepsilon^2$. When we estimate the corresponding regression (with the group and year dummies included), the coefficient on X_{it}^2 is 0.0019 with a standard error of 0.0029. The point estimate suggests a standard deviation of v over time of just 0.04, and the upper end of the two-standard-error confidence interval is only 0.09.

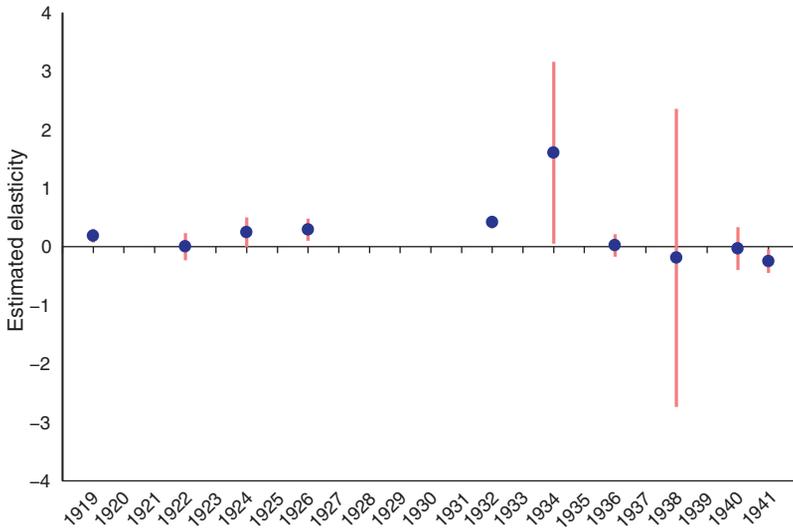


FIGURE 6. ESTIMATED ELASTICITY WITH RESPECT TO THE AFTER-TAX SHARE BY YEAR
(with two-standard-error ranges)

Notes: The figure shows the coefficients (the dots) from a regression where the tax variable is interacted with a dummy variable for each year, along with the two-standard-error confidence ranges (the lines). The estimated elasticity is not defined in years when there are no policy-induced changes in the after-tax share. Those observations are therefore not shown in the figure.

Goolsbee includes capital gains in his income measure while we do not. Over this period, the after-tax share for noncapital-gains income fell more for the wealthiest taxpayers than for other groups, and their noncapital-gains income rose less than the noncapital-gains income of other groups. As a result, our estimation procedure (which considers only noncapital-gains income) finds a positive elasticity. However, the reported capital gains of the wealthiest taxpayers rose sharply, causing total income for this group to rise relative to that for other groups—leading Goolsbee’s procedure (which considers total income) to find a negative elasticity.

The case for excluding reported capital gains from the income measure used to estimate the elasticity of taxable income appears particularly strong for the 1934–1938 period. The Revenue Act of 1938 increased the fraction of capital gains that taxpayers were required to report in their income. Thus, some of the apparent movements in reported income inclusive of capital gains over this period reflect not behavioral responses, but simply a change in what was included in reported income. The Revenue Act of 1938 also switched to a system where long-term capital gains of high-income taxpayers were taxed at a flat rate. As a result, the after-tax share that enters both our analysis and Goolsbee’s was no longer relevant to long-term capital gains. In contrast, there were no noteworthy changes in the tax treatment of noncapital-gains income in this period other than the changes in after-tax shares. Thus, this is a period where excluding capital gains almost surely leads to more reliable estimates of the elasticity of income to the after-tax share.¹⁶

¹⁶There was also an important change in the tax treatment of capital gains over the 1931–1935 period, though it has a smaller effect on the estimates than the 1938 change. In addition, Goolsbee’s assumption that net income

C. Results for Different Types of Income

In the previous analysis, we focused on the response of overall taxable income to changes in the after-tax share. The *Statistics of Income* provides considerable information about the composition of income. In particular, gross income is divided into various types, and there are figures for the deductions and exemptions that are subtracted from gross income to yield taxable income. It is natural to ask if some of this disaggregate information can shed additional light on the effects of marginal tax rates.

To do this, we follow Piketty and Saez (2003) and group the types of gross income into three categories: capital income (comprising dividends, interest, and rents and royalties; as in the rest of the paper, we exclude capital gains), entrepreneurial income (business income and partnership income), and labor income (wages and salaries). As described in Section ID, the ways in which taxpayers could respond to changes in marginal rates differed considerably across these types of income. Thus, the responsiveness of the types of income to changes in marginal rates may have differed. We also examine the behavior of overall gross income.¹⁷ Since changes in deductions and exemptions affect net but not gross income, one would expect gross income to be less responsive than net income to changes in marginal rates.

The results for the categories should be treated cautiously. Capital income is about two-thirds of gross income (where capital gains are excluded from both the numerator and denominator of this calculation), and entrepreneurial and labor income are each less than a quarter. As a result, the errors in estimating changes in income by category are likely larger than for the estimates concerning total taxable income. Deductions and exemptions, in contrast, are generally small relative to taxable income. As a result, the errors in estimating the behavior of gross income are likely to be small.

Table 4 shows the results. Perhaps the most interesting finding is that gross income (line 2) appears less responsive than taxable income (line 1) to the after-tax share. This suggests that an important part of the response of taxable income that we find operates through changes in deductions and exemptions.¹⁸ That is, taxpayers appear to respond to changes in the after-tax share in part by attempting to shield income legally by qualifying for deductions, exemptions, and credits.

The results concerning the categories of gross income indicate only modest differences. The estimated effects for all three categories are small, and in each case the null hypothesis that the responsiveness equals the estimated responsiveness of overall gross income cannot be rejected. To the degree that one wants to focus on small differences in point estimates, labor income (line 3) is estimated to be more responsive than entrepreneurial income (line 4) or capital income (line 5).

directly determined marginal rates is largely accurate for 1934 and 1935 but not for the other years he considers, when capital gains were taxed separately. Our more thorough computation of marginal rates (described in the online Appendix) addresses this problem. Although we believe that our approach improves on Goolsbee's, our results support his conclusion that the interwar period does not provide evidence of large responses of taxable income to marginal rates.

¹⁷Gross income includes "other income" in addition to capital, entrepreneurial, and labor income. Other income is zero until 1932, and almost always less than 2 percent of income thereafter.

¹⁸Deductions and exemptions averaged only about 17 percent of gross income. Thus only a small part of the difference in the estimated elasticities arises mechanically from the fact that gross income is larger than net income.

TABLE 4—TIME-SERIES/CROSS-SECTION RESULTS BY TYPE OF INCOME

	Type of income	Elasticity of taxable income with respect to after-tax share	Observations
(1)	Taxable income (baseline: OLS, no lags, full sample, line 1 of Table 2)	0.207 (0.031)	230
(2)	Gross income (taxable income plus deductions and exemptions)	0.132 (0.034)	230
(3)	Gross labor income (wages and salaries)	0.221 (0.063)	230
(4)	Gross entrepreneurial income (business income and partnership income)	0.130 (0.151)	230
(5)	Gross capital income (dividends, interest, and rents and royalties)	0.139 (0.043)	230

Notes: See notes to Table 2 for a description of the basic specification. All regressions are estimated by OLS and include group and year dummies, and are estimated with no lags. The sample period is 1919–1941. Standard errors are in parentheses.

D. Discussion

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 interpret our estimates is to ask what they imply about the optimal top marginal tax rate. Saez (2001) shows that if the upper tail of the income distribution is well approximated by a Pareto distribution, income effects are small, and the social marginal value of consumption by the wealthiest taxpayers relative to the social value of government spending is small, the optimal top marginal rate is approximately $1/(1 + \theta\gamma)$, where θ is the Pareto parameter and γ is the elasticity of taxable income with respect to the after-tax share. As described in the online Appendix, the value of θ for a typical year in our sample period is around 1.7. Thus our estimated elasticity of 0.21 implies an optimal top marginal rate of 74 percent. Our highest estimated elasticity, 0.38, implies an optimal top rate of 61 percent.

A second, and perhaps more concrete, way to interpret our estimates is to consider what they imply about the effects of a moderate change in tax rates. For example, consider a switch from a constant tax rate of 40 percent to a constant rate of 45 percent. If taxable income did not respond, the change would increase revenues by 12.5 percent. With an elasticity of taxable income with respect to the after-tax share of 0.21, the rise in revenues is 10.5 percent. And with an elasticity of 0.38, revenues increase by 8.9 percent. That is, even our largest estimated responsiveness of taxable income is sufficiently small that it has only a modest impact on the revenue effects.

As described by Saez, Slemrod, and Giertz (2012), the studies of tax responsiveness using postwar data 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. Thus, an elasticity of taxable income of 0.21 is toward the low end of postwar estimates, particularly for high-income taxpayers. There are several factors that could contribute to this finding.

One possibility is that taxpayers were less sophisticated in the interwar era, and so responded less to changes in marginal rates. However, it would be a mistake to

think of interwar taxpayers 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 (Blum 1959; Brownlee 2000; Romer and Romer 2012). According to Blum (1959, 334), there were “45,000 registered tax attorneys and accountants” in the United States in 1937, even though only 6 million tax returns were filed and only 700,000 had taxable incomes over \$5,000 (*Statistics of Income* 1937, 119). Thus, we are skeptical that changes in sophistication are the main source of the difference over time.

Changes in the composition of income are probably not an important source of the change. High-income taxpayers in the interwar period had higher capital income and lower labor income than their modern counterparts. But we estimate only a slightly greater responsiveness of labor income than capital income to marginal rates. And 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).

The effects of changes in enforcement are unclear. One would expect that the more aggressive use of audits in the interwar period made taxpayers more reluctant to respond to changes in marginal rates by following strategies that were illegal, or even questionable. But the absence of withholding likely operated in the opposite direction. It is difficult to know which force was stronger.

A factor that very likely contributes to 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 lower responsiveness. Nonitemizers 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). In the case of gross income versus taxable income, we obtain the same result for the interwar period.

The final possibility is the most prosaic: perhaps the true difference between the eras is smaller than the difference in point estimates. The postwar estimates, in particular, have nontrivial 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, our estimated elasticity of 0.21 is within the confidence interval for both estimates. Sampling error may therefore be important to the difference in the estimates between the two eras.

IV. Time-Series Evidence on Broader Effects of Changes in Marginal Rates

The time-series/cross-section analysis 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. However, many concerns about the effects of marginal tax rates involve more than taxpayers' short-run responses.

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 near-term response of taxable income do not rule out such long-run 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, a low-earning potential entrepreneur may consider what will happen if an investment is successful enough to lift him or her into the tax-paying brackets. 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 affect long-run growth.

To test such long-run effects, this section investigates the responses of available interwar indicators of investment and entrepreneurial activity to policy-induced changes in the after-tax share at the top of the income distribution. This exercise is inherently more tentative than the examination of the response of taxable income in the rest of the paper. 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, our reliance on the time-series variation means that the effects we are interested in may be confounded by the enormous shocks affecting the economy over the interwar period.

A. Behavior of Investment Indicators

To make use of the times-series variation in marginal tax rates, it is useful to consider high-frequency indicators of productive investment and entrepreneurial activity. While the number and quality of such indicators is much more limited for the interwar period than for today, there are some potentially useful measures.

Series Analyzed.—One available investment indicator is the Federal Reserve index of industrial production for machinery.¹⁹ This series measures the production of this type of investment goods rather than actual investment, but the two are likely to be highly correlated. The series appears to be relatively consistent over the interwar period.

A second series is the real value of construction contracts for commercial and industrial buildings from Lipsey and Preston (1966, 88–90). 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 are reasonably consistent and accurate.²⁰

¹⁹This series is available in US Board of Governors of the Federal Reserve (1943a, 26 and 49). We use the seasonally adjusted version of the series. The data begin in January 1923.

²⁰The number of states covered rises from 27 before 1923 to 36 in 1923 and 1924 to 37 starting in 1925. 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 and deflate it by the Consumer Price Index. 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, accessed January 19, 2011).

A third series is an index of business incorporations from Evans (1948, table 38, 80–81) based on detailed data culled from various states. The states covered vary 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. The resulting index is a measure of one type of business formation.²¹

As discussed in Section ID, interwar policymakers believed that high marginal tax rates skewed investment 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.²²

Aggregate Tax Variables.—Since we are focusing on time-series relationships, 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 in the same way as in our earlier analysis. That is, we calculate the income-weighted average log after-tax share of the top one-twentieth 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 consider the interest-rate ratio. If tax-free and taxable bonds are otherwise comparable, the interest-rate ratio will equal 1 minus the marginal tax rate of the marginal investor. In analyzing this series, we therefore focus on the policy-induced change in the after-tax share of wealthy taxpayers (rather than in the log of this series).

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 they went into effect (which is always January of a given year). When a change was retroactive, we date the changes as occurring when the legislation was passed.

The Time-Series Relationship between Marginal Tax Rates and Taxable Income.—As one piece of evidence of the reliability of inferring the effects of interwar tax changes from time-series data, we begin by examining the aggregate time-series counterpart of the baseline regression in Section II. That is, using annual data, we regress the change in log real reported taxable income of the entire top one-twentieth of 1 percent of the income distribution on a constant and the aggregate policy-induced change in the log of the marginal after-tax share of this group (and potentially on lagged values).

²¹ 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. Thus using it may lead to underestimates of the impact of the personal income tax on overall business formation. As noted above, however, Goolsbee (1998) finds only small evidence of such effects in our sample period.

²² The data on both interest rates are from US Board of Governors of the Federal Reserve System (1943b, table 128, 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.

The results of this exercise are, not surprisingly, much less precise than the time-series/cross-section results, but they are broadly consistent with them. When only the contemporaneous value of the tax variable is included, the coefficient is 0.20—remarkably similar to the baseline time-series/cross-section estimate—but the t -statistic is only 0.9. When one lag is included, the sum of the coefficients rises to 0.61 ($t = 2.0$). When two lags are included, the sum is 0.37 ($t = 1.4$). Thus, this exercise suggests that the time-series relationships may be at least somewhat informative, and that, if anything, they may lead to overestimates of the importance of tax changes.

The Behavior of the Investment Series.—Figures 7 and 8 present graphs of each indicator and the relevant aggregate tax variable. Panel A of Figure 7 shows the behavior of the production of machinery, for which the data begin in 1923. It suggests no clear link between tax rates and this type of investment. Machinery production grew strongly after the 1924 tax cut, but was largely flat after the larger 1926 cut. It likewise changed little 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 suggests that fluctuations in construction were also driven largely by factors other than taxes. Commercial and industrial construction contracts more than quadrupled after the large tax increase in 1919 to pay for World War I. After plummeting in the recession of 1920–1921, they rose over the 1920s, but choppily and in a way not clearly related to the tax cuts in this period. Contracts changed little following the 1932 tax increase, and rose temporarily after the 1935 increase. And like machinery production, they rose strongly after the tax increases leading up to World War II.

Panel C indicates a possible relationship between business formations 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.

Finally, Figure 8 shows the ratio of the interest rate on municipal bonds to the rate on corporate bonds, along with the level of the after-tax share.²³ To the degree the two series are positively related (as one would expect), 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 after 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.

²³For ease in comparing the two series, the figure shows the actual level of the after-tax share, not the policy-induced change. Furthermore, 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.

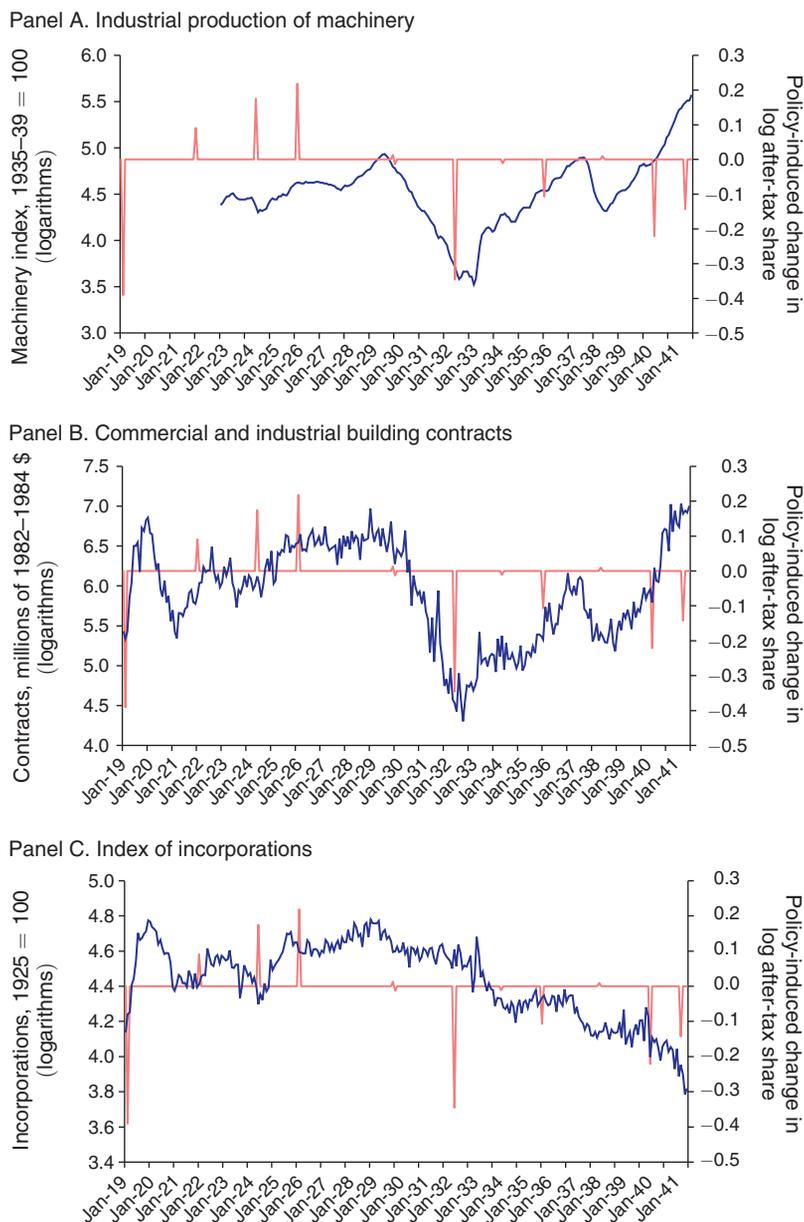


FIGURE 7. INVESTMENT AND POLICY-INDUCED CHANGES IN THE LOG AFTER-TAX SHARE

B. Statistical Tests

We now turn to more formal examination of the relationship between our investment indicators and policy-induced changes in the after-tax share.

Specifications.—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 one-twentieth of 1 percent of the income distribution.

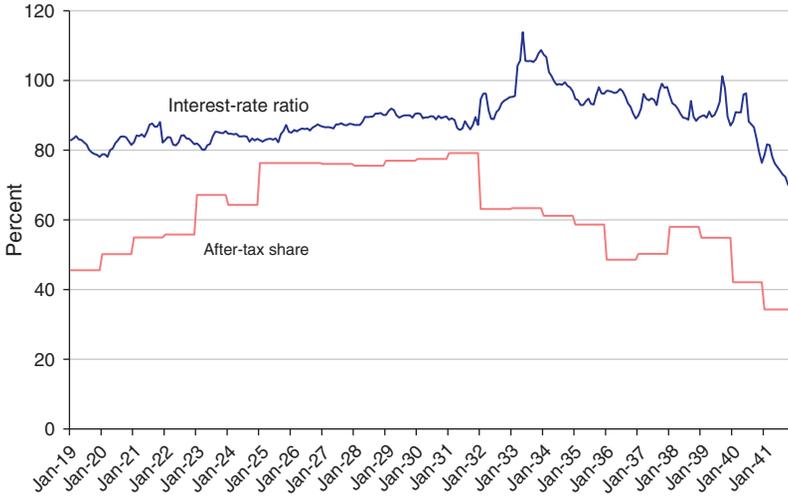


FIGURE 8. RATIO OF MUNICIPAL BOND RATE TO AAA RATE AND AFTER-TAX SHARE

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.²⁴

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

To help address the fact that there were enormous macroeconomic fluctuations in this era, we consider two alternative specifications. The first adds the overall index of industrial production (in logs) to the VAR. This controls for movements in overall economic activity prior to the tax changes. The second and larger variation includes the contemporaneous value and 12 lags of industrial production but treats them as exogenous. This specification asks how investment behaves in the wake of changes in tax rates given the path of overall economic activity following the changes. This approach 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

²⁴That is, the system we estimate is

$$\Delta \ln(1 - \tau)^{PI} = a_1 + \sum_{j=1}^{24} b_{1j} \Delta \ln(1 - \tau)^{PI}_{t-j} + \sum_{j=1}^{24} c_{1j} \ln I_{t-j} + u_{1t},$$

$$\ln I_t = a_2 + \sum_{j=1}^{24} b_{2j} \Delta \ln(1 - \tau)^{PI}_{t-j} + \sum_{j=1}^{24} c_{2j} \ln I_{t-j} + u_{2t},$$

where $u_{1t} = e_{1t}$ and $u_{2t} = \varphi e_{1t} + e_{2t}$ and where e_{1t} and e_{2t} are uncorrelated. Here I is the investment measure; $\Delta \ln(1 - \tau)^{PI}$ is the policy-induced change in the log after-tax share of the top one-twentieth of 1 percent of the income distribution; and e_1 and e_2 are the underlying shocks to the log after-tax share and investment, respectively. The conditions $u_{1t} = e_{1t}$ and $u_{2t} = \varphi e_{1t} + e_{2t}$ capture the assumption that investment can potentially respond to taxes within the month but tax changes cannot respond immediately to investment.

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

When we consider interest rates, 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 the after-tax share. 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 1941:12.²⁵ As with our analysis of the responses of taxable income, we consider the effects of using a range of alternative sample periods as well.

Results.—Figures 9 and 10 show the results for the baseline two-variable VARs. Figure 9 graphs the impulse response functions of the three investment measures to a 1-percentage-point innovation in the measure of policy-induced changes in the after-tax share, together with the two-standard-error confidence bands.

The results echo the patterns suggested by the plots in Figure 7. For machinery investment and construction, there is no evidence that increases in the after-tax share raise investment. 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 10 shows the estimated response of the interest-rate ratio to a rise in the after-tax share of 1 percentage point. The short-run response is positive, but very small. The hypothesis that the relationship is one-for-one is overwhelmingly rejected at all horizons, and at medium and long horizons the estimated effect is negative rather than positive.

The findings are robust to the changes in specifications and samples described above. Including overall industrial production in the VARs and considering different samples does not change the qualitative features of the results.

For incorporations, an important additional check involves the decadal patterns in the data. 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 responsible for the correlation. To check for this possibility, we add a dummy variable equal to one beginning in 1929:9. This addition reduces the estimated impact by about a third and cuts its statistical significance somewhat. But the effect remains substantial and significant: the peak effect is 0.64, and the maximum *t*-statistic is 2.8. Thus, although the

²⁵ 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.

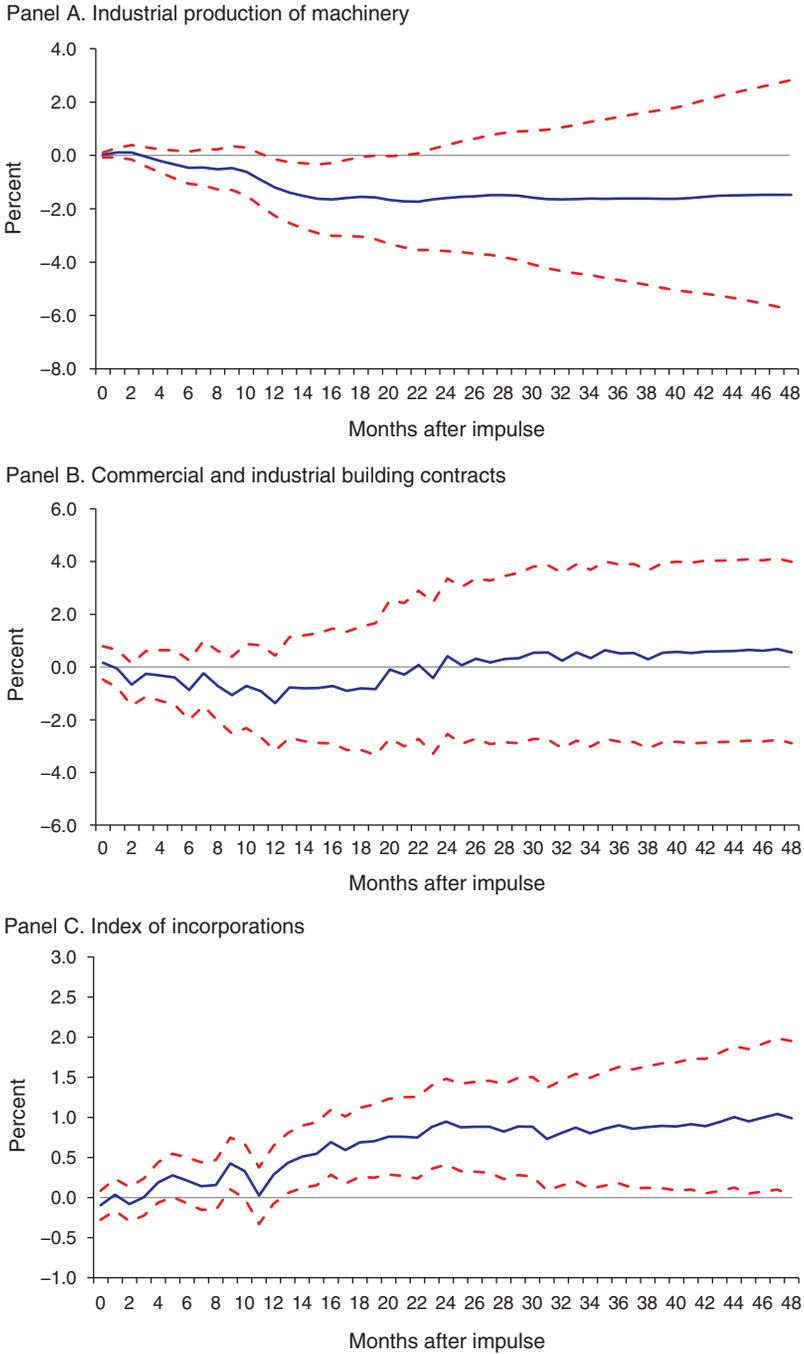


FIGURE 9. IMPULSE RESPONSE OF INVESTMENT TO A 1 PERCENT RISE IN THE LOG AFTER-TAX SHARE (with two-standard-error bands)

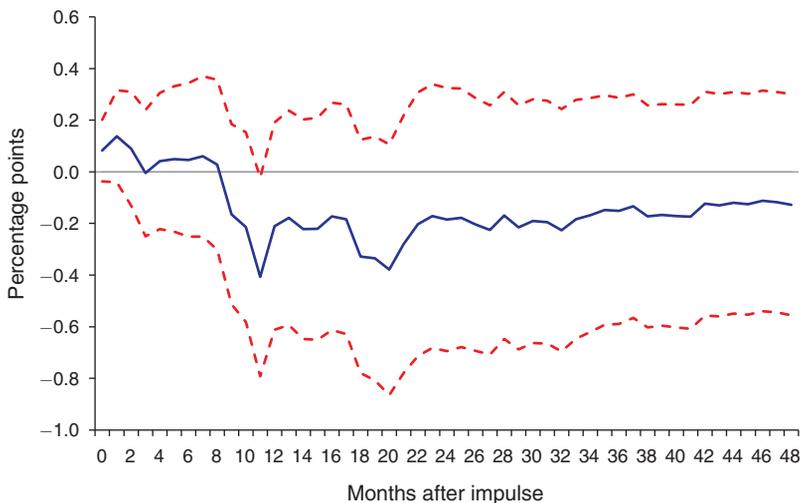


FIGURE 10. IMPULSE RESPONSE OF THE INTEREST-RATE RATIO TO A 1 PERCENTAGE POINT RISE IN THE AFTER-TAX SHARE (with two-standard-error bands)

decadal swings in taxes and incorporations are contributing to the estimates, they are not their main source.²⁶

Discussion.—The finding that is easiest to interpret concerns the interest-rate ratio. First, the rise in the ratio following the 1932 tax increase is likely related to the financial and fiscal stress of this period, including the threat of municipal defaults. Second, for the remainder of the sample, it appears that the taxpayers at the top of the income distribution 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 those 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. In the interwar time series, there is no evidence of an important effect of changes in marginal income tax rates on machinery investment and commercial and industrial construction, with hints of a possible perverse effect. The time-series results for incorporations, on the other hand, provide some evidence that cuts in marginal rates increase business formation. This result could mean that, despite the apparent lack of strong impacts on taxable income and investment, marginal tax rates may nevertheless have had some effects on long-run economic performance.

²⁶ 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. However, incorporations were quite volatile during and immediately after World War I, so it is perhaps appropriate to exclude the 1919 observation.

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 a valuable laboratory for investigating this issue. Changes in marginal rates were frequent, often dramatic, and very heterogeneous across groups at the top of the income distribution. In addition, 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 through time-series/cross-section regressions examining the responsiveness of taxable income to marginal rates. The time-series/cross-section data allow us to control for potential sources of differential trends in income across subgroups 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 is approximately 0.2. This is considerably smaller than the findings of postwar studies (though generally within their confidence intervals). Finally, the estimates are extremely robust.

We also examine how changes in overall marginal rates are related to potential determinants of long-run growth. We find clear evidence that—contrary to the concerns of interwar policymakers—marginal rates 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 along this dimension. We find no evidence that cuts in marginal rates increased machinery investment or business construction, but suggestive evidence that they increased business formation. This last result, which clearly deserves further study, identifies one possible channel through which changes in marginal rates might have had important supply side consequences.

The obvious disadvantage of the interwar period for studying the incentive effects of marginal rates is that the economic environment was very different from today's. Thus, one issue 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. Both of these are important questions for further study.

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