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Evidence on the High-Income Laffer Curve from Six Decades of Tax Reform

IN THE 1980s, federal income tax policy took center stage in the political arena. An influential group of "supply-side" economists argued that high marginal tax rates were severely reducing the incentives of people to work, and that cutting tax rates, by stimulating people to work harder and earn more income, could actually raise revenue. This idea is known in popular parlance as the Laffer curve, after the economist Arthur Laffer, who (according to rumor) sketched out the idea on a cocktail napkin. In fact, political debate in the United States over whether cutting rates can raise revenue dates back many years.[1]

Even if they do not pay for themselves, if cuts in taxes lead to large behavioral responses by individuals, the implications are quite important for the making of tax policy. Basic theory suggests that high marginal rates cause an inefficiency that rises with the square of the tax rate. The greater the behavioral response, the less revenue is raised by the higher rates. In

I wish to thank Gerald Auten, Robert Carroll, Martin Feldstein, Robert Hall, Lawrence Katz, Peter Klenow, Steven Levitt, Bruce Meyer, William Nordhaus, James Poterba, Joel Slemrod, and participants at meetings of the National Bureau of Economic Research and the National Tax Association for helpful comments. I also thank Charles Hadlock for help with the data on executive compensation from the 1930s.

1. Andrew W. Mellon, secretary of the Treasury in the 1920s, was the chief public advocate of the position in that era and discussed it at length in his book *Taxation: The People's Business* (Mellon, 1924). Irwin (1997) documents that a central component of the debate over tariffs in the 1880s was the issue of whether "excessive" government surpluses could be eliminated by *increasing* tariff rates.

the extreme, if the Laffer curve is correct and high rates fail to raise any revenue, they are, quite literally, less than worthless.

As a testable hypothesis, however, the Laffer curve has not fared well. Somewhat unfairly, the public has taken the explosion of budget deficits following the rate cuts of the 1980s, and the elimination of deficits following the rate increases of the 1990s, as a refutation of the idea. More careful econometric analysis has not been any more supportive. An extensive literature in labor economics has shown that there is very little impact of changes in tax rates on labor supply for most people, particularly for prime-age working men.[2] This would seem to indicate that the central tenet of the Laffer curve is demonstrably false—marginal rates seem to have little impact on the amount that people work.

The past decade or so in public finance, however, has seen the birth of a new and important literature very much in the spirit of the Laffer curve, but more sophisticated and potentially much more persuasive. I call it the New Tax Responsiveness (NTR) literature. Perhaps most associated with the work of Lawrence Lindsey and Martin Feldstein but including many others, the NTR literature's main hypothesis is that high marginal rates have major efficiency costs and fail to raise revenue at the top of the income distribution.[3] In doing this damage, high tax rates need not induce people to work less. Instead, they need only lead people to shift their income out of taxable form. The work of Lindsey, Feldstein, and others has shown that, if people do shift their income in this way, it can imply the same revenue and deadweight loss problems as in the original Laffer curve *even if the elasticity of labor supply is zero*. The NTR literature has tried to estimate the impact of this shift with data on high-income people, and it has tended to find large effects. If true, this work means that the marginal deadweight cost of the income tax is quite high, and it calls the progressivity of the tax code into serious question.

The central goal, then, of the NTR literature is to estimate the elasticity of taxable income with respect to the marginal tax rate (or, more pre-

2. See the work of Pencavel (1986), MaCurdy (1992), Heckman (1993), and Moffitt and Wilhelm (forthcoming). The statement is less true for women deciding whether to enter the labor force (see Eissa, 1996, for recent work on the subject), and possibly for certain groups of workers such as doctors or entrepreneurs (see the results of Showalter and Thurston, 1997; Carroll and others, 1998).

3. See, in particular, Lindsey (1987); Feldstein (1995). Discussions of the literature can be found in Slemrod (1998c) and Goolsbee (forthcoming-b).

cisely, to one minus the tax rate). This parameter is critical for determining the deadweight loss of the income tax, the revenue implications of tax changes, even the optimal size of government.[4] As Joel Slemrod has put it, "recently . . . much attention has been focused on an elasticity that arguably is more important than all others, because it summarizes all of what needs to be known for many of the central normative questions of taxation. This is the elasticity of taxable income with respect to the tax rate."[5]

As one might expect of something so influential, considerable controversy surrounds the magnitude of this elasticity. Indeed, estimating it has been one of the most active areas of research in public finance of the last decade. The basic methodology of the NTR work has been the natural experiment, that is, relating changes in the relative incomes of groups following a tax change to changes in their relative tax rates brought about by the tax change. Commonly referred to as difference-in-differences estimation, this method has tended to find large taxable income elasticities when applied to the tax cuts of the 1980s.

The methodology is not without its critics. Some have questioned its validity when comparing high-income people with others.[6] Others have been more generally critical.[7] The potential biases have led many to wonder whether the high estimates from the 1980s are the result of an upward bias in the approach.

Although the difficulties associated with using natural experiments to analyze the behavior of very rich people are potentially serious, in this paper I will not seek to criticize the methods of the NTR literature. Instead, my goal will be to use those same methods but apply them to different time periods than the familiar tax changes of the 1980s and 1990s, to see how robust the case is for a large taxable income elasticity. The results based on tax-based natural experiments from six different tax reforms between 1920 and 1975 suggest that the case may not be particularly robust.

The advantage of using historical data to examine these issues is that there were numerous major tax changes throughout these six decades, both cuts and increases, to provide perspective. The trends in income inequality and other factors potentially biasing work on the 1980s were much differ-

4. See Feldstein (1996); Slemrod and Yitzhaki (1996).
5. Slemrod (1998b, p. 774).
6. Slemrod (1996); Goolsbee (forthcoming-b).
7. See the discussion in Blundell, Duncan, and Meghir (1998); Heckman (1996).

ent in these other periods. The drawback of looking at the historical experience is that the data are substantially worse than those available for more recent periods. In most cases prior to the 1980s only aggregate cross-sectional data are available, requiring statistical interpolation to calculate incomes and tax rates. Where micro-level panel data exist, they lack the detail of tax return data.

The paper begins with an overview of the NTR approach, including the basic theory and the natural experiment methodology. It then examines the empirical approach of the NTR literature and the existing estimates from the 1980s and 1990s. Next a procedure for using cross-sectional tax return aggregates to estimate the tax elasticity is outlined, and the results are checked using data on the period surrounding the Tax Reform Act of 1986. Results from explicit natural experiments using cross-sectional data on five major tax reforms since 1920 are then presented. Finally, the paper turns to panel data on the compensation of high-income corporate executives in the 1930s and the 1970s to examine the impact of tax changes on these individuals' behavior.

The New Tax Responsiveness Approach

Theory

One of the basic premises of the NTR literature is that what matters for calculating the marginal deadweight loss from taxation or the revenue impact of taxation is *not* the elasticity of labor supply with respect to the tax rate. Even if that is literally zero, there can still be major impacts of tax policy on the economy. What matters is the elasticity of taxable income with respect to the tax rate.

An individual maximizing utility subject to a budget constraint who has forms of income or consumption that are not taxable (such as fringe benefits, nontaxed perquisites, or tax deductions) will make choices between labor and leisure when taxes change, as in the standard model. But he or she will also make choices about shifting income and consumption out of taxable forms. Even if shifting into leisure is very small (that is, if labor supply is inelastic), so long as tax changes lead people to do a lot of shifting into tax-free income, many of the implications of the Laffer curve analysis remain. This argument is set forward most clearly in the work of Martin Feldstein.[8]

8. Feldstein (forthcoming).

To a standard model with consumption, *C,* and leisure, *L,* Feldstein adds nontaxable income, *E,* and nontaxable consumption, *D*. The individual maximizes utility over all of these arguments, $U(C,L,E,D)$, subject to the budget constraint that $C = (1 - \tau)[w(1 - L) - E - D]$, where w is the wage rate and τ is the marginal tax rate. The term in square brackets on the right-hand side is defined as taxable income. It is total compensation minus deductions and tax-exempt income. Rearranging the budget constraint makes it obvious why the deadweight loss depends on more than labor supply. If we define $1 + z$ to be $1/(1 - \tau)$, the budget constraint can be written as:

$$C(1 + z) = w(1 - L) - E - D. \tag{1}$$

In this model a rise in the standard income tax (τ) raises the price of taxable consumption, but it does not change the relative price of *L, E,* or *D*. In other words, all of the nontaxed factors make up a composite outside good. The deadweight loss of the income tax is, then, equivalent to the deadweight loss from a sales tax at rate z on taxable consumption. Such a deadweight loss depends on how much taxable consumption falls. It does not matter if the lower *C* increases *L, E,* or *D*. So long as the individual is not at a corner solution, it is not necessary to know the elasticity of substitution in the utility function between these types of untaxed goods. All that one needs to know is the extent to which the individual shifts away from taxable consumption when rates change.

Feldstein shows that the deadweight loss will be:

$$= \frac{1}{2}\left(\frac{z}{1 + z}\right) e_C z C, \tag{2}$$

where e_C is the elasticity of taxable consumption with respect to $1 + z$. Feldstein goes on to show that, for compensated changes, this is equivalent to:

$$= \frac{1}{2}\tau^2\left(\frac{1}{1 - \tau}\right) e_{TI} TI, \tag{3}$$

where *TI* is taxable income and e_{TI} is the elasticity of taxable income with respect to the net-of-tax share $(1 - \tau)$. In principle, all of the elements in this equation can be directly estimated.

The issue of the corner solution is critical. If taxed and nontaxed income are perfect substitutes, a tax change will lead to a large amount of shifting, making the elasticity of taxable income very large, but there will be no

deadweight loss. If they are perfect substitutes, however, it should lead to a corner solution: the taxpayer should switch completely out of the more costly type of income. The fact that wage income is tax disadvantaged, yet people continue to take it, means that it cannot be a perfect substitute for nontaxed compensation. There must be some additional negative associated with taking nontaxed compensation that keeps people at the margin from shifting all of their income into the tax-advantaged form, and that additional negative is what creates a deadweight loss.

Indeed, in this simple model the marginal welfare cost of a tax change is the same whether it shifts the individual out of taxable income into untaxed leisure or into other untaxed forms of compensation or consumption.[9] It is leading the individual to take more of something that he or she would not want if it were not for taxes. This result is quite important and should be better known.

Motivated by this observation, the NTR literature has set out to estimate the elasticity of taxable income and determine whether it is significantly larger than the elasticity of labor supply (thus implying a larger deadweight loss from taxation). The standard approach to identifying the elasticity has been to use natural experiments generated by changes in the progressivity of the income tax.

The Natural Experiment Approach

The idea of a tax-based natural experiment is to start with at least two different groups that experience tax changes of different magnitudes. To control for various unobservable characteristics, the "experiment" assumes that the two groups' reported taxable incomes would grow at identical rates were it not for the changes to their relative taxation. In this literature the groups are usually the very rich and the somewhat rich.

Suppose that the reported taxable income, Y, for an individual or group of identical individuals A (indexed by time, t) is a function of the net-of-tax share with a constant elasticity:

9. The idea that the deadweight loss is *exactly* the same whether it is a shift in hours worked or in form of compensation is probably a bit extreme. There may be social externalities to working, for example, that do not accrue to tax avoidance. More important, Slemrod and Kopczuk (1998) consider the case where the government can directly affect the elasticity of taxable income through its enforcement regime and show that the implications may be rather different from those in this basic model.

$$\ln(Y_t^A) = \alpha_A + \beta \ln(1 - \tau_t^A) + \delta_t + \eta_t^A, \tag{4}$$

where α is a fixed effect for the group, β is the elasticity of taxable income, τ is the marginal tax rate facing the group and is indexed by time, δ is a year effect indexed by time, and η is a random term that is distributed normally. Time-series data on the group before and after a tax change will not be sufficient to identify the elasticity term. Differencing this equation across years yields:

$$\ln(Y_t^A) - \ln(Y_{t-1}^A) = \beta[\ln(1 - \tau_t^A) - \ln(1 - \tau_{t-1}^A)] + \delta_t - \delta_{t-1} + \varepsilon^A. \tag{5}$$

Although this eliminates the group effect α, it cannot eliminate the impact of the time effects. Observing a group's taxable income before and after a tax change will not yield the true taxable income elasticity unless there are no other changes (in the business cycle, for example) that influence income at the same time.

The way around this problem in the natural experiment literature is to use as a control another group of individuals, B, who are thought to have the same characteristics and behavior as the individuals in group A except that they face a different tax change. In other words, they have the same year effects as group A and the same elasticity of taxable income. In this case, the differenced equation for group B is:

$$\ln(Y_t^B) - \ln(Y_{t-1}^B) = \beta[\ln(1 - \tau_t^B) - \ln(1 - \tau_{t-1}^B)] + \delta_t - \delta_{t-1} + \varepsilon^B, \tag{6}$$

and taking the difference of the two differenced equations yields:

$$\Delta\ln(Y_t^A) - \Delta\ln(Y_t^B) = \beta[\Delta\ln(1 - \tau_t^A) - \Delta\ln(1 - \tau_t^B)] + \tilde{\varepsilon}. \tag{7}$$

If group B is a valid control, the year effects will cancel in the second difference. Given data on reported incomes and tax rates, a difference-in-differences calculation will provide a consistent estimate of the true elasticity of taxable income:

$$\hat{\beta} = \frac{\Delta\ln(Y_t^A) - \Delta\ln(Y_t^B)}{\Delta\ln(1 - \tau_t^A) - \Delta\ln(1 - \tau_t^B)}. \tag{8}$$

This is exactly the type of estimate used by Feldstein and others to get the taxable elasticity.[10] A regression counterpart when there are more than two groups is straightforward.

10. Feldstein (1995).

As summarized by James Heckman, one troubling feature of such an estimate is that if the control group is not perfect (that is, if the year effects are not the same), say, because of secular trends in income inequality between groups, the difference-in-differences estimator will not be consistent.[11] The direction of the bias will depend on how the different growth rates are correlated with the relative tax changes, since:

$$E[\hat{\beta}] = \beta + \frac{\Delta\delta^A - \Delta\delta^B}{\Delta\ln(1-\tau_t^A) - \Delta\ln(1-\tau_t^B)}. \tag{9}$$

To illustrate, consider the tax cut included in the Tax Reform Act of 1986 (TRA86). Let the rich be group A and the almost-rich group B. Since the rich received the largest relative tax cut and also had the largest relative income gains, the natural experiment suggests that taxes matter. Indeed, Feldstein calculates that the elasticity exceeds one.[12] If non-tax-related trends in income inequality, however, were driving up the incomes of the rich relative to other groups over this time period, the estimates would clearly be biased upward, from the second term in equation (9). Note that this direction of bias results only because, in this case, the tax change and the unobserved trend moved in the same direction. If TRA86 had imposed a tax increase on the rich while their relative incomes were trending upward, the second term would be negative, and the elasticity would be biased downward. That is one of the primary motivations of looking at natural experiments in other periods.

Three caveats regarding the standard approach are in order at the outset. First, the theory largely relates to compensated elasticities, whereas the natural experiments provide information primarily on the uncompensated effects. Second, numerous types of shifting, such as temporary shifts in the timing of compensation or shifts from the corporate to the individual tax base, may appear as large behavioral responses in the natural experiment approach but may not have the same implications for deadweight loss and revenue. Third, taxes have many potentially important long-run impacts, for example, on occupational choice or age of retirement, which are neglected in the standard approach. This paper focuses strictly on an analysis of the relatively short-run responses to taxation, in keeping with the

11. Heckman (1996).
12. Feldstein (1995).

NTR literature. Although using tax return data to identify the magnitude of the longer-term effects is almost impossible, this does not imply that such factors are unimportant.

Revenue Implications

The discussion above and the results presented later in this paper lie a bit afield of the popular notion of the Laffer curve. The academic debate is predominantly about estimating the behavioral response to taxation, that is, the elasticity of reported income with respect to the net-of-tax share. The popular conception, on the other hand, concerns where the top of the Laffer curve is—at what marginal tax rate does tax revenue start to decline? In some sense, this is the elasticity of tax revenue with respect to tax rates. Obviously, these are not the same issue.

One reason that economists have not spent as much time examining the popular conception of the Laffer curve is that since the tax system has a schedule of marginal rates, the conventional Laffer curve does not exist. The revenue impact of a marginal rate change depends on the tax structure facing the individual's entire income. I will follow the public finance literature and examine the theoretically well defined behavioral response of individuals to a change in the marginal net-of-tax share, and will spend little time on revenue implications. A convenient way, however, to get a suggestive sense of the revenue effects of taxes, given an estimated elasticity with respect to the net-of-tax share (that is, to translate between the NTR elasticity and the Laffer curve), is to note that if there were only a single tax rate in the economy, and if the elasticity of taxable income with respect to the net-of-tax share is e, the revenue-maximizing tax rate (that is, the top of the Laffer curve) would be $1/(1 + e)$. In other words, taxes would raise revenue so long as the elasticity did not exceed $(1 - \tau)/\tau$. Although the tax code does not have this simplistic structure, at least it provides a benchmark.

Findings of the New Tax Responsiveness Literature

Tax Responsiveness in the 1980s

Because the NTR literature has by now grown quite voluminous, I will selectively choose from it in order to set the stage for why looking at tax

reforms in previous decades might be useful.[13] I will focus exclusively on work that directly estimates the elasticity of taxable income. Related literatures on the impact of marginal tax rates on fringe benefits, capital gains distributions, charitable giving, and so on, are important but beyond the scope of this paper.[14]

NTR estimation of the elasticity of taxable income and the behavioral responses to taxation really begins with the work of Lawrence Lindsey.[15] He uses cross-sectional data from the early 1980s for various income groups to show that the reported incomes of taxpayers at the top of the income distribution rose dramatically at the same time that their marginal tax rates were falling. Lindsey argues that, if the people at the top of the income distribution are the same people over time, the repeated cross-sections are similar to panel data. Given this assumption, his reasoning is explicitly natural experiment based. He compares the rich with other groups and argues that the marked difference in relative income growth rates at the top arose from differences in tax treatment. He estimates that the elasticity of taxable income for the highest-income taxpayers was well in excess of one.

Daniel Feenberg and James Poterba use cross-sectional data from aggregate tax return data from the 1950s to 1990 and from micro tax return data from 1979 to 1991 in order to calculate the share of total income accruing to those taxpayers with the highest incomes (the top ½ percent of the income distribution).[16] Their primary area of interest is the significant increase in the share of income going to the wealthy in the 1980s. Feenberg and Poterba find that most of this increase was due to a significant rise in 1987 and 1988 in the incomes of the extreme tail of high-income people, and that this is consistent with people responding to the tax incentives in TRA86. Although they do not put their findings in an elasticity context, theirs is certainly consistent with a natural experiment approach. Incomes rose dramatically for the group that had the largest relative cut in its marginal tax rates.

Because only cross-sectional data are available for most of the historical tax changes discussed in this paper, it is important to note at the out-

13. Slemrod (1998a, 1998b) surveys some components of the NTR literature.

14. See Auerbach (1988), Clotfelter (1997), and Woodbury and Huang (1991) for surveys of some of these topics.

15. Lindsey (1987).

16. Feenberg and Poterba (1993).

set the criticisms raised against the cross-sectional studies. First, in any analysis of the impact of tax changes on reported income, changes in the tax code often change the definition of income as well as the tax rate. It is basically impossible to maintain constant definitions of income with aggregate data. In existing work that corrects for this problem in the micro data of Feenberg and Poterba, however, the results do not change much.[17]

Second, and more important, several analysts have questioned whether people remain in the same relative income categories across time. Slemrod discusses the potential importance of temporary income and rank reversals for drawing conclusions about relative income changes.[18] Capital gains income, for example, is often realized in spikes. He finds that the composition of high-income groups does have some significant turnover from year to year. Because of this problem, the work of the NTR literature has generally turned to panel data to check whether the elasticities calculated with cross-sectional data would be affected.

Feldstein explores the tax cuts of TRA86 with panel data.[19] TRA86 included a major tax cut whose largest effect was at the top of the income distribution. Feldstein compares income growth for people in the 49–50 percent brackets, the 42–45 percent brackets, and the 22–38 percent brackets before TRA86. He finds that the incomes of the very rich rose the most and that the very rich were also the group that received the biggest tax cut. The resulting elasticities of taxable income averaged between 1 and 1.5, with some as high as 3.

Feldstein's results were criticized for including only a small number of observations of the highest-income people and for not using a statistical method that could indicate the precision of the estimates.[20] Gerald Auten and Robert Carroll, however, using an internal Treasury sample of thousands of high-income tax returns and a regression methodology, were again able to find significant elasticities, although smaller than those Feldstein had estimated.[21] With these data, which are not publicly available, they also had information on occupation and other nontax factors as reported on the tax returns, and they found that controlling for these fac-

17. Slemrod (1996).
18. Slemrod (1992, 1994, 1996).
19. Feldstein (1995).
20. Slemrod (1995).
21. Auten and Carroll (1995, forthcoming).

tors and the weighting of the sample did make some difference to the results. Their preferred estimate of the elasticity of taxable income was around two-thirds.

Tax Responsiveness in the 1990s

The work from the 1980s consistently shows large elasticities in a natural experiment context. One lingering concern about such work, however, is the possibility that other factors coincidentally correlated with tax changes are, in reality, driving the relative income changes, be they unobserved economic changes or, in the case of TRA86 in particular, numerous other tax changes in addition to marginal rate cuts.[22] Although clearly there need not be a unique elasticity across time, having results from other tax changes that agree with the elasticities from TRA86 would be more persuasive, since so much else happened at that moment

A large literature in labor economics has noted that, for reasons unrelated to taxation, income inequality was rising throughout the 1980s.[23] If this pattern extended to the top of the income distribution, this would mean that the NTR experiments examining tax cuts at the top of the distribution suffer from potentially serious upward bias, since taxes decreased for the same people whose relative incomes were trending upward.[24]

These facts have made results from the 1990s quite important for evaluating individuals' responses to marginal tax rates. In the 1990s, secular trends in inequality continued, but President George Bush and later President Bill Clinton *raised* marginal tax rates on high-income taxpayers. Feldstein and Feenberg present a preliminary analysis of the 1993 tax increase on the rich using aggregate cross-sectional tax return data. They

22. Indeed, there is enough literature on the effects of TRA86 on various aspects of economic behavior that Auerbach and Slemrod (1997) could write an entire survey on the subject. Fullerton (1996), Gordon and Slemrod (forthcoming), and others stress the changes brought about by TRA86 in the incentives to shift income from the corporate base to the individual base.

23. See Katz and Murphy (1992) or the survey by Levy and Murnane (1992).

24. Slemrod (1996) shows that such trends may eliminate all the estimated effects of tax policy except in the case of 1986. Goolsbee (forthcoming-b) shows that when secular trends are included in analyses of the compensation of very high income people such as executives and professional athletes, even the elasticities from 1986 are much smaller.

find that the incomes of the approximately 1 million richest taxpayers fell significantly from 1992 to 1993, while the incomes of lower-income groups rose, indicating a large elasticity.[25] Their work, however, cannot distinguish temporary from permanent shifts—a potentially important issue, since President Clinton proposed the 1993 tax increase in late 1992, giving people a chance to realize income in the earlier year to avoid the higher tax.[26]

In my own work using compensation data from several thousand corporate executives, I have shown that as much as 20 percent of the total wage and salary decline of the top 1 million taxpayers in 1993 may be attributed to the change in the reported incomes of just 10,000 corporate executives (and more than 2 percent from a single individual). These changes were driven almost exclusively through a one-time cash-out of stock options in late 1992 in anticipation of the higher rates.[27] In these data, the short-run elasticity of income exceeds one, as in other NTR studies, but the elasticity after one year is closer to one-third or less. The results also indicate that not correcting for secular time trends in inequality creates a substantial bias in the data. Other work using detailed tax return data has tended to bear out the finding of smaller elasticities than those found in the 1980s.[28]

As Slemrod has observed, the implications for government policy if the elasticity is, say, 0.4 rather than 1.4 are tremendous.[29] The marginal deadweight loss is more than three times higher in the second case, and progressive tax increases are unlikely to raise any additional revenue. The evidence on the question is conflicting. Results based on the 1980s suggest that the elasticity is close to one, or even above one. The literature based on the 1990s suggests that it is significantly smaller than one. But that is, essentially, all the evidence there is. There is almost no econometric work

25. Feldstein and Feenberg (1996).

26. See Parcell (1996). Slemrod (1992, 1994, 1996) discusses in detail the general importance of timing shifts in the reporting of income.

27. Goolsbee (forthcoming-a).

28. Sammartino and Weiner (1997), using a Treasury panel of tax returns in the 1990s, argue that the evidence shows little effect of tax rates on taxable income. Carroll (1998) uses a long panel of individual tax returns from 1989 to 1995 drawn from Treasury data to show that, although it is not near one, there is a significant elasticity of around 0.4 to 0.5.

29. Slemrod (1998b).

based on any other time period to provide perspective on the debate, even though there have been numerous tax changes through time.[30]

Estimating Elasticities with Aggregate Data Alone

Method

I first estimate the elasticity of taxable income using cross-sectional data from tax returns. Of course, natural experiments with these data suffer from all the standard problems mentioned above. To get results, one must assume there are no rank reversals within the income distribution over time. Furthermore, it is impossible to control for changes in temporary income, and I do not separate out different types of taxable income such as capital gains. Later in the paper I present results using panel data that address some of these problems.

In examining older periods, one must immediately confront the fact that no individual-level tax return data are available that can be used to estimate the elasticity of taxable income. The only data are those given by the annual income histograms in the *Statistics of Income* published by the Internal Revenue Service (IRS). These data show the number of returns and the total income reported for several income classes, such as from $50,000 to $100,000, from $100,000 to $200,000, and so on. Table 1 gives an example.

Unfortunately, these income brackets are fixed in nominal dollars over time. Thus, even if there were no rank reversals, the number of people in each reporting group changes. The data may report, for example, that in the starting year there were 1,000 people with incomes over $1 million. Four years later, there may be 1,500 people with incomes over $1 million. It would clearly be wrong to compare the mean incomes for the same nominal bracket, since the composition of the group has changed dramatically. To calculate an accurate income change for the original 1,000 peo-

30. Recent exceptions include the work of Saez (1999a, 1999b). Saez (1999a) estimates the impact of marginal rate increases caused by inflationary "bracket creep" from 1979 to 1981. Saez (1999b) examines the impact of tax rates on the number of returns by income class in the period before World War II and uses a procedure similar to the one adopted here.

Table 1. Number of Tax Returns and Total Income by Level of Adjusted Gross Income, 1985 and 1989

Income range[a]	*Thousands of tax returns, 1985*	*Income from all returns, 1985*[a]	*Thousands of tax returns, 1989*	*Income from all returns, 1989*[a]
30–40	11,635	402,942	12,100	420,231
40–50	6,702	297,914	8,590	389,689
50–75	5,629	333,710	9,921	594,483
75–100	1,263	107,424	3,059	261,107
100–200	909	119,200	2,090	276,331
200–500	238	68,986	613	179,115
500–1,000	41	27,541	116	78,516
1,000+	17	40,100	58	151,465

Source: Internal Revenue Service, *Statistics of Income* (1985, 1989).
a. In thousands of current dollars.

ple requires somehow observing the mean income of the 1,000 people with the highest incomes out of the 1,500 people in the later sample.

Although direct observation is not possible, if the incomes are distributed according to a known distribution, it is possible to compute the mean income of those top 1,000 people. To make such a calculation, I extend a common interpolation approach from the literature and assume that incomes in the later year are distributed according to a Pareto distribution.[31] This means that the probability that an individual's income exceeds I is:

$$P(Y > I) = \left(\frac{k}{I}\right)^{\theta}, \tag{10}$$

where k and θ are the parameters of the distribution. This distribution has been shown to fit the top of the income distribution well.[32] As described in the appendix, this distribution can be easily estimated with the IRS histograms and seems to approximate these data well. The key parameter is θ, the shape parameter, which specifies the relative likelihood of high incomes.

The essence of the approach is straightforward. Suppose that in the starting year there were three tax brackets—$100,000 to $500,000, $500,000 to $1 million, and over $1 million—and in these brackets there were 10,000, 5,000, and 1,000 people, respectively. In the earlier year

31. See Feenberg and Poterba (1993) or Saez (1999b).
32. References can be found in Johnson and Kotz (1970).

one observes the mean income for each of these groups and would like to know what happens to the mean incomes of these same groups in a later year.

Suppose that in the later year the numbers of people in the three brackets are 12,000, 8,000, and 2,000. If the incomes making up this later year's histogram are Pareto-distributed with known parameters, the formulas derived in the appendix can be used to solve for the new cutoff income levels for the top 1,000, the next 5,000, and the next 10,000, to match them to the original groups. The equations can also be used to calculate the mean incomes of those groups. Assuming no rank reversals, comparing these mean incomes with those in the earlier year for each group gives a measure of income change and becomes the dependent variable for the regressions relating relative income changes to relative tax changes.

To arrive at the independent variable in the regression, the difference in the net-of-tax share for each group between the earlier year and the later year, requires dealing with a potential endogeneity problem. It is valid to calculate the marginal tax rate based on observed income in the base year, since this is before the tax change. However, it is not valid to take the observed marginal tax rate from reported income in the later year, because this is endogenous: the level of reported income directly affects the observed marginal rate.[33] To get a tax rate that is not endogenous, I take the mean taxable income in the base year and inflate it at the rate of nominal GDP growth to the later year. I then calculate the marginal tax rate faced by an individual with that income and use that rate for the later year.

In the pre–World War II samples, the histograms are divided by taxable income, and so the results account for changes in deductions and the like. For the two experiments after the war, however, the histograms represent gross income categories, and so I have to estimate the Pareto distribution using gross income. To convert gross income to taxable income, I assume that the ratio of taxable to gross income is constant. Although this rules out tax-induced changes to deductions, in these two samples this makes little difference to the results because the ratio remained fairly constant across the experiments. From 1948 to 1952, when the net-of-tax share for people earning more than $500,000 a year fell by 57 percent, the ratio of taxable to gross income for people in the same nominal bracket

33. This is explained further in Carroll (1998), Triest (1998), and Moffitt and Wilhelm (forthcoming).

fell only from 0.86 to 0.83 (these data include only persons who itemized deductions). From 1962 to 1966, when the net-of-tax share for people earning more than $500,000 a year rose by more than 200 percent, the ratio of taxable to gross income rose only from 0.78 to 0.80. This is similar to the finding of Carroll, using an extensive panel data set, that the elasticities estimated with adjusted gross income differ by about 0.1 or less from those using full taxable income.[34]

Checking the Method: The Tax Reform Act of 1986

I use data from the TRA86 episode as a means of demonstrating the method and of checking whether the approach just described gives plausible answers. Since we have panel data results from before and after TRA86, we have a good idea, a priori, of what the results should be.[35]

Table 1 presents the aggregate data given by the IRS for 1985 and 1989 for all categories above $30,000 of income. The number of returns in each category rises from the first year to the second. There were 17,000 taxpayers with more than $1 million of gross income in 1985, and their average income was almost $2.4 million. By 1989, however, there were 58,000 people with incomes over $1 million. I need to calculate, assuming the same 17,000 people were at the top of the income distribution in 1989, the average income of those top 17,000 out of the 58,000 people in 1989. To do this, I estimate the Pareto distribution on the 1989 data and get a shape parameter of 1.887 (all the Pareto estimates are listed in the appendix table). The standard error was 0.056, so this parameter is estimated somewhat precisely; the R^2 for the regression exceeded 0.99, despite having only eight observations.

Using this shape parameter, I solve for the new cutoff levels in 1989 for the top 17,000, as derived in the appendix. Table 2 presents the results. To be in the top 17,000 in 1989 required an income of at least $1.9 million,

34. Carroll (1998). A different way to think about this is to note that there can be a large elasticity of deductions with respect to the tax rate but that this may have very little effect on the elasticity of total taxable income, if deductions make up a small part of total income.

35. This is not meant to imply that the large existing estimates from TRA86 reflect the "true" elasticity. As described above, trends in inequality and simultaneous changes to many parts of the tax code may be the source of the large estimated elasticities. The goal here is rather to test whether the Pareto method gives results similar to the micro data for the same tax change.

Table 2. Estimates of Income Growth by Income Group, 1985 and 1989

Income range, 1985[a]	*Pareto-estimated income range, 1989*[a, b]	*Mean income, 1985*[a, c]	*Pareto-estimated mean income, 1989*[a]	*Change in log of income*[d]	*Change in log of net-of-tax share*[e]
30–40	38–52	35	44	0.232	–0.041
40–50	52–66	44	58	0.272	0.072
50–75	66–106	59	82	0.322	0.150
75–100	106–159	85	127	0.402	0.144
100–200	159–354	131	221	0.521	0.197
200–500	354–1,000	290	527	0.598	0.365
500–1,000	1,000–1,916	672	1,319	0.675	0.365
1,000+	1,916+	2,359	4,077	0.547	0.365

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income* (1985, 1989).
a. In thousands of current dollars.
b. Range of incomes in 1989, assuming no rank reversals, of the individuals in the corresponding 1985 income range.
c. Calculated from table 1.
d. Calculated as the log of the 1989 Pareto-estimated mean income minus the log of 1985 mean income.
e. Calculated as described in the text.

and this group had a mean income of more than $4 million, up from $2.4 million in 1985. The incomes of people with the same relative rankings as the $100,000 to $200,000 group in 1985 had, by 1989, increased to between $159,000 and $354,000, and the mean income had increased as well. The 1985 net-of-tax share is calculated from the observed income data before the tax change. The 1989 net-of-tax share comes from growing the 1985 mean income at the rate of nominal GDP growth (30.1 percent over the period) and using that income to calculate the 1989 marginal rate.

The essence of the NTR approach is to compare the percentage change in income for each group with the percentage change in the net-of-tax share for the group. The table shows that incomes generally rose most at the top of the distribution, where the tax cuts were largest. I calculate the elasticities in two ways. The first method is suggestive but less preferable than the second, as described below. The first approach breaks the income distribution into three groups and computes a relative elasticity rather than estimating a regression. I do this to parallel the original work of Lindsey and Feldstein.

For TRA86 the groups I use are those with incomes from $30,000 to $100,000, from $100,000 to $500,000, and over $500,000. (In this and subsequent analyses, the group with the lowest incomes of the three is des-

Table 3. Computed Relative Elasticities of Taxable Income for the 1986 Tax Change

Income group	*Income range, 1985*[a]	*Thousands of returns, 1985*	*Mean income, 1985*[a]	*Pareto-estimated mean income, 1989*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	30–100	25,229	45.3	59.0	0.265	0.072
B	100–500	1,147	164.1	277.2	0.525	0.197
C	500+	58	1,166.2	1,898.1	0.487	0.365

Comparison	*Elasticity*[c]
C versus B	–0.22
C versus A	0.76
B versus A	2.07

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.
a. In thousands of current dollars.
b. Nominal GDP in 1989 was 1.301 times that in the base year 1985. The shape parameter θ used in the Pareto calculation was 1.887 (see table A1).
c. Difference-in-differences elasticity, calculated as in text equation (9).

ignated group A, the middle group B, and the highest group C.) Obviously, these are aggregated from the finer histogram data. The relative elasticities for each pair of groups are shown in table 3. As described above, the estimate of the elasticity is the difference in changes in the logarithm of income for the two groups divided by the difference in the change in the logarithm of the net-of-tax shares for the two groups. Comparing group A with group C, for example, the difference-in-differences elasticity is equal to (0.487 – 0.265)/(0.365 – 0.072), or 0.76. Comparing groups A and B, the elasticity is 2.07. Comparing groups B and C, however, accentuates the weaknesses of the computation-based approach. The elasticities are often rather sensitive to the income groups chosen, and there is no standard error to allow one to perform statistical tests. In this comparison, the net-of-tax share change goes in the opposite direction from the income change, and thus the elasticity is negative.

To get around these problems and to use all of the information available in the histogram data, regression estimates are preferable. Table 4 takes all of the income categories listed in tables 1 and 2 and reports a regression of the change in log income on the change in log net-of-tax share (that is, using the last two columns of table 2). Since the variables are in logarithmic form, the coefficient on the tax term is a direct estimate of the elasticity of taxable income. Note that this is still the same natural

Table 4. Regression Estimates of the Elasticity of Taxable Income for the 1986 Tax Change[a]

	Baseline	*Higher* θ[b]	*Lower* θ[b]
	4-1	*4-2*	*4-3*
Constant term	0.243	0.250	0.235
	(0.038)	(0.048)	(0.035)
Change in log of net-of-tax share	1.003	0.875	1.149
	(0.150)	(0.193)	(0.141)
No. of income categories	8	8	8
R^2	0.88	0.77	0.92

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Estimates use values for the shape parameter θ two standard deviations above (column 4-2) or below (column 4-3) the value used in column 4-1 (1.887).

experiment as above, but it is now using all of the information to estimate the elasticity.

Column 4-1 of table 4 presents the results of this regression. The estimated elasticity is approximately 1, with a standard error of 0.15. The standard error here is biased downward, since the taxable income is calculated using the estimated Pareto distribution as if it were known with certainty. Since the calculation of log income given θ is somewhat complex, it is a bit complicated to correct the standard errors. Instead, to demonstrate the robustness of the results to the value of θ, columns 4-2 and 4-3 reestimate the regression using the changes in log income based on values of θ that are two standard errors above and two standard errors below the point estimate (the standard errors are listed in the appendix table). The resulting elasticities are 0.88 and 1.15, respectively, which are still large.[36]

36. I also tried to test for the importance of the Pareto assumption itself, since it enforces a smoothness to the income distribution that may not exist. Using the estimated Pareto distribution, I calculated the imputed mean income for the observed nominal brackets in the later year (as opposed to the mean income for the same people in the previous year that is used in the standard results). This has the advantage that the true value is reported in the later year's data, so I can compare the mean income estimated using the Pareto method with actual mean income. To create an adjustment factor, I added the log difference between the predicted and the observed income to the incomes used in the text. In other words, if predicted mean income for people with over $1 million of income in 1989 was 10 percent lower than the actual mean income of that group, I added 10 percent to the mean income of the highest income group in the natural experiment regressions (those with more than $1.9 million in 1989). The

In general, although based on data that are much more sparse, these Pareto results give elasticity estimates close to those in the existing NTR literature for TRA86. The elasticity seems to be close to one.

Cross-Sectional Evidence from Six Decades of Tax Reform

I now apply the same methodology to an examination of five major tax reforms from 1920 to 1966. I purposely avoid examining the tax increases during the world wars, although they were sizable, simply because so much else was taking place simultaneously that it would be hard to conclude much about taxable income or labor supply in such periods, particularly during World War II.

One important caveat should be noted in addition to those about using aggregate cross-sectional data, mentioned previously. Although most of the NTR literature seeks to estimate "the" elasticity of taxable income, there is no reason to expect that the elasticity should be equal across years or across different types of people.[37] I have tried to choose years sufficiently separated in time to avoid temporary shifting, but clearly the prevalence and ease of use of tax shelters and other avoidance schemes have varied greatly over time. In addition, the natural experiments are not on the same types of taxpayers in each tax change. In the early years of the income tax, only the very rich paid any income tax at all, whereas since then the tax has become quite broad based. Finally, the tax avoidance technologies of different income groups may be quite different, and the sensitivity of high-income people to economic fluctuations may be greater, implying that relative elasticities may differ depending on the state of the business cycle or other factors.[38]

Note, too, that any biases arising from spurious correlation of changes in income inequality with tax changes will lead to bias in these experiments as well. Trends in income inequality have varied greatly since 1910,

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estimated elasticities were very similar in all of the cases, because the differences between predicted and actual income were almost always minimal.

37. Slemrod (1998a) and Slemrod and Kopczuk (1998) have emphasized that the taxable income elasticity will depend directly on the enforcement regime and other aspects of the tax system.

38. See the evidence in Saez (1999b) and Goolsbee (forthcoming-b).

however, and my hope is that looking at different decades will help indicate how important this factor is.[39]

The goal of the elasticity calculations here is not to isolate the "true" elasticity. Such a number probably does not exist. The goal is, instead, to repeat standard methods on other time periods to get a sense of whether the NTR results from the 1980s are historical outliers.

The Tax Cut of 1924–25

The first income tax was enacted in 1913 amid rancorous debate and only after a constitutional amendment specifically allowed it. A few years later the government relied on the progressive income tax rather heavily to finance U.S. involvement in World War I. Technically, in the years before World War II, the "income tax" (also known as the "normal tax") referred to the low and generally rather flat rate above some exemption. The progressive portion of the income tax was known as the "surtax." The surtax was just a rate added to the normal rate, and it varied by income level. The true marginal rate was the sum of the two.

Even in the early years of the income tax, marginal tax rates could get very high at the top of the income distribution. Table 5 lists the top marginal tax rates for each year of the decade from 1913 to 1923.[40] During World War I the top bracket rose as high as 77 percent. Note, however, that at this time the tax code as a whole applied to only a small part of the population, and the highest rates applied to a select group indeed. The 77 percent rate, for example, applied only to income in excess of $1 million (in 1918 dollars); only sixty-seven people were in that top bracket.[41] When the war ended, rates did not return to their prewar levels. President Woodrow Wilson discussed lowering tax rates in his final message to Congress in 1920, but rural opposition among Democrats prevented tax reform in the latter part of his administration.

In 1921, Republican Warren G. Harding swept into the presidency, and Republicans took control of Congress. The Republicans were traditionally opposed to the income tax, and tax reform was viewed as one of the most

39. Goldin and Margo (1992), Katz and Murphy (1992), and Goldin and Katz (1999) analyze the trends in detail.

40. The data are taken from Pechman (1983) and Internal Revenue Service, *Statistics of Income* (1940).

41. Internal Revenue Service, *Statistics of Income* (1940).

Table 5. Top Marginal Tax Rates, 1913–23
Percent

Year	*Top marginal rate*[a]
1913	7
1914	7
1915	7
1916	15
1917	67
1918	77
1919	73
1920	73
1921	73
1922	58
1923	58

Source: Pechman (1983) and Internal Revenue Service, *Statistics of Income* (1940).
a. Sum of the normal tax rate and the surtax rate.

pressing issues facing the country. Business leaders were somewhat doubtful of Harding's commitment to cutting tax rates, but when he appointed Andrew W. Mellon to be the secretary of the Treasury, they were ecstatic.[42] Mellon was an inveterate proponent of reducing tax rates. The arguments in his 1924 book, *Taxation: The People's Business,* bear striking resemblance to those of the 1980s. Mellon advocated lower surtaxes to encourage high-income people to stop wasting effort avoiding taxes through shelters, municipal bonds, and so on and, instead, start investing productively. He argued that high tax rates and serious progressivity outside of wartime were both inefficient and anti-American. At one point he asserts succinctly, "if the price is too high, the taxpayer, through the many means available, avoids a taxable income and the Government gets less out of a high tax than it would out of a lower one."[43]

Republicans would succeed in reducing taxes in the Revenue Act of 1921, but the biggest cuts would take place in 1924 and 1925 under the leadership of President Calvin Coolidge. Coolidge was an anti-tax Republican who wanted rates reduced.[44] From 1922 to 1926, the top marginal tax rate fell from 58 percent to 25 percent. Because these tax changes flattened the rate structure, they created a natural experiment not unlike those of

42. Murray (1969).
43. Mellon (1924, p. 81).
44. Sobel (1998).

Table 6. Marginal Tax Rates for Selected Income Levels, 1922 and 1926

Income (thousands of current dollars)	*Marginal rate[a] (percent)*	
	1922	*1926*
5	8	3
10	10	6
25	18	12
50	31	18
100	56	25
500	58	25
1,000	58	25

Source: Internal Revenue Service, *Statistics of Income* (1940).
a. Sum of the normal tax rate and the surtax rate.

the 1980s. The tax cut applied to all incomes but was largest for those at the top of the income distribution. Table 6 summarizes marginal tax rates for a variety of income points in 1922 and in 1926.

The 1920s may also have been similar to the 1980s in the potential bias arising from worsening income inequality. Several analysts have claimed that the 1920s were a period of rising income and wealth inequality.[45] Unfortunately, most of the data on the subject seem to come from tax records, and so do not provide an independent source of information. The data of Claudia Goldin and Lawrence Katz suggest that inequality may not have risen much in the 1910s and 1920s.[46] However, if there were a secular trend in inequality in this sample, it would likely lead to an upward bias in the estimated elasticity for the same reasons as in the 1980s—tax cuts and non-tax-related income trends moving in the same direction.

I start with the difference-in-differences discrete computations. I choose the two years before and after the marginal rate changes of 1924 and divide taxpayers into three groups. Group C consists of those with more than $100,000 of income in 1922, group B of those with incomes from $50,000 to $100,000, and group A of those with incomes from $25,000 to $50,000. Adjusted using the GDP deflator, the upper bound even of group A would be greater than $250,000 per year in 1996 dollars.[47] The tax rate is calculated by increasing initial incomes by the 33.6 percent increase in nominal GDP over the period. As table 7 outlines, the net-of-tax share rose most

45. See Lampman (1967) or the data in Kuznets (1953).
46. Goldin and Katz (1999).
47. Gordon (1998).

Table 7. Computed Relative Elasticities of Taxable Income for the 1924–25 Tax Change

Income group	*Income range, 1922*[a]	*No. of returns, 1922*	*Mean income, 1922*[a]	*Pareto-estimated mean income, 1926*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	25–50	35,478	34.1	48.2	0.347	0.048
B	50–100	12,000	67.1	104.3	0.441	0.124
C	100+	4,031	221.5	448.7	0.706	0.556

Comparison	*Elasticity*[c]
C versus B	0.61
C versus A	0.71
B versus A	1.24

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. In thousands of current dollars.

b. Nominal GDP in 1926 was 1.336 times that in the base year 1922. The shape parameter θ used in the Pareto calculation was 1.608 (see table A1).

c. Difference-in-differences elasticity, calculated as in text equation (9).

for group C, and similarly for reported incomes. Using the shape parameter for 1926 from the appendix, the difference-in-differences elasticities listed at the bottom of the table are relatively large. Two of the implied elasticities are around 0.6 to 0.7, and the third is 1.24.

Results from the preferred, regression method, using the full histogram of income categories with at least $5,000 a year and the baseline value of the shape parameter, are shown in column 8-1 of table 8.[48] The regression coefficient is a direct estimate of the elasticity and is 0.59 in this period. In column 8-2 I examine only those with incomes of at least $25,000 per year; the elasticity is similar at 0.54, indicating that perhaps there is not a serious problem comparing high-income people with lower-income people. Columns 8-3 and 8-4 demonstrate that the elasticity estimates are quite robust to the choice of the shape parameter. Changes in log income calculated using shape parameters two standard errors higher (column 8-3) or lower (column 8-4) yield elasticities of 0.56 and 0.62, respectively. Although these elasticities are not as large as those in the NTR literature from the 1980s, they are significantly greater than zero.

48. Because of the extremely small number of tax returns involved, I combine all taxpayers with incomes greater than $300,000 into a single category.

Table 8. Regression Estimates of the Elasticity of Taxable Income for the 1924–25 Tax Change[a]

	Baseline[b]	*Higher income*[c]	*Higher* θ[b, d]	*Lower* θ[b, d]
	8-1	*8-2*	*8-3*	*8-4*
Constant term	0.292	0.320	0.291	0.294
	(0.046)	(0.086)	(0.037)	(0.055)
Change in log of net-of-tax share	0.591	0.539	0.562	0.622
	(0.120)	(0.192)	(0.099)	(0.144)
No. of income categories	7	5	7	7
R^2	0.83	0.72	0.87	0.79

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Sample includes all groups with at least $5,000 in income in 1922.

c. Sample includes all groups with at least $25,000 in income in 1922.

d. Estimates use values for the shape parameter θ two standard deviations above (column 8-3) or below (column 8-4) the value used in column 8-1 (1.608).

The Tax Increase of 1932

In 1929 the U.S. economy started to collapse as the Great Depression began. Soon the government began to run budget deficits as incomes around the country fell rapidly. Congressional leaders and influential journalists such as Walter Lippmann regularly denounced the unbalanced budgets.[49] Some argue that President Herbert C. Hoover privately believed that getting out of the depression would require some deficit spending. But when the Federal Reserve produced a monetary contraction in 1931, Hoover feared that deficit spending would increase the competition for credit between the government and private borrowers, and that the deficits were also the source of market fears about the U.S. dollar.[50] In 1932 Hoover raised taxes to try to reduce the deficit. The rate increases were quite progressive. Table 9 summarizes tax rates for selected income levels in 1931 and 1935. The result, again, is a natural experiment, if one is willing to argue that different groups are valid controls for one another.

I look at the change from 1931 to 1935. Over this period, nominal GDP fell by about 5 percent, and the number of high-income tax returns also fell. I purposely avoid using 1929 or 1930 as the base year because the output drops were much more dramatic in those years. As a result, the period

49. Burner (1979).

50. Brownlee (1996).

Table 9. Marginal Tax Rates for Selected Income Levels, 1931 and 1935

Income (thousands of current dollars)	*Marginal rate[a] (percent)*	
	1931	*1935*
5	2	8
10	6	11
25	12	21
50	18	34
100	25	56
500	25	61
1,000	25	63

Source: Internal Revenue Service, *Statistics of Income* (1940).
a. Sum of the normal tax rate and the surtax rate.

also includes some of the tax increases of the Roosevelt administration (as described in the next experiment).

Since nominal GDP growth was negative and the number of taxable returns fell, the Pareto/histogram method is applied here a bit differently than for the other periods. The normal method needs to be, in some sense, run in reverse. Rather than calculating what share of people moved from a lower income group into a higher one, now I must calculate what share of the higher group fell into the lower group in order to derive the new mean income. The interpolation procedure is the same, however.

For the difference-in-differences computational approach I choose groups with incomes of $25,000 to $50,000, $50,000 to $100,000, and over $100,000. The tax rate change is large only for the highest-income group C. For the other two groups the tax change was very similar, so their relative elasticity is quite unstable (the denominator is close to zero) and is therefore not reported. The results, reported in table 10, show that incomes did fall most for the highest-income taxpayers, those with the greatest fall in net-of-tax share. The magnitude of the decline, however, is relatively modest. The implied difference-in-differences elasticities of taxable income are between about a quarter and a third.

The regression results using all the data are reported in table 11. They also show modest responses. The elasticities for taxpayers with more than $5,000 of income (column 11-1) and for those with at least $25,000 (column 11-2) are 0.23 and 0.27, respectively. Even allowing for the two-standard-error changes in the shape parameter when calculating the change in log income (columns 11-3 and 11-4), the elasticities are between

Table 10. Computed Relative Elasticities of Taxable Income for the 1932 Tax Change

Income group	*Income range, 1931*[a]	*No. of returns, 1931*	*Mean income, 1931*[a]	*Pareto-estimated mean income, 1935*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	25–50	24,308	33.8	33.8	0.00	–0.01
B	50–100	7,830	67.4	64.4	–0.05	–0.02
C	100+	3,184	244.7	204.3	–0.18	–0.59

Comparison	*Elasticity*[c]
C versus B	0.24
C versus A	0.31
B versus A	—[d]

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. In thousands of current dollars.

b. Nominal GDP in 1935 was 0.952 times that in the base year 1931. The shape parameter θ used in the Pareto calculation was 1.816 (see table A1).

c. Difference-in-differences elasticity, calculated as in text equation (9).

d. Not reported because the changes in log net-of-tax share are almost identical for the two groups, so that the denominator is close to zero.

0.20 and 0.26. All of these are quite modest compared with results from the 1980s. Further, they may be biased upward by the narrowing inequality of the 1930s, since the relative tax increases were largest for the rich.[51] On the other hand, it is possible that high-income people are more responsive to demand conditions and that this contributes to a lower elasticity.

The Tax Increase of 1935

In 1933 President Franklin D. Roosevelt took office, and the Democrats took control of Congress. Over the 1934–38 period, national output did rise at somewhat normal rates, but from a much reduced base. Roosevelt needed money to fund his many new government programs. There was also considerable public pressure for redistribution from the wealthy. Roosevelt proposed a sharp increase in progressive taxation along with a steep inheritance tax, gift taxes, and an increase in corporate income taxes. Opponents reacted vociferously. William Randolph Hearst instructed the editors of the newspapers he owned to, from that point forward, refer to the New Deal as the Raw Deal, and to characterize the tax plan as Roosevelt's

51. The evidence in Goldin and Katz (1999) and the earlier data of Lebergott (1947) and Kuznets (1953) clearly show a narrowing in the period.

Table 11. Regression Estimates of the Elasticity of Taxable Income for the 1932 Tax Change[a]

	Baseline[b]	*Higher income*[c]	*Higher* θ[b, d]	*Lower* θ[b, d]
	11-1	*11-2*	*11-3*	*11-4*
Constant term	0.003	0.026	0.006	–0.001
	(0.049)	(0.071)	(0.043)	(0.056)
Change in log of net-of-tax share	0.233	0.272	0.255	0.208
	(0.099)	(0.135)	(0.086)	(0.113)
No. of income categories	8	7	8	8
R^2	0.48	0.45	0.59	0.36

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Sample includes all groups with more than $5,000 in income in 1931.

c. Sample includes all groups with at least $25,000 in income in 1931.

d. Estimates use values for the shape parameter θ two standard deviations above (column 11-3) or below (column 11-4) the value used in column 11-1 (1.816).

attempt to "soak the successful."[52] The Revenue Act of 1935 passed, however, by a comfortable majority, and the top rates rose substantially, as illustrated in table 12. The top marginal tax rate rose from 63 percent to 79 percent.

Part of the Roosevelt tax program was also greater aggressiveness in tax enforcement—the Treasury even prosecuted former Treasury Secretary Mellon for tax evasion. (He was found innocent but was forced to pay $400,000 for "mistakes" he had made in his favor.)[53] Since this stepup in enforcement occurred simultaneously with the rate increase, the elasticity results may be biased downward.

I choose for analysis the two years before and after the rate increase of 1936. In the natural experiment computation, I again aggregate the sample into three high-income groups. Unlike in the 1931–35 period, the numbers of returns increased over this period, so the method described in the appendix works well. As table 13 indicates, the decrease in log net-of-tax share was virtually identical for the $50,000-to-$100,000 group (group B) and for the over-$100,000 group (group C), and so I do not calculate the difference-in-differences elasticity between these two groups. That said, it is apparent from looking at the income numbers that the results of this experiment are not consistent with a positive elasticity: the tax increase

52. Leuchtenburg (1963).

53. Brownlee (1996).

Table 12. Marginal Tax Rates for Selected Income Levels, 1934 and 1938

Income (thousands of current dollars)	*Marginal rate[a] (percent)*	
	1934	*1938*
5	8	8
10	11	11
25	21	21
50	34	35
100	56	62
500	61	74
1,000	63	77
5,000	63	79

Source: Internal Revenue Service, *Statistics of Income* (1940).
a. Sum of the normal tax rate and the surtax rate.

was biggest for the rich, yet their reported incomes grew the fastest. According to the natural experiment methodology, this yields a *negative* elasticity of taxable income.

The regression estimates, reported in table 14, confirm this negative elasticity with the full histogram data for people with at least $5,000 (column 14-1) and at least $25,000 (column 14-2). Either way, the elasticities are fairly negative at –0.55 and –0.83, respectively. Varying the shape parameter when calculating the change in log income, as reported in columns 14-3 and 14-4, does not change the estimated elasticity much at all: the range is from –0.5 to –0.6. Recall, too, the evidence of Goldin and Katz that inequality was falling in this period. If anything, this should have been contributing an *upward* bias to the estimates here, because relative income changes were moving in the same direction as the relative tax changes. Perhaps the increase in enforcement can explain the perverse results. It was also the case that the corporate tax rate rose from 13.75 percent to 19 percent, so there may have been income shifting out of corporate form.[54]

The Tax Increase of 1950–51

During World War II the income tax became much more broadly applied, and by 1945 a large majority of Americans were income taxpayers. Marginal tax rates had risen dramatically to help fund the war. After the war, the Republicans controlled Congress while Democrat Harry S Truman occupied the White House. They fought acrimoniously over many

54. Goolsbee (1998) examines the relative incentives in this period.

Table 13. Computed Relative Elasticities of Taxable Income for the 1935 Tax Change

Income group	*Income range, 1934*[a]	*No. of returns, 1934*	*Mean income, 1934*[a]	*Pareto-estimated mean income, 1938*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	25–50	20,931	33.4	37.7	0.108	–0.083
B	50–100	6,093	66.6	75.8	0.129	–0.271
C	100+	1,907	220.1	267.4	0.194	–0.272

Comparison	*Elasticity*[c]
C versus B	—[d]
C versus A	–0.46
B versus A	–0.11

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. In thousands of current dollars.

b. Nominal GDP in 1938 was 1.302 times that in the base year 1934. The shape parameter θ used in the Pareto calculation was 1.765 (see table A1).

c. Difference-in-differences elasticity, calculated as text equation (9).

d. Not reported because the changes in log net-of-tax share are almost identical for the two groups, so that the denominator is close to zero.

things, including tax policy: Truman wanted to retire war debt; the Republicans wanted to cut taxes. In 1947 Truman vetoed the Republicans' tax reduction bill. According to some, this made him the first president in modern history to openly oppose a tax relief measure.[55] Eventually, the Revenue Act of 1948 did reduce marginal rates, but the top rate remained over 82 percent in 1948, as outlined in table 15. In 1950 and again in 1951, to help pay for the Korean War, marginal rates rose and in a progressive way. Although a rise in the top rate from 82 percent to 92 percent may not seem drastic, the impact on net-of-tax share is immense. Taxpayers earning over $400,000 a year saw their net-of-tax share cut by more than half, from about 0.18 to 0.08. This should have caused a noticeable decline in the relative taxable incomes of high-income people if the claims of the NTR literature are correct. The estimated elasticity may, in addition, be biased upward by the narrowing trend in income inequality during this period.[56] At the same time, however, there was a sizable increase in the

55. McNaughton and Hehmeyer (1948).

56. Goldin and Margo (1992) present evidence that income inequality fell around this period—part of what they term "the great compression." Their data do not include the incomes of the very rich, however. If the pattern extended throughout the distribution, marginal rates rose most on people whose relative incomes were already declining.

Table 14. Regression Estimates of the Elasticity of Taxable Income for the 1935 Tax Change[a]

	Baseline[b]	*Higher income*[c]	*Higher* θ[b, d]	*Lower* θ[b, d]
	14-1	*14-2*	*14-3*	*11-4*
Constant term	0.079	–0.208	0.081	0.076
	(0.067)	(0.106)	(0.059)	(0.076)
Change in log of net-of-tax share	–0.550	–0.825	–0.499	–0.607
	(0.217)	(0.302)	(0.191)	(0.247)
No. of income categories	9	7	9	9
R^2	0.48	0.60	0.49	0.46

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Sample includes all groups with at least $5,000 in income in 1934.

c. Sample includes all groups with at least $25,000 in income in 1934.

d. Estimates use values for the shape parameter θ two standard deviations above (column 14-3) or below (column 14-4) the value used in column 14-1 (1.765).

corporate tax rate (from 38 percent to 52 percent), which might have led people to shift income out of the corporate sector.

For the difference-in-differences computations, I choose two years before (1948) and one full year after (1952) the two-year tax increase of 1950–51 as the points of reference. For the three income groups I use $50,000 to $100,000, $100,000 to $500,000, and over $500,000. The results are presented in table 16. The net-of-tax share fell most for those with the highest incomes (group C). Consistent with the NTR theory, this group also had the smallest increase in income. The magnitudes, however, are again quite modest: the implied elasticities of taxable income range from 0.03 to 0.44. The full regression results using the complete his-

Table 15. Marginal Tax Rates for Selected Income Levels, 1948 and 1952

Income	*Marginal rate (percent)*	
(thousands of current dollars)	*1948*	*1952*
10	22.9	29
25	37.8	48
50	51.9	66
100	66.0	77
250	78.3	90
500	82.1	92

Source: Pechman (1983).

Table 16. Computed Relative Elasticities of Taxable Income for the 1950–51 Tax Change

Income group	*Income range, 1948*[a]	*No. of returns, 1948*	*Mean income, 1948*[a]	*Pareto-estimated mean income, 1952*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	50–100	52,725	66.7	71.4	0.069	–0.351
B	100–500	15,716	160.6	170.1	0.057	–0.737
C	500+	564	944.6	951.7	0.007	–0.850

Comparison	*Elasticity*[c]
C versus B	0.44
C versus A	0.12
B versus A	0.03

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. In thousands of current dollars.

b. Nominal GDP in 1952 was 1.330 times that in the base year 1948. The shape parameter θ used in the Pareto calculation was 2.107 (see table A1).

c. Difference-in-differences elasticity, calculated as in text equation (9).

togram data for incomes of at least $15,000 (table 17, column 17-1) and at least $30,000 (column 17-2) confirm these magnitudes. The estimated elasticities are less than 0.16 in both cases and not significantly different from zero in the second column. Checking the robustness to varying the shape parameter (columns 17-3 and 17-4) gives estimates very similar to column 17-1 of around 0.15. Here again the effect of tax policy seems rather modest.

The Tax Cut of 1964

In the early 1960s, President John F. Kennedy's economic advisers wanted a Keynesian stimulus for the economy. A mild recovery had followed the 1960–61 recession, but growth had seemed to slow. Chairman of the Council of Economic Advisers Walter W. Heller supported lowering the high marginal tax rates, but Secretary of the Treasury C. Douglas Dillon favored a balanced budget.[57] Kennedy himself was not sure about the timing of the cut, so the elimination of the high marginal rates was postponed. It was finally enacted in the Revenue Act of 1964. Given the prevailing rate structure, this flattening of tax rates caused a rather sub-

57. Reeves (1993).

Table 17. Regression Estimates of the Elasticity of Taxable Income for the 1950–51 Tax Change[a]

	Baseline[b]	*Higher income*[c]	*Higher* θ[b, d]	*Lower* θ[b, d]
	17-1	*17-2*	*17-3*	*17-4*
Constant term	0.162	0.122	0.164	0.160
	(0.028)	(0.054)	(0.025)	(0.033)
Change in log of net-of-tax share	0.157	0.102	0.168	0.146
	(0.048)	(0.081)	(0.042)	(0.055)
No. of income categories	8	6	8	8
R^2	0.64	0.28	0.73	0.59

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Sample includes all groups with at least $15,000 in income in 1948.

c. Sample includes all groups with at least $30,000 in income in 1948.

d. Estimates use values for the shape parameter θ two standard deviations above (column 17-3) or below (column 17-4) the value used in column 17-1 (2.107).

stantial change in the progressivity of the tax code and served as another major natural experiment, as described in table 18. The net-of-tax share for the highest-income taxpayers rose from 0.09 in 1963 to 0.23 in 1964 and 0.30 in 1966 as a result of the 1964 act. In logarithmic terms, this was a very dramatic tax cut.

For the difference-in-differences computation (table 19) I again use the categories $50,000 to $100,000, $100,000 to $500,000, and over $500,000. Note that despite the major cut in the tax rate at the top of the income distribution, the increase in reported taxable income for group C is not noticeably larger than for other groups. The second-highest income group (group B) records a larger increase, despite a smaller increase in net-of-tax share. In all of the combinations, the elasticities are very close to

Table 18. Marginal Tax Rates for Selected Income Levels, 1962 and 1966

Income	*Marginal rate (percent)*	
(thousands of current dollars)	*1962*	*1966*
10	26	22
25	43	36
50	59	50
100	75	62
250	89	70
500	91	70

Source: Pechman (1983).

Table 19. Computed Relative Elasticities of Taxable Income for the 1964 Tax Change

Income group	*Income range, 1962*[a]	*No. of returns, 1962*	*Mean income, 1962*[a]	*Pareto-estimated mean income, 1966*[a, b]	*Change in log of income*	*Change in log of net-of-tax share*
A	50–100	121,250	65.7	88.8	0.301	0.169
B	100–500	25,841	161.8	228.3	0.344	0.693
C	500+	1,146	1,051.7	1,322.6	0.229	1.204

Comparison	*Elasticity*[c]
C versus B	–0.22
C versus A	–0.07
B versus A	0.08

Source: Author's calculations using data from Internal Revenue Service, *Statistics of Income,* various issues.
a. In thousands of current dollars.
b. Nominal GDP in 1966 was 1.346 times that in the base year 1962. The shape parameter θ used in the Pareto calculation was 2.063 (see table A1).
c. Difference-in-differences elasticity, calculated as in text equation (9).

zero, and two are actually negative. The regression results using the full data (table 20) confirm the idea that the elasticity is small when looking at all groups of at least $20,000 in income (column 20-1) and at least $50,000 (column 20-2). (The IRS changed the number of reported brackets during this period, hence the smaller number of observations.) The elasticity is almost exactly zero in both cases, and the standard errors are not large. Allowing for variation in the shape parameter, as reported in columns 20-3 and 20-4, again yields elasticities very close to zero. One factor that may contribute to a downward bias in this case is the fact that income inequality was falling in this period.[58]

Summary and Discussion

Taken as a whole, these five natural experiments using cross-sectional data suggest that the elasticities of taxable income in response to most of the major tax changes in U.S. history were not nearly as large as those estimated for the 1980s. In the regression analyses, the largest elasticity was less than 0.6, and the average was much smaller. The sizable reported behavioral responses to the tax changes of the 1980s, where an elasticity of 0.7 is something of a lower bound, are quite atypical in historical con-

58. See Goldin and Margo (1992); Katz and Murphy (1992).

Table 20. Regression Estimates of the Elasticity of Taxable Income for the 1964 Tax Change[a]

	Baseline[b]	*Higher income*[c]	*Higher* θ[b, d]	*Lower* θ[b, d]
	20-1	*20-2*	*20-3*	*20-4*
Constant term	0.293	0.315	0.285	0.301
	(0.020)	(0.025)	(0.030)	(0.013)
Change in log of net-of-tax share	0.022	0.001	0.006	0.041
	(0.024)	(0.027)	(0.036)	(0.016)
No. of income categories	6	5	6	6
R^2	0.17	0.01	0.01	0.62

Source: Author's regressions using data from Internal Revenue Service, *Statistics of Income,* various issues.

a. The dependent variable in each regression is the change in log income for each income group as calculated using the Pareto method described in the text. Standard errors are in parentheses.

b. Sample includes all groups with at least $20,000 in income in 1962.

c. Sample includes all groups with at least $50,000 in income in 1962.

d. Estimates use values for the shape parameter θ two standard deviations above (column 20-3) or below (column 20-4) the value used in column 20-1 (2.063).

text. This may reflect a "true" elasticity in the 1980s that was much higher than in previous periods because of institutional or other factors, but perhaps more plausibly, it might reflect the importance of various biases in the 1980s estimates.

It is important to note that the results presented here are not meant to imply that the elasticity of taxable income is zero. Most of the results so far indicate a positive elasticity, suggesting that revenue estimates should include at least some element of dynamics. And the higher the marginal rate, the more important that dynamic element is. The behavioral responses, however, at least in these historical periods, are substantially smaller than claimed in the recent literature for the 1980s.

As discussed at the outset, the elasticities of taxable income estimated here cannot be used to calculate whether a given tax change raised or lowered revenue. To obtain some idea about magnitudes, however, note that if there were only one rate in the tax code, the revenue-maximizing tax rate given the elasticity estimated using the 1985–89 data (column 4-1 of table 4) would be 42 percent. It would be 63 percent using the 1922–26 data, 83 percent using the 1931–35 data, 86 percent using the 1948–52 data, and 98 percent using the 1962–66 data. (Technically, the revenue-maximizing rate would be at the maximum of 100 percent using 1934–38 data, since the elasticity was negative.) These rates are high, well in excess of average rates on high-income people in these time periods, but marginal rates on the very highest income brackets sometimes did reach these levels. Note

also that the revenue-maximizing rate is not in any sense the "optimal" tax rate. The fact that efficiency costs rise with the square of the tax rate is likely to place the optimal rate well below the revenue-maximizing rate, even in a very simple tax code.

Panel Data on High-Income Corporate Executives

To address some of the concerns raised by the fact that the previous results were based on aggregate, cross-sectional data, I turn here to panel data on the incomes of corporate executives from two distinct time periods. Unfortunately, the panels of tax return data that have been available for study in the 1980s and 1990s simply do not exist for earlier time periods. Following on previous work, however, some panel data sources do exist on high-income chief executive officers (CEOs) of major corporations. Regulations of the Securities and Exchange Commission (SEC) require all companies whose stocks are publicly traded to report the compensation of their five highest-paid employees each year in their proxy statements.[59] Although the potential universe of such declarations covers many decades, it is difficult to locate such statements well after the fact.

For two time periods, however, data are readily available. First, the well-known *Forbes* magazine survey of executive compensation began in 1970, and it so happens that in 1971 and 1972 the top marginal rate on earned (noncapital) income fell from 70 percent to 60 percent and then to 50 percent. Second, soon after the introduction of the SEC regulations in 1934, the federal government (as part of a Works Progress Administration project) began collecting and publishing, in the *Survey of Listed American Corporations,* executive compensation information along with balance-sheet data for the major corporations of the United States. These are the data used by Charles Hadlock and Gerald Lumer.[60] The data cover the same period (1934–38) as one of the cross-sectional experiments described above.

There are advantages and disadvantages to using compensation data to estimate behavioral responses to taxation. Some advantages are that the data provide information on large numbers of high-income people, that the

59. See Goolsbee (forthcoming-a).
60. Hadlock and Lumer (1997).

income definitions are consistent over time, that the data are in panel form, and that it is possible to control for firm-level factors in the compensation regressions. A serious disadvantage is that no information is available about deductions or about other forms of income. The results from more recent data suggest that this problem is relatively unimportant in those data, but there is no way to know this for the earlier time periods.[61] These data suffer from the additional problem that they report only direct compensation and not stock or stock options. Given the rise of stock options in recent decades, this is a more important issue now than in the 1930s.[62]

To arrive at a marginal tax rate for the first year for each executive, I take the executive's income in the period before the tax change and use that as taxable income. To get the marginal tax rate for other years in a way that is not endogenous, I use one of two measures. The standard approach takes the executive's salary in the first period, inflates it at the rate of nominal GDP growth, and then uses the tax rate for that income level. I also calculate an alternative tax rate by predicting the executive's pay using solely the firm's or the individual's subsequent performance and characteristics.

The Tax Cut of 1971–72

The tax cut of the early 1970s was not a typical rate cut. It lowered the marginal rate only on earned income. This was quite relevant for corporate executives, however, since so much of their income comes in the form of salary. As table 21 shows, from 1970 to 1973 the net-of-tax share at the top of the income distribution rose substantially for wage and salary income. For anyone below about $90,000 in income in 1971 or $50,000 in 1972, there was no change at all, because their marginal rates were below the new caps. This ought to generate the standard NTR natural experiment by creating larger incentives to shift nontaxable income into salary form for the higher-income executives.

Table 22 presents summary statistics for the compensation of CEOs in this sample. With a mean income of more than $150,000 in 1970 ($558,000 in 1998 dollars), these executives were obviously quite well off. Their incomes ranged from $32,000 to $757,000, so there was substantial variation in their earned income.

61. Goolsbee (forthcoming-a).

62. Discussions of the relative magnitudes of option compensation and salary compensation for various periods can be found in Hall and Liebman (1998) and Lewellen (1968).

Table 21. Marginal Tax Rates on Earned Income for Selected Income Levels, 1970, 1971, and 1972–74

Income (thousands of current dollars)	*Marginal rate (percent)*		
	1970	*1971*	*1972–74*
25	36.9	36	36
50	51.3	50	50
75	56.4	55	50
100	63.6	60	50
250	71.8	60	50
500	71.8	60	50

Source: Pechman (1983).

To examine the NTR hypothesis, I estimate a regression for salary and bonus income using four years of data from the *Forbes* surveys. Quantitative information on the firms is relatively limited in these early years of the survey: the magazine reports only the number of employees and the age and tenure of the CEO. I estimate a regression for the income of executive j in year t of the form:

$$\ln(Y_{jt}) = \alpha_j + \beta \ln(1 - \tau_{jt}) + \Gamma' Z_{jt} + \delta_t + \varepsilon_{jt}, \tag{11}$$

where Y is income, α is an individual fixed effect, and τ is the individual's marginal tax rate in year t; Z is a vector of firm-specific variables including age and the square of age, tenure as CEO and its square, and the log of the number of employees in year t; δ is a year effect; and ε is an error term. Including year dummy variables in the specification is the regression equivalent of the natural experiment results, because the results are identified from the cross-sectional variation in the change in tax rates

Table 22. Distribution of Corporate Chief Executive Officer Compensation, 1970

Percentile	*Salary and bonuses (dollars)*
10	77,000
25	100,000
50	140,000
75	190,000
90	250,000
Mean	154,427
Standard deviation	77,789
N	2,338

Source: *Forbes* CEO Compensation Survey (1971).

across different executives within a given year. In specifications that do not include year dummies, I include the real growth rate of GDP.

The regressions are reported in table 23. Columns 23-1 and 23-2 use the tax rates calculated by inflating income at the nominal GDP growth rate. Columns 23-3 and 23-4 use tax rates calculated using income predicted from the number of employees, the CEO's age and its square, and his tenure and its square in later years. In all four cases there appear to be, at most, rather modest effects of taxation on the taxable component of CEO salaries. In the pure natural experiments (the regressions including year dummies) the elasticities are actually negative, suggesting that the highest-paid executives had the biggest tax cuts but the smallest salary increases. Without the year dummies, the elasticities are less than 0.25. It is possible that the wage-price controls of the Nixon administration during part of this period provided at least some constraint on salary growth for the highest-income executives relative to other executives. Overall, however, executives' nominal incomes rose by almost 10 percent per year during this period, so these pressures may not have been too great.

The Tax Increase of 1935

The 1930s, as noted above, saw large increases in marginal tax rates for high-income taxpayers. Using the panel data of Hadlock and Lumer, I examine the role that these taxes played in the compensation of corporate executives over this period. The sample is drawn from the universe of firms with returns data at the end of May 1933 on the Center for Research and Security Prices (CRSP) New York Stock Exchange monthly tape that were also listed in *Moody's Industrial Manual* for 1933 and had a book value of assets greater than $20 million. Firm survival rates in the sample were extremely high (only six firms exited), and the turnover of management was low by modern standards.

Compensation data for these companies come from the *Survey of American Listed Corporations* for the years 1934–38, spanning the same tax change described in one of the natural experiments above. The survey was a Works Progress Administration program supervised by the SEC to report compensation and balance-sheet data for publicly traded firms. The data report the direct compensation of the highest-paid employee in the firm. Hadlock and Lumer track the timing of top management changes in the

Table 23. Regression Estimates Using Panel Data of the Elasticity of Taxable Income of Corporate Chief Executive Officers to the 1971–72 Tax Change[a]

Independent variable	*23-1*	*23-2*	*23-3*	*23-4*
Log of net-of-tax share	0.083 (0.033)	–0.361 (0.057)	0.219 (0.034)	–0.185 (0.107)
Log of no. of employees	0.051 (0.012)	0.030 (0.012)	0.044 (0.012)	0.033 (0.012)
GDP growth rate	–0.006 (0.179)		–0.494 (0.182)	
Tenure as CEO	0.012 (0.003)	0.004 (0.003)	0.008 (0.003)	0.003 (0.003)
Tenure squared	–0.0004 (0.0001)	–0.0002 (0.0001)	–0.0003 (0.0001)	–0.0002 (0.0001)
Age of CEO	0.010 (0.003)	–0.001 (0.003)	0.003 (0.003)	–0.002 (0.003)
Age squared	–0.00002 (0.00001)	–0.00000 (0.00001)	–0.00001 (0.00001)	–0.00000 (0.00001)
Year dummies	No	Yes	No	Yes
R^2	0.90	0.90	0.90	0.90
Method of calculating tax rate	Predicted by GDP	Predicted by GDP	Predicted by firm characteristics	Predicted by firm characteristics

Source: Author's regressions using data from *Forbes*, various issues.
a. The dependent variable is the log of real income for the executive in a given year. Standard errors are in parentheses. The number of observations is 2,869 in all regressions.

sample, making it possible to create an (unbalanced) panel of high-income executives. There were 298 such executives in the sample. Table 24 illustrates that their incomes in 1934 were very high: average salary and bonus exceeded $70,000 (about $840,000 in 1998 dollars).

Table 25 gives the marginal tax rates for people in the relevant income ranges. The tax increase was very progressive. If there is a large behavioral response to taxation among high-income people, the reported incomes of the highest-paid group of executives should have risen at a slower rate than those of the lower-paid executives as they shifted more income out of nontaxable forms.

To estimate the effect of taxes on reported compensation, I regress the same specification as for the *Forbes* CEO data above but with different firm-level controls. The regressions use the log of the firm's market value,

Table 24. Distribution of Corporate Chief Executive Officer Compensation, 1934

Percentile	*Salary and bonuses (current dollars)*
10	32,000
25	40,000
50	60,000
75	85,000
90	126,000
Mean	73,007
Standard deviation	51,789
N	888

Source: Author's calculations from data in Securities and Exchange Commission (1944).

the annual return for the firm, and, in specifications without year dummies, the annual return for the market and the real growth rate of GDP. Again I calculate the tax rate using the nominal GDP growth rate on their income at the start of the sample and by using income predicted from firm performance.

The results are presented in table 26. The panel evidence does not show a sizable elasticity of taxable income. Three of the four specifications yield negative elasticities, and the one elasticity that is not negative (column 26-1) is only 0.28. These data suggest that although taxes rose most for the very rich, this sample provides little evidence that their relative incomes declined. Indeed, they may well have risen.

Table 25. Marginal Tax Rates for Selected Income Levels, 1934–38

Income (thousands of current dollars)	*Marginal rate[a] (percent)*	
	1934–35	*1936–38*
25	21	21
50	34	35
75	46	51
100	56	62
250	58	68
500	61	74

Source: Internal Revenue Service, *Statistics of Income* (1940).
a. Sum of the normal tax rate and the surtax rate.

Table 26. Regression Estimates Using Panel Data of the Elasticity of Taxable Income of Corporate Chief Executive Officers to the 1935 Tax Change[a]

Independent variable	*26-1*	*26-2*	*26-3*	*26-4*
Log of net-of-tax share	0.278	–0.347	–0.088	–0.587
	(0.177)	(0.900)	(0.190)	(0.140)
Log of firm's market value	0.113	0.142	0.115	0.100
	(0.031)	(0.028)	(0.042)	(0.041)
Firm's annual return[b]	–0.009	–0.105	–0.001	0.004
	(0.022)	(0.052)	(0.027)	(0.027)
Market annual return[c]		–0.019		–0.041
		(0.022)		(0.057)
GDP growth rate		–0.154		–0.236
		(0.174)		(0.188)
Year dummies	Yes	No	Yes	No
R^2	0.934	0.931	0.927	0.924
Method of calculating tax rate	Predicted by GDP	Predicted by GDP	Predicted by firm characteristics	Predicted by firm characteristics

Source: Author's regressions using data from Securities and Exchange Commission (1944).
a. The dependent variable is the log of real income for the executive in a given year. Standard errors are in parentheses. The number of observations is 755 in all regressions.
b. Change in the firm's log market value for the year.
c. Return for the year on the entire market, from Hadlock and Lumer (1997).

Conclusions

This paper has used evidence from seven analyses of six different tax changes since 1922 to examine the evidence in support of the high-income Laffer curve and the New Tax Responsiveness literature. Although that work emphasizes the potential importance of behavioral responses to marginal tax rates, the results in this paper suggest that the evidence on which those conclusions are based—evidence from the 1980s—is atypical in the historical experience. Using the same methods that NTR authors have used for the 1980s, the elasticities of taxable income calculated for other tax changes seem to be much more modest, with several indistinguishable from zero. This is true in the aggregate cross-sectional tax return data as well as in panel data on executive compensation. The largest regression estimates of the taxable income elasticity from all of the previous historical periods are lower than the smallest estimates in the literature based on the 1980s. Given the importance of the behavioral response to

taxation, one hopes that these findings will stimulate further research on the topic using data outside of conventional tax returns in the 1980s and 1990s.

The notion that governments could raise more money by cutting rates is, indeed, a glorious idea. It would permit a Pareto improvement of the most enjoyable kind. Unfortunately for all of us, the data from the historical record suggest that it is unlikely to be true at anything like today's marginal tax rates. It seems that, for now at least, we will just have to keep paying for our tax cuts the old-fashioned way.

Appendix: Income Statistics and the Pareto Distribution

Statistical discussions of the Pareto distribution can be found in the volume by Norman Johnson and Samuel Kotz.[63] The distribution function of the Pareto is:

$$P(x \leq X) = 1 - \left(\frac{k}{X}\right)^{\theta},$$

so the density is:

$$f(x) = \theta\, k^{\theta} x^{-\theta-1}.$$

Estimating the Pareto Distribution

Three steps are needed to calculate the income statistics used in the results. The first is to estimate the Pareto parameters using the histogram data. To do this, I follow the method described by Johnson and Kotz. For any income cutoff, L, call the number of observations in the data with income greater than the cutoff N_L. Using the distribution function, we know that this number is:

$$N_L = \left(\frac{k}{L}\right)^{\theta} N,$$

where N is the total number of observations in the full sample.

The histogram gives a set of data on the number of observations greater than each income level listed in the histogram. The parameter θ can be

63. Johnson and Kotz (1970).

estimated directly by using these observations within a given year to estimate the regression:

$$\ln(N_L) = a - \theta \ln(L) + \varepsilon.$$

Since the constant term is actually a function of N, k, and θ, it would be easy to solve for the implied value of k, but the elasticity calculations will use only the value of θ, so I do not bother.

The estimates for the years used in this paper are listed in table A1. The fit of the data is extremely good (the typical R^2 exceeds 0.99), and the predicted mean incomes match the observed means quite well. This is perhaps not surprising since the Pareto distribution has long been known to fit the top of the income distribution.

Calculating the New Income Brackets

The second step, given the value for the shape parameter θ, is to find the new cutoff levels for the original income groups. There are two types of groups to consider. The first is the highest income group. This group starts with incomes on an interval $[L^*, \infty]$, where L^* is, say, \$100,000 in the base year. Let the size of this group be N_t in the base year and N_{t+1} in the later year (where $N_{t+1} > N_t$). To calculate the lower bound, L, on the incomes of the richest N_t people (among the N_{t+1} now in the bracket), I take the share of the later-year bracket that consists of the original group, N_t/N_{t+1}, and then solve for L to match this probability according to the ratio that comes from the original Pareto distribution function:

$$\frac{N_t}{N_{t+1}} = \frac{(k/L)^\theta}{(k/L^*)^\theta}.$$

The second type of group has income in some closed interval $[L^*, H^*]$, say, between \$100,000 and \$200,000. In the later year, some people will have been added from below, and some will have moved up into the higher bracket. Call the number of people in the interval $[L^*, H^*]$ before the tax change M_t and the number of people after the change M_{t+1}. We know from the first calculation that $N_{t+1} - N_t$ people from this income group must have moved into the group above. The upper bound, H, on the interval for this group is, therefore, the lower bound previously calculated for the higher group. The more difficult question is what is the lower-bound cutoff income, L, such that there are exactly M_t people in the interval $[L, H]$. To

Table A1. Estimates of the Pareto Distribution[a]

	1926	*1935*	*1938*	*1952*	*1966*	*1989*
θ	–1.608	–1.816	–1.765	–2.107	–2.063	–1.887
	(0.017)	(0.019)	(0.032)	(0.022)	(0.050)	(0.056)
Constant	16.509	16.271	16.077	19.556	20.522	16.897
	(0.084)	(0.092)	(0.156)	(0.196)	(0.257)	(0.278)
N	8	8	8	7	6	8
R^2	0.999	0.999	0.998	0.999	0.998	0.995

Source: Author's calculations based on data from Internal Revenue Service, *Statistics of Income,* various issues.

a. Each column estimates the shape parameter θ for a Pareto distribution using histogram data from the indicated year, as described in the text. The dependent variable is the log number of tax returns greater than some amount, and the right-hand side variable is the log of that amount. Standard errors are in parentheses.

calculate this number, note that the probability that taxpayer x has income within $[L^*, H^*]$ is:

$$P(H^* \geq x > L^*) = \left(\frac{k}{L^*}\right)^{\theta} - \left(\frac{k}{H^*}\right)^{\theta}.$$

The share that was in that same group originally (that is, who neither rose up from the group below or moved up into the group above) can then be matched to the probabilities of observing these shares, yielding L according to the following formula:

$$\frac{M_t - (N_{t+1} - N_t)}{M_{t+1}} = \frac{\left(\frac{k}{L}\right)^{\theta} - \left(\frac{k}{H^*}\right)^{\theta}}{\left(\frac{k}{L^*}\right)^{\theta} - \left(\frac{k}{H^*}\right)^{\theta}}.$$

Calculating the New Mean Incomes

Given the cutoffs for the new income brackets, the final step is to calculate the mean incomes of the groups in the later year in order to calculate the groups' change in log income. Again there will be two types of groups. We first find the mean income of people in the highest group, those who have incomes greater than L (the lower bound calculated in the previous section). This will be:

$$E[x \mid x > L] = \frac{\int_{L}^{\infty} x\theta\, k^{\theta} x^{-\theta-1} dx}{\left(\frac{k}{L}\right)^{\theta}} = L^{\theta}\left(\frac{\theta}{1-\theta}\right) x^{1-\theta}\Big|_{L}^{\infty}$$

$$= L\left(\frac{\theta}{\theta-1}\right).$$

For incomes in a lower group in a closed interval $[L, H]$, the expected value will be:

$$E[x \mid H \geq x > L] = \frac{\int_{L}^{H} x\theta\, k^{\theta} x^{-\theta-1} dx}{\left(\frac{k}{L}\right)^{\theta} - \left(\frac{k}{H}\right)^{\theta}} = \frac{\left(\frac{\theta}{1-\theta}\right) x^{1-\theta}\Big|_{L}^{H}}{\left(\frac{1}{L}\right)^{\theta} - \left(\frac{1}{H}\right)^{\theta}}$$

$$= \left(\frac{\theta}{\theta-1}\right)\left[\frac{\left(\frac{1}{L}\right)^{\theta-1} - \left(\frac{1}{H}\right)^{\theta-1}}{\left(\frac{1}{L}\right)^{\theta} - \left(\frac{1}{H}\right)^{\theta}}\right]$$

$$= HL\left(\frac{\theta}{\theta-1}\right)\left[\frac{L^{\theta-1} - H^{\theta-1}}{L^{\theta} - H^{\theta}}\right].$$

Comments and Discussion

Robert E. Hall: First, a couple of comments on the title. As Austan Goolsbee points out, Arthur Laffer was hardly the first to recognize that the elasticity of revenue with respect to taxes depends on the elasticity of income with respect to taxes. I don't think his name belongs in the title. Also, I question the accuracy of the claim implicit in the title that the six decades under consideration—the 1920s through the 1980s—were ones of continuous reform. Many pernicious provisions of the tax code were dreamt up during that period. Tax reform has been episodic—it happened in the 1920s, in 1969, in 1981, and in 1986—with tax crimes committed in between and since.

Goolsbee observes that the central question of research in this area can be stated compactly: What is the elasticity of income with respect to the tax residual (defined as one minus the tax rate, or the fraction of income the taxpayer gets to keep)? All of the results of the paper are expressed in this standard form. The justification, presumably, is that people's responses are likely to have roughly constant elasticities with respect to after-tax prices and wages, and the elasticity of those quantities with respect to the tax residual is one. But as Goolsbee notes, the focus on the elasticity with respect to the tax residual means that the issue of taxation—does an increase in a tax rate raise or lower revenue?—cannot be read directly from the results. The crossover point where revenue reaches its maximum with respect to the tax rate, and further tax rate increases are completely perverse, lies where the elasticity of income with respect to the tax rate is minus one. The corresponding point in the framework of the paper is where the elasticity of income with respect to the tax residual is $(1 - \tau)/\tau$.

Thus, for a taxpayer in the 80 percent bracket, a Goolsbee elasticity of 0.25 is enough to put a taxpayer at the maximum of revenue. Since most of the elasticities reported in the paper are above this threshold, the paper supports the suggestion of the literature that high tax rates are perverse, although not as strongly as Lawrence Lindsey and Martin Feldstein originally suggested. Also, the threshold elasticity is much lower during periods such as the 1940s through the 1970s, when top marginal rates were close to 100 percent, than later, when top marginal rates have been below 40 percent. As Goolsbee notes, for most taxpayers most of the time, the tax rate was well below the revenue-maximizing level.

I agree with Goolsbee's point that it is a terrible oversimplification of the U.S. tax system to speak of "the" tax rate and to describe changes in the tax system as no more than changes in a rate. I think Goolsbee would probably agree that the primary explanation of the dramatic variations in elasticities that he and others find is the result of changes in the tax system not taking the form of simple changes in rates.

A related issue is the intertemporal response to tax changes. Goolsbee notes that there was a burst of income recorded in 1992 to beat the 1993 tax increase. He does not comment—but many others have—that the Tax Reform Act of 1986 created a capital gains window, which was exploited by many taxpayers who had complete legal discretion about the timing of the realization of capital gains. Many taxpayers have opportunities to determine the scheduling of their income. Whenever a tax change creates predictable changes in tax rates, taxpayers—especially rich ones in high tax brackets—will time their deductions when rates are high and their income when rates are low. It appears, however, that most tax rate changes have not had this character. The 1986 and 1992 tax changes are outliers in that respect. Most changes have been surprises, took effect in the year they became known, and have not created expectations of future changes. For them we can read something like the long-run effect from the first-year effect.

The episodes Goolsbee studies include some tax increases as well as cuts. The striking feature of the elasticities is their diversity. Differences across episodes are much larger than would be expected from the standard errors of the individual estimates. There is at least a strong hint that the diversity is not accidental and is not an artifact of measurement methods. Different authors seem to agree that taxable income rose by an exceptional amount as a result of the 1986 tax reform, given the magnitude of

the tax rate decrease. There is also agreement that the elasticity for the 1990s was much lower, and well below the point of revenue maximization.

A primary finding of the paper is that the elasticity associated with the 1986 tax reform is much greater than that for any other period. Research based just on 1986—suggesting that tax rates are perversely above the revenue-maximizing level—may be misleading. Rather than supporting the notion that the government could raise revenue by cutting tax rates, the findings justify a much more limited conclusion: Another tax reform with a package of changes resembling those of 1986 could raise taxable income sufficiently to pay for further tax rate reductions.

It was a central theme of the politics of the 1986 reform to broaden the base so as to permit large rate reductions. Much of the broadening was forced on taxpayers by changes in law and should not be interpreted as their behavioral response to lower tax rates. In particular, the reform blocked many popular arbitrages between taxpayers in different tax brackets. The quintessential pre-1986 tax shelter was one in which a top-bracket taxpayer borrowed from a nontaxed pension fund. The income disappeared from the tax system because the deduction for interest paid was not offset by any payment of taxes (immediately) by the fund or its beneficiaries (who pay taxes later at generally lower rates).

Tax shelters arose because the U.S. income tax system was (and still is, to a lesser extent) an uncomfortable combination of two ways to achieve a consumption tax: One is to grant businesses write-offs for investment, and the other is to let individuals accumulate before-tax income and only pay taxes on the accumulation when it is consumed. The overlap between the two approaches creates arbitrage opportunities. Some of us pointed out in 1986 that the right way to reform was to pick just one of these. I advocated moving everything to the business write-off side, so that individuals would pay taxes only on their wages, whereas others advocated moving everything to the individual side, by means of a cash-flow consumption tax.

The tax reform of 1986 retained overlapping saving-investment incentives but attacked the resulting arbitrage opportunities directly. Passive-loss limitations effectively prevent high-bracket taxpayers from combining investment write-offs and interest deductions to offset other income. These limitations brought large amounts of income into taxable income from existing tax shelters and largely killed the creation of new shelters.

Another important feature of the 1986 tax change was the capital gains window I have already mentioned. For 1986 alone, taxpayers could enjoy the new lower tax rate but also the old partial exclusion. Effective gains rates were temporarily at an extraordinarily low level. High-bracket taxpayers acted accordingly.

Again, it would be folly to take these and other special features of the 1986 tax bill and interpret the resulting changes in taxable income as representing the general behavior of taxpayers to rate reductions. Instead, the episode shows that we can combine base broadening and rate reductions.

Goolsbee notes that a rather nice experiment in pure rate reduction occurred starting in 1970. One of the least-noticed tax reforms in U.S. history was the 1969 reduction in the top marginal rate on salary income from 70 percent to 50 percent. Goolsbee finds low elasticities in his study of executives. This episode seems ripe for further study using panel data, because it was not clouded by structural alterations in the tax system.

The bottom line of this commendable paper is that the elasticity of taxable income with respect to the tax residual is not a fundamental structural parameter. The elasticity can range from 0 to 3, depending on what set of tax changes occur. Good tax reforms, like that of 1986, generate high elasticities. Regression in the tax system, as in 1993, generates low elasticities.

Lawrence F. Katz: Austan Goolsbee has produced a stimulating historical analysis of the responsiveness of the behavior of very high income individuals to changes in marginal tax rates. Goolsbee, following the tradition of the New Tax Responsiveness literature, argues that a key summary measure of the costs of behavioral responses by the rich to tax changes is their elasticity of taxable income with respect to the marginal tax rate. This elasticity not only depends on the responsiveness of labor supply and entrepreneurial effort to changes in marginal tax rates, but also includes the responsiveness of income shifting across categories with different tax rates and other types of tax avoidance and evasion. An understanding of this elasticity is potentially important for determining the deadweight losses of high marginal tax rates on the very rich, for estimating the revenue impacts of tax changes, and for thinking clearly about the equity-efficiency trade-offs involved in the design of a well-functioning tax system.

Much work has tended to find a low responsiveness of traditionally measured labor supply to taxes for prime-age males; Nada Eissa, however, finds a fairly high responsiveness for the wives of high-income men.[1] On the other hand, Martin Feldstein has found substantial responses of taxable income of the rich to tax changes in the 1980s, especially to the Tax Reform Act of 1986.[2] The responsiveness at the top appears from this work to be greater than one for one, suggesting that a high-income Laffer (or, more appropriately, Mellon) curve does exist. Research on the responses to tax changes in the 1990s (marginal rates were raised for high-income individuals in 1993) suggests substantial one-time tax shifting (through the cashing in of stock options in 1992) but much smaller permanent responses. The contrast between these two experiences has left much controversy concerning the responsiveness of the rich to changes in marginal tax rates. Goolsbee argues, rather persuasively, that special factors in the 1980s (tax base widening and other tax code changes in 1986 and a strong preexisting upward trend in inequality) may explain the finding of a high elasticity of the taxable income of the rich to marginal rate changes in 1986.

Goolsbee's clever approach in this paper is to look at a larger sample of the historical record and use the same natural experiment (difference-in-differences) methodology of the NTR literature to examine the responses of the rich to a large number of tax reforms from the 1920s to the 1970s. He examines both repeated cross-sectional data on incomes for several income groupings of high-income taxpayers during several episodes of tax reform, and panel data on high-income executives for two such episodes. His findings indicate that the reported incomes of high-income people responded only modestly to marginal rate changes in almost all the episodes from the 1920s to the 1970s (with the possible exception of the 1920s). He concludes from this exercise that the results from the 1980s are an outlier, and that the evidence strongly implies that modest elasticities (far below one) of the taxable incomes of the rich to marginal tax rates should be used today in thinking about tax changes. He rejects the possibility of a high-income Laffer curve today.

Although I have some concerns with specific aspects of the implementation of Goolsbee's empirical methodology, I find his basic conclusion

1. Eissa (1995, 1996).
2. Feldstein (1995 and forthcoming).

of fairly modest elasticities from the 1930s to the early 1970s to be rather persuasive. I am a little skeptical of his attempt to use this historical record to make strong inferences about the magnitude of current behavioral responses of the rich to tax rate changes. Recent decades have seen, in addition to these rate changes, large changes in the details of the tax code, the tax law enforcement regime, attitudes toward government, and opportunities for tax shifting. But Goolsbee's complementary work using data on the compensation of executives to directly examine behavioral responses to marginal rate changes in the 1990s seems more consistent with the earlier historical record than with estimates from the 1980s.[3]

Three issues seem worth examining in more detail. First, I will explore the "credibility" of Goolsbee's identification assumptions and the plausibility of his empirical work on income responses of the rich to tax changes from the 1920s to the 1970s. Second, his findings raise the issue of what explains the differences in the tax responsiveness of high-income individuals across time periods. Third, I will try to step back and reassess the case for focusing on the elasticity of taxable income to marginal tax rates as a decisive factor in making tax policy.

I am quite sympathetic to Goolsbee's use of a difference-in-differences methodology to examine the responses of the reported incomes of the rich to changes in marginal tax rates. He is to be commended for using what appear to be reasonable comparison groups of very high and modestly high income individuals facing different marginal tax rate changes in each of his episodes. But, as Goolsbee realizes, two strong assumptions are required for this approach to generate consistent estimates of the tax responsiveness of the higher-income group. The first is that the mean incomes of the treatment and the comparison groups would have changed by the same proportion in the absence of the tax change. The second is that the elasticities of income to tax rates are the same for both the treatment and the comparison groups. There are good reasons to worry about both of these assumptions. In particular, the incomes of the highest-income groups appear to be more cyclically sensitive (at least prior to World War II) than those of modestly high income groups.[4] There may also be differing trends in the incomes of these groups even in the absence of tax changes (a rising or falling inequality trend). And other transitory shocks may affect

3. Goolsbee (forthcoming-a).
4. Saez (1999b).

the groups differently, given their differences in reliance on different forms of income (for example, on capital income or capital gains versus employee compensation). Goolsbee's simple empirical analyses of tax reform presented in tables 7, 10, 13, 16, and 19 do little to address these issues directly. The problems of transitory shocks confounding his findings are magnified by his use of data from only a single year before and after each tax change. The focus on a single year after each tax change also does not allow one to distinguish the importance of transitory tax shifting in the timing of the realization of income from more permanent responses to tax changes.

Cyclical shocks to the economy may also importantly distort the findings. For example, strong economic growth in the boom of the 1920s means the incomes of the highest-income group may have grown more rapidly than the comparison groups even in the absence of a larger tax cut. This factor will generate an upward bias in the estimates of responses of the very rich to tax cuts in the 1922–26 period (table 7). The rapid economic growth during the recovery from the near trough of 1934 also could have led to much faster income growth for the highest-income group. This suggests that an opposite (downward) bias could be present in the analysis of the 1935 tax increase in table 13.

A simple improvement in this part of Goolsbee's analysis would be to look at some additional years before and after the tax changes, to check the sensitivity of the results to the exact choice of years and better gauge possible biases from transitory shocks. A more ambitious extension would be to explore all the tax changes from the 1910s to the 1970s in a panel data approach analogous to Goolsbee's approach to examining executive compensation in tables 21 through 26. One could use the income histograms from the *Statistics of Income* for each year, information on the total number of households (or population), and Goolsbee's Pareto distribution assumption to calculate mean incomes for different fixed upper-end quantiles of the income distribution (for example, the upper 0.1 percent, the next 0.9 percent, the next 1 percent, and so on) for each year. One could also use the tax schedules to get marginal tax rates for each of these upper-income quantiles each year. One could then run pooled panel data models regressing the real income of each quantile group on fixed effects for the group, year dummy variables, and the logarithm of the relevant marginal tax rate, $\ln(1 - \tau)$. This approach essentially would be like pooling across all years the regressions Goolsbee presents in tables 8, 11, 14,

17, and 20. One could then break the data into subperiods to allow different tax responses (for example, in war years). This approach, by using all the data, would reduce the problems of transitory shocks affecting outcomes from an arbitrary choice of years. One could then allow differential responses of each income quantile to cyclical variables (such as GDP, the unemployment rate, or stock market returns) and possibly add group-specific trends in income growth. Emmanuel Saez uses a somewhat crude version of this approach for the 1913–41 period and finds somewhat larger average elasticities to marginal tax rates of high-income individuals (in the 0.7 to 0.8 range).[5]

Goolsbee's analyses of the responses of the salaries and bonuses of CEOs to marginal tax changes over the periods 1934–38 and 1970–74 are a bit more compelling than his analyses of IRS data in that he uses multiple years and more information. A possible weakness is the lack of information on other components of CEO pay. It could be that these other components (such as stock options and deferred compensation) are the more important margin for adjustments to tax changes. Stock holdings by CEOs were quite large in the 1930s,[6] and other aspects of executive pay grew rapidly in the 1950s and 1960s and were probably quite important by the early 1970s. For example, Wilbur Lewellen reports that salary and bonuses already represented only 38 percent of CEO compensation on average for large manufacturing firms during the 1955–63 period.[7] The 1930s also represented a regime shift, with the introduction of the Securities and Exchange Commission and greater public scrutiny and reporting of executive pay. These changes may have more greatly affected the more prominent, extremely well paid CEOs and may have had an independent effect on their relative salary and bonus growth that could bias downward Goolsbee's estimates of tax effects in the 1930s.

Despite my qualms about some of the details of the empirical work, Goolsbee's basic conclusion of more modest responses of the income of high-income individuals to marginal tax rates from the 1930s to the 1970s appears quite plausible. This raises the question of the implications of these estimates for thinking about tax responsiveness today and how to interpret the much larger estimated elasticities in the 1980s. Goolsbee's interpreta-

5. Saez (1999b).
6. Jensen and Murphy (1990).
7. Lewellen (1971, table 3, p. 45).

tion is that the 1980s estimates are an outlier that should not be relied upon for policy today. He argues that the weight of evidence over the century suggests only modest tax elasticities for high-income taxpayers. This conclusion contains a hidden assumption of a relatively time-invariant underlying true elasticity.

An alternative view is that the behavioral response of the rich to tax changes may depend on the other aspects of the tax code (for example, on the interaction of capital gains rates, corporate rates, and individual rates). It may also depend on the enforcement regime and on innovations in tax avoidance and evasion opportunities and technologies. Thus one could ask why the elasticities seemed to be very small in the 1930s to early 1970s, fairly large in the 1920s, modest in the 1990s, and very large in the 1980s. One striking aspect of the periods for which Goolsbee obtains large elasticity estimates (the 1920s and 1980s) is that both involved very large tax cuts (large changes in t): top marginal rates fell from over 50 percent to less than 30 percent. Most of the tax cuts showing low responses in the 1934–74 period started from much higher marginal tax rates and involved smaller changes in τ, although large changes in $\ln(1 - \tau)$. A constant-elasticity assumption might simply be wrong. It could be that it is changes in the tax rate (τ) itself and not in $\ln(1 - \tau)$ that determine proportional responses in income. It could also be that there are fixed costs of income shifting and tax avoidance. Once marginal tax rates reach a high enough level (say, above 60 percent), there may be little responsiveness left, and there may be little responsiveness to changes in tax rates at very low levels also (income shifting may just not be worth it). It might be that the range of marginal tax rates within which taxpayers are willing to rearrange their finances to avoid taxes is around 40 to 60 percent.

Another possibility is that tax enforcement regimes differed across the different historical episodes. The anti-tax Republican administrations of the 1920s and 1980s may have been lax in enforcement of tax laws. This low enforcement could have made tax shifting quite easy in these periods and increased the responsiveness of the wealthy to tax changes. For example, Andrew W. Mellon, the Treasury secretary and leader of the tax cut movement in the 1920s, was the fourth-largest taxpayer in the United States in 1925 (and his brother Richard was the seventh-largest).[8] Mellon

8. Information on the highest taxpayers in the United States in 1925 was reported in the *New York Times*, September 6, 1925.

clearly was sympathetic to those trying to avoid high taxes. As Goolsbee notes, the new Democratic administration under President Franklin D. Roosevelt even indicted Mellon for tax evasion after he left office, and although never convicted, he paid fines to settle the case. Thus one may reasonably believe that tax enforcement regimes clearly differed for the tax changes that Goolsbee studies in the 1920s and 1930s. Similarly, other aspects of tax changes (in capital gains taxes, in corporate taxes, and so on) may differ across these time periods.

The final issue I will discuss is the extent to which the elasticity of taxable income to marginal tax rates of the rich really is the holy grail for determining both optimal tax rates and the deadweight loss of taxes on high-income families. As Joel Slemrod has shown, changes in tax enforcement regimes could greatly affect the social costs of tax avoidance behaviors and this elasticity.[9] There may be strong political economy reasons—mentioned, for example, by Friedrich A. Hayek and by Slemrod[10]—linking marginal tax rates on the rich to other tax rates. For example, tax rates on the rich may need to be higher than on others to make taxes on the middle class politically palatable. Thus, even if the rich have a high elasticity of taxable income to marginal tax rates, the upper middle class may have a low elasticity. Then the joint optimal tax regime could include high marginal rates on the rich, even if it loses revenue from them, given political constraints requiring higher statutory tax rates on the rich. Additionally, there are issues of the social as opposed to the private costs of income-shifting behavior on the part of the rich (with possible externalities from philanthropic behavior, for example) and of a possible low social welfare weight on their consumption distortions. Knowledge of the exact nature of the behavioral responses of the rich (for example, how tax changes affect their charitable giving as well as their choices between salary and perquisites) may be important for tax policy, even given knowledge of their taxable income elasticity to marginal tax rates.

General discussion: Several panelists expanded on the difficulties of interpreting the paper's results for the 1986 tax reform. William Gale agreed with Robert Hall that the 1986 results were probably driven largely by changes in the tax base rather than the change in tax rates. Gale rein-

9. Slemrod (1998c).
10. Hayek (1960); Slemrod (1998c).

forced Hall's argument that taxes on individuals should not be considered in isolation. The 1986 changes made the highest individual income tax rate lower than the corporate rate for the first time, creating an incentive to shift income from corporations to individuals. He thought that this change in individual relative to corporate rates was likely to be a major factor behind the large increases in individual income tax receipts and the significant decline in corporate tax revenue after the 1986 reform.

Gale also emphasized that research by Joel Slemrod has shown that the elasticity of taxable income with respect to the marginal tax rate depends on the tax base and on the aggressiveness of enforcement. With the many exclusions and loopholes that characterized the U.S. tax system between the 1950s and the 1970s, it was easy for individuals to find ways to avoid the top tax rates. An important feature of the 1986 law, therefore, was its attempt to reduce loopholes. To isolate the effect of changes in the base, Gale suggested comparing the 1981 tax changes (when the base shrank and rates fell) with the 1986 changes (when the base broadened and rates fell).

Nordhaus thought it important to distinguish among the variety of ways taxable income can change: through changes in the sources of income, including changes in the supply of inputs such as labor; through changes in factor prices; through changes in the time at which income is realized (for example, capital gains or compensation in the form of stock options); and through changes in tax evasion. Nordhaus thought that changes in the sources of income had been important in 1986, when, as Gale had mentioned, there was an incentive to shift income from corporations to individuals.

David Laibson thought that a period of two or three years provides insufficient experience to estimate elasticities for many types of tax changes. Some effects play out over decades rather than years. For example, the recent reform of the tax treatment of 401(k) plans will apparently give rise to a thirty-year learning curve for corporations. Even sophisticated economic agents like corporations take a long time to learn how to respond optimally to the tax code, and most households presumably take even longer. Such informational frictions are very real and suggest that one learns relatively little from some two- or three-year experiments. Laibson recognized that previous studies have looked at similarly short time spans, and he thought it useful to see whether those earlier results can be

replicated. But he suggested that we still have a lot to learn about how people respond to changes in the tax.

Bradford DeLong found Laibson's argument about informational frictions persuasive with respect to tax provisions that affect a broad mass of taxpayers, like 401(k)s and the earned income tax credit. However, he was skeptical about its applicability to the small group of very wealthy taxpayers and corporations. Hall concurred, saying that the learning time might actually be negative, as wealthy individuals, tax accountants, and lawyers often anticipate changes in the tax provisions and prepare for them in advance. Christopher Carroll agreed that taxpayers will respond to clear-cut, unambiguous changes quickly, and he imagined that the tax advisers to the top 1 percent of taxpayers respond quickly to more complex changes, but that for many changes the vast majority of households only respond over a period of years.

Lawrence Katz observed that while higher rates of growth of incomes for the highest income class than for the next highest class biases upward the estimated elasticities for the 1986 reform, both types of data used in the literature, panel and repeated cross-sectional data, are imperfect and can give rise to other biases. For example, with panel data, if the comparison group is picked on the basis of base-year income, the presence of transitory income results in regression to the mean. This effect is likely to be greatest for the highest income group, leading to an underestimate of the elasticity when looking at a tax cut and an overestimate in the case of a tax increase. Similar problems can contaminate the results from repeated cross-sectional data. What one would like to know for each individual is what his or her income would have been in the absence of the tax change. But that information can only be inferred by observing individuals with the same income and other characteristics who did not face the same change in tax rates.

Benjamin Friedman questioned the assumption that individuals are typically at interior solutions, where the marginal welfare loss from a tax change is the same whether the individual shifts out of taxable income into untaxed leisure or into untaxed forms of compensation. In most situations, he said, it would seem more appropriate to assume a corner solution as the starting point. Not only are many workers at a taxable income corner, but changes in tax law that induce some types of tax avoidance (for example, employer-provided dental plans) result in the worker being at

another corner. In some cases this is the result of legal limits, but in others (such as the dental plans), it is because demand is inelastic. In such situations nontaxable and taxable consumption may be close to perfect substitutes up to the limit, and the tax loss may be of a different order of magnitude than the welfare cost.

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