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Lessons for Tax Reform

by

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LEADING IN THOUGHT AND ACTION

Lessons for Tax Reform

Henry J. Aaron

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DRAFT
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LESSONS FOR TAX REFORM

by
Henry J. Aaron[1]

Public finance economists have long preached that tax systems should be judged on the basis of their effects on economic efficiency, ease of administration, and fairness. Few public economists thought that the Tax Reform Act of 1986 would do much to ease administration of the income tax. Some gains in simplicity were anticipated from the hoped-for reduction in the number of tax shelters. Some former itemizers would become standard deducters. But these gains in simplicity were substantially offset--some would say, swamped--by additional headaches for taxpayers with large interest deductions, passive losses, and other complicated capital transactions. Any added complexity, it was hoped, would be amply repaid in the coin of reduced horizontal inequities.

Nor did Tax Reform Act hold out much promise for those who dislike the current distribution of tax burdens, since distributional neutrality became virtually a binding constraint on the tax debate. In the end, the Tax Reform Act slightly increased progressivity, as conventionally measured (Pechman, 1990). But wealthy taxpayers may have been the major welfare gainers because the Tax Reform Act reduces the risk they bear in equilibrium (Galper, et al., 1988).

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The major disputes among economists before enactment of the Tax Reform Act dealt rather with its effects, hoped for or feared, on economic efficiency. Advocates claimed that reduced marginal rates would increase labor supply, a prediction supported by empirical research (Hausman and Poterba, 1987). They also claimed that reduction in the dispersion of effective rates of tax on tangible investment would improve efficiency by excluding projects with low before-tax but high-after-tax rates of return. Critics of the Tax Reform Act held that increased effective rates of tax on capital income would reduce saving, which most economists regard as too low. They also held that the increased dispersion of effective rates between tangible and intangible investments would cause the efficiency of investment to fall, not rise.

Nothing has happened during the three years since passage of The Tax Reform Act to confirm either the worst fears of its opponents or the best hopes of it supporters. Investment rates have not moved outside the range of past fluctuations. Saving rates rose modestly, but no one I have seen has suggested the tax reform deserves the credit. Some slight increase in labor supply may have occurred, but it is hard to detect (Burtless, 1989). No point of inflection is visible in trends in investment rates or growth of total factor productivity, but none probably should have been expected at all and certainly not this fast.

Most of the papers presented at this conference reinforce casual observation that the Tax Reform Act of 1986 has had little effect on the broad measures of real economic activity in which most economists are interested. In fact, several of the papers indicate that predicted effects were, if anything, much larger than anything actually observed.

Clotfelter finds that charitable giving has not fallen off, as it was predicted to do. Courant and Gramlich find that the types of taxes state and local governments impose and the amounts they spend bear almost no relationship to what a standard equation would have forecast. Auerbach and Hassett fail to find any of the shift from equipment to structures that past investment equations indicate the Tax Reform Act should have caused. Poterba remains convinced that the Tax Reform Act will reduce construction and drive up rents, because of theoretical reasoning, but not because of anything that has yet happened. Skinner and Feenberg join a long list of reserarchers who cannot find much evidence that after-tax real rates of return are consistently related to household saving. We have no paper on labor supply, probably because no good micro-data are yet available with which to measure possible effects, but perhaps also because nothing dramatic was expected.

In contrast, tax changes may have affected asset values, corporate organization (according to Gordon and Mackie-Mason) and the international flow of capital (according to Slemrod), but even these judgments must be hedged because so much else was going on.

As of now, in short, both casual observation and the research unveiled here make one wonder what all the fuss about gains or losses of economic efficiency was about. If Congress forbears from legislative infanticide on tax reform, it is of course possible that previously obscure effects will become apparent and that today's namby-pamby judgments may have to be modified. While major effects may one day become apparent I am going to assume in the remainder of this paper that

the verdict does not change, that no major effects of tax reform, currently obscured from our view, emerge.

This preliminary appraisal of the most important tax legislation in a generation provokes a number of observations about the future course of tax reform and of analyses of taxes. First, is it possible that our original models were correct and that the findings of little or no tax-induced response are false? Second, if we reject this possibility, what are the implications of this finding for tax research? Third, what are the implications of the findings of this paper for debate about tax policy?

How Could We Turn Out to be Wrong?

Any reasonably imaginative theorist or econometrician can come up with several reasons why the models of economic behavior that predict large behavioral effects of tax policy are right and the findings of the papers at this conference are misleading.

The first is that the Tax Reform Act of 1986 was not a clear signal in most dimensions. Marginal personal rates rose for a significant minority of taxpayers, even if they fell for a plurality. While marginal corporation income tax rates fell, the investment tax credit was repealed and depreciable lives were lengthened. Hence, effective tax rates for most types of investment actually increased. Furthermore, the Tax Reform Act modified a host of other provisions that affect various capital transactions in different ways, but that do not enter the usual calculations of effective tax rates or most investment equations.

Second, taxpayers may have regressive expectations regarding tax legislation. They may believe that changes enacted in 1986 will prove

evanescent. The current debate over capital gains and individual retirement accounts lends substance to such views. The existence of such expectations could serve to rehabilitate the models of behavior of state and local governments, for example, whose attachment to sales taxes would then be recognized as percipient, not perverse. Investors could be seen as making long-term plans dependent not on short-lived aberrations in tax law, but on the average tax environment they anticipate over the life of current projects. While regressive expectations may explain the imperturbability of investors, it deepens the puzzle about charitable donors. Why, if taxpayers see higher rates in their futures, has giving been so well maintained?

Third, we may simply have gotten lags wrong. People may expect current law to remain in effect, but adjust their behavior slowly, which is just another way of saying that data next year or the year after may return to predicted patterns.

Fourth, the experience of 1986 may be rationalized by appeal to excluded variables. In the case of saving, there is nothing really to rationalize, since past empirical efforts to establish the effects of taxes--really, of real, after-tax rates of return--on saving have been so inconclusive. But the fact that marginal personal tax rates on saving went down for taxpayers who do most of the saving, while capital taxes at the entity level went up, provide an almost infinite menu of specifications that can explain any conceivable sequence of events. One can almost pity journal editors perspiring under the heavy load of studies reaffirming the insubstantiality of the corporate veil. Future efforts to explain the rise of state and local spending after 1986 may

cite the drop in exhaustive federal spending for non-defense programs; in such models, state and local voters not only would be influenced by tax prices and income but also would use services of lower level governments as alternatives for declining national outlays.

The problem with each of these explanations, and with many others like them that we are likely to see, of course, is that they are all ex post and ad hoc. Any set of facts can be rationalized ex post by sufficient data mining. Even if new equations showing large responses to taxes can be found, they should do little to revive our confidence that such responses will in fact occur. If the goal of fitted relationships is to help us predict, the failure, chronicled in papers presented at this conference, of previously estimates relationships to predict the consequences of tax reform should instill a new and unaccustomed modesty in us all. While the past two years may end up as outliers in equations that reaffirm what we thought we knew, let us proceed, following the papers presented at this conference, as if the Tax Reform Act of 1986 should cause us to reduce our estimate of the power of taxes to influence economic behavior.

Implications for Research

Like ambulance-chasers who profit from injury and death, econometricians prosper from the demise of old economic relationships. The Tax Reform Act is for economists what the San Francisco earthquake is for construction companies--a splendid opportunity to build replacements for structures that collapsed under stress. One of the safer predictions about the effect of the Tax Reform Act of 1986 is that it will lead to a

bull market in dissertations and journal articles trying to match specification to fact.

While this prosperity will trickle down to many branches of public economics, I want to focus especially on the importance of the results presented here for analysis of tax incidence. The history of tax incidence studies reflects the gradual abandonment of naive notions of burden. For decades economists warned lawyers and others not to identify legal liability with economic burden. Early tax incidence studies assigned burdens without much empirical basis or even economic justification. In general, the burden of taxes on labor income have been assumed to fall on workers, although most studies have always found that elasticities of labor supply are positive. Taxes on capital income were assigned in ways that now seem arbitrary in the extreme--half of the property tax to renters, half to consumers, for example. The next generation of studies chose rules of thumb consistent with theoretical developments--assign corporation income taxes to all owners of capital, following Harberger's classic analysis, for example. Recently, full-blown general equilibrium models based on the assumptions of utility and profit maximization have been used to incorporate the effects of taxes on pre-tax incomes. The strength, and it now appears perhaps the weakness for incidence analysis, of these models is that their calculations rest on independent estimates of the relevant elasticities.

But if such elasticities lead to exaggerated predictions of the effects of tax changes, they may well spawn misleading estimates of tax incidence. To put matters more simply, if behavioral effects on saving as well as on labor supply are small, the relatively naive methods of

estimating tax incidence according to rules of thumb look far more attractive than they did before. Furthermore, the validity of revenue estimates made with relatively naive methods widely scorned by economists for failing to take adequate account of induced behavioral effects, looks better in direct proportion to the shrinkage of estimates of behavioral response.

The kinds of results reported here would, if replicated and sustained in future work, cast a shadow over the celebrated conclusion of optimal tax literature that welfare-maximizing tax rates leave little room for progressivity and that actual tax schedules almost certainly exceed those levels. As marginal excess burden of taxes drops, so does the marginal social cost of public spending, which suggests that cost-benefit analyses done without regard to excess burdens may not be far off the mark. As behavioral effects shrink, the utilitarian case against public spending weakens and the case for progressivity strengthens. If the behavioral elasticities are small, the relative importance of administrative effects such as those analyzed by Slemrod (forthcoming) also increase.

Implications for Policy

If behavioral effects are smaller than we thought, the importance of efficiency gains and losses falls. But the relative importance of equity, both horizontal and vertical, increases, at least for end-state theorists, who work in the utilitarian mainstream of economics.[2] In

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2. For those who adhere to process theories of distribution, which hold that any distribution of income is just if the processes that generated it were just, a drop in estimates of behavioral effects has little to do with the desirability of policies to change the distribution of income.

particular, it is useful to consider how debates concerning some of the major issues of tax policy now under debate would be changed, if at all, by a judgment that previous estimates of behavioral effects and, hence, efficiency losses, were too high. Such an exercise is inescapably speculative because each person's answer will depend on the shape of his or her tradeoffs between efficiency and equity.

Capital gains

Some proposals for cutting tax rates on realized capital gains, such as those before Congress in 1989, are so outlandish from the standpoint of tax policy that it is hard to take them seriously. Accordingly, I shall consider instead the simple proposal to return to a permanently lower tax rate on gains from sale of assets held more than a minimum period. The case for a reduced rate on income from long-term capital gains rests on three claims.

The first is that nominal gains overstate real gains because of inflation. The problem is indisputably real, but the solution of reducing gains is clearly dominated by indexing, and, as Brinner showed, a concessionary rate designed to offset inflation should rise steadily with the length holding period, a very counterintuitive and legislatively implausible idea. (Brinner, 1976)

The second is that incentives to undertake risky investments will be increased if rates are reduced, partly because reduced lock-in will augment the supply of capital and partly because increased rewards to entrepreneurs will spur demand. The third is that revenues will rise permanently because realizations grow.

Evidence to support the second argument, other than bald assertion put forward as if it were self-evident, is virtually nonexistent. Evidence to support or to refute the third argument is abundant, but contradictory. Everyone acknowledges that cuts in capital gains rates have been associated with increases in realizations; but Auerbach (1988) has shown that past studies do not clearly establish whether the effect is temporary or permanent and that data available now or likely to become available are unlikely to settle the issue.

The papers presented here can be read in either of two ways. On the one hand, the papers by Gordon and MacKie-Mason and by Slemrod lend some support to the proposition that asset transactions, as divorced from real investment or saving, are sensitive to tax rules. They support the view that decisions about when and, more importantly, whether to realize capital gains are sensitive to tax rates. But no one ever doubted the qualitative argument. The debate is quantitative, and none of the papers bears on that question.

On the other hand, the insensitivity of investment to rather blunt tax incentives should, I think, marginally increase one's skepticism that reduced capital gains rates will affect supposedly high-expected-payoff, high-risk investments enough to matter. Those who hold that reduced capital gains rates will spur such investment have not even tried to pick up the burden of proof to show that such an effect will be quantitatively significant. The papers presented here reinforce my prior view that until someone shoulders this burden of proof, their claims should not be taken seriously.

This line of reasoning suggests that the distributional effects of the proposed reductions in capital gains rates are the principle substantive consideration in appraising concessionary rates. Calculation of such distributional effects is straightforward with respect to gains that would have been realized in any event; the old and simple methods of allocation are about right.

The benefits from induced realizations are more difficult to calculate. One knows that households sell some assets at reduced rates that they would have sold later or not at all if rates were not cut. On income from such sales households certainly enjoy a welfare gain, but the gain is smaller than the difference between tax at the old and tax at the new rates. Indeed, it is not possible to calculate either incidence or overall welfare effects without knowing whether realizations are increased permanently or temporarily and how the increased realizations are distributed over income classes.

It seems clear, however, that the tables compiled by the Joint Committee on Taxation, which show that more than half of the gains from the proposals before Congress this year accrue to taxable units with incomes in excess of $200,000 per year, describe the distribution of benefits with rough accuracy. To focus on these distributional tables should not be seen as inflaming class warfare, but as attending to the only evidence of any solidity advanced so far in the debate.

Individual Retirement Accounts

Skinner and Feenberg point out that some provisions of the Tax Reform Act of 1986 discouraged and others encouraged personal saving. The net effect is hard to sort out. They claim to have found in data

for the 1980s a positive relationship between real after-tax rates of return and household saving. However, strikingly absent from their story is any mention of household net worth or the stock market or the anomalies associated with the agricultural drought of 1988. Their paper certainly does not strengthen the case that individual retirement accounts perceptibly add to household saving. This case rests largely on microdata as analyzed in a succession of papers by Venti and Wise (1987, 1988) that were criticized in turn by Gale and Scholz (1989) and even more fundamentally by Gravelle (1989). On balance, the Skinner and Feenberg paper does nothing to strengthen and may marginally weaken the credibility of claims that IRAs boost saving.

The general tone of the papers surely undercuts faith that the new version of the IRA, in which deposits are taxable but withdrawals are not, will have any meaningful effect on household saving. For households to react to this version, they have to be motivated by tax savings that they will enjoy some years or decades in the future, rather than by tax savings realized currently. For fully-rational households such discounting poses no difficulty. But some of the better arguments that old-style IRAs boosted household saving pointed to the irrationality of households to support their claims. In particular, rational households would make their IRA deposits as early in the tax year as possible. In fact, deposits were found to be bunched just before deadlines for filing tax returns, a pattern of behavior that suggests deposits were motivated to an irrational degree by current tax savings. While such evidence provides some support for a belief that old-style IRAs boosted household saving by some fraction of deposits, it also suggests that new-style

IRAs, which do not reduce current taxes at all, are likely to be far less effective.

Integration

Economists and tax lawyers have long supported some form of integration of personal and corporation income taxes. The only opponents were business leaders, who have preferred to take out their tax cuts as liberalized depreciation and investment tax credits or reduced rates, and labor leaders, who opposed any cuts in corporation taxes at all.

Resurgent concern about corporate borrowing, in general, and leveraged buy-outs, in particular, have revived interest in integration. Recent theoretical work by Gravelle and Kotlikoff (1989) suggests that a nonintegrated corporation income tax may produce larger welfare losses than prior work had suggested. Their argument rests on alleged efficiency advantages of corporations that are not realized because corporations are handicapped in competition with unincorporated competitors. The paper by Auerbach and Hassett finds that real investment has not responded as predicted to past changes in the relative and absolute treatment of structures and equipment. It elevates the question of whether the claimed efficiency gains would actually be realized from elimination of the corporate tax wedge, gains that would allegedly result from a shift in real investments to efficient companies, now operating as corporations, from inefficient companies, now operating as partnerships and proprietorships.

In contrast, however, the papers alleging the sensitivity of asset transactions to relative tax rates leave undamaged the hope that elimin-

ation or reduction of the corporate tax wedge would reduce tax-motivated corporate restructuring and leveraging.

Cash-flow Taxation

Many of the economists here have argued that cash-flow taxes are superior to income taxes. Some take this position only if gifts and bequests are subject to tax, others don't much care, and still others actively oppose their inclusion. All share the view that the annual focus of the income tax leads to horizontal inequities and inefficiencies based on the timing of consumption.

Work by Hall (1988) that finds little evidence to support an intertemporal elasticity of consumption different from zero weakens the efficiency arguments. So does the drift of the papers presented here that report less sensitivity of most real economic decisions to tax rates than prior analysis led the authors to expect. These calculations call into question the estimates of welfare gains from consumption taxation that have emerged from numerous simulations, including those by Summers (1981), Auerbach and Kotlikoff (1987), and various combinations of Stanford authors. In general, it seems, we ought to estimate the welfare effects based on utility functions in which the timing of consumption is less sensitive to tax rules than we supposed. Since the welfare gains estimated in these models from the switchover were very large, even reduced estimates may still seem to justify an eventual conversion to cash-flow or other consumption-type rules. But I am skeptical whether efficiency considerations will justify the rather considerable upheaval necessary to reverse the movement toward comprehensive income taxation initiated in 1986. If we wish to justify

cash-flow taxation, we may have to have recourse to the weak reed of horizontal inequity. (Pechman, 1990)

Conclusion

I recognize that I may be exaggerating the ramifications of the kinds of findings reported in the papers presented here for tax incidence and for how we think of the use of the tax system for income redistribution. But the economics profession has been part of a large and powerful intellectual movement that has undermined the respectability of egalitarian impulses. If the case for progressive taxation was never better than uneasy, economic analysis in recent years has made it seem to be a product of hormonal excess rather than clear thinking. The desire to curb "distinctly evil or unlovely" inequality (in Henry Simon's characterization) has has been put to shame by careful analyses showing that efforts to cut inequality would detract from welfare by producing rampant inefficiency. Results such as those presented here cast doubt on the models, even those that sported high coefficients of inequality aversion, that found welfare maximized at low marginal tax rates and that contributed to the respectability of inequality.

I confess also to being influenced in the drafting of these comments by the memory of Joseph Pechman, who always retained more interest than was sometimes fashionable in the distribution of income and in the power of taxes to change it. He would have chuckled as he read the papers written for this conference.

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