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THE IMPACT OF TRA ON STATE AND LOCAL FISCAL BEHAVIOR

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Just three years have elapsed since TRA was signed and most of its provisions regarding states and localities became widely known. Most of these provisions have been in effect for less than three years. One or two years of post-TRA aggregate data on states and localities are available, one or no years of data for some of the key micro variables. This is not very much information, given various lags in behavioral responses and the degree to which subtle effects emanating from TRA can be concealed by more dramatic behavioral shocks. Our purpose here then is to take a first look at whether TRA is having the various impacts it was thought likely to have. There is no question that these early indications may become revised or outmoded as time passes, behavior changes, and new data become available.

We investigate three main topics:

o The impact of TRA on the aggregate fiscal policy of states and localities -- spending, the level and composition of taxes, and asset stocks.

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1. We are grateful to Steven D. Gold, Donald Peters, and Gilbert Metcalf for making available recent data, interpreting recent events, and commenting on earlier drafts of the paper. We are also grateful to Charlotte Mack, who provided able research assistance.

o The impact of TRA on the economic activity, property values, and tax bases of local jurisdictions.

o The impact of TRA on the market for state and local bonds.

The first topic is investigated by comparing aggregate national income accounts data for 1987 and 1988 with those of earlier years through time series regression analysis. The second is investigated by a form of event study. Rather than conducting what seemed to be a hopeless examination of mobility and property values for all jurisdictions, we focus on adjacent border jurisdictions in states that should be quite differentially affected by TRA. We then look for shocks in a variety of indicators of population shifts, including building permits, assessed values, and county employment levels. The third topic is investigated by comparing the return on state and local bonds relative to taxable bonds, both before and after TRA.

I. State and Local Fiscal Behavior

We first discuss a simple theory of state and local fiscal behavior in the presence of differential federal tax treatment of different taxes and the pre-TRA empirical predictions of its impact. We then give our own early empirical estimates of what seemed to have happened, and compare these estimates with more informal reports of what is going on.

A Simple Theory

Consider a jurisdiction that levies a variety of taxes and user fees, receives grants from other governments, and spends the money on goods, services, and transfers. Without going into the underlying social choice

mechanism, we can posit that the jurisdiction has a demand for public spending that varies positively with the community's resources and negatively with the total cost to the jurisdiction's residents of a dollar's worth of public expenditure (E). Thus, jurisdictions will have demands for public spending that are downward-sloping in the cost per dollar of such spending, drawn as the MB schedule in Figure 1.

The supply schedule for this jurisdiction's public expenditure is more complicated. If the jurisdiction behaves rationally, this supply schedule will be the usual horizontal sum of the marginal cost functions of the various sources of revenue. First will come lump-sum grants (G). These are fixed in total amount, and once this amount is reached, are supplied inelastically. Then come the various taxes the jurisdiction can assess. To anticipate later analysis of TRA, these taxes will be disaggregated into those that were deductible before and after TRA, D, those taxes and fees that were non-deductible before and after TRA, N, and sales taxes, S, which were deductible before but not after TRA. The marginal cost for each of these taxes is weakly increasing in revenue raised, because of direct burden, excess burden, or political costs. These marginal cost schedules are shown as D, N, and S respectively in Figure 1.

The jurisdiction will choose its revenue sources so that the three marginal costs are equalized. At this point the marginal benefits of public spending will also equal these common marginal costs, as shown in Figure 1. The line indicated by MC is the horizontal sum of all revenue sources before TRA and E is the equilbrium level of public spending, financed by the fixed level of G and the designated level of each of the taxes.

Now assume a federal tax change such as was made by TRA. This bill would change the fiscal position of the jurisdiction by:

o Lowering the marginal federal tax rate faced by most itemizers in the jurisdiction.

o Lowering the fraction of taxpayers who itemize.

o Removing the sales tax deduction altogether[2].

TRA did not affect the marginal cost schedule for non-deductible taxes and fees, so there would be no shift in the N schedule.

The standard way of representing the impact of changes such as this has become to focus on the mean voter in the jurisdiction. With this focus, even if the median voter in the jurisdiction does not itemize deductions, the loss of, say, the sales tax deduction is costly for the jurisdiction because some voters in the jurisdiction are worse off and now find public services to be more costly.[3] Letting t stand for the average marginal federal tax rate of itemizers in the jurisdiction before TRA, t' for this rate after TRA, a for the share of taxpayers who itemize before TRA, and a' for the share of taxpayers who itemize after TRA, the other two tax schedules would shift as follows:

1) $D' = D(1-a't')/(1-at)$

2) $S' = S/(1-at)$

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2. Other changes affecting the state and local bond market will be discussed below

3. The issue is dealt with in more depth by both Courant and Rubinfeld (1987) and Feldstein and Metcalf (1987).

These shifts are shown in Figure 1, as is the shift in the horizontal sum schedule to MC'. The jurisdiction would reoptimize at the new expenditure level, E', and the higher after-tax marginal tax cost of all three taxes.

One could then make the following predictions of the impact of TRA on state and local budgets:

- o State and local spending (E) would fall.
- o Non-deductible taxes and fees (N) would rise.
- o Sales taxes (S) would fall.
- o Deductible taxes (D) would fall.

The first two predictions would be true with any normally-sloped spending demand function and marginal cost function for non-deductible taxes. These two predictions in turn imply that the sum of S and D will fall. If the pre-tax cost functions of these two taxes are similar, S is likely to fall by more in percentage terms because its after-tax cost increases by more. But there are some instances where either tax may not drop, although the sum still will.

One such complication is mentioned by Inman (1989), who gives a reason why sales taxes could rise in response to TRA. Inman points out that in eliminating a tax deduction primarily used by the rich, TRA might have thrown the distributional balance of states and localities out of whack, forcing these governments to raise regressive sales taxes to restore their distributional balance. Another complication that works in the same direction is raised by Metcalf (1989), who shows that when sales taxes are exported to other jurisdictions, the relative cost of sales taxes could rise less than that of income taxes with TRA: the price of the component borne at home rises

more than for income taxes but the price of the exported component does not rise. This latter effect would be shown on Figure 1 with a very elastic S schedule, so that even a higher percentage change than for the D curve might raise the marginal cost at the initial revenue mix by less.

These predictions are also only relevant for changes in the after-tax cost of certain taxes, which in effect pivot the MC curve counter-clockwise. Further complications ensue when some of the schedules in Figure 1 also shift for independent reasons. If, for example, there were an outward shift in the MB function, the new intersection would be on the MC' schedule at a higher expenditure level. The higher after-tax marginal cost and benefit would lead to greater use of all taxes, with the consequence that S and D might both rise in order to finance this higher spending. If, on the other hand, grants were cut, the MC' schedule would be shifted in, the new equilibrium would be at a lower spending level but again a higher after-tax cost of all taxes, and again all taxes could be increased to finance the cut in grants.

One final factor is what is known as the "windfall effect" of TRA. TRA raised the federal tax base, and many states use this federal tax base as the base for their own tax. In such a case TRA could shift the D curve outward because of this windfall effect, again making it more likely that D would increase.

Pre-TRA Empirical Predictions

Before the fact almost everyone predicted that TRA would lower state and local spending. Using various estimates of marginal rates and numbers of itemizers before and after, Courant and Rubinfeld (1987), Kenyon (1987), and

Gramlich (1987) all predicted very slight declines in state and local spending, on the order of 1 percent. Using empirical estimates from a sample of local governments, Holtz-Eakin and Rosen (1988) predicted very sharp declines in local spending, on the order of 8 percent. Applying their elasticity estimates to state spending gives even larger percentage declines for states.[4] This is one battle to be joined.

The second prediction is that TRA should have raised the share of non-deductible taxes and fees. While this prediction is clear from the theory, nobody was able to get very sensible parameter estimates before the fact. Feldstein and Metcalf (1987) obtained mixed results on the question, while Holtz-Eakin and Rosen (1988) and Inman (1989) estimated coefficients, albeit insignificant ones, that implied that fees would fall, not rise, in response to TRA.

The third and fourth predictions are that the mix would shift away from those taxes where marginal costs are increased. Hettich and Winer (1984), Inman (1989), and Noto and Zimmerman (1985) all found relatively small effects of TRA on the revenue mix. In contrast, Feldstein and Metcalf (1987) and Holtz-Eakin and Rosen (1988) found much larger effects. Feldstein and Metcalf estimated very high elasticities (with large standard errors) of revenue from deductible taxes with respect to marginal cost. At face value, their estimates implied that TRA should reduce income taxes by at least 12 percent

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4. Holtz-Eakin and Rosen's estimate of the elasticity of local spending with respect to the price of deductible taxes is -1.8. The tax price of local spending rose by 4 or 5 percent as a result of TRA; that for state spending, because of the sales tax, by about 6 percent.

and sales taxes by 30 percent or more. The similar numbers for Holtz-Eakin and Rosen (with smaller standard errors) were that local deductible taxes should fall by 6 percent and state sales taxes by 14 percent. Another battle to be joined.

As mentioned above, all of this gets much more complicated when other large changes are occuring simultaneously. And there were no shortage of these. One of the most dramatic is the change in the fiscal relationship between the federal government and states and localities. Grants from the federal government have been cut significantly. A decade ago grants were 3.4 percent of GNP and rising as a share; by 1988 grants were 2.3 percent of GNP and falling as a share. Provisions on the big income distribution grants -- AFDC and medicaid -- have been tightened, general revenue sharing has been cut out altogether, categorical grants have first been converted to block form and then killed or reduced. But while these cuts were occurring, the federal government has actually mandated increased state in local spending in areas such as health care, environmental protection, and human services.

Regarding windfalls, Gold (1988) estimated the state personal income tax windfall to be $6.3 billion, of which only $1.1 billion was likely to be retained after all discretionary rate cuts took effect. Aten (1987) initially estimated the state corporate tax windfall to be $3.4 billion, though subsequent data suggest it was less (Aten and Gold, 1989).

A Time Series Model

To see whether the various theoretical and empirical predictions of the impact of TRA stand up, we fit a model to aggregate time series data.

Obviously it would have been possible to take a case study type look at the behavior of particular state or local governments, as Chernick and Reschovsky (1989) recently did, but there is always the problem of generalizing to the whole on the basis of particular governments. And there are so many individual local governments (about 80,000 right now) that one could never say anything sensible about local governments with this approach.

Our first approach then was to do what researchers would naturally do in investigating any other aspect of TRA -- its effect on investment, saving, housing, or whatever. That is to use aggregate national income accounts data. These can now be broken down by state general governments and local general governments separately, which also makes sense in view of the fact that different taxes and types of spending are used by the different levels of government. Separated state and local fiscal data are now available on an annual basis from 1959 to 1988 (Levin and Peters, 1986, 1987, Peters, 1988). We applied a time series model modified from Gramlich (1978) to annual time series observations from 1959 to 1986, the period of the old tax law, and then made out-of-sample extrapolations for the post-TRA years 1987 and 1988. From these extrapolations, we can see whether the residuals moved in line with the theoretical and empirical predictions.

The model postulates a state or local objective function made up of three arguments:

- o Spending
- o After tax private income
- o Fund balances.

The latter is included because available fund balances can be turned into future spending or private income. Utility is maximized subject to the budget constraint:

3) $\Delta B = D + S + N + G - E$

where ΔB refers to the change in the stock of fund balances, or the surplus, of governments and all other variables are as defined above. The level or stock of fund balances, B, is defined as:

4) $B = B_{-1} + \Delta B$

Combining 3) and 4), it can be seen that:

5) $B_{-1} + G = E - D - S - N + B$

which is the basic budget identity of the model. The left side variable of 5) is grants and previously unallocated balances. These can be distributed to spending (E), tax cuts (-D, -S, and -N), and currently unallocated balances (B) according to the actions of state or local politicians.

To focus on shifts in tax shares resulting from TRA, we estimate separate regressions for seven or six dependent variables:

- o Direct spending (E for localities, one component of E for states)
- o Grants to localities (the other component of E for states)
- o Deductible personal taxes (one component of D, mainly income taxes for states, mainly property taxes for localities)
- o Deductible business or corporate taxes (the other component of D)
- o Sales taxes (S)

- o Non-deductible taxes, fines, and user fees (N)
- o Currently unallocated balances (B).

Following the standard utility maximization calculation (detailed in Gramlich, 1978), each of these variables can be shown to be a function of income (GNP less federal taxes plus federal transfers to persons), a price term, and grants and previously unallocated balances (B_{-1} + G).

A model like this could be fit either in money or real terms. The budget identity actually works in money terms and since most state tax systems are not indexed for inflation, most tax equations should also be formulated in money terms. On the other side, spending equations are usually fit in real terms, as if voters make decisions about real spending levels. Since our main interest is on the tax side and in the budget identity -- to see how impacts are allocated across all budgetary categories -- we fit the model in money terms. Each dependent variable and dollar flow independent variable is measured in money terms and each equation includes the state and local purchases deflator as a separate independent variable. Previous versions of this model disaggregated grants according to whether they did or did not reduce the prices of favored activities at the margin, but since most actual grants these days do not reduce prices at the margin (are closed-ended), and since there is little time series variation in matching rates for open ended grants, we simplified the model to treat all grants from higher level governments as exogenous closed-ended grants with no price effect at the margin.

There was serial correlation in the level version of the model, so we fit the equations under the assumption that the time series residuals for all equations (u) followed the first order process

6) $u = .75u_{-1} + e$

where e represents the new shock in any period. The coefficient .75 must be the same in the equations for all budgetary components to preserve the budget identity, and it was selected by examining the uncorrected residuals from all seven or six budgetary equations.

The set of identities given above requires that the sum of all dependent variable equals $(B_{-1} + G)$, so the coefficients of this variable sum to one and the constants and coefficients of all other variables sum to zero. This constraint was automatically built into the coefficient estimates by the simple expedient of including every independent variable in the equation for every dependent variable. There are more elaborate ways to incorporate constraints if some of the coefficients have to be zeroed out, but most of our coefficients made reasonable sense, and since our main interest was in the residuals anyway, we followed golfers' summer rules and played the coefficients as they lay.[5]

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5. Using the constraint in this way does lead to one complication we have not been able to resolve. The constraining variable B is a lagged dependent variable, which means that there could be serial correlation in the residuals beyond that taken out by our correction. One might want to construct an instrument for B and use that in the estimation. Knowing what to do about the constraints is then not straightforward, however, because in principle all dependent variables should then be recomputed.

The estimates for state governments are shown in Table 1. The row sum of coefficients for the constant and each independent variable other than grants and previously unallocated balances is zero and that for grants and previously unallocated balances is one, as discussed above. Hence in this first year a dollar of federal grants raises direct spending by .2935, grants to localities by .3155, causes .0371 worth of tax cuts, and raises the surplus by .3538. Next year this .3538 goes into unallocated balances and is further distributed to spending increases and tax cuts. On the other hand, in the first year a dollar rise in GNP raises all taxes by .1013 (.0312 + .0121 + .0442 + .0138), spending by .0682 (.0473 + .0209), and the surplus by .0332. Next year this amount too causes further rises in spending and this time slight cuts in taxes. Since the spending coefficient on untied grants is well above that for private income, there is an important "flypaper" effect -- funds inflows stick where they hit. Such an effect is quite characteristic of empirical models of state and local behavior and there have been many, many theoretical rationales developed to explain it.

A one percent (.01) increase in the gross price deflator raises money spending by $.58 billion, implying a price elasticity of spending demand of about -.8 at present levels of the variables. The finding that state spending demand is inelastic is standard, though it is puzzling that a rise in the gross price and money spending is coupled with tax cuts, leading to a large decline in fund balances. But the gross price variable (the state and local purchases deflator) in a time series analysis is certainly measured very poorly, it behaves pretty much as a trend, and it should not interfere with our attempt to discern TRA-induced changes in residuals.

Table 2 gives the residuals for these equations. Since the equations were fit assuming a first order serial correlation process, it is not straightforward to know which residuals to give. The upper part of the Table contains the residuals before any correction for serial correlation, u in equation 6), for 1987 and 1988 in comparison with those for the rest of the 1980s. Up to 1986 the residuals were within the regression sample, the 1987 residuals are out-of-sample extrapolations based on 1987 values of the exogenous variables, and for two of the series, deductible personal taxes and sales taxes, this calculation could also be done for 1988. As before, the residuals sum to zero across all budgetary components for each date.

The next panel shows the component of these residuals that would have been predicted at the end of 1986, the last year before TRA went into effect. To measure these predicted residuals, we have used whatever information was available at the end of 1986. The uncorrected 1986 residuals were known and forecast through equation 6). Given these, the model solved for the 1987 value of S, which in turn was used to generate predicted 1988 residuals across all seven budgetary equations. As the footnote to the Table explains, since the 1986 prediction of 1987 stocks was higher than actual by $2.5 billion, this $2.5 billion was allocated across all seven budgetary equations by the estimated coefficients to give predicted 1988 residuals that sum to the same amount. Then these predicted residuals were subtracted from the uncorrected residuals to give the new shocks for 1987 and 1988, shown in the bottom panel.

These new shock residuals suggest the following story about the first year impact of TRA on state budgets:

o There was a positive shock in the direct spending residual of $6.6 billion for 1987. The authors who predicted a small decline in state spending, such as Courant and Gramlich, were wrong, but not nearly as far wrong as those who predicted a large decline in spending, such as Holtz-Eakin and Rosen. The shock could be partly caused by the non-grant mandating of expenditures discussed earlier, but even the extreme estimates of the governors would not peg it as this large.

o There was a positive shock in the residual for state grants to localities of $2.7 billion. This shock could well be due to federal grant policy, because federal grants to localities were cut by the large total of $4.5 billion between 1986 and 1987. This rise in state grants to localities could then be compensating for some of the decline in federal grants to localities.[6]

o There was a positive shock in deductible personal taxes of $4.7 billion (the residual of the negative of taxes was negative). Gold's windfall amount mentioned earlier would seem to account for only a small share of this total, but there are many reasons why our residual could be above the net calculated by Gold. States could still be benefitting from a capital gains windfall in their 1987 refunds, or their discretionary measures to return the windfall could simply not have been made yet or taken effect yet. In view of these possibilities, our shock of $4.7 billion is likely to be quite close to the mark.

o There was a positive shock in deductible business taxes of $3.6 billion, again very close to Aten's windfall amount.

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6. A recent paper by Helen Ladd (1989) finds strong evidence of such behavior.

o Sales taxes showed a positive shock of $0.9 billion. Apparently the financing effect described above outweighed the price effect, belying the predictions of those who argued the reverse.

o Non-deductible taxes and user fees showed a negative shock of $3.4 billion, a change that makes sense under none of the hypotheses discussed earlier. Even Inman's ingenious account of why sales taxes might have increased cannot simultaneously explain why regressive user fees might have decreased. The simultaneous decline in oil prices could explain some drop in severance taxes, part of the reason for the decline in this catchall, but there is no obvious story for the sharp drop in "fines", which accounts for most of the drop. The only consolation is that all other pre-TRA estimates of user fee sensitivity also had the wrong sign.

o The surplus showed a negative shock of $3.6 billion, the net implied by all of these spending and tax changes.

Then we move on to 1988. At present data are available for only two of the taxes, personal deductible taxes and sales taxes. But they both show that taxes are moving in the expected direction. The personal tax windfall now seems like it is down to zero, much as Gold might have forecast, and the positive shock in sales taxes is also very small. So price effects are not of the wrong sign, but they are obviously not of the right sign either. In fact, the 1959-86 equation works remarkably well for these two taxes through 1988.

Given the large state spending residuals and the windfalls, it is not terribly surprising that state deductible taxes rose in the short run. And it may not even be surprising that state sales taxes rose. But what is very

definitely surprising from almost any standpoint is that state non-deductible taxes and fines went down.

Regressions for localities are shown in Table 3. The equations are presented in the same form, the only change being that there is no analogue for state grants to localities -- all local spending is direct. The equations again show a powerful flypaper effect and again imply a price elasticity of spending demand in the neighborhood of -.5, a standard result. This time rises in the gross price do not lead to any noticeable changes in taxes but are entirely financed by drawing down balances.

The residuals in Table 4 tell a very similar story about the impact of TRA:

- o The first year new shock for direct spending is $10.3 billion, only about $1 billion of which could be attributed by the cuts in higher government grants to localities (federal grants down by $4.5, state grants up by $2.7, and the coefficient of grants on spending is .6600).
- o Both types of deductible taxes show a positive new shock of $2.1 billion. Since these deductible taxes are mainly residential property taxes, there is no windfall and the entire rise must be attributed to the need to finance the rise in direct spending.
- o There is no new shock for sales taxes. Indeed, the 1987 value of $24.9 billion shown in the Table indicates that local sales taxes are pretty minor to begin with.
- o User fees have a slight positive shock of $1.4 billion.
- o Fund balances show a negative shock of $6.8 billion.

Again we have the large positive residual in spending, also observed at the state level. There is some rise in deductible taxes to finance this spending demand, and this time some rise in nondeductible taxes and fees as well. But for the most part in 1987 local governments had not gotten around to financing the spending increase but just let balances fall.

By 1988 some of these tax responses begin to occur, but not in directions pleasing to those who predicted large price effects. Unlike for state governments where the embarrassing positive shocks in deductible and sales taxes are at least getting smaller, at the local level these shocks seem to be getting larger. That could reflect the financing effect of continued spending pressures for higher revenue: it certainly does not confirm a change in the revenue mix due to tax price changes.

In the theory outlined above state and local spending is endogenous, and is reduced by TRA. These econometric estimates make it hard to see price impacts on the revenue mix because instead of falling, both state and local spending are rising so rapidly. In order to see if our results were being driven by the spending residuals, we re-estimated the model, this time making state and local spending exogenous so that its residual is constrained to equal zero. Then the equations are re-fit with only the tax and unallocated balances residuals adding to zero. Instead of equation 5), the constraining identity becomes

7) $B_{-1} + G - E = - D - S - N + B$

This variant of the model is given in Tables 5 through 8. The equations are, if anything, more sensible than before because prices can be assumed to

affect spending, spending to affect taxes, and the puzzling direct link between prices and taxes can be suppressed. But the residuals tell about the same story as before. The new state shocks in Table 6 still show the windfall gradually dying out for deductible taxes. This time the sales tax shock begins negative and then becomes positive in 1988, exactly the reverse pattern of the earlier estimates. Nondeductible fees and fines are still down. Hence by and large the recursive model for states still gives no comfort to those who believe in large price effects.

The new local shocks in Table 8 are generally pretty small. As in Table 4, local user fees move the right way, and sales taxes residuals are very small. Deductible taxes rise for some reason (the windfall effect is still inoperative for local property taxes), as do balances.

Hence as regards fiscal flows, it is frankly hard to see a big impact of TRA on either state or local governments. For some reason direct spending went up at both levels of government. This put pressure on governmental finances and led to some non-windfall-induced rises in deductible taxes at both levels. Sales tax residuals were generally positive, if fairly small, indicating that the financing effect seemed to dominate the price effect. Non-deductible taxes and fees, which should have risen sharply at both levels of government, in fact did not, rising only slightly at the local level and falling at the state level. The main financing of the spending surge was then left over for fund balances. To the extent that taxes changed to finance this surge, knowing about TRA would not have helped one to make very good predictions of changes in the revenue mix. And the predictions of large

budgetary effects by Holtz-Eakin and Rosen and Feldstein and Metcalf are simply not supported by the experience so far.

Other Evidence

These regression residuals seem quite consistent with other evidence of post-TRA fiscal changes for state and local governments. It is hard to document the puzzling fall in non-deductible taxes and fees, made up of countless items not even recorded separately in the aggregate accounts. But sales tax changes are generally made at the state level, and there it is easier to see what is going on. According to Gold (1988), Gold and collaborators (1987, 1988) and Fabricus and collaborators (1989), the behavior of many states was consistent with the pattern of our residuals. In 1987 and 1988 seven states substantially increased their sales taxes, in 1989 five more states significantly increased sales taxes, and a number had less significant increases. Both the regression results and this more informal evidence then confirm the puzzling move toward greater use of state sales taxes.

The situation with income taxes is more interesting, and goes beyond a simple explanation of the residuals. Even though no clear story emerges from these residuals, there does seem to be evidence of deeper structural change in state income tax systems that mirrors changes at the federal level. Gold (1988) and Fabricus and collaborators (1989) detail a number of these:

o By 1989 eight states had restructured their entire income tax, generally increasing conformity with the federal income tax.

o By way of redistributing their windfall gains, nineteen states reduced income tax rates and only six increased them as of 1989.

o By 1988 eighteen states had increased their personal exemption or credit, twenty states had increased their standard deduction, and twelve states eliminated all taxes on poor families. More states moved in these directions in 1989.

The net result of these changes is to reduce administrative costs by increasing the conformity between state and federal tax systems, and to enhance economic efficiency by generally lowering state marginal tax rates. Equity is also improved, to the extent that many poor families are removed from state as well as federal tax rolls. In the long run these structural effects of TRA should be much more important than the price effects economists spend much more time discussing. And we might say that even if we could find some price effects.

II. Mobility and Capitalization

As was seen already, TRA affected the tax price of state and local public expenditure differentially in different states. Tax prices rose in all states because of the drop in marginal federal tax rates and the elimination of the sales tax deduction. But because the incidence of deductible and sales taxes varies a good deal across states, the effect of TRA on effective tax burdens also varied a good deal. In this section we try to exploit that variation by looking at economic and fiscal behavior in adjacent cities (sometimes counties) on opposite sides of state lines before and after TRA.

By looking at adjacent cities we can make the convenient assumption that everything other than tax prices changed in the same way on both sides of the state boundary between 1986 and 1987. Take two cities, say Fargo, ND, and

Moorehead, MN. These cities are in the same labor market, the same grain market, and the same tractor market. Even fairly localized changes in economic environment should affect them in about the same way. But TRA will not: it will cause a much higher rise in effective tax burdens in high tax Minnesota than in low tax North Dakota.

What would then happen? A first possibility is that governments would adjust, with Minnesota, say, eliminating its sales tax. In the first part of the paper we found minimal evidence of such adjustments to take advantage of TRA for states and localities as a whole, though it is always possible that there would be more adjustment in our key border towns. If there is no governmental adjustment, then the private sector may adjust its location of business or residential activity. Our basic strategy then is to pick pairs of cities where the differential effect of TRA on tax prices was large, and to analyze a number of measures of economic behavior for each pair. The reason for looking exclusively at cases where the differential effect was large is simply that little time and less data have passed since TRA; if TRA is going to have measurable effects, it is most likely to have them across these pairs of cities.

Even in this purposely biased sample, there are probably few cases where differences in the change in the local cost of public services are large enough to induce people to bear the cost of moving across a state border (the biggest differences are on the order of 0.5 percent of annual income for the average taxpayer). But the differences could be large enough to affect the behavior of both households and firms that are potentially moving within or into the economic area. Such effects are both a consequence of local and

state fiscal policy and impose a constraint (or opportunity) on the fisc. The town on the favored side of the border will enjoy a fiscal windfall at the expense of the town on the unfavored side. Direct evidence on these fiscal effects with one year of data seems too much to hope for, but direct evidence for the economic effects has a better chance. The capitalized value of a half percent of annual income is perhaps $1000 for the average household, much more for the average itemizing household or the average rich household.[7] If all of the change is capitalized immediately, there should be noticeable changes in property values. If some of the change remains available to new entrants, there should be noticeable increases in population and economic activity on the favored side of the border.

Ideally, we would look at changes in the market value of existing real property, which would measure the extent to which changes in local tax benefits were valued in local markets. In practice, none of the measures available is anywhere near that good. For some of our pairs we do have data on changes in assessed value, but we have no idea how much the change is due to improvements and how much to revaluation of existing property, nor do we know the relationship between assessed value and market value. Moreover, none of the data sets we used has complete information for every city, and this makes things especially difficult when the unit of analysis is the city pair. If, for example, data of tolerable-looking quality are available for 60 percent of the cities, we will only be able to use 36 percent of the pairs.

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7. For high income itemizers differences in the change in the local cost of public services across neighboring jurisdictions could exceed 1 percent of a much higher income. The annual change for high income itemizers could be on the order of $600; the capitalized value more than $10,000.

In order to choose pairs of cities (sometimes counties) that might be expected to show effects of TRA, we constructed a measure of the change in the cost of state and local taxes collected from residents. As before, t stands for the mean marginal tax rate faced by itemizers in a state, a for the fraction of taxpayers who itemize, S for sales taxes, and D for deductible taxes. If governments do not change their tax structure as a result of TRA, the increase in state and locally borne cost of tax collections as a share of state income ($\Delta C/Y$) can be expressed as

8) $\Delta C/Y = atS/Y - \Delta atD/Y$

We calculated $\Delta C/Y$ for each of the contiguous 48 states (Alaska and Hawaii did not seem promising sources of city pairs), using Metcalf's NBER TAXSIM calculations of a and t for 1985 (the most recent year available) and actual state budget data for 1986. Δat was estimated by assuming that $\Delta at/(1-at) = -.46$ in all states, the average estimate that Courant and Rubinfeld (1987) derived from data used by Hausman and Poterba (1987).

Having estimated $\Delta C/Y$ for 48 states, we plotted the values on a map and looked for those pairs of states that had cities near to each other and that had the largest differences in $\Delta C/Y$. We required that there be cities of noticeable size both because that increased the probability of finding useful data and because it enhanced the plausibility of the maintained hypothesis that the relevant labor market conditions would be the same on both sides of the border.[8]

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8. But we were unable to use data on the fiscal policies of the cities themselves. Each of these cities had to be assigned the average fiscal

Fourteen pairs of adjacent states had values of $\Delta C/Y$ of .002 or greater. The largest difference (Pennsylvania-New York) was .004. For each of the fourteen pairs of states, we looked for adjacent cities on both sides of the border. Some (Greenwich, CT - Port Chester, Rye, NY; Portsmouth, NH - Kittery, ME) are very close indeed. Others (Billings, MT - Sheridan, WY) are much farther apart. And some simply do not exist, such as on the border between Oregon-California or, curiously, Massachusetts-New York. For some states there is more than one potentially usable pair. Pennsylvania and New York, for example, have a long border, and cities (or counties) can be matched across it in more than one place. Information about the city pairs and their data availability is given in Table 9.

For each city (or county) pair for which data were available, we analyzed data on building permits, assessed property values, and county employment. In all cases, we computed the share of the favored member of the pair in the total volume of activity in the pair and measured the change in the share between 1986 and 1987.

Usable building permit data for single-family houses are available for eleven pairs of cities; for multi-family houses, no cities. TRA should have caused the share of building permits to rise in the favored state. But it did so in only four of the eleven cases, with an average change in share of -7.5 percent. When these data were smoothed by subtracting the average of available 1984-86 shares from the 1987 share, the share rose in five of the

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policy of all state and local governments in the state, a potential sources of measurement error.

eleven cases, with an average change in share of -4.8 percent. It is also possible that some building permits taken out in 1986 could have been in response to TRA, so we compared the 1986 and 1987 average share with the share in the previous three years. In this test, six of the eleven cases were positive, with an average change in share of 0.1 percent. All of this leads to the conclusion that 1986 was an unusually good year for the favored cities, but 1987 an unusually bad year. TRA could explain the first change but not the second, and certainly not the fact that the two year average was unchanged from the previous three year average. One example of this relationship between $\Delta C/Y$ and changes in the share of building permits is plotted in Figure 2. What should be a neat positive relationship entirely in quadrant I is in fact a jumble.

Regarding assessed values, the use of the Moody's (1988) series for calculations such as these can be seriously misleading if there are changes in assessment rules from year to year. But excluding those city pairs where neither element element had a change in assessment practices between 1986 and 1987 left only four city pairs.[9] The share for the favored pair did rise in three of them, but this is hardly a statistically significant finding.[10]

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9. While Moody's does not report a reassessment for either Billings, MT, or Portland, ME, it is clear from the numbers that there must have been one.

10. We can only interpret these data qualitatively, not quantitatively, because the denominator of the share, "total assessed value" for the pair, does not mean anything when different jurisdictions have different ratios of assessed to market value. Only the sign of changes in the share are meaningful, only when assessment rules have not changed.

Finally, *County Business Patterns* provides extensive information on economic activity in U.S. counties, and is in principle suited to shedding some light on the problem at hand. Unfortunately, the Census Bureau has not yet released 1987 for most regions of the country. New England and the Mountain states are all that are available as of September, giving us three New Hampshire pairs and Montana-Wyoming. More data are due to be released soon. Shares of total employment and total payroll did rise in three of our four pairs, but again these are awfully sparse data from which to generalize.

The upshot of our attempts to look at early evidence on mobility and capitalization is more randomness. In the previous section of the paper there was some suggestion from the early data that TRA was not having large effects on the budget position of states and localities: here a more appropriate conclusion is that it is just too early to do analyses of this sort.

III. The Market for State and Local Bonds

TRA was also predicted to have powerful effects on the market for state and local bonds. The most important reason is that the value of the exemption of state and local bond interest is now reduced, both by the lower personal and corporate tax rates and by the fact that half of bond interest is now included in the base for the corporate alternative minimum tax. Other provisions that could have an effect are those that eliminate the interest deduction for banks that use the borrowing to buy tax-exempt securities, and those that limit the issuing of private-purpose tax-exempt bonds by state and local governments.

To see how all this works, a general expression for the interest rate on tax-exempt municipal bonds, m, can be written as:

6) $m = r(1-bt)$

where r is the interest rate on taxable securities, t is now the marginal federal tax rate applied to the taxable interest income of the highest income taxpayers, and b is an adjustment factor.[11] Were all marginal tax rates the same and state and local securities perfect substitutes for private securities, b would equal one. But if municipal bonds are held by investors with lower tax rates than this maximum, b can be less than one empirically. Poterba (1989, from whom much of the following analysis is taken) shows that over the 1980s b has been about .93 for short term bonds (based on the corporate rate paid by banks, alleged to be the marginal investors in this market) and about .55 for long term bonds (based on the rate paid by high income individuals, alleged to be the marginal investors in this market). The different top bracket tax rates are used because historically there has been a good deal of segmentation between the short and long term markets -- governments are either forced to use, or find it advantageous to use, long term bonds to finance construction projects even though the implied interest rate is higher, and banks have been arbitrageurs in the short but not the long term market (Mussa and Kormendi, 1979).

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11. In principle one could use either the highest marginal rate (.33) or the rate for the highest income taxpayers (.28) in 6). That there is now a difference is one of the little nuggets of TRA. Since most wealth available for investment in state and local bonds is in the highest income range, we will use .28 throughout this section.

When m and r differ, Gordon and Slemrod (1986) point to the existence of a number of separate arbitrage opportunities involving state and local bonds:

o Classic governmental arbitrage, where the government borrows at the tax-exempt rate m and lends at the taxable rate r. Citizens gain r-m on every dollar borrowed. The Internal Revenue Service (IRS) has rules against this form of arbitrage, but Gordon and Slemrod show how difficult it is for the IRS to enforce these rules and Metcalf (1989) shows that a lot of this form of arbitrage does seem to exist.

o Brokerage arbitrage, where citizens use their government as a broker. Citizens borrow at r(1-t) and prepay taxes so their government can invest and earn r. Citizens must trust their friendly local government to credit them the prepaid taxes. If they are so willing, they can gain rt. Note that bonds are not involved in this transaction.

o High income arbitrage, where citizens borrow at r(1-t) and invest in municipal bonds at m, gaining m-r(1-t). The citizens' own local government plays no role in this transaction. The gap between m and r(1-t) also corresponds to the annual gain per dollar borrowed realized by high income people from this sort of tax subsidy for state and local governments, in that these high income people earn m-r(1-t) more than they would if they were the marginal investors and b were equal to one.

o Low income arbitrage, where low income citizens have their government borrow at m, cut taxes, and then let the citizens invest the proceeds at $r(1-t_l)$, where t_l is the tax rate faced by low income people, gaining $r(1-t_l)-m$ in the process. Unlike the above form of arbitrage, where it is assumed that high income citizens face a high marginal rate, these low income citizens face much lower effective progressive marginal rates. They are able to make more

in after-tax returns on taxable securities than they pay on tax-exempt municipals. Gordon and Slemrod do find that in 1977 communities with low marginal tax rates borrowed more than communities with high marginal tax rates, other things equal.

How is all this changed by TRA? There are at least five provisions that could have an impact:

o The reduction in individual and corporate marginal tax rates means that the gap between r amd m should diminish. To state and local governments, this means that the tax-exempt rate should rise relative to other interest rates.

o That marginal tax rates are compressed eliminates at least one of the arbitrage channels that previously benefitted low marginal tax rate communities.

o That banks can now no longer deduct interest when they buy tax-exempt securities means that they are now less likely to hold, and abritrage, short term state and local bonds.

o That tax exempt interest is now included in the base for the corporate alternative minimum tax means that municipal bonds are now not tax-free for some banks, again reducing banks demand for short term state and local securities.

o Volume caps on private purpose tax-exempt securities should lower the supply of these.

The cut in marginal tax rates and the bank provisions should raise the tax-exempt rate relative to other rates, reducing the tax subsidy for state and local governments. This prediction has come true: according to Poterba's (1989) calculations the short term tax-exempt rate averaged .564r from 1982-85

and .685r in 1988; the long term tax-exempt rate averaged .792r from 1982-85 and .845r in 1988.[12] If banks in fact are no longer the marginal investors in the short term market, this short term rate could also become more volatile. Over time this reduced subsidy should show up in higher state and local interest costs and perhaps in reduced state and local construction spending, although given all the opportunities for financial market arbitrage it is not at all clear that m, rather than r, sets the hurdle rate for state and local physical capital formation.[13]

Secondly, the cut in the high income individual rate from .5 to .28, and in the corporate rate from .46 to .34, means that all of the gaps leading to arbitrage possibilities should either diminish or disappear altogether:

o The return to classic governmental arbitrage has fallen from r(1-.564) = .436r to r(1-.685) = .315r for short term bonds, and from r(1-.792) = .208r to r(1-.845) = .155r for long term bonds (Poterba, 1989). These declines, and the volume caps on private purpose bonds, should reduce both the reward from and the ability of governments to engage in this sort of arbitrage.

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12. In the spirit of giving all pre-TRA predictions however embarrassing, we should report that Courant and Rubinfeld (1987) speculated that by cutting back on other tax shelters, TRA "might" actually lower m relative to r. As the numbers show, it did not.

13. This could go either way, and bears further study. If state and local governments are constrained by various anti-arbitrage provisions, they would exhaust arbitrage opportunities as far as possible, and then buy tangible capital to the point where the return was m. In this case, TRA would reduce real capital formation by raising m. If the constraint was instead on the amount of debt that they could issue, they would only buy tangible capital that earned at least r, and TRA would have no effect on real capital formation.

o The return to brokerage arbitrage has fallen from .46r to .34r if done by corporations and from .5r to .28r if done by high income individuals. Even when this return was much larger, Gordon and Slemrod found little of this sort of arbitrage, perhaps because high income investors did not trust their governments as brokers, but just in case the trust grows, the opportunities are now constricted.

o The return to high income arbitage fell from r(.792-.5) = .292r to r(.845-.72) = .125r for high income personal investors in long term bonds, and by similar large amounts for other combinations of individual and corporate investors in long and short term securities. Since state and local governments play no role in this form of arbitrage, it will be hard to notice anything on state and local books, but there will, as stated, also be an implied reduction in the excess returns realized by high income taxpayers from the tax subsidy for state and local bonds.

o The return on low income arbitrage is made negative. There are now no positive taxpayers for whom $r(1-t_1)$ exceeds m. This change would seem to block out entirely this arbitrage channel and should make it difficult to replicate the Gordon-Slemrod finding that low marginal tax rate communities borrow more in the post-TRA world.

Thirdly, there is at least indirect evidence that the volume caps on private purpose securities are having an impact. There was a flood of these offerings in late 1986, after TRA was passed but before its provisions took effect, confirming again the proposition that the best place to look for impacts of taxes on behavior is in the timing of actions that groups are planning to take anyway.

In all cases credit market efficiency is improved and both the revenue losses and the social losses associated with tax arbitrage are mitigated. One must go through financial data much more carefully than we have to make definitive quantitative estimates of the gains from this side of TRA, but the gains could be sizeable.

IV. Conclusions

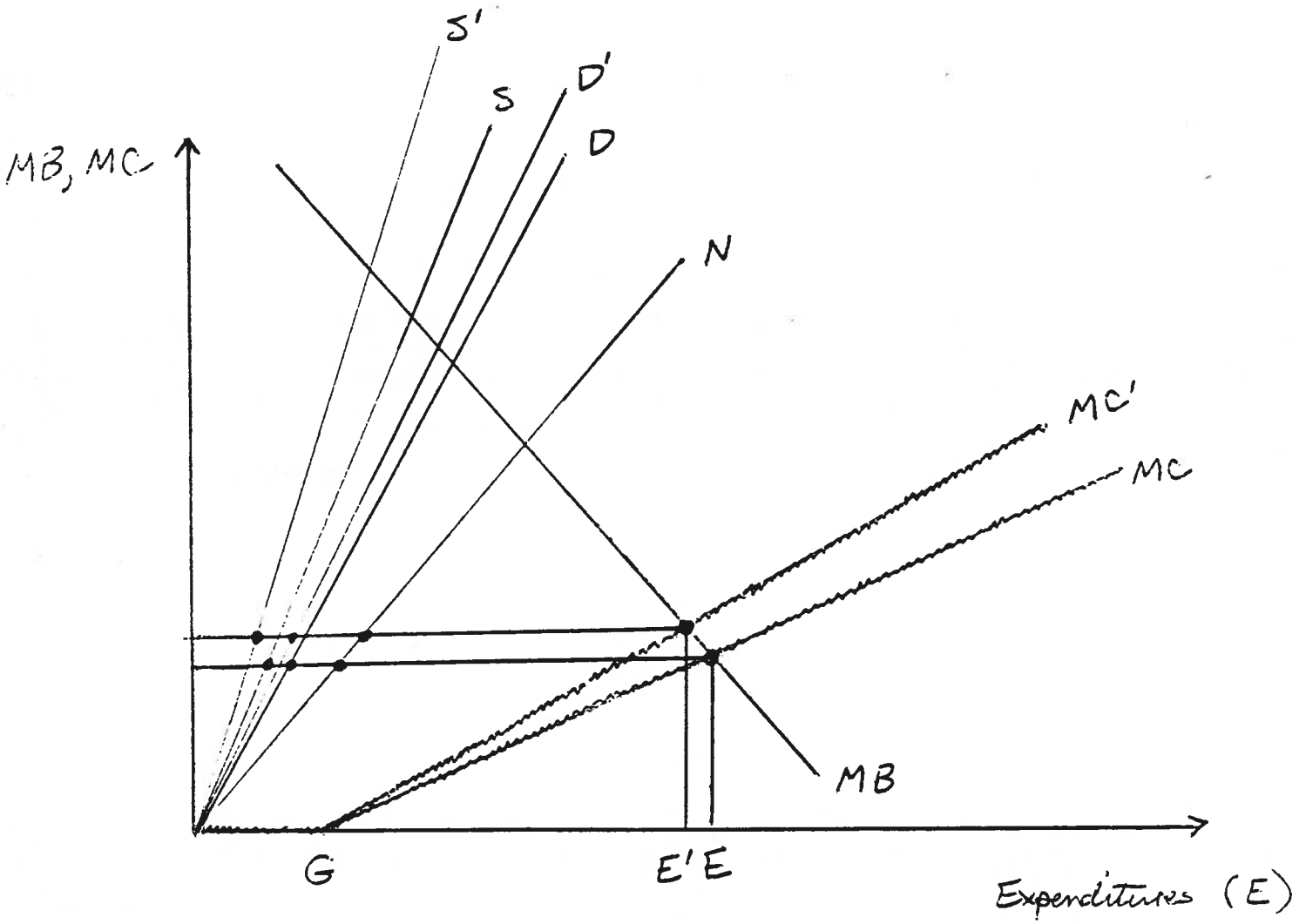
The most widely discussed aspect of the impact of TRA on state and local behavior is in its alteration of the locally borne cost of different types of taxes. Sales taxes are made much more costly and income and property taxes somewhat more costly. Despite many predictions and some econometric work that suggested that these tax changes would have large effects, as of 1988 they had not seem to have had much impact on state or local budgets as measured by out-of-sample fiscal data. Generally spending went up, contrary to both theoretical and empirical predictions, fines and user fees went down, contrary to theoretical predictions, and one has to squint very hard to make sense of the movements in deductible taxes and sales taxes. In any cases, the movements are quite small, even in those rare cases where they are in the expected direction.

Another way to search for effects of TRA is to compare activity measures for pairs of state border communities affected quite differently by TRA but quite similarly by everything else. Using border communities introduces a very large bias towards finding large effects, and even with this large bias, the effects on measures of activity such as building permits seem miniscule.

Indeed, our point estimate is that they have the wrong sign. Here, however, we are mining early data so hard that our conclusions must be quite tentative.

So what are the effects of TRA on the state and local sector? In the end we are forced back to rather subtle factors. TRA stimulated an unusual amount of state legislative activity regarding taxes, and the result will be state income taxes that generally have lower rates, broader bases, and less taxation of the poor, mimicking the federal tax changes. TRA reduced the value of the tax exemption for state and local bonds, and the result will be higher interest costs for states and localities, reduced efficiency losses, reduced arbitrage revenue losses, and perhaps reduced capital spending. TRA may have done some other things as well, but at this point nearly three years after the fact its impacts are much smaller than predicted, and there is no evidence that its long run effects will be large.

1. The Impact of TRA on State and Local Budgets



Budget Constraint: $\Delta E = \Delta D + \Delta S + \Delta N$

TRA Impact: $D' = D\left(\frac{1-a't'}{1-at}\right)$

$$S' = S\left(\frac{1}{1-at}\right)$$

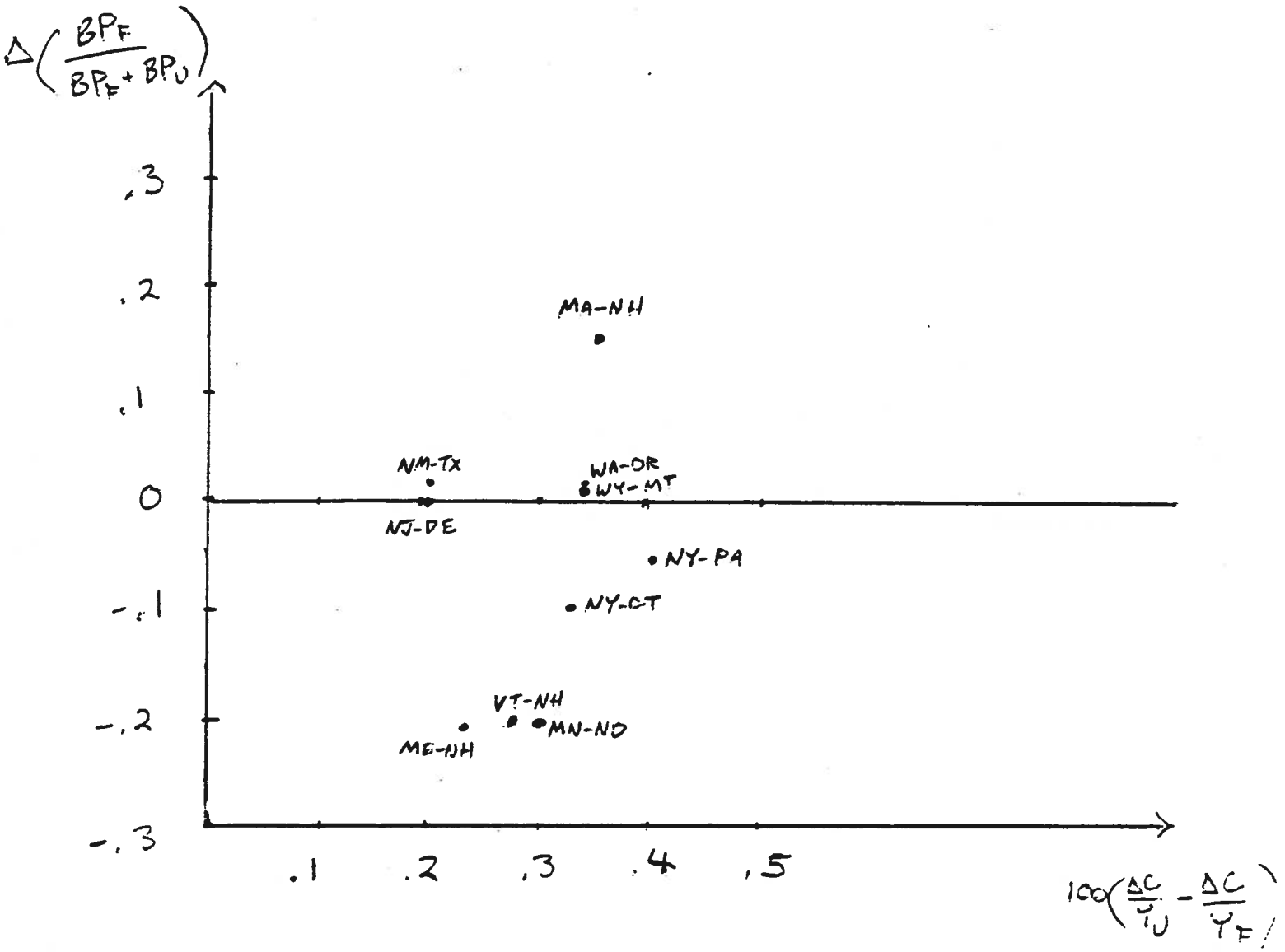
Predictions: $\Delta E < 0$

$\Delta S < 0$

$\Delta D < 0$

$\Delta N > 0$

2. Change in Building Permit Shares and Tax Costs



BP_F : Building Permits in state favored by TRA

BP_U : Building Permits in state not favored by TRA

$\frac{\Delta C}{Y_F}$, $\frac{\Delta C}{Y_U}$ defined as in equation 8) in text, 1985-86 data.

Building permit shares are based on 1987 data over the 1984-86 average.

1. Budget Constraint Model, State Governments
Annual Observations, 1960–86
Semi-first differences ($\rho = .75$)
t ratios below coefficients

Independent Variable	Direct Spending	Grants to Localities	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
	Dependent Variables						
Constant ($ Billion)	−14.38	−2.56	4.67	−2.32	−6.78	4.44	16.96
Grants & Previously Unallocated Balances	.2935 (2.3)	.3155 (4.4)	.0617 (1.2)	−.0113 (0.3)	.0382 (0.6)	−.0515 (1.1)	.3538 (2.5)
Income less Federal Withdrawals	.0473 (5.3)	.0209 (4.1)	−.0312 (8.7)	−.0121 (5.4)	−.0442 (9.2)	−.0138 (4.3)	.0332 (3.3)
Gross Price	34.54 (1.1)	23.26 (1.3)	30.30 (2.4)	23.66 (2.8)	47.63 (2.8)	−4.67 (0.4)	−154.72 (4.4)
	Residual Statistics (after correction for serial correlation)						
R^2	.9912	.9902	.9881	.9318	.9893	.9828	.5617
Standard Error ($ Billion)	2.05	1.16	0.82	0.55	1.09	0.74	2.25

2. 1980s Residuals, State Budget Model
Billions of Current $

Date	Direct Spending	Grants to Localities	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Uncorrected residuals (u)							
1980	−5.3	0	0.1	−1.9	1.0	2.0	3.8
1981	−8.1	−2.4	1.8	−1.1	2.9	0.1	6.5
1982	−4.7	−2.9	1.1	0.6	2.7	−0.6	3.6
1983	−3.0	−3.0	0.9	0.5	2.5	−0.3	1.9
1984	−3.2	−2.4	−0.7	0.5	1.5	−0.5	4.6
1985	0.0	−0.7	−1.0	0.6	−0.5	−0.8	2.1
1986	4.3	0.3	−0.1	0.2	−0.7	−1.0	−3.3
1987	9.8	2.9	−4.8	−3.4	−1.5	2.6	−6.1
1988	n.a.	n.a.	0.1	n.a.	−0.7	n.a.	n.a.
1987 value	248.6	135.2	−83.4	−25.7	−123.8	−57.7	−23.1
Predicted as of 1986[a]							
1987	3.2	0.2	−0.1	0.2	−0.6	−0.8	−2.5
1988	3.1	1.0	0.1	0.1	−0.4	−0.7	−1.0
New Shock (Uncorrected less predicted)							
1987	6.6	2.7	−4.7	−3.6	−0.9	3.4	−3.6
1988	n.a.	n.a.	0	n.a.	−0.3	n.a.	n.a.

a. Since the 1986 prediction of 1987 unallocated balances was higher than actual by $2.5 billion, this amount is allocated across all seven components according to the regression coefficients.

3. Budget Constraint Model, Local Governments
Annual Observations, 1960–86
Semi-first differences ($\rho = .75$)
t ratios below coefficients

Independent Variable	Direct Spending	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
	Dependent Variables					
Constant ($ Billion)	8.20	−8.48	−0.10	1.59	5.20	−6.42
Grants & Previously Unallocated Balances	.6600 (4.4)	.0395 (0.4)	−.0026 (0.7)	−.0312 (2.7)	−.0216 (0.5)	.3561 (2.4)
Income less Federal Withdrawals	.0097 (0.5)	−.0289 (2.3)	−.0009 (1.8)	−.0049 (3.3)	−.0176 (3.3)	.0426 (2.3)
Gross Price	151.71 (2.9)	−3.03 (0.1)	2.06 (1.5)	−0.65 (0.2)	7.53 (0.5)	−157.62 (3.1)
	Residual Statistics (after correction for serial correlation)					
R^2	.9888	.9534	.8137	.9890	.9763	.8275
Standard Error ($ Billion)	3.23	2.12	0.09	0.25	0.90	3.13

4. 1980s Residuals, Local Budget Model
Billions of Current $

Date	Direct Spending	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Uncorrected residuals (u)						
1980	−3.0	7.8	−0.2	1.0	3.7	−9.5
1981	−2.8	6.6	−0.3	0.9	3.0	−7.6
1982	−3.9	2.5	−0.3	0.2	−0.1	1.3
1983	−3.6	2.4	−0.2	0.1	−0.9	2.1
1984	−7.1	2.7	−0.2	−0.1	−0.3	4.8
1985	−6.7	0.4	−0.4	−0.5	−1.3	8.3
1986	1.9	−2.6	−0.5	−0.3	−1.6	2.9
1987	11.7	−3.6	−0.8	−0.3	−2.6	−4.6
1988	n.a.	−3.8	n.a.	−0.9	n.a.	n.a.
1987 value	368.2	−124.6	−2.2	−24.9	−62.6	29.6
Predicted as of 1986[a]						
1987	1.4	−2.0	−0.3	−0.2	−1.2	2.2
1988	−0.4	−1.6	−0.2	−0.2	−0.9	0.9
New shock (Uncorrected less predicted)						
1987	10.3	−1.6	−0.5	−0.1	−1.4	−6.8
1988	n.a.	−2.2	n.a.	−0.7	n.a.	n.a.

a. Since the 1986 prediction of 1987 unallocated balances was less than actual by $2.2 billion, this amount is deducted from all six components according to the coefficients in Table 4.

5. Recursive Budget Constraint Model, State Governments
Annual Observations, 1960–86
Semi-first differences ($\rho = .75$)
t ratios below coefficients

Independent Variable	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Constant ($ Billion)	8.52	1.20	−1.22	4.02	−12.52
Grants − Spending & Previously Unallocated Balances	.0621 (1.1)	.0037 (0.1)	.2103 (3.1)	−.0054 (0.1)	.7292 (5.3)
Income less Federal Withdrawals	−.0168 (3.7)	−.0055 (1.8)	−.0140 (2.6)	−.0165 (4.6)	.0528 (5.0)
Residual Statistics (after correction for serial correlation)					
R^2	.9847	.9087	.9894	.9817	.5885
Standard Error ($ Billion)	0.91	0.62	1.06	0.75	2.14

6. 1980s Residuals, State Model
Billions of Current $

Date	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Uncorrected residuals (u)					
1980	1.3	−0.9	1.6	1.5	−3.5
1981	2.5	−0.2	2.4	−0.3	−4.4
1982	2.6	2.1	4.7	−1.0	−8.1
1983	1.5	1.7	3.8	0	−7.0
1984	−1.4	1.0	0.9	0	−0.5
1985	−1.2	0.9	−0.8	−0.7	1.9
1986	−0.7	−0.1	−1.5	−1.1	3.4
1987	−5.0	−3.7	−0.4	2.8	6.3
1988	−1.8	n.a.	−3.6	n.a.	n.a.
1987 value	−83.4	−25.7	−123.8	−57.7	−23.1
Predicted as of 1986[a]					
1987	−0.5	−0.1	−1.2	−0.8	2.6
1988	−0.5	−0.1	−1.4	−0.6	0
New Shock (Uncorrected less predicted)					
1987	−4.5	−3.6	0.8	3.6	3.7
1988	−1.3	n.a.	−2.2	n.a.	n.a.

a. Since the 1986 prediction of 1987 unallocated balances was ~~higher~~ less than actual by $2.6 billion, this amount is ~~allocated across~~ deducted from all five components according to the regression coefficients.

7. Recursive Budget Constraint Model, Local Governments
Annual Observations, 1960–86
Semi-first differences ($\rho = .75$)
t ratios below coefficients

Independent Variable	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Constant ($ Billion)	0.55	0.36	1.84	5.48	−8.22
Grants − Spending & Previously Unallocated Balances	.2519 (3.0)	.0013 (0.3)	−.0187 (1.5)	−.0380 (0.9)	.8036 (6.5)
Income less Federal Withdrawals	−.0191 (6.3)	−.0005 (3.0)	−.0074 (16.4)	−.0179 (11.9)	.0448 (10.0)
Residual Statistics (after correction for serial correlation)					
R^2	.9659	.7848	.9865	.9764	.8743
Standard Error ($ Billion)	1.78	0.09	0.27	0.88	2.62

8. 1980s Residuals, Recursive Local Model
Billions of Current $

Date	Minus Deductible Personal Taxes	Minus Deductible Business Taxes	Minus Sales Taxes	Minus Non-Deductible Taxes	Unallocated Balances
Uncorrected residuals (u)					
1980	8.3	0.3	0.9	3.8	−13.0
1981	7.6	0.2	1.2	3.1	−11.8
1982	4.2	0.3	0.5	0.2	−4.9
1983	3.5	0.4	0.5	−0.7	−3.4
1984	1.8	0.3	0.5	−0.1	−2.2
1985	−1.3	0.1	−0.2	−1.2	3.0
1986	−3.5	0	−0.4	−1.9	6.3
1987	−2.0	−0.3	−0.5	−3.3	6.5
1988	−5.2	n.a.	−0.4	n.a.	n.a.
1987 value	−124.6	−2.2	−24.9	−62.6	29.6
Predicted as of 1986[a]					
1987	−2.6	0	−0.3	−1.5	4.7
1988	−3.1	0	−0.1	−1.0	−0.3
New Shock (Uncorrected less predicted)					
1987	0.6	−0.3	−0.2	−1.8	1.8
1988	−2.1	n.a.	−0.3	n.a.	n.a.

a. Since the 1986 prediction of 1987 unallocated balances was ~~higher~~ less than actual by $4.7 billion, this amount is ~~allocated across~~ deducted from all five components according to the regression coefficients.

9. Information on City and State Pairs

States	100 C/Y	City, COUNTY	Data
NY-PA	.73-.33=.4	Jamestown -Warren	
		Olean -Bradford	
		Bing'ton, John City, Endicott	BP
		- Dunmore, Carbondale	
MA-NH	.47-.12=.35	Lawrence, Lowell, Methuen - Nashua	BP
		ESSEX, MIDDLESEX - HILLSBOROUGH, R'HAM	CBP
		Dracut - Nashua	AV
		Lawrence, Methuen - Salem	AV
WA-OR	.61-.27=.34	Vancouver - Portland	BP,AV
WY-MT	.47-.13=.34	Sheridan -Billings	BP
NY-CT	.73-.41=.32	Rye - Greenwich, Stamford	BP
MN-ND	.51-.21=.30	Moorehead - Fargo	BP
		CLAY - CASS	CBP
UT-ID	.58-.29=.29	No places	
VT-NH	.39-.12=.27	Brattleboro - Keene	BP
		WINDHAM - CHESHIRE	CBP
MD-DE	.50-.24=.26	No data	
NY-MA	.73-.47=.26	No places	
CA-OR	.51-.27=.24	No places	
ME-NH	.34-.12=.22	Kittery - Portsmouth	BP, AV
		YORK - ROCKINGHAM	CBP
NM-TX	.42-.22=.20	Las Cruces - El Paso	BP
		Clovis - Amarillo, Lubbock	BP
NJ-DE	.43-.24=.20	Pennsville - Wilmington	BP

AV = Assessed Value
BP = Building Permits
CBP = County Business Patterns

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