Causation, Spending, and Taxes: Sand in the Sandbox or Tax Collector for the Welfare State?

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Causation, Spending, and Taxes:
Sand in the Sandbox or Tax Collector
for the Welfare State?

By KEVIN D. HOOVER AND STEVEN M. SHEFFRIN*

Causal relations between federal expenditure and taxation are analyzed using an approach based on the invariance of econometric relationships in the face of structural interventions. Institutional evidence for interventions or changes of regime combined with econometric tests for structural breaks are used to investigate the relative stability of conditional and marginal probability distributions for each variable. The patterns of stability are the products of underlying causal order. We find two distinct causal structures operating in the postwar era. Before the mid-1960's, taxes appear to cause spending. After the late 1960's, taxes and spending are causally independent. (JEL E62, E65)

"My favorite part of the tax bill is the indexing provision—it takes the sand out of Congress's sandbox." [Secretary of the Treasury Donald Regan][1]

"Republicans have served too long as tax collectors for the welfare state." [House Minority Whip Newt Gingrich][2]

A key political folktale of the 1980's was that, at the federal level, taxes cause spending. The quotation from Donald Re-

[1] Comments to Treasury staff following passage of the 1981 tax bill.

[2] From a speech chiding fellow Republicans for such behavior; *Washington Post*, 19 November 1984.

gan reflects a common belief that controlling the level of taxes would curtail the growth in government expenditure. Later in the decade, Senator Daniel P. Moynihan argued that the Reagan administration deliberately caused the deficits in order to curtail government spending. The causal priority of taxes over spending is central to these views.

In the macroeconomic literature, the most influential positive theory of deficits is Robert J. Barro's (1979) tax-smoothing hypothesis: the path of government expenditure is taken to be exogenously given, and taxes are adjusted to minimize distortions, while the budget is balanced intertemporally. In Barro's model, spending causes taxes. There are, of course, two other possibilities: spending and taxation could be jointly determined (the case of mutual causation), or they could be independently determined (the case of no causal linkage).

Causality is a slippery concept, but the causal question at stake in this discussion is clear: if it is possible to intervene to control one of the variables (spending or taxation) directly, would that yield control over the other variable? There have recently been several attempts to determine the direction of causation between spending and taxation (see Edward M. Gramlich, 1989). However, these attempts typically employ a concept of

causality that is not germane to questions of control. The usual approach is to use single-equation regressions or vector autoregressions or tests of Granger-causality, none of which addresses the question of control.[3]

The concept of causality that will be employed in this paper is based on relations of block recursion in the true, unobservable process that generates the observed data. This approach is a modification and amplification of Herbert Simon's (1953) work on causal structure.[4] Simon notices that, although it is possible to characterize causal direction in an econometric structure, data from a *single* regime underdetermine the admissible causal structures (i.e., contradictory causal structures are observationally equivalent). Thomas J. Sargent's (1976) famous paper on observational equivalence makes a similar point, while Salih Neftçi and Sargent (1978) attempt to use data subject to regime changes to discriminate between otherwise observationally equivalent models.

Our approach is more systematic but employs essentially the same insight as Neftçi and Sargent. The idea is that the joint probability distribution of taxes and spending can be factored into conditional and marginal distributions in two ways: $D(T, S) = D(T|S)D(S) = D(S|T)D(T)$. Within a single regime, there is no true choice between these factorizations; they are observationally equivalent. However, suppose that there are interventions in the process governing taxation; then, one would expect both $D(T)$ and $D(T|S)$ to change. Now, if spending causes taxes, such an intervention should leave $D(S)$ invariant, while changing $D(S|T)$.[5] Similarly, mutatis

mutandis, if taxes cause spending, $D(T)$ should be invariant to interventions in the spending process. This point has some intuitive appeal, but it will be explained in greater detail below.

Pragmatically, the problem in employing this idea as a means of inferring causal relations is that the underlying data-generating process is unobservable. The usual econometric response to this fact is to derive a (causally ordered) theoretical structure from deep principles of economic theory and to impose it upon the data at the time of estimation. This is the textbook approach to econometric identification. The problem with the case of taxing and spending, as with many other economic examples, is that perfectly respectable economic theories generate competing causal orderings. The problem then becomes not one of estimating a given causally ordered model, but of using the data to discriminate between alternative classes of models.

Our approach will be to use a priori information about the structure and institutional history of the budget and tax processes to identify potentially important changes or interventions in the processes governing taxation (e.g., major tax reforms) and expenditure (e.g., foreign wars) separately. Such an institutional history should suggest which periods, if any, are tranquil. Within such periods, observational equivalence should be the order of the day. Outside of these periods, it should be possible to identify interventions as structural breaks in regression equations representing the appropriate conditional and marginal distributions. A central practical problem is to isolate genuine structural breaks, for regression equations may break down simply because they were misspecified. We employ the specification search techniques of David F. Hendry and his colleagues to improve the chances of obtaining correctly specified regressions within our tranquil periods and

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[3] On the lack of informativeness of tests of Granger-causality with respect to controllability, see Clive W. J. Granger (1980 pp. 351).

[4] Also see Simon and Yumi Iwasaki (1988). Hoover (1990) relates Simon's approach to the philosophical literature and gives a general defense of its soundness. Hoover (1991) applies this approach to the problem of the causal direction between money and prices.

[5] If the nature of the relationship between spending and taxes is such that there are no cross-equation

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restrictions in the data-generating process, then it is also true that $D(T|S)$ would be invariant to interventions in the spending process (see Section II).

thus of identifying genuine structural breaks out of sample.

I. Models and Causal Orderings

A. Spend and Tax

There are four possible classes of causal orderings between taxes and spending: spending may cause taxes, taxes may cause spending, taxes and spending may be mutually determined, or taxes and spending may be causally independent. To illustrate our ideas about causal orderings, let us consider Barro's (1979) model as an illustration of a model in which spending causes taxes. Our treatment follows Sargent (1987).[6]

Model A: A Model of Tax Smoothing.— The government is assumed to have rational expectations and to take the path of government spending ($\{G_t\}$, $t = 0, 1, \ldots, \infty$) to be exogenous. The government chooses the path of taxes $\{(T_t)\}$ to minimize its expected costs of collection, which are assumed to be quadratic:

$$\min E \sum_{t=0}^{\infty} \beta^t \left[\mu_1 T_t + \tfrac{1}{2} \mu_2 T_t^2 \right] \qquad 0 < \beta < 1$$

subject to

$$B_{t+1} = R[B_t + G_t - T_t]$$

B_t bounded for all t

B_0 given

where $R = 1 +$ the rate of interest > 1.

A necessary condition on the time path of taxes for an optimum is the Euler equation:

$$(1) \qquad E_t T_{t+1} = -\alpha + (\beta R)^{-1} T_t$$

$$\alpha = \mu_1 \left[1 - (\beta R)^{-1} \right] \big/ \mu_2.$$

Assuming that $\beta R = 1$, this condition states that taxes follow a random walk.

To calculate an explicit expression for taxes, it is necessary to specify the stochastic process for government spending. Let this process be given by

$$(2) \qquad G_t = g + g(L)\varepsilon_t$$

where $g(L)$ is a polynomial in the lag operator (L) and ε_t is white noise. Sargent shows that, when $\beta R = 1$, this implies that the tax process is given by

$$(3) \quad T_{t+1} - T_t = (1 - 1/R) g(R^{-1}) \varepsilon_{t+1}.$$

As a specific example, let $g(L) = 1/(1 - \delta L)$. The joint government spending and tax processes are then given by

$$(4a) \quad G_{t+1} = g + \delta[G_t - g] + \varepsilon_{t+1}$$

$$(4b) \quad T_{t+1} = T_t + [(R-1)/(R-\delta)]$$
$$\times [G_{t+1} - \delta G_t + (\delta - 1)g]$$

where (4b) is derived by using (4a) to eliminate ε_{t+1} from (3).

B. Causal Order

Let us now consider the causal ordering of the system (4). The discussion here is derived from the analysis in Hoover (1990). System (4) is composed of three types of causal factors: the parameters (g, δ, and R), the variables (G and T), and the random shock (ε).[7]

The parameters represent the scope for interventions in the system. For example, a fundamental change of government expenditure policy might be represented by a change in g or δ. Such a change might represent the deliberate control of a single policymaker or a shift in the balance of influence among the agents who jointly control the process.

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[6]This version of the tax-smoothing model is very tightly parameterized. William Roberds (1991) and Lars Peter Hansen et al. (1991) discuss tests of intertemporal budget constraints in more general models. These tests, however, do not shed any light on causal issues.

[7]The parameters of the government's cost function should also count as parameters of the final model. They vanish in system (4), however, because of the auxiliary assumption that $\beta R = 1$.

A causal ordering is a relationship among the variables. Informally, if the value of a variable, X, can be determined in a subsystem of the complete system that does not involve the variable Y, but Y can only be determined once the value of X is known, then X causes Y. In a linear system with no cross-equation restrictions, X causes Y if and only if X is determined in a block that is recursively ordered ahead of Y.

System (4) is interesting partly because it involves a cross-equation restriction. The definition of causal ordering then must take account of the relationship between the parameters of subsystems as well. The technical condition which must now be added if X is to cause Y is that the parameters of the subsystem in which X is determined must form a *proper* subset of the parameters of the system (see appendix in Hoover [1990]). The essential insight remains the same: given the values of the parameters in the subset that determines X, changes in the value of Y due to changes in other variables causing Y or other parameters in the system determining Y must leave X unaltered if X is to be a cause of Y and not Y of X.

Ignoring the role of ε_{t+1} for a moment, it is clear that, in system (4), G causes T. T does not appear in equation (4a), while G, both current and lagged, does appear in (4b). Further, the parameters of (4a), $\{g, \delta\}$, form a proper subset of the parameters of the system, $\{g, \delta, R\}$. Thus, if g and δ are fixed, G_{t+1} is determined, while T_{t+1} is not. Any variation in R will change the value of T_{t+1}, but will leave G_{t+1} unaffected. T, therefore, does not cause G.

The random shock, ε_{t+1}, presents a problem of interpretation. If a unified authority could set the desired level of government expenditure, then (4a) would represent a policy-reaction function. If, further, the authority deliberately randomized its policy, it would have direct control over the parameters governing the distribution of ε_{t+1}. These should in turn be added to the set of parameters already identified and should be dealt with accordingly. The criteria for G causing T would still be met. On the other hand, ε_{t+1} may represent an implementation error over which the authorities have

no deliberate control. Alternatively, we prefer to interpret equation (4a) as not being a reaction function. In either case, ε_{t+1} may then belong to what Hoover (1990), borrowing a term from the philosophical literature, calls "the causal field"—roughly, background conditions which may be taken as given.

C. *Tax and Spend or Spend and Tax*

Barro's model (Model A) illustrates one causal ordering, but there are other models with equally sound theoretical underpinnings that illustrate alternative causal orderings.

Model B: A Model of Expenditure Smoothing.—By reversing the roles of spending and taxes in Model A, we obtain a model in which taxes are exogenous and the path of spending is "smoothed" to minimize distortions. This model corresponds to the "sand-in-the-sandbox" remark of Donald Regan that heads this essay: taxes cause spending.

Model C: A Double-Sided Cost–Benefit Model.—Models A and B recognize only the costs due to distortions of either taxation or spending, but not both. Spending and taxation may, however, impose costs simultaneously. Assume that welfare is decreasing in taxation at an increasing rate and also decreasing in the outstanding stock of debt, and assume that welfare is increasing in spending but at a decreasing rate. Marginal benefits from spending are uncertain, as are marginal costs of taxation. Spending and taxes are chosen to maximize expected welfare. To be concrete, let the problem be

$$\max_{T_1, G_1} E_0 \left\{ \left(\varepsilon G_1 - \tfrac{1}{2} b G_1^2 \right) \right.$$
$$\left. - \left(\eta T_1 + \tfrac{1}{2} e T_1^2 \right) - \tfrac{1}{2} B_1^2 \right\}$$

where $B_1 = R(B_0 + G_1 - T_1)$, B_0 is given, and ε and η are white-noise random shocks with means $\bar{\varepsilon}$ and $\bar{\eta}$.[8]

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[8]Certainty equivalence holds in this model. It is easily adapted to more interesting stochastic environments.

The levels of spending and taxes can be selected by setting expected marginal costs equal to expected marginal benefits. Thus, the first-order conditions are

$$(5a) \qquad \bar{\varepsilon} - bG_1 - R^2(B_0 + G_1 - T_1) = 0$$

$$(5b) \qquad -\bar{\eta} + eT_1 + R^2(B_0 + G_1 - T_1) = 0.$$

System (5) is clearly simultaneous. Causation is mutual between T_1 and G_1.

Model D: A Constant-Share Model.— Rather than following any of the three optimizing schemes represented in Models A–C, taxes and spending may be set by rules of thumb as fixed shares of GNP. The target shares need not be coordinated. For example, let

$$G = aY + \varepsilon$$

and

$$T = bY + \eta$$

where $Y = $ GNP and ε and η are white-noise random shocks representing implementation errors. Dividing through by Y yields

$$(6a) \qquad G/Y = a + \varepsilon'$$

$$(6b) \qquad T/Y = b + \eta'$$

where ε' and η' are ε and η scaled by GNP. System (6) clearly shows that the rates of government spending and taxation are causally independent. Interventions represented by changes in a do not affect T/Y, and interventions represented by changes in b do not affect G/Y. Yet how far G and T can drift apart in the long run is governed by the difference between a and b, and how far they can drift apart in the short run is governed by the variances of ε and η.[9]

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[9]Models C and D need not satisfy the intertemporal budget constraint as conventionally measured. Testing whether the intertemporal budget constraint is satisfied in this sense is an active area of investigation (see e.g., Bharat Trehan and Carl E. Walsh, 1988).

II. An Inferential Scheme for Causal Direction

A. *The Problem of Causal Inference*

The fundamental problem of causal inference is that, although we can characterize causal order within models such as those in Section I, we cannot observe causal order directly from the data. If we were willing to suppose that one of the models of Section I were correct, then, providing that identification criteria are met, we could estimate its parameters. However, the models in Section I are just illustrations; the details of the underlying models are too arbitrary to provide *credible* identifying restrictions.

Observational equivalence tells us that we will not be able to discriminate among different causal orderings on a single set of data. However, if the data-generating process is subject to interventions of the right sort, with each intervention defining a change in regime, it may be possible to determine which causal structures are consistent with the observed data.

B. *A Simple Example*

Consider a simple rule-of-thumb model in which government expenditure causes taxes:

$$(7) \qquad T = \alpha G + \varepsilon \qquad \varepsilon \sim \mathcal{N}(0, \sigma_\varepsilon^2)$$

$$(8) \qquad G = \beta + \eta \qquad \eta \sim \mathcal{N}(0, \sigma_\eta^2)$$

where $\mathcal{N}(\cdot, \cdot)$ indicates a normal distribution characterized by its mean and variance; and $\text{Cov}(\varepsilon, \eta) = 0$, $E(\varepsilon_t \varepsilon_s) = 0$, and $E(\eta_t \eta_s) = 0$ for $t \neq s$.

Assume that equations (7) and (8) describe the true, but unobservable, data-generating process. The problem is how to use the data to discriminate between "G causes T" and any alternative causal ordering when G in fact causes T but when only G and T can be observed.

The reduced forms of equations (7) and (8) are

$$(9) \qquad T = \alpha\beta + \alpha\eta + \varepsilon$$

$$(10) \qquad G = \beta + \eta.$$

Equations (9) and (10) describe the joint probability distribution of T and G. Elementary statistical theory tells us that such a joint distribution can be partitioned into a conditional distribution and a marginal distribution in two ways:

$$D(T,G) = D(T|G)D(G)$$

$$= D(G|T)D(T).$$

Standard formulas can be applied to compute these distributions from equations (9) and (10) (see e.g., Alexander M. Mood et al., 1974 Ch. 10 [section 5]):

(11) $D(T|G) = \mathcal{N}(\alpha G, \sigma_\varepsilon^2)$

(12) $D(G) = \mathcal{N}(\beta, \sigma_\eta^2)$

(13) $D(G|T) = \mathcal{N}\left(\dfrac{\alpha \sigma_\eta^2 T + \beta \sigma_\varepsilon^2}{\alpha^2 \sigma_\eta^2 + \sigma_\varepsilon^3}, \dfrac{\sigma_\eta^2 \sigma_\varepsilon^2}{\alpha^2 \sigma_\eta^2 + \sigma_\varepsilon^2} \right)$

(14) $D(T) = \mathcal{N}\left(\alpha\beta, \alpha^2 \sigma_\eta^2 + \sigma_\varepsilon^2 \right).$

The parameters of the tax process are α and σ_ε^2, and the parameters of the government-spending process are β and σ_η^2. Now suppose that we have some way of assigning interventions not to particular parameters (for we assume that the actual data-generating process cannot be observed), but to one or the other of these two processes. For example, suppose that a war changes fundamentally the level or the variability of the government-spending process; then, either β or σ_η^2 changes. In either case, $D(G|T)$ and $D(G)$ will change, as is to be expected; but notice that $D(T)$ will also change and, crucially, that $D(T|G)$ will remain invariant. Suppose on the other side that a major tax reform alters either α or σ_ε^2. In either case, $D(T|G)$ and $D(T)$ will change; but notice that $D(G|T)$ will also change and, crucially, that $D(G)$ will remain invariant. The partition $D(T|G)D(G)$ is stable in the sense that interventions in the tax process or interventions in the spending process leave the other process unaffected. The partition

$D(G|T)D(T)$ is not stable in this sense. The difference arises from the fact that government spending causes taxes in the true, underlying data-generating process. Had taxes caused government spending in the data-generating process, these results would, of course, have been reversed.

This suggests a general strategy for identifying causal orderings. First, if it is possible to use historical and institutional knowledge to determine periods in which there are no important interventions in either the tax or the spending processes, regression equations corresponding to each of the conditional and marginal distributions in equations (11)–(14) can be estimated and should show stable coefficients. Second, if we can then identify periods in which there are interventions clearly associated with the spending process and periods in which there are interventions clearly associated with the tax process, we can check the patterns of relative stability of the alternative partitions and thereby determine which causal ordering (if any) is consistent with the data.

C. A Complication

The data-generating process just examined is particularly simple. It is analogous to Model D above, in which the parameters of each process are distinct. Models A, B, and C, however, involve cross-equation restrictions. Consider a data-generating process identical to equations (7) and (8) above, except that $\alpha = \varphi + \beta$. Now, α is no longer a true parameter according to the definitions in Section I. The new model is the analogue of Barro's model (Model A) above in which spending causes taxes. A parameter such as β (or δ in Model A) which appears both in the equations describing taxes and in those describing spending represents a cross-equation restriction commonly introduced through the operation of rational expectations. As we saw in Model A, causal order may remain well defined even with such a cross-equation restriction. However, now an intervention that shifts the mean of the spending process, β, will change $D(T|G)$, which had been invariant

in the previous example. Notice, however, that $D(G)$ remains invariant to changes in the parameters of tax process (φ or σ_ε^2). This remains the basis for causal inference.[10]

D. Changes in Policy

In his critique of econometric policy evaluation, Robert E. Lucas (1976) employed two different types of examples. In the first set of examples, he examined the consequences of econometric relations (e.g., the consumption function) when the driving stochastic processes changed. In these examples, agents do not form expectations about the changes in stochastic processes. In the example of the investment credit, however, Lucas did allow agents to form probability distributions over the enactment and repeal of the credit.

Lucas's examples point to two different approaches to policy analysis. The example of the investment credit suggests that policy actions are to be viewed as realizations from a fixed probability distribution, while his other examples suggest that policy actions are shifts of the parameters governing the distribution. We believe that, in principle, both characterizations are useful: for example, many of the frequent tax bills of the 1970's seem on their faces to be "technical" and routine, so that we are not surprised not to find breaks in the stochastic process for revenues corresponding to them, while some major tax bills and wars seem to be nonrecurrent events over which agents find it difficult to form coherent and consistent

probability distributions.[11] Although we regard our characterization of policy as reasonable and commonplace, we recognize that it is controversial.[12]

E. Changes in the Causal Field

Causal ordering can be thought of as a property of the relationship between variables that is invariant to interventions in the parameters of the data-generating process. This supposes the stability of standing or background conditions—what we earlier called the "causal field." The examples given so far presume that interventions in the tax or spending processes can be described simply as changes in the values of the parameters of the data-generating process. It is also possible, however, that there may be changes so disruptive that the causal ordering itself is changed. In such a case, we would expect to find not merely structural breaks corresponding to changes in the values of the estimated coefficients in the regressions of the alternative partitions, but qualitative changes in the specification of those regressions.

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[10] For the case of money and income, David F. Hendry (1988) uses the difference between a data-generating process with an expectational mechanism that induces cross-equation restrictions and a rule-of-thumb model to reject rational expectations in the demand for money. He shows that it is possible for U.K. data to specify a money-stock equation which remains invariant to interventions in the income process. This would only be possible if there were no cross-equation restrictions in D(money|income). Hendry's test discriminates between rule-of-thumb and expectational models but presupposes that causal direction runs from income to money.

[11] Our approach appears to be robust to models in which agents do form prior probabilities over alternative regimes, so long as the switching probabilities are low. Suppose that the data are generated by a Barro model in which r, interpreted as the average real rate of interest, could be high or low, but in which the probability of a switch is low. Suppose that we estimated our model over a "low-r" regime. Then, Ronald Reagan comes into office and there is an unprecedented switch to a "high-r" regime. Our methods would find a break when *ex hypothesi* there is no break, only a rare realization of a regime switch. Still, given the causal structure of the Barro model, the marginal distribution of government spending would be unaffected, while all other distributions would show breaks, and we would correctly infer that government spending causes taxes. If such changes in interest-rate regimes were frequent, then our statistical models would be unlikely to detect a break when a regime change actually occurred, and our approach would not, then, be informative.

[12] Hoover (1988a pp. 193–7, 1990 pp. 9–10) documents the debate and defends the present view against that of Thomas F. Cooley et al. (1984a,b), who regard policy as always governed by a fixed probability distribution.

TABLE 1—A CHRONOLOGY OF TAXING AND SPENDING INTERVENTIONS

Year	Events
1950	Korean War begins; Excess-Profits Tax enacted
1951	Individual income taxes raised for war finance; Korean War truce talks begin (February)
1953	Korean War truce signed (July), war taxes removed (December)
1964	Tonkin Gulf Resolution (August); major tax cut
1965	Major troop buildup in Vietnam begins (July)
1968	Tax surcharge enacted
1969	Surcharge removed; major tax bill; Vietnam troop withdrawals begin
1970	Buildup of entitlements and transfer payments begins
1975	Tax rebate
1976	Tax Reform Act
1978	Revenue Act
1980	Entitlements stabilize
1981	Reagan tax cuts
1982	Cut in budget share of nonmilitary purchases
1986	Tax reform

III. A Chronology of the Tax and Spending Processes

The method of causal inference described in Section II requires that both stable periods and interventions that can be clearly associated with the tax and spending processes be identified. A chronology is summarized in Table 1. It begins in 1950 and runs until the first quarter of 1989. This corresponds to the sample period for data used in the empirical investigation in Section IV. The chronology was prepared by examining standard works on government policy in the political-science literature (e.g., John F. Witte, 1985) in advance of econometric examination of the data. We report only what appeared to us to be the most significant events. There was no guarantee that these would prove to be significant in our econometric investigations or that other events would not be missed.

The history of spending is straightforward. Two wars, Korea and Vietnam, dominate the spending chronlogy. Their effective starting and ending dates are given in Table 1. Two other major events are the rapid growth of entitlements in the 1970's (from 6.2 to 10.2 percent of GNP) and the change in the composition of federal spending toward military spending in the early 1980's.

The history of taxation is somewhat more complicated. Tax bills changing the rates and rules under which taxes are collected came fast and furious throughout the post-war period. However, only a few of these bills represent changes of sufficient magnitude or new departures in the character of the tax process to count as important interventions.

In response to the revenue needs associated with the Korean War, two tax bills were enacted: the excess-profits tax of 1950 and the Revenue Act of 1951. The former was a corporate-tax increase, while the latter raised individual income-tax rates. The excess-profits tax was scheduled to expire on June 30, 1953; however, legislation extended this expiration date until December 31, 1953, which coincided with the expiration date for the individual-rate increases in the Revenue Act of 1951.

The next major change in the tax code is the tax cut of 1964. This was partially reversed in 1968 with the introduction of a 10-percent tax surcharge aimed at cooling excess demand in the economy. The tax surcharge was removed in 1969, and a major tax bill was introduced. A series of tax bills adjusting or modifying the 1969 act were introduced during the remainder of Richard Nixon's presidency.

During the Gerald Ford and Jimmy Carter years, there were several other tax acts. There were tax rebates in 1975, the Tax Reform Act of 1976, and the Revenue Act of 1978. All these acts featured many complex structural changes and, typically, some

individual- and corporate-tax reduction. The revenue changes, however, were relatively small. From 1974:4 to 1980:4, for example, tax revenues grew (including these cuts) at approximately the rate of $45 billion per year. The Joint Tax Committee estimates the effects of the cuts at about $6 billion per year (Witte, 1985 p. 164). We tentatively regard these as minor relative to the tax bills associated with the Korean War, the John F. Kennedy administration and the Ronald Reagan administration. Moreover, they should be viewed as partially predictable mechanisms to reduce the large revenues that would flow in automatically from the interaction of the progessive tax system with the lack of indexation for inflation.

On entering office in 1981, Reagan sought a 30-percent cut in income-tax rates, finally agreeing with Congress for a 25-percent cut phased in over three years.[13] The central tax reform of the Reagan presidency was the 1986 tax bill. Its provisions included a simplification and reduction of rates, indexing of tax brackets, and abolishing the distinction between capital gains and ordinary income.

Comparing the histories of taxes and spending, we are able to identify two periods in which there do not appear to be any major interventions in either process: 1954–1963 and 1974–1979. These tranquil periods are the starting place for the empirical investigation of the next section.

IV. Evidence

A. The Data

Where possible, data are taken from the National Income and Product Accounts. As a first approach, we operate at a highly aggregated level. Taxes are represented by total federal government receipts. Expenditures are total federal outlays net of interest payments. We assume that the levels of

taxes and spending are set relative to potential output. Therefore, all variables are scaled by potential output in order to remove common trends.[14] Since we are looking for discretionary changes in policy, we would like to abstract from the effects of inflation and cyclical movements of GNP on receipts and from the effects of automatic stabilizers on expenditure. Therefore, we regressed receipts scaled by potential output on a constant, the current and three lags of the GNP gap ($= 1 - [\text{GNP}/\text{potential GNP}]$), to capture cyclical effects and on the current and three lags of inflation ($= \Delta \log[\text{GNP}]$ price deflator). The residuals from this regression, RPOF (receipts scaled by potential output and filtered) comprise our tax variable. Similarly, our expenditures variable XPOF (filtered expenditures scaled by potential output) is obtained by regressing expenditures scaled by potential output on a constant and the current and three lags of the GNP gap.[15]

B. Characterization of Distributions

The first step in empirically implementing the strategy outlined in Section II is to characterize the conditional and marginal distributions in the tranquil periods identified in Section III. If they have been correctly identified as tranquil, regressions estimated over these periods should be invariant (i.e., show stable estimated coefficients). To obtain well-specified regressions, we use the general-to-specific modeling technique of Hendry and Jean-François Richard (1982).[16]

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[13]The 1981 law also included provisions to index tax brackets at a later date. This explains Donald Regan's remark about removing the sand from Congress's sandbox. These indexing provisions were not implemented.

[14]We adopt the Federal Reserve's measure of potential output, which is derived from applying a Kalman filter to an Okun's-law relationship and accounts for changes in productivity (Peter K. Clark, 1983). While there are other estimates of potential output, this measure is widely used, and we know of no clearly superior measure.

[15]These adjustments are similar to those used by George M. von Furstenberg et al. (1986 pp. 180–3). Exact sources and definitions are given in a working-paper version of the paper, available from the authors upon request.

[16]For general defenses of this technique, see Hendry (1983, 1987), Michael McAleer et al. (1985), Christopher L. Gilbert (1986), and Peter C. B. Phillips (1988).

To apply the general-to-specific modeling technique, we begin with a deliberately overfitting general dynamic form, a regression of the *level* of the dependent variable on a constant, five lags of the dependent variable, and the current and five lags of the other variable, where applicable. Sometimes we refer to a reparameterized equivalent form of these regressions, which regresses the first *difference* of the dependent variable on a constant, four lags of the differenced dependent variable, the level of the dependent variable lagged once, and (where applicable) the current and four lags of the first difference of the other variable and the level of the other variable lagged once. Table 2 presents some summary information about both of these equivalent forms of the unrestricted distributed-lag regressions. Coefficient estimates are not reported because they are difficult to interpret and because we are ultimately seeking a more parsimonious representation of the data. The estimation periods reported are shorter than the tranquil periods identified in Section III by four periods on each end (or only two periods when degrees of freedom are scarce) to allow for an out-of-sample check of coefficient stability within the tranquil period. For no regression can we reject the null hypotheses of normal residuals, no residual autocorrelation (usually up to fourth order), and no autoregressive conditional heteroscedaticity (again usually up to fourth order).

First, consider regression (i) in Table 2. The low Durbin-Watson statistic on RPOF suggests that RPOF is nonstationary. Differencing once seems to render RPOF stationary as indicated by the Durbin-Watson statistic of 2.43 on ΔRPOF. The F statistic testing the explanatory power of the regressors, which is 10.17 ($d.f. = 11, 20$) for the level drops to 1.08 for the difference. This suggests that none of the regressors has explanatory power for ΔRPOF. The simple

fact that the standard deviation of ΔRPOF is already as low as the standard error of regression for regression (i) further bears this out. Therefore, a random walk seems to be a likely specification. Regression (iv) in Table 3 estimates the random-walk specification. The diagnostic statistics reported show that we cannot rule out that the residuals of this regression are normal, conditionally homoscedastic white noise. The Chow test reported in Table 3 splits the estimation period in two to check for stability within samples.[17] The random-walk specification is nested in both regressions (i) and (ii) in Table 2, and we cannot reject the null hypothesis that it is a valid restriction of them both. Thus, a model that is marginal of all expenditure variables and of all but last period's receipt variable appears to characterize RPOF adequately.

In the second tranquil period, the story is somewhat different. The standard deviation of the level, RPOF, is nearly the same as the standard error of regression [regressions (iii) and (iv) in Table 2]. According to the F tests, none of the independent variables has any explanatory power for the level or the difference of RPOF. RPOF then appears to be stationary, rather than a random walk. Regression (v) in Table 3 regresses RPOF on a constant and RPOF lagged once. It appears to have stable coefficients and normal, homoscedastic white-noise residuals. It cannot be rejected against regressions (iii) or (iv) in Table 2. The fact that both the constant and the lagged level term are statistically insignificant confirms the stationary specification. Clearly, then, the specification of the receipts process changes markedly between the first and second tranquil periods, although expenditures are not involved in either period. Even though the coefficient on $RPOF_{-1}$ is insignificant, regression (v) in Table 3 is specified so that it nests both the random-walk and the white-noise specifications; this may be helpful in

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Adrian R. Pagan's (1987) survey compares it with the approaches of Edward E. Leamer (1978) and Christopher A. Sims (1980). For applied examples, see Andrew K. Rose (1985) and Hoover (1988b).

[17]This is Gregory C. Chow's (1960 pp. 595–9) second test, the one commonly described in elementary econometrics texts; it is more powerful than Chow's first test, but it requires that each subsample have enough degrees of freedom to be estimated separately.

TABLE 2—UNRESTRICTED DISTRIBUTED-LAG REGRESSIONS

| Regression | Dependent variable | Descriptive statistics | | | Regression summary statistics | | | | | |
		Mean	Standard deviation	Durbin-Watson	Standard error of regression	Sum of squared residuals	Normality $\chi^2_{[2]}$	AR(·) $F(·,·)$	ARCH(·) $F(·,·)$	Explanatory power $F(·,·)$
(i) Receipts conditional, 1955:1–1962:4	RPOF	0.0014	0.0062	0.24	0.0030	0.00018	0.30	0.17 (4, 16)	0.35 (4, 12)	10.17 (11, 20)
	ΔRPOF	0.0005	0.0030	2.43						1.08 (11, 20)
(ii) Receipts marginal, 1955:1–1962:4	RPOF	−0.0014	0.0062	0.24	0.0030	0.00023	1.05	0.17 (4, 22)	0.61 (4, 18)	21.96 (5, 26)
	ΔRPOF	0.0005	0.0030	2.43						1.38 (5, 26)
(iii) Receipts conditional, 1974:3–1979:2	RPOF	−0.0030	0.0031	1.51	0.0030	0.00007	0.90	0.73[a] (1, 9)	0.01[a] (1, 8)	1.08 (11, 8)
	ΔRPOF	0.0000	0.0038	2.58						2.00 (11, 8)
(iv) Receipts marginal, 1975:1–1978:4	RPOF	−0.0034	0.0031	1.54	0.0034	0.00012	0.31	6.56 (1, 9)	3.34 (1, 8)	0.37 (5, 19)
	ΔRPOF	0.0002	0.0039	2.81						1.82 (5, 10)
(v) Expenditure conditional, 1955:1–1962:4	XPOF	0.0360	0.0160	0.37	0.0077	0.0012	0.37	0.18 (4, 16)	0.26 (4, 12)	3.08 (11, 24)
	ΔXPOF	−0.0005	0.0097	1.75						2.67 (11, 20)
(vi) Expenditure marginal, 1955:1–1962:4	XPOF	0.0360	0.0160	0.37	0.0080	0.0017	0.27	1.61 (4, 22)	0.19 (2, 93)	18.60 (5, 26)
	ΔXPOF	−0.0005	0.0097	1.75						3.85 (5, 26)
(vii) Expenditure conditional, 1974:3–1979:2	XPOF	−0.0280	0.0190	0.24	0.0098	0.00077	0.99	5.75[a] (1, 9)	1.92[a] (1, 8)	5.50 (11, 8)
	ΔXPOF	0.0001	0.0095	1.48						0.88 (11, 8)
(viii) Expenditure marginal, 1975:1–1978:4	XPOF	−0.0220	0.0160	0.30	0.0090	0.00082	2.40	0.07 (1, 9)	0.03 (1, 8)	7.33 (5, 10)
	ΔXPOF	−0.0005	0.0099	1.57						1.60 (5, 10)

Notes: All regressions were run on *PC-GIVE*, Version 5.0 (Hendry, 1989). Conditional regressions correspond to $Y_t = \alpha + \sum_{j=1}^{5} \beta_j Y_{t-j} + \sum_{k=0}^{5} \gamma_k X_{t-k} + \varepsilon_t$, and the marginal regressions correspond to $Y_t = \alpha + \sum_{j=1}^{5} \beta_y Y_{t-j} + \varepsilon_t$, where, when Y = receipts, X = taxes, and vice versa. Equivalent reparameterizations, $\Delta Y_t = \alpha + \sum_{j=1}^{4} \beta_j' \Delta Y_{t-j} + \sum_{k=0}^{4} \gamma_k' \Delta X_{t-k} + \delta_1 Y_{t-1} + \delta_2 X_{t-1} + \varepsilon_t$ and $\Delta Y_t = \alpha + \sum_{j=1}^{4} \beta_j' \Delta Y_{t-j} + \delta Y_{t-1} + \varepsilon_t$, are also reported. The Durbin-Watson statistic is calculated as $\text{DW}(X) = \sum_{t=2}^{T} (X_t - X_{t-1})^2 / \sum_{t=1}^{T} (X_t - \bar{X})^2$, where X is the dependent variable. The normality test reported is the C. M. Jarque and Anil K. Bera (1980) test for normal residuals, which is distributed as $\chi^2_{[2]}$ under the null hypothesis of normality. The F statistics reported are as follows:

AR(·) = Lagrange-multiplier test for autocorrelated residuals; the F-distribution equivalent is reported, which is distributed $F(·,·)$ under the null hypothesis of no residual autocorrelation up to the order indicated by the degrees of freedom in the numerator;

ARCH(·) = Lagrange-multiplier test for autoregressive conditional heteroscedasticity; the F-distribution equivalent is reported, which is distributed $F(·,·)$ under the null hypothesis of no autoregressive conditional heteroscedasticity up to the order indicated by the degrees of freedom in the numerator;

Explanatory power = F test distributed as $F(·,·)$ under the null hypothesis that all of the regressors are zero. Degrees of freedom are reported in parentheses.

[a] Calculated over 1974:3–1979:4 because of insufficient degrees of freedom.

TABLE 3—PARSIMONIOUS CHARACTERIZATIONS OF THE CONDITIONAL AND MARGINAL DISTRIBUTIONS

A. *Regression Equations:*

Regression	Equation
(i) Expenditures conditional, 1955:1–1962:4	ΔXPOF $= 0.21 \Delta_4$XPOF$_{-1} - 0.96 \Delta$RPOF$_{-3} - 0.40($XPOF$-$RPOF$)_{-1} + 0.016$ (-0.0005) (0.057) (0.41) (0.082) (0.0035) $[0.0097]$ $[0.094]$ $[0.51]$ $[0.140]$ $[0.0053]$
(ii) Expenditures marginal, 1955:1–1962:4	ΔXPOF $= 0.30 \Delta_3$XPOF$_{-2} - 0.40$XPOF$_{-1} + 0.015$ (-0.0005) (0.085) (0.10) (0.0041) $[0.0097]$ $[0.129]$ $[0.14]$ $[0.0051]$
(iii) Expenditures marginal, 1975:1–1978:4	ΔXPOF $= 0.30 \Delta_3$XPOF$_{-2} - 0.38$XPOF$_{-1} - 0.010$ (-0.0005) (0.10) (0.16) (0.0042) $[0.0099]$ $[0.06]$ $[0.16]$ $[0.0035]$
(iv) Receipts marginal, 1955:1–1962:4	ΔRPOF $= 0.00050$ (0.0005) (0.00054) $[0.0030]$ $[0.00054]$
(v) Receipts marginal, 1974:3–1979:2	RPOF $= 0.20$RPOF$_{-1} - 0.0026$ (-0.0030) (0.26) (0.0012) $[0.0031]$ $[0.33]$ $[0.0013]$

B. *Summary Statistics:*

Regression	R^2	Standard error of regression	Sum of squared residuals	Normality $X^2_{[2]}$	AR($\cdot$) $F(\cdot,\cdot)$	ARCH($\cdot$) $F(\cdot,\cdot)$	Chow $F(\cdot,\cdot)$	Nested UDL(5) $F(\cdot,\cdot)$
(i)	0.54	0.0069	0.0013	0.45	0.70 (4, 24)	0.18 (4, 20)	1.82 (4, 24)	0.36 (8, 20)
(ii)	0.38	0.0079	0.0018	0.68	0.32 (4, 25)	0.12 (4, 21)	2.77 (3, 26)	0.61 (3, 26)
(iii)	0.44	0.0080	0.0083	3.71	0.11 (2, 11)	0.23 (3, 7)	1.18 (3, 10)	0.05[a] (3, 10)
(iv)	0.042	0.0030	0.00029	1.01	1.36 (4, 27)	0.68 (4, 23)	0.010 (1, 30)	1.37[b] (5, 26)
(v)	0.04	0.0031	0.00013	0.61	1.22 (2, 12)	0.26 (3, 8)	0.42 (2, 12)	0.34[c] (4, 10)

Notes: All regressions were run on *PC-GIVE*, Version 5.0 (Hendry, 1989). Beneath each dependent variable, the mean is given in parentheses, and the standard deviation is given in brackets. Beneath each coefficient estimate, the standard error is given in parentheses, and the heteroscedasticity-corrected standard error is given in brackets. The normality test reported is the Jarque and Bera (1980) test for normal residuals, which is distributed as $\chi^2_{[2]}$ under the null hypothesis. The F statistics reported are as follows:

AR($\cdot$) = Lagrange-multiplier test for autocorrelated residuals; the F-distribution equivalent is reported, which is distributed $F(\cdot,\cdot)$ under the null hypothesis of no residual autocorrelation up to the order indicated by the degrees of freedom of the numerator;

ARCH($\cdot$) = Lagrange-multiplier test for autoregressive conditional heteroscedasticity; the F-distribution equivalent is reported, which is distributed $F(\cdot,\cdot)$ under the null hypothesis of no autoregressive conditional heteroscedasticity up to the order indicated by the degrees of freedom of the numerator;

Chow = Chow test of first half of sample versus second half;

Nested in UDL(5) = F test of exclusion restrictions versus corresponding unrestricted distributed-lag (UDL) model in Table 2.

Degrees of freedom are given in parentheses beneath each F statistic.

[a] F test of exclusion restrictions versus regression (vii) in Table 2, reestimated over 1975:1–1979:4 to overcome the limited degrees of freedom: $F = 0.26$ ($d.f. = 9, 8$).

[b] F test of exclusion restrictions versus regression (i) in Table 2; $F = 1.07$ ($d.f. = 11.20$).

[c] F test of exclusion restrictions versus regression (i) in Table 2; reestimated over 1975:1–1978:4: $F = 0.45$ ($d.f. = 10, 4$).

picking up the shift between the two tranquil periods when we turn to out-of-sample estimates.

Now consider the expenditure regressions in Table 2. In the first tranquil period, the Durbin-Watson statistic on XPOF [regression (v) or (vi)] is very low, while it is nearly 2 on ΔXPOF, suggesting that XPOF is nonstationary. We have already seen that RPOF is nonstationary in the first tranquil period. Together, these facts suggest that RPOF and XPOF may be cointegrated. Regression (i) in Table 3 is an error-correction specification in which XPOF must equal RPOF in the long run.[18] Regression (i) appears to be a stable regression with normal, conditionally homoscedastic white-noise errors. It cannot be rejected as a valid restriction of regression (v) in Table 2. It will serve as our characterization of the conditional expenditure distribution in the first tranquil period.

Regression (ii) in Table 3, which is marginal of all receipts variables, equally well characterizes regression (vi) in Table 2. It will serve as the marginal expenditure distribution for the first tranquil period.

As on the receipts side, behavior on the expenditures side changes markedly between the first and second tranquil periods. Although it was derived from an independent specification search, regression (iii) of Table 3 has the identical form and, aside from the sign of the constant, quite similar coefficients as regression (ii) of Table 3. It also passes the same battery of diagnostic tests. It cannot be rejected as a valid restric-

tion of regression (viii) of Table 2 at any conventional level of significance. What is more, it cannot be rejected against regression (vii) of Table 2. Receipt terms, then, seem to have no explanatory power for expenditures in the second tranquil period.

C. Observational Equivalence in Practice

At first glance, the regressions reported in Table 3 for receipts and expenditures might lead one to conclude that taxes cause spending. This is because receipts seem to be a random walk, while changes in expenditures are driven, in part, by past levels of taxes. Therefore, it may appear that taxes are the driving force in the system; but this presumption would be much too hasty. In fact, Barro's tax-smoothing model, in which spending causes taxes, can lead to estimated regressions similar to those reported in Table 3.

In the tax-smoothing model, taxes follow a random walk. Moreover, taxes will Granger-cause spending if the government uses information other than the past history of spending to forecast future spending. Thus, it would appear that our regressions are potentially consistent with a tax-smoothing world as well.

Before rushing to embrace the alternative causal ordering, spending causes taxes, we note that our regressions do not satisfy one crucial implication of the tax-smoothing model. If current government spending conveys any information about future spending, then the change in taxes should be correlated with the current change in government spending. However, current expenditure was included as a potential regressor in our model of receipts but was not statistically significant.

This discussion, nonetheless, highlights in a concrete way the general points about observational equivalence made in Section II. To make further progress in discriminating between alternative causal orderings, we turn to an analysis of the stability of the regressions reported in Table 3 in the face of interventions.

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[18]In addition to the informal indicators cited in the text, we have also performed augmented-Dickey-Fuller and Durbin-Watson tests for cointegration between XPOF and RPOF. They were inconclusive. In general, given the known low power of these tests in short samples, our informal discussion adopts about the amount of precision possible given the circumstances. In the particular case of regression (i) in Table 3, the high "t statistic" on the error-correction term (4.88) provides a direct test of the existence of a cointegrating vector between XPOF and RPOF. The correct critical values for this "t statistic" are not conventional but lie somewhere between those of the Dickey-Fuller test and those of a normal distribution; see Jeroen J. M. Kremers et al. (1989).

TABLE 4—TESTS OF STRUCTURAL STABILITY

Projection	Table 3 regression	Direction	Projection period	Max Chow test	Fluctuation test	One-step-ahead Chow test		Sequential Chow test
						Ratio	Maximum value	Break point
(i) Expenditures conditional	(i)	backward	1954:4–1950:1	1.3	1.7	0.30	3.9	1953:1
(ii) Expenditures conditional	(i)	forward	1963:1–1975:1	1.5	4.3	0.06	1.7	1965:1
(iii) Expenditures marginal	(ii)	backward	1954:4–1950:1	0.9	1.9	0.30	4.3	1953:1
(iv) Expenditures marginal	(ii)	forward	1963:1–1975:1	1.6	4.9	0.06	1.5	—
(v) Expenditures marginal	(iii)	backward	1974:4–1962:4	0.8	2.2	0.07	1.8	—
(vi) Expenditures marginal	(iii)	forward	1979:1–1989:1	0.3	0.9	0.05	2.3	1980:4
(vii) Expenditures marginal	(iii)	backward	1983:4–1979:4	0.5	0.8	0.19	3.4	1982:4
(viii) Receipts marginal	(iv)	backward	1954:4–1950:1	0.8	0.6	0.15	4.0	1954:1
(ix) Receipts marginal	(iv)	backward	1954:4–1950:1 (omitting 1954:1)	0.9	0.6	0.16	2.0	1951:1
(x) Receipts marginal	(iv)	forward	1963:1–1975:1	0.2	0.6	0.15	2.3	1964:2
(xi) Receipts marginal	(v)	backward	1974:4–1962:4	0.7	2.0	0.12	2.7	1969:1
(xii) Receipts marginal	(v)	forward	1979:1–1989:1	1.7	3.3	0.05	3.9	—
(xiii) Receipts marginal	(v)	forward	1979:1–1989:1 (omitting 1985:1 and 1985:2)	2.0	4.1	0.05	1.4	—

Notes: Test statistics are described in the text. The values of all statistics are expressed as ratios with their 5-percent critical values. For the max Chow tests and the fluctuation tests, the sample period includes the estimation period from Table 3 and the projection period. For the one-step-ahead Chow test, the ratio is the number of violations of the 5-percent critical value divided by the number of observations in the projection period. For the sequential Chow test, the break point is the date at which the Chow statistic first exceeded its 5-percent critical value.

D. Methods for Identifying Structural Breaks

There are two distinct questions relating to relative stability. Does a structural break occur at all? And, given that it occurs, exactly when? A number of formal tests have been developed to answer the first question. The second question remains less well understood and can, at present, be addressed only with informal methods.

Table 4 presents four tests of structural stability for the regressions reported in Table 3. Using recursive regression techniques (Andrew C. Harvey, 1981 pp. 54–57), each regression is projected backward and

forward from the baseline tranquil periods. The first stability test is the max Chow test. The statistic is the maximum value of the set of Chow statistics computed for every possible break point in the sample. The statistic reported in the table is scaled by the 5-percent critical value calculated by Donald W. K. Andrews (1990 p. T-1), so that a value greater than unity indicates rejection.

The second test is the fluctuation test of Werner Ploberger et al. (1989) modified according to C. James Chu (1990) to use the Euclidean rather than the infinite norm. The test compares the coefficient estimates for each recursive regression to those for

TABLE 5—SUMMARY OF STRUCTURAL BREAKS

| Expenditures | | Receipts | |
Conditional	Marginal	Marginal	Interventions
		1951:1	Korean War tax bills
1953:1	1953:1		effective end of Korean War
		1954:1	Korean War taxes removed
		1964:2	tax cut
1965:1	1965		Vietnam War buildup
		1968	tax surcharge
		1969:2	surcharge removed; major tax act
		1981:2	Reagan tax cut
	1982:4		Reagan military buildup

the whole sample. The reported statistic is the maximum value scaled by its 5-percent critical value (Chu, 1990).[19]

The third test is the one-step-ahead Chow test.[20] The statistic reported under "value" is the maximum value [scaled by the 5-percent critical value for an $F(1, T - K)$ test]. Since the test is based on the innovation errors of the recursive regression, each of the one-step-ahead Chow tests is independent (Harvey, 1981 pp. 55–6). Thus, in large samples, using a 5-percent critical value, 5 percent of the statistics should violate their 5-percent critical value. Therefore, the column labeled "ratio" reports the ratio of the number of violations to the whole number of tests in the projection period.

These three tests are properly tests of whether or not there is a break within the projection period. The fourth test, the sequential Chow test, addresses the second question: where does the break occur? The regression for the baseline period is compared to a sequence of regressions corre-sponding to the sequence of recursive regressions across the projection period. The plots of these statistics (scaled by their 5-percent critical values to maintain compa-rability) are examined visually. Instead of reproducing an indigestible mass of such plots, we summarize each one by reporting the date of the endpoint of the regression (in the direction of projection) for which the Chow statistic first violates its 5-percent crit-ical value (taken from the standard F table). Since the Chow statistics in such a sequence are not independent, this is not a formal test of the existence of a break. Informal experimentation suggests that, in conjunc-tion with more formal tests, it may nonethe-less be a useful way of dating break points.

Experimentation also suggests that the most useful method of dating structural breaks is to examine the plots of the recur-sively estimated coefficients against their standard errors. In the interest of conserv-ing space, only a few of these plots will be reported directly.[21]

E. Out-of-Sample Projections

Table 4 generally supports our identifica-tion of the tranquil periods. Only for the backward projection of the receipts process from the first tranquil period to 1950:1 [pro-jection (viii)] does the sequential Chow test indicate a structural break within the tran-

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[19]Monte Carlo studies by Andrews (1990) and Yongxin Cai (1990) indicate that max Chow tests and fluctuations tests are consistently more powerful than other tests of structural breaks, such as cumsum or cumsum-square tests. In addition to the fluctuation test with the Euclidean norm, we also examined the fluc-tuation test with an infinite norm. Both tests always indicated the same result.

[20]This test and the sequential Chow test are de-scribed in detail in Hendry (1989 p. 44). They are both based on Chow's first test (Chow, 1960 pp. 594–8), which can be used even when there are insufficient degrees of freedom to estimate regressions over two separate subsamples.

[21]The interested reader will find more, but by no means all, of these plots in the working-paper version of this paper, which is available from the authors upon request.
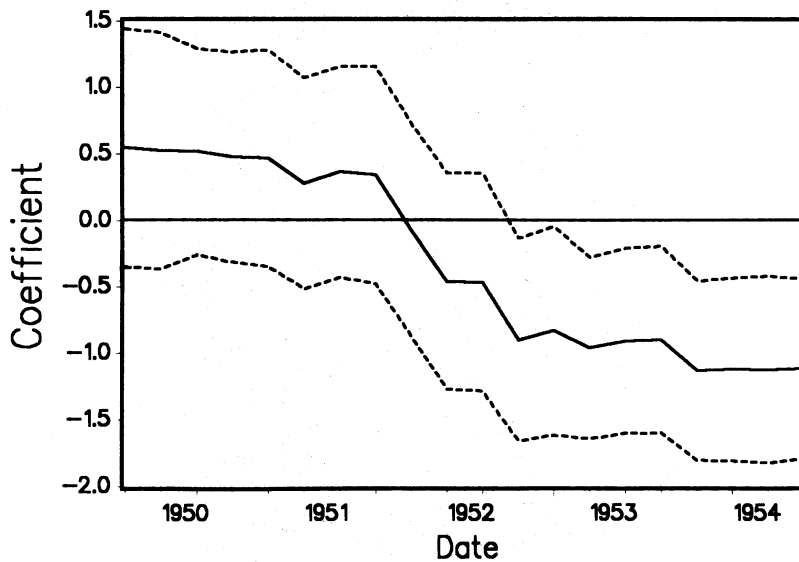
FIGURE 1. COEFFICIENT ON ΔRPOF$_{-3}$ IN PROJECTION (i):
REGRESSION (i) OF TABLE 3 BACKWARD FROM 1954:4 TO 1950:1

Note: The dashed lines represent ± 2 standard errors.

quil period. This case will be discussed in due course.

The first three columns of Table 5 report the structural breaks we identified in projecting the regressions reported in Table 3 out of sample. These results will be interpreted in Section V. The remainder of this section presents the evidence that supports the entries in Table 5. Readers who are willing to take our evidence for these structural breaks on faith should skip immediately ahead to Section V.

Interventions in the Period 1950–1954.—Projection (viii) in Table 4 presents the receipts regression (iv) of Table 3 projected backward from the first tranquil period to 1950:1. Although the max Chow and the fluctuation tests do not register any structural break, the one-step-ahead Chow test does, with the sequential Chow test locating the break at 1954:1. The Chow statistic on the one-step-ahead test is probably too large, at four times its 5-percent critical value, to be sampling error. Projection (ix), therefore, reruns projection (viii), but with 1954:1, the data of the maximum observation for the one-step-ahead statistic, dum-

mied out. Now the sequential Chow test indicates a structural break at 1951:1. The evidence thus supports two changes in the tax process: 1951:1 and 1954:1.

The break in 1954:1 appears to impinge on the tranquil period identified as 1954:1–1963:4. As we shall see in Section V, this break appears to be associated with the removal of extraordinary tax measures at the end of December 1953. It is, then, hardly surprising that the data do not pick up the break until the first quarter of 1954.

Turn now to the expenditure specification over this same period. Consider first the conditional expenditure specification, regression (i) in Table 3. Projection (i) shows that all four tests indicate a break. The sequential Chow test dates a break at 1953:1. The coefficient plot for ΔRPOF$_{-3}$ (Fig. 1) shows that the behavior before 1952:2 is strikingly different from the behavior after 1952:4, even given fairly wide standard-error bands.[22] The coefficient plot for the

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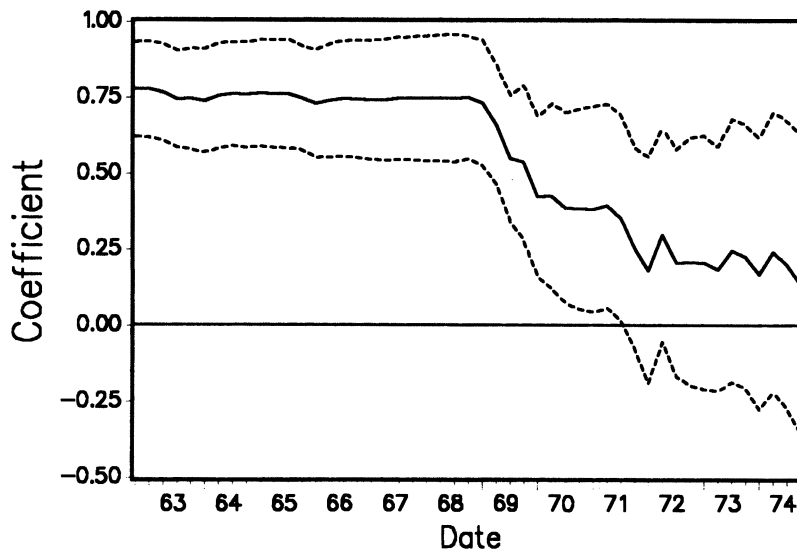[22]One should recall that these are not plots of forecasts, but of successive reestimations. The stan-

FIGURE 2. COEFFICIENT ON RPOF$_{-1}$ IN PROJECTION (xi):
REGRESSION (v) OF TABLE 3 BACKWARD FROM 1974:4 TO 1962:4

Note: The dashed lines represent ±2 standard errors.

error-correction term, $(XPOF - RPOF)_{-1}$ (not shown) shows a similar break, while the other coefficient graphs show no dramatic change, given their standard errors. These seem then to confirm a single break at 1953:1.

Projection (iii) presents the marginal-expenditures model over the same period. Except for the max Chow text, the pattern of the tests is similar to that for the conditional model [projection (i)]. Again, this is dramatically reflected in two of the three recursive coefficient estimates (not shown). The plot of the coefficient on $XPOF_{-1}$ shows that it is clearly different before 1953:1 than after 1954:1. The plot of the

constant reveals a similar, but less pronounced, pattern.

Interventions in the Period 1964–1974.— Projection (x) is the receipts regression (iv) of Table 3 estimated for the first tranquil period projected forward to the beginning of the second tranquil period (1975:1). The max Chow and fluctuation tests indicate no break; but the one-step-ahead Chow test does indicate a break, which the sequential Chow test locates at 1964:2. The one-step-ahead Chow statistics indicate a cluster of violations of the 5-percent critical value between 1968 and 1970, which suggests another possible structural break.

Attacking this period from the other side, projection (xi) projects the marginal-receipts specification [regression (v) of Table 3] backwards from the second tranquil period to the end of the first tranquil period. Now the fluctuation test also indicates a structural break, although it is not located by the one-step-ahead Chow test. The plot of the coefficient on $RPOF_{-1}$ (Fig. 2) clearly shows that the specification after 1970:2 is different from that before 1969:2. Indeed, one can see from the standard-error bands
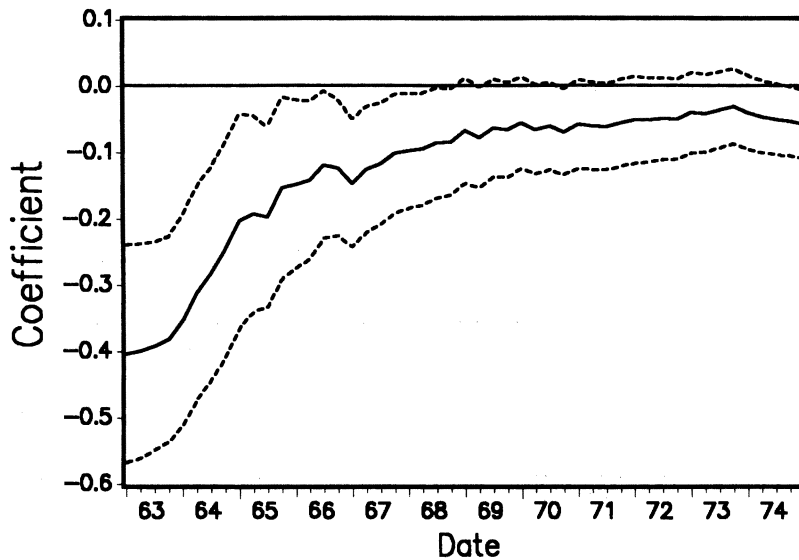
FIGURE 3. COEFFICIENT ON XPOF$_{-1}$ IN PROJECTION (iv):
REGRESSION (ii) OF TABLE 3 FORWARD FROM 1963:1 TO 1975:1

Note: The dashed lines represent ±2 standard errors.

that after 1971:1 the coefficient on RPOF$_{-1}$ is insignificantly different from zero, thus marking the shift in the receipts regression from a random walk in the earlier period to a stationary distribution in the later period. The plot of the constant (not shown) confirms the break. Together, the evidence from forward and backward projections suggests that there is a break at about 1964:2 and another at about 1969:2. The period between 1964 and 1969 most likely contains other structural breaks, although the precise timing is difficult to determine using our methods.

Now consider the conditional-expenditures specification. All four tests for projection (ii) indicate a structural break possibly at 1965:1. The coefficient plot for ΔRPOF$_{-3}$ (not shown) indicates that, after 1965:1, the coefficient is insignificantly different from zero. The coefficient plot of the error-correction term, (XPOF − RPOF)$_{-1}$, indicates a less sharp but more pronounced break at 1965:1. This pattern is repeated for the constant. The shift in causal structure identified in the initial consideration of the regressions for the tranquil periods (preceding

subsection) is confirmed in these plots. After 1968, only the coefficient on Δ_4XPOF$_{-1}$ is always significantly different from zero.

The marginal-expenditures specification of projection (iv) shows a similar pattern, although the one-step-ahead Chow test fails to locate it. The one-step-ahead Chow statistics just barely reject the null hypothesis of constant coefficients. However, just as for the coefficient on (XPOF − RPOF)$_{-1}$ in projection (ii), the coefficient plot on XPOF$_{-1}$ (Fig. 3) shows a pronounced break at 1965:1. The constant in projection (iv) behaves similarly. Only the coefficient on Δ_3XPOF$_{-2}$ (not shown), never becomes insignificant.

Backward projection (v) of the marginal-expenditures specification [regression (iii) of Table 3] from the second tranquil period to the beginning of the first tranquil period suggests that the break in the expenditures process can be localized further in 1965. The fluctuation test and the one-step-ahead Chow test indicate a break. The coefficient plot of Δ_3XPOF$_{-2}$ (not shown) suggests that the break occurs between 1964:2 and 1965:4. The plots of the other coefficients of
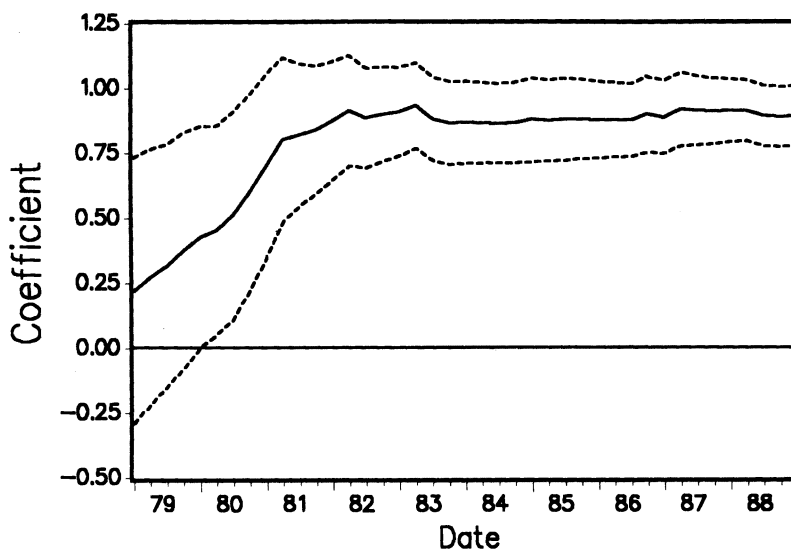
FIGURE 4. COEFFICIENT ON RPOF$_{-1}$ IN PROJECTION (xiii):
REGRESSION (v) OF TABLE 3 FORWARD FROM 1979:1 TO 1989:1
(OMITTING 1985:1 AND 1985:2)

Note: The dashed lines represent ±2 standard errors.

the marginal specification indicate similar, though less pronounced, shifts.

Interventions in the Period 1980–1989.— The one-step-ahead Chow statistics of the projection of the marginal-receipts specification forward to 1989:1 [projection (xii)] shows the Chow statistics for 1985:1 and 1985:2 to be, respectively, nearly four and nearly two and a half times their 5-percent critical values. Inspection of the plot reveals that these are the only violations. They are probably too high to be dismissed as sampling error. This was at first puzzling, because our initial reading of the historical record suggested no intervention in either tax or spending processes at this date. Looking more closely, however, we discovered that computer malfunction at the IRS delayed payments of tax refunds. Over $6.8 billion of tax refunds were delayed from March to April (i.e., from the first to the second quarter).[23] Once annualized these huge, but economically meaningless, aberra-

tions in the receipts process, easily explain the break discovered.

The receipts specification was reprojected forward dummying out 1985:1 and 1985:2 in projection (xiii). The fluctuation and max Chow tests indicate a break, although the sequential Chow test does not locate it. The one-step-ahead Chow tests are borderline. The behavior of the coefficient on RPOF$_{-1}$ (Fig. 4) is distinctly different after 1981:2 and especially after 1982:2 than before 1980:1. The plot of the constant (not shown) shows precisely parallel behavior. Receipts appeared to be stationary in the second tranquil period. After 1982:3, not only is the coefficient on RPOF$_{-1}$ statistically significantly different from 0, it is not significantly different from 1. Once again, the receipts process appears to be close to a random walk.

The marginal-expenditures specification in projection (vi) at best shows borderline rejection of coefficient constancy on the one-step-ahead Chow test only. The plot of the constant (not shown) shows a distinct, although just barely statistically significant,

[23]*Wall Street Journal*, 6 May 1985, p. 1.

change between 1982:2 and 1982:4. The co-efficients on $XPOF_{-1}$ and $\Delta_3 XPOF_{-2}$ indicate parallel, although statistically insignificant, breaks. To confirm this break, the same specification was estimated over 1984:1–1989:1 and projected backward to the end of the second tranquil period in projection (vii). The constant-base and constant-horizon Chow tests suggest a break at 1982:4. The plots of the coefficients on the constant and on $XPOF_{-1}$ (not shown) indicate a parallel break; their behavior before 1982:3 is distinctly different from their behavior after 1983:1. Again, similar, but statistically insignificant, changes are indicated for the coefficient on $\Delta_3 XPOF_{-2}$.

V. Interpretation

The last column of Table 5 assigns interventions from the chronology presented in Section III to the most nearly corresponding structural breaks identified statistically in Section IV. The chronology and the statistical investigation were conducted independently. These are the natural assignments; no attempt was made to search for particular kinds of interventions for particular structural breaks.

A number of features of Table 5 deserve notice. First, all of the structural breaks in the expenditures specifications are assigned to interventions in the spending process. Second, in projections from the first tranquil period, when it was possible to estimate both a conditional and a marginal regression for expenditures, both regressions show structural breaks at the same times. These two facts suggest that the breaks are truly in the spending process. Third, although we were not able to specify an independent conditional-receipts regression, every break identified for marginal receipts corresponds to an intervention in the tax process.

In addition to the information in Table 5, recall that the specification of the tax and spending models changed between the two tranquil periods. In the first period, there was a cointegrated relationship between taxes and spending, but in the second tranquil period this relationship disappeared.

These facts first suggest that between the first and second tranquil periods (sometime

in the late 1960's or early 1970's) there was a change in the "causal field." The evidence for this is the lack of cointegration between spending and taxes that surfaces in the second period. This means that the entire causal relationship between taxes and spending changed between the two periods, and causal relationships must be identified within each period.

After the change in the causal field, again sometime in the late 1960's or early 1970's, the evidence strongly suggests that receipts and expenditures are causally independent. Structural interventions in the spending process, identified by examination of the institutional record, are closely related to structural breaks in the statistical spending process but are not associated with breaks in the marginal-receipts process. This implies that spending does not cause receipts. Similarly, structural interventions in the receipts process are associated with breaks in the statistical model for receipts but are not associated with breaks in the marginal model for expenditures. This implies that receipts do not cause expenditures. Thus, we are left with the causal independence of our two series, not unlike Model D (see Section II-C).

Our interpretation of the first part of our sample, before the change in causal field, is a bit more complex. It would appear from Table 5 that the same type of causal independence is found in the period 1950–1963 as is found in the later period. Interventions in one process are not apparently associated with breaks in the other process. This cannot be the complete explanation, however; if taxes and spending are truly independent processes, why is it that receipts and expenditures appear to be cointegrated in the first tranquil period?

Our preferred resolution of this puzzle is that taxes do cause spending in the first tranquil period, but the Korean War, which jointly affects spending and taxes, masks this relationship in the early part of the sample. Specifically, with the onset of the Korean War, the 1951 tax bills were enacted with built-in expiration dates at the end of 1953. The expiration dates clearly envisaged the end of the conflict by that time. The structural breaks in the tax process are asso-

ciated with these bills. The war itself formally ended in early 1953, although the fighting ended somewhat earlier. Thus, when the war ended there was no reason not to anticipate that the prior tax increases would expire as scheduled by law. The expenditure break occurs at the end of the Korean War.

This evidence is consistent with the hypothesis that taxes cause spending. Conditional on the war ending, taxes were scheduled to fall by law. Thus, we would anticipate that, as the war ended, total spending would fall and a break in the spending process would occur. Moreover, since this break occurs in early 1953, we would not expect a further break in the spending process when the tax decrease went into effect. The scheduled tax decrease was already factored into the prior spending decrease.

It would be somewhat more difficult to argue that spending caused taxes. In that case, as the Korean War ended, we would have anticipated a break in the receipts process, which did not occur. One could argue that there was a tax decrease already scheduled, but the lack of action to move up the expiration date would have to be explained if spending truly caused taxes. One piece of evidence in favor of the spending-causes-taxes view is that the 1964 tax cut is not directly associated with a break in the spending process. Yet, this observation is potentially contaminated by the rapid onset of the Vietnam War.

Thus, the fact that the breaks in spending are associated with wars (a third cause) in this period makes it difficult to determine the causal direction definitively. The cointegration of spending and taxes clearly points to some causal links. Our preferred explanation is that taxes cause spending. However, it would be possible to entertain the alternative model.

VI. Conclusion

Three principal conclusions have emerged from our causal investigation. First, there was a change in the causal field or causal relation between taxes and spending which occurred sometime in the late 1960's and early 1970's. Second in the period following

the change in the causal field, taxes and spending were causally independent. Finally, in the early period, taxes and spending were causally linked, and there is some mild evidence in favor of taxes causing spending.

Scholars of such diversity as Robert E. Lucas and Aaron Wildavsky have also discussed the changes in the relationship between taxes and spending at a time similar to the one that we uncovered. In a lecture delivered in 1984, but not published until two years later, Lucas (1986 p. 133) noted that "the tendencies towards permanent deficit finance and inflation that have emerged in our economy in the *last fifteen years* have much deeper roots than a succession of transient external shocks and internal mistakes. They arose, I believe, because the implicit rules under which monetary and fiscal policy is conducted have undergone a gradual but fundamental change" [emphasis added]. Lucas clearly suggests that the normative tax-smoothing model in his essay is not an accurate positive description of the fiscal process after the late 1960's.

Wildavsky, a well-known scholar of the budget process, notes a similar phenomenon. In a chapter entitled "The Collapse of Consensus" in his recent book, *The New Politics of the Budget Process* he discusses this change:

Shortly after the standard accounts of classical budgeting were published in the early 1960's—Richard Feeno's *The Politics of the Purse* and the first edition of my *Politics of the Budgetary Process*—that process began to collapse.... In retrospect, the pattern is clear; Congress and the Presidents have trouble agreeing.
(Wildavsky, 1988 p. 120)

In Wildavsky's initial book, he had argued that budgeting was incremental, in part, because all parties agreed on the fundamentals and adjustments could be made on the margin. At some point, however, this consensus disappeared.

Our finding that spending and taxes are causally independent in the latter part of the postwar era is intriguing from the stand-

point of budgetary history. The period since 1970 has been marked with several major attempts to create causal interdependence through institutional reform. The first major institutional change was the Congressional Budget and Impoundment Act of 1974, which created the budget committees in the House and Senate. These committees were charged to integrate spending and tax decisions. The second major change was associated with the Gramm-Rudman laws, which were again designed to create causal interdependence. Our analysis suggests that institutional reform was undertaken precisely to counteract the lack of causal interdependence between spending and taxes. Moreover, at least by our measures, these reforms were not successful.

From the perspective of comparative politics, the lack of causal ties between taxes and spending is perhaps not that surprising. Compared to parliamentary democracies, the United States has many important actors with divergent interests and agendas. It would be valuable to apply the methods in this paper to countries such as Canada in which budgetary and tax initiatives are more closely tied.

Our results have implications for recent work in the area of fiscal policy. Several recent papers have considered fiscal policy in more general frameworks than Barro's model. For example, V. V. Chari et al., (1990) analyze Ramsey taxation in a real-business-cycle model with government. Unlike Barro's model, they find that taxes on labor need not follow a random walk. Nonetheless, their model also assumes that government spending is exogenous. Our empirical work suggests that, at least for the United States, this may not be the appropriate assumption.

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