

Comment on Karl Brunner's "Fiscal Policy in Macro Theory: A Survey and Evaluation"

Robert J. Gordon

NORTHWESTERN UNIVERSITY
AND NATIONAL BUREAU OF ECONOMIC RESEARCH

1. Introduction

It is a great pleasure for me to discuss the essay by Karl Brunner; this continues a tradition of discussing each other's work that dates back at least a decade. Much of Brunner's massive survey is admirable, reflecting a deep and careful analysis of central issues in fiscal policy, and the way events have changed perceptions of fiscal policy. The comprehensive and up-to-date reference list adds to the value of the contribution. Because of its length, I cannot delve into every issue raised by Brunner, but I will concentrate on three areas where, I believe, his analysis needs to be qualified and supplemented: (a) the intellectual history of the monetary-fiscal debate; (b) the interpretation of St. Louis equations; and (c) Ricardian equivalence and other theoretical issues.

2. The Interaction of Events and Ideas

Brunner drags up from his dusty shelves of old journals the infamous "battle of the radio stations (AM-FM)" and defends what has long been dismissed—an evaluation of alternative viewpoints with simple correlations between spending and money on the one hand and autonomous spending on the other. The essence of Brunner's defense is that "the 'single equation with single variable' was the appropriate choice for an evaluation of a class of hypotheses seriously presented in textbooks and class teachings."

Most spectators of the AM-FM debate found the single-variable framework unappealing because they could find no example of any influential

economist at that time who believed that “only fiscal policy matters,” and even if such an economist did exist, he never would have expected to find a stable and constant coefficient of total spending on autonomous spending because the output multiplier was a variable, not a constant, depending on, among other things, a host of changing tax rates. The Andersen–Jordan St. Louis contribution was taken more seriously precisely because its point of departure was a two-variable test of monetary and fiscal policy together.

Brunner begins with intellectual history and then delves into econometric details. Let me begin with my own version of the intellectual history that emphasizes the influence of events (rather than journal debates) on the evolution of ideas. This account is sprinkled with a few quotes to indicate what people actually believed in the 1950s and early 1960s. We then turn to an interpretation of the empirical issues.

The central paradigm of macroeconomics as it emerged from World War II was indeed the Keynesian multiplier theory and its endorsement of an activist fiscal policy to overcome the inherent instability of private investment. Monetary theory lurked in the shadows, discredited at least temporarily as a result of a major event that dominated early postwar ideas—namely the juxtaposition between early 1938 and late 1940 of a weak economic recovery, explosive monetary growth, and a short-term interest rate that was rapid and constant between early 1938 and late 1941, the economy’s recovery floundered until military spending began in earnest in late 1940, after which real GNP suddenly jumped by almost 20 percent in a single year. This chronology ingrained a deep-seated belief in the potency of fiscal policy and the “pushing on a string” analogy for monetary policy.

But money was not ignored totally in the late 1940s, and many economists took note of the fact that the quantity of nominal money had tripled between 1940 and 1945. Contemporary accounts displayed a curious inconsistency, with a monetary expansion viewed as impotent but a monetary contraction viewed as too dangerously potent to risk, as in Lawrence Seltzer’s (1945) remark that “there is great risk that the deflationary effects of a radical rise in interest rates might be so severe as to throw the whole economy into a crushing depression” (p. 844).

As for the teaching of undergraduates, I have never managed to obtain Samuelson’s 1948 first edition, but I do have the 1951 second edition, which, I hasten to add, was not the edition that I used in college but was obtained at a used book sale. Samuelson in 1951, more than a decade before the AM–FM debate, does not reveal himself as a hard-line “only fiscal policy matters” guy. Instead, his treatment reflects the uncomfortable asymmetry of early postwar Keynesian ideas. There are over 25 index entries for money and monetary policy and another 20 for the

interest rate. On page 342 are listed as effects of a \$1 billion open-market purchase:

the general easing of interest rates and the increased availability of credit to would-be investors, . . . the upward shift in the earlier chapters' investment-income schedule resulting from the lowered rate of interest, . . . the primary and secondary increases in income resulting from the increased flow of investment, . . . and the increased stock of buildings, equipment, and inventories that will later result from the cumulation of a high rate of investment.

In typical late 1940s style, this account is immediately followed by three qualifications that make monetary policy "at best a supplement to other stabilization policies, such as fiscal policy." The three qualifications are these: (1) "Changes in the amount of money may have very weak effects on the rate of interest if rates are already very low." (2) "Even if there are some changes in the rate of interest, the rate of investment spending may turn out to be relatively little affected by changes in interest rates. The prospects for investment may depend much more on the depressed state of business." (3) "The Central Banker may be unwilling to push monetary policy very far." Clearly the first two qualifications reflected events of the late 1930s, and the third the Fed's pegging of interest rates in this pre-Accord edition of the textbook. The late 1940s asymmetry is implicit in the assumption that the problem of monetary policy is pumping up a depressed economy rather than slowing down an overheated one.

Over the following decade there was a gradual but continuous shift of opinion toward an increased role for monetary policy, marked by mileposts including the Patman Committee Inquiry, the negative reaction of many economists to the downgrading of money in the Radcliffe report, and the influence of the monetary research of Milton Friedman, his students, and others. The growing belief in the importance of money can be traced to several episodes in the first postwar decade. Those who believed that the large outstanding stock of public debt prevented effective monetary action and required the pegging of interest rates either lost credibility or changed their opinions when the higher interest rates that followed the Treasury-Fed Accord failed to have any disastrous consequences for debt management or the economy's performance in general. The relative mildness of the 1954 recession was due partly to countercyclical monetary policy and helped to lessen the belief that monetary policy was only effective in countering inflation and suffered from an asymmetric impotence in dealing with slack demand. The continued acceleration of inflation despite rising interest rates in 1956-57 tempered the belief that monetary policy had unique curative powers to combat inflation. By 1962 Harry Johnson was able to observe that "the wheel has come full circle, and pre-

vailing opinion has returned to the characteristic 1920s view that monetary policy is probably more effective in checking deflation than in checking inflation.” Although Johnson may have been ahead of his time, influenced as he was by monetary research at the University of Chicago, nevertheless his account provides a picture far from Brunner’s, with the “hard-line fiscalists” hard to find.

Turning now to more contemporary quotes—I seem to have saved my final exam in Economics 1 at Harvard, taken in May 1959—I find interesting evidence to support the idea of “fiscal dominance,” but I find no evidence at all of the influence of Brunner’s “hard-line fiscalists” believing “money doesn’t matter.” Fiscal dominance is reflected in the fact that the first half of the exam consisted of two questions: one hour on fiscal policy and a half hour on monetary policy. Inspection of the four-part fiscal policy question makes one scoff in retrospect at the idea of regressing aggregate spending on autonomous spending, because 1959 Harvard undergraduates were supposed to know that the multiplier effect of a change in government spending depended on whether or not the spending was financed by increased taxes, and they were given the option of concluding that an increase in spending accompanied by an equal increase in taxes might raise unemployment or leave it unchanged. The half-hour monetary question reflected not a ritual belief that “money doesn’t matter,” but rather the same old asymmetry. To quote the question, “It is frequently argued that monetary policy is effective in controlling inflation, but less successful in fighting unemployment. Trace the mechanism through which the tools of monetary policy operate under alternative cyclical conditions, and comment on their effectiveness.”

Returning to the influence of events, the AM–FM debate coincided with the heyday of activist fiscal policy, dubbed the “new economics.” By then, changes in government spending were recognized to involve gestation lags and to have allocative side effects, and so the central policy tool had become changes in income tax rates, which of course involves changes in the spending multiplier rather than a stable multiplier, as in Brunner’s caricature of fiscalism. Although the consensus policy paradigm of 1965 did not neglect monetary policy nor deny that monetary tightness could interfere with the pace of economic expansion, monetary policy was basically kept in the background and relegated to the role of maintaining a low and stable level of long-term interest rates to foster the goal of stimulating long-term economic growth.

This policy framework collapsed with amazing speed after 1967 as the result of the interaction of events and economic writings. My graduate school classmates and I were acutely aware of the timing of this turn in the intellectual tide, as we began our first teaching jobs in the fall of 1967 and almost immediately found our graduate school education incapable

of explaining the evolution of the economy. The most important ingredient in this revolution was the Friedman–Phelps “natural rate hypothesis,” the role of which is well known and not our subject today. More relevant was the blow struck by Andersen and Jordan in 1968. Although activist advocates eventually regrouped and presented convincing evidence of fatal statistical flaws in the St. Louis procedure, particularly the contribution of Goldfeld and Blinder, their disarray lasted long enough to partially discredit fiscal activism. To add to the overall indictment of fiscal policy provided by the St. Louis equation, Robert Eisner in 1969 made an important attack on the efficacy of the temporary tax changes favored by mid-1960s policy activists. Using the framework of Friedman’s permanent income hypothesis of consumption, Eisner argued that a temporary income tax cut or surcharge would fail to alter permanent income and thus would have a lower spending multiplier. Further, the lag in the effect of fiscal policy might be long and/or unpredictable, with the length of the lag depending on the public’s subjective assessment of the likelihood that the tax change soon would be reversed.

These academic criticisms of the activist case might not have been so persuasive if they had not been accompanied by supporting events. The dramatic drop in the personal saving rate in late 1968 and the failure of spending growth to slow appreciably in response to the temporary tax surcharge was consistent both with the St. Louis claim that monetary multipliers had previously been underestimated and fiscal multipliers overestimated and with the Eisner critique. Blinder’s retrospective econometric evidence of this period shows that temporary tax changes are not completely ineffective, but their multiplier impact may be as little as one-half of tax changes regarded as permanent, and the effect on consumption of any tax change may take several years to occur.

3. Empirical Issues in St. Louis Equations

The empirical issues involved in the AM–FM debate and subsequent St. Louis equation are so well known that little time need be spent reviewing them. The St. Louis equation represented an advance over Friedman and Meiselman in three main dimensions: testing the effects of monetary and fiscal policy in the same equation, using full employment instead of actual government spending and revenues, and expressing variables in first differences. However, the St. Louis reduced form was vulnerable to the central criticism that coefficients of both the monetary and fiscal policy variables were biased if there were any correlation between either policy

variable and the error term in the equation, representing the whole panoply of omitted demand and supply shocks that drove changes in aggregate demand.

The general case for this point was best expressed by Goldfeld and Blinder, and once their case was stated, everyone understood the argument that the monetary policy coefficients were biased upward, since during all of the original Andersen–Jordan sample period the Fed was acting to stabilize interest rates rather than money, thus creating a passive positive response of money to any demand shocks in either the money or commodity markets. And there was no surprise when Ando and Modigliani reported their experiment that, when estimated to artificial data generated by the MPS model, the Andersen–Jordan technique substantially overstated monetary effects and understated fiscal effects. But this still left open the source of the fiscal bias. If a downward bias on fiscal policy coefficients in the St. Louis equation occurs because active fiscal policy has been pursued within the sample period, thus creating a negative correlation between government spending and the error term, what were these episodes when fiscal activism was so effective? In two published comments (1971, 1976), I pointed to the set of events in the Eisenhower administration that led to this result.

Most important, there was a huge negative correlation between the decline in defense spending that took place between 1953 and 1956, and the (dare I say) autonomous bursts of automobile spending associated with new models in 1955 and export spending partly associated with the Suez crisis in 1956. (With reference to McCallum’s paper in this volume, it is important to note that this negative relationship displays a positive serial correlation extending over two years.) I recall Paul Samuelson’s injunction to us fledgling graduate students in the mid-1960s that he would flunk anyone who produced an econometric explanation of the high level of auto sales in 1955. I later told the story, to explain the Andersen–Jordan result in terms of efficacious fiscal policy, that “President Eisenhower had decided to stop the Korean war in 1953 because he could see the 1955 auto boom and 1956 Suez crisis coming, and he wanted to get defense spending out of the way to avoid overheating the economy.” What is not facetious is the remarkable record of the Eisenhower administration in the 1958 recession in creating a time path for nondefense government purchases that rose as the economy fell and fell as the economy recovered. In my comment (1976) I showed, by alternatively including and excluding a proxy for autonomous spending from a St. Louis equation, that in the Eisenhower period fiscal coefficients were low and downward biased, in the Nixon–Ford period they were high and upward biased as a result of procyclical fiscal policy, and in the Kennedy–Johnson era they were in between. The original St. Louis equa-

tion was dominated by the Eisenhower sample period and by the negative correlation between the post-Korean decline in defense spending and the mid-1950s business expansion.

Viewing this whole literature from the mid-1980s, we find naive the entire literature on “autonomous spending,” because (as McCallum’s paper suggests) nothing is truly autonomous. Recent papers have more fruitfully viewed business cycles as being generated by “innovations” in both financial and real variables, where “innovation” is defined as the error in an equation that relates the variable in question to its own past values and the past values of everything else. In this context I have recently completed a research project that reexamines the behavior of household and business investments in newly created quarterly data extending back to 1919 [see Gordon and Veitch (1984)]. Strong evidence is provided to support both sides of the AM–FM debate, for innovations in the money supply have a substantial influence on both household and business investments, but there is still room for a major impact on the business cycle of autonomous innovations in structures investments (both residential and nonresidential).

4. Ricardian Equivalence and Other Issues

The rest of Brunner’s paper is more satisfactory. There is a sensible discussion of “intergenerational altruism” and “intergenerational selfishness” in the context of the Barro–Ricardo equivalence theory. As a matter of historical record, I wish that Brunner had cited Patinkin’s incorporation into macroeconomic analysis of a variable proportion (k) of outstanding government bonds treated as net private wealth. Patinkin’s treatment anticipated many of the implications of Barro’s analysis without taking any position over whether k is at an extreme value of zero, as assumed by Barro, or unity, as assumed in some traditional Keynesian analysis.

Brunner recognizes that “the context of risk could explain . . . the appearance of bequest without a bequest motive [as] formalized by Barro.” Risk, however, is just one of the reasons why I was never convinced by the Barro logic, however dutifully I continue to teach it in the graduate school classroom. As one without children and likely to leave a substantial bequest, it immediately became evident that there is a more important reason than risk to explain why individuals often leave bequests without any necessary altruism for future generations. After all, we are supposed to be able to insure ourselves against risk by buying annuities. But a more important additional set of factors—high transactions costs and inconvenience, as well as imperfect capital markets—make it almost

impossible for a well-off person to “go out” with a zero net worth. There is no rental market for the type of house I live in, so in order to buy an annuity with all my assets, I would have to move. Renting a car is expensive, and renting my personal library of books and journals would be impossible. People like me are likely to behave according to a permanent income theory of the flow of consumption services and to leave whatever assets are necessary to maintain that flow of services to worthy charities. Since my heirs are likely to be nonprofit and nontaxable organizations, there is simply no present value of future tax liabilities to consider, and the Barro theorem falls to the ground. The Reagan tax cuts financed by deficit spending have made me feel good, and I have spent some of the proceeds.

Of course, I have not spent all of the proceeds, because simultaneously there have been substantial increases in Keogh and IRA ceilings that have induced me to save more as well. This tug of war between conflicting incentives bears on the empirical evidence “on the Ricardian theme” reviewed at such length in Brunner’s paper. Reduced-form equations can be useful, and I have estimated plenty of them in my work on inflation and, more recently, on investment. But the equations of Feldstein–Kormendi type, summarized by Brunner, seem unlikely to provide any reliable evidence of the issues at hand. First, the inclusion of government spending and tax revenues as explanatory variables in a consumption equation runs afoul of the Goldfeld–Blinder critique for the same reasons as does the St. Louis equation. Second, the tax schedule is progressive, and if people in different tax brackets have different propensities to consume, the schedule relating total consumption to total tax revenue will be nonlinear. Third, the lags that Blinder found between changes in taxes and changes in spending are neglected. Fourth, no distinction is made between temporary and permanent tax changes. Fifth, tax law changes that alter disposable income, like a neutral surcharge, can have totally different effects than legislative changes that twist the incentives to consume and to save, as in my IRA–Keogh example. Surely Brunner is aware of all this, so I wonder why he takes all this empirical work so seriously.

5. Concluding Remarks

Brunner’s paper treats numerous other issues that do not appear to require comment here. I would enter only two qualifications. First, Brunner’s long and sensitive discussion of the Sargent–Wallace deficit analysis is slightly marred from his uncritical analysis of the Mankiw–Summers paper on consumption and money demand. There are at least three problems with that paper that shed doubt on its credibility: (a) the implausible assump-

tion that the responsiveness of money demand to changes in investment is zero; (b) the lack of evidence for the postwar period of any difference in the fit of GNP and consumption when entered into a money demand equation with a flexible lag structure [see Gordon (1984)]; and (c) the awkward fact that a larger share of demand deposits is held by business firms than by households. Mankiw and Summers do not make a convincing case that would support contractionary effects of tax cuts.

The second qualification refers to Barro's tax smoothing hypothesis on the behavior of deficits. Although somewhat skeptical of the empirical robustness of Barro's approach, Brunner calls it "the only game in town." Yet a paper by Barro (1984) that Brunner does not cite was dismissed at a recent conference as being unsuccessful both on theoretical and empirical grounds. As one discussant asked, "How is it that policymakers in Washington figured out the Ramsey optimal tax rule 50 years before public finance economists?" Consider social security, for which tax smoothing means full actuarial funding of the expected program of benefits. And the theory fails empirically, because the coefficient in a debt change equation on temporary government spending is zero rather than unity as required by the theory, and the World War II years have to be thrown out.

A concluding comment is that, given the length of Brunner's present paper, the attention given to reduced-form empirical evidence on various issues seems excessive. Other, perhaps more interesting, issues regarding fiscal policy might have been covered instead. He might have reconsidered the effects of changes in the monetary-fiscal policy mix on real interest rates and the exchange rate, with and without Ricardian equivalence. Was the shift in the policy mix with the ensuing appreciation of the dollar the real key to the extent of disinflation; if so, what are the theoretical arguments for reversing the mix to avoid future deficits or for maintaining the mix to hold the benefits of disinflation? What are the implications of tax reform for macro theory, particularly a shift to base broadening with lower marginal rates, or a shift to a broad-base progressive consumption tax? What are the implications of current large government deficits for the public choice idea that the best way to reduce government spending is to reduce tax rates—this just hasn't happened when defense spending is included. Overall, I cannot help feeling that the empirical work examined is too flimsy to merit so much attention from this fine theorist, and I cannot help wishing that a paper on fiscal policy in macro theory had contained more about theory.

References

- Barro, Robert J. 1984. The behavior of U. S. deficits. NBER Working Paper 1309. March. In *The American Business Cycle: Continuity and Change*, ed. R.J. Gordon. Chicago: University of Chicago Press. Forthcoming.

- Eisner, Robert. 1969. Fiscal and monetary policy reconsidered. *American Economic Review* 59: 897–905.
- Gordon, Robert J. 1971. Notes on money, income, and Gramlich. *Journal of Money, Credit, and Banking* 3: 533–45.
- . 1976. Perspectives on monetarism. In *Monetarism*, ed. J. Stein, pp. 52–66. Amsterdam: North-Holland.
- . 1984. The 1981–82 velocity decline: A structural shift in income or money demand? In *Conference on Monetary Targeting and Velocity*, pp. 67–99. Federal Reserve Bank of San Francisco.
- ed. n.d. *The American Business Cycle: Continuity and Change*. University of Chicago Press. Forthcoming.
- Gordon, Robert J. and John M. Veitch. 1984. Fixed investment in the American business cycle, 1919–83. In *The American Business Cycle: Continuity and Change*, ed. R.J. Gordon. Chicago: University of Chicago Press. Forthcoming.
- Johnson, Harry G. 1962. Monetary theory and policy. *American Economic Review* 52: 335–84.
- Patinkin, Don. 1965. *Money, Interest, and Prices*, 2d ed. New York: Harper & Row.
- Seltzer, Lawrence H. 1945. Is a rise in interest rates desirable or inevitable? *American Economic Review* 35: 831–50.