Comments and Discussion

Stephen M. Goldfeld: This is a useful and eminently readable paper. It treats a number of important issues that have been around for a while, but Friedman manages to pull them together in a quite neat way. There are several major topics covered in the paper and I will say a few words about each.

The first issue is that of transactions crowding out. Friedman's main contribution here is in providing some estimates of the degree of crowding out, while indicating the potential need for a term-structure adjustment when long-term rates enter the *IS* curve but short-term rates enter the *LM* curve. There is one minor technical inelegance in the calculations—the use of elasticity estimates, stemming from log-linear *LM* and *IS* curves, is not quite reconciled with Friedman's development based on a linear model. However, the effect of this is probably small. There is also an asymmetry of sorts—Friedman computes the degree of crowding out for alternative estimates of the *LM* curve but for only one estimate of the *IS* curve. Because there is hardly unanimity on spending elasticities, it would be nice to know the sensitivity of the results to alternative *IS* curves. In this regard, some evidence available from simulations of existing econometric models could be brought to bear. The one virtue of these, as opposed to the estimates Friedman reports, is that they cope with a variable price level and take account of the cyclical state of the economy, both of which are factors that should influence the extent of transactions crowding out.

The second topic covered in the paper, and really the most important one, is the discussion of portfolio crowding in or crowding out. Friedman derives a simple, understandable result in terms of the coefficients of the asset-demand equations and makes clear what empirical magnitudes are

necessary for deciding in favor of crowding in or crowding out. It should, of course, be noted that the specific formula that is obtained is quite dependent on a number of strong assumptions about specification. These include linearity of the asset-demand functions in interest rates, income, and wealth; and symmetry of the interest rate coefficients across equations. Relaxing these assumptions would clearly change the formula but would not affect the main point. In fact, a recent paper by Cohen and McMenamin, referred to by Friedman, arrives at qualitatively similar results in the context of a model with somewhat different functional specifications.

Functional details aside, there is another sense in which Friedman's setting is restrictive: it is a static one-period story. While that is the simplest way to clarify what is occurring, it does leave out some potentially important features. There is, for example, the question of stability as originally examined by Blinder and Solow. The Cohen and McMenamin paper mentioned above does examine this question and finds problems of stability when bonds are close substitutes for equities. I assume the same difficulty would arise in a dynamic version of Friedman's model so that one should probably not take too seriously the case where portfolio crowding out leads to a negative fiscal multiplier.

Another dynamic aspect which is missing is that portfolio crowding in or crowding out in the real world is not a timeless phenomenon. One has to be precise about the relevant horizon because it is easy to construct situations in which portfolio crowding in prevails over one time period and crowding out over another. Indeed, the presence of lags in the kinds of asset-demand equations alluded to in the latter part of the paper could well, as an empirical matter, produce this result. A related point concerns the treatment of the various interest rates. Once one moves beyond the one-period model or undertakes empirical work, the relevant interest rates are holding period yields. Furthermore, the relevant yields may differ in the asset equations and in the *IS* sector. As a consequence, considerable effort may be necessary to define "the" bond rate or "the" equity rate.

Taken as a whole, these considerations suggest that a complete condition for portfolio crowding in or crowding out will be somewhat more complicated than Friedman's formula and, further, that empirical evaluation of any such formula is no simple task.

A third issue Friedman addresses is the role of wealth in the demand function for money. In the absence of a wealth variable, portfolio crowd-

ing out cannot occur. To preserve this possibility, Friedman seeks to demonstrate the statistical significance of a wealth variable. (For crowding out to emerge, the wealth effect would have to be "large" and not just statistically significant, but this issue is not addressed.) While I believe a plausible case can be made for a statistically significant wealth effect, I do not consider the evidence Friedman offers particularly persuasive. He observes that, while wealth is insignificant in a basic money-demand function estimated through 1972, extending the sample period to 1977 makes wealth "work." However, this equation hardly forms the basis for a strong case: it fails a stability test, and has some quite strange parameter estimates. Friedman does report that an equation including wealth but excluding income is stable and that at least one version of such an equation extrapolates well in the post-1972 period. This, however, is not a serious specification because, as Friedman notes, the issue is not whether to exclude income but rather whether to include wealth. Furthermore, the wealth-only equations have implausibly slow speeds of adjustment and, as I have found, perform terribly in out-of-sample extrapolations for earlier sample periods. It seems, then, that there is no fully satisfactory equation exhibiting a significant wealth effect.

Some evidence exists, however, to support Friedman's case in my 1976 paper in *BPEA,* which Friedman cites. There, I reported that a nominal adjustment model in per capita terms exhibited both significant wealth and income variables, even if the sample period stopped in 1973. Furthermore, the various details contained in tables 7 and 8 of that paper show that in extrapolations starting as early as 1966, the equation with wealth and income forecasted better than the equation with income alone. I did not develop these results because I was focusing on the post-1973 experience and there, as is evident from Friedman's results, the equation with wealth and income is not satisfactory. On the whole, I would regard this earlier evidence as bolstering Friedman's case, although to me the recent period remains somewhat of a puzzle.

Friedman does note these issues in his discussion of Michael Hamburger's results, and I should like to indicate briefly why I do not think Hamburger has solved the recent money puzzle. The main wrinkle in Hamburger's equation seems to be the use of two long-term rates of return, including the dividend-price ratio. Friedman, in fact, improves on the extrapolative performance of this equation by substituting wealth for the dividend-price ratio. I believe, however, that neither Hamburger's

original equation nor Friedman's modification stand up to close scrutiny since the real reason these equations work is that they constrain the income elasticity to unity. This serves to drive up the coefficient of the lagged money stock (that is, lower the speed of adjustment) by a substantial amount, something that tends to happen to the "basic" equation as it falls apart (see Friedman's table 4). It is therefore not surprising that this kind of restriction tends to produce a better forecast. Unfortunately, the restriction is not valid because the hypothesis of the unitary income elasticity is readily rejected by the data through 1973. Furthermore, when this restriction is relaxed in the context of Hamburger's specification, the resulting equation both fails a stability test and forecasts poorly. On balance, then, I am not persuaded that the wealth variable has a major role to play in understanding the recent behavior of money demand.

My final brief comment is on the policy role for debt management posited in the last part of the paper. The argument makes good sense, but I doubt that the empirical magnitudes warrant much of a practical role for debt management. The evidence offered linking changes in the maturity of the federal debt and the recent behavior of investment, while interesting, seems circumstantial at best. Furthermore, because of problems in coping with the need for expectations in constructing holding period yields, this is a difficult problem on which to get solid empirical evidence. Nevertheless, Friedman himself has already done important work in this area and should be encouraged to carry out further research along the lines he indicates at the end of the paper.

John H. Kareken: Friedman has given us a thorough appraisal of the claim that "debt-financed deficits 'crowd out' interest-sensitive, private-sector spending," and we should all, I think, feel indebted to him for it. As he suggests in his paper, though, for those who are sure that price stability is desirable, there is another argument against government deficits, even those financed by bond issues. In his words, it is that "what matters for prices is not only the money stock but some combination of money plus the outstanding interest-bearing government debt." And because Friedman has been so thorough in his appraisal of the "crowding-out" argument, I will spend the time allotted me on that possibility. That may be irresponsible, but I do not think so. The routine of the Brookings panel is after all a little curious, at least in one regard. Not long ago I gave

Friedman my criticisms of his first draft, and I am quite willing to accept his appraisal of them, as revealed by the changes he saw fit to make.

In his paper Friedman remarks that of late no one has taken at all seriously, to the point of carefully examining it, the possibility that how government deficits are financed is of no consequence. His explanation is that the monetarist tide has been running too strong. But Friedman is not exactly right. My colleague Neil Wallace has considered the possibility. The paper in which his analysis appears, "The Overlapping Generations Model of Fiat Money," has not yet been published, or indeed even widely circulated, so Friedman can hardly be chided for not having been more diligent in his search of the literature.

What Wallace shows in his paper is that, to a first approximation, it matters not at all how the government deficit is financed. The size of the deficit certainly matters; but how it is financed, whether by issuing bonds or by printing more money, does not. To rephrase Wallace's result, open market operations amount to nothing (or little) more than central bank busy work. That, it seems to me, is right, and what I thought I would do now, if without much hope of convincing anyone, is to give a loose paraphrase of the proof of that proposition. I want to be clear that the paraphrase is mine alone. Wallace may have bungled badly, but no one should conclude that before perusing his paper.

To isolate the effects of an open market operation, it is necessary to hold fiscal policy constant. On that, I believe, there is general agreement. For a single economy of the sort I have in mind, it suffices to hold the government budget deficit (or, more accurately, the time path of the deficit) unchanged. Because government spending has a social optimum, transfer payments and tax receipts, or the net thereof, must therefore be adjusted in such a way as to offset any change in the government's net worth that results from the open market operation. With a properly defined or truly ceteris paribus open market operation, the net worth of the government is unchanged. But it follows that, in effect, the balance sheet of individuals is also unchanged. And in consequence the equilibrium of the economy is unaltered. With the balance sheet of individuals unchanged, or in effect unchanged, future-period consumption options are precisely what they were, and therefore the current-period equilibrium is precisely what it was. Not even the price level changes as a result of an official asset exchange. That is the inevitable result of assuming, reason-

ably enough, that for money, as for all other assets, the essential question is "What rate of return does it offer?"

An example may be helpful. Imagine a two-asset economy with, say, real capital in the form of a storable consumption good, and paper money, the liability of the government. And suppose for definiteness that the government, engaging in an open market operation, increases the amount of capital it owns and perforce its money indebtedness. Since its budget deficit cannot change, it must transfer back to individuals whatever change in its net worth is implied by the assumed exchange. The transfer payments must be distributed appropriately. The distribution of income cannot change with an open market operation. If it did, fiscal policy would not be unchanged. Equally obvious, the transfer payments may be negative. If the capital acquired by the government is risky, as it must be for portfolio diversification, there may be an implied decrease in net worth, and it may therefore be required that individuals be taxed.

For an unchanged government deficit it is necessary that transfer payments depend on the current state of the world. That observation is basic, and explains why, whatever appearances may be, the balance sheet of individuals does not really change. Because the government must return whatever it earns on any increment of capital (or, more generally, earning assets) that it acquires, the amount held by individuals does not really decrease. Nor in effect is there any increase in the real balances owned by individuals.

The conventional analysis goes astray in concentrating on seeming changes, on changes that disappear with the necessary adjustments that keep the net worth of government constant. It takes seriously that the money held by individuals increases when, for example, the government buys back some of its bonds. But that increase is, in a word, fictitious. I can put the argument another way. One gets the right answer to the question "what happens when the government engages in an open market operation?" by looking at the consolidated balance sheet of individuals and government. Clearly, whatever assets are exchanged by the government and individuals, that balance sheet remains what it was.

Or to put the argument yet another way, individuals pierce the veil of government. They are forced to do that by the required adjustment in transfer payments. In the world of Franco Modigliani and Merton Miller, conjured up a couple of decades ago, individuals pierce the corporate

veil, if perhaps not quite in the same way that judges have. And in Wallace's world, individuals pierce the government veil. What Wallace has given us then is Modigliani and Miller all over again. The corporations of the world of Modigliani and Miller are financial intermediaries, and so is the government of Wallace's world.

As might be expected, the conclusions that open market operations are pointless is not perfectly general. An official asset exchange of sufficient magnitude can change the equilibrium price of money. If the government acquires more capital than individuals would have, individuals will want less money, and the price of money will therefore adjust. That is not to say, though, that the traditional analysis is right. Moreover, as John Bryant and Wallace have shown, if there is a reserve requirement, then how the government is financed does matter.[1] And depending on what government and private transactions costs are, it may. But that there may be more or less deadweight loss, depending on how a given fiscal policy is financed, is not the traditional conclusion, and it would seem reasonable that as a practical matter macroeconomists can safely ignore whatever changes in deadweight loss result from open market operations.

Before stopping I want to anticipate a couple of possible objections to what I have said. The first can be cast in question form. What about all those studies which show that money and prices move together? The difficulty is, though, that many of the most dramatic changes in the money supply were not produced by official asset exchanges. A coinage debasement is not an open market operation. The discovery of gold in Mexico was not. The several U.S. banking panics were not. There is all the difference in the world between an open market operation and a change, however brought about, in private wealth. All the simple regression studies that have been done can therefore simply be dismissed. And the evidence from multiple regression studies is hardly more impressive. There are few if any that are not subject to Robert Lucas' criticism. I know of none.

The other possible objection, which does have to be taken more seriously, is that of the overlapping generations or money-as-a-store-of-value

1. See John Bryant and Neil Wallace, "The Inefficiency of a Nominal National Debt," staff report 28 (Federal Reserve Bank of Minneapolis, 1977) (*Journal of Political Economy*, forthcoming); and Bryant and Wallace "Open Market Operations in a Model of Regulated, Insured Intermediaries," staff report 34 (Federal Reserve Bank of Minneapolis, 1978).

model. Allegedly, it can deliver only half-truths. In the real world, so the argument goes, money serves both as a store of value and as a medium of exchange. But in the world of overlapping generations, it serves only as a store of value. Unfortunately, there is no time to argue the issue. All I can do is say that I believe the criticism is wrong. I would grant though, that until someone has managed the near-impossible, until someone has modeled a world of more or less continuous exchange of goods and money, we will not be quite sure about the overlapping generations model or the radical conclusions that it yields.

Michael Hamburger: I liked Friedman's paper, particularly because it examined the relative degree of substitutability among different assets. The relative substitutability issue is a way of gaining insight into the debate over monetarism, because monetarists believe that money is a substitute for a wide range of both financial and real assets, while non-monetarists confine the range of money substitutes to a narrow range of short-term financial assets. I was disappointed, however, that the paper did not advance our empirical knowledge on the substitutability question. All the money-demand equations estimated by Friedman contain only two interest rates, both yields on nominal financial assets. He goes to great pains to argue that the return on the real asset that I used—the dividend-price ratio—served primarily as a proxy for the price of equities or wealth. That claim is not supported by other work I have done on U.S. money-demand equations for a variety of periods or on similar equations for the United Kingdom, in which this ratio was a better explanatory variable than was the price of equities or wealth.

The analysis of the effects of putting wealth in the money-demand function is also important. According to Stephen Goldfeld's discussion, his finding that wealth is not a significant explanatory variable stems largely from the 1950s and early 1960s. Friedman's results raise doubts about the generality of these findings and thus tend to support Brunner and Meltzer on the importance of wealth in the money-demand function.

I would be interested in seeing the results Goldfeld discussed which, in his view, suggest that my analysis solves the recent money puzzle *only* because the income elasticity is constrained to unity. Without examining his findings, I can only report that the constraint on the nominal income elasticity is not binding; when it is estimated freely, it comes out to be almost exactly unity. Moreover, although the real income elasticity is

significantly less than one during the sample period, this finding has no important effect on the out-of-sample residuals. Hence, the results I am aware of indicate that the solution to the post-1974 money puzzle is not dependent on the constraint on the income elasticity of money demand. If others have different results, I would like to see them.

Finally, contrary to Friedman, I hope that we do not try to establish debt-management policy as an important element of stabilization policy in the United States. The unhappy British experience of using monetary policy largely for debt-management purposes should warn us against such a course. In addition, there is a great deal of evidence that changes in the composition of debt do not have much effect. It seems noteworthy that the average maturity of the debt rose substantially during periods such as the mid-1960s, which were excellent ones for real investment.

General Discussion

Friedman addressed some comments to his formal discussants. He first observed that, at least according to John Kareken's presentation, the paper by Neil Wallace had finally supplied the thesis that "money does not matter." Since the recent positions in the profession range from "*only* money matters" to "money *also* matters," the Kareken position clearly expands the spectrum of views on the efficacy of monetary policy. Friedman said that he hoped that people who advanced the view that income is determined by bonds plus money would recognize their disagreements with monetarists who see income determined by money alone.

Friedman agreed with Goldfeld on the point that, in principle, expected holding period yields—that is, yields that included expected changes in asset prices—rather than measured yields ought to be in asset-demand functions, and with Michael Hamburger's view that the yields on a wide spectrum of assets should appear in the money-demand function. In fact, in empirical work carried out for the paper and mentioned but not reported, he had attempted (with only partial success) to relate money demand to the expected holding-period yields, adjusted for inflation, on money itself and four alternative assets.

Saul Hymans felt that Friedman had provided a valuable exposition of the framework for analyzing crowding out and crowding in. He added

that crowding in was the long-run prediction of the Michigan model, which specified a demand function for liquidity aggregating money and bonds. As Hymans saw it, his work suggested that bonds are a closer substitute for money than they are for capital, and hence that crowding in prevailed.

Other discussants, however, introduced a variety of reasons why crowding in might be less likely than Friedman's paper implied. Rudiger Dornbusch, George von Furstenberg, and Frederic Mishkin all questioned the implicit assumption of the paper that financial effects on investment demand depended solely on changes in the return on capital. They pointed out that, if investment demand were linked to the cost of capital (equity and debt) or to James Tobin's q, which reflected both equity and debt valuation, higher bond yields associated with financing deficits would show up as a greater depressant of investment, thus decreasing the probability of crowding in. Friedman agreed that, in a complete model including private debt and corporate equities, the investment-demand function would be linked to both corporate bond and equity yields. He explained that he had simplified the analysis for expositional purposes by adjusting for debts within the private sector, and he argued that the simplification did not alter the qualitative considerations affecting crowding in or crowding out. Arthur Okun supported Friedman's response, suggesting that his verdict could be upset only if bonds and real capital were gross complements—which seemed highly unlikely.

Von Furstenberg remarked that the short-run character of Friedman's analysis biased the result toward crowding in. The paper focused on a situation in which the volume of government bonds increases, while the quantities of money and capital are unchanged. In that situation, it is not surprising that the required return on capital is likely to decline. But von Furstenberg argued that, in such a case, the government neither absorbs cash nor uses resources, merely distributing bonds to the public as gifts (or creating a "rainshower" of bonds). In an actual deficit operation, however, the quantity of capital can remain unchanged in the face of government dissaving only if that dissaving is offset fully by extra private saving. For the actual deficit operation, von Furstenberg expressed his judgment that crowding in was at most a "curiosity." Friedman countered that he saw no problem in assuming that, for the short-run, extra private saving offset the government dissaving—indeed that was consistent with

standard models of income determination. He agreed, however, that an analysis of the long-run consequences had to rely on a dynamic model taking account of other considerations affecting capital formation.

Edmund Phelps elaborated on some of the distinctions and interrelationships between short-run and longer-run adjustments. For the short run, he felt that an unanticipated antirecessionary increase in public expenditure might exert its primary stimulative effect on inventory investment, fixed investment being largely predetermined for quite a while. But if an addition to public expenditure is expected to persist over the long run and to have ultimately an unfavorable impact on fixed capital formation, that adverse expectation may affect the short run. Under those conditions, the stock market might fall promptly, and thus fixed investment might be dampened rather than stimulated in the near term.

Michael Wachter and Martin Feldstein suggested that crowding in would appear less likely in a more realistic model that took into account supply constraints and some degree of price flexibility. Wachter surmised that the various elasticities might be different at various stages of the cycle, shifting toward the crowding out result in periods of high utilization. Feldstein stressed that price flexibility strengthened the traditional mechanism of crowding out: the inflation generated by fiscal stimulus would reduce real monetary balances. Friedman agreed that the supply side effects from which he had abstracted would push toward crowding out; but he noted that he had also abstracted from accelerator effects on investment that would push toward crowding in.

The discussion also focused on the policy implications of Friedman's analysis. Dornbusch doubted that changing the maturity structure of the federal debt could be an effective policy, and cited evidence that short-term and long-term securities are highly substitutable. Franco Modigliani shared that view, and reported on his analysis of the one historical attempt to affect interest rates through debt management, the so-called operation twist. He had found that the changes in the relative supplies of long-term and short-term debt had had no effects. Moreover, this was fortunate since actually operation twist had lengthened, rather than shortened, the maturity of the debt—accomplishing the opposite of what had been intended. Friedman noted, however, that the recent degree of lengthening of the debt was considerably larger than that during operation twist. He stressed furthermore that both Modigliani's research and that mentioned by Dornbusch had used an unrestricted reduced-form meth-

odology in contrast to the richer structural approach adopted in his own and Roley's work that had found much lower estimated elasticities of substitution.

Modigliani was also unconvinced that wealth played an important role in the money-demand function. He agreed with Goldfeld's interpretation of the empirical results. Furthermore, he stressed that the demand for money in recent years *should* be overpredicted by any function not explicitly allowing for recent innovations in banking that had facilitated economizing on demand deposits. Thus he was skeptical of any equation that fitted well without allowing for these institutional changes. Moreover, he saw no analytical reason for wealth to influence money demand. Friedman countered that portfolio shifts generate a transactions demand for cash, and their magnitude had to be related to the total size of portfolios; that alone could account for the small, although significant, wealth elasticity he had found.

Robert Hall suggested that the paper presented an overly optimistic view of fiscal policy by underestimating the interest elasticity of investment demand. He contrasted Friedman's use of differing estimates of the interest elasticity of the demand for money with his concentration on a single estimate of that of investment demand. Hall considered that estimate implausibly low. Estimates of the interest-elasticity of investment demand that seemed more accurate to Hall implied that fiscal policy would have little efficacy. Friedman mentioned a paper by Olivier Blanchard that had obtained similar empirical results to his on the interest elasticity of investment while using an analytical approach more sympathetic to Hall's. But he felt that professional knowledge of that magnitude was weak and identified it as an important item on the agenda for future research.

Speaking of other research needs for the future, William Brainard emphasized the difficulty of estimating the required rate of return on capital, which is important in determining the relative substitutability among assets relevant to the crowding-out question. The valuation of corporations reflects the "expected marginal product of capital"—profit expectations, taxes and the like as well as the required return on real capital. It is difficult to distinguish changes in the required return from changes in these other factors affecting market valuation. He also indicated that his joint work with Tobin, which used panel data to study the determinants of the valuation of firms, suggested substantial year-to-year changes in the

required return on capital and a looser relationship than is typically assumed between this rate and the rates of return on financial assets.

William Poole pointed out that the typical discussion of fiscal policy ignored the effect of inflation in reducing the real value of the federal debt. When the real capital losses of bondholders are taken into account, it becomes evident that fiscal policy has been much less stimulative in recent years than it appears when viewed in purely nominal terms. Modigliani strongly supported Poole's point, and emphasized that the national income accounts should be adjusted to reflect it: a major portion of government interest payments does not really represent income, but is merely a restitution of the real principle of bondholders. George Jaszi said that, while it was potentially an important economic phenomenon, the estimation of the inflation premium in federal interest payments posed analytical issues that lay outside the scope of accounting procedure.

Michael Boskin noted a number of other measurement and conceptual issues about the public debt. In principle, as he saw it, the key fiscal variable is the debt of total government—federal, state, and local—and that total has been declining relative to GNP in recent years. He also identified as an important unsettled issue the appropriate treatment of the implicit debts associated with government commitments for future benefits in social insurance programs.