

The New Monetarism

by *Nicholas Kaldor*

THE Keynesian Revolution of the late 1930s has completely displaced earlier ways of thinking and provided an entirely new conceptual framework for economic management. As a result, we think of day-to-day problems—of inflationary or deflationary tendencies, unemployment, the balance of payments or growth—on different lines from those of economists of earlier generations. We think of the pressure of demand as determined by autonomous and induced expenditures, and we seek to regulate the economy by interfering at various points with the process of income generation: by offsetting net inflationary or deflationary trends emanating from the private sector or the overseas sector by opposite changes in the net income generating effect of the public sector. Previously, economists had thought of the level of demand—the volume of spending—as being directly determined by the supply of money and the velocity of circulation; and thought of regulating the level of expenditure mainly by monetary controls.

For the last twenty or thirty years we have felt we have much better insight into the workings of the market mechanism than our predecessors, and felt much superior to them. However, we now have a “monetary” counter-revolution whose message is that during this time we have been wrong and our forbears largely, if not perhaps *entirely*, right; anyhow, on the right track, whereas we have been shunted on to the wrong track. This new doctrine is assiduously propagated from across the Atlantic by a growing band of enthusiasts, combining the fervour of early Christians with the suavity and selling power of a Madison Avenue executive. And it is very largely the product of one economist with exceptional powers of persuasion and propagation: Professor Milton Friedman of Chicago. The “new monetarism” is a “Friedman Revolution” more truly than Keynes was the sole fount of the “Keynesian Revolution”. Keynes’s *General Theory* was the culmination of a great deal of earlier work by large numbers of people: chiefly Wicksell and his followers, Myrdal and Lindahl in Sweden, Kalecki in Poland, not to speak of Keynes’s colleagues in Cambridge and of many others.

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The new school, the Friedmanites (I do not use this term in any pejorative sense, the more respectful expression "Friedmanians" sounds worse) can record very considerable success, both in terms of the numbers of distinguished converts and of some rather glittering evidence in terms of "scientific proofs", obtained through empirical investigations summarized in time-series regression equations. Indeed, the characteristic feature of the new school is "positivism" and "scientism"; some would say "pseudo-scientism", using science as a selling appeal. They certainly use time-series regressions as if they provided the same kind of "proofs" as controlled experiments in the natural sciences. And one hears of new stories of conversions almost every day, one old bastion of old-fashioned Keynesian orthodoxy being captured after another: first, the Federal Reserve Bank of St. Louis, then another Federal Reserve Bank, then the research staff of the IMF, or at least the majority of them, are "secret", if not open, Friedmanites. Even the "Fed" in Washington is said to be tottering, not to speak of the spread of the new doctrines in many universities in the United States. In this country, also, there are some distinguished and lively protagonists, like Professor Harry Johnson and Professor Walters, though, in comparison to America, they write in muted tones and make more modest claims; which makes it more difficult to discover just what it is they believe in, just where the new doctrine ceases to be a matter of semantics and becomes a revelation with operational significance.

ELEMENTS OF NEW DOCTRINE

What are the essential propositions of the new doctrine? For this, it is no good turning to the "moderates", who do not really say anything, or to the "extremists"—like Messrs. Anderson, Jordan and Keran of St. Louis—who both vulgarize and discredit the new creed by the blatant simplicity of their beliefs and the extravagance of their claims. One must turn to the archpriest, Friedman himself, and such of his close disciples, like Meiselman, Anna Schwartz and Philip Cagan, who can be relied on to follow the master closely and interpret him correctly.

The essential elements of the creed can, I think, be summarized in the following four propositions:

(1) Money alone matters in determining "*money things*", such as the *money* GNP, the level and the rate of changes of *money* prices, and the level and the rate of change of *money* wages. Per contra, other things—such as fiscal policies, taxation, trade union behaviour, etc.—do not (or do not really) matter.

(2) Money cannot change "*real*" things, except temporarily, and in the manner of throwing a spanner into the works—a "monkey-wrench into the machine", to use Friedman's more homely expression¹—at the cost of painful adjustments afterwards. There is a unique real equilibrium rate of real interest, a unique real equilibrium real wage, an equilibrium level of real unemployment. By monkeying around with money, these things can temporarily be made to change—interest reduced, unemployment cut, the real wage cut (or raised, I am not sure which)—only by making, in each case, reverse changes (abnormally high interest rates, abnormal unemployment, etc.) the inevitable sequel.

All this part of the Friedman doctrine is closely reminiscent of the Austrian school of the 'twenties and the early 'thirties—the theories of von Mises and von Hayek—a fact which so far (to my knowledge) has received no acknowledgment in Friedmanite literature. (Very few people these days know the works of the Mises-Hayek school; unfortunately, I am old enough to have been an early follower of Professor Hayek, and even translated one of his books, and there is nothing like having to translate a book, particularly from the German language, to force you to come to grips with an argument.) Friedman differs from Mises and Hayek in being more liberally spiced with the new empiricism. On the other hand, he misses some of the subtleties of the Hayekian transmission mechanism, and of the money-induced distortions in the "structure of production."

(3) While the money supply alone determines money expenditures, incomes and prices, it does so with a time lag which is, unhappily, not a stable one. It can vary, for reasons yet unknown, between two quarters and eight quarters. This is what the regression equations show.

(4) Hence, while control of the money supply is the only powerful instrument of control, it is hopeless for central banks to pursue a positive stabilization policy by varying the money supply in a contra-cyclical manner. Indeed, their attempts to do so may have been the very cause of the cyclical instabilities in the economy which they aimed to prevent. Hence, the best thing for stability is to maintain a steady expansion of the money supply of 4–5 per cent. (in the latest version, the ideal has come down to 2 per cent.) and, sooner or later, everything will fall into line. There will be steady growth without inflation.

All this is argued not, like the Keynesian theory, in terms of a

¹ "The Role of Monetary Policy", *American Economic Review*, March, 1968, p. 12.

structural model which specifies the manner of operation of various factors. The results are based on direct and conclusive historical evidence; on statistical associations which appear—to the authors—so strong and clear as to rule out other interpretations. The actual mechanism by which exogenous changes in the supply of money influence the level of spending—how the money gets into circulation, who it is received by, whether the recipients treat it as an addition to their spendable income or to their wealth, or whether it comes into existence in exchange for other assets without augmenting either wealth or income—is hardly considered by the orthodox Friedman school. It is significant perhaps that when Friedman in his latest essay does attempt a graphic description of how an increase in the money supply leads to a rise in prices and incomes, the money is scattered to the population from the air by a helicopter.¹

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The basis of all this is the “stable demand function for money”, derived from empirical observations over longer and shorter periods; with varying definitions of money, and varying time lags between changes of money and income, where the choice of the time lag, and the choice of the definition of what is “money”, are both determined by the criterion of the best statistical “fit” (in terms of R^2 and “t” values) for the regression equation. It is sometimes expressed in terms of a “money supply multiplier” which is clearly implied by the “stable demand function”, though the empirical values of the “multiplier” are not consistent with a unity elasticity in the demand for money (*i.e.*, an equi-proportional relationship between the change in money and that of money income) which the quantity theory postulates; sometimes in terms of a relationship between changes in the money supply and changes in consumption expenditure, together with the demonstration that the money multiplier invariably “outperforms” the Keynesian multiplier. (This latter contention, for what it is worth, has been shown to be dependent on arbitrary and inappropriate definitions of “autonomous” expenditures in a Keynesian model.)²

Friedman interprets his empirical findings in a strict Walrasian (or Marshallian) manner, as an indication that “people” wish to keep a constant proportion of their real income (or their permanent real income) in the form of money, a proportion which is not (very)

¹ Cf. Friedman *The Optimum Quantity of Money*, Macmillan, 1969, pp. 4ff.

² Cf. Ando and Modigliani, “Velocity and the Investment Multiplier”; De Prano and Mayer, “Autonomous Expenditures and Money”; together with Replies and Rejoinders, *American Economic Review*, September, 1965.

sensitive to interest rates. But who are “the people” in this connection? Are they the wage- and salary-earners, who, between them, account for 70 per cent. of the national income, but hold, at any one time, a much lesser proportion, perhaps 10 to 20 per cent., of the total money supply? Or are they the “rentiers”, whose “portfolio selection” and “portfolio shifts” are much influenced at any time by short-term expectations, as well as by the relative yields of various types of financial assets? Or are they businesses, for which holding money is just one of a number of ways of securing liquidity—unexploited borrowing power, unused overdraft limits and so on being other ways—and for which the state of liquidity is only one of a number of factors that influence current expenditure decisions?

RELATION OF MONEY TO GNP

Before we consider these contentions further, one might pause to ask whether there is anything surprising in a “stable money” function.

Clearly, in a broad sense the “money supply”, however defined, correlates with the money GNP—so does everything else: consumption, investment, wealth, the wage-bill, etc. All these things move over time, normally upwards, and in any time series the movement of any one item is bound to be highly correlated with the others. Thus Richard Stone demonstrated years ago that for the U.S. economy in the inter-war years *all* principal items of income and expenditure (eighteen of them) were closely correlated with three independent factors, which he identified as the GNP, the change in the GNP and a time trend.¹

The important questions to ask are:

First, does a high correlation indicate a causal relationship either way? Does it imply that the supply of money determines the level of income, or the other way round? Or are both determined simultaneously by a third factor (or factors)?

Second, does the existence of a strong statistical association imply that by controlling one of the variables, say the money supply, one can induce a predictable variation in the other? In other words, would the “money multiplier” survive if it were subjected to serious pressure?

In the U.K., the best correlation is undoubtedly found, not between the so-called “money supply” and the GNP, or that and consumers’ expenditure, but between the quarterly variation in the

¹ R. Stone. “On the Interdependence of Blocks of Transactions”, *Journal of the Royal Statistical Society, Supplement*, vol. 8 (1947).

amount of *cash* (that is, notes and coins) in the hands of the public, and corresponding variations in personal consumption at market prices.¹ This, of course, was broadly known long before multiple regressions were invented (or computers to calculate them with ease). Every schoolboy knows that cash in the hands of the public regularly shoots up at Christmas, goes down in January and shoots up again around the summer bank holiday.

Nobody would suggest (not even Professor Friedman, I believe) that the increase in note circulation in December is the cause of the Christmas buying spree. But there is the question that is more relevant to the Friedman thesis: Could the "authorities" prevent the buying spree by refusing to supply additional notes and coins in the Christmas season?

Of course, most people would say that it would be quite impossible to prevent the rise in the note circulation without disastrous consequences: widespread bank failures, or a general closure of the banks as a precautionary measure. If I were asked to advise, I would say that it could be done by less dramatic means: by instructing the banks, for example, not to cash more than £5 at any one time for each customer; by keeping down the number of cashiers, so as to maintain reasonably long queues in front of each bank window. If a man needed to queue up ten times a day, half an hour a time, to get £50 in notes, this would impose a pretty effective constraint on the cash supply.

But would it stop Christmas buying? There would be chaos for a few days, but soon all kinds of money substitutes would spring up: credit cards, promissory notes, etc., issued by firms or financial institutions which would circulate in the same way as bank notes. Any business with a high reputation—a well-known firm which is universally trusted—could issue such paper, and any one who could individually be "trusted" would get things on "credit". People who can be "trusted" are, of course, the same as those who have "credit"—the original meaning of "credit" was simply "trust". There would be a rush to join the Diners Club, and every one who could be "trusted" to be given a card would still be able to buy as much as he desired.

The trust-worthy or credit-worthy part of the population—

¹ Thus, for 83 observations in the period 1948–69, the R^2 is .884, the "cash multiplier" 2.3, the "t" value 3.7, after allowing for seasonality. The "cash multiplier" is 6.1, the "t" value 9, the R^2 is .494, without correction for seasonality. Even better sounding results can be attained by relating the change in expenditure to both current and lagged changes in the cash supply, lagged for each of the four quarters, which yield positive and negative multipliers in regular sequence—which only goes to show what "t" values and R^2 s are worth. (For equations, see Appendix on page 18.)

the people who can be trusted not to spend in excess of what they can afford to spend—would thus live on credit cards. The rest of the population—the mass of weekly wage-earners, for example, who have no "credit", not being men of substance—would get paid in chits which would be issued in lieu of cash by, say, the top five hundred businesses in the country (who would also, for a consideration, provide such chits to other employers). And these five hundred firms would soon find it convenient to set up a clearing system of their own, by investing in some giant computer which would at regular intervals net out all mutual claims and liabilities. It would also be necessary for the member firms of this clearing system to accord mutual "swops" or credit facilities to each other, to take care of net credit or debit balances after each clearing. When this is also agreed on, a complete surrogate money-system and payments-system would be established, which would exist side by side with "official money".

CHARACTERISTICS OF MONEY

What, at any time, is regarded as "money" are those forms of financial claims which are commonly used as means of clearing debts. But any shortage of commonly-used types is bound to lead to the emergence of new types; indeed, this is how, historically, first bank notes and then chequing accounts emerged. To the extent that no such new forms have emerged recently—in fact, they are emerging, though not as yet in a spectacular way—this is only because the existing system is so managed as to make it unnecessary—with the "authorities" providing enough money of the accustomed kind to discourage the growth of new kinds. They thereby also condition our minds into thinking that money is some distinct substance, a real entity, whose "quantity" is managed and controlled quite independently by the monetary authority.

Of course, within limits, the ultimate monetary authority can and does exercise control over the volume of borrowing, because it can control interest rates, particularly at the short end, through open market operations, far more powerfully than other operators; and because, within limits, it can control the volume and direction of lending by the clearing banks, which have such a powerful rôle in the system as suppliers of credit. But, as the Radcliffe Committee has shown, when credit control is operated as an independent instrument—as a substitute for fiscal policy, and not as a complement to it—any forceful initiative by the monetary authorities weakens their hold over the market by diverting business from the

clearing banks to other financial institutions. The post-war experiments in monetary policy caused a lot of disorganization—"a diffused difficulty of borrowing", in the words of Radcliffe, with firms having to borrow money from unaccustomed sources, or else to delay paying bills so as to achieve a better synchronization between receipts and outlays—but with little discernible effect on spending. When the central bank succeeds in controlling the quantity of "conventional money", lending and borrowing is diverted to other sources, and the "velocity of circulation", in terms of conventional money, is automatically speeded up.

VELOCITY OF CIRCULATION

Friedman's main contention is that the velocity of circulation, in terms of conventional money¹, has been relatively stable. That may well be, but only because, in the historical periods observed, the supply of money was unstable. In other words, in one way or another, an increased demand for money evoked an increase in supply. The money supply "accommodated itself" to the needs of trade: rising in response to an expansion, and vice versa. In technical terms, this may have been the result of the objective of "financial stabilization", of maintaining the structure of interest rates at some desired level, or the so-called "even keel policy", of ensuring an orderly market for government debt.²

¹ The precise meaning of "conventional money" differs from author to author (and from country to country); in the U.K. context it is usually defined as cash plus clearing bank deposits (both current and deposit accounts) in the hands of the public.

² A great deal of the current discussion on the importance of "money" is devoted to the issue of the "interest elasticity" of money balances—i.e. to the question of how the ratio between the "money supply" (as conventionally defined) and the national income can be expected to vary with changes in interest rates. Evidence of a low-interest elasticity is supposed to support the "monetarist school", while a high-interest elasticity is supposed to lend support to the "Keynesian" view. In fact, it does neither the one nor the other. The interest-elasticity of the demand for money really concerns a different issue: the power of the monetary authorities to vary the money supply in an exogenous manner. The *less* prepared the public is to absorb more cash in response to a reduction in interest rates, or to release cash in response to a rise, the *less* is it possible for the monetary authorities to expand the "money supply" relative to demand, or to prevent it from rising in response to a rise in the public's demand. This is because the authorities' sole policy instrument for changing the "money supply" is the buying and selling of financial assets in exchange for money; this presupposes that such sales or purchases can be effected in reasonable amounts without creating violent instabilities in the financial markets. Hence, the more Friedman and his followers succeed in demonstrating the insensitiveness of the demand for money to interest rates, the more they denigrate the rôle of money as an autonomous influence on the economy. The "stable money function" is evidence, not of the "importance of money", but only of the impotence of the authorities in controlling it. If it required a 50 per cent. fall in Consols to effect a 5 per cent. reduction (or to prevent a 5 per cent. rise) in the amount of money held by the public (i.e., assuming an interest elasticity of 0.1), any *autonomous* regulation of the "money supply" would in practice be rendered impossible by the exigencies of the financial and banking system. Those who hold that an "excess supply" of money under these

More fundamentally (and semi-consciously rather than in full awareness) it may have sprung from the realization of the monetary authorities, be it the Federal Reserve or the Bank of England, that they are in the position of a constitutional monarch: with very wide reserve powers on paper, the maintenance and continuance of which are greatly dependent on the degree of restraint and moderation shown in their exercise. The Bank of England, by virtue of successive Acts of Parliament, has a monopoly of the note issue, at least in England and Wales. But the real power conferred by these Acts depended, and still depends, on maintaining the central rôle of the note issue in the general monetary and credit system; and this, in turn, was not a matter of legal powers, but of the avoidance of policies which would have led to the erosion of this rôle.

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The explanation, in other words, for all the empirical findings on the "stable money function" is that the "money supply" is "endogenous", not "exogenous".

This, of course, is the crux of the issue, and it is vehemently denied by the monetarist school. They base their case on two kinds of evidence:

(1) The first is the time lag. Peaks and troughs in the money supply (in the U.S., at any rate) have regularly preceded peaks and troughs in GNP, though with a variable lag of two to six quarters, and one that tended to shorten in the post-war era to one quarter

circumstances would *directly* increase spending forget that, barring helicopters, etc., the "excess supply" could never materialize.

One of the main contentions of the Friedman school is that, whenever the central bank changes the money supply by open market operations, say, by selling bonds in exchange for cash, it does not follow that the individuals who buy the bonds which the central bank sells will reduce their holding of money correspondingly—they may continue to hold the same amount of money, and economize instead on the buying of "goods". In this way, it is contended, a reduction in the money supply will have a "direct effect" on the demand for goods, and not only an "indirect effect", via the rate of interest. But there is a confusion here between "stocks" and "flows". The amount of money held by an individual is part of his *stock* of wealth; if he buys additional bonds, and this purchase represents an addition to his total stock of wealth, and not merely a substitution between one form of holding wealth and another (i.e. he continues to hold the same amount of money, *plus* a larger amount of bonds) this is only another way of saying that the individual bought the additional bonds out of income (i.e. out of forgone consumption), which in plain language means that he was induced to save more as a result of the opportunity of buying bonds on more attractive terms. No one has ever denied that monetary policy operating through changes in interest rates (or through direct controls over the volume of bank lending) could have an effect on the propensity to save as well as on the inducement to invest. But, unless the monetarists assume a high-interest elasticity in the propensity to save, and attribute the major influence of monetary action to this factor (in which case this should be made explicit), they *cannot* be saying anything different from Keynes—i.e. that the effects of "monetary action" on the level of demand depend on the effects of the consequential changes of interest rates (or, what comes to the same thing, of credit rationing by the banks) on the level of investment.

or less.¹ If the money supply changes first, and the level of income (or business activity) afterwards, it is contended that the one that came first must have been the *cause* of the other.

(2) The second is the contention that in the U.S., at any rate, banks are always "loaned up", more or less. Hence, the "money supply" (which includes bank deposits, as well as notes and coins held by the public) is fairly closely related to "high-powered money"—to the so-called "monetary base", which is under the sole control of the Federal Reserve, and who exercise their power, wisely or foolishly, but quite autonomously.

In my opinion, neither of these arguments *proves* that money plays—in the U.S., let alone in the U.K.—the causal rôle: that the "money supply" governs the level and the rate of growth of money incomes or expenditures.

THE TIME LAG

With regard to the time lag, it is now fairly generally admitted that it does not prove anything about the nature of the causal relationships. If one assumed a purely Keynesian model where expenditure decisions govern incomes, and if one assumed a purely passive monetary system—with reserves being supplied freely, at constant interest rates—it would still be true that the turn-round in the money supply would precede the turn-round in the GNP, for much the same reasons for which the Keynesian multiplier invariably involves a time lag.

Suppose the initiating change is a decision of some firms to increase their inventories, financed by borrowing. The first impact is to cause some other firms whose sales have increased unexpectedly to incur some involuntary disinvestment. It is only when that is made good by increased orders that productive activity is expanded; any such expansion will cause higher wage outlays, which in turn may involve further borrowing. The ultimate effects on income involve further increases in productive activity arising from the expenditure generated by additional incomes. There is every reason for supposing, therefore, that the rise in the "money supply" should precede the rise in income—irrespective of whether the money-increase was a cause or an effect.

There may be other explanations which would need to be investigated, such as the contra-cyclical behaviour in the fiscal balance which, particularly in the U.S., has been a very important

¹ Richard G. Davis, "The Role of the Money Supply in Business Cycles", *Monthly Review of the Federal Reserve Bank of New York*, April, 1968, p. 71.

feature of the scene, especially in the post-war years. I am referring to the so-called "built-in fiscal stabilizer", which means that the fiscal deficit automatically rises in times of declining activity and automatically falls in times of rising activity. Owing to lags in tax collection, particularly in taxes on corporate profits, this operates so that the maximum swings occur sometimes after the turning point in economic activity.

Now, it is well known that changes in the government's net borrowing requirement are the most important cause of changes in the money supply. This is only partly due to the fact that the government's own balances are excluded from the "money supply", so that any depletion of such balances automatically augments the money supply. Partly it is due to the fact that the government is the one borrower with unlimited borrowing power: an increase in government borrowing, whether due to a decline in tax receipts, a rise in expenditure, or both, involves an increase in the money supply as an automatic result of a "passive" monetary policy, which supplies reserves as part of a policy of stabilizing interest rates or simply to ensure orderly conditions in the bond market.¹ Moreover, since, in the U.S. at any rate, the government's borrowing requirement is largest when the economy is depressed, it occurs at a time when the Federal Reserve system is least inclined to follow a "tough" credit policy; whilst in times when it wishes to restrain the expansion of credit, the government itself is likely to be in surplus. Hence, the large observed fluctuations in the money supply, preceding in time the business cycle, may merely be a reflection of the operation of the built-in fiscal stabilizer.

An interesting bit of evidence for this view is the abnormal behaviour of the money supply following the Korean War, when the money supply peaked about a year after, not in the year preceding, the peak of the post-Korean boom. A possible explanation is that the rise in government expenditure (and the deficit) followed on this occasion the sharp rise in activity, which was induced, no doubt, by the large rise in military procurement but which had been reflected in a sharp increase in federal expenditure only some time later on, when the bills came to be paid.

¹ As Hawtrey has repeatedly emphasized, in the case of private borrowing the maintenance of orderly conditions in the bond market invariably involved some policy of "credit-rationing" or rather "issue rationing" by the issuing houses, who made sure that the volume of issues for public subscription at any one time was no greater than what the market could absorb. This is his explanation for the long-term rate of interest being largely a "conventional phenomenon". (Cf. e.g. *A Century of Bank Rate*, London 1938, pp. 177ff.) But there is nothing equivalent to this in the case of government borrowing.

CHANGES IN MONEY SUPPLY: U.S. EXPERIENCE

This brings me to Friedman's second contention and the one on which he would himself lay the most emphasis: that in the United States, at any rate, changes in the money supply have been "exogenous" and were largely determined by autonomous policy decisions of the Federal Reserve Board. Since Friedman and Anna Schwartz have written a book of eight hundred pages to prove this point,¹ it is not easy to deal with their massive evidence in a few sentences at the tail end of a lecture. Nonetheless, I shall try, but will confine myself to some key issues and to some general observations.

In the first place, while the correlation between the "monetary base" (defined above) and the "money supply" was good in general, it was not all that good to be able to regard changes in the one as being the equivalent of changes in the other. In particular, it appears that on occasions when the Federal Reserve went out of its way to increase reserves (as in the 1929-39 period), the reaction on the total money supply was small. Moreover, the effects of changes in the "monetary base" on the "money multiplier" were consistently negative in all periods.²

More important than this, the variations in the "monetary base" are themselves explained by factors—such as the desire to stabilize interest rates, or to ensure government debt financing (the so-called "even keel" objective³)—which makes the "monetary base" automatically responsive to changes in the demand for money. In other words, if variations in the money supply were closely related to changes in the "monetary base", this is mainly because the latter has also been "endogenous", as well as the former.

Friedman himself regards the monetary history of the Great Contraction, 1929-33, as the ultimate test of his basic contention. It is worth quoting the critical passage in his Presidential Address to the American Economic Association⁴ at some length:—

The revival of belief in the potency of monetary policy was fostered also by a re-evaluation of the role money played from 1929 to 1933. Keynes and most other economists of the time believed that the Great Contraction in the United States occurred despite aggressive expansionary policies by the monetary authorities—that they did their best, but their best was not good enough. Recent

¹ *A Monetary History of the United States, 1867-1960*, National Bureau of Economic Research, Princeton University Press, 1963.

² Cf. Keran, "Monetary and Fiscal Influences on Economic Activity—The Historical Evidence", *Review of the Federal Reserve Bank of St. Louis*, November, 1969, Tables VII and VIII.

³ Keran, *op. cit.* Table VI.

⁴ "The Role of Monetary Policy", *American Economic Review*, March, 1968, p.3. (My italics.)

studies have demonstrated that the *facts* are precisely the reverse: the U.S. monetary authorities followed highly *deflationary* policies. The quantity of money in the United States fell by one-third in the course of the contraction. And it fell not because there were no willing borrowers—not because the horse would not drink. It fell because the Federal Reserve System *forced or permitted* a sharp reduction in *the monetary base*, because it failed to exercise the responsibilities assigned to it in the Federal Reserve Act to provide liquidity to the banking system. The Great Contraction is tragic testimony to the power of monetary policy—not, as Keynes and so many of his contemporaries believed, evidence of its impotence.

I cannot understand the reference to the "sharp reduction in *the monetary base*" in the above passage, which is absolutely critical to the argument. According to Friedman's own figures,¹ the amount of "high-powered money", which is Friedman's own synonym for the "monetary base" (*i.e.* currency held by the public plus member bank reserves with the Federal Reserve) in the U.S. increased, not decreased, throughout the Great Contraction: in July, 1932, it was more than 10 per cent. higher than in July, 1929, whereas it was held constant in the three previous years (1926-29). The Great Contraction of the money supply (by one-third) occurred *despite* this rise in the monetary base. This was partly because the ratio of currency held by the public to bank deposits rose substantially. This is attributed by Friedman to a confidence crisis: the public's diminished confidence in the banks. But it is important to observe that this dramatic rise in the ratio of currency held by the public to bank deposits was never reversed subsequently. In July, 1960, it was still at approximately the same level as in July, 1932, which in turn was nearly twice as high as in July, 1929. If it was a matter of confidence in the banks, why was it not reversed in the subsequent thirty years? The fact that the currency-deposit ratio was at its highest during the war years, 1944-45 (when it stood 45 per cent. *above* the July, 1932, level) suggests rather that the main explanation may lie elsewhere—in the change in the pattern of expenditure between goods (or assets) normally paid for in cash, and those normally paid for by cheque; which was due partly to the fall in the volume of financial transactions in relation to income transactions (this would explain why the deposit-currency ratio rose so much during the years of the Wall Street boom²); and partly also to the rise in the share of wages, and the fall in the share of property incomes, during the slump.

¹ Friedman and Schwartz, *op. cit.* Table B-3, pp. 803-804.

² The demand for money is usually considered as a function of income and wealth; this is legitimate on the assumption that the volume of money transactions is itself uniquely related to income and wealth. However, in times when people make frequent "switches" in their portfolios, and the volume of financial transactions is large relatively to the total value of assets, it is inevitable that the amount of money held by speculators as a group should also relatively be large, even if no one individual intends to hold such balances for more than a short period.

The other reason was the fall in the ratio of bank deposits to bank reserves—in other words, a rise in commercial bank liquidity by some 27 per cent. between July, 1929, and July, 1932—which may have reflected prudential motives by the banks, but may also have been the consequence of an insufficient demand for loans—of the horse *refusing* to drink (particularly the fall in the demand for loans for speculative purposes). There is nothing in these figures, in my view, to support the far-reaching contentions which I have just quoted; and, in a complex issue of this kind, I would put far more trust in the “feel” and judgement of contemporary observers, like Keynes or Henry Simons, than in some dubious (and tendentious) statistics produced thirty years later.

I have also perused the one hundred and twenty pages devoted to the Great Contraction in the book on the monetary history of the U.S.; and, while I would agree that he makes out a good case for saying that the policy of the Federal Reserve, particularly after Britain's departure from the gold standard, was foolish and unimaginative, and that the succession of bank failures in the course of 1932 might have been avoided if the Federal Reserve had followed more closely the classic prescription for a financial panic of Mr. Harman of the Bank of England in 1825 (quoted by Bagehot)¹—of lending like mad on the security of every scrap of respectable looking paper—I do not believe that it would have made all that difference. In particular, I do not believe that the Great Depression (with all its tragic consequences, Hitler and the second world war) would not have occurred but for Governor Benjamin Strong's untimely retirement and death in 1928. Indeed, I am not sure whether Governor Strong's policies in the years prior to 1928 might not have contributed to the financial crisis following the crash in 1929. For he kept the volume of reserves—the supply of “high-powered money”—rigidly stable in the years 1925–29. This occurred at a time when the U.S. economy and the national income was expanding, with the result that the banking system became increasingly precarious: the ratio of bank deposits to bank reserves, and the ratio of deposits to currency in the hands of the public, rose well above the customary levels established prior to the first world war, and to very much higher levels than these ratios have ever attained subsequently.²

* * *

Indeed, the best answer to Friedman's main contention is

¹ Bagehot, *Lombard Street*, London, 1873, pp. 51–52; quoted in Friedman and Schwartz, *op. cit.* p. 395.

² Friedman and Schwartz, *op. cit.* Table B-3, pp. 800–808.

provided by Friedman himself, in comparing U.S. and Canadian experience during the Great Contraction.¹ In Canada, there were no bank failures at all; the contraction in the money supply was much smaller than in the U.S.—only two-fifths of that in the U.S., or 13 against 33 per cent.—yet the proportionate contraction in money GNP was nearly the same. The difference in the proportional change in the money supply was largely offset by differences in the decline in the velocity of circulation: in the U.S. it fell by 29 per cent., in Canada by 41 per cent. This clearly suggests that the relative stability in the demand for money is a reflection of the instability in its supply; if the supply of money had been kept more stable, the velocity of circulation would have been more *unstable*.

This last statement may appear to be in contradiction to Friedman's empirical generalization according to which the movement in the velocity of circulation in the U.S. has historically been positively correlated with movements in the money supply—the velocity of circulation was at its most stable when the money supply was most stable. But the two propositions are not inconsistent, which shows how easy it is to draw misleading conclusions from statistical associations. If one postulates that it is the fluctuation in the economy that causes the fluctuations in the money supply (and not the other way round), but that the elasticity in the supply of money (in response to changes in demand) is less than infinite, then, the greater the change in demand, the more *both* the supply of money and the “velocity” will rise in consequence. If the supply of money had responded less, the change in velocity would have been greater; if the supply of money had responded fully, no change in velocity would have occurred (under this hypothesis).

WHAT ABOUT BRITAIN?

In this country, at least since the second world war, it is even less plausible to argue that the “money supply” is under the direct control of the monetary authorities, regulated through the rate of creation of bank reserves. Clearly, it is not controlled through the 8 per cent. minimum cash ratio, for there is an agreement between the Bank and the clearing banks to supply sufficient reserves to validate this ratio week by week without any window-dressing. Nor can it be said that the “money supply” is controlled by the agreement of the clearing banks to observe the 28 per cent. prudential liquidity ratio, since there are numerous ways open to the banks to maintain this latter ratio which do not involve recourse to central bank credit.

¹ *Ibid*, p. 352.

What, then, governs, at least in the U.K., the changes in "money supply"? In my view, it is largely a reflection of the rate of change in money incomes and, therefore, is dependent on, and varies with, all the forces, or factors, which determine this magnitude: the change in the pressure of demand, domestic investment, exports and fiscal policy, on the one hand, and the rate of wage-inflation (which may also be partly influenced by the pressure of demand), on the other hand. This basic relationship between the money supply and GNP is modified, however, in the short period by the behaviour of the income-expenditure relation (or, as I would prefer to call it, the receipt-outlay relation) of those particular sectors whose receipt-outlay relation is particularly unstable—in other words, whose net dependence on "outside finance" is both large and liable to large variations, *for reasons which are endogenous, not exogenous, to the sector*. This is true, of course, to a certain extent of the business sector, though business investment in fixed capital and stocks has not been nearly as unstable in the last twenty years as it was expected to be in pre-war days. But it is chiefly true of the public sector, whose "net borrowing requirement" has been subject to very large fluctuations year by year. I am convinced that the short-run variations in the "money supply"—in other words, the variation relative to trend—are very largely explained by the variation in the public sector's borrowing requirement.¹

Over the last five years we have witnessed a dramatic change in the rate of increase in the money supply: it fell from 9·8 per cent. in 1967 to 6½ per cent. in 1968 and to only 2·9 per cent. in 1969. The last of these years has also witnessed a dramatic turn-round in the balance of payments. This is regarded as a "feather in the cap" for the monetarists, who point with pride to the effectiveness of monetary policy—not in stopping wage and price inflation, for this unfortunately has not happened—but at least in restoring a healthy balance of payments. They forget that the same period witnessed an even more dramatic turn-round in the net borrowing requirement of the public sector—from over £2,000 millions in 1967–68 to *minus* £600 millions in 1969–70. The recent "credit squeeze" is not really a "credit squeeze" but a "liquidity squeeze". It is a direct consequence of a big fall in the receipt-expenditure

¹ In fact, a simple regression equation of the annual change of the money supply on the public sector borrowing requirement for the years 1954–1968 shows that the money supply increased almost exactly £ for £ with every £1 increase in the public sector deficit, with $t = 6.1$, $R^2 = .740$, or, in fashionable language, 74 per cent. of the variation in the money supply is explained by the deficit of the public sector *alone*. (See Appendix.)

relationship of the business sector which, in turn, was a reflection of the big improvement in the receipt-expenditure relationship of the public sector, only partially offset by the (more recent) improvement in the receipt-outlay relationship of the overseas sector.

* * *

What, if anything, follows from all this? I have certainly no objection to Friedman's prescription that the best thing to do is to secure a steady expansion of x per cent. a year in the money supply. But I doubt if this objective is attainable by the instruments of monetary policy in the U.S., let alone in the U.K. If it is ever attained, it will be because, contrary to past experience, we shall succeed in avoiding stop-go cycles emanating from abroad, or from the private business sector, or, what is more likely, from the very changes in fiscal policy which aim to compensate for other instabilities; and if, by some combination of incomes policy and magic (but more by magic), we shall also succeed in keeping the rate of increase in money wages in both a stable and a reasonable relationship to the rate of growth of productivity.

March, 1970

Nicholas Kaldor

TECHNICAL APPENDIX

- I *Regression Equations relating Changes in Consumers' Expenditure in the U.K. to Changes in Currency in Circulation held by the Public.*
 Data: Quarterly changes in £ millions; 1948 II—III to 1969 II—III
 Notation: ΔC = Change in Consumers' Expenditure
 ΔN = Change in average currency in circulation with the public;

d_1
 d_2
 d_3
 d_4 } Dummy variables = 1 for quarter to quarter changes $\left\{ \begin{array}{l} \text{I—II} \\ \text{II—III} \\ \text{III—IV} \\ \text{IV—I} \end{array} \right\}$, 0 otherwise.
 Standard deviation in brackets; R^2 unadjusted; s = standard error (adjusted for degrees of freedom).
 Lags in quarters denoted by negative subscripts.

Results:

$$\begin{aligned} \Delta C &= -65.99 + 6.127\Delta N & R^2 &= 0.494 \\ & \quad (24.23) \quad (0.681) & s &= 183.8 \\ \Delta C &= 170.35 - 166.77d_2 + 3.71d_3 - 476.48d_4 + & & \\ & \quad (38.78) \quad (29.74) \quad (40.57) \quad (49.83) & & \\ & 2.350\Delta N & R^2 &= 0.884 \\ & \quad (0.636) & s &= 89.5 \\ \Delta C &= 5.77 + 3.855\Delta N - 2.565\Delta N_{-1} + 2.725\Delta N_{-2} - & & \\ & \quad (23.52) \quad (0.639) \quad (0.411) \quad (0.417) & & \\ & 5.640\Delta N_{-3} + 4.220\Delta N_{-4} & R^2 &= 0.878 \\ & \quad (0.417) \quad (0.644) & s &= 94.62 \\ \Delta C &= 96.84 - 31.51d_2 + 37.54d_3 - 310.88d_4 + 2.624\Delta N - & & \\ & \quad (49.17) \quad (63.52) \quad (69.18) \quad (68.15) \quad (0.672) & & \\ & 2.062\Delta N_{-1} + 1.528\Delta N_{-2} - 2.205\Delta N_{-3} + 1.947\Delta N_{-4} & R^2 &= 0.920 \\ & \quad (0.706) \quad (0.699) \quad (0.702) \quad (0.665) & s &= 78.33 \end{aligned}$$

- II *Regression Equations showing the Relationship of Changes in the Money Supply in the U.K. to the Public Sector Borrowing Requirement.*

Data: Annual figures in £ millions, relating to calendar years.
 Notation: ΔM = increase in money supply.
 P = Net acquisition of financial assets by the public sector.
 Standard deviation in brackets.

Results:

$$\begin{aligned} (1) \text{ Period 1954-68} & & R^2 &= 0.740 \\ \Delta M &= -299.1 - 1.035P & s &= 210.2 \\ & \quad (0.170) & & \\ (2) \text{ Period 1960-68} & & R^2 &= 0.714 \\ \Delta M &= -246.3 - 0.979P & s &= 212.1 \\ & \quad (0.231) & & \end{aligned}$$

The Multinational Enterprise

By John H. Dunning

INDIVIDUALS, firms and businesses have long traded with each other across national boundaries; to this extent, the internationally-oriented enterprise is no new phenomenon. Similarly, the economic prosperity of nations has always been influenced by the terms on which they have exchanged goods and services. Since the early 19th century, an active international capital market has existed, while the international flow of knowledge has an even longer pedigree, dating back to the exodus of the Huguenots in the 17th century and the smuggling of drawings, designs and machinery out of Britain to the American colonies more than one hundred years later. But, until fairly recently, most international transactions had two things in common. First, each was generally undertaken independently of the other and by different economic agents. Second, most transactions were between unassociated buyers and sellers, and were concluded at market or "arm's length" prices.

During the last half century, and particularly in the last twenty years, a new and separately identifiable vehicle of international economic activity has emerged: production by the rapid expansion of foreign direct investment. The distinctive features of foreign direct investment are two-fold. First, it embraces, usually under the control of a single institution, the international transfer of separate, but complementary, "factor inputs"—notably equity capital, knowledge and entrepreneurship—and sometimes of goods as well. Nowadays, direct investment accounts for 75 per cent. of the private capital outflows of the leading industrial nations, compared with less than 10 per cent. in 1914. Payments for proprietary knowledge, e.g. royalties, technical service fees etc. between related institutions accounted for over half of all such payments made across national boundaries by British enterprises in 1968 and, in the same year, about a quarter of their manufactured exports were sent directly to their foreign subsidiaries.

The second unique quality of direct investment is that the

The author is Professor of Economics in the University of Reading. This article is an abridged version of a paper presented by Professor Dunning at a conference on the multinational enterprise held at the University of Reading, May 28-30, 1970. The full proceedings of the conference will be published by Allen & Unwin in due course.