

AN APPRAISAL OF KEYNESIAN ECONOMICS

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I

The *topic* assigned to me is, I am afraid, much too ambitious. I cannot do more than select some questions that seem to me important for an appraisal of Keynesian economics. I shall in part be going over ground I have already tried to explore at some of our earlier meetings and elsewhere, but I do hope to make some further progress.

Keynes's greatest virtue, I have always felt, was his interest in economic policy. Economic theorizing seems to me pointless unless it is aimed at what to do. All the great theorists, I think, have had policy as their central interest, even if their policy was merely *laissez faire*. If, nevertheless, I have been skeptical of theory, in its traditional form, it is because of its pretension to universality. Economic theory is an exercise in logic, involving abstraction from what the theorist regards as nonessential. Added to the simplifications of selection and emphasis is that involved in the one-thing-at-a-time method of analysis. Our dilemma is, and has always been, that, as Keynes said, without theory we are "lost in the woods." Without hypotheses for testing, we have no basis for economic inquiry. But one can reject with Bagehot what he long ago called the "All-Case" method of the German historical school, while questioning, as he did, the range of validity of what he called the "Single-Case" method of English political economy.¹ This is the kind of question that has chiefly interested me with regard to Keynesian, as well as classical, economics.

As the reference to Bagehot indicates, Keynes was not the first great English critic of classical economics. As a graduate student, nothing interested me more than the writings of the heretics. I found more penetrating discussion of the relativity of economic concepts than Bagehot's *The Postulates of English Political Economy*; and I returned repeatedly to ponder over Cliffe Leslie's savage outcry against generalizations . . . which have passed with a certain school of English economists for economic laws . . . generalizations which were once useful and meritorious as first attempts to discover causes and sequence among economic phenomena, but which have long since ceased to afford either light or fruit, and become part of the solemn humbug of economic orthodoxy.² The weakness of such men, from the stand-

¹Walter Bagehot, "The Postulates of English Political Economy," in *The Works of Walter Bagehot* (Hartford, Conn., 1889), Vol. V, pp. 249, 253.

²Thomas Edward Cliffe Leslie, "The Movements of Agricultural Wages in Europe," *stays in Political Economy* (Dublin, 1888), p. 379.

point of the impression they made on later generations of economists or their own, was that they set up no rival system.³ By the nature of their objections they could not, and had no interest in trying. The strength of Keynes, again from the standpoint of the impression he has made, stems from the fact that he did set up a rival system, for which, like his classical predecessors, he claimed universal validity. To reduce classical economics to the status of a "special" case under his "general" theory, as he so dramatically did in his single-page first chapter, was to stake out his claim on what he undoubtedly regarded as the highest conceivable level; it probably has no parallel in economic literature. But the questions remain: how valid is his system as a picture of reality, what is the range of its application, how useful is it as a guide to economic policy?

In one of the most interesting essays in *The New Economics*, Arthur Smithies, whom I have always considered a good Keynesian, says that Keynes's theory must be regarded as the beginning rather than the end, and calls upon us to construct a really "general" theory, in which Keynes's theory would be a "special" case.⁴ This is welcome evidence—and one could cite much besides in the recent work of men who have been ardent Keynesians—of a willingness to appraise Keynesian economics more critically than was apparent in the first wave of enthusiasm that greeted the appearance of *The General Theory* in the thirties. Perhaps it will help us to get away from the tendency to classify everyone as Keynesian or a non-Keynesian. That never seemed to me a helpful starting point for considering objectively either what Keynes's contribution has been or what its limitations are. I doubt, however, whether "dynamizing" Keynes's static equilibrium analysis, which is what Smithies, Klein, and other mathematical economists seem to have in view, will remove the limitations. To my mind, they are inherent in the nature of equilibrium analysis, especially when applied to income as a whole.⁵

³How they affected my own thinking about international trade theory I tried to show in my old paper, "The Theory of International Trade Reconsidered," *Economic Journal*, June, 1929. Reprinted as Chapter 12 in my book, *Postwar Monetary Plans and Other Essays* (3rd ed., New York, 1947).

⁴"Effective Demand and Employment," in *The New Economics: Keynes' Influence on Theory and Public Policy* (New York, 1947), Ch. XXXIX,

⁵The limitations of mathematical economic theory were never better expressed than by Keynes himself: "It is a great fault of symbolic pseudo-mathematical methods of formalising a system of economic analysis . . . that they expressly assume strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed; whereas, in ordinary discourse, where we are not blindly manipulating but know all the time what we are doing and what the words mean, we can keep 'at the back of our heads' the necessary reserves and qualifications and the adjustments which we shall have to make later on, in a way in which we cannot keep complicated partial differentials 'at the back' of several pages of algebra which assume that they all vanish. Too large a proportion of recent 'mathematical' economics are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the

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Keynes leaves no room for doubt that, in his view, his principle of effective demand revolutionized traditional economic theory. In the preface to *The General Theory* he speaks of "treading along unfamiliar paths/' and of his long "struggle of escape." It is clear, too, that he regarded his contribution as monetary. The evolution of his thinking covered the greater part of the interwar period, and the stages in it were marked by the *Tract on Monetary Reform* (1923), the *Treatise on Money* (1930), and *The General Theory* (1936). It is clear all the way through that he was intensely concerned with the problems of his day, and particularly with those of England. In this sense all his books are dated. The first deals with the monetary disturbances of the early twenties, with a large emphasis on international monetary policy; it is dedicated to the "Governors and Court of the Bank of England, who now and for the future have a much more difficult and anxious task than in former days."⁵ The second is a monumental work—analytical, statistical, historical—whose central theme is a monetary theory of the business cycle (mainly on closed economy lines) and a policy of control of the cycle by the central bank. There is no evidence as yet of preoccupation with unemployment as a chronic tendency, booms are emphasized quite as much as depressions (nothing interested him more than our stock market boom), underconsumption and oversaving theories are given only passing reference.

In a famous passage of *The General Theory*, every sentence of which has a special relevance for his own theory, Keynes refers to "the completeness of the Ricardian victory" as "due to a complex of suitabilities in the doctrine to the environment into which it was projected."⁶ It was, I have always felt, a similar complex of suitabilities that accounted not only for the great impression made by Keynes's theory but also for its origin. It was not a coincidence, or a misinterpretation of Keynes, that the first great development of the theory by his disciples was the stagnation thesis, that the war was regarded as a superlative demonstration of what could be accomplished to sustain employment by a really adequate volume of effective demand, and that the weight of expectation of Keynesian economists was that we would relapse after the war into mass unemployment unless vigorous antideflation measures were pursued. There is no better short statement of the stagnation thesis than that given by Keynes: "The richer the community, the wider will tend to be the gap between its actual and its potential complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols." *The General Theory of Employment, Interest and Money* (London, 1936), pp.297-298.

⁵•Preface, p. vi.

⁶ Pp. 32-33.

auction; and therefore the more obvious and outrageous the defects of the economic system. . . . Not only is the marginal propensity to consume weaker in a wealthy community, but, owing to its accumulation of capital being already larger, the opportunities for further investment are less attractive." In an article in the *New Republic* which I have often quoted, Keynes concluded: "It appears to be politically impossible for a capitalistic democracy to organize expenditure on the scale necessary to make the great experiment which would prove my case . . . except in war conditions."⁷

I find it increasingly suggested that we should distinguish between Keynes's "personal opinions" and his "theory." I agree there is often a real point in the distinction between what Keynes says and what his theory says. The book contains many obiter dicta which do not fit into the skeleton of his theory, and indeed provide in some cases valid grounds for *objection to* it. But it has been my belief that the stagnation thesis constitutes the essential content of the theory, and that as we move away from the circumstances that thesis envisaged, the difficulties for the determinancy of the theory are increased and its force as a formula for economic policy is decreased. I have, however, been skeptical of the stagnation thesis, and some of my reservations about Keynes's theory date back to that phase of the discussion.

III

Keynes's main interest was in monetary theory and policy. The development of his thinking was directed toward "pushing monetary theory back toward becoming a theory of output as a whole."⁸ His progress can be traced in the transition from $MV = PT$ to $I + C \sim Y$. There is the question in each case of distinguishing between the truism and the theory. In the traditional quantity theory (which Keynes endorsed without reservation in the *Tract*),⁹ V and T were assumed constant, or independently determined, though in the later writings on the subject this is qualified by such statements as "normally," "except in transition periods," "apart from the business cycle." On these assumptions M affected only P (though some thought the connection often ran the other way), which was a complete demonstration that money was merely a *numeraire* and could be ignored in real analysis.

⁷P. 31.

⁸July 29, 1940.

⁹*The General Theory*, Preface, p. vi.

¹⁰P. 81: "This theory is fundamental. Its correspondence with fact is not open to question." But in the accompanying footnote he quotes with approval a statement by Pigou which seems to me to raise rather than settle the essential question: "The Quantity Theory is often defended and opposed as though it were a definite set of propositions that must be either true or false. But in fact the formulae employed in the exposition of that theory are merely devices for enabling us to bring together in an orderly way the principal causes by which the value of money is determined."

The main concern of business cycle theory, whether monetary or non-monetary, has been with fluctuations of income, output, and employment. In this sense, we had half a century and more of "macro-economics" before *The General Theory* appeared. But there have been formal difficulties with both sides of the quantity equation. In Keynes's *Treatise*, so far as the "fundamental equations" were concerned, the effects of monetary changes were registered exclusively in *P*. As he later said, the equations "were an instantaneous picture taken on the assumption of a given output."¹¹ Moreover, as his critics pointed out, they were identities, his excess of investment over saving (via the quantity of money and the interest rate), his windfall profit rise, and his price rise being the same thing, with no causal relationship disclosed, so far as the equations were concerned.¹² There has been difficulty also in the business cycle literature with *MV*. *V* has often been treated as a constant (whatever the writer may have said about it in chapters outside his formal theory), or as reinforcing the effects of changes in money quantity. But there is also discussion of demand for money as a factor to be offset by control of the supply, and of the concept of the natural rate of interest as the equator of saving and investment. All these versions, I think, appear in the *Treatise*, though the last undoubtedly interested Keynes most and constitutes a main theme of the book. But the chief emphasis is on business deposits. Regarding income deposits, so crucial for his later theory, his statement in the *Treatise* is: "I incline to the opinion that the short-period fluctuations of *V*' (velocity of income deposits) are inconsiderable," which appears to mean that consumers' demand for money is not a determinant of prices or output (consumers spend what—or in proportion to what—they get), and contains no hint of the later marginal-propensity-to-consume analysis.¹⁴

¹¹ *The General Theory*, Preface, p. vii.

¹² I agree with Lawrence Klein's statement (*The Keynesian Revolution* [New York, 1947], p. 17), though it comes oddly from a mathematician, that there is more to the *Treatise* than the equations. In my own review (*Quarterly Journal of Economics*, August, 1931), I referred only briefly to them, though pointing out their truistic nature, and dealt chiefly with the responsiveness of investment and the price level to the interest rate (which seemed to me the core of the book), his monetary analysis, and my reasons for doubting the effectiveness of his central bank policy.

¹⁴ *Treatise*, Ch. J5, p. 246. It is not possible to find a consistent monetary analysis in the *Treatise*. Sometimes he speaks of business deposits *A* as interacting with income deposits, as though it were merely the quantity of the former (in response to the central-bank-determined interest rate) that mattered; at other times the main emphasis is on business deposits *B* (a part of the financial circulation); at other times, and particularly in the statistical and historical chapters, it is on transfers between "cash deposits" and "savings deposits," a part of the analysis that always seemed to me particularly oversimplified and unrealistic; see my review above. In the "bear position" there is some anticipation of liquidity preference, but, as Keynes pointed out, they are by no means the same thing (*The General Theory*, p. 173). For an interesting and suggestive interpretation of the extent to which the *Treatise* foreshadowed *The General Theory* (as Keynes thought it did), see John Lintner, "The Theory of Money and Prices," *The New Economics*, pp. 515-526.

In *The General Theory*, $MV = PT$ is replaced by $I + C = Y$, but one can readily see the old equation underneath. Y is PT . Investment and consumption are the components of income through which monetary changes register their effects. Though not in the equation, the quantity of money (together with "liquidity preference") determines the interest rate, which (in relation to the expected profit rate—"the marginal efficiency of capital") determines the volume of investment. The demand for money is broken down into the three strands that had been implicit in the analysis since Marshall. Velocity becomes the multiplier, command-over-consumption-units becomes the propensity to consume, and the distinction between the decision to save and the decision to invest becomes liquidity preference. The identity equation $I + C = Y$ becomes the causal equation $I + C(Y) = Y$. It is the development of the analysis of demand for money which constitutes, I think, the chief innovation of *The General Theory*, and upon it, and the use Keynes makes of it, mainly turns the answer to the question whether he has succeeded in "pushing back the theory of money to becoming a theory of output as a whole." But a question hardly secondary is what has become in the new theory of P . In the *Treatise*, as I have said, T was constant; in the new theory it is P that has become constant, or neutral.

Having shown the development of Keynes's income equation out of the quantity equation, I must add a brief statement of the theory in his own terms. As he sums it up on page 29, "the essence of *The General Theory*" is that "the volume of employment in equilibrium depends on (i) the aggregate supply function, (ii) the propensity to consume, and (iii) the volume of investment." The supply function is the supply price of total output, measured in unit labor costs, assumed (up to full employment) to be constant or neutral. With the cost-price level thus stabilized, changes in effective demand are registered in output and employment. Of the two components of effective demand, the schedule of the relation of consumption to income is a stable function (which may, however, have a characteristic cyclical pattern) determined by the "psychological law" of the "marginal propensity to consume," which is that as income rises a part of the increment is saved. It follows that for every point on the schedule a multiplier can be computed. With consumption and the multiplier thus given, changes in investment (the "autonomous" factor), together with their multiplied effect, determine changes in the level of output and employment, which may settle at any point (up to full employment as the limiting case) determined by the quantity of effective demand. Thus, the lower the marginal propensity to consume, at a full-employment level of income,

the greater will need to be the volume of investment if that level of income and employment is to be maintained. As a society grows richer, its marginal propensity to consume grows "weaker . . . but, owing to its accumulation of capital being already larger, the opportunities for further investment are less attractive." Therefore, the state must intervene, through monetary and fiscal policy, to compensate for the widening "gap between actual and potential production" and maintain a full employment level of effective demand.

IV

I have stated the theory baldly because that, I think, is the only way to get at its logic. After that has been done, the rigor of the assumptions may be relaxed, but this is a process of relaxing also the conclusions, and leads back to the questions I asked earlier about the validity of the theory as a picture of reality and a basis for policy.

The paradox of the book (and one of its chief weaknesses) is that while its central thesis is long run, its formal analysis is short run, not in the business cycle sense (to which Keynes devoted only a chapter of "Notes"), but, as Hicks pointed out, in the sense of Marshall's short-run equilibrium. It is in this sense a special rather than a general theory, and a theory more static than the classical theory it was intended to supplant. Moreover, as has been shown by various writers,¹⁵ some of the more novel features of Keynes's interest and wage theory rest on special assumptions, and are less damaging to classical theory (on the appropriate "level of abstraction") than he supposed. In this sense, too, he falls short of presenting an acceptable general theory.

But much of the formal wage and interest theory seems to me secondary. Keynes's main concern was monetary, and it was the quantity equation, and particularly his long meditation over the Marshallian K (plus the impact upon him of the Great Depression), that led him to formulate his income equation and his income theory. Having done so, he worked out the interest theory that seemed to him appropriate, took over such parts of traditional wage theory as seemed to fit and rejected those that seemed not to fit. His great contribution was in focusing attention upon income and in challenging on monetary grounds the assumption, implicit in classical economics, of a full employment level of income automatically sustained. But the important question to ask, I think, is not how much his theory differs in its formal *logic from* classical economics but how much it differs from business cycle theory, the relation of which to classical equilibrium theory had been becoming increasingly tenuous for at least half a century; and whether in

¹⁵ E.g., Schumpeter, Hicks, Lange, Leontief, Tobin, Modigliani.

attempting to push the analysis of economic fluctuations back into an abstract framework of equilibrium theory he has done economics a service or a disservice.

As I said earlier, the study of economic fluctuations had of course been concerned all along with "macro-economics." But the main emphasis had been placed on fluctuations in investment. To this Keynes adds little that is conceptually new, unless it is the emphasis on expectations, which comes oddly in a book that is otherwise not only static, with constant technique, but very short run. The emphasis on declining investment opportunities, though part of his central thesis, is certainly not new; it had made its appearance in each preceding major depression. As a practical problem it seems remote today, as it has in each previous period of renewed expansion.¹⁶ Yet as a statement of a long-run tendency (wars apart) it has seemed to me not only plausible but desirable that new investment should become a decreasing part of total income in an advancing society, with qualitative technological change taking over more of the role of progress on the side of supply, and the benefits going increasingly to consumption on the side of demand. But Keynes himself did not discuss technology, and in any case the real seat of his pessimism and the core of his theory lie in his views about consumption. It is here, too, that his theory differs fundamentally from business cycle theory.

V

Keynes's law of the propensity *to* consume is the important novel feature of his theory. It has been also the most controversial. It was the main question raised by my paper on "Deficit Spending" at our meeting in 1940,¹⁷ by Kuznets' review of Hansen's *Fiscal Policy and Business Cycles* in 1942,¹⁸ and (along with his attack on equilibrium economics generally) by Burns's recent papers on Keynesian economics.¹⁹

As a first statement, apart from the business cycle or other special circumstances, Keynes's "law" that as income rises consumption rises by less than unity is a plausible hypothesis; but it does not mean, nec-

¹⁶The reader is doubtless familiar with the literature of the controversy over declining opportunities for investment. In addition to the references elsewhere in the paper, I should mention (among others) Terborgh, *The Bogey of Economic Maturity* (Chicago, 1945), and Wright, "The Future of Keynesian Economics," *American Economic Review*, June, 1945, and "The Great Guessing Game: Terborgh versus Hansen," *Review of Economic Statistics*, February, 1946.

¹⁷*American Economic Review*, February, 1941; see my *Postwar Monetary Plans*, *op. cit.* Ch. 9.

¹⁸*Review of Economic Statistics*, February, 1942, pp. 31-36.

¹⁹Arthur F. Burns, *Economic Research and the Keynesian Thinking of Our Times* (New York, 1946), and also his paper on "Keynesian Economics Once Again," *Review of Economic Statistics*, November, 1947, pp. 252-267.

essarily, that consumption is the "passive" factor or that the consumption function is stable. These two assumptions—(1) that consumption is dependent on income and (2) that there is a "regular" or "stable" or "normal" relation between them, such that the consumption function can be derived as a given datum of the system and used as a basis of policy and prediction—constitute the essence of Keynesian economics. They bear a striking resemblance to the basic assumption of the quantity theory, that demand for money could be treated as a given factor, with the difference that, whereas that assumption was used to support the classical conclusion of full-employment equilibrium (apart from the business cycle), the new law of demand for money becomes the basis of the new equilibrium theory in which full employment is merely the limiting case. The whole structure rests upon the validity of the new law of the demand for money.

Historically, there seem to me to be ample grounds for doubting both the assumptions I have stated. They do not, for example, account for the effect of the rise of the automobile, a consumption good—or of new products generally—upon the growth of national income, where we have had a dynamic response of consumption and investment, each to the other. The application of an investment "multiplier" to consumption as a passive, given factor in order to account for such changes seems wholly unrealistic. Nor would, I think, any "dynamizing" of Keynes's technique by mathematical methods get us much further. Keynes's proposition that autonomous changes in investment determine changes in income, and hence in consumption (according to the "law"), is probably no better than its opposite, that spontaneous changes in consumption determine changes in income, and in investment. The *interdependence* of consumption and investment, each responding to the other—and both responding (spontaneously rather than systematically) to changing ideas, methods, resources—seems to me to be the essence of economic progress. But it does not lend itself readily to equilibrium analysis, which is probably the reason why it has been the concern of the historians and the more imaginative kind of statisticians rather than of the pure theorists. As between Keynesian and classical economics, however, the latter provides, in many respects, a more realistic point of departure for a study of progress.

The rise of consumer durable goods has been the outstanding economic phenomenon of our times. From the standpoint both of long-run growth and of business cycle behavior it raises serious questions for Keynesian analysis. Between the two wars expenditures on such goods were fully as large as those on capital goods, and their fluctuations fully as great; nor can we make any clear generalization as to which played the greater role in initiating cyclical changes. As "outlets for

saving" they played as large a role, and the same kind of role, as new investment; nor is there any more reason for applying a "multiplier" to the one kind of expenditure than to the other. They make the Keynesian statements about "oversaving," or "institutional factors which retard the growth of consumption/' or consumption as the "passive" factor, seem much less realistic than they might otherwise.

Historically, however, the growth of consumer durable goods accounts only in part for the rise in real consumption. Kuznets' paper on "Capital Formation, 1879-1938," at the University of Pennsylvania Bicentennial Conference constitutes an important landmark in the modification of Keynesian theory.²⁰ He demonstrated that, while national income rose *greatly* during that period, standards of living rose correspondingly, and the great bulk of the increase in income went into consumption. Saving, as measured by real investment, remained a constant fraction of income, with an apparent moderate tendency in the twenties (on which he does not insist) for consumption to increase relative to income.²¹ In England before the war, according to Colin Clark's data, saving had been a diminishing fraction of a growing national income for at least a generation.²² Since Kuznets' paper, the "secular upward drift" of the consumption function, to which no reference is made in Keynes,²³ has become a standard part of the statement of the consumption function. Its practical effect has been to bring the plane of discussion (the possible "gap between actual and potential production") back pretty much to where it had been before Keynes wrote, by disposing of the more serious version of his law and the one which I think he himself believed—that consumption, as a society grew richer, became a diminishing fraction of income—and limiting the stagnation thesis to a discussion of declining opportunities for investment.

But while the "secular upward drift" is now regularly included in consumption function formulae, its implications for the analysis have not been sufficiently examined. One thing it means, I think, is the point mentioned earlier, the dynamic interaction of consumption and invest-

* *Studies in Economics and Industrial Relations* (Philadelphia, 1941), pp. 53-78.

* Had residential housing been counted as consumption rather than investment, the upward tendency of consumption would have been more marked.

"His figures on net investment as a percentage of national income show a decline from 12.2 per cent in 1907 to 8.1 per cent in 1924, 7.2 per cent in 1929, and 6.9 per cent in 1935. His conclusion was: "I believe the facts have destroyed the view up till now generally prevalent, that the rate of economic growth was primarily dependent upon the rate at which capital could be accumulated. The very rapid expansion at the present time [before the war] is taking place at a time of heavily diminishing capital accumulation. What is more remarkable, practically none of the capital which is being saved is being put into productive industry proper." *National Income and Outlay* (New York, 1938), p. 270.

²³ Hansen's *Fiscal Policy and Business Cycles* (New York, 1941), Ch. 11, p. 233, contains, so far as I know, his first reference to it. It is accompanied by a footnote referring to Kuznets' forthcoming data (the paper mentioned above); they were both present at the Pennsylvania Conference.

ment. No application of the growth of investment and a multiplier to the consumption existing at the beginning of Kuznets' period, on the assumption of passivity (in the way that was so commonly being done in the thirties) could ever account for the income-consumption relation at the end; and if instead we take a historical regression of the previous relation and project it forward, we are merely begging the question.

Another part of the explanation, without doubt, has been the cost reducing function of investment, with which, because it is too short run, Keynes's analysis does not deal. As I tried to show in an earlier paper, investment is significant, not primarily because of the money income and the employment provided by the capital-goods industries themselves, but because of the fact that by producing consumer goods in more efficient, and therefore cheaper, ways it releases consumer income for expenditure on other goods and services, and by increasing productivity per worker makes possible upward adjustments of income and increased voluntary leisure. This has been the heart of the productive process under the free-enterprise system. It points to the importance of price-wage-profits relationships which in the Keynesian system become submerged, and to the inadequacies in these directions of the Keynesian monetary and fiscal policies as the means of sustaining full employment in an advancing society.²⁴

VI

Since the war Keynesian economics has undergone a number of significant shifts. Faced with a condition of inflation as alarming, and seemingly as intractable, as the deflation Keynes faced when he wrote his book, the stagnation thesis has receded into the background of the theory. This is mainly what is meant by distinguishing between Keynes's opinions and his theory. But, as I said earlier, the difficulties for the determinacy of the theory have been increased by the new conditions, and its applicability to policy has become less clear cut. One of the new questions is the relative importance of monetary and fiscal policies—control over the broad aggregates of the income equation—as against more specific (including direct control) policies. Is Beveridge's program for full employment,²⁵ and that of the six Oxford economists,²⁶ a logical following out of Keynesian theory (as they

²⁴"Free Enterprise and Full Employment," in *Financing American Prosperity* (New York: Twentieth Century Fund, 1945), pp. 360-373; see also William Fellner, "The Technological Argument of the Stagnation Thesis," *Quarterly Journal of Economics*, August, 1941; and E. D. Domar, "The Prospect for Economic Growth," *American Economic Review*, March, 1947. This is a point I have emphasized in virtually all my papers on Keynesian economics since my review of the *Treatise*, *op. cit.*, pp. 554-555.

²⁵Lord Beveridge, *Full Employment in a Free Society* (London, 1944).

²⁶*The Economics of Full Employment* (Oxford: Oxford Institute of Statistics, 1944).

assume) or a contradiction of it? Keynes did not favor a planned or regimented economy (except in war), and regarded his theory as a defense against it. Another important set of questions relates to the cost-price effects of monetary expansion, which seemed secondary in deep depression when there were large unemployed resources. Another relates to the longer-run relations of costs, prices, profits, productivity which Keynes's analysis ignores, but which seem to me more important for stability and progress than the short-run monetary factors which his theory selects for emphasis.

Most interesting has been the postwar development of the consumption function. Keynes's book, despite his distrust of mathematics, has undoubtedly given a great impetus to the study of econometrics, and the consumption function in particular has given the mathematicians, whether Keynesian or non-Keynesian, an ideal concept for building models of national income and making forecasts. Thus far, the forecasts have been almost uniformly bad. Though I am quite incompetent to judge, my suspicion has been that the explanation is twofold: first, the stagnation bias carried over from prewar Keynesian economics; second, the fact that in the depressed thirties the income-consumption relation (as well as investment) was abnormally low, reflecting consumers' insecurity and pessimistic expectations. In any event, it does seem significant that the chief error made in the forecasts has not been in the estimates of postwar investment but in the consumption function, the one element theoretically derivable from within the Keynesian system.

After the appearance of the "secular upward drift," the emphasis was on the assumed short-run stability of the consumption function. But postwar experience has cast doubt also on this. It seems now to be agreed among econometricians that the "simple relation" between income and consumption, as Keynes stated it, is unstable. In searching for a more complex relation which may have some promise of greater stability, hypotheses have been introduced which contradict Keynes's own theory. For example, liquidity is now commonly accepted as a factor affecting consumption, whereas in Keynes's theory liquidity affected only investment. Such a change strikes at Keynes's whole structure of demand for money, with its elaborately worked out separation into the three distinct strands I discussed earlier. Instead of the simple relation between current income and current consumption on which Keynes built his theory, we are today working with various hypotheses, including saving out of past income, liquid assets, capital gains, the last highest income reached in a boom, expectations of future income, and other possible factors affecting the income-consumption relation. That expectation should be brought in to explain consumption,

whereas with Keynes it affected only investment, is surely a major departure. But it seems unnecessary, and even misleading, to pick out any particular points of difference. The broad fact seems to me to be that we have nothing left of this basic concept of the Keynesian theory other than that consumption is an important component of income and deserves all the study we can give it. The same is of course true of investment, the other component of income. That this is not now being studied with equal intensity by the econometricians is doubtless due to the fact that the changes in it are not derivable from within the system and do not lend themselves as readily to mathematical manipulation.²⁷

Scarcely less significant among the postwar developments is the growing recognition of Keynes's underemphasis on the price aspect of monetary changes. As I said earlier, in deep depression this could be ignored, but the practical problem that confronts us, except in that unique condition, is that a volume of effective demand that is adequate for full employment appears to have cost-price effects which not only expand money income at the expense of real income but create a highly unstable economic situation. In other words, Keynes's stable equilibrium (even if we could concede it on other grounds) would seem not to include full employment as the limiting case, but something substantially short of that. This seems to me our most serious practical dilemma. It has both short- and long-run aspects. It presents a question whether we have to make a choice between allowing for a certain amount of slack (and fluctuation) in our use of resources, in a free-market system, or, if we insist on continuous full employment, recognizing the need for more specific controls. But this leads on to the question, not only of our scheme of values (political and social as well as economic), but also of the vitality of the system, whether in a more planned and controlled system we would not weaken the dynamic forces which promote growth and which might, with further study, be directed toward the achievement, not of stable equilibrium in any exact sense, but of a less

²⁷ Lawrence Klein has recognized that for a true equilibrium system both investment and consumption should be determinable from within the system, see "A Post-Mortem on Transition Predictions of National Product," *Journal of Political Economy*, August, 1946, pp. 302-303. He lists the relations we must know before we can make good forecasts: "A principal failure of the customary models is that they are not sufficiently detailed. There are too many variables which are classified as autonomous when they are actually induced. . . . The surplus of autonomous variables results from a failure to discover all the appropriate relationships constituting the system. In addition to the assumption function, we should have the investment function, the inventory function, the housing function, the price-formation equations, etc." In *Econometrica*, April, 1947, he made his own forecast for the fiscal year 1947, and said that if he were wrong the reason would probably be his failure to take account of the further rise of prices. (Why should not prices be predictable from within the system?) The actual price level was not significantly different from the one he chose to use; his estimate of investment was too high (though not seriously); but his forecast of national product was too low because he underestimated the consumption function.

unstable economy than we have had hitherto. Much, I think, could be accomplished through the further study of price-wage-profit practices and policies. As I said in an earlier paper, though these relations have long been a main concern of (classical) economic theory they have been overlaid in recent years by preoccupation with monetary and fiscal analysis, and the tendency has been to regard price-cost behavior as a kind of *force majeure* to be "offset" rather than corrected. It is surprising how little we know, and can agree upon, with regard to these relationships, and what course to steer in order to avoid merely (a) letting them take their course, (b) compensating for them by monetary and fiscal manipulation, or (c) subjecting them to direct control.²³

Chapter 21, on "The Theory of Prices," is for me one of the high spots of *The General Theory*. One of Keynes's characteristics was that while he was as sharp as anyone could wish in seeing possible qualifications and objections to his theory, he never permitted them to interfere with his conclusions. Chapter 21 (in which occurs the passage on mathematical economics) is an excellent discussion of the reasons why before full employment is reached, monetary expansion affects prices and costs as well as output and employment. It is interesting that the chapter runs in terms of the quantity theory of money, which suggests again that his own theory is a recast version of the quantity theory.

If there is perfectly elastic supply so long as there is unemployment, and perfectly inelastic supply so soon as full employment is reached, and if effective demand changes in the same proportion as the quantity of money, the quantity theory of money can be enunciated as follows: "So long as there is unemployment, *employment* will change in the same proportion as the quantity of money; and when there is full employment, *prices* will change in the same proportion as the quantity of money."

Inserting Keynes's new concept of demand for money, this is not a bad statement of his own theory. But he goes on to introduce five qualifications: effective demand will not change in exact proportion to the quantity of money; resources are not (a) homogeneous, and (b) interchangeable, so that their supply elasticities vary; the money wage-unit will tend to rise before full employment; the remuneration of the factors entering into marginal cost will not all change in the same proportion. I cannot reproduce the discussion here. It contains references to bottlenecks, collective bargaining, boom and depression psychology, and other factors. One would need nothing more than this chapter to explain not only the kind of dilemma that confronts us today, but the inflationary conditions of 1936-37 on a comparatively low level

²³See my statement on "The Employment Act of 1946" before the Joint Congressional Committee on the President's Economic Report, July 2, 1947, reprinted in my book, *Post-war Monetary Plans*, *op. cit.*, Appendix I, p. 240.

*Pp. 295-296.

of employment.³⁰ But so far as I can see, Keynes does nothing to resolve the dilemma, and this chapter has no place in either the logic of his theory or his policy prescription. It is on a par with similar qualifications of his fundamental equations in the *Treatise*, which he said did not "affect in any way the rigor or validity of our conclusions."³¹ In distinguishing between what Keynes says and what his theory says, it is this kind of difference that seems to me significant. I can offer no explanation of it except that it is what equilibrium analysis seems to do to us. The key, I think, lies in what Keynes says about the rise of money wage rates before full employment (he might equally have said it of any of the other qualifications): "They have . . . a good deal of historical importance. But they do not readily lend themselves to theoretical generalizations."³²

VII

I am afraid I am outrunning the space assigned *to me*, but some other topics must be briefly mentioned. Keynes's claim to having put monetary analysis into real terms depends largely on his assumption of constant prices; price and wage changes would affect the consumption function, liquidity preference, and investment. He overstated his point (with which I have long sympathized) that the interest rate does not determine saving. He was wrong in saying that investment does not affect the interest rate but is only affected by it, though we had a striking demonstration during the war of how far an easy money policy can go in freezing the rate at a low level. His point that there is a minimum rate below which liquidity preference will not permit the rate to be driven is valid but needs elaboration. So far as the time risk is concerned, our experience with a frozen pattern of rates demonstrated that rates on long-term governments would fall progressively toward the shortest. But so far as the income risk is concerned, an easy money policy widens the gaps in the interest-rate structure and suggests the need of other methods of attack. An all-out easy money policy, such as some Keynesians have favored, designed to saturate liquidity preference, carries both short-run inflationary dangers (as we are now recognizing) and longer-run dangers of undermining the whole fabric of the private capitalistic economy.³³

"One of the peculiarities of an inflationary volume of effective demand is, apparently, that the slope of the consumption function is no longer necessarily less than unity. For a discussion of this and other aspects of the behavior of the consumption function under war and postwar conditions, see a forthcoming paper, "Use of the Consumption Function in Economic Forecasting," by Robert V. Rosa.

³⁰ See my review, *op. cit.*, pp. S56-S58.

³¹ *The General Theory*, p. 102.

³² In my last talk with Keynes, a few months before his death, it was clear that he had got far away from his "euthanasia of the rentier." He complained that the easy money policy was being pushed too far, both in England and here, and emphasized interest as an

Keynes's emphasis on wages as income and on the downward rigidity of money wage rates and his insistence that unemployment could not be cured by a policy directed primarily at cutting wage rates are among his most important contributions from a practical standpoint, whatever their theoretical merits on some abstract level. But as related to monetary business cycle analysis they have always seemed to me less novel than he supposed. Monetary policy had not run primarily in terms of wage cuts but in terms of compensating for wage and price rigidities. His conclusion, moreover, is subject to two large reservations: the effect of cost reduction on investment and its effect (which he recognized) on foreign trade. Moreover, from a purely economic standpoint, there is no reason why cost-reduction policies should not be combined with monetary policies of expansion, as Sweden and Australia did with notable success in the Great Depression.

One of the points most commonly agreed upon, even by Keynesians, is that the aggregates of the income equation must be broken down. A point that has especially interested me is the need of breaking down the saving function to differentiate between business and consumers' saving. I have never understood how Samuelson's findings could be offered in verification either of Keynes's propensity to consume or of Hansen's chapter to which they are appended. His analysis yielded the striking conclusion that consumers in the aggregate spent virtually all their increases in money income and that any additional saving accompanying rising income almost wholly took the form of business saving.²⁴ The implications of such a conclusion for economic policy are of course very great.

Finally, there is the now familiar point that the Keynesian saving-investment concept (like so much else in the analysis) has tended to submerge the study of the *process* of economic change. We have again,

element of income, and its basic importance in the structure and functioning of private capitalism. He was amused by my remark that it was time to write another book because the all-out easy money policy was being preached in his name, and replied that he did think he ought to keep one jump ahead.

How greatly Keynesian fiscal policy (and war finance) have complicated the problem of varying the interest rate as an instrument of cyclical control (because of the public debt), we are only now beginning to recognize fully.

For a discussion of these and other aspects of the interest-rate problem, see my paper, "Implications of Fiscal Policy for Monetary Policy and the Banking System," *American Economic Review*, March Sup., 1942, reprinted as Ch. 10 in my book, *Postwar Monetary Plans*, *op. cit.*; see also H. C. Wallich, "The Changing Significance of the Interest Rate," *American Economic Review*, December, 1946.

²⁴See Alvin H. Hansen, *Fiscal Policy and Business Cycles*, *op. cit.*, Ch. 11, Appendix, pp. 250-260, by Paul A. Samuelson.

Samuelson's analysis is based on Kuznets' data (1919-35). For consumers he finds a marginal propensity to consume of 0.97, and for business enterprises a marginal propensity to save of 0.49. "This [business saving] accounts for most of the leakages incident upon net investment: as far as these data go, the leakages incident upon household savings are much smaller and possibly negative" (p. 257). In his conclusion (p. 260) he again emphasizes "the very sensitive relation of consumption to aggregate income payments."

as in the *Treatise*, "instantaneous pictures." How saving and investment must always be equal in real terms, and yet how sometimes the equality denotes equilibrium and sometimes it does not, has caused endless confusion. We can make some headway by differentiating between a "normal" income-saving relation and a process of adjustment to the normal relation. But Keynes does not discuss process, and "normal" saving begs the questions I raised earlier. For a study of change the Swedish *ex ante*, *ex post*, or Robertson's time-period analysis seems much more realistic.³⁵

VIII

As I look back over my paper, my appraisal of Keynesian economics seems to be mostly critical. The most difficult thing to appraise is one's own bias. No doubt my appraisal has in it some element of unfavorable reaction, both to Keynes's own showmanship and his tendency to oversimplify and overstate his case, and to the sheer mass and exuberance of the claims made by his followers in his behalf. I admit all this has been working on me for a long time. Economic instability is equaled only by the instability of economists; what we need most, and often seem to have little of, is perspective. While I have no fondness for prediction, I do believe that the wave of enthusiasm for the "new economics" will, in the longer perspective, seem to us extravagant. And perhaps it will be only then that we shall be able to appraise objectively Keynes's contribution.

Beyond question it was very great. No one in our time has shaken up economists as much or been as influential in bringing economic analysis to bear on public policy. What he has given us, in particular, is a much stronger sense than we had before of the need for consumption analysis. It was the combination of the man and the times that did it. But I do have to insist again that it was policy, in Keynes's case, that led to theory, and that the weakness (as well as the strength of the impression made) lies in the overgeneralization. What we shall probably find ourselves doing is bringing back the things he temporarily submerged, the study of the processes of short- and long-run change, the emphasis on productivity, and on price-cost-profit relationships. If the conditions to which his theory was mainly directed should reappear, we shall probably find ourselves swept far beyond the kinds of remedies he favored, and forced into things he thought his theory and policies would avoid. But if we can maintain reasonable stability and, by the study of forces and relationships he largely ignored, continue to promote growth, his policies should play an effective role in a more

* See, among recent discussions of this point, David M. Wright, *The Economic* o) Disturbance* (New York, 1947), Ch. II.

rounded economic policy. I have sympathized all along with the idea of a cyclically unbalanced budget and with tax policies designed to promote stability and growth. But these, for Keynesians, at least before the war, were relative!}' mild objectives. Moreover, these are not exclusively Keynesian policies, but have been quite as popular with economists in Sweden, for example (where Keynesian economics has never really taken hold), as anywhere else.

What I find increasingly said, as the stagnation thesis recedes into the background, and the postwar questions about the consumption function, the price effects, and the like cast further doubts upon the theory as Keynes stated it, is that (and here the analogy with the quantity equation is striking) he has arranged the elements affecting the income equation in a useful form. This, I think, is true, with all the qualifications I have made. Undoubtedly, his formulation has greatly intensified the study of national income and its composition, though it is interesting that, as I indicated earlier, men like Kuznets and Colin Clark, who have pioneered such studies, dissented from his theory.

What it comes down to is that Keynes's analysis would appeal to me more if he had not claimed too much for it. As with his predecessors, it is the pretension to universality, and the equilibrium technique, that offend me, with the further point that in his case the defect seems to me worse. There is a legitimate and important role in economics for partial equilibrium analysis but the analogy with it of the Keynesian type of total equilibrium analysis seems to me most imperfect, because in the nature of the case the "other things equal" condition is invalid. Consumption, investment, total income interact, and they comprise all the "other things." Until, at least, the econometricians make more headway in deriving them (and their parts) from "within the system," this will be the nature of my skepticism.