KEYNES TRIUMPHANT:
A STUDY IN THE SOCIAL HISTORY OF ECONOMIC IDEAS

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KEYNES'S THEORY

The rise of Keynesianism is obviously one of the most striking intellectual, social and even cultural phenomena of the twentieth century, and it is closely associated with political developments of great significance. Keynes's own expectation that his *General Theory* would "revolutionise—not, I suppose, at once but in the course of the next ten years—the way the world thinks about economic problems"[1] turned out to be astonishingly accurate: the book had an enormous influence on the development of economic thought. Moreover, the rise of Keynesianism was intimately related to the rise of the welfare state: it provided the intellectual basis for the policy of maintaining a high level of employment, and was linked to the idea of spending for social welfare purposes. One can even

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argue that the rise of Keynesianism had an impact on the development of general Western culture since World War II; the values and beliefs associated with this doctrine—its optimism and mild collectivism, the belief that a new depression could be avoided permanently and especially the idea that spending, even wasteful spending, was better than thrift when any "involuntary unemployment" existed—these all tended to undermine the individualistic values of an earlier day.

Given all this, it is odd that so little is known about the nature of the process by which Keynesianism emerged. Not surprisingly, the economists who have produced most of the literature on the subject typically take a positivist line. The "Keynesian revolution" is seen essentially as an "intellectual breakthrough," a milestone in the advance of the discipline—a view, incidentally, shared even by the few historians who have studied the subject in any depth.[2]

But if there is one thing that work in the history of science over the past three or four decades has shown, it is that the development of even the natural sciences cannot be understood in essentially positivist terms. It follows for obvious reasons that there are particularly strong reasons for assuming that the evolution of a social science like economics has to be seen in its social and cultural context. In the present case, this means more than merely admitting that in the 1930s, when Keynes's views took shape, unemployment was a pervasive social problem intimately related to basic political problems, i.e., that social concerns merely provided the occasion for theoretical development. It is rather to be assumed that the whole matrix of social attitudes and values was organically related to the particular form the theory took and to its eventual success in the world as well.

Notions of this sort are common enough. Keynes himself analyzed the triumph of Ricardian economics in similar terms:

> The completeness of the Ricardian victory is something of a curiosity and a mystery. It must have been due to a complex of suitabilities in the doctrine to the environment into which it was projected. That it reached conclusions quite different from what the ordinary uninstructed person would expect, added, I suppose, to its intellectual prestige. That its teaching, translated into practice, was austere and often unpalatable, lent it virtue. That it was adapted to carry a vast and consistent logical superstructure, gave it beauty. That it could explain much social injustice and apparent cruelty as an inevitable incident in the scheme of progress, and the attempt to change such things as likely on the whole to do more harm than good, commended it to authority. That it afforded a measure of justificiation to the free activities of the individual capitalist, attracted to it the support of the dominant social forces behind authority.[3]

The problem, however, is to go beyond a highly abstract treatment of this sort—a treatment so broad, so lacking in empirical content, that people are free either to accept it or reject it as they please.

But how is it possible to study the process through which ideas take root? One can assume in general that conceptual change has both "social" and purely intellective components (where the word *social* is used as a kind of shorthand

reference to all nonintellective factors). But how can one determine where the balance should be struck?

What I propose to do here is to examine the logic of Keynes's arguments in the *General Theory*. This will involve a more or less conventional interpretation of his assumptions in mathematical terms and then some manipulation of those assumptions to draw conclusions which will then be compared with assertions Keynes made in the book. Because of the nature of mathematical reasoning – in the final analysis because of the definition of a mathematical demonstration[4]—any divergence will be evidence of the faulty logic of the nonmathematical argument; mathematical reasoning, assuming it is carried out properly, is necessarily compelling from a purely logical point of view. The aim, however, is not to attack Keynesian theory as an end in itself. Indeed, the points made here in large measure echo the common criticisms that were directed against the theory from the very start. The only thing that is new, I believe, is that the arguments are developed explicitly on the basis of an analysis of the *internal* structure of Keynes's argument—they are not simply counterarguments that are raised against it from the outside.

What this analysis will show is, first, that there is a bias in Keynes's argument and, second, that this bias is systematic: the effectiveness of spending is exaggerated, saving is denigrated and the negative consequences of a policy of spending are played down. All this will then be taken as a kind of license sanctioning in a general way a social interpretation of the ideas in question. In other words, one takes rational analysis—this is, the view that ideas develop according to their own internal logic and that the dynamic of conceptual development is essentially autonomous—as far as it goes, and only when it breaks down does one bring in nonintellective factors. This procedure, of course, has no absolute validity; ideas that develop along purely rational lines (in mathematics, for example) may nevertheless be highly conditioned by social factors, while defective reasoning may have no social cause at all—the limited intelligence of the author, or even mere carelessness, may be sufficient explanations in themselves.[5] I take the notion that "social" interpretation is valid only to the extent that a defect in logic is first demonstrated solely as a working hypothesis: the idea is that the less a conclusion is able to stand on a firm, logical base, the more plausible it is to assume that it was shaped by social factors.[6] Moreover, it is plausible to assume that the particular nature of those factors can be inferred from the nature of the divergence between the assertions in the text and the conclusions yielded by the mathematical analysis; the nature of the bias, in other words, will point to the social determinants of the argument.

The limitations of this method are obvious, but there is one basic point to be made in its defense. One can, of course, write history from whatever point of view one likes. One can take as one's premise the idea that social factors necessarily predominate in the shaping of a doctrine like Keynesianism, or the basic assumption might be that the development of a theory of this sort has to be

understood in essentially intellective terms. But if the aim is to grasp the basic nature of the process by which such a theory emerged, interpretations of this sort simply will not do. If, for example, the problem is to assess the degree to which Keynesianism has to be interpreted in social terms, we need a method which does not provide the answer in advance (by adopting it as a premise), but rather one which allows the empirical and analytical evidence to have a significant bearing on the answer. A method of this sort, no matter how crude and imperfect it is, is preferable to one which is in the final analysis nothing more than an argument from preconception. At the very least, therefore, the analysis should throw light on the nature of doctrinal change in a field like economics: Is it the power of reason and the cogency of open-minded argumentation which basically determine the development of theory, or do ideas essentially owe their power to the values with which they are liked and to the social and political functions which they serve?

Because mathematical analysis provides an objective way of testing, at least to some degree, the intellectual quality of the theory, it enables us to get a handle on this kind of problem. It will therefore play a fundamental role here. The methods used are not very abstruse—basically just some differential calculus and high school algebra. But the use of calculus is not common in historical work, and many readers might not be able to follow the mathematical analysis in the next few sections in any detail. Anyone willing to skim through these sections and accept their conclusions on faith will not, however, lose much of substance; the main thrust of the argument can be understood even by those with no mathematical training at all.

The Postulates of the General Theory

The study of Keynesianism must begin with a close analysis of its most basic book, Keynes's *General Theory of Employment, Interest and Money*. What were the postulates of this theory? Or perhaps it might be better to begin with a somewhat different question: Can the theory be reduced to a set of basic assumptions? This question is by no means trivial, since the book's method was not really deductive; much of it in fact takes the form of an extended discourse on economic phenomena where any argument that came to mind could be used. Nevertheless, what makes the discussion into a theory is that it does have a conceptual core, which can be stated as a set of axioms; if that were missing, the book would be little more than an amorphous string of ad hoc arguments. The problem then is to state the axioms without doing serious violence to the spirit of the work as a whole. But the task is not hopeless. This analysis will be based on the conventional interpretation that emerged in its fundamentals shortly after the *General Theory* was published.

In this interpretation, the markets for labor, goods and money are analyzed separately, and the analysis can be translated into mathematical terms. In each

market demand is set equal to supply; this equilibrium condition can then be represented by an equation. Taken together, these equations define a general equilibrium toward which the economy is assumed to move. The process of change, that is, the question of dynamics, is thus not the focus of analysis. But the effect on equilibrium of changes in the underlying parameters of the system can be analyzed; this is what economists call *comparative statics*.

Although Keynes was not as explicit about this as he might have been, there can be little doubt that the *General Theory* was a theory of this sort. This is evidenced first by the centrality of the notion of "equilibrium" in his analysis and second by his immediate—indeed enthusiastic—acceptance of the early simultaneous-equations interpretation of his theory by John Hicks and Roy Harrod. Hence, claims that the simultaneous equations model was a gross distortion of Keynes's theory must be unconvincing.[7]

In the *General Theory*, Keynes began with the labor market. In the classical theory as he presented it, the two equations which together determined equilibrium in this market—the equations for the supply of, and the demand for, labor—by themselves determined both real output y and the real wage w/p (where w is the money wage and p represents the price level). For a given "production function" defined by the technology and the capital stock, relating real output to the input of labor (N), with the usual properties that increasing labor input also increases real output (i.e., that labor has a positive marginal product) and that the increase in output for each additional unit of labor diminishes as the total amount of labor increases (diminishing returns to scale), the demand for labor is determined, under the assumption of perfectly competitive, profit-maximizing firms, by setting the real wage equal to the marginal product of labor. In mathematical terms,

$$y = y(N) \qquad y'(N) > 0, \qquad y''(N) < 0; \tag{1}$$

$$y'(N) = \frac{w}{p}. \tag{2}$$

In the classical theory the supply of labor, according to Keynes, depended directly on the real wage: $N = N_s(w/p)$. But putting this equation together with Eq. (2) would yield two equations in two unknowns; it would thus be possible to solve for both the real wage and the level of employment; substituting in Eq. (1), this would define real output as well.

How then could deep and persistent unemployment, of the sort Britain suffered from for most of the interwar period, be explained? If the classical postulates defining the equilibrium level of employment were to be maintained, then the problem could only be due to a sclerotic mechanism of adjustment. In that case, the relevance of the underlying theory would be sacrificed: the concept of "equilibrium" would have little practical significance. If the clarity, simplicity and manageability of the equilibrium method of analysis were not to be lost, too

much importance could not be attached to the adjustment mechanism. The equilibrium approach had to be salvaged in some way.

Or to make the same point somewhat differently: a theory which claimed that the supply and demand functions for labor by themselves fully determined the level of employment simply denied that factors outside the labor market could have more than a transitory influence on the real side of the economy. But it was evident that such factors—most obviously monetary factors—had had a tremendous bearing on economic activity in the whole period since 1914. The dramatic shifts in the level of employment and output in this period were clearly not to be imputed merely to changes in the production function or in the labor supply curve. How could this perception be integrated into the analysis? At least one of the classical labor market postulates had to be altered. But which one?

The demand for labor function was based on the assumption of perfect competition. If this assumption were abandoned, that is, if the fact of oligopoly were recognized, then the demand for labor would no longer be a function solely of the real wage, and the labor market would no longer by itself determine real ouput and the level of employment. Keynes would then have the opening he needed; changes in spending and the money supply could then be shown to have an effect on real economic activity.

Keynes did not take this route, explicitly accepting the classical postulate on the demand for labor.[8] His reasoning remains somewhat obscure. Did he consider challenging the assumption of perfect competition, but then decide that this would be taken as too radical a departure? Much of the rationale for capitalism was based on the argument that perfect competition guaranteed the most efficient allocation of resources, and although oligopoly was a fact of life in many sectors of the economy, it was important to maintain the idea that the economy at least worked as though it were a system of perfectly competitive firms. To deny this might be interpreted as an anticapitalist position. Can one assume that someone as politically sensitive as Keynes, and so concerned that his work affect policy, maintained the assumption of perfect competition on the basis of this kind of consideration?

Unfortunately, there is little evidence one way or another. We know only that Keynes gave little attention to the question. Presumably if he discussed these issues with anyone at all, it would have been with his close collaborator Joan Robinson; her *Economics of Imperfect Competition* was first published in 1933. Yet according to Mrs. Robinson, Keynes was not "much interested in imperfect competition," and this is a view borne out by a rapid survey of the Keynes papers.[9]

In any case, given Keynes's acceptance of the classical argument about the demand for labor, he had little choice but to challenge the assumption about labor supply.[10] But here too there is a problem of interpretation. He rejected the classical postulate about the supply of labor being a function of the real wage, but did not say unambiguously what he proposed to replace it with. Is the amount of

labor offered solely a function of the money wage, $N = N(w)$? Did he make the stronger assumption that this function had infinite slope, that is, that money wages were determined exogenously, $w = w_0$ (where w_0 is a constant)? At times he came close to saying these kinds of things, but much of this was an artifact of a certain reluctance or inability on his part to conduct the analysis in functional terms. Keynes considered the effect of labor "offering its services at a gradually diminishing money-wage," and not the effect of an upward shift in the labor supply function.[11] For it is clear that he did not believe that the money wage was determined exogenously, as the function $w = w_0$ would imply. Indeed, he explicitly stated that the wage unit tended to rise when demand increased.[12] Nor did he ever contend, in so many words, that the price level had nothing at all to do with the supply of labor. In fact, at the beginning of the book, where he dealt with the question most directly, he was always careful to temper his assertions with a qualifying phrase:

> In other words, it may be the case that *within a certain range* the demand for labour is for a minimum money-wage and not for a minimum real wage.
>
> Now ordinary experience tells us, beyond doubt, that a situation where labour stipulates (*within limits*) for a money-wage rather than a real wage, so far from being a mere possibility, is the normal case.[13]

From this it seems that the most one can infer is that labor supply is determined by both money wages and prices, but in some sense more by wages than by prices—that is, that an equiproportionate rise (or fall) in both does have an effect and, in fact, increases (or, respectively, decreases) the amount of labor offered. To formulate this relationship in mathematical terms, we assume that the labor supply function is not homogeneous of degree 0 and that in fact

$$N = N_s(w, p), \frac{\delta N_s}{\delta w} > 0, \frac{\delta N_s}{\delta p} \leq 0, \frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N}{\delta p} > 0. \qquad (3)$$

The proof of this equivalence is sketched in Appendix I.

An assumption of the sort laid out in Eq. (3) is sometimes said to presume the existence of "money illusion" on the part of labor. The other versions of the Keynesian labor supply function mentioned above, $N = N_s(w)$ and $w = w_0$, are in fact special cases of this assumption; so whatever is proved on this basis must follow also for these special cases.

Putting Eqs. (2) and (3) together will simplify the final system somewhat. By Eq. (2), $w = py'(N)$. Substituting in Eq. (3) yields $N = N_s(py'(N), p)$, which defines a new equation, $N = N(p)$. Similarly, $y = y(N) = y(N(p))$ defines the new function $y = y_s(p)$, which will be called, using this term to mean something other than what Keynes meant by it, the *aggregate supply function*. To find the

slope of these functions, differentiate Eqs. (2) and (3) with respect to p, and solve for dN/dp:

$$y''(N)\frac{dN}{dp} = \frac{1}{p}\frac{dw}{dp} - \frac{w}{p^2};$$

$$\text{so } \frac{dw}{dp} = py''(N)\frac{dN}{dp} + \frac{w}{p}.$$

$$\frac{dN}{dp} = \frac{\delta N_s}{\delta w}\frac{dw}{dp} + \frac{\delta N_s}{\delta p} = \frac{\delta N_s}{\delta w}\left(py''(N)\frac{dN}{dp} + \frac{w}{p}\right) + \frac{\delta N_s}{\delta p}.$$

$$\text{So } \frac{dN}{dp} = \frac{(w/p)\,\delta N_s/\delta w + \delta N_s/\delta p}{1 - py''(N)\,\delta N_s/\delta w}.$$

It follows immediately from previous assumptions that dN/dp is positive, and thus dy/dp = y'(N) dN/dp is also greater than zero. Thus summing up the assumptions made so far, we have

$$y = y_s(p), \qquad y_s'(p) > 0. \tag{4}$$

An important conclusion about the shape of this curve immediately follows. Since the supply of labor is physically finite, it is obvious that there exists an N* such that N(p) is less than N* for all p. The N(p) function is thus bounded from above; let $\bar{N}$ be the least upper bound. In that case N(p) must approach $\bar{N}$ asymptotically; similarly, $y_s(p)$ must approach a value $\bar{y} = y(\bar{N})$ asymptotically (see Figure 1).

Note that if we allowed the labor supply function to be homogeneous of degree 0, so that we could rewrite Eq. (3) as $N = N_s(w/p)$, we would have

$$\frac{\delta N_s}{\delta w} = N_s'\left(\frac{w}{p}\right)\frac{1}{p} \text{ and } \frac{\delta N_s}{\delta p} = -N_s'\left(\frac{w}{p}\right)\frac{w}{p^2};$$

$$\text{so } \frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} = 0,$$

and therefore $N'(p) = y_s'(p) = 0$. This is what Keynes defined as full employment; "involuntary unemployment" referred to a situation where $N'(p) > 0$. The flat portions of the N(p) and $y_s(p)$ curves thus approximate a "full employment" situation — one, that is, where shifts in demand have no effect on the level of employment. But strictly speaking the nonhomogeneity assumption means that full employment in Keynes's sense is simply not possible—a point Keynes himself seemed to recognize when he pointed out that the classical postulate about labor supply (which he was clearly ruling out, on both empirical and theoretical grounds) corresponded to the case of full employment.[14]

Equation (4) may be taken as summarizing the assumptions about the labor market — or the "supply side," to use another conventional term. To complete the system, we need the postulates for the "demand side," i.e., the markets for

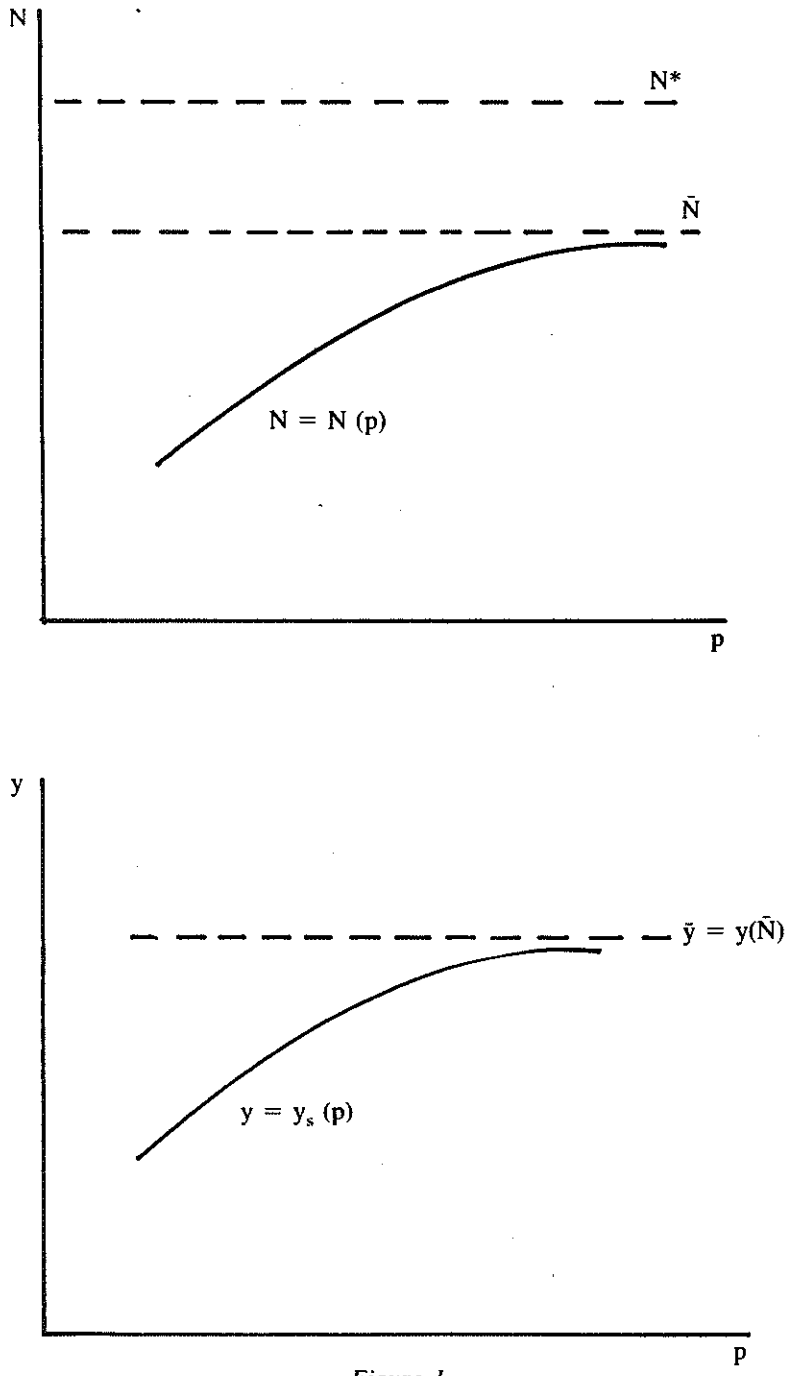

Figure 1.

goods and money. These can be set out rapidly. Total spending Y is equal to revenue from the sale of output: $Y = yp$. In a three-sector economy, Y is composed of consumer spending C, business spending or "investment" I and government spending G: $Y = C + I + G$. The first assumption is that real consumption $c = C/p$ depends on real income Y/p, which is identical to output y, and that when real income increases, so does real consumption, but not by as much. In other words there is a "consumption function" $c = c(y)$ such that $1 > c'(y) > 0$. It should be pointed out that in the *General Theory* Keynes presented the consumption function in a somewhat different way: the variables were measured in terms of the wage rate, rather than in terms of the price level. But defining the function in real terms, i.e., deflating by p instead of w, does not change any of the basic conclusions. As noted above, Keynes's admission that the wage rate rises with increasing demand excludes the idea of an exogenously determined money wage; once one rules out the possibility of a labor supply function of the form $w = w_0$, it can easily be verified by making parallel calculations that the analysis based on a consumption function defined in real terms yields essentially the same conclusions as one using a consumption function whose variables are measured in terms of the wage rate. The change we are making here will serve to clarify certain features of Keynes's analysis; in particular, it will help us understand why Keynes defined some of his variables in terms of the wage rate in the first place. But the ultimate justification for the shift is that in the *General Theory* variables defined in terms of the wage unit were explicit surrogates for "real" variables, and there is no need to be bound by a substitute when one can use the genuine article.[15]

Next, in Chapter 11, Keynes assumed that investment depends on the interest rate r in such a way that when r rises, I falls: $I = I(r)$, $I'(r) < 0$.[16] Therefore, defining real government spending $g = G/p$, we have

$$y = c(y) + \frac{I(r)}{p} + g, \qquad 1 > c'(y) > 0, \qquad I'(r) < 0. \tag{5}$$

Finally, equilibrium in the money market closes the system. The demand for money L, according to Keynes, has two components: an upward-sloping transactions demand $L_1(Y)$ and a speculative demand $L_2(r)$ with negative slope. Their sum yields the liquidity preference function: $L(Y, r) = L_1(Y) + L_2(r)$. In equilibrium, demand for money must equal the money supply M, which is treated as a constant; so (substituting py for Y) we have[17]

$$M = L_1(py) + L_2(r), \qquad L_1'(py) > 0, \qquad L_2'(r) < 0. \tag{6}$$

Equations (4) through (6) sum up the postulates of Keynes's system. We are now in a position to compare the arguments in the *General Theory* with conclusions yielded by the mathematical manipulation of these equations as a way of gauging the degree to which the theory can be understood in essentially intellectual terms, and as a first step toward considering alternative social explanations.

The Theory of the Multiplier

Consider first the theory of the multiplier. There can be little doubt about the importance of the multiplier doctrine in the *General Theory*. As Keynes himself wrote, "about half the book is really about it."[18] The doctrine itself, as outlined for example in Chapter 10 of the book, can be summarized briefly; once again, the only significant change is that the variables will be measured in real terms, instead of in terms of the wage rate. In a two-sector economy, where income is divided into consumption and investment, $Y = C + I$; so $y = c + i$, where $i = I/p$ represents real investment. Given the consumption function $c = c(y)$, where $1 > c'(y) > 0$, and treating i as exogenous, we have $y = c(y) + i$. Differentiating with respect to i and solving for dy/di yields the investment multiplier:

$$\frac{dy}{di} = \frac{1}{1 - c'(y)}.$$

This, the argument goes, tells us how much of an increase in real income an increase in real investment will generate; you simply multiply the increase in real investment by the multiplier to find out how much increased income is produced. Since $c'(y)$ is between 0 and 1, the multiplier itself must be greater than 1, and thus an increase in investment—or for that matter in any exogenous component of spending—is more than matched by the increase in income it causes.

How does this particular doctrine relate to the line of reasoning developed in the *General Theory* as a whole? The larger argument insisted on the importance of integrating real and monetary phenomena. At the very beginning of his book Keynes had criticized what he called the classical theory for assuming a neat disjunction between the two—for assuming, that is, that the demand and supply for labor functions, taken together, determined by themselves real variables like the levels of employment and output, whereas the rest of the system could only influence things like the price level. His point was that forces outside the labor market, summed up in what he called effective demand, could affect real output.[19] But note how the multiplier analysis is couched in exclusively real (or in Keynes's case, quasi-real) terms. Indeed, the multiplier argument is laid out before such monetary phenomena as liquidity preference—crucial, as will be seen, for Keynes's theory—were even introduced. And in fact Keynes had come to accept the multiplier theory as early as 1930, long before the *General Theory* as a whole had been developed.[20]

Thus the multiplier doctrine did not develop from basic assumptions about the system as a whole, and it was not derived from these assumptions in the book. But how is all this to be taken? Perhaps by "working backwards" Keynes had developed a theory that would yield the multiplier as he had presented it. And perhaps the multiplier was inserted at this point mainly for heuristic purposes, i.e., not to be taken at face value, but serving primarily to "hook" the reader by

giving nothing more than a foretaste of the real argument. But if this is so, one would expect the analysis of income determination in the broader theory to be worked out independently; the multiplier would be derived solely from the postulates of the theory as a whole, rather than carried over intact from the original simple analysis. This is the only logical procedure. But what Keynes did was to consider the original multiplier analysis as valid "as a first approximation." Monetary factors were viewed as having mere "repercussions" on the analysis, increasing its complexity, but not as affecting it so fundamentally as to call for a fresh analysis; labor market effects were not considered at all.[21]

The validity of this point of view can be tested by comparing the multiplier implied by the theory as a whole with the simple multiplier Keynes continued to use. To do this, it is necessary to consider the general theory as a set of n equations in n + 1 unknowns, that is, the Eqs. (4) to (6) in the four variables y, p, r and g (where g can be taken either as real government spending or, in a two-sector model, as the exogenous component of either real consumer spending or real investment). Under the usual conditions the variables y, p and r are thus determined as functions of g; the multiplier dy/dg is thus well defined, and can be determined by differentiating the three equations with respect to g via the chain rule, and then solving for dy/dg. The calculation is straightforward and only the result will be presented here; for the full calculation see Appendix II. The multiplier yielded by the general theory, giving the effect on real income of an exogenous change in real spending, is thus

$$\frac{dy}{dg} = \frac{1}{[1 - c'(y)] + [I'(r)\, L_1'(py)/L_2'(r)]\,(1 + y/py_s'(p)) + 1/p^2 y_s'(p)} \quad (7)$$

The corresponding simple multiplier would be $1/[1 - c'(y)]$.

What conclusions are to be drawn from this analysis and how do they differ from Keynes's assertions in the *General Theory?* First, it follows from previous assumptions about signs that the two final terms in the denominator of Eq. (7) are positive. Therefore,

$$\frac{1}{1 - c'(y)} > \frac{dy}{dg} > 0.$$

Retaining the simple multiplier, even as a "first approximation," necessarily exaggerates the effect on real income of an exogenous increase in real spending. Nothing in the assumptions rules out the possibility that the difference between the two might be substantial.

The second point is that it is possible to make certain empirical assumptions that will enable Eq. (7) to collapse into the simple multiplier that Keynes took for granted. In the version of the Keynesian theory used here, where the consumption function is defined in real terms, it would be necessary both that $y_s'(p)$ be

infinite and that either $L_2'(r)$ be infinite or $I'(r)$ be zero. The first condition, however, about the aggregate supply curve, does not hold in general. Since

$$y_s'(p) = (w/p)\,(dN/dp)$$

it is easy to see from the formula for dN/dp above that $y_s'(p)$ is greatest (taking w/p and y"(N) as given) when $\partial N_s/\partial p$ is negligible and $\partial N_s/\partial w$ is infinite — or, in other words, when the labor supply function has the form $w = w_0$. Even for this case, however, the slope of the aggregate supply function would in general be finite:

$$y_s'(p) = \frac{w}{p}\frac{dN}{d_{pt}} \qquad y_s(p) = y(N(p)), \qquad y_s'(p) = y'(N)\frac{dN}{dp};$$

$$\text{if } y'(N) = \frac{w_0}{p^2}, \qquad y''(N)\frac{dN}{dp} = -\frac{w_0}{p};$$

$$\text{so } y_s'(p) = \frac{-w_0^2}{p^3 y''(N)}.$$

It is thus necessary to assume also that y"(N) be negligible, i.e., that the economy is in the area of the production function approximating constant returns to scale. As for the second condition, while it has sometimes been assumed that investment is not particularly sensitive to the interest rate, Keynes himself did not argue along these lines, tending to stress the liquidity preference function.[22]

Was it the case, however, that Keynes assumed that for a great depression the necessary empirical assumptions may be taken as valid, and thus that the simple multiplier he presented was essentially correct? Even putting aside the point that the theory claimed to be general—that it did not bill itself as "depression economics"—it is important to note that Keynes did not make any of these assumptions, even when analyzing depression periods. To be sure he alluded to the possibility of what came to be called a "liquidity trap," i.e., that $L_2'(r) = -\infty$. But he treated it as a theoretical curiosity: "whilst this limiting case might become practically important in future, I know of no example of it hitherto."[23]

As for the assumption about the supply of labor, it has already been pointed out that Keynes did not assume in general that this had the form $w = w_0$. This function, moreover, was tantamount to an assumption that labor is in a position to dictate the money wage. Given that, it is important to note that whatever the general validity of that assumption, it was actually weakest for periods of mass unemployment, a point Keynes himself admitted in the book: "Labour is not more truculent in the depression than in the boom—far from it."[24]

This is not to imply that issues like the degree of liquidity preference and the shape of the labor supply function have little theoretical interest. It is clear from Eq. (7) that the analysis is crucially, rather than just marginally, dependent on

assumptions about liquidity preference and about the labor supply's relative insensitivity to the price level. For if either $y_s'(p)$ or $L_2'(r)$, relative to $I'(r)$, were close to zero, that is, if the economy were on the flat part of the aggregate supply curve or if Keynes had greatly exaggerated the importance of liquidity preference, then the whole multiplier would be close to zero as well. In that case, exogenous changes in spending could have only a negligible effect on the level of employment.[25] Thus in judging the validity of the multiplier analysis, there are two problems to consider. First, how well are the implications of the assumption about labor supply taken into account in the elaboration of the Keynesian system? Second, how well established (from a strictly logical point of view) is the liquidity preference doctrine?

We have already seen how the assumption about the nonhomogeneity of the labor supply function implied an aggregate supply function of the sort illustrated in Figure 1. By the same kind of analysis, one can easily verify how Eqs. (5) and (6) define, taking M and g as parameters, what can be called an *aggregate demand function* $y_d(p)$ with negative slope: differentiating Eqs. (5) and (6) with respect to p and solving for dy/dp yields

$$y_d'(p) = \frac{-I'(r)\, L_1'(py)\, y/pL_2'(r) - I/p^2}{[1 - c'(y)] + I'(r)\, L_1'(py)/L_2'(r)} \quad . \tag{8}$$

By inspecting the sign of each terms, it can be readily seen that the numerator is negative while the denominator is positive, so that the fraction as a whole is negative. Putting the aggregate supply and demand curves together, as in Figure 2, it is easy to see how shifts in the demand function, due to changes in the values of the parameters, change final output and the price level.

Thus assume that the aggregate demand curve has moved up from y_d to $\hat{y}_d$ for its entire length by an amount a. Clearly equilibrium output has gone up only by an amount b, which is less than a. It is as though the increase in the price level from p to p′ has consumed much of the increase in demand. If we are on the flat portion of the y_s curve, the great bulk of the increase in demand will be taken up in this way. All this is fairly clear. How is this dealt with in the *General Theory?*

The important thing to note is that while Keynes recognized that part of an increase in demand is consumed by a rise in prices (attributing it, however, to secondary factors like bottlenecks between goods, rather than deriving it from purely macroeconomic assumptions), he tended systematically to play down this kind of effect.[26] Thus he did not even include price effects in the list of "offsets" to be taken into account in actually estimating the multiplier.[27] It is the presumed fall in the marginal propensity to consume c′(y) rather than price effects that he sees as dampening the multiplier as full employment is approached.[28]

It is characteristic of Keynes's approach that throughout the book the supply side is slighted, in spite of the fact that a key assumption on the supply side—the

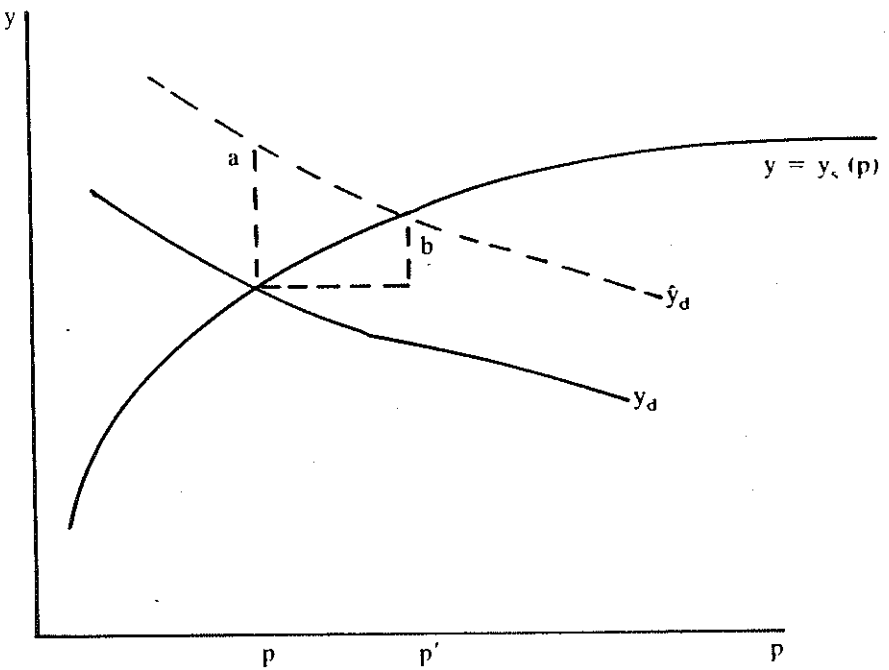

Figure 2.

nonhomogeneity of labor supply—was one of the basic assumptions of the theory. Demand is seen as fundamental in itself; he preferred to talk about changes in the *quantity* of effective demand, rather than shifts in the aggregate demand *function*. The cumulative effect of this is to give the reader the impression that when demand increases by an amount a, as in Figure 2, so more or less does real output.[29]

The neglect of the supply side and the consequent playing down of price effects must be understood in the context of Keynes's desire to "sell" his theory and see his policies implemented. For the way he developed his argument served to exaggerate the effect of increased spending on real output, and served also to obscure the inflationary effects of a policy of spending. What is peculiar about this is that the policies he advocated were inflationary almost by definition: for he defined "involuntary unemployment," which was the only kind of unemployment he was really concerned with, as unemployment which would diminish if the price level went up (relative to the wage unit) as a result of shifts in demand.[30]

Given the definitions and assumptions which were the point of departure for the theory, it was something of an intellectual feat to create the impression that price effects were relatively unimportant. One of the devices Keynes employed

to create this effect was the use of the money wage as the unit of account. Thus Eq. (5) would have to be rewritten:

$$\frac{Y}{w} = c^*\left(\frac{Y}{w}\right) + \frac{I\,(r)}{w} + \frac{G}{w}. \qquad (5^*)$$

where $C/w = c^*(Y/w)$ is the consumption function as Keynes had it. Now if the wage rate is viewed as determined exogenously "by the bargains reached between employers and employed"—and Keynes insisted in the chapter where he summed up his argument that it could "sometimes" be regarded in this way—then w would be a constant in Eq. (5*).[31] This equation together with Eq. (6) would then be a system of two equations in the two unknowns Y and r (since G and M are taken as constants); given the usual assumptions, money income and thus income measured in terms of the wage rate would therefore be determined. Assuming a connection between the latter variable and the amount of labor used would mean that the level of employment would also be determined.[32] The effect of all this is to restore much of the hermetic separation between the supply and demand sides that Keynes had attacked in the classical theory, except that now it is the demand side that is seen as the more important of the two in determining real output and employment.

The assumption behind the argument was, however, extreme. The idea that the wage rate was determined exogenously, or equivalently that the labor supply function could be reduced to the equation $w = w_0$, simply did not follow from the basic discussion about supply and demand for labor in chapter 2, and indeed later in the book, Keynes very explicitly recognized that "increasing effective demand tends to raise money wages."[33] Why then did he avoid a more general assumption about labor supply, for example, even the assumption that it depended solely on money wages [$N = N_s(w)$]? Partly it is because the assumption $w = w_0$ fit in with his tendency to play down the role of the supply side and ignore price effects, for in that case it is the demand side that determines everything. But also it was because any more general assumption would have yielded a more complex interaction between the labor market and the rest of the economy; the analysis of systemic effects would have called for more mathematics than Keynes was willing or able to provide.

The Defense of the Liquidity Preference Theory

This point will perhaps become clearer when we turn to one of Keynes's arguments about liquidity preference. The importance of this concept is obvious. Not only does the degree of liquidity preference have a crucial bearing on the size of the multiplier, but as Keynes stressed, it also plays a key role in determining the effectiveness of monetary policy.[34] The concept is basic to all variants of the

Keynesian model and indeed is implicit even for the simplest Keynesian case of a fixed set for economy with an autonomous level of investment.[35]

From the outset, however, the discussion of liquidity preference was perceived to be weak area in Keynes's analysis. Jacob Viner, for example, charged him with assigning it grossly exaggerated importance. We are not concerned here with the nature of these criticisms or with the substantive question of whether Keynes was right; our sole interest is the way Keynes handled the attack. Could Viner's charges be tested? Keynes said that if Viner were correct, then "a small decline in money-income would lead to a large fall in the rate of interest," and experience showed that this was not true.[36]

But what in the context of the model does this mean? That dr/dY is high if $L_2'(r)$ has a low absolute value? But dr/dY has no unambiguous meaning in the context of the model. If the model is limited to endogenous variables—exogenous unknowns being taken as given—then all variables are determined; you end up with points, not functions, and the derivative as such is therefore not defined. Of course, one can relate money income to the rate of interest if both are functions of some exogenous variable x, but then the particular relation would depend upon which variable x is chosen. Thus instead of dr/dY one would have (dr/dx)/(dY/dx), which, if Keynes were right, would depend inversely on $L_2'(r)$. But what is the source of variation to be? Let the exogenous variable be the money supply M, for example. The endogenous variables can all be conceived of as functions of M. Since $Y = py$,

$$\frac{dY}{dM} = p\frac{dy}{dM} + y\frac{dp}{dM},$$

so

$$\frac{dy}{dM} = \frac{1}{p}\left(\frac{dY}{dM} - y\frac{dp}{dM}\right).$$

Let $Y = Y_s(p) = py_s(p)$. Then

$$\frac{dY}{dM} = Y_s'(p)\frac{dp}{dM} \text{ and } \frac{dp}{dM} = \frac{1}{Y_s'(p)}\frac{dY}{dM}.$$

Therefore

$$\frac{dy}{dM} = \frac{1 - y/Y_s'(p)}{p}\frac{dY}{dM}.$$

Differentiating Eq. (5) with respect to M yields

$$\frac{dy}{dM} = c'(y)\frac{dy}{dM} + \frac{1}{p}I'(r)\frac{dr}{dM} - \frac{I}{p^2}\frac{dp}{dM}$$

Substituting for dy/dM and dp/dM and rearranging terms, we get

$$\frac{dr/dM}{dY/dM} = \frac{(1 - c'(y))(1 - y/Y_s'(p)) + I/pY_s'(p)}{I'(r)}$$

Note that $L_2'(r)$ does not enter into this fraction; and since a low $L_2'(r)$ does not in itself imply anything definite about the value of the various terms in this fraction—it does not guarantee, in particular, that either p or $Y_s'(p)$ or $I'(r)$ will be low—Keynes's claim turns out in this case not to be valid.

On the other hand, the exogenous variable may lie outside Eq. (6). Let x be any such parameter (marginal propensity to consume in a linear consumption function, real government spending and so on). Then differentiating Eq. (6) with respect to x yields

$$0 = L_1'(Y)\frac{dY}{dx} + L_2'(r)\frac{dr}{dx},$$

and thus

$$\frac{dr/dx}{dY/dx} = \frac{-L_1'(Y)}{L_2'(r)},$$

which is greater than zero. This conforms to Keynes's claim: if $L_2'(r)$ were small, a small decline in money income would correspond to a large fall in the interest rate. But what all this means is that Keynes was wrong in claiming that the statistical relation between r and Y was a test of the liquidity preference theory. Even assuming the basic validity of the theory as a whole—a dubious assumption, since it is precisely this which presumably is being questioned—the presumed relation depends on the particular parameter, or set of parameters, whose shifts cause the variation in r and Y.

This is significant mainly for what it reveals about Keynes's mode of argumentation. As in the case of the multiplier, his defense of his liquidity preference argument was purely verbal. A simple mathematical analysis shows how Keynes's analysis was misleading; it also indicates that the nonmathematical method was fundamental to Keynes's reasoning, for a number of key arguments could not have remained intact if they were recast in mathematical terms.

Capital Formation and the Propensity to Save

One final example might suffice to clarify the connection between the very strong conclusions typical of Keynesian doctrine and the dubious logical foundation on which they rest. Consider Keynes's argument that a low propensity to

consume, that is, a high propensity to save, holds back the growth of capital.[37] From the standpoint both of policy and of historical analysis, this is a very important contention. Nevertheless, it can be shown that the basic assumptions of his theory imply quite the opposite and that a decrease in the propensity to consume actually increases real investment (and thus the rate of capital growth).

To show this, it is first necessary to define what is meant by an increase or decrease in the propensity to consume. Since the "propensity to consume" was defined in the *General Theory* as a *function* (p. 90)—in fact, as the consumption function—an increase would be a rise in that function for all values of y, and a decrease would similarly be an across the board lowering of it. Consider therefore a rise from $c = \hat{c}(y)$ to $c = c^*(y)$ (or equivalently a fall from c^* to $\hat{c}$). Then for any y, $c^*(y) > \hat{c}(y)$. Let $c(y, m) = mc^*(y) + (1 - m)\hat{c}(y)$. (This function of two variables can be thought of as defining a continuous process, controlled by a parameter m, for "moving" the function from $\hat{c}$ to c^*; as m moves from 0 to 1, the function goes from $\hat{c}$ to c^*.) Then for all m such that $1 \geq m \geq 0$, $1 > \delta c/\delta y > 0$: since $\delta c/\delta y = mc^{*\prime}(y) + (1 - m)\hat{c}'(y)$, $\delta c/\delta y$ is positive because both $c^{*\prime}(y)$ and $c'(y)$ are positive, and both m and $1 - m$ are nonnegative, with at least one of them being positive. Furthermore, let $\tilde{c}'(y)$ equal the greater of $c^{*\prime}(y)$ and $\hat{c}'(y)$. Then $1 > \tilde{c}'(y) = m\tilde{c}'(y) + (1 - m)\tilde{c}'(y) \geq mc^{*\prime}(y) + (1 - m)\hat{c}'(y) = \delta c/\delta y$. Moreover, $\delta c/\delta m = c^*(y) - \hat{c}(y)$; so $\delta c/\delta m > 0$. Then Eq. (5) can be written in the following form:

$$y = c(y,m) + \frac{I(r)}{p} + g,\ 1 > \frac{\delta c}{\delta y} > 0,\ \frac{\delta c}{\delta m} > 0,\ I'(r) < 0 \qquad (5')$$

Taking g and M as constants, Eqs. (4), (5′) and (6) thus form a set of three equations in the four unknowns y, r, p and m, and thus all the other variables are determined as functions of m. In particular, real investment $i = I(r(m))/p(m) = i(m)$. The question then is whether i(0), real investment corresponding to the lower propensity to consume $\hat{c}$, would be less than i(1), which corresponds to the higher one c^*. Keynes claimed that it would, and it is this claim that will now be tested.

First, differentiate Eqs. (4) and (6) with respect to m, and solve for dr/dm:

$$\frac{dr}{dm} = \frac{-L_1'(py)}{L_2'(r)}\left(p + \frac{y}{y_s'(p)}\right)\frac{dy}{dm}. \qquad (9)$$

Since the coefficient of dy/dm is positive, dy/dm and dr/dm have the same sign. Second, differentiating Eq. (5′) with respect to m, substituting for dr/dm from

Eq. (9) and for dp/dm from the differentiated version of Eq. (4), and then solving for dy/dm yields

$$\frac{dy}{dm} = \frac{\delta c/\delta m}{(1 - \delta c/\delta y) + I/p^2 y_s'(p) + [I'(r)\, L_1'(yp)/pL_2'(r)]\,[p + y/y_s'(p)]} \quad (10)$$

by inspecting the sign of each term in this fraction, it can easily be verified that dy/dm is positive for all m between 0 and 1. Therefore dr/dm is also positive for this domain of m. Next, since $y = y_s(p)$, $dy/dm = y_s'(p)\, dp/dm$; since $y_s'(p)$ is positive, dy/dm and dp/dm also have the same sign; so dp/dm is also positive. Finally,

$$\frac{di}{dm} = \frac{d(I/p)}{dm} = \frac{I'(r)}{p}\frac{dr}{dm} - \frac{I}{p^2}\frac{dp}{dm}$$

It follows at once from what has been shown about the signs of all the terms in this equation that di/dm is negative for all m between 0 and 1. Thus the i(m) function is downward sloping for this domain, and it therefore follows that i(1) is less than i(0). In other words, a decrease in the propensity to consume necessarily increases real investment. This is the opposite of Keynes's claim. In his zeal to prove the virtue of even unproductive spending, Keynes denied to saving a positive role in capital formation, a conclusion unjustifiable on the basis of his own assumptions.

Method and Motives

Thus once again Keynes's failure or refusal to use mathematical analysis allowed him to make misleading arguments—but particularly forceful ones, characteristic of his whole doctrine. The doctrine could not have taken the form Keynes gave it if the method by which it was developed had been more rigorous, more appropriate to the questions posed and the assumptions posited.

Does it therefore follow that the defective logic of the theory was necessary to its worldly success? I think it is clear that if Keynes had been more rigorous, the conclusions to be drawn would have been much less striking than the conclusions he actually drew. But a bland and highly nuanced analysis, granting every policy its good and bad points, could not have had such an impact on the real world. A more dramatic argument might be far more effective. As we have seen, the effect of Keynes's departures from rigor was to dramatize his argument: the benefits of spending were exaggerated, and the possibility of inflation was minimized.

How much of this was deliberate on Keynes's part? We cannot answer this question definitively, but we can note a number of points. First, it is clear that Keynes viewed the development of economic thought in "political" as opposed to formally rational terms; the power of an idea depended not on its "truth" or on the logic of the argument that supported it, but rather on how persuasive it

was. Persuasion was more important than proof; polemic was the midwife of doctrinal change. New ideas were rooted in the economist's intuition.[38] Ideas were therefore not to be conveyed through a logical exposition, but rather to be sketched in such a way that the basic notions could come through clearly. As he wrote in 1934, when an economist writes economic theory "he is composing neither a document verbally complete and exact so as to be capable of a strict legal interpretation, nor a logically complete proof." The idea was to give a "simple statement, so to speak, out of all the things which could be said, intended to suggest to the reader the whole bundle of associated ideas, so that, if he catches the bundle, he will not in the least be confused or impeded by the technical incompleteness of the mere words which the author has written down, taken by themselves." It was the spirit of the argument that was important; it would be wrong to examine it by focusing on detail: "This means, on the one hand, that an economic writer requires from his reader much goodwill and intelligence and a large measure of cooperation; and, on the other hand, that there are a thousand futile, yet verbally legitimate, objections which an objector can raise." Rhetoric was thus more important than logic: "In economics you cannot *convict* your opponent of error; you can only *convince* him of it. And, even if you are right, you cannot convince him, if there is a defect in your own powers of persuasion and exposition or if his head is already so filled with contrary notions that he cannot catch the clues to your thought which you are trying to throw to him."[39]

This had long been Keynes's view. Throughout his career his aim had been to convince, exemplified in his deep involvement in politics and journalism throughout the interwar period. Indeed, in his campaign against the Versilles treaty in the early 1920s, it is clear that he was writing essentially for political effect. Thus the argument against reparation in *The Economic Consequences of the Peace* was very much at variance with the theory of international trade, even in the form it then had: Keynes simply assumed that the existing balance of trade was relatively unchangeable, whereas the established theory stressed the ability of financial movements (like the payment of an indemnity) to reshape the commercial balance.[40] Given Keynes's grounding in the classical theory, it is hard to avoid the conclusion that this was in some measure deliberate and that he was trying to strengthen the case against the treaty by using the most persuasive—and not necessarily the most valid—arguments. And in his second book on the subject, *A Revision of the Treaty*, it is clear that he simply ignored key evidence which bore very directly on his central argument. Thus at the end of 1920, the French government had pressed vigorously for a plan, the "Seydoux Plan" as it was called, to allow the Germans in effect to pay in paper marks. The scheme would thus have largely solved the transfer problem, the ultimate obstacle according to Keynes; although this scheme was widely publicized at the time, there is not a word about it in the book.[41] Clearly, Keynes was making a case, and

wanted to use only what would strengthen the force of his argument. Accuracy was a secondary consideration. If fallacious arguments—and Keynes still thought that protectionist sentiment was rooted in fallacy—led to the desired goal of revision, then "Heaven forbid that I should discourage them."[42]

Of course, this campaign against the Versailles peace made Keynes one of the most prominent figures on the British (indeed, the international) scene throughout the interwar period. He was extraordinarily active, in both academic and public life. His colleagues in the economics profession did not hold his political activities against him; in fact, one has the impression that they sympathized with his politics and were thus disposed to sympathize with his economics. According to Jacob Viner, this was why he was never actively criticized for what Viner termed his "defective" analysis of the problem of reparation transfer. Referring to *The Economic Consequences of the Peace*, Viner wrote:

> As I recall it, economists at the time regarded its economics as undistinguished in general and technically defective at some crucial points, especially in its treatment of the alleged difficulties of "transfer" of reparations. But the political views which Keynes expounded with great force of exposition were those which Anglo-Saxon liberals of the 1920's, including the economists, shared almost to a man, and I suppose there then seemed little point in exposing technical flaws in an economic argument which had the virtue of leading to the desired political conclusions.[43]

When he wrote this, Viner probably had in mind his old teacher F. W. Taussig, a specialist in the theory of international trade and perhaps the most eminent American economist of his generation. Taussig's pronounced Germanophilia during the period of World War I probably disposed him favorably to Keynes.[44] In any case, it is clear that his liking for Keynes was one of the factors that led him, as editor, to devote a whole issue of the *Quarterly Journal of Economics* to a discussion of the *General Theory* shortly after it came out—a most unusual way to treat a new book, and one which signaled its importance to the whole profession. Taussig, for example, urged Viner to begin his contribution to this symposium with "something about the man's work in general, an expression of the admiration which all economists feel for him."[45]

The style with which Keynes tackled the problem of unemployment was similar to that which characterized his approach to reparation. Passion was more powerful than detached, rational argument. Only shock treatment could really be effective. "Words", he wrote in 1933, "ought to be a little wild, for they are the assault of thoughts upon the unthinking."[46] Arguments were a kind of battering ram. The object of the *General Theory*, he told John Hicks, was "to press home as forcibly as possible certain fundamental opinions—and no more."[47] When his friend Roy Harrod urged him to moderate his argument, Keynes replied that his "assault" should rather be intensified: "I *want*, so to speak, to raise a dust."[48]

Given all this, there can hardly be any doubt as to *whether* political concerns played a role in shaping Keynes's theory. His was an argument designed to

support conclusions intuitively reached in advance; the theory's most distinctive conclusions were not the result of a rational process which started from basic assumptions and took them wherever they led. There is nothing very surprising about this, nor is it, I imagine, uncommon in intellectual activity as a whole. In consequence, however, the development of the theory must be interpreted in largely nonintellective terms. The real question is *what* these terms are.

The most obvious answer is that Keynes had intuitively understood that the way to increase the level of employment was by expanding demand, but the economists of his day had more or less rejected this policy, basing their argument on the classical theory. Even today it is widely believed that economists before Keynes denied that changes in spending could affect the level of output and that the only way to cure the depression was to cut wages. It was therefore, the argument runs, Keynes's great contribution to clear up these misconceptions, opening the way for a policy of spending and monetary expansion.

Unfortunately, this interpretation is based on facts which turn out to be false. As one recent study has demonstrated, for example, "a large majority of leading U.S. economists" had advocated policies which would now be considered "Keynesian" prior to the publication of the *General Theory*.[49] Pigou, to Keynes the epitome of a classical economist, was on the same side as Keynes in the 1930s policy debates; both endorsed government spending as the way out of the depression.[50] As early as 1929, Keynes himself asserted that none of the leading British economists supported the "Treasury view" that government spending could not decrease unemployment.[51] He exaggerated somewhat, but most British economists did believe in the potency of monetary factors, whatever their views on public works. In a 1937 letter to Hicks, Keynes himself agreed that in the period before the *General Theory* economists had generally "slipped away from the pure classical doctrine without knowing it" in admitting that an "increase in the quantity of money is capable of increasing employment."[52]

Indeed, it would have been a stunning intellectual phenomenon if this were not the case—if economists, that is, had actually held what Keynes called the "classical" view and denied that shifts in spending and changes in the money supply could affect output. Even if the economists of the time had looked only at recent British experience, how could any of them have argued that the enormous monetary dislocations of the period after 1914 had no real effect on the level of economic activity—that wartime spending had nothing to do with the boom of the war years or that the postwar policy of deflation was unrelated to Britain's economic distress from late 1920 on? As for the British Treasury itself, that holy of holies of financial orthodoxy, it certainly never asserted that monetary policy could have no real economic effect, and in the 1930's at times grudgingly admitted the merits of the new "view" that government spending might be effective in combating unemployment.[53]

As for the point about wage cutting, we again have it on Keynes's authority that while economists in the early nineteenth century might have argued in such

terms, with Marshall's age came "a change of heart out of proportion, perhaps, to the amount of change in theory. One of Marshall's earliest publications was a gentle defence and justification of trade unionism as a means of ameliorating the conditions of the working class; and all living economists were brought up to respect and plead for the activities of trade unions as they existed in the latter half of the nineteenth century and before the war." With persistent unemployment in the 1920s, views began to change. Nevertheless, the economists were not about to launch a crusade against high wages: "Public opinion in modern conditions is so decidedly opposed to a retrograde movement in wages that scarcely anyone, whatever he might think, dares to breathe in public the view that wages are too high. People grumble under their breath; they maintain that all other solutions of present difficulties are futile; but they are reluctant to put forward their own."[54]

So it can hardly be claimed that Keynes's great accomplishment was to destroy the dogma that wage cutting was the only way out of the depression. It is often said that Keynes refuted the idea that wage cutting was needed to restore equilibrium by proving that equilibrium was possible at less than full employment. But again, from an intellectual standpoint, this was hardly an accomplishment at all: on the one hand, any absolute amount of unemployment was perfectly consistent with equilibrium in the classical theory as he presented it; on the other hand, the fact that the classical theory ruled out involuntary employment at equilibrium while Keynes's theory permitted it was nothing more than a pure and direct artifact of the special definition of involuntary unemployment Keynes used, together with his assumption about the nonhomogeneity of the labor supply function.

Keynes, of course, is not to be blamed—not excessively, at any rate—for the way others assessed his work. The myth that grew up about his work and about the views of his predecessors and contemporaries has to be understood in functional terms: it came into being because it served certain purposes. And in fact the exaggeration of Keynes's greatness was an important element in the establishment of Keynesianism as an orthodoxy; the elaboration of this myth must therefore be understood as an integral part of the process by which Keynesianism took hold.

But to get back to the main track of the argument: if the basic function of Keynes's theory was not to radically change his fellow economists' minds about policy, then its function must have been to legitimize a policy that Keynes, and others, desired on grounds which had little to do with formal theory itself. This explains, in part, why Keynesianism took root so quickly despite its logical flaws; the theory was not strongly resisted because people already believed in the policy associated with it.

Nevertheless, one thing distinguished Keynes from his colleagues: the force of his conviction. He was certain, while in his view their ideas were confused and their beliefs inconsistent.[55] What was the source of his belief? The answer to this

question can be inferred from the nature of the bias which, as we have seen, was introduced into the argument—the direction, that is, in which Keynes's conclusions diverged from his assumptions. As noted before, the thrust of the argument was to exaggerate the benefits of spending, to disparage saving and to minimize the problem of inflation. This is not to say, of course, that his views on these subjects were devoid of nuance; indeed, some scholars have been surprised to discover that Keynes was quite sensitive to the danger of inflation as early 1937.[56] Nevertheless, such concerns were based on "structural," microeconomic considerations (bottlenecks and so on), and were not derived from the basic argument of the *General Theory*. And indeed, in spite of such nuances, it was this set of biases about spending, saving and inflation that gave the theory its distinct flavor in the 1930s. What kind of social and political views would such attitudes be associated with?

There is no need to look very far. Keynes himself gives the answer at the end of the book, in his "Concluding Notes on the Social Philosophy Towards which the General Theory Might Lead." The assumption here, based on the whole of the foregoing argument, is that it is legitimate to reverse the logical connection and treat Keynes's social views more as a source than as a consequence of the entire doctrine he had laid out in the book. It is clear from this final chapter that Keynes sought to increase the power of the state, to set up "certain central controls"—the instruments of what we would now call fiscal and monetary policy—to deal with unemployment.[57] But beyond that he thought "a somewhat comprehensive socialisation of investment" would be necessary to solve the problem of unemployment.[58] Unless the state stepped in in this mild fashion, liberal institutions and the free market system with which they were linked would be swept away by the social crisis, replaced by a distasteful and inefficient authoritarian regime:

> The authoritarian state systems of to-day seem to solve the problem of unemployment at the expense of efficiency and of freedom. It is certain that the world will not much longer tolerate the unemployment which, apart from brief intervals of excitement, is associated—and, in my opinion, inevitably associated—with present-day capitalistic individualism. But it may be possible by a right analysis of the problem to cure the disease whilst preserving efficiency and freedom.[59]

If Keynes therefore had a clear sense of the kind of political and economic system he wanted, then the *General Theory* as a whole can be viewed as a key part of an effort to "sell" this vision to the entire community—or really to oversell it, since to Keynes a stark and rather extreme argument had greater persuasive force than a mild one.

What, finally, underlay Keynes's vision of an active liberal state? Was he motivated by a burning desire for social justice—a drive to create a state that would spend freely to promote social welfare and use its power to redistribute

income as an end in itself? Evidently not. Scholars who have studied the issue in some depth have been struck by Keynes's deep-seated social conservatism. For example, in her essays on Keynes, Elizabeth Johnson stressed his "class insularity" and his "archaic" view of the world: "in his ideal society, he desired security and independence for everybody. However, he could conceive of it only in terms of his own experience: social happiness was employment for every one, each in his appointed place, his own niche."[60] Keynes was not passionately committed to social reform as an end in itself. The real basis for his vision of the state lay elsewhere, in his conception of history.

For Keynes believed strongly that there was a long-term tendency toward unemployment as societies increased in wealth. The rate of saving increases when income rises; so the richer the society, the greater the gap between income and consumption. The gap, representing total savings, can be made up only if investment is sufficient to cover it. Otherwise total demand, i.e., consumption and investment, would be insufficient to maintain full employment. His basic assumption is that market mechanisms cannot automatically assure that all savings will be invested at any level of income and, indeed, that the greater the level of income, the greater the gap is likely to be.[61]

The notion that as societies grow richer, there will be an increasing glut of savings and the idea that consumption and investment will become increasingly unable to maintain a high level of employment are so basic to Keynesianism that this doctrine may be fairly characterized as an underconsumptionist theory. This is hardly a novel observation. Schumpeter, for example, stressed the point long ago, and Keynes himself, judging from his favorable references to earlier underconsumptionist theories, would hardly have quibbled with the characterization.[62] But it seems clear that this conception of history was the real source of his intuition about the need for a policy of spending, and the need for the kind of state that would implement it. It was a view which led him to see the depression not just as part of a particularly severe business cycle, and thus as something that might be dealt with by means of relatively superficial measures, but as the result of a profound secular trend—something that called for more basic reform. For if he was right, it was not possible to just ride out the storm and wait for things to return to normal. They never really could; the problem could only grow worse. If liberal values were to be preserved, the argument for fundamental change had to be presented as forcefully as possible.

One consequence of this line of reasoning should be noted. The idea that a worsening problem of unemployment was an inevitable concomitant of economic growth sanctioned an ever-increasing role of the state in the economy. Merely sporadic state intervention aimed at nipping incipient depressions in the bud would not suffice. Since the gap between savings and "normal" investment was always widening, the amount of slack to be taken up by the state (through a "socialization of investment") must necessarily also be on the increase.

The Problem of Wage Cutting

Thus Keynes was very eager to win people over to his views, and this had a direct effect on some of the key arguments of the *General Theory*. The most important example of this is his argument that a reduction in money wages need not increase employment. Indeed, the presumption is that any attempt to increase employment by cutting money wages would most likely fail to achieve the desired result.[63]

What, however, does "wage cutting" mean in the context of his system? Once again, it is important to stress that when a variable is fully determined, it makes no sense to talk about the effect of changes in any one such variable; variables like the wage rate cannot be allowed to change, unless this change is the result of a shift in some exogenous factor, and in that case, what one is really talking about is the effect of change in that exogenous variable.

But in the case of wage cutting, what was the outside source of variation? A decline in demand would reduce the money wage, but this kind of wage cut does not bear directly on the issue. The wage cut should in some sense lead to, and not simply be the consequence of, a shift in the level of economic activity. It was for this reason that one of the arguments Keynes used to support his thesis about wage cutting was off base. He claimed in the *General Theory* that a statistical inquiry would probably show that money wages and real wages "almost always" move in opposite directions; a rising money wage and a falling real wage both accompany an increasing level of employment, but the former falls and the latter rises when employment slackens off. Thus a declining money wage accompanies an economic decline. As he later noted, Keynes was talking here about the effect of changes in aggregate demand; it was true enough in his system that a fall in the money wage and a reduced level of employment would accompany a decline in demand.[64] But this statistical inquiry was no way of getting at the problem of wage cutting; one needed a case where a wage cut was an agent, and not a mere result, of a change in the level of economic activity.

What then was left? Did "wage cutting" refer to a tougher bargaining attitude on the part of management? In Keynes's theory, the attitude of management does not enter into the system. The demand for labor, being completely determined by the production function—that is, by the technology and capital stock—does not shift with changes in firms' attitude toward labor. Nor did wage cutting have anything to do with government policy; government regulation of wage rates was not an integral part of the system. It is clear in fact from the text that Keynes was considering what would happen "if labor were to respond to conditions of gradually diminishing employment by offering its services at a gradually diminishing money wage"—he was analyzing the effects of a shift in the supply curve of labor.[65]

What then do the basic assumptions of the *General Theory* imply about

changes in the labor supply function? To take the simplest Keynesian case, where the money wage is taken as given (dictated, for example, by the labor unions), the problem would be to determine the effect of a change of that exogenously determined wage. In that case, it is rather easy to show that, contrary to what Keynes argued, a decline in the wage rate would necessarily increase the level of employment. This conclusion is not a result of the manner in which Keynes's version of the consumption function was modified above; it follows even if, for this purpose, the wage unit is retained as the unit of account. And finally, even for the more general case defined by Eq. (3), it can be shown that an increased willingness on the part of the workers to supply more labor at any given configuration of wages and prices (i.e., the analogue of wage cutting in the simple model) also necessarily increases levels of employment and output. All these things are proved in Appendix III.

Why then the argument that wage cutting would most likely be ineffective in increasing the level of employment? First, it is clear that Keynes opposed wage cutting on social grounds: to attempt a large cut in wages, he said, would "shake the social order to its foundations."[66] Making a theoretical argument that wage cutting would be ineffective even in a technical sense would add to the force of the case against it; the fact that it served this function perhaps explains to some degree why the argument was made.

Beyond that, it is important to note that the argument solved a basic political problem with Keynes's theory. For it is clear that there was a fundamental asymmetry in Keynes's theory of the labor market, one that had strong pro-management implications. As noted above, since the demand for labor was derived solely from the technologically determined production function, the role of business in bargaining was necessarily passive; the attitude of management toward labor played no role, whereas the workers' *attitude* determined the supply of labor function. This implied that insofar as events in the labor market had a real economic effect, only the workers could be held responsible; business could not possibly be blamed. These implications were not only counterintuitive; they also conflicted with Keynes's own sense of the process by which wages were set—a view which by no means treated business as passive, but rather tended to see management and labor as coequal adversaries in a bargaining process.[67] But the argument against wage cutting tended to neutralize this bias and thus restore a kind of political balance to the theory. Indeed, an argument against wage cutting would certainly be taken as a kind of touchstone of a "pro-labor" position; it could be a bridge to the moderate left, in the same way that the neomercantilist overtones of Keynes's argument and his public support of a protectionism could help win over the conservatives. Can one say that considerations of this sort played no role in shaping the theory at a time when Keynes's own Liberal party had little chance of even coming close to the exercise of exclusive power?

It is indeed clear that the argument against wage cutting was viewed warmly by at least certain elements on the left. The point about wage cutting was central

in their analyses of Keynes's book, and it seems reasonable to suppose that their pleased reaction to this particular argument had something to do with their sympathetic reception of Keynes's whole doctrine. G. D. H. Cole, for example, in his review of the *General Theory* for the *New Statesman and Nation*, stressed how Keynes "reduces to sheer absurdity the prevalent view that lower wages are a cure for unemployment."[68]

But objective consequence does not directly answer the question of subjective intent: Did the argument about wage cutting have anything to do with a desire on Keynes's part to win the left over to his ideas? Obviously Keynes's attitude toward the Labour party, and toward the Left generally, bears directly on an answer to this question; the evidence indicates clearly enough that he had long had a certain interest in winning at least the moderate Left over to his views.[69] Just after the *General Theory* was published, for example, some of his disciples worked hard to convince some of the young economists from the London School of Economics (LSE) of the validity of Keynes's doctrine. According to the editor of Keynes's *Collected Writings*, they met with them a number of times on "neutral ground" to thrash out the issues, and Joan Robinson had followed up these discussions "with more prolonged arguments" with Abba Lerner, one of the LSE economists. Lerner then had strong left-wing views. He was won over to Keynes's theory and wrote an article outlining it to the worker-oriented readership of the *International Labour Review*. The article itself, the editor of the *Review* noted, had been read in advance and approved by Keynes himself. A note from Joan Robinson underscores the importance that Keynes and his circle attached to winning over people like Lerner. "Don't you think Lerner is a credit to me?" she wrote Keynes in November 1936. "I have got Heffers to sell offprints of his article which will be useful for our young men."[70]

But there was a limit to how far Keynes would go in cultivating labor. Thus at one point he asked Francis Williams, editor of the most important Labourite paper, the *Daily Herald*, to help bring labor around to his views. "Since I regarded him as a genius with solutions that could save the world," Williams wrote, "I was both delighted and flattered by the invitation. But not to much avail. Whenever Keynes actually met Labour or trade union leaders he managed to insult them."[71] And it is clear from a number of sources that he had little sympathy or respect for the Labour party in general. It was, he said in a striking phrase, a class party, "and the class is not my class." "The *class* war," he added, "will find me on the side of the educated *bourgeoisie*."[72] Or consider the tone of a comment he made in 1931 on communist doctrine: "How can I adopt a creed which, preferring the mud to the fish, exalts the boorish proletariat above the bourgeoisie and the intelligentsia, who with all their faults, are the quality of life and surely carry the seeds of all human achievement?"[73]

Yet no matter how much distaste he felt for the left, the political importance of labor was simply a fact that had to be faced, above all, for a doctrine like his which coincided in so many ways with left-wing views. Keynes's stress on the

need for an active policy of combatting unemployment, for the institution of instruments of central planning, and for a "somewhat comprehensive socialisation of investment"—all of this meant that labor was a natural constituency for his views. Anything that would facilitate its acceptance of his doctrine would have a particular appeal, and the argument against wage cutting served this purpose admirably. It is hard, therefore, to avoid the conclusion that this aspect of Keynes's doctrine was to some degree rooted in "political" considerations of this sort.

A Political Theory?

Keynes's attitude toward mathematics also bears on this question of motivation. Although he was in general rather ambivalent about the use of mathematical method in economics, in the *General Theory* he deliberately avoided it. Moreover, the particular argument he used to justify this was invalid.[74] There are several derogatory remarks about mathematical economics in the book, but the most important attack is on pp. 297–298: "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. . . . 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 complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols."

This is not the usual argument against mathematical method that one finds among social theorists: that it is spuriously precise, that it ignores the human factor and so on. Keynes, rather, criticizes it for not doing what it in fact does best: the technique of representing an economic system by a set of simultaneous equations serves better than any other to describe that interdependence that Keynes says mathematical methods rule out. This point is so obvious that one must wonder whether someone as intelligent and as intellectually cultivated as Keynes really believed his own argument. For maybe this argument masked a motive of a different sort, namely, that Keynes perhaps intuitively understood that if the doctrine was to gain acceptance, it had to preserve a cutting edge of forceful and unusual contentions, and that he could only give it this character if he avoided the constraints, both rhetorical and logical, that a mathematical method would impose.

Does all this, however, imply that Keynes deliberately, and perhaps even dishonestly, framed his analysis on the basis of such "political" considerations? A conclusion of this sort does not follow from the evidence presented, nor is it necessary from the standpoint of the present analysis. For the present purposes, it is sufficient to note that Keynes's mode of reasoning was loose enough to permit,

and indeed almost to assure, that "social" factors would play a key role in shaping the theory, and that they had something to do with giving it its distinct coloration. And this is in fact the key point; studying Keynes's departures from rigor, the most characteristic and the most "social" features of his theory can be flushed out.

In general, moreover, this issue of motivation is not to be analyzed in black or white terms; there is no sharp line between conscious and unconscious motivation, between "scientific" and "political" concerns—or for that matter between honesty and deceit. What people want to believe always conditions what they write, even if they are not themselves fully aware of their own desires. The degree to which Keynes deliberately and consciously gave the theory its distinct character is therefore ultimately a question of secondary importance. The real interest of this analysis is due to the light it throws on the process by which Keynesianism took root. For again, to the degree that that process cannot be understood in purely intellectual terms, we are justified in looking at the phenomenon from a "social" point of view; and what those social factors are can be inferred from the social features of Keynes's theory that have already been brought out. It is this general problem of the establishment of Keynesianism that will now be considered.

THE RISE OF KEYNESIANISM

The analysis of the rise of Keynesianism necessarily begins with a close study of Keynes's own theory. But it cannot possibly end there. As Keynes said about Ricardo, the eventual victory of his ideas "must have been due to a complex of suitabilities in the doctrine to the environment into which it was projected." It is necessary therefore to look at things from the other side, so to speak—from the standpoint of those who received and accepted Keynes's ideas.

In particular, the discussion will focus on the problem of how the American economics profession was won over. Is the development of Keynesian theory, and its assimilation into the mainstream of American economic thought, to be understood in essentially intellective terms? How did the theory evolve, and was its evolution a function of cogent, rational criticism—criticism, that is, that focused on the internal, logical shortcomings of the original theory? And what role did the radical shift in social context play in the development of the theory? How did a doctrine born in the Britain of the 1930s adjust so successfully to the self-confident America of the period after World War II?

The first question, it is important to note, is not disposed of by the previous analysis of the *General Theory*, for it is possible that as the doctrine became more widely accepted, all of its problems were resolved, and the subsequent development of Keynesianism proceeded on strictly scientific lines. One has the

impression in fact that many economists today would admit that Keynes's book had its defects (and would agree that these should be explained as products of Keynes's ambitions for political influence). Nevertheless, many of these same people would argue that the problems were eventually worked out and had no lasting effect on mainstream macroeconomic theory. Keynesianism, even in its "scientific" form, of course, encountered problems in the real world. Now everyone admits that the problem of inflation had been underestimated, although this is often implicitly assumed to be because certain empirical judgments had been mistaken, rather than because of any internal logical problems with the theory.

I want to show how this view is seriously misleading. But first it is important to stress one area in which the positivist and intellective view is correct—one area, that is, in which the integration of Keynesianism into the mainstream of economic thought was intimately bound up with a real intellectual advance: the acceptance of mathematical method, and in particular the acceptance of the formal simultaneous-equations model. This was a method of great analytical power. Defining the system as a set of equations whose solution defined an "equilibrium" toward which the real economy moved enabled conclusions (about the effect of change in various parameters) to be reached with real rigor; in the simple technique of matching the number of equations with the number of unknowns, it gave an easy way of largely avoiding problems of over- and underdeterminacy. Most important, it made it possible to conceptualize the system as a set of forces interacting simultaneously, whereas purely verbal description—where one had to talk about the various interactions in succession, as though they followed each other in a chain of causation—precluded this obviously superior mode of conceptualization.[75] Thus the mathematical approach represented a real advance not so much because it permitted quantification as because it allowed the logic of the theory to be tightened up. The axiomatic method—where the equations represented the axioms of the theory—gave the theory a structure, a coherence and a predictive force that a mere congeries of tenuously related ad hoc arguments could never have.

Almost from the beginning, Keynesianism was interpreted in simultaneous-equation terms; the key articles presenting this interpretation were those of Roy Harrod, J. E. Meade and especially John Hicks.[76] This particular interpretation was crucial to the assimilation of Keynesianism into the mainstream of economic thought, and to its emergence as something of an orthodoxy. For the power and clarity of the simultaneous-equations method gave a theory framed in these terms an obvious appeal, especially to younger and more mathematically oriented economists. The only perplexing thing is why the emergence of this technique and its embodiment in what came to be called macroeconomic theory took so long in coming about. For the simultaneous-equations method had been an important part of economic theory since it was developed, most notably by Léon

Walras, in the late nineteenth century. To be sure, the problems were different: Walras was concerned with the allocation of resources and not with income determination in Keynes's sense. But one would think that once the analytical power of the method was grasped, it would have been a relatively simple matter to apply it to the analysis of a variety of economic problems. That the technique was applied so belatedly to the analysis of problems like unemployment probably had something to do with a split within the economics profession between those who worked on high theory (often devoid of empirical content) and those who studied the real world (who tended to spurn theory as irrelevant). An approach that would bridge the gap would serve an integrative function, and this in itself would presumably give a theory like Keynesianism, once it was interpreted in a simultaneous-equations sense a certain appeal to the profession.

What is curious about this "mathematization" of Keynesian theory—its assimilation into the simultaneous-equations model—is that it took place without Keynes's direct participation. Indeed, as we have already seen, Keynes disliked mathematical method. After the *General Theory* was published, he accepted Harrod's and Hicks's mathematical interpretations, but when Harrod argued in 1938 that economic theory should be based on the general equilibrium model—that it should be a set of axioms, in the form of equations, with as much empirical content as possible—Keynes condemned this attempt to turn economics "into a pseudo-natural science."[77] Keynes preferred a more literary approach; in the methodological debates of the time, he sided less with those who sought to turn the discipline into a "science" than with old-fashioned economists like Jacob Viner and Friedrich Hayek.[78]

What this suggests, therefore, is that the development of Keynesianism did have an independent dynamic of its own—that the impulse did not derive solely from Keynes himself—and that this dynamic was to a considerable extent intellective in nature. But the fact that the theory became assimilated into the general equilibrium mold also serves to spotlight the degree to which nonintellective factors played a role—for seeing the theory more sharply should have focused attention on its defects; that these defects were to a large degree perpetuated shows that the development and entrenchment of the theory cannot be understood as a basically intellective process. In other words, once the theory was seen as a set of equations, it would have been a fairly simple matter to derive the multiplier that conformed to the system as a whole, to consider the effect of wage cutting and so on—conclusions which, as we have seen, would have fundamentally changed a number of central Keynesian doctrines. The fact that this was on the whole not done, the fact that when these doctrines were dropped, it was not because critical analysis had discredited them, shows even more clearly than in the case of Keynes himself that something other than pure logic was at work.

To see this in more detail, one can consider several works which, by reputation at least, played important roles in the process by which Keynesianism was

absorbed into the economic mainstream. Consider, for example, Hicks's "Mr. Keynes and the 'Classics': A Suggested Interpretation," which became the basis for so many textbook accounts. Given that Hicks's stated purpose was to contrast Keynes with the "classics," what is most striking here is the way he ignored the distinction Keynes himself had stressed with great force at the outset of the book: the different assumptions about the supply of labor. The supply side was not really discussed; the effect was to give the impression that the demand side was all that really mattered. As Keynes himself pointed out in a letter to Hicks commenting on the article, one could get away with this only as long as the wage rate was taken as a constant—in other words, as long as there was some assumption from the supply side; but the assumption about a fixed money wage had been explicitly dropped by Hicks.[79]

Much the same is true of Paul Samuelson's influential article, "The Simple Mathematics of Income Determination" (1948).[80] The analysis here was conducted on the assumption of a fixed price level; it ignored the supply side and thus excluded a priori the possibility that shifts in demand could affect the price level. This kind of approach, he later admitted, was something of a "caricature" of Keynes's theory, useful mainly for teaching purposes.[81] Nevertheless, in his *Foundations of Economic Analysis*, a book obviously not aimed at the novice, his paraphrase of the "Keynesian system" made no reference to the labor market; his set of equations did not include the equivalent of Eq. (4) but was limited to the equivalents of Eqs. (5) and (6).[82] The effect, again, was to perpetuate Keynes's own tendency to ignore the supply side and neglect price effects, at least for periods of less than full employment.

Consider, finally, Lawrence Klein's 1947 article, "Theories of Effective Demand and Employment," much of which is devoted to an analysis of the Keynesian system.[83] For a variety of reasons, Klein was uncomfortable with Keynes's assumption about the supply of labor; unlike Keynes, he was unwilling to admit the existence of money illusion on the part of the workers.[84] But as Keynes realized (given the assumption, shared by Keynes, Klein and the classics, that the demand for labor was a function of the real wage), if workers bargained in terms of the real wage, labor market equilibrium would in itself determine output and the level of employment; these variables would be unaffected by any changes that took place on the demand side. How therefore could Klein sacrifice money illusion and still claim that demand-side phenomena are crucial? He presented as an alternative the following model:

$$(2.4)\quad \frac{dy}{d\bar{N}} = \frac{w}{p}$$

$$(2.9)\quad \bar{N} = \text{labor supply (where } \bar{N} \text{ is a constant)}$$

$$(2.10)\quad \frac{d(w/p)}{dt} = g\,(\bar{N} - N)$$

But it can be shown that this model, if the real wage can be observed for some point in time t, defines the time path for the real wage, and thus also the time paths for output and employment. (For the proof, see Appendix IV.) In other words, these time paths are *unaffected* by anything on the demand side of the system. So Klein was unable to maintain the essence of the Keynesian analysis after he sacrificed money illusion. The fact that he thought he could—that he thought that "the important parts of the Keynesian theory are independent of Keynes's own theories of wages and the labor market"[85]—indicates that he must have been attracted to those "important parts" of the theory for reasons having little to do with the logic of the system as he presented it.

What all this suggests, I think, is the existence of a certain degree of dogmatization. Those features of the Keynesian theory which gave it its distinct flavor did not really follow from the mathematical analyses in which the theory was elaborated. They were logically prior to these analyses; indeed, the systems of people like Hicks and Samuelson guaranteed that the proper Keynesian conclusions would be drawn. The demand side continued to be the sole determinant of the level of economic activity; the price mechanism was not an integral part of the basic theoreteical framework; the implications about the virtue of spending and the dangers of saving followed directly from these models.

This impression about a degree of dogmatization is confirmed by some of the literary evidence. Professor Austin Robinson, one of Keynes's collaborators, noted with great distaste the tendency to treat Keynes's theory as sacrosanct—the tendency to begin a sermon "by quoting a text from the Holy Writ of Maynard."[86]

Indeed, it seems clear that there was a certain tendency on the part of his followers to press more extreme arguments than Keynes himself did. There was, as Harry Johnson wrote in a retrospective article on Keynes, a "hardening of certain of Keynes's conclusion into rigid dogmas in the hands of his disciples." Johnson focused on the Keynesians' "bias against money and monetary policy," which, as Johnson pointed out, Keynes himself did not really share.[87] There can, in fact, be hardly any doubt that in the immediate post–World War II period many Keynesian economists did tend to play down the role of money.[88] And although Keynesian economics tended over time to assign greater importance to monetary policy than it did in that period, it is interesting to note that even as late as 1961 Klein attacked Johnson's article with the remark that "maybe money is as important as Harry Johnson says it is, but he has a lot of proving to do before I shall believe it."[89] This was certainly a curious fate for a theory that Keynes had originally wanted to call "The Monetary Theory of Production."[90]

But the argument about dogmatization is one that should not be pushed too far. On the one hand, it is clear that a basic set of beliefs, values and biases gave the theory its distinct personality. But on the other hand, it is important to recognize that a number of key dogmas were sacrificed or abandoned. As noted above, Keynes's underconsumptionist theory of history had played a fundamental role in

his system; and this particular theory, with its assumption of an increasing amount of slack that the state would have to take up, was basic to Keynes's whole political and social philosophy. Alvin Hansen, Keynes's most prominent American disciple, had developed this theory into the doctrine of "secular stagnation" in the late 1930s. But, as Herbert Stein showed, it was not long before the doctrine collapsed:

> It took a little while after the first promulgation of the stagnation thesis by Alvin Hansen in 1938 before rebuttals from "scientific" sources began to be heard. By 1940 and 1941, however, negative analysis was reaching a flood. It came from Schumpeter, Angell, Hardy, Simons and many others. The whole thesis was under considerable suspicion by the time George Terborgh dealt it a most damaging blow in his 1945 book, *The Bogey of Economic Maturity*.[91]

Indeed, as early as 1943, Hansen himself began to change his tune and warn about the dangers of inflation and excessive demand—a shift for which he was criticized by his former student Paul Samuelson, who still in 1944 took a pessimistic line about postwar unemployment.[92]

The underconsumptionist premises of Keynes's theory had been the basis for his idea that a certain "socialisation of investment" would be necessary, and the theory of secular stagnation provided an important rationale for a policy of large-scale government spending. The curious thing here, however, is that at the very same time that the secular stagnation doctrine was being abandoned, Keynesian theory as a whole took on a highly fiscalist cast; direct government spending was seen as crucial, and other factors, such as monetary policy, were played down. Yet, as noted above, the extreme fiscalism of the immediate postwar period was eventually replaced by a more moderate and balanced approach to policy. The multiplier analysis, which provided the basic theoretical argument for a policy of spending, came to appear overly mechanical. In its original crude form, moreover, it was obviously ill suited to a period of high employment; if the economy was already operating at full capacity, output could hardly be increased (as the simple multiplier implied) by a new injection of spending. In the textbooks, the multiplier was expanded to take the money market, although not the labor market, explicitly into account. It thus became somewhat more sophisticated, but for purposes of serious analysis, even this version of the doctrine gradually ceased to play a central role.

Moreover, Keynes's argument that a high rate of saving normally hurt capital formation was more or less dropped. After World War II, there were hardly any references to this particular doctrine. Arthur Smithies, one of the Harvard Keynesians, explicitly disowned Keynes's attack on saving in 1951.[93] Moses Abramowitz, in a 1952 survey of the literature on the economics of growth, gave this doctrine only a very lukewarm endorsement.[94] By the mid-1950s, the argument against saving seems to have disappeared completely. The Subcommittee on Tax Policy of the U.S. Congress's Joint Committee on the Economic Report held a

forum in 1955 on the economic effects of federal tax policy, none of the leading economists who took part argued that savings should be discouraged by taxation in order to sustain a high level of employment.[95]

Finally, there was a basic shift in the theory of wages. Keynes had argued that cutting money wages as a general rule would not increase the level of employment, and indeed would probably decrease it. But the equilibrium model implicit in the *General Theory*, and made explicit by Keynes's interpreters, was perfectly symmetrical: if increasing a parameter shifted a variable one way, then decreasing it should shift that variable in the opposite way. In other words, these were not dynamic models which allowed, for example, for some "stickiness downward" of wages but flexibility upward; there were no ratchet effects. The final equilibrium level depended simply on the equations that defined the system, and did not depend at all on where the system was initially or in what direction it was initially disturbed. What this means is that an autonomous rise in the wage rate should affect employment in exactly the opposite way as an autonomous fall; Keynes's doctrine about wage cutting (which, as noted above, corresponded in the more general model to an upward shift in the supply curve of labor) would thus imply that a wage increase, brought about, for example, by trade union pressure, should probably increase, or at least not decrease, the level of employment.

The surprising thing, however, is that in the postwar period there was a widespread acceptance, among both Keynesians and non-Keynesians, of exactly the opposite idea: all other things remaining constant, increased wages bring increased unemployment. This corresponded to the intuitive notion that if wages rise, a fixed amount of demand supports a smaller number of wage earners. If it is assumed that the government pursues a policy of maintaining demand at a high enough level to assure full employment, this becomes a theory of inflation; the increased demand created to prevent unemployment from rising to the level that would otherwise result from higher wages also raises prices. The increased prices lead to a new round of wage increases, as labor seeks to protect its real income, and thus to further rises in wages and prices. A full-employment policy thus tends to accommodate, and in effect to institutionalize, a process of wage inflation.[96]

This theory of wages was widely accepted, and as a result, some of the values associated with the original Keynesian theory were abandoned. In the 1930s, Keynesianism had been oriented toward the moderate Left: union wage policies were defended, and the workers were not in any way blamed, even in a theoretical sense, for the high level of unemployment. It was still possible after 1945 for left Keynesians to maintain this point of view, but because their arguments tended to write off inflation as a serious problem, they gradually lost influence and Keynesianism as a whole moved toward the political center. Abba Lerner, for example, argued in 1946 that a "rise in wages by itself would have no general effect on employment but would bring with it a general rise in prices";

if, however, the government tried to block the price rise by refusing "to provide the liquidity necessary to maintain employment," this "deficiency in the amount of money" would be the real cause of any unemployment that would result. "The whole difficulty could be avoided very simply by the authorities creating the additional money needed"; they would fail to do so only if they believed "that the unemployment was preferable to the increase in the amount of money."[97] The mechanism of wage inflation was recognized, but the values of the 1930s were maintained. The argument that the unions could be responsible for unemployment was deflected; it was the state that would be held responsible if demand were not kept at the level needed to prevent unemployment. The effect, clearly, was to load the dice in favor of an inflationary policy; but most economists felt that inflation should be taken as a serious problem, and for this reason the attempt to salvage left Keynesian values in this area was to prove unsuccessful.

In all the cases examined here—the decline of stagnationism, fiscalism, the multiplier doctrine, and Keynes's doctrines about saving and changes in the wage rate—we find the same general phenomenon: a softening of the theory, a greater concern with balance and moderation, a move toward the political center. It is important to stress that these shifts in doctrine did not result from the internal development of the theory, that is, from rigorous, critical analysis that brought out the defects in the original theoretical framework. There were, of course, many critics of the Keynesian theory, and their attacks focused on many of the points stressed here. J. M. Clark, for example, parodying Keynes's language in 1942, spoke of a "propensity to obliviousness of the importance of wage and price adjustments"; the general claim of these conservative critics was that Keynesianism neglected the supply side and did not take the problem of inflation seriously enough.[98] But such attacks had little effect on what in the postwar period had become a new orthodoxy. The criticism, being nonmathematical, was unable to lay bare the defective internal structure of the Keynesian theory and could be turned aside as the lament of old-fashioned economists.

As for economists in the new mainstream, they too did not apply the new mathematical tools to the analysis of the central dogmas Keynes had laid down in the *General Theory*. The use of differential calculus to analyze the effect of a parameter change on the variables in a model was still not a natural reflex in the postwar period. Thus Richard Goodwin, in the article on "The Multiplier" he wrote for Seymour Harris's important volume *The New Economics*, concluded that for the presumably normal case where price changes accompany shifts in employment, "precise and complete analysis" of the multiplier "is difficult, because the equations cease to be linear."[99] There seems to have been a certain preference for two-equation models, like the famous Hicksian IS-LM model, which could be analyzed by drawing graphs, over more complex systems (such as would result from adding an equation to represent the labor market), for whose analysis some calculus would be essential.

On the particular issue of the effect of a change in the wage rate (i.e., of a shift in the supply curve of labor), it is striking how unmathematical the analysis was in the postwar years, in spite of the rapid development of the mathematical model as the basic mode of analysis. James Tobin's important article "Money Wage Rates and Employment" is a good case in point.[100] The analysis is similar in style to that used by Keynes in the *General Theory*. There is no explicit model, thus no systematic examination of the effect of a parameter change on the variables in the model; since the problem of wage cutting is not defined explicitly in terms of a shift in the labor supply function, it is in the final analysis not well defined at all. For in a system where all variables are determined, change in any such variable is possible only if the parameters that determine them are allowed to change—and a change due to a shift in one parameter will have different effects from the same change brought about by a shift in a different parameter. Tobin's conclusion about the effect of a shift in the wage rate was in any case indeterminate, the result of his reliance on a series of ad hoc arguments. But the important point is that because of the looseness of method, the development of the theory was relatively unconstrained, in the sense that the same assumption could yield a variety of conclusions.

How then is the development of the theory to be understood? The obvious answer would focus on the radical change in economic conditions that had taken place in the 1940s; depression was replaced by relatively full employment, and some of the older doctrines simply became irrelevant. Moreover, the new views of the postwar period did not sharply contradict Keynes's older ideas; there was no compelling need for rationalization or for a rethinking of basic ideas. After all, the *General Theory* had clearly stated that in a period of full employment the classical analysis was applicable (as a limiting case). In such a situation, a greater prepensity to save would raise the level of investment, and an increase in spending would affect only prices, not output.

Even in the case of wage changes, Keynes had included a passage describing the inflationary effects of a full-employment policy. High employment, he said, would enable the labor unions to insist on ever higher money wages, and buoyant demand would allow businessmen to accommodate labor in this area. This argument became quite common. It was taken up by some of Keynes's followers (Joan Robinson, most notably) and by conservatives like Joseph Schumpeter.[101] The effect was to obscure the contradiction between Keynes's original theory of wages and the new view which took hold after the war. After all, hadn't he already accepted the basic conclusion that wage demands could be an important cause of inflation? It was the conclusion that was important, not the nature of the particular argument by which it was reached.

Yet the contradiction was real: because of the symmetry of the theory, if a rise in the money wage led (all other things held constant) to an increase in unemployment, a fall in the wage would (contrary to Keynes's argument) therefore cut unemployment. If theories develop along basically rational lines—if the dynamic

of conceptual change is essentially intellective in nature—then one would expect criticism to have focused on this point. One would in that case expect economists to have asked what role the doctrine about wage cutting played in the *General Theory*, how it was related to the central thrust and analytical style of the theory as a whole and what the implications of its rejection would be, insofar as other Keynesian doctrines were concerned.

All of this sheds some light on the nature of the theory and the process by which it developed. Again, the most important point is that the theory was sufficiently loose to accommodate new views that were not logically consistent with the dogmas that gave the original theory its distinct character. While this gave Keynesianism a certain flexibility and vitality, it also meant that rigorous analysis was not essential to its development. The theory could grow by accretion; new views were simply added on as conditions warranted. This was possible because the overwhelming concern of the profession was with current theory and current issues; the problems of the *General Theory* were not a matter of vital concern, since the book itself could be viewed as already out of date, and an understanding of the origins and development of current theory was not viewed as very important.

The theory therefore moved with changing social concerns, unencumbered by any obsession with internal logical consistency. As the problem of unemployment declined in terms of social importance, and as attention shifted to the problem of inflation, the theory itself moved along similar lines. What this meant was that Keynesianism in the postwar period became more consensual. It lost its edge, but not its essence. Its basic values and beliefs (about the need to fight unemployment, the basic responsibility of the state, the desirability of a "mixed economy") were in large measure perpetuated. As a result, the tone of the doctrine remained optimistic. Fiscal policy was seen as a highly effective agent in securing full employment; a mild inflation, it was thought, was on balance acceptable, and in any case could easily be controlled. But one need only assume a direct relation between money illusion and price stability to see how Keynesianism could be a victim of its own success, how a full-employment policy could contain within itself the seed of its own destruction. For the longer even a mild rate of inflation persists, the more likely workers would be to insist on bargaining in what would amount to real, as opposed to purely monetary terms, and thus the less effective Keynesian techniques would inevitably be.

It follows that an ambitious policy of seeking perpetually high levels of employment would result in inflation and thus gradually destroy the phenomenon of money illusion, whose existence was what made a Keynesian policy effective. A more restrained policy of merely clipping the wings of incipient depressions by the sporadic application of Keynesian techniques might have conserved price stability and thus workers' money illusion, but early Keynesian theory did not think it was necessary to settle for such limited goals.

The tendency to oversell the effectiveness of macroeconomic policy was directly related to the disarray within the profession that resulted in the 1970s when events made it abundantly clear that Keynesian techniques had been oversold—that unemployment was no longer subject to easy manipulation and that inflation was becoming more of a problem than Keynesianism had anticipated. The existence of this sense of disarray can hardly be denied. Consider, for example, the titles of some of works in which a number of eminent economists discussed the problems of the profession: *The Crisis in Keynesian Economics*, by John Hicks (New York, 1975), Sidney Weintraub's "The Keynesian Light that Failed,"[102] and William Fellner's "Lessons from the Failture of Demand-Management Policies: A Look at the Theoretical Foundations."[103] A good many other works could be cited; Walter Heller had already in 1974 compiled a long list of articles critical of the profession.[104] In the 1970s the attack on Keynesianism from the right came to be taken much more seriously. Friedrich Hayek began his "campaign against Keynesian inflation" in 1974, when he won a Nobel Prize and touched on the issue in his Nobel lecture; another attack of his on Keynes was reprinted in the Op-Ed page of the *New York Times* on November 15, 1974.[105] Perhaps the sharpest attack from within the economics profession came from the distinguished monetary economist Harry Johnson, who argued that Britain paid "a heavy long-run price for the transient glory of the Keynesian Revolution." Johnson stressed that Keynesianism had led to "the corruption of standards of scientific work in economics."[106]

But my real concern here is not so much with consequences as with causes. Why did Keynesianism triumph among the economists? If the phenomenon is not to be understood in purely intellective terms, what kind of alternative explanation makes sense? The most obvious answer is that Keynesianism was accepted because the political views with which it was associated were particularly attractive. This explanation seems especially appropriate for the generation of economists who came into the profession during the period, say, from 1936 to 1950. Their views were bound to have an abnormally large impact on the field, simply because of the rapid expansion of the profession, especially after World War II: between 1945–1946 and 1951–1952, American universities awarded about 1800 Ph.D.'s in economics, whereas fewer than 4000 doctorates in the field had ever been awarded prior to 1945.[107] Rapid expansion meant a change in the age composition of the profession, and the creation of extraordinary opportunities for advancement; this obviously meant that the views and values of younger scholars would, as a group, have greater and quicker impact than would normally have been the case.

What was this younger generation of economists like? One can assume that those economists who came into the profession in the 1930s and 1940s had a number of things in common. It is likely that anyone drawn to the profession in this period of crisis had become interested in economics for more than purely

intellectual reasons. As James Tobin wrote, "Economists of my generation grew up in the Depression and learned economics at the birth of the Keynesian Revolution. We were attracted to the subject by the happy combination of intellectual excitement and promise of dramatic social improvement."[108] The profession, it seems reasonable to assume, was particularly attractive to people who were sensitive to the suffering caused by the depression and wanted to do something about it. Such people, in all likelihood, shared many of the values of the political left: that there was something basically wrong with laissez-faire capitalism, and that the state should assume a more active role in solving social and economic problems. At the same time, in the light of what was then happening in Europe, people who came of age politically in the 1930s might well have worried whether the goals of social justice, economic prosperity and political liberty were all consistent with each other.

It is easy to see why Keynes's doctrine should appeal to such people. The theory was activist, rather than essentially descriptive, in tone; it implied that the state, by instituting minimal central controls, could solve basic economic problems without any real loss of freedom. Thus the doctrine tended to resolve the contradiction between social and liberal values. Its fiscalist bias meshed well with the idea of spending for social welfare—and it is not hard to find leading Keynesians arguing for what amounted to a welfare state in periodicals that reached a broad public.[109] Its distaste for wage cutting and its favorable attitude toward a redistribution of wealth also conformed to the values of the moderate left.

Keynesianism thus functioned as a bridge to the left, in the intellectual sphere as in the larger social sphere. Keynes accepted, for example, the economic interpretation of imperialism, a characteristic left-wing doctrine, but cast the argument in the form that imperialism was not necessary to maintain demand, since spending at home had the same effect as acquiring markets abroad.[110] At least some of his followers took up this point. For example, Samuelson was still arguing along these lines in the 1970s.[111] Thus, one could condemn the system in the past without having to condemn it in the future; one could accept the prevailing system without having to repudiate one's earlier criticism of it. Keynesianism thus served to ease the integration of liberal and left-leaning intellectuals into the social (and professional) mainstream in the same way that Keynesian policies tended to aid the cause of social integration. Indeed, the fact that it could have this effect would in itself tend to recommend Keynesianism to many in the political center, for whom social integration was a prime concern. One suspects, moreover, that this aspect of Keynesianism was of particular importance, given the shift in the political climate from the late 1930s to the early 1950s. The swing to the right was bound to affect economists; anything that would help ease the problem of adjustment would have a certain utility, and Keynesianism clearly seems to have facilitated the transition from a "radical" to a "liberal" political stance.

The case of Abba Lerner, who was to become one of the most prominent Keynesian economists, provides a good example of this. In the summer of 1936, Keynes had been arguing very forcefully against the *New Statesman*'s line that the left should oppose British rearmament as long as a conservative government was in power. Lerner, together with the Marxist economist Paul Sweezy, joined the debate in the September 15 issue. The current British government, they argued, "as the representative of the ruling class," was no longer a "champion of democracy," but rather had "changed sides"; its main concern was to defend the capitalist order against the left, and its only other interest was in quarreling with foreign capitalists. In either case it could not be trusted with increasing armaments. Lerner and Sweezy praised Keynes's economic writings, but concluded that "it would be a pity if Mr. Keynes' progressive work in pure economic theory should lend authority to a political attitude which can only be described as reactionary and obscurantist."[112]

But by 1960 Lerner's views had shifted dramatically. Now the Soviet Union was the enemy, and the great value of Keynesianism was that it could "set us free to apply our vast resources for the defense of the free world and the economic development of the poor world in our fight with the totalitarian world. This is the task and this is the case for political economy."[113] And indeed this theme about the importance of full-employment policies in the American struggle with the Soviet Union was rather prominent in the Keynesian literature in the late 1950s and early 1960s.[114]

The rise of Keynesianism cannot be understood without taking political factors of this sort into account. But it would be a mistake to argue that they provide in themselves a sufficient explanation for the triumph of the doctrine and for its emergence as a new orthodoxy by the early 1950s. Doctrinal change always takes place in a particular institutional setting, and in this case the structure and position of the profession as a distinct social entity had an important bearing on the evolution of economic thought.

In fact, the special institutional situation that existed after World War II served to intensify the process by which Keynesianism became entrenched. The production of Ph.D.'s was at this time highly centralized, with Harvard occupying an extraordinary position of dominance. Between 1945–1946 and 1950–1951, it produced 257 economics Ph.D.'s, 17.1 percent of the total for American universities. (This compared with an output of 121 for the whole decade between 1935–1936 and 1944–1945, thus representing a virtual quadrupling of the annual rate.) Columbia came second with 132 Ph.D.'s in the six-year postwar period, accounting for 8.8 percent of the total, as opposed to 11.5 percent in the previous decade. Next was Chicago with 6.3 percent, Wisconsin with 6.1 percent, Cornell with 4.5 percent and Illinois with 4.4 percent; Berkeley produced only 2.7 percent of the total.[115]

Harvard's dominance in the training of young theoreticians was even greater than these raw figures suggest. It is well known that departments place their

Ph.D.'s "down the institutional prestige ladder"; a Harvard Ph.D. is more likely to get a position carrying real weight in the profession than one from Illinois.[116] Assuming (as argued before) that young economists at this point played a strategically crucial role in bringing about change in accepted theory, and assuming also that there was a basic outlook, at least regarding macroeconomic theory, associated with the Harvard department that was in some measure transferred to the economists it produced, the effect of this high degree of centralization was to facilitate rapid doctrinal change throughout the profession as a whole. In a more decentralized system, the attitude of a few key figures at Harvard would have been far less decisive; the process might have been more pluralistic and perhaps less doctrinaire in tone.

Alvin Hansen was the most prominent of the Harvard Keynesians in the late 1930s and 1940s. It was, however, only after Hansen had moved to Harvard from Minnesota in 1937 that he was converted to Keynesianism. Oddly enough, given his later reputation as the "American Keynes," he had just published a review of the *General Theory* that was sceptical, if not downright hostile. The reasons for Hansen's conversion remain something of a mystery. But it is instructive to note how economists handle the issue: it was simply a "breakthrough to the new perspective" (R. Musgrave). Samuelson wrote: "On the train from Minnesota, so to speak, Hansen must have seen the light." The economists' accounts, however, agree that Hansen played a key role in spreading Keynesian ideas. Samuelson's view is typical: "considering the strategic importance of Harvard at that juncture in history, Hansen through his hold on his students wielded a tremendous influence on the course of modern economic stabilization policy."[117]

The institutional framework was thus unusually favorable, and the theory took root rapidly. Keynesianism, now in the ascendant, profited from the normal and familiar mechanisms through which dominant ideas become entrenched: the sanctions against eccentricity and being "old-fashioned," the corresponding incentives for conforming to the prevailing trend and in general the whole phenomenon of "cultism." As one author put it, in 1942:

> The so-called "halo effect" not only gives renown to the mediocre works of a great savant, but also sheds some rays upon his followers. (Attaching oneself to a school of thought is often a matter of necessity in some highly ritualized field, however, for the subject matter may be monopolized by networks of "academic cartels," similar to the "Chaucer trust" and others in literary scholarship.) Hence an idea originally effective attracts to it men who are conscious of the power it bestows upon them as a symbol; these late devotees and followers may use it intelligently or unintelligently. . . .
>
> As long as the cult thrives, particularism flourishes. Members of the in-group are favored by one another in book reviews; complimentary references are made only to the writings of authors with "approved" points of view. The clique itself may be sufficiently well established to keep all but the elect in subordinate positions. . . .[118]

Of course, anyone who has ever taught in a university knows how "political" academic affairs can often be. Evidence of this kind of thing, for obvious reasons, is rarely preserved in the official papers of academic departments, or in other archival sources for that matter. But every now and then one gets a glimpse of this side of the decision-making process. In the Jacob Viner papers, for example, there is a copy of some correspondence exchanged in 1935 between Paul Douglas and Frank Knight, both very eminent economists and Viner's colleagues at the University of Chicago. Knight and Douglas were engaged in a very sharp dispute over a personnel matter. Douglas, according to Knight, was trying to "get" friends of his, and the conflict had very obvious political overtones. "Your thirst for blood," Knight wrote, "does not surprise me so very much. I have read a little history, and it shouldn't take a great deal to teach one to expect, at best, nothing less than complete ruthlessness from anyone of a strong general reforming interest when any personal prejudice is involved in an issue."[119] (Douglas was a liberal, and Knight a conservative.)

Or to give another kind of example, consider the following comments regarding hiring made by no less a figure than Joseph Schumpeter in November 1940 (at this point Schumpeter was seriously considering moving to Yale from Harvard). A certain candidate was "irreproachable in views and character" and would be "a most amiable and gentlemanly colleague," while another had left Germany because he was married to a Jewess. "But he is neither a Jew nor a socialist himself," Schumpeter wrote, "and [is] perfectly free from all the less desirable traits sometimes observable in emigrants."[120] In general it seems clear that purely academic considerations are not the only ones that determine advancement, but to anyone who has ever had a chance to see how scholarly communities operate, this conclusion will not be very surprising.[121]

The real question, once again, is one of degree: How much does academic judgment count for in determining the shape of the profession, that is, who gets the best jobs, who gets published (and where) and how prestige is allocated? Since an adequate mass of documentary evidence is simply not available, it is hard to deal with this problem in any meaningful way; the few sources I was able to see indicated that "political" considerations were not quite as powerful as I had originally thought. Consider, for example, the case of the Yale Economics Department. In the immediate post–World War II period, the department was seen as academically "sub-standard for Yale"—i.e., distinctly inferior to its counterpart at Harvard—and both the dean and the new chairman, Kent Healy, worked hard to build it up. The papers in the Yale archives give the distinct impression that purely academic criteria or, at the very least, considerations of professional and institutional prestige were decisive; the department sought to attract both Keynesians (like Metzler and Tobin) and non-Keynesians (Viner and Schumpeter).[122] The same concern for prestige and academic quality is evident in a working paper Samuelson wrote for the Dean of Humanities at M.I.T. in 1949, in which he defined the goal of the department as follows: "The economics department at M. I. T. should aim to be among the first half-dozen centers of economic research and teaching in the country. It should acquire, and earn, the reputation of being one of the leading places and of turning out an increasing

number of trained people needed in public administration, in business and in universities."[123]

From such archival sources, one gains the impression—and it is little more than that—that political considerations were essentially peripheral to such matters as hiring. There might have been a strong prejudice against the far left during the late 1940s, for example. To cite one case of this, J. R. Killian, then president of M.I.T., wrote to Henry Luce, the publisher of *Time*, in 1949, in response to criticism of M.I.T.'s economics department printed in the magazine. Quoting the department's chairman, Killian wrote that no M.I.T. economist had "any patience with Communist doctrine. We are lined up, every man, in opposition."[124] But such attitudes appear to have had little bearing on the careers of economists, whether Keynesian or non-Keynesian, in the academic mainstream.

What all this indicates is that something is still missing. Political values, and the institutional structures through which they made themselves effective, while of fundamental importance, do not in themselves provide an adequate and compelling explanation for the triumph of Keynesianism. This kind of approach is too one-sided: the values that are projected into the economics profession from the outside are seen as crucial, while the internal life of the profession is neglected. It is as though the profession itself were a passive agent, not an active force—as though its concerns and interests, its needs and desires as a corporate entity, were of no relevance to the analysis.

Yet is is clear, first of all, that the dynamic of doctrinal change was internal to some degree and that to understand the rise of Keynesianism, it must be seen in the context of the other doctrines against which it was in some sense competing. But it would be wrong to look only at high theory as it existed before World War II. For in the United States, in the 1920s at any rate, institutionalism—the highly empirical study of how economic institutions and systems actually operate—set the tone for the profession. At the time, institutional economics seemed to be, in Gunnar Myrdal's words, the "wind of the future."[125]

The leading institutionalists were rather hostile to theory as such. One might laugh at their naivete today, but it is easy to see the appeal of their approach in the context of the time. They argued that theoretical economics told one nothing about the real world; ultimately, it was just an intellectual exercise. What the world needed was a real science. Thus Wesley Mitchell, the most eminent figure in this school, wrote J. M. Clark in 1928 about his education in economics: "The technical part of the theory was easy. Give me premises and I could spin speculations by the yard. Also I knew that my 'deductions' were futile. It seemed to me that people who took seriously the sort of articles which were then appearing in the QJE might have a better time if they went in for metaphysics proper." But, he wrote, "there seemed to be one way of making real progress, slow, very slow, but tolerably sure. That was the way of natural science." Economics should therefore model itself on sciences like physics and chemistry: "They had been built up not in grand systems like soap bubbles, but by the patient processes

of observation and testing."[126] This point of view had itself been institutionalized with the creation of the National Bureau for Economic Research, where Mitchell was the most important figure within the academic world proper. Columbia University, where Mitchell taught, was its chief stronghold.

Institutional economics was, moreover, loosely associated with a certain approach to economic policy. Mitchell, for example, was an ardent supporter of "planning," by which he meant thinking seriously about economic problems and elaborating policy accordingly—a very vague conception, as notions of "planning" go. Insofar as the institutionalist approach was more specific, it tended to focus on the implications of imperfect competition; the concentration of economic power was the basic fact entering into the analysis.[127] Indeed the problem of monopoly was central to policy formation during the New Deal period; the fundamental assumption was that because competition was imperfect, market mechanisms had not been able to assure the full use of resources; and the debates about what to do about unemployment—whether to break up the monopolies through effective antitrust legislation, or "regulate" the economy, either by the state or by the business community directly—took this assumption as their point of departure.[128]

But within the economics profession, even by the early 1930s, this whole approach was coming to be seen as somewhat sterile. The conceptual world of institutionalism was a thick forest, but what the country needed was a clear path out of the woods. Institutionalism was coming to be viewed with some distaste: the young Milton Friedman, for example, wrote his old teacher Jacob Viner about the "extreme institutional atmosphere at Columbia," where he was then attending graduate school. "The density of the atmosphere is amazing," and J. M. Clark was "the only one whose institutionalism seems to be at all intelligent."[129]

And indeed reading the American Economic Association (AEA) presidential addresses from this period on, one has the sense that economics was suffering from a kind of crisis of irrelevance in the 1930s. Thus even J. M. Clark himself declared at the end of 1935 that "in a time of desperate need economics has not furnished the unified and authoritative guidance which many have thought, rightly or wrongly, that they had a right to expect."[130] Alvin Johnson developed the point in his presidential speech a year later: "We all know, indeed, that the intelligent public is discontented with the economists." He argued that "the time has come when the economists need to organize themselves effectively to do the work which society may reasonably require of them."[131] And the same kind of theme ran through Viner's 1939 address, which, however, coming after Keynesianism had begun to take root, was already somewhat retrospective in tone. The older theorists, Viner said, were brought up to think in terms of the long run and thus tended to slight immediate problems. The result was that younger economists, deeply impressed by the depression and more directly concerned with issues of immediate policy, turned against "the traditional corpus of economic

theory, which they look upon as an instrument for the exercise of the tyranny of the dead mind over the living." The world of the classical economists appeared "too far divorced from present-day realities and values to warrant faith in its usefulness as an aid to the guidance of social policy." It was not Viner's purpose to defend the old economics, but rather to keep the pendulum from swinging too far in the other direction: "For various reasons, but chiefly as a psychological reaction to the impact of continued and acute depression, some economists have been discarding too indiscriminately their inherited intellectual ballast, with the result that they sway too easily with each passing wind."[132] It is clear from context that Viner was alluding to the spread of Keynesianism; his address stands out as an explanation of the phenomenon in terms of the perceived—and in large measure admitted—irrelevance of the old theoretical framework.

But this feeling of social inadequacy largely vanished after World War II; one notes a certain complacency and self-congratulatory tone in the 1950s and 1960s, reaching a high point in George Stigler's 1964 address:

> I would gloat for one final moment over the pleasant prospects of our discipline. That we are good theorists is not open to dispute: for 200 years our analytical system has been growing in precision, clarity and generality, although not always in lucidity. The historical evidence that we are becoming good empirical workers is less extensive, but the last half century of economics certifies the immense increase in the power, the care, and the courage of our quantitative researches. Our expanding theoretical and empirical studies will inevitably and irresistably enter into the subject of public policy, and we shall develop a body of knowledge essential to intelligent policy formulation. And thus, quite frankly, I hope that we become the ornaments of democratic society whose opinions in economic policy shall prevail.[133]

Wassily Leontief's 1970 address marked the transition to a new period. What is striking in the presidential addresses in the 1970s is an upsurge in self-criticism, a sense that the profession was entering on a new period of crisis. "Economics today," wrote Leontief, "rides the crest of intellectual respectability and popular acclaim. . . . And yet an uneasy feeling about the present state of our discipline has been growing in some of us who have watched its unprecedented development over the last three decades." This uneasiness, he said, was caused "not by the irrelevance of the practical problems to which present day economists address their efforts, but rather by the palpable inadequacy of the scientific means with which they try to solve them."[134] Indeed, the one defense of the profession in this period, Walter Heller's "What's Right with Economics," by its very existence attests to the pervasiveness of this sense of unease and the loss of self-confidence experienced by the profession as a whole.[135]

Keynesianism, of course, was not the only factor involved in these changing attitudes—both the rise of econometrics and of mathematical economics in general were also of great importance. But to the extent that the AEA presidential addresses provide a rough measure of the concerns and interests of the profession, one can make a strong argument that the rise of Keynesianism was significantly related to the interests of the economists as a corporate group. The chronology alone clearly suggests a certain linkage between Keynesianism and the fortunes of the profession. It is evident that Keynes's doctrine implied that what economists did was basic to the fate of the entire society; economics was no longer peripheral or irrelevant. As long as Keynesianism seemed to work the status of economists was high, and their self-confidence as a profession was correspondingly great. But when events, far more than theoretical reasoning, demonstrated its shortcomings for all to see, the self-confidence of the profession was shaken and its prestige was threatened. One can readily understand why economists, and especially younger economists, would (in the 1940s and 1950s) be attracted to a doctrine which promised to greatly elevate the status of their profession, for Keynesianism implied that depression and the social and political dislocation associated with it could be avoided easily only if the authorities accepted economists' advice. As long as Keynesianism seemed to work, this image of the profession seemed plausible; it performed a role of vital social importance. The effect was to raise considerably the status of the profession as a whole.

It is in this context that economists' claims about the great social value of the profession, and of Keynesianism in particular, have to be understood. Assertions of this sort have been quite common. Samuelson, for example, remarked in 1944 that World War II could be called as much an economists' as a physicists' war.[136] Lerner, in 1960, stressed how "relearning" the lessons of Keynesianism was "a matter of the utmost importance for the persistence of a free society."[137] And of course the rise of the profession has been celebrated by those "new economists" who played a leading role in shaping economic policy along Keynesian lines in the 1960s.[138] "Macroeconomic policy," Walter Heller, for example, stated, "capped by the tax cut, was the major force holding the postwar economy on a vastly higher plan than the prewar economy."[139]

There were few attempts to actually prove this kind of claim, and much of the evidence seems to cut against it. As Harry Johnson pointed out, countries other than Britain (where Keynesian policies were adopted earlier than elsewhere) "have had at least as good luck without following Keynesian policies or even knowing what they are—the 'new economics' won acceptance in the United States only as recently as the tax cut of 1964, and Japan's economic policy seems to have been orthodox in the extreme."[140]

The basic point, however, is not whether Keynesianism was responsible to any significant degree for the prosperity enjoyed by the West since World War II; it is simply that the belief that it was was a central part of an ideology that directly served the interests of the economics profession. For there are many channels through which a profession is brought to recognize that its prestige and the rewards bestowed upon it are directly related to its apparent social utility. It followed that the profession had an interest in convincing society that it served a crucial function; Keynesianism was the intellectual vehicle through which soci-

ety could be persuaded. Thus the phenomenon of dogmatization was intimately related to the interests of the economics profession as a corporate group.

This becomes even clearer when the rivalry in the 1930s with institutionalism is taken into account. This rivalry was in no sense absolute, either on the theoretical level or on the policy level. The kind of planning associated with both the Keynesian and institutionalist approaches appealed to similar groups, with the result that both approaches were occasionally mixed together, with little concern for the differences in the underlying theoretical orientation. The staff's final report to the Temporary National Economic Committee is a good case in point; whereas the bulk of the report is concerned with structural problems, the two "Keynesian" chapters, XII and XVI, are clearly animated by a completely different spirit.[141]

Nevertheless, it is important to bear in mind that the spirit of the two approaches did differ, and that from the standpoint of the economics profession, Keynesianism had certain advantages over these rival approaches to economic reform. For unlike the Keynesian approach, the various institutionalist approaches to planning did not assign a really central role to the economics profession. For if the basic problem was seen as an excessive concentration of economic power, then the solution would also be essentially political, and economic expertise could play at best a secondary role. Should the monopolies be broken up, or should the free market be suppressed through a "regulation" of the economy, whether by the state or by the business community? In either case, the basic process for reform and economic governance would be political at its core; even if there were overall planning under the aegis of the state, the elaboration of the plan would inevitably involve bargaining between competing interests, with the outcome of the process determined by the relative power and influence of the interests involved rather than by technical economic considerations. If goals were to be so specific, and planning so detailed, then no one could claim that economic policy could be politically netural, and thus best left to the experts to work out.

Professional interest in the realm of policy in fact dovetailed with a different kind of professional concern: the economics of imperfect competition was viewed as distasteful because mathematically it is so inelegant. It is much easier to develop theory on the basis of a perfectly competitive model. And finally the existence of rival prescriptions which took the problem of monopoly as basic meant once again that Keynesianism could occupy the central ground in policy debates, halfway between full-fledged planning ("organized capitalism") and laissez-faire, and could thus again serve a certain integrative function. In the context of the policy debates of the 1930s, the rise of Keynesianism can thus be seen as a kind of "revolution from the middle."

The conclusion of the argument, then, is that professional interest may provide the crucial missing element needed to account for the rise of Keynesianism. But this conclusion was based mainly on a functionalist mode of analysis, that is, on

the assumption that there were certain presumed needs which Keynesianism met in certain ways, and that this largely accounts for its acceptance. But this is in many ways unsatisfactory. The basic problem, of course, is that it is too easy—it readily explains anything that happens—because it just dismisses the question of the mechanism through which needs shaped events: did it operate on a conscious level? If not, how can it be studied and how can its existence be proven? These are sensitive questions because they are so closely related to the problem of the subjective honesty of the people involved: how can one assert that professional interests and political values influenced the development of theory if the economists in question really thought of themselves as dispassionate scientists, aloof from considerations of that sort?

It is hard to get a handle on this kind of problem. Indeed, the assumption about the prevalence of dispassionate, "scientific" values should not be accepted too readily. Even so distinguished and professional an economist as Paul Samuelson wrote Alvin Hansen about how he had "deliberately overstated" a certain point in Congressional testimony in order to counteract an impression left by a more conservative economist.[142] Such evidence of intentional distortion for political reasons is of course rare, but this does not mean in itself that its existence is uncommon. Indeed, it is safe to presume on general grounds that political and social beliefs to some extent directly shape the ideological content of economic doctrines. But the problem of the degree to which this process operated at the conscious level is one that is very difficult to bring into focus and study in a meaningful way.

The same kind of thing is true of the problem of the role that professional interest plays in conditioning doctrinal change. This too is a difficult subject to study, but there are a few touchstones by which the importance of this kind of factor can be assessed. For example, if economists had been consciously working to enhance the status of their profession, one would expect them to have played an active role in pressing for the Employment Act of 1946, which by setting up the Council of Economic Advisors (CEA) gave official recognition to their social importance; one would expect active lobbying on their part to have had something to do with the decision of the National Science Foundation (NSF) to support research in the field, thus giving the discipline a kind of official recognition as a science. Yet neither of these things seems to have been the case. With the Employment Act, a few Keynesian economists played a certain part in getting it enacted, but the provision setting up the CEA was not their fundamental goal, and in fact the council came into being almost by accident.[143] Nor did any campaign by the profession have anything to do with the NSF's decision in the mid-1950s to fund the social sciences—or at least this is the impression given by the best recent studies of the NSF.[144]

It follows, I think, that whatever mechanism that does exist is likely to be much looser and more subjective in nature. There is above all the sense that society, which in the final analysis supports the profession, has the right to

expect something in return and that if the profession fails to meet society's needs, then eventually some penalty will be exacted. The message is reinforced in all sorts of ways; research funds from the foundations, which are of course of strategic importance for career advancement, are often for example allocated with some regard to social need.[145] It is perhaps mainly through this kind of indirect channel that interests, beliefs and values come to condition intellectual endeavor.

All this may be rather vague, but the analysis here does not pretend to provide anything like a compelling explanation for the rise of Keynesianism. The basic aim was really negative, to demonstrate that it cannot be understood essentially as an "intellectual revolution." This, I think, is not a trivial conclusion, given the prevailing set of beliefs within, and even outside, the economics profession. Beyond that, the aim here was in essence to suggest the kind of factors that must have had some bearing on the success of the doctrine. Economists will certainly resist this kind of analysis in any case; the profession is concerned with affirming its status as a science, and one of the touchstones of a science is a positivist self-image, an image that is projected onto the discipline's recent past. The irony is that such a self-image is counterproductive in its own terms, for it is impossible to control for nonintellectual factors unless one admits their existence. And their existence and importance have been demonstrated here, I think, beyond reasonable doubt.

NOTES AND REFERENCES

1. Keynes to George Bernard Shaw, Jan. 1, 1935, in *The Collected Writings of John Maynard Keynes*, vols. XIII and IV, *The General Theory and After*, ed. Donald Moggridge, Part I, *Preparation* and Part II, *Defence and Development* (London, 1973), XIII, 492. This source will henceforth be cited as "CW."

2. Consider, for example, Donald Winch's *Economics and Policy* (New York, 1969), which is certainly one of the best historical studies in this general area. Winch states at the outset that he has sought to avoid portraying things as "simply blind or brilliant stumblings toward the present light" (p. 25). But this does not prevent him from treating conformity to Keynesian assumptions as a measure of the validity of a policy; thus, the Council of Economic Advisors provided "protection" from the "weaknesses" of the Employment Act of 1946 (p. 277). And in general he makes a sharp distinction between "ideology" and "economic understanding"; he believes that an "intellectual revolution," completed by a process of education bringing the political elite "to a state of understanding approaching that of the professional economic community" can replace "old fetishes" with "a genuinely rational approach to the problems of economic management" (pp. 307 and 311).

3. J. M. Keynes, *The General Theory of Employment Interest and Money* (London, 1936), pp. 32–33. This will henceforth be cited as "GT." Note incidentally that Keynes's argument about the completeness of Ricardo's victory is contradicted by his later claim that in the nineteenth century, the government consciously sought to mitigate unemployment through "improving" the balance of trade—a most un-Ricardian policy (GT: pp. 348, 349, 382). It is instructive also to compare these claims about Ricardian economics with a passage Keynes wrote in the early 1930s denying the practical ascendancy of economic theory. Orthodox theory, he wrote, always stressed the importance of saving, while "uninstructed public opinion and the common sense of the business world" favored policies directed at increasing investment "or even, in some cases, toward diminishing saving. On these matters economists have seldom succeeded in convincing the public. Their failure to do so and the deep-rooted suspicions as to their competence which their unsuccessful attempts have aroused, are, I think, at the bottom of the explanation of the low standing of economists regarded as practical experts and the unwillingness of statesmen and businessmen to accept their advice" (CW: XIII, p. 406). The two passages are mutually contradictory, yet each has considerable persuasive force. Taken together, they illustrate Keynes's extraordinary rhetorical skill—his ability to argue whatever side of a question fit his immediate purposes.

4. See, for example, J. B. Rosser, *Logic for Mathematicians* (New York, 1953), esp. pp. 56–57.

5. Robert Merton, in his essay "The Sociology of Knowledge, stressed this point: "The 'Copernican revolution' in this area of inquiry consisted in the hypothesis that not only error or illusion or unauthenticated belief but also the discovery of truth was socially (historically) conditioned. . . . The sociology of knowledge came into being with the signal hypothesis that even truths were to be held socially accountable, were to be related to the historical society in which they emerged." The essay was published in his *Social Theory and Social Structure* (Glencoe, 1957, pp. 456–488; the passage quoted is on pp. 459–460).

6. See Larry Laudan, *Progress and its Problems: Toward a Theory of Scientific Growth* (Berkeley, 1977), p. 201ff.

7. I am trying to deal here with the claim advanced in recent years by a number of scholars that Keynes's basic ideas have been essentially misunderstood—that the "Keynesianism" of people like Hicks and Samuelson was radically different from the "economics of Keynes" himself. These scholars are not merely arguing that Keynes did not share the fiscalist bias of many of his followers—a point which seems fairly well established: see, for example, D. E. Moggridge and Susan Howson, "Keynes on Monetary Policy, 1910–1946," *Oxford Economic Papers*, XXVI:2 (July 1974), 226–247, or Chapter 12 of Don Patinkin's *Keynes' Monetary Thought: A Study of its Development* (Durham, N.C., 1976). Rather, scholars in this school argue against the whole idea of interpreting Keynes's theory in terms of a model, that is, in terms of a set of equations representing the assumptions of the theory. See especially Axel Leijonhufvud, *On Keynesian Economics and the Economics of Keynes* (New York, 1968), p. 9. Such claims have given rise to a certain debate: Leijonhufvud's argument, for example, has been criticized in Herschel Grossman's "Was Keynes a 'Keynesian'? A Review Article," *Journal of Economic Literature*, X (1972), 26–30, and Richard Jackman's "Keynes and Leijonhufvud," *Oxford Economic Papers* XXVI:2 (July 1974), 259–272. For a recent contribution to this literature, see T. W. Hutchison, *Keynes versus the Keynesians?* (London, 1977).

My own view is that Leijonhufvud's thesis, and, for example, the related argument of Sidney Weintraub that "Hicksian Keynesianism" is a travesty of Keynes's essential theory, are in the final analysis unconvincing. [See Weintraub's "Hicksian Keynesianism: Dominance and Decline," in his *Modern Economic Thoughts* (Philadelphia, 1977), and various essays in his *Keynes, Keynesians and Monetarists* (Philadelphia, 1978).] There are two basic reasons for this. First, Leijonhufvud's real concern is with theory, and not with history; as a result, the purely exegetical side of his analysis is somewhat slighted: "it is Keynes' *Gestalt*-conception of how a modern capitalist economy works, and not 'what he really said' that we ultimately want to grasp" (p. 10). This method leaves much room for the exercise of the author's imagination; at times, he seems closer to considering what Keynes should have said than to examining his actual writings. But more important, there is the test of Keynes's reaction to the alleged distortion of his theory by the "Keynesians". If the "reinterpretation" of Keynes à la Leijonhufvud is correct, then Keynes should have objected to the way the "Keynesians," beginning with Hicks, were misinterpreting him. But as I noted in the text, he very explicitly accepted their interpretations. Thus he commented to Hicks about his "Keynes and the 'Classics'" article: "I found it very interesting and really have next to nothing to say by way of criticism," and to Harrod he wrote (about what was to become the "Mr. Keynes and Traditional Theory" article): "I like your paper . . . more than I can say. I have found it instructive and illuminating, and I really have no criticisms. I think that you have re-orientated the argument

beautifully" (Keynes to Hicks, March 31, 1937, CW, XIV, 79, and Keynes to Harrod, August 30, 1936, CW, XIV, 84). Moreover, as noted in the text, Keynes made explicit use of the equilibrium concept: see, for example, GT, p. 249, and CW, XIV, 182.

8. See GT, pp. 5, 17–18. By 1939, however, as a result of criticism by J. G. Dunlop and L. Tarshis, Keynes drew back from this assumption, without, however, abandoning it entirely, and without explicitly replacing it with anything equivalent. See his "Relative Movements of Real Wages and Output," *Economic Journal* XLIX (March 1939), 34–51.

9. Joan Robinson to the author, Oct. 26, 1978; D. E. Moggridge to the author, Dec. 11, 1978.

10. This assumes that the level of aggregation on which Keynes conducted the analysis is taken for granted. Suppose, however, one disaggregated into a two-good economy with separate production functions for consumer goods and capital goods, that is, the output of each is a separate function of both labor and capital. In that case, assumptions about profit maximization, perfect competition and so on would not in themselves allow the labor and capital markets to determine the level of economic activity, even if the labor supply were a function of the real wage. The demand side would thus still be needed to determine equilibrium.

11. GT, p. 269.

12. For example, GT, p. 301.

13. GT, pp. 8, 9 (emphasis added).

14. See GT, pp. 15, 26. Keynes's definition on p. 15 was somewhat imprecise: "Men are involuntarily unemployed if, in the event of a small rise in the price of wage-goods relatively to the money wage, both the aggregate supply of labor willing to work for the current money-wage and the aggregate demand for it at that wage would be greater than the existing volume of employment." The problem with this is that given Keynes's acceptance of the classical postulate about the demand for labor, a "small rise" in p/w—which is another way of saying a small decline in w/p—necessarily increases the level of employment. The question thus should not be what the effect of such a shift would be, but whether it is possible in the first place—for it clearly is not if the supply of labor is a function of the real wage. Nevertheless, Keynes's essential meaning is clear enough from context: he was trying to get at the problem of how labor market equilibrium is affected by a price increase resulting from a shift in demand, i.e., if it goes up, dN/dp is positive and "involuntary unemployment" exists, but if it does not change and DN/dp = 0, then full employment prevails.

15. GT, pp. 91–92.

16. GT, pp. 135–137.

17. GT, pp. 199–202.

18. Keynes to W. H. Beveridge, July 28, 1936, CW, XIV, 57.

19. See especially GT, Book I.

20. CW, XIII, 177, 188–189, 340.

21. GT, pp. 248–249.

22. Defining the consumption function in terms of the wage rate yields similar conclusions, except that in this case there is no need for an assumption about constant returns to scale: a fixed wage and an infinite $L_2'(r)$ suffice to yield the simple multiplier.

23. GT, pp. 172, 207. In these passages, Keynes referred to circumstances "in which even a large increase in the quantity of money may exert a comparatively small influence on the rate of interest," i.e., $dr/dM = 0$. To see that this is equivalent in his system to $L_2'(r) = -\infty$, differentiate Eqs. (4) and (5) with respect to M, and then solve for dr/dM in terms of dy/dM. It follows immediately that if $dr/dM = 0$, then dy/dM and thus also dp/dM are also zero. Then differentiate Eq. (6) with respect to M and again solve for dr/dM. If dy/dM and dp/dM are zero, $dr/dM = 1/L_2'(r)$. So if $dr/dM = 0$, $L_2'(r)$ must be infinite; and since it is negative, $L_2'(r)$ must be $-\infty$.

24. GT, p. 9.

25. It is understood, of course, that these claims are true only in the context of the theory as a whole. For by varying its assumptions, one can easily generate other models where, for example, changes in demand can affect output even if $L_2'(r) = 0$. Suppose we considered monetary policy as defining not simply an arbitrary stock of money, but rather a money supply function relating the

supply of money to the rate of interest [i.e., $M = M(r)$, instead of $M = M$] and that $M'(r)$ was positive. This would correspond to a central bank policy that sought to some degree to restrain the variation in the interest rate that would result from changes in spending, or one which sought to accommodate increased demand that it felt through pressure on the interest rate. Then this assumption, in the context of the rest of the Keynesian theory, would yield exactly the same kind of conclusions about the effectiveness of fiscal policy and so on as Keynes's assumption about liquidity preference.

26. GT, pp. 300–303.

27. GT, pp. 119–120.

28. GT, pp. 120–121, 127.

29. For example, GT, pp. 182, 298–299. This would be justified in a practical sense only if the slope of the aggregate supply function were infinite; then changes in demand would be identical to changes in final output. However, even under the extreme assumption that the labor supply function is $w = w_0$, labor market equilibrium would yield $y'(N) = w_0/p$, and thus $y''(N)\, dN/dp = -w_0/p^2$. The slope of the aggregate supply function would, even in that case, be finite:

$$y_s'(p) = y'(N)\frac{dN}{dp} = \frac{-w_0^2}{p^3 y''(N)}. \qquad (19)$$

30. GT, p. 15. See also note 14 above.

31. GT, pp. 246–247.

32. Strictly speaking, the assumption was that employment was "uniquely correlated with the volume of effective demand measured in wage-units" (GT, p. 260).

33. GT, p. 301. See also GT, p. 249.

34. GT, pp. 173, 207.

35. In this case, the basic equation is $y = c(y) + g$, where g simply represents the exogenously determined component of real spending (whether it comes from government, business or private households is of no importance for this analysis). This gives the multiplier $dy/dg = 1/[1 - c'(y)]$. But if the demand for money did not depend on r and the supply of money was a constant, differentiating Eq. (6) would yield $L_1'(py)(p\, dy/dg + y\, dp/dg) = 0$; so $p\, dy/dg + y\, dp/dg = 0$. Differentiating Eq. (4) yields $dy/dg = y_s'(p)\, dp/dg$; so since $y_s'(p)$ is positive, dy/dg and dp/dg have the same sign. Since p and y are both positive, the only way $p\, dy/dg + y\, dp/dg = 0$ can hold, therefore, is for $dy/dg = dp/dg = 0$. But this contradicts the basic assumption about the consumption function which yielded a multiplier, that is, a value for dy/dg—greater than zero (and in fact greater than one). It follows that a willingness to contemplate a simple system defined by $y = c(y) + g$ thus took for granted (given the other assumptions of the theory) the existence of liquidity preference.

36. Reprinted in Seymour Harris, ed., *The New Economics: Keynes's Influence on Theory and Public Policy* (New York, 1947), p. 189.

37. GT, pp. 372–373. He makes an exception for the case of full employment where, he says, the reverse is true, but we have already seen how the assumption about the nonhomogeneity of the labor supply has ruled out this case.

38. See the Keynes quotation in D. E. Moggridge, "Keynes: The Economist," in Moggridge, ed., *Keynes: Aspects of the Man and his Work* (London, 1974), p. 63.

39. CW, XIII, 469–470, for the 1934 quote. See also Elizabeth Johnson, "John Maynard Keynes: Scientist or Politician?" *Journal of Political Economy*, LXXXII:1 (Jan.–Feb. 1974), 105 (emphasis in original).

40. For the orthodox theory of international trade as it existed at the time, and in particular as it was applied to the analysis of reparation transfer, see esp. F. W. Taussig, "Germany's Reparation Payments," *American Economic Review* (henceforth: AER), X:1, Supplement (March 1920), 33–49; Jacob Viner's remarks in the Round Table discussion of "Economic Problems Involved in the Payment of International Debts," AER, XVI:1, Supplement (March 1926), 91–97; and in

general Lloyd Metzler, "The Theory of International Trade," in H. S. Ellis, ed., *A Survey of Contemporary Economics* (Philadelphia, 1948), p. 213ff.

On the transfer problem, see especially the debate between Keynes and Bertil Ohlin in the *Economic Journal* in 1929. Ohlin accused Keynes of ignoring the role that transfers of purchasing power" played in facilitating the transfer of real wealth. Given the way Keynes's general ideas about what he was to call "effective demand"—much the same thing as "purchasing power"—were developing, it is not surprising that he ultimately came to accept Ohlin's argument. He made the key concession in a letter to Ohlin in January 1931; but characteristically he never conceded the point publicly. See Ohlin, "On the Slow Development of the 'Total Demand' Idea in Economic Theory," *Journal of Economic Literature*, XII:3 (Sept. 1974), 891. For the somewhat sophistical nature of Keynes's argument in the 1929 dispute with Ohlin, see Jacob Viner, *Studies in the Theory of International Trade* (New York, 1937), pp. 307–311.

41. I go into these matters in greater detail in my *Reparation in World Politics* (New York, 1980).

42. *A Revision of the Treaty* (New York, 1922), p. 100.

43. From Viner's review of Mantoux's *The Carthaginian Peace*, in the *Journal of Modern History*, XIX:1 (March 1947), 69.

44. See especially his letter to Woodrow Wilson of January 3, 1918, Woodrow Wilson Papers, Series 5A, Library of Congress, Washington (reel 384 in the microfilmed edition): Germany should get back her colonies, the war should not be continued for the sake of Alsace-Lorraine, etc. Taussig was then head of the U.S. Tariff Commission; his views were quite pro-German by the standards of the time.

45. F. W. Taussig to Viner, March 9, 1936, Viner Papers, Box 61.

46. "National Self-Sufficiency," part 5, *New Statesman and Nation*, July 15, 1933, quoted in E. Johnson, p. 102.

47. Keynes to Hicks, June 1935, in Hicks, "Recollections and Documents," *Economica*, XL (new series, Feb. 1973), 8.

48. Keynes to Harrod, August 27, 1935, CW, XIII, 548. Emphasis in original.

49. J. R. Davis, *The New Economics and the Old Economists*, (Ames, Iowa, 1971), p. 6. There is a good deal of additional evidence that Davis does not cite. Thus the eminent American economist Irving Fisher in a long series of letters to President Roosevelt and in a number of private meetings with him urged an inflationary policy as a way out of the depression. See, for example, Fisher to FDR, March 2, April 30 and July 5, 1933, and Oct. 22, 1934, and Fisher to his wife, April 6 and August 9, 1933, Irving Fisher Papers, Sterling Memorial Library, Yale University. For a more complete account, see William R. Allen, "Irving Fisher, FDR, and the Great Depression," *History of Political Economy*, IX:4 (1977), 560–587.

50. See T. W. Hutchison, *Economics and Economic Policy in Britain 1946–1966* (London, 1968), appendix on "Pigou and Keynes on Employment Policy," pp. 277–301.

51. *Nation and Athenaeum*, May 18, 1929, p. 227, cited in Herbert Stein, *The Fiscal Revolution in America* (Chicago, 1969), p. 482, n. 17.

52. Keynes to Hicks, March 31, 1937, CW, XIV, 79. See also Keith Hancock, "Unemployment and the Economists in the 1920's," *Economica*, XXVII (new series, Nov. 1960), 305–321.

53. On monetary policy, see Susan Howson, *Domestic Monetary Management in Britain, 1919–38* (Cambridge, 1975); for a particular example, see Basil Blackett's memorandum of July 2, 1920, Great Britain, Public Record Office, Class "Cab 24," vol. 108, paper CP 1560. Blackett was the highest permanent official in the Treasury at the time. On fiscal policy, see G. C. Peden, "Keynes, the Treasury, and Unemployment in the Late Nineteen-Thirties," *Oxford Economic Papers*, XXXII:1 (new series, March 1980), 1–18; Howson, pp. 127–128, 143; and especially the testimony in 1930 of the key Treasury official Sir Richard Hopkins (under Keynes's questioning), Great Britain, Parliament, Committee on Finance and Industry, II (London, 1931), 17–21. The Treasury was never "dogmatic"; its opposition to certain schemes was based on practical considerations;

indeed Hopkins admitted that [illegible] spending schemes would increase employment and denied that the Treasury ever meant to argue that [illegible] spending could not possibly do any good. [illegible] much the signal as expression of any views that have come from us." *Ibid.*, p. 201.

54. Keynes, "The Question of High Wages," *Political Quarterly*, I (1930), 110, 124.

55. Keynes to Hicks, March 31, 1937, CW, XIV, 79–81.

56. See his articles in the *Times* (London), Jan. 12, 13, 14, and March 10, 1937, and the discussion in Peden, "Keynes, the Treasury and Unemployment," pp. 1, 11–18.

57. GT, p. 377.

58. GT, p. 378.

59. GT, p. 381.

60. Quoted from her essays "Keynes from his Papers" and "Scientist or Politician?" in Elizabeth and Harry Johnson, *The Shadow of Keynes* (Chicago, 1978), pp. 14–16, 28–29.

61. GT, pp. 31, 104–106, 378–379. The idea that there exists a function relating savings (S) to investment (I) in such a way that as savings increase, investment cannot increase by as much [i.e., $I = I(S)$, $I'(S) < 1$] is in fact basic to the Keynesian approach to income determination. For only in the case that $I'(S)$ is not equal to 1 can setting $I(S) = S$ define a unique solution for S, and thus for income as well; since Keynesian analysis obviously rules out $I'(S) > 1$, this leaves $I'(S) < 1$. This assumption is clearly distinctive of Keynesianism. For if it were assumed that market mechanisms were such that any increase in savings could be absorbed as investment (which presumably was the classical assumption), then $I'(S)$ would equal 1, and $I(S) = S$ would hold for all S if it held for any the I-S equality would not uniquely define the level of income, but would be consistent with any level of income the rest of the system would define.

62. Joseph Schumpeter, "Science and Ideology," AER, XXXIX:2 (March 1949), 355; GT, chap. 23.

63. GT, chap. 19. See also Abba Lerner, "Mr. Keynes' 'General Theory of Employment, Interest and Money,'" *International Labor Review*, XXXIV:4 (Oct. 1936), 438–440. The editor noted at the beginning of this article that Keynes had approved it in advance.

64. See his "Relative Movements of Real Wages and Output," *Economic Journal*, XLIX (March 1939), 35.

65. GT, Chapter 2, and esp. p. 269.

66. CW, XIII, 360.

67. The idea that wages are determined by the balance of power between business and labor, with the latter generally pressing for, and the former in varying degrees resisting, wage hikes, comes out, for example, on p. 301 of the GT: "Since each group of workers will gain, ceteris paribus, by a rise in its own wages, there is naturally for all groups a pressure in this direction, which entrepreneurs will be more ready to meet when they are doing better business."

68. "Mr. Keynes Beats the Band," *New Statesman and Nation*, Feb. 15, 1936, pp. 220–222. See also A. L. Rowse's tract *Mr. Keynes and the Labour Party* (London, 1936), and also Ben Pimlott, *Labour and the Left in the 1930s* (Cambridge, 1977), Chapter 4 and pp. 200–202.

69. Roy Harrod, *The Life of John Maynard Keynes* (New York, 1951), pp. 439, 374.

70. CW, XIV, 148. The article in question is the one cited in n. 63 above.

71. Francis Williams, *Nothing So Strange* (London, 1970), p. 110, quoted in Pimlott, p. 40.

72. Quoted in E. Johnson, "John Maynard Keynes: Scientist or Politician?" p. 24. On Keynes's relations with Labour in general, see the appendix "Keynes and the British Left in the Interwar Period," in Donald Winch, *Economics and Policy: A Historical Study* (New York, 1969), pp. 339–350.

73. Quoted in Robert Heilbroner, *The Worldly Philosophers* (New York, 1953), p. 270.

74. On Keynes's ambivalence about mathematical method, see Don Patinkin, "Keynes and Econometrics: On the Interaction between the Macroeconomic Revolutions of the Interwar Period," *Econometrica* XLIV:6 (November 1976), 1091–1123, section 2.

75. For examples, see GT, pp. 173, 248.

76. Roy Harrod, "Mr Keynes and Traditional Theory," *Econometrica* (Jan. 1937), pp. 74–86; J. E. Meade, "A Simplified Mode of Mr. Keynes' System," *Review of Economic Studies*, IV (1937), 98–107; John Hicks, "Mr Keynes and the 'Classics': A Suggested Interpretation," *Econometrica*, V (Apr. 1937), 147–159.

77. Keynes to Hicks, March 31, 1937, CW, XIV, 79; Keynes to Harrod, Aug. 30, 1936, CW, XIV, 84; Harrod, "Scope and Method of Economics," *Economic Journal* (Sept. 1938), pp. 383–412, esp. section II, p. 397ff; Keynes to Harrod, July 4 and 16, 1938, CW, XIV, 295–301. See also Abba Lerner's rejoinder to Cassel's criticism of Keynes (*International Labour Review*, XXXVI:5 [Nov. 1937], 587); Cassel's use, Lerner wrote, of "the 'general analysis' system of simultaneous equations is one that cloaks the economic system in mystery rather than illustrates how it works." Lerner's rejoinder, the editor again noted, had been approved in advance by Keynes.

78. Consider the divergence revealed by the following exchange between Viner and Nicholas Kaldor, one of Keynes's younger disciples at the time, and later one of the most famous Keynesian economists. Viner had not read a paper Kaldor had sent him. He wrote Kaldor that he could not afford to waste his time with "models" based on "arbitrary postulates," Kaldor replied that to work from models "is really nothing more than to pursue theoretical analysis." Viner to Kaldor, Feb. 7, 1938, and Kaldor to Viner, March 1, 1938, Viner Papers, Mudd Library, Princeton, Box 44. See also Friedrich Hayek, "Scientism and the Study of Society," *Economica*, IX (August 1942), reprinted in his *The Counter-Revolution of Science* (Chicago, 1952).

79. Keynes to Hicks, March 31, 1937, CW, XIV, 80.

80. First published in L. A. Metzler et al., *Income, Employment and Public Policy* (New York, 1948).

81. "A Brief Survey of Post-Keynesian Developments," in Robert Lekachman, ed., *Keynes' General Theory: Reports of Three Decades* (New York, 1964), p. 338.

82. Paul Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass., 1947), p. 276.

83. *Journal of Political Economy*, April 1947, pp. 108–131; reprinted in HR Williams and JD Huffnagle, *Macroeconomic Theory: Selected Readings* (Englewood Cliffs, N.J., 1969).

84. Williams and Huffnagle, pp. 56–57.

85. *Ibid.*, p. 57.

86. In Moggridge, ed., *Keynes: Aspects of the Man and his Work*, p. 101.

87. "The General Theory after Twenty-Five Years," AER, LI:2 (May 1961), 1–17. The quotations are on pp. 14–15.

88. See the examples cited in Milton Friedman, "The Role of Monetary Policy," AER, LVIII:1 (March 1968), pp. 3–4. See also D. E. Moggridge and Susan Howson, "Keynes on Monetary Policy, 1910–1946," *Oxford Economic Papers*, XXVI:2 (July 1974), 226–247, esp. p. 226, n.2.

89. AER, LI:2, 23.

90. This was the title of the key chapter in the first draft of what eventually became the *General Theory*: CW, XIII, 381. See also *ibid.*, pp. 408–411, for an article written by Keynes during this same period which used the same phrase as its title.

91. Herbert Stein, *The Fiscal Revolution in America*, (Chicago, 1969), p. 176.

92. One of Hansen's articles, "Wanted: Ten Million Jobs," was published in the *Atlantic* in September 1943; for a typical warning about inflation, see p. 69 in that issue. Samuelson, however, thought in 1944 that any "reasonably well-informed layman" would "instantly recognize that a serious storm is on the horizon." He was appalled that professional economists were strangely unconcerned with the possibility of a big postwar depression: "The academic economist who only yesterday was preaching the imminence of secular stagnation and the need for government offsets to saving, who scoffed at the naive notion that a little bit of spending would cut the vicious spiral of depression and 'permit' the economic system to carry forward in perpetuity on its own steam—this same individual today is worrying lest purchasing power privately created in the postwar period be greater than even peaks of wartime production can fill" ("Unemployment Ahead," *New Republic*, Sept. 11, 1944, pp. 297–299). The phrase "secular stagnation" clearly was an allusion to Hansen,

and this interpretation is confirmed by Samuelson's reference to this affair in his obituary article on Hansen: "Hansen correctly predicted no end-of-the-war stagnation . . . (Some of his disciples were not so prescient!)" See "Alvin Hansen as a Creative Economic Theorist," *Quarterly Journal of Economics*, XC:1 (February 1976), 31.

93. Arthur Smithies, "Reflections on the Work and Influence of John Maynard Keynes," *Quarterly Journal of Economics*, LXV (1951), 596.

94. Moses Abramowitz, "Economics of Growth," in Bernard Haley, ed., *A Survey of Contemporary Economics*, II (Homewood, Ill., 1952), 146–149.

95. 84th Congress, 1st Session, Joint Committee on the Economic Report, "Federal Tax Policy for Economic Growth and Stability. Papers Submitted by Panelists appearing before the Subcommittee on Tax Policy" (Washington, 1956).

96. For example, see William Fellner, "Hansen on Full-Employment Policies," *Journal of Political Economy*, LV (1947), 255; M. Bronfenbrenner, "Postwar Political Economy: The Presidents' Reports," *Journal of Political Economy*, LVI (1948), 383; Jacob Viner, "Full Employment at Whatever Cost," in his *The Long View and the Short* (Glencoe, Ill, 1958), esp. p. 157, originally published in the *Quarterly Journal of Economics*, LXIV (1950), 385–407; and the survey by George H. Hildebrand, "The Economic Effects of Unionism," in Neil Chamberlain et al., *A Decade of Industrial Relations Research*, 1946–1956 (New York, 1958), esp. pp. 109–115.

97. Abba Lerner, "The Problem of 'Full Employment'—Discussion," AER, *Papers and Proceedings* (May 1946), pp. 330–335.

98. J. M. Clark, "Economic Adjustments after Wars: The Theoretical Issues," AER, *Papers and Proceedings* (1942), p. 9.

99. Richard Goodwin, "The Multiplier," in Seymour Harris, ed. *The New Economics* (New York, 1947), p. 499.

100. Tobin, "Money Wage Rates and Employment," in Harris, ed., *The New Economics*.

101. *Essays in the Theory of Employment* (Oxford, 1947), pp. 1–10. This book was first published in 1937. The argument was a development of a point Keynes himself had made on p. 301 of the *General Theory*. The point was also taken up by conservatives: see, for example, Joseph Schumpeter, "The March into Socialism," AER, *Papers and Proceedings* (May 1950), esp. pp. 453–456.

102. Published in his *Keynes, Keynesians and Monetarists* (Philadelphia, 1978).

103. *Journal of Economic Literature*, XIV:1 (March 1976), 34–53.

104. Heller, "What's Right with Economics?" AER, LXV:1 (March 1975). It is important to note, however, that criticism of the profession in the 1970s had another source as well: the feeling that much of economics was becoming nothing more than an arcane branch of mathematics, devoid of empirical relevance. See esp. Nicholas Kaldor, "The Irrelevance of Equilibrium Economics," *Economic Journal* (Dec. 1972).

105. See Hayek, *New Studies in Philosophy, Politics, Economics and the History of Ideas* (Chicago, 1974), Chaps. 2 and 13. The *Times* article originally appeared in the *Daily Telegraph* in London.

106. Johnson, "Keynes and British Economics," in Milo Keynes, ed., *Essays on John Maynard Keynes* (London and New York, 1975), p. 122.

107. Howard R. Bowen, "Graduate Education in Economics," AER, XLIII:4, part 2 (September 1953), 29.

108. James Tobin, *The New Economics One Decade Older* (Princeton, 1974), p. 57.

109. See for example Alvin Hansen, "Wanted: Ten Million Jobs," *Atlantic Monthly* (September 1943), p. 68, and Paul Samuelson, "Unemployment Ahead," *New Republic*, September 11 and 18, 1944.

110. GT, p. 382.

111. Paul Samuelson, *The Samuelson Sampler* (Glen Ridge, N.J., 1973), pp. 24–25, 217. The first piece originally appeared on the op-ed page of the *New York Times* on October 30, 1970.

112. *New Statesman and Nation*, July 11, 18, August 8 and 15, 1936, pp. 40–41, 82–83, 188, 219, for the original editorial and Keynes's counter-arguments; and September 15, 1936 for the Lerner-Sweezy letter.

113. Abba Lerner, "Economics and the Control of Man," *American Scholar*, XXIX:3 (Summer 1960), 377–385. The quotation is on p. 385.

114. See especially James Tobin, "Defense, Dollars and Doctrines," *Yale Review* XLVII (March 1958), 321–34, republished in his *National Economic Policy* (New Haven, 1966); Alvin Hansen, "The Economics of the Soviet Challenge," *The Economic Record* XXXVI:5 (March 1960), 5–12, republished in Harry Shaffer, ed., *The Soviet Economy* (New York, 1963), pp. 308–316; Arthur Okun, *The Political Economy of Prosperity* (New York, 1970), p. 35. It is interesting to note that similar concerns had an important bearing on the development of Soviet economic thought in this period. The rivalry with America and the international political implications of the declining rate of Soviet economic growth were perhaps the basic factors leading to the revival of economic thought and to the important debates on economic reform which took place in the Soviet Union beginning in the late 1950s and intensifying in the 1960s. See Moshe Lewin, *Political Undercurrents in Soviet Economic Debates* (Princeton, 1974), Chapter 6.

115. Bowen, p. 209.

116. Theodore Caplow and Reece McGee, *The Academic Marketplace* (New York, 1958), p. 211.

117. For a chronology of Hansen's conversion, see Stein, p. 164. The *Quarterly Journal of Economics*, XC:1 (February 1976) contains a number of memorial articles on Hansen. The quotations in the text are drawn from the articles by Samuelson ("Alvin Hansen as a Creative Economic Theorist," pp. 24–29) and Musgrave ("Caring for the Real Problems," p. 4).

118. The quotation is from Logan Wilson, *The Academic Man: A Study in the Sociology of a Profession* (London and New York, 1942), pp. 208–209. See also the quotation from C. H. Cooley in Caplow and McGee, p. 9. Actually there is little empirical evidence to gauge the role played by factors of this sort. Thus a key question concerns the role played by doctrinal sympathy in determining the publication of scholarly articles. The one article I was able to find on economics periodicals does not even raise this "ideological" question: Robert Coe and Irwin Weinstock, "Editorial Policies of Major Economics Journals," *Quarterly Review of Economics and Business*, VII (Winter 1967). The more general study by Diana Crane, "The Gatekeepers of Science: Some Factors affecting the Selection of Articles for Scientific Journals," *American Sociologist*, II (Nov. 1967), is sensitive to the question, but by no means answers it.

119. Knight to Douglas, Jan. 5, 1935, Viner Papers, Box 79.

120. "Memorandum concerning certain appointments to be made in the Department of Economics," Nov. 15, 1940 (Schumpeter is identified as the author in a covering letter by E. D. Smith, chairman of the Yale department), Yale University Provost's Office, Box 2, File "Economics, Dept. of," Yale University Archives, Sterling Memorial Library, Yale University. Samuelson, incidentally, alludes to Schumpeter's anti-Semitism in a lengthy comment on the subject quoted in Leonard Silk, *The Economists* (New York, 1976), pp. 14–15.

121. On the "political" character of the British economics profession during this period, or of its Cambridge branch at any rate, see Harry Johnson's two essays, "Cambridge in the 1950's" and "The Shadow of Keynes" in Elizabeth and Harry Johnson, *The Shadow of Keynes* (Chicago, 1978).

122. See especially Dean Devane's reports, in Yale University, Reports to the President, Yale College (the quotation is from Devane's report for 1945–1946), and Healy's reports in Yale University, Secretary's Office, Annual Reports: Reports of the Economics Department, all in the Yale University Archives, New Haven.

123. Samuelson's Working Paper No. 7 (on the question of a master plan for the humanities), Nov. 28, 1949, John E. Burchard Papers, File "School of Humanities—General," MIT Archives, MIT, Cambridge, Mass.

124. Killian to Luce, Feb. 28, 1949, Burchard Papers, file "Economics Department."

125. *Against the Stream: Critical Essays in Economics* (New York, 1972), p. 6.

126. Wesley Mitchell to J. M. Clark, August 9, 1928, Mitchell Papers, Columbia University Archives, Butler Library, Columbia University, New York.

127. See the various talks in the Mitchell Papers, Series N (1 box), and esp. his Messenger Lectures at Cornell in 1935.

128. See especially Ellis Hawley, *The New Deal and the Problem of Monopoly* (Princeton, 1966).

129. Friedman to Viner, Dec. 14, 1933, Viner Papers, Box 33.

130. "Past Accomplishments and Present Prospects of American Economics," AER, XXVI:1 (March 1936), 1.

131. "The Economist in a World in Transition," AER, XXVII:1 (March 1937), 1–3.

132. "The Short View and the Long in Economic Policy," AER, XXX:1 (March 1940), 1–15.

133. "The Economist and the State," AER, LV:1 (March 1965) 17.

134. "Theoretical Assumptions and Nonobserved Facts," AER, LXI:1 (March 1971), 1.

135. AER, LXV:1 (March 1975), 1–26.

136. Samuelson, "Unemployment Ahead," *New Republic*, Sept. 11, 1944.

137. Lerner, "Economics and the Control of Man," p. 383.

138. See especially Walter Heller, *New Dimensions of Political Economy* (New York, 1967), pp. 1–4, and Arthur Okun, *The Political Economy of Prosperity* (New York, 1970) pp. 1–2.

139. "What's Right with Economics?" AER, LXV:1 (March 1975), 17.

140. "Keynes and British Economics," in Milo Keynes, ed., p. 120. On the late turn to Keynesian policy in the United States, see Winch, *Economics and Policy*, pp. 297–310. It is interesting, moreover, to note today, after Keynesianism has fallen into disarray, how claims about the social utility of the profession have shifted. Now the microeconomic contribution is stressed. See especially Samuelson's comments in a recent *Forbes Magazine* article about the prosperity of the profession. Both he and Otto Eckstein told the author that "stagflation" was an intractable problem, and economics had nothing useful to suggest by way of solution; it was for this reason, Samuelson suggested, that the profession was shifting from macroeconomics to microeconomics, and he emphasized how microeconomic theory is used by business firms in their day-to-day operations. Lawrence Minard, "Cheerful Days in the Dismal Science," *Forbes Magazine*, Jan. 8, 1979, pp. 35–39.

141. United States, Temporary National Economic Committee, *Final Report of the Executive Secretary* (Washington, 1941).

142. Samuelson to Hansen, Jan. 29, 1954, Alvin Hansen Papers, Box 2, Harvard University Archives, Pusey Library, Harvard University, Cambridge, Mass.

143. Byrd L. Jones, "The Role of Keynesians in Wartime Policy and Postwar Planning, 1940–46," AER (May 1972), pp. 125–133; Winch, *Economics and Policy*, esp. pp. 263–264, 274–275; Stephen Bailey, *Congress Makes a Law* (New York, 1964), orig. pub. 1950, pp. 167–171.

144. This is based on an unpublished draft of a history of the National Science Foundation by the official NSF historian, J. Merton England, pp. 19–34. See also Robert P. McCune, "Origins and Development of the National Science Foundation and its Division of Social Sciences, 1945–61," Ed.D. diss., Ball State University, 1971. On the other hand, once the NSF extended its activities to the social sciences, the professions affected (including the economists) did show an interest in using the organization to achieve "enhanced status and increased funding"—see the report of an important meeting between NSF staff people and "representatives of the social science community," held September 12, 1980: SSRC, *Items*, XXXIV:3/4 (December 1980), 58–59. And of course leading representatives of the economics profession complained vociferously when the Reagan administration in early 1981 proposed drastic cuts in NSF funding of the social sciences (see, for example, Leonard Silk, "Budget Cuts and Economics," *New York Times*, April 3, 1981, p. D2).

145. See Robert Calkins's remarks in the "Round Table on the Organization and Financing of Economic Research," AER, *Papers and Proceedings*, XLIX:2 (May 1959), esp. p. 562.

APPENDIX I

In the text, it was claimed that the assumption that an equiproportionate rise in wages and prices increased (and an equiproportionate fall decreased) the supply of labor was equivalent to the assumption that the labor supply function $N = N_s(w, p)$, where $\delta N_s/\delta w > 0$, $\delta N_s/\delta p \leq 0$, was such that

$$\frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} > 0. \tag{20}$$

I want now to outline the proof of this equivalence.

First, assume that

$$\frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} > 0. \tag{21}$$

We need to prove that if there is an equiproportionate rise in w and p, i.e., $\Delta w/w = \Delta p/p$, then $\Delta N > 0$ if the change in w and p is positive, and $\Delta N < 0$ if $\Delta w, \Delta p < 0$.

By assumption, $w\,\delta N_s/\delta w > -p\,\delta N_s/\delta p$; therefore, representing small changes in w and p by the differentials dw and dp, respectively,

$$\frac{(\delta N_s/\delta w)\,dw}{dw/w} > -\frac{(\delta N_s/\delta p)\,dp}{dp/p} \tag{22}$$

For a small equiproportionate rise, $dw/w = dp/p > 0$; so $(\delta N_s/\delta w)\,dw > -(\delta N_s/\delta p)\,dp$. But since

$$dN = \frac{\delta N_s}{\delta w}\,dw + \frac{\delta N_s}{\delta p}\,dp, \tag{23}$$

this implies $dN > 0$; for an equiproportionate fall, the inequality is reversed and $dN < 0$. This conclusion carries over to the case of larger equiproportionate changes in the usual way. The proof would be by contradiction: suppose there were a case in which the proper inequality did not hold. But the change could be divided up into sufficiently small equiproportionate segments so that the differential analysis would hold for each segment. At least one of these segments would have to have the same effect on N as the larger change had, but this contradicts the conclusion derived from the differential analysis.

The first half of the equivalence may therefore be taken as proven. To complete the proof, it is necessary to demonstrate the converse: if equiproportionate changes in w and p shift the supply of labor in the same direction, then

$$\frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} > 0. \tag{24}$$

Since the assumption applies to all equiproportionate changes, it applies in particular to very small ones. For simplicity we take only the positive case; the case of negative changes is exactly analogous:

$$\frac{dw}{w} = \frac{dp}{p} > 0, \qquad dN = \frac{\delta N_s}{\delta w}\,dw + \frac{\delta N_s}{\delta p}\,dp > 0; \tag{25}$$

so

$$\frac{(\delta N_s/\delta w)\,dw}{dw/w} + \frac{(\delta N_s/\delta p)\,dp}{dp/p} > 0,$$

and thus

$$w\frac{\delta N_s}{\delta w} + p\frac{\delta N_s}{\delta p} > 0,$$

or, since $p > 0$,

$$\frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} > 0.$$

which would complete the proof.

APPENDIX II

To derive the multiplier for the full system, simply differentiate Eqs. (4) to (6) with respect to g and then solve for dy/dg.

$$y = y_s(p), \qquad y_s'(p) > 0. \tag{4}$$

$$y = c(y) + \frac{I(r)}{p} + g, \qquad 1 > c'(y) > 0, \qquad I'(r) < 0. \tag{5}$$

$$M = L_1(py) + L_2(r), \qquad L_1'(py) > 0, \qquad L_2'(r) < 0. \tag{6}$$

Therefore,

$$\frac{dy}{dg} = y_s'(p)\frac{dp}{dg}, \tag{4'}$$

$$\frac{dy}{dg} = c'(y)\frac{dy}{dg} + \frac{I'(r)}{p}\frac{dr}{dg} - \frac{I}{p^2}\frac{dp}{dg} + 1, \tag{5'}$$

$$0 = L_1'(py)\left(p\frac{dy}{dg} + y\frac{dp}{dg}\right) + L_2'(r)\frac{dr}{dg}. \tag{6'}$$

By Eq. (4')

$$\frac{dp}{dg} = \frac{1}{y_s'(p)}\frac{dy}{dg};$$

substituting into Eq. (5') and (6') and rearranging terms, we get

$$\frac{dy}{dg} = \frac{1}{[1 - c'(y)] + I/p^2 y_s'(p)}\left(1 + \frac{I'(r)}{p}\frac{dr}{dg}\right) \tag{5''}$$

$$\frac{dr}{dg} = \frac{-L_1'(py)}{L_2'(r)}\left(p + \frac{y}{y_s'(p)}\right)\frac{dy}{dg} \qquad (6'')$$

Substituting dr/dg from Eq. (6″) into Eq. (5″) and rearranging terms yields the multiplier dy/dg, as given by Eq. (7) in the text.

APPENDIX III

I want to show that contrary to what Keynes contended, the basic assumptions of his theory lead to the conclusion that wage cutting (in his sense of a shift in the supply curve of labor) necessarily increases the level of employment. We consider three cases: (a) the simplest Keynesian case of an exogenously determined money wage, (b) the pure Keynesian case where the variables in the consumption function are defined not in real terms but in terms of the money wage, and (c) the general case, where the labor supply function is given by Eq. (3) in the text.

Case (a): An Exogenously Determined Money Wage

In this case, the labor supply function is $w = w_0$. Then for any parameter w_0, $y'(N) = w_0/p$ defines labor market equilibrium, and thus yields the aggregate supply function $y = y_s(p, w_0)$. But a shift in the labor supply function, i.e., a change in the wage rate imposed by organized labor, has no effect on the aggregate demand curve $y = y_d(p)$. The aggregate supply and demand curves taken together define equilibrium y and p for any w_0; they thus define a function $y = y(w_0)$ whose derivative will show the effect of a change in the wage rate on real output:

$$y = y_s(p, w_o), \qquad \frac{dy}{dw_o} = \frac{\delta y_s}{\delta p}\frac{dp}{dw_o} + \frac{\delta y_s}{\delta w_o}; \qquad (27)$$

$$y = y_d(p), \qquad \frac{dy}{dw_o} = y_d'(p)\frac{dp}{dw_o},$$

where $y_d'(p) < 0$, as shown by Eq. (8) above, so

$$\frac{dy}{dw_o} = \frac{\delta y_s}{\delta p}\frac{1}{y_d'(p)}\frac{dy}{dw_o} + \frac{\delta y_s}{\delta w_o}$$

$$\frac{dy}{dw_o} = \frac{\delta y_s/\delta w_o}{1 - [1/y_d'(p)]\,\delta y_s/\delta p} \qquad (28)$$

But

$$y'(N) = \frac{w_o}{p}, \qquad y''(N)\frac{\delta N}{\delta w_o} = \frac{1}{p};$$

so

$$\frac{\delta y_s}{\delta w_o} = y'(N)\frac{\delta N}{\delta w_o} = \frac{w_o}{p}\,\frac{1}{py''(N)},$$

and

$$y''(N)\frac{\delta N}{\delta p} = -\frac{w_o}{p^2};$$

so

$$\frac{\delta y_s}{\delta p} = -\frac{w_o^2}{p^3 y''(N)}.$$

Therefore

$$\frac{\delta y_s}{\delta w_o} < 0 \quad \text{and} \quad \frac{\delta y_s}{\delta p} > 0;$$

so by Eq. (28),

$$\frac{dy}{dw_o} < 0.$$

In other words, if labor increases the nominal wage, output (and therefore employment) will necessarily fall, whereas a cut in money wages increases the level of employment (see Figure 3).

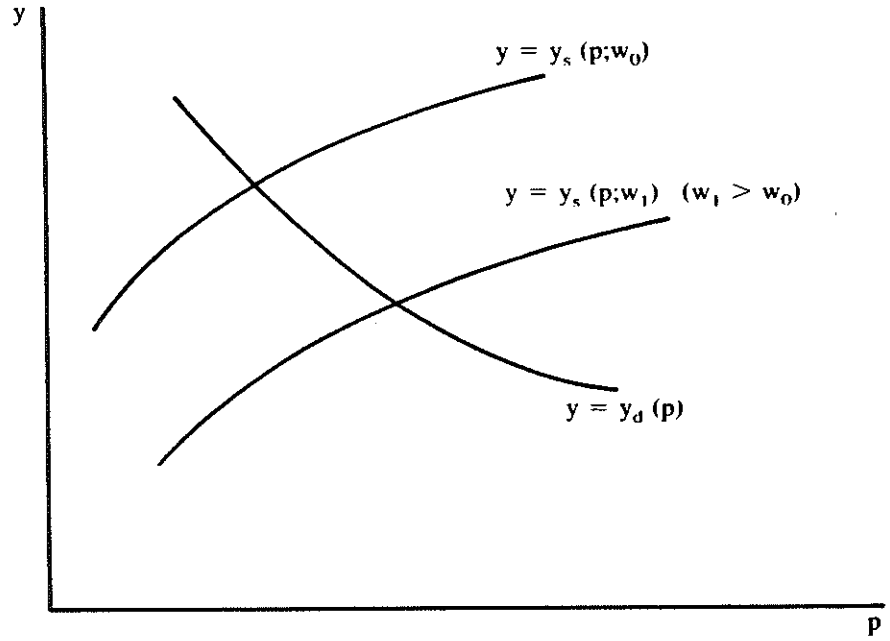

Figure 3.

Case (b): The Wage-Based Consumption Function

Some people might object that the foregoing argument distorts Keynes's original analysis, which used the money wage as the unit of account. But it can easily be shown that this makes no difference in the conclusion. Here we have two

equations which together define for any money wage w the level of output measured in terms of the wage rate (Y_w), which is taken as determining the level of employment:

$$Y_w \equiv \frac{Y}{w}, \qquad C_w \equiv \frac{C}{w},$$

$$Y_w = C_w(Y_w) + \frac{I(r)}{w}, \qquad 1 > C_w'(Y_w) > 0, \qquad I'(r) < 0;$$

$$M = L_1(wY_w) + L_2(r), \qquad L_1'(wY_w) > 0, \qquad L_2'(r) < 0.$$

Differentiating these two equations with respect to w and solving for dY_w/dw yields

$$\frac{dY_w}{dw} = \frac{-(Y_w/w)\, I'(r) L_1'(Y)/L_2'(r) - I/w^2}{[1 - C_w'(Y_w)] + I'(r) L_1'(Y)/L_2'(r)}$$

This by inspection turns out to be negative. Thus an increase in the money wage decreases the level of employment, even on the basis of Keynes's own assumptions (see Figure 4).

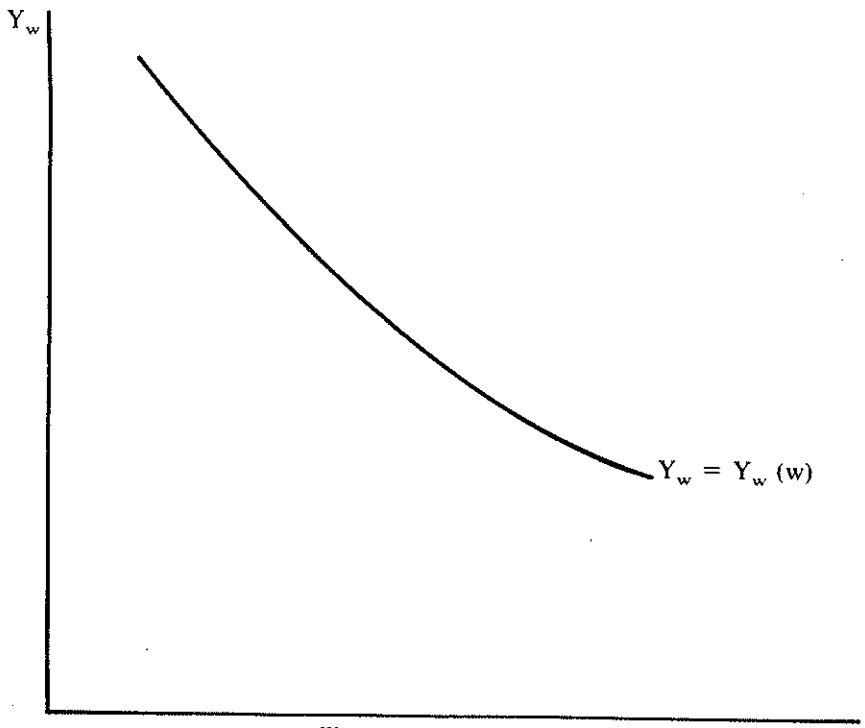

Figure 4.

Case (c) The General Case

The basic conclusion about the effect on output and employment of a shift in the supply of labor function holds also for the general case defined in Eq. (3):

$$N = N_s(w,p), \qquad \frac{\delta N_s}{\delta w} > 0, \qquad \frac{\delta N_s}{\delta p} \leq 0$$

and

$$\frac{w}{p}\frac{\delta N_s}{\delta w} + \frac{\delta N_s}{\delta p} > 0. \tag{3}$$

For this general case, the simple notion of wage cutting does not apply. But its obvious analogue is an upward shift in the labor supply function, i.e., an increase in the amount of labor forthcoming at any given combination of wages and prices. This will also be called an *easing* of the supply of labor, and its reverse will be called a *tightening*. What we have to prove is that an easing expands and a tightening shrinks both output and employment. Or more precisely, given two functions for the supply of labor, both satisfying the conditions in Eq. (3), $N = N_s(w, p)$ and $N = N_s^*(w, p)$, if $N_s^*(w, p) > N_s(w, p)$ for all ordered pairs (w, p)—or, to use the usual shorthand, $N_s^* > N_s$—then a shift from N_s to N_s^* will be called an *easing* of the labor supply, and the reverse will be called a *tightening*.

First it will be shown that an easing of the labor supply—the analogue of wage cutting in the simpler model—raises the corresponding labor equilibrium function. If $N_s^* > N_s$, then $N_s^*(p) > N(p)$ for all p, where $N = N_s(py'(N), p)$ defines $N = N(p)$ and $N = N_s^*(py'(N), p)$ defines $N = N^*(p)$.

The proof is by contradiction. Suppose there exists a $\bar{p}$ such that $N_s^*(\bar{p}y'(\hat{N}), \bar{p}) \leq N_s(\bar{p}y'(\bar{N}), \bar{p})$, where $\hat{N}$ satisfies (i.e., solve) the equation $\hat{N} = N_s^*(\bar{p}y'(\hat{N}), \bar{p})$, and $\bar{N}$ similarly is such that $\bar{N} = N_s(\bar{p}y'(\bar{N}), \bar{p})$. In other words, suppose for some $\bar{p}$, $N^*(\bar{p}) = \hat{N} \leq N(\bar{p}) = \bar{N}$. Then because of diminishing marginal returns, $y'(\hat{N}) \leq y'(\bar{N})$. But therefore $N_s^*(\bar{p}y'(\hat{N}), \bar{p}) \geq N_s^*(\bar{p}y'(\bar{N}), \bar{p}) > N_s(\bar{p}y'(\bar{N}), \bar{p})$, because $\delta N_s/\delta w > 0$ and $N_s^* > N_s$. This, however, contradicts the initial assumption. Therefore, if $N_s^*(w, p) > N_s(w, p)$ for all (w, p), then $N^*(p) > N(p)$ for all p.

It follows immediately that the corresponding aggregate supply curve also rises. Let $y_s^*(p) \equiv y(N^*(p))$, $y_s(p) \equiv y(N(p))$; if for any p, $N^*(p) > N(p)$, then because the y(N) function is upward sloping, $y(N^*(p)) > y(N(p))$; so $y_s^*(p) > y_s(p)$ for all p. But the aggregate demand function slopes downward [see Eq. (8)]. It is therefore obvious from Figure 5 that an easing of the supply of labor entails an upward shift in output, and therefore also in employment, and a fall in the price level: Or more formally, let $y_s^*(p) = y_d(p)$ define (as its solution) the equilibrium point (y_e^*, p_e^*). Similarly, $y_s(p) = y_d(p)$ defines its equilibrium

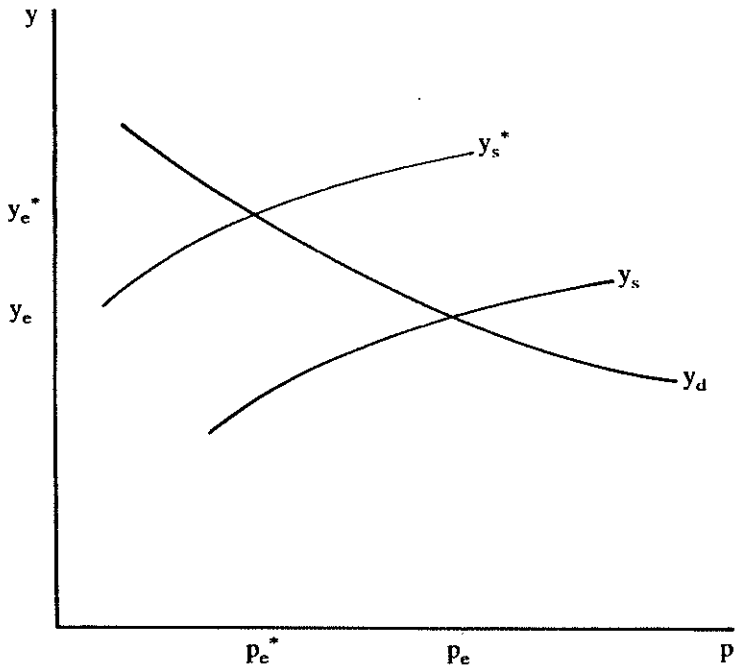

Figure 5.

point (y_e, p_e). The proof is again by contradiction. Suppose $y_e^* \leq y_e$. Then because $y_d'(p) < 0$ for all p, $p_e^* \geq p_e$. Because $y_s^{*\prime}(p) > 0$, $y_e^* = y_s^*(p_e^*) \geq y_s^*(p_e)$. But $y_s^*(p_e) > y_s(p_e) = y_e$, since $y_s^* > y_s$. Thus this yields $y_e^* > y_e$, contradicting the assumption. Hence $y_e^* > y_e$; since y'(N) is positive, the level of employment also expands as the labor supply eases. Since everything is symmetrical, the reverse is also true of a tightening of the labor supply: when the negotiating attitude of labor tightens, i.e., when the unions become tougher bargainers, or whatever, levels of output and employment decline.

APPENDIX IV

In the text it was claimed that the version of the Keynesian system presented in Klein's article, "Theories of Effective Demand and Employment," led to the conclusion that the time path for employment was fully determined by the supply side equations, and thus was not—contrary to the essence of the Keynesian theory—influenced by any changes on the demand side. The model consists of the following equations:

$$(2.4) \qquad \frac{dy}{d\bar{N}} = \frac{w}{p}$$

$$(2.9) \qquad \bar{N} = \text{labor supply (where } \bar{N} \text{ is a constant)}$$

$$(2.10) \qquad \frac{d\,(w/p)}{dt} = g(\bar{N} - N)$$

Klein also assumes the stability conditions $g(0) = 0$ and $g'(\bar{N} - N) < 0$, i.e., that the system moves toward equilibrium, and that once there it says there, but that the movement toward equilibrium is damped.

Equation (2.4) can be rewritten as $N = N(w/p)$. That this has negative slope can be seen by differentiating Eq. (2.4) with respect to the real wage:

$$y''(N)\frac{dN}{d\,(w/p)} = 1;$$

so

$$\frac{dN}{d\,(w/p)} = \frac{1}{y''(N)}$$

which is negative since y''(N) is negative. Substituting in Eq. (2.10) yields

$$(2.10') \qquad \frac{d\,(w/p)}{dt} = g\,(\bar{N} - N\,(w/p))$$

This is a separable, first-order differential equation, and can be solved in the usual way. Define

$$f\left(\frac{w}{p}\right) = \frac{1}{g\,(\bar{N} - N\,(w/p))}$$

This function is continuous for the domain where $\bar{N} > N(w/p)$, i.e., for all $w/p > (w/p)^*$, where $(w/p)^*$ is the real wage that equilibrates supply and demand, $\bar{N} = N((w/p)^*)$. This is the domain we are interested in since whenever demand is equal to or exceeds supply [$w/p \leq (w/p)^*$], employment will be determined by the supply ceiling $\bar{N}$ and will not move anyway.

Thus $dt = f(w/p)\,d(w/p)$; so

$$t = \int f(w/p)d(w/p), \text{ and } t = t\,(w/p) + c,$$

where c is the constant of integration. If we can observe the real wage at some initial time t_0, then c is determined and the solution can be defined exactly for the domain where $w/p > (w/p)^*$. It is clear from the stability assumption that the functions $g(\bar{N} - N(w/p))$ and thus also $f(w/p)$ are negative in this domain. Therefore, $dt/d(w/p) = f(w/p)$ is also negative for this domain. Thus the function $t = t(w/p)$ is monotonically decreasing. It thus can be rewritten in the form $w/p = h(t)$, where it is sufficient to limit the domain for t to all $t > t_0$, the initial point

of observation. Thus the time path for the real wage is uniquely determined; the time paths for N and y can be inferred in the usual way from the production function and Eq. (2.4); and none of these time paths are the least bit dependent on anything on the demand side.