ILLUSTRATIONS OF DEGREES OF VALIDITY IN ECONOMICS

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In this paper I will try to show that well-known economic assertions and attitudes exhibit relative degrees of validity which can be satisfactorily distinguished on the basis of the adequacy of the methodology and the confirming evidence which underlies them. Degrees of validity, and the underlying problem of the sources of validity, will be illustrated here through a discussion of Irving Fisher's equation of exchange and quantity theory of money as developed in his *Purchasing Power of Money*, Fisher's 100% money proposal as developed primarily in his *100% Money* but also in his *Booms and Depressions*, and Alvin H. Hansen's *Fiscal Policy and Business Cycles*. The *Purchasing Power of Money* will be treated here as an example of "pure science," while the other works mentioned will be treated as policy proposals.

These books have been selected for illustrative analysis because of the widespread public attention they have received, and also because they represent disagreement and change in one broad area of economic thought. The fact that they are not at the leading edge of current discussion is an advantage for my purposes, since it permits a calmer consideration of methodological issues. I will try to show that criteria of methodological adequacy reveal that Fisher's equation of exchange is more valid than his quantity theory of money, and that Hansen's cyclical proposal, his mature economy proposal, and Fisher's 100% money proposal rank as to validity in the order named. I will not attempt here to compare the validity of the examples of pure science with the validity of the proposals.

I should emphasize that this is a study in methodology, and that no general evaluation is intended of the quantity theory of money, the equation of exchange, or the proposals as such. I will confine my comments (except for occasional supporting references) to the four books mentioned above, treating them only to the extent which seems to me to be necessary to establish a relative degree of validity. This paper is a by-product, which seemed to me to possess general interest, of an effort to characterize economic methodology as it is actually exhibited in the works of leading economists.

ASSUMPTIONS AND METHODS OF THIS STUDY

In making this study, I have assumed, in the first place, that scientific validity is at best a matter of degree, except in the case of a purely deductive assertion

which claims relevance to nothing but the proposition from which it is inferred.[1] I think there are very few who would disagree with this today, but of course it has not always been the prevailing view. A second assumption is that the ultimate test of truth is a methodological one.[2] On this, also, I think there is wide agreement. Further, it is assumed that any example of scientific inquiry exhibits two basic methodological procedures which are ordinarily used in close association with each other. I will use the term *empirical* to refer to the inductive or fact-finding approach, and the term *logical* for the deductive or inferential approach.[3]

In order to judge the validity of an assertion, it is first necessary to identify the type of methodology on which it rests. With respect to assertions which are empirical in character, two types will be recognized here. These will be called *personal generalizations* and *survey-based assertions*. Personal generalizations are assertions made on the basis of facts and items of information accumulated in a non-systematic manner during the ordinary course of life. While an explicit systematic accumulation of information is not present, personal generalizations do involve reflection—they are organized and clarified through memory and intellectual manipulation. A very large number of assertions in the field of economics are based on the method of personal generalization. Indeed, until recently, perhaps a majority of economic thought represented empirical assertions of this type combined with the logical deductions made from them.[4] Personal generalization is not necessarily to be disdained as a method of inquiry. It may underlie assertions whose validity is as high as can be attained. Survey-based assertions, on the other hand, are the outcome of specific attempts to marshal and take account of relevant facts in an explicit fashion. There is direct, systematic recourse to the economy for the purpose of collecting, as far as possible, the facts and circumstances which bear on a particular problem or situation.

The degree of validity of each of these two types of empirical assertions may be judged in terms of the success with which the method of inquiry has been employed in supporting the assertion. In the pages which follow, empirical assertions of the two basic types which possess a high degree of validity will be

[1] See Morris R. Cohen and Ernest Nagel, *An Introduction to Logic and Scientific Method*, (New York: Harcourt, Brace and Co., 1934), Book II.

[2] See Frank H. Knight, *Freedom and Reform*, (New York and London: Harper & Bros., 1947), p. 218.

[3] John Neville Keynes points out that economics ought to be an "unprejudiced combination" of both approaches. See his *Scope and Method of Political Economy* (London: Macmillan & Co., 1891), p. 164. Keynes names other methodological approaches (pp. 28–30), but in a formal sense they all reduce to two. Professor Knight has said that, in terms of abstract methodology, the social sciences are not significantly different from the physical sciences in representing a combination of the inductive and deductive methods (*op. cit.*, p. 243). Felix Kaufmann, in his *Methodology of the Social Sciences*, (New York: Oxford University Press, 1944) deals with these two basic methods and these two alone throughout his analysis.

[4] For an excellent classical example of this type of methodology see Adam Smith, *The Wealth of Nations* (New York: Modern Library, 1937), Book I, Chap. VII and Book II, Chap. I.

called *rigorous survey-based assertions* or *safe personal generalizations*. A rigorous survey-based assertion is supported by explicitly compiled statistical or other factual data, and the nature of the data is such that it does in fact support the assertion. The last condition is the difficult one. It is easy to accumulate statistical data, but only rarely is the data adequate for the purpose at hand, so that it really confirms the proposition in question to the point of near certainty. In the case of a safe personal generalization, while the data have not been accumulated systematically, it is still adequate to make the validity of the assertion nearly certain. Assertions of these two types which possess a lesser degree of validity will be identified by the term *plausible*, and those of the lowest degree of validity, but still not entirely unwarranted, by the term *tentative*. All other empirical assertions will be classified as unwarranted empirical assertions.[5] Additional degrees of validity beyond these four could readily be provided, but this increases the uncertainty in making the classification, and is unnecessary for the purposes at hand. The classification of an assertion on the basis of the evidence offered is partially a matter of judgment. I will try to demonstrate, however, that such judgment is reasonably objective in character —sufficiently so that when a conclusion is drawn as to all the important assertions represented in a doctrine, the final judgment as to the relative validity of the whole doctrine is an acceptable one.

Logical adequacy does not appear to be a problem in economics nearly to the extent that empirical adequacy is.[6] Pure logical deduction, in the cases which I have examined so far, is generally simple and straightforward in character, and can be easily judged to be correct or incorrect, almost always the former. Consequently, I will deal here only with *strict logical inferences*—those which appear to be established as far as the inference itself is concerned.

There are, however, instances in which logical support is erroneously given for a proposition when actually the issue is an empirical one. The concept "logical" appears to be loosely used in economics as in common speech, and examples of this error are not completely absent in economic inquiry. Two special forms of this are the deduction of one assertion from another, the latter of which has not been established adequately on an empirical basis, and, often more subtle and difficult to detect, deduction from a definition or model which does not really correspond with the situation in the actual world which it is intended to characterize. In both of these cases the conclusion should be assigned the same degree of empirical validity as its premise.

There are certain limitations in this attempt to determine the relative validity of a doctrine by classifying its central assertions according to the mode of analysis which I have just outlined. In the first place, in this brief illustration

[5] I use the term "unwarranted" because the fact that an investigation has not established a doctrine as valid does not mean that it is invalid.

[6] The increasing number of investigations which are mathematical in character constitute an exception to this statement. While logical considerations predominate in much of mathematical economics as long as the inquiry is purely formal, empirical validity becomes paramount as soon as the conclusions are used for proposals or predictions.

I have had to ignore the extensive literature which precedes and lies behind the work of Fisher and Hansen. It so happens, however, that their books which are considered here can be dealt with, for the most part, in terms of the evidence directly presented in them (one exception in the case of Hansen's cyclical proposal will be noted). I have accepted some assertions not established in the book in question, but they involve situations so obviously confirmed or agreed upon, that I don't think this alters my conclusions. In the second place, the type of analysis presented here is useful only to the extent that economics is considered to be a science. In terms of the intentions of both Fisher and Hansen, I think this perfectly appropriate. In some instances, for example the attitude of J. S. Mill and Henry George toward the "unearned increment," an attempt to determine the degree of validity on the basis of rules of scientific procedure would perhaps be inappropriate. Doctrines such as these need to be judged to some extent as ideals. The question of the methodological basis of ideals and their role in economics is a very important one which does not arise in this paper.[7] There is also the closely related question of the "interest" which an assertion may arouse regardless of the question of its truth or validity. It is clear that interest (which often involves an element of hope) is at least as important a basis for the adoption of an economic proposal as is validity in a scientific sense. This, also, will not be considered here. Finally, as will become clear, very high standards of scientific validity are employed in this paper—neither Fisher nor Hansen have attained a consistently high degree of validity in terms of the standards which I have used. I recognize, of course, that such standards of "scientific" attainment may be beyond economics, at least in its present state. It is impossible to establish a relative degree of validity, however, except by grading down from the strictest criteria possible.

[7] Whether economics should deal explicitly with ideals is, of course, a controversial issue. I think it fair to say that the majority of contemporary economists who have not explicitly concerned themselves with methodology feel that economics should consider itself a science and that this implies the exclusion of end or value formulation. Among those who have written directly about methodology, Lionel Robbins lies at one extreme, taking an attitude of strict neutrality towards ends. See his *Essay on the Nature and Significance of Economic Science* (2d ed.; London: Macmillan & Co., 1935), esp. Chap. II. J. N. Keynes prefers to think of economics as a positive science, although he discusses it as a normative science and as an art (*op. cit.*, Chap. II). Knight feels that as a science economics cannot be concerned with ideals or ends (*op. cit.*, pp. 41–43, 218), but he also thinks that the work of the economist will always be limited if he confines himself to a purely scientific inquiry. See *The Ethics of Competition* (2d ed.; London: George Allen & Unwin, 1936), esp. pp. 85, 87, 91–93, 129–130. (Rutledge Vining has recently discussed Professor Knight's position in this respect in detail. See his article, "Methodological Issues in Quantitative Economics," *American Economic Review*, XL (1950), p. 267.) C. E. Ayres is among the few contemporaries who feel that economics is necessarily concerned with values whether explicitly or implicitly. See *The Theory of Economic Progress* (Chapel Hill: University of North Carolina Press, 1944), esp. Chap. IV. It is my own opinion that ideals and ends are very important in economics and should be explicitly considered, but this will not be developed here.

The equation of exchange is a highly valid doctrine whose validity rests on strict logical inference from a set of definitions. It can be established by safe personal generalization, however, that the definitions refer to situations which are widespread and important in the actual world.

The basic proposition which underlies the equation is that in an act of exchange the amount of money spent equals the money value of the goods received (spending equals receiving). While Professor Fisher has not formally set up the defining terms of an act of exchange which make this assertion true, it is present as an implicit model from which his reasoning proceeds. Some of the minimum elements in such a definition would be: (1) There is only one spender of money in the act of exchange or, if more than one, they can all be treated as one group necessarily involved in the exchange and having to do only with the exchange. (2) There is only one receiver of goods or, if more than one, there is a group understandable in the same way as the spenders. (3) The individuals or groups designated under (1) and (2) are identical. (4) Money is spent for the goods in question and for no other goods, and all the money involved in the exchange is expended for this purpose. Given conditions such as these, it is possible to take the step of defining the money value of the goods as being equivalent to the money spent on them in an act of exchange.[8]

The further breakdown of receiving and spending into four components is also accomplished by means of definition. Thus, receiving is analyzed as the product of p and q on the basis of a definition of price, namely, that price results from dividing the amount of money paid by the number of units involved in the transaction.[9] Similarly, spending is broken down by defining M and V so as to make their product equal to it.[10] Finally, it is asserted that PT equals MV holds on an aggregate basis. If we understand this assertion as a sum of microscopic elements, and not as a new direct macroscopic approach, it is completely valid on the basis of the necessary arithmetic and algebra. Strictly speaking, any

[8] The illustrations given by Professor Fisher, and the fact that he offers no statistical confirmation for the equation as initially established, show that he proceeded in this logical fashion.

[9] See *The Purchasing Power of Money* (2d ed.; New York: Macmillan Co., 1931), p. 3. I have here reduced a general statement of Fisher's to specific terms involving money. It is also necessary to reflect that the meaning of the concept "unit" in this context establishes that every act of exchange will always involve some number of units.

[10] In the interest of simplicity, M' and V' will be ignored in this portion of the discussion. In this case, Fisher initially thinks of spending as involving one unit of time rather than one act of exchange. This doesn't affect the validity of the assertion as here analyzed, since the unit of time could be so small as to involve only one act of exchange, and no matter how many acts of exchange it covered it would still be valid that spending equals receiving. Fisher makes it completely clear that the validity of the equation of exchange rests, not on national aggregates, but on the analysis of an individual act of exchange. See *ibid.*, pp. 358ff. and 362ff., and also Chap. II, esp. pp. 16, 26, and 28. In his appendix he tries to find magnitudes to correspond to the equation as an aggregate, but in this he is assuming the validity of the equation rather than testing it.

direct national aggregate computation of the equation of exchange could not derive its validity from the same source as Fisher's equation, except on the assumption that these national figures were sums (or averages) of the concepts of the microscopic equation exactly as defined. Such a correspondence would, of course, be extremely difficult to establish.

Although inference from definition gives the equation of exchange its ultimate validity, it can be established by safe personal generalization that acts of exchange as defined, including the definitions involved in the breakdown of the two sides of the equation, are important and widespread in our economy. Again, this type of evidence is to some extent implicit in Fisher's analysis,[11] but it is the ultimate basis of the empirical validity of the equation.[12] It should be noted that this methodology, while it yields a high degree of validity, is inherently qualitative in character. It is clear that acts of exchange as defined are widespread and important, but it cannot be told how widespread and important—that is, what portion of the economy is covered and what portion is not. This would have to be determined by a special survey-type investigation. This question is very relevant to many economic doctrines—it determines the importance of a doctrine, which is a different question than validity. We also know by personal generalization that in some cases we would have to reorganize our usual understanding of the world in order to make it correspond with the definitions of the equation of exchange.[13] For example, when a government subsidy is included in a transaction, when there is installment buying and interest payments, or when tips or gratuities are included in exchange, spending does not equal receiving in any simple or direct sense, although we can reorganize our usual way of analyzing these situations so as to make them do so. Finally, while a logical approach may yield a high degree of conceptual clarity, this is often attained at the expense of empirical applicability. This is shown, in Fisher's equation, by the many difficulties which an individual would face in calculating his own M and V. Spending, on the other hand, from which M and V are derived, is a relatively easy empirical concept, and the one whose existence is confirmed by safe personal generalization.

It should be noted that the validity-basis of the equation of exchange does not warrant one important use which Fisher made of it. He expressed the attitude

[11] For examples, see *ibid.*, pp. 16, 28.

[12] Assertions based on safe personal generalization are often called self-evident. This phrase is misleading and dangerous and in my opinion should not be used at all since it gives a quality of incontestable truth to an assertion where no such quality exists. There is always some methodological basis for a so-called self-evident assertion. Lionel Robbins (*op. cit.*, Chap. IV) describes much the same thing to which I am referring by the phrase "safe personal generalization" when he examines the basis for the validity of certain postulates of economic theory. Later, however (p. 117), he says that the truth of deduction from elementary facts of experience is "independent of further inductive test." Here he is unjustifiably sweeping over many problems which are not cleared up by his discussion elsewhere.

[13] Arthur W. Marget has conducted a full discussion of the question of the importance of the definitions in the equation in relation to the problem of what it covers. See his *Theory of Prices* (New York: Prentice-Hall, 1938), Vol. I, esp. pp. 47, 49–67.

that in M, V, and T he had found the "only three sets of causes" of P.[14] Elsewhere, he held that these three are "the only influences which can directly affect the level of prices."[15] It is evident that there are innumerable ways in which the relationship, receiving equals spending, could be logically analyzed, and that other relationships involving P could be set up than the equation of exchange. Any one such analysis would have as strong a claim to setting forth the causes of P (or the factors which directly affect P) as any other. The discovery of the factors which cause changes in P could be accomplished only on the basis of empirical investigation of an entirely different character than that used by Fisher in developing his equation of exchange.

FISHER'S QUANTITY THEORY OF MONEY

Unlike the equation of exchange, Professor Fisher's quantity theory of money possesses a low degree of validity when judged by the criterion of methodological adequacy. Two basic methodological faults in the investigation as a whole should first be noted. In the first place, Fisher has not clearly stated what hypothesis he is testing. In the main, his formulation of the quantity theory is to the effect that prices change normally in direct proportion to changes in money (M).[16] This attitude is exhibited consistently through Chapter VIII, which is a direct consideration of the quantity theory, and the concluding sentence in the chapter is a reaffirmation of it. In the first part of his book, however, he always qualifies it with the proviso that other factors in the equation do not also change[17] and, in the concluding sections, he appears to think of the quantity theory as almost identical with the equation of exchange.[18] In the second place, his investigation consists of an examination of each of the five so-called "causes"—M, V, M', V', and T—in order to determine which affect the price level. Since the assertion that these are the five causes of P is not highly valid, such validity as may attach to the whole investigation is necessarily limited.[19]

In his investigation of the causes of P, Fisher attempts to show that M is causal and P resultant, and that M', V, V', and T are relatively passive factors. His main investigation is empirical in character, conducted independently of the logic of the equation of exchange, as it must be. The most fundamental part of it, however, is conducted solely on the basis of personal generalization, which does not appear to yield adequate results in this instance. Fisher relies on this

[14] *The Purchasing Power of Money*, p. 14.
[15] *Ibid.*, pp. 74, 150.
[16] *Ibid.*, pp. 156–157.
[17] *Ibid.*, pp. 19–20, for example.
[18] For example, *ibid.*, pp. 292–296. See also Marget, *op. cit.* p. 20.
[19] Fisher does investigate numerous "indirect causes" in Chaps. V and VI. On this subject see Marget's discussion, *op. cit.*, pp. 81–85. This investigation of Fisher's is relegated to a minor position, however, and does not govern his conclusions. I think that his undue emphasis upon a limited number of "direct" causes partially prevented him from giving more serious consideration to the possibility of long-run institutional changes in the economy of the type covered by the "indirect causes."

type of evidence to the extent of remarking that it establishes the quantity theory on an "a priori" basis.[20] For example, in showing that M' holds a constant and definite relation to M, and that, consequently, changes in the former will not reduce or nullify the effect of the latter on P, Fisher asserts that bank reserves are kept in a definite ratio to bank deposits, and that individuals, firms, and corporations preserve more or less definite ratios between their money and deposit balances.[21] While the banking law of his day offers some support for his assertion, Fisher's principal argument takes the form of a review of certain customs, beliefs, and behavior patterns. This can be no more than tentative personal generalization, since there are too many possible exceptions, also known by personal generalization, and a survey on the basis of personal generalization affords too limited a sample. It is inappropriate to generalize as to the habits of tens of millions of individuals and millions of firms on the basis of non-systematic personal experience alone. This is not an attempt to affirm the widespread existence, in qualitative terms, of a simple situation such as an act of exchange, nor is there any deduction from simple and relatively constant institutional factors in the economy. Reflection, against a background of personal experience, does not yield any element which establishes the long-run stability, in precise quantitative terms and without significant exception, of the economic behavior patterns which Fisher is here investigating. One cannot, in this manner, reason "a priori" from personal knowledge of human beings, since so many entirely different behavior patterns are perfectly compatible with human nature as the stimuli are varied even within the existing institutional framework of society. One can only find out, by explicit survey, how people are in fact behaving, and even then, of course, one has no assurance that they will continue to behave in this fashion through an extended period of time.

The analysis of the relationships between V and V' and M makes use of further personal generalization of the type just described, and also of logical inference from the constant relation between M and M', which, as we have seen, does not possess a high degree of validity.[22] In the case of T, Fisher tries to show it is not affected by the quantity of money, and consequently cannot change so as to interfere with the directly proportional relation between M and P.[23] This investigation is characterized by a too limited selection of the possible factors which affect T and, again, it relies primarily on personal generalization.

Finally, in showing that P is the one "absolutely passive" element in the equation of exchange,[24] Professor Fisher relies on logical inference from the

[20] See *The Purchasing Power of Money*, p. 276, for one example.

[21] This discussion is conducted on pp. 49–53, 162–164 of Fisher's book.

[22] See *ibid.*, pp. 79–88, 152–153. Marget expresses the opinion that a dependent relationship between any two variables of the equation of exchange has not been established. See *op. cit.*, pp. 24–25, 27.

[23] See *The Purchasing Power of Money*, pp. 74–79, 155.

[24] *Ibid.*, pp. 169–172. Lionel D. Edie feels that the long controversy over which elements in the equation of exchange are causal has never settled the question and probably never will. See his *Money, Bank Credit, and Prices* (New York: Harper & Bros., 1928), pp. 198–199.

concept of a gold standard economy in order to show that P will not change M. Since a gold standard existed in his day, this inference possesses some validity. He then asserts, however, that M', V', V and T will not change if P does, since he has already shown that these hold a constant relation to M. The entire argument for the quantity theory of money, then, when formulated in strictly proportional terms, rests on tentative personal generalization.

He does consistently qualify the quantity theory with the proviso that it does not hold during "transitional periods."[25] While the "trade cycle" is analyzed in some detail in Chapter IV, where Fisher lays the basis of a theory which he developed more fully in the 1930's, the full implications of his qualification in terms of the interpretation of the quantity theory to which it leads are never precisely formulated. More basically, however, if we cannot accept the evidence for the short-run existence and stability of those institutions, customs, and habits discussed above, we are even less justified in accepting the same evidence in the long run.

Certain direct survey-type evidence is presented in Chapters XI and XII and in the appendices which accompany them. As has been mentioned, the formulation of the quantity theory shifts markedly in this section, and we cannot be sure what the hypothesis is.[26] Chapter XI, dealing with the earlier periods, shows with some validity that over a long historical period M appears to have had an important effect on P, with certain short-run exceptions, particularly in the nineteenth century. Fisher agrees that the evidence is inadequate and this does not, of course, establish an exactly proportionate relationship between M and P. The conclusion to Chapter XII, strictly speaking, actually denies the quantity theory of money as formulated earlier in the book.[27] In final summary, Fisher simply asserts that M is perhaps the most important factor in the equation which controls P.[28]

FISHER'S 100% MONEY

100% Money will be dealt with here as though it were concerned only with the problem of booms and depressions. Considered in this light, its logical organization consists of (1) determining the causes of booms and depressions, (2) outlining the proposal, and (3) showing that the remedy will overcome the defect.

The explanation of booms and depressions is made in terms of "over-indebtedness" and "deflation," or the "debt disease" and the "dollar disease."[29] The assertion that a condition of over-indebtedness periodically appears is based on a considerable volume of survey-type evidence, which is limited in

[25] See Edie, *op. cit.*, pp. 195–198, and Morris A. Copeland, "Money, Trade, and Prices—A Test of Causal Primacy," *Quarterly Journal of Economics*, XLIII (1928–29), p. 649. Both of these writers feel that Fisher has not adequately dealt with transitional periods.

[26] Cf. *The Purchasing Power of Money*, pp. 234, 274–275, 276, 277, 280, 292–296, 298.

[27] *Ibid.*, pp. 304–305.

[28] *Ibid.*, pp. 307ff.

[29] See *100% Money* (3rd ed.; New Haven: City Printing Co., 1945), pp. 120–121.

scope and which is presented but largely unanalyzed.[30] The figures themselves (with the conspicuous exception of brokers' loans) do not show that debt increases in the 1920's were necessarily out of line with the expansion in other economic magnitudes, a necessary condition of over-indebtedness as defined, and Fisher does not refer to other studies in support of his assertion. Consequently, as a survey-based assertion, this should probably be classed as tentative. It is supported by personal generalization to the effect that over-indebtedness would naturally be caused by new opportunities to invest at a big profit compared with ordinary profits and interest.[31] While this would help strengthen the assertion that indebtedness expands in boom times, it does not establish the relative concept of over-indebtedness, which is defined by Fisher in such a way as to make its existence provable only by measuring it in comparison with other magnitudes.

The assertion that from over-indebtedness, followed by alarm and liquidation, it is possible to "deduce" that a depression will ensue, has no meaning and cannot be taken seriously.[32] The empirical evidence offered for this contention consists of nothing beyond a few historical generalizations, and it is unwarranted if considered strictly in the light of Fisher's own methodology.

With respect to the dollar disease, which is really central in Fisher's business cycle analysis,[33] the assertion that real debts increase in time of depression is a rigorous survey-based assertion, but no substantial evidence is offered in support of the contention that this is the central factor in depressions. This also must be considered an unwarranted assertion in terms of Fisher's methodology. Again personal generalization type evidence is offered, but it does not seem to me to strengthen the assertion, although it would be at least plausible if this factor were merely considered as one among many elements in depressions. Finally, it should be noted that Fisher has summarily dismissed, without warrant, other possible explanations of the business cycle.[34]

In establishing that commercial banks are the institutional causes of the debt disease and the dollar disease, Fisher shows through manipulation of a model that commercial banks can create and destroy money to the extent of ten times their primary deposits (based on 10% reserves). The model does not adequately represent commercial banks, which is the crucial issue, and therefore conclusions pertaining to the real behavior of commercial banks cannot be inferred from it with a satisfactory degree of validity.[35] Even if the models did show that

[30] This appears in *Booms and Depressions* (New York: Adelphi Co., 1932), Chap. VII and Appendix III.

[31] *100% Money*, pp. 130ff.; *Booms and Depressions*, Chap. IV.

[32] *100% Money*, p. 122. It is impossible to tell whether Fisher really intended it to be.

[33] See *ibid.*, p. 125. Here the dollar disease is said to be "the chief secret of most, if not all, great depressions."

[34] See *ibid.*, p. 120.

[35] For example, Professor Walter E. Spahr in *The Fallacies of Professor Irving Fisher's 100% Money Proposal* (New York: Farrar and Rinehart, 1938) showed, according to a standard technique, that banks could more likely lend about $1.10 rather than $9.00 for each dollar of primary deposits received. (See pp. 20ff.)

banks are capable of performing the action in question, it would have to be established by a special survey that they in fact do so. Certain survey type figures are presented in *Booms and Depressions*,[36] but these are not used by Fisher to show the extent to which banks do cause over-indebtedness.[37] With respect to the contraction of M', although chief emphasis is still placed on the models, Fisher does present figures which show the extent of bank credit contraction during the downswing, and he also develops the explanatory concept of a "contest for cash."[38] While the personal generalization which establishes the existence of the latter has at least a tentative degree of validity, the further assertion that it is this factor which causes the debt disease in unwarranted. The figures on bank credit contraction in themselves show nothing apart from the "contest for cash" explanation since the mere fact that M' contracts doesn't establish that banks are responsible for it.[39] This is necessary in relation to a proposal directed solely toward banks, since, if other factors are causing the contraction of M', these other forces may assert themselves in some way with deflationary effects, even if the banks are forced to hold M' constant.

With respect to the cure itself, the model analysis intended to show a relatively constant M' under a 100% system is free of the principal defects in the 10% model, and the doctrine probably represents logical inference from at least plausible personal generalization as to the characteristics of commercial banks.[40] Fisher has not shown, however, that the increased constancy of M' would mean a less fluctuating price level. It is widely recognized, of course, that the concept of velocity is of great importance in this connection,[41] and Fisher merely continues to maintain the attitude of the *Purchasing Power of Money* that velocity holds a constant relation to money. He mentions velocity control,[42] but he makes nothing of this. He also proposes that the Currency Commission expand and contract M as necessary, chiefly by buying bonds, but he doesn't show that this would alleviate booms and depressions, except in terms of deduction from the analysis in the *Purchasing Power of Money*.[43] The abstract

[36] See particularly Appendix II and scattered through Chaps. VII and VIII.

[37] Frank D. Graham has remarked that the degree to which banks create the money which they lend is undetermined, but there can be no question about the fact. See his article, "Partial Reserve Money and the 100 Per Cent Proposal," *American Economic Review*, XXVI (1936), p. 431. This matter of degree is of central importance in relation to the concept of over-indebtedness.

[38] *100% Money*, pp. 77ff. and also pp. 5-6.

[39] See Spahr, *op. cit.*, p. 26.

[40] M' might change as before due to the activities of the Currency Commission, but Fisher quite legitimately hopes that it will have the skill and integrity to avoid this.

[41] See Albert G. Hart, "The Chicago Plan of Banking Reform," *Review of Economic Studies*, II (1935), esp. p. 107 and James W. Angell, "The 100 Per Cent Reserve Plan," *Quarterly Journal of Economics*, L (1935-1936), esp. pp. 16 and 28.

[42] *100% Money*, pp. 101-102; *Booms and Depressions*, p. 140.

[43] Actual spending by the Currency Commission is treated by Fisher as a distant possibility. This, which figures more prominently in recent discussions, is entirely different than the 100% money proposal. The prevailing attitude seems to be that the 100% money plan may be useful, but in terms of much more limited objectives than were originally

phases of the economy on which Fisher has chosen to base his cure are unduly limited in scope as compared to the total forces at work. He has not shown that what he has discussed is of controlling importance in relation to all the forces which he has omitted.

HANSEN'S CYCLICAL PROPOSAL

Professor Hansen's cyclical proposal will be dealt with here, for purposes of illustration, in the narrowed form that bond-financed federal government expenditures be used to combat low income levels in time of depression.[44] Four main subsidiary assertions appear to me to be necessary to support this proposal: (1) The government can obtain money to spend by selling bonds without withdrawing directly from the private expenditure stream, (2) the government can dispose of the funds so procured in such a way as to augment the national income, (3) the large-scale sale of bonds by the government in time of depression is feasible, (4) these government operations can be conducted so as not indirectly to reduce private spending to such an extent as to off-set the increased government spending.

The first of these assertions can be divided into two parts, the first of which is that the government can obtain funds by selling bonds to commercial banks without encroaching upon the private expenditure stream. This is a rigorous survey-based assertion resting on historical data, and it is also supported by deduction from the institutional characteristics and environment of commercial banks.[45] The second part of the assertion relates to the possibility of borrowing savings. Hansen presents data showing the large magnitude of savings, but he is interested in this largely in relation to his mature economy proposal, and he has not shown that the government could absorb savings in depressed times without encroaching upon private investment needs. It is a rigorous survey-based assertion that the savings stream is large in prosperous times, and that investment in depressed periods does not take place at the boom-time rate. This does not establish, of course, that such savings as are made when the national income is low are available to the government for purposes of expanding national expenditures. Historical studies of consumer behavior, such as are presented in *Fiscal Policy and Business Cycles*, make it appear plausible that government expenditures on a large enough scale in depressed periods would create additional savings, assuming that such expenditures raised the national income. This does not really strengthen the assertion, however, since investment might be stimu-

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claimed for it. See James W. Angell, *op. cit.*, pp. 16, 28; Frank D. Graham, *op. cit.*, pp. 438–440; and Benjamin Higgins, "Comments on 100% Money," *American Economic Review*, XXXI (1941), pp. 91–96.

[44] His book, of course, presents a rounded program of cyclical proposals. The most conspicuous omissions here are his tax and boom-time proposals, and his attitudes toward monetary controls and price flexibility.

[45] See *Fiscal Policy and Business Cycles* (New York: W. W. Norton & Co., 1941), pp. 175–188 and Chap. VI. Although he presents certain data, Hansen does not argue directly in support of this proposition, which can be taken as well-established in the literature.

lated and, in such a case, savings would still not be available as a surplus to the government and, also, it cannot be shown that such savings would be comparable in magnitude to the government expenditures.[46]

Hansen does not discuss directly the second assertion mentioned above, that the government can spend money in such a way as to raise the national income. He takes this for granted and is concerned with the more complicated question of how such expenditures can be made most effectively. It appears to be a safe personal generalization, however, confirmed by historical survey, that the government can establish contacts which will result in its expenditures increasing the income of individuals in the economy (leaving aside for the moment the problem raised by the fourth assertion listed above). The concept of the national income is such that, if individual incomes are raised, so will the national income be.

The assertion that the large-scale sale of bonds by the government in time of depression is feasible cannot be assigned a degree of validity as such. It is not a question of possibility, of "yes" or "no," but of desirability in relation to a given magnitude of debt. The government can, if it wants to, force banks to buy bonds in quantities which would be large relative to an actual need it may have for selling bonds. The real question is one of the effects of the operation, in whatever magnitude is necessary, upon the existing institutional arrangements of society, and whether such effects are desirable or undesirable in relation to the potential gains.[47]

Professor Hansen's direct discussion of this question is largely concerned with the fear of what might be called federal "insolvency" and the fear of inflation, both of which fears are concerned with serious disturbances to existing institutional arrangements which might arise from the federal fiscal operations which he contemplates. With respect to the first of these, he has shown with a high degree of validity that the government has more latitude in issuing bonds than does an individual firm operating on the ordinary, traditional precepts of private finance. The federal government faces the entire economy, and it is the only single institution which constitutes a significant portion of the economy in itself; it does not have to depend upon voluntary markets for its receipts; its "solvency" is a matter of national concern, directly related to the interests of all or most of the individuals in the economy; in considering its capacity to pay interest, unlike a private firm, it may take into consideration the effect of its own expenditures upon the national income and taxable capacity. These are all safe personal generalizations, supported, at some points, by survey data.

[46] The discussion of these matters occurs in many places in *Fiscal Policy and Business Cycles*. See particularly pp. 228–247, 327–332, 342–344, 430–434, and the statistical data in Chap. IV and on pp. 384–388.

[47] It is in areas such as these, it seems to me, that questions of value, in the philosophical sense, inevitably arise. The ideal is often expressed that economics should take ends as given, showing possible "economic" outcomes of a proposal with respect to a particular end, or else showing the extent to which a given action would realize various ends. Whether such an ideal is desirable or not, I doubt that most economists actually adhere to it. Judgments as between ends, whether explicit or implicit, almost invariably creep into discussions such as the one having to do with the federal debt.

They represent an empirical analysis of the institutional circumstances which the government faces in its financial operations, as compared with the circumstances which a private firm faces. With respect to principal repayments, Hansen has shown with considerable validity, on the basis of historical study and an analysis of current savings habits and investment practices, that in some respects a large supply of federal bonds is desirable and that it may be economically undesirable to retire bonds under certain circumstances. The principle that any debt must be repaid is not the cardinal precept of federal debt management; the government should consider rather the general economic effects of the debt and of a program for repaying it. With respect to the problem of inflation, he has shown with perhaps a plausible degree of validity, that, as a general principle, federal spending at low levels of employment will not have a serious inflationary effect if the government conducts itself wisely, although his analysis is confined largely to the immediate effects at low levels of employment, rather than to possible long-run effects.

This type of discussion, however, is very general in character. I think it is fair to say that, in his book under consideration, Hansen left a specific, concrete discussion of the debt question largely to others.[48] In terms of a strict conception of validity, Hansen's analysis merely makes it appear plausible that there are margins of safety with respect to federal debt incurrence. Consequently, the more minimal the terms in which one frames the proposal, the more workable it seems to be. While there is a great deal of data bearing on the question in his book, Hansen has not made any computation (except in the most general terms) as to the possible magnitude of the debt which might have to be created in depressions of varying degrees of seriousness, and he has not analyzed with any precision the long-range consequences as this remedy is successively applied in depression after depression. In order to make the choices and consequences involved in the proposal more clear (in a "scientific" fashion), it would be necessary to delineate, on the basis of an analysis of the existing institutional arrangements of society, the types of strains which would appear under various assumptions as to a rise in the public debt, and the types of controls or institutional changes necessary to deal with such strains, or which might result from such strains. While such an analysis would be complex, it is not entirely out of the question. Hansen has indicated many of the factors which would have to be considered in such an analysis, but he has not made the analysis in precise, systematic terms. It was the fear of certain institutional changes, and not the problem of validity as such, which underlay the controversy over some aspects of Hansen's work, such as the "we owe it to ourselves argument" (improperly attributed to Hansen if taken in an extreme form), and the assertion that "borrowing is always to be preferred to taxation" because it is more expansionist.[49] Finally, it should be noted that Hansen's investigation of alternative

[48] For a summary of some of the work which has been done on this subject, see Seymour E. Harris, *The National Debt and the New Economics* (New York: McGraw-Hill Book Company, 1947), pp. 173–177.

[49] *Ibid.*, p. 179, I have not given the qualifications which appear in the rest of the passage. In connection with these particularly controversial matters see Harold G. Moulton, *The*

means of financing government expenditures is not nearly as thorough or well-founded as his discussion of the increased debt method.[50] The assertion that the increased debt method is the best method is no more than a tentative assertion in his analysis, if considered in the light of strict rules of scientific investigation.

The final assertion which needs to be considered is the one that these government operations can be conducted so as not to reduce private spending indirectly to such an extent as to off-set the increased government spending. In the first place, against the argument that such indirect contractions off-set the federal deficit spending programs of the 1930's, Hansen has shown, with a high degree of validity, that it is not necessary to draw on this hypothesis to account for the worst of the problems of that period of depression.[51] This, of course, does not establish a positive validity for the assertion in question. The problem can be approached more positively on the basis of the magnitude of the private expenditures which might be expected to contract, as compared with total private expenditures. Hansen has not undertaken a specific analysis of this question, but that the possible contraction is a relatively small proportion of total private expenditures is shown with some validity by the figures in Chapter IV. Even if one assumes that the entire deficiency in investment and consumption in 1936 and 1937 as compared with a normal growth from the 1920's was due to the government spending program alone, it is evident that the portion of the economy which expenditures would have to override in offsetting this tendency is a relatively small proportion of the total. Finally, Hansen asserts, with respect to investment expenditures, that these fluctuate in accordance with powerful factors which are more fundamental than a psychological reaction to a rising public debt, and that they will not be adversely affected to any great extent by government fiscal operations. In terms of Hansen's own analysis, this assertion, representing a very complicated piece of institutional generalization, is hardly more than unwarranted, but of course he rests his case on an extensive literature dealing with innovation and investment which preceded his own work, and to which he frequently refers.[52] It is impossible in this paper to consider the validity of this whole other literature, and I will have to treat it arbitrarily as though it were not a part of Hansen's proposal. If one accepts this assertion it of course strengthens Hansen's analysis at this point. No attempt is made in *Fiscal Policy and Business Cycles* to deal with whatever indirect effects government deficit-financing may have on consumption expenditures. We are left then with the same problem of institutional frictions noted previously. It is clear that government depression expenditures could be large enough to override any

New Philosophy of the Public Debt (Washington: The Brooklings Institution, 1943); David McCord Wright, "Moulton's *The New Philosophy of the Public Debt*," *American Economic Review*, XXXIV (1943), p. 573; and Hansen's "Moulton's *The New Philosophy of the Public Debt*" in Alvin H. Hansen and Harvey S. Perloff, *State and Local Finance in the National Economy* (New York: W. W. Norton & Co., 1944), p. 285.

[50] See *Fiscal Policy and Business Cycles*, pp. 175–179.

[51] *Ibid.*, Chap. IV, and pp. 25–27.

[52] *Fiscal Policy and Business Cycles* of course makes a contribution to this theory, but not with respect to the problem whether private expenditures might be discouraged because of federal deficit financing.

indirect curtailment in private expenditures which they might cause; whether this would be worth the price in terms of the institutional adjustments necessary is not clear.

HANSEN'S MATURE ECONOMY PROPOSAL

A very brief discussion of Professor Hansen's mature economy proposal will suffice to indicate its relative degree of validity as compared with the other two. In the first place, the proposal requires establishment of the existence of the problem. This has not been mentioned in the case of the other two proposals, because the proposition that there are wide fluctuations in the national income is obviously a rigorous survey-based assertion. Much excellent statistical data, such as that exhibited by Hansen, could be offered in the 1930's to confirm the mature economy thesis, but it was not such as to probe very deeply into the complicated problems of institutional change involved, and, in addition, the new trend had only just appeared. Also, the great depression can be adequately explained on the basis of other hypotheses than the mature economy one, as Professor Hansen has partially done in the first part of Chapter I of his book. The assertion that the statistical data available in the late 1930's revealed fundamental and permanent structural changes in American society of the mature economy type should not, it seems to me, be classed as more than a tentative hypothesis.[53] The implications of a comparison of extensive and intensive expansion do not seem to me to strengthen the argument greatly,[54] since no conclusive evidence can be offered which makes possible a long-range prediction as to the future of technological development.[55] Hansen agrees that the survey type evidence which he offers with respect to this problem is unreliable.[56] I do not mean to imply in these remarks that the mature economy thesis was or is now irrelevant; I mean merely to say that it was a tentative hypothesis which could only be confirmed over a period of time.[57]

The proposal, of course, is the same as the cyclical proposal, but it involves greater debt magnitudes on a more continuing basis. It cannot be established by pointing out general margins of safety, which are more reassuring in the case of a short-run consideration of the cyclical proposal. The absence of a more concrete analysis of institutional resistances in relation to increases in the debt of various

[53] Evsey D. Domar has suggested that the settlement of the mature economy thesis depends on additional empirical verification, which can be forthcoming only over a period of time. He points out that most of those who have written on this subject have neither given nor referred to adequate empirical evidence. See his article, "The Problem of Capital Accumulation," *American Economic Review*, XXXVIII (1948), esp. p. 788.

[54] See *Fiscal Policy and Business Cycles*, pp. 364–365 for one of many instances of Professor Hansen's discussion of this problem.

[55] See George Terborgh, *The Bogey of Economic Maturity* (Chicago: Machinery and Allied Products Institute, 1945), Chap. VI and pp. 194ff., and Howard R. Smith, "The Status of Stagnation Theory" (Part I), *Southern Economic Journal*, XV (1948), esp. p. 200.

[56] *Fiscal Policy and Business Cycles*, Chap. XVII.

[57] Benjamin Higgins has recently summarized current attitudes as to the continuing importance of this doctrine. See "The Concept of Secular Stagnation," *American Economic Review*, XL (1950), p. 160.

magnitudes weakens the mature economy proposal much more than it does the cyclical proposal.[58] A continuing long-run increase in the public debt, in a situation in which the private sector of the economy is less buoyant than before, is much more apt to become unworkable in relation to the attitudes and existing institutional arrangements of society than is the case with the cyclical proposal.

SUMMARY AND CONCLUSIONS

I have tried to show that Fisher's equation of exchange has a high degree of validity, although the methodology which underlies it is such that care must be used in applying it to the actual world. His quantity theory of money is at best tentative in the strictly proportional form in which he most frequently puts it. Of the proposals, Hansen's cyclical proposal is evidently the strongest. Since Professor Hansen's proposal is basically an offset rather than a cure, it does not depend on any one analysis of the causes of economic fluctuations. Undoubtedly his particular analysis of economic fluctuations was partially responsible for his proposing an offset, but this is not relevant to the question of the validity of his proposal. Two of the key assertions in this proposal have a high degree of validity, namely, that the government can obtain funds in substantial quantities in time of depression by selling bonds to commercial banks, and that the government can spend this money so as to raise the national income. That this can be done in some unspecified limited fashion without substantial danger of federal "insolvency" or inflation is plausible in terms of Hansen's analysis. Finally, it can be tentatively asserted that the necessity of overriding such indirect private contractions in expenditures as the federal operation might cause would not add to the magnitude of the deficit financing to such an extent as to increase markedly the dangers of the operation. Beyond this, the long-run consequences of the proposal in terms of the institutional rearrangements which it would require are not clear. Taken in an indefinite short-run sense, however, Hansen's cyclical proposal does rest upon assertions which are valid to some degree, when considered in the light of strict rules of scientific investigation. Fisher's 100% money proposal, on the other hand, is all but unwarranted in terms of the validity given it by his own methodology, and if regarded as the basic cure for depressions. It rests on a particular analysis of the business cycle, which he has not established, and it would only work if this analysis is valid. In addition, he has not shown that the 100% money plan would eliminate or substantially reduce the price level fluctuations which he regards as central.[59] Hansen's mature economy proposal possesses an inter-

[58] It is difficult to tell from the book under consideration how serious Hansen considers the mature economy problem to be, and what the scope of his long-range proposal is. Cf. *Fiscal Policy and Business Cycles*, pp. 117, 173, 194–195, 307, 365, 409. This problem in Professor Hansen's work was clearly brought out in the controversy between Moulton and Wright in 1944. See the *American Economic Review*, XXXIV (1944), pp. 116–121.

[59] There are many who still feel that 100% money would help to alleviate cyclical fluctuations. Even this milder proposal doesn't seem to me to be more than tentative in terms of Fisher's own methodology, although it could perhaps be better established by a different type of study. Fisher claimed that 100% money would substantially eliminate depressions.

mediate degree of validity as compared with the other two. There is statistical evidence for the existence of the problem, but it is not conclusive. The price which would have to be paid in order to make the program work is not clear—the general margins of safety which exist in connection with the cyclical proposal are not convincing when considered in relation to a long period of continually increasing debt. These conclusions, which I have tried to base solely on formal methodological considerations rather than hindsight, have, in general, been borne out by the history of the doctrines in question.

While I have not explicitly attempted in this paper to evaluate the methodological practices of economists, some concluding observations are perhaps permissable, recognizing that they are based only on the limited examination of economic doctrine which has just been conducted. The difficulties which stand in the way of a completely successful use of the scientific method in economics have often been noted: the abstractions which must be used are hard to define with precision and they are intricately interrelated, the method of laboratory experiment is largely inapplicable, economists can hardly avoid becoming involved in the pressures and problems of the day, and there are many other factors which could be mentioned. At the same time, every assertion does have some methodological basis, and it is entirely possible that economists have given too little explicit attention to what this basis is in particular cases. If the character of the evidence which supports an assertion or a whole doctrine were always made clear and explicit, it would keep constantly in view the problem of the nature and feasibility of the task of verification. In some instances, when an ideal or a social value supports an assertion, the process of empirical verification is largely inapplicable. In other cases, empirical verification may be possible, either easily, or with considerable, perhaps even unsurmountable, difficulty or expense. If the character of the investigation or debate necessary to confirm a doctrine could be made clear when differences of opinion exist, it would help greatly in determining just what the argument is about, and what the chances are of settling it. The attainment of such an ideal is very difficult in the realm of new investigation, but it seems to me there is room for improvement, assuming that the aim of economics is to attain scientific objectivity, rather than to arouse interest.

It would perhaps be helpful if unrelated doctrines and proposals could be disentangled in economic analysis, especially when they are laid before the public. In the case of Hansen's work, for example, his theory of explanation of the business cycle is, for the most part, not necessary to support his cyclical proposal. The proposal had to be given a thorough public discussion; the theory of explanation was less pressing and of less immediate importance to public action. If the two had been disassociated a more clear-cut discussion of the proposal might have taken place. In the case of Professor Fisher's proposal, on the other hand, the theory of explanation is indispensable and cannot be separately treated. In this instance discussion, whether professional or public, had to deal with both.

If there are discernible degrees of validity among the proposals and doctrines

of economists, some attempt could perhaps be made to reserve the more doubtful matters largely for professional discussion. This takes place automatically when doubtful matters are so labelled. The simultaneous advancing of the mature economy problem and proposals and the cyclical proposal by Keynes, Hansen, and others, unquestionably confused the discussion of the cyclical proposal. In addition, the stronger proposal was often received with the skepticism aroused by the weaker. The action proposed in the 1930's was substantially the same under either proposal, whether the economy was stagnant or merely depressed. The injection of the stagnation problem merely confused the argument, since it was not a problem which had to be settled by the public at the time. Of course the mature economy thesis was of public interest, but I think it would have been better to separate it from the cyclical problem, and to describe it as somewhat more tenuous. Other sciences, like medicine, partially as a result, I think, of adherence to stricter methodological rules, seem to have learned better than economics the desirability of debating uncertain issues as far as possible at the professional level.

Finally, however difficult and, in some cases, impossible is the accumulation of adequate empirical information in economics, I believe that the discipline would be strengthened rather than weakened by clear and explicit recognition that empirical evidence is inadequate when this is actually the case. Felix Kaufmann, in his excellent work, the *Methodology of the Social Sciences*, previously referred to, has analyzed and stressed the importance of the rules which should govern the incorporation of a new assertion into a scientific discipline and the abandonment of an old one.[60] Such rules, informal and to some extent implicit in actual practice, are perhaps not sufficiently rigorous in economics. A particular aspect of this is the evident failure to make a greater use of hypotheses.[61] There must be some level of validity below which a doctrine should be plainly labelled hypothetical. If the analysis in this paper is correct, Irving Fisher should not have been so persuasive and rhetorical in *100% Money*, treating the proposal almost as though it were certain knowledge; he should rather have taken a tentative and hypothetical tone.

[60] See especially Chaps. IV and XVII.

[61] See Wesley C. Mitchell's excellent brief statement on this question, "Facts and Values in Economics," the *Journal of Philosophy*, XLI (1944), p. 212.