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READINGS
IN
ECONOMIC ANALYSIS

A Series Edited by

RICHARD V. CLEMENCE

Wellesley College

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VOLUME ONE

General Theory

VOLUME TWO

Prices and Production

READINGS
IN
ECONOMIC ANALYSIS

VOLUME ONE
General Theory

Edited by

RICHARD V. CLEMENCE

Wellesley College

1950

ADDISON-WESLEY PRESS INC.

CAMBRIDGE 42, MASSACHUSETTS

Printed in U.S.A.

CONTENTS

PAGE

PREFACE	vii
INTRODUCTION	ix
ACKNOWLEDGEMENTS	xi
CHAPTER	
1 Scope and Method of Economics by R. F. HARROD, <i>Economic Journal</i> , September 1938, pp. 383-412	1
2 Implicit Theorizing: A Methodological Criticism of the Neo-Cambridge School by W. LEONTIEF, <i>Quarterly Journal of Economics</i> , February 1937, pp. 337-351	31
3 A Reconsideration of the Cobweb Theorem by N. S. BUCHANAN, <i>Journal of Political Economy</i> , February 1939, pp. 67-81	46
4 A Reformulation of Certain Aspects of Welfare Economics by A. BERGSON (A. Burk), <i>Quarterly Journal of Economics</i> , February 1938, pp. 310-334	61
5 Capital, Interest, and Diminishing Returns by F. W. TAUSSIG, <i>Quarterly Journal of Economics</i> , May 1908, pp. 333-363	86
6 Increasing Returns and Economic Progress by A. A. YOUNG, <i>Economic Journal</i> , December 1928, pp. 527-542	117
7 The Instability of Capitalism by J. A. SCHUMPETER, <i>Economic Journal</i> , September 1928, pp. 361-386	133
8 On a Certain Ambiguity in the Conception of Stationary Equilibrium by L. ROBBINS, <i>Economic Journal</i> , June 1930, pp. 194-214	159
9 Dynamics, Statics, and the Stationary State by P. A. SAMUELSON, <i>Review of Economic Statistics</i> , February 1943, pp. 58-68	180
10 Some Economic Consequences of a Declining Population by J. M. KEYNES, <i>Eugenics Review</i> , April 1937, pp. 13-17	192
11 An Exposition of Keynesian Economics by L. TARSHIS, <i>American Eco- nomic Review, Supplement</i> , May 1948, pp. 261-272	197
12 An Appraisal of Keynesian Economics by J. H. WILLIAMS, <i>American Economic Review, Supplement</i> , May 1948, pp. 273-290	209
13 Review of Keynes's <i>General Theory</i> by J. A. SCHUMPETER, <i>Journal of the American Statistical Association</i> , December 1936, pp. 791-795	227
14 Stability and Full Employment by N. KALDOR, <i>Economic Journal</i> , De- cember 1938, pp. 642-657	232
15 Unemployment in the Theories of Schumpeter and Keynes by E. G. BENNION, <i>American Economic Review</i> , June 1943, pp. 336-347	248
16 Theories of Effective Demand and Employment by L. R. KLEIN, <i>Journal of Political Economy</i> , April 1947, pp. 108-131	260

PREFACE

This series of volumes has as its object the collection of important papers in the field of economics in such a form and at such cost as to make them readily accessible to students.

All papers will, as a rule, be included complete, and will be reproduced by photography exactly as they first appeared in print. Scholars who cannot obtain the originals may thus be assured that no changes have been made other than those specifically noted. With the needs of such scholars in mind, I have merely added new page numbers without removing those used in the original publications.

R. V. C.

January 1950

INTRODUCTION

The papers in this volume have been collected primarily for the use of undergraduates in courses in economic theory. All the articles have been thoroughly tested in my own classes, and all are suitable for undergraduate instruction. I have tried to include treatments of the chief topics usually covered in general theory courses, and to make sure that different points of view are represented. The student is thus given an opportunity, not only to detect errors in economic literature, but also to distinguish between errors and valid differences in outlook.

Although my choices have been made strictly with a view to the capacities of undergraduates, the essays in this volume are well worth the attention of advanced students. I believe that other professional economists will, like myself, be able to reread the papers with profit, and graduate students will certainly find ample material to engage their interest. Most of the papers deal with questions that should be pursued beyond the boundaries of an undergraduate course, and at the higher levels of instruction the volume is adequate to provide much of the desired reading.

The order of arrangement of the essays is only one of a number of logical possibilities. There is nothing sacred about it, and it is merely the order in which the papers are introduced in my own course. The development of an argument may, however, be detected in much of the present sequence.

My major contribution to the usefulness of this book has been to keep all my own comments out of it. This has naturally been difficult, but I have had to recognize that anything I might add would diminish the value of the volume for the purpose it is primarily intended to serve. It seems clear that the more penetrating my comments might be, the more harm they would do, and that nothing worth saying could advantageously be said. I suspect that the absence of my remarks will be regretted chiefly by my own students, and that it is not my views in which others will be particularly interested.

Since so many distinguished authors have supplied the contents of this volume, I can hardly try to disarm criticism by inserting the conventional disclaimer of any intent to produce a good book. I shall therefore confess openly that I have tried hard to make the book good, and that the result is the best I have been able to do. For its particular purpose, however, the contents can doubtless be improved, and I shall be grateful for suggestions

designed to contribute toward that end when another edition is attempted.

The only change made in any of the essays is the addition of the note on page 183 that refers to the supplementary paragraphs from Professor Samuelson's *FOUNDATIONS OF ECONOMIC ANALYSIS*. With the insertion of this supplementary material at the indicated point, the discussion becomes identical with those passages in the *FOUNDATIONS* in which the classification was completed. I am indebted to Professor Samuelson for calling my attention to this fact, and for allowing me to reproduce the material in question.

R. V. C.

ACKNOWLEDGEMENTS

The essays in this volume are reproduced through the courtesy of the editors of THE AMERICAN ECONOMIC REVIEW, THE ECONOMIC JOURNAL, THE EUGENICS REVIEW, THE JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION, THE QUARTERLY JOURNAL OF ECONOMICS, and THE REVIEW OF ECONOMICS AND STATISTICS (formerly THE REVIEW OF ECONOMIC STATISTICS), and through that of the University of Chicago Press. Special requests were also granted by R. F. Harrod, Mr. Nicholas Kaldor, and Professors Lionel Robbins, Paul A. Samuelson, and Joseph A. Schumpeter. Extraordinary courtesies were extended by the editors of THE EUGENICS REVIEW, and by Mr. Austin Robinson of The Royal Economic Society.

The time and place of original publication of each essay are identified in the Table of Contents of this volume.

R. V. C.

THE ECONOMIC JOURNAL

SEPTEMBER, 1938

SCOPE AND METHOD OF ECONOMICS ¹

IN my choice of subject to-day, I fear that I have exposed myself to two serious charges: that of tedium and that of presumption. Speculations upon methodology are famous for platitude and prolixity. They offer the greatest opportunity for internecine strife; the claims of the contending factions are subject to no agreed check, and a victory, even if it could be established, is thought to yield no manifest benefit to the science itself. The barrenness of methodological conclusions is often a fitting complement to the weariness entailed by the process of reaching them.

Exposed as a bore, the methodologist cannot take refuge behind a cloak of modesty. On the contrary, he stands forward ready by his own claim to give advice to all and sundry, to criticise the work of others, which, whether valuable or not, at least attempts to be constructive; he sets himself up as the final interpreter of the past and dictator of future efforts.

My sense of immodesty is greatly enhanced by the occasion and place of this gathering. As economists we are singularly happy in having this meeting of the British Association in Cambridge. There is no need for me to emphasise the unique contribution which this university has made to economic studies in recent times; the great names of masters dead and living are in all our minds. And here I come, a tyro from a University, which, albeit the home of revered economists—may I be forgiven for mentioning Locke, Senior, W. F. Lloyd and Edgeworth—must in the modern period recognise its own juniority of status, and dare to lay down the law in this holy of holies. In the sphere of methodology the Cambridge economists have contributed much both by way of parenthesis in their major works and by occasional papers. I must refer also to the classic treatise on *Scope and Method* by Dr. John Neville Keynes, who is still happily with us.

¹ Presidential Address before Section F of the British Association, Cambridge, August 1938.

As a small extenuating circumstance I may mention that after taking my degree at Oxford I spent an all-too-brief but highly stimulating period here as the pupil of Mr. Maynard Keynes. And it is a source of particular pride and pleasure to me that on the first meeting of the Association in Cambridge thereafter I should revisit it in this honourable capacity.

My substantial excuse for choosing methodology to-day is that I feel a strong inner urge to say something. Also the time appears to be fitting. English writers have been on the whole wisely chary of the subject; but recently there has been an outcrop of speculation upon it. There is Professor Robbins' brilliant essay. My differences from him on certain matters of emphasis will become manifest; his effective and conclusive exposure of many popular fallacies regarding the nature and assumptions of pure theory considerably lightens my burden. Professor Fraser has contributed some important articles, and his book on *Economic Thought and Language* lies on the borderland of methodology. Most recently we have Mrs. Wootton's jeremiad.¹ While her case against too grandiose claims for our subject is unassailable, I am confident that a circumspect statement of its achievement and utility would be proof against her shafts. Most melancholy of all I find her unappetising programme for the future development of economics.

A word of warning is in place at the outset. In view of the prospective intensification of economic studies in this country, it might be thought timely to lay down the lines or set up some finger-posts for the work which might most profitably be done. Such an attempt would indeed be presumptuous, and would depart altogether from proper methodological procedure. The principles by which progress in a science proceeds can only be reached by observing that progress. They cannot be deduced *a priori* or prescribed in advance. There are, no doubt, certain general logical rules to which all genuine advance in knowledge is subject. The study of these constitutes logic itself. Each science or discipline has its own special limitations and conditions; its method of progress has its own special characteristics; within the wide field of logical possibilities some are selected as especially adapted to its problems; it is with this selection that methodology is concerned. And for this reason the methodologist is bound to occupy the rear, and not the vanguard. He studies the specific nature of the selected principles after the selection has been

¹ *Lament for Economics*. Cf. also Dr. Lancelot Hogben, *Political Arithmetic*, Introduction.

made. Methods, of course, change from time to time; but the actual worker on special problems is more likely than the methodologist to be able to judge the best line of advance. The methodologist's contribution is more indirect.

It is when they endeavour to adopt a forward position that the methodologists are most apt to lapse into barren controversy. The historical school scolds the deductive school, and the deductive school scolds back. Captions and battle-cries are devised. The "institutionalists" appear on the scene. These rival schools endeavour to prescribe what economic method *ought* to be. The function of the methodologist is to say what in fact is, or, more strictly, has so far been. The proper and final reply to the would-be reformer is, "Stop talking and get on with the job; apply your method, and, if it is productive, you will be able to display your results."

On first glance this relegation of the methodologist to the rear might seem to give public endorsement to what has all the time been the inward suspicion of the pioneer that he is an utterly useless being. But in fact by reducing his claims he at once becomes much more useful. The forward worker is inevitably influenced by methods used in the past; methods that have already achieved good results may be expected to achieve more; tools ready to hand are taken up. By going over the old ground and making a stricter survey, the methodologist may considerably modify this influence of the past upon the present. For instance, by a minute examination of assumptions he may show that there are certain limitations in principle to the productiveness of a given method, and that it has in fact already yielded all the results that its assumptions allow. Or, he may show that propositions usually deemed to constitute constructive knowledge do not in fact do so, but consist essentially of definitions of the terms employed. Or, he may show that conclusions often presented as the fruits of deductive reasoning were suggested by observation of the facts and have no other support, the premises used in the pedagogic demonstration being hypotheses otherwise unsupported. These elucidations may alter the forward worker's sense of proportion and the reliance he implicitly places on certain tools. They may give him a greater understanding of the nature of past achievements, and so insensibly influence him in his gropings towards fresh discovery. To do this is very different from trying to lay down the lines on which he *ought* to work.

This survey of economics is confined to what may be called its scientific aspect—namely, the formulation of general laws and

maxims. Many economists are, naturally, concerned with much besides this. They are concerned with the bare description of institutions, with compiling statistics and presenting them in an informative way. Study of this sort may be regarded as contemporary economic history. It has serious methodological problems of its own, which are not considered here.

It must not be inferred that this paper is solely concerned with so-called deductive economics. Quite the contrary. Its purpose is to emphasise the limitations of deduction and the importance of observation of the facts. Facts may be observed for their own intrinsic interest, or as tending to establish or overthrow some generalisation. It is the latter type of observation that falls within this survey.

It may be of assistance at this point to sketch out certain broad conclusions which the following reasoning seeks to establish. An advance statement of this kind may make the course of the argument more easy to follow.

I propose to divide what is commonly regarded as the pure theory of traditional economics into two sharply distinguished sections. Confusion appears to me to have arisen from the failure to make this distinction. On the one hand there is the theory of value and distribution; on the other is the maxim that productive resources should so be distributed among occupations as to yield an equi-marginal social net product.¹

The theory of value and distribution seeks to show how a number of circumstances taken as given (the fundamental data)—namely, the preferences and capacities of individuals and the available resources—serve to determine a structure of output and prices. If a change in these data occur, the theory professes ability to predict the consequences, within certain limits, on the price-output structure. This professed ability to predict implies that we have available certain general laws concerning the succession of events—causal laws, in fact. Rigid demonstrability and certainty, of an almost geometric kind, are claimed for them. Since the laws concern the succession of phenomena, they must have an empirical basis; and since the phenomena of economics are notoriously highly complex and unamenable to scientific handling, it is a paradox that the laws derived from their study should have the high degree of certainty claimed for them.

The paradox is resolved when we consider that the laws in

¹ Cf. Professor Pigou, *Economics of Welfare*, 1st ed., pt. II, ch. 2, sec. 5.

question are deducible from a single simple principle,¹ itself based on experience, but on an experience far wider than that vouchsafed by the study of markets and prices and extending back to the earliest phases of man's self-conscious existence—namely, the Law of Diminishing Utility or the Law of Demand, to be defined more precisely presently. The experience is so broad that the principle may be taken as an axiom of the highest possible degree of empirical probability.

But against this very high degree of probability of the principle and the laws deduced from it must be set their complementary degree of generality. The degree of generality is, indeed, so great that, I shall submit, the power of prediction vouchsafed by them is almost nugatory.

Next, economists, even the most theoretical, have been prone to give advice on the basis of theory. And I believe that economists would claim that much of the advice so given since Adam Smith has been valid. A type of the advice I have in mind, though this by no means covers the whole field, is the recommendation of Free Trade. Now, it will at once occur to the critic to ask, How, if it is true that the laws of value and distribution are so general that they yield but a nugatory power of prediction, can a quite copious array of advisory propositions, admittedly based on pure theory, be justified?

The reply is that these prescriptions are based on the other department of what is commonly regarded as pure theory. They are derived from the maxim that productive resources should be so distributed among occupations as to yield an equi-marginal social net product. The nature and justification of this maxim must be considered.

In order to derive from it precepts, which are applicable in the real world, certain knowledge about that world is necessary. This knowledge does not, however, relate primarily to causal sequences, nor does it consist of a bare enumeration of particular features and events. It arises rather from a simultaneous chart or survey of the economic field, and the main work of the cartographer is analysis and classification. This analytical work is required *both* as a preliminary to the construction of the map and to the derivations of specific causal laws from the law of demand. I venture to submit that it is this identity of the preliminary groundwork which has tended to obscure the fundamental distinction between the set of conclusions which relate

¹ Cf. Prof. Lionel Robbins, *Nature and Significance of Economic Science*, pp. 77-82.

to causal sequences and involve predictory power on the one hand, and the comprehensive but simultaneous conspectus of the field as a whole, on which the validity of the prescriptions depends, on the other.

I regard this division of analysis into two departments as of importance (i) because it reconciles the fairly copious array of economic precept with the very limited power of prediction and, (ii) because only by it can the empirical grounds of our general propositions be properly sorted out. I should add that recent methodological speculation appears to attach too much importance to the part played by the general theory of value and too little to that of the equi-marginal maxim in the history of economic thought.

✓ Recently economists have had the very proper ambition of obtaining greater knowledge of causal sequences than is vouchsafed by deductions from the Law of Demand. The phenomena of the trade cycle have been a special stimulus in this direction. But once they leave the plane of high generality which pertains to those deductions, their generalisations are likely to have a much lower degree of probability. All the difficulties associated with the complex and unamenable nature of the phenomena, which they have to study, come to the surface. They must say good-bye for ever to the claims to certainty which they could make so long as they remained within the confines of their geometrical system. From being one of the most exact, albeit narrowly circumscribed, sciences, economics of necessity becomes one of the most conjectural.

Yet the conjecture of the trained observer may be of value. In the recent period economists have already offered advice on the basis of their conjectures in this dubious field. To this department belong many of the recommendations concerning control of the trade cycle; they are based on propositions concerning causal sequences not derived from the Law of Demand—on propositions, therefore, which are to some extent conjectural. Hence the recent conflict of prescriptions, of which we have heard so much. Thus we may account for the transition from the unanimity of advice, common in the last century, of which Free Trade is a good instance, to present-day disagreements. The former was based on the analytical map, making no claim to causal knowledge; the latter is based on the necessarily conjectural propositions of cycle theory, which must make such a claim, and are conjectural precisely because they entail such a claim.

But the new realm of conjecture, though it may drive out the

old knowledge from its position of central interest in the economist's mind, does not invalidate that knowledge. It will be a thousand pities, if the conflicting nature of prescriptions of the new type, which economists are right to give, albeit without claim to certainty, since they must give of their best, undermines the authority of the advice given on the basis of the analytical map.

I now proceed to a more detailed examination. What remains is divided into four parts. The first I call the economic criterion, which deals with the nature and authority of the prescriptions given on the basis of the analytical map. The second is the theory of value and distribution which considers the scope and validity of the causal knowledge derived from the Law of Demand. There remain the recent strivings after causal knowledge outside that ambit. Within this field I carve out a section named dynamic theory, for reasons which will be explained. The residual section I call empirical study. This must not be taken to imply that the knowledge considered in the earlier sections is not based on experience. I expect the studies falling under this fourth head to be the most important in the future; but, owing to my rearguard position, I shall not be able to say much about them. I hope that appreciation of the necessary limitations to the scope of the other types of knowledge may serve to stimulate the new empirical work.

I. THE ECONOMIC CRITERION

The train of thought here to be considered is derived from Adam Smith. His chief claim to fame consists in his origination of it, his work on this topic having far greater cogency and authority than his particular formulation of the labour theory of value or his speculations on the forces determining wages, profit and rent. Furthermore, I conceive it to be the central core of classical economics, entitled to an easy priority over the theory of value and distribution to which more recent writers, by reason of the growing precision of its formulation, have tended to give pride of place.

The contribution of this department of theory must be considered under two heads: (i) the choice of the criterion itself, (ii) the mechanism for testing how far existing or proposed arrangements and practices fulfil its requirements.

The criterion may be defined dogmatically as follows: If an individual prefers a commodity or service X to Y , it is economically better that he should have it. Similarly, if the individual

prefers work X to Y , or dislikes it less, it is economically better that he should do it. The economic good is thus the preferred. If we may adopt Professor Robbins' method of regarding the inner structure of thought rather than the verbal formulation of it,¹ this choice of a criterion may be attributed to Adam Smith.

The act of choice cannot be regarded either as a discovery or a hypothesis, though it partakes to some extent in the nature of each. He perceived that, by means of it, it would be possible to make sense of the confused and conflicting arguments of economic doctors and reduce chaos to order. This choice involved scientific insight of a high order. Its merits may be judged by its fruits.

In appraising institutions and practices and making recommendations, the economist has this criterion in mind; it constitutes his standard of good and bad.

Zealous protagonists for the scientific character of economics have been disposed, especially recently, to define the advisory capacity of the economist somewhat differently. Realising that in fully developed sciences laws of causation have primacy of position and practical maxims issue as corrolaries from them, they have been unwisely eager to assimilate economics to this category. Consequently they have suggested that the economist, in his advisory capacity, should state that a given interference will lead to certain consequences $X, Y, Z . . .$ and then remain silent, leaving his client to decide whether $X, Y, Z . . .$ is a state of affairs which he wishes to bring about. This formulation is in manifest conflict with the actual practice of economists. If the methodologist urges that this *ought* to be their actual practice, he trespasses beyond his proper province, which has already been defined. Also this formulation claims both too much and too little.

It claims too much because it gives an exaggerated idea of the economist's power of prediction at the present juncture. It claims too little because it entails that his advisory power is confined within the narrow limits of his predictory power. Moreover, it would make him present his information in a form in which it would be of no use to his client.

Suppose, for instance, an import duty on wheat is under consideration. He may feel confident that this will cause the price of wheat and wheaten bread within the country to be higher than it would otherwise be. He knows also that the duty will have effects on the prices of other commodities, on the incomes of various classes, on the foreign exchanges and the circulation of money. But he cannot put these effects into

¹ Cf. *Nature and Significance of Economic Science*, pp. 35-6.

quantitative terms, and in some cases he may not know the direction of the consequential movements. To do so he would have to have much more detailed causal laws at his disposal than there is any immediate prospect of his having.

But even if he could know all these things, his advice would still be in a form of little use to his client. Having heard all the prospective changes, the client will want to know whether the last state of affairs is in sum better or worse than the first, and will be unable by his unaided intelligence to decide.

By resorting to his analytical map, presently to be described, the economist may be able to come by a short cut to the required answer. He may be able to say outright and with substantial authority that on the whole the individuals of the community will be in a worse position, even although his power of predicting the actual course of prices and incomes is negligible. Any definition of the economist's advisory scope which does not recognise this is unrealistic, and fails to do justice to the usefulness of the economist, even with his present limited powers.

Strictures upon the economist's proneness to give advice come also from another quarter—namely, politicians or moral philosophers. What right, they say, has the economist to lay down that such and such ought to be done, since this depends in part upon the ends sought? Surely the economist must wait until the ends are furnished to him by the politician. This criticism is not valid.

The economist is entitled to his criterion of individual preference. The politician may then say to him, "I am not so much interested in individuals getting what they prefer, as in the country being self-sufficient. What I want to know is how to achieve this." But there are an infinite number of ways of achieving it. Which shall the economist prescribe? The politician may add, "Oh, well, I want to do it in the most economical way." The economist then interprets this as meaning that, subject to the over-riding condition of self-sufficiency, individuals are to get what they prefer. Without his own criterion he cannot choose among the infinite variety of possibilities. Thus he has to employ it, even when a specific end is furnished to him.¹

¹ The position may be more complex. The economist may be asked to provide not for absolute self-sufficiency, but for a higher degree of it than obtained before. He will then be able to lay down the conditions for the attainment of the greatest amount of economic advantage in connection with any given degree of self-sufficiency, and he may be able to give some idea of the successive rates of economic sacrifice involved in the attainment of successively higher degrees of self-sufficiency.

He uses his criterion both to give advice simpliciter and to give it subject to an over-riding end furnished to him. If it were true that there is a latent ethical or political bias when he gives advice simpliciter, it would be equally true when he advises on the means to achieve an end laid down by moralists or politicians. Without his own criterion, he is entirely stultified. With it, he can give advice of precisely equal validity and freedom from ethical bias whether a specific end is furnished to him or not.

We proceed to our second head within this field of thought : the mechanism for testing whether the requirements of the criterion are fulfilled. Here again our main debt is to Adam Smith. He perceived that the complex phenomena of markets and prices might be regarded as the result of the efforts of individuals to inform each other of their preferences. This is the basis of the analytical map. He correctly maintained that economic study arises from the fact of division of labour. Robinson Crusoe directs his energies in relation to his own standard of preferences; he needs no outside advice. He may, indeed, misdirect his efforts from ignorance of agriculture or engineering; in this the technicians in these subjects can alone correct him; the economist has no place. The need for the economist arises from the division in person between the producer and the consumer.

Economists have constructed a map or model in which individuals are seen informing each other of their preferences. (It may help the reader to regard this map as the "theory of perfect competition," provided that all reference to the sequence of events is excluded from that "theory.") In order to construct the map in a way which corresponds with the observed phenomena of the real world, certain important analytical work was necessary. The relevant propositions may be stated in the form of truisms or tautologies, such as that the price of an article is equal to the sum of rewards to all persons contributing to its production, or again, if services of the same type get equal rewards in different occupations, the prices of commodities will be proportional to the quantity of service required for their production.¹ The intellectual intuition behind these formulations is primarily one of classification. Indeed, it may be said that the major part of traditional economic theory consists of classification. Classification is a highly respectable scientific activity of which

¹ More strictly, the prices of commodities will be the sums of parts *a*, *b*, *c* . . . charged in respect of services *A*, *B*, *C* . . . the value of each of which parts will be proportional to the quantity of the corresponding service used.

economists have no need to be ashamed. By referring more to it and less to so-called "laws," their claim to scientific status, albeit more modest, would be less suspected.

The map is to some extent hypothetical. It supposes that various activities may be interpreted as notifications of preferences. On the other hand, it is drawn with reference to the facts of the situation, assuming, if appropriate, such matters as private property, private ownership of land, unequal division of wealth, even special types of banking institution, company organisation, etc., and traces how the mutual notification, which it supposes to be intended, operates in these conditions.

Two points may be noted. (i) By means of the map we are enabled to get a view of the economic field as a whole. This is necessary for prescription. A particular piece of legislation may be well designed to secure its specific object. All reasonable men will wish to know, and it is the economist's task to say, how this fits in with the larger purpose, for which the whole economic mechanism is designed. To what extent does the specific objective militate against or further the more general purpose? ¹ This can be studied by reference to the analytical conspectus. (ii) Our right to interpret observed phenomena as constituting the mutual expression of preferences depends in the last analysis on introspection. An observant visitor from Mars who knew nothing of the nature of desire, purpose and will, might well be unable to make this necessary link; he could become expert in the knowledge of causal sequences, but for lack of the necessary interpretation would be unable to give advice on the basis of the conspectus.²

The map is related to the criterion of preference by this principle, that the more effective the system of mutual notification attained, the more fully are preferences likely to be realised. Reference may be made to the example of an import duty on wheat. We may know enough of the existing organisation of markets to be sure that this will impose an obstruction to effective mutual notification. We infer that in the presence of this obstruction preferences are less likely to be secured. The validity

¹ If I interpret him aright, this account is in accordance with the view expressed by Professor Robbins in his section on "rationality" in the concluding section on *The Nature and Significance of Economic Science*. Cf. also Professor G. Cassel, *Fundamental Thoughts on Economics*, p. 14.

² This is in principle the position to which Professor Cassel would reduce economists by extruding all reference to ability from economics. Cf. *Fundamental Thoughts on Economics*, pp. 66-70. In another place, however, he recognises the fundamental part played by the notion of need, which is only another word for ability. Cf. *Theory of Social Economy*, Vol. I, pp. 3-9 (Ed. McCabe).

of this inference depends upon the correctitude of our interpretation of existing market processes. It is independent of knowledge how individuals will react to the obstruction¹—namely, the consequent course of prices, wages, etc., which we should have to know if we were required to give a full statement of consequences before prescribing, but which we only could know if our causal knowledge were fuller than it is.

How far the facts of real life correspond to those envisaged in the model is a matter of observation, and it should be subjected to continuous check. Economists of the past were perhaps too hasty in assuming exact correspondence. On the basis of the assumption and the criterion that the economic objective was to achieve the preferred position the maxim of *laissez-faire* was exalted and a wealth of recommendations vouchsafed.

These may be defended at least negatively. A given interference, unless specifically designed to shape the real world to a closer approximation to the map, is likely to distort it farther from it. In this case reference to the criterion makes valid condemnation possible.

More recently there has been a proper tendency to go beyond this negative attitude and to consider what interferences might be introduced to make the real world more like the map. Recommendations of this sort must be based on a vigilant observation of the actual working of real institutions (but they do not rest on causal laws or predictory powers).

In this connection reference may be made to the formulation by Professor Pigou, already referred to, that the marginal social net product of resources in different occupations should be equal. Time forbids me to consider the definitions and classifications required to support this. It is the necessary but not sufficient condition for the fulfilment of the criterion that individuals should get what they prefer, and may be regarded as a (partial) restatement of it.

The fact that a large part of Professor Pigou's *Economics of Welfare* consists in the appraisal of institutions and proposals in the light of his criterion is evidence that this line of thought still has vitality.

Recent theorems relating to Imperfect Competition, which, in my own mind at least, have a direct intellectual connection with Professor Pigou's consideration of Increasing Returns in

¹ In exceptional cases the precise nature of this reaction might be relevant. Our map read in conjunction with out interpretation of the market should warn us if there is any probability of this.

the light of his criterion, appear to have their principal value, not in the realm of causal laws or prediction, but as an endeavour to show in an orderly and systematic way how real markets are distorted by comparison with those of the map.

In spite of these interesting developments, I feel that there is a danger that this part of economic speculation, the field of its most signal triumphs in the past, may suffer an undeserved neglect, whether owing to the economist's absorption in rival interests or to his discouragement at the overthrow of Free Trade. A mistaken methodological ban on advice-giving might also contribute something.

The widespread growth of Government interference makes this function more and not less important. Officially sponsored rationalisation schemes, arrangements for the semi-public operation of services, public policy with regard to road and rail transport, marketing-board arrangements all require vigilant scrutiny in the light of the criterion, to say nothing of more full-blooded socialist programmes. Even if public policy appears to violate the advice which the economist would give simpliciter, this is no excuse for him not to take an interest in the fulfilment of his criterion subject to the over-riding demands of policy. He may think that there is no case for giving agriculture special protection; in the face of the opposite policy, he has scope enough to criticise the arrangements introduced to give effect to it. If he loses interest in this field of thought, the country is only too likely to get tied up with red tape and be subject to vast avoidable wastage.

One further topic remains for consideration in this section.

The preference criterion which forms the basis of the kind of investigation here considered was stated in a form not involving the comparison of the claims of different individuals with one another. The preferences notified in the model market are of the form that a given individual prefers an n th unit of X to an m th of Y . The need of one individual is not compared with that of another.

Yet one is tempted to make such comparisons. For example, Marshall says in the *Principles* that the marginal utility of two pence is greater in the case of a poorer man than in that of a richer. If such comparisons are allowed, recommendations for a more even distribution of income seem to follow logically. They give scope for a wide range of recommendations not sponsored by our original criterion.

Objection to this enlargement of the field of prescription may be based on two grounds.

(i) It may be urged that the economist hereby goes outside his proper "scientific" field. This point is strongly urged by Professor Robbins. Whether the n th unit of X has greater or less utility than the m th of Y to a given individual may be made the subject of test. He can be given the choice. But there are no scientific means of deciding whether the n th of X has greater or less utility to individual P than the m th of Y has to another individual Q . The choice can never be put. This implies that we cannot in fact decide whether two pence have more utility to a millionaire or a beggar. Yet we may have a shrewd suspicion. But this, we are told, is "unscientific," for lack of a test. This objection would be very weighty if economics itself were a mature and exact science. Yet in fact its achievements outside a limited field are so beset on every side by matters which only admit of conjecture that it is possibly rather ridiculous for an economist to take such a high line. *παιδευμένον γὰρ ἐστὶν ἐπὶ τοσούτων τὰ κριβές ἐπιζητεῖν καθ' ἕκαστον γένος, ἐφ' ὅσον ἡ τοῦ πράγματος φύσις ἐπιδέχεται.*¹ Can we afford to reject this very clear finding of common sense? Of course great caution must be exercised in not pushing the matter too far. Since the evidence is vague, we must not go farther than a very clear mandate from common sense allows.

It is not altogether certain that the gulf between the prescriptions of the classical economists and those of, shall I call them, the welfare school is as great as Professor Robbins implies. There is no doubt that the marginal utility of two pence to a given man at a given time and in given other circumstances is less if he has £1,000,000 a year than if he has £25 a year, since he will spend the £25 on things which he prefers per a penny of cost to the things on which he would spend the remaining £999,975. The further postulate that the two pence has lower utility to a millionaire than to a £25-p.-a. man is based on some sort of assumption about the equality of men in regard to their needs which must not be pressed too far. But so also do the prescriptions favourable to free markets. For the individuals who gain by the opening of a market are often different from those who suffer some loss. Consider the Repeal of the Corn Laws. This tended to reduce the value of a specific factor of production—land. It can no doubt be shown that the gain to the community as a whole exceeded the loss to the landlords—but only

¹ Aristotle, *Ethica Nicomachea*. 1094b. "For an educated person should expect to obtain precision in each branch of study to the extent which its nature permits."

if individuals are treated in some sense as equal. Otherwise how can the loss to some—and that there was a loss can hardly be denied—be compared with the general gain? If the incomparability of utility to different individuals is strictly pressed, not only are the prescriptions of the welfare school ruled out, but all prescriptions whatever. The economist as an adviser is completely stultified, and, unless his speculations be regarded as of paramount æsthetic value, he had better be suppressed completely. No; some sort of postulate of equality has to be assumed. But it should be carefully framed and used with great caution, always subject to the proviso “unless the contrary can be shown.” In the case of the free-market arguments there is usually no characteristic attaching peculiarly to the beneficiaries of restriction other than that they are beneficiaries. In the case of the uneven distribution of income, there are many special characteristics of the rich as a class to which due consideration must be given.

(ii) Objection may be raised on more general grounds which appear to me to have greater weight. The distribution of income is intimately connected with the balance of social and political forces, the study of which is outside the economist's province. In prescribing here he knows without being told that there are other considerations. This is not to say that he should avoid all questions with political entanglements, for then again he would be almost completely stultified. Most vested interests can whip up some political support. It is a matter of degree and sense of proportion.

It might further be urged that since re-distribution is a straightforward matter widely understood, the economist might well leave it alone, since he can but reinforce in technical language an argument already before the public. Projects of re-distribution, however, may have complicated ramifications which the economist is especially qualified by his other training to trace out. For instance, in his *Public Finance* Professor Pigou has worked out with great elaboration the principles and consequences of a re-distributive system of taxation. It may safely be said that this work would have been beyond the powers of any but a highly trained economist.

II. GENERAL THEORY OF VALUE AND DISTRIBUTION (STATIC THEORY)

We now enter the territory which has increasingly come to be regarded as the special domain of the economic theorist. It

is here that we find the laws relating to the succession of phenomena, claiming a high degree of authority, on which prediction is based.

It is not altogether clear why this department of thought has been so greatly elevated. The trouble may have begun with Ricardo. He wrote: "In different stages of society, the proportions of the whole produce of the earth which will be allotted to each of these classes, under the names of rent, profit, and wages will be essentially different . . . to determine the laws which regulate this distribution is the principal problem of Political Economy."¹ Why the principal problem? We are not told.

The method of procedure is to take certain elements in the situation as given—namely, the preference lists of individuals for goods and services, the terms on which they are willing to contribute their assistance in production and the current state of technology—and to take other elements as unknown—namely, the prices of all commodities and of factors of production, the amounts of commodities which will be produced and of factors which will be employed, and the precise methods of production among the variety of those technically possible which will be used. If the elements taken as known were in fact known, it would be possible to write down a number of equations expressing some of the unknowns as functions of the others. The object of this procedure would be to provide means of showing how changes in the fundamental data, desires, etc., will govern the course of events.

I regard the most notable intellectual achievement in this department to be the classification of factors of production required as a preliminary to the formulation of the equations. (This classification has also proved of great service in elaborating the analytical map already considered.) There is the analysis of the contribution of capital to production as consisting essentially of waiting. There is all the work concerning the relation between direct and overhead costs. The so-called law of rent has given rise to a number of dichotomies of great interest. The concept of profit as a reward for skill and judgment has been rendered fairly precise. Professor Knight has shed a penetrating light upon the relation of profit to uncertainty-bearing, but some puzzles here remain. Meanwhile Mr. Keynes has produced another concept—liquidity-sacrifice, which bids fair to find a place as an independent factor; it needs further elaboration,

¹ *Principles of Political Economy and Taxation*, Preface, p. 1.

and its relation to the general concept of uncertainty-bearing requires precise definition.

These concepts are then applied and their values are expressed as unknown quantities in a number of forms of functional equations. These relate to the demands for commodities considered as functions of the prices of commodities, the quantities of factors used to produce commodities considered as functions of the prices of factors, and the quantities of factors on offer considered as functions of their prices. Satisfaction is expressed if there are as many forms of equations as there are unknown quantities.

But we run at once into the difficulty that the matters taken as known for the sake of argument are in fact not known. We may write down that the quantity of a commodity demanded depends on its price and on the prices of other commodities. But this does not take us far unless we know the precise law of dependence. We can only say that there should be an equation here, and if it could be written out along with a number of other equations, we should be able to determine the value of the unknowns and the effect of any specified change upon them. But in fact we have not got these equations, but only a number of blank forms, which are nothing more than aspirations to have such equations!

If this were the end of the matter, this department of theory would yield no causal laws and no power to predict whatever. The situation is not quite so bad. It is at this point that the Law of Demand is brought into play. With its aid we are able to say something about the demand equations. We say that they will have this in common, that the quantity of a commodity demanded will be less the higher its price.¹ We are still unable to formulate the demand equations precisely, but we have this very general piece of knowledge about their structure. Having regard to it, and also assuming that the other equations relating to supply and productive methods are *not of a very odd structure*,² limited powers of prediction with regard to the direction, though not the quantitative value of changes consequent upon a change in fundamental data, are rendered possible.

¹ Even to this there may be exceptions; cf. Marshall, *Principles of Economics*, (8th ed.), p. 132.

² It is possible that the crucial point in the argument by which Mr. Keynes throws doubt on the consequences usually supposed to flow from certain changes, on the basis of the theory of value, is his demonstration that the real supply schedules of the prime factors are, owing to actual offer terms being expressed in money, precisely of the odd structure required to invalidate the reasoning.

How do we come by this Law of Demand? Here we are certainly at the very centre of traditional economic theory. I do not believe this to be based on an observation of markets in the ordinary sense. There the confusing influence of many forces is operative, and though scatter diagrams may give a faint suggestion of the law, we hold it with much more feeling of assurance than they would vouchsafe.

Consider the Law of Diminishing Utility. Is this based on some psycho-physiological principle, the diminishing reaction to stimuli? Is the main constructive part of our theory based on a generalisation borrowed from elsewhere, the verification of which depends on the observations of others? I do not think so. I believe the matter to be simpler.

It appears to me that we have here an *a priori* axiom, albeit based in an indirect way on observation. In markets we are concerned with commodities divisible into parts. The parts are homogeneous in one respect—namely, in all their sensible properties—so as to be perfectly substitutable one for another, but heterogeneous in another respect—namely, the use to which they may be put. The parts may be used separately. Each occasion of their use has its own importance. Not each occasion is likely to have precisely the same importance, save in an exceptional case. This is all that is required for the Law of Diminishing Utility. If supply is restricted, use will be confined to the most important occasions. This appears more general than, and independent of, the law of diminishing reaction to stimuli. The axiom arises directly out of homogeneity in one respect and heterogeneity in another. That homogeneity and heterogeneity thus reside together in exchangeable objects is of course known by observation, ultimately by introspection and the assumption that other selves exist and have similar states of consciousness to our own. The existence of the law explains how it is possible to make prediction on the basis of equations, which themselves seem, and claim to be, independent of detailed economic investigation.

With the aid of the general Law of Demand we are able to predict some immediate consequences of changes in fundamental data. But we cannot go far. In the absence of more precise quantitative knowledge we soon run into alternative possibilities.

This being so, the next step would appear to be to obtain more precise knowledge. This must come from empirical investigation. But when we leave the sure ground of the Law of Demand in its general form, we are at once confronted with the appalling problems which the shift and change in the economic scene, with

its plurality of causes and unamenability to experiment, present. Heroic attempts have been made by such workers as Dr. Schultz¹ to obtain quantitative Laws of Demand, and Professor Douglas² has made assaults on other parts of the structure of equations. Interesting results have been obtained, and more are to be expected.

If this is really the heart and centre of economic science, all our resources should be put at the disposal of such investigations. But is it? We come back to the *obiter dictum* of Ricardo. Can it be justified?

It may be hazarded that there has been some concentration on the development of this part of pure theory, precisely because to a certain point it was possible to proceed by way of deduction from our demand axiom. But when we proceed beyond this point it is necessary to make hypotheses about alternative possibilities, and, although with the aid of mathematical tools elaborate chains of deduction may be forged, the basis remains hypothetical. It does not seem probable that the predictory power in the theory of value can be enlarged, save by such empirical observations as make it possible to fill in the blank-forms of equations with quantitative data.

This may be done. It should be noted that the results obtained will at best not have a very high degree of probability. Yet it must be said that if real equations could be substituted for the present empty forms, even if the former were conjectural and hazardous in the extreme, economics would be on its way to looking much more like a mature science than it does at present. Only by abandoning the theological claim to certainty, and explicitly allowing a wide margin of error, can economics rebut the charge of scholasticism and claim scientific status.

To sum up. The adoption of individual preference as the criterion for testing arrangements has proved convenient for getting a systematic ordering of thought. Incompletely but validly formulated as the principle that the marginal social net product of productive resources should be equal, it may be used to test existing arrangements or proposals. A map may be constructed, resembling our economic system, in which individuals notify each other of their preferences. Interferences may be condemned for not taking account of this map. Alternatively interferences may be recommended designed to make our economic system resemble the map more closely. Both

¹ Cf. *Statistical Laws of Demand and Supply*.

² Cf. *The Theory of Wages*.

kinds of advice spring from and are dependent on a vigilant observation of the actual working of our system. It is highly important that this part of the economist's function should not fall into desuetude.

The causal laws of static theory are deducible from the Law of Demand. This is well based on a very wide experience; it is in no need of verification; further attempts to verify it could not add to the assurance with which we already hold it. But the laws are of a very general form, and little prediction can be based upon them, nor are they the source of the recommendations of traditional economics. More specific laws would have to be based on detailed empirical research and would be highly conjectural. While great interest attaches to such empirical work, it is not clear that this should be the main avenue for future developments; but, if it is not to be, then the general theory of value must itself be displaced from its central position.

III. DYNAMIC ECONOMICS

There is no reason why the quest for causal laws should be limited to those propositions which may be derived from the law of demand. We may well expect future progress to lie outside that ambit.

Out of the wide field of possibilities I choose for first consideration one department, which I propose to call dynamic economics. In using this terminology I am aware that I am departing from recent usage. There has been a tendency to use the expression broadly for any set of generalisations lying outside static theory. More specifically it has been used for the study of the influence of expectations—but these may find full expression in a system of static equations—or, again, for the study of time-lags in a process of adjustment to a new static condition. These studies all have their own place.

I believe that there ought to be, alongside of static theory, a body of laws relating to the increase (or decline) of economic magnitudes, and that with the aid of a very few empirical generalisations, having high authority if somewhat less than the Law of Demand itself, it may be possible without more ado to construct such a body of laws. I conceive the analogy between the relation of dynamics to statics in mechanics and that of this branch of economics to the static theory to be much closer than that implied in recent uses of the word dynamics in economics. While the equilibrium price determined by the maintenance of a steady

flow of demand and supply corresponds to a state of rest, new equations would be formulated to determine regular movements in the economic magnitudes under the influence of growth of population, savings, inventions, etc.

This line of thought is not, of course, new. The classical economists attached great importance to the alleged tendencies of rent to rise and profits to fall. Such considerations are not absent from Marshall. But generalisations of this kind have tended to recede from view, owing both to their conjectural character and to the more precise formulation of static propositions in a mathematical garb. The existence of this formulation has in turn tended to lead monetary and trade-cycle theorists, who are interested in change as such, to regard the phenomena of their study in terms of transitions from one static equilibrium to another. It may be that they would be greatly assisted if they could regard them as departures from or oscillations about a path of growth; but they can only do this effectively if the laws governing increase are as precisely formulated as the static laws. We need a system of fundamental equations using simplifying assumptions—cf. the frictionless surface, etc.—in which rates of increase will themselves figure as unknown terms.

One reason for holding development along these lines to be needed is the unsatisfactory condition of the theory of interest in static economics. I refer now not to the results reached by Mr. Keynes in his important study of the dual nature of capital supply (waiting and liquidity sacrifice), but to a still more fundamental difficulty.¹ Using the assumptions required for static price determination—namely, persistence of tastes, technology and supply of factors unchanged—the demand for new saving at any given rate of interest is zero, since so long as the fundamental conditions and the equilibrium are maintained, the volume and method of production must be unchanged. To put the same thing in other words, the static equations determine the price of *capital* and the quantity of it which will be used. It is the quantity of capital in use which, along with the quantity of land and labour in use, remains unchanged throughout the maintenance

¹ I regret that it is not possible within the scope of this paper to consider from a methodological point of view the great contributions to thought recently made by Mr. Keynes. My division into sections was necessarily guided by reference to economics as a whole, and his contribution, although internally highly coherent and constituting a unified structure, belongs in part to all my divisions, so that a full discussion would not be wholly relevant to and would unduly swell any one. See *Econometrica*, January 1937, R. F. Harrod, *Mr. Keynes and Traditional Theory*.

of a given equilibrium. But if the quantity of capital in use is the same, the rate of saving is zero. I have the impression that writers, other than the most careful, tend to get one dimension wrong at this point, and suppose that the "Laws of Supply and Demand" (static theory) may determine not the quantity of capital, but the amount of saving—*i.e.* rate of increase in the quantity of capital at a given level.¹

That it is possible to reach interesting conclusions on the basis of the static assumption of no saving may be seen from Mrs. Robinson's article on "The Long Period Theory of Employment." The paradoxical air of that essay may well be due precisely to her strict adherence to the static assumption. The fact that she quite properly compels us to consider the true effect of any change in the light of its consequences in the state of equilibrium only reached when all saving has fallen to zero, suggests that it would be expedient to tackle the problem more directly. In place of a succession of static equilibria we need the concept of motion under the influence of steadily operating forces.

The laws will govern the relation between and determine the mutual consistency of the rates of increase of various magnitudes—*e.g.* working population, technical powers, quantity of capital, of circulating medium, etc. Some empirical foundation is necessary. Bare study of mutual implications will not yield much, since there is an infinite variety of possibilities. But I have the impression that a few basic empirical laws, of a generality not much inferior to that of the Law of Demand in statics, may yield, in connection with the study of mutual implications, an elaborate structure of deductive theory.

An example of a basic empirical generalisation may be found in the proposition put forward by Mr. Keynes in his recent work, that at a given rate of interest people will save a larger absolute amount from a larger income. We could get still further if we

¹ We might imagine a static state as follows. People would save out of *earned* income in their early years and invest in life annuities such sums as would make their income rise at a rate which would make its marginal utility fall at a rate equal to the rate of interest. Meanwhile the rate of interest would be fixed at a critical level, sufficient to make them hand on their *inherited* capital intact, despite their inferior regard for their heirs. These conditions would, on the assumption of a stationary age-distribution, make saving equal to zero. If their regard for their heirs happened to be as great as their regard for themselves then, with a positive rate of interest and supposing the state of Bliss described by Ramsey in his well-known article not to be reached, there would be positive saving, and the assumptions of static theory would be mutually inconsistent. Similarly a socialist state in conditions otherwise static should arrange for positive saving.

could establish—but this is perhaps too audacious for the early stages—that people save a larger proportion of a larger income. Both these propositions are clearly open to empirical verification. They will be subject to *ceteris paribus* clauses regarding the distribution of income and institutional arrangements, but these would probably not impair their high scientific utility. The statistical work of verification required is no doubt substantial, but light compared with that required to fill in the blank forms of the static-theory equations. The phenomena are much more amenable to the attainment of reliable results in this field than in that of static supply and demand schedules. The *de facto* growth of society assists the former, while it hinders the latter type of statistical inquiry.

May I be excused for touching on a theory in which I believe—subject, of course, to the eroding researches of historians of thought—that I have certain proprietary rights? If it is true that the most important factor governing the demand for new capital is the rate of growth of the system, and the most important factor governing its supply is the absolute size of the system, then, having regard to the truism that demand must be equal to the supply, a host of interesting conclusions should follow. Premises containing these peculiar mathematical relations should surely be a gift, precious beyond compare, to economists of mathematical bent seeking new conclusions. I risk saying that if, when trade-cycle theory comes to be established on firm and agreed foundations, these relations are not judged to have central causal significance, I shall be dumbfounded.

IV. EMPIRICAL STUDIES

I now come to the most difficult, the most tentative and withal the most important section: the search for causal laws outside the realm of deductions from the Law of Demand or the simple laws of growth.

Having previously tended to belittle the causal significance of the theory of value and distribution, I should like to pay tribute to the high importance of the work of classification, not achieved without much toil and the insight of genius, which is the groundwork of that theory as well as of the analytical map. This is likely to prove a valuable and indeed indispensable tool for further investigation, and the empiricist, however radical, is likely to flounder if he is unable to use it. In the classificatory work I include truisms like the quantity theory of money and

the wages-fund theory, which serve to give precision to the concepts.

How shall I proceed into this unmapped territory? At this stage there should be no dispute on matters of principle. On the one hand, for every proposition purporting to relate to the succession of events it must be possible to point to the empirical evidence. Any attempt to assume superior airs may be met with the rejoinder that if empirical evidence is lacking, the proposition can be no more than a definition of the terms which it employs. On the other hand, attention must be paid to the mutual consistency of generalisations, and each one must be valued according to the extent to which it contributes to making the whole system more coherent.

One might draw up a methodological classification by reference to how the investigator spends his day. There is armchair cogitation; there is the application of statistical technique to the great body of statistical raw material already available, which may well require an elaborate apparatus and assistant workers; there is the compilation of fresh statistical material by work in the field; there is also the field-work directed to gaining a closer knowledge of how institutions actually work and the motives which govern behaviour. It may safely be said that all these kinds of activity have utility; they may be regarded as "factors" in the production of economic truth to be mixed in due proportions in accordance with the general principles of production; what is a due proportion depends in part upon the abilities and temperaments of the workers available. I will only add that the institutional arrangement whereby most professional economists are heavily burdened with teaching and administrative duties may militate against a sufficient admixture of the more laborious forms of statistical and field work. The remedy for this, now already in process of application, is the endowment of full-time workers of the right temperament and the provision of adequate laboratory equipment and skilled assistants. It may be noticed with satisfaction also that statistical method, on which economic advance depends, has recently displayed a great vitality under the influence of such distinguished pioneers as Dr. Ragnar Frisch.

There is, however, a more fundamental difference between the outlook of the more and the less empirically minded. This consists of a difference of judgment as to the most hopeful source of clues for the future development of the subject. On the one hand there are those—I believe that it is fair so to represent the view of Professor Wesley Mitchell—who believed that clues are

most likely to be obtained by the diligent scrutiny, arrangement and rearrangement of the empirical data. The facts will one day speak for themselves. By patient and continuous observation, the investigator will find the appropriate generalisation borne in upon him. On the other hand, some believed that clues are more likely to be found by an inspection of the existing body of theory. Close examination of it will reveal gaps, and in those very gaps may be found clues suggesting new generalisations which will render the theory more coherent, or even wider generalisations leading to a revolution of the kind which occurs from time to time in physics. Or, more moderately, they may lay some stress on observation, but urge that this should be done very much in the light of existing theory, to test hypotheses directly suggested by that theory.

Both schools must be given our cordial blessing. Past achievements are still too exiguous for us to be sure which is the method most naturally adapted to our study.

It is sometimes claimed that the major part of established generalisations have been reached in the less empirical way. But my feeling is that the great fruitfulness of the analytical map in yielding valid prescriptions has obscured the extreme paucity of our knowledge with regard to causal sequences. Two circumstances militate against the more deductive method. One is the impossibility of the crucial experiment. In the mature sciences which rely mainly on this method, such as physics, or, to name a more recent comer, genetics, the crucial experiment is of central importance. Secondly, it is extremely difficult to test hypotheses by the collected data of observation. The operation of the plurality of causes is too widely pervasive. Thus numerous hypotheses are framed, and never submitted to decisive test, so that each man retains his own opinion still.

I do not wish to press these considerations hard, but only sufficiently to upset the complacency of dogmatic upholders of one exclusive method. To give a contrary example, I believe that in so far as the monetary explanation and the demand-for-capital-goods explanation of the trade cycle be regarded as *rival* hypotheses suggested by theoretical considerations, the course of events in this country and the United States in the last ten years enhances the probability of the latter. It should be possible to devise statistical methods to increase the cogency of this indication of experience. I assume that even the more deductive or hypothetical method of advance should be fortified by statistical verification.

It is a doubtful point whether the more radically empirical method has been as barren as is sometimes suggested. To give a rather trivial example, Gresham's Law is an instance of the facts speaking. However convincing the *ex post* theoretical explanation of the phenomena, the process of discovery was by observation rather than hypothesis. A more striking example may be derived from trade-cycle studies. It is an accepted generalisation, not indeed possessing the universal validity of the Law of Demand, but none the less of substantial authority and interest, that in the upswing of production prices have a rising tendency and in the downswing a falling tendency. It may safely be said that this could not be deduced from the propositions of static theory nor from that part of monetary theory, which is deducible from them. Falling prices would be regarded as an equally (if not more!) likely accompaniment of rising output, and vice versa. The generalisation is a direct result of observation, an excellent example of the facts speaking for themselves. And if theoretical explanations have subsequently been woven round it, this must not blind us to the true source of our knowledge. If rather crude observational data can yield appetising morsels of this sort, may we not legitimately hope that when subjected to refined statistical treatment they will yield more fruit in plenty? It will still be necessary to relate such generalisations to each other and to those of a more deductive origin in an orderly fashion.

Having made this plea for the more radical empiricist, I will conclude by mentioning one or two types of investigation suggested by the present condition of theory. If I make no mention of others now under way, I hope it will be understood that this is not because I regard them as unimportant, but for lack of space and because the former happen to have caught the speaker's eye first.

Emphasis has recently quite properly been placed upon the importance of expectations with regard to the future in determining the present actions of the individual, and upon the slender basis of knowledge on which he is obliged to form his expectations. Speculation upon the consequences of this may therefore be regarded as arising directly out of theoretical considerations.

Ignorance with regard to the future drives the agent back to an imperfectly rational dependence upon past experience, particularly his most recent experience. It is reasonable on this basis to make the hypothesis of a time-lag between certain adjustments. By introducing a systematic lag it is possible to give a

mathematical demonstration that an oscillation of behaviour must result. The interesting survey by Dr. Tinbergen in *Econometrica*¹ discusses a number of hypotheses of this nature.

Statistical verification may proceed from two ends. On the one hand it may be possible to verify the particular lag assumed by reference to two statistical series. On the other the cycle mathematically deducible from the assumption of such a lag may be compared as to its general features with the real cycle. One might hope that even with the data already available the determination of lags in this empirical manner might give us a theory of the trade cycle, which would be self-consistent and consistent with the broader generalisations of theory, and also subject to fairly approximate empirical verification at both ends. Fortified by such tests, with what far higher degree of confidence might we call upon legislatures to take remedial measures! I may add that the framework of equations within which the lag hypothesis should be applied are those of dynamic economics. This gives another reason for wishing an early precise formulation of these.

I now pass to an entirely different type of empirical work. General considerations suggest that the entrepreneur acts under the influence of certain defined forces. When we come to examine these, it is surprising how largely the entrepreneur must be ignorant of their precise value. This is evident enough in the case of capital outlay, decisions regarding which must be based on prognostication. But even current output is properly determined by reference to the value of the loss or gain of customer goodwill and to that of "user cost,"² both of which depend upon prognostication. And apart from the future, there are other matters of uncertainty. Correct behaviour in the field of imperfect competition—and this is the greater part of the whole field—presupposes knowledge of the value of marginal revenue, which in its turn requires knowledge of the current elasticity of demand. Yet even that magnitude of central importance, which theorists are apt so glibly to take as given, is one about which many entrepreneurs are quite in the dark.

Having regard to the fog of uncertainty by which the entrepreneur is thus shrouded, it has seemed to some of us in Oxford that valuable information about how he does in fact steer his course might be gained by the method of direct question. It is desirable to obtain a wide sample, and to conduct the question-

¹ July 1935.

² Cf. J. M. Keynes, *General Theory of Employment*, Ch. VI.

naire in such a way as to make it probable that the victim will speak his true mind. I select two lines of thought for mention.

(i) Theory may assume that the change in a certain magnitude—*e.g.* the rate of interest—will cause a defined change in the entrepreneur's behaviour. But in fact if his margins of possible error, owing to uncertainty about various factors, are very wide, such a specific change, even although definitely known, may be treated by him as of too small account to affect his reckoning. The method of direct question does not seem an unreasonable one for obtaining reliable information about this.

(ii) The entrepreneur lives by action; even if ignorant of the relevant data, he must decide one way or another. Nor can each and every decision be reached by an independent act of judgment; some rules of thumb are necessary to the efficient conduct of a business. In the absence of data, the rules must be supplementary to those envisaged in static theory. What are they? Again this seems a suitable subject for direct question. Generalisations may be possible and valuable, even if confined to certain types of industry. For instance, an irrational but systematic and consistent treatment of overhead costs might give rise to a pattern of behaviour of significance in the trade cycle.

I believe that we may be on the eve of a great advance in economic theory, taking us right outside the ambit of the static system of equations. The wealth of statistical data, together with the indications resident in the trade cycle that the succession of events is governed by laws still undiscovered, should be a spur to the inventiveness and enthusiasm of every student to whom the ways of science make appeal. He may reasonably feel that any day he may light upon some general relation of wide validity, satisfying to the intellect and capable of yielding vast benefit to humanity. The prospect is an inspiring one.

Kindled by it, the worker who is an economist at heart will reject with contempt proposals for relegating him to the banausic work of the mere cataloguer. Nor will he be likely to wish to take up a position of polite subordination to the sociologist or anthropologist, as Mrs. Wootton has recently suggested. All honour be to those allied branches of investigation into human behaviour. I hope that I have indicated that the economist should take a broad view; he should be very much awake to the possibility of obtaining hints from and using the results of workers on the periphery of his subject. But if the status of a subject may be judged by the number and width of its general laws

established on a firm foundation, then, even adopting my very modest assessment, the economist may still claim without insolence that his subject is more mature than other sociological studies. And it may be added that the wealth and precision of the data at his disposal suggest that a further advance on a broad front is likely to occur in the near future. The notion that investigators in other branches of social study should be asked to help forward their lame brother economist and guide him on his proper path must, in the interest of intellectual honesty, be set down as fatuous and derisory.

To some minds it may seem that in the field of the social studies, workers who treat of human values in direct, simple and intelligible terms are the most useful members of the fraternity. But not to minds well informed of the progress of the sciences. To reach general laws it is usually necessary to abandon the straightforward terms of common sense, to become immersed for a time in mysterious symbols and computations, in technical and abstruse demonstrations, far removed from the common light of day, in order to emerge finally with a generalisation which may then be re-translated into the language of the workaday world.

Zealous humanitarians may be impatient for quick results. All men of goodwill may see without more ado that there is much amiss with the world. Should not social students postpone their abstruse intellectual problems, of fascination mainly to themselves, and get together in a sort of academic tea-party to list our known abuses and our known resources and arrive at a programme of reform on the basis of mutual goodwill? And do they not in fact, so the critic proceeds, bury themselves in unintelligible jargon, because they fear that, if they proceeded with their more immediate duties, they would disturb vested interests, incur social odium and signally fail to feather their own nests?

The criticism misconceives the duty of the student and the true source of his power for good. It may be the case that much could be put to rights without further scientific knowledge. But the sociologist will agree that if known abuses are not redressed it is not for lack of a catalogue of them, or even for lack of men of goodwill. He may not be able to formulate the sociological or psychological laws by which society is held in a fatal equilibrium of internecine hostility. But his experience will lead him to suspect that the equilibrium is not likely to be shattered by the breath of an academic tea-party. Nor have academic students a monopoly of goodwill or the power to express it.

Only in one way can the academic man change the shape of things, and that is by projecting new knowledge into the arena. In goodwill he may partake in greater or less degree along with more practical persons, and he is at liberty to join with them in political parties or social-welfare groups. His specific contribution is the enlargement of knowledge, and particularly of the knowledge of general laws. The task of the economist is rendered arduous by the intractable nature of the phenomena which he has to study; but he is better placed than other social students, and if he turn a deaf ear to cavillers, the past achievements of his subject and its present vitality may buoy him with a reasonable hope.

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IMPLICIT THEORIZING: A METHODOLOGICAL CRITICISM OF THE NEO-CAMBRIDGE SCHOOL

SUMMARY

I. Distinction between logical and methodological problems, 337. — II. Methodological pattern of a deductive theory, 339. — III. Implicit vs. explicit definitions, 342. — Fundamental limitations of implicit theories, 344. — IV. Examples of implicit theorizing: Joan Robinson's "corrected units," 346. — R. F. Kahn's theory of "ideal output," 347. — J. E. Hicks' generalized concept of elasticity of substitution, 348. — J. M. Keynes' concepts of aggregate supply and demand curves, 349.

I

Interest in methodological problems has so often been branded as a sign of theoretical frustration that the subject has practically vanished from the field of economic discussion. The silence which for a number of years surrounds this range of questions is so persistent as to become conspicuous. It is particularly notable in view of the fact that since the advent of the new Cambridge School, the methods of theorizing have undergone fundamental transformation.¹ Yet it would seem to be the form of analysis rather than the material content of his theories which imprint upon so many a prominent member of our scientific community the unmistakable mark of a "Cambridge economist." A show-down between the Cambridge (or rather the Neo-Cambridge) and the orthodox type of theory, if it ever comes, must be fought out on methodological grounds. The solution of the fundamental issue cannot be advanced through persistent but dispersed scimmages concerning such questions as equality of Saving and Investment, the significance of the so-called "Multiplier," and the like.

The difference between the two lines of thought appears to

1. It is interesting to note that the latest contribution to this field came not from a Cambridge Economist, but from the orthodox pen of Professor Lionel Robbins. Not less significant is the fact that no other than Professor Cannan was given the task of reviewing the "Nature and Significance of Economic Science" for *The Economic Journal*.

be neither a disagreement in final scientific outlooks, nor a divergence in identification of the immediate objects of observation — the economic realities of common experience. With both, the point of departure and the ultimate goal are essentially the same. What is different is the intermediate path.

From the point of view of Laplacian superhuman intelligence, which would be able to see without the least mental friction all the infinite number of logical implications of any given system of assumptions, however large — an intellect for which “logically necessary” is synonymous with “obvious” — the difference between the two paths would be no greater than that between two algebraic equations, one written in Latin script and the other in Greek, otherwise identical. But in the actual process of scientific investigation, which consists in its larger part of more or less successful attempts to overcome our own intellectual inertia, the problem of proper arrangement of formal analytical tools acquires fundamental importance. For a hypothetical person with absolute logical pitch, the choice of one or another type of definition, of one or another method of proof, would be entirely divorced from the analysis of the truth-content of any theoretical statement and for such a person questions concerning any kind of formal set-up in general cease to be of vital methodological importance. But for a limited human intellect the problem of choosing from among the infinite number of logically equivalent procedures that which reduces the chance of a logical mistake (inconsistency) to a minimum becomes *the* methodological problem of theoretical science. In course of the following argument, I shall adhere to this specific distinction between logical and methodological aspects of the analytical procedure, bearing in mind that the latter is definitely psychological in its nature.²

There exists no generally accepted criterion of the “sim-

2. The older, classical logic had a definite tendency to confuse these two different problems. With the advent of the new formalistic schools, the psychological or methodological, in our sense of the word, questions seem to have been driven into the background.

plicity" of a logical argument, because the nature of the mental friction which makes a theoretical proposition appear more or less "difficult" is different from one person to another. One economist finds it easier to understand five lines of algebraic formula than to follow five pages of verbal proof, while another would rather read twenty pages of text than to decipher a single equation. And still there exists a definite "statistical" correlation between different (but logically equivalent) patterns of theoretical analysis and the degree of mental resistance which arises in connection with their actual use. Multiplication of CLXXVIII and LXIX with exclusive use of Roman numerals would for a majority of people appear to be much more difficult than an equivalent calculation with the help of an Arabic system of numerals; and for a few exceptional minds the solution $178 \times 119 = 21,182$ is so obvious that they could dispense with any type of mathematical notation.

The degree of mental resistance which accompanies the use of one or another formal pattern is furthermore rather closely (altho also only "statistically") and positively correlated with the chance of committing logical mistakes. Mistakes of this kind may manifest themselves either in the inability to perceive the "evidence" of a correct argument or in the practically much more dangerous readiness to be convinced by a false one.

In the following pages, I attempt to indicate the fundamental characteristics which distinguish the logical pattern used by Cambridge economists from the formal set-up of the "orthodox" type of theories, and then try to show how and why the Cambridge pattern is liable to increase the danger of theoretical errors and fallacies. Finally, I shall substantiate my general contentions by discussing a few typical cases of implicit theorizing.

II

The outstanding characteristic of what for brevity will hereafter be referred to as the Cambridge pattern lies in a peculiar use of definitions.

Within the structure of any theoretical analysis, we can distinguish two elements. The first comprises a set of fundamental statements, which are introduced into the argument from outside and are not supposed to be scrutinized within the body of the given theory to any large extent, only so far as may be necessary to verify their logical compatibility. The source of these fundamental postulates may be direct observation; or they may be derived as the conclusions of some other theory; or they may be inductively unverifiable (i.e. normative) postulates. The nature of their origin, however, can in no way affect the position of these initial statements within a theoretical system. The logical implications of the "profit motive," within a system of economics, for example, will be the same whether we interpret it as an observable fact or only as an ideal, normative postulate.

The other part of a theory consists of a larger or smaller number of logical implications *obtained from* the primary set of fundamental propositions. The formal validity of all these different implications is absolutely equal, but their (psychological) evidentness is not at all uniform. Thus we can single out a class of implications which have immediate evidence, then a second class comprising statements which do not appear to be obvious from the point of view of the basic postulates, but are evident in relation to (i.e. can be directly derived from) those placed in the first class, and so on. The distinction between these successive classes of theorems (we shall use this term as synonymous with "deductively derived theoretical statement") will not necessarily be indicated with clearness by their distance from the set of fundamental postulates. If — as is mostly the case in the field of empirical sciences, which take many of their initial propositions from common experience — the number of these basic statements is very large, no *immediate* implications obtained by simultaneous use of *all* the given "data" are possible. In other words, while theorems of the first class can be derived by using only one part of these postulates, those of the second class might be derived from those of the first class plus some additional data and so on. Thus we face very often, not a

linear progress from the initial postulates toward implications of greater and greater remoteness, but an intricate network of methodological interconnections. Each particular theorem will frequently be derived from propositions of many different classes. For the purpose of the following discussion, the class (or "type") of a theorem may be defined as being one degree higher than that of the highest class of theorems used in its derivation. The possibility of choosing for the purpose of logical implication only a part of theorems belonging to a given class very often causes a branching of the argument into many different paths. These separate branches for obvious reasons are always logically compatible with each other.

The method of progressing gradually from stage to stage enables us to reach remote conclusions which lie far beyond the normal horizon of immediate logical perception. If, however, the scientist finds himself unable to use any concepts other than those contained in the set of initial fundamental propositions, his progress will be impeded by the increasing bulk of ever more and more complicated theorems. This burden would very soon become absolutely prohibitive, and is made bearable only by use of the powerful device of *intermediate definitions*. Like any other methodological tool, every such definition must first of all pass a test of logical validity. The introduction of an intermediate definition means, from a formal — logical — point of view, adaptation of a new, auxiliary postulate side by side with the initial set of basic propositions. It contains one *new* term (the defined term) and a number (at least two) of concepts already contained among the previously accepted postulates (these are the defining terms³). As the only formal test which has to be satisfied by an initial postulate is that of compatibility with the other simultaneously accepted postulates, every intermediate definition obtains its logical license automatically: Containing a new term, hitherto not used, such proposition can never be in contradiction with any of the other previously

3. From a purely formal point of view a definition could contain a single defining term. Methodologically, this type of definition, $A \equiv B$, cannot possibly be of any advantage. Hence it is actually never used.

accepted postulates and definitions. The familiar assertion that no definition can ever be logically wrong is incontestable; but definitions can be methodologically useless and often harmful.

In introducing an intermediate definition whenever his propositions become too cumbersome, the theorist is able to formulate the next stage of the argument with the same simplicity as the previous one. The economy is particularly great if the same theorem is used in many parallel branches of his argument, so that a new definition introduced at the point of division can be utilized in each of these separate lines of analysis.

So long as no logical mistake has been committed, any theorem expressed this way can be easily traced back to the original postulates. *The elimination of intermediate definitions at each and every stage of the argument can be accomplished by automatic substitution without raising any additional methodological difficulties.*

Skilful use of definitions enables the scientist to extend his deductive analysis to the remotest stages of implication, such as otherwise would be far beyond his mental reach. If not skilfully used, it often confuses his methodological coördination, leads him in circles and toward formal inconsistencies, i.e., open logical mistakes.

III

A typical methodological mistake of this kind can be characterized as the method of *implicit solutions*. Its logical pattern is simple. Given a number of compatible fundamental postulates expressed in terms of A, B, C . . . we can make, without infringing upon rules of logic, *any* other statement concerning the same elements A, B, C . . . provided we introduce into it at least one new term, X. The reason for this freedom lies in the fact that the new term can subsequently always be defined so as to make our additional statement compatible with the initial set of basic postulates. In other words, the initial postulates taken in conjunction with

the new theorem give an *implicit definition* of the term, X.

The profound methodological difference between an explicit and an implicit definition cannot be overemphasized. The second one presents a methodological problem of which the first is a solution. Error arises when it is assumed that the formulation of such an implicit theorem (i.e., a theorem containing implicitly defined terms) is methodologically equivalent to a complete solution of a given problem. It is true that the perfect Laplacian intellect could at once supply the necessary explicit definition which, if substituted in our implicit statement, would transform it into a desired explicit theorem. It could indeed go so far as to visualize unlimited series of intermediate substitution, each of which if applied "backward" would return us to the original set of fundamental postulates.

The question is whether an ordinary mind would be able to do the same. If it could, the intellectual effort used to perform the feat would be equal to that demanded by the use of orthodox methods. Most probably it would lose its bearings in the maze of intermediate definitions, and then, the task having been dropped short of its ultimate goal, all the intermediate results must be written off as a complete loss.

An implicit statement if not accompanied by a note describing in which direction it is supposed to be developed, can mean almost anything. The theorem $A = B$ includes, implicitly, the whole of economic theory. Defining A as product of a quantity of money and its velocity of circulation and B as the sum total of transactions, so we obtain the well-known monetary equation of exchange. If one of the two implicit terms, say A, is interpreted to denote the marginal revenue, and the other, B, the marginal costs, the same statement is transformed into the fundamental theorem of the theory of production.⁴

Ironically enough, those who most often use the method

4. This example might explain why it is comparatively easy to discover similarity between some propositions of economic theory and thermodynamic, electrodynamic, or other types of physical equations.

of implicit solutions very seldom undertake themselves the onerous task of explicit interpretations. They formulate a number of implicit theorems, extend the argument one or two steps forward or backward, and then let the reader find the way home by himself. If an uncautious critic ventures to express some doubts as to the "correctness" of the whole procedure, the short-cut theorist triumphantly points out that his implicit definition necessarily has *some* explicit meaning. If the critic follows a path of his own, makes a serious attempt to find his way out and gets lost, the theorist rightly but uncharitably accuses him of logical inconsistency and of inability to understand the correct meaning of the theorem. Scientific discussion degenerates into a comedy of errors and mistaken identities.

At first sight it might appear astounding that a great number of scientists, including the proponents of this procedure themselves, do not seem to notice the dangers and limitations of the described method. On the contrary, every new implicit leap into the theoretical unknown seems to elicit nothing but ever increasing admiration for the miraculously painless method of scientific progress and additional contempt for pedestrian pluggers of the more cautious school. The explanation of this uncritical attitude lies apparently in the fact that the methodological outlook of most theorists is still dominated by habits developed through the use of conservative patterns. Within the framework of an orthodox theory all definitions are explicit definitions and as such they cannot hide any unsolved problems. The superficial likeness which exists between explicit and implicit formulation induces unsuspecting minds to accept the latter with the same uncritical attitude as the former. Within the orthodox pattern, the progress of a theory could be judged, so to speak, by the position of its most advanced foreposts. Applying the same criterion to measure the progress of an implicit theory, we are easily impressed by finding its forces advanced far ahead of the regular army. The fact that these implicit scouting parties have no communication whatever with their own theoretical basis and thus are in the position of prisoners,

remains entirely unnoticed. Under these conditions a competitive debasement of theoretical standards becomes unavoidable.

The previous discussion makes it clear that it is the position of a statement within the given theoretical pattern — in particular its relation to the fundamental set of primary assumptions — which gives it the character of an implicit theorem. Specifically, it is the impossibility (without additional analytical efforts) of expressing the meaning of such a theorem in terms of these original assumptions.

Thus a short-cut theorist can always preclude the request for an explicit statement of any of his implicit propositions by expressly elevating the questionable theorem to the status of a fundamental postulate and by interpreting the undefined new term as an independent datum. The controversial issue acquires formally an entirely different aspect. The methodological task of finding the explicit meaning of an implicit proposition is now replaced by that of showing that the explicit theorem could be derived without making use of the additional postulate, i.e., of proving that the augmented set of fundamental assumptions has been made larger than is logically necessary. A problem remains unsolved.

The main difficulty in dealing with implicit theorizing is that it is impervious to logical criticism. The weakness of its short-cut methods consists not in formal mistakes but rather in the irrelevance and unconvincingness of the results obtained. If accused of not being able to find the explicit meaning of his own statements, an implicit theorist usually replies that such a demand is unreasonable. If, on the other hand, on consenting to elucidate a theory, he actually commits a logical mistake, he hardly will be ready to admit that this slip is the consequence of error in the methodological set-up.

One of the most effective devices used by the short-cut theorists consists in restating an explicitly derived proposition in implicit terms. The outward impressiveness of this kind of display is very great; its real significance is obviously nil.

IV

In the following paragraphs a few typical examples of *implicit analysis* are adduced. I do not entertain the hope of settling the methodological controversy: all that I wish to do is to present a few illustrations — circumstantial evidence of this kind of intellectual delinquency. The examples are chosen not on the basis of their material importance, but as typical instances of the Cambridge pattern of short-cut reasoning.

Corrected Units. The case of “corrected units” is particularly interesting for two reasons: first, it contains a perfect example of implicit definition, and second, the methodological futility of the procedure has been finally admitted with most commendable frankness by its author, Mrs. Joan Robinson.

The concept of efficiency unit was devised as a means of “simplifying” the theory of production by making all the physical production functions linear and all the physical marginal returns constant. The new corrected unit is defined as the physical quantity of any factor of production which, if added to any total quantity previously employed, would increase the output by the same fixed amount (measured in its natural, non-adjusted units). So, for example, if some particular production process described in original non-adjusted terms is subject to decreasing returns, each additional adjusted unit of the cost factor will contain more and more unadjusted units, the changing proportion being varied in such a way as to render its marginal productivity constant. The underlying idea is obviously that by some appropriate transformation of coördinates any manifold, however complicated, can be changed into another manifold of a simpler shape.

What Mrs. Robinson did not seem to have noticed while proposing the new concept is that all the mental energy which was saved by using the new simplified production function instead of the old one ought to have been spent in figuring out the appropriate transformation formula. As

mentioned above, in this particular instance, she realized her methodological error.⁵

Short-cut solution of theoretical problems with help of implicit transformations of coördinates has been repeatedly used by Cambridge economists when faced with intricacies of quantitative analysis (see, for example, Mr. J. M. Keynes' definition of "labor units" and "wage units" in the *General Theory of Employment*, pp. 41-44).

Ideal Output. The more intricate an analytical task appears to be, the greater is the temptation to tackle it with the apparatus of implicit analysis. One of the most difficult (very likely unsolvable) theoretical questions an economist has ever to face is the welfare problem.

Mr. R. F. Kahn⁶ begins his discussion of *ideal output* by assuming that "the price of any commodity (is) denotes its marginal utility" and that the "differences in the marginal utility of money to different people" do not exist. After having stated curtly that the analysis of these assumptions "belongs to a separate compartment of economics of welfare" he plunges into an elaborate discussion of the remaining problems without giving the discarded issue a second thought. The term which gives Mr. Kahn's theory an implicit character, is his concept of the "marginal utility of money." Mr. Kahn intimates that it presents an unsolvable problem by referring us to the "other department"; and when mentioning in the next passage the "average consumer," he even indicates the typical form in which so many an implicit concept enters economic theory — the form of an index number.⁷

The impasse thus created is not as harmless as in the case of Mrs. Robinson's adjusted units. All the efforts to solve the theoretical index number problem have hitherto been entirely unsuccessful. Very likely the concept can be shown

5. "I should like to take this opportunity of pointing out that the device suggested in my "Economics of Imperfect Competition" (p. 332) for getting over the difficulty by constructing "corrected natural units," is completely worthless, "Euler's Theorem. . . ." *The Economic Journal*, September, 1934, p. 402.

6. "Some Notes on Ideal Output," *Economic Journal*, March, 1935.

7. In tracking down implicit concepts, it is in general very useful to look first of all for "theoretical" index numbers.

to be fundamentally irrational. Mr. Kahn's implicit assumption would become in this case not only methodologically useless, but also logically false, and the entire structure of his theory of ideal output would have to be razed from top to bottom.

Elasticity of Substitution. The discussion centering around the elasticity of substitution gives an illuminating example of what an unsettling influence such an implicitly defined term is likely to have. Since Mr. Hicks first introduced this concept in his "Theory of Wages," no less than nine Cambridge and two orthodox economists have tried, in over twenty articles, notes and replies, to clarify and explain to each other its exact meaning. Now (in November, 1936) after three years of intensive discussion, Mr. Hicks asks himself, "What sense (if any) is left in our standard proposition⁸ . . .?" and finds the situation so precarious that he is already looking toward a "second line of hypothesis (?) in case the first gets us into difficulties." Following Mr. Hicks' ingenious attempts to catch the elusive "sense" of his own short-cut definition, it is interesting to note how easily his explanations end up in the emergence of new implicit theorems. Discussing "complication" number two (that of imperfect competition) he solves the difficulty by introducing a new parameter with a new name, the *measure of exploitation*. It is defined with the help of a new formula which indicates that the *elasticity of substitution*, multiplied by the *measure of exploitation*, gives the distribution ratio of the product between the two factors of production. "It seems, therefore, that we shall not get a very (?) wrong impression if we use our theory even under conditions of imperfect competition — provided we remember to ask the supplementary question, have any new opportunities emerged for exploitation — . . .?" Mr. Hicks does not appear to see that his method of solving the problem consists in replacing one implicit assumption by another.

8. The "standard proposition" is this: "an increase in the supply of a factor will increase the factor's share in the social dividend if the elasticity of substitution between it and the other resources employed is greater than unity." Review of Economic Studies, October, 1936, p. 10.

With equal logic he could put down another formula, according to which the distribution ratio would be equated to the ratio of the physical inputs of the two factors multiplied by a new parameter entitled, say, the *coefficient of redistribution*. He could even retain the final sentence as it stands, merely changing the phrasing of the "supplementary question" from "opportunities for exploitation" to "opportunities for redistribution."

I do not doubt that it is logically possible to find a definition of elasticity of substitution which (unlike the narrow technical interpretation) would make Mr. Hicks' standard proposition explicit, consistent and universally true at the same time. I doubt, however, whether an intellectual *tour de force* of that kind would yield results of greater theoretical significance than those which could be and have been already obtained, with much smaller loss of mental energy on the basis of orthodox methodological pattern.

Aggregate Supply and Aggregate Demand Functions. No list of implicit theories could be complete without mentioning Mr. Keynes' economic writings. It is the embarrassment of plenty in his case which makes the proper choice of an example particularly difficult. The new concept of aggregate supply and demand functions used in the General Theory of Employment presents an interesting instance of implicit treatment of the general equilibrium problem.

The classical concept of general equilibrium presupposes the existence of a great number of *independent data* (production functions, indifference functions, etc.) which simultaneously determine the quantities, prices and all other variables of the system. In putting forward a *specific* set of equations, the orthodox economist first makes an attempt to verify the logical consistency of his fundamental postulates, secondly, he expects to discover in this way some less obvious aspects of the price-quantity mechanism.

Mr. Keynes' implicit equations of aggregate supply and aggregate demand are removed a great number of steps from any basic assumption and data. Even so Mr. Keynes himself would hardly deny the obvious observation that both func-

tions depend upon an identical set of primary data, i.e., that they are fundamentally interdependent. Methodologically, these two functions are analogous to the distribution formula devised by Mr. Hicks for analysis of a not perfectly competitive situation. (See p. 348 above.) In one case, the implicit element consists of a *newly introduced term* — the coefficient of exploitation; in the other it is represented by the entirely *indefinite form* of the postulated relations.

Without imperilling the logical consistency or the theoretical usefulness of his statements, Mr. Keynes could interpret the D and C of the two equations not as the aggregate demand and aggregate supply, but as the total chicken food supply and total chicken food demand respectively: there obviously must exist *some* kind of relation between each of these two quantities and the total amount of employment, N. The "equilibrium value" of N could be defined by the point of intersection of these two curves—the point at which the total demand for chicken food would be equal to its total supply.

The methodological danger connected with the use of such implicitly defined concepts of aggregate supply and demand curves reveals itself as soon as an attempt is being made to apply them in the analysis of economic change. Discussing in one of the last chapters of his book the response of total employment to a given change in aggregate demand, Mr. Keynes makes this reaction dependent upon the shape — in particular, the elasticity — of the aggregate supply function. He obviously implies a situation in which one of the two functions "shifts" while the other retains its shape and position unchanged; a situation which is, because of the fundamental interdependence of the two functions, highly improbable not to say impossible. The methodological danger of potential misinterpretation, which is inherent in any implicit statement, here actually leads to an open logical inconsistency.

In choosing these examples from the writings of Cambridge economists, I do not wish to imply that theirs is the only school of economic thought which has indulged in this type of short-cut reasoning. A bare mention of the Austrian con-

cept of the "period of production" or of the Marxian concept of the "socially necessary labor" (used as the measure of value) would suffice to dispel any such impression. It would seem, however, that a procedure which in the work of other authors appears to be an occasional methodological lapse, becomes in the hands of a now conspicuous group of theorists the major analytical weapon.

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A RECONSIDERATION OF THE COBWEB THEOREM

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THE cobweb theorem has come to occupy an important place in recent formulations of static economic theory since it endeavors to demonstrate that even under highly simplified assumptions there is no certainty that price and output ultimately will adjust to the equilibrium represented by the intersection of the supply and demand curves. To be sure, the theorem has been held to apply only when certain requirements are fulfilled. These conditions have been summarized recently by Ezekiel as follows:

(1) Where production is completely determined by the producers' response to price, under conditions of pure competition (where the producer bases plans for future production on the assumption present prices will continue, and that his own production plans will not effect the market); (2) where the time needed for production requires at least one full period before production can be changed, once the plans are made; and (3) where the price is set by the supply available.¹

Providing these conditions are fulfilled, the cobweb theorem contends there are three distinguishable possibilities concerning adjustment of supply to demand consequent upon a shift of the demand or supply curve which disturbs the pre-existing equilibrium. For demand curve shifts there is first the case of perpetual oscillation in which the given supply curve and the new demand curve have equal slopes (Fig. 1); second, the case of divergent fluctuation in which the supply curve has a greater slope than the new demand curve (Fig. 2); and, third, convergent fluctuation in which the supply curve has a lesser slope than the new demand curve.² As is well known, according to the theorem

¹ M. Ezekiel, "The Cobweb Theorem," *Quarterly Journal of Economics*, LII (1938), 272. The third condition would seem to be implied in the assumption of pure competition.

² W. Leontief has shown that if the curves have unusual shapes—i.e., if they are not regular but change their slopes suddenly—it is possible to combine all three

only in one case, the third, will price and quantity ultimately come to rest at the intersection of the demand and supply curves. Hence it is argued that even under static assumptions an equilibrium price if disturbed by a shift in the demand curve (or of the supply curve) will not necessarily set in motion a series of adjustments leading to a new equilibrium.

It is the purpose of the present paper to examine more closely than seems to have been done heretofore the implications for the theorem of the assumptions concerning the nature of the supply curve employed in its exposition.

I

1. A supply curve purports to show what quantities of a given product will be forthcoming at various prices. And, under the assumption of competitive conditions, different supply curves for any given product will result according to the length of the time period over which the adaptation of quantity to price is assumed to take place. What properties are implicit in the supply curve as used in the cobweb theorem?

In the first place, the theory of cobweb oscillations induced by a shift in the demand curve presupposes throughout that, while output changes according to the assumption that producers always expect the price ruling in the last period to prevail in the next, the supply curve as such remains unaltered. In other words, the supply curve is completely reversible throughout its whole length with respect to each period. This means that, production having increased in one period over that of the previous period, it can contract again to its original, or any lower, level without complications arising from the fact that production had previously expanded. But a completely reversible supply curve with respect to each production period must mean that for each firm cost outlays in any one period are incapable of yielding valuable services in production beyond that one period. To assume otherwise is to assume away the reversibility of the supply curve which

“cases” in one pair of demand and supply curves (“Verzögerte Angebotsanpassung und partielles Gleichgewicht,” *Zeitschrift für Nationalökonomie*, V, Heft 5 [December, 1934], 670-76).

is taken for granted in, and is indispensable to, the cobweb analysis. This property of reversibility, or, alternatively, complete ease of adjustment to any anticipated price in the interval between production periods, imparts to the supply curve some of the characteristics of long-period competitive supply curves. For it means that the entry—or withdrawal—of firms and the expansion—or contraction—of output by established firms in response to any expected price requires an interval no longer than that between production periods—whatever that may be. Is not a further deduction possible, however? If, as indicated, cost outlays exhaust their service-rendering ability in each production period, each entrepreneur may recombine the factors of production completely afresh in planning his next “year’s” output on the assumption that the present price will hold. But if all expect the same price to rule in the ensuing period, and strictly competitive conditions prevail, then all will have identical average costs per unit and, should the anticipated price actually prevail, no producer would secure more, or less, than normal profits.³ In other words, the conclusion would seem logically to follow that a competitive supply curve, positively sloped and reversible throughout with respect to each production period, implies also an equality of average cost to (expected) price and as between firms.⁴

Having indicated certain characteristics of the supply curve as used in the cobweb theorem, we may pass to a consideration of certain implications of the theorem itself. And let us begin by assuming that the cobweb fluctuations are initiated by a shift in the demand curve.⁵

2. Taking first the case of perpetual oscillation, assume that in Figure 1 the original demand curve is dd with price and output in equilibrium at P_0 and Q_0 , respectively. If now the demand

³ Under completely competitive conditions the only differential in (expected) returns between entrepreneurs would be that reflecting the differences, if any, between their respective entrepreneurial abilities.

⁴ For competitive conditions to prevail, the number of firms, of course, must be large, but the number need not be constant for all points on the supply curve.

⁵ The alternative possibility of a shift in the supply curve is treated in Sec. II.

curve should shift to the left, represented by the new curve DD according to the theorem, price will fall to P_1 , since output is fixed for the time being at Q_0 . But if producers expect this lower price P_1 to prevail, they will produce quantity Q_1 , causing price to rise in the next period to P_2 . Output is then raised to Q_2 (equal to Q_0), causing price to fall again to P_3 (equal to P_1). The path is then retraced a second time, a third, and so on *ad infinitum* without equilibrium ever being reached.

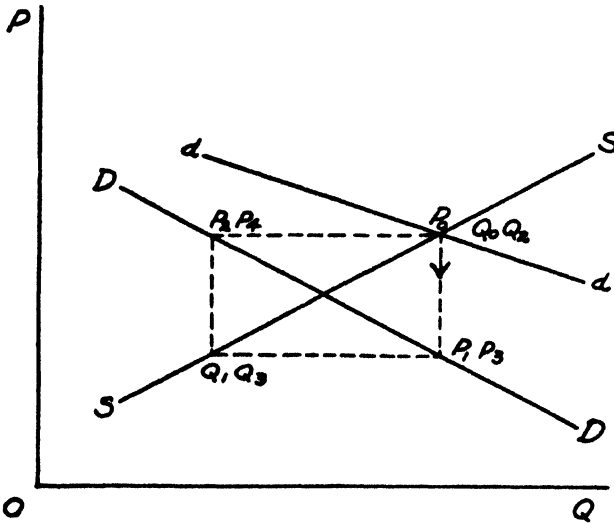


FIG. 1

Let us note a corollary of this continuous fluctuation of output and price. When the amount Q_2 is produced in anticipation of price P_2 , yet actually sold at P_3 , producers suffer aggregate losses of $Q_2(P_2 - P_3)$. When, however, they produce amount Q_3 in anticipation of price P_3 , they actually receive price P_4 and secure aggregate excess profits of $Q_3(P_4 - P_3)$. It is obvious at once, however, that aggregate losses in each "pair" of periods will always exceed aggregate excess profits by the amount $(P_2 - P_1)(Q_2 - Q_1)$. Hence producers always lose more in the interval of low prices than they secure in the period of high prices. This means that through time they will never succeed in "breaking even" by securing normal profits.

For the divergent case a similar conclusion follows. In Figure 2 let dd be the original demand curve and DD the new demand curve. The shift to the new demand curve causes price to rise from P_0 to P_1 , which in turn leads to output Q_1 , causing price to fall to P_2 . The subsequent prices are then P_3, P_4, P_5 , etc., and the quantities Q_2, Q_3, Q_4 , etc. Here again, however, the excess profits caused by a rise of price from P_0 to P_1 will be more than wiped out by the resulting losses when output increases to Q_1 and price

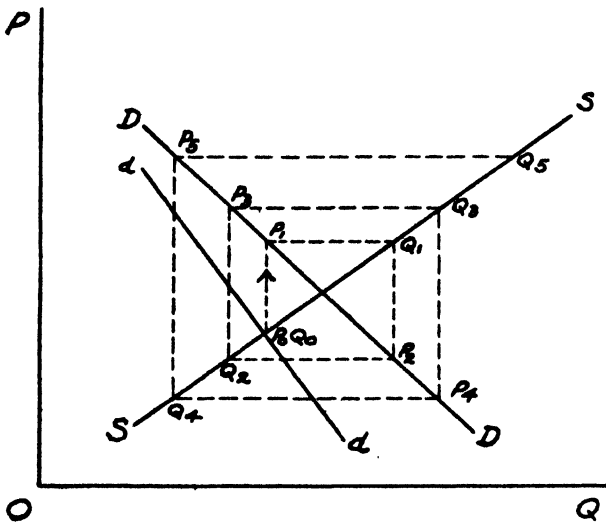


FIG. 2

falls to P_2 . For the divergent case aggregate losses will always exceed aggregate excess profits for each pair of periods: the average loss per unit is always greater and the number of units larger; hence aggregate losses will always be greater than aggregate excess profits.

It is easily shown that the same result follows, is reinforced in fact, if the initial change in demand is a movement of the curve downward and to the left. In this instance we merely start with losses instead of profits.

For the convergent case (Fig. 3) it is uncertain whether aggregate losses will exceed aggregate profits or not. If, as before, the new demand curve is DD , causing price to rise from P_0 to P_1 , producers will initially secure excess profits of $Q_0(P_1 - P_0)$. In the

succeeding period they will, to be sure, suffer losses in the amount $Q_1(P_1 - P_2)$, but there is no certainty that these will exceed the aggregate excess profits. If the demand curve is very elastic and the supply curve relatively inelastic, it is almost certain that profits will exceed losses. With this combination, moreover, equilibrium is closely approached after comparatively few adjustments.

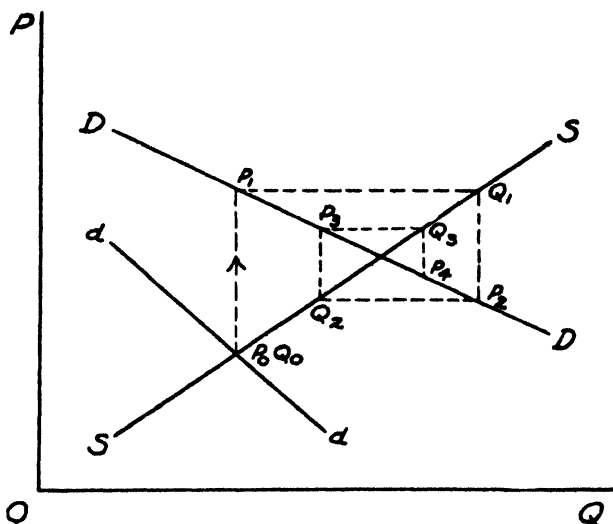


FIG. 3

If the initiating cause is a fall in demand instead of an increase, in Figure 3, the initial loss is greater than the immediately subsequent gain, and so for each following pair of periods. In this case, therefore, losses must again exceed gains.

Thus it would appear that with the exception of the convergent case moving toward equilibrium as a consequence of an increase in demand, the fluctuations of price and output postulated by the theorem necessarily involve producers in greater losses than gains as long as the cobweb path is traced.

Are not certain deductions possible from this conclusion, however?

3. We have already shown (sec. 1) that the supply curve employed in the cobweb theorem is reversible with respect to each period and that therefore each period's cost outlays exhaust their

productivity in that period. This being so, it means that in Figure 1, when price is P_3 and aggregate losses incurred by producers are $Q_2(P_2 - P_3)$, these losses are direct out-of-pocket losses. They are not such things as failure to cover depreciation, for instance. In the perpetual oscillation case (Fig. 1) producers in the aggregate suffer direct losses in each pair of periods of a net amount $(P_2 - P_1)(Q_2 - Q_1)$. In order for the cobweb path in Figure 1 to be retraced *ad infinitum*, one would have to assume that there is always a batch of new producers ready to rush in (and inevitably dissipate part of their capitals) as soon as prices such as P_2, P_4 , etc., prevail.⁶ The possibility of perpetual oscillation rests ultimately on the additional assumption, heretofore not expressly stated, that there is always a group of new producers willing to venture and lose their capitals in each pair of periods.⁷

It seems more reasonable to suppose, however, that the continuous losses and the regular exodus of firms would lead to a more sharply rising supply curve, i.e., the original *SS* curve would inevitably become more inelastic.⁸ But as *SS* becomes more in-

⁶ Producers whose capital has been depleted by a swing of the cobweb cycle might be assumed to borrow more or withdraw funds from alternative uses. Such replenishment of capital by established producers and the influx of new firms would together allow production to increase again.

⁷ There would seem to be an additional complication here. When price falls from P_0 to P_1 (Fig. 1), those producers already established lose a portion of their capital because of the out-of-pocket losses incurred. Now, are we to assume that those producers originally committed and some new producers together produce Q_1 , or that no new producers come in until Q_1 has been produced and sold at P_1 ? There seems no reason to suppose that new producers would be attracted by the losses when price is P_1 . Possibly, however, the assumed persistence of price P_1 is sufficient to induce new firms to enter.

⁸ The question might be raised as to why the *SS* curve is upsloping in any case. Ordinarily two possibilities are open: (1) what Professor Viner has called "Ricardian" increasing costs, which means some nonaugmentable factor, e.g., land, which is being used more intensively, or (2) external diseconomies of larger production (cf. J. Viner, "Cost Curves and Supply Curves," *Zeitschrift für Nationalökonomie*, Band III, Heft 1 [September, 1931]). Ricardian increasing cost, however, is scarcely compatible with the required assumption already indicated of the complete reversibility of the supply curve *SS* with respect to each period of gestation, although enough additional assumptions could perhaps be made to cover it. Otherwise, increasing cost presumably results from the necessity of bidding variable factors of production away from other employments, or from external diseconomies of large production.

elastic than DD , we have the convergency case of Figure 3. So also in Figure 2, SS would become more inelastic than DD and the divergent case would be converted into one of convergency. In other words there would be a movement toward equilibrium for the reason that the persistent excess of losses over profits in both Figure 1 and Figure 2 is unstable and must cause the supply curve to become more inelastic.⁹ The conclusion seems justified that if SS possesses its required properties, neither perpetual oscillation nor divergency could long persist. And these are the two cases which imparted interest to the cobweb theorem since there was ostensibly no movement toward equilibrium.

4. Let us consider briefly the possibility that the supply curve meant in the cobweb theorem is a short period supply curve where the number of producers is given and fixed.¹⁰ For any given (expected) price each producer will extend output until marginal cost is equal to it, unless such price be less than his least average direct cost when he will not produce at all. In other words, each point on such a supply curve represents the aggregate response of a "fixed" number of producers to anticipated prices when each firm extends output until marginal cost equals the expected price—a short-run equilibrium position for each and every firm. On these assumptions there is no requirement that average

⁹ Some readers may feel a little uneasy about the suggested conclusion that the supply curves will become more inelastic as a consequence of the persistent excess of direct out-of-pocket losses over gains. And the present writer confesses that he was himself bothered by it for some time. The source of irritation, however, is simply that it seems to introduce in a back-handed way the assumption that producers learn from experience; which is of course expressly excluded by the cobweb theorem. But after some mulling over the writer feels that to argue that the inevitable dissipation of producers' capitals leading to a curtailment of production and hence a more sharply rising supply curve is not the same as abandoning the assumption that producers expect the prevailing price to hold in the next period. In the first case producers do not change their expectations; they merely find it impossible to extend output as much as before by reason of the losses already incurred.

Of course if we allow some partial adaptation of output to anticipated price in the interval between production periods, i.e., a distinction between long-period and short-period supply curves, then the short-period curve will be less elastic and we get a gradual movement toward equilibrium, as Lange has shown. See *infra*, n. 18.

¹⁰ For reasons already given the case of a variable number of producers is incompatible with the reversibility of the supply curve except under the special conditions noted above.

cost be equal for all producers. The particular average costs of the individual firms when their marginal costs are all equal to the respective distances vertically downward from SS to the quantity axis may be indicated by a series or family of "particular expenses" or "entrepreneurs' differential cost curves."¹¹ In Figure 4 let SS be a short-period supply curve of the type described. Also let dd be the original demand curve such that price and quantity are in equilibrium at P_0 and Q_0 , the intersection

¹¹ "Particular expenses" or "entrepreneurs' differential cost curves" may be (and have been) interpreted in either a theoretical or an empirical sense.

In the theoretical sense used here for a fixed number of producers there is a particular expenses curve extending to the left from each point on the short-period supply curve and each particular expenses curve is constructed in the following manner: Competitive conditions being assumed, each producer will extend his output until his own marginal cost is equal to price. Hence, for each and every price there is a definable output for each producer; and the total output forthcoming at any given price is simply the summation of the outputs of the individual firms at that price. SS , a short-period supply curve for a fixed number of producers, is simply the locus of a point relating prices and total outputs. For any point on SS , therefore, the distance horizontally is the resulting total output when all the firms equate marginal costs and price, i.e., the distance vertically downward from that point. What of the particular expenses curve from that same point? It shows the consequences in terms of average costs to the individual firms which result when they individually extend output until their respective marginal costs are all equal to the given price. The firms' average costs are arranged in ascending order from left to right by a series of "thin parallelograms or thick straight lines." The line drawn through the tops of these thin rectangles is the particular expenses curve for the given point on the supply curve. Since for any other price each firm would produce a different output, and hence their average costs would be different, it therefore follows that there will be a definable "particular expenses" curve or "entrepreneurs' differential cost" curve from each point on the SS curve. Something of this kind, it seems to the writer, was what Marshall had in mind when he labeled his curve a "particular expenses" curve. Cf. *Principles* (8th ed.), pp. 810-12.

An entirely different version of what is often called a particular expenses curve has been in empirical studies that are necessarily backward-looking in character. Here the curve simply shows the array of average costs, as computed from, say, accounting records for some past period, of all those firms which contributed to the known total output of that period. "Cost" in this sense of course is a far less precise concept than that used in formal theory; and the resulting curves will be affected by the accounting policies of the firms in question. And, as has been pointed out to me, it is impossible "to move in a definite manner from the particular expenses curves [of this latter sort] to the long-run supply curves or vice versa without special assumptions."

It is the formal version of the particular expenses curve as described in the earlier paragraph that the writer wishes to employ here.

point of the two curves. Now let the demand curve shift to the new position DD , causing price to fall to P_1 which in the ensuing period calls forth output Q_1 sold at price P_2 , etc. The cobweb is then traced in the manner already indicated. What of profits and losses?

In Figure 4 let EE represent the entrepreneurs' differential cost curve before the shift in demand to DD . Whether or not profits will exceed losses under the perpetual oscillation of output from

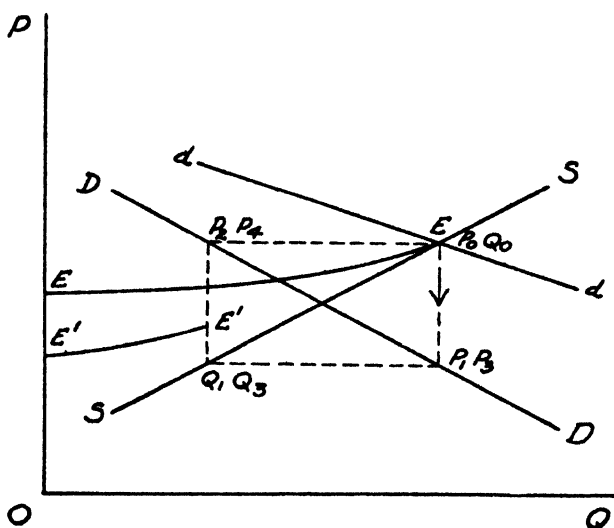


FIG. 4

Q_0 to Q_1 and of price from P_1 to P_2 will depend upon the position of the entrepreneurs' differential cost curve at the lower output, Q_1 . If at Q_1 most firms have marginal costs lower than average costs, then the differential cost curve will lie mainly above a horizontal line through P_1 . On the other hand, if EE lies substantially below a horizontal through P_0 , then $E'E'$ may still be below P_1 throughout most of its length. Clearly, whether profits exceed losses or not will depend upon the relative position of EE with regard to $E'E'$ when demand falls from dd to DD . While not certain, to be sure, it seems likely that a fall in demand is likely to impose aggregate net losses on producers in each pair of periods.

Conversely, if the cobweb oscillations are induced by an in-

crease in demand, then profits are more likely to exceed losses on each complete cycle. For, if most producers were operating beyond the point of least average total cost before the increase in demand, i.e., their respective marginal costs were greater than their respective average costs, then, for the larger output in response to the expected higher price, the differentials between marginal costs and average costs would be even greater.¹²

No great purpose is served, however, by introducing this version of the supply curve with the assumption of a fixed number of producers. For, if we retain the postulate of pure competition, the supply curve will be altered by the entry or withdrawal of firms, and the resulting oscillation will be modified accordingly.¹³

5. We have now considered the implication in terms of profits and losses of interpreting the supply curve in the cobweb theorem first as a long-period and second as a short-period curve with the number of producers given and fixed. What type of supply curve, however, have the expositors of the theorem had in mind? This is not always clear from the published versions.

Ricci's review of Moore's *Synthetic Economics* in 1930 was one of the first expositions of the cobweb diagrams. His curves, however, are explicitly the statistical demand and supply curves of the type developed in Moore's book.¹⁴ Tinbergen, in the same issue of the same journal, while primarily dealing with other matters, also introduced the cobweb diagrams.¹⁵ His supply curves seem to be quasi-long-period curves, although a misinterpretation here is possible. Kaldor's curves are definitely long-

¹² Each producer, of course, is assumed to extend output until his marginal cost equals the anticipated price.

¹³ If entry to the industry is restricted by other than a scarcity of ability and resources, conditions of pure competition do not prevail.

¹⁴ U. Ricci, "Die synthetische Ökonomie von Henry Ludwell Moore," *Zeitschrift für Nationalökonomie*, I, Heft 5 (April, 1930), 649-68, at 655-56. Moore's technique need not be described here, but see his *Synthetic Economics* (New York, 1929), esp. chap. iv and pp. 92-100, where he works out demand and supply curves for potatoes from statistics of production and farm prices.

¹⁵ J. Tinbergen, "Bestimmung und Deutung von Angebotskurven: Ein Beispiel," *Zeitschrift für Nationalökonomie*, I, Heft 5 (April, 1930), S. 669-79, at S. 670-71.

period curves.¹⁶ And Leontief states explicitly that he is using a Marshallian supply curve;¹⁷ while Lange specifically calls his supply curve a Marshallian long-period curve.¹⁸ Ezekiel's very recent and extended discussion of the cobweb theorem uses a supply curve which makes allowances for carry-overs from one period to the next, and although either interpretation is possible, his subsequent discussion suggests that his curve is a long-period curve.¹⁹

II

1. The same three-type cases of the cobweb theorem may equally well arise if the initial disturbance is a shift in the supply curve. And in the same manner the resulting cobweb depends upon the relative slopes of the new supply curve and the (unchanged) demand curve. In the matter of profits and losses the same general arguments as we have already advanced are applicable. If the supply curve moves to the left, then producers initially start with excess profits; while if it moves to the right, they begin the cycle with losses. In the convergent case these first losses will exceed any subsequent excess profits; but initial excess profits may or may not exceed subsequent losses, depending upon how steep the new supply curve is in relation to the demand curve. A shift in the supply curve does not alter our previous conclusion concerning the inevitability of losses exceeding profits for both divergency and perpetual oscillation. It seems unnecessary to offer diagrams for these obvious conclusions.

¹⁶ N. Kaldor, "A Note on the Determinateness of Equilibrium," *Review of Economic Studies*, I, No. 2 (February, 1934), 122-36, at 134-35. Kaldor seems to have christened the analysis the cobweb theorem.

¹⁷ *Op. cit.*, p. 670: "Gegeben ist eine Marshallsche Nachfragekurve und eine Angebotskurve. . . ."

¹⁸ O. Lange, "Formen der Angebotsanpassung und Wirtschaftliches Gleichgewicht," *Zeitschrift für Nationalökonomie*, VI, Heft 3 (August, 1935), 358-65, at 360. Presumably this means using the concept of the representative firm as Marshall did to develop or construct the supply curve.

¹⁹ *Op. cit.*, p. 260. He writes: "SS', however, is the supply curve for the quantity produced in the *next succeeding* period in response to price in the previous period" (italics in the original). Hence SS' might be either long period or short period.

2. One rather minor point in connection with shifts in the supply curve may be noted in passing.

If the cobwebs resulting from a shift in the supply curve are to be symmetrical with those following upon a shift in the demand curve, it is necessary to assume that the change inducing a new supply schedule exerts its full effect in the interval between two production periods. Not infrequently, however, a change in sup-

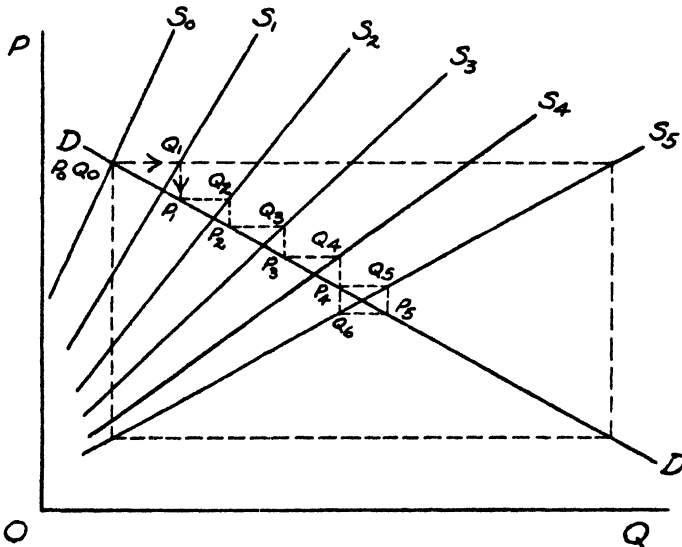


FIG. 5

ply conditions produces its complete results not immediately but only gradually. In other words, cases are conceivable where several production periods must elapse before the new supply schedule appropriate to the altered conditions is finally achieved; for instance, a technological improvement in the production of an agricultural product.²⁰ Before such final adjustment is reached, however, new supply curves, representing partial adaptations and relevant for one production period, come into existence and determine output: the supply curve moves by “jumps,” so to speak, from its old position to the new final position appro-

²⁰ This case must not be confused with one of Ezekiel's which traces the consequences of a several-periods lag in the response of output to price but with the supply curve unchanged (*ibid.*, pp. 266-71).

priate to the changed conditions. This type of adjustment gives different cobweb paths than if the adaptation of supply conditions to the technological change is complete and final between two production periods. In Figure 5 the difference is illustrated for the case of perpetual oscillation. If, as a consequence of changed supply conditions, the supply curve moves in the interval between two production periods from S_0 to S_5 , the cobweb resulting is the large rectangle indicated. If, however, several production periods must elapse before S_0 can shift to its position of final adaptation, say S_5 , and only gradually approaches it by steps such as S_1 , S_2 , S_3 , etc., then the ultimate perpetual oscillation will be via the much smaller rectangle $P_4Q_5P_5Q_6$. Clearly an approach to the new supply curve by degrees reduces the amplitude of the resulting perpetual oscillations of price and output. Since the adjustment of supply to altered conditions of production often—perhaps usually—requires several production periods, the conclusion indicated is not without interest. It also corresponds to what one would expect on a priori grounds.

A similar assumption with respect to cases of convergent and divergent oscillation does not alter their essential character: they remain convergent or divergent, but the cobweb paths traced out are different and sometimes quite fantastic. Any reader can easily graph such cases for himself.²¹

III

We may briefly summarize our conclusions. The cobweb theorem has been offered as a formal statement of a certain combination of conditions under which the displacement of equilibrium does not set in motion a chain of events leading to a new equilibrium of an enduring sort. If our analysis is valid, we have shown that neither perpetual fluctuation at a given amplitude nor expanding fluctuation is theoretically possible if the supply

²¹ I suppose that one could assume that demand curves also undergo a gradual change and thereby affect the cobweb paths. But the obstacles to a complete and final adjustment of supply in the interval between production periods have no counterpart in demand; which is not to say, of course, that demand always changes abruptly and never gradually, but only that the impediments to change are less significant economically.

curve is a competitive supply curve such as most writers apparently had in mind in their exposition of the doctrine. We have sought to show that losses will inevitably exceed profits and that the required reversibility of the supply curve implies that these losses are direct out-of-pocket losses. On the special assumption that there is always a group of new producers willing to rush in and dissipate their capitals with each swing of the cycle, the theorem may perhaps be valid. This special case needs to be made explicit, however, and probably requires further assumptions. For the short-period curve with a fixed number of producers the conclusion as to profit or loss is partially indeterminate; but by definition such a curve is only an intermediate and not a final equilibrium adjustment under competition. Thus, for those empirically established fluctuations in output and price it is doubtful if the cobweb theorem provides a logically acceptable explanation for the reasons indicated.

Finally, one can scarcely resist emphasizing that it is difficult, nay almost impossible, to discover industries in real life which even roughly correspond to the very rigid conditions necessary for the cobweb oscillations. There is first the not easily attained requirement of pure competition. More difficult to approximate in real life, however, is the assumption of the complete inability of producers to alter final output once they have made their plans and commitments: fruit rotting on the trees and potatoes in the ground is a not unfamiliar sight. Moreover, almost all agricultural commodities are produced under technological conditions such that the final output from given input is at least partially unpredictable because of uncontrollable weather and climate factors: certainly crop yields vary a good deal in any one region from year to year. Last, the inviolable assumption that people never learn from experience, no matter how protracted, is at least debatable.

A REFORMULATION OF CERTAIN ASPECTS OF WELFARE ECONOMICS¹

SUMMARY

Assumptions, 310.— I. General conditions for maximum welfare, 311.— II. The Lerner conditions, 316; the Pareto-Barone-Cambridge conditions, 318; the Cambridge conditions, 320. III. Review and comparison of the relevant points of the various expositions, 323.—IV. The sign of dE , 330.

The object of the present paper is to state in a precise form the value judgments required for the derivation of the conditions of maximum economic welfare which have been advanced in the studies of the Cambridge economists,² Pareto and Barone, and Mr. Lerner.³ Such a formulation, I hope, will clarify certain aspects of the contribution of these writers, and at the same time provide a basis for a more proper understanding of the principles of welfare.

I shall develop my analysis under a set of assumptions which in certain respects differ from those introduced in the welfare studies. It will be assumed throughout the discussion that the amounts of all the factors of production, other than labor, are fixed and, for convenience, non-depreciating. While a variable capital supply is included in some of the

1. I am very grateful to Mr. Paul Samuelson for suggestions on many points.

2. I use this caption to designate those economists whose names are directly attached to the Cambridge School — Marshall, Professor Pigou, Mr. Kahn — as well as others, such as Edgeworth, whose welfare analysis is in all essentials the same as that of the Cambridge group. But in the course of my discussion I shall refer mainly to the studies of the first group of economists. This will ease my task considerably, and, I believe, will involve no loss of generality.

3. The studies referred to are Marshall, *Principles* (all references to the Third — 1895 — Edition); Pigou, *Economics of Welfare* (all references to the Fourth — 1932 — Edition); Kahn, *Economic Journal*, March, 1935; Pareto, *Cours d'Economie Politique* (all references to the Lausanne — 1897 — Edition); Barone, *The Ministry of Production in a Socialist State* (Translated from the Italian article of the same title in *Giornale degli Economisti*, 1908; the translation appearing in Hayek, *Collectivist Economic Planning*); and Lerner, *Review of Economic Studies*, June and October, 1934.

welfare studies, this is not a well developed part of the analysis, and for our present purposes it will be desirable to confine to the simpler case the discussion of the evaluations required.⁴ I shall assume, also, that the variables involved in the analysis — the amounts of the various commodities consumed and services performed — are infinitesimally divisible. This assumption will be interpreted more strictly than is usually done. Otherwise it is the postulate of the welfare writers, and its introduction here will involve no significant departure from their analysis. Finally, I shall assume that there are only two kinds of consumers' goods, two kinds of labor, and two factors of production other than labor in the community, and that each commodity is produced, with labor and the other factors, in a single production unit. This assumption is introduced only to simplify the notation employed. The discussion will apply, with no modification, to the many commodity, many factor, and many production unit case.⁵

I

Among the elements affecting the welfare of the community during any given period of time are the amounts of each of the factors of production, other than labor, employed in the different production units, the amounts of the various commodities consumed, the amounts of the different kinds of work done, and the production unit for which this work is performed by each individual in the community during that period of time. If we use *A* and *B* to denote the two kinds of labor; *C* and *D* to denote the two factors

4. On a simple model, similar to that of Barone, the analysis may be extended to the case of a variable capital supply.

5. The assumption that each commodity is produced in one production unit, it is true, excludes an element of "external economies" from the analysis. But in the present essay I am interested only in the maximum conditions for the community's welfare, and not in the departures from the maximum under a given institutional set-up. To the extent that, in the many production unit case, there are external economies, these will require no modification in the maximum conditions I shall present, for these conditions relate only to marginal *social value* productivities.

of production other than labor; and X and Y to denote the two consumers' goods; we may express this relationship in the form

$$(1.1) \quad W = W(x_1, y_1, a_1^x, b_1^x, a_1^y, b_1^y, \dots, \\ x_n, y_n, a_n^x, b_n^x, a_n^y, b_n^y, C^x, D^x, C^y, D^y, r, s, t, \dots).$$

Here C^x and D^x are the amounts of the non-labor factors of production C and D employed in the production unit producing the consumers' good X ; C^y and D^y are the amounts of these factors employed in the production unit producing the consumers' good Y ; x_i and y_i are the amounts of X and Y consumed by the i^{th} individual; and a_i^x , b_i^x , a_i^y , and b_i^y are the amounts of each kind of work performed by him for each production unit during the given period of time.⁶ The symbols r, s, t, \dots , denote elements other than the amounts of commodities, the amounts of work of each type, and the amounts of the non-labor factors in each of the production units, affecting the welfare of the community.

Some of the elements r, s, t, \dots , may affect welfare, not only directly, but indirectly through their effect on (say) the amounts of X and Y produced with any given amount of resources, e.g., the effects of a change in the weather. On the other hand, it is conceivable that variations in the amounts of commodities, the amounts of work of each type, and the amounts of non-labor factors in each of the production units also will have a direct and indirect effect on welfare; e.g., a sufficient diminution of x_i and y_i may be accompanied by an overturn of the government. But for relatively small changes in these variables, other elements in welfare, I believe, will not be significantly affected. To the extent that this is so a partial analysis is feasible.

I shall designate the function,

$$(1.2) \quad E = E(x_1, y_1, a_1^x, b_1^x, a_1^y, b_1^y, \dots, \\ x_n, y_n, a_n^x, b_n^x, a_n^y, b_n^y, C^x, D^x, C^y, D^y),$$

which is obtained by taking r, s, t, \dots , in (1.1) as given, the Economic Welfare Function.⁷

6. I am assuming that an individual's labor time may be divided among the different types of work in any desired proportions.

7. It should be emphasized that in (1.2) other factors affecting wel-

Let us write the amounts of X and Y produced respectively by the X and Y production units as functions,

$$(1.3) \quad X = X(A^x, B^x, C^x, D^x); \quad Y = Y(A^y, B^y, C^y, D^y),$$

where A^x and B^x are the amounts of the two kinds of labor and C^x and D^x are the amounts of the other two factors of production employed in the X production unit; and A^y, B^y, C^y, D^y are defined similarly for the Y production unit.

If we assume that E varies continuously with x_1, y_1, \dots , we may write as a general condition for a position of maximum economic welfare that, subject to the limitations of the given technique of production and the given amounts of resources,

$$(1.4) \quad dE = 0.$$

Equation (1.4) requires that in the neighborhood of the maximum position any small adjustment will leave the welfare of the community unchanged. By use of (1.3) and (1.4) it is possible immediately to state in general terms the conditions for a maximum welfare.⁸

One group of maximum conditions relates to the consumption and supply of services by each individual in the community. They require that the marginal economic welfare of each commodity and the marginal economic dis-welfare of each type of work be the same with respect to each individual in the community.⁹ If we denote the marginal economic welfare of commodity X with respect to the i^{th} individual, $\frac{\partial E}{\partial x_i}$, and of Y , $\frac{\partial E}{\partial y_i}$, the first group of these con-

ditions are taken as given. I do *not* assume that economic welfare is an independent element which may be added to other welfare to get total welfare.

8. The conditions I shall develop in this section are a group of necessary conditions for a maximum. They are also the conditions for any critical point, and are sufficient in number to determine the location of such a point (or points) if there is one. In section IV below I shall consider the problem of determining whether a given critical point is a maximum or not.

9. This rather awkward terminology is adopted instead of, say, the phrase marginal economic welfare of the i^{th} individual in order to include the possibility that an increment of X or Y given to the i^{th} individual will affect the welfare of others.

ditions requires that, for all i , and for some p , q , and ω ,

$$(1.5) \quad \frac{\partial E}{\partial x_i} = \omega p$$

and

$$(1.6) \quad \frac{\partial E}{\partial y_i} = \omega q.$$

Similarly if we denote the marginal economic diswelfare of the various types of work with respect to the i^{th} individual $\frac{\partial E}{\partial a_i^x}$, $\frac{\partial E}{\partial b_i^x}$, $\frac{\partial E}{\partial a_i^y}$, $\frac{\partial E}{\partial b_i^y}$, the second group of these conditions requires that, for all i and for some g^x , h^x , g^y , h^y , and for the ω already chosen,

$$(1.7) \quad -\frac{\partial E}{\partial a_i^x} = \omega g^x, \quad (1.8) \quad -\frac{\partial E}{\partial b_i^x} = \omega h^x,$$

$$(1.9) \quad -\frac{\partial E}{\partial a_i^y} = \omega g^y, \quad (1.10) \quad -\frac{\partial E}{\partial b_i^y} = \omega h^y.$$

The minus signs and the multiplicative factor ω are inserted in these equations for convenience.

The remaining maximum conditions relate to production. They require that the economic welfare of the consumers' goods produced by a marginal increment of each type of work should equal the negative of the diswelfare of that increment of work, and that the increment of economic welfare due to the shift of a marginal unit of factors C and D from one production unit to another should equal the negative of the diswelfare caused by this adjustment. Using the notation $\frac{\partial X}{\partial A^x}$ for the marginal productivity of A^x , and a sim-

ilar notation for the other marginal productivities, we may write these conditions in the form,

$$(1.11) \quad p \frac{\partial X}{\partial A^x} = g^x, \quad (1.12) \quad p \frac{\partial X}{\partial B^x} = h^x,$$

$$(1.13) \quad q \frac{\partial Y}{\partial A^y} = g^y, \quad (1.14) \quad q \frac{\partial Y}{\partial B^y} = h^y,$$

and,¹

$$(1.15) \quad \omega \left(p \frac{\partial X}{\partial C^x} - q \frac{\partial Y}{\partial C^y} \right) = - \left(\frac{\partial E}{\partial C^x} - \frac{\partial E}{\partial C^y} \right),$$

$$(1.16) \quad \omega \left(p \frac{\partial X}{\partial D^x} - q \frac{\partial Y}{\partial D^y} \right) = - \left(\frac{\partial E}{\partial D^x} - \frac{\partial E}{\partial D^y} \right).$$

In equations (1.11) through (1.14), ω , which was present in all terms, has been divided out.²

It will be convenient to designate p the *price* of X , q the *price* of Y , and g^x, g^y, h^x, h^y , the *wage* of the types of work A^x, A^y, B^x, B^y . Equations (1.5) and (1.6) thus require that the marginal economic welfare per "dollar's worth" of each

commodity, $\frac{\partial E}{\partial x_i} \cdot \frac{1}{p}$ and $\frac{\partial E}{\partial y_i} \cdot \frac{1}{q}$, be the same for each com-

modity and for all individuals in the community. Similarly equations (1.7) through (1.10) require that the marginal economic diswelfare per "dollar's worth" of each kind of work be the same with respect to each kind of work and each individual in the community; equations (1.11) through (1.14) require that the wages of each type of labor should equal the marginal value productivity of that type of labor;³ and with an analogous interpretation, equations (1.15) and (1.16) require that the marginal value productivity equal the cost due to a shift in C or D from one use to another.

II

The maximum conditions presented in section I are the general conditions for a position of maximum economic

1. The derivatives on the right hand sides of (1.15) and (1.16) indicate the effect on welfare of an adjustment in C or D for which all other elements — x^i, y^i , etc. — in welfare are constant. Such an effect would arise, for example, through a positive or negative evaluation of the relative amounts and kinds of "factory smoke" emitted in the two production units for varying amounts of one or the other factors employed in each unit.

2. Strictly speaking this procedure assumes a value proposition, which we shall introduce later, to the effect that ω is unequal to zero.

3. In the present essay it will be understood that all value productivities are *social* value productivities. Compare footnote 5, p. 311, *supra*.

welfare for any Economic Welfare Function. The maximum conditions presented in the welfare studies relate to a particular family of welfare functions. Their derivation thus requires the introduction of restrictions on the shape of the Economic Welfare Function I have presented. Three groups of value propositions suffice for this purpose.

I shall designate the various maximum conditions derived by the names of those writers, or groups of writers, who have been especially responsible for their elucidation. For reasons which will appear I have altered somewhat the content of the conditions, and there are differences in the analyses of the various writers which must also be noted. The latter differences will be pointed out in this section and in the one following.

THE LERNER CONDITIONS

The First Group of Value Propositions: *a shift in a unit of any factor of production, other than labor, from one production unit to another would leave economic welfare unchanged, provided the amounts of all the other elements in welfare were constant.*

The First Group of Value Propositions enables us to state certain of the maximum conditions in terms of the production functions alone. From these evaluations the right hand side of (1.15) and of (1.16) must equal zero.⁴ The two equations thus may be written,

$$(2.1) \quad p \frac{\partial X}{\partial C^x} = q \frac{\partial Y}{\partial C^y},$$

$$(2.2) \quad p \frac{\partial X}{\partial D^x} = q \frac{\partial Y}{\partial D^y},$$

and they now impose the condition that the marginal value productivity of factors other than labor be the same in every use.

Equations (2.1) and (2.2) still contain the variables p

4. The net effect on the community's welfare of the "factory smoke" arising from a shift of the non-labor factors from one use to another is zero. (Cf. footnote 1, p. 315.)

and q , which involve derivatives of the Economic Welfare Function. If we combine (2.1) and (2.2), however, we have two equations,

$$(2.3) \quad \frac{q}{p} = \frac{\partial X}{\partial C^x} / \frac{\partial Y}{\partial C^y} = \frac{\partial X}{\partial D^x} / \frac{\partial Y}{\partial D^y},$$

the second of which involves only the derivatives of the production functions. It requires that in the maximum position the ratio of the marginal productivity of a factor in one use to its marginal productivity in any other use be the same for all factors of production, other than labor. The first equation of (2.3) requires that all these ratios equal the price ratio.

The significance of (2.3) for the determination of maximum welfare may be expressed in the following manner: whatever the relative evaluations of commodity X and commodity Y , that is, in Barone's terminology, whatever their ratio of equivalence, (2.3) requires that in the maximum position given that one factor C is so distributed that a small shift from one production unit to another would alter the amounts of X and Y in such a manner as to leave welfare unchanged,

i.e., given that C is so distributed that $\frac{\partial X}{\partial C^x} / \frac{\partial Y}{\partial C^y}$ equals the ratio of equivalence of the two commodities, then the other factors in order to be so distributed must have a ratio of marginal productivities equal to $\frac{\partial X}{\partial C^x} / \frac{\partial Y}{\partial C^y}$.

The condition (2.3) can be interpreted in another manner, which however does not bring out as directly the significance of the condition for a position of maximum *welfare*. The equality of the marginal productivity ratios implies that there is no possible further adjustment for which the amount of one commodity will be increased without that of another being reduced. A shift in one factor from X to Y can at best be just compensated by a shift of another from Y to X , if (2.3) is satisfied.⁵

5. Mr. Lerner, as far as I am aware, is the only economist to present

THE PARETO-BARONE-CAMBRIDGE CONDITIONS

The Fundamental Value Propositions of Individual Preference: *if the amounts of the various commodities and types of work were constant for all individuals in the community except any i^{th} individual, and if the i^{th} individual consumed the various commodities and performed the various types of work in combinations which were indifferent to him, economic welfare would be constant.*

The First Group of Value Propositions implies that under the assumption that the amounts of the factors of production other than labor are constant, the Economic Welfare Function may be written as

$$(2.4) \quad E = E(x_1, y_1, a_1^x, b_1^x, a_1^y, b_1^y, \dots, \\ x_n, y_n, a_n^x, b_n^x, a_n^y, b_n^y).$$

For from these propositions a shift in C or D from one production unit to another would have no effect on welfare, if all the other elements were constant. The Fundamental Value Propositions require that E be some function of the form,

$$(2.5) \quad E = E[S^1(x_1, y_1, a_1^x, b_1^x, a_1^y, b_1^y), \dots, \\ S^n(x_n, y_n, a_n^x, b_n^x, a_n^y, b_n^y)],$$

where the function,

$$(2.6) \quad S^i = S^i(x_i, y_i, a_i^x, b_i^x, a_i^y, b_i^y),$$

and interpret (2.1) and (2.2) in the form of (2.3), his interpretation being the second of the two alternatives I have noted. In the studies of Pareto, Barone, and Marshall the conditions (2.1) and (2.2) are presented with the price ratios already equated to the individual marginal rates of substitution (cf. *infra*). In the studies of Professor Pigou and Mr. Kahn the procedure is the same as that of Pareto, Barone, and Marshall except that these two writers include in their analysis the possibility of departures from (2.1) and (2.2) due to such effects as are discussed above in footnote 1, p. 315.

Mr. Lerner advances the conditions (2.3) for all factors of production, labor as well as non-labor (Review of Economic Studies, October, 1934, p. 57). On the face of the matter this formulation is inconsistent with Mr. Lerner's own advocacy of the supremacy of individual tastes in the sphere of consumption, and I have therefore taken the liberty to modify his conditions accordingly. The other economists also do not allow for individual preferences as between production units in their analysis.

expresses the loci of combinations of commodities consumed and work performed which are indifferent to the i^{th} individual.

The Fundamental Value Propositions enable us to state all the consumption and labor supply conditions in terms of the individual indifference functions, S^i , as ratios of (1.5), or of any other of their number. For consider the equation,

$$(2.7) \quad \frac{\partial E}{\partial x_i} \bigg/ \frac{\partial E}{\partial y_i} = \frac{p}{q},$$

obtained from (1.5) and (1.6) by division. Using the Fundamental Value Propositions,

$$(2.8) \quad \frac{\partial E}{\partial x_i} \bigg/ \frac{\partial E}{\partial y_i} = \frac{\partial E}{\partial S^i} \frac{\partial S^i}{\partial x_i} \bigg/ \frac{\partial E}{\partial S^i} \frac{\partial S^i}{\partial y_i} = \frac{\partial S^i}{\partial x_i} \bigg/ \frac{\partial S^i}{\partial y_i}.$$

The last ratio in (2.8) is one of the slopes of the indifference locus of the i^{th} individual, or in the Hicks and Allen terminology, the marginal rate of substitution of commodity Y for commodity X .⁶ Thus (2.7) requires that the marginal rate of substitution of the two commodities be the same for all individuals. By successively combining (1.5) with equations (1.7) through (1.10), the same result is obtained with respect to the other elements of welfare.

All the production conditions may now be stated in terms of the indifference functions and the production functions. For equations (1.11) through (1.14), the statement that the wage of each type of work should equal the marginal value productivity of that type of work may be interpreted to mean that the marginal product of a given type of work employed in producing a given commodity should equal the marginal rate of substitution of that commodity for that type of work. In the same manner conditions (2.2) not only require that the ratios of marginal productivities of the various factors other than labor be equal, but that these ratios should equal the marginal rate of substitution of the two commodities.

The Fundamental Value Propositions thus require that,

6. Cf. *Economica*, February, 1934.

whatever the ratios of equivalence between the various commodities and types of work, given that the types of work performed and commodities consumed by one individual are so fixed that for any small adjustment among them economic welfare is unchanged, i.e., given that the marginal rates of substitution and marginal productivities for this individual equal the respective ratios of equivalence, then for all other individuals to be similarly situated, their marginal rates of substitution must be the same as those of this individual. Under our implicit assumption of homogeneous factors, the respective marginal productivities of course must in any case be equal for all individuals.

Again the Fundamental Value Propositions may be interpreted also to mean that in the maximum position it is impossible to improve the situation of any one individual without rendering another worse off.⁷

THE CAMBRIDGE CONDITIONS

Let us designate

$$(2.9) \quad m_i = px_i + qy_i - g^x a_i^x - h^x b_i^x - g^y a_i^y - h^y b_i^y,$$

7. The Pareto-Barone-Cambridge Conditions are developed by Marshall in the Principles (pp. 413-415, 526-527; Append. XIV), but the derivation of the production conditions is based upon the very simple illustrative assumption of a producer-consumer expending his capital and labor in such a manner as to maximize his utility. Under more general assumptions the conditions are developed, without the utility calculus used by Marshall, by Pareto (Cours, Vol. I, pp. 20ff., Vol. II, pp. 90ff.) and Barone (Ministry of Production), and with the utility calculus, by Professor Pigou (Economics of Welfare, particularly pp. 131-143) and Mr. Kahn (Economic Journal, March, 1935). All of these writers either develop the consumption conditions independently of their formulation of the production conditions (Marshall, Pareto) or assume the consumption conditions *ab initio* (Barone, Pigou, Kahn); and, as we shall indicate, the interpretations vary. Mr. Lerner in his study in the Review of Economic Studies, June, 1934, presents all the conditions together, and interprets them most lucidly in the second of the two senses we have pointed out.

As I have noted elsewhere (footnote 5, p. 317) none of these writers includes in his analysis individual preferences between production units. Also, Professor Pigou and Mr. Kahn include the possibility of departures from (2.3), and perhaps from (1.11), (1.12), (1.13), (1.14), for the direct effects on welfare of shifts of the factors of production from one use to another.

the Share of the i^{th} individual. In (2.9), $p, q, \text{etc.}$ are taken proportional to the respective marginal rates of substitution. Thus m^i is defined, aside from a proportionality factor. The sum of m^i for the community as a whole is equal to the difference between the total wages and the total value of consumers' goods in the community.

The Propositions of Equal Shares: *If the Shares of any i^{th} and k^{th} individuals were equal, and if the prices and wage rates were fixed, the transfer of a small amount of the Share of i to k would leave welfare unchanged.*

The Propositions of Equal Shares enable us to state in terms of the distribution of Shares the remaining condition (1.5). According to these evaluations, if the Shares of i and k are equal, then for the price-wage situation given,

$$(2.10) \quad dE = \frac{\partial E}{\partial m_i} dm_i + \frac{\partial E}{\partial m_k} dm_k = 0,$$

for $dm_i = -dm_k$. Equation (2.10) is equivalent to the condition imposed by (1.5) that the marginal economic welfare per "dollar's worth" of X is the same for i and k .⁸ Thus if the Shares of all individuals are equal, the condition (1.5) is satisfied.⁹

8. The proof is as follows:

$$\frac{\partial E}{\partial m_i} = \frac{\partial E}{\partial x_i} \frac{\partial x_i}{\partial m_i} + \frac{\partial E}{\partial y_i} \frac{\partial y_i}{\partial m_i} + \frac{\partial E}{\partial a_i^x} \frac{\partial a_i^x}{\partial m_i} + \frac{\partial E}{\partial b_i^x} \frac{\partial b_i^x}{\partial m_i} + \frac{\partial E}{\partial a_i^y} \frac{\partial a_i^y}{\partial m_i} + \frac{\partial E}{\partial b_i^y} \frac{\partial b_i^y}{\partial m_i}$$

By (2.9)

$$1 = p \frac{\partial x_i}{\partial m_i} + q \frac{\partial y_i}{\partial m_i} - g^x \frac{\partial a_i^x}{\partial m_i} - h^x \frac{\partial b_i^x}{\partial m_i} - g^y \frac{\partial a_i^y}{\partial m_i} - h^y \frac{\partial b_i^y}{\partial m_i}.$$

Using this equation (2.7), and similar equations for the commodities and services,

$$\frac{\partial E}{\partial m_i} = \frac{\partial E}{\partial x_i} \cdot \frac{1}{P}.$$

9. Among the welfare studies the Cambridge Conditions are the distinctive characteristic of the writings of the members of the Cambridge School. They are advanced in the works of all the Cambridge economists, and in none of the other welfare studies we have considered. But certain qualifications must be noted.

The Cambridge economists require an equal distribution of incomes, ($px_i + qy_i$), rather than of Shares as the condition for equality of the marginal economic welfare per "dollar" for all individuals (with quali-

The three groups of value propositions are not only sufficient for the derivation of the maximum conditions presented in the welfare studies. They are necessary for this procedure. For it is possible, and I shall leave the development of the argument to the reader, to deduce from the maximum conditions presented the restriction imposed upon the Economic Welfare Function by the value judgments introduced.

But it should be noted that the particular value judgments I have stated are not necessary to the welfare analysis. They are essential only for the establishment of a particular group of maximum conditions. If the production functions and individual indifference functions are known, they provide sufficient information concerning the Economic Welfare Function for the determination of the maximum position, positions which we shall note directly, cf. Kahn, *Economic Journal*, March, 1935, pp. 1, 2; Pigou, *Economics of Welfare*, pp. 82ff.; Marshall, *Principles*, p. 795). If it is assumed that the amounts of the various types of labor performed by each individual in the community are given, this condition is of course the same as ours. But otherwise for a requirement of equal incomes there is unlikely to be any position which satisfied all the conditions for a maximum. For it would be necessary that in the neighborhood of the maximum position the marginal productivity and marginal diswelfare of each type of work be zero.

[The condition of equal incomes is not necessarily inconsistent with the other postulates. There might be some indifference functions and production functions such that all the maximum conditions are satisfied. But it may be noted here, in general, as a minimum requirement that the various conditions must be consistent with each other. Compare Lange, *Review of Economic Studies*, October, 1936, pp. 64, 65, and Lerner, *ibid.*, p. 73.]

For convenience I have presented the Cambridge Conditions in a rather simple form. In a more elaborate exposition of the conditions advanced by the Cambridge economists I should have to introduce — and on *a priori* grounds I believe it desirable to introduce — modifications in the distribution of Shares for changes in the price-wage situation which might affect different individuals differently — some moving to a more preferable position, and others to a less preferable one — and for other special differences between individuals. (Cf. Marshall's reference to the distribution of *wealth*, *op. cit.*, pp. 527, 595, and Pigou's reference to the distribution of the *Dividend*, *op. cit.*, p. 89; but cf. also Kahn's reference to the distribution of *money incomes*, *p. cit.*, pp. 1, 2.)

if it exists.¹ In general, any set of value propositions which is sufficient for the evaluation of all alternatives may be introduced, and for each of these sets of propositions there corresponds a maximum position. The number of sets is infinite, and in any particular case the selection of one of them must be determined by its compatibility with the values prevailing in the community the welfare of which is being studied. For only if the welfare principles are based upon prevailing values, can they be relevant to the activity of the community in question. But the determination of prevailing values for a given community, while I regard it as both a proper and necessary task for the economist, and of the same general character as the investigation of the indifference functions for individuals, is a project which I shall not undertake here. For the present I do not attempt more than the presentation of the values current in economic literature in a form for which empirical investigation is feasible.²

III

The formulation I have used to derive the maximum conditions of economic welfare differs in several respects from that of the welfare studies. It will be desirable to review briefly the relevant points of the various expositions, and the departures of the present essay from them. I shall continue to use the set of assumptions stated on page 310.

1. Cf. footnote 8, p. 313

2. This conception of the basis for the welfare principles should meet Professor Robbins' requirement that the economist take the values of the community as data. But in so far as I urge that the economist also *study* these data it represents perhaps a more positive attitude than might be inferred as desirable from his essays. (*The Nature and Significance of Economics*, London, 1932, particularly chapter VI.) Whether the approach will prove a fruitful one remains to be seen.

It may be noted that the Professor Robbins is averse to the study of indifference curves (pp. 96ff.) his own analysis requires an assumption that a movement of labor from one use to another is indifferent to the laborer and that a shift of other factors of production is indifferent to the community. Without these assumptions, for which I can see no *a priori* justification, his whole discussion of alternative *indifferent* uses, and his references to the most adequate satisfaction of demand from a given amount of means are without basis.

In the Cambridge analysis³ the welfare of the community, stated symbolically,⁴ is an aggregate of the form,⁵

$$(3.1) \quad \bar{E} = \Sigma U^i(x_i, y_i, a_i^x, b_i^x, a_i^y, b_i^y).$$

In this expression U^i is some function of the indifference function, S^i , and measures the satisfactions derived by the i^{th} individual from $x_i, y_i, a_i^x, b_i^x, a_i^y, b_i^y$. If individual temperaments are about the same, that is, if individuals are capable of equal satisfactions, the marginal utilities or derivatives of the utility functions of different individuals, it is assumed, will be equal for an equal distribution of Shares.⁶

It is possible to derive all the maximum conditions, in specific terms, from the equation

$$(3.2) \quad \Sigma dU^i = 0.$$

The technique used by the Cambridge economists is less direct and varies in certain respects. For our present purposes these procedural differences are of little special interest, but it will facilitate our discussion of the analysis of Pareto and Barone if we append the following notes.

Marshall develops the Pareto-Barone-Cambridge consumption and labor supply conditions separately from the rest of his analysis.⁷ These conditions are that for some price-wage situation p, q, g^x, h^x, g^y, h^y , and for all i ,

$$(3.3) \quad w^i = \frac{U_1^i}{p} = \frac{U_2^i}{q} = -\frac{U_3^i}{g^x} = -\frac{U_4^i}{h^x} = -\frac{U_5^i}{g^y} = -\frac{U_6^i}{h^y}$$

3. The passages in the Cambridge studies which are particularly informative as to the Cambridge concept of welfare are Marshall, *op. cit.*, pp. 80ff., 200ff., 527, 804; Pigou, *op. cit.*, pp. 10-11, 87, 97; Kahn, *op. cit.*, pp. 1, 2, 19; and also Edgeworth, *Papers Relating to Political Economy*, Vol. II, p. 102 (from the *Economic Journal*, 1897).

4. Aside from Marshall's appendices, the exposition of Marshall, Professor Pigou, and Mr. Kahn is non-mathematical, but the few relationships we discuss here may be presented most conveniently in a mathematical form. This will also facilitate comparison with the studies of Pareto and Barone.

5. In the analyses of Professor Pigou and Mr. Kahn some modification of (3.1) would be introduced to take care of the direct effects on aggregate welfare of shifts of factors of production from one use to another (cf. footnote 1, p. 315).

6. With the qualifications of footnote 9, p. 321.

7. Cf. the references in footnote 7, p. 320.

In (3.3), w^i is the marginal utility of money to the i^{th} individual and U_1^i, U_2^i, U_3^i , etc., are the marginal utilities of the various commodities and disutilities of the various types of work. In Marshall's exposition it is shown that, for any given amounts of X, Y, A^x, B^x, A^y, B^y , if the conditions (3.3) are not satisfied some U^i can be increased without any other being decreased. Thus for (3.2) to hold, (3.3) must be satisfied. Professor Pigou and Mr. Kahn do not develop the conditions (3.3), but assume them *ab initio* in their analysis.

If the conditions (3.3) are satisfied, (3.2) may be written in the form

$$(3.4) \quad \Sigma w^i \Delta_i = 0,$$

where

$$(3.5) \quad \Delta_i = p dx_i + q dy_i - g^x da_i^x - h^x db_i^x - g^y da_i^y - h^y db_i^y.$$

The remaining conditions again may be derived from (3.4). However, in Mr. Kahn's reformulation of Professor Pigou's analysis,⁸ it is assumed also that the Shares are distributed equally, and the remaining conditions are developed from the requirement that

$$(3.6) \quad \Sigma \Delta_i = 0.$$

The summation in (3.6), with certain qualifications, is Professor Pigou's index of the National Dividend.⁹ The procedures of Professor Pigou and Marshall differ from this, but the variances need not be elaborated here.¹

Pareto and Barone also assume initially that conditions (3.3) are satisfied, but Pareto like Marshall shows in an early section of his work that, otherwise, it is possible to increase the *ophélimité* of some individuals without that of any others being decreased.² To develop the remaining con-

8. Economic Journal, March, 1935.

9. Professor Pigou's index does not include cost elements; it relates to large adjustments — whence the problem of backward and forward comparisons; and it is expressed as a percentage of the total value product at the initial position. Cf. Economics of Welfare, Chap. VI.

1. But cf. section IV, *infra*.

2. Cours, Vol. I, pp. 20ff.

ditions, aside from the Cambridge Conditions, Pareto expressly avoids the use of (3.2) on the ground that

nous ne pouvons ni comparer ni sommer celles-ci [dU^1 , dU^2 , etc.], car nous ignorons le rapport des unités en lesquelles elles sont exprimées.³

Instead Pareto proceeds directly to (3.6) and deduces the maximum conditions for production from it. In this, evidently for the same reason, Barone follows.⁴ Neither Pareto nor Barone introduces the Cambridge Conditions into his analysis. Pareto merely assumes that the shares are distributed "suivant la règle qu'il plaira d'adopter," or in a "manière convenable,"⁵ and Barone that they are distributed according to some "ethical criteria."⁶

The basis for developing production conditions directly from (3.6), for Pareto, is that this equation will assure that if the quantities of products

étaient convenablement distribuées, il en résulterait un maximum d'ophélimité pour chaque individu dont se compose la société.⁷

Barone adopts the requirement that the sum be zero because this

means that every other series of equivalents different from that which accords with this definition would make that sum negative. That is to say, either it causes a decline in the welfare of all, or if some decline while others are raised, the gain of the latter is less than the loss of the former (so that even taking all their gain from those who gained in the change, reducing them to their former position, to give it completely to those who lost, the latter would always remain in a worse position than their preceding one without the situation of others being improved).⁸

Mr. Lerner, in the first of his two studies on welfare, advances as a criterion for a maximum position the condition that it should be impossible in this position to increase the welfare of one individual without decreasing that of another. From this criterion he develops graphically the

3. *Ibid.*, Vol. II, p. 93.

4. Cf. Ministry of Production, p. 246.

5. *Cours*, Vol. II, pp. 91, 93, 94.

6. *Op. cit.*, p. 265.

7. *Op. cit.*, pp. 93, 94.

8. *Op. cit.*, p. 271.

first two groups of maximum conditions. Like Pareto and Barone he does not introduce the Cambridge Conditions into his analysis but, as he indicates, ignores the problem of distribution.⁹ In his later paper Mr. Lerner presents the first group of maximum conditions alone, on the basis of the criterion for a maximum that it should be impossible to increase the production of one commodity without decreasing that of another.¹

In my opinion the utility calculus introduced by the Cambridge economists is not a useful tool for welfare economics. The approach does not provide an alternative to the introduction of value judgments. First of all, the comparison of the utilities of different individuals must involve an evaluation of the relative economic positions of these individuals. No extension of the methods of measuring utilities will dispense with the necessity for the introduction of value propositions to give these utilities a common dimension. Secondly, the evaluation of the different commodities cannot be avoided, even tho this evaluation may consist only in a decision to accept the evaluations of the individual members of the community. And finally, whether the direct effects on aggregate utility of a shift of factors of production from one use to another are given a zero value, as in Marshall's analysis, or a significant one, as in the analyses of Professor Pigou and Mr. Kahn,² alternatives are involved, and accordingly value judgments must be introduced.

While the utility calculus does not dispense with value judgments, the manner in which these value judgments are introduced is a misleading one. Statements as to the aggregative character of total welfare, or as to the equality of marginal utilities when there is an equal distribution of Shares, provided temperaments are about the same, do have the ring of *factual* propositions, and are likely to obscure the

9. Review of Economic Studies, June, 1934.

1. Ibid., October, 1934.

2. Cf. footnote 1, p. 315 and footnote 3, p. 324.

evaluations implied. The note by Mr. Kahn, in reference to his own formulation of the maximum conditions for economic welfare, that

many will share Mr. Dobbs' suspicion "that to strive after such a maximum is very much like looking in a dark room for a black hat which may be entirely subjective after all."³

is not one to reassure the reader as to the nature of the welfare principles derived in this manner. To the extent that the utility calculus does conceal the rôle of value judgments in the derivation of welfare principles, the criticism directed against the Cambridge procedure by Professor Robbins and other students of economics⁴ is not without justification.

The approach, it must also be noted, requires a group of value propositions additional to those I have presented. Insofar as the Cambridge economists require that the economic welfare of the community be an *aggregate* of individual welfares, value judgments must be introduced to the effect that each individual contributes independently to the total welfare. These value propositions, which imply the complete measurability of the economic welfare function aside from an arbitrary origin and a scalar constant, are not necessary for the derivation of the maximum conditions, and accordingly are not essential to the analysis.⁵

The derivation of conditions of maximum economic welfare without the summation of individual utilities, by Pareto, Barone, and Mr. Lerner, is a stride forward from the Cambridge formulation. Pareto's exposition of the basis for the procedure is somewhat ambiguous. Properly stated, the argument for developing production conditions directly from (3.6) is the same as that used in developing consumption conditions. The increment Δ_i in (3.5) indicates the prefer-

3. Economic Journal, March, 1935, footnote, p. 2.

4. Cf. Robbins, *The Nature and Significance of Economic Science* (London, 1932); Sutton, C., *Economic Journal*, March, 1937.

5. Lange's discussion of utility determinateness (*Review of Economic Studies*, June, 1934.) errs insofar as it implies that welfare economics requires the summation of the independently measurable utilities of individuals, i.e., his second utility postulate.

ence direction of the i^{th} individual.⁶ If Δ_i is positive, the i^{th} individual moves to a preferable position. The condition that $\Sigma \Delta_i$ be equal to zero does not assure that the *ophélimité* of each individual be a maximum, but that it be impossible to improve the position of one individual without making that of another worse. This, disregarding the misleading comparison of losses and gains, is the interpretation of Barone, and it is also the condition for a maximum used by Mr. Lerner.

But in avoiding the addition of utilities, Pareto, Barone, and Mr. Lerner also exclude the Cambridge Conditions from their analysis. None of the writers indicates his reasons for the exclusion, and I believe it has not proved an advantageous one. The first two groups of value propositions are introduced in the studies of Pareto and Barone by the use of, and the argument as to the use of, (3.6) as a basis for deriving maximum conditions, and in the analysis of Mr. Lerner by the criteria adopted for a maximum. In this respect the formulations differ little from that of the Cambridge economists. With the accompanying statements by Pareto and Barone that *the distribution of Shares* is decided on the basis of some "ethical criteria" or "rule," or with the complete exclusion of the problem by Mr. Lerner, this approach is not more conducive to an apprehension of the value content of the first two groups of maximum conditions. In the case of Mr. Lerner's study a misinterpretation does in fact appear. For in his analysis the first group of maximum conditions are advanced as objective in a sense which clearly implies that they require no value judgments for their derivation.⁷

Further, it must be emphasized, tho the point is surely an obvious one, that unless the Cambridge Conditions, or a modified form of these conditions, is introduced there is no reason in general why it is more preferable to have the other two groups of conditions satisfied than otherwise. Placing $\Sigma \Delta_i$ equal to zero does not assure that there are

6. Cf. Allen, *Economica*, May, 1932.

7. Review of *Economic Studies*, October, 1934, p. 57.

no other positions for which welfare is greater, but only that there are no other positions for which the welfare of one individual is greater without that of another being less. In general if the third group of maximum conditions is not satisfied, it is just as likely as not that any position for which $\Sigma\Delta$ does not equal zero will be *more* desirable than any position for which it does equal zero.

In the Pareto-Barone analysis, tho not in that of Mr. Lerner, there is reason to believe that, in a general form, the third group of maximum conditions is assumed to be satisfied. While the distribution of Shares is not specified, it is consistent with some "ethical criteria," or "rule." Whatever the rule is, it should follow that in the maximum position the marginal economic welfare "per dollar" with respect to all individuals is the same. Otherwise, in the light of that rule, some other distribution would be preferable. If this interpretation is correct, the special exposition used by Pareto and Barone to support their derivation of maximum conditions is inappropriate. In (3.6) it is true that each dollar does not express the same amount of utility in the Cambridge sense, since the value propositions of independence are not introduced. But each dollar does express the same amount of welfare. The argument used to place (3.6) equal to zero is thus not the Pareto-Barone one, but that if it were unequal to zero, a further adjustment increasing the summation would be possible, and this would directly increase welfare, *regardless* of whether the position of some individuals were improved and that of others worsened by the change.⁸

IV

I have noted elsewhere that the conditions for a maximum welfare which are presented in sections I and II are the conditions for any critical point. They are sufficient to inform us whether or not we are at the top or bottom of a hill, or at the top with respect to one variable, and the bottom with respect to another. The requirement for a

8. This argument is more fully developed in section IV, *infra*.

maximum position is that it be possible to reach the position from any neighboring point by a series of positive adjustments. For the determination of such a position, it is necessary to know the sign (+, -, 0) of any increment of welfare.

In the welfare studies the sign of dE is specified only for limited groups of adjustments. It will be of interest to note these conditions, and the value judgments required, tho I shall not review again the formulations of the various writers.

(1) If we assume that all the conditions for a critical point are satisfied, except those relating to the distribution of the factors of production between different uses, one additional group of value judgments gives us sufficient information concerning the shape of the Economic Welfare Function to determine the sign of an increment of welfare. These value propositions are: *if all individuals except any i^{th} individual remain in positions which are indifferent to them, and if the i^{th} individual moves to a position which is preferable to him, economic welfare increases.* If we denote a more preferable position by a positive movement of S^i , these value propositions require that

$$(4.1) \quad \frac{\partial E}{\partial S^i} > 0,$$

for any i . Let us write from (2.5),

$$(4.2) \quad dE = \sum \frac{\partial E}{\partial x_i} dx_i + \frac{\partial E}{\partial y_i} dy_i + \frac{\partial E}{\partial a_i^x} da_i^x + \frac{\partial E}{\partial b_i^x} db_i^x \\ + \frac{\partial E}{\partial a_i^y} da_i^y + \frac{\partial E}{\partial b_i^y} db_i^y.$$

Using the equations (1.5) through (1.10), and the notation of (3.5),

$$(4.3) \quad dE = \omega \Sigma \Delta_i.$$

By (4.1) and the equations (1.5) through (1.10), ω must have the same sign as the price-wage rates in Δ_i . We shall take this sign as positive. Thus if the Shares are distributed equally, and if the prices and wage rates are proportionate to the marginal rates of substitution of the different kinds

of commodities and types of work, economic welfare has the sign of Professor Pigou's index of the National Dividend. It will be increased by any adjustment which has as a result the movement of factors of production to a position of higher marginal value productivity.

(2) If the assumption that the Cambridge Conditions are satisfied is relaxed, (4.3) may be written in the form

$$(4.4) \quad dE = \Sigma \omega^i \Delta_i$$

where ω^i is the marginal economic welfare per dollar with respect to the i^{th} individual. Using the evaluation in (4.1) it follows that, for any adjustment for which no Δ_i decreases and some Δ_i increases, economic welfare will increase.

(3) Continuing to use the assumptions of (2), let us write

$$(4.5) \quad \lambda_{ik} = \frac{\omega^i}{\omega^k},$$

and

$$(4.6) \quad dE = \omega^k \Sigma \lambda_{ik} \Delta_i.$$

Let us introduce the value propositions: for a given price-wage situation, and any i and k , if the Share of i is greater than that of k , a decrease in the Share of k would have to be accompanied by a larger increase in the Share of i , for economic welfare to remain unchanged. Since it can be shown that if the Share of the i^{th} individual increases by dm_i a concomitant decrease, $-\lambda_{ik} dm_i$, in the share of the k^{th} will leave economic welfare unchanged,⁹ these value propositions require that λ_{ik} be less than unity. It follows that, for any given adjustment, if $\Sigma \Delta_i$ is positive, and if Δ_i does not vary with m_i , or if it decreases with m_i , economic welfare will increase. In other words, if the change in the National Dividend is not counteracted by a change in its distribution, the welfare of the community will be increased, even if some Δ_i increase and others decrease.

The adjustments in (1) are those considered by Mr. Kahn;

9. This relationship follows immediately from the equations:

$$dE = \frac{\partial E}{\partial m_i} dm_i + \frac{\partial E}{\partial m_k} dm_k = \omega^i dm_i + \omega^k dm_k.$$

in (2) by Pareto, Barone, and Mr. Lerner; and in (3) by Marshall and Professor Pigou. As Professor Pigou has pointed out,¹ the sign of an increment of welfare for some adjustments is left undetermined in his analysis. To determine the sign of dE for all adjustments, all the λ 's would have to be evaluated, and a similar group of value judgments for the case where prices and wages are not proportional to the marginal rates of substitution would have to be introduced. On *a priori* grounds there is no reason why more information should not be obtained, since the comparison involved in evaluating the λ 's is the same as that required for the Value Propositions of Equal Shares. For some additional and fairly rough evaluations, the range of adjustments included can be extended considerably, tho an element of uncertainty is involved. Two such approximations, perhaps, are of sufficient interest to note, tho they are not introduced in the welfare studies.

(4) The assumptions of (2) are retained. Let us suppose that with respect to some individual, say the k^{th} ,

$$(4.7) \quad \Sigma \lambda_{ik} = N$$

the sum being taken for all i . Thus ω^k is the average ω . If we write

$$(4.8) \quad \alpha_i = \lambda_{ik} - 1; \quad \beta_i = \Delta_i - \frac{\Sigma \Delta_i}{N};$$

then

$$(4.9) \quad dE = \omega^k (\Sigma \alpha_i \beta_i + \Sigma \Delta_i).$$

The first term in the brackets may be regarded as an index of the distribution of the National Dividend. It follows immediately from (4.9) that (a) if Δ_i is positively correlated with λ_{ik} , dE will increase with an increase in the Dividend and conversely; (b) if the coefficient of variation of the ω 's is less than one hundred per cent, that is, if the standard deviation of λ_{ik} is less than unity, and if the coefficient of variation of Δ_i is also less than one hundred per cent, dE

1. *Economics of Welfare*, p. 645.

will have the sign of the index of the Dividend *regardless* of changes in its distribution.²

To determine precisely whether the conditions enumerated are satisfied, of course, would require a complete evaluation of the λ 's. But the following rough evaluations would be sufficient to assure the likelihood of the results. For (a), it must be possible to say that "on the average" the change in distribution does not affect the "poor" more than the "rich" or vice versa. For (b) it is necessary to conceive of an individual or group of individuals who are, on the whole, in an average position from the point of view of welfare, and to determine whether, for a given position, ω^i "on the average" is likely to be somewhat less than twice the marginal economic welfare per "dollar" for the average individuals, that is, less than twice ω^k . (This should be stated in terms of the average shift in Shares for which welfare remains unchanged.) If it is determined that such a position is occupied, it would be likely that if tastes did not vary greatly — that is, if the relative variation of Δ_i were not very large — dE would increase for an increase in the Dividend. Since, however, the relative variation of Δ_i would ordinarily become excessively large as $\Sigma\Delta_i$ approached zero, it would be highly uncertain, for adjustments close to the maximum, whether or not an unfavorable change in distribution would obliterate the change in the Dividend.

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2. From (4.9),

$$\begin{aligned} dE &= \omega^k (Nr_{\lambda\Delta} \sigma_{\lambda\sigma_{\Delta}} + \Sigma\Delta) \\ &= \omega^k (Nr_{\lambda\Delta} \sigma_{\lambda\sigma_{\Delta}} / \Sigma\Delta + 1) \Sigma\Delta. \end{aligned}$$

The proposition (a) follows immediately, and (b) is based on the fact that $r_{\lambda\Delta}$ must be less than unity.

THE
QUARTERLY JOURNAL
OF
ECONOMICS

MAY, 1908

CAPITAL, INTEREST, AND DIMINISHING
RETURNS.

SUMMARY.

I. The Ricardian view as a starting-point: advances to laborers, 334.—Are there advances? Clark's view considered, 336.—Do capital goods reproduce themselves, and does their maintenance involve abstinence? Advances to laborers in a complex society, 339.—II. The law of diminishing returns as applied to capital by Carver, 348; by Clark, 350; by Böhm-Bawerk, 352.—Criticism of this view, 353.—Static and dynamic conditions, 356.—The law of diminishing returns and the law of diminishing utility not the same, 360.—Conclusion, 362.

THE debate carried on in the columns of this Journal between Professors Böhm-Bawerk and Clark has raised once again the fundamental questions as to the nature and cause of the return on capital, and its relation to the return for labor.¹ Some phases of these questions I propose still further to consider.

That an increase of capital—the number of laborers and the state of the arts remaining the same—lowers

¹See the articles by these scholars in the issues for November, 1906, and February, May, and November, 1907. I would add that my own article was completed and put into type before the receipt of that by Professor Veblen in the last issue (February, 1908).

interest and raises wages has been laid down by all economists since the days of Adam Smith and Ricardo. The unsettled problems are as to the mechanism by which these results are brought about, the rate at which the decline in interest takes place, the extent to which capital can continue to increase and still get a return, the conditions on which the past and future of interest depend. There is another problem even more important, and no less unsettled, in the background,—the grounds on which the receipt of interest can be defended as part of the social order. To some of these unsettled problems I propose to give attention.

I.

I will begin by recalling the older view, as outlined by Ricardo himself, and as stated more explicitly by Mill and other followers. According to this, all the operations of capitalists are resolvable into a succession of advances to laborers. Profits or interest (practically the same thing was meant in the earlier terminology by these words) arose from an excess of what the laborers produced over and above what was turned over to them. As we all know, this mode of treating the problem was associated with the wages-fund doctrine. It is not material to the fundamental proposition here under consideration whether the wages-fund doctrine be rejected *in toto* with contempt or whether some elements of truth in it be admitted. The things which are supposed to be advanced to the laborers may or may not be dubbed a "fund" or "wages-fund"; and they may or may not be conceived as predetermined in amount.¹ The essential things are

¹These matters I have considered in my volume on *Wages and Capital* (1896). The substantive conclusions there reached I have seen little occasion to change. On one point, however, not unimportant, I should make a modification. The term "wages-fund" ought to be discarded. Possibly a "wages-flow" might be spoken of without causing misconception; but even this is of doubtful serviceableness, since it suggests a flow of wages distinct from the flow of social income in general.

that laborers are assumed to be hired by capitalists, that the existing possessions of the community are supposed to be the property of a limited number of such capitalists, and that the mechanism by which wages are adjusted is a hiring of laborers by these owners, or capitalists.

There is here, obviously, a close resemblance to the "surplus value" version of the Marxian socialists. In that, also, all gains of the capitalist class—whether considered as one homogeneous mass or classified under the heads of interest, rent, business profits, monopoly gains—arise from a surplus. The socialists go further, and say that no part of this surplus has justification. The strict Marxians, too, maintain doctrines as to the abstract relation of "value" to the labor embodied or applied. These corollaries drawn by the socialists do not bring them into inconsistency or difference with the original proposition; namely, that the source of all capitalist gains is an excess of the product of labor over and above what is received by the laborers.

This proposition seems to me sound. A recognition of it, an acceptance of its consequences, and reasoning based upon it seem to me essential to an understanding of the phenomena.

The grounds on which the proposition itself rests are simple. They are, on the one hand, that production takes time, and, on the other hand, that there is inequality of possessions. These are facts so patent that no proof of them can be needed. The time-using character of highly organized production has been repeatedly dwelt on by writers of all schools, and has been especially illustrated and emphasized in the brilliant exposition of Böhm-Bawerk. The inequality of possessions is a great historic fact, doubtless not in accord with ideals of the best human progress, but to be faced as a characteristic

of almost all developed societies, and not least of modern societies. Resting on it is the other great fact, comparatively modern, of the preponderance of hired labor. Inequality has been somewhat mitigated during the last half-century by some accumulations on the part of hired laborers through savings-bank deposits and the like. But these accumulations are still insignificant as compared with those of the possessing classes. Much the greater part of the property in society is owned by the comparatively small number of the latter. Hence it follows that the support and reward of most laborers during the period of production are secured through advances made (*i.e.*, wages paid) to them by the owners of existing wealth. Recurrently, those owners get into their hands the wealth newly produced, and turn part of it over to the laborers again. They steadily retain for themselves a surplus, which is the source of all capitalistic gains.

At least one fundamental assumption in all this has been questioned. It has been maintained, most explicitly by Professor Clark, that there is no such thing as an "advance" by capitalists to laborers. Before proceeding further, it will be well to consider the objections raised by him.

The only advances made, according to Professor Clark, are by the producers of finished articles to the producers of articles not finished. The producers are represented by him as being in groups A, A', A'', A'''. The group A is supposed to turn out raw materials; group A' transforms that raw material somewhat; A'' brings it still nearer completion; A''' finally "produces" finished or consumable commodity. Now, says Professor Clark, there may be an advance by A''' to the other groups, but there is no other advance. "The whole question whether goods are advanced by one class of persons to another,

in order to tide that other class over an interval of waiting, clearly has reference, not to the relation of capitalists in general to laborers in general, but to the relation of certain sub-groups to other sub-groups in the producing series."¹

Professor Clark here seems to me to confound two essentially different things: on the one hand, the division of labor between different groups of successive producers; on the other hand, the relations of laborers and capitalists to one another in each single group and in all the groups taken as a whole.

The division of labor between different sets of successive producers is a familiar matter. The illustration of the groups A, A', A'', A''' (with the addition, if you please, of other similar groups,—B, B', B'', B''', and so on), fits it perfectly. All this is part of "the roundabout or time-using mode of using labor," to quote Professor Clark himself.² But to suppose, as Professor Clark does, that such a time-using process brings also a "synchronizing" of labors and return seems to me quite erroneous. I find myself in complete accord, on this subject, with what has been said by other critics, notably by Professor Carver³ and more recently by Professor Böhm-Bawerk.⁴ What A''' does is to put the finishing touches on work brought nearly to the stages of completion by the previous labor of A, A', A''. If one wishes to use a method

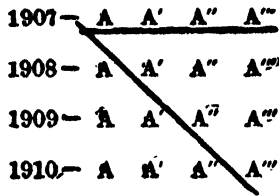
¹ *Distribution of Wealth*, p. 305. I have simplified Professor Clark's illustration by referring only to one series of producers, A, A', A'', A''', as he has himself done at p. 315. The case is the same if there be supposed several series, A, A', A'', . . . , B, B', B'' . . . , C, C', C'', . . . , and finally H, H', H'' . . . ; in which case the A group stands for the successive workers on wheat, flour, and bread, the B group for the workers on wool, cloth, and garments, the C group for those on logs, lumber, houses, and the H group finally for those on ore, iron, tools. This more elaborate supposition is made by Professor Clark at pp. 268, 269.

² Page 309.

³ In his review of *Distribution of Wealth* in this Journal, vol. xv., p. 594.

⁴ In this Journal, vol. xxi., p. 266.

of letters and diagrams, the following indicates the actual situation:—



During any one period (say in 1907) all four A's are working simultaneously (say growing wool, erecting spindles and looms, manufacturing cloth, making garments). But the material on which A has worked in 1907 is passed on to A' in 1908. That which A' has partly fashioned is passed on to A'' in 1909. A''' finally gives the finishing touches in 1910. It is not the horizontal line running through 1907 that represents the course of production, but the oblique line that runs through all four periods.¹

Probably Professor Clark would say, with reference to the above illustration, that it really fits into his own view. He would maintain—I trust I am right in interpreting his reasoning—that, when once the preparatory work of A, A', A'', has been done, it makes no difference which order we consider. Both lines—the horizontal and the oblique—show the same series of A, A', A'', A'''. When once the wool has been grown and is in existence, when once the looms and factories are made and ready for use, it is *as if* the present work of A brought an immediate consumable product in the garments to which A''' is now giving the finishing touches.

But it is not *as if*. There are essential differences. There is not, in fact, any “synchronizing” of production

¹ This mode of representation is used in my *Wages and Capital*, chap. i., p. 23. I repeat it here, as the briefest way of stating my opinion.

or any "instantaneous" clothing of the people. The difference appears perhaps most strikingly in another closely related matter, on which again I must differ with Professor Clark; one, too, which brings into view the whole conception of capital and labor. It is the relation of "abstinence" to the genesis of new capital and the maintenance of existing capital.

Elsewhere in his book Professor Clark maintains that "abstinence *originates* new capital," but that, "once the series of capital-goods is created and set working, there is no further waiting to be done." This is because "the keeping up of the series of capital-goods is, in a sense, automatic. The mill, the ship, virtually replace themselves as they are worn out." "Abstinence is confined to the genesis of true capital; none of it is involved in maintaining an endless series of capital goods."¹

This seems to me fundamentally untrue. And the insertion by Professor Clark of the qualifying phrases "in a sense" and "virtually" indicates, as Professor Böhm-Bawerk remarks of their use in other parts of the book, an uneasy sense of the inaccuracy.

Turn again to the set A, A', A'', A'''. In what sense can it be said there is abstinence in the maintenance of the sheep and wool, the looms and spindles, on which A and A' are working? Evidently, in this sense: A''' is turning out in each period enough to supply *all* of them, not only A''' himself, but A, A', A''. These workers in the earlier stages might knock off, and during the whole current period not suffer thereby. We may imagine either that A, A', A'', drop their work completely, leaving A''' to continue, as before, with the finishing touches, or that they help A''' on his finishing work, each of the four

¹ I quote from pp. 133, 134, and from the summary of the chapter at p. xviii.

then doing one-quarter of the usual daily stint, and each being thus enabled to loll or play for three-quarters of the time. The group as a whole, in other words, has its choice: it may either enjoy once for all on easy terms in the present or it may continue to work in the present, and so maintain the machinery of production for sustained enjoyment through the future.

This is precisely what "abstinence" or "waiting" must mean, with reference to a stage when laborers are not hired, but own and use their own tools. The first capital must have been made in spare time; that is, in time which did not have to be given to labor for satisfying immediate wants. There is, then, a choice between idleness (or play) and work which provides for the future.¹ That choice recurrently presents itself as tools wear out and materials are used up. The same choice would recurrently present itself to a collectivist or socialistic community. The whole body of socialists might for a time shorten their hours immensely, almost cease working, by simply using up the stocks on hand and doing nothing but put the finishing touches on the things nearly completed. But if they wish to keep their productive apparatus intact, they must refrain from this presumably agreeable relaxation, and work away at their tools and materials.

Of course, when the first irksome steps in the way of abstinence, or waiting, have once been taken, it is *much easier* to keep the process up. The primal savage who

¹I will not enter on the psychological and anthropological questions whether the very earliest work on tools in fact involved irksomeness and meant a sacrifice. Professor Clark assumes that the original making of capital involves an onerous waiting, and the same assumption has been made by most writers on this subject, including Professor Böhm-Bawerk (see the latter's *Positive Theory*, Book II., chap. iv.) All this very likely is, in Professor Veblen's phrase, "harmless misinformation" concerning the doings of primeval man. For the purposes of the present discussion its accuracy is not material. The only inquiry here is whether the first making of tools and their later maintenance call for radically different operations; and it is on this point that I must differ with Professor Clark.

figures so commonly in representations of these operations surely finds it much easier to replace his canoe when he has got together, by the use of the first canoe, an abundant store of fish. But, tho it is easier, the irksome thing must still be done. He must work rather than bask in the sun. When tools have once been provided, the process both of getting new capital and of maintaining existing capital becomes progressively easier. Labor becomes more productive; the available inflowing supply of consumable goods becomes larger; and less and less sacrifice of immediate relaxation is entailed in giving part of your labor to keeping up your apparatus or in making new apparatus.

When we leave these supposed simple conditions (of workmen owning and using their own tools) and come to the common situation of modern societies, we have a very different state of things to deal with. The farmer who digs his own drainage ditches "abstains" in the manner of the primal savage. But in the ordinary conditions of our complex societies, abstinence, or waiting, is vicarious, so to speak. It is done not by the workmen themselves, but by others who have present means and have the choice of hiring the workmen either for making things immediately enjoyable or for making tools. Professor Clark puts it thus: "Abstinence consists in taking one's income in the form of producers' goods,—electing to take draft horses instead of driving horses, trading vessels instead of steam-yachts, factories instead of pleasure palaces, always as part of the income of the men who do the abstaining."¹

This is true and well stated. But it should be supplemented by adding that the election in the end is between *hiring laborers* to do the one thing or the other. Neither trading vessels nor steam-yachts came into the world ready-

¹Page 134.

made. They are fashioned by labor, and the direction to which the labor is turned rests upon the choice of those who have free income; in other words, on the direction of demand. Freight boats and factories wear out; so do yachts and palaces. Neither sort reproduces itself. Both are made by labor, both can be replaced only by labor. The same abstinence that is involved in the first making of a factory is involved in its remaking.

Take the concrete case of a cotton-mill. The owners know that it will wear out in time. The "life" of the machinery in such a mill is about ten years. At the end of that period it is nothing but scrap-iron. Therefore, the owners put aside every year something for "depreciation"; that is, they do not divide all that they might (say in the form of dividends, if the organization be that of a stock company), but reserve annually so much as will enable them at the end of the ten years to buy another set of machinery. There is nothing to compel them to do so.¹ They may conclude that the business is not likely to be profitable, may wind up the whole thing, and turn over to the stockholders in cash what had been set aside for depreciation. The stockholders then can do as they please. They can reinvest in other directions—that is, cause laborers to be hired in making other capital—or they can "spend" on palaces or yachts; that is, cause laborers to be hired in making things of this sort. The strong probability of course is that the manufacturing corporation will be kept up as a going concern, and that the depreciation fund will be used in buying new machinery to

¹Professor Clark says (*Distribution of Wealth*, p. 133): "The loom in the factory that is worn out and is about to be replaced has, during its career, earned its share of dividends for the stockholders of the mill, and, besides this, *has earned for them a sum that will buy a new loom*. It is not necessary, therefore, to take the cost of the new loom out of the stockholders' incomes. That would impose on them the necessity for a genuine act of abstinence." The italics here are mine. Is this "earned sum that will buy a new loom" necessarily committed to buying a new loom? *Must* it be reinvested? Has not the stockholder precisely the same freedom as to what he will do with this as he has with his other income?

replace the old. In other words, labor will be turned to making the new machinery. But there is nothing automatic in the process, no certainty of replacement, no difference between the mode in which existing capital is replaced and that in which new capital comes into existence.¹

The substantial difference between Professor Clark's view on this point and my own can perhaps be best indicated by a practical application. Suppose a tax were levied precisely equal to the interest on capital now in existence. Would that capital continue in existence indefinitely? Surely, yes, according to Professor Clark. Existing capital, he says, replaces itself automatically or "virtually." Its replacement, he believes, entails no further abstinence or sacrifice to the owner. Hence he must admit that society could appropriate the whole return without suffering ill results from a diminution of its outfit of capital. To me it seems clear that, since "abstinence" ordinarily entails some degree of sacrifice,—or, to put it in more modern phraseology, since present goods or present income are ordinarily preferred to future goods or future income,—capital would cease to be maintained with the complete disappearance of return on it. This, of course, on the assumption that the régime of private property persists. I will not digress to the consideration of capital and its maintenance in a collectivist society.

The value of a distinction lies in its pointing to propositions which hold good of one of the things distinguished and do not hold good of the other. The particular propositions or conclusions which Professor Clark deduces from his distinction between capital and capital goods seem to me quite untenable,—thus, as to the "synchronizing" of labor and its product, or the replacement of capital

¹I venture to refer on this whole subject to my *Wages and Capital*, pp. 56-63, 67, 225. The subject seems to me very simple.

without abstinence. There may be other conclusions from this sort of distinction, as to the social merit or justification of returns on "capital goods" as distinguished from returns on "capital." I suspect, however, that the conclusions which might be deduced on such social questions would be very different from those which run through Professor's Clark's writings. They would point not to the same justification for all kinds of "capital" (such of course is the drift of Professor Clark's reasoning), but to a discrimination between "capital" and "capital-goods," and to a still further discrimination between those capital goods which are fashioned by man and those agents which are the free gifts of nature. But these are matters not pertinent to the subject of the present discussion. So far as this is concerned, the distinction between capital and capital goods only beclouds the situation, in no way clarifies it.

Let us return now to the question of advances. As the relation of present labor to past labor, and that of abstinence to the making and replacement of capital, presented themselves in different form according as we considered independent or hired laborers, so does the question of advances to laborers present itself differently.

Consider, first, the case where there are independent laborers only, and no employing capitalists. Professor Clark suggests that in the group A, A', A'', A''', the last worker, A''', who puts the finishing touches in the series, may possibly be conceived as supporting the others whose work is in the earlier stages, and as making "advances" to them. I should not myself consider such a phraseology apt. A''' of course turns out all the consumable goods, and is their proximate owner. But he must have had the tools and materials which A, A', A'', are making. If, indeed, he is richer than the others, and owns once for all

the whole outfit of tools and materials, he is a possible employer and exploiter. But if he is on a par with them, is simply a worker, putting the finishing touches on the half-made or three-quarter-made things which come into his hands for completion, he turns over to the others a proportional part of the consumable goods. Even if there be no conscious sharing, he must buy tools and materials. He *exchanges* part of his bread (suppose this to be the bread-making group) for flour to be baked into more bread, just as the flour-maker exchanges part of his flour for new grain. There is division of labor and exchange within the group. Each of them owns a part of the capital of the group, each contributes to the output; and each will get (if they compete freely and are equal in endowments) the same share of the output. The group as a whole may perhaps be conceived as recurrently hiring all the laborers in the various stages to keep at their work and to maintain the tools and material as well as turn out the completed goods. A socialist community may be described, if one likes that turn of speech, as so dealing with its members.

All this, however, is idle speculation, or at least analysis of hypothetical conditions very different from those of the actual world. The sort of hypothesis which yields good results in economic reasoning is that which conforms to facts,—which only simplifies the facts and strips them of non-essentials. Let us assume the actual situation in modern societies. Suppose that at each stage there are, not independent laborers, owning their own tools and materials in common or severalty, but bare-handed laborers, having little or nothing, and employed by capitalists. This of course is the case (stated baldly) in modern societies.

Here the laborers A—those who grow the wool, to vary the illustration—are hired by capitalists. The laborers

who card and spin and weave in factories are hired by other capitalists, those who make up the cloth into garments by others still. All the laborers are dependent for the means of livelihood on the bargain they make with the capitalists. This is as true of those who turn out finished garments as of those who tend sheep and shear the wool. Whether or no the garment-maker can be conceived as "advancing" anything to the others if all are independent workmen, they cannot be so conceived when they have no ownership in what they turn out.

I will not weary the reader with elaboration as to the details by which the dealings of employers and laborers are worked out in a complex society. Of course the laborers first get money. They buy with the money some of the enjoyable commodities to which the last touches are being constantly given in the several groups. They buy, for example, some of the garments turned out by the A''' workers in our supposed group. They do not buy all of the garments, for a portion of the workers turn out clothing for the use of the capitalists and their associates. These last become real interest, profits, rent, just as the laborers' clothing becomes real wages. The capitalists make (*i.e.*, hire laborers to make) and exchange among themselves tools and materials. The replacing of tools and materials goes on systematically. Machinery is manufactured, and flax, wool, cotton, are grown. All this takes place not because there is any automatic reproduction, but because the immense majority of the possessing classes are known to be disposed to keep their accumulations intact, and not to turn their all into palaces and pleasure yachts. Some of the laborers work at consumable commodities which are to be capitalists' income; some at consumable commodities which are to be their own (laborer's) income; some at materials and tools in various stages destined to be in part one kind of income, in part

the other. They are all hired by capitalists, either to keep capital intact or to turn out income.

On one point I agree with Professor Clark. When the consumable commodities get into laborers' hands, they are no longer capital, or at least are no longer producers' capital. And I agree, too, that there is no fixed store of such goods stored up in some limbo. The nearest approach to such a store is in the stocks held by retail dealers,—stocks constantly drawn on and constantly replenished. The source from which wages and all incomes are derived is the inflowing stream of consumable goods. If we wish to use figurative language, we may speak of a wages-flow rather than of a wages-fund, or rather of an income-flow. But the mechanism by which this flow is directed toward hired laborers is that of advances by the capitalist possessors who may do as they please with their own.¹

II.

Assuming now that the mechanism of advances by capitalists and production by laborers operates substantially as the Ricardian school conceived it, let us consider some of the possibilities of its operation. More particularly let us consider how far advances by capitalists can be indefinitely increased and a return on capital still be secured. On this subject Professor Clark and some of his critics, such as Professors Böhm-Bawerk and Carver, are very much in accord, at least as to the eventual out-

¹ Needless to say, the question whether consumer's goods should also be dubbed "capital" ("consumer's capital," perhaps) does not enter here. I have simply gone with Professor Clark in his views on this subject, which indeed are also in accord with Böhm-Bawerk's. An enlargement or modification of our conception or definition of capital would not affect the present discussion, tho it would raise other and important questions as to the conditions both of demand and supply for capital in the wider sense. Throughout this article I have confined myself to capital or surplus means used "in production"; admitted on all hands, I believe, to be the dominant use in determining interest for modern communities.

come; whereas it seems to me that a considerable qualification of their conclusions is called for.

Let us follow the Ricardian view one step further. If "capital" (meaning thereby the sum total of things used in advances to laborers) and the number of laborers increase *pari passu*, and if the laborers remain efficient or productive in the same degree, the process of investment and hiring can go on indefinitely. Ricardo had always before him, it is true, the prospect of diminishing returns to labor in agriculture; *i.e.*, less productiveness of labor, and hence lowering of the surplus and of "profits." But even without pressure from diminishing returns the sustained accumulation of capital and the consequent increased advances to laborers might bring about a decline in profits. This result would ensue if capital increased faster than the number of laborers, or—to put the case in its simplest form—if capital increased and the number of laborers remained stationary. More would then be turned over to each laborer, the same amount would be produced for each laborer, and the excess or surplus would diminish. If, indeed, improvements were introduced at the same time with the added accumulations and advances, the decline in profits would be arrested. But the mere fact of accumulation had no connection with improvements and no tendency to bring them about.

Many modern writers, however, including both Clark and his critics, maintain more or less explicitly that there is precisely such a connection. More capital serves *per se* to increase the output. The more abundant the equipment of the laborers with tools, materials, and all the apparatus of production, the more they will produce; and therefore there is no limit to the amount with which they can be profitably supplied.

This view is perhaps most unequivocally stated by Professor Carver:—

The productivity of capital is, like that of land and labor, subject to the principle of marginal productivity, which is, as we have seen, a part of the general law of diminishing returns. Increase the number of instruments of a given kind in any industrial establishment, leaving everything else in the establishment the same as before, and you will probably increase the total product of the establishment somewhat; but you will not increase the product as much as you have the instruments in question. Introduce a few more looms into a cotton factory without increasing the labor or the other forms of machinery, and you will add a certain small amount to the total output. . . . One man with two looms would turn out more per man, but slightly less per loom, because there would be a few more stops. One man with four looms would turn out still more per man, but still less per loom, and so on. . . . That which is true of looms in this particular is also true of plows on a farm, of locomotives on a railway, of floor space in a store, and of every other form of capital used in industry.¹

The implication is—it would seem even the express statement—that the mere addition of more instruments and tools causes the output to increase. Supply the farmer with more plows, the carpenter with more saws and planes, the weaver with more looms,—then more grain, more wooden ware, more cloths, will be turned out.

I do not believe this to be the fact. Supply the laborers with more tools *of the same kind*, and there is no reason to suppose that the output will increase indefinitely, or even will increase at all. Let the farmer have a second plow, then three, four, a dozen, and he will accomplish no more. The additional equipment will be so much surplusage. Possibly one extra plow, to be turned out in case the first should need repair, will be worth having; but it is a question whether its occasional use (probably rare) will add enough to offset the loss from its own depreciation. Similarly, the addition of more looms will not in itself enable the weaver to turn out more cloth. Where,

¹ Carver's *Distribution of Wealth*, p. 220.

indeed, *new kinds* of power looms are in process of introduction and trial, there is a problem as to how many can be run to advantage by one weaver. A similar problem arises when weavers of a different degree of intelligence and alertness are being tried. The most advantageous adjustment of the labor supply to a given kind of tool or machine settles itself after such a process of experiment. When once it is settled, the mere addition of more tools of the given kind adds nothing. No one would say that a second, third, fourth, hand loom enables a weaver to turn out more cloth. The same is true of power looms or other machinery, when once things have settled down, and when it has been learned how to adjust labor to machinery,—or, to put it in wider terms, how to adjust present labor to past labor.

Professor Clark lays down the same proposition as Professor Carver, and in terms even less qualified.¹ But he adds that the increase in the quantity of capital is accompanied by a change in its character. "Society pulls down its barns and builds others better as well as larger; it carries its mercantile buildings farther into the air, and makes them fire-proof and durable; it substitutes steel ships for wooden ones and steamers for sailing craft; it takes the curves and grades out of its railroads."² "Qualitative improvement" takes place with the increase in quantity; in other words, there are tools of a different kind.

Both Professors Clark and Carver deduce a general—nay, a universal—principle of diminishing return. In the clear-cut statement of the latter, every increase of capital, the number of laborers being the same, brings

¹ Professor Clark states it as one of the universal laws of economics. "Supply capital in successive units to a fixed force of laborers, and everywhere you get, as a result, smaller and smaller additions to the output. This is a universal law." *Distribution of Wealth*, p. 50.

² *Distribution of Wealth*, p. 184.

an increase of output, but with a diminishing rate of increase. The same doctrine appears in Professor Fetter's *Principles of Economics*.¹

The readers of this Journal will recall Professor Böhm-Bawerk's criticism.² It is that Clark supposes his capital to drop from heaven, so to speak. The same criticism might be directed to Carver's exposition, for in this also capital is spoken of as if it appeared once for all and ready-made. There is no inquiry as to how it comes into existence. Capital must have been made by labor, but there is no consideration of the part played by the labor which made it or of the remuneration got by that labor. Is there a separate product of the capital or only a product of the various sorts of labor which first made the tools and materials and then used them?

The criticism seems to me deserved. In too much of recent economic speculation, capital has been treated as if it were ready-made. I believe that much of the discussion of land and capital, of rent and interest, which runs through the later chapters of Professor Clark's book, and also through the writings of Professor Fetter, rests on a tacit assumption of this sort. Land and capital are treated as if their conditions of supply were the same. "Capital," in the sense in which most of us use that term,—instruments made by man,—involves a sacrifice. In fact, it involves two kinds of sacrifices: labor on the part of the workmen; and "abstinence," or "waiting," or "exchange of present goods for future goods" (whichever phrase is preferred) on the part of the owners or of those who have hired the laborers. No analysis of capital and interest can probe the problem to the bottom which does not recognize these conditions of supply as to capital. The obvious ear-mark of what we call "land"

¹ Fetter's *Principles*, chap. ix., especially p. 71.

² In this Journal, vol. xvi., p. 252, *seq.*

is, on the other hand, that its conditions of supply are fixed by nature. Hence I agree with Böhm-Bawerk in finding a serious gap in Clark's reasoning. He fails to inquire how capital came into existence, and what is the function, what the source of reward, of the labor that made the capital. And this defect obviously is due to his peculiar doctrine as to the automatic reproduction of capital. If tools, materials, goods in process, "virtually" reproduce themselves, there is no occasion for asking whether and how they came to be made by labor.

But, notwithstanding this important difference, the final result reached by the contending debaters is, after all, very similar, and Professor Böhm-Bawerk's concluding thesis is open to the same criticism as that of Clark and Carver. Böhm-Bawerk's doctrine, it will be recalled, is that, as labor is applied in a different way,—as it is spread over more and more time, with more elaborate steps in previous making of tools and plant, with further and further postponement of the attainment of an enjoyable product,—the final output becomes greater. And there is, in his view also, a tendency to diminishing returns. The longer in time the process is, the greater will be the eventual output of consumable goods. Each extension, however, leads to a less increase than the previous extension. The increase in output due to the last extension determines the return to capital in the way of interest. As this extension in time involves a further postponement of present enjoyment, or a further exchange of present goods for future, it will not take place unless there are eventually more of present goods. The theory of the invariable attractiveness of present goods over future has more than a family resemblance to the old doctrine that "abstinence," or "waiting," is something irksome; while the doctrine of the invariable gain from using present goods as a means of spreading labor

advantageously over time, has again a resemblance to the doctrine of a "productivity" of capital.

Thus, while Böhm-Bawerk's analysis follows the process more faithfully in its details, its outcome is the same. The marginal increase in the productivity of labor spread over time, and the marginal productivity of capital,—these seem to me to come to the same thing in the end. There is agreement as to a far-reaching tendency to diminishing yield, and agreement that the marginal yield settles the rate of return on capital.

My own view is that in its historical significance, whether we look backward or try to look forward, this generalized statement is far too sweeping. More particularly, I believe that the theorem of a general tendency to diminishing return as more capital is used—*i.e.*, as more advances are made to laborers—cannot be maintained.

Taking a cross-section of industry at any given period, I should admit that the marginal "productivity" of capital determined the rate of return on it. Imagine that a community, now in possession of a stock of tools and materials, is compelled to part by successive steps with instalments of this capital. Clearly, it would first relinquish those parts which contributed least to the efficiency of labor, and then, as more and more had to be given up, would relinquish others in the inverse order of their serviceableness. It would reserve to the very last those constituents of capital—that is, those ways of roundabout production—which added most to the efficiency of labor. These ways—the last to be given up, the first to be used, if the community possessed its present knowledge of the arts,—would doubtless be, on the one hand, the agricultural operations which, in the temperate zone, run through the seasons and require something like a year's supply of food materials; and,

on the other hand, the metallurgical processes which yield iron, the prime tool of civilization. And, conversely, such a society, supplied in succession with the means of getting back its present stock of tools and materials, would acquire (*i.e.*, would make with the labor that was available) first the more essential, and then, one after another, those less effective in adding to the productiveness of labor.¹

The gain, or premium, or interest, which will be secured by the owners of the capital in any such supposed stage, will be determined by the least effective or helpful use; or, to use the accurate Böhm-Bawerkian phraseology, by the addition to the enjoyable product of labor which results from the least effective phase of the roundabout or capitalistic process. Those who use capital in the more effective ways cannot permanently retain the superior gain for themselves. All who have capital at command, or the means of getting it made, could turn to the more effective ways. Competition among them will prevent any one set of persons from securing higher gains than the rest. Hence it is the effectiveness of the least productive utilization of surplus possessions (*i.e.*, of capital) which determines the rate of gain for all capital.

But all this marshalling of capital is in the way of cross-section. It arranges the constituent parts of existing capital, as we now know about them, in an order of usefulness. But this order of usefulness is not necessarily, or even probably, the historical order. It would indeed have been the historical order if men had started at the outset with all the knowledge of the arts which they now have. But there is no reason, in my opinion, for supposing that in the past the more effective uses were first turned to, nor that in the future less and less effective

¹This is the sort of case assumed by Professor Marshall, *Principles of Economics*, Book VI., chap. i., § 9, p. 590 (4th edition), § 8, p. 520 (5th edition).

uses will be turned to. Whether we look backward or forward, no general or certain tendency to diminishing returns can be made out.

Let me illustrate my meaning by a consideration of what has happened in the civilized world during the last century or two, say since the middle of the eighteenth century. Suppose there had been during this period, so far as the bettering of capital goes, strictly "static" conditions. Suppose there had been none of the inventions which brought about the industrial revolution and made the modern world,—no steam-engine, no textile machinery, no railways, no steamships, no new iron and steel making processes. Assume, on the other hand, that accumulation had gone on during this long period at the rate which, in fact, has prevailed,—savings and surplus means valued by thousands of millions. Assume that the only thing which could have been done with this enormous mass of surplus means, in the way of bringing into existence more capital ("capital-goods"), had been an increase in the supply of instruments such as were familiar in 1750,—more spinning-wheels, more hand-looms, more broad-wheeled wagons and stage-coaches, more wooden sailing-vessels. Is it not obvious that before long the multiplication of these things would have led to no further gain? The persons who had the large surplus means, and who invested them in hiring laborers to make more and more of the old-fashioned tools, would have been brought very soon to the stage of no further increase in productiveness, of rapid decline in the rate of interest, and, if they persisted willy-nilly in accumulating and investing, complete disappearance of any return at all on their investments. The mode in which these consequences have been staved off is also obvious: it has been by the march of improvement and invention, the discovery of ways of applying labor to making more

elaborate tools than before, to ways of getting eventually a larger product in proportion to the total labor applied. These newly discovered ways have not been less effective than those previously followed. They have been probably more effective. The steam-engine and the railway—to mention two great transforming agents—stand for increasing returns, not diminishing returns.

But it will be said that I am confounding static and dynamic conditions. The proposition as to diminishing returns from an increase of capital, it will be said, holds good only in a static state, whereas during the last one hundred and fifty years the civilized world has been under highly dynamic conditions.

I confess not to be certain as to what is meant by a static state, and suspect that confusion between “static” and “dynamic” conditions appears among those whose reasoning rests on the supposed distinction. Thus Professor Clark speaks of going through an “illustrative dynamic process,” and observes that the process by which capital changes its form, as more or less of it is added, “is not a static process.”¹ It is not material what phrases are used. It is material only to keep clear just what is the way in which more capital is supposed to be added, and what is the sort of “natural” change that takes place as this is done. Elsewhere Professor Clark says that “every increase of capital shows itself primarily *in transmitting poor appliances into better ones.*”² This seems to me essentially a “dynamic” operation. It is an operation which is assumed by Professor Clark to take place in a “natural” or necessary way, following from the mere presence of more available means,—of more possibilities of making capital. The substitution of steel ships for wooden ones, of power machinery for hand tools, and sundry other improvements, are referred to by him as

¹Distribution of Wealth, p. 178.

²Ibid., p. 183.

taking place simply with the greater abundance of capital. But, in fact, they result not merely from that abundance, but from the irregular march of invention and discovery. To quote another passage,¹ "As capital grows more abundant, . . . society also makes all its machinery as nearly automatic as it can, so that one laborer's guidance shall keep much machinery in motion." "Society," I submit, does nothing of the kind. Individual schemers and inventors are trying to find out how to achieve such results. They may or may not succeed. If they do succeed, they may or may not need more "capital"; *i.e.*, may or may not call for a greater preparatory application of labor in making the automatic machinery.

The "static" state, if we are to use that phrase, means a condition in which the arts are stationary. An increase of capital, in such a state, means an addition of tools and materials of the same kind that were used before. By supposing such a situation, we can reason with some clearness as to what would happen if there were simply an increase of capital, and nothing more. We isolate that factor hypothetically, after the familiar practice of deductive reasoning in economics. I have already stated my belief that in such a static state the mere duplication of instruments of the same kind would lead to practically no increase in productivity. This much indeed is implied in Professor's Clark's supposition that with the changes in quantity of capital there are also changes in quality. The changes in quality would not need to be assumed if mere addition of quantity brought an indefinitely continuing gain. The essential point on which I differ is as to the regularity or predictableness of the changes in quality. These changes seem to me, in fact, very variable and subject to no certain law.

In Professor Böhm-Bawerk's treatment of this subject

¹ *Distribution of Wealth*, p. 184.

the form of statement is more guarded. The greater efficiency of the roundabout process is said to be a fact of general experience, very possibly subject to exceptions. Similarly, the tendency to diminishing returns as to the process becomes more roundabout, is set forth not as a "natural" law, but again as an empirical fact. Yet in the later development of his reasoning the acute Viennese thinker seems to me to forget the nature of the premises from which he starts. All his ingenious numerical illustrations (which remind one of Ricardo's illustrative figures) are worked out on the assumption of an increase of efficiency that goes on steadily, yet at a diminishing rate. This may be done, very properly, for the purpose of precision in the hypothetical reasoning, just as Professor Clark's figures may be justified as precise statements of a hypothesis. But Professor Böhm-Bawerk, not less than Professor Clark, draws substantive conclusions of importance. Interest, the former says, *must* appear; for there is always the possibility of using present goods as a means of extending the production period.¹ In other words, no matter how great the accumulation of a general subsistence fund, or, in less technical terms, no matter how great the volume of means pressing for investment, a return in the way of interest can always be secured. Stated in such unqualified terms, the proposition seems to me untenable.

Let me say something more as to the possibilities of an increasing use of capital. To prove that those possibilities are indefinitely extensible, reference is made to the many unused opportunities for applying capital which lie about on every side. There are, it is said, known and perfected devices, as yet only in partial use, to which new accumulations can be directed with clear advantage.

¹ *Positive Theory*, p. 333. See also pp. 377-378, and Chapters I., II., III., of Book VII., *passim*.

Here is a field virtually unlimited, tho one in which further exploitation must face the probability of diminishing returns.

Much of this is true of such a highly "dynamic" state as the world is now in. Inventions and improvements are not adopted by all producers at one fell swoop. They make their way step by step, first adopted by one person then by another, and come into use over the whole field by a gradual process. Professor Clark has effectively pointed out that a characteristic source of employer's profits is in the shrewd appreciation and early adoption of improvements. During the period when they are in process of adoption, very likely a long period, there are these visible and certain ways of applying new accumulations to advantage. If there be a succession of improvements, each new one opens such a vista, and at every instant of time there are unused opportunities for productive investment. Precisely this is what we have seen during the last one hundred and fifty years. We are living in the midst of the greatest burst of invention the world has ever known, one, too, which shows no signs of subsiding. So far as we can see into the future,—that is, for a generation or thereabouts,—there is no indication of any relaxation of the advance in the arts. It may be, as the more optimistic predict, that we are only on the threshold of further great conquests of natural forces. These conquests during the last century have involved more and more plant, and thus have involved more capital. They seem likely to do so for the immediate future, tho in what degree and with what certainty or regularity we are quite unable to say.

But these, after all, are the incidents of a period of transition. If we conceive the transition to be completed,—the current improvements to be applied universally,—then we reach the stage at which we can judge whether

there are unlimited opportunities for investment, unlimited possibilities of increasing the product, merely by adding more instruments of the kind already in use. Then we have the "static" state, and the naked question whether mere increase in the increments of capital (Clark's phrase) or mere extension of the production period (Böhm-Bawerk's phrase) serves to add to the output. To that question the answer, it seems to me, should be in the negative: whereas the question as to what may happen in the dynamic state—when there are "qualitative" improvements or advances in the arts—is not susceptible of such clear-cut answer as both thinkers seem to suppose.

One sort of limitation of the possibilities of using capital must not be overlooked. There are obstacles to the spread and utilization of known improvements which make many of them practically unavailable for great parts of mankind. The use of modern agricultural machinery by the peasants of British India would greatly increase the productiveness of their labor. Were they able to use it well, they could afford to pay handsome interest to those providing it. But lack of intelligence and education, all the rooted conditions of a primitive social state, make this application of capital out of the question. The American traveller in many parts of Europe sees unbounded opportunities for using labor-saving appliances. But, so long as the people are not ready to turn to them, there is here no opening for investment. A change in the intelligence and skill of a great mass of mankind is as much a "dynamic" operation as is the invention of a new mechanical process.

One other aspect of this supposedly far-reaching law of diminishing returns deserves attention. It is sometimes spoken of as if it were but a phase or application

of the general theory of value. Successive increments of any one commodity have diminishing utility and declining exchange value. Diminishing returns on capital are supposed to result directly from the diminishing utility of commodities, the first-named principle being simply the result or manifestation of the second-named.¹

These two tendencies, or laws, seem to me entirely different. In the one case we have to deal with the utilities and the values of the several units of quantity: in the other we have to deal with those units of quantity themselves,—with physical units. The law of diminishing returns as to land, so often referred to as the type and proof of the wider theorem, neither says nor implies anything as to utilities or as to value. It says only that more and more labor (capital also, if you choose to think of capital as something different from labor) applied to a given area does not remain continuously productive at the same rate; and the productiveness of the several doses is measured in terms of bushels of wheat or tons of hay, not in terms of value. Measured in terms of value, it is by no means necessarily true that there is any tendency to diminishing returns as to land. The smaller quantity of wheat or hay which accrues from the last dose of labor on the land will very likely have not less value, but the same value as preceding products of the same labor. Similarly, the problem as to the increase of return due to added doses of capital is one of quantity. Will more wheat, more cloth, more shoes, be got by making and using more tools or more elaborate tools? The law of diminishing utility, on the other hand, bears on the utilities or satisfactions derived from added units of the same commodity, and so on the *relative* value of the several

¹"Diminishing returns of indirect agents is a special case of the diminishing utility of goods." Fetter's *Principles of Economics*, p. 71. Professor Clark does not seem to hold this opinion; for in his recently published, *Essentials of Economic Theory* (p. 56) he refers to the one law as parallel to the other, not identical with it.

products. It thus affects the distribution of labor and capital towards the making of more or less of each product. The one principle has to do with the relative value of different commodities and with the income of satisfactions ("psychic" income) which mankind gets from its exertions. The other has to do with gains in physical quantities, and with the variations in such gains—whether at an increasing, a decreasing, or a stationary rate—from different ways of applying labor.

Returning now to the question as to the law of diminishing returns for successive increase of capital, I may sum up my conclusion by saying that the view which maintains this law seems to me essentially historical, and in that sense unreal. Successive increases of instruments of the same kind lead to no increase of return: they bring mere surplusage. The addition of instruments of a different and better kind—what Professor Clark calls qualitative increase—obeys no law. It is dependent on the progress of invention, and its course is irregular and unpredictable. If we have a steady increase of capital of the first kind,—quantitative only,—the return to capital will soon disappear. If we have a steady increase of the qualitative kind, there is no telling whether the addition to the total output, and so the return in the way of interest, will be at a steady rate, an increasing rate, or a diminishing rate.

The mind of man strives for generalization. It seeks to arrange phenomena in law or regular sequence. To this striving, I suspect, is due the attempt to formulate a universal law of diminishing returns. The attempt is like that to reach sweeping generalizations in history or politics, or—to come closer to the sphere of economics—that to find far-reaching or universal laws of economic development. In fact, the phenomena are not susceptible

of such clear-cut generalization; or at least we do not know enough about them to perceive clearly any underlying general forces. We must content ourselves with learning as much as we can of the irregular forces and the puzzling facts, and with stating our conclusions often in hypothetical terms. *If* an increase of capital (or a spreading of labor over more time) always brings a greater output, interest will continue, however great the increase of capital. *If* such an increase always brings a greater output, but at a diminishing rate, interest, while it will continue, will tend to be lower and lower in rate. *If* an increase of capital brings no greater output at all, and if none the less it takes place regardless of consequences, it will lead infallibly to the complete disappearance of interest. In some such forms as these we can state conclusions with sharpness of outline. But just in what way the increase of capital will in fact take place,—what will be the march of invention and discovery,—on this we are not able to forecast the future or the working of the productive forces in the future.

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THE ECONOMIC JOURNAL

DECEMBER, 1928

INCREASING RETURNS AND ECONOMIC PROGRESS ¹

My subject may appear alarmingly formidable, but I did not intend it to be so. The words economic progress, taken by themselves, would suggest the pursuit of some philosophy of history, of some way of appraising the results of past and possible future changes in forms of economic organisation and modes of economic activities. But as I have used them, joined to the other half of my title, they are meant merely to dispel apprehensions, by suggesting that I do not propose to discuss any of those alluring but highly technical questions relating to the precise way in which some sort of equilibrium of supply and demand is achieved in the market for the products of industries which can increase their output without increasing their costs proportionately, or to the possible advantages of fostering the development of such industries while putting a handicap upon industries whose output can be increased only at the expense of a more than proportionate increase of costs. I suspect, indeed, that the apparatus which economists have built up for dealing effectively with the range of questions to which I have just referred may stand in the way of a clear view of the more general or elementary aspects of the phenomena of increasing returns, such as I wish to comment upon in this paper.

Consider, for example, Alfred Marshall's fruitful distinction between the internal productive economies which a particular firm is able to secure as the growth of the market permits it to enlarge the scale of its operations and the economies external to the individual firm which show themselves only in changes of the organisation of the industry as a whole. This distinction has been useful in at least two different ways. In the first place it is, or ought to be, a safeguard against the common error of assuming that wherever increasing returns operate there is necessarily an effective tendency towards monopoly. In the second

¹ Presidential Address before Section F (Economic Science and Statistics) of the British Association for the Advancement of Science, Glasgow, September 10, 1928.

place it simplifies the analysis of the manner in which the prices of commodities produced under conditions of increasing returns are determined. A representative firm within the industry, maintaining its own identity and devoting itself to a given range of activities, is made to be the vehicle or medium through which the economies achieved by the industry as a whole are transmitted to the market and have their effect upon the price of the product.

The view of the nature of the processes of industrial progress which is implied in the distinction between internal and external economies is necessarily a partial view. Certain aspects of those processes are illuminated, while, for that very reason, certain other aspects, important in relation to other problems, are obscured. This will be clear, I think, if we observe that, although the internal economies of some firms producing, let us say, materials or appliances may figure as the external economies of other firms, not all of the economies which are properly to be called external can be accounted for by adding up the internal economies of all the separate firms. When we look at the internal economies of a particular firm we envisage a condition of comparative stability. Year after year the firm, like its competitors, is manufacturing a particular product or group of products, or is confining itself to certain definite stages in the work of forwarding the products towards their final form. Its operations change in the sense that they are progressively adapted to an increasing output, but they are kept within definitely circumscribed bounds. Out beyond, in that obscurer field from which it derives its external economies, changes of another order are occurring. New products are appearing, firms are assuming new tasks, and new industries are coming into being. In short, change in this external field is qualitative as well as quantitative. No analysis of the forces making for economic equilibrium, forces which we might say are tangential at any moment of time, will serve to illumine this field, for movements away from equilibrium, departures from previous trends, are characteristic of it. Not much is to be gained by probing into it to see how increasing returns show themselves in the costs of individual firms and in the prices at which they offer their products.

Instead, we have to go back to a simpler and more inclusive view, such as some of the older economists took when they contrasted the increasing returns which they thought were characteristic of manufacturing industry taken as a whole with the diminishing returns which they thought were dominant in agriculture because of an increasingly unfavourable proportioning

of labour and land. Most of them were disappointingly vague with respect to the origins and the precise nature of the "improvements" which they counted upon to retard somewhat the operation of the tendency towards diminishing returns in agriculture and to secure a progressively more effective use of labour in manufactures. Their opinions appear to have rested partly upon an empirical generalisation. Improvements had been made, they were still being made, and it might be assumed that they would continue to be made. If they had looked back they would have seen that there were centuries during which there were few significant changes in either agricultural or industrial methods. But they were living in an age when men had turned their faces in a new direction and when economic progress was not only consciously sought but seemed in some way to grow out of the nature of things. Improvements, then, were not something to be explained. They were natural phenomena, like the precession of the equinoxes.

There were certain important exceptions, however, to this incurious attitude towards what might seem to be one of the most important of all economic problems. Senior's positive doctrine is well known, and there were others who made note of the circumstance that with the growth of population and of markets new opportunities for the division of labour appear and new advantages attach to it. In this way, and in this way only, were the generally commonplace things which they said about "improvements" related to anything which could properly be called a doctrine of increasing returns. They added nothing to Adam Smith's famous theorem that the division of labour depends upon the extent of the market. That theorem, I have always thought, is one of the most illuminating and fruitful generalisations which can be found anywhere in the whole literature of economics. In fact, as I am bound to confess, I am taking it as the text of this paper, in much the way that some minor composer borrows a theme from one of the masters and adds certain developments or variations of his own. To-day, of course, we mean by the division of labour something much broader in scope than that splitting up of occupations and development of specialised crafts which Adam Smith mostly had in mind. No one, so far as I know, has tried to enumerate all of the different aspects of the division of labour, and I do not propose to undertake that task. I shall deal with two related aspects only: the growth of indirect or roundabout methods of production and the division of labour among industries.

It is generally agreed that Adam Smith, when he suggested that the division of labour leads to inventions because workmen engaged in specialised routine operations come to see better ways of accomplishing the same results, missed the main point. The important thing, of course, is that with the division of labour a group of complex processes is transformed into a succession of simpler processes, some of which, at least, lend themselves to the use of machinery. In the use of machinery and the adoption of indirect processes there is a further division of labour, the economies of which are again limited by the extent of the market. It would be wasteful to make a hammer to drive a single nail; it would be better to use whatever awkward implement lies conveniently at hand. It would be wasteful to furnish a factory with an elaborate equipment of specially constructed jigs, gauges, lathes, drills, presses and conveyors to build a hundred automobiles; it would be better to rely mostly upon tools and machines of standard types, so as to make a relatively larger use of directly-applied and a relatively smaller use of indirectly-applied labour. Mr. Ford's methods would be absurdly uneconomical if his output were very small, and would be unprofitable even if his output were what many other manufacturers of automobiles would call large.

Then, of course, there are economies of what might be called a secondary order. How far it pays to go in equipping factories with special appliances for making hammers or for constructing specialised machinery for use in making different parts of automobiles depends again upon how many nails are to be driven and how many automobiles can be sold. In some instances, I suppose, these secondary economies, though real, have only a secondary importance. The derived demands for many types of specialised production appliances are inelastic over a fairly large range. If the benefits and the costs of using such appliances are spread over a relatively large volume of final products, their technical effectiveness is a larger factor in determining whether it is profitable to use them than any difference which producing them on a large or a small scale would commonly make in their costs. In other instances the demand for productive appliances is more elastic, and beyond a certain level of costs demand may fail completely. In such circumstances secondary economies may become highly important.

Doubtless, much of what I have said has been familiar and even elementary. I shall venture, nevertheless, to put further stress upon two points, which may be among those which have

a familiar ring, but which appear sometimes to be in danger of being forgotten. (Otherwise, economists of standing could not have suggested that increasing returns may be altogether illusory, or have maintained that where they are present they must lead to monopoly.) The first point is that the principal economies which manifest themselves in increasing returns are the economies of capitalistic or roundabout methods of production. These economies, again, are largely identical with the economies of the division of labour in its most important modern forms. In fact, these economies lie under our eyes, but we may miss them if we try to make of *large-scale* production (in the sense of production by large firms or large industries), as contrasted with *large* production, any more than an incident in the general process by which increasing returns are secured and if accordingly we look too much at the individual firm or even, as I shall suggest presently, at the individual industry.

The second point is that the economies of roundabout methods, even more than the economies of other forms of the division of labour, depend upon the extent of the market—and that, of course, is why we discuss them under the head of increasing returns. It would hardly be necessary to stress this point, if it were not that the economies of large-scale operations and of “mass-production” are often referred to as though they could be had for the taking, by means of a “rational” reorganisation of industry. Now I grant that at any given time routine and inertia play a very large part in the organisation and conduct of industrial operations. Real leadership is no more common in industrial than in other pursuits. New catch-words or slogans like mass-production and rationalisation may operate as stimuli; they may rouse men from routine and lead them to scrutinise again the organisation and processes of industry and to try to discover particular ways in which they can be bettered. For example, no one can doubt that there are genuine economies to be achieved in the way of “simplification and standardisation,” or that the securing of these economies requires that certain deeply rooted competitive wastes be extirpated. This last requires a definite concerted effort—precisely the kind of thing which ordinary competitive motives are often powerless to effect, but which might come more easily as the response to the dissemination of a new idea.

There is a danger, however, that we shall expect too much from these “rational” industrial reforms. Pressed beyond a certain point they become the reverse of rational. I have

naturally been interested in British opinions respecting the reasons for the relatively high productivity (per labourer or per hour of labour) of representative American industries. The error of those who suggest that the explanation is to be found in the relatively high wages which prevail in America is not that they confuse cause and effect, but that they hold that what are really only two aspects of a single situation are, the one cause, and the other effect. Those who hold that American industry is managed better, that its leaders study its problems more intelligently and plan more courageously and more wisely can cite no facts in support of their opinion save the differences in the results achieved. Allowing for the circumstance that British industry, as a whole, has proved to be rather badly adjusted to the new post-war economic situation, I know of no facts which prove or even indicate that British industry, seen against the background of its own problems and its own possibilities, is less efficiently organised or less ably directed than American industry or the industry of any other country.

Sometimes the fact that the average American labourer works with the help of a larger supply of power-driven labour-saving machinery than the labourer of other countries is cited as evidence of the superior intelligence of the average American employer. But this will not do, for, as every economist knows, the greater the degree in which labour is productive or scarce—the words have the same meaning—the greater is the relative economy of using it in such indirect or roundabout ways as are technically advantageous, even though such procedure calls for larger advances of capital than simpler methods do.

It is encouraging to find that a fairly large number of commentators upon the volume of the American industrial product and the scale of American industrial organisation have come to surmise that the extent of the American domestic market, unimpeded by tariff barriers, may have something to do with the matter. This opinion seems even to be forced upon thoughtful observers by the general character of the facts, whether or no the observers think in terms of the economists' conception of increasing returns. In certain industries, although by no means in all, productive methods are economical and profitable in America which would not be profitable elsewhere. The importance of coal and iron and other natural resources needs no comment. Taking a country's economic endowment as given, however, the most important single factor in determining the effectiveness of its industry appears to be the size of the market. But

just what constitutes a large market? Not area or population alone, but buying power, the capacity to absorb a large annual output of goods. This trite observation, however, at once suggests another equally trite, namely, that capacity to buy depends upon capacity to produce. In an inclusive view, considering the market not as an outlet for the products of a particular industry, and therefore external to that industry, but as the outlet for goods in general, the size of the market is determined and defined by the volume of production. If this statement needs any qualification, it is that the conception of a market in this inclusive sense—an aggregate of productive activities, tied together by trade—carries with it the notion that there must be some sort of balance, that different productive activities must be proportioned one to another.

Modified, then, in the light of this broader conception of the market, Adam Smith's dictum amounts to the theorem that the division of labour depends in large part upon the division of labour. This is more than mere tautology. It means, if I read its significance rightly, that the counter forces which are continually defeating the forces which make for economic equilibrium are more pervasive and more deeply rooted in the constitution of the modern economic system than we commonly realise. Not only new or adventitious elements, coming in from the outside, but elements which are permanent characteristics of the ways in which goods are produced make continuously for change. Every important advance in the organisation of production, regardless of whether it is based upon anything which, in a narrow or technical sense, would be called a new "invention," or involves a fresh application of the fruits of scientific progress to industry, alters the conditions of industrial activity and initiates responses elsewhere in the industrial structure which in turn have a further unsettling effect. Thus change becomes progressive and propagates itself in a cumulative way.

The apparatus which economists have built up for the analysis of supply and demand in their relations to prices does not seem to be particularly helpful for the purposes of an inquiry into these broader aspects of increasing returns. In fact, as I have already suggested, reliance upon it may divert attention to incidental or partial aspects of a process which ought to be seen as a whole. If, nevertheless, one insists upon seeing just how far one can get into the problem by using the formulas of supply and demand, the simplest way, I suppose, is to begin by inquiring into the operations of reciprocal demand when the commodities exchanged

are produced competitively under conditions of increasing returns and when the demand for each commodity is elastic, in the special sense that a small increase in its supply will be attended by an increase in the amounts of other commodities which can be had in exchange for it.¹ Under such conditions an increase in the supply of one commodity *is* an increase in the demand for other commodities, and it must be supposed that every increase in demand will evoke an increase in supply. The rate at which any one industry grows is conditioned by the rate at which other industries grow, but since the elasticities of demand and of supply will differ for different products, some industries will grow faster than others. Even with a stationary population and in the absence of new discoveries² in pure or applied science there are no limits to the process of expansion except the limits beyond which demand is not elastic and returns do not increase.

If, under these hypothetical conditions, progress were unimpeded and frictionless, if it were not dependent in part upon a process of trial and error, if the organisation of industry were always such as, in relation to the immediate situation, is most economical, the realising of increasing returns might be progressive and continuous, although, for technical reasons, it could not always proceed at an even rate. But it would remain a process requiring time. An industrial dictator, with foresight and knowledge, could hasten the pace somewhat, but he could not achieve an Aladdin-like transformation of a country's industry, so as to reap the fruits of a half-century's ordinary progress in a few years. The obstacles are of two sorts. First, the human material which has to be used is resistant to change. New trades have to be learnt and new habits have to be acquired. There has to be a new geographical distribution of the population and established communal groups have to be broken up. Second, the accumulation of the necessary capital takes time, even though the process of accumulation is largely one of turning part of an increasing product into forms which will serve in securing a further increase of product. An acceleration of the rate of accumulation encounters increasing costs, into which both technical and psychological elements enter. One who likes

¹ If the circumstance that commodity *a* is produced under conditions of increasing returns is taken into account as a factor in the elasticity of demand for *b* in terms of *a*, elasticity of demand and elasticity of supply may be looked upon as different ways of expressing a single functional relation.

² As contrasted with such new ways of organising production and such new "inventions" as are merely adaptations of known ways of doing things, made practicable and economical by an enlarged scale of production.

to conceive of all economic processes in terms of tendencies towards an equilibrium might even maintain that increasing returns, so far as they depend upon the economies of indirect methods of production and the size of the market, are offset and negated by their costs, and that under such simplified conditions as I have dealt with the realising of increasing returns would be spread through time in such a way as to secure an equilibrium of costs and advantages. This would amount to saying that no real economic progress could come through the operation of forces engendered *within* the economic system—a conclusion repugnant to common sense. To deal with this point thoroughly would take us too far afield. I shall merely observe, first, that the appropriate conception is that of a *moving* equilibrium, and second, that the costs which (under increasing returns) grow less rapidly than the product are not the “costs” which figure in an “equilibrium of costs and advantages.”

Moving away from these abstract considerations, so as to get closer to the complications of the real situation, account has to be taken, first, of various kinds of obstacles. The demand for some products is inelastic, or, with an increasing supply, soon becomes so. The producers of such commodities, however, often share in the advantages of the increase of the general scale of production in related industries, and so far as they do productive resources are released for other uses. Then there are natural scarcities, limitations or inelasticities of supply, such as effectively block the way to the securing of any important economies in the production of some commodities and which impair the effectiveness of the economies secured in the production of other commodities. In most fields, moreover, progress is not and cannot be continuous. The next important step forward is often initially costly, and cannot be taken until a certain quantum of prospective advantages has accumulated.

On the other side of the account are various factors which reinforce the influences which make for increasing returns. The discovery of new natural resources and of new uses for them and the growth of scientific knowledge are probably the most potent of such factors. The causal connections between the growth of industry and the progress of science run in both directions, but on which side the preponderant influence lies no one can say. At any rate, out of better knowledge of the materials and forces upon which men can lay their hands there come both new ways of producing familiar commodities and new products, and these last have a presumptive claim to be regarded as em-

bodying more economical uses of productive resources than the uses which they displace. Some weight has to be given also to the way in which, with the advance of the scientific spirit, a new kind of interest—which might be described as a scientific interest conditioned by an economic interest—is beginning to infiltrate into industry. It is a point of controversy, but I venture to maintain that under most circumstances, though not in all, the growth of population still has to be counted a factor making for a larger *per capita* product—although even that cautious statement needs to be interpreted and qualified. But just as there may be population growth with no increase of the average *per capita* product, so also, as I have tried to suggest, markets may grow and increasing returns may be secured while the population remains stationary.

It is dangerous to assign to any single factor the leading rôle in that continuing economic revolution which has taken the modern world so far away from the world of a few hundred years ago. But is there any other factor which has a better claim to that rôle than the persisting search for markets? No other hypothesis so well unites economic history and economic theory. The Industrial Revolution of the eighteenth century has come to be generally regarded, not as a cataclysm brought about by certain inspired improvements in industrial technique, but as a series of changes related in an orderly way to prior changes in industrial organisation and to the enlargement of markets. It is sometimes said, however, that while in the Middle Ages and in the early modern period industry was the servant of commerce, since the rise of “industrial capitalism” the relation has been reversed, commerce being now merely an agent of industry. If this means that the finding of markets is one of the tasks of modern industry it is true. If it means that industry imposes its will upon the market, that whereas formerly the things which were produced were the things which could be sold, now the things which have to be sold are the things that are produced, it is not true.

The great change, I imagine, is in the new importance which the *potential market* has in the planning and management of large industries. The difference between the cost per unit of output in an industry or in an individual plant properly adapted to a given volume of output and in an industry or plant equally well adapted to an output five times as large is often much greater than one would infer from looking merely at the economies which may accrue as an existing establishment gradually extends the

scale of its operations. Potential demand, then, in the planning of industrial undertakings, has to be balanced against potential economies, elasticity of demand against decreasing costs. The search for markets is not a matter of disposing of a "surplus product," in the Marxian sense, but of finding an outlet for a potential product. Nor is it wholly a matter of multiplying profits by multiplying sales; it is partly a matter of augmenting profits by reducing costs.

Although the initial displacement may be considerable and the repercussions upon particular industries unfavourable, the enlarging of the market for any one commodity, produced under conditions of increasing returns, generally has the net effect, as I have tried to show, of enlarging the market for other commodities. The business man's mercantilistic emphasis upon markets may have a sounder basis than the economist who thinks mostly in terms of economic statics is prone to admit. How far "selling expenses," for example, are to be counted sheer economic waste depends upon their effects upon the aggregate product of industry, as distinguished from their effects upon the fortunes of particular undertakings.

Increasing returns are often spoken of as though they were attached always to the growth of "industries," and I have not tried to avoid that way of speaking of them, although I think that it may be a misleading way. The point which I have in mind is something more than a quibble about the proper definition of an industry, for it involves a particular thesis with respect to the way in which increasing returns are reflected in changes in the organisation of industrial activities. Much has been said about industrial integration as a concomitant or a natural result of an increasing industrial output. It obviously is, under particular conditions, though I know of no satisfactory statement of just what those particular conditions are. But the opposed process, industrial differentiation, has been and remains the type of change characteristically associated with the growth of production. Notable as has been the increase in the complexity of the apparatus of living, as shown by the increase in the variety of goods offered in consumers' markets, the increase in the diversification of intermediate products and of industries manufacturing special products or groups of products has gone even further.

The successors of the early printers, it has often been observed, are not only the printers of to-day, with their own specialised establishments, but also the producers of wood pulp, of various

kinds of paper, of inks and their different ingredients, of type-metal and of type, the group of industries concerned with the technical parts of the producing of illustrations, and the manufacturers of specialised tools and machines for use in printing and in these various auxiliary industries. The list could be extended, both by enumerating other industries which are directly ancillary to the present printing trades and by going back to industries which, while supplying the industries which supply the printing trades, also supply other industries, concerned with preliminary stages in the making of final products other than printed books and newspapers. I do not think that the printing trades are an exceptional instance, but I shall not give other examples, for I do not want this paper to be too much like a primer of descriptive economics or an index to the reports of a census of production. It is sufficiently obvious, anyhow, that over a large part of the field of industry an increasingly intricate nexus of specialised undertakings has inserted itself between the producer of raw materials and the consumer of the final product.

With the extension of the division of labour among industries the representative firm, like the industry of which it is a part, loses its identity. Its internal economies dissolve into the internal and external economies of the more highly specialised undertakings which are its successors, and are supplemented by new economies. In so far as it is an adjustment to a new situation created by the growth of the market for the final products of industry the division of labour among industries is a vehicle of increasing returns. It is more than a change of form incidental to the full securing of the advantages of capitalistic methods of production—although it is largely that—for it has some advantages of its own which are independent of changes in productive technique. For example, it permits of a higher degree of specialisation in management, and the advantages of such specialisation are doubtless often real, though they may easily be given too much weight. Again, it lends itself to a better geographical distribution of industrial operations, and this advantage is unquestionably both real and important. Nearness to the source of supply of a particular raw material or to cheap power counts for most in one part of a series of industrial processes, nearness to other industries or to cheap transport in another part, and nearness to a larger centre of population in yet another. A better *combination* of advantages of location, with a smaller element of compromise, can be had by the more

specialised industries. But the largest advantage secured by the division of labour among industries is the fuller realising of the economies of capitalistic or roundabout methods of production. This should be sufficiently obvious if we assume, as we must, that in most industries there are effective, though elastic, limits to the economical size of the individual firm. The output of the individual firm is generally a relatively small proportion of the aggregate output of an industry. The degree in which it can secure economies by making its own operations more roundabout is limited. But certain roundabout methods are fairly sure to become feasible and economical when their advantages can be spread over the output of the whole industry. These potential economies, then, are segregated and achieved by the operations of specialised undertakings which, taken together, constitute a new industry. It might conceivably be maintained that the *scale* upon which the firms in the new industry are able to operate is the secret of their ability to realise economies for industry as a whole, while presumably making profits for themselves. This is true in a way, but misleading. The scale of their operations (which is only incidentally or under special conditions a matter of the size of the individual firm) merely reflects the size of the market for the final products of the industry or industries to whose operations their own are ancillary. And the principal advantage of large-scale operation at this stage is that it again makes methods economical which would be uneconomical if their benefits could not be diffused over a large final product.

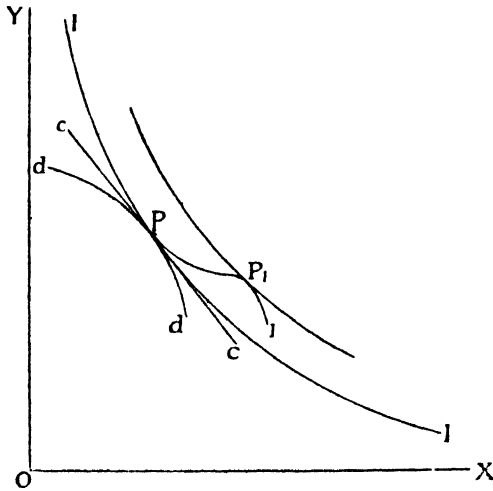
In recapitulation of these variations on a theme from Adam Smith there are three points to be stressed. First, the mechanism of increasing returns is not to be discerned adequately by observing the effects of variations in the size of an individual firm or of a particular industry, for the progressive division and specialisation of industries is an essential part of the process by which increasing returns are realised. What is required is that industrial operations be seen as an interrelated whole. Second, the securing of increasing returns depends upon the progressive division of labour, and the principal economies of the division of labour, in its modern forms, are the economies which are to be had by using labour in roundabout or indirect ways. Third, the division of labour depends upon the extent of the market, but the extent of the market also depends upon the division of labour. In this circumstance lies the possibility of economic progress, apart from the progress which comes as a result of the new knowledge

which men are able to gain, whether in the pursuit of their economic or of their non-economic interests.

ALLYN A. YOUNG

NOTE

IN the accompanying construction (which owes much to Pareto), a collective indifference curve, I , is defined by the condition that, at equal cost, there would be no sufficient inducement for the community to alter an annual production of x units of one commodity and y units of another in order to secure the alternative combination of the two commodities indicated by any other point on the curve.¹ Each commodity might be taken as representative of a special class of commodities,



produced under generally similar conditions. Or one commodity might be made to represent "other goods in general," the annual outlay of productive exertions being regarded as constant. Alternatively, one commodity might represent "leisure" (as a collective name for all non-productive uses of time). The other would then represent the aggregate economic product.

There will be equilibrium (subject to instability of a kind which will be described presently) at a point P , if at that point a curve of equal costs, such as d , is tangent to the indifference curve. The curve of equal costs defines the terms upon which the community can exchange one commodity for the other by merely producing less of the one and more of the other (abstraction being made of any incidental costs of

¹ The collective indifference is to be taken as an expository device, not as a rigorous conception. The relative weights to be assigned to the individual indifference curves of which it is compounded will depend upon how the aggregate product is distributed, and this will not be the same for all positions of P .

change). Negative curvature, as in d , reflects a condition of decreasing returns, in the sense that more of either commodity can be had only by sacrificing progressively larger amounts of the other. Although a sufficient condition, the presence of decreasing returns is not a necessary condition of equilibrium. There would be a loss in moving away from P if equal costs were defined by the straight line c , which represents constant returns. Increasing returns, even, are consistent with equilibrium, provided that the degree of curvature of their graph is less than that of the indifference curve. It might happen, of course, that returns would decrease in one direction and increase in the other. Curve d , for example, might have a point of inflexion at or near P .

Consider now the conditions of departure from equilibrium. The curve i is drawn so as to represent *potential* increasing returns between P and P_1 , which lies on a preferred indifference curve. If these increasing returns were to be had merely for the taking, if i were, for example, merely a continuation of the upper segment of d or c , P would not be a point even of unstable equilibrium. The advance from P to P_1 would be made by merely altering the proportions of the two commodities produced annually. To isolate the *problem* of increasing returns it is necessary to assume that P is a true point of equilibrium in the sense that it is determined by a curve of equal costs, such as d or c . The problem, then, has to do with the way in which the lower segment of d or c can be transformed into or replaced by such a curve as i . This requires, of course, that *additional* costs be incurred, of a kind which have not yet been taken into account. To diminish the amount of the one commodity which must be sacrificed for a given increment of the other, some of the labour hitherto devoted to its production must be used indirectly, so that the increase of the annual output of the one lags behind the curtailing of the output of the other.

This new element of cost might be taken into account by utilising a third dimension, but it is simpler to regard it as operating upon Δx , the increment in x accompanying the movement from P to P_1 , so as to move the indifference curve upon which P_1 lies towards the left. It would be an error, however, to think that the combinations of x with y and $x + (\Delta x)$ with $y - \Delta y$ (where (Δx) is the contracted form of Δx) are themselves indifferent, so that P_1 is, in effect, brought over on to the original indifference curve, I , and no advantage is reaped. The path from P to P_1 is a *preferred* route, not merely a segment of an indifference curve. The cost of moving along that route is a function of the *rate* (in time) of the movement. An equilibrium rate (which need not be constant), such as would keep the movement from P to P_1 continuous and undeviating, would be determined by the condition, not that (Δx) and $-\Delta y$ should negate one another, but that either an acceleration or a retarding of the rate would be costly or disadvantageous. Because a mountain climber adjusts his pace to his physical powers and to the conditions of the ascent, it does not follow that he might as well have stayed at the foot. Or, alternatively but not inconsistently,

the movement from P to P_1 may be conceived as made up of a series of small steps, each apparently yielding no more than a barely perceptible advantage, but only because the scale of reference for both costs and advantages depends at each step upon the position which has then been reached.

Several sets of circumstances will affect the amount and direction of the movement. (1) Even if i has no point of inflexion, such as has been indicated at P_1 (merely to simplify the first stages of this analysis), it will sooner or later (taking into account the "contraction" of Δx) become tangent to an indifference curve. In the absence of any other factor making for change, progress would then come to an end. (2) There may be another possible alternative path of increasing returns extending upwards from P and curving away from I . The most advantageous route will then be a compromise between (or a resultant of) the two limiting alternatives. In such circumstances the only effective limitation imposed upon the extent of the movement may come from the failure of elasticity of demand on one side or the other. (3) Successive indifference curves cannot be supposed to be symmetrical, in the sense that dy/dx remains the same function of y/x . If, for example, the slope of successive indifference curves at points corresponding to given values of y/x decreases (indicating that the demand for the commodity measured in units of y is relatively inelastic), freedom of movement in the direction of P_1 is reduced, while it becomes advantageous to move a little way in the opposite direction along even such a path as c or d . Under inverse conditions (with $-dy/dx$ increasing relatively to y/x for successive indifference curves) the extent of the possible movement in the direction of P_1 is increased. This conclusion amounts to no more than the obvious theorem that the degree in which the decreasing returns encountered in certain fields of economic activity operate as a drag upon the securing of increasing returns in other fields depends upon the relative elasticities of demand for the two types of products. But this consideration, like the others of which note has been made, serves to make clear the general nature of the reciprocal relation between increasing returns and the "extent of the market." (4) Discoveries of new supplies of natural resources or of new productive methods may have either or both of two kinds of effects. They may tilt the curves of equal cost and they may modify their curvature favourably. In either event a point such as P is moved to a higher indifference curve, and the paths along which further progress can be made are altered advantageously.

THE ECONOMIC JOURNAL

SEPTEMBER, 1928

THE INSTABILITY OF CAPITALISM

I. *Economic Stability under Static Conditions*

§ 1. THE many "instabilities" created by the War and by post-war vicissitudes, whilst very properly engaging the attention of economists in all countries both as to diagnosis and as to remedial policy, do not, in themselves, present to science any new or startling problems. There is nothing strange in the fact that events such as the breakdown of Russia or, generally, disturbances arising from without the sphere of economic life, should affect its structure, its data and its working. In this paper I shall disregard them entirely, and deal merely with the question whether or not the capitalistic system is stable in itself—that is to say, whether or not it would, in the absence of such disturbances, show any tendency towards self-destruction from inherent economic causes, or towards out-growing its own frame. The interest of such an investigation is primarily scientific; still, an answer to that question is not without some diagnostic value, and, therefore, not without some, if remote, bearing upon policy; especially as there is, it seems to me, a marked tendency to reason upon post-war figures and about post-war problems, exactly as if they reflected something like the normal working of our economic system, and to proceed, on this basis, to conclusions about the system as such.

By way of clearing the ground, it may be well, first, to distinguish the kind of stability or instability we propose to discuss, from other phenomena covered by the same terms. Looking, for instance, at France, with her stationary population and enterprise and her vast colonial empire, and at the opposite state of things in Italy, the observer may well have an impression of instability—let us call it, "political" instability—which, however, has nothing to do with economic instability in our sense; for in the economic systems of these countries there might still be perfect stability. Or if we assume a state of things in which the whole of the industry of a country is

No. 151.—VOL. XXXVIII.

monopolised by one single firm, we should probably agree in calling such a system unstable in a very obvious sense—let us label the case as one of “social instability”—whilst it could be highly stable economically. Instability in still another sense would obtain in a system, for which equilibrium wages were at a point below what workers will put up with—although there need not be any tendency in the economic conditions themselves to produce any change at all *by the mere working of the system*. Finally, special cases of instability may arise from particular influences from without, which cannot properly be charged to the economic system at all. England’s return to the gold standard is a case in point. “Stabilising” the pound at what was, viewed from the standpoint of existing conditions, an artificial value, naturally meant dislocating business, putting a premium on imports and a tax on exports, intensifying losses and unemployment, thereby creating a situation eminently unstable. But this instability is evidently due to the act of politicians, and not to the working of the system which, on the contrary, would have evolved a value of the pound exactly fitting the circumstances. In short, the economic stability we mean, although it *contributes* to stability in other senses, is not *synonymous* with them, nor does it *imply* them. This view must, of course, seem highly superficial to anyone who assumes the existence of as close a relation between the economic and other spheres of social life as, for instance, Marx did. As, however, it would be waste of time to prove to English readers the necessity of separating these several spheres, I may confine myself to these remarks.

Secondly, we have to define what we mean by “our economic system”: We mean an economic system characterised by private property (private initiative), by production for a market and by the phenomenon of credit, this phenomenon being the *differentia specifica* distinguishing the “capitalist” system from other species, historical or possible, of the larger genus defined by the two first characteristics. Although few things seem to me to be more firmly established by historical research than the fact that economic history cannot be divided into epochs corresponding to different systems, it is still permissible to date the *prevalence* of capitalistic methods from about the middle of the eighteenth century (for England), and to call the nineteenth century *κατ’ ἐξοχήν* the time of *competitive*, and what has so far followed, the time of increasingly “*trustified*,” or otherwise “*organised*,” “*regulated*,” or “*managed*,” capitalism.

Thirdly, capitalism may be stable or not, simply in the sense that it may be expected to last or not. Its history might be full of the most violent fluctuations or even catastrophes—as it undoubtedly has been so far—and these fluctuations or catastrophes might even be inherent in its working—which precisely is what we want to form an opinion about—and we might still, in a real sense, have to call it “stable” if we have reason to expect it to last. Whenever we mean no more than this—that is to say, when we merely mean to speak of the question of what may be termed the institutional survival of capitalism, we will henceforth speak of the capitalist *order* instead of the capitalist *system*. When speaking of the stability or instability of the capitalist *system*, we shall mean something akin to what business men call stability or instability of business conditions. Of course, mere instability of the “system” would, if severe enough, threaten the stability of the “order,” or the “system” may have an inherent tendency to destroy the “order” by undermining the social positions on which the “order” rests.

§ 2. The business man's meaning of stability we have now to translate into the language of theory. It will shorten matters and facilitate exposition if I state at the outset that, barring differences on a number of particular points, the following remarks run entirely on Marshallian lines. But I could equally well call them Walrasian lines. For within serious economic theory there are no such things as “schools” or differences of principle, and the only fundamental cleavage in modern economics is between good work and bad. The basic lines are the same in all lands and in all hands: there are differences in exposition, in the manner—and mannerism—of putting things, for example, according to the relative importance different authors attach, respectively, to rigour and generality or to vicinity to “real life.” Then there are differences in technique, the very greatness of Menger, Böhm-Bawerk and Wieser, for example, consisting in their having achieved so much with such shockingly clumsy and primitive tools, the use of which was an insurmountable bar to correctness. There are, furthermore, differences in individual pieces of the analytic machine—as, for example, between the Walrasian and the Marshallian demand curves, or between the rôle assigned to coefficients of production respectively by Marshall and Walras—Pareto—Barone. Finally, there are differences as to particular problems, the most important of which are the theories of interest and of the business cycle. But this is all. There is no difference in fundamentals—Clark's productivity or Walras' equilibrium or

the Austrian imputation or Marshall's substitution or Wicksell's compound of Walras and Boehm-Bawerk being all of them in the last analysis the same thing, and all, in spite of appearances to the contrary, equally far removed from, and at the same time and in the same sense descendants of, Ricardo's patchwork.

The economic system in the sense of conditions and processes reduces itself for the purposes of Theory to a system in the scientific sense of the word—a system, that is, of interdependent quantities—variables and parameters—consisting of quantities of commodities, rates of commodities and prices, mutually determining each other. This system has been found to be stable, and its stability to be amenable to rational proof, under static conditions. Not as stable, it is true, as economists would have held sixty years ago, when most of them—nearly all, in fact, except the Marxists—would have most confidently asserted absolute stability both of the capitalist *order* and the capitalist *system*: stability has fared very much as the theory of maximum satisfaction did. Just as newer methods, whilst yielding correct proof of what they left of the competitive maximum, have considerably taken away from its importance, so similarly, whilst showing that we have, generally, as many equations as we have “unknown” quantities, and therefore a determined state of equilibrium corresponding to a given set of certain data which turns out to be stable under appropriate conditions, they have also shown that the exceptions to this general “determinateness” are considerable. Even apart from cases such as the possibility of the offer curve of labour ¹ curling back or such as the case of the value of money in a system of bimetallism without legal ratio,² we have many instances where equilibrium cannot be said to be determinate. The case where both supply and demand are inelastic, is an example.³ It may be said, for example, that the

¹ This, of course, does not make equilibrium entirely indeterminate, but only makes the system have several, mostly two, different solutions.

² It is worth while emphasising, however, that there is no indeterminateness when two or more commodities circulate as money and every transaction is concluded specifically in one of them. The instability only arises if contracts are in terms of “money” generally, so that payment can be made in any of those commodities.

³ Another has been pointed out by Wicksell, *Geldwert und Güterpreise*: If coefficients of production be constant and if there be no alternative use for the factors of production—their quantity being, moreover, fixed—then there would be indeterminateness of their shares in the product. Still others have been discussed by Marshall, Edgeworth, Taussig (“Is Market Price Determinate?” *Quarterly Journal of Economics*, 1921, and Divisia (*Economique rationnelle*, 1928, p. 410: This case of indeterminateness arises only from the absence of any true marginal utility of money. It has been pointed out before by Prof. Cassel, and is, of course, easily remedied.)

home demand for wheat in the United States is highly inelastic within a considerable interval of price. Supply, again, though very variable, is equally inelastic—if it be permitted to apply this term to supply for shortness sake—within intervals of time too short to allow for extension or contraction of acreage; and this may, perhaps, partially explain the instability of American farming.

But although illustrations of this and other cases abound, the determinateness of static equilibrium under competitive conditions is yet a broad basic fact, and this equilibrium is stable, provided that supply price¹—the price of “willingness to sell”—is an increasing function of quantity of product. This condition rests on the fundamental fact that the extending of production by any given industry means withdrawing quantities of factors of production from increasingly “important” other uses, which, of course, does not show within single firms—any more than the influence on demand price of increasing output shows within the field of action of single firms in a state of pure competition—but is yet the force the balancing of which against decreasing marginal utilities of product determines the distribution of resources between industries. There is, it is true, an interval for practically every industry in which this condition is not satisfied, owing to the tendency which it embodies being over-compensated by fixed costs distributing themselves over an increasing number of units of product. As long as this is the case, there cannot be a point of stable equilibrium.² But the

¹ The supply price schedule meant here is the series of supply prices at which, given the methods of production actually in use and embodied in given plants and under given general conditions and trade practices, the respective quantities of product would be forthcoming. The schedule, therefore, refers, in an obvious sense, to a point of time. It does not, however, take account of chance occurrences, such as momentary market situations on the one hand; and it does not, on the other hand, take account of any but marginal adjustments, *capable of being decomposed into infinitesimal steps*: so it might be called a short period, normal. But the objections to this would be the implication of the existence of some long-period normal and, besides, the emphasis which this manner of expression lays on the element of time, whilst the important thing is not the lapse of time as such, but what happens during it.

² Not even if, in the familiar illustration, the demand curve cut the supply curve negatively. For even then it must be to the interest of every single producer, who *ex hypothesi* neglects the influence of his own action on price, to go on producing in this case. Whilst this lasts, there is *movement* towards equilibrium (and this distinguishes *this* case of “increasing returns” fundamentally from others), but not equilibrium itself. Whilst other cases of the compound called “increasing returns” *vires acquirunt eundo*, and thereby may lead up to a monopoly, this one can hardly do so. It may offer, however, instances of increasing cost for an industry as a whole in the face of the presence of decreasing unit cost in every single firm.

effect of this spends itself necessarily and, therefore, stable equilibrium will nevertheless eventually emerge, although there may, and often will, be a prior instability—instability of the kind which is one of the sources of what is called “over-production.”

Any other cause of “increasing cost” is excluded by the static hypothesis, the justification for accepting such an arrangement being that it separates clearly different sets of phenomena, which stand in need of different treatment. Innovations in productive and commercial methods, in the widest sense of the term—including specialisation and the introduction of production on a scale different from the one which ruled before—obviously alter the *data* of the static system and constitute, whether or not they have to do with “invention,” another body of facts and problems. And so does that part of “external economies,” which is represented by such instances as the trade journal, the bureau of standards, the “pooling” of reserve stocks of materials incident to the presence of a large market in them and so on. The reader is asked to stay judgment about the exclusion of these things until later. Here it is only necessary to point out that we should have to emphasize the heterogeneous nature of all these phenomena the very moment we included them. In any case we should have to recognise that there is no “law of decreasing cost” of the same kind as, and symmetrical to, the law of increasing cost.¹ The relation of the two can, perhaps, be best seen by means of the analogy with the “demand side”

¹ By law of increasing cost we may mean four things entirely independent of one another: first, we may, as above, mean what is of the very essence of the economic process and, indeed, only another way of stating the law of satiable wants, that the significance of successive doses of means of production must always increase as they are drawn into any one industry for the reason that they are actually or virtually taken away from others. Secondly, we may, as pointed out before, mean that successive doses of any one factor of production applied to a constant quantity of the others yield a decreasing physical increment of product, everything, especially method, remaining the same. The most “practical” way of making use of this proposition is to consider a given plant, embodying both a given method of production and an inelastic set of supplementary costs, and to vary elements of prime cost one at a time. This is perhaps the best tool we have to deal with the routine work of the management of a single firm. It has, however, nothing whatever to do, thirdly, with a community being driven in the process of expansion of production to exploit less and less fertile productive opportunities. This has been well stated in Prof. Sraffa's acute study, “Relazioni fra costo e quantità prodotta,” *Annali di Economia*, 1925, epitomised in an article in this Journal, December 1926, and commented on by Prof. Pigou in the issue for June 1927. And, fourthly, there is the prophecy to which Ricardo owes the epithet of pessimist, that improvements (in agriculture) of productive methods will in the long run fail to counterbalance increasing costs in the second and third sense, in case population should keep on increasing.

of the problem. Empirically we evidently could arrive in very many cases at demand curves which would slope upwards instead of down (cp., for example, Prof. Moore's demand-curve for pig iron). And there are, of course, very many similar cases, the special point of interest about the pig-iron curve being the fact that its periodicity is indicative of the business cycle. Nobody, however, thinks less on that account of what is universally considered to be the "true" slope of the theoretic demand curve. Everybody, on the contrary, recognises that what happens in such cases is a shifting—by which term we mean to cover inexactly not only displacement but also distortion—of the theoretic curves, every one of which retains its fundamental characteristic in obedience to the "law" it has been constructed to represent, and that any curve displaying a positive slope is merely a statistical¹ or historical curve fitted through a family of successive theoretic ones. The same applies to—if I be permitted to waive for the sake of shortness the objections to speaking of so doubtful a thing—supply curves. There is only one theoretic supply curve; and it slopes upwards in all cases. Changes of data do not make it slope down, but shift it, or, more correctly, break it off² and start a new one. And through these changing positions—in all of which these curves retain their slope and meaning—we may, if we so choose, fit historical curves, which will certainly often slope down. They will, in fact, display

¹ The theoretic curve can, of course, be determined statistically without ceasing to be a theoretic curve, the above distinction not turning on the fact, or possibility, of statistical determination, but on whether or not the curve expresses or illustrates a *theorem*, thereby acquiring logical unity as distinguished from what could be termed "descriptive" unity. Now I am far from overrating the importance of this distinction: On the one hand, theory itself is only a way of describing facts; on the other hand, any descriptive unity may, by some progress of analysis, turn into a logical unity any moment—in fact, the frontier between the two continually shifts in the progress of science. But this is no reason for simply ignoring it and for co-ordinating things, which do not stand on the same plane.

² This links up with another distinction, the importance of which is best seen by means of an example: Von Böhm-Bawerk's theory of interest stresses the importance of the "roundabout" process of production. But it is not the *running* of production of a given degree of roundaboutness which matters, but the *act of introducing* greater "roundaboutness." There is a drop—in its nature discontinuous, irregular, "unpredictable" and "historically" unique—in costs the moment production starts on the new plan (on *any* successful new plan, no matter whether it involves roundaboutness or not), but there is no further and continuous saving of costs per unit of product in the running of it. Generalising: Changes of *data* may be represented by lines connecting the displaced and distorted theoretic curves. If they are small and frequent, these lines may themselves *look* like our curves. But they never *are* theoretic curves and have not, in this sense, any theoretic meaning.

no regularity at all. It may not even be quite easy, in some cases, to guard against the supreme misfortune of total cost being actually smaller for a greater output than for a lesser one, for changes of *data*, once admitted, would sometimes produce this result, which could not, in competitive circumstances, be handled by assuming that the larger quantity would be produced but partially destroyed.¹

There is nothing new or startling in thus limiting the scope of this part of our analytic engine. In fact, we are doing no more than to sum up what has been an unmistakable doctrinal tendency ever since it came to be recognised, first, that increasing cost in the sense of decreasing physical response to productive effort applied to a constant quantity of one of the factors is no peculiarity of agriculture, but a general phenomenon—a phenomenon which, given the same conditions, applies to all kinds of production and, given other conditions, does not apply even to agriculture; secondly, that there is a more fundamental tendency at work to make the second derivative of total cost with respect to output positive, and one which has nothing to do with the physical “law of decreasing returns,” whence the difficulty of filling certain empty boxes. We are merely clinching, on the one hand, what seems to us to be the true real-cost-phenomenon, and, on the other hand, what seems to us to be both the meaning of economic “statics” and the nature of static equilibrium. That this is perfectly in keeping with the fundamental drift of Marshallian analysis, I will try to show in a footnote.²

¹ Cf. H. Schultz, “Theoretical Considerations Relating to Supply,” *Journal of Political Economy* for August 29, p. 441. Therefore the assumption $\frac{dy}{dx} > 0$ remains arbitrary, unless reinforced by Cunynghame’s criterion $\frac{dy}{dx} > \frac{y}{x}$.

² Marshall, indeed, repeatedly protests against the limitations of the static apparatus (cf. especially a letter of his to Prof. John B. Clark). Now if it were true that reasoning by means of it is “too far removed from life to be useful,” then the greater part of the analysis of the *Principles* would be useless—as would be the greater part of any exact science: For Marshallian analysis rests just as much on static assumptions as Prof. Clark’s structure. But it is not true. There is nothing unduly abstract in considering the phenomena incident to the running of economic life under given conditions taken by themselves. On the contrary, it means giving this class of problems the treatment they require. And Marshall himself has contributed substantially to the perfection of this treatment by forging such invaluable tools as his consumer’s surplus and his quasi-rent. He has, furthermore, made use of static assumptions both in his theory of distribution and in the fundamentals of his catallactics; in fact, in one decisive point, when dealing with refinements calling for rigour of analysis, he has confined his argument to increasing cost. And he has, finally, himself insisted on the irreversibility of, and on the difficulties peculiar to, a declining supply curve, and come, in doing so, very near to saying much the same as what has been said above. Loyalty to

§ 3. There seem to be, however, two other sources of instability due to indeterminateness within the precincts of the "static" system. By universal consent, single monopoly yields determined and stable equilibrium, but dual and multiple monopoly, or, generally, the case in which firms can and do take account of their own influence on price, is held, by very high authorities, to fail to do so. Cournot's treatment and the objections raised against it, first by Bertrand and then by Edgeworth, are well known. As this case is not only more important practically than either of the cases of "free, pure or simple" competition on the one hand, and of single monopoly on the other, but also the more general one in a theoretic sense—for the competitive hypothesis is, after all, an additional condition and very much in the nature of a crutch—the breach in our wall seemed a rather serious one. To clear up the matter has been one of the last of the many services Knut Wicksell has rendered to science.¹

tradition, aversion to appearing too "theoretical"—which carried so much weight with him—and that tendency of his, to which we owe so much in other respects, to take short cuts to the problems of practical life, may account for his not taking the final step and for what I cannot but agree with Mr. Keynes in considering the least satisfactory part of his analysis, successfully assailed by Prof. Sraffa. This entailed a string of consequences, but fundamentally what we have said is but a development of a trend overlaid indeed by other things, but yet present in the *Principles*.

We may add the weight of Prof. Pigou's authority. For in the article quoted in a previous note, he excludes, for the sake of "logical coherence" of the cost function, the bulk of those phenomena, which we ourselves propose to exclude for the same reason. He, indeed, even rules out what we have called the fundamental law of cost ($\phi''(x) > 0$). But this he does merely on the technical ground that it is "impossible to construct a cost function" in the event of changes in the relative values of factors of production being liable to occur in consequence of changes in the scale of production of an industry. On the other hand, he does not entirely rule out external economies. But what he retains of them are merely "variations in aggregate costs associated with, and due to, variations in the scale of output" (*l.c.* p. 189); and if we insert, as we must, the word "automatically" in this sentence, very few, if any, cases will be found to answer the criterion, as has been pointed out by Prof. Young (*Quarterly Journal of Economics*, August 1913, p. 678). Of course, expansion and improvement are closely allied in real life. But, as we shall try to explain in the text, the main causation is the one from improvement to expansion and cannot adequately be dealt with by static analysis at all. If this be correct, Prof. Pigou's position will be seen to approach closely the one taken up in the text, if the reader take hold of the fact, that economies, before becoming "external," must generally be internal ones in some firm or firms of the same or some other industry.

I do not mean, furthermore, to raise by what I have said objections to the attempts to determine cost functions statistically. On the contrary, I am a humble admirer of the pioneer work done by Prof. H. L. Moore and his followers, even though I beg leave to point out that to speak of "moving equilibria" may prove misleading, in the face of the fact that what really happens is *destruction of equilibria* in the received meaning of this term.

¹ It is with reluctance that I contradict the great shade of Edgeworth. But there seems to be no warrant to assume indeterminateness in the case of what

The simplest form of the second case of what I call "corresponsive prices" is presented by exchange between two monopolists. It is again Prof. Edgeworth's authority which accounts for well-nigh universal acceptance of the view—first expressed by him in his *Mathematical Psychics*—that there is indeterminateness of price within an interval (on the contract curve) which must in general be considerable. He even went so far as to describe the state of things in a trustified economic world as a "chaos." Here, therefore, is a rich source of instability opened up. Naturally, any theorist might well be tempted to link up what instabilities he sees with this possible explanation of them. Nor can we reply by pointing to the fact that prices fixed

Prof. Pigou calls Monopolistic Competition. Taking into consideration the limiting instance only, that of Duopoly, which can be easily generalised, and assuming both competitors to be in exactly the same position, we are, first, faced by the fact that they cannot very well fail to realise their situation. But then it follows that they will hit upon, and adhere to, the price which maximises monopoly revenue for both taken together (as, whatever the price is, they would, in the absence of any preference of consumers for either of them, have to share equally what monopoly revenue there is). The case will not differ from the case of conscious combination—in principle—and be just as determinate. The only other alternative which presents itself in the absence of any hope of driving the competitor out of the market, is best "visualised" by starting from one monopolist controlling the market and then introducing a second one (Cournot's procedure). It is perhaps more "realistic" to assume that the first monopolist will not, as would be to his ultimate advantage, readily surrender half of his market to the newcomer, but that the latter will have to force his way in. And this case is equally determinate, as has been shown by Wickcell in his review article on Prof. Bowley's "Groundwork" (*Ekonomisk Tidskrift*, 1925, and *Archiv für Sozialwissenschaft*, 1927). Taking, as the unit of the price p , that price at which the output would be zero, and, similarly, as the unit of the quantity sold x , that quantity which could be disposed of at the price zero (Edgeworth), we have $p = 1 - x$. A single monopolist would, if there are no costs, maximise px and charge a price of $\frac{1}{2}$, selling $\frac{1}{2}$. The second man, having to face this situation, will obviously maximise *his* output, x , multiplied by price—that is, $x_2p = x_2(\frac{1}{2} - x_2)$, and, therefore, sell $\frac{1}{4}$. Whereupon the first will have to readjust *his* output, x_1 , and to offer $\frac{3}{8}$ and so on. This finally leads to a limit at the price of $\frac{1}{3}$, when each of the two sell $\frac{1}{3}$, the price being higher and the quantity sold smaller than under competition. There is nothing absurd in this. It cannot be objected that neither of the two competitors is justified to assume, in deciding on how to adjust his output, that the other will stick to *his*. For no such assumption is really involved, the above argument aiming only at describing the process of *tâtonnement*, out of which the equilibrium price is finally bound to emerge, and things would remain substantially the same if some of the steps were to drop out—just as the equilibrium of perfect competition does not necessarily come about by every one of the theoretical steps of bidding actually taking place in practice. Nor can it be said that the two monopolists would, on reaching what we have called the equilibrium price, try to retrace their steps. For neither of them could do so singly without losing his customers. They could do so only together—the case would become one of single monopoly. The same result has been independently arrived at by Dr. Chamberlin in his *Monopolistic Competition*, as yet unpublished.

by trusts display in many and important instances much less fluctuation than could be expected under competitive conditions; for non-economic forces, pressure of public opinion or fear of government action, for instance, might account for that. And the authority of Prof. Edgeworth has been reinforced by the not less weighty authority of Prof. Pigou.

Now it is perfectly true that there is, in this case, just as in the case of one-sided monopoly, much less *guarantee* of a tendency towards equilibrium prices actually asserting itself. We have much less reason to expect that monopolists will, in either case, charge an equilibrium price, than we have in the case of perfect competition; for competing producers *must* charge it as a rule under penalty of economic death, whilst monopolists, although having a *motive* to charge the monopolistic equilibrium price, are not forced to do so, but may be prevented from doing so by other motives. Furthermore, it is quite true also, that such things as bluffing, the use of non-economic force, a will to force the other party to their knees, have much more scope in the case of two-sided monopoly—just as cut-throat methods have in the case of limited competition—than in a state of perfect competition.

But there is yet more than academic interest in stating that our theory does not break down at this point. Equilibrium is determinate even in this case—even if we take so extreme an instance as a trade union comprising all the workmen of a country, quite sure of the allegiance of its members, capable of preventing immigration from abroad or from other strata of society, and an employers' union similarly constructed. If we assume that each party has a definite monopoly-demand-curve and knows the curve of the other; that each party wants to get the best terms it can—the workmen's union offering varying amounts of labour and providing for those of its members who may have to be kept unemployed—without attempting to attain victories or to inflict defeats; and that the contract is to cover the whole period of account (the "*uno actu*" condition), then the barter point between the parties is perfectly determined, and *not* only the range within which there will be barter. It could be indeterminate only for reasons which would make the case indeterminate also in competition. Nor can it be held that the assumptions alluded to are so very far from reality. They are, if anything, nearer to reality than the assumptions implied in the idea of theoretically perfect competition: It is, for instance, much more common than observers believe whose attention is naturally focussed on abnormal cases, for employers and workmen to meet in precisely

the frame of mind assumed, and to view with misgivings all the economic, political and social risks of holding out or of a struggle, which may turn out bad business even in the case of success. By proceeding by way of Walras' *prix crié par hazard* or simply by inspecting the two schedules plotted against one another, our statement will too readily be found to hold good to make it necessary to give formal proof.¹

§ 4. So there is rather more of stability² about the economic system than we should expect on most of the authoritative statements. But how much this amounts to, depends entirely on the nature of that other restriction, which we have introduced alongside of the competitive assumption just discarded: the "static state," which we define both by a distinguishable set of facts and by an analytic apparatus or theoretical point of view. The set of facts consists in the sum of operations which form the essence of the ever-recurring circular process of production and consumption and which make up a self-contained whole. It is no valid objection to say that this process cannot be thought of independently of growth or, generally, change. For it can.

¹ The well-known Edgeworthian apparatus commonly used to prove the contrary merely shows that the *elements described by it* do not suffice to determine more than a range. Prof. Bowley in his "Groundwork" reaches, in dealing with the case of one employer and one workman, the result of incompatibility of the respective maxima only by implying that the workman could produce the product by himself. The "Groundwork" contains, however, two most suggestive approaches to the problem of universal monopoly, the one embodied in a note carrying that title, the other leading to the theorem that there is determinateness in the case of *either* the products *or* the factors—but not both of them—being monopolised. Arguments analogous to those of our text seem to show that at least the same sort of determinateness obtains in these cases too.

² This stability is of the same nature, and its exact proof of the same value, as the stability of any other exact system. Of course, it is compatible with a large amount of instability in the actual phenomenon. Part of this instability is unimportant, both for theoretical and for practical purposes; another part, whilst practically important, is yet uninteresting in a discussion of principles; still another, however, has, as we shall see, both practical and theoretical importance. None of these groups of cases affects the fundamental importance of exact proof of stability in the sense meant, as would be obvious everywhere except in economics, where the sterility incident to the prevalence of interest in the "practical problem" has yet to be overcome and where scientific refinement is still an opprobrium. But it must be borne in mind that our arrangement excludes all important cases of determined but unstable equilibrium. For the above argument, therefore, and within our meaning of terms, determinateness spells economic stability under static conditions, although, of course, these two things do not coincide logically and always require separate proof. The shortest way to satisfy oneself on this point is by verifying the statement, that of all cases of equilibrium known to Marshallian analysis, only the stable ones remain—apart from chance equilibria which occur during the process of Walrasian *étonnement*—for a static theory as above defined. Correct proof of this stability has not been given so far, but does not seem to meet with any great difficulty.

Just as a child's blood circulation, although going on concurrently with its growth or, say, pathological change in its organs, is yet capable of being singled out and dealt with as a distinct real phenomenon, so that fundamental circular process can be singled out and dealt with as a distinct real phenomenon, and *every analyst*¹ and *every business man* does so deal with it—the latter realising that it is one thing to figure out the outlay on, and the income from, a building in given circumstances and another thing to form an idea about the future prospects of the neighbourhood, or that it is one thing to manage an existing building and another to pull it down and replace it by another of a different kind. Nor is our analogy with the circulation of the blood idle. For the first complete analysis of the static economic process, Quesnay's, was directly inspired by Harvey's discovery. The analytic apparatus or theoretic point of view of statics is presented by the concept of a determined equilibrium, the use of which, however, is not absolutely confined to the explanation of the circular process, as temporary equilibria occur outside of this process.

Because a set of facts, which form a coherent whole and are, in many cases, capable of statistical separation from the rest, corresponds to static theory, the static state is not merely a methodological device, still less a pedagogical one. And its range is much widened by the fact that it is not a state of rest. It is first, of course, no state of absence of motion, as it implies the ever-changing flow of productive services and consumers' goods, although this flow is looked upon as going on under substantially unchanging conditions. But, secondly, conditions need not be entirely constant. We can allow seasonal oscillations. We can also allow, without leaving the precincts of statics, chance variations, provided reaction to them is merely adaptive, in the sense of an adaptation *capable of being brought about by infinitesimal steps*. And we can, finally, deal with the phenomenon of mere growth of population, of capital and, consequent thereupon, of the National Dividend. For these changes occur continuously, and adaptation to them is essentially continuous. They may

¹ Of course, only a minority of economists are aware of the fact. And some of those who are, spoil the edge of the tool by speaking of a "stationary" state. Some of these, again, construct a state of harmonious progress to occupy the ground between "statics" and what too obviously lies outside of it. There is no objection to such a construction. But it is not always recognised that, owing to the fact that it implies consideration of long periods, the "normal," which pertains to it, is much bolder and much more dangerous an abstraction than the static one.

condition discontinuous changes; but they do not, directly and by their mere presence, bring them about. What they do bring about automatically are only variations at the margins.¹ Increase of population, for instance, will, by itself, merely tend to make labour cheaper, and diagnosis of the state of any particular nation in any particular point of time will have to recognise this as a real and distinct element of the situation, however much it may be compensated by other factors. From this it follows that mere growth is not in itself a source of instability of either the System or the Order of Capitalism, within the meaning given to "stability" in this paper. This disposes of some, if not most, theories of "disproportionality," past and present, and gives further help towards "localising" causes of instability.

II. *Stability and Progress*

§ 5. This might very well be all: Economic life, or the economic element in, or aspect of, social life might well be essentially passive and adaptive and *therefore, in itself, essentially stable*. The fact that Reality is full of discontinuous change would be no disproof of this. For such change could without absurdity be explained by influences from without, upsetting equilibria that would, in the absence of such influences, obtain or only shift by small and determined steps along with what we have called continuous growth. We could, of course, even then fit trend lines through the facts succeeding one another historically; but they would merely be expressions of whatever has happened, not of distinct forces or mechanisms; they would be statistical, not theoretical; they would have to be interpreted in terms of particular historic events, such as the opening up of new countries in the nineteenth century, acting on a given rate of growth—and not in terms of the working of an economic mechanism *sui generis*. And if analysis could not detect any purely economic forces within the system making for qualitative and discontinuous change, we

¹ Although, therefore, even these influences do not work within a given state of equilibrium and do not tend towards a given centre of gravitation, but displace this centre and propel the economic organism away from the old position, the static apparatus is admirably competent to deal with them. Treatment of such questions has been called "dynamics" by some authorities, foremost among whom was E. Barone. It would, perhaps, be best to drop the terms statics and dynamics altogether. Certainly they are misnomers, when used in the sense given to them in the text, and care should be taken not to think of them by way of analogy with their meanings in mechanics and not to confuse the different meanings attached to them by different writers. All the different meanings, I suppose, lead back to John Stuart Mill, who owes the suggestion to Comte, who, in his turn, expressed indebtedness to the zoologist de Blainville.

should evidently be driven to this conclusion,¹ which can never lack verification, as there are always outside influences to point to, and as a great part of the facts of non-equilibrium must in any case be explained largely on such lines, whether there be a definite piece of non-static mechanism in them or not.

Now it is always unsafe, and it may often be unfair, to attribute to any given author or group of authors clear-cut views of comprehensive social processes, the diagnosis of which must always rest largely on social vision as distinguished from provable argument. For no author or group of authors can help recognising many heterogeneous elements, and it is always easy to quote passages in proof of this. The treatment of the history of the analysis of value, cost and interest affords examples in point,² and it must be left to the reader to form his own opinion about the correctness or otherwise of our thus formulating what seems to us to be received doctrine: Industrial expansion, automatically incident to, and moulded by, general social growth—of which the most important purely economic forces are growth of population and of savings—is the basic fact about economic change or evolution or “progress”; wants and possibilities develop, industry expands in response, and this expansion, carrying automatically in its wake increasing specialisation and environmental facilities,

¹ As a matter of fact, this is what the position of our highest authorities comes to. It is certainly the position of Ricardo and John Stuart Mill, whose discussion of “progress” mainly turns on the question of relative growth of population and capital, occasionally affected by improvement of methods of production, which they glance at in passing as a disturber of the normal course of things. Such is the position, too, of Walras or, for that matter, of Böhm-Bawerk, who both of them seem convinced that everything of a purely economic nature must needs fit into one homogeneous body of doctrine, which is frankly “static” with Walras, whilst Böhm-Bawerk always rejected the static conception precisely because it excludes some things which yet are undoubtedly “purely economic.” John B. Clark is the one outstanding exception, but Marshall, although embracing within his wide horizons every one of the elements essential to a distinct theory of “dynamics,” still forced all of them into a frame substantially “static.” The present writer believes that some of the difficulties and consequent controversies about Prof. Pigou’s argument in his *Economics of Welfare* are traceable to the same source, and his work on *Industrial Fluctuations* is a monument to the view that economic life, in itself essentially passive, is being continually disturbed and propelled by “initial impulses” coming from outside.

² Even within the narrower precincts of problems such as these, it has become a fashion—a justified reaction, perhaps, from the opposite vice—to interpret older authors so very broadly as to make them “see” everything and *definitely say* nothing, and to frown on another way of stating their views as ungenerous. I submit, however, first that whilst this attitude is the correct one in evaluating individual theorists—provided that the same generous broadness be vouchsafed to all—it is not useful in bringing out characteristics; secondly, that mere “recognition” of a fact means nothing unless the fact be welded into the rest of the argument and made to do theoretic work.

accounts for the rest, changing continuously and organically its own *data*.

Grounds for dissent from this view present themselves on several points, but I am anxious to waive objections in order to make stand out *the* objection. Without being untrue, when taken as a proposition summing up economic history over, say, a thousand years,¹ it is inadequate, or even misleading, when meant to be a description of that mechanism of economic life which it is the task of economic theory to explain, and it is no help towards, but a bar to, the understanding of the problems and phenomena incident to that mechanism. For expansion is *no* basic fact, capable of serving in the rôle of a cause, but is itself the result of a more fundamental "economic force," which accounts both for expansion and the string of consequences emanating from it. This is best seen by splitting up the comprehensive phenomenon of general industrial growth into the expansion of the single industries it consists of. If we do this for the period of predominantly competitive capitalism, we meet indeed at any given time with a class of cases in which both entire industries and single firms are drawn on by demand coming to them from outside and so expanding them automatically; but this additional demand practically always proceeds, as a secondary phenomenon,²

¹ Different sets of problems require different distances from the objects of our interest; and different propositions are true from different distances and on different planes of argument. So, *e.g.*, for a certain way of describing historic processes, the presence of a military commander of Napoleonic ability may truly be said to be of causal importance, whilst, for a survey farther removed from details, it may have hardly any importance at all. Our analytic apparatus consists of heterogeneous pieces, every one of which works well on some of the possible "planes" of argument and not at all on others, the overlooking of which is an important, and sometimes the only, source of our controversies.

² We may conveniently enumerate, partly anticipating and partly repeating, the more important types of those secondary phenomena, which we hold received opinion, neglecting the primary phenomenon, exclusively deals with, and which would not entirely, but almost entirely, be absent without the primary one.

(1) Expansion of some industries called forth by primary expansion in others, as stated above: If a new concern establishes itself, grocers' businesses will expand in the neighbourhood and so will producers of subsidiary articles. *The expansion of all industries, which do not themselves display any break in their practice during the time under consideration* is to be accounted for thus.

(2) If the primary change results in turning out better tools of production, naturally this will expand the industries which use them. This must be taken account of in judging the comparative success of some State-managed railways surrounded by private industries, which force on them improved engines, fittings, and so on.

(3) Every given change starts from a given environment, and would be impossible without its facilities. But every given environment embodies the results of previous primary change, and, therefore, cannot be taken, except within static theory, as an ultimate datum, acting autonomously, but is itself, in great part, a secondary phenomenon.

from a primary change in some other industry—from textiles first, from iron and steam later, from electricity and chemical industry still later—which does not *follow*, but *creates* expansion. It *first*—and by its initiative—expands its own production, thereby creates an expansion of demand for its own and, contingent thereon, other products, and the general expansion of the environment we observe—increase of population included—is the *result* of it, as may be visualised by taking any one of the outstanding instances of the process, such as the rise of railway transportation. The way by which every one of these changes is brought about lends itself easily to general statement: it is by means of new combinations of existing factors of production, embodied in new plants and, typically, new firms producing either new commodities, or by a new, *i.e.* as yet untried, method, or for a new

(4) So is, in great part, what we have called growth. This is specially clear in the case of saving, the amount of which would be very much smaller in the absence of its most important source, the entrepreneurs' profits. It is also true as to increase of population. And expansion, incident to what would be left of growth in the absence of primary change, would soon be quenched by a (physical) law of decreasing returns acting sharply. *This, then, is the main reason why we think so little of the autonomous—as distinguished from secondary—importance of external economies incident to mere expansion and of what is left of increasing returns, if we exclude all that is either primarily or secondarily due to the cause we are about to consider.*

(5) Industrial evolution inspires collective action in order to force improvement on lethargic strata. Of this kind was, and is, Government action on the Continent for improving agricultural methods of peasants. This is not "secondary" in the sense we mean it, but if it comes to creating external economies by non-economic influence, it has nevertheless been due so far mainly to some previous achievement in some private industry.

(6) Successful primary change is followed by general reorganisation within the same industry, more and more other firms following the lead of some, both because of the profits to be gained and the losses to be feared. During this process, what have at first been the internal economies of the leaders soon become external economies for the rest of the firms, whose behaviour need be no other than one of passive adaptation (and expansion) to what *for them* is environmental advantage. But for us, the observers, to look upon the process as one of adaptation to expanding environment is to miss the salient point.

(7) Incident to all the phenomena glanced at, are, among other things, secondary gains going to all kinds of agents, who do not display any initiative. There is, however, another, a secondary, initiative, stimulated by the possibility of such gains becoming possible—extensions of businesses, speculative transactions and so on, calculated to secure them. The periodic rise and fall of the level of prices—an essential piece, as we shall see, of the mechanism of change in competitive capitalism—carries in its wake extensions and, to finance them, applications for credit merely due to the fact of prices rising, which greatly intensify the phenomenon. And this secondary phenomenon is being as a rule realised much more clearly by observers than the primary phenomenon which gives rise to it.

Our analysis neither overlooks nor denies the importance of these things. On the contrary, it aims at showing their cause and nature. But in a statement of fundamental principles within so short a compass they cannot loom large in the picture.

market, or by buying means of production in a new market. What we, unscientifically, call economic progress means essentially putting productive resources to uses *hitherto untried in practice*, and withdrawing them from the uses they have served so far. This is what we call "innovation."

What matters for the subject of this study is merely the essentially discontinuous character of this process, which does not lend itself to description in terms of a theory of equilibrium. But we may conveniently lead up to this by insisting for the moment on the importance of the difference between this view and what I have called the received one. Innovation, unless it consists in producing, and forcing upon the public, a new commodity, means producing at smaller cost per unit, breaking off the old "supply schedule" and starting on a new one. It is quite immaterial whether this is done by making use of a new invention or not; for, on the one hand, there never has been any time when the store of scientific knowledge had yielded all it could in the way of industrial improvement, and, on the other hand, it is not the knowledge that matters, but the successful solution of the task *sui generis* of putting an untried method into practice—there may be, and often is, no scientific novelty involved at all, and even if it be involved, this does not make any difference to the nature of the process. And we should not only, by insisting on invention, emphasise an irrelevant point—irrelevant to our set of problems, although otherwise, of course, just as relevant as, say, climate—and be thereby led away from the relevant one, but we should also be forced to consider inventions as a case of external economies.¹

¹ There is another point which arises out of the usual treatment of these things: Nobody can possibly deny the occurrence or relevance of those great breaks in industrial practice which change the data of economic life from time to time. Marshall, therefore, distinguishes these, which he calls "substantive" inventions and which he deals with as chance events acting from outside on the analogy, say, of earthquakes, from inventions which, being of the nature of more obvious applications of known principles, may be expected to arise in consequence of expansion itself. This distinction is insisted upon by Prof. Pigou in the paper quoted above. This view, however, cuts up a homogeneous phenomenon, the elements of which do not differ from one another except by degree, and is readily seen to create a difficulty similar to that of filling the empty boxes. Exactly as the failure to distinguish different processes leads, in the case of the boxes, to a difficulty in distinguishing between groups of facts—and leads, also, to that state of discussion in which some authors hold that most industries display *increasing*, others that most industries display *decreasing*, still others, that normally any industry shows *constant*, returns—so it is obviously impossible to draw any line between those classes of innovations, or, for that matter, inventions; and the difficulty is not one of judging particular cases, but one of principle. For *no* invention is independent of existing data; and *no* invention is *so* dependent on them as to be automatically produced by them. In the case of important invention, change in data is great; in the case of unimportant invention it is small. But this is all, and the *nature* of the process and of the special mechanism set in motion is always the same.

Now this hides part of the very essence of the capitalist process. This kind of external economics—and, in fact, nearly every kind, even the trade journal must, unless the product of collective action, be somebody's business—characteristically comes about by first being taken up by one firm or a few—by acting, that is, as an internal economy. This firm begins to undersell the others, part of which are thereby definitely pushed into the background to linger there on accumulated reserves and quasi-rents, whilst another part copies the methods of the disturber of the peace. *That* this is so, we can see every day by looking at industrial life; it is precisely what goes on, what is missing in the static apparatus and what accounts both for dissatisfaction with it and for the attempts to force such phenomena into its cracking frame—instead of, as we think it natural to do, recognising and explaining this as a distinct process going on along with the one handled by the static theory. *Why* this is so, is a question which it would lead very far to answer satisfactorily. Successful innovation is, as said before, a task *sui generis*. It is a feat not of intellect, but of will. It is a special case of the social phenomenon of leadership.¹ Its difficulty consisting in the resistances and un-

¹ This does not imply any glorification. Leadership itself does not mean only such aptitudes as would generally command admiration, implying, as it does, narrowness of outlook in any but one direction and a kind of force which sometimes it may be hardly possible to distinguish from callousness. But economic leadership has, besides, nothing of the glamour some other kinds of leadership have. Its intellectual implications may be trivial; wide sympathies, personal appeal, rhetorical sublimation of motives and acts count for little in it; and although not without its romance, it is in the main highly unromantic, so that any craving for personal hero-worship can hardly hope for satisfaction where, among, to be sure, other types, we meet with slave-trading and brandy-producing puritans at the historic threshold of the subject.

Apart from this source of possible objections, there is a much more serious one in the mind of every well-trained economist, whom experience has taught to think little of such intrusions into theory of views savouring of sociology, and who is prone to associate any such things with a certain class of objections to received doctrine, which continually turn up however often they may have been refuted—sublimely ignorant of the fact—such as objections to the economic man, to marginal analysis, to the use of the barter hypothesis and so on. The reader may, I think, satisfy himself that no want of theoretic training is responsible for statements which I believe to tally fundamentally with Marshallian analysis.

No difficulty whatever arises as to verification. That new commodities or new qualities or new quantities of commodities are forced upon the public by the initiative of entrepreneurs—which, of course, does not affect the rôle of demand within the static process—is a fact of common experience; that one firm or a small group of firms leads in the sense meant above, in the process of innovation, thereby creating its own market and giving impulse to the environment generally, is equally patent (and we do not deny facts of other complexion—the secondary or “consequential” ones); and all we are trying to do is to fit the analytic apparatus to take account of such facts without putting its other parts out of gear.

certainties incident to doing what has not been done before, it is accessible for, and appeals to, only a distinct type which is rare. Whilst differences in aptitude for the routine work of "static" management only result in differences of success in doing what every one does, differences in this particular aptitude result in only some being able to do this particular thing at all. To overcome these difficulties incident to change of practice is the function characteristic of the entrepreneur.

Now if this process meant no more than one of many classes of "friction," it certainly would not be worth our while to dissent from the usual exposition on that account, however many facts might come under this heading. But it means more than this: Its analysis yields the explanation of phenomena which cannot be accounted for without it. There is, first, the "entrepreneurial" function as distinct from the mere "managerial" function—although they may, and mostly must, meet one another in the same individual—the nature of which only shows up within the process of innovation. There is, secondly, the explanation of entrepreneurs' gain, which emerges in this process and otherwise gets lost in the compound of "earnings of management,"¹ the treating of which as a homogeneous whole is unsatisfactory for precisely the same reason which, by universal consent, makes it unsatisfactory so to treat, say, the income of a peasant tilling his own soil, instead of treating it as a sum of wages, rent, quasi-rent and, possibly, interest. Furthermore, it is *this* entrepreneurs' profit which is the primary source of industrial fortunes, the history of every one of which consists of, or leads back to, successful acts of innovation.² And as the rise and

¹ The function in question being a distinct one, it does not matter that it appears in practice rarely, if ever, by itself. And whoever cares to observe the behaviour of business men at close quarters will not raise the objection that new things and routine work are done, as a rule, indiscriminately by the same manager. He will find that routine work is done with a smoothness wholly absent as soon as a new step is to be taken, and that there is a sharp cleavage between the two, insuperable for a very worthy type of manager. This extends far into the realm of what we are wont to consider as automatic change, bringing about external economies and increasing returns. Take the instance of a business letting out motor cars on the principle "drive yourself." A mere growth of the neighbourhood, sufficient to make such a business profitable, does not produce it. Someone has to realise the possibility and to found the firm, to get people to appreciate its services, to get the right type of cars and so on. This implies solution of a legion of small problems. Even if such a firm already exists and further environmental growth make discontinuous extension feasible, the thing to be done is not so easy as it looks. It would be easy for the trained mind of a leading industrialist, but it is not so for a typical member of the stratum which does such business.

² It is, as has been said in a previous note, not the *running* of a business according to new plan, but the act of *getting it* to run on a new plan, which accounts

decay of industrial fortunes is *the* essential fact about the social structure of capitalist society, both the emergence of what is, in any single instance, an essentially temporary gain, and the elimination of it by the working of the competitive mechanism, obviously are more than "frictional" phenomena, as is that process of underselling by which industrial progress comes about in capitalist society and by which its achievements result in higher real incomes all round.

Nor is this all. This process of innovation in industry by the agency of entrepreneurs supplies the key to all the phenomena of capital and credit. The rôle of credit would be a technical and a subordinate one in the sense that everything fundamental about the economic process could be explained in terms of goods, if industry grew by small steps along coherent curves. For in that case financing could and would be done substantially by means of the current gross revenue, and only small discrepancies would need to be smoothed. If we simplify by assuming that the whole circular process of production and consumption takes exactly one period of account, no instruments or consumers' goods surviving into the next, capital—defined as a monetary concept—and income would be exactly equal, and only different phases of one and the same monetary stream. As, however, innovation, being discontinuous and involving considerable change and being, in competitive capitalism, typically embodied in new firms, requires large expenditure previous to the emergence of any revenue, credit becomes an essential element of the process. And we cannot turn to savings in order to account for the existence of a fund from which these credits are to flow. For this would imply the existence of previous profits, without which there would not be anything like the required amount—even as it is, savings usually lag behind requirements—and assuming previous profits would mean, in an explanation of principles, circular reasoning. "Credit-creation," therefore, becomes an essential part both of the mechanism of the process and of the theory explaining it.

for entrepreneurs' profits, and makes it so undesirable to try to express them by "static" curves, which describe precisely the phenomena of the "running" of it. The theoretical reason for our proposition is, that either competition or the process of imputation must put a stop to any "surplus" gain, even in a case of monopoly, in which the value of the patent, the natural agent or of whatever else the monopoly position is contingent on, will absorb the return in the sense that it will no longer be profit. But there is also a "practical" observation to support this view. No firm ever yields returns indefinitely, if only run according to unchanged plan. For everyone comes the day when it will cease to do so. And we all of us know that type of industrial family firm of the third generation which is on the road to that state, however conscientiously it may be "managed."

Hence, saving, properly so called, turns out to be of less importance than the received doctrine implies, for which the continuous growth of saving—accumulation—is a mainstay of explanation. Credit-creation is the method by which the putting to new uses of existing means of production is brought about through a rise in price enforcing the “saving” of the necessary amount of them out of the uses they hitherto served (“enforced savings”—cp. Mr. Robertson’s “imposed lacking”).

Finally, it cannot be said that whilst all this applies to individual firms, the development of whole industries might still be looked at as a continuous process, a comprehensive view “ironing out” the discontinuities which occur in every single case. Even then individual discontinuities would be the carriers of essential phenomena. But, besides, for a definite reason that is not so. As shown both by the typical rise of general prices and the equally typical activity of the constructional trades in the prosperity phase of the business cycle, innovations cluster densely together. So densely, in fact, that the resultant disturbance produces a distinct period of adjustment—which precisely is what the depression phase of the business cycle consists in. *Why* this should be so, the present writer has attempted to show elsewhere.¹ *That* it is so, is the best single verification and justification of the view submitted, whether we apply the criterion of its being “true to life” or the criterion of its yielding explanation of a phenomena *not itself implied in its fundamental principle*.

If, then, the putting to new uses of existing resources is what “progress” fundamentally consists in; if it is the nature of the entrepreneur’s function to act as the propelling force of the process; if entrepreneur’s profits, credit, and the cycle prove to be essential parts of its mechanism—the writer even believes this to be

¹ “Theorie der wirtschaftlichen Entwicklung,” 1911, 2nd ed. 1926. Cp. also “The Explanation of the Business Cycle,” *Economica*, 1927. The failure of the price-level to rise in the United States during the period 1923–1926 will be seen to be no objection but a further verification of this theory. It has, however, been pointed out to the writer, by a very high authority, that prices did also fail to rise in the United States in the prosperity immediately preceding the War. It could be replied that the factors which account for the stability 1923–1926 had been active already before the War. But the U.S. Bureau of Labour figures for 1908–1913 are 91, 97, 99, 95, 101, 100. Cp. also Prof. Persons’ chart in *Review of Economic Statistics*, Jan. 1927. It may be well to mention that constructional trades and their materials need not necessarily show their activity fully by *every* index. Iron, e.g., being an international commodity, need not rise in price if the phases of the cycle do not quite coincide in different countries. As a matter of fact, they generally do. But the right way to deal with iron and steel is to use the Spiethoff index (production + imports – exports), and this has, so far, always worked satisfactorily.

true of interest—then industrial expansion *per se* is better described as a consequence than as a cause; and we should be inclined to turn the other way round what we have termed the received chain of causation. In this case, and as those phenomena link up so as to form a coherent and self-contained logical whole, it is obviously conducive to clearness to bring them out boldly; to relegate to one distinct body of doctrine the concept of equilibrium, the continuous curves and small marginal variations, all of which, in their turn, link up with the circuit flow of economic routine under constant data; and to build, alongside of this, and *before* taking account of the full complexity of the “real” phenomenon—secondary waves, chance occurrences, “growth” and so on—a theory of capitalist change, assuming, in so doing, that non-economic conditions or data are constant and automatic and gradual change in economic conditions is absent. But there is no difficulty in inserting all this. And it would seem to follow that the organic analogy is less adapted to express faithfully the nature of the process than many of us think; although, of course, being a mere analogy, it may be so interpreted as not to imply anything positively wrong and as to avoid the idea of an equilibrium growth *ad instar* of the growth of a tree, which it may, but need not necessarily, suggest.

Summing up the argument and applying it to the subject in hand, we see that there is, indeed, one element in the capitalist process, embodied in the type and function of the entrepreneur, which will, *by its mere working and from within*—in the absence of all outside impulses or disturbances and even of “growth”—destroy any equilibrium that may have established itself or been in process of being established; that the action of that element is not amenable to description by means of infinitesimal steps; and that it produces the cyclical “waves” which are essentially the form “progress” takes in competitive capitalism and could be discovered by the theory of it, if we did not know of them by experience. But by a mechanism at work in, and explaining the features of, periods of depression, a new equilibrium always emerges, or tends to emerge, which absorbs the results of innovation carried out in the preceding periods of prosperity. The new elements find their equilibrium proportions; the old ones adapt themselves or drop out; incomes are rearranged; prosperity inflation is corrected by automatic self-deflation through the repayment of credits out of profits, through the new consumers’ goods entering the markets and through saving stepping into the place of “created” credits. So the instabilities, which arise from

the process of innovation, tend to right themselves, and do not go on accumulating. And we may phrase the result we reach in our terminology by saying that there is, though instability of the *System*, no economic instability of the *Order*.

§ 6. The instability due to what we conceive to be the basic factor of purely economic change is, however, of very different importance in the two historic types of capitalism, which we have distinguished.

Innovation in competitive capitalism is typically embodied in the foundation of new firms—the main lever, in fact, of the rise of industrial families; improvement is forced on the whole branch by the processes of underselling and of withdrawing from them their means of production, workmen and so on shifting to the new firms; all of which not only means a large amount of disturbance as an incident, but is also effective in bringing about the result, and to change “internal” economies into “external” ones, only *as far as* it means disturbance. The new processes do not, and generally cannot, evolve out of the old firms, but place themselves side by side with them and attack them. Furthermore, for a firm of comparatively small size, which is no power on the money market and cannot afford scientific departments or experimental production and so on, innovation in commercial or technical practice is an extremely risky and difficult thing, requiring supernormal energy and courage to embark upon. But as soon as the success is before everyone’s eyes, everything is made very much easier by this very fact. It can now, with much-diminished difficulty, be copied, even improved upon, and a whole crowd invariably does copy it—which accounts for the leaps and bounds of progress as well as for setbacks, carrying in their wake not only the primary disturbance, inherent to the process, but a whole string of secondary ones and *possibilities*, although no more than possibilities, of recurrent catastrophes or crises.

All this is different in “trustified” capitalism. Innovation is, in this case, not any more embodied *typically* in new firms, but goes on, within the big units now existing, largely independently of individual persons. It meets with much less friction, as failure in any particular case loses its dangers, and tends to be carried out as a matter of course on the advice of specialists. Conscious policy towards demand and taking a long-time view towards investment becomes possible. Although credit creation still plays a rôle, both the power to accumulate reserves and the direct access to the money market tend to reduce the importance of this element in the life of a trust—which, incidentally, accounts

for the phenomenon of prosperity coexisting with stable, or nearly stable, prices which we have had the opportunity of witnessing in the United States 1923-1926. It is easy to see that the three causes alluded to, whilst they accentuated the waves in competitive, must tend to soften them down in trustified, capitalism. Progress becomes "automatised," increasingly impersonal and decreasingly a matter of leadership and individual initiative. This amounts to a fundamental change in many respects, some of which reach far out of the sphere of things economic. It means the passing out of existence of a system of selection of leaders which had the unique characteristic that success in *rising* to a position and success in *filling* it were essentially the same thing—as were success of the firm and success of the man in charge—and its being replaced by another more akin to the principles of appointment or election, which characteristically divorce success of the concern from success of the man, and call, just as political elections do, for aptitudes in a candidate for, say, the presidency of a combine, which have little to do with the aptitudes of a good president. There is an Italian saying, "Who enters the conclave as prospective pope, will leave it as a cardinal," which well expresses what we mean. The types which rise, and the types which are kept under, in a trustified society are different from what they are in a competitive society, and the change is spreading rapidly to motives, stimuli and styles of life. For our purpose, however, it is sufficient to recognise that the only fundamental cause of instability inherent to the capitalist system is losing in importance as time goes on, and may even be expected to disappear.

§ 7. Instead of summing up a very fragmentary argument, I wish to emphasise once more, in concluding, that no account whatsoever has been taken of any but purely economic facts and problems. Our diagnosis is, therefore, no more sufficient as a basis for prediction than a doctor's diagnosis to the effect that a man has no cancer is a sufficient basis for the prediction that he will go on living indefinitely. Capitalism is, on the contrary, in so obvious a process of transformation into something else, that it is not the fact, but only the interpretation of this fact, about which it is possible to disagree. Towards this interpretation I have wished to contribute a negative result. But it may be well, in order to avoid misunderstanding, to state expressly what I believe would be the positive result of a more ambitious diagnostic venture, if I may presume to do so in one short and imperfect sentence: Capitalism, whilst economically stable, and even gaining

in stability, creates, by rationalising the human mind, a mentality and a style of life incompatible with its own fundamental conditions, motives and social institutions, and will be changed, although not by economic necessity and probably even at some sacrifice of economic welfare, into an order of things which it will be merely matter of taste and terminology to call Socialism or not.

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ON A CERTAIN AMBIGUITY IN THE CONCEPTION OF STATIONARY EQUILIBRIUM

I

THE idea of an equilibrium of forces is one which is common to many sciences, but there are few in which it plays a more important part than in theoretical economics. It has been implicit in our discussions since the time of the Physiocrats,¹ and as the methods of economics have become more and more self-conscious it has become, in one shape or other, one of the main instruments of theoretical analysis. We describe the various forces we have to study by reference to their place in our conception of equilibrium. We measure their variations by reference to equilibrating norms. It is not too much to say that in so far as we pretend to enunciate economic laws at all it is the assumption that, within some limits, an equilibrium of some sort is conceivable that is the justification of our procedure.

Now, of course, many conceptions of equilibrium are possible, from the simple notion of a balance between the supply of and demand for one commodity to the sublime conceptions of the mathematical economists, in which all the quantities contemplated move together in orderly change. But hitherto the dominating conception has been one midway between these extremes. We abstract from various causes of change, and conceive the remaining economic quantities as stationary, and inquire as to their mutual relations. Or, we ask what will be the relation of certain given quantities when certain tendencies to change have come to rest. The stationary state and static laws have been the main subject of investigation.

Whatever the imperfections of this method, it cannot be denied that by its aid, in the past, much has been done which does, in some way or other, elucidate the working of economic forces. The man who holds that nothing has yet been accomplished may deserve pity but certainly not respect. Nevertheless,

¹ See Schumpeter, *Epochen der Methoden- und Dogmengeschichte*, pp. 45-48, for a very interesting discussion of the Physiocratic contribution viewed in this aspect.

at the present day, it would be idle to pretend that, even within this limited sphere, entire finality has been reached. There are still important differences of opinion among economists, both as regards the form and the significance of static analysis. Some of these differences relate to matters of actual *logic*—it is probable that the dispute with regard to consumers' surplus is a difference of this order. But some arise much more because of certain initial differences of *assumption* which are not always clearly realised. The logic is thought to be faulty because the premises are not clearly stated. It is the contention of this paper that the conception of stationary equilibrium, as used in modern "literary economics," itself involves an ambiguity as regards its ultimate assumptions. It is contended further that this ambiguity is responsible for some of the most important doctrinal confusions of the past, and that even at the present day it has led, in at least one instance, to analysis which is definitely erroneous. The nature of the ambiguity is simple. It can be stated in a couple of sentences. But its full significance cannot be grasped until we have traced its origin in the classical system and followed up some of the confusions which arise from its existence. An historical method of approach will therefore be utilised.¹ The history, however, makes no pretension to exhaustiveness. It is used solely in order to elucidate positive analysis.

II

1. The idea of a stationary state is clearly implicit in the *Tableau Economique* of Quesnay and the Physiocrats.² The underlying notion of that complicated arithmetical diagram is one of a state of affairs in the body economic in which production and consumption are in a condition of perfect

¹ The argument of this paper was first developed in lectures on the history of theory which I delivered at New College in the winter 1928-29. At that time I thought that the contrast which I drew between static states according to suppositions concerning the supply schedules of the factors of production, although, of course, obvious to anyone familiar with mathematical economics, had not been made by any "literary economist." Since then, however, I have discovered a footnote in Professor Knight's *Risk, Uncertainty and Profit* (p. 143) in which this is done, and, as I had read that admirable work many times, I suppose I must have been unconsciously influenced by Professor Knight's comment. Professor Knight's note is, however, very brief, and I therefore venture to hope that my historical exegesis and contemporary applications may not be altogether otiose.

² On the significance of the *Tableau Economique*, see Oncken, *Geschichte der Nationalökonomie* (pp. 386-402); also Cannan, *A Review of Economic Theory* (pp. 25-34).

balance. Year in, year out the same processes recur, the same circulation of goods takes place through the same channels. But the use made of this conception is different from the use which has been most customary since that day. Modern economists use the stationary state to exhibit the nature of certain *tendencies*, to show under what conditions certain kinds of change may be expected to cease. Quesnay used it rather as a model of certain *processes* and a demonstration of the conditions under which these processes could be said to be functioning healthily. It is obvious from the variants of the table given by Mirabeau that its main use was conceived to be that of a formulation of the minimum conditions of civic well-being, rather than a hypothetical construction whereby purely causal phenomena could be studied. It was hoped that actual statistics would be collected of the quantities exhibited in the table; and then, by a comparison of their mutual relationships with the ideal relationships of the table, a diagnosis could be made of the prosperity of the area of collection. It would therefore be unwise to attribute very great importance to the Physiocratic contribution to the analysis of equilibrium.

2. So far as I am able to discover, the first use of the conception in its more familiar form, and the first mention of the actual term, the stationary state, occur in the course of the celebrated chapter on wages in the *Wealth of Nations*.¹

Adam Smith, it will be remembered, commences his treatment of wages in an "advanced society" by laying down what may be called a buyer's monopoly theory of subsistence wages. The unfortunate worker, who, in "that original state of things which precedes both the appropriation of land and the accumulation of stock," secured the whole produce of labour, now finds himself compelled to bargain with the employers for a share of it. In the face of their tacit combination he is helpless. His wages, therefore, are forced down to subsistence level—a wage, that is to say, which will keep him alive and enable him to bring up a family of just such a size as, allowing for the average expectation of life among the children of the working classes, will keep the population constant.

But, no sooner has Smith elaborated this theory, than he at once commences to replace it by another. His strong sense of reality compels him to admit that wages do not behave as such generalisations would lead one to suppose. (In a buyer's monopoly theory of subsistence wages there is, of course, no question of a

¹ Cannan's Ed., Vol. I. pp. 66-88.

tendency which may be concealed by countervailing tendencies. *So long as the monopoly is operative it can act at once.*) He therefore proceeds to elaborate a new theory, a theory in which, when subsistence wages emerge at all, their emergence is due as much to forces acting on the supply side as to buyers' combinations on the side of demand. On the demand side, this theory anticipates the wage fund theory; on the supply side, the population theory of the first edition of Malthus' *Essay on Population*. And it is in the course of the development of this theory that the conception which is to be analysed in this paper first emerges.

According to this new version of the theory of wages, the demand for labour is conceived to come from certain funds which in some somewhat mysterious fashion have been "destined for the maintenance of labour." When these funds are increasing faster than the increase of labour, the tacit combination of masters breaks up. "The scarcity of hands occasions a competition among masters, who bid against one another in order to get workmen, and thus voluntarily break through the natural combination of masters not to raise wages." In these circumstances, wages rise above subsistence level. This, Smith thought, was what was happening in the England of his own day and also in North America. But if for any reason these funds cease to increase, if, to use the jargon of a later day, the wage-fund becomes stationary, then wages tend to fall. The wretched labourers multiply, and their share per head of this constant fund is reduced until it reaches that level which is sufficient to maintain the labourer and enable him to support a family of the size necessary to keep population constant. This, he thought, was what had happened in China, whose condition, he said, had "long been stationary." Finally, if the fund actually diminished, as, he thought, was happening in Bengal, then wages fall until "want, famine and mortality" have so reduced the number of labourers as once more to make a subsistence wage possible. Wages therefore form, as it were, an index showing whether the state is advancing, stationary or retrogressive. "It is in the progressive state, while the society is advancing to further acquisition, that the condition of the labouring poor seems to be the happiest and most comfortable. It is hard in the stationary and miserable in the declining state. The progressive state is in reality the cheerful and the hearty state for all the different orders of the society. The stationary is dull; the declining melancholy."

3. This then is the stationary state as it first appears in economic theory. Population is constant, capital is constant,

wages are constant and profits are constant.¹ There is life, in that, year in year out, the same things are repeated. But the fundamental conditions of economic activity remain unchanged.

But why is it stationary?

We have to reconstruct the answer from different parts of Smith's analysis. Population is stationary because the funds destined for the maintenance of labour have ceased to increase and wages are down to subsistence level. The funds destined for the maintenance of labour are stationary because the rate of profit has fallen so low that there is no further incentive to accumulate. How low this is, is difficult to say. Smith admits that the level will vary with the current risk of investment. In China he thinks, because of bad government, accumulation has ceased at 12 per cent. But no other possibility of variation is mentioned. There is no suggestion that there are different static rates in different states of society. Risk is the only cause of variation. Compensation for risk apart, the rate "must always be something more than what is sufficient to compensate the occasional losses to which every employment of stock is exposed. . . . Were it not more, charity or friendship could be the only motive for lending."² And it is suggested that where great risk is absent (as in Holland) the rate is so low that only the very wealthiest can live on interest.

4. Superficially, the stationary state which we encounter in Ricardo is much the same sort of conception.³ Wages are at their "natural level," dictated by the cost of producing labour. Population is constant. Capital is accumulated to such a point that, the rate of profit having fallen to the minimum necessary to induce accumulation, no further saving is being made. And Ricardo equally with Smith regards the prospect of arriving in this condition with feelings of considerable repugnance:—"If we should attain the stationary state, from which I trust we are far distant." . . .

But, in fact, if we are willing to dig a little beneath the surface, we find a considerable difference. The structure is more complicated, the possibilities of variation more numerous. In the Smithian conception, the rate of profit is the only supply price

¹ Presumably technical knowledge has ceased to grow. This, however, is an assumption which was not elucidated until much later. It is not until we came to Marshall and Clark that the technical presuppositions of Static analysis are thoroughly and clearly stated.

² *Op. cit.*, pp. 97-98.

³ See e.g. *Works* (Ed. McCulloch), pp. 59, 104, 120. There is an interesting passage in the *Letters to Malthus*, p. 188, in which the distinction between stationariness and stagnation is discussed.

which admits of any variation. And as we have seen, what variation there is here derives entirely from variations in the risk of investment. So far as wages are concerned the ultimate figure is more or less *objectively* determinate. It is a *physiological* subsistence level—the lowest wage which will keep the labourer alive and enable him to bring up a family. No doubt it will vary according to climate and the death-rate. But there is no suggestion that it may vary with varying *psychological* dispositions.

But by the time Ricardo came to write his *Principles*, crudities of that sort were becoming a thing of the past so far as the inner circle of classical economists was concerned. The second edition of Malthus' *Essay on Population* had made it plain that, if people wished, population could be kept within limits other than the limit of physiological subsistence. And the researches of Torrens¹ had shown that in fact the "conventional subsistence" wage varied greatly from country to country. And therefore, although in one or two parts of his work there is a harking-back to the idea of physiological subsistence wages,² in the main chapter on wages, Ricardo makes it clear that for him the "natural wage" is not so much a wage which *enables* the labourer to subsist and bring up the conventional family, as a wage which *induces* him to do so. It is not a physiological so much as a psychological variable. He is clear that it varies from place to place and from time to time. And he hopes that by wise legislation this natural supply price of labour shall be pushed continually upwards. "The friends of humanity cannot but wish that in all countries the labouring classes should have a taste for comforts and enjoyments, and that they should be stimulated by all legal means in their exertion to procure them."³

All this means, of course, that at least one of the supply prices, the supply price of labour, is now recognised to be dependent on states of mind. Whatever the rigidity of the "natural" rate of profit, it is clear that in the Ricardian conception stationariness is conceivable at various "natural" levels of wages. There may be stationary equilibrium with a (relatively) large population and a low "natural" rate of wages. Or if the friends of humanity have had their way, it is possible that there may be stationary equilibrium with a (relatively) small population, and a high "natural" level of wages. It means, therefore—although Ricardo did not recognise this—that the stationary state is robbed of many

¹ *Essay on the External Corn Trade*, p. 68.

² In the chapter on Gross and Net Revenue, for instance.

³ *Principles* (MacCulloch's Ed.), p. 55.

of its terrors. According to the Smithian conception, the stationary state must be "dull"—to put it mildly. But according to Ricardo, as J. S. Mill pointed out in a celebrated passage, it need not be dull at all.¹ It may all be very pleasant and cultivated.

5. In Mill's work we get still further refinements. With Smith and Ricardo, as we have seen, if we exclude variations due to variations of risk, the supply price of capital is treated as more or less invariable. Mill does not explicitly introduce any refinement of this conception when he is dealing specifically with the stationary state, but, in the chapter on the increase of capital, it is quite clear that he has learned from John Rea² the notion of a natural rate of profit which varies according to what Fisher would call the prevalent conditions of time preference, and what Rea called the effective desire of accumulation. Hence there is now the possibility of an infinite diversity of stationary equilibria with natural wages and profits at different levels.

6. All these tendencies to improvement reach what for our purpose we may regard as a culminating point in the work of Marshall. As is well known, Marshall had a certain aversion to the concept. Partly, I think, because of his temperamental dislike of all extreme abstractions, partly because of a not very clearly formulated distrust of certain apparently similar conceptions which were being elaborated on the other side of the Atlantic. "I cannot conceive," he wrote to J. B. Clark, "of a static state which resembles the real world closely enough to form a subject of profitable study. . . . I could no more write one book about the statical state and another about the dynamical state than I could write one book about a yacht moving three miles an hour through the water which was running against it and another about a yacht moving through still water at five miles an hour."³ Instead he preferred the "less violent assumptions" of what he said was "not quite accurately called the statical method," by which "we fix our minds on some central point; we suppose it for the time being to be reduced to a *stationary* state; and we then study in relation to it the forces that affect the things by which it is surrounded and any tendency there may be to equilibrium of these forces."⁴

¹ *Principles of Economics* (Ashley's Ed.), pp. 248-75.

² *New Principles of Political Economy* (1834).

³ *Memorials*, p. 415.

⁴ *Principles* 5th ed., p. 369. For an interesting discussion of the contrast between Marshall's "statical method" and Clark's "static analysis" see Opie, *Die Lehre von Quasirent und die Marshallshen Lehrgebäude, Archiv für Socialwissenschaft*, Bd. 60, pp. 251-79. I suspect Mr. Opie of the opinion that the

Nevertheless, from time to time a stationary state does make its appearance in the *Principles*.¹ It is employed (very tentatively, it is true) to demonstrate the relationship (or perhaps the absence of a relationship) between real costs and expenses of production. It appears again in certain versions of the preliminary view of distribution, and I should be prepared to argue, in spite of Marshall himself, that it underlies much of what there is of validity in the results achieved by the "statical method." It is no doubt in one sense a much more attenuated instrument than the original conception. Smith thought the stationary state might actually arrive, that it had, in fact, appeared in China and other places. Marshall is quite clear that the stationary state is a fiction, an analytical instrument simply.² But the basic conception is there, and that, of course, in a form whose effectiveness is vastly enhanced by all Marshall's own analytical improvements. There is no longer any doubt as to the variability of the various supply functions. There is no longer any question of causal pre-eminence for any one factor in the total equilibrium. It is the stationary state of the classics lifted on to a new plane of scientific precision.

III

1. Meanwhile, there was being developed, on the other side of the Atlantic, a method of analysis apparently similar to the one whose history we have been following, but, in fact, in important

aversion from heroic abstraction was a virtue in Marshall in that it made his constructions more realistic. Personally I should be inclined to urge that in certain connections (Increasing Returns, *e.g.*) it was responsible for a good deal of theoretical confusion. This is not to say that Clark's static state is superior to Marshall's statical method, but rather that, as suggested above, the stationary state itself is superior to the more limited conceptions.

¹ To trace the shadowy appearances, disappearances and reappearances of this "famous fiction" in the various editions would demand a paper in itself, and I suspect it would be well worth doing. We shall never understand the real significance of the Marshallian system until a *variorum* edition of the *Principles* is published. The main sections in which it is used relate to the connection between real costs and expenses of production (*e.g.* 3rd ed., p. 425, and Appendix H, para. 4, 6th ed., p. 810), and to the conception of normal price and the Representative Firm (*e.g.* 6th ed., p. 366 seq.). It appears also in earlier editions of the *Preliminary View of Distribution* at the commencement of Book VI.

Appendix H on the *Limitations of the use of statical assumption in regard to increasing returns* is very important as an indication of Marshall's attitude with regard to equilibrium analysis in general, and shows clearly, I think, the nature of the step which has to be taken if the various disputes now current with regard to the laws of returns are ever to be satisfactorily settled. I hope to return to this in another paper.

² I am indebted to my friend, Mr. A. W. Stonier of Christ Church for some very valuable comments on this aspect of Marshall's system.

respects substantially different. To these developments we must now turn.

The innovation derives from Mill. We have seen already that in his chapter on the stationary state Mill uses a conception not radically dissimilar from that of his predecessors. So too in other passages. But there is one passage which bears a different interpretation. It occurs at the commencement of the section on the influence of Progress.¹ Mill is reviewing the earlier sections on production, distribution and value, and making plans, as it were, for future developments.

“The three preceding parts,” he says, “include as detailed a view as our limits permit, of what, by a happy generalisation of a mathematical phrase, has been called the statics of the subject. We have surveyed the field of economic facts, and have examined how they stand related to one another as causes and effects: what circumstances determine the amount of production, of employment for labour, of capital and population: what laws regulate rent, profits and wages. . . . We have thus obtained a collective view of the economic phenomena of society, considered as existing simultaneously. We have ascertained, to a certain extent, the principles of their interdependence, and when the state of some of the elements is known, we should now be in a position to infer in a general way the contemporaneous state of most of the others. All this, however, has only put us in possession of the . . . laws of a stationary and unchanging society. We have still to consider the economical condition of mankind as liable to change. . . . We have to consider what these changes are: what are their laws and what their ultimate tendencies; thereby adding a theory of motion to our theory of equilibrium . . . the dynamics of political economy to the statics.”

Now it is probable that, in writing this passage, Mill had nothing more in mind than the existing notion of the stationary state, and the difference between the phenomena of such a state and the phenomena of a society which is still advancing. That at any rate is what seems to emerge from a study of the actual content of the subsequent chapters of the section.

But it is clear that, taken apart from its general context, it is capable of another interpretation. Taken as it stands, it seems to say, “We have studied what happens when the factors of production are constant. Now we must proceed to ask what causes their numbers to change.” That at least is suggested by the

¹ Ashley's Ed., p. 695.

passage, "we have still to consider the condition of mankind as liable to change . . . we have to consider what these changes are and what are their laws." In fact, as stated earlier in the passage, the laws of change in the numbers of the population and the quantity of accumulated capital were considered in the first part of the book. But it is easy to see how reading this passage, and being impressed perhaps by the verbal distinction, one might come to divide the statics and dynamics of the subject on this plan. The statics should deal with what happens when the factors are given. The dynamics, with the laws of change in the quantity of the factors.

2. Whether this interpretation of Mill's influence be true or not, it was the plan actually followed by J. B. Clark in his celebrated classification of static and dynamic phenomena in the opening chapters of *The Distribution of Wealth*. And it is usually held that, in the matter of terminology at least, Clark's debt to Mill is obvious.

Clark reaches his conception of what he calls a static state by abstracting the forces of social progress. "In any given society," he says,¹ "five generic changes are going on, every one of which reacts on the structure of society by changing the arrangements of that group system which it is the work of catallactics to study.

1. Population is increasing.
2. Capital is increasing.
3. Methods of production are improving.
4. The forms of industrial establishments are changing.
5. The wants of consumers are multiplying."

These influences he thinks are to be called dynamic. A world from which they were absent would be a static state.

To study this state, therefore, we must consider changes of this sort absent. "We must in imagination sweep remorselessly from the field the whole set of influences that we have called dynamic. We shall . . . stop . . . every one of the five organic changes that are actually moving and relocating the economic agents. . . . Population and capital are treated as neither increasing nor diminishing . . . inventions are not made, and processes of production do not change."²

3. Now at first sight a static state of this sort would appear to be exactly similar to the stationary state we find in the classics and in Marshall. And there can be no doubt that Clark thought

¹ *Distribution of Wealth*, p. 56.

² *Op. cit.*, p. 71 seq.

it to be similar. "The term natural," he says,¹ "as used by classical economists in connection with standards of wages and interest, was unconsciously employed as an equivalent of the term static. . . ." And again,² "What the Ricardian theory unconsciously and imperfectly accomplished was the separation of static from dynamic forces. It was really studying a static world, but it studied that world with no complete idea of its nature. There was not in the mind of any of these early writers any connection of the two distinct sets of forces that are really acting together; and there could therefore be no systematic plan for studying them separately. In reality their 'natural prices' were static prices. They were those to which an actual market would conform if dynamic influences were wholly to cease. . . . Stop all increase of population and wealth . . . but let industry go on and perfect competition continue, and you bring the world into a state in which the standard theoretical prices will be the real ones."

But, plausible as this identification may appear at first sight, it is, I believe, completely mistaken. It is perfectly true that in both the Clarkian and the classical construction the quantities of the factors of production are constant. *But*—and this is the fundamental difference which it is desired here to exhibit—in the one, this constancy is the condition of equilibrium; in the other, it is simply one of the resultants of the equilibrating process. In the Clarkian state, population and capital are to be constant—they are *not allowed* to vary. In the classical constructions, population and capital are constant, but this is because, together with wages and interest, etc., they have reached a position of rest. You can no more say of such a construction that wages and interest are constant because population and capital are constant, than you can say that population and capital are constant because wages and interest are constant. All that you can say is that, owing to absence of change of consumers' demand and knowledge of the technique of production on the one hand, or fundamental change in the human or material equipment of production on the other,³ a position of rest has been reached. The Marshallian analogy of the balls in the bowl was designed expressly to elucidate this conception.

¹ *Distribution of Wealth*, p. vi.

² *Ibid.*, p. 69.

³ This rather awkward wording has been chosen in order to make it clear that anything like exhaustion of mineral resources must be excluded from the strict conception of stationary equilibrium. I am not sure that it is always fully realised that this is the case. A conception of equilibrium that is to be fully realistic *must* be a conception of moving equilibrium. But this does not make the remoter construction any less convenient as a first approximation.

There is one passage in the *Distribution of Wealth* in which Clark appears to contemplate this hypothesis. In this passage¹ he allows that "the fixed condition" of capital, assumed in static analysis, "cannot exist . . . unless the motive for saving something from incomes is not equal to the motive for spending it. In the static state there is no abstinence, or creation of new capital, because, with the capital now on hand, men would lose more by foregoing pleasure and making their fund larger than they would gain by doing so." This passage is important. It shows that on essentials there is no difference of opinion as to facts—that is to say, tendencies operative in the real world—between the Clarkian and the neo-classical tradition. But it cannot be held to vitiate the interpretation which has just been given of the general Clarkian conception of static laws and static conditions. For in his very next sentence Clark goes on to say that "the whole subject of creating capital belongs to the dynamic division of the science of economics." Now it should be abundantly clear that if as a matter of hypothesis you stop tendencies to change *other than those assumed to be implicit in the form of the supply and demand functions*, then capital creation and population may go on for some time before stationary conditions in the classical sense are ultimately attained. At any given time, if you stop the creation of capital and the increase of population (demand being assumed constant), then static equilibrium in the Clarkian sense is only so far distant as economic friction delays it. But if you merely stop ultimate changes of taste and technique, then, if the effective desire of accumulation is of a certain order, capital increase (to say nothing of the increase of population²) may go on for an almost infinitely long period.

Moreover, the whole treatment of the rest of the book is at variance with this particular passage. Again and again we are told that we must stop in imagination the growth of population and the increase of capital if we are to be in a position to examine the working of static laws. But, of course, if you assume stationary conditions as Marshall and the classics assumed them, this degree of hypothetical violence is unnecessary. All that is necessary is that technique and demand of all kinds (including demand for income in terms of effort and abstinence) shall remain unchanged, and, if exhaustible natural resources are not employed, the

¹ *Op. cit.*, p. 136.

² In the classical and Marshallian constructions there is supposed to be a functional connection between wages and the production of labourers. It is a matter for dispute how far such a supposition is justifiable.

stationariness desired will then arrive as part of the general equilibrium. This is perfectly clear from the Marshallian treatment. It is no less implied by the constructions of his predecessors. It was the absence of improvements which characterised the stationary state of Ricardo.¹

Finally, in this connection, we may appeal to Clark's own criterion of static conditions. "The absence of any flow of labour or of capital from one group to another is the sure outward sign," he says, "of the static condition."² Is not this insistence on what may be called horizontal adjustment according to demand for different commodities, rather than on such adjustment *combined* with what may be called vertical adjustment according to demand for income from work and abstinence, proof sufficient of the contention here urged? It is impossible to conceive that one who held consistently the view that a stationary state of population was a result rather than a pre-established condition of the stationary equilibrium he was contemplating would have failed to mention it at this juncture. Of course it could be argued that stationariness at any one point implies stationariness at any other, so that stationariness between groups is as good an index of stationary equilibrium in its fullest sense as stationariness in the total quantities of factors of production *and* stationariness between groups. And up to a point that argument might be sustained. But it can scarcely be adduced in the interpretation of one who has insisted that, in order to study static conditions, we must keep constant the supply of factors, and watch the resulting equilibrium.³

IV

1. If this analysis of the position be correct, then we must recognise not one general class of "static states" and "static laws," but two: the classical conception in which the condition of stationariness is the resultant of the balancing of forces tending to change, and the Clarkian in which the factors of production are stationary by hypothesis, and equilibrium is attained within these

¹ Mill in an incautious moment suggested that improvements might continue in a stationary state. Of course if stationary state is used to designate merely a community where population has ceased to grow, this is true. But if it is used in the technical sense we are discussing it is highly improbable. It would be an odd series of inventions which had no influence on the supply of savings.

² *Op. cit.*, p. 400.

³ It could be argued further that unless the interpretation of Clark given above is correct, Clark's static laws of wages, etc. are wrong. If supply is not fixed it is false to say that wages are *determined* by marginal productivity. But on all this see below.

conditions. Both rule out inventions and fundamental changes in nature and human beings. But the one admits the possibility of variations of labour and capital, the other excludes these by definition. In an ultimate classification of course the Clarkian conception can be regarded as a limiting case of the wider concept—the supply curves of capital and labour exhibiting absolute inelasticity. But it is clear that this is not how it is conceived by its inventor.

Now in a matter of abstractions of this sort there is no question of rightness or wrongness. If Clark chooses to assume that labour and capital do not increase, and if he calls the description of what happens under such circumstances, static analysis, we cannot gainsay him. In judging such constructions we do not ask whether they are *right or wrong*—that is a question which is only relevant to the logic of the subsequent inferences: we ask only whether they are *appropriate*.

In fact, of course, each construction is appropriate for particular stages of analysis. It is convenient for some purposes to suppose that the supplies of the ultimate factors are given. For other purposes it is more convenient to assume that they have a certain flexibility. If we wish to study the short period effects of a change in technique, *e.g.*, the Clarkian hypothesis is sufficient. If we wish to take account of the subtler but more far-reaching effects over time, we do well to have recourse to the wider conception. The modern economist with even the most fleeting acquaintance with the mathematical theory of equilibrium, will recognise in the two constructions we have been examining, *not competing abstractions, but successive stages of exposition*.

None the less it is fundamentally important that they should be kept apart, that the difference should be clearly shown and its implications fully realised. We have seen already, in discussing the significance of the two hypotheses as regards the distance in time from the ultimate equilibrium of any given disequilibrium, how vast this difference may be. We see it still more vividly if we survey a few of the more prominent controversies of modern times in which the distinction has not been clearly recognised. It is arguable, I think, that most of the more respectable disputes of the past hundred years have arisen just because of a failure on the part of the disputants to define clearly their ultimate assumptions with regard to the conditions of equilibrium.

2. We may take as our first example the celebrated disputes of the 'nineties and the first ten years of this century concerning the ultimate nature of real costs. Are the forces limiting the

supply of particular commodities to be regarded as being of the same order as the forces limiting demand? Or are we to admit elements of real costs which are not of this nature? Are all costs ultimately resolvable into foregone products, or are labour-pain and abstinence to be regarded as ultimate? Here there was a battle of the giants: on the one side Marshall and Edgeworth, on the other the great Austrians, together with Wicksteed and some of the Americans.¹

Now I am far from arguing that there was nothing but misunderstanding in this controversy. No doubt it did bring to light fundamental difficulties in the old doctrine of real costs, and pave the way for the now almost universal recognition that even when disutilities are taken into account they are ultimately to be regarded as being the pull of foregone leisure or foregone present income—opportunity costs rather than disutilities in the sense of the old hedonistic calculus.² But, surveyed from the calm distance of a quarter of a century, it is abundantly clear that the main difference of opinion arose not so much from a difference of opinion as to the totality of forces operative in the economic system, but from a failure on the part of the participants to perceive that each was adopting a different assumption with regard to the conditions of equilibrium. As we have seen, Marshall did not greatly favour the use of the Clarkian terminology, and the great Austrians Wieser and Böhm-Bawerk were writing before it had been made known by Schumpeter to continental circles. But close examination of their respective work reveals precisely the difference of assumption we have been examining. Marshall and Edgeworth were assuming the fluidity of supply of capital and labour which was characteristic of the classical conception of equilibrium. Böhm-Bawerk and Wieser were assuming the fixity of supply which is the assumption of the Clarkian statics. Granted this initial difference of assumption, the conclusions were bound to be different. If

¹ See, e.g., Edgeworth's review of Böhm-Bawerk's *Positive Theory of Capital* (Papers relating to Political Economy, III); Böhm-Bawerk, *Der Letzte Masstab des Güterwertes*, Gesammelte Schriften, pp. 404–70; D. I. Green, *Pain Cost and Opportunity Cost* (Q.J.E. Vol. VIII p. 218); Davenport, *Value and Distribution*; Wicksteed, *Commonsense of Political Economy*, Chapter IX.

² Mr. Henderson's *Supply and Demand* may be regarded as indicating the definite abandonment of the old absolute conception of real costs. Professor Pigou's treatment of the Real Costs of War (*Political Economy of War*, Chapter III) also follows the Austrian conception. But Mr. Henderson does not show very clearly how, if the supply of capital and labour are to be taken as variable, the resistances are to be worked into the opportunity cost concept. This, I think, has been more satisfactorily worked out by Professor Knight. (*Risk, Uncertainty and Profit*, Chapter III.)

the supply of factors is fixed, "disutility," whether conceived in the terms of old-fashioned hedonism or in the more modern sense of displaced resistances, is simply a matter of foregone *products*. If, however, flexible supplies are contemplated, clearly it has a wider significance. We tend to regard it as foregone leisure in the case of work, foregone income in the present in the case of the use of capital, whereas Marshall and Edgeworth were still thinking in terms of something more absolute; but the contrast with the narrower hypothesis is the same. Eventually this came to light,¹ but not before much ink had been wasted.

3. Secondly, we may take certain disputes concerning the theory of wages. Clark, as is well known, asserted that in a state of pure competition, wages are *determined* by final productivity. Others, recollecting the variability of labour supply, have insisted that, since until the form of the supply function is known one cannot tell what productivity is to be regarded as final, this cannot be admitted. Wages measure or express final productivity, but the process of determination is multiple. Of course these objections are fully justified in so far as the Clarkian theory pretends to be a complete explanation of wage determination. But in so far as Clark is merely asserting what would happen in his static state they miss the point. In the Clarkian state the supply of labour is given. There is no need to elaborate this point at length, as it is well known. Carver dealt with this limitation of the productivity theory at a very early stage,² and Mr. Dobb has a very elegant discussion of the same matter in his recent work on *Wages*.³

4. Thirdly, we may take the age-long controversies concerning rent and cost. In what sense, if any, is it true to say that rent does not enter into cost of production? Here, too, we have a case in which much of the controversy of recent years is seen to depend on the last analysis upon differences of hypothetical construction rather than differences of actual logic.⁴

If we are considering a state of equilibrium in which the total supplies of the factors of production are fixed by hypothesis—

¹ See Edgeworth, "Böhm-Bawerk on the Ultimate Standard of Value" (*Papers relating to Political Economy*, III. pp. 59-64).

² Carver *Distribution of Wealth*, pp. 134-184.

³ Dobb, *Wages* (Chapters IV and V).

⁴ Of course this is not the whole story. It would be hard to contend that all who have supported the proposition have been alive to the consideration regarding the reversibility of the rent analysis adduced by Clark and Wicksteed, or that all who have opposed it have understood the implications of the idea of the intensive margin. But we are dealing here with the differences of opinion of really capable economists!

the Clarkian static state—then clearly rent has the same relation to price as any other income. It is true that if we go to the rentless margin we can say that rent does not “enter into” cost there. But it is equally true that if we go to the wageless margin we can say that wages do not “enter into” cost there. But this is a mere tautology, and, of course, proves nothing. In the conditions here contemplated, costs are obviously the outlays which have to be made in order to secure the use of the factors of production for the production of this commodity rather than for the production of that. Neither from the point of view of the entrepreneur, nor from the point of view of society, is there any reason to regard payment for “land,” in the Ricardian sense, as being on a footing different from the payment for other agents.

And, so far as the point of view of the entrepreneur is concerned, this is still true when we turn to contemplate the phenomena of a stationary equilibrium, in which the supplies of the technically variable agents are free to vary—the stationary state of the classics. It was Marshall himself who insisted that “land is but a particular form of capital from the point of view of the individual producer. The question whether a farmer has carried his cultivation of a particular piece of land as far as he profitably can, and whether he should try to force more from it or to take in another piece of land, is of the same kind as the question whether he should buy a new plough or try to get a little more work out of his present stock of ploughs. . . .”¹ But from the point of view of society there is this difference between the payments made for the use of Ricardian “land” and agents in fixed supply in general, and payments for the use of factors in flexible supply, that we must assume that, if prices were different, the supplies of the flexible factors would be different; but we need not make any such assumptions about the supply of the fixed factors. By definition their supply would be unaltered. It is still undesirable to say that rent does not form part of cost. But in the sense in which cost is to be interpreted as the price of keeping supply constant, there is significance of a sort in the Ricardian proposition that rent does not “enter into” this conception. “Rent in the Ricardian sense is still a transfer expense. Other costs are stationary supply prices. It was clearly recognition of this which led Marshall to remark that “it is *wisest not* to say that rent does not enter into cost of production, for that will confuse many people. But it is *wicked* to say that rent does enter into cost of production, because that is *sure* to be applied in such

¹ *Principles* (8th ed.), p. 430.

a way as to lead to the denial of subtle truths which, in spite of their being subtle, are of the very highest importance scientifically, and also in relation to the practical well-being of the world.”¹

It is improbable that at the present day there would be found many economists who would regard it as “wicked” to say that rent does enter into costs of production. But it is true that, if we are contemplating a stationary equilibrium of the kind conceived by the classics, the proposition that it does not, does imply, even if it does not state correctly, subtle truths which we should be ill advised to lose sight of. And it is significant that those who have urged most strongly for its retention have been those who have learnt their analysis from classical sources whereas those who have opposed it have been very largely under the influence of Clark.

5. Finally, we may take a case of more recent interest—the dynamic theory of interest propounded by Professor Schumpeter. This is a theory which, in my view, is quite definitely wrong. It cannot be salvaged by making its assumptions more explicit. Nevertheless, it seems possible that the ambiguity we have been examining may be responsible for what seems to be the flaw in Professor Schumpeter’s reasoning.

The theory may be summarised in two propositions.² The first is negative. Under static conditions, says Schumpeter, there can be no interest. All costs are to be imputed back to the two ultimate factors of production, Labour and Land. All incomes, therefore, are either wages or “rent.” There is no third class of static incomes. It follows—and this is the second and positive proposition—that interest must be a dynamic income. It emerges only when conditions are changing, and if change were to cease it would disappear. Clark urged that profit was essentially a dynamic surplus; Schumpeter goes one further and urges that the same is true of interest.

Now, there are certain features of Schumpeter’s theory with which agreement is possible. We may agree, for instance, that in periods of change there are operative certain frictions which may

¹ *Memorials*, p. 436.

² It is naturally not possible to provide a complete account of Professor Schumpeter’s theory here. I hope to do this on some future occasion. This part of his work is not as well known as it should be to English readers, and although I believe his theory of interest to be wrong, it is wrong, I believe, in a way which has positive significance. Certainly no one can read either the *Wesen und Hauptinhalt der Theoretische Nationalökonomie* or the *Theorie der Wirtschaftliche Entwicklung* without feeling that his outlook on certain parts of economic analysis can never be quite the same again. For a thorough exposition of the theory in question these two works must be consulted.

result in more saving being imposed on the community than would have been undertaken voluntarily if these frictions had not been operative. On this point Mr. Robertson's analysis remorselessly drives home one of the main contentions of the Schumpeterian theory of development. We may admit, too, that in so far as interest is defined as the yield of *new* capital in the sense of net additions to the social stock, it is a phenomenon which will be absent from any conception of static conditions, for, by definition, capital is not increasing.

But in so far as interest is conserved not as a payment for new accumulation, but as a net return (*reinertrag*) to produced means of production (*produktzierte produktionsmitteln*)—and, in spite of some ambiguities, this is, I think, the interpretation which Schumpeter would have us put on the negative part of his theory—the theory does not seem to be acceptable. So long as we assume private property and exchange it is difficult to conceive a static state in which there exists produced means of production which earn no net income. Why should labour and the use of material factors be devoted to the maintenance of the produced means of production if no net remuneration is forthcoming? It is when we encounter a theory of this sort that we realise the unassailable core of truth underlying the old abstinence theory.

But why should Schumpeter have propounded such a theory?

Partly, no doubt, because of a sense of the practical importance of elements of transitory gain in the determination of actual rates of interest. This I should be inclined to say is the element of truth in the background. But partly, I think, because of a curious misunderstanding of the Clarkian analysis and a reliance upon the Clarkian construction in a way which prevents recognition of the existence of "static tendencies" in the wider sense of this term. Schumpeter's debt to Clark in this matter is obvious. He adopts the terminology. He bases his initial discussion of development on Clark's five-fold classification. And if this diagnosis is correct, in the last analysis, it is the rigidity of the supply of capital in the Clarkian construction which blinds him to the nature of the forces making for the emergence of interest under conditions which are not dynamic.

We can see this quite clearly if we look closely at his argument in the first chapter of the *Entwicklung*. There is no abstinence in the static state, he argues, following a passage of Clark's which we have already examined.¹ And, as we have seen, if abstinence

¹ *Entwicklung*, p. 48.

is to be regarded merely as that degree of refraining from present consumption which gives rise to net addition to the supply of capital, we can agree with him. But if abstinence is to be regarded, not only as a refusal to consume in the present in order that the sources of future income may be *enlarged*, but also as a refusal to enhance one's consumption now in order that the income of the future may not be depleted, then it seems to me that he is at fault and the Clarkian terminology is misleading.¹ But Schumpeter has foreseen some such objection as this, and he proceeds to argue that under static conditions there is no reason to alter the distribution of one's income through time. Given constant needs, he argues, as others have argued before him,² the maximum satisfaction will be attained by a constant flow through time. Under such conditions, to encroach upon capital, now, to enhance the income of the present at the expense of the income of the future, would be folly. The gain now does not counterbalance the loss then. The argument is valid, but it does not prove that interest is absent when conditions are not changing. On the contrary, it is the most important part of the case for the view that there must be interest in such circumstances. *For if there were no yield to the use of capital (no reinertrag) there would be no reason to refrain from consuming it.* If produced means of production are not productive of a net product, why devote resources to maintaining them when these resources might be devoted to providing present enjoyment? One would not have one's cake rather than eat it, if there were no gain to be derived from having it. It is, in short, *an* interest rate, which, other things being given, keeps the stationary state stationary—the rate at which it does not pay to turn income into capital or capital into income. If interest were to disappear the stationary state would cease to be stationary. Schumpeter can argue that no *accumulation* will be made once stationary equilibrium has been attained. But he is not entitled to argue that there will be no *decumulation* unless he admits the existence of interest.

What has happened, I think, is this. Schumpeter has based his static analysis upon Clarkian constructions in which the supply of capital is *held rigid*. Then when he comes to argue that there is no interest unless there is dynamic change, ignoring the fact

¹ Clark himself, of course, held a productivity theory of interest. We have seen that in this one passage he correctly formulates the conditions of stationary equilibrium in the wider sense.

² *E.g.* Landry, whose *Intérêt du Capital* is in many ways the best treatment of this important subject.

that a wider conception of stationary equilibrium is possible, he is blind to the consideration that if things are free to move, stationariness depends *inter alia* upon the interest payment being at a certain level. Such an interpretation may seem fantastic. Yet it is the only reason I am able to conceive for his apparent unawareness of the significance of the possibility of decumulation.¹ It is no accident, I suggest, that when he is discussing the distribution of income over time, he chooses a case where the possibility of decumulation in a straightforward way is absent—the income of a pensioner of the state!²

¹ There is one passage in the *Entwicklung* (p. 48) in which Schumpeter admits the possibility of abstinence in the sense of refraining from decumulation, but he brushes it aside as irrelevant to his construction: "Von Abstinenzun sinne von Nichtkonsumtion der Ertragsquellen kaun—nicht die Rede sein, weil es unter unsem Voranwetzungen andre Ertragsquellen als arbeit und Boden nicht qift." I confess I find this incomprehensible. Either there exist "produced means of production," or there do not. If they exist, then, if they are to be maintained, other factors of production must provide for their maintenance, and decumulation will consist in using these factors for making provision for present enjoyment. Or they do not exist, in which case to deny the existence of a return to them is merely tautologous and proves nothing whatever. To state dogmatically that "arbeit und Boden" are the only "Ertragsquellen" in any static conditions is, of course, to beg the question completely.

² It might be argued—I do not think it would be argued thus by Schumpeter himself, but it might occur to readers unacquainted with his actual theorem—that all he is denying is the existence of interest in what have been here called Clarkian conditions. I do not think that this would save the theory (Professor Knight has shown that even under such conditions a rate of capitalisation is implicit: *Risk, Uncertainty and Profit*, pp. 129–140), but in any case the interpretation cannot be accepted. Schumpeter's main contention is the positive one that interest is only conceivable under dynamic changing conditions. If the argument outlined above is correct, a rate of interest would exist if there were no change in the economic system but the factors were free to vary. Such a state of affairs, the stationary state of the classics, cannot by the wildest stretch of terminology be described as dynamic!

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DYNAMICS, STATICS, AND THE STATIONARY STATE

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I

UNTIL now, much of dynamical economic analysis has been concerned with the business cycle. This may seem so natural as to be hardly worthy of explicit comment. Nevertheless, it was not inevitable; and if in the future the business cycle, as we have known it, should undergo extreme modifications, a need for dynamical analysis in connection with many economic problems would still exist. Thus, we should still need a theory of the path by which a given market approaches its equilibrium position, not for sake of the theory alone, but for the information that such knowledge throws upon the direction of displacement of the new equilibrium position as well.¹

In comparatively recent times, significant advances have been made in analytical dynamics. A rigorous differentiation between statics and stationariness, between dynamics and history, is now possible. The present essay attempts, first, to elucidate the nature of these concepts and to contrast them with some other prevalent usages of the terms; and, second, by means of the concepts to go back to analyze the very important notion of the *circular flow*. In doing so, I am not attempting to improve upon what I consider a logically consistent argument, but rather am endeavoring to amplify the discussion at critical points where confusion has arisen.

II

Often in the writings of economists the words "dynamic" and "static" are used as nothing more than synonyms for good and bad, realistic and unrealistic, simple and complex. We damn another man's theory by terming it static, and advertise our own by calling it dynamic. Ex-

¹This truth is summarized in what I have called the *correspondence principle*, which points out the intimate connection between the purely dynamical aspects of a system and its comparative static properties. See P. A. Samuelson, "The Stability of Equilibrium, I and II," *Econometrica*, ix (1941), pp. 97-120; x (1942), pp. 1-25.

amples of this are too plentiful to require citation.

Some writers attempt to distinguish between statics and dynamics by analogy with what they understand to be the relationship in theoretical physics. That this is a fruitful and suggestive line of approach cannot be doubted. But it is too much to suppose that very many economists have the technical knowledge necessary to handle the formal properties of analytical dynamics. Consequently, they become bogged down in the search for economic concepts corresponding to mass, energy, inertia, momentum, force, and space. A case in point is Professor Frank Knight's otherwise stimulating essay on *Statics and Dynamics*.²

It is certainly true, notably in the writings of Marshall,³ that economists have made use of biological as well as of mechanical analogies, in which evolution and organic growth is used as the antithesis to statical equilibrium analysis. In general the results seem to have been disappointing; viz., the haziness involved in Marshall's treatment of decreasing cost. And if one examines the more exact biological sciences, one looks in vain for any new weapon, secret or otherwise, for discovering scientific truths. If the bloodstream is capable of a simple, abstract, rigorous description in terms of the usual laws of physical thermodynamics, so much the better; if not, one must be content with more complicated, unwieldy explanations. Indeed, according to the late L. J. Henderson the very notion of a stable equilibrium, so characteristic of physical theory, was actually

²Chapter VI of *The Ethics of Competition* (New York, 1935). This is an English translation of an article in *Zeitschrift für Nationalökonomie* for 1930.

³See the references to *statical method* and *biology* in the Index to the eighth edition of the *Principles*. In none of his writings does Marshall show more than a passing familiarity, such as might be expected of any intelligent layman, of the biological notions of his time. Therefore, he could not be expected to have discerned the lasting truths from the fashions of the moment. Nevertheless, writing at the time that he did, it was inevitable that he should have been influenced, if not convinced, by the Spencerian doctrines popular at the end of the nineteenth century.

first observed empirically in connection with the resistance of the human body to disease, and formulated by the ancients as the well-known *vix medicatrix naturae*.⁴

Nor should the problems encountered in the biological field be considered necessarily more complex and less subject to simple formulation than those in the physical field. Few biological sciences are less "exact" than meteorology, which must certainly be included in the physical sciences.⁵ Here, simple and abstract theories spun out from a few assumptions are likely to be inferior to the intuitive hunches of experienced practitioners, but this is only a reflection of the present primitive state of the subject. New truths are ascertained in the same way as in more advanced subjects, and it is to be hoped that rule of thumb may be replaced by more exact and unambiguous formulations.⁶

III

Leaving aside all analogies with other fields, there has necessarily been within the main corpus of economic theory a preoccupation with dynamics, if only implicitly. The Classical economists from Smith through Mill had theories of the long-run movements of population and accumulation.⁷ J. B. Clark rigidly separated the static from the dynamic in his thought.⁸ (Other examples could be multi-

⁴ See J. A. Schumpeter, *The Theory of Economic Development* (Cambridge, Mass., 1934, English edition), Preface, p. xi, for Mill's notions of statics and dynamics and their intellectual origins.

⁵ Of course, it can be said that experimentation is not possible in meteorology as in other physical sciences. But what about astronomy, in some ways the most exact of all, in which no experimentation is possible?

⁶ In discussing the limitations of mathematical methods in economics, Professor Viner expresses the belief that the biological character of the subject, so to speak, makes such methods of limited applicability. By this I take him to mean that the subject is complex and difficult, not that any fundamentally different methods of investigation are required. See "Marshall's Economics, the Man and His Times," *American Economic Review*, XXXI (1941), pp. 223-36.

Gustav Cassel in his *Fundamental Thoughts in Economics*, Chapter I, considers Economic Dynamics to be a third stage of analysis, following a pure Static Economy and a "quasi-static" Uniformly Progressive Economy.

⁷ See L. Robbins, "On a Certain Ambiguity in the Conception of Stationary Equilibrium," *Economic Journal*, XL (1930), pp. 194-214.

⁸ J. M. Clark has wished to carry on where his father left off, to construct a dynamics which would supplement statics. See J. M. Clark, *A Preface to Social Economics* (New York, 1936).

plied.) Clark's celebrated static state and the "circular flow" of Professor Schumpeter raise a rather vexing point of terminology — the relation of static to stationary — now cleared up more or less to the satisfaction of everybody by Professor Frisch.

Stationary is a descriptive term characterizing the behavior of an economic variable over time; it usually implies constancy, but is occasionally generalized to include behavior periodically repetitive over time. Used in this sense, the motion of a dynamical system may be stationary: e.g., the behavior of a pendulum satisfying Newton's Laws of Motion, but subject to no disturbance and hence remaining at rest; or the behavior of national income after a change in investment has given rise to dwindling transient geometric progressions of the usual "block-diagram" character.

Statical then refers to the form and structure of the postulated laws determining the behavior of the system. An equilibrium defined as the intersection of a pair of curves would be statical. Ordinarily, it is "timeless" in that nothing is specified concerning the duration of the process, but it may very well be defined as holding over time. A simple statical system as defined above would also have the property of being stationary; but as we shall see in a moment, statical systems can be devised which are not stationary over time.

In defining the term *dynamical*, at least two possibilities suggest themselves. First, it may be defined as a general term including statical as a special rather degenerate case. Or on the other hand, it may be defined as the totality of all systems which are *not* statical. Much may be said for the first alternative; the second, however, brings out some points of controversy in the literature and will be discussed here. This decision involves no point of substance, since only verbal problems of definition are involved.

We may say that a system is *dynamical* if its behavior over time is determined by functional equations in which "variables at different points of time" are involved in an "essential" way. This formulation is to be attributed to Professor Frisch.⁹ Special examples of such systems

⁹ Ragnar Frisch, "On the Notion of Equilibrium and Disequilibrium," *Review of Economic Studies*, III (1935-36), pp. 100-106.

are those defined by *difference* equations, i.e., those involving a variable and its lagged values; integral equations in which the preceding values of the variable enter in a "continuous" way. By a liberal interpretation of the circumlocution "variable at a different point of time," we may bring differential equations under the definition, remembering that differential coefficients characterize the behavior of a function in the neighborhood of a point. Mixed types and more general functionals are included.

Attention is called to the fact that variables at different points of time must enter into the problem in an *essential* way. Thus, a system involving a rate of production per unit time, i.e., a time derivative, may yet be statical. This is because the variable of which the rate is the time derivative may have no economic significance. It can be interpreted as the cumulated amount of production from the beginning of time or from an initial date; no significant economic process depends upon this variable. The necessity for the present insistence may be apprehended if it is realized that every variable can be written as the derivative of something, namely its own integral. Moreover, a system may be pseudo-dynamic in the sense that formal manipulation of it permits us to reduce it to statical form. Unless, therefore, we reserve the designation dynamics for systems which involve economically significant variables at different points of time in an *irremovable* way, we shall find that no non-dynamic systems exist.

According to the present definition the historical movement of a system may not be dynamical. If one year the crop is high because of favorable weather, the next year low, and so forth, the system will be statical even though not stationary. The same is true of a system showing continuous growth or trend, if the secular movement is taken as a datum and if the system adapts itself instantaneously.¹⁰

On the other hand, a truly dynamical system may be completely non-historical or causal, in the sense that its behavior depends only upon its initial conditions *and the time which has elapsed*, the calendar date not entering into the process. For many purposes, it is necessary to

work with systems which are both historical and dynamical. The impact of technological change upon the economic system is a case in point. Technological change may be taken as a historical datum, to which the economic system reacts non-instantaneously or in a dynamic fashion. Another instance is provided by a business cycle of a regular periodic character, which results from impressing an oscillatory outside force upon a mechanism with an intrinsic (damped) period of its own.

We may distinguish, then, four distinct cases made up of all possible combinations of static-dynamic and historical-causal:

1. Static and stationary
2. Static and historical
3. Dynamic and causal (non-historical)¹¹
4. Dynamic and historical

Almost all systems can be placed in one of these categories; and, depending upon the point of view or purpose at hand, the analysis may be formulated so as to put a given system arbitrarily in one category rather than another. Thus, if a system is very heavily damped so that it approaches its equilibrium value extremely rapidly, its dynamic features may be passed over in order to simplify the analysis.

Or a system which is causal from a very broad viewpoint may be regarded as historical if certain movements are taken as unexplained data for purposes of the argument. (In fact, every historical system is to be regarded as an *incomplete* causal system.) To a meteorologist-economist a business cycle caused by weather disturbances and sunspots would constitute a causal process. But ordinarily the economist is willing to regard the causation as unilateral and to adopt a division of labor in which he does not study astronomy but considers his job as done when he has pushed economic analysis to a "non-economic" cause.¹² However, there is nothing sacred about the conventional bound-

¹⁰The notion of causation in a closed interdependent system is exceedingly slippery and ambiguous. As used here, a system is said to be causal if from an initial configuration it determines its own behavior over time. While it is not appropriate to say that one subset of variables causes another to move, it is permissible to speak of a change in a given parameter or datum as causing changes in the system or in its behavior over time.

¹¹J. A. Schumpeter, *Theory of Economic Development*, Chapter I.

¹²I conceive Henry Moore's moving equilibrium to be of this statical type, although the movements around the secular trend are dynamic in character. H. L. Moore, *Synthetic Economics* (New York, 1929).

aries of economics; if the cycle were meteorological in origin, economists would branch out in that direction, just as in our day a political theory of fiscal policy is necessary if one is to understand empirical economic phenomena.*

In his recent book *Value and Capital*,¹⁸ Professor Hicks has given an exceedingly simple definition of dynamics: "I call Economic Statics those parts of economic theory where we do not trouble about dating; Economic Dynamics those parts where every quantity must be dated" (p. 115).

In terms of the above four categories this definition is overly general and insufficiently precise. The second category consisting of historically moving static equilibria would certainly require dating of the variables, but it would not thereby become dynamic. My objection is to his definition, not to his practice, for many of the systems which he analyzes are in the strict sense dynamic.¹⁴

* See supplementary note following this article.

IV

We are now in a position to utilize the above concepts to analyze the famous notion of the stationary state, which received its highest expression in the first chapter of Professor Schumpeter's classic.¹⁵ Upon rereading this chapter once again I have been struck by the degree to which the exposition is poetic in form. It is like a parable, and its wisdom lies between, as much as within, the lines.

It is, above all, clear that the *stationary* state is not *statical*. The recurring economic processes take place in time, and decisions are made with respect to variables at different points of time. Although the process is repetitive and "synchronized," there is not simultaneity between the application of inputs and the appearance of their imputable output.¹⁶ The circular flow is a stationary solution of a dynamical

process. If once established it would repeat itself. Moreover, the equilibrium is stable in the sense that, once disturbed from equilibrium, the system would move over time so as to approach it again asymptotically. It does not matter for the purpose at hand that empirically new inventions will be forthcoming so as to prevent the system from remaining at rest, nor that ideally the system will only approach equilibrium asymptotically and never reach it.

Simply to fix ideas the system may be compared with a pendulum moving in a viscous fluid. If disturbed by a shock, it will depart from the equilibrium position; but its motion will be damped by the dissipation of energy due to the "friction" resulting from its passage through the viscous fluid. Strictly speaking, even if all shocks were to cease, the system would never regain the equilibrium position, although it would approach indefinitely close to that position.

In the essay referred to above, Professor Robbins pointed out that economists who write on the stationary or static state (between which he does not distinguish) differ in their assumptions. Some simply postulate fixity of the amounts of the productive factors; others postulate fixity of the supply schedules of the productive factors, with stationary behavior of the amounts of the factors emerging along with the fixity of all of the unknowns of the solution as a consequence of the conditions of equilibrium which make the system determinate. Professor Robbins expressed the belief that J. B. Clark belonged to the first group. He goes on to argue that the same is true of Professor Schumpeter, and that because of this fact there is a serious flaw in his theory of interest.¹⁷

It is clear that Professor Schumpeter's theory of a zero rate of interest in a stationary circular flow economy could be dispensed with, and no great harm would be done to his theory of the cycle or of development. Instead of tending to rebound to a zero rate, the interest rate would tend, after a period of innovation, to return to some other rate, alleged to represent an intrinsic rate of time preference or impatience, or any other broad margin which the ingenuity of an economist can devise. Nevertheless, his interest rate theory has received a large share of the attention of critics.

¹⁷ Robbins, *op. cit.*, pp. 211-14.

¹⁸ J. R. Hicks, *Value and Capital* (Oxford, 1939).

¹⁴ One wide class of processes cannot easily be brought under my classification; viz., stochastic processes. The initial idea of such processes seems to be due to G. U. Yule. See the references to the works of Yule, Slutsky, and Frisch in H. Wold, *A Study in the Analysis of Stationary Time Series* (Uppsala, Sweden, 1938).

¹⁵ I do not know, however, to what extent Professor Schumpeter would agree with the following remarks.

¹⁶ This does not imply that capital can be measured by any time period of production, or that there is any meaning or significance to any average period of production.

Therefore, an examination of Professor Robbins' argument, to see whether or not there is an intrinsic flaw in the assumption of a zero rate of interest, is not unimportant. Our answer may be stated in advance: Professor Robbins is wrong, economically, mathematically, and logically. His argument may be briefly summarized. If in the circular flow the amount of capital is to be constant with the rate of interest zero, it must be shown that this is consistent with the motivated activity of the people whose actions will make the amount of capital remain constant. But this is impossible since some bait will be necessary to prevent people from consuming their capital, or from attempting to do so. Thus, he says:

"Why should labour and the use of material factors be devoted to the maintenance of the produced means of production if no net remuneration is forthcoming?" (p. 212) . . . "For if there were no yield to the use of capital (no *reinertrag*), there would be no reason to refrain from consuming it" (p. 213). . . . "It is in short, an interest rate, which, other things being given, keeps the stationary state stationary — the rate at which it does not pay to turn income into capital or capital into income" (p. 213).

I may note in the first place that this argument is distinct and quite independent from other criticisms of the zero interest rate, such as that of Professor Knight which is discussed below.

In the second place, a detailed structural analysis of the argument will show it to be an incomplete demonstration. At every step the controversial point at issue is begged. Why should capital be maintained at a zero rate of interest? Why should it not? If an [Robbins' italics] interest rate is needed to keep the stationary state stationary, why should it not be a zero rate? In the economic literature we are quite accustomed to an *entrepreneur faisant ni benefice ni perte* and who is yet maximizing his profits. It is often necessary to keep running just in order to stand still! And so here, in this quite different connection, part of the problem is to see whether or not it is impossible for a rational being to refrain from eating up his capital at a zero rate of interest.

Now, of course, one cannot prove the possibility of a zero rate by such verbal circum-

locutions as these any more than Professor Robbins could demonstrate his thesis by the same methods. Economic theory, fortunately, can put a matter like the present one to a decisive test. For a question of fact is not being debated, but rather a question of logical possibility and necessity emerging from a particular set of agreed-upon axioms.

Let us assume, therefore, a condition of perfect certainty and an economy consisting of one or more individuals. We further assume, since otherwise the discussion can end before it starts, that there is no intrinsic rate of time preference.¹⁸ We need not speculate as to whether or not this implies infinite life expectations for the individual, for his family, etc., etc., since in any case we are not concerned here with the realism or the usefulness of the argument. For our purposes it is convenient to adopt the quite arbitrary assumption that utility is a given function of income in each period; more specifically, that it is the same function at each instant of time and that the individual acts so as to maximize the sum of utilities thus defined over all future time.¹⁹

Actually, this seems to be Professor Schumpeter's assumption.²⁰ Probably today he would be less likely to read normative, or ethical, significance into this assumption, and would be less likely to regard deviations from it as reflecting irrationality. On the other hand, although perhaps only as a coincidence, this assumption is nearer the modern tendency to regard savings as taking place institutionally and quasi-automatically for reasons of power, insecurity, etc., and only secondarily as a result of a weighing of present and future consumption preferences. Thus, time preference theories are rather at a discount these days; indeed, many would go further than Professor Schumpeter and expect *attempts* to make positive accumulations even at a zero or negative rate of interest.

¹⁸ The observational significance of the concept of time preference is rarely discussed in the literature, and need not concern us here.

¹⁹ Hicks, Tintner, and others have discussed the problem of consumers' behavior over time under much less restrictive assumptions. See particularly numerous articles by the latter in *Econometrica* of the last half a dozen years. In the *Review of Economic Studies*, IV (1937), I discuss some of these issues under the title, "A Note on Measurement of Utility," pp. 155-61.

²⁰ *Theory of Economic Development*, p. 35, *passim*.

Our problem now becomes a simple mathematical exercise. We assume one or more individuals with fixed incomes throughout the future, with diminishing marginal utility, operating under conditions of perfect certainty so that by borrowing or lending at a zero rate of interest they can modify their consumption streams over time. How will such individuals behave? Will they borrow, that is eat up their capital, so as to have more consumption now; will the absence of any net yield make them unwilling to refrain from consumption out of capital?²¹ Professor Robbins would have to answer *yes*.

But he may be shown to be wrong. The question may be treated mathematically as a simple iso-perimetric problem in the Calculus of Variations, or as a problem in many variables. The fact that an infinite set of variables is involved (consumption over all future periods) provides no real difficulty. In fact, intuition without mathematics leads to the same result. The substitution, on even terms, of future consumption for present, would never pay if in the original situation one planned to consume evenly over time. For the increment of future consumption would add marginal units of utility which are lower simply because they are superimposed upon existing income. On the other hand, because of diminishing utility, the subtractions from present income would result in greater losses of utility because of the smallness of present income. Only an even distribution of income over time is optimal, if the rate of interest is zero, and if there is no time preference. This means no decumulation of capital, and a similar argument shows that there would be no accumulation.

This is an example of a commonly encountered fallacy. Literary writers often become enmeshed in the notion of the infinitesimal involved in the intensive margin. Because the marginal unit "just pays" with no surplus of satisfaction, it does not follow that it will be a matter of indifference whether the marginal unit is secured or not. On the contrary, any

other action would result in less satisfaction. The fact that the price of wheat in Chicago exceeds its price in Kansas City by the amount of transportation cost does not mean that no wheat will flow between the two cities, nor that an indeterminate amount will flow, but rather that precisely the necessary amount to keep prices in this relation will flow. And so with the problem at hand. A rate of interest other than zero would result in behavior tending to lower it, so that the only equilibrium level would be at a zero rate.

This concludes the refutation of Professor Robbins' belief that there is a contradiction in the existence of a zero rate of interest in the circular flow economy. In justice to him, however, we should admit that the reasoning in the *Theory of Economic Development* is rather obscure at this point. Thus, Professor Schumpeter is correct in his insistence that the abstinence necessary to start a given capitalistic process is different from that necessary to keep it in existence once the process is *synchronized* (p. 39). Still, is it not misleading to say, "Whatever may be the nature of waiting, it is certainly not an element of the economic process which we are here considering, because the circular flow, once established, leaves no gap between outlay or productive effort and the satisfaction of wants. Both are, following Professor Clark's conclusive expression, automatically synchronized. . . . I emphasize once more outlay and return are *automatically* synchronized with one another under the accelerating and retarding influence of profit and loss" (p. 38)?

Here Professor Schumpeter appeals to one of the most doubtful features of the Clarkian system, and if we were to take his statement at face value, he would indeed be open to the objections of Professor Robbins. True, capital is constant in the circular flow, but only because of the inadmissibility of virtual deviations from this condition, and not as a matter of definition. In our own day Professor Knight has fallen into the same error of regarding synchronization as a methodologically necessary axiom, rather than as a deduced condition of equilibrium. I am not an adherent of the neo-Austrian capital theory, but on this point I think that Professor Hayek is definitely in the

²¹ Alternatively, one can consider an economy in which the decisions made by people with respect to present and future consumption are not financial market decisions, but real production ones. Would Robinson Crusoe, if constituted as above, drink up his wine stores without laying down more, simply because at the margin there was no net yield?

right, just as years ago Bohm-Bawerk was correct in his polemic against Clark.²²

That Professor Schumpeter does not rely upon a synchronization which begs the question is clear from a passage in which he deduces the constancy of capital: "In the normal circular flow one has not periodically to withstand a temptation to instantaneous production, because one would *immediately* fare worse by succumbing" (p. 38). What he means to say is that in the circular flow one *does* have periodically to withstand the temptation to eat up one's capital, but that a balancing of advantages will motivate one to resist the temptation every time. Moreover, the word immediately, which Professor Schumpeter italicized, should preferably be omitted, since one is harmed at once in only an indirect sense. In a direct sense, one can go along consuming at a higher clip if one is willing to dip into capital. But, of course, at a later date the reverse will be the case; only the current reckoning of the utility which one will enjoy over all time is immediately reduced in amount.

Again a few pages later (p. 45 *passim*), the fact of motivated constancy and synchronization is so stretched in meaning as to lead to a denial of the existence of a stock of goods in process which is being turned over, and maintained, or which could be consumed. This is no more convincing than the related arguments of Professor Knight: (1) that it is methodologically necessary to regard increments to capital as being set aside in perpetuity; and (2) that furthermore in all modern communities capital is growing and is not invested; and, for any who are still not convinced, (3) that it is impossible to disinvest capital anyway. Argument (1) begs the question at issue; while (2) and (3) are questions of fact, of which the former is irrelevant. In this connection, it is not without interest that Professor Knight's dicta on the impossibility of capital disinvestment were reaching a crescendo in the early thirties, precisely at a time when the number of automobiles and the stock of unused mileage embodied therein were decreasing, when the average age of all machine tools was increasing, when capital facilities of almost all kinds were being used

in excess of replacement; in short, when capital disinvestment was taking place in the United States at a considerable rate.²³ In any case, we should want a general theory of capital which is applicable to non-progressive as well as progressive societies. And in the present writer's opinion, it is a mistake to think that the cessation of growth of capital, or even the introduction of a small amount of decrease, would result in a radically different rate of interest and analysis for explaining it.

We have shown that there is no logical inconsistency in Professor Schumpeter's theory of interest on the side of supply. On the contrary, the strict assumption of absence of time preference implies no decumulation at a zero rate of interest, and no cessation of accumulation at any positive rate of interest.

V

We may now turn to objections from the demand or productivity side. After all, our previous argument has not shown that the rate of interest will be zero, but rather that, if it is not zero, there will be disequilibrium in the sense of a continuation of accumulation. In fact, we may reverse Professor Robbins' previous assertion; *it is the existence of a zero rate of interest which keeps the stationary state stationary*. Perhaps the system will not approach this equilibrium state, i.e., the position of equilibrium may not be a stable one. Of much less importance, but still worthy of notice, is the possibility that the system may

²² See S. Kuznets, *National Income and Capital Formation, 1919-1938* (New York, 1937). Now that we are at war, it will be part of deliberate social policy to divert resources from maintaining civilian capital intact, thereby considerably augmenting our national war potential. Knight regards capital as a familiar bottled drink cooling machine, in which the bottles pass through a tube surrounded by ice. A warm bottle is forced into one end and this moves every bottle in the tube so that a cold one comes out the other end. An initial number of bottles must be invested before a cold bottle is available; after this minimum number of bottles, for each one added, a cold one comes out the other end. The process is synchronized, and one may neglect if one wishes the "bottles in process." If one should now wish to disinvest, disappointment will arise. For as soon as one stops adding bottles at one end, no more come out the other. The bottles in the tube are irretrievably lost—unless of course one can take the contraction apart, or unless one cheats the machine by putting in an empty bottle at one end!

Of course, there are some processes like this, but they are only a few of many special cases. There is no need to erect a theory of capital on this basis, particularly as there is no analytic necessity or convenience for so doing.

²³ What I regard as Professor Knight's definite, and unnecessary, error on this point in no way invalidates his criticisms of the period of production concept.

approach but never *reach* the stable stationary equilibrium state.

I must confess to a lack of knowledge of the relevant German literature, but I should expect Bohm-Bawerk, Professor Schumpeter's teacher, to have been a little uneasy with the Schumpeterian theory. The former might have wondered whether interest could fall to zero while the third of the celebrated grounds for interest still persisted: namely, "the productivity of roundabout means of production," or if one wishes to avoid period of production concepts, simply the existence of a technical net productivity of capital.

If this point were raised, Professor Schumpeter would simply point out that the question was being begged, and counter with a point-blank denial of its validity in the stationary state.²⁴ Or so I should have thought he would. But in one place, in connection with goods which are said to increase "automatically" (herds, seed-corn, etc.), he is willing to entertain the possibility, although correctly stating that much of the gross return should really have expenses deducted from it before the net return is computed. At this point, he definitely, in my opinion, takes the wrong way out. In an analogous, but quite distinct case, of wine which improves with age, he correctly points out that the physical transformation is of no consequence, that there would be imputed back to the grape the full value of the wine, providing that the rate of interest were zero. Even under the latter condition, equilibrium would be possible for reasons similar to those discussed above under the heading of supply.²⁵

But the case where a good undergoes a percentage rate of increase per unit-time in terms of identical units is quite different. Equilibrium at a zero rate of interest and constant prices would be quite impossible. It would pay anyone to borrow, at a zero rate, buy some of this magic substance, hold it while it increases, and sell it at a handsome profit. Hence there could not have been equilibrium in the first place.

Schumpeter's assertion that imputation will take care of even this case and leave nothing to interest is not satisfactory. He says, ". . . for the crop and the herds are certainly dependent

upon seed-corn and breeding cattle, and the latter must therefore be valued according to the values of the former. . . . Their price would be equal to the price of the product imputed to them." Now there is nothing which contradicts the usual notion of equilibrium in the assertion that a gallon of green wine is worth as much as a gallon of mature wine — any more than there would be a contradiction in the statement that wheat sells for as much in Kansas City as it does in Chicago, there being no cost of transportation between them. But it is definitely in contradiction to the usual notion of equilibrium to state that the price of corn is constant over time, and yet one hundred units of corn are today worth as much as one hundred and ten bushels are worth tomorrow. But this is what is implied in the Schumpeterian assertion that there will be reflected in today's corn the full value of tomorrow's output stemming from it.

I may put the matter in another way. By hypothesis, corn has an own-rate of interest different from zero, namely equal to its percentage rate of increase per unit time. The notion of an own rate, or real rate other than a money rate, is associated with the names of Thornton, Marshall, Wicksell, Sraffa, and above all Fisher. Equilibrium coexistent with a zero money rate of interest would be possible only if prices violated the constancy postulated of the stationary state. If capital in general had a continuing, real, net, own productivity, the money rate of interest could be zero only if prices were falling at a percentage rate equal to that of the productivity. It is not without interest in this connection that the value of perpetual *real* income streams would be finite even with a zero rate of interest, the undiscounted sum converging. This shows by the way, if it is not already self-evident, that the third ground of Bohm-Bawerk, even if it existed in real terms, would not necessarily imply a positive rate of interest on money.

From these remarks it must be clear that Professor Schumpeter does not really want to take this way out. (We shall consider what would seem to be the more correct resolution of the difficulty in a moment.) Here and at other places a reader may sense an element of circularity in the argument. Actually if one takes the argument as a whole, such is not the

²⁴ *Theory of Economic Development*, Chapter V.

²⁵ *Ibid.*, pp. 170-71.

case, as we shall see. But at isolated points the charge of circularity can perhaps be sustained. Too much is attributed to a vaguely defined process of "imputation," which is asserted to cause the full value of the product to be decomposable into the original factors, land and labor. I refer the reader to the discussion around page thirty of Professor Schumpeter's volume. Occasionally it is suggested that the maximization of profits will bring about the necessary arbitrage. That this is not directly the case can be seen from the fact, well explained in his Chapter V, that no attempts to maximize profits will wipe out interest if there should really be time preference.²⁶

If Professor Schumpeter had not been under the necessity of keeping the discussion from becoming overlong, he would no doubt have developed at greater length the analysis of the dynamical path by which equilibrium is approached. But, of course, the stationary state was only preparatory to his real purpose, and he could not have known in advance that his theory of interest would prove such a stumbling block to his readers.

Not only would the analysis of the approach to equilibrium be of value for its own sake, but, at the same time, it would have enabled him to banish his concern over the danger of proving the existence of interest after circularly assuming its existence.²⁷ Historically, his concern with the problem of circularity is bound up with his belief that one must first decide whether or not interest will exist, and then what its numerical value will be; that the qualitative problem of the "essence" of interest must precede the quantitative problem.

²⁶ It would have been better if the Walras in Schumpeter had been able to dominate the Bohm-Bawerk in him. He might have avoided then completely what seems to me the utterly false problem of imputation. There simply is no problem of dividing up social product among cooperating factors. Of course, J. B. Clark and the Austrians always wrestled with it, but that tells us nothing about its truth or falsity as a problem. Walras and Pareto quite properly considered it as only a problem of pricing the factors of production as well as finished goods. This involves showing how every firm and family unit will behave when confronted with a given set of prices, and how all together determine the prices with which each as a small individual is faced.

²⁷ Thus on p. 35 of the *Theory of Economic Development* he says: "No possibility of investing savings at interest exists—for if we were to grant this we should assume the element of interest beforehand and come dangerously near to circular reasoning."

These preoccupations seem a little strange in an admirer of the mathematical school of economists, since in mathematical analysis "almost all" circles of reasoning turn out to be "virtuous" rather than "vicious." With reference to the problems at hand, if one believes in the necessity of a zero rate of interest, one gladly invites an opponent to assume that the rate of interest is not zero; i.e., he is given enough rope to hang himself. The assumption is shown to lead to a contradiction, constituting a perfectly valid, albeit indirect, proof of the original proposition. On the other hand, if one were able to produce supply and demand conditions showing that the rate of interest would always stay at ten per cent, it would not be necessary to question further concerning the essence of the phenomena.

The key to these paradoxes lies in the fact that while every great economist changes the thought of his day, he also reacts to it and selects for emphasis the problems acutely discussed by his colleagues. We must not forget the dualism mentioned above that Professor Schumpeter stems from the Austrian School as well as the Mathematical School. That he was in way of being the *enfant terrible* of the Austrians only adds to the plausibility of this interpretation.

Let us then break into an historical process in which technological change has recently taken place. According to anybody's theory, a positive rate of interest may exist. Arbitrage will tend to equalize the net yield on all assets, so that the same rate of interest will be charged upon purely consumption loans, and the same return will be earned even in those lines of endeavor where there has been no technological change. Nobody expects that the current rate will necessarily be maintained in perpetuity. Therefore, in calculating the net return which is to be equalized, the yield is *not* converted into the equivalent perpetual yield, but rather the yield is reckoned only after making algebraic correction for changes in asset values.²⁸ Under the assumption which we now make explicit, that all technological change ceases, we

²⁸ Contrast Professor Knight's statement, "... the correct standard of comparison is that of a perpetual income of a given size with the value of the wealth yielding such an income" (*op. cit.*, p. 258), with my paper, "Some Aspects of the Pure Theory of Capital," *Quarterly Journal of Economics*, 11 (1937), pp. 469-96.

shall see that the interest rate will fall through time. Under conditions of perfect certainty, this would be realized by all interested parties, so that it would be very inconsistent of them to think that a five per cent interest rate between this period and the next would imply a perpetual realization of an equivalent annuity on the same principal.

In the short run, capital will be simply what it is. The limitation in its amount in a certain sense explains its net yield in the short run. But with the utility assumptions of earlier passages, in which utility is not subject to psychological discount for time, at the positive rate of interest positive net savings would take place. There is no paradox in the assertion that in the short run capital is constant, and at the same time is showing an (instantaneous) rate of growth. Over time, the cumulation of this rate of capital formation will result in ever increasing capital, however defined. Even if capital is constructed with the use of the services of both labor and capital goods, if we assume a stationary population and the usual "laws" of proportions and returns, this will necessarily imply a diminution of the net yield of the increased stock of capital goods. And so the rate of interest will fall over time.²⁹

Will the process end at any positive rate of interest? Professor Schumpeter would answer, no; and I believe his reasoning would proceed along the indicated lines. As stated the argument can hardly be controverted. Technological development may produce a positive rate of interest, but after it ceases, only time preference can keep the rate up. Actually, a rigorous mathematical demonstration of the proposition under simplified conditions is provided by Ramsey's brilliant article on Savings.³⁰ This is

²⁹ Numerous models of simplified kinds of capital processes can be brought in to illustrate the process. One may define point input and point output processes for which the crudest Austrian period of production concepts become correct. Or we may neglect all circulating capital in favor of fixed capital goods. Finally, the process may feed back on itself in the sense that so-called produced goods of production may themselves contribute to produce their own kind. Nor need any special concern be shown for the division of productive factors into "original," produced, or permanent categories. All of these considerations acquire importance in terms of realistic appraisals of concrete historical processes such as do not concern us here. However, even from this standpoint it is important to keep factual and logical questions rigidly distinguishable.

³⁰ F. P. Ramsey, "A Mathematical Theory of Saving,"

far from being inconsistent with what I understand to be Professor Knight's theory of interest.

The real difficulty which Professor Knight and other theorists find with the Schumpeterian interest theory lies in the passage to the limit. Does the rate of interest reach zero, or simply approach it? The argument is one about asymptotes and limits. Such processes are notorious breeders of paradoxes, but should yield to careful rigorous mathematical analysis.

The few words that follow attempt to discuss some of the problems involved in the limiting process. Because the problem is not a real one, the discussion must necessarily seem unreal. For Professor Schumpeter does not believe that the rate of zero becomes zero in real life, nor even that there is any secular tendency for interest to fall. We have seen above that Schumpeter's theories of development and of the cycle would be unaltered in essentials if there were a non-zero rate toward which interest were tending. Similarly, these theories would require no modifications if interest simply approached a zero rate without ever reaching this limit.

Professor Knight's objections on the demand side relate to the question of whether or not the limit could *conceptually* be reached, and must be given only the importance which this whole question deserves.³¹ I say *conceptually* because the mathematical treatment of Ramsey deals with a case where the rate of interest *could* become zero; but, in fact, a rational maximizing individual would only find it optimal to increase his capital at a decreasing rate so as to approach a zero rate rather than ever reach it. The Ramsey argument also shows that *without logical inconsistency the rate of interest can become zero without all goods becoming free*, as has been denied by Professor Knight and his disciple Stigler.

Economic Journal, xxxvii (1928), pp. 543-59. Note particularly that a rational maximizer would act so as to reduce interest only gradually over time, rather than precipitously.

³¹ They are summarized in the sentences: ". . . Schumpeter assumes that the rate [of interest] is zero. It is difficult to see the reasons for this assumption: there is no limit to the use of capital even in the absence of new inventions, although the rate of return would of course fall indefinitely low as investment proceeded." (*Ethics of Competition*, p. 257.)

The significant Knightian objection rests upon the purely factual, technical question as to the nature of the "absolute" returns to capital. If we write "Product" as a function of "Labor" and "Capital," does this function attain a maximum for a finite value of C as of a fixed value of L ? The argument is one about the horizon, from its nature incapable of a significant answer. The following remarks must therefore be taken with a grain of salt.

I believe that Bohm-Bawerk was in error in thinking that the methods of technologically advanced economies are more "roundabout" than those of simpler communities. The word in quotations is used too often as a synonym for complexity and efficiency. That a pun is involved is hidden by mistaken allusion to the amount of "roundaboutness" and abstinence which would be involved in an attempt to rebuild our technical structure from scratch. Today, our machines for the purpose of building machines are so efficient that less, rather than more, "waiting" may be tied up in stock of capital goods.

I further believe that society's greatest stock of productive capital inherited from the past is knowledge, not capital goods. If the last century had seen current rates of savings less by *ninety per cent* than actually occurred, but accompanied by a *quadrupling* of the community's resources devoted to scientific and engineering research, we might today have a much more productive industrial structure. The piling up of capital is itself not very productive. With a moratorium on further acquisitions of knowledge, no amount of capital could raise the level of national income to the equivalent of very many times the present American potential. This view would seem to be at variance with that of Professor Knight. For him, diminishing returns set in very slowly. I repeat that these are difficult questions of technology, upon which neither he nor I as economists can lay any special claim to knowledge.

Of course, all resources which have a perpetual net rental yield per unit-time give rise to certain difficulties once the rate of interest becomes literally zero. Their capitalized value increases indefinitely as the rate of interest becomes a smaller positive number, and the expression for capitalized value has a mathematical "pole" for interest equal to zero. But

if people really had no time preference, because they lived forever or otherwise, they clearly would never part with the title to such a perpetual income stream for any finite price, however high. Of course, these perpetual assets might exchange in terms of each other at finite ratios; e.g., two acres of land of a given grade equal to twice the value of one acre. Actually, in feudal societies in which the clan is expected to continue indefinitely, one would as soon sell the children as the family acres. These mores are reflected in the legal form of *entail*. I see no reason why Professor Schumpeter should object to the statement that the value of such assets is infinite; i.e., in strict mathematical terms, higher than any preassigned positive number.³²

I may summarize the above discussion briefly as follows: (1) It is a matter of only esoteric interest whether the rate of interest reaches zero, or approaches it asymptotically. As Ramsey has shown, even if technological conditions make it possible that capital should have a strictly zero net productivity beyond a certain point, utility motives would require that this point only be approached asymptotically. (2) It is an equally esoteric technological question as to whether Knight is right in his belief that a maximum output, even if approached, is never attained for a finite value of capital. For purposes of the business cycle, it is of the greatest importance to know whether or not there is a broad margin of capital opportunities, but this is a factual rather than an *a priori* question. (3) If the infinite value of permanent assets in a zero interest rate economy seems anomalous, the paradox springs from the unreal character of the assumption that men maximize utility in terms of an infinite horizon. It is questionable whether the whole process of saving is illuminated by the attempts to explain it in terms of adjusting consumption streams over time. (4) My own preference is not to reify the limit by asking what really happens at a zero rate of interest, but rather to concentrate upon the dynamic path toward this limiting condition. But much may be said, nevertheless, for the dramatic value of Professor Schumpeter's expository device.

³² If correct, this obviates Professor Schumpeter's necessity of denying that value of assets is reckoned by summation of income streams over time. (*Development*, p. 166.)

“DYNAMICS, STATICS, AND THE STATIONARY STATE”

(From *Foundations of Economic Analysis*)

BY PAUL A. SAMUELSON

An important class of phenomena cannot conveniently be brought under the above four categories. I refer to dynamical stochastic processes such as that realized if a damped pendulum is subjected to “random” shocks. We shall have reason to discuss such processes in connection with dynamical problems arising in the study of the business cycle.

For a given pattern of shocks, as determined by the particular workings of chance in the sequence of time under discussion, we have simply a dynamic historical system of type 4 above. But if we consider the totality of all possible shocks which may be expected to occur if they are regarded as random draws from a fixed universe, it is clear that calendar time is not really involved, but only the time which has elapsed since the beginning of the process. In this sense it is like type 3 rather than type 4, although the word causal may no longer seem appropriate.

It seems best, therefore, to specify two more categories:

5. Stochastic and nonhistorical.
6. Stochastic and historical.

The latter occurs when we have a dynamical system containing stochastic variables, and where either the structure of the system varies in an essential way with time, or where the universes characterizing the random variables change in an essential way with time.

SOME ECONOMIC CONSEQUENCES OF A DECLINING POPULATION*

By J. M. KEYNES

I

THE future never resembles the past—as we well know. But, generally speaking, our imagination and our knowledge are too weak to tell us what particular changes to expect. We do not know what the future holds. Nevertheless, as living and moving beings, we are forced to act. Peace and comfort of mind require that we should hide from ourselves how little we foresee. Yet we must be guided by some hypothesis. We tend, therefore, to substitute for the knowledge which is unattainable certain conventions, the chief of which is to assume, contrary to all likelihood, that the future will resemble the past. This is how we act in practice. Though it was, I think, an ingredient in the complacency of the nineteenth century that, in their philosophical reflections on human behaviour, they accepted an extraordinary contraption of the Benthamite School, by which all possible consequences of alternative courses of action were supposed to have attached to them, first a number expressing their comparative advantage, and secondly another number expressing the probability of their following from the course of action in question; so that multiplying together the numbers attached to all the possible consequences of a given action and adding the results, we could discover what to do. In this way a mythical system of probable knowledge was employed to reduce the future to the same calculable status as the present. No one has ever acted on this theory. But even to-day I believe that our thought is sometimes influenced by some such pseudo-rationalistic notions.

Now I emphasize to-night the importance of this convention by which we assume the

future to be much more like the past than is reasonable—a convention of behaviour which none of us could possibly do without—because, as I think, it continues to influence our minds even in those cases where we do have good reason to expect a definite change. And, perhaps, the most outstanding example of a case where we in fact have a considerable power of seeing into the future is the prospective trend of population. We know much more securely than we know almost any other social or economic factor relating to the future that, in the place of the steady and indeed steeply rising level of population which we have experienced for a great number of decades, we shall be faced in a very short time with a stationary or a declining level. The rate of decline is doubtful, but it is virtually certain that the change-over, compared with what we have been used to, will be substantial. We have this unusual degree of knowledge concerning the future because of the long but definite time-lag in the effects of vital statistics. Nevertheless the idea of the future being different from the present is so repugnant to our conventional modes of thought and behaviour that we, most of us, offer a great resistance to acting on it in practice. There are, indeed, several important social consequences already predictable as a result of a rise in population being changed into a decline. But my object this evening is to deal, in particular, with one outstanding economic consequence of this impending change; if, that is to say, I can, for a moment, persuade you sufficiently to depart from the established conventions of your mind as to accept the idea that the future will differ from the past.

II

An increasing population has a very important influence on the demand for capital. Not only does the demand for capital—

* The Galton Lecture, delivered before the *Eugenics Society* on February 16th, 1937.

apart from technical changes and an improved standard of life—increase more or less in proportion to population. But, business expectations being based much more on present than on prospective demand, an era of increasing population tends to promote optimism, since demand will in general tend to exceed, rather than fall short of, what was hoped for. Moreover a mistake, resulting in a particular type of capital being in temporary over-supply, is in such conditions rapidly corrected. But in an era of declining population the opposite is true. Demand tends to be below what was expected, and a state of over-supply is less easily corrected. Thus a pessimistic atmosphere may ensue; and, although at long last pessimism may tend to correct itself through its effect on supply, the first result to prosperity of a change-over from an increasing to a declining population may be very disastrous.

In assessing the causes of the enormous increase in capital during the nineteenth century and since, too little importance, I think, has been given to the influence of an increasing population as distinct from other influences. The demand for capital depends, of course, on three factors: on population, on the standard of life, and on capital technique. By capital technique I mean the relative importance of long processes as an efficient method of procuring what is currently consumed, the factor I have in mind being conveniently described as the period of production, which is, roughly speaking, a weighted average of the interval which elapses between the work done and the consumption of the product. In other words the demand for capital depends on the number of consumers, the average level of consumption, and the average period of production.

Now it is necessarily the case that an increase in population increases proportionately the demand for capital; and the progress of invention may be relied on to raise the standard of life. But the effect of invention on the period of production depends on the type of invention which is characteristic of the age. It may have been true of the nineteenth century that improvements in

transport, standards of housing and public services were of such a character that they did tend somewhat to increase the period of consumption. It is well known that highly durable objects were characteristic of the Victorian civilization. But it is not equally clear that the same thing is true to-day. Many modern inventions are directed towards finding ways of reducing the amount of capital investment necessary to produce a given result; and partly as the result of our experience as to the rapidity of change in tastes and technique, our preference is decidedly directed towards those types of capital goods which are not too durable. I do not believe, therefore, that we can rely on current changes of technique being of the kind which tend of themselves to increase materially the average period of production. It may even be the case that, apart from the effect of possible changes in the rate of interest, the average period may be tending to diminish. Moreover an improving average level of consumption may conceivably have, in itself, the effect of diminishing the average period of production. For as we get richer, our consumption tends to be directed towards those articles of consumption, particularly the services of other people, which have a relatively short average period of production.

Now, if the number of consumers is falling off and we cannot rely on any significant technical lengthening of the period of production, the demand for a net increase of capital goods is thrown back into being wholly dependent on an improvement in the average level of consumption or on a fall in the rate of interest. I will attempt to give a few very rough figures to illustrate the order of magnitude of the different factors involved.

Let us consider the period of just over fifty years from 1860 to 1913. I find no evidence of any important change in the length of the technical period of production. Statistics of quantity of real capital present special difficulties. But those which we have do not suggest that there have been large changes in the amount of capital employed to produce a unit of output. Two of the most

highly capitalized services, those of housing and of agriculture, are old-established. Agriculture has diminished in relative importance. Only if people were to spend a decidedly increased proportion of their incomes on housing, as to which there is indeed a certain amount of evidence for the post-war period, should I expect a significant lengthening of the technical period of production. For the fifty years before the war, during which the long-period average of the rate of interest was fairly constant, I feel some confidence that the period was not lengthened by much more than 10 per cent., if as much.

Now during the same period the British population increased by about 50 per cent., and the population which British industry and investment was serving by a much higher figure. And I suppose that the standard of life must have risen by somewhere about 60 per cent. Thus the increased demand for capital was primarily attributable to the increasing population and to the rising standard of life, and only in a minor degree to technical changes of a kind which called for an increasing capitalization per unit of consumption. To sum up, the population figures, which are reliable, indicate that about half the increase in capital was required to serve the increasing population. Perhaps the figures were about as follows, though I would emphasize that these conclusions are very rough and to be regarded only as broad pointers to what was going on :

	1860	1913
Real capital ...	100	270
Population ...	100	150
Standard of life ...	100	160
Period of Production	100	110

It follows that a stationary population with the same improvement in the standard of life and the same lengthening of the period of production would have required an increase in the stock of capital of only a little more than half of the increase which actually occurred. Moreover, whilst nearly half of the home investment was required by the increase in population, probably a substantially higher proportion of the foreign

investment of that period was attributable to this cause.

On the other hand it is possible that the increase in average incomes, the decline in the size of families, and a number of other institutional and social influences may have raised the proportion of the national income which tends to be saved in conditions of full employment. I do not feel confident about this, since there are other factors, notably the taxation of the very rich, which tend in the opposite direction. But I think we can safely say—and this is sufficient for my argument—that the proportion of the national income which would be saved to-day in conditions of full employment lies somewhere between 8 per cent. and 15 per cent. of the income of each year. What annual percentage increase in the stock of capital would this rate of saving involve? To answer this we have to estimate how many years of our national income the existing stock of capital represents. This is not a figure which we know accurately, but it is possible to indicate an order of magnitude. You will probably find when I tell you the answer that it differs a good deal from what you expect. The existing national stock of capital is equal to about four times a year's national income. That is to say, if our annual income is in the neighbourhood of £4,000 millions, our stock of capital is perhaps £15,000 millions. (I am not here including foreign investment, which would raise the figure to, say, four and a half times.) It follows that new investment at a rate of somewhere between 8 per cent and 15 per cent. of a year's income means a cumulative increment in the stock of capital of somewhere between 2 per cent. and 4 per cent. per annum.

Let me recapitulate the argument. Please take note that I have been making so far two tacit assumptions—namely that there is no drastic change in the distribution of wealth or in any other factor affecting the proportion of income that is saved; and further, that there is no large change in the rate of interest sufficient to modify substantially the length of the average period of production. To the removal of these two assumptions

we shall return later. On these assumptions, however, with our existing organization, and in conditions of prosperity and full employment, we shall have to discover a demand for net additions to our stock of capital amounting to somewhere between 2 per cent. and 4 per cent. annually. And this will have to continue year after year indefinitely. Let us in what follows take the lower estimate—namely 2 per cent.—since if this is too low the argument will be *a fortiori*.

Hitherto the demand for new capital has come from two sources, each of about equal strength: a little less than half of it to meet the demands of a growing population; a little more than half of it to meet the demands of inventions and improvements which increase output per head and permit a higher standard of life.

Now past experience shows that a greater cumulative increment than 1 per cent. per annum in the standard of life has seldom proved practicable. Even if the fertility of invention would permit more, we cannot easily adjust ourselves to a greater rate of change than this involves. There may have been one or two decades in this country during the past hundred years when improvement has proceeded at the rate of 1 per cent. per annum. But generally speaking the rate of improvement seems to have been somewhat less than 1 per cent. per annum cumulative.

I am here distinguishing, you will see, between those inventions which enable a unit of capital to yield a unit of product with the aid of less labour than before, and those which lead to a change in the amount of capital employed *more* than in proportion to the resulting output. I am assuming that the former class of improvements will proceed in the future as in the recent past, and am ready to take as my assumption that they will proceed in the near future up to the best standard we have ever experienced in any previous decade; and I calculate that inventions falling under this head are not likely to absorb much more than half of our savings, assuming conditions of full employment and a stationary population. But in the second category some inventions cut

some way and some the other, and it is not clear—assuming a constant rate of interest—that the net result of invention changes demand for capital per unit of output one way or the other.

It follows, therefore, that to ensure equilibrium conditions of prosperity over a period of years it will be essential, *either* that we alter our institutions and the distribution of wealth in a way which causes a smaller proportion of income to be saved, *or* that we reduce the rate of interest sufficiently to make profitable very large changes in technique or in the direction of consumption which involve a much larger use of capital in proportion to output. Or, of course, as would be wisest, we could pursue both policies to a certain extent.

III

What relation do these views bear to the older Malthusian theory that more capital resources per head (chiefly envisaged by the older writers in the shape of Land) must be of immense benefit to the standard of life, and that the growth of population was disastrous to human standards by retarding this increase? It may seem at first sight that I am contesting this old theory and am arguing, on the contrary, that a phase of declining population will make it immensely more difficult than before to maintain prosperity.

In a sense this is a true interpretation of what I am saying. But if there are any old Malthusians here present let them not suppose that I am rejecting their essential argument. Unquestionably a stationary population does facilitate a rising standard of life; but on one condition only—namely that the increase in resources or in consumption, as the case may be, which the stationariness of population *makes possible*, does actually take place. For we have now learned that we have another devil at our elbow at least as fierce as the Malthusian—namely the devil of unemployment escaping through the breakdown of effective demand. Perhaps we could call this devil too a Malthusian devil, since it was Malthus

himself who first told us about him. For just as the young Malthus was disturbed by the facts of population as he saw them round him and sought to rationalize that problem, so the older Malthus was no less disturbed by the facts of unemployment as he saw them round him and sought—far less successfully so far as his influence on the rest of the world was concerned—to rationalize that problem too. Now when Malthusian devil P. is chained up, Malthusian devil U. is liable to break loose. When devil P. of Population is chained up, we are free of one menace; but we are more exposed to the other devil U. of Unemployed Resources than we were before.

With a stationary population we shall, I argue, be absolutely dependent for the maintenance of prosperity and civil peace on policies of increasing consumption by a more equal distribution of incomes and of forcing down the rate of interest so as to make profitable a substantial change in the length of the period of production. If we do not, of set and determined purpose, pursue these policies, then without question we shall be cheated of the benefits which we stand to gain by the chaining up of one devil, and shall suffer from the perhaps more intolerable deprivations of the other.

Yet there will be many social and political forces to oppose the necessary change. It is probable that we cannot make the changes wisely unless we make them gradually. We must foresee what is before us and move to meet it half-way. If capitalist society rejects a more equal distribution of incomes and the forces of banking and finance succeed in maintaining the rate of interest somewhere near the figure which ruled on the average

during the nineteenth century (which was, by the way, a little *lower* than the rate of interest which rules to-day), then a chronic tendency towards the under-employment of resources must in the end sap and destroy that form of society. But if, on the other hand, persuaded and guided by the spirit of the age and such enlightenment as there is, it permits—as I believe it may—a gradual evolution in our attitude towards accumulation, so that it shall be appropriate to the circumstances of a stationary or declining population, we shall be able, perhaps, to get the best of both worlds—to maintain the liberties and independence of our present system, whilst its more signal faults gradually suffer euthanasia as the diminishing importance of capital accumulation and the rewards attaching to it fall into their proper position in the social scheme.

A too rapidly declining population would obviously involve many severe problems, and there are strong reasons lying outside the scope of this evening's discussion why in that event, or in the threat of that event, measures ought to be taken to prevent it. But a stationary or slowly declining population may, if we exercise the necessary strength and wisdom, enable us to raise the standard of life to what it should be, whilst retaining those parts of our traditional scheme of life which we value the more now that we see what happens to those who lose them.

In the final summing up, therefore, I do not depart from the old Malthusian conclusion. I only wish to warn you that the chaining up of the one devil may, if we are careless, only serve to loose another still fiercer and more intractable.

A CONSIDERATION OF THE ECONOMIC AND
MONETARY THEORIES OF J. M. KEYNES
AN EXPOSITION OF KEYNESIAN ECONOMICS

By LORIE TARSHIS
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In this paper I shall attempt to give a simple and acceptable account of Keynesian economics. I have no particular qualifications for this task; there are many others who know more than I about *The General Theory* and the writings that derive from it. But since my responsibility is, as I understand it, to discuss not the fine points but only the broad outline, this lack of special knowledge may not be a serious handicap. My hope is to present a picture of Keynesian economics that will be found generally accurate by most of you, and helpful, as a review or refresher, by a few.

I cannot expect this account to appeal to economists as accurate in its details. The very development of Keynesian doctrine would make any such expectation unreasonable. The doctrine had, for most of us, its beginnings in *The General Theory*; it was molded and qualified in the writings of Robertson, Hicks, Lange, Samuelson, Shaw, and many others; and it has been modified still further in our minds by the events of the last decade. It would be strange, consequently, and indeed disappointing, if *The General Theory* said the same things to us today that it did on its publication. And it would be strange too if this account of contemporary Keynesian doctrine conformed in all details to yours. But perhaps the essential points of the outline will not be challenged.

I. *Dependence on Neoclassical Doctrine*

Before introducing the novel aspects of Keynesian economics, I should like to emphasize its considerable dependence upon classical and neoclassical tradition. That this dependence should exist will not surprise those who know of Keynes's background. He was surrounded by and he lectured on the Cambridge version of neoclassical economics for many years. A strong reliance on these doctrines is consequently to be expected. That he himself did not in *The General Theory* point out this dependence—but, instead, sought to emphasize his break from the earlier doctrines—must be regarded as a tactic of persuasion rather than as an objective statement of the relation between his own work and conventional doctrine. His *Treatise on Money* marks his greatest departure from neoclassical economics and by the time he wrote *The General Theory* he had found a way to reconcile his mone-

tary theory and the neoclassical tradition. Indeed, a good case can be made for the assertion that the doctrine of *The General Theory* was mothered by the *Treatise* and fathered by neoclassical economics. If so, we should have to admit that the child suffered from an extreme Oedipus complex.

The Aggregate Supply Function and Classical Economics. There are two points in particular at which classical doctrine enters. Let us consider them briefly. Keynes introduces the first of them under the title "the aggregate supply function." His concept is obviously closely related to the familiar supply function of neoclassical economics. This function expresses the relation between the output of a firm or industry and the price offered; normally, the higher the price, the greater is the output. His concept though it differs in minor ways is basically similar. In place of the output of a single commodity, he deals with the total production of the economy; instead of measuring total production in physical units, he measures it in terms of total employment; and finally, rather than expressing the independent variable as an offer price, he uses for his variable, businessmen's expectations of sales receipts. His aggregate supply function is, then, a generalization of the substance of the classical function. While the latter gives a determinate answer to the question as to how much a firm with given costs will produce when the demand for its product is known, the aggregate supply function implies that there is a determinate answer to the broader question of how much will all firms produce, or how many men will they hire, given their costs, when the size and composition of the aggregate demand are known?

The General Theory, I believe, marked a great advance over the *Treatise*; and this advance depended to an important degree upon a shift in emphasis. In the *Treatise*, Keynes was interested directly in the general price level; and in *The General Theory*, in the national income. The shift in his emphasis occurred, according to my lecture notes, in 1932, or possibly earlier, and it was at this time that he introduced the aggregate supply concept. The significance of this common date should be noted.

Income, the Value of Output, and Classical Economics. The second aspect of classical doctrine that Keynes adopted consists in his use of a part, though certainly not the whole, of the classical law of markets. He does not, of course, go as far as Say; he rejects Say's law that supply creates its own demand. But he goes part of the way; if supply does not create its own demand, at least it creates the income from which a part of the demand stems. This statement, that production is the source of all income, or more precisely, that the national income or social dividend equals the value of current output, has been accepted generally by

classical economists. Moreover, the equality of the value of output and the national income has long been recognized by national income statisticians. For instance, in its first publication, *Income in the United States—1909-1919*, the National Bureau of Economic Research stated: "The fundamental concept of the National Income which underlies the Estimate by Sources of Production is the same as that underlying the Estimate by Incomes Received. In both estimates the National Income is taken to consist of the commodities and services produced by the people of the country or obtained from abroad for their use."¹ Or again: "Hence it seems that an estimate of the incomes received by all individuals, plus the undistributed incomes of business enterprises, should produce the same figures as the Estimate by Sources of Production, were the data complete and correct on all heads."²

At this point some must be asking which of the many available income concepts is concerned in this equality. The answer that I prefer (the gross national product) cannot, I regret, be developed in the time available. Keynes did not, of course, when he was writing *The General Theory* have the valuable July supplement to the *Survey of Current Business* and it is not easy to match his income concepts to the newer ones. But in any case a clear understanding of Keynesian doctrine can be got without examining these technicalities.

The main points of Keynesian doctrine are developed upon these classical foundations—the aggregate supply function and the equality between national income and the value of output. Indeed, starting from the neoclassical position, we can go much of the way towards a formulation of income theory in Keynesian terms before we have to introduce anything that explicitly contradicts other parts of the classical doctrine. Let us see what kind of structure we can build upon these foundations.

II. *Income and Aggregate Spending*

In accordance with what Keynes accepted of the law of markets, we conclude that when the value of the economy's output in a year is, say, 225 billion dollars, the national income is 225 billions. Our first step, then, is that the national income varies directly with, and indeed equals, the value of output.

What then determines the value of output? Output is valued in the only possible way—by what buyers pay for it. If the value of current output is 225 billion dollars, it indicates that buyers paid 225 billions for it. The national income for a year equals the annual spending of all buyers on current output.

In accordance with the aggregate supply function, output and em-

¹ *Loc. cit.*, p. 42.

² *Ibid.*, p. 43.

ployment are seen to vary directly with total purchasing, or, looking at it from the buyers' viewpoint, with total spending. Therefore we may conclude that output and employment vary directly with the national income and this, as we have already seen, equals aggregate spending on current output.

Our problem is to discover the factors that determine the size of the national income and the level of employment. The first stage on the way to a helpful answer is reached when we see that the sought-for determinant is aggregate spending. But this does not go far enough, and further analysis is needed. Before taking this next step, however, it is desirable once again to emphasize that there is nothing especially Keynesian about our answer—certainly not in this formulation. While a classical economist would perhaps not find it helpful, he would, I believe, be forced to recognize that it was consistent with at least a part of classical theory.

III. *Further Analysis of Aggregate Spending—Consumption and Investment*

The objection to ending the analysis here is this: aggregate spending covers such a multitude of activities and it is guided by such a variety of motives that it seems impossible to say anything meaningful about the factors that cause it to vary. After all, a business decision to order a new rolling mill rests upon considerations as different as possible from a government decision to hire school teachers or a consumer decision to buy more clothing. Since the spending stream is made up of such diverse elements, we can expect to find an explanation for changes in its volume only by looking into it more carefully.

The obvious next step is to divide the spending stream into its relatively homogeneous components, which means to classify spending by type of buyer. While buyers could be divided in various ways—for example, as they purchase durable or nondurable goods—Keynes finds it useful to separate them into four groups: consumers, business concerns, government bodies, and foreigners. The sums spent by consumers, he calls consumption; the amounts spent by the other types of buyers are, in order, private investment (normally this category includes housing), public investment, and foreign investment. Consequently, total spending on current output equals consumption plus investment, private, public, and foreign. And total spending on current output also equals the national income. Hence, the national income is equal to consumption plus investment.

It is doubtful whether anything novel, or at any rate at variance with the classical tradition, has been introduced up to this point. If a classical economist can accept the equality between the national income and

the value of output, he should have no difficulty, once that output has been classified into goods for consumers, business, government, and foreigners, in accepting the above formulation. Though the formulation at which we have arrived sounds somewhat Keynesian, the sense can still be derived directly from classical teachings.

IV. *Reformulation: Income, Investment, and the Propensity to Consume*

But this formulation is also open, as we shall see, to a serious objection, and a different way of putting it is called for. We substitute for the statement, income equals consumption plus investment, the statement, income depends upon the propensity to consume and investment. In this form we say no more than before, and the transformation is straightforward once we have introduced the propensity to consume. Why then, we may ask, should we bother to introduce this confusing concept, and simply restate a truism?

The answer, I believe, would run as follows: because in the revised form it discloses factors that are operationally significant; secondly, because it gains in simplicity and elegance.

The objection to the formulation "income equals consumption plus investment" is easily seen. Income depends partly upon consumption, but obviously consumption depends amongst other things upon income. Hence income depends in part upon income. Evidently one avenue in the maze through which we are trying to thread our way leads promptly back to the beginning, which is awkward. Moreover, this false trail diverts attention from other paths that do lead to a solution. What we have, to use an analogy with elementary algebra, is an equation with an unknown that is represented at least by implication, on both sides. It is not a fatal difficulty—the circularity is benign rather than malignant—but it is inconvenient. By re-expressing it, we can avoid this inconvenience, and at the same time provide helpful guidance to the investigator—guidance that will keep him from wandering up blind alleys and along roads that lead only to the starting point.

The Propensity to Consume. It was noted above that consumption depends, amongst other things, upon the national income; these other things, or rather their joint influence, Keynes brings together under the title "The Propensity to Consume." This is the name, then, for the function that relates consumption and income. It is not of course the *ratio* of the two, unless the function can be expressed in this simple form—and there is no statistical evidence that it can. It is simply, to repeat, the relation between consumption and income—the income-consumption function or the income demand for consumers goods. It follows, of course, from the definition of the propensity to consume that

consumption is determined by it and the national income, and by these two factors alone.

Now let us return to the formulation to which we objected earlier: income equals investment plus consumption. We can bring out into the open and thus rid our formulation of the element of circularity present in it, if we substitute for consumption its two determinants. Doing so we have: income depends upon investment, the propensity to consume, and income. Now that we have isolated the unknown on both sides of the expression, we can simplify to: the national income depends upon the propensity to consume and investment.

This is the formulation at which Keynes arrives in *The General Theory*. I think it is worth emphasizing that while a classical economist may not find it useful, he would agree that it is based upon an equality accepted in conventional, pre-Keynesian theory. The dependence of the national income upon investment and the propensity to consume is derived directly from the fact that the value of current output equals the income earned in producing it. Once that fact is accepted, the final statement follows.

But to say that this statement about the determinants of the national income can be derived from pre-Keynesian doctrine is not to imply that Keynes said nothing new, or that the classical economist must accept the Keynesian position. What it does suggest is that the points of substantial difference are to be found elsewhere. They will be found, I shall try to show, in the analysis of investment, or rather in the relation between consumption and investment. It is here that the Keynesians and those who do not accept his analysis are farthest apart.

V. *Analysis of the Propensity to Consume*

Before explaining these differences it is desirable to examine the Keynesian doctrine more carefully. Since income is seen to depend upon the propensity to consume and investment, we shall have to investigate each of these determinants in turn.

What Keynes has to say about the propensity to consume is straightforward. This function, since it covers the effects of all the factors that influence consumption except for income, will be affected in many ways. To indicate some of them, it will be affected by a change in the price level, in the distribution of income, in attitudes towards thrift, in holdings of liquid assets, in the state of the stock market, in the tax structure, in the interest rate, in the dividend policy of corporations, and by changes in many other variables. In fact the list is as long as the list of forces that determine consumers' spending. To say that the national income depends, in part, upon the propensity to consume does

not provide a complete answer to our question, but it does suggest directions for further analysis.

VI. *Analysis of Investment*

The analysis of investment is rather more complicated. Investment can be broken down, as we have seen, into three components: private, public, and foreign. The economist in his professional capacity can say very little about the forces that determine the second of these—public investment—except possibly to point out that his voice is normally not heeded when such investment decisions are made. Keynes had very little to contribute to conventional doctrine on the determinants of foreign investment. Exchange rates, comparative prices, costs of transport, and so on, are the critical factors. His major contribution—and most important break with earlier doctrine—is in his analysis of the determination of private investment.

Private investment consists of the spending of business concerns, except for expenditures that just maintain working capital inventories, and in addition of the spending on private housing. Private business investment—the spending of business firms—is directed towards the acquisition of plant and equipment (including repairs) and to the building up of inventories of raw materials, goods in process, and finished goods. Of course, when inventories are allowed to run down, this part of private investment is negative.

Since private business investment is undertaken by firms generally seeking to maximize their profits, it follows that the amount of their expenditures will depend in some way upon profit considerations. A firm will embark upon an investment project when it expects that course of action to be profitable; otherwise, it will not undertake the project. It will, to put this concretely, order an extension to its plant, or arrange to have its equipment repaired, order new equipment, or build up its stocks of raw materials when it expects to earn more by doing any of these things than it would earn by doing nothing.

Hence the test an investment project must pass if it is to be carried out is this: the money invested in the project must be expected to yield a rate of profit before paying interest that exceeds the interest rate applicable to the firm. If money can be borrowed by a certain firm at 3 per cent, and if that firm believes it can acquire investment goods that will, over their life, return 4 per cent annually on their original cost, after allowing for depreciation but before subtracting the interest charge, it is worth while for the firm to make the investment expenditure. It will, by doing so, add to its annual profits during the life of the asset a sum equal to 1 per cent of the original outlay. Thus, at

any date it is worth while for firms to initiate, carry on, or complete every investment project whose anticipated yield measured against the cost of acquiring the good exceeds the particular interest rate facing them.

The Marginal Efficiency of Capital. Keynes names the anticipated yield over the cost of any particular project "the marginal efficiency of capital of that type"; while the schedule of yields anticipated on all possible projects he calls "the marginal efficiency of capital." We have already seen that a project will be undertaken if its marginal efficiency exceeds the rate of interest; hence the dollar value of projects to be carried on depends upon the marginal efficiency of capital, in the schedule sense, and the interest rate.

As Keynes has pointed out, his marginal efficiency of capital concept is identical to Irving Fisher's "rate of return over cost," and it is similar to Marshall's "marginal utility of capital." Because of its familiarity, there should be no serious difficulty in grasping it.

The factors that determine the marginal efficiency of capital are as numerous as those that determine the propensity to consume, though they are, perhaps, more uncertain in their operation and more sensitive to sharp shifts of judgment. For instance, if it is proposed to put up a new plant, it is necessary in estimating its lifetime per cent return over cost to guess about the market for its product for perhaps forty years into the future, and to do the same for the cost of operating the plant. It is obvious that any long-term market or cost forecast of this kind will be uncertain and subject to drastic revision. Some of the factors that could be expected to condition these forecasts are: the existing market for the product, the likelihood that new competitive products will be developed, the productive capacity of the industry, the cost of the capital goods, the general state of business confidence, and so on. In brief, we should want to include in the list all but one of the factors that determine how much expansion it is profitable to undertake, that one being the interest rate, which is considered separately.

VII. *Analysis of the Interest Rate*

The marginal efficiency of capital is but one of the determinants of private investment; the interest rate is also important. Hence, if we are to round out this analysis we must analyze the forces that set the interest rate. In Keynes's account, its determination rests upon monetary factors—as it should, so he thinks, since it is the price paid for holding wealth in the form of money or, in other words, for borrowing.

When the interest rate is set at any level—that is, when the prices of bonds and other debt instruments are established—it shows that the market does not wish on balance to alter the form in which it is hold-

ing its wealth. Those with wealth are content to maintain the existing distribution of their wealth as between money, bonds, and other assets. This must be so for, if they were disposed to change that distribution, they would, in attempting to do so, bid up or reduce the price of bonds. Hence, when the interest rate is set, it means that the economy does not wish to hold either more or less of its wealth in the form of money. It is satisfied, in the circumstances, to hold the amount of money it has. And of course the amount of money it has is precisely the amount that has been created by the monetary system, since after all every bit of money in existence must have a resting place somewhere. Thus we may conclude that the interest rate is determined by two data: the strength of the economy's desire to hold its wealth in money form and the amount of money in existence.

A more detailed examination of the considerations that determine how much of their wealth the members of the economy wish to hold in the form of money will show more clearly how the interest rate enters into the picture.

The Liquidity-Preference Function. The motives for holding money are threefold: first, to provide convenience in transactions; second, to provide protection and the means to exploit opportunity in an uncertain world; and, third, to avoid a capital loss feared because of an expected decline in securities' prices. In other words, money is a desirable form in which to hold some wealth, because bonds cannot be spent and they may sometimes be expected to fall in price. Bonds are neither a medium of exchange nor a satisfactory store of value, and money is both. But money, unlike bonds, does not yield anything except the convenience and speculative utilities already noted. Hence against these advantages of liquidity, the holder of money must set the disadvantage that it does not multiply, that his wealth held in that form does not grow. Consequently, we should expect the economy to choose to hold less of its wealth in liquid form when interest rates are raised, and vice versa. This conclusion is, I believe, obvious insofar as it concerns the convenience—and precautionary—motives for liquidity. In considering the amount of money held for speculative considerations, we must remember that that depends upon expectations as to the future course of bond prices. When the views of a part of the market shift to the bearish side, and bond prices are expected by that part to fall, there will be a tendency to move out of bonds and into money, and a shift in the opposite direction will occur when a part of the market comes to expect bond prices to rise. We can suppose that market opinion will become more bearish when interest rates fall to an abnormally low level; that is to say, when bond prices rise to a figure that seems abnormally high; while we may expect the market to become increasingly bullish as

interest rates rise towards a level that appears abnormally high. This means that at very low interest rates the speculative motive for holding money will strongly reinforce the convenience motive and the economy will accordingly want to hold a great deal of its wealth in liquid form; at very high rates, the amount of liquidity desired on this account will be much lower.

The relation between the amount of liquidity desired and the rate of interest, Keynes calls "liquidity preference" or the "liquidity function." And since, as we have already seen, the interest rate is determined at the point that equates the amount of money people wish to hold with the amount in existence, it follows that the interest rate depends upon the liquidity function and the amount of money.

VIII. *Relation Between Consumption and Investment—Contrast with Classical Doctrine*

This analysis of the determinants of the interest rate, perhaps re-expressed in terms of loanable funds, could be expected to appeal to specialists in money and banking. But economists who prefer to explain the interest rate in *real* terms would obviously be less happy with it. There are, of course, many reasons for this, but one of them is particularly worth noting. In the Keynesian account, an increased *desire to save* which, of course, is not at all the same thing as a reduced *desire for liquidity*, would not be expected to lead to a lower rate of interest, or if it does so, only by bringing about a fall in business activity and the national income. If the interest rate does not fall or falls only because of a decline in the national income, investment will not increase by enough to offset the decline in consumption. Indeed we may go further. If consumption expenditures should decline, businessmen would normally consider the inducements to purchase investment goods weaker. Hence, there is no reason to expect the interest rate to act as an equilibrating force that serves to maintain a full prosperity national income, when for instance thriftiness increases.

When the demand for consumers goods falls, we may then expect a reduced demand for investment goods; and as the multiplier process suggests, when the initiating force is a decline in investment, we can expect a decline in the demand for consumers goods. Instead, then, of a model in which a change in the demand for, say, consumers goods is likely to be offset by a change in the opposite direction in the demand for investment goods, Keynes proposes a more realistic model, one in which a change in the demand for the goods of one type is likely, except when we start with full employment, to cause a change in the same direction in the demand for the other. Consumption and investment, in the normal case, move together. The economy does not normally, if it

cuts down the output of one, find a motive for increasing the output of the other.

IX. *Summary*

Before considering some of the implications of this analysis let me briefly summarize.

The skeleton of the theory is simple: The national income depends upon investment and the income-consumption function. Investment, or more accurately private business investment, depends upon the marginal efficiency of capital and the rate of interest; the rate of interest depends upon the liquidity function and the amount of money. The determinants of the marginal efficiency of capital and the propensity to consume are very numerous; some of them were listed earlier.

X. *Some Implications*

The skeleton alone gives us some suggestions for policy; for instance, that when there is unemployment, efforts should be made to increase investment (private, public, and foreign) and the propensity to consume. It also implies that an economy can be in equilibrium at less than full employment, that circumstances can rule in which there is no natural tendency towards peak prosperity. I am sorry that I have no time to consider the other alleged equilibrating factor—changes in wage rates. Keynes's conclusions on this are, I suppose, well known.

The application of this analysis to an actual situation requires judgments as to the quantitative response of the determinants to various changes. How greatly, for instance, can we expect investment to be affected when the interest rate is lowered? How would a change in the wage rate affect investment and the propensity to consume? Is investment greatly influenced by the rate of growth of population? Would a 50 per cent increase in the stock of capital goods bring about a large reduction in the marginal efficiency of capital or a small one? These are important questions and the answers put flesh on the skeleton of Keynesian economics. But Keynesian economics does not consist in the answers to these questions. An economist who accepted the Keynesian outline could claim, though most Keynesians would disagree, that private investment could be greatly increased by a minor reduction in the interest rate, and he would then urge a mild expansionist banking policy during depression instead of a policy of, say, public works. Or one might support a wage cut if he believed that it would favor investment and the propensity to consume. Keynesians do not all have to prescribe the same medicine.

It is commonly believed that Keynesian economics should be identified with the "mature economy thesis," or with a predilection in favor

of government controls. This is nonsense. Not all who accept these insidious, as they are now regarded, views are Keynesians. And likewise it is not necessary for all those who are optimistic about our long-term prospects, who wish to encourage private investment, and who abhor government intervention, to oppose the central themes of Keynes's doctrine, though obviously many of them will do so.

Keynes's account of the determinants of the national income in terms of investment and the propensity to consume seems to me to represent his important contribution. His views about the quantitative aspects of the implied relations should be judged separately. Perhaps they were right for England in 1936; perhaps they were applicable to this country in the thirties. Perhaps they are applicable today and will continue to be. But in any case these matters should be kept quite separate from his account of the determination of the national income in a capitalist economy. Whatever our views on, say, the interest elasticity of the demand for investment goods, on the significance of business confidence in determining the marginal efficiency of capital, or on the prospects for important technological improvements in the next decade or two, I think we can regard the Keynesian statement that the national income depends upon the propensity to consume, the marginal efficiency of capital, the liquidity function, and the amount of money as true and useful. That is the final test.

AN APPRAISAL OF KEYNESIAN ECONOMICS

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I

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

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

As the reference to Bagehot indicates, Keynes was not the first great English critic of classical economics. As a graduate student, nothing interested me more than the writings of the heretics. I found no more penetrating discussion of the relativity of economic concepts than Bagehot's *The Postulates of English Political Economy*; and I returned repeatedly to ponder over Cliffe Leslie's savage outcry against "generalizations . . . which have passed with a certain school of English economists for economic laws . . . generalizations which were once useful and meritorious as first attempts to discover causes and sequence among economic phenomena, but which have long since ceased to afford either light or fruit, and become part of the solemn humbug of 'economic orthodoxy.'"² The weakness of such men, from the stand-

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

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

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

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

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

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

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

II

Keynes leaves no room for doubt that, in his view, his principle of effective demand revolutionized traditional economic theory. In the preface to *The General Theory* he speaks of "treading along unfamiliar paths," and of his long "struggle of escape." It is clear, too, that he regarded his contribution as monetary. The evolution of his thinking covered the greater part of the interwar period, and the stages in it were marked by the *Tract on Monetary Reform* (1923), the *Treatise on Money* (1930), and *The General Theory* (1936). It is clear all the way through that he was intensely concerned with the problems of his day, and particularly with those of England. In this sense all his books are dated. The first deals with the monetary disturbances of the early twenties, with a large emphasis on international monetary policy; it is dedicated to the "Governors and Court of the Bank of England, who now and for the future have a much more difficult and anxious task than in former days."⁶ The second is a monumental work—analytical, statistical, historical—whose central theme is a monetary theory of the business cycle (mainly on closed economy lines) and a policy of control of the cycle by the central bank. There is no evidence as yet of preoccupation with unemployment as a chronic tendency, booms are emphasized quite as much as depressions (nothing interested him more than our stock market boom), underconsumption and oversaving theories are given only passing reference.

In a famous passage of *The General Theory*, every sentence of which has a special relevance for his own theory, Keynes refers to "the completeness of the Ricardian victory" as "due to a complex of suitabilities in the doctrine to the environment into which it was projected."⁷ It was, I have always felt, a similar complex of suitabilities that accounted not only for the great impression made by Keynes's theory but also for its origin. It was not a coincidence, or a misinterpretation of Keynes, that the first great development of the theory by his disciples was the stagnation thesis, that the war was regarded as a superlative demonstration of what could be accomplished to sustain employment by a really adequate volume of effective demand, and that the weight of expectation of Keynesian economists was that we would relapse after the war into mass unemployment unless vigorous antideflation measures were pursued. There is no better short statement of the stagnation thesis than that given by Keynes: "The richer the community, the wider will tend to be the gap between its actual and its potential pro-

complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols." *The General Theory of Employment, Interest and Money* (London, 1936), pp. 297-298.

⁶ Preface, p. vi.

⁷ Pp. 32-33.

duction; and therefore the more obvious and outrageous the defects of the economic system. . . . Not only is the marginal propensity to consume weaker in a wealthy community, but, owing to its accumulation of capital being already larger, the opportunities for further investment are less attractive."⁸ In an article in the *New Republic* which I have often quoted, Keynes concluded: "It appears to be politically impossible for a capitalistic democracy to organize expenditure on the scale necessary to make the great experiment which would prove my case . . . except in war conditions."⁹

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

III

Keynes's main interest was in monetary theory and policy. The development of his thinking was directed toward "pushing monetary theory back toward becoming a theory of output as a whole."¹⁰ His progress can be traced in the transition from $MV = PT$ to $I + C = Y$. There is the question in each case of distinguishing between the truism and the theory. In the traditional quantity theory (which Keynes endorsed without reservation in the *Tract*),¹¹ V and T were assumed constant, or independently determined, though in the later writings on the subject this is qualified by such statements as "normally," "except in transition periods," "apart from the business cycle." On these assumptions M affected only P (though some thought the connection often ran the other way), which was a complete demonstration that money was merely a *numéraire* and could be ignored in real analysis.

⁸ P. 31.

⁹ July 29, 1940.

¹⁰ *The General Theory*, Preface, p. vi.

¹¹ P. 81: "This theory is fundamental. Its correspondence with fact is not open to question." But in the accompanying footnote he quotes with approval a statement by Pigou which seems to me to raise rather than settle the essential question: "The Quantity Theory is often defended and opposed as though it were a definite set of propositions that must be either true or false. But in fact the formulae employed in the exposition of that theory are merely devices for enabling us to bring together in an orderly way the principal causes by which the value of money is determined."

The main concern of business cycle theory, whether monetary or non-monetary, has been with fluctuations of income, output, and employment. In this sense, we had half a century and more of "macro-economics" before *The General Theory* appeared. But there have been formal difficulties with both sides of the quantity equation. In Keynes's *Treatise*, so far as the "fundamental equations" were concerned, the effects of monetary changes were registered exclusively in P . As he later said, the equations "were an instantaneous picture taken on the assumption of a given output."¹² Moreover, as his critics pointed out, they were identities, his excess of investment over saving (via the quantity of money and the interest rate), his windfall profit rise, and his price rise being the same thing, with no causal relationship disclosed, so far as the equations were concerned.¹³ There has been difficulty also in the business cycle literature with MV . V has often been treated as a constant (whatever the writer may have said about it in chapters outside his formal theory), or as reinforcing the effects of changes in money quantity. But there is also discussion of demand for money as a factor to be offset by control of the supply, and of the concept of the natural rate of interest as the equator of saving and investment. All these versions, I think, appear in the *Treatise*, though the last undoubtedly interested Keynes most and constitutes a main theme of the book. But the chief emphasis is on business deposits. Regarding income deposits, so crucial for his later theory, his statement in the *Treatise* is: "I incline to the opinion that the short-period fluctuations of V^1 (velocity of income deposits) are inconsiderable," which appears to mean that consumers' demand for money is not a determinant of prices or output (consumers spend what—or in proportion to what—they get), and contains no hint of the later marginal-propensity-to-consume analysis.¹⁴

¹² *The General Theory*, Preface, p. vii.

¹³ I agree with Lawrence Klein's statement (*The Keynesian Revolution* [New York, 1947], p. 17), though it comes oddly from a mathematician, that there is more to the *Treatise* than the equations. In my own review (*Quarterly Journal of Economics*, August, 1931), I referred only briefly to them, though pointing out their truistic nature, and dealt chiefly with the responsiveness of investment and the price level to the interest rate (which seemed to me the core of the book), his monetary analysis, and my reasons for doubting the effectiveness of his central bank policy.

¹⁴ *Treatise*, Ch. 15, p. 246. It is not possible to find a consistent monetary analysis in the *Treatise*. Sometimes he speaks of business deposits A as interacting with income deposits, as though it were merely the quantity of the former (in response to the central-bank-determined interest rate) that mattered; at other times the main emphasis is on business deposits B (a part of the financial circulation); at other times, and particularly in the statistical and historical chapters, it is on transfers between "cash deposits" and "savings deposits," a part of the analysis that always seemed to me particularly oversimplified and unrealistic; see my review above. In the "bear position" there is some anticipation of liquidity preference, but, as Keynes pointed out, they are by no means the same thing (*The General Theory*, p. 173). For an interesting and suggestive interpretation of the extent to which the *Treatise* foreshadowed *The General Theory* (as Keynes thought it did), see John Lintner, "The Theory of Money and Prices," *The New Economics*, pp. 515-526.

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

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

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

IV

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

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

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

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

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

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

V

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

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

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

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

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

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

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

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

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

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

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

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

²⁰ *Studies in Economics and Industrial Relations* (Philadelphia, 1941), pp. 53-78.

²¹ Had residential housing been counted as consumption rather than investment, the upward tendency of consumption would have been more marked.

²² His figures on net investment as a percentage of national income show a decline from 12.2 per cent in 1907 to 8.1 per cent in 1924, 7.2 per cent in 1929, and 6.9 per cent in 1935. His conclusion was: "I believe the facts have destroyed the view up till now generally prevalent, that the rate of economic growth was primarily dependent upon the rate at which capital could be accumulated. The very rapid expansion at the present time [before the war] is taking place at a time of heavily diminishing capital accumulation. What is more remarkable, practically none of the capital which is being saved is being put into productive industry proper." *National Income and Outlay* (New York, 1938), p. 270.

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

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

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

VI

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

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

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

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

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

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

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

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

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

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

unstable economy than we have had hitherto. Much, I think, could be accomplished through the further study of price-wage-profit practices and policies: As I said in an earlier paper, though these relations have long been a main concern of (classical) economic theory they have been overlaid in recent years by preoccupation with monetary and fiscal analysis, and the tendency has been to regard price-cost behavior as a kind of *force majeure* to be "offset" rather than corrected. It is surprising how little we know, and can agree upon, with regard to these relationships, and what course to steer in order to avoid merely (a) letting them take their course, (b) compensating for them by monetary and fiscal manipulation, or (c) subjecting them to direct control.²⁸

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

If there is perfectly elastic supply so long as there is unemployment, and perfectly inelastic supply so soon as full employment is reached, and if effective demand changes in the same proportion as the quantity of money, the quantity theory of money can be enunciated as follows: "So long as there is unemployment, *employment* will change in the same proportion as the quantity of money; and when there is full employment, *prices* will change in the same proportion as the quantity of money."²⁹

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

²⁸ See my statement on "The Employment Act of 1946" before the Joint Congressional Committee on the President's Economic Report, July 2, 1947, reprinted in my book, *Post-war Monetary Plans*, *op. cit.*, Appendix I, p. 240.

²⁹ Pp. 295-296.

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

VII

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

³⁰ One of the peculiarities of an inflationary volume of effective demand is, apparently, that the slope of the consumption function is no longer necessarily less than unity. For a discussion of this and other aspects of the behavior of the consumption function under war and postwar conditions, see a forthcoming paper, "Use of the Consumption Function in Economic Forecasting," by Robert V. Rosa.

³¹ See my review, *op. cit.*, pp. 556-558.

³² *The General Theory*, p. 302.

³³ In my last talk with Keynes, a few months before his death, it was clear that he had got far away from his "euthanasia of the rentier." He complained that the easy money policy was being pushed too far, both in England and here, and emphasized interest as an

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

One of the points most commonly agreed upon, even by Keynesians, is that the aggregates of the income equation must be broken down. A point that has especially interested me is the need of breaking down the saving function to differentiate between business and consumers' saving. I have never understood how Samuelson's findings could be offered in verification either of Keynes's propensity to consume or of Hansen's chapter to which they are appended. His analysis yielded the striking conclusion that consumers in the aggregate spent virtually all their increases in money income and that any additional saving accompanying rising income almost wholly took the form of business saving.³⁴ The implications of such a conclusion for economic policy are of course very great.

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

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

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

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

³⁴ See Alvin H. Hansen, *Fiscal Policy and Business Cycles*, *op. cit.*, Ch. 11, Appendix, pp. 250-260, by Paul A. Samuelson.

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

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

VIII

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

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

³⁵ See, among recent discussions of this point, David M. Wright, *The Economics of Disturbance* (New York, 1947), Ch. II.

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

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

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

The General Theory of Employment, Interest and Money, by John Maynard Keynes. London: Macmillan and Company. 1936. xii, 403 pp. \$2.00.

A book by Mr. Keynes on fundamental questions which are right at the heart of the practical discussions of the day is no doubt an event. Those who had the opportunity to witness the expectations of the best of our students, the impatience they displayed at the delay in getting hold of their copies, the eagerness with which they devoured them, and the interest manifested by all sectors of Anglo-American communities that are up to this kind of reading (and some that are not) must first of all congratulate the author on a signal personal success, a success not in the least smaller in the cases of negative reaction than in those in which the book elicited fervent admiration. The unfavorable reviews in a sense but testify to the reality of that success, and I for one, being about to write another of those unfavorable reviews, heartily rejoice in this implication and wish it to be understood that what I am going to say is, in its own unconventional way, a tribute to one of the most brilliant men who ever bent their energies to economic problems. Expression of a teacher's gratitude should be added for the gift of what is, in its vigorous exposition and extreme simplicity, an invaluable starter of discussions. Speaking to us from the vantage ground of Cambridge and from its author's unique personal position, defended by a group of ardent and able disciples, the book will undoubtedly dominate talk and thought for some time.

In his preface Mr. Keynes underlines the significance of the words "General Theory" in his title. He professes to address it primarily to his fellow economists and seems to invite purely theoretical discussion. But it is not quite easy to accept that invitation, for everywhere he really pleads for a definite policy, and on every page the ghost of that policy looks over the shoulder of the analyst, frames his assumptions, guides his pen. In this sense, as in another, it is Ricardo all over again. The advice offered implicitly and the social vision unfolded explicitly, do not concern us here. That advice (everybody knows what it is Mr. Keynes advises) may be good. For the England of today it possibly is. That vision may be entitled to the compliment that it expresses forcefully the attitude of a decaying civilization. In these respects, this book invites sociological interpretation in the Marxian sense, and nothing is more certain than that such interpretation will be administered to it before long.

It is, however, vital to renounce communion with any attempt to revive the Ricardian practice of offering, in the garb of general scientific truth, advice which—whether good or bad—carries meaning only with references to the practical exigencies of the unique historical situation of a given time and country. This sublimates practical issues into scientific ones, divides economists—as in fact we can see already from any discussion about this book—according to lines of political preference, produces popular successes at the moment, and reactions after—witness the fate of Ricardian economics—neither of which have anything to do with science. Economics will never have nor merit any authority until that unholy alliance is dissolved. There

is happily some tendency towards such dissolution. But this book throws us back again. Once more, socialists as well as institutionalists are right in judging economic theory as they do.

Ricardian as the book is in spirit and intent, so it is in workmanship. There is the same technique of skirting problems by artificial¹ definitions which, tied up with highly specialized assumptions, produce paradoxical-looking tautologies, and of constructing special cases which in the author's own mind and in his exposition are invested with a treacherous generality. In one fundamental point it actually falls short of the line already reached by those writers who in the sixties of the past century criticized some of the tenets of what *to them* was "classical" doctrine,² notably Longe and Thornton. These knew perfectly that the old supply and demand apparatus renders its very limited service only if applied to individual commodities, strictly speaking to individual commodities of relatively small importance, and that it either loses or changes its meaning if applied to comprehensive social aggregates. This was in fact their foremost objection to the wage fund theory. Mr. Keynes' fundamental construction (which is all we can consider here) rests on a contraposition of expected³ net "proceeds," equal to expected profits plus expected current payments to factors (for definition see page 24), and *those* proceeds the expectation of which would be sufficient and not more than sufficient to induce entrepreneurs to decide on producing the corresponding output. Two schedules or functions are imagined in order to describe the behavior and the relation to one another of these two fundamental variables. The analogy of the first with the ordinary Marshallian demand curve and the analogy of the second with the ordinary Marshallian supply curve are obvious. In fact, Mr. Keynes speaks of Aggregate Demand in the one case and Aggregate Supply in the other and makes them yield a unique "point of intersection." There is as little justification for this extension of the "Marshallian cross" as there is for its application to the case of money, which has remained a besetting sin of the Cambridge group to this day.

Transition to the central theme of the book is effected by relating those two fundamental variables not to output but to employment, and not to employment of resources in general but to employment of labor. Mr. Keynes is as careful to point out that number of workmen employed is not proportional to output as Ricardo was to point out that value cannot be proportional to quantity of labor. But exactly as Ricardo reasoned as if it were, so Mr. Keynes assumes that employment of labor is an "adequate" index of the output resulting from it. The arguments offered by both authors, in support of what is a procedure obviously inadmissible in anything that pretends to be a "general" theory, are curiously alike. In particular both display a desire to banish the variations of output—or, in Ricardo's case, of "riches"—from the realm of theory.

It should be clearly realized what that means. Readers of this *Journal* will shrug their shoulders at a theory which deserts the statistician in his struggle with the momentous problems surrounding the Index of Production.

But disregarding this, reasoning on the assumption that variations in output are uniquely related to variations in employment imposes the further assumption that all production functions remain invariant. Now the outstanding feature of capitalism is that they do not but that, on the contrary, they are being incessantly revolutionized. The capitalist process is essentially a process of change of the type which is being assumed away in this book, and all its characteristic phenomena and problems arise from the fact that it is such a process. A theory that postulates invariance of production functions may, if correct in itself, be still of some use to the theorist. But it is the theory of another world and out of all contact with modern industrial fact, unemployment included. No interpretation of modern vicissitudes, "poverty in plenty" and the rest, can be derived from it.

The central thesis that under-employment can exist in a state of stable equilibrium and that saving is responsible for it is then made to follow from two additional hypotheses. The one—embodied in the concept of Propensity to Consume—is that "when aggregate real income is increased aggregate consumption is increased, but not by so much as income" (page 27). This Mr. Keynes dignifies, in the worst style of a bygone age, into a "Psychological Law." The question of fact apart—statistics of installment selling and other forms of consumers' credit obviously suggest the possibility of doubt—such a "propensity" is again nothing but a *deus ex machina*, valueless if we do not understand the mechanism of the changing situations, in which consumers' expenditure alternatively increases and contracts, and redundant if we do. Postulating, however, an independent and systematic tendency to that effect, Mr. Keynes finds a "gap" in expenditure resulting from it which may or may not be filled by investment and tends to widen as communities grow more wealthy. This amounts to introducing another hypothesis: the hypothesis of failing "Inducement to Invest."

Since Mr. Keynes eliminates the most powerful propeller of investment, the financing of changes in production functions, the investment process in his theoretical world has hardly anything to do with the investment process in the actual world, and any proof, even if successful, that (absolutely or relatively) falling "Inducement to Invest" will produce under-employment would have no greater practical importance than a proof that motor cars cannot run in the absence of fuel. But that proof, even under its own assumptions and granting that in Mr. Keynes' world there would be a systematic tendency for Inducement to Invest to grow weaker,⁴ meets the obvious objection that Propensity to Consume and Inducement to Invest are not independent of each other. In some passages (for example, page 30) Mr. Keynes seems indeed to hold that they are. We can absolve him, however, from the grave error this would spell, because each time (for example, page 31) he in fact admits the existence of an equilibrating mechanism. But then the whole *theoretical* case, that is, the case in terms of fundamental features of the economic process, collapses, and we are *practically* left with friction, or "stickiness," institutional inhibitions, and the like, which in particular may prevent the rate of interest from reacting promptly or, in general, pre-

vent the whole of that equilibrating mechanism from functioning adequately.

Space forbids our entering into a discussion of the Multiplier, its relation to the Propensity to Consume, the system of Wage Units, and other tools by means of which Mr. Keynes works out his basic ideas. I wish however to welcome his purely monetary theory of interest which is, as far as I can see, the first to follow upon my own. Unfortunately, I must add that the similarity stops there and that I do not think my argument open to the objections which this one is sure to meet. Some differences would vanish, if the concepts of a demand for money stocks and of "liquidity preference"—which is another *deus ex machina*; there is a whole Olympus of them—were replaced by concepts drawn from the economic processes that lie behind the surface phenomena denoted by those two. But then many of the striking inferences would also vanish. The whole vision of the capitalist process would change. Interest would lose the pivotal position which it holds in Mr. Keynes' analysis by virtue of the same technique which made it possible for Ricardo to hold that profits depend upon the price of wheat. And a completely different diagnosis of modern difficulties would follow.

The less said about the last book the better. Let him who accepts the message there expounded rewrite the history of the French *ancien régime* in some such terms as these: Louis XV was a most enlightened monarch. Feeling the necessity of stimulating expenditure he secured the services of such expert spenders as Madame de Pompadour and Madame du Barry. They went to work with unsurpassable efficiency. Full employment, a maximum of resulting output, and general well-being ought to have been the consequence. It is true that instead we find misery, shame and, at the end of it all, a stream of blood. But that was a chance coincidence.

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¹ The definition of involuntary unemployment, page 15, may serve as an example. Taken literally (which of course it would be unfair to do) it would mean that there is no practically conceivable case in which workmen are not partially unemployed by definition. For if prices of wage goods rise a little, other things being equal, it is clear that both the demand for, and the supply of, labor will increase under competitive conditions, the latter at least as long as the flexibility of the marginal utility of income to the workmen is what present statistics lead us to believe.

² Mr. Keynes' definition of the word "classical," which is made to include Professor Pigou, who cannot be counted among classics by virtue of any criterion except the one of outstanding achievement, reminds me of a little experience I had in a group of students. I observed that one of the members kept on referring to a highly unconventional proposition as "orthodox." I asked him why he did so, seeing that the proposition was no part of received doctrine. His answer was, "I simply call orthodox everything I don't like." Protest should be filed in passing against Mr. Keynes' methods of criticism. But beyond that it is regrettable that so brilliant a leader should set so bad an example of utter absence of *sercundia*. I am no Marxian. Yet I sufficiently recognize the greatness of Marx to be offended at seeing him classed with Silvio Gesell and Major Douglas. Mr. Keynes is unjust even to Major Douglas for there is no warrant whatever for thinking little of that writer once one has accepted the views of this book. Certainly Marx and the classics (in the proper sense of the word) were grievously at fault in very many points as it is natural that pioneers should be. Yet they are right as against Mr. Keynes. His attitude toward Marshall's teaching is for Marshallians to judge.

³ The emphasis on *expected* as against *actual* values is in line with modern tendencies. But expectations are not linked by Mr. Keynes to the cyclical situations that give rise to them and hence become independent variables and ultimate determinants of economic action. Such analysis can at best yield

purely formal results and never go below the surface. An expectation acquires explanatory value only if we are made to understand *why* people expect *what* they expect. Otherwise expectation is a mere *deus ex machina* that conceals problems instead of solving them.

⁴ To many people statement of such a tendency will sound "realistic." This is however entirely due to recent experience and would have equally been the case after, say, 1720 or 1825 or 1873. No support of the theory in question can be derived from this, since it rests exclusively on observation of the surface mechanism of a deep depression *already in progress*, the explanation of which must be worked out independently of it.

STABILITY AND FULL EMPLOYMENT

I

1. The purpose of this paper is to examine and classify the various problems involved in a policy of maintaining a continuous state of full employment. The economic literature of the last few years has thrown a great deal of light on the question of how employment can be *stimulated*; and I think one can say that there is much greater agreement on this subject than there was. Most economists are agreed that Governments have greater power in this matter than they are in the habit of employing, and we are nearing agreement also as to the factors that determine the efficiency of different policies to that end.

But I think there is less agreement, and perhaps also less awareness, about the further problem, of how to keep the system *stable*, at a reasonable level of prosperity: in other words, how to maintain a state of full employment, once it has been achieved.

Indeed, to some people these two problems might appear as one. But they are not; the difference between them could perhaps be illustrated as follows. The first concerns the question of how to convert a state of depression into a state of prosperity; the second of how to prevent a state of prosperity from turning into a state of depression. The solution of the second problem is more difficult than that of the first; to a certain extent it calls for different types of measures, which may even conflict with those aiming at a speedy re-attainment of prosperity. And, as I hope to show, there is also far less certainty as to whether it can be accomplished. For, given the presence of certain conditions, a state of full employment will be inherently unstable.

2. I should like to begin by drawing attention to two technical peculiarities of modern industrial societies: peculiarities which in themselves are fairly obvious, but in discussions of trade-cycle policy are often lost sight of. Their consideration should also enable us to clarify our ideas as to the meaning of "full employment." One of these peculiarities is the *complementarity* of factors of production, and the other their *specificity*. Both are what one might call "short-run" factors—*i.e.*, they are strictly

true only in the short period, but, for reasons which will become clear in the subsequent argument, the long-run forces which could remove them are often entirely inoperative.

(a) *Complementarity*.—Production requires the co-operation of three essential groups of factors: equipment, labour and raw materials. It is a significant fact of modern technique that *given the type of equipment in existence* there is a relation of strong complementarity between them: that is to say, the extent to which the proportions of these factors can be varied in production is highly limited. (This may not be true of *all* industries, but I think it holds for the majority of them.) Hence, in order to increase the volume of production in any firm or industry, it is necessary to increase the rate of employment (or utilisation) of *all three* types of factors at the same time.

That production cannot be increased merely by increasing the amount of labour employed, without using more raw materials, or by using more raw materials without more labour, is sufficiently obvious without further consideration. It is less obvious that the same holds true of equipment as well. Yet most industrial machinery is of such a character that neither its productive capacity *per hour* nor the amount of labour necessary to work it can be varied outside narrow limits. Hence when more labour and raw materials are employed, more machinery is used also, either by working the machines for longer hours or by using machines previously laid up. It is not for technical, but only for institutional, reasons that the demarcation line between employed and unemployed equipment is not so clear cut as between employed and unemployed labour.

Among these three factors, raw materials have a high elasticity of supply, even in the short period. Apart from exceptional circumstances or a few exceptional industries, the expansion of production is not likely to be held up for long for lack of raw materials. And this means that in considering the productive capacity of an industry, we can generally ignore raw materials. From our point of view, therefore, the two important factors are *equipment* and *labour*. It is necessary that unemployed equipment should be available in order that unemployed labour should be absorbed in production; and in the same way, the existence of unemployed labour is necessary to utilise unused equipment. The point where the output of a particular industry becomes inelastic is the point where *one of these factors* is fully employed.

If we define "full employment" in an industry by this criterion—as the state of affairs where the output of the industry

is inelastic¹—this might imply either full employment of equipment or full employment of labour. Only in particular instances—in a transitory phase of the Trade Cycle—will it imply full employment of both. Since equipment deteriorates in a way labour does not, the limit to the productive capacity of an industry is more likely to be set by the *scarcity of equipment* after a period of low activity; and since the amount of available equipment can be expanded more readily than the supply of labour, it is more likely to be set by the *scarcity of labour* after a period of high activity.

(b) *Specificity*.—Most equipment is “specific” in the sense that it can only be used in the production of certain commodities or groups of commodities. Similarly labour, even unskilled labour, is attached to particular industries whence it can only be transferred slowly, especially where industries are localised in different areas. This specificity implies, on the other hand, that in order to have “full employment,” the composition of production must be such as to make it possible to use the type of equipment and the type of labour that exists. Full employment, therefore, not only means a certain level of real income; it also implies a real income of a certain composition. If we divide industries into two large groups, consumption industries and investment industries, we can say that *full employment presupposes a division of real income between real consumption and real investment in a certain proportion*. There are, of course, different kinds of consumption industries and different kinds of investment industries, the demand for each of which expands when consumption or investment expands; and it is always possible that there should be *horizontal* maladjustments, in the sense that some

¹ To say that the output of an industry has become inelastic does not necessarily imply that it is *technically* impossible to increase output further; it might merely mean that further expansion of output is economically impossible, since any such tendency is checked by a rise in the rate of wages. In the case where the limit to productive capacity is set by the scarcity of equipment, rather than labour, the elasticity of marginal costs, and hence real wages, falls rapidly as the technical limit is approached. There is a point beyond which Trade Unions do not allow *real wages* to fall—a point, that is to say, beyond which any further rise in the level of prices is matched by an equivalent rise in the rate of wages. It is therefore this minimum level of real wages which determines the amount industry can produce. In the opposite case, where the technical limit is set by the scarcity of labour rather than equipment, there is no similar tendency for real wages to fall as the limit is approached; but there is a similar point beyond which any further tendency for employment to rise is checked by a rise in wage rates—a point, that is to say, beyond which Trade Unions do not allow unemployment to fall. In both cases, therefore, the point where money-wages rise sets the maximum level of output (cf. also Mrs. Robinson, *Essays on the Theory of Employment*, ch. 1).

of the consumption industries or some of the investment industries should be over-developed in relation to the others. (Such is the case where full employment is reached earlier in certain industries than in others in the same group.) In this paper, however, I want to ignore the existence of such "horizontal" maladjustments (although practically they may be very important—*e.g.* in post-War Britain), chiefly because I believe that, as regards these, forces of adjustment are operative in the long run which are inoperative as between the consumption and the capital goods industries.¹

These two attributes of factors of production—complementarity and specificity—present complications which make the task of maintaining full employment very much harder. While in some respects each raises problems peculiar to itself, in other respects it is their combination which is the real source of difficulty.

II

3. In a modern industrial society, fluctuations in activity are always much larger in the investment-goods industries than in the consumption-industries group. Hence if we start from a "typical" state of subnormal activity, the percentage of unemployment and unused capacity of equipment will be much larger in the former group than in the latter. The elimination of unemployment means, therefore, largely the elimination of unemployment in the investment-goods industries. It presupposes an increase in activity which is largely, if not entirely, an increase in the production of investment goods.

4. A Government policy which takes the existing industrial situation as given—which is only another way of saying that it desires to achieve results in the near future—must therefore aim at the stimulation of investment. It can do so in three ways. It can enforce, by banking policy, a reduction in interest rates, including of course the long-term rate of interest. It can borrow and invest directly in public works of all kinds, such as the creation of roads, houses or battleships. It can subsidise private investment in a number of ways, such as subsidies on wage-payments, guarantees on loans, etc. In the terminology of Mr.

¹ This is not meant to suggest that the degree of specificity is necessarily greater as between consumption and capital goods industries than between individual industries in either group. As will be argued below (p. 653), the forces of adjustment are likely to be weaker—for economic, rather than technical reasons.

Keynes, it can lower effective liquidity preference through increasing the quantity of money, or raise the marginal efficiency of capital through public investment or subsidies, or it can do both. There can be little doubt that either of these, or a combination of these policies can always be made successful. It is all a question of how quickly we want to have results and how drastically the authority is willing to act. The possibility of securing large changes in the (long-term) rate of interest has often been doubted; but there need be no real obstacle here, provided radical changes are made in the nature and scope of open-market operations. The policy of public works also has often been regarded as insufficient, owing to the limited scope of the investments undertaken by public authorities. Here again, this scope could be conveniently enlarged, and if re-armament continues to accelerate on the present scale, this deficiency will automatically disappear. And there is, finally, a large unexplored field in the form of employment or production subsidies (financed initially out of loans and subsequently out of taxes on profits) which could easily be resorted to, once the political prejudices against measures of this type are overcome. If we add that Governments in these days could, if they wanted to, take effective steps to prevent a flight of capital abroad, it becomes obvious that there need be no insuperable *technical* difficulties for an effective short-run policy to deal with a depression.

5. It is not our purpose, however, to discuss the relative merits of these different policies, beyond noting the fact that they can be relied upon to deliver the goods: to bring about a state of affairs which we can call full employment, or reasonably full employment. Rather we ought to start from the point where this desirable state of affairs is already achieved. Suppose the rate of interest is set so low, or the rate of Government expenditure is kept so high, that the investment industries are "fully employed": their unemployment is reduced to some "normal" or minimum level. Would such a situation be stable?

I should like to examine this question under two heads. First, in the "short run"—*i.e.*, assuming that the investment activity has not continued long enough for significant changes in the quantity of equipment to occur. Secondly, in the "longer run"¹ allowing for the effects created by the increase in equipment which follows upon investment activity.

¹ I use the term "longer run" to distinguish it from a true long-run analysis which works under the assumption that the (net) investment activity has already come to an end.

III

6. We have already seen that, given the available capacity in equipment and labour which is an inheritance of the past, full employment implies that the levels of real investment and real consumption stand in a certain proportion to each other. Given this real income, and given the distribution of income between wages and profits (including salaries), which again depends on certain technical elements of the situation, such as the elasticity of marginal costs at the relevant output levels and the degree of monopoly ruling in the different industries, we can assume that the community will have a certain propensity to save—*i.e.*, it will wish to divide its income between consumption and saving in a certain proportion. I can see no reason why the proportion between consumption output and investment output, when both industries are fully employed (and which is determined by the distribution of the available production capacity—labour and equipment—in the two groups) should be the same as the proportion in which the community as an aggregate of individuals wishes to divide its income between consumption and saving. If I may express this in a way which is slightly unfashionable, I should say that at the position of full employment, Investment will either exceed or fall short of Savings, but only as a result of a mere accident could it be equal to Savings. And *in either case* (whether Investment exceeds Savings, or Savings exceed Investment) forces will be in operation likely to cause, sooner or later, a reduction in the level of activity.¹

(i) *If Savings exceed Investment*, full employment in the investment-goods industries will not be sufficient to secure full employment in the consumption-goods industries. For even if in the latter industries full employment obtained temporarily, entrepreneurs would find that their receipts continued to fall

¹ As Mr. Keynes has shown (*General Theory*, ch. 7), Investment and Savings are always and necessarily equal when Savings are defined as Actual Income — Expenditure. (This will be true quite independently of any arbitrariness in the definition of Investment. For any change in the basis of calculating Investment will affect the calculation of Income in a corresponding way.) But there are at least three other ways in which Savings can be defined, viz.: (1) Normal Income — Expenditure (Mr. Keynes' definition in the *Treatise on Money*); (2) Expected Income — (Actual) Expenditure (this may be regarded as the Swedish definition); (3) Full-Employment-Income — Expenditure (the definition here adopted). This latter could also be defined as the proportion of Income that would be saved if total real income were such that all industries are fully employed.

Each of these definitions may have its uses for different purposes. Our own definition is intended to focus attention on the peculiar problems created by the specificity of factors.

below expectations until the real income produced in the consumption-goods industries was reduced to a point where Investment and Savings were no longer unequal.

Moreover if this implies that a considerable amount of equipment in the consumption-goods industries is unemployed, the situation in the investment-goods industries itself will be highly unstable: with excess equipment in the lower stages, the demand for new investment is bound to sag. In order to maintain activity in the investment-goods industries, the Government would have to enlarge further and further the scope of public investment (or else it has to bring about continuous further reductions in the rate of interest, which may prove unavailing).

It might be objected that since we started from a "typical" state of subnormal activity where the scope for expansion in the consumption-goods industries was relatively small, the scope for expansion in the investment-goods industries relatively large, and since an increase in employment in the investment-goods industries is bound to increase the demand for consumption goods, it is highly unlikely that full employment in the investment-goods industries should be reached *before* full employment is reached in the consumption-goods industries. The increase in employment, however, may have been associated with a considerable shift in the distribution of income in favour of profits (owing to diminishing returns or diminishing elasticities of demand) in which case the expansion of consumption, consequent upon investment, might be relatively small.

The real importance of this case, however, as will be shown below, is not likely to be at the initial period of a boom (*i.e.*, of a high investment activity), but after investment activity has proceeded for a certain period.

(ii) *If Investment exceeds Savings*, the maintenance of full employment will be associated with a cumulative inflation of prices. For in this case the money-receipts from the sale of consumption goods, at any period, will be higher than the money outlays for those goods (including expected profits); this will raise the money outlays in the next period, and also the money demand for investment, and so on. If the rate of interest is kept constant, there need be no limit to the rise in prices and money incomes. This is the well-known Wicksellian case where the "money rate" of interest is lower than the "natural rate," which latter is defined as the rate which keeps Investment equal to Savings. The point which Wicksell did not seem to take into consideration is that if equipment and labour are specific, equality

of Investment and Savings is not compatible (or need not be compatible) with a state of full employment. There is one rate of interest which secures full employment, and there is another rate of interest—the natural rate—which equates Savings and Investment (*i.e.*, which restricts investment activity to the point where it no longer exceeds savings).

There are two things to be noted here: (1) This cumulative process presupposes that the increase in the demand for the products is followed by a *rise in wage rates*. If wages do *not* rise—suppose, *e.g.*, that they are kept steady by Government decree or by a combination of entrepreneurs—the rise in prices will imply a shift to profits (in the distribution of income), and since such a shift increases savings (because capitalists save a higher proportion of their income than wage-earners) the cumulative process will come to an end when profits have risen sufficiently to provide the savings.¹ (In other words, the likelihood of such a hyperinflation depends on *at what point* of the expansion will wage-increases occur: on how far Trade Unions allow real wages to fall [and/or employment to rise] before they enforce a rise in wage-rates. The later this point, the greater the share of profits [and/or the greater total real output].)² (2) If the limit in the production of consumption goods has been reached because of scarcity of equipment rather than scarcity of labour, the gradual emergence of new equipment will alter the proportion between real consumption and real investment, in favour of consumption; it will also imply a rise in total real income and hence a rise in savings. This check, however, is bound to be slow in operation relatively to the rate at which money incomes would be rising. It can only be effective if the monetary expansion is not stopped earlier—on account of the

¹ It might be objected that the extra profits of the entrepreneurs in the consumption-goods industries provide an extra stimulus to investment, and hence a rise in the demand for investment goods. But this could only keep the cumulative process going if in consequence the entrepreneurs in the investment-goods industries expand their consumption to an extent that would offset the tendency for the propensity to consume to fall on account of the reduction of the real income of labour. (The short-period consumption of entrepreneurs is likely to be inelastic, not only because entrepreneurs are, on the whole, rich people, but also because they normally regard their income as fluctuating, and do not adjust their standard of living to short-period changes. On the other hand, the rise in profits may lead to widespread Stock-Exchange speculation, and hence to temporary bursts of consumption by a much wider class of people.)

² Since labour's bargaining power is greater when labour is scarce, relatively to equipment, than in the opposite case, the cumulative process is more likely when the limit to expansion is set by the scarcity of labour, rather than equipment. But this is likely to be the case at the *end* of the boom when, however, an excess of Investment over Savings is unlikely for other reasons. Cf. p. 650 below.

inflationary rise in prices—by the banking system. But it is important to bear in mind that in theory any excess-investment situation would gradually convert itself (provided the process of monetary expansion is allowed to go on), via the rise in consumption output, into a situation with excess-savings.¹

Both these maladjustments (an excess of Investment over Savings, or of Savings over Investment) can be remedied by measures aiming to regulate the propensity to save. Indeed, it seems that without a policy regulating savings, policies which aim merely to ensure that there should be a sufficient level of investment could hardly have lasting success. The key to any such regulation is to be found in the fact that savings, for any given total income, largely depend on the distribution of income. If Investment exceeds Savings (in other words, if the productive capacity in the investment-goods industries is too large), the Government can increase savings by altering the distribution of income in favour of profits, and vice versa. The most convenient way of doing this is by raising or lowering taxes on wages or subsidies on wages. A tax on wages, by raising marginal costs relatively to wages, will alter distribution in favour of profits; a subsidy on wages, by lowering marginal costs relatively to wages, will alter distribution in favour of labour. (In Great Britain, such wage-taxes already exist in the form of unemployment and health insurance contributions which could easily be made to serve this purpose.) Similarly the imposition (or removal) of turnover taxes, taxes on consumption goods (excise), should have a similar effect. In other words, the Government can always alter the propensity to save by changing the proportion of revenue raised by direct and indirect taxation, respectively.

Without some such policy the double purpose of securing a high level of activity and securing stability of incomes cannot be achieved. It cannot be achieved by acting on the rate of interest, or the level of public-loan expenditure or the level of money wages alone. (In other words, to secure full employment, it is not enough to lower liquidity preference or to raise the

¹ It would then appear at first sight that a situation can be regarded as one of excess investment, when the boom comes to an end on account of a rise in interest rates. This, however, is not so. For a scarcity of cash can arise merely on account of the fact that money incomes rise when real incomes rise, even if there is no insufficiency of savings, and hence no tendency for the rise in money incomes to be cumulative. It would be more correct to say that excess investment (a shortage of savings) is one reason why a boom comes to an end through a rise in interest rates; but this rise alone is no proof that investment has been excessive (*i.e.*, that the rise of money incomes would have proceeded indefinitely, had the rates of interest not been allowed to rise).

marginal efficiency of capital. The propensity-to-consume function has to be regulated as well.)^{1, 2}

IV

7. So far we have been dealing mainly with the problem of stability in the "short period"—*i.e.*, under the assumption that investment activity has not yet had time to bring about significant changes in the amount of available equipment. But as investment activity goes on, newly constructed equipment becomes an increasingly significant factor in the situation. Capacity becomes enlarged both in the consumption- and investment-goods industries.

(i) If the limit in the previous level of output was set by the scarcity of equipment, rather than the scarcity of labour, the creation of new equipment enables an increase in production through a further increase in the amount of labour employed. It is reasonable to assume that the capacity to produce consumption goods increases relatively to the capacity of investment output.³ Since savings increase with an increase in real income, and since potential consumption output increases relatively to investment, a further adjustment in the distribution of income is necessary to ensure that the demand for consumption goods increases sufficiently with the increase in the capacity to produce them. Assuming that this can be done, the situation can continue: the demand for new investment can be maintained.

(ii) Sooner or later, however, the point is reached where all the available labour is absorbed in production. Even if the installation of additional equipment goes on still further, current

¹ Moreover it is not a question of a once-and-for-all adjustment in the propensity to consume, but rather of a continuous adjustment. For, on account of the gradual emergence of additional productive capacity, both the relation of the output of the two industries (*i.e.*, of consumption and investment) and the distribution of income will be continuously changing.

² An alternative way of removing the discrepancy (between investment and savings) is by varying the relative wage-levels in the consumption- and investment-goods industries; in this way the money value of investment can be altered relatively to total money incomes. This policy, however, creates difficulties of its own which cannot be gone into here.

³ This is likely to be the case, because investment on a large scale in any particular kind of equipment presupposes that excess capacity of that kind of equipment has been largely eliminated. Since at the beginning of the boom, surplus capacity in the investment-goods industries is likely to be much larger than in the consumption-goods industries, there must be a phase during which equipment in the consumption-goods industries is already fairly fully utilised, whilst equipment in the investment-goods industries is still excessive. While this situation lasts, the main incentive for investment in fixed capital will be in the consumption industries.

production cannot be increased much further, however much the propensity to consume is stimulated. For if "machines" and "labour" are complementary in production, and there is not enough labour to work all the machines, output cannot be augmented by adding more machines. Thus excess capacity in equipment will make its appearance, which in turn will lead to a breakdown in the demand for investment.

It might be objected that the new equipment will be more "labour-saving" than the old equipment. It will not be the case of an increase in the number of machines, but the replacement of old machines by new and superior machines, capable of larger output, but without requiring more labour.

This might be true to a certain extent; and in so far as it is the case, it is likely to be due to the purely extraneous fact that technical knowledge is continually improving, rather than to any *inherent* tendency for the type of equipment created to become more "automatic" when the scale of production of equipment is increasing.¹ But it is not enough that the new equipment should be more labour-saving (*i.e.*, should give a higher output per man-hour). In order to prevent the emergence of excess capacity in equipment, the new "machines" must be *so much* more labour-saving that the aggregate amount of labour required to work the newly installed equipment per unit of time, should be no greater than the amount of labour simultaneously "released" through the disappearance of that part of the old equipment which is worn out and has to be scrapped.² This depends not only on

¹ The new equipment can be more labour-saving than the old equipment for two reasons: (i) either because it embodies newly invented features; (ii) or because it possesses a higher degree of "capital intensity," *i.e.*, the entrepreneur found it profitable, at the time when the construction of the equipment was planned, to spend more capital per unit of planned output. In the real world, however, the mechanism of expansion does not provide for an automatic increase in "capital intensity" when the scale of investment is increasing; if anything, the capital intensity of investments is likely to fall when the scale of investment is rising, and vice versa. This problem will be analysed in another paper by the present author on *Capital Intensity and the Trade Cycle*.

² It would be wholly fallacious to suppose that the installation of the new equipment *causes* old equipment to be scrapped (which would not be scrapped otherwise), and thereby automatically secures the labour necessary to work it. It is true, of course, that the competition of the new equipment will reduce the profits, and hence lower the value, of the old; as a result of which the old equipment will not be renewed (in its *old* form) after it has been worn out. But so long as there are *any* profits in working the old equipment (*i.e.*, so long as the receipts from its operation more than cover prime costs), it will continue to be used; and it will compete effectively with the new equipment for the available supply of labour, however much its value might have fallen below its original cost of production. (If this were not so, there would be no more railwaymen left; they would all have turned into busmen.) Redundant capacity implies in this

the rate of technical progress, but also on the level of investment activity; and the condition will be all the less likely to be fulfilled the greater the aggregate amount of new equipment produced, per unit of time. Moreover, as time goes on, the old equipment will be more and more "labour-saving," and hence the amount of labour released through scrapping will fall; to offset this, the labour-saving capacity of the newly produced equipment would have to increase still further.

Thus, unless Providence decrees that there should be an adequate rate of technical progress—the output capacity of the investment-goods industries determining what is adequate—there is no mechanism to ensure that the aggregate quantity of equipment and the quantity of equipment which can be combined with a unit of labour should increase continuously in the same ratio—or rather, that the number of men required per machine should decrease in the same ratio in which the number of machines increases. As investment activity continues at a high level, excess capacity of equipment is bound to make its appearance.

Once redundant capacity appears, it will be almost impossible to maintain activity undiminished, unless State investment activity is extended so wide as to replace private investment. Suppose the scarcity of labour, relative to machines, leads to a rise in wages relative to product prices, and a fall in profits. The individual entrepreneur only sees that wages are high, or that product prices are low, his profits are low and his recent investment projects have turned out badly. The scarcity of labour might appear to him in the guise of an insufficiency of sales and the under-employment of his plant. Since it is his equipment which is redundant, he is just as likely to attribute the cause to lack of demand as to anything else; and until he feels confident again, even a drastic reduction in the rate of interest may be insufficient to induce him to embark on further investment.

8. It is this factor that is ultimately responsible for that "temporary exhaustion of investment opportunities" with which several economists explain the breakdown of the boom; that explains why investment cannot run smoothly in time, but must proceed by jerks. It is not the *only* cause of a boom coming to an end: rather, it is a sort of ultimate or final cause, which must bring it to an end if it is not stopped earlier by any one of a number of other causes. And, unlike some of the other factors which

case that profits on the *new* equipment will be less than what was expected and what is necessary to continue to produce it. (It is for this reason that the "compulsory scrapping" of "obsolete" capacity improves the profits on new capacity.)

bring prosperity periods to an end, it is not one whose operation could be prevented, or suspended, by appropriate economic policy. For it is no longer a question of savings being too much or too little. An excess or a deficiency of savings, as we have seen, could be speedily remedied; a maladjustment between equipment and labour can not.

A natural remedy to this situation—but a rather slow and painful one!—would be the transfer of labour from the investment-goods industries to the consumption-goods industries. This would enable the equipment in the consumption-goods industries to be more fully employed—again assuming that the propensity to consume is properly looked after!—and also, investment activity to continue, on a smaller scale. In reality, however, such a transfer rarely takes place on a large scale, and for this not only the technical immobility of labour is responsible, but also the fact that when the investment-goods industries are prosperous, there will be no incentive for such a movement, while when the investment-goods industries are depressed, the consumption-goods industries are also slack; the consumption-goods industries will have unemployment as well.¹ The renewal of activity, apart from Government action, must await the disappearance of redundant equipment.

Thus the ultimate cause of instability in the economic system must be sought in those technical factors which prevent an even rate of real capital accumulation: the difficulty of so adjusting technique as to prevent redundancy of equipment when the rate of accumulation is very fast. (In the last resort, it is the complementarity and specificity of factors which is responsible for this.) The instability, or potential instability, is thus all the greater, *the larger the capacity of the investment industries, relatively to the consumption industries*: the higher is the rate at which investment *can* proceed, in times of prosperity. The Trade Cycle, as Mr. D. H. Robertson once said, is the price to be paid for a high rate of economic progress.

9. There is a passage in a recent paper by Mr. Robertson which puts the moral of this reasoning admirably (although I would not like to commit Mr. Robertson to the preceding argument with which he may not be in agreement):—²

¹ It is this fact—that the consumption and investment industries are likely to be prosperous and depressed *at the same time*—which makes the problem of specificity more difficult in this case than in the case of “horizontal” maladjustments.

² “A Survey of Modern Monetary Controversy,” reprint of a paper read before the *Manchester Statistical Society*, 1937, p. 13. Italics mine.

“The distribution of productive resources between the consumption and the capital-making trades is the result of the cyclical process from which we are seeking escape, and can neither be permanently taken for granted nor altered in the twinkling of an eye. Thus in respect of fullness of employment the ‘normal’ now speedily attainable is inferior to the normal of our dreams—the normal of the society which has never lapsed from an even rate of progress. . . . The only hope of attaining it lies in checking the cumulative expansion at some point selected with what judgment and wisdom we can command, and *then letting the slow processes of occupational readjustment get to work*. If, however, some such clean-up could once be effected, and a true Blondinian policy thereafter be pursued,¹ we might indulge a reasonable hope for the future.”

What Mr. Robertson does not make clear² is that such a policy requires not only a once-and-for-all clean up, in the form of occupational readjustment, but also a permanent alteration in the structure of income distribution which would enable a much higher proportion of income to be devoted to consumption and a much lower proportion to saving. For the present distribution of resources between consumption and capital-making trades is not *merely* the outcome of cyclical causes: it is also—perhaps even largely—the consequence of the fact that, with our present level of productivity per head and our present distribution of income, only a high ratio of investment output is consistent with a high level of employment and real income.

The only way of carrying out such a policy, therefore, is to stimulate the propensity to consume sufficiently to make the consumption-goods industries go at full blast; and to prevent, at the same time, by means of a conservative banking policy, or taxation, or anything else, the output of the investment industries from reaching beyond a certain moderate level. If the one could be kept prosperous and the other depressed, *at the same time and for a sufficiently long period*, the distribution of resources would be gradually altered. But this is a long-run policy which is likely to bear the stamp of failure while it goes on; and since it involves low (real) profits and high (real) wages, it is likely to be unpopular with certain classes. To secure that change in the distribution

¹ The Blondinian policy here mentioned would not be easy to define, but I hope it is not too different from the one advocated here.

² Though he alludes to the problem at the end of the paper: *loc. cit.*, p. 19.

of income which is needed to give a much higher propensity to consume might appear as a policy of expropriation. And even if it succeeds, I do not think that we can hope from it the complete absence of cyclical tendencies; it would only ensure that fluctuations were confined to much narrower limits.

It is thus a question of Progress *versus* Stability.¹ And apart from the obvious gain in income-distribution, it is not at all certain, on second thoughts, that the first ought to be sacrificed for the second. We must bear in mind that a society which has a high state of technique, but a relatively small capacity in the capital-making industries, has a much lower degree of adaptability to unexpected changes. A Britain ruled on the principles of Mr. Robertson would be a more pleasant place to live in, but it would be in a much weaker position to resist unexpected demands of a Mussolini or a Hitler.

10. Failing such a policy, we must put up with the fact that construction activity moves in waves and cannot proceed at an even rate. This does not mean that the depression need necessarily be very bad. There still remains the policy of making public investment anti-cyclical (concentrating all public works in times of slump), which theoretically could remove the unevenness of investment activity altogether. But I doubt whether in practice this could ever be achieved. Apart from the great difficulty of correct timing, there is the fact that public works of the ordinary kind do not necessarily make use of the resources of the depressed industries; while public works of an extraordinary kind—introduced just in order to provide work for the unemployed—are hardly satisfactory as a permanent feature. Hence this policy—anti-cyclical public works—is to be regarded more in the nature of a palliative to the instability of the system, than its remedy.

V

11. We may now summarise the conclusions of this paper. We have found that there is no *single* factor which brings boom-periods to an end, but there are a number of such factors; and there is no reason to expect that in past history different boom periods were always brought to an end by the same cause.

¹ It could be argued that a small rate of investment, provided it is steady and continuous, might even represent a higher rate of progress than a larger rate which fluctuates. But there is no reason to assume that the smaller rate would be entirely steady.

A level of activity which can be called "full-employment-activity" might come to an end:—

(i) because rising interest rates check investment, on account of the fact that the banking system is unwilling to provide the quantity of cash needed to carry on the high level of activity (the credit-restriction case);

(ii) because rising interest rates put a stop to a process of cumulative inflation (the excess-investment case);¹

(iii) because the demand for consumption goods fails to expand sufficiently, with the increase in the capacity to produce them (the excess-saving case);

(iv) because equipment becomes redundant, owing to the scarcity of labour.

These four causes are likely to appear *successively* in time; the first two in the early stages, the last two in the later stages. Thus a boom is like a peculiar steeplechase, where the horse is bound to fall at one of four obstacles. If it survives the first, it might be checked on the second, the third or the fourth. It is probably a rare horse which survives until the last hurdle.

A wise Government can remove the first obstacle by appropriate monetary policy, and it can remove the second and the third by an appropriate system of taxation. But it can only suspend the operation of the last obstacle by reorganising the distribution of resources between different industries—though it could alleviate its consequences by making public investment anti-cyclical.

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¹ The difference between these two cases was explained in footnote, p. 649, above. In the one case interest rates rise because the *quantity* of cash is insufficient; in the other case, because without a rise in the rates, no amount of cash *could* be sufficient.

UNEMPLOYMENT IN THE THEORIES OF SCHUMPETER AND KEYNES

By E. G. BENNION*

Professor Lange, in his review of Professor Schumpeter's *Business Cycles*, has suggested that "It is not at all clear how the employment cycle can be tied up with Professor Schumpeter's theory."¹ Moreover, one could scarcely fail to garner from Professor Lange's review the impression that cyclical unemployment is not an integral part of Schumpeter's schema. One of the purposes of this paper is to demonstrate that cyclical unemployment is an integral part of Professor Schumpeter's theory and to tie in the employment cycle explicitly with that theory.

Lange has also stated that the real weakness of Schumpeter's theory is ". . . the lack of an adequate theory of employment (in the sense of Mr. Keynes) to serve as a basis for the theory of the business cycle."² Aside from the fact that a theory of employment is obviously unnecessary as a *basis* for a theory of the business cycle, this criticism is perfectly valid.

It is, however, frequently forgotten (often even not recognized) that Schumpeter in his *Business Cycles* and Keynes in his *General Theory of Employment, Interest and Money* are primarily concerned with two quite different things. This tends to foster, at best, the conclusion that the two theoretical schemata are totally unrelated and, at worst, the conviction that the two theories are mutually exclusive. Is it not conceivable that the two are, at least under certain circumstances, complementary rather than antithetical? Does not each theory perhaps fill, under certain conditions, a gap present in the other?

Keynes sets for himself the problem of explaining involuntary unemployment. To solve this problem he proceeds on the assumption of unchanging production functions. His theory is not, however, logically self-contained (although his conclusions certainly retain practical significance) in the sense that his determinants can *indefinitely* continue to take values which yield an involuntary unemployment equi-

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¹ *Rev. of Econ. Stat.*, Vol. XXIII (Nov., 1941), p. 193.

² *Ibid.*, p. 192.

librium only if Schumpeter's theory—or some other dynamic theory of a similar nature—is established as a foundation upon which Keynes's theoretical superstructure can rest.

Schumpeter's essential interest is in developing a theory of the business cycle, changing production functions being the very heart of his theory. Logically his theory is self-contained but it is vastly strengthened by the inclusion of Keynes's technique as a means of explaining a given volume of unemployment *at a given point in time*.

If this is a fair statement of the principal objectives of the two theories, no symbiosis of the two schemata is possible in an economy where "economic progress" has become entirely a thing of the past. If, on the other hand, "secular stagnation" is viewed as "permanent" only in the sense that adequate investment outlets may be lacking for a protracted length of time because the economy is in the depression phase of a Kondratieff, a synthesis of the theories of Schumpeter and Keynes is not only possible but even distinctly helpful from an analytical viewpoint. Such a synthesis is the second purpose of this paper.³

Unemployment in Schumpeter's Theory

Schumpeter's primary concern is with economic evolution, a process which appears to him to unfold (under capitalism) in successive business cycles. He begins his treatment of the cycle with his Pure Model or First Approximation.⁴ This is a two-phase cycle model of prosperity and recession which starts from a stationary economy in which income equals consumption and full employment exists; moreover, the first of these conditions prevails at all successive equilibria, *i.e.*, at the minima of the money income cycle.

In this model two points call for passing notice. First, at least some unemployment is necessarily implicit in the model. Although starting with full employment (and even assuming that a return to full employment is a characteristic of the successive equilibria), it is difficult

³ In this paper Schumpeter's theory is given somewhat more space than is Keynes's and, with reference to the synthesis of the two, I have expressed myself more in the terminology of the former than of the latter. To interpret this as a bias in favor of Schumpeter's theory would be an inaccurate appraisal of my opinion of the two theories. I have followed the course which I have for two reasons: first, it is, after all, the connection between Schumpeter's theory and the employment cycle which has been suggested as nebulous. Second, Keynes's theory may safely be assumed to be more or less common knowledge; indeed, the advantage (although it is also, in some respects, a disadvantage) of the beautiful simplicity of this theory of comparative statics has endeared it to economists and has given even the layman a bowing acquaintance with it. No such assumption can be made about Schumpeter's theory. As a really dynamic theory it defies easy summation and to wade through his two volumes is a formidable task which many economists have not as yet got around to doing.

⁴ *Business Cycles* (New York, McGraw-Hill, 1939), Vol. I, pp. 130-45.

convincingly to dispute that the down-turn would bring with it temporary unemployment. This follows from Schumpeter's position that for some firms the emergence of innovations spells economic death. Several heroic assumptions are made in the Pure Model but none so heroic as to assure instantaneous reemployment by other firms of those factors of production which become unemployed as some firms are put out of business.

Second, the Pure Model demonstrates that a theory of employment is unnecessary as a *basis* for a theory of the business cycle. More precisely, fluctuations in the volume of employment are certainly not devoid of causal influence in the cycle; but they become causal only after having been brought into existence by more fundamental factors. This becomes even more obvious when one passes on to the Second Approximation.

Express recognition is accorded unemployment by Professor Schumpeter in his Second Approximation. He specifically states that "Imperfections of both competition and equilibrium . . . may account for the presence of unemployed resources independently of the cyclical process of evolution. We have not introduced this fact into our *pure model* in order to relieve the latter of . . . secondary elements; but it can now be inserted . . . and be taken account of in any given case which presents them. Besides, since our process itself produces both imperfections of competition and disequilibria which account for under-employment that may outlast the cyclical unit which produced it, we include, by recognizing that every cycle is heir to preceding cycles, *also that this source may contribute to the total unemployment with which any given unit starts. This would have been circular reasoning in the Pure Model, but it meets with no objection now.*"⁸

It is important to realize, furthermore, that the Second Approximation would be little more than the Pure Model under a different name, were cyclical unemployment not introduced. Perfect flexibility of prices, perfect mobility of the factors of production, perfect foresight and perfect competition—assumed in the Pure Model—are not assumed in the Second Approximation. It seems quite obvious that, in a dynamic economy, these four conditions are a *sine qua non* to constant full employment. Only by an extremely rigorous definition of these four conditions is it possible to impute constant full employment even to the Pure Model. It is, therefore, certainly not apparent how the discarding of these four conditions can be reconciled with a constant level of full employment and hence with a cycle in which real consumption and real investment vary inversely at all times. Consequently,

⁸ *Ibid.*, Vol. I, p. 161; italics supplied.

the Second Approximation clearly implies fluctuations in employment; but the fluctuating employment is not the essence of the cycle.

It is, however, one thing to insist that Professor Schumpeter's theory is a realistic one to the extent of recognizing an employment cycle and quite another thing to say that he has an adequate theory of employment. He has much more to say about unemployment than probably most people realize.⁴ But if by "an adequate theory of employment" is meant one by which a given amount of unemployment can be explained with a determinateness and neatness akin to Keynes's, Schumpeter does not have an adequate theory.

An Unchanging Production Function Model

Keynes is little concerned with the cycle; his principal concern is with the development of a technique by which any given state of unemployment can be explained. As such his theory is a brilliant and significant contribution to economic literature. If, however, Professor Schumpeter is guilty of failure to incorporate an adequate theory of employment in his theoretical schema, Keynes is guilty of erecting a masterful superstructure without first establishing some foundation upon which the superstructure can rest; for, by his rigid assumptions, he excludes the capitalistic process which Schumpeter so carefully considers.

As is well known, the Keynesian unemployment equilibrium can be described in terms of his four determinants: the supply of money and the three schedules of liquidity preference, marginal propensity to consume and marginal efficiency of capital. Given these, together with the quantity and quality of labor and capital equipment, the state of technique, the degree of competition and the money wage of the labor unit, the volume of unemployment follows automatically. Thus, in these terms, any point on a cyclically fluctuating curve of money income (or employment) can be neatly explained.

Consider, however, the implications of a sine curve (or any other symmetrical curve) of fluctuating money income and employment—the curve being, for simplicity, devoid of trend—under the assumption of unchanging production functions. This model represents a curve of fluctuating employment, money income and real income, all three moving in the same direction at all times. It differs from Schumpeter's Second Approximation in one important respect: real income is identical at successive inflection points in the unchanging production function model, whereas real income is higher at these points in the Second Approximation.

⁴ Cf., *ibid.*, Vol. II, pp. 509-19, and pp. 561-78.

This is a significant difference in that it emphasizes the highly artificial nature of an unchanging production function model. More specifically, it shows the impossibility of trying to impute to Keynes a theory of the cycle⁷ on the strength of his *General Theory* analysis which does run in terms of unchanging production functions.⁸ This is so because the model is one in which—in effect, by definition—there can be no net investment or disinvestment *for the cycle as a whole*.

That is to say, average real income must be unchanged from cycle to cycle where the cycle is represented by a sine curve of fluctuating employment devoid of trend and possessing the property of an unchanging production function.⁹ But average real income can remain unchanged from cycle to cycle only if net investment for the entire cycle has been zero. Therefore, the investment (and the increase in consumption) in the upper half of the curve must be precisely offset by the disinvestment (and decrease in consumption) of the lower half of the curve; and income must equal consumption at the inflection points.

Given this state of affairs, one must have some misgivings as to the ability of Keynes's determinants to continue to take indefinitely such values as would yield a cycle unless changing production functions are introduced; except, of course, if extra-economic factors (or some endogenous factor akin to the Tinbergen lag) are used as a basic explanation of the cycle. One of two things would almost certainly follow: (1) either the fluctuations in employment would gradually diminish until a stationary state of full employment was reached—a process in which some net investment over each successively narrowing cycle would exist, net investment declining to zero as full employment was reached; or (2) the economy would level off at an employment volume of the height of the inflection points (if wages were rigid downward)—a point at which net investment would also be zero and

⁷ Unless some endogenous factor, such as a Tinbergen lag, is introduced to furnish the impulse; theoretically, a cycle would then be possible—at least for a time.

⁸ This is a fact but it does Keynes less than justice. For, *behind* his analysis is the *idea* of a slowing down of innovation even though, by assumption, he excludes it from the formal body of his theory.

⁹ Strictly speaking, this involves the assumption that income equals consumption—that gross investment is precisely equal to replacement because the marginal efficiency of capital and the interest rate are just equated for that amount of gross investment—at the inflection points. It also implicitly assumes that the interest rate does not fall farther than it already has. These assumptions are not, in this model, so ridiculous as they may at first appear to be; this, because the interest rate cannot fall forever and because, with an unchanging production function, such a position would soon be reached in Keynes's schema once the interest rate ceased to fall. I do not suggest, however, that Keynes believes it likely that cyclical fluctuations around this point, in an unchanging production function world, would follow, although at least one instance could be cited from which one might infer this. See *The General Theory of Employment, Interest and Money* (New York, Harcourt Brace, 1936), pp. 217-18.

at which the unemployed would be subsidized by income recipients. In either event there would be a cessation of the cycle.

It might be added parenthetically that there is no reason to think Keynes would not agree with this conclusion. Indeed, it is largely over the second alternative that he is genuinely concerned. For he wishes to hasten alternative (1)—which embraces full employment—by various measures because he fears that private investment cannot achieve this end alone and because he fears that alternative (2) bids fair to become a permanent condition, consumption being assumed by him to be a dependent variable.

The principal defect of Keynes's theory is, then, that it is insufficiently basic in the sense that the ability of his determinants indefinitely to assume values which will yield an unemployment equilibrium is dependent upon factors the genesis of which is ultimately traceable to whatever it is that makes the economy dynamic. His liquidity preference schedule and his marginal propensity to consume schedule rest almost entirely upon psychological bases; and his marginal efficiency of capital schedule—apparently, at least—rests very largely on the same basis.

Can any other explanation possibly exist for these bases except the fact that the economy is a dynamic one? In a stationary economy as defined by Schumpeter, would there be any point in hoarding out of the speculative motive? Could the effective marginal efficiency of capital schedule outrun, first to the right and then to the left, the true marginal efficiency of capital schedule?¹⁰ Could the true marginal efficiency of capital schedule, in fact, move at all in a stationary economy? It is difficult to see how. If this is true, Keynes has literally conjured into being the frictions so indispensable to his theory. Thinking primarily of the unemployment problem at given points of time, he has worked with unchanging production functions, yet he has, by implication, brought changing production functions in through the backdoor; just as Schumpeter, concentrating principally upon an explanation of what he believes to be the primary explanation of a dynamic economy under capitalism, brings in unemployment as a causal factor in the cycle largely by implication rather than by explication.

The magnitude of Keynes's achievement should not be dwarfed by recognition of this defect. Once one postulates the existence of the cycle or of the frictions, one cannot deny the validity of the technique which he has developed for explaining so clearly any given volume of involuntary unemployment.

¹⁰ That *at least* these two schedules of the marginal efficiency of capital exist in Keynes's mind is evidenced by chap. 12 of *The General Theory*.

The Changing Production Function Model

A method of synthesizing the theories of Schumpeter and Keynes is now quite obvious. We need merely to assume a sine curve of cyclically fluctuating employment, money income and real income, accompanied by changing production functions. In this model all three of the fluctuating elements move in the same direction, with real income showing a rising trend. The model is made dynamic by the innovational element stressed by Schumpeter; and any point on this curve of cyclically fluctuating employment can be explained in terms of Keynes's determinants.

To trace briefly the secondary effects of fluctuating employment in Schumpeter's terminology requires but a moment. Starting at the inflection point of any upswing, we have some given volume of unemployment. Some of this may be the result of monopoly or imperfect competition; some of it will be cyclical and the inheritance from preceding cycles—an inheritance caused by the imperfections and disequilibria of the preceding cycles outlasting those cyclical units. Equilibrium, the existence of conditions favorable once more to further innovation, means entrepreneurial borrowing for the purpose of financing the innovations. Entrepreneurial spending leads, in turn, to increasing employment and the familiar cumulative effects begin to unfold; and all of the other phenomena so carefully traced out by Schumpeter in his Second Approximation follow.

Ultimately, temporary saturation of certain markets sets in; it becomes increasingly difficult to plan new things and the risk of failure increases greatly; hence autodeflation ushers in recession. Unemployment now flows from the effects of the innovations on some of the old firms and from the abnormal liquidation of the Second Approximation. In this fashion the economy is forced down below an equilibrium neighborhood and into the depression phase. Moreover, secondary though the rôle of unemployment is in Schumpeter's analysis, once brought into existence by more fundamental factors it does become causative. For a time, therefore, unemployment feeds upon itself, each increment to unemployment begetting further unemployment.

Because the depression phase is below equilibrium, however, revival begins as the economy starts to grope its way back up to equilibrium. This it does once the depressive phase stops of its own accord, and Schumpeter argues that the depressive phase will stop of its own accord because of what he calls "diffusion of effects" and "depression business."¹¹ This, in turn, means a return to equilibrium accompanied

¹¹ Among other things, he mentions unemployment in this respect: "Each addition to unemployment will cause further and further unemployment but, *taken individually*, at a decreasing rate." *Business Cycles*, Vol. I, p. 135.

by rising money income and employment until the previous level identified with the inflection points of our sine curve is reached. At this point a cycle is completed.

This process can be described in Keynesian terminology, but it is easier to do so by starting at the trough of a cycle. Excessive optimism in the preceding cycle having resulted in abnormal liquidation and its concomitant depressive factors, each trough constitutes a point below equilibrium. As the "diffusion of effects" and "depression business" make themselves felt, investment ceases to fall, reverses its trend and increments to investment become larger and larger. In Keynesian terminology we could say that these factors which encourage a rise in investment have shifted the marginal efficiency of capital schedule to the right. At equilibrium (the inflection point), the schedule begins to move even farther to the right under the stimulus of innovation. With a stable consumption function we have some given multiplier to apply to these successive increments to investment.¹² Thus consumption also has larger and larger increments which must be added to the increments of investment in order to obtain money income.

The downswing is begun by a collapse of the marginal efficiency of capital schedule¹³ and investment begins to fall, the increments to investment now becoming smaller and smaller. Also, applying our constant multiplier to these increments of investment, the increments to consumption become smaller and smaller (although there is, of course, a lag between consumption and investment increments) until investment ceases to fall.¹⁴

This is the briefest of statements of the changing production function model in Keynesian terminology. It deals with but two of Keynes's four determinants: the consumption function and the marginal efficiency of capital schedule. This is done to emphasize the rôle played by innovation (changing production functions) as the prime factor behind the movements of the marginal efficiency of capital. Our statement could now be made more complete by the inclusion of changes in

¹² The acceleration principle is purposely ignored here. In part, I have done so because Keynes considers only the multiplier—quite legitimately for his purpose. Mostly, however, I have ignored it because assumption of a cycle of spontaneous investment in conjunction with a constant multiplier and a constant value for the acceleration coefficient yields not one cycle but short cycles within a longer cycle. This interesting phenomenon to which I have as yet found but one exception appears not to have been noted heretofore.

¹³ This is Keynes's tentative explanation for the down-turn. See chap. 22 of his *General Theory*.

¹⁴ A. H. Hansen, in his *Fiscal Policy and Business Cycles* (New York, Norton, 1941), pp. 289-92, has a special case of this nature in which net investment falls to zero in the troughs. But this is not a necessary requirement, even to the Keynesian unemployment equilibrium. The limited data we have show only one such occurrence for annual data; and in that case, net investment fell to a minus figure. See S. Kuznets, *National Income and Capital Formation, 1919-1935* (New York, Nat. Bur. Econ. Res., 1937).

the supply of money and of shifts in the liquidity preference schedule as they follow the movements of the marginal efficiency of capital schedule.¹⁵ The nature of these movements is, however, sufficiently obvious to make this unnecessary; and in tracing these one would note further similarities between the two theories, such as the fact that Schumpeter's non-spending performs much the same function in the downswing as Keynes's liquidity preference and/or propensity to consume.

The Bases for Unemployment in the Two Schemata

It is now clear that a synthesis, which takes Schumpeter's changing production functions as the power behind spontaneous investment and which incorporates Keynes's technique as a means of explaining the volume of unemployment existing at a given point in time, is possible only under the assumption that economic progress is not yet dead. If that assumption is dropped, the two schemata are irreconcilable. To drop that assumption is to define Schumpeter's scheme out of existence.¹⁶

On the other hand, if we admit the existence of economic progress, any short cycle situation can be described in Keynesian terminology, whatever the amount of unemployment in a particular case. In itself this is not a particularly important observation since any theory can be described in terms of Keynes's technique about as well as Schumpeter's can. But if we take a short cycle in which unemployment is relatively large—in the Schumpeter schema, a Juglar in the depression phase of a Kondratieff, which certainly approximates more closely what Keynes visualizes than does a cycle in which unemployment is inconsequential—we are better able to examine what lies behind unemployment in the two schemata. We shall find that the bases for unemployment are essentially the same. This is an observation of some importance and it does much to strengthen the conviction that the two theories are compatible.

Since Schumpeter does not expressly stipulate in his theory what the fundamental reason for unemployment is, it is necessary to examine

¹⁵ We should not forget, however, that even then we should not have the Keynesian theory of the cycle. Unless one chooses the doubtful course of identifying Keynes with one of the various theories of the cycle advanced by his followers, it is no exaggeration to say that no one really knows what Keynes's theory of the cycle is. He identifies himself (chap. 22 of his *General Theory*) with the underconsumption theory; but this is a theory of crisis, "secular stagnation," or what you will; it is not a theory of the cycle.

¹⁶ This would mean, ultimately, either a reversion to Schumpeter's stationary economy or, if one believes Keynes's determinants to be so independent of frictional factors that they could exist indefinitely in an unprogressive economy, it would mean a stationary economy of less than full employment.

the conditions requisite to the direct variation between real income and money income, typical in short cycles embracing fluctuating employment.

The first condition for this direct variation is obviously the existence of some unemployment; for real income cannot fluctuate in the same direction as money income at all times, unless employment can move in the same direction as both of the former.¹⁷ Secondly, we need superimposition of one cycle upon another in the manner of Schumpeter's Third Approximation; for the existence of under-utilization is largely traceable to this superimposition.

Consider what the Kondratieff cycle underlying the Juglar means in Schumpeter's theory. Clearly innovations may have different periods of gestation and absorption of their effects; also successive cyclical units are not necessarily completely independent of each other; and, finally, cyclical units may result from processes which have effects other than those which show up in the cyclical units themselves.¹⁸ For these reasons the shorter cycles more or less run their courses upon the backs of the longer cycles.

What does this mean with respect to our two conditions for direct variation between real income and money income? It means that, although both conditions are probably nearly always fulfilled, we should expect the degree of fulfillment to vary substantially over time. It does so vary—with formidable unemployment in the depression phase of the Kondratieff and with little unemployment in the prosperity phase of the Kondratieff. In different words, we should expect the tendency for real income and employment to move in the same direction as money income during Juglars to be more pronounced during the downswing of the Kondratieff than during the upswing of the Kondratieff; this, because the volume of unemployment with which any given Juglar starts ought to be, according to this reasoning, greater in the downswing of the Kondratieff than in the upswing of the Kondratieff.

We can now proceed to the real bases of unemployment in the schemata of Schumpeter and Keynes. Cyclical unemployment to Keynes is not what is generally called "frictional" unemployment. If this appears to be an unnecessary observation, let it be noted that to Schumpeter cyclical unemployment is, by his own words, frictional; hence, unless one carefully considers the sense in which each uses this word, misunderstanding can easily arise.

Strictly speaking, the basis of cyclical unemployment in Keynes's theory is inextricably bound up with all four of his determinants.

¹⁷ This does not mean that, given fluctuating employment, employment and real income must *at all times* fluctuate in the same direction.

¹⁸ For a detailed description of these three factors, see *Business Cycles*, Vol. I, pp. 166-68.

Greatest emphasis, however, clearly should be placed upon the propensity to consume. More precisely, an involuntary unemployment equilibrium is possible, essentially because of the prevalence of two conditions: (1) the consumption function is relatively stable for "short" periods of time, and (2) private investment cannot always be counted upon to fill the gap, required to be filled for full employment, left by a propensity to consume schedule which has a slope smaller than unity.¹⁹

The basis of the propensity to consume is predominantly psychological in character and Keynes submits an exhaustive list in this respect.²⁰ In a stationary economy, much of this list would disappear as contributing factors to net social saving, even though the interest rate were positive. If one concedes the validity of Schumpeter's contention that the interest rate would be zero in a stationary economy,²¹ all of the list would disappear as factors contributing to net social saving. This, however, is really irrelevant since we are proceeding under the assumption of a dynamic economy; and, given a dynamic economy, Schumpeter would certainly not deny the importance of the factors which Keynes has listed as the reasons for the propensity to consume less than the whole of an increment to income. Overlooking purely terminological differences with respect to the definition of saving, there is no really important difference in the reasons for saving as between Schumpeter's schema and Keynes's schema.

Before turning to the basis of unemployment in Schumpeter's theory, we should note two other things about Keynes's theory. First, although it is true that any point on a curve of fluctuating employment can be described in his terminology, the very fact that employment is changing means that we do not have a Keynesian equilibrium. Instead, although Keynes's analysis is really a timeless one, employment would have to be unchanging, during some time, for equilibrium to exist.

Secondly, although I think many of us have fallen into the habit of thinking this is not so, Keynes does *not* insist that his determinants can indefinitely assume values such as would yield an involuntary unemployment equilibrium. Among others, two examples may be cited: (1) with respect to the propensity to consume, he says that "Over a

¹⁹ The first condition is made subject to certain qualifications, most of which Keynes regards as unimportant, and the elimination of changes in the wage-unit in terms of money is necessary; but, in general, the consumption function is viewed as relatively stable. (See *The General Theory*, pp. 89-98.) In the event of a collapse of the marginal efficiency of capital schedule, the second condition follows almost without regard to the supply of money and liquidity preference because of the shape of the schedule of the latter, *i.e.*, the fact that it becomes almost perfectly elastic at a low level of interest rate. (*Op. cit.*, chap. 15, especially, sec. II.)

²⁰ *Ibid.*, chap. 9.

²¹ *The Theory of Economic Development* (Cambridge, Harvard Univ. Press), chap. 1.

long period substantial changes in the rate of interest probably tend to . . . [affect] the subjective propensity to spend . . ."²² and (2) he suggests that the money supply and the liquidity preference schedule will have the effects which he ascribes to them "*unless reasons are believed to exist why future experience will be very different from past experience. . . .*"²³

Professor Schumpeter calls his cyclical unemployment frictional; but it is not frictional as that word is generally used. This follows from the fact that cyclical unemployment and technological unemployment are, to him, exactly the same thing—technological unemployment being taken to mean all unemployment arising from innovational disturbance, including the effects of autodeflation. Cyclical unemployment is thus frictional in the sense that it would be stillborn in an economic system which possessed powers of instantaneous adaptation. But, recalling what we have said about the reasons for shorter cycles running their courses upon the backs of longer cycles, it is all too evident that the process of adaptation may, at times, be a very long one indeed.

What, then, is the real basis for cyclical unemployment in Schumpeter's theory? It is quite apparent that, as in Keynes's schema, the basis is that the process of adaptation has not yet progressed to the point once more sufficiently favorable to investment to employ all those who wish to work at the prevailing wage rate. But this is not possible unless consumption refuses to fill the gap left by declining investment. That is to say, Schumpeter's theoretical structure implicitly assumes that consumption is a dependent variable and that the consumption function is, for a fairly long period, relatively stable.²⁴ This is not only not inconsistent with his theory; it could even be argued rather plausibly that these assumptions depend, for their validity, upon a theory similar to his.

²² *Ibid.*, p. 93; he adds, however, that it is difficult to say in which direction the subjective propensity to spend might move.

²³ *Ibid.*, p. 202; italics supplied.

²⁴ I am sure, however, that Schumpeter regards the consumption function as being less stable than it is in Keynes's view. The pertinent fact is that Schumpeter would readily agree that there is some point below which the consumption function would tend not to fall, despite the tendency for it to rise in the upswing under certain circumstances (particularly under the circumstance of widespread speculative gains).

THEORIES OF EFFECTIVE DEMAND AND EMPLOYMENT¹

LAWRENCE R. KLEIN

THERE is much talk about such matters as the downward rigidity of wage rates, the relationship of wages to employment and output, the influence of liquid assets on the level of economic activity, and the stage of maturity of the American economy. The various theories of employment must be examined in the light of these concepts in order to get some clear answers to important economic problems. The Keynesian theories are often accused of being based on assumptions of rigid wage rates or interest-elastic liquidity preferences, but there may be much less truth in these assertions than is commonly thought to be the case. The purpose of this paper will be to study three theories of employment—(1) the classical, (2) the Keynesian, and (3) the Marxian—in order to attempt to clear up some confusions that still exist. One of the main objectives will be to try to show the distinctions between necessary and sufficient assumptions that underlie each theory.

I. THE CLASSICAL THEORY

Since the publication of the *General Theory*, there have been numerous discussions in the professional literature comparing Keynes and the Classics. As a result of these discussions, we now have a good idea as to the form of the classical model. The simplest version is as follows: (1) The supply of and demand for labor

determine the real wage rate and the level of employment. (2) The technological input-output relationship determines the level of real output since the input of labor services has been determined by step 1. It is, of course, assumed that the stock of fixed capital is given. (3) The equation of savings and investment determines the rate of interest. (4) Given output from step 2, the constant velocity of circulation and the given supply of cash determine the absolute price level (quantity theory).

The mathematical version of this system is

$$M = k p Y \quad (\text{quantity equation}), \quad (1.1)$$

$$S(i) = I(i) \quad \left. \begin{array}{l} (\text{savings-invest-} \\ \text{ment equation}), \end{array} \right\} \quad (1.2)$$

$$Y = Y(N) \quad \left. \begin{array}{l} (\text{production} \\ \text{function}), \end{array} \right\} \quad (1.3)$$

$$\frac{dY}{dN} = \frac{w}{p} \quad (\text{demand for labor}), \quad (1.4)$$

$$N = f\left(\frac{w}{p}\right) \quad (\text{supply of labor}), \quad (1.5)$$

where M = cash balances, p = price, Y = output, i = interest rate, N = employment, w = wage rate. Given the amount of money, there are five equations to determine p , Y , i , N , and w .

The classical economists not only counted relations and variables; they also assumed that the forms of their relations were such that a unique solution was possible. This solution will always be one of full employment because all who

¹ Some of the ideas on Keynesian economics contained in this article are more fully discussed in the author's forthcoming book, *The Keynesian Revolution* (New York: Macmillan Co.).

want to work at the going real wage rate can find a job; equation (1.5) tells us that. This equation shows how much employment will be offered at any real wage rate. If all the equations of the system are consistent, as was classically assumed, equation (1.5) must hold, i.e., all who offer their services at prevailing real wages can find employment. In this model, since all equations hold simultaneously, the solution must be on the supply curve of labor, which is what is meant by full employment.

It is easy to make a slight generalization of this model and still get the same results. Those defending the classical doctrine against Keynes's 1936 attack were quick to point out that the classical economists did not neglect the fact that the demand for money depends on the rate of interest or that savings and investment depend on income. The same results, so far as the level of employment is concerned, follow even if the quantity equation and the savings-investment equation are modified. Steps 1 and 2 of the process of solving the classical model remain as before. Steps 3 and 4 become: (3') Given the level of output from step 2, the equation of savings and investment determines the rate of interest. (4') Given the level of output from step 2 and the level of the interest rate from step 3', the given supply of cash determines the absolute price level.

Equations (1.1) and (1.2) are replaced by

$$\frac{M}{P} = L(i, Y), \quad (1.1')$$

$$S(i, Y) = I(i, Y). \quad (1.2')$$

The other equations remain as before.

As presented here, the classical system is static and should be looked upon as the equilibrium solution of a more general

dynamical system. It is evident that the equilibrium will always be one of full employment. In the general case—when the system is not at its equilibrium position—there may be unemployment, but this unemployment will be only temporary if the dynamic movements are damped, as the classical economists implicitly assumed. When unemployment does occur in the state of disequilibrium, there is always an appropriate remedial policy available—namely, an increase in the amount of money or (its equivalent) a cut in prices or in wages. Every variable in the classical system can be expressed in terms of the autonomous supply of money as a parameter, and it is easy to calculate the effect upon the system of varying the quantity of money. The assumptions of the structure of the classical system are such that variations in the quantity of money tend to raise the level of output and employment when there is a deviation from the full-employment equilibrium.

II. THE KEYNESIAN THEORY

The Keynesian theory is quite different from the classical theory. The basic hypothesis of the Keynesian theory is that people make two kinds of decisions in our present type of economy. They decide, on the basis of their income, whether to spend or save; and they decide, on the basis of the rate of interest, the form in which they want to hold their accumulated savings—cash or securities. In the classical theory income is the strategic variable in the money equation (1.1), and interest is the strategic variable in the savings-investment equation (1.2). Exactly the reverse is true in the Keynesian system. Keynes's great contribution was to replace the classical savings-investment theory of interest with a savings-

investment theory of the determination of income.

The simplest Keynesian theory is the following: Savings as a function of the level of income equals autonomous investment. This is one equation in one variable, namely, the level of income. Investment is considered to be autonomous because it depends upon such factors as the expectations of future market demand, innovations, fiscal policy, etc. It is obvious, however, that the validity of the Keynesian theory does not depend on the fact that investment is autonomous, for, if investment is also a function of income, the Keynesian theory of the savings-investment determination of the level of income still holds.

One pillar of support for the simplest Keynesian model is that it is not contradicted by the data. If the hypothesis is that savings as a function of income equals autonomous investment, there should be a close correlation between income and investment. The published data (United States) on disposable income (constant dollars, per capita) are very highly correlated with investment—defined as the difference between disposable income and consumer expenditures (constant dollars, per capita)—and lagged disposable income during the interwar period. There is nothing artificial in this high correlation, and statisticians have never found a similar confirmation of the alternative classical theories from the available data.

The Keynesian revision of the savings-investment theory is of profound importance. Since the Keynesian theory does not involve the introduction of any new variables and since it merely involves a change of form of some of the classical equations, it would seem natural that the system (1.1)–(1.5) could be re-written with the suggested revisions, so that we would again have a model of full-em-

ployment equilibrium. However, this supposition is not correct. The revised model would be

$$\frac{M}{P} = L(i) \quad \left. \begin{array}{l} \text{(liquidity-preference} \\ \text{equation)}, \end{array} \right\} (2.1)$$

$$S(Y) = I(Y) \quad \left. \begin{array}{l} \text{(savings-investment} \\ \text{equation)}, \end{array} \right\} (2.2)$$

$$Y = Y(N) \quad \left. \begin{array}{l} \text{(production} \\ \text{function)}, \end{array} \right\} (2.3)$$

$$\frac{dY}{dN} = \frac{w}{P} \quad \text{(demand for labor)}, (2.4)$$

$$N = f\left(\frac{w}{P}\right) \quad \text{(supply of labor)}. (2.5)$$

There is a basic contradiction and indeterminacy in this system. The supply of and demand for labor, plus the production function, determine the level of output. But the savings-investment equation also determines the level of output, and there is no obvious mechanism to insure that these two levels of output will be the same. Furthermore, the liquidity-preference equation cannot determine both the price level and the rate of interest.

There are various ways out of the difficulties that arise in the system (2.1)–(2.5). The liquidity-preference and savings-investment equations can be generalized; the supply-of-labor equation can be changed; or possibly other changes may be suggested. It should be pointed out, however, that there is little that can be done to either the production function or the demand for labor. The production function cannot be changed, because it is a technological phenomenon. The laws of nature cannot be tampered with, while the hypotheses of economic behavior can. Many empirical studies have shown that the aggregate production function can

be closely approximated by a linear-logarithmic relation. From the theories of profit maximization it follows that a linear-logarithmic production function implies a demand equation for labor such that the wage bill is proportional to the aggregate value of output. This constancy of labor's share of the national product is precisely what the data show. In dynamic econometric models this relation can be improved by saying that the wage bill is a linear function of the value of current output, lagged output, and a time trend. A demand equation for labor, of this generalized dynamic type, can be easily derived from empirical production functions. In various econometric models that the author has constructed, there is no relation that is more stable than the demand for labor; hence it seems unwise to attempt to clear up the theoretical difficulties of the above model by altering (2.3) or (2.4). We must concentrate our attention on (2.1), (2.2), and (2.5). This is precisely the Keynesian approach.

If the generalized forms of the money equation and the savings-investment equation presented in the previous section—(1.1') and (1.2')—were substituted for (2.1) and (2.2), the Keynesian theory would appear to be coincident with the classical theory. But such a conclusion would be hasty. Suppose that (2.1) and (2.2) are replaced by (1.1') and (1.2'). If there was formerly a contradiction between the level of output determined from one part of the model, (2.3)–(2.5), and from another part of the model, (2.2), a classical economist would argue that the contradiction is now avoided because the interest rate would adjust itself so that investment would offset savings out of the same income that is determined by (2.3)–(2.5). But, according to the Keynesian theory, an interest-rate adjust-

ment is not generally possible. There is no assurance that the equation

$$S(i, Y_0) = I(i, Y_0) \quad (2.6)$$

has a solution in $i > 0$ when Y_0 is the full-employment level of income determined from (2.3)–(2.5). In fact, if savings and investment are both interest-inelastic, the chances are very great that there will be no solution to this equation. Interest-inelasticity of these schedules is one of the fundamental assumptions of modern Keynesian theory. The extreme case occurs when i is omitted as a variable from the savings and investment schedules. Econometric and questionnaire investigations have always shown the influence of the interest rate on savings and investment to be small or absent; it remains for the opponents of Keynes to show that there is high interest-elasticity in these schedules.

One of the main reasons why savings are interest-inelastic is that some savings respond positively to variations in the interest rate (savings for wealth accumulation), while other savings respond negatively to variations in the interest rate (savings for annuities). On balance, the total effect is in doubt in regard to sign. In the modern society, savings are regulated largely by habits and considerations of economic security and have little to do with the rate of interest.

On the side of investment, it is well known that businessmen make capital outlays on the basis of a very short horizon (one to five years) and that the shorter the horizon the smaller is the effect of interest rates.² Furthermore, the increased use of internal financing—coupled with a failure to charge imputed interest

² See G. L. S. Shackle, "Interest Rates and the Pace of Investment," *Economic Journal*, LVI (1946), 1-17.

—have intensified the neglect of the interest rate in the formation of investment decisions. These are two of the main reasons why the investment schedule is interest-inelastic.

There is a method of assuring a full-employment solution to the system, although it is highly artificial and unobserved in the real world. Professor Knight has suggested that the investment schedule be made infinitely interest-elastic. If this were assumed, there would always be full-employment equilibrium. Knight has written, "The heart of a correct theory of interest is the fact, corresponding more or less to infinite 'elasticity of demand for capital,' that the investment market is capable of absorbing savings at the maximum rate at which they are forthcoming, . . ." ³ If the investment schedule possessed infinite interest-elasticity, equation (2.6) would always have a solution and the contradiction would be solved. However, Knight's assumption—which comes to exactly the same thing as Say's law—is untenable in the light of statistical data or any other knowledge that we have of the facts in the savings-investment market.

Supporters of Knight's views on capital theory may point out that the foregoing quotation applies only to a long-run situation. In this event, the term "elasticity of demand for capital" must take on a new connotation. Elasticities are ordinarily computed as logarithmic partial derivatives, which means that other variables are held constant. In the long run these other variables are not constant. If Knight is referring only to long-run processes in the quotation, his remarks are not related to the problem that we are discussing.

³ Frank H. Knight, "Capital, Time, and Interest Rate," *Economica*, N.S., I (1934), 285.

Professor Pigou⁴ was one of the first classically minded economists to point out clearly that the amended system may be overdetermined with the added condition $i > 0$. Pigou acknowledged that savings and investment may be sufficiently interest-inelastic that the interest rate cannot be relied upon to bring them into balance at full employment. He suggested a further alteration in the savings-investment equation in order to salvage the classical doctrine of full-employment equilibrium. His suggestion would mean replacing the savings-investment equation by

$$S\left(i, Y, \frac{M}{p}\right) = I(i, Y), \quad (2.2')$$

with the assumption that savings vary inversely with the real stock of cash.⁵ The solution to an unemployment disequilibrium is now obvious. If wages are cut with M held constant by the banking system, M/p can be pushed to sufficiently high levels so that savings and investment are in balance at full employment. Since prices (equally well, wages) enter as a denominator in real cash balances, there is no limit to the size of M/p as a result of wage cuts and hence no limit to the extent to which savings can be lowered.⁶ Thus, by always restoring the system toward its full-employment equilibrium, competitive wage cuts during

⁴ A. C. Pigou, "The Classical Stationary State," *Economic Journal*, LIII (1934), 343-51.

⁵ Other economists, notably Professor Haberler, have made the same suggestion, although none has been so explicit as Pigou.

⁶ The "real" models of this paper have been constructed in terms of the price level, p , as a deflator, but we could just as easily have constructed the system in wage units with w as the deflator. If the system is written in wage units, the appropriate variable for (2.2') is M/w . This form makes it possible to see more directly how wage cuts are used as a lever to raise the level of real balances.

periods of unemployment solve the problem for Pigou.

Equation (2.2') rests on an unconfirmed hypothesis, namely, that savings vary inversely with the real stock of cash balances. Just as the classical assumptions about the influence of interest rates on savings and investment have never been discovered to hold empirically, so has it never been discovered that consumption or savings patterns are significantly influenced by the stock of cash balances. The data of the interwar period show that cash balances, at best, had a very mild influence on consumption. If we adopt the following simple model for purposes of statistical investigation,⁷

$$\left. \begin{aligned} S &= a_0 + a_1 Y + a_2 Y_{-1} + a_3 \left(\frac{M}{p} \right)_{-1} \\ &= I = \text{autonomous} \end{aligned} \right\} (2.7)$$

or

$$Y = \frac{-a_0}{a_1} - \frac{a_2}{a_1} Y_{-1} - \frac{a_3}{a_1} \left(\frac{M}{p} \right)_{-1} + \frac{1}{a_1} I,$$

the least-squares estimates of the parameters are

$$\begin{aligned} Y &= 186.53 + .30 Y_{-1} + .13 \left(\frac{M}{p} \right)_{-1} \\ &\quad (.13) \quad (.10) \\ &\quad + 2.36 I. \\ &\quad (.34) \end{aligned}$$

The standard error of the estimate of $1/a_1$ is relatively small, .34. On the other hand, the standard error of the estimate of a_3/a_1 is relatively large. The coefficient of M/p could easily be close to zero, but since $1/a_1$ is definitely not zero, it follows that a_3 could be zero. Pigou's hypothesis is not confirmed. Even if the true value of a_3 is not zero, it may not be

⁷ All variables are per capita in 1935-39 dollars. The time period is 1922-41. The figures in parentheses below the estimated parameters are standard errors of the estimates. Y = disposable income, S = personal savings, I = net investment, M = total cash balances (current dollars).

very large. The main point, however, is that the size (and sign) of a_3 is very uncertain. There is no "proof" of Pigou's hypothesis.

The size of the coefficient relating savings to cash balances is very important for Pigou's theory. Recall that the systems of this paper are regarded as equilibrium solutions of more complex dynamical systems. The classical theory implicitly assumes that the system returns rapidly to its equilibrium when it is displaced to a position of disequilibrium. This implies that the dynamical system is damped. But do wage-and-price cuts always lead to damped processes in time? In order to insure that the classical assumption of dampening is correct, it will be necessary to assume that a small cut in wages, for example, will tend to restore the system immediately to its position of equilibrium. Thus it is required that the multiplier effect of wage cuts (or increases in the real stock of cash) be very large. The statistical calculations of (2.7) do not show this. There exists the possibility, but not the necessity, that the increase may be practically zero. Instability may develop in a model like this. There is an initial position of unemployment. Wages fall, but employment and income increase little or not at all. Wages fall still further, but unemployment is still not eradicated. This is a perfect setting for expectations of further wage cuts, the very conditions that make the system unstable and make it likely that wage cuts will push the system away from rather than toward its full-employment equilibrium.

If there are expectations of falling wages, entrepreneurs will postpone production until a time when labor costs will be lower yet. Wage-earners will feel very insecure and spend as little as possible. Hyperdeflation will never cure unem-

ployment. The only way that unstable situations of hyperdeflation can be stopped is by direct, autonomous action on the part of the state or some other authoritative agency, as was the case in the period 1929-33 in the United States. Admittedly, the process of hyperdeflation is the worst set of circumstances that can arise in Pigou's system, yet—on the basis of the available data—an assumption of such unfavorable conditions is legitimate even though other assumptions can safely be made also. The problem, as yet, remains unsettled.

In the most general model—in which the savings-investment equation is (2.2') and the liquidity-preference equation is (1.1')—the expression for the rate of change of real income with respect to real cash balances is more complicated. Without going into the mathematics of this expression, it is possible to present certain results on an intuitive basis. If savings are insensitive to variations in i and M/p and if investment is insensitive to variations of i , then it will follow that real income will not be greatly stimulated by increases in real cash balances. These are the properties of the savings-investment equation that have already been discussed in the preceding pages. The conclusion about small variations in real income associated with variations in real balances is *reinforced* if we appeal to the Keynesian assumptions about the shape of the liquidity-preference equation. Keynes put forth the hypothesis that the demand for cash is infinitely elastic with respect to the interest rate in the neighborhood of low interest rates. Some economists have singled out this hypothesis of Keynes as his strategic assumption which is necessary for the validity of his theories. The truth of the matter is that high interest-elasticity of liquidity preferences is sufficient in many

cases but never necessary. The validity of the theory of employment does not depend on the validity of the assumption about the form of liquidity preferences. It is obvious that the simplest version of the Keynesian theory (savings as a function of income equals autonomous investment) has nothing to do with the theory of interest.

It is instructive to examine the empirical relationship between the interest rate and cash balances to see whether or not the Keynesian hypothesis is correct. If we identify active cash balances as circulating currency plus demand deposits, and idle cash balances as savings deposits, we find for the interwar period very strong linear correlations (*a*) between active balances, net national product and trend and (*b*) between idle balances, corporate-bond yield, lagged corporate-bond yield, lagged idle balances, and trend. The data also show that the corporate-bond yield is not a statistically significant variable in *a* and that net national product is not a statistically significant variable in *b*. These latter findings imply that the empirical split between active and idle balances is not bad.

The fact that idle balances are linearly related to the interest rate in the interwar period implies that the Keynesian hypothesis of infinite elasticity cannot be correct. But the postwar data show something different. The current data are consistent with Keynes's hypothesis. While the interwar demand relation for active balances is close to the postwar facts, the interwar demand relation for idle balances gives a computed level of idle balances, for observed interest rates, much lower than the actual level. There are several explanations for the breakdown of this empirical function in the postwar years. One explanation is that the whole relation has shifted. Another explanation

is that some variable, which was relatively unimportant in the past, is now important and accounts for the discrepancy. A third explanation, which is very appealing, is that the Keynesian hypothesis is correct. If the liquidity-preference function were approximately linear for interest rates above 3 per cent and asymptotic to the line, interest rate = 2.5 per cent, it would fit the interwar data, the post-war data, and the Keynesian hypothesis. There are a variety of simple mathematical functions which have the required properties.

The intuitive significance of the various assumptions about interest-elasticities can be summed up briefly. Assuming that the mechanism to maintain full-employment equilibrium is a fluctuating stock of real balances, it follows that these fluctuations will have little influence on the interest rate if the liquidity preferences are highly elastic, and it follows further that they will have little influence on savings and investment if these schedules are interest-inelastic. It may seem that much weight is attached to the interest rate, but the opposite is the case. The complex of elasticities assumed in the Keynesian theory makes the interest rate extremely unimportant. The same results can be obtained by altogether dropping interest as an independent variable from the system.

The other available alternative by which the contradictions of the system may be reconciled is the modification of the supply curve of labor. This is the alternative that Keynes chose for himself. Before discussing this alternative, however, several points should be made clear. We have been able to demonstrate a basic contradiction in the working of the capitalist system when the traditional supply curve of labor is used. The recognition of this contradiction represents a

great step forward in economic theory, and this contribution has nothing to do with any special assumptions about wages. The truly important ideas of Keynes, contrary to much of popular belief, are independent of any special assumptions about the labor market. Keynesian theories of the savings-investment process superimposed on the classical theory of the labor market show that full employment is not automatic under capitalism.

Keynes recognized that full employment was not the equilibrium position for the real world, and he set about to develop a theory of an unemployment equilibrium by changing the classical supply curve of labor and by adopting a new definition of unemployment. It is this part of his theory that many of the modern Keynesians would like to give up while still retaining the savings-investment theory of income determination. The strict Keynesian approach amounts to replacing (2.5) by

$$N = F(w) \quad (2.8)$$

and adopting the well-known definition of involuntary unemployment found in the early pages of the *General Theory*. It is assumed that the new supply curve of labor has infinite wage-elasticity up to the full-employment point. This system is rigged to get an unemployment equilibrium as much as the classical system is rigged to get a full-employment equilibrium. Neither approach is entirely acceptable.

There are at least two criticisms of the Keynesian solution. In the first place, Keynes's definition of unemployment has the unsavory implication that the cause of unemployment is a money-illusion on the part of workers; if workers would only bargain in terms of real wages instead of in terms of money wages, there

would be no problem of unemployment, other than the frictional variety. Surely, a small thing like a money-illusion cannot be responsible for the existence of unemployment. Second, the supply curve of labor given by (2.8) has never been tested against the facts and may not hold if it is tested. The behavior patterns of recent years (since the Little Steel Formula) give the impression that workers do not bargain exclusively in terms of money wages. They are very conscious of the relation between wages and the cost of living, and it does not seem correct to assume that they are fooled by any money-illusion. Many of the parts of the Keynesian system have withstood the test of being consistent with observed data, but all that we can say about equation (2.8) is that we do not know about its validity. It must be re-emphasized, however, that the important parts of the Keynesian theory are independent of Keynes's own theories of wages and the labor market.

Joan Robinson has made a very important remark that holds the key to an answer to the problem. She said: "Again, the orthodox conception of wages tending to equal the *marginal disutility* of labour, which has its origin in the picture of a peasant farmer leaning on his hoe in the evening and deciding whether the extra product of another hour's work will repay the extra backache, is projected into the modern labour market, where the individual worker has no opportunity to decide anything except whether it is better to work or to starve."⁸ The essence of capitalism is that there exists a definite legal respect for private ownership of the means of production. The owners of the means of production, the capitalists, make all the final decisions with regard to the use of the means of produc-

tion. The workers have nothing to say about the amount of employment that will be forthcoming at any point of time. Either the entire concept of the supply curve of labor must be dropped, or the supply curve of labor must become a curve of *virtual* points on which observations do not occur. The first alternative means that the demand for labor is given by profit maximization (marginal-productivity theory); the supply of labor is an exogenous variable represented by the labor force and determined by demographic factors; the wage rate is determined by a market adjustment between demand and supply (collective bargaining). The mathematical model would be

$$\frac{dY}{dN} = \frac{w}{p} \quad (\text{demand for labor}), \quad (2.4)$$

$$\bar{N} = \text{labor supply}, \quad (2.9)$$

$$\frac{d\left(\frac{w}{p}\right)}{dt} = g(\bar{N} - N). \quad (2.10)$$

Equation (2.10) could be replaced by

$$\frac{dw}{dt} = h(\bar{N} - N) \quad (2.10')$$

if all the other equations of the system are used also. The same arguments about expectations and damping apply to the path by which this system approaches or diverges from equilibrium. If the system is damped and $g(0) = 0$ or $h(0) = 0$, we have a model of full-employment equilibrium.

It was pointed out above that equation (2.4) is based on sound empirical verification. Similarly, market adjustment equations like (2.10') are also consistent with the data. First differences in the general wage rate (U.S.A., interwar period) are highly correlated (inversely) with unemployment and the lagged wage rate. The parameters of this empirical

⁸ Joan Robinson, *An Essay on Marxian Economics* (London: Macmillan & Co., 1942), pp. 2-3.

equation suggest that small wage cuts are not associated with large increases in employment and that $h(0) \neq 0$, from which we conclude that the system does not have a stable equilibrium of full employment.

If the concept of a supply curve of labor is to be retained, it must be interpreted in a new way. We can say that the supply curve of labor shows how much the people would *like* to work at any given real wage. It does not mean, as in the classical system, that people's desires become effective. In this situation the supply curve of labor exists as a set of virtual points which are never observed. However, it is known that the demand curve for labor represents a set of observed points. This means that we shall have an observed point on the demand curve and off the supply curve. If this point is such that supply exceeds demand (at the same wage) there is unemployment, and if this point is such that demand exceeds supply (at the same wage) there is overemployment. This concept of unemployment is not easily measurable, however, since it involves virtual, unobserved points. In order to measure unemployment in this model, we would have to sample the population, questioning them on the amount of employment that they would like to supply at prevailing wage rates.

Thus far we have attempted to point out the main differences between Keynesian and classical economics. But there is also an important aspect of similarity, namely, methodology. For both types of systems, macroeconomic models have been studied in this paper. The macroeconomic models are similar except for emphasis. A single model with one set of parameters yields the classical theory and with another set of parameters yields the Keynesian theory. However, the macro-

economic models are not the basic elements of either system. It is necessary to analyze the considerations that lie behind the macrosystem, i.e., the microsystem. It will be found here, too, that the methodologies of classical and Keynesian economics do not differ. There are two steps in the formation of the macroeconomic systems. First, it is necessary to formulate the behavior pattern of individuals. Both theories are based on household utility-maximization to get the demand for consumer goods and household cash-holdings, and on business-firm profit- (or utility-) maximization to get the demand for producer goods, labor, and business cash-holdings. The second step is to show how to pass from a theory involving individual firms, households, factors, and commodities to a theory involving communities of individuals, composite factors, and composite commodities. This step involves the index-number problem. The discussion of both these subjects is important but lengthy. The reader is referred to other works for more extensive analysis.⁹ The point to be emphasized at this stage is that the methodology is the same for classical and Keynesian economics at all steps in the process of deriving the macrosystems.

⁹ On the problem of the theories underlying the Keynesian and classical macroeconomic systems see Klein, *op. cit.* On the problem of aggregation see Francis W. Dresch, "Index Numbers and the General Economic Equilibrium," *Bulletin of the American Mathematical Society*, XL (February, 1938), 134-41; Lawrence R. Klein, "Macroeconomics and the Theory of Rational Behavior," *Econometrica*, XIV (April, 1946), 93-108; and "Remarks on the Theory of Aggregation," *Econometrica*, XIV (October, 1946), 303-12; Kenneth May, "The Aggregation Problem for a One-Industry Model," *Econometrica*, XIV (October, 1946), 285-98; Shou Shan Pu, "A Note on Macro economics," *Econometrica*, XIV (October, 1946), 299-302.

III. THE MARXIAN THEORY¹⁰

There are two important subsections of the modern theories of employment which need to be clarified. One subsection is the stagnation thesis, and the other is the relation between wages, profits, and employment. The modern version of the stagnation thesis is an outgrowth of the Keynesian developments in American economic thinking. The opposite theory of the stationary state is a natural outgrowth of the classical system. But neither model, as usually stated, gives an adequate analysis of the theory of economic development. It is possible to modify these theories with the introduction of trend variables, the stock of capital, etc., in order to get some information about the economic laws of motion of society; but it seems preferable to go to a theory which deals directly with this subject. From a historical point of view it is also fitting to use the theory which first tackled the problems related to the stagnation thesis. The Marxian theory of the falling rate of profit is one of the first, and probably one of the best, tools for analyzing the stagnation theory. Since Marxian theory comes to conclusions similar to those of the modern stagnationists, but for different reasons, it will also be instructive to study it in some detail.

The other problem of the relation between wages, profits, and employment is of great current interest but also cannot be properly analyzed within the customary frameworks of Keynesian and classical economics. These theories can also be modified by distinguishing in the consumption function between wage income and profit income. But the Marxian theory is based fundamentally on the interrelationships between wages and prof-

its. The Marxian theories of reproduction are well suited for the study of this problem.

Here it will be necessary to digress for a few pages in order to show explicitly the structure of the Marxian model. This model will then be compared with the Keynesian model and used for the analysis of the stagnation theory and the relationship between wages, profits, and employment.

The methodology of the Marxian approach is quite different from that of Keynes and the Classics: Instead of studying the behavior of individuals, Marx studied the behavior of classes directly. His theory is probably the origin of macroeconomics. But the Marxian system of macroeconomics differs essentially from the Keynesian and classical systems. The macrounits in the latter systems are producers and consumers, and this overlapping fails to bring out some essentials. The macrounits of the Marxian system are not only producers and consumers but also workers and capitalists. The latter two groups are, practically speaking, exclusive, and their basic conflict of interests can more easily be singled out as one of the moving forces in the system.

The economic writings of Marx were not presented in the form of systems of simultaneous equations. The equation-system approach to economics came at a later date. There are various equations throughout Marx's writings, but these equations are mainly definitions. They state, for example, that total output can be broken up into three components: constant capital, variable capital, and surplus value. Various manipulations are carried out with these components, but complete systems of equations are not formulated. However, imbedded in Marx's literary discussion and numerical

¹⁰ The author is indebted to Professor Kenneth May for helpful criticisms in this section.

examples, there are several hypotheses and assumptions that can be used to build a system of equations. The validity of the equation system depends upon the validity of the hypotheses made. It is the function of the empirical studies to test the validity of these equations.

The supply-and-demand equations of orthodox economics also are based upon some assumptions the validity of which cannot be assumed a priori. The systems of supply-and-demand equations are usually based on the assumptions that households maximize their individual utility functions subject to certain constraints. The assumptions produce the maximization equations which are essentially the supply-and-demand equations. In the same way, we shall have to introduce Marxian assumptions in order to construct an equation system out of *Capital*.

A concrete example will demonstrate clearly the relation between definitional equations and behavior equations (or refutable hypotheses). Suppose we write, as did Marx,

$$c + v + s = \text{total value}, \quad (3.1)$$

$$\frac{s}{v} = \text{rate of surplus value}, \quad (3.2)$$

$$\left. \begin{aligned} \frac{c}{c+v} = \\ \text{organic composition of capital}, \end{aligned} \right\} (3.3)$$

$$\frac{s}{c+v} = \text{rate of profit}, \quad (3.4)$$

where c = constant capital, v = variable capital, s = surplus value.¹¹ Equations (3.1)–(3.4) are definitions. They define four different terms and hold, regardless of any economic behavior patterns. We

¹¹ For the individual firm, c consists of depreciation and raw materials; v consists of wage payments; and s consists of profit, interest, and rent.

cannot test the validity of any of these equations because they must hold by definition. They are not refutable hypotheses.

According to the simplest rules of algebra the following equation,

$$\frac{s}{c+v} = \frac{s}{v} \left(1 - \frac{c}{c+v}\right), \quad (3.5)$$

must hold¹² because

$$\frac{s}{v} \left(1 - \frac{c}{c+v}\right) = \frac{s}{v} \left(\frac{c+v-c}{c+v}\right) = \frac{s}{c+v}.$$

Equation (3.5) is not a refutable hypothesis either. It, too, must hold, regardless of the actual values of the variables c, v, s . Equation (3.5) merely states the truism that

$$\frac{s}{c+v} = \frac{s}{c+v}.$$

In so far as Marxian economics is based on equations (3.1)–(3.5) no real progress can be made. None of these equations tells us anything about fundamental economic behavior. The extensive use by Marx and the Marxists of equations similar to (3.1)–(3.5) has undoubtedly led Oscar Lange to remark: "This whole [Marxist] literature tries to solve the fundamental problems of economic equilibrium and disequilibrium without even attempting to make use of the mathematical concept of functional relationship."¹³

But Marx was probably not so guilty as Lange's remark implies. In Volume III of *Capital*, when discussing the theory of the falling rate of profit, Marx¹⁴

¹² For the use of such equations in Marxian economics see Paul M. Sweezy, *The Theory of Capitalist Development* (New York: Oxford University Press, 1942), p. 68.

¹³ Oscar Lange, "Marxian Economics and Modern Economic Theory," *Review of Economic Studies*, II (June, 1935), 196.

¹⁴ Karl Marx, *Capital*, III (Chicago: Charles H. Kerr & Co., 1909), 247.

made specific assumptions in his numerical examples. He assumed that s/v in equation (3.5) is constant. Thus he was able to say that the rate of profit, $s/(c+v)$, varies inversely with the organic composition of capital, $c/(c+v)$. Here is a refutable hypothesis, namely, $s/v = \text{constant}$. This is an economic hypothesis that can be tested. We can examine data on wages, profits, interest, and rent to see whether or not s and v have a constant ratio. By making this assumption, Marx was able to develop the theory of the falling rate of profit which states that the rate of profit falls as the organic composition of capital rises. From equations (3.1)–(3.5) we can say nothing about the behavior of the economic system, but from equations (3.1)–(3.5) and the assumption $s/v = \text{constant}$ we can say very much. However, the system is not yet complete even at this stage.

It is worth pointing out that this confusion is not peculiar to Marxian economics. It has arisen in non-Marxian economics in connection with the quantity theory of money. Let us define M = total stock of money; V = average number of times a monetary unit is spent in a given period on newly produced goods and services; p = average price of newly produced goods and services; X = aggregate output of newly produced goods and services.¹⁵ It follows by definition that

$$MV = pX. \quad (3.6)$$

Equation (3.6) tells us nothing about economic behavior. In its present form it is of the same nature as equation (3.5). There is no refutable hypothesis contained in either (3.5) or (3.6).

The classical economists did the same thing about (3.6) that Marx did about

(3.5). They assumed that certain variables in (3.6) were known numbers. Specifically, they assumed $V = \text{constant}$ and $X = \text{full-employment output}$. For them, V was determined by institutional and psychological phenomena such as the frequency of wage payments, attitudes toward holding cash, etc. With V and X known, the classical economists could say that the price level varies directly with the amount of money. The validity of this theory depends upon the validity of the assumptions about V and X .

These examples illustrate our method. We shall search through Marx's literary explanations and numerical examples for the strategic hypotheses that will produce a determinate system of equations.

First we must define the variables carefully. We shall retain Marx's notation of c , v , s . When referring to the individual firm, c consists of depreciation and purchases of raw materials, v consists of wage payments, and s consists of profit plus interest plus rent. The aggregate value of output for the individual firm is $c + v + s$. When referring to the economy as a whole, we must redefine constant capital in order to avoid double counting. For the entire system, constant capital, denoted by C , is defined as the value of depreciation charges. Constant capital does not include raw materials for the system as a whole because such an inclusion would lead to excessive double counting in determining the value of output. Variable capital for the entire system will be denoted by V and will include all wage payments. Surplus value for the entire system will be denoted by S and will include total profits, interest, and rents. In modern terminology, we have

$$\begin{aligned} C + V + S &= \text{gross national income,} \\ V + S &= \text{net national income.} \end{aligned}$$

¹⁵ The aggregates p and X are constructed so that their product, pX , is exactly equal to the total value of newly produced output.

National income can be considered from two sides—production and factor payments. National income as the sum $V + S$ represents total factor payments.¹⁶ From the side of production, national income can be considered as equal to the total production of two types of goods and services—consumption and investment (consumer goods and producer goods). Consumer goods are those that flow to households and producer goods those that flow to business firms. We shall denote consumption by R and net investment by I . Net national income will be denoted, as usual, by Y . We have, thus far, the two following definitional equations:

$$V + S = Y, \quad (3.7)$$

$$R + I = Y. \quad (3.8)$$

The variables V, S, Y, R, I , are all measured in real terms, for example, constant dollars.

It is now necessary to develop behavior equations to show how these variables are determined. First consider R , consumption. Marx divided consumers into two strategic groups—workers and capitalists. He *assumed* that workers spend all their incomes on consumer goods and services. In fact, he wrote: “. . . the variable capital advanced in the payment of the labor-power of the laborers is mostly spent by them for articles of consumption; . . .”¹⁷ This assumption is also carried through in a purer form in his numerical examples of reproduction schemes in Part III, Volume II of *Capital*. In the numerical ex-

amples he always put workers’ consumption exactly equal to wages (not approximately equal). In the quotation he said that wages are “mostly spent” (but not entirely spent) on consumer goods and services. As a matter of fact, empirical data suggest that Marx’s quoted assumption is the correct one. The marginal propensity to consume out of wages is not unity, although it is very close to unity.

It is less obvious how to determine the behavior pattern for capitalist consumption in the Marxian system. The main clue comes from a study of numerical examples that Marx used to analyze capitalist reproduction schemes. The theory of simple reproduction is not much of a clue, for in that scheme a steady state is assumed in which variable capital (wages) and surplus value are always exactly spent on consumer goods and capital is replaced without any net investment taking place. The schemes of accumulation and reproduction on an enlarged scale, found at the end of Volume II of *Capital*, provide the basis for a theory of capitalist consumption.

In his examples on accumulation, Marx divided the economic system into two departments—the department (I) producing producer goods and the department (II) producing consumer goods. In the first department, workers were assumed to spend all their wage income on consumer goods produced by the second department, while capitalists were assumed to spend only a part of their surplus-value income on consumer goods. The exact relation for capitalist behavior in Department I was

$$\text{consumption} = \frac{1}{2} (\text{surplus value}).$$

This is the consumption function for capitalists in Department I. In a consistent theory it should be expected that capi-

¹⁶ It is only in orthodox economics that S represents a factor payment. In Marxian terminology, S represents expropriation. The term “factor payment” is used in the text only because it is customarily used today in discussions of national income statistics.

¹⁷ Karl Marx, *op. cit.*, II, 466.

talists in Department II would also behave in a similar fashion, their consumption being a function of their surplus-value income. True, Marx assumed that the capitalists in Department II consumed out of their surplus-value income, but he did not assume that there existed an independent relation between consumption and surplus value for capitalists in Department II. The behavior of capitalists in the consumer-goods industry was entirely passive in the sense that their consumption was calculated as a residual. This residual consumption was taken to be the difference between total surplus value in Department II and that part of surplus value which was transferred to expenditure on constant and variable capital. The latter expenditure was calculated by Marx so that the reproduction scheme could work smoothly without a glut of the market. Marx did not assume, by any means, that capitalism works smoothly; but he set down in his reproduction schemes the conditions under which capitalism could work smoothly. He argued that if his conditions were not met a crash would occur. One step in a possible method of introducing fluctuations into the model, with recurring crises and recovery, is to make capitalist consumption in Department II also a function of surplus value. We can even simplify the entire system by doing away with the distinction between departments I and II. Let us assume instead that capitalists behave the same way in both departments. Identical behavior is assumed for workers in these two departments, and it seems reasonable to assume that capitalists should not have different consumption habits according as they produce consumer goods or producer goods. Hence we shall assume that the consumption of capitalists is a function of surplus value.

Denoting the consumption of workers by R_1 and the consumption of capitalists by R_2 , we have the two consumption functions¹⁸

$$R_1 = V, \quad (3.9)$$

$$R_2 = a_0 + a_1 S, \quad 0 < a_1 < 1. \quad (3.10)$$

The total consumption function is given by

$$R_1 + R_2 = R = a_0 + a_1 S + V. \quad (3.11)$$

In a more general formulation, where the workers' marginal propensity to consume is not unity, we have

$$R_1 = a_2 + a_3 V, \quad 0 < a_3 < 1, \quad (3.9')$$

$$R = (a_0 + a_2) + a_1 S + a_3 V, \quad a_3 > a_1. \quad (3.11')$$

The next step is to derive the demand for the other type of good in the system—investment or producer goods. We shall first derive the demand relation for constant capital (capital used up) according to Marx and then transform the demand for constant capital into investment. Workers buy only consumer goods in the Marxian system, for that is what distinguishes workers from capitalists. The demand for constant capital will be based entirely on the behavior of capitalists. Again, we rely on the examples of expanded reproduction in order to discover the variables influencing capitalists' demand for constant capital.

In Volume II, Marx assumed that capitalists in Department I (the producer-goods industry) spend from surplus value on constant capital. His relation was

$$\text{constant capital} = C_0 + k (\text{surplus value}), \\ 0 < k < 1,$$

where C_0 = the initial level of constant capital and k = a fraction which is the product of the fraction of surplus value

¹⁸ As a first approximation, we shall assume a linear system.

to be accumulated in both variable and constant capital and the fraction of total capital represented by constant capital.

The expenditures on constant capital in Department II were like the expenditures by capitalists on consumer goods in that department in the sense that both expenditures were calculated as a residual. The capitalists in Department II did not decide, independently, to accumulate capital but based their decision entirely on the relationship between expenditures in both departments so that the process would run smoothly without a glut of the market. We can again do away with the assumption of a smooth-working capitalist system by supposing that capitalists behave the same way in both departments in so far as the demand for constant capital is concerned. We shall assume that capitalists in both departments demand constant capital as a fraction of surplus value.

There is one condition, implicit in Marx's example, which must be avoided for our model. Marx assumed that whatever capitalists do not spend out of surplus value on consumer goods they spend on constant or variable capital. We shall assume, instead, an independence between the marginal propensity to consume and the marginal propensity to invest. We must point out, however, that Marx made this assumption only to obtain the conditions for a smooth-working system. He did not imply that these conditions held in the real world. Our alternative assumption is one way of achieving the conditions of the real world in the Marxian spirit.

We now have the equation

$$C = \beta_0 + \beta_1 S. \quad (3.12)$$

Since we are going to work with the variable I instead of C , it will be necessary to carry out a transformation of variables.

The transformation involves common-sense technological relations which are constructed by the present author and do not appear in *Capital*.¹⁹

The variable C represents the amount of fixed capital used up in the production process. The amount of capital used up (depreciation) will depend upon the stock of fixed capital in existence. The capital in existence will, in turn, be made up of the elements of durable capital, plant, and equipment—acquired at various stages of past history. Denoting the capital acquired during the p th preceding time period by x_{-p} , we have

$$C = C(x, x_{-1}, x_{-2}, x_{-3}, \dots) \quad (3.13)$$

or in a linear approximation²⁰

$$C = \delta_0 + \delta_1 x + \delta_2 x_{-1} + \delta_3 x_{-2} + \dots \quad (3.14)$$

In statistical work we cannot measure separately the capital purchased during every preceding time period, but we can approximate all these variables with a proxy variable which represents all the capital accumulated up to the time period under consideration. Instead of (3.14), let us write

$$C = \delta_0 + \delta_1 x + \delta'_2 Z_{-1}. \quad (3.14')$$

The stock of existing fixed capital, Z_{-1} , can be written in terms of the net investment of all preceding periods as

$$Z_{-1} = \sum_{t=-1}^{\infty} I_t. \quad (3.15)$$

Equation (3.14') at least makes the distinction between new and old capital, but

¹⁹ These transformations are so obvious that it is assumed that anybody wishing to work with I instead of C would use approximately the same transformations.

²⁰ Since the linear function is an approximation, we shall not assume the constant term equal to zero, although logically there should be no constant term in this equation.

it is not so complete as (3.14), which makes the distinction between capital of all different age groups. This distinction is useful because the capital in different age groups has different productivities, the newest capital being technologically superior.

It is net investment rather than gross investment which is of primary importance for the particular model of this paper. We can obviously write

$$x = I + C. \quad (3.16)$$

Substituting (3.16) into (3.14') we get

$$C = \delta_0 + \delta_1(I + C) + \delta_2'Z_{-1} \quad (3.17)$$

or

$$C = \frac{\delta_0}{1 - \delta_1} + \frac{\delta_1}{1 - \delta_1} I + \frac{\delta_2'}{1 - \delta_1} Z_{-1}.$$

We can now eliminate C between (3.12) and (3.17) to get

$$I = \beta_2 + \beta_3 S + \beta_4 Z_{-1}. \quad (3.18)$$

This is the final form of our investment function.

There is now lacking one more equation for the completion of the system. Capitalists demand commodities in the form not only of producer and consumer goods but also in the form of labor power. Our equation of the demand for labor power will appear in a disguised form. We shall develop an equation which serves to determine the aggregate amount of variable capital, V . But this variable represents the total remuneration paid out by capitalists for labor power. The equation which serves to determine V in our system is the same thing as the demand equation for labor power.

Those familiar with Marx will recall

that he regarded the surplus value as transformed into variable and constant capital in his schemes of expanded reproduction. We could have made $C + V$ a function of S instead of making C alone a function of S . However, since Marx always assumed a definite relation between C and V , we were able to eliminate V in the above relation. He imposed the condition that variable and constant capital be used in the same proportions throughout the production process; hence we were able to develop a relation between C and S not involving V . While Marx assumed a definite relation between C and V , he also assumed a definite relation between S and V . It may appear that we are getting too many equations, but both these relations (that between C and V , and that between S and V) are not independent. Suppose that total capital is a function of surplus value

$$C + V = f(S) \quad (3.19)$$

and that variable capital is also a function of surplus value

$$C + V(S) = f(S) \quad (3.20)$$

or

$$C = f^*(S).$$

This forms the basis of equation (3.12). It is evident that there must also be a relation between C and V , since

$$S = V^{-1}(V) \quad (3.21)$$

and

$$C = f^*(V^{-1}[V]) = f^{**}(V).$$

This simple demonstration shows that a relation between C and S and a relation between V and S imply a relation between C and V . The latter relation is not

independent of the other two; hence there are not too many equations.²¹

As was seen above in the brief discussion of the theory of the falling rate of profit, the assumption, $S/V = \text{constant}$, led to very important conclusions. In the numerical examples of expanded reproduction, Marx maintained a constant ratio between S and V . This assumption implies that labor will receive a constant fraction of net national income. Economists have long been puzzled by the fact that national-income statistics have shown labor's share of total income to be nearly constant over a long time period. There has possibly been some trend in these data which show that labor's share has been gradually increasing. This trend term could be explained by the institutional phenomenon of a growing labor movement in the United States.

The next equation is thus

$$V = \gamma_1 S. \quad (3.22)$$

We may introduce the trend by a modification to

$$V = \gamma_0 + \gamma_1 S + \gamma_2 t. \quad (3.22')$$

Since $V + S = Y$, it is equivalent to say that V and S are proportional or that V and Y are proportional. In recent years the stability of labor's share has usually been discussed in terms of V and Y rather than V and S . As an alternative formulation, we could write

$$V = \gamma_3 + \gamma_4 Y + \gamma_5 t. \quad (3.23)$$

²¹The above demonstration is a method of keeping the system from becoming overdetermined. However, it is questionable whether Marx intended the relation between C and V to be dependent on other relations or whether he intended it to be an independent technological phenomenon. From a technological point of view, there is no reason why labor and capital should be used in a fixed relation during the entire production process; hence we have not made use of an independent technological relation between C and V .

The Marxian system is now complete. The entire set of equations is²²

$$R = a_0 + a_1 S + a_2 V, \quad (3.24)$$

$$I = \beta_0 + \beta_1 S + \beta_2 Z_{-1}, \quad (3.25)$$

$$V = \gamma_0 + \gamma_1 Y + \gamma_2 t \quad (3.26)$$

$$Y = S + V, \quad (3.27)$$

$$Y = R + I, \quad (3.28)$$

$$\Delta Z = I. \quad (3.29)$$

Equation (3.24) follows from (3.11'), (3.25) from (3.18), (3.26) from (3.23), (3.27) from (3.7), (3.28) from (3.8), and (3.29) from (3.15). We have, in (3.24)–(3.29), six equations and six endogenous variables R, V, S, I, Z, Y . All variables are measured in "real" units, and we have been able to complete the system without introducing the quantity of money.

Several observations are called for before we go on to some problems of economic analysis based upon this model. While it is true that this version of the Marxian theory has been developed largely through an examination of Marx's writings and by a slight generalization of his own methods (i.e., a generalization of his numerical examples into functional relationships), the same model can readily be developed from other considerations. By assuming certain behavior patterns for workers and capitalists, like utility- and profit-maximization, we can obtain the same mathematical model. The reader will also notice that the model (3.24)–(3.29) is very similar to Kalecki's theories. Practically no model implies a unique theoretical basis. Furthermore, we have not utilized Marx's methods to their fullest extent. Only

²²We have renumbered all subscripts on the parameters for purely aesthetic reasons.

those aspects of Marx's theories are used that are necessary to build a complete system of equations. Many Marxian theories are unrelated to the principle of effective demand, but even some of those parts of his theory that are related to effective demand have been left out. It was necessary to make the latter omission in order to keep from getting an overdetermined model. For example, Marx assumed that the *wage rate* would be determined by the value of the means of subsistence of a worker, where the means of subsistence, in turn, depends upon the traditional standard of life in the particular region where the worker lives. But it is easy to show that the model cannot contain this theory of an autonomous wage rate as well as the theory underlying equation (3.26). Suppose that equation (3.26) is accepted as a correct theory. The model then enables us to determine the real wage bill and the level of output. Every system must contain a technological input-output relationship. In the Marxian system, input is given by the employment of labor power and the depreciation of fixed capital, C . From our discussion there are enough relations to determine output and C ; hence the other type of input, employment of labor power, is uniquely determined. Since the real wage bill and employment are known, the real wage rate is also known. There is no room in this system for an autonomously determined wage rate. The strong empirical foundation behind equation (3.26) is an argument for using this Marxian hypothesis rather than the other hypothesis of a given wage rate. It is certain that both hypotheses cannot be used simultaneously within the framework of our model. This example serves to show that the above model is not the only mathematization of *Capital*. There are a variety of models that can be de-

veloped from the Marxian theories, and we have chosen one that is plausible, simple, and useful for the analysis of specific problems.

It is interesting to make certain comparisons between the Keynesian and the Marxian models. A simple version of the Keynesian theory—in which the quantity of money and the interest rate do not appear as variables—is a special case of the Marxian model. By substitution from (3.26) and (3.27) into (3.24), it is possible to make consumption a function of income; and, by substitution from (3.26) and (3.27) into (3.25), it is possible to make investment a function of income and the stock of capital. For the short-run theories, Keynes took the stock of capital as given; thus, such a reduced version of the Marxian model comes to the same thing as the simple Keynesian model. The primary advantage of the Marxian model is that it provides more information than does the Keynesian system. In the former model the complete solution always gives the demand for consumer goods, producer goods, and employment, while in some forms²³ of the latter model, the complete solution gives only the demand for consumer goods and the demand for producer goods. The demand for factors of production (employment and producer goods) determines supply; hence the Marxian model has the virtue of always giving the full conditions of demand and supply. This cannot be said, in general, of the Keynesian model.

It is not meant to imply that Marx fully anticipated the Keynesian theory of effective demand. Our model is intended as an extension of the Marxian analysis to a logical conclusion in terms of a theory of effective demand. Actually, Marx

²³ This is true in those forms of the Keynesian theory in which the savings-investment equation alone is used to determine the level of output.

laid the groundwork for a complete equation system to determine the level of income (effective demand) but did not build the complete system. In his discussions of the reproduction schemes in Volume II of *Capital*, Marx set forth some conditions under which there would not be excessive savings in the system, conditions under which all savings are offset. He then showed that these conditions are very complex and that it is not reasonable to assume that they will always be met, hence the crisis. But he did not offer an exact theory to show the quantitative extent to which they will not be met. Keynes's theory also shows the conditions for full employment and argues that they will not always be met, but Keynes went one step further: He provided a general theory to determine the level of employment when it is not one of full employment. The Keynesian model shows how any level of employment is determined. Our procedure in this paper has been to introduce mathematical extensions of the Marxian theory to show how any level of income (or employment) is determined. In case the conditions for full employment—or for no glut of the market in Marx's sense—are not met, our mathematical model shows precisely what level of employment will ensue under the less-than-full-employment conditions.

It should be pointed out that the author has applied various methods of statistical estimation to the Marxian model and has found the estimated parameters to be very reasonable in size. Moreover, the model fits the observed data very closely. Except for small random error, workers and capitalists have, in fact, behaved as the Marxian model says they behave. Lags, government investment, taxes, etc., were introduced in the statistical models in order to depict the real

world more exactly. A discussion of the statistical results is too lengthy to be included in this paper, and the conclusions are mentioned only to inform the reader that the model is not purely hypothetical.

IV. THE STAGNATION THESIS

It has become very popular of late to criticize the stagnation thesis severely and to assert that ours is still a young, vigorous, expanding economy. The critics have been quick to forget the lesson of the thirties and have misunderstood the thesis. Negative though most criticism has been, the spirit of this section is one of constructive criticism, by which some new ideas that support the thesis may be injected into the argument.

Despite the fact that the stagnation thesis grew out of the discussions of Keynesian economics of the past decade, the foundations of the theory are much older, going back to Marx's theory of the falling rate of profit. The critics would have had a much more difficult time finding evidence against a mature-economy doctrine based on the theory of the falling rate of profit than against the doctrine based on such factors as population growth, disappearance of the frontier, and growth of depreciation reserves. They were quick to point out that population growth slowed down and the frontier disappeared long before the decade of the thirties, yet stagnation did not then set in.

The Marxian theory states that, with a constant rate of surplus value (S/V), the rate of profit will vary inversely with capital accumulation. Equation (3.5) shows that the rate of profit is the product of "the rate of surplus value" and "one minus the organic composition of capital." Capital accumulation implies a rising organic composition of capital and,

hence, a falling rate of profit from (3.5). The main hypothesis of this theory, the constancy of the rate of surplus value, is known to be valid, as shown by the available data. This theory can easily be applied to the interwar period. The application runs as follows: After World War I the profit outlook in manufacturing (especially automobile), utilities, and housing appeared to be good and persistent. Capitalists accumulated all during the twenties. They built so many plants and houses and so much equipment that the rate of return on the expanded volume began to fall. The rate of return on the greatly expanded capital structure was so small during the thirties that there was little capital investment and the system was depressed for a decade. It was the capital accumulation of the twenties which led to the fall in the rate of profit and the consequent stagnation of the thirties. The theory does not say that the stagnation or maturity is permanent. It is no contradiction of the theory to observe that housing capital, *relative to the population*, declined during World War II, thus generating a high rate of return on housing capital and a building boom again. Similarly, the present capital expansion in other industries is no contradiction of the theory. However, the theory indicates specifically that the capital expansion will not continue indefinitely. Once a large stock of capital has been accumulated again, the mature-economy doctrine should predict another stagnant period of a decade or more.

In the Marxian model, (3.24)-(3.29), it will be observed that the demand for investment goods depends upon two variables—profits and the stock of capital. The essence of the Marxian theory is that both variables must be in this relation. The dependence on profit is positive, and

the dependence on capital is negative. The stock of capital becomes a very serious drag upon the system. Many of the present author's statistical investigations in separate industries, as well as for the economy as a whole, have shown that the stock of fixed capital is negatively related to investment. The more capital there is, other things unchanged, the less is the desire for new capital. The consequences of capital accumulation have never been fully explored. For example, if we drop the capital variable from the Marxian model or if we use the customary forms of the Keynesian model, the multiplier equation for the whole system usually takes the form:

$$\left. \begin{aligned} Y + a_1 Y_{-1} + a_2 Y_{-2} + \dots \\ + a_n Y_{-n} = \beta G, \end{aligned} \right\} (4.1)$$

where Y = real income and G = real exogenous investment. If, on the other hand, the variable, Z_{-1} = stock of fixed capital, is introduced in the equation of demand for producer goods, the multiplier equation will have the form:

$$\left. \begin{aligned} Y + a_1 Y_{-1} + a_2 Y_{-2} + \dots \\ + a_n Y_{-n} = \beta_1 G + \beta_2 G_{-1}. \end{aligned} \right\} (4.2)$$

The difference between (4.1) and (4.2) is significant. The values of β and β_1 will be positive, but if capital has a depressing influence on investment, the value of β_2 will be negative. Both the truncated and the untruncated multipliers from (4.2) will be smaller, the larger is the negative value of β_2 . The depressing influence of capital accumulation operates not only partially in the demand equation for producer goods but also permeates the entire system with a depressing influence. The stimulative shocks given to the system by exogenous investment, such as new industries and government spending, will be cushioned by the de-

pressing influence of capital accumulation.

The reason for introducing the stock of fixed capital in the investment-demand equation of the Marxian system is that in this form the equation fits in so well with the theory of the falling rate of profit. It is also possible to argue that an implied "theory of the declining marginal efficiency of capital" in the Keynesian theory would call for the introduction of a variable representing capital accumulation in the Keynesian investment schedule. In the past, economists have modified the Keynesian investment function in this way, but only for the long-run theory in which investment is zero. The real world, however, is not one of long-run equilibrium in which investment is zero or one of short-run equilibrium in which the stock of capital is taken as given. The real world falls between these extremes, and the Marxian model of this paper is a representation of the compromise.

V. REDISTRIBUTION OF INCOME

No theory has received more vulgarizations than has the theory of the effect on employment of the redistribution of income. The correct results need to be systematized with all assumptions stated explicitly. For simplicity, we shall consider redistribution between only two types of income, wages and nonwages (= profits). One type of vulgarization is to look at wages only as a demand factor and not at wages as a cost factor. The argument is that a redistribution from profits into wages will always increase income and employment.

Many old-fashioned trade-unionists argue that the only way to cure a condition of unemployment is to redistribute income from profits into wages. They see faulty distribution as the principal flaw in the economic system and regard its

correction as a sufficient policy to insure smooth working of the social mechanism. Many economists who call themselves Keynesians have also relied very heavily on redistribution of income as a powerful antidepression policy. They have often overemphasized the demand aspects of wages to the neglect of the cost aspects.

There is another group of economists who look at wages purely as a cost factor and neglect the influence of wages as a demand factor. Most of the supporters of wage cuts as a policy for curing depressions are in this category. They argue that, if wages are cut, capitalists will have lower costs and hence will be able to expand their plants. This argument is wrong not only because it is based on an incorrect analysis of redistribution but also because it does not take into account the possibility that falling wages may generate adverse expectations.

Obviously, the most proper type of model for analyzing the effects of redistribution is one that gives full effect to wages as a cost factor and to wages as a demand factor. The Marxian model is very well suited for this purpose. The consumption function distinguishes between wages and profits as separate demand factors, while the investment function—an equation of capitalist behavior alone—depends on profits, which means that wages enter as a cost factor. If our analysis is limited to the instantaneous effect on output of redistribution of income *within a given period*, we can neglect the influence of capital accumulation as a variable in the investment function. The term $\beta_2 Z_{-1}$, in (3.25), can be incorporated with the constant term because $\beta_2 Z_{-1}$ is predetermined and thus given for any single time period.

The following result can be stated for our model: If the capitalists' marginal propensity to spend (consume and in-

vest) is greater than the workers' marginal propensity to consume, redistribution from profits into wages will decrease income. If the two marginal propensities are the same, income will be unaffected by the redistribution, and if the latter marginal propensity is greater than the former, redistribution from profits into wages will increase the level of income. It is by no means certain, a priori, which propensity is greater. Capitalists like to accumulate, and workers like to consume. Only by making accurate quantitative measurements of the propensities can the final result be determined. The author has found that some methods of statistical estimation give one result, and some methods give another. By any method of estimation used thus far, the confidence intervals for the parameters are so large that no definite conclusion can be drawn.

The intuitive explanation of the foregoing propositions is very simple. If a dollar is taken away from a capitalist, he will cut expenditures by the amount of his marginal propensity to spend, and, if this dollar is given to a worker, he will increase expenditures by the amount of his marginal propensity to consume. The quantitative effect on income depends on the extent to which these marginal propensities diverge. The data upon which the statistical models are based show that the marginal propensities are, at least, close together. If we take into account the capitalists' marginal propensity to spend on producer goods as well as the marginal propensity to spend on consumer goods, we find that the total marginal propensity to spend is probably between .7 and .9. The workers' marginal propensity to spend is also in the same neighborhood, between .8 and .9. In the discussion of redistribution, economists often tend to consider only the two groups' marginal propensities to con-

sume, which are, of course, much farther apart.

There are special cases in which unequivocal results can be obtained. Marx has been interpreted as having claimed that the workers spend all their income, i.e., have a marginal propensity to consume equal to unity. If, as seems reasonable, the capitalists have a marginal propensity to spend which is less than unity, it follows by assumption that redistribution from profits into wages will always stimulate production. It can be shown that, for this case in the Marxian model, the increase in income is always greater than twice the amount redistributed. This is not a realistic case, however, because time-series and family-budget data both show that the marginal propensity to consume out of wages is not so great as unity. The budget data show little or no aggregate savings in the low-income classes, but some investigators have wrongly interpreted this to mean that the marginal propensity to consume is unity. The thing to look at is not the aggregate savings in the low-income groups but the slope of the savings or consumption function in this income range. The slope is definitely not unity throughout the range \$0-\$3,000 income per year. In this income range there are both dissaving and saving, which cancel each other to a large extent and make the total appear small. But the dissaving can always be more or less than the observed amount, and it is not correct to infer that the existence of dissaving means that low-income families consume exactly 100 per cent of every extra dollar of income that they receive.

Another special case in which the effects of redistribution can be more exactly assessed is that of exogenous investment. If it is believed that investment decisions of businessmen are unrelated to

variables internal to the system—depending instead on innovations, psychological expectations, legislative decisions, etc.—the only relevant parameters for the redistribution problem are the marginal propensities to consume of workers and capitalists. The data show definitely that the marginal propensity to consume of the former class is greater than that of the latter class; therefore, within the framework of the model of exogenous investment, redistribution from profits into wages will always stimulate income.

There are also special models where redistribution from profits into wages certainly decreases income. For example, there is a tendency on the part of many model-builders to assume that total income (wages plus profits) is the relevant variable in the consumption function. This assumption gives equal weight to wages and profits on the side of demand for consumer goods. If, to this assumption, is added the assumption that investment expenditures depend on profits, the marginal propensity to spend out of profits will be greater than the marginal propensity to consume out of wages, and redistribution will have the above-stated effect.

There is nothing in the uncertainty of the conclusions of this section to contradict either the Marxian or Keynesian theoretical systems. This point must be made clear because many supporters of these theories make more extravagant claims about redistribution than can be justified on the grounds of the theories of employment alone, convincing though these claims may be from the point of view of economic welfare, equity, and justice.

In the Marxian theory, to state matters mildly, there is no hint that redistribution of income is a sufficient policy to insure that capitalism will always provide uninterrupted full production and employment. This is consistent with the findings that the marginal propensity to spend out of profits is not very different from the marginal propensity to spend out of wages, so that the redistribution effect is minimized. If the system is such that the latter marginal propensity exceeds the former, one must conclude that workers are kept so close to physical subsistence that they are forced to spend practically all their income. This is the situation which calls for redistribution from profits into wages as an employment-creating policy. If the former marginal propensity exceeds the latter, the Marxian explanation is that capitalism generates such fears and uncertainties about the future in the minds of the workers that they are forced to save for the "rainy day." Precautionary saving of this type is enough to drive their marginal propensity to consume below the marginal propensity to spend out of profits. Under such circumstances, redistribution from profits into wages which does not alleviate the fear of the future²⁴ will not create employment. In the Marxian theory, redistribution policies which do not alter the mode of production are not adequate to solve the problem of the occurrence of crises.

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²⁴ Social security planning is a type of redistribution which does alleviate the fear of the future.

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