



Some Notes on the Stockholm Theory of Savings and Investments II

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The Economic Journal, Vol. 47, No. 186. (Jun., 1937), pp. 221-240.

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THE ECONOMIC JOURNAL

JUNE, 1937

SOME NOTES ON THE STOCKHOLM THEORY OF SAVINGS AND INVESTMENTS II.¹

8. *The Rate of Interest.*²—Obviously the rate of interest cannot—with the terminology used above³—be determined by the condition that it equalises the supply of and the demand for savings, or, in other words, equalises savings and investment. For savings and investment are equal *ex definitione*, whatever interest level exists on the market. Nor can one say that the rate of interest equalises planned savings and planned investment, for it obviously does not do this. How, then, is the height of the interest level determined?

The answer is that the rate of interest is simply the price of credit, and that it is therefore governed by the supply of and demand for credit. The banking system—through its ability to give credit—*can* influence, and to some extent does affect, the interest level. As a matter of fact, it is often useful as a first approximation to analyse practical problems on the assumption that the banking system fixes the rates of interest which make the interest “level.” Does this mean that its height has no connection with the disposition of individuals and firms to save and with other elements in the price system? Of course not. But it has such a connection only indirectly. One object of interest theory is to explain the nature of this connection.

Given a certain disposition to save and certain income expectations, *i.e.* certain consumption and savings plans, the level of the rate of interest relatively to profit expectations, etc., determines the volume of investment and the way in which production, trade and prices develop. Thus, incomes are made to differ from expected incomes, savings from planned savings, and investment from planned investment in such a way that savings and investment agree. *Ceteris paribus*, in-

¹ The first part appears in the ECONOMIC JOURNAL for March, 1937. This second part is with particular reference to Mr. Keynes' *General Theory of Employment, Interest and Money*.

² The Swedish books contain only a scanty analysis of interest theory, so I do not know to what extent the *second* half of this section—which goes beyond my book of 1934 and may be influenced to some extent by Keynes' *General Theory*—is accepted by my Stockholm colleagues.

³ Cf. Part I. of this article.

creased investment without a corresponding increase in planned savings raises the sum total of purchases and, thus, production or prices or both. But it should be noted that the "*ceteris paribus*" assumption includes "constant income expectations." If they rise, and consumption with them, an expansion will result even if planned saving should happen to be equal to planned investment. The essence of the matter is simple: how do consumption purchases plus investment purchases vary from one period to another? But to explain this, plans and expectations and their relation to the "realisations" of earlier periods have to be considered.

Other things being equal, a change in the interest level will cause a different kind of economic development. An important conclusion follows. Which rate of interest one wants to call "normal" depends on what kind of economic development one considers "normal." Some people regard a constant price level of some sort as natural, and they are then entitled to call the rate of interest "normal"—if there is one—which leads to this constancy. But there is, of course, no special reason for looking at the price situation alone instead of at the economic situation in general. In brief, the rate of interest, or rather the combination of rates of interest, which is compatible with the economic development one chooses to call "normal," is also normal, and so is the volume of savings and of investment which goes with it. If the interest level should be lower and the volume of investment greater than what corresponds to this development, then a process of relative expansion—of output or prices or both—is the outcome. Thereby the total quantity of savings is increased. As this economic development is *ex definitione* not "normal," the extra savings can also be called "not normal." Part of them is of the "unintentional" kind, the rest is planned on the basis of income expectations which are enlarged by the process in question.

According to Wicksell, who used different, somewhat ambiguous terms, a cumulative process of expansion was bound to ensue as long as the actual rate of interest was lower than the normal rate. What is the situation in this respect with the above terminology? Obviously, to say that the process of relative expansion continues so long as the actual rate falls short of the normal rate is a mere tautology, at least if we assume, as Wicksell did, that a lower rate always leads to greater investment than a higher rate. Wicksell's idea was that the normal rate—which he thought of as closely related to a natural

rate corresponding to the marginal productivity of capital or of round-about methods of production in some Böhm-Bawerkian sense—changed very slowly if at all through the increase in savings caused by the process of expansion. Hence, he expected prices to continue to rise until the actual rate of interest on the market was raised. This latter opinion is not tenable, except perhaps if certain special assumptions are made as to expectations concerning the future. In a general analysis one has to stress the point that expectations and, thereby, the “normal” rate can change any day. There is, in my opinion, nothing more “physical” about it, as the Austrian theory wanted us to believe. The cumulative process—meaning a continuing rise in total purchases relative to the “normal” development—goes on as long as *expectations are such* that the investment purchases and the consumption purchases involve a relative rise in total purchases. This rather meaningless conclusion is not without importance, as it shows clearly that the “cumulative” character of the process depends on the fact that certain kinds of expectations are set up. A rise in the prices of consumption goods will, under certain conditions—*e.g.* if entrepreneurs at every moment expect existing prices to continue—raise the subjective value of capital goods and increase the demand for them, leading indirectly to greater income expectations and incomes and to a higher demand for consumption goods, etc., independently—in my opinion—of any shift of productive agents from one line of industry to another.

The important thing to stress is that the distinction between “normal” and “not normal” interest rates and savings depends on arbitrary assumptions that one kind of economic development, *e.g.* a constant wholesale price level, is “normal.” Besides, it is far from certain that there is always one interest level which guarantees the existence of this normal development. On the one hand, it is possible that *no* interest level can do this. On the other hand, a great many and rather different interest levels may satisfy the condition of being compatible with this development. Obviously, in a dynamic analysis one has to give up the idea of an equilibrium rate of interest in the sense of the static equilibrium theory.

The fact that no sharp distinction is possible between “normal” and “not normal” savings throws some light on the above-mentioned attempts to distinguish between “true savings” (“*echte Sparmitteln*”) and other savings, usually called “forced.” On static assumptions it is possible to define a certain interest

level and the corresponding volume of savings which is compatible with the maintenance of static equilibrium. Savings and interest rates which are not compatible with this equilibrium get a flavour of being "abnormal" or "artificial." But on dynamic assumptions such ideas have to be given up. It is, of course, conceivable that someone may in the future define a dynamic equilibrium in such a way as to make it useful for the analysis of practical problems, and that thus the distinction between equilibrium and non-equilibrium interest rates and savings may become important. But until this has been done—I doubt if it will ever happen—it seems necessary to emphasise the looseness of all ideas about "normalcy" in connection with interest rates, etc., and to attempt the study of time-using processes with the aid of more relativistic terms.

The reasoning so far is only an indication of the effects produced when the banking system fixes certain interest rates. But does the banking system actually alone determine the height of these rates? Of course not. Only the discount rate is usually fixed by the central bank. As to the other rates, *e.g.* the bond yield, the banking system is only one of many factors which affect demand, supply, and price. This requires further explanation.

Here again it is important to distinguish between an *ex-post* and *ex-ante* analysis. *Ex-post* one finds equality between the total quantity of new credit during the period, and the sum total of positive individual savings. (Of course, a person who uses his own savings is then said to give credit to himself; this supply and this demand offset one another and exert no influence on the price of credit.) Thus, there is a connection between the rate of interest, which is the price of credit, and the process of economic activity, of which the flow of saving is a part.

To explain how the rates of interest are actually determined, we need, however, a causal analysis which runs chiefly in *ex-ante* terms. What governs the demand and supply of credit? Two ways of reasoning are possible. One is *net* and deals only with *new* credit, and the other is *gross* and includes the outstanding *old* credits. The willingness of certain individuals during a given period to *increase* their holdings of various claims and other kinds of assets *minus* the willingness of others to *reduce* their corresponding holdings gives the supply curves for the different kinds of new credit during the period. Naturally, the quantities each individual is willing to supply depend on the interest rates. In other words, the plans are in the nature of alternative purchase and sales plans. Similarly, the total supply of *new* claims *minus*

the reduction in the outstanding volume of *old* ones gives the demand—also a function of the rates of interest—for the different kinds of credit during the period. The prices fixed on the market for these different claims—and thereby the rates of interest—are governed by this supply and demand in the usual way.

The demand for claims of different sorts can be explained partly in terms of the same expectation-analysis as demand for investment goods. In discussing this latter question above nothing was said about the former, *i.e.* the way people planned to handle their own savings and “free capital.” Except when they want to use them for direct investment—purchases of goods for investment purposes—they must decide in favour of acquiring claims, including cash. The psychology behind the choice between the different possibilities in this respect has been much illuminated by Keynes’ discussion of “liquidity-preference.”

A similar kind of reasoning can, of course, be applied *gross*, *i.e.* including the old claims which were outstanding when the period began. People’s willingness to hold the different claims and other kinds of assets every day governs the supply of credit. The total supply of claims, etc., governs the demand for credit. In each market for the different claims, etc., supply and demand are made equal by price. These prices for interest-bearing claims on certain fixed sums determine the rates of interest. It is quite obvious that this reasoning in gross terms leads to the same result as the net analysis above.

I must pass over the question about the differences between the different kinds of credits, *e.g.*—(1) the length of the contract and the right to get the sum back on short or long notice and (2) the security given and the credit-worthiness of the borrower. The changing valuation of these things, and the ideas concerning the profit possibilities of other assets than claims, affect demand, supply, and price in the different markets.

Let me add a few words about the market which is given a special position by Keynes, the demand and supply for cash and claims “quickly” convertible into cash. It goes without saying, that the interest rates existing at any given moment fulfil the condition that they make people willing to hold as cash—which term in the following includes the last-mentioned claims—the total amount outstanding. But the same is true of all other claims and assets. The total quantity of cash is not fixed by the banking system at a certain figure, but depends on the economic development and on the actions of a number of individuals just as does the quantity of bonds outstanding. The “market” for cash

has no key position in relation to the other markets. It is not even certain that the rate of interest obtained on cash holdings is zero. Up to a few years ago the Swedish banks used to pay interest on cheque deposits. In many countries sums on savings accounts could be withdrawn readily and, therefore, could serve as money. Of course, one can ask: how intensively does each individual prefer holding a certain part of his fortune in form *A* (cash) rather than in form *B*? It is simpler and clearer to ask directly what sums people want to hold in form *A*, what in form *B*, etc., in a certain price situation and with certain expectations, *e.g.* with a certain constellation of interest rates, share prices, etc.¹ There is no need of a theory of interest in terms of differentials, similar to the Ricardian theory of rent.

In my opinion, the theory of interest can be regarded as falling into three parts: (1) An analysis of the markets for claims and other assets, where their prices and, thus, the rates of interest are determined. This includes the phenomena of credit policy by banks, *e.g.* open-market operations. (2) An explanation of what kinds of processes with regard to the quantities of planned and unintentional savings and investment result from the existence of certain interest rates or, rather, from certain movements in interest rates. (3) An account of the connection between these processes and the transactions on the markets first mentioned. One process is apt to increase the willingness to hold long-term bonds, while another process reduces it, and this changed willingness is much dependent on the changes in incomes and in planned savings. Consumers buy consumption goods, business men buy capital goods, *i.e.* invest in a real sense, but there is a third kind of purchases to be explained—namely, “financial investment,” *i.e.* the purchases of bonds, shares and bank deposits and the failure to use savings either for real or financial investment, which is identical with an increase in cash. It is noteworthy that Keynes, who has presented so interesting an analysis of the desire to vary cash holdings and of the psychology of financial investment, *i.e.* the willingness to buy bonds, shares, etc., on the one hand, pays so little attention to the connection between changes in production, income and savings on the one hand and the *ability* to make financial investments on the other. Without a consideration of this latter circumstance, the analysis of the markets for claims of different maturity, where the rates of interest are determined, is incomplete. Such

¹ Dr. Johan Åkerman some years ago in conversation outlined some such approach to interest theory. I am not aware whether he has followed it up.

a theory as I have here only briefly indicated is of course different from any equilibrium theory of the text-book kind. But it agrees with that theory and differs from Keynes' construction in one essential respect: it brings out the relation of the rates of interest to the other elements of the price system and to their movements, whereas Keynes' construction—unless it is interpreted in a way which he probably does not accept—seems to regard the rates of interest as determined largely “outside” the price system, or at least as having almost no connection with the system of mutually interdependent prices and quantities.

9. *Some Aspects of a Theory of Employment.*—As Mr. Keynes has—rightly, I think—put much emphasis on the consequences of his theoretical approach for the theory of employment and unemployment, I shall add a few words about the attitude towards this problem in the Stockholm theory. What I say is naturally much influenced particularly by Dr. Alf Johansson's monograph on *Wage Development and Unemployment*. Points (a) and (b) below are a summary of some of his argument. To be as brief as possible I shall only enumerate some of the salient conclusions.

(a) Permanent unemployment need not be “due to” a failure to reduce wages. In other words, it is far from certain that a reduction in wage rates would reduce unemployment to what one calls a “frictional” minimum. In the post-war discussion economists have sometimes *assumed* that there is an equilibrium wage which would make demand equal the available quantity of labour and, thus, lead to a state of no unemployment, except of the frictional type. Thereafter, they have proceeded to state that the existence of permanent unemployment is a *proof* that “wages are too high.” It is evident that nothing is proved and that the latter statement is simply a repetition of the original assumption. Once the static equilibrium reasoning is given up, it becomes obvious that the relation between wages and unemployment is much more complicated. The level of wage rates is only one element of many, which have to get into certain relationships in order that the available labour force shall be employed. Discussing the unemployment during a period of prosperity, Dr. Johansson writes: “*A priori* one cannot expect that under all conditions—independently of the character, strength and speed of the structural changes and their relation to one another—a flexible wage should be completely effective as a regulator of equilibrium on the labour market for each period, which is longer than the business cycle. Why should the time-using . . . adaptations of the elements which govern supply

and demand on the labour market and which are called forth by an adaptation of wages . . . have exactly the strength and time to develop exactly so far as is needed, in order that unemployment shall be reduced to an 'irreducible minimum' at the culminating stage of a business cycle, which may develop during a period of secular decline in prices and an intense technical rationalisation of a labour-saving sort?" (*op. cit.*, p. 9). If this is true of a period of prosperity, how much more uncertain must it not be that wage flexibility can prevent continued unemployment during a long depression?

(b) When labour is set free through labour-saving technical changes there is no automatic compensation in increased employment elsewhere. What is set free is not "purchasing power," which will buy more of other goods than those cheapened by the invention, so that the expansion of output of such goods will provide employment for the discarded labourers. On the contrary, it is "productive power" which is made available, and it will not be re-employed unless some new impulse to expansion comes forward.

(c) Wage increases can lead to larger output and employment. The effect depends chiefly on how the investment demand of entrepreneurs reacts. Under certain conditions it will grow when wages go up, *e.g.* because people expect prices to rise later on. Under other conditions the opposite is true. The reaction of consumption demand is easier to determine. The outcome with regard to output and employment depends much on the speed of the various reactions of different kinds of investment demand as well as consumption demand. The possible rise in employment has nothing, as such, to do with a rise in prices or costs of living. The Stockholm theory thus denies the validity of the "orthodox" thesis which Keynes defends—namely, that an increase in employment must be accompanied by a reduction in the real wage. Elsewhere I hope to point out wherein, in my opinion, Keynes' mistake lies.

Obviously, wage changes affect the course of events differently during different cyclical processes. As a first approximation one can say that wage changes are in the short run "neutral" towards employment, as they increase the "cost side" of output as much as the "demand side." It depends on the price policy pursued by the sellers and on the effects of wage changes on investment and consumption demand, whether the quantity of labour employed goes up or down in the short run. (To analyse a great many such processes is the essential part of a "dynamic"

theory of wages and employment.) In the "longer run" the tendencies towards a change in the combination of productive factors must of course also be considered. A similar reasoning shows that an increase in import duties may well increase employment considerably, without any reduction in the "real wage."¹ As a matter of fact, wage reductions during depressions have usually been very small. There is no evidence of any *general* reductions in wage rates from the end of the eighteen-eighties up to the World War. This speaks against the Austrian argument that wage reduction, with the consequent decline in consumption, is the decisive factor which starts recovery.

It would carry me too far to describe—in terms of the above period-analysis—different courses of events which may follow upon changes in wage rates. The interested reader can, no doubt, do that for himself. Neither can I attempt to summarise the analysis in the Final Report of the Unemployment Committee, where the "frictional" types of unemployment are considered together with those connected with variations in the sum total of "monetary demand," *i.e.* with processes of general expansion and contraction. The Committee emphasises the fact that the total demand in terms of money will be increased (1) if foreign countries buy more of our products, (2) if investment is increased, and (3) if consumption purchases are increased. For obvious reasons the possibilities of increasing investment during a depression are particularly studied. But the Committee is careful not to assume that measures to maintain investment are all that is needed to guarantee practically complete employment. Even the largest volume of investment which is during a certain period compatible with a desirable stability in price conditions and in the external value of the currency may leave considerable unemployment if the mobility of labour is small, or if wage rates are "too high." It is not much less dangerous to concentrate attention exclusively on the volume of investment in its relation to the propensity to consume than to think only about some of the other relationships involved, *e.g.* wage flexibility.

After this brief survey of some salient aspects of the Stockholm theory, I shall turn to a discussion of Keynes' analysis of the same set of problems with a somewhat different set of tools.

C. *Some Observations on Mr. Keynes' Theory.*

1. *The Characteristics of the New Approach.*—As I see it, the two outstanding characteristics of Keynes' theoretical system

¹ My book of 1934 contains an extensive discussion of this problem and a criticism of the opposite view taken by Harrod in his *International Economics*.

are the following. First, his reasoning runs in monetary terms instead of in "real" terms, as do the theories of Marshall, Pigou and their followers, who regard money as a "veil" which one has to take away to see things clearly. A reasoning in monetary terms does not prevent any amount of considerations of the "real" implications, whenever such considerations may be desirable, *e.g.* in a discussion of policy. But it has the advantage of permitting a much simpler and less sophisticated explanation of the market phenomena, which are *price* phenomena. For this reason, it has long ago been accepted by almost all schools of economic thought outside England. One sometimes gets the impression that Keynes is unaware of this. Professor P. Douglas—in well-known works—and Professor Bagge¹ have both given us extensive treatments of wage and unemployment problems by means of reasoning in monetary terms.

The second and more important aspect of Keynes' work is that it is free from some basic assumptions tacitly made, I believe, in all systematic treatments of the pricing of commodities and productive factors, *i.e.* in the so-called theory of price and distribution (but not in money and cycles theory). In price theory it is assumed that the changes which are studied—*e.g.* changes in the supply and demand for a particular commodity—*do not react on the price system as a whole sufficiently for these repercussions outside the field of analysis to need to be considered.*² A special type of repercussion, which is thereby eliminated, is that which would occur if general processes of expansion and contraction—in terms of quantity or value of output—were to be started or affected by the *partial* processes under examination. *E.g.* in a study of the influence of a new invention the possibility that it will cause an expansion in the total volume of investment leading to inflation is not considered. This, no doubt, is a useful and fruitful method. However, this simplification would be quite absurd in the discussion of the prices and employment of the factors of production, *e.g.* in the determination of the wage and interest level, total employment, etc. The analysis there touches upon considerable changes in the whole price system, and is no longer chiefly concerned with a small part of it, as in the case of a particular commodity market. Hence, to avoid the consideration of such phenomena as general contraction and expansion processes, which

¹ Professor Bagge's *Causes of Unemployment* (published in Swedish), 1930, gives an excellent survey of all those aspects of unemployment, which are independent of instability in the field of money, and touches upon some other aspects.

² This is the well-known method of the analysis of "particular equilibrium."

is deferred to sections dealing with monetary and business cycle analysis, price and distribution theory proper is made to rest on the tacit assumption of what might be called “*monetary stability.*” It is not possible to say what meaning is given to this in the various textbooks, for the authors do not seem to be aware of the assumption they have made. The loose idea behind their discussion has some similarity with the Say doctrine, that supply creates its own demand, but involves something more than that, for there is nothing in this assumption—that total proceeds from sales equal total costs—which prevents changes in the volume of employment and output and in price-levels. (Say’s assumption rules out “profit inflation,” but not “income inflation.” See Keynes’ *Treatise on Money.*) Perhaps the tacit assumption means “a constant sum total—in terms of money—of all industrial transactions,” or “a constant national income in terms of money” or “a national income which only changes in proportion to the variation in the quantities of productive factors.”¹ Through some such assumption all other causes of incomplete employment than those connected with monopoly—including monopolistic trade union policy—and “friction” are ruled out, as well as movements in the general price-levels. Thus, the basic assumption in conventional price and distribution theory is—in my opinion—*not* one of relatively full employment. The simplification includes more than that, inasmuch as it also eliminates changes in general price-levels, *i.e.* that kind of process which is commonly called inflationary and deflationary.²

An economic analysis on this basis (“monetary stability”) can throw light upon a number of phenomena both in the labour market and in other markets. But the larger the size of the phenomena and processes considered, the greater is the probability that in the real world reactions will follow which change the total volume of output and national income, both in monetary and real terms. Hence the greater is the need for studying what this

¹ See my *Interregional and International Trade*, p. 376.

² It follows from the above that, in my opinion, Keynes’ attempt to explain why the textbook theories of price and distribution deal only with frictional and monopolistic unemployment is unsuccessful. Few twentieth-century writers have assumed that “the utility of the wage is equal to the marginal utility” of the existing amount of employment. But all writers on price and distribution, so far as I know, rule out the general processes of expansion and contraction in the value of output, and, *thereby*, both changes in employment and output—with the above-mentioned exceptions—and changes in general price-levels. In Chapter 19 Keynes mentions that the changes “in the amount of aggregate effective demand” have been ignored in conventional distribution theory. But this fact is not considered in the earlier part of the book, where the criticism of the “classical” theory is based on its utility assumptions.

change will be. When this is done, we get a theory both of "variations in employment and output as a whole" and of movements in price-levels. Not that the conclusions concerning the pricing and employment of the factors of production, based on the assumption of "monetary stability," are entirely wrong. But only under special conditions are they sufficiently correct to be interesting, except as an introduction.

What we need is an analysis which makes no such assumption of monetary stability and which concentrates attention on the effects which all kinds of *partial processes* have on the *total* volume of employment and national income, the latter in terms of money as well as in terms of quantities of goods and services. An analysis of this type involves a consideration in price and distribution theory of those problems which have hitherto been discussed in the sections on money and business cycles in textbooks on economic principles. Thereby, the whole theory of the pricing process—which must be an account of the *time-using* process—can be given a unity which it has so far lacked. And the theory of wages, employment and interest becomes rather different from the theory built up on the basis of the "monetary stability" assumption. (See §§ 8–9 above.) The works by Lindahl, Hammarskjöld, Johansson, Myrdal and myself, that were published during the depression, represent an attempt to provide pieces of such a theory.

In a world of booms and depressions such a discussion of wages, unemployment and the rates of interest, as well as the study of all kinds of economic policy, is—as already observed—apt to be more useful than an analysis based on an assumption of some sort of "monetary stability." But it would be foolish to dispense with the latter altogether, especially as an introduction. I believe that it represents one of the most fruitful simplifications that are used in economic science. Take, *e.g.*, the analysis of certain phenomena within the individual firm or the study of so-called "frictional" unemployment, with all its considerations of different labour qualities, the various kinds of labour mobility, etc. Certainly, a number of conclusions concerning these phenomena hold good also under less stable monetary conditions, but are more easily reached when the difficulties concerning processes of general expansion and contraction are not introduced. Such knowledge concerning the labour markets as has been reached on the basis of monetary stability has to be incorporated in any *general* theory of employment worthy of its name.

It is understandable that Keynes, in writing his treatise, has

concentrated on those aspects which had to do with the changes in output as a whole, and, therefore, pays very little attention to the other aspects. But it is all the more important that the relation of his reasoning to the "old" one shall be made quite clear. In comparing his theories with what he calls the "classical" theory, Keynes seems to me to mix together and confuse the differences arising from two distinct sources, mentioned above: (1) Those which depend on the fact that what he calls the "classical" theory is a Cambridge type of analysis in "real" terms and based on certain specific assumptions as to the supply of labour, whereas he thinks in monetary terms and has given up some—though not all—of these assumptions. In this respect his attitude resembles that of Cassel and several other contemporary economists. (2) Those differences which arise because both this "classical" theory and textbooks in price and distribution theory from U.S.A., Vienna, Stockholm, etc., rest on some such assumption as I have called "monetary stability," which rules out most of the large changes in the volume of output, while Keynes' own analysis—like the books by Stockholm economists mentioned above—is concentrated on a world where there are frequent and large changes in the total volume of employment and national income. From the point of view of economists who are used to discussing in monetary terms without the special "classical" assumptions about labour supply—I believe this is true of the overwhelming majority of economists in the world since the war—the former aspect of Keynes' book is simply the long-awaited conversion of a Cambridge economist to the almost generally accepted standpoint elsewhere. It is the second characteristic which gives the book a somewhat "revolutionary" flavour, from the point of view of economic theory. In my opinion, Keynes' greatest achievement in this work lies in the fact that he attempts—and in spite of his special assumptions concerning wage inflexibility etc. to a great extent succeeds—to provide a theory for changes in total employment and price-levels, which can also be called the theory of processes of general contraction and expansion.

Of course, not all the knowledge thereby reached is new. The theories of money and business cycles, even before the present depression, and still more in recent years, have given us much knowledge concerning changes in employment and rates of interest. What Keynes calls the "classical" theory does not seem to include any theory of money and cycles, otherwise it is difficult to see how he can say that this theory has "never given a single

thought ” to the question : “ Will fluctuations in investment have any effect on the demand for output as a whole and, consequently, on the scale of output and employment ? ” Business cycle theory has also taught us much about the influence of wage changes, which Keynes has failed to notice. Few economists, at least outside the Vienna school, maintained that during the severe depression of 1932–33 a reduction in wages and an increased willingness to save would have been certain to increase employment. Theoretical discussion was concentrated on the effect on the entrepreneurs’ expectations about the future course of prices, wages and profits, and upon the possibility that, thereby, wage reductions would give fresh impetus to a process of deflation and contraction of economic activity. Keynes makes the statement : “ The idea that we can safely neglect the aggregate demand function is fundamental to the Ricardian economics, which underlie what we have been taught for more than a century ” (p. 32). While some such idea underlies the price and distribution theories—as I have already argued—it is certainly not so with monetary and cycles theory, which is based on the very opposite idea. Recent discussion in this field has resulted in conclusions—*e.g.* on the effects on wage changes—which can be regarded as pieces of a theory of output as a whole. Economists who have followed this discussion will find Keynes’ analysis of wage reductions (on pp. 262–64) and the stress on their influence on profit expectations and the volume of investment very familiar. I am sure, therefore, that most readers of the *General Theory* have been much surprised in finding (on p. 21) that the classical theorists—this expression seems to cover all others than Keynes himself and the “ underworld ” of economists—“ are fallaciously supposing that there is a nexus which unites decisions to abstain from present consumption with decisions to provide for future consumption.” Practically all monetary theorists take account of the fact that saving accompanied by “ hoarding ” by some people need not lead to investment by other people. Furthermore, it is the very essence of Wicksell’s theory of money and “ cumulative processes ” that there is no such nexus between plans to save and decisions to invest. It has become the basis for most of the recent analyses of processes of expansion and contraction. Besides, D. H. Robertson, next door to King’s College—and probably not much under the influence of Wicksell—has since 1926 presented several substantial pieces of “ process analysis,” obviously not based on the above-mentioned fallacy. The same is true of Hawtrey. Yet Keynes (p. 32) expresses the opinion that the correct idea “ could

only live on furtively, below the surface in the underworlds of Karl Marx, Silvio Gesell or Major Douglas."

Let us return for a moment to the two sources of differences between Keynes and the so-called "classical" theory. It is not a mere chance that he fails to distinguish between them. In fact, he maintains explicitly that there is only one such source (pp. 21-22). The classical theory is said to depend on the assumption that "the real wage is equal to the marginal disutility of existing employment," and that "supply creates its own demand in the sense that the aggregate demand price is equal to the aggregate supply price for all levels of output and employment." But these assumptions and a third one "all amount to the same thing, in the sense that they all stand and fall together, any one of them logically involving the other two." Surely, however, the Say doctrine that supply creates its own demand has nothing to do with the psychology of the labourer. Even if a universal thirty-hours week were fixed by law, and the workers had no disutility whatsoever from work—the first assumption would then be absurd—Say's doctrine would not be affected thereby. It runs in terms of total supply and total demand, *in terms of money*, at prices which cover money costs. Conversely, even if the special assumptions concerning the supply of labour are accepted, this does not preclude an analysis of processes which are incompatible with Say's assumption.

In my opinion the vitally important distinction between the "old" type of analysis, as represented by conventional price and distribution theory, and the "new" one, represented by Keynes, the Stockholm school, and—to some extent—more or less the whole theory of money and business cycles, lies in the former's fundamental assumption (not identical with the Say doctrine) which rules out the *general* processes of expansion and contraction of employment, output and prices, thereunder all other changes in the volume of employment than those connected with monopoly and friction. The central task for economic theory to-day, towards the solution of which Keynes has made such important contributions, is the construction of a body of analysis, free from such assumptions. This amounts to a co-ordination of the theories of price, money and cycles.

2. *Keynes' Equilibrium Theory versus a Process Theory of the Stockholm Type.*—If Keynes' theoretical system is modern in the respect I have touched upon above, it is equally "old-fashioned" in the second respect which characterises recent economic theory—namely, the attempt to break away from an explanation of economic events by means of orthodox equilibrium

constructions. No other analysis of trade fluctuations in recent years—with the possible exception of the Mises–Hayek school—follows such conservative lines in this respect. In fact, Keynes is much more an “equilibrium theorist” than such economists as Cassel and, I think, Marshall.

The central thesis in Keynes’ theory is that the volume of employment depends upon the volume of investment. As most theories of business fluctuations, in their explanation of changes in employment, concentrate attention on changes in the volume of investment, Keynes’ emphasis on this latter point is not new. The novelty lies in his construction of an *equilibrium*, governed by the quantity of money, the propensity to consume, the marginal efficiency of capital, and the liquidity preference. These “independent” variables determine the rate of interest, the volume of investment and, thus, the volume of employment.

The most fundamental objection to this theory is the following. The propensity to consume expresses “the functional relationship between a given level of income in terms of wage units and the expenditure on consumption out of that income.” Given a certain propensity to consume, which we can call k , we obtain $E(1 - k) = I$. The income E will vary in the same proportion as the volume of investment I . However, this holds good only in reference to a period which is finished, i.e. *ex-post*. It would have been better, therefore, to talk about the “realised consumption ratio,” instead of the “propensity” to consume, expressing the relation between the volume of consumption and the realised income. $(1 - k)$ is the “realised savings ratio,” which can be defined as the relation between realised income and realised saving, i.e. $\frac{E}{S}$. But the latter is the same as realised investment.

Hence, $(1 - k) = \frac{I}{E}$. The equation above only expresses a truism, showing that the definitions are consistent with one another, and explains nothing. The relationship in question does not throw any light on the question “what *determines* the position of employment at any time,” as Keynes claims his theory to do. Neither does it indicate an equilibrium position, towards which the economic system tends and which, if reached, will remain stable, in the absence of new changes in the independent variables. As a matter of fact, this equation holds true for *every* period, even in the most *unstable* situations ¹

¹ See p. 23 on “the income resulting from a certain employment.” See also pp. 28, 57, 62, and 115.

To explain the development or the actual tendencies one must use terms which refer to the expectations, plans and actions based thereupon, an *ex-ante* terminology, as indicated in the first part of my paper in the last issue of this JOURNAL. Keynes probably has had a feeling of this, as he has used such a word as "propensity." But he has defined his terms income, investment and propensity to consume as *ex-post* concepts. Perhaps he has meant them *ex-ante*? But there is no such relation between expected income, planned consumption and planned investment as he indicates. Thus, either Keynes' reasoning is *ex-post*, and then it explains nothing, or it is *ex-ante*, and then it is entirely wrong. There is no reason why the planned investment plus the planned consumption should be equal to the expected total income for society as a whole. In other words, the planned investment will differ from the planned saving, unless they should happen to be equal by mere chance. Owing to this difference, expectations will not be fulfilled. At the end of the period people will find that their incomes, investment and savings during that period have not been what they expected them to be. Consequently, the expectations, plans and actions with reference to the next period will differ from what they were in the last period. The economic situation will change in a way which can only be explained through a study of how these differences between expectations and the actual course of events during one period influence expectations and actions in the future.¹

Should, however, by mere chance, planned saving and investment be equal, then expectations will come true, not for each individual or firm, but as far as total income, saving and investment are concerned. This is consistent with, but does not necessarily mean a stable situation. For people may have been expecting growing employment and income, and when these expectations are fulfilled, they may expect still further growth in income and employment. This is an exemplification of the fact that the series of events during the preceding periods may well lead to a change in planned savings or planned investment for the next period, even if expectations during the last period came true. Take another example. The volume of investment during the last period may have been influenced by old contracts which have now expired. In a thousand and one ways the situation at the beginning of the new period may be different from what it was at

¹ Note the difference between the equation above and Lindahl's, which is in *ex-ante* terms: $E_a(1 - s_a) = C$, where s_a is the planned savings ratio and C is consumption. Only for consumption can one assume that plans are always realised.

the beginning of the preceding period. Hence, the plans concerning savings and investment may be different also. A change in the economic situation will follow.

Let us start from a position where expectations have on the whole been fulfilled for some time and conditions have been subject only to relatively small changes. If we want to know the effects of a certain reduction in the planned volume of investment¹—caused, *e.g.*, by some political changes leading to pessimism in general—then one evidently has to follow the process through a study of the successive changes in expectations and plans in actual events, in the differences between them, and in the consequent reactions of the new expectations, plans, and actions, etc. In such a sequence leading to a considerable reduction in total employment and output it is *a priori* probable that many elements in the price system will be affected. The rate of interest will probably fall as a result of the smaller willingness to invest, given a certain willingness to save. (This is in accordance with the interest theory which has been briefly indicated above, and which is different from Keynes' theory.) Furthermore, the willingness to save will decline, although this may only start at a later stage than the fall in the interest level. When people come to regard their expected income as *temporarily* unusually low, the consumption will be a greater percentage of their expected income than under other conditions. (Keynes does not accept this; see p. 95). Much here depends on the speed with which their realised incomes and income expectations fall. Investment will be affected by the rapidity of the reactions in prices, quantities and interest rates, and in the willingness of banks to give credit to firms and individuals with declining solvency. Thus, I cannot find that the economic system tends towards a stable equilibrium described by simple reference to the change in the volume of investments. It is highly improbable that the system ever gets to a state where expectations are fulfilled, in the above-mentioned sense. Nor is there a tendency to move in the direction of some such position. And if the system should happen to get into such a position, this does not mean that it tends to remain there.

Keynes' opposite view, that his so-called equilibrium will indicate a stable position towards which the system tends—a position determined by the four independent variables—is due to the facts that he (1) *assumes* that the other three elements will not

¹ Investment plans, like consumption plans, are realised, as far as *purchases* go. But investment is influenced also by sales from stock of goods, and these sales may differ from expectations.

vary when the fourth one changes, even though the situation may shift from boom to depression; (2) overlooks the fundamental difference between the *ex-post* and *ex-ante* concepts, using a relation between the realised consumption and income as if it meant the planned consumption ratio. It is a consequence of this latter defect that he ignores the influence of *the speed* of the various reactions. A comparison of two equilibria—consistent with different volumes of investment—supplemented with some indications concerning certain repercussions is, of course, unable to take into account this speed of reactions, the importance of which I have illustrated in the first part of this paper and shall return to in the discussion of wage changes.

The fact that the realised savings ratio, which is identical with the relation between the volume of investment and the volume of income, varies a great deal when general business conditions change, needs no other proof than reference to the well-known fact that the production of capital goods fluctuates much more than the production of consumers' goods. Changes in the quantity of commodity stocks are small in comparison therewith, and cannot make the volume of investment reach anything like the same proportion of total income during depressions as during booms. Hence, even if the psychological willingness to save were somewhat constant, it becomes absurd to assume a relatively constant multiplier. As a matter of fact, the willingness to save fluctuates, for reasons already mentioned, and the unintentional positive or negative savings, partly connected with losses, make the realised savings fluctuate more than the planned savings, but in the same direction.

This seems to me to be a rather damaging criticism of the theory of the multiplier, and this criticism holds also if the theory is stated in terms of the *marginal* propensity to consume. Even if the marginal willingness to save—the marginal planned savings ratio—were somewhat constant during varying conditions of good and bad trade, the marginal realised savings ratio, which is identical with the relation between the increase in investment and in total production, would not be constant; for the unintentional savings come in. As a matter of fact, however, people do not decide to save the same percentage of an expected increase in income during the beginning of a recovery as they do during a boom. The necessity to pay off debts, or doubts whether the increase in income is going to be lasting, may make them decide to save 50 per cent. of the expected increase in income during the first year of recovery, whereas they would want to save only 10 per

cent. at a later stage of the recovery. Thus, if we want to form some idea as to the size of the effects of an increase in investment, *e.g.* in public works, we can only be misled by figures concerning some normal multiplier from which the actual effect is supposed to differ only slightly. Keynes mentions some circumstances which make for changes in the marginal consumption ratio and multiplier, but the whole tendency of his argument (see p. 121) is that it varies only little. The chief reason why the multiplier theory can tell us but little about the effects of a certain increase in investment is not its fluctuation, but the fact that it leaves out of account the reaction of a certain change in the volume of output and in the general business situation on profit expectations and the willingness to invest (the marginal efficiency of capital). At the bottom of a depression public works for a moderate sum may start a recovery, which would not otherwise have come, at least for a year or two. Hence the total increase in production due to these public works may during a certain period be ten times the sums spent. In another situation an increase in public works may scare the business world to such an extent that private investment activity declines, and total output is therefore increased by less than the sum allocated to public works. Thus, the multiplicatory effect may easily—if the reactions of private investment are included—at one time be ten or more, and at another time considerably less than one.¹

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¹ “Since the business cycles are mainly characterised *by variations* in this relation—between the value of new investments and consumption expenditures . . . *the theory must explain the changes in the multiplier* instead of assuming that the latter is given.” Erik Lundberg, *Studies in the Theory of Economic Expansion*, p. 37. Mr. Lundberg’s criticism of Keynes’ *General Theory* is partly the same as mine, which was worked out before I had occasion to read his book. I shall, however, refer to it in some footnotes.