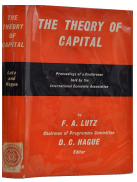

THE THEORY OF CAPITAL
Proceedings of a Conference held by
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F. A. LUTZ Chairman of Programme Committee
D. C. HAGUE Editor

SUMMARY RECORD OF THE DEBATE

BY

DOUGLAS C. HAGUE

THE DISCUSSION OF PROFESSOR LUTZ'S PAPER

Professor Lindahl introduced the paper by saying that it aimed to re-incorporate into modern, model-building capital theory some elements from the older classical theory. Professor Lutz had said in his paper, 'What we require are growth models based on something like the old production function which took care of these flexibilities. It is a sign that this feeling is shared by others that some recent model-builders seem to be moving back in this direction.' [1] Professor Lindahl sympathized with this approach. He thought it was very useful to study some parts of capital theory in a more simplified manner — as the classical economists had done — for that would clear up some of the controversy in modern discussions.

Professor Lindahl said, to begin with, he would take the opportunity to comment on a problem that was not solved in the classical theory and which was still somewhat controversial, the problem of the relation between saving and an increase in the value of capital. He would use a simple Wicksellian model, given in Fig. 20, which was basically the same as the one Professor Lutz had used in his exposition of Wicksell's capital theory in his book *Zinstheorie*.[2]

All who knew Wicksell's *Lectures* would remember the simplifying assumptions. One commodity, wine, was produced by inputs of labour on free land, and the quality of the wine improved with the length of time for which it was kept. The capitalist-entrepreneurs stored each year's vintage, paying wages for its production and receiving interest on their capital as the increase in value of the matured wine sold on the market. The society was a stationary one, the number of barrels of grape juice stored each year being the same as the number of barrels of matured wine sold. The $O(t)$ curve, showing the increase in value of the wine as a function of the time of investment, was given. If the time of investment, which was an expression of the *quantity* of the capital, was known, all other magnitudes could be determined. The rate of interest corresponded to the marginal productivity of time. Total wages were equal to the discounted value of the matured wine to be sold, and total interest was the difference between total output and total wages. The total *value* of the capital was equal to the total interest accruing in a period, capitalized at the prevailing rate of interest.

[1] P. 17. [2] (1956), p. 32.

Professor Lindahl said that Fig. 20 showed two static equilibria, with different amounts of capital. The rate of interest was determined by the highest curve for compound interest that touched the $O(t)$ curve at the point corresponding to the time of investment. It had fallen from r_1 in the first situation to r_2 in the second. Total wages had risen from $OK=MA$ to $OL=NE$, and total interest had fallen from AB to ED. On the other hand, the total value of the capital had increased from $OKBM$ to $OLDN$.[1] As saving in this case meant the postponement of

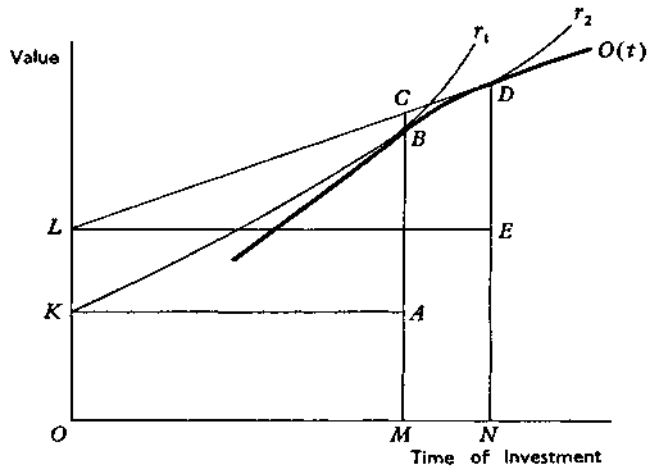

FIG. 20

consumption of the wine until it had reached a greater age, implying an increase in the quantity of stored wine, the total amount of saving necessary to arrive at the second equilibrium was shown in the figure by the area $MBDN$. This area was smaller than the area representing the increase in value of the capital, the difference being the area KLD. This latter area represented the increase in value of the stored wine as a result of the fall in the rate of interest. It seemed appropriate to call such an increase a *gain* which was not included in saving *ex ante*. Only a small part of this total gain, referring to the wine stored in the income period and which could be called 'income gain' (for example the area BCD), was included in

[1] Let K be the value of the capital, I total interest, W total wages and r the rate of interest. Then one could write

$$K=\frac{I}{r}=\frac{We^{rt}-W}{r}=\frac{W}{r}(e^{rt}-1).$$

On the other hand, the capital value, as an area limited by the interest curve, was obtained by the equation

$$K=\int_0^t We^{rt}dt=\frac{W}{r}(e^{rt}-1),$$

which thus led to the same result as the first equation.

saving *ex post*. The other part of the total gain, the area *KLCB*, was usually called 'capital gain'.

This model was very primitive, but made it clear that capital accumulation and saving were not the same thing. Income gains had to be added to saving *ex ante* in a certain period in the calculation of saving *ex post*; and this latter magnitude had to be augmented by the capital gains in calculating the total increase in the value of capital in the given period. (When the gains were negative, they were losses.) This statement was valid generally and was of some importance in a more refined analysis of the process of capital accumulation.

Commenting on page 6 of Professor Lutz's paper, Professor Lindahl wondered if there really was any difference between the Wicksell and Fisher methods for determining the investment plans of individual entrepreneurs, provided one recalled that Wicksell assumed a credit market. However, the most important part of Professor Lutz's paper was its critical survey of modern types of model. Professor Lindahl sympathized with Professor Lutz's views here and in any case those concerned could reply for themselves. He concluded by saying that this type of capital theory (the Böhm-Bawerk-Wicksell type) was familiar enough to the Austrian, German and Swedish schools, but not the Anglo-Saxon and French economists. Wicksell had been greatly disappointed because he got no response from his great contemporaries Marshall, Walras, Pareto and J. B. Clark. Was it too much to hope that at last there might be some little rehabilitation of this type of capital theory ?

Professor Fellner suggested that the Wicksell-Fisher methods only gave the same results if perfect competition was assumed; then one could not have any monopoly rent or windfall profit because firms would be in full equilibrium. A credit market was also necessary if firms were to be able to equalize opportunity costs. Commenting on Professor Lindahl's diagram, Professor Fellner pointed out that it was assumed that the discounted value of the wine equalled the wage bill at time zero. If this were not so, the present value of the wine would include a monopoly rent, and this would be shown by the fact that the intercept of the interest-rate line would exceed the wage bill, as measured on the ordinate. But even in that situation the tangency of the interest-rate line with the $O(t)$ curve would mark the equilibrium.

Professor Samuelson continued the discussion on the alleged difference between the Wicksell and Fisher approaches. With perfect competition in the capital market, one could arrive at identical decisions, because then the firm could maximize either the present discounted value of its output or the internal rate of return. For the marginal unit in perfect competition, one would get the same result — an internal rate of return equal to the external market rate. With imperfect competition in the capital markets, however, new criteria were needed. For example, when would a farmer with no trade consume his own product ? It would be very odd if to maximize internal rates of return meant maximizing utility. Returning to perfect competition, however, Professor Samuelson said he could

not understand Professor Lutz's remark on page 6 about G. F. Shove. A higher real wage could only be paid by a Fisher-Jevons perfect competitor if the interest rate were cut sufficiently to permit him to use a period of production which was long enough to give a marginal discounted labour productivity high enough to equal the new wage. Professor Samuelson did not think Shove would deny this.

Mr. Kaldor said that while reserving his general position on the significance of classical and neo-classical capital theory, he would like to make one or two specific comments. First, it seemed to him that the Wicksell approach of maximizing the rate of return on investment was more useful as a mere *modus operandi* than the alternative method which operated with a marginal rate of interest on borrowed capital. The reason was that there were few economic units where the rate of profit on new investments varied with the scale of investment. It was usual to assume that individual entrepreneurs took the rate of profit as something which did not vary as a continuous function of investment. If one drew two curves, one a declining marginal efficiency of capital curve and the other a horizontal curve giving the (constant) cost of borrowing, one had the Shove situation. It was perfectly correct to say that in this situation changes in wages would not affect techniques, because the rate of interest was constant. More typical, however, was Ricardo's situation and indeed that of the classical economists generally, and of course of Wicksell, which assumed that, whether the rate of return on capital was declining or not, the capital at the disposal of the firm was limited. One had various marginal efficiency of capital curves and the entrepreneur was limited in his borrowing power by his own capital. By going to the capital market, the entrepreneur could borrow only within his borrowing capacity which was some fraction of his own capital. In this situation the rate of interest would have no effect at all on techniques, and this seemed much more typical of the real situation than the model underlying Shove's views.

Professor Samuelson said he did not want to discuss which model of imperfect competition was the most useful one. Wicksell had been concerned with perfect competition and there free entry was important. *Mr. Kaldor* replied that if one included in full perfect competition equilibrium unlimited capacity to borrow at a given rate of interest, and if one also included constant returns to scale, no equilibrium was possible. This kind of situation had never been part of even the most comprehensive definition of perfect competition. Nor had it ever been realistic to assume that an individual capitalist had more than a limited amount of capital at any one time.

Professor Hicks said that whether or not Wicksell had been discussing perfect competition, he certainly *was* discussing stationary equilibrium, so that the question of how much capital a firm could get hold of was not important. If a firm were below optimum size, stationary equilibrium would not be achieved, and a situation of stationary equilibrium where firms were unable to expand because of a shortage of capital was quite unrealistic.

Professor Samuelson said that the mathematics of Professor Lutz's footnote on page 6 would confirm the inverse relation between the real wage (expressed in new tree value units) and the lengthening of the period of production (t) for a competitive firm of the Fisher type maximizing present discounted value. The trick was to realize that under free entry the identity $V - C = O$ had to be respected : this relation for t, i, and the real wage would, together with the relation $i = f'(t)/f(t)$, permit one to solve for t as an increasing function of the wage — just as i became an inverse function of it. With land free, the price of wood would rise to wipe out losses or profits. With land scarce and earning rent, there could be no certain expectation of the reaction of t to a change in real wage, and hence there was no danger of the Fisher model contradicting one's reasonable expectation.

Professor Lutz wondered whether, if one did *not* consider stationary equilibrium and if one looked at the individual firm, this would lead to different results.

Professor Samuelson replied that if one considered the time patterns of investors in imperfect competition, someone investing in imperfect competition could take a shorter view than in perfect competition, where he would have to maximize his return. Thus Wicksell was not interested in subjective time preferences, whereas the Kaldor model introduced them.

Professor Lutz explained that he had assumed capital to be equally divided among entrepreneurs. On the basis of this and the further assumption that entrepreneurs maximized the internal rate of return, it was difficult to show how, in stationary equilibrium, the market rate of interest became equal to the internal rate. If one did not assume that all entrepreneurs were equipped with equal amounts of capital and assumed in addition that they would maximize the present value of profits, then the mechanics of the process which led to an identity between the internal rate and the interest rate became much clearer.

M. Malinvaud wanted to associate himself strongly with Professor Lutz when the latter said that the importance of capital theory was not only to describe, but also to set normative rules according to which investment decisions should be taken. The theory of the efficient allocation of resources told us that each enterprise, public or private, ought to maximize the present value of its profits at the market rate of interest. But, in practice, many firms had only a limited amount of funds to invest, and had to apportion this capital in the best way. Here the internal rate of return on the various projects provided a useful guide.

Professor Haberler commented on Professor Lindahl's view that, in the end, Professor Lutz supported Wicksell rather than Fisher. Professor Haberler felt he must point out that Professor Lutz had contrasted the emphasis on time in the Wicksell model with the emphasis on other things in the Anglo-American ones. He was not sure, like Professor Lindahl, whether the contrast was a valid one. It might well be that Fisher would accept the Wicksell model for, while Fisher had said little on wages in his

books, in his *Econometrica* article on marginal productivity theory [1] he had said that it was capital accumulation which put up real wages. So Fisher did deal with this problem after all.

Professor Haberler therefore thought there was no basic difference between the Fisher and the Wicksell approaches; any such difference was expository and little more. But there *was* a difference between the neo-classical (Fisher, Wicksell, Samuelson) approach and certain more recent approaches, such as those of Kaldor and Domar. Both Kaldor and Domar had much to say on investment but they brought in factors neglected by the classicists, for example, short-run considerations. The difficulty arose when they transferred what was valid in the short run to the long run. Mr. Kaldor, for example, accepted the fact of a constant capital-output ratio and then went on to say that it was not constant merely by chance, and that he believed that there was a mechanism of adjustment keeping it constant. From a business cycle point of view, this might be correct but he himself was convinced of the validity of the Wicksell model and thought that the capital-output ratio adjusted to the savings proportion and to changes in the production function. There was a basic difference here unless the new approach were confined to the short run. As far as empirical evidence was concerned, he could see no trace of constancy in the capital-output ratio, as it appeared in the statistics circulated by Dr. Goldsmith.[2] He saw cyclical and secular changes in it.

Professor Haberler concluded by saying that he thought the difference between participants was not a difference between the presence or absence of competition. He thought it went much deeper.

Mr. Kaldor agreed entirely with Professor Haberler that there was a basic difference between him and the neo-classical school. Compared with this, he thought, differences *within* the neo-classical school, like those between Marshall, Clark, Wicksell and Böhm-Bawerk, were relatively unimportant. They had tried to show how factor prices and distributive shares depended on the production function, and he thought this was all nonsense. Professor Haberler would know that he had himself at one time defended Wicksell from the attacks of Professor Knight. He was now convinced that all he had written in defence of neo-classical theory was wrong and that Professor Knight was right. The difficulty was that the neo-classical school tried to apply a generalized marginal productivity theory to the economy as a whole, a job which he now felt simply could not be done. Yet Professor Lutz, having clearly pointed to all the snags inherent in capital theory, ended up by pleading that we should keep the production function. In this he slid over the fact that all of his own argument made nonsense of any attempt to measure the marginal productivity of labour and capital as factors of production, let alone to show how any of this could explain wage rates, profit rates and distributive shares.

Mr. Kaldor pointed out that other people had thought neo-classical

[1] Fisher 'A Twin Dimensional Representation . . . ' *Econometrica*, October 1939.

[2] See p. 338 (footnote).

theory nonsense. Not only Marx and Ricardo had taken this view but von Neumann's growth model also showed that the neo-classicists were wrong. The difficulty was that the neo-classical school took so much for granted by using long-period stationary equilibrium models with no technical progress and some very extraordinary simplified assumptions. Professor Solow, for example, had one kind of machine and a given marginal rate of substitution between labour and machines. If prices changed, then production methods altered. Mr. Kaldor did not think it was true to say that the whole of economic science apart from himself was in favour of neo-classical economics. Even Sir Dennis Robertson belonged to the camp of the sceptics. Twenty years ago, in his *Wage Grumbles*, Professor Robertson had given the final objection, he thought, to the marginal productivity theory of distribution. When one was calculating the marginal productivity of labour, one had, for example, to say what happened when one added an eleventh man to ten men working with ten spades. Either one had to assume that ten spades could be used in varying proportions with the men, or else one had to invent a method of converting ten ordinary spades into eleven slightly inferior spades which would contain exactly the same volume of capital as the ten previous spades.

So, even in this abstract world, one had to make all sorts of unrealistic assumptions about the substitutability of 'capital' and labour. Not only did one have to assume given technical knowledge; in addition all the equipment in existence had to be supposed to have been produced with exactly the same technical knowledge. One also had to introduce unrealistic assumptions of substitutability between capital instruments of particular kinds and labour to make sense of it. Abstractions were inevitable but they must allow one to see how to advance from a more to a less abstract theory. In this instance, one never did get any further than a very abstract theory. The Wicksell and neo-classical theory in itself was completely isolated, and the question was how to remove the scaffolding around it when it had to apply to a world where techniques were always changing. Capital goods of particular kinds and labour were not substitutes for each other, and the nature of the capital stock depended on the different techniques being used at the particular dates when the various machines were made.

In reply to Professor Lutz, who asked whether Mr. Kaldor denied substitutability between capital and labour, Mr. Kaldor said that it had no relevance in determining the share of profits in incomes, the rate of profits, the rate of interest and so on. He agreed that one could go on using more and more capital per man, but this was not substitutability in the sense in which that term was used in a production function of the Cobb-Douglas type. Substitutability was not important in terms of prices in general and prices of capital assets in particular. The important thing was the state of capital accumulation already attained.

In the United States, where more and more capital was employed with the passage of time, there was nevertheless a decline in the capital-output ratio. The present state of the American economy was one where there

was an inverse relation between capital per man and the degree of roundaboutness in production: there was *decreasing* roundaboutness. On the other hand, in India, wages were low in terms of commodities and it would not pay to use bulldozers to make roads. The Americans *did* use bulldozers, yet the rate of profit was much the same in America as in India. It followed that the price of labour in terms of commodities was very important. But it did *not* mean that either the Cobb-Douglas or the Wicksell production function was necessary to any explanation of what happened.

Professor Domar said he was honoured to be in the company of a heretic. On the other hand, while he did not like Wicksell's theory, he did not think it nonsensical; nor were production functions useless. Time would show which approach was more useful. Mr. Kaldor had worked out one extreme case, but surely there was no point in insisting that the other side was engaged in propagating nonsense. Perhaps one side was more right than another, but both were very likely to be partially correct. On the use of bulldozers in India as compared with more developed countries, Professor Domar felt that the rate of interest was not here the determining factor. Technological progress had reduced the cost of machinery in relation to labour in advanced countries. In the United States, a typewriter was quite cheap compared with the salary of a typist. In a less-developed country, the situation was reversed. These relationships provided a better explanation of the use of capital equipment than Mr. Kaldor's statement about bulldozers.

Professor Hicks sympathized with what Professor Domar had said and pointed out that Mr. Kaldor's remarks about the difficulty of substituting capital for labour were unnecessary to the basic position, largely for the reasons which Professor Lutz had quoted from one of his own earlier writings.[1] The effects of changes in the commodity-mix were much the same as those of changes in factor proportions. Professor Hicks added that, although he believed in a world where there was more substitution between capital and labour than some people thought, he did not think he could go back completely to his earlier view. He still agreed with what he had said in the early work quoted by Professor Lutz, but would now make the important distinction that one could believe in a production function without being thereby obliged to believe in the marginal productivity theory of distribution. Whereas with a Cobb-Douglas production function of the first degree the product was exhausted when each factor was paid its marginal product, one had some product left over if the indices added up to less than one. If they added up to more than one, the whole theory broke down for there was not enough output to give each factor its own marginal product. One could not believe both in marginal productivity theory and in all the various kinds of production function.

Professor Solow wondered if it was possible to discover what precisely was the bee in Mr. Kaldor's bonnet. Was it the continuity assumption, or was it the capital-in-general idea? Professor Solow claimed that von

[1] See p. 12.

Neumann was not a friend of Kaldor, but was on the side of Professor Solow himself. Other members of the Round Table, for example M. Malinvaud, could pursue the point that the pricing side of von Neumann's model contained assumptions which took us back to Menger, Walras and the marginal productivity theory. Continuity was not essential to marginal productivity theory.

On capital, Professor Solow was not sure whether or not he agreed with Mr. Kaldor. The idea of capital-in-general was a matter of pure convenience in a production function ; no one had ever seen anyone buy or produce capital. What people bought or produced were capital goods. For most of the neo-classical economists, the production function was a relation between inputs and outputs in unambiguous physical terms. He therefore thought it was reasonable if we simplified, and described output as a function of capital and labour. We could do this in several ways, as for instance when we said that we could ignore the heterogeneity of labour in the same way as we ignored that of capital goods. We could therefore suppose that there was only one kind of capital, of labour and of output, which left us building up a theory from these three things, whereas Ramsey had only had two — grain and food. But in this way all theory became an allegory. Alternatively, we could do what Professor Champernowne had done and try to find under what assumptions we could get precise definitions of quantity of output. Both methods seemed valid to him.

On distribution theory also, everyone agreed that the wages of labour, the rate of interest and the price of capital goods were determined by supply and demand. What then lay behind these supplies and demands ? The main thing was the basic fact of life that people could make technological choices. Granted that there was a choice of techniques, one of the sets of forces determining the relative demand for inputs was the possibility of substituting cheaper for more expensive ones. Two difficulties remained. First, there was the analytical problem to be considered in a paper in *Econometrica* (April 1959) by Leif Johansen. Johansen supposed that at the time when investment was actually carried out, there was a choice about how labour-intensive the investment should be. But once the capital good had been produced, its complement of labour had been fixed and could not be changed, and one therefore had a progression of *layers* of capital equipment. This made life difficult, but not impossible. The second question was how people behaved in making capital investment when they could not foresee the future and when techniques and prices might well change. Any plausible explanation of this could be incorporated into neo-classical theory.

Professor Jöhr asked Mr. Kaldor what central idea he would introduce into his theory of distribution in place of marginal productivity.

Mr. Kaldor replied that the theory of distribution was essential to economic science, but the crucial problem was not whether one should introduce technical choice. There was *always* technical choice, but his point was that this was of no importance so far as changes in the rate of interest were concerned. Mr. Kaldor said he only disagreed with Professor

Solow in that he felt von Neumann's model represented a different way of dealing with the same problem as he was interested in. He agreed that linear programming was a different way of determining technical choice. Morreshima and Solow used similar equations to his own, while Marx eliminated labour altogether, each man being represented by his wage. The essential feature of von Neumann's theory was that, like Marx, he assumed constant real wages. So long as the conversion ratio between labour and commodities was given, then von Neumann's model would give us determinate prices. Mr. Kaldor did not agree that von Neumann's model was merely Wicksell, Marshall or the whole neo-classical school in a new guise. Von Neumann assumed that real wages were constant, so that profits depended on the excess of goods produced over the amount of input required to produce them. Mr. Kaldor held that his own theory was a macro-economic one and therefore different from the neo-classical theories.

Professor Solow found it difficult to say anything as confidently as Mr. Kaldor did. It was true that in the von Neumann model there was no problem in determining real wages, since labour would produce if fed with commodities in given amounts. It was equally true that one could insert into von Neumann's model a view of labour as a primary, non-reproducible factor and so preserve the notion of marginal value product.

Professor Nakayama said that Professor Lutz distinguished macro-economic and micro-economic treatments of capital. Surely the two approaches did not differ radically for both had to deal with the same problem. How would Professor Lutz approach the distinction in the light of this ?

Professor Lutz replied that the distinction was a rather fluid one and the dividing line was not very clear. Böhm-Bawerk's theory was at least partly a macro-economic one. Professor Lindahl's approach was entirely micro-economic, as was Professor Hicks' in *Value and Capital.* Since one could not carry the whole apparatus along the whole time, one had to choose certain important aggregate concepts in such a way that nothing of significance was left out. Views on what was important might differ. For example, he himself thought substitution between capital and labour important but, if there was disagreement, what did one do ? For judgments about what was important affected ones choice of aggregate concepts.

Professor Nakayama pressed his point. If one took the Wicksell theory of factor pricing, Wickell's idea was not very different from that of Böhm-Bawerk. Again, Walras had a theory of marginal productivity which really represented one kind of macro-economic system. Therefore marginal productivity theory could be interpreted as macro-economic, and he himself could not see much difference between macro-economic and micro-economic theories.

Mr. Kaldor replied to Professor Nakayama. He said that all such distinctions were arbitrary and one could not give *the* distinction. There were models of general and of partial equilibrium, but not every general equilibrium model was necessarily 'macro-economic'. He himself would

say that a micro-economic model was one which depended on the method of scientific individualism — aggregates being merely the summation of individual decisions — while a macro-economic approach made use of some overriding condition operating on the markets, which restricted the individual rather than vice versa. For example, Ricardo assumed wages given in terms of corn and therefore assumed a constant ratio between wages and corn prices which had far-reaching effects on equilibrium relations between prices, etc. Similarly, Keynes held that because savings equalled investment, total savings could not increase without some prior increase in capital outlay, so that one had a new explanation of what happened when *individual* savings increased. For that reason, the Keynesian theory of savings and investment was essentially macro-economic.

Professor Hicks commented that Mr. Kaldor's doctrine was a very extraordinary one. He was saying that macro-economics comprised those parts of economic theory which led to paradoxical results. Surely Keynes' theory was macro-economic simply because it dealt with aggregates. Yet Keynes' consumption function, for example, depended on individual decisions, which were in a sense micro-economic.

Professor Lutz replied to the discussion. He said there seemed to be some agreement on the question of profit maximization as seen by Fisher on the one hand and Wicksell on the other. Both led to the same result in stationary equilibrium, but if equilibrium were not stationary then they gave different results. If he had to choose, he would choose the Fisher/Keynes criterion. Professor Lutz pointed out that Wicksell was not interested in the capital-output ratio. What interested him was the total stream of output from investment. Professor Lutz suggested that without a production function there was a lack of flexibility in a system of macro-economic models ; important variables were left out. This was a matter for judgment, but he was anxious that we should hesitate before we decided to throw over all the teaching of a hundred years. He was very concerned for economics if future developments proceeded mechanically on the basis of so few assumptions. It was true that it was hard to measure capital, yet in Mr. Kaldor's model in the *Economic Journal* article, the quantity of capital also played an important rôle despite the difficulties of measuring it. He did not see what else one could do. Very few concepts were precisely measurable, but one just had to pretend that they were. No one objected, for example, to economists talking about the real wage rate or the price level ; similarly one had to use the term capital as if it were accurately measurable and leave things at that.

THE DISCUSSION OF PROFESSOR HICKS' PAPER

Professor Solow introduced the paper. He said that in dealing with economic questions most of us chose to evade the index number problem, assuming it away. In this paper, however, Professor Hicks addressed

himself directly to this problem ; it was a problem because, as with the problem of good and evil, there was no answer, except in cases that were so special as to be uninteresting. The aim was to describe a list of objects by a number. It was as though one was trying to obtain a single number to denote a Sears Roebuck mail order catalogue with 1500 pages each 1 foot square, and with pictures of various articles for sale — ranging from women's dresses to donkeys and mules. In attempting to describe this by a vector, or number, one invariably had to take short cuts. Professor Hicks started by taking the situation where one list was bigger than another, as in Fig. 21.

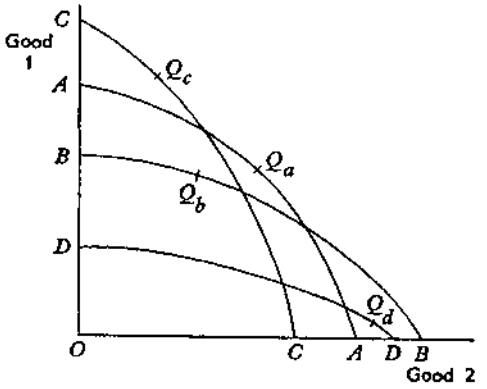

FIG. 21

This was a purely static situation, with two bundles of commodities viewed as outputs. One could show, with only two commodities, why there was no answer. Suppose the Sears Roebuck catalogue was represented by point Q_a in Fig. 21. At any time, the production possibility curve for 'given techniques' was convex, as with AA. There was also a different, but similar, curve for situation B, with B-resources and B-techniques. The standard way of deciding whether the bundle of commodities in the A- or B-situation was larger was to show whether Q_a was greater than Q_b. Since, in Fig. 21, Q_b lay within the production possibility curve, AA, the B-bundle was producible with the A resources and techniques, but the A-bundle could not be made with the B-resources. So, according to Professor Hicks, A was greater than B.

If, now, one took the point Q_c, one reached the standard paradox. The A-bundle was not producible with C-resources, nor the C-bundle with A-resources. Professor Hicks hoped that in many cases the first situation would occur, so that comparison would be possible. But even at best, where the index number comparison *was* possible, it could not generally produce a valid ordering because one could find a situation where, in the same diagram, A was bigger than B, and B was bigger than D, but A and D were not comparable. Here one could not say that A

was bigger than *D*. The same thing could, of course, be shown in terms not of production possibility curves, but of equal product curves.

All this was purely static, and Professor Hicks went on to extend his reasoning to production processes over time which used durable assets. The nature of this problem was well known. One had two kinds of input — capital goods and resources — and two kinds of output — consumption and capital goods. The analysis had to include the *remaining* capital goods among the outputs at the end of the period.

Professor Hicks interjected that it was important to concentrate attention on production within a given period of time, which was a self-contained unit.

Professor Solow continued by explaining that the first mention of this situation was in von Neumann, where production took one unit of time and led, as output, to one-period-older machines. One could proceed in the same way. With two stocks of capital goods, *A* and *B*, the question was whether the stock was bigger or smaller in the *A*- or the *B*-situation. The capital stocks could be regarded as outputs of preceding periods or inputs of the succeeding period. If one compared two capital stocks *A* and *B* as outputs, one could consider the process of production which led to the *A*-stock of capital goods. Could that process have been directed otherwise, so as to give the same consumption and to produce the *B*-bundle ? Here again, we might find an incomparability problem. In his paper, Professor Hicks wrote as though there was only a single process preceding the one considered. But one had to consider all such processes, and not only the actual preceding one. Would any of these give an unambiguous answer ? Then, too, the relation that resulted was not a complete ordering, and would not yield consistent results for more than two capital stocks. Other problems arose from the attempt to measure in terms of preceding or succeeding processes, and from the length of time considered. If this method were to be useful, a long period would be needed, but that, in turn, introduced other difficulties.

A further problem was the time-shape of the consumption flows while capital stock was being turned into the *A* or *B* forms. There were three possibilities. First, one could ignore the time-shape and look only at the 'pure time integral'. Second, one could insist on an exactly identical time-shape. Third, one could insist on the same utility over time — on an inter-temporal utility function. The first idea seemed useless for a lengthy process. The second, Professor Hicks rejected because it was unfair. One ought not to insist that a capital stock making cars should be able to produce mediaeval palaces. This left us with the utility comparison, which was difficult. Different societies or situations might give us different sets of tastes, which led Professor Solow himself to favour method two. It was true that there was the problem of adaptation, so that the moral was to limit ourselves to comparisons of capital stocks which were not very different in the first place. One could not compare New Zealand in 1950 with Peru in 1200.

Professor Hicks said that Professor Solow had covered the main points

in his paper, so he would merely make two comments. He fully agreed that, as a matter of general principle, all these comparisons were intransitive, but our problem as economists was to try to distinguish those comparisons which gave most hope of an approximation to transitivity. We should try to isolate the comparisons which gave coherent results from those which did not. We might conclude that none of the comparisons had any prospect of coherence at all, and this was a possibility which we must allow ; but he was not quite so pessimistic as that. He was inclined to think that some comparisons were fairly coherent, though not all were, and we might find ourselves talking nonsense.

Professor Hicks' reason for rejecting the second alternative — the condition that identically the same consumption goods should be turned out by both processes at exactly the same dates — was that because, if it were maintained strictly, he did not think that even the closest comparisons could survive. We must therefore loosen it up a bit, and there was no way of loosening-up that had no reference to some kind of preference on the utility side. He felt that the utility reference was a somewhat extraneous influence, and so he disliked relying on it, but we could not dispense with it altogether.

Professor Hoffmann wanted to know if the same basic point did not apply to the labour stock. A production function was relevant only to a given time. This recalled Professor Fellner's comment, in his book, that it was nonsense to talk of a capital-output ratio without explicit reference to time.

Mr. Kaldor explained that this was because a stock was divided by a flow, and any flow must be for a given period of time if we were to obtain an intelligible answer. With capital we could talk simply of a stock, but we were always concerned with labour as a flow.

Professor Hicks held strongly that, in principle, with heterogeneous labour the amount of labour applied must be measured with a time dimension firmly in mind.

Dr. Barna explained that he had spent a great deal of time measuring capital statistically and had found Professor Hicks' paper useful in helping him to clarify his ideas. He rather despaired, because the theoretical literature seemed to have so little connection with the empirical, but in some circumstances Professor Hicks' analysis led to very useful conclusions. The position was similar to that in the measurement of national income ; one could easily claim that measurement was impossible, yet over the last 15 or 20 years we had developed the empirical study of national income in useful ways. This was not yet true of the measurement of capital, partly because of the greater degree of difficulty, and partly because of the complete absence of any link between theory and measurement. The situations which the theorists studied were not important in real life, yet surely the theorists ought to be capable of dealing with real problems. He would make two points. The first concerned measurement in terms of input or output, while the second was on the measurement of capital by imputation.

Dr. Barna said he was prejudiced in favour of Professor Hicks' views. He had started his own analysis by looking at existing indices of prices of capital goods — for example, Dr. Goldsmith's; all these indices were similar, but he found them impossible to define in any logical terms. He found it necessary to use two alternative concepts, which he believed to be similar to those of Professor Hicks. He had called the one capital in terms of resources and the other capital in terms of its own productive efficiency. This difference between the measurement of input and of output was important. In 1940, Professor Hicks had written about measuring income in terms of production or in terms of welfare. The difference between the two methods of comparing income was not serious; production and welfare would generally move in the same direction. But in the field of capital there was a constantly growing gap between the two measures — with technical progress, the resources embodied in capital yielded more and more final output. So, whenever we measured the stock of capital over time we were bound to get a difference between the two measures. The fundamental need was to be careful which measure we chose, otherwise we should get biased conclusions. Professor Lutz had skated over this difficulty by understating the importance of capital measurement. We must make it clear whether we were using the input or the output measure. One could show that all the so-called indices of capital goods measured neither input nor output but something in between. The usual statement was that it was a price-index ignoring 'quality change'. Quality change was very difficult to define and it was much easier to define and measure Professor Hicks' two indices.

Dr. Barna emphasized the importance of measurement by imputation. It often appeared possible to construct an index at market prices and to use the chain method to arrive at the volume of capital values, but this led to fallacious results. Because of technical progress the nature of capital was changing all the time, inevitably involving the 'changing quality' problem. With consumer goods one had such problems where motor cars were concerned, but cars were really capital goods. A more typical consumer good was beer, the quality of which depended on its alcoholic content. An elegant O.E.E.C. paper by J. R. N. Stone had proposed proceeding on the assumption that at any given moment of time one had two qualities of beer, their relative prices giving the weights to be used. With consumer goods, the quality changes were reversible and depended on supply and demand conditions. There was no permanent tendency towards improvement of quality, and consumers might wish to raise or to lower quality at any given moment of time. In the case of capital goods, however, one had a continuous improvement in quality as the result of technical progress, though a reversal of this process was not inconceivable. So a comparison of different capital goods in terms of market prices was wrong. We must go back to imputation methods, even if market prices were available. One could easily show how the general method based on market prices went wrong. With two types of machine — a new one in the prototype stage, and an old one on the way out, prices

did not reflect either cost or utility. For example, if there were a long-term series for the prices of Parker pens, one could not compare the end of this chain with the beginning. When Parkers produced a new pen the price of the old one was halved, so that a long-period time series was nonsense.

Mr. Kaldor said that the advantage of a meeting like this was that it comprised two kinds of individual, namely those who made theoretical models and those who produced statistics. For, as time went by, the degree of communication between these two groups of people seemed to be becoming less and less. Could the theorists give some guiding rules to the statisticians ? Only rough approximations were possible, but we could ask what kinds of problem we should be considering. Mr. Kaldor said that at one recent conference he had attended, the economists had brushed aside the statisticians, whose problem was put by them as follows. One might be trying to find the American cost of living in terms of well-established products. If one were considering motor cars, for example, should one regard changes in wheelbase or in engine size as changes in quality ? The American statisticians did not, but the introduction of automatic transmission was treated differently. Should one allow the higher price of cars to which such improvements gave rise to cause an increase in the cost of living in the former case and not in the latter ? If so, what was the principle underlying the distinction ?

Many other examples could be given. If iron buckets were replaced by plastic ones costing only one-tenth as much, there was no effect on the cost-of-living index — the new bucket was regarded as a different commodity. Yet if the cost-of-living index was intended to show the monetary cost of achieving a given level of satisfaction, clearly one should use a method which would reflect this. The problem of capital measurement was an analogous one. There was no clear general principle by the aid of which the statisticians could explain to the economists what they were trying to do ; and the economists in turn could not tell them what they ought to do, except advise them to be guided by common sense.

In the case of capital measurement, the two extremes were clear enough and everyone was trying to do something in between them, but with no very clear notion where the 'middle' was. One extreme case was to assume that there was no technical progress in the production of capital goods but that these always required the same amount of real resources. This was obviously quite unrealistic. At the other extreme, one could say that a unit of capital was whatever unit was capable of producing a given output in a given year — ignoring both longer and shorter output streams. Here any distinction between the quantity of capital and its productivity was washed away by the definition itself. Any idea that capital might have varying productivities was lost ; its output was always constant. So both these procedures were inappropriate and this raised the problem of what the statisticians should do. Measurement had to assume some general rate of increase in the productivity of the capital goods industries so that units of capital could be measured in terms of

cost, corrected by this general change in productivity. The measurement of capital would not therefore lead to any special problem if the relative prices of different kinds of capital asset remained constant, but by the nature of the case they could not when technical progress was going on. In the real world, the problems arising over the construction of indices were serious, even though one could make some allowance for technical change, quality improvements and inventions, when measuring the quantity of capital. But there was no general principle underlying the construction of the index.

Nevertheless Mr. Kaldor did not feel that these difficulties of measurement should lead us to abandon all attempts at measurement. The possibility of measurement depended on the purpose for which the measures were required. For some purposes, the measures needed to be more exact than for others.

Both Professors Lutz and Hicks were inspired with a healthy scepticism and recognized the problems. Yet they managed to end on what, in the light of their own analyses, seemed to be an inappropriately hopeful note. Nothing in these two papers justified the optimism of their endings. The whole marginal analysis was born of Ricardo's attempt to explain the share of rent in the national income, and to show why some rents on some lands were higher than on others. Ricardo was driven from the extensive to the intensive margin in defending his explanation, and to arguing that one could in some sense vary the proportions between labour and land, on a given piece of land. However, in this case, at least, 'land' could be measured. One could measure it in acres and produce 'corrected' units, adding together different acres weighted by their relative prices. But the same could be done for labour and it *had* been done by everyone from Ricardo to Keynes, using relative wages as the basis of weights. With capital, the problem was an entirely different one, since there was no unit in which we could reduce capital to homogeneous units. Professor Hicks' hope of a vague measurability was not what we wanted. For some purposes both income and capital were usefully taken as monetary magnitudes, and there was no index number problem here if one regarded the monetary values themselves as the quantities to be investigated. If there were rapid changes in the value of money, propositions about money income and money investment were misleading, but otherwise the index number problem did not matter. It was possible to use money income and money capital without necessarily converting them into 'real' magnitudes.

Mr. Sraffa thought one should emphasize the distinction between two types of measurement. First, there was the one in which the statisticians were mainly interested. Second there was measurement in theory. The statisticians' measures were only approximate and provided a suitable field for work in solving index number problems. The theoretical measures required absolute precision. Any imperfections in these theoretical measures were not merely upsetting, but knocked down the whole theoretical basis. One could measure capital in pounds or dollars and introduce this into a production function. The definition in this case

must be absolutely water-tight, for with a given quantity of capital one had a certain rate of interest so that the quantity of capital was an essential part of the mechanism. One therefore had to keep the definition of capital separate from the needs of statistical measurement, which were quite different. The work of J. B. Clark, Böhm-Bawerk and others was intended to produce pure definitions of capital, as required by their theories, not as a guide to actual measurement. If we found contradictions, then these pointed to defects in the theory, and an inability to define measures of capital accurately. It was on this — the chief failing of capital theory — that we should concentrate, rather than on problems of measurement.

Professor Hicks was not quite clear about this. Did Mr. Sraffa mean to equate models with theories ? He could see that in a particular model one could only make that model water-tight by introducing drastic simplifications. Only thus, for example, could one have a clear and precise definition of capital stock. But some simplifications were so drastic that he himself was simply not interested in any theory based on them.

Mr. Sraffa replied that Wicksell's might be a simple model in that he worked out a simple and general theory for future development. Surely the usefulness of any theory lay in its explanatory value. Was one only interested in a theory if one could fit actual figures into it ; or was one interested independently of that ?

Professor Hicks argued that if a theory was to explain the working of the social mechanism, it ought to be capable of having measurable concepts fitted into it.

Mr. Sraffa took the view that if one could not get the measures required by the theorists' definitions, this was a criticism of theory, which the theorists could not escape by saying that they hoped their theory would not often fail. If a theory failed to explain a situation, it was unsatisfactory.

M. Malinvaud said he agreed with Dr. Barna about some of the difficulties raised by the measurement of capital, but these difficulties should not be over-emphasized. If properly used, market prices need not lead one to illogical results. One must, however, realize that market prices took into account all known future developments affecting the efficiency of capital goods and the utility of various products. Any measure based on market prices was bound to be 'forward-looking' according to the distinction drawn by Professor Hicks. This was only natural since the value of capital depended on its future efficiency.

The introduction of a forward-looking measure of capital into a production function did not make the latter a mere tautology, as Professor Hicks put it. When computing an index of aggregate capital, a proper weight for each separate capital good must take into account its future efficiency. Such a procedure seemed perfectly logical if the aggregate was intended to be inserted in a production function. This aggregation principle left the relation between aggregate capital, labour and production undetermined, and the production function which described this relation was still well worth knowing.

Professor Fellner thought that the principles underlying Professor Hicks' paper pointed to ordinal ranking; there was a gulf between these principles and the methods of statisticians, which implied cardinality. He thought it plausible to argue that when statisticians said that the capital stock was now larger or smaller than it had been, this kind of statement would stand up quite well to analysis in terms of Professor Hicks' principles. Where cardinal statements were made, it was impossible to get any check unless one agreed that the cardinal measurement of utility was possible. The fact that many of the statements of statisticians were essentially ordinal rather than cardinal diminished the gulf between the theorists and the statisticians.

Professor Fellner agreed with Professor Solow that if one took situations fairly close together in time or character one could insist on the capital assets in question producing the same goods. However, if the situations were far apart in basic characteristics then these differences entered with considerable force. But did not the attempt to escape the problem by bringing in utility raise difficulties over differences in tastes between two societies or two periods, and therefore a problem of reversibility of tastes? Surely *no* reasonably good answer was possible if the two situations were far apart.

Finally, if, in spite of everything, we were willing to accept the identical utility requirement, would we not have to say that we insisted on each individual member of each society getting exactly the same utility? We were comparing a real with a hypothetical situation (which was the main objection to the compensation principle) and that was the weak point of the whole approach.

Professor Jöhr said we had heard much of the difficulties of measurement, so perhaps we should stress that measurements were needed for various quite different purposes. As there was no ideal method, should not our methods be differentiated? We needed to measure capital-output ratios, and also the addition to capital stock which was (or was not) equal to savings. Then again, we needed to calculate the wealth of different societies. We also needed a marginal productivity theory, but there was no point in using this to explain profits. We should deduct profits first and then calculate the shares of labour and of capital by marginal productivity theory.

Professor Delivanis returned to the possibility of comparing capital on the basis of market prices and mentioned an additional difficulty. Market prices were based on the supposition that there were only isolated transactions and we all knew that in these circumstances prices would differ very considerably according to whether the asset in question was sold as a single unit or in a number of parts.

Professor Nakayama stressed that we usually measured capital not for its own sake but in relation to other aggregates. Thus measurement in Professor Hicks' sense was closely related to national income analysis or to welfare in general. In this connection we must remember that Keynes had to face the problem in his own way. Keynes' theory was apparently

confined to national income and had little to say about capital. Even so, Keynes could not avoid touching on the stock of capital at the beginning and at the end of the production period, although he avoided discussing it during the period itself. Consequently Keynes tackled the problem via the concept of user cost, which made possible the comparison of these two magnitudes — capital stock at the beginning and at the end of the period — and avoided all the problems of inter-temporal comparison.

Dr. Goldsmith was happy in general with Professor Hicks' paper. It provided some justification for the statistical procedures actually used and Professor Hicks gave more than he took away. However, Dr. Goldsmith did wish that Professor Hicks had gone a few steps further by framing his theoretical conclusions so that statisticians could obtain operational advice from them. Professor Hicks started, as he must, with imputed values, for often we could not use actual market prices. There were, however, some important types of capital for which market values were available — houses and automobiles, for example. Dr. Goldsmith agreed that the proportion of the total number of houses and cars changing hands in a single year was not large, but that would not matter if those which did change hands in a single year could be regarded as a random sample of the total. Of course there was no market at all in that sense for many kinds of government, industrial and commercial buildings so that in practice one had to use imputed values.

Professor Sylos Labini noted Professor Hicks' view that we could use the utility concept to measure the output stream. Were we dealing with two measures which did not supplement each other ? He would rather suggest that we were measuring one thing with two different standards — rather like using kilometres and pounds. A major problem was that of the time-shape of the output flow, but was not the main difficulty on the cost side ? He had no great enthusiasm for inter-temporal comparisons, for they led to vicious circles, as in oligopoly theory. One could attribute views to entrepreneurs which fitted the results but merely led to the conclusion that business men do what they do. Professor Sylos Labini was happy with the first two-thirds of Professor Hicks' paper ; the basic problem was how to find a fairly homogeneous method of allowing for time.

Mr. Thalberg said that in our theoretical models it was reasonable to think of capital as made up of homogeneous productive units. So in the real world too we tried to construct an index of capital input which we thought we could use *as if* we were dealing with homogeneous units of capital. Probably the best advice to give to the statisticians today was that they would come closer to the problem of capital as an input if they emphasized more thoroughly the durability and the depreciation of capital.

Professor Samuelson pointed out that a rigorous theorist could design a theory of interest rates or of pricing over time without bringing in aggregate social capital, only inputs and outputs as a series of vectors. When a theorist did this, he obtained, under conditions of perfect competition, a set of mutually interdependent pricing relations similar to those of the neo-classical theorist — Fisher, Wicksell or others.

Professor Hicks closed the discussion by saying that he was glad that his paper had led to such an interesting debate but would not take up the comments in any detail, since there would be further opportunities to discuss most of them. Instead he would concentrate on a few particular points. First, he emphasized strongly that the problem he had studied most was the measurement of capital as such and bore no particular relation to distribution. Whether one considered distribution or not, there was the basic question of how to value the capital stock. He had deliberately abstracted from distributional problems and also from any kind of price mechanism. He had studied principles for the measurement of capital in terms of input and output that would hold in *any* economy. The translation of these principles into price terms would be a second chapter which he was not going to tackle here.

Professor Hicks felt that this was an important exercise because the capital values used in practical studies were imputed values. He had been interested to hear Dr. Goldsmith suggest that houses had firm values. This might be true from the point of view of a statistician, but he would like to point out that many of his own comments had arisen from his experience of the practical difficulties of valuing houses for taxation or rating. It was clear that the most that could be done was to take actual transactions as a sample, and value houses which had not changed hands by analogy.

Professor Hicks said he was now aware, as a result of being able to stand back from the problem and also as a result of the discussion, that he had not made a sharp enough distinction between his two pairs of concepts — between forward-looking and backward-looking measures and between cost and utility measures. Since production came before consumption, one naturally thought of cost in a backward-looking sense and of utility in a forward-looking one. But the cost/utility and the forward/backward distinctions were not the same thing. He accepted the point, which M. Malinvaud and Professor Sylos Labini had made, that it was ultimately impossible to avoid introducing a utility factor into the backward-looking measure. He would, however, emphasize that he had been driven to introduce the utility element to take account not of the existence of different consumption goods but of the time-shape of the consumption flow. If one were using a backward-looking measure, this must first be in terms of a hypothetical process leading up to the date in question, which meant that one must use replacement and not original cost. Granted that, one still had the question of depreciation and of user cost. What bothered him was the correct theoretical basis of that using-up process, and he had reached the conclusion that the right thing was to base the argument on the stream of consumption goods which were, or which could have been, produced from the capital goods. It was this which brought utility into the backward-looking measure. Since this stream of consumption goods had a time-shape, and that time-shape was significant, we must introduce some sort of time preference, some utility element, to deal with it.

Professor Hicks agreed with Professor Fellner that his assumption of unchanged wants was a weak link in the whole construction, since it meant that only close comparisons were realistic. But if one could introduce consumption flows with a similar time-shape, how one discounted these might not matter much and might seem a more formidable difficulty in theory than it was in practice. Professor Hicks said that the theoretically important point which emerged in the last two pages of his paper was that a forward-looking measure of capital was in fact not a measure of capital at all, but a measure of product. If we said that the production function was such that when we used more capital, we got more product, then the product had to be defined in a sense which was correlated with a forward-looking measure of capital. But if capital was to be considered as the product of resources of given size, then we had to measure capital by a backward-looking method. To say the things we needed to be able to say, one had to be able to sort the whole problem out tidily and this was what he had tried to do in his paper.

THE DISCUSSION OF PROFESSOR SAMUELSON'S PAPER

Mr. Little introduced what he described as 'a very long paper with a high specific gravity', a successor to the article in *Oxford Economic Papers* where income had been valued in static conditions with all output consumed. The model considered one output which could go into either investment or consumption and it assumed radioactive depreciation. The paper examined various questions, the first being whether we should think in terms of gross or net capital formation, Professor Samuelson arguing in terms of net. This was theoretically simplest, because in the Samuelson model depreciation was easily measured.

The second question involved a quality change in the capital good. Suddenly, instead of capital good K_1 we got capital good K_2, with twice the productivity, but produced from the same inputs. What happened? The old K_1 stopped being produced and an old K_1 now became worth half as much as a new K_2. The question now was what happened to net national product at various times and, most important, how could we measure it at all future dates. Gross product now became $f(K + 2K_2, L)$ instead of $F(K, L)$. Professor Samuelson subtracted from this, which was gross output, full depreciation at the 'radio-active rate', (M), old machines being valued at half their original cost or second-hand market value. This seemed perfectly correct. Before increased production became available, there would be no increase in gross national product, but net national product would rise instantaneously because it now needed only half as many resources to maintain capital intact. This was Professor Samuelson's answer to the old problem of how to maintain capital intact.

A third question dealt with in the paper was that of allowing for a

perfectly foreseen and constant rate of inflation. Professor Samuelson's argument on this point was not too easy to explain shortly. What happened was that the market rate of interest was inflated by the rate of increase of prices. If everyone knew that inflation existed, it could be taken for granted that this would happen. Next came the question how, in theory, firms could pay this inflated rate of interest. Did not inflation over-exhaust the total product ? The answer was that it did, unless one included capital gains on the income side ; or perhaps one should rather say that the capital term $K\frac{dp}{dt}$ *was* actually income. In this situation, firms would be permanently short of cash, but the answer seemed to be 'so what'. Their position would not deteriorate if they continually increased their borrowing at the same rate as inflation was going on.

The whole of the second half of Professor Samuelson's paper was concerned with a different problem, namely what was the proper measure

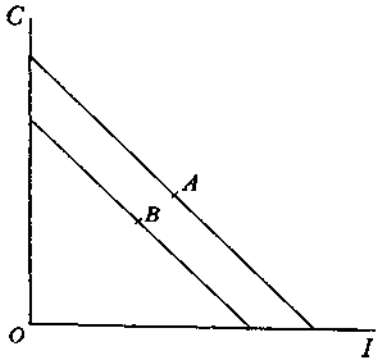

FIG. 22

of income. Professor Samuelson reached highly destructive conclusions. In Fig. 22, showing production possibility curves with consumption and investment goods on the two axes, one society was at position *A* and another at *B*. The problem was now the old one of how to make consumption and investment commensurate for national income purposes. For one did not want investment goods for their own sake. Fig. 22 showed a very strong case where, at *A*, there was both more investment and more consumption than at *B*. By any normal measure of income, people were unambiguously better off at *A*, yet Professor Samuelson was not happy even with this. He felt one could not really give a preference weighting to investment and consumption and so make them commensurable, because one society might have much more rapidly diminishing returns to capital than the other. The society with less investment and less capital might soon become better off. In other words, Professor Samuelson insisted on bringing future prospects into the discussion, and the upshot was that we must measure not income but wealth — in the

sense of the present discounted value of all future consumption. Professor Samuelson recognized that this could not be done in practice but, nevertheless, claimed that it was what *should* be done.

Mr. Little said that this was where he parted company with Professor Samuelson. He would agree that prospects were important but surely so were current standards, and he wondered whether one could not after all measure current standards. Clearly it was not possible just to use consumption; one must bring in savings or investment. This was what led to the question how to measure savings or investment. The first point was that if one wanted to take something as a welfare measure, why restrict oneself to one single figure. It was not necessary to combine consumption and savings. The second point was that one could look at both together by making an assumption which would make them commensurate. A number of assumptions would be more plausible than assuming that everyone knew exactly what was going to happen in the future. The simplest assumption seemed to be that savings could be treated like consumption. Was that really playing indefensible tricks with people's psychology? In support of his welfare measure, Professor Samuelson did say that all decisions were really about wealth, whether personal decisions or decisions in political economy. Mr. Little did not see that this was an argument against there being some value in measures of current standards of income or welfare. It was not only a question of taking decisions, there was also the problem of description.

Professor Drewnowski thought the central problem of Professor Samuelson's paper was on page 47 where he gave alternative answers to the simplest economic problem — of a static Robinson Crusoe one-period world. There was a close analogy here with what Polish economists had called the general theory of the planned economy, though more recently they had abandoned this idea. The similarity consisted in the fact that the central problem of the planned economy could be put in a diagram similar to Professor Samuelson's Fig. 4a. Professor Drewnowski did this in Fig. 23. Here the production possibilities curve *CI* was confronted, in a planned economy, with the indifference curve of the state. The state would choose an equilibrium position for production on the basis of this indifference curve, which gave a measure of collective utility. Polish economists had also developed the idea of objective and subjective waste, where the subject was the state and not the consumer. If the economy was at any point within the area *OCI* (as at point *A*) there was objective waste. At point *B*, there was subjective waste. The economy was on the production possibility curve but not at a position of equilibrium. During the last two years this question had reappeared in economic discussion in Poland and the problem of state preference as the criterion of policy was being considered.

Dr. Goldsmith wondered where this theoretical discussion led. Professor Samuelson's conclusion was clear so far as the measurement of income was concerned, but what were his conclusions for the measurement of the stock of capital? He was tending to substitute wealth measures for

income measures, but did he want us to give up measuring the capital stock in view of the criticisms made in his final two paragraphs ? If not, how could the statisticians commit as few sins as possible against Professor Samuelson's theoretical precepts ?

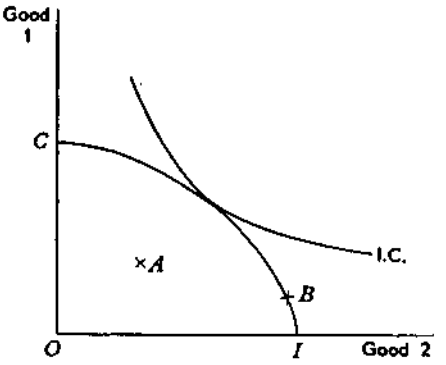

FIG. 23

Professor Samuelson replied that he could not give the statisticians too much comfort. When he had considered depreciation, he had pointed to the need to use one of the least pleasant measures — market values. For there were not enough transactions to give accurate measures of market value. Professor Samuelson did not think that the theorists should throw cold water on the work of the empiricists, but the theorists did have a duty to tell the truth and, if necessary, to announce bad news. They could then give some pointers to the empiricists who should at least know what they were trying to do and how useful it was. Some national income statisticians were, after all, nihilists in their theoretical welfare principles. Professor Samuelson said that what he had done here was not intended to be operational. The idea was to take all future prices and all future consumption and apply his principle to them.

Dr. Goldsmith stressed that his only question was whether what had been done in the past was of no use at all. When he had read Professor Samuelson's paper, he had decided that what Professor Samuelson regarded as the right thing, simply could not be done. But measurements had to be made, and was it really true that national income figures were theoretically worthless ? Even Professor Samuelson used them in his policy recommendations. Dr. Goldsmith wanted to try to get the theorists to say on what lines one should compromise and to show how one could achieve a reasonable approximation to what they regarded as the truth. What the practical statistician wanted to be told was which, among a number of imperfect estimates, were the least imperfect.

Dr. Barna supported Dr. Goldsmith in asking for a two-way traffic between the theorists and the statisticians. The rôle of the empiricist was to evaluate the accuracy of the theorists' assumptions. On whether

one should use gross or net income, Dr. Barna suggested that the difference was not quantitatively very important in practice. For example, the capital-output ratio was little different whether one calculated it as a gross or as a net concept. It was not true that all statisticians preferred to work with gross concepts; European statisticians traditionally preferred net concepts but had been persuaded to use gross ones by their American colleagues. In any case, there was more than one net concept and Professor Samuelson had eliminated the whole question of which was best by using his notion of radioactive depreciation. The two most obvious ways of arriving at net investment were (a) to deduct from gross investment capital actually withdrawn from use; (b) to deduct depreciation as calculated by the accountants. Dr. Barna did not think it made much quantitative difference which of these concepts we used, and it was not at all clear how Professor Samuelson's radioactive concept fitted into this particular discussion.

The concept of capital ran through the whole of Professor Samuelson's paper. Professor Hicks had pointed to two basic concepts for the measurement of capital, one measuring it in terms of resource inputs and the other in terms of productive efficiency. Professor Samuelson dealt entirely in terms of the second concept, and his paper was therefore restricted in its practical application. As Professor Hicks had said, it made the Cobb-Douglas theorem a tautology; most of the neo-classical literature had worked in terms of the other concept. Professor Samuelson dealt with the problem of efficiency by valuing all capital in terms of new capital. No one had measured capital in this way in any time series, and if that were done Dr. Barna did not think the results would be very interesting. In his own paper there was a chart very similar to Professor Samuelson's Fig. 1 where he had assumed all capital to be revalued in terms of new capital.

On the income and wealth concept, Dr. Barna suggested that one should refer to the Appendix to Mr. Kaldor's book on *An Expenditure Tax* where the argument reached similar conclusions, namely that it was much harder to define personal income than social income. The corollary was that it was more difficult to evaluate social than personal capital.

Professor Samuelson suggested that his footnote on page 48 bore on this problem. He wished to correct the impression that he had said that we ought to tax the windfall gains resulting from a rise in the price level. The government could tax money interest if it wanted, or instead, real interest which represented real income. The ideal situation in which capital gains should be taxed was where these were due to a foreseen fall in the rate of interest. In other words, one should tax certain rather than uncertain gains. This idea also was non-operational. Professor Samuelson asked Dr. Barna why he thought it uninteresting to consider depreciation in terms of the Samuelson method. So long as one worked within this model, one had a single mortality rate which was a technical fact.

Dr. Barna replied that in the Samuelson model depreciation was not depreciation but mortality. In other words, it resulted from accident

and not from the age of assets ; efficiency was constant over the whole of their lives.

Professor Samuelson explained that his model used a survival curve, and was the only model in which there was no heterogeneity of capital goods. He had been able, with his methods, to avoid the problem of whether one was dealing with four- or five-year-old cars, or with different qualities of land as in Ricardo's theory. The model was so simple that everything was kept under control. Professor Samuelson said that in empirical work he would use a model which had three or four capital goods and in which age was the factor leading to mortality. Perhaps one would then have to put greater weight on market values as indicators of a foreseeable future, and we should be back with the Fisher model.

Professor Domar wondered if we could not consider two questions. First, how should one value the stock of social capital ? He would suggest that, for the purpose in hand, current market prices should be used. Second, there was the choice of the depreciation method, and he thought that the two choices were not necessarily interdependent.

Professor Samuelson said that the paper joined up here with the morning's discussion. One had valuation of assets in terms of hopes of the future as markets reflected them.

Mr. Kaldor had great sympathy with Mr. Little's final point. It was wrong to think that there was ultimately any single, right way of defining concepts. This was particularly true when one was concerned with the notion of income or welfare over time. Professor Samuelson's reasoning reminded him of the problem he was dealing with in his book *An Expenditure Tax*, namely, how to define income and how to define maintaining capital intact. He could see a similarity between Professor Hicks' position and that of Professor Hayek. Professor Hicks took the view that income was that permanent stream corresponding to any particular pattern of receipts or consumer expectations over time. This was analogous to Hayek's idea of maintaining capital intact. It followed that income was the magnitude which could never be expected to change, because any expected change was allowed for in the definition of income. This was also Professor Samuelson's position. One could not say that nation *A* was unambiguously better off than nation *B*, if one was aware that *A* lived on physical resources which were known to be exhaustible. So one could not be sure that *A*'s income would be greater than *B*'s in the long run. But surely if we could not say whether *A* was better off than *B*, this implied that we could not say that any person or nation was better off over a certain period than another. If so, we ended up with nothing at all.

One naturally asked a question such as this : Is a millionaire with an expectation of 5 years' life better off than a poor man who has 20 years to live ? Mr. Kaldor suggested that we must bring in a time period, and take into account the rate of accumulation of goods and services over that period. Perhaps we should also allow for the fact that in some cases an increment of wealth might add to income because people desired wealth

just as much as they desired bread or wine. He would therefore be content to say that the welfare of A was equal to the welfare of B, when he was satisfied that this held true for whatever chosen period of calendar time we had agreed to use.

Professor Samuelson felt that a paradox remained, because Mr. Kaldor was ignoring the virtual character of the situation. If one took production plateaux and compared them, one saw that virtual character. Professor Drewnowski had brought up production analyses and the relation between countries A and B in Fig. 4a. This would be easier to relate to the morning's discussion in terms of production, but figures for two countries represented a particular case of non-transitive comparison. One could find many such cases.

Mr. Kaldor replied that this was precisely why Mr. Little had picked on a strong case and went on to ask whether there was not a time horizon beyond which the future was quite unknown. This surely provided a natural limit to the theory.

Professor Samuelson answered that the future became more certain in a progressive way. One did not have a given time period within the time horizon beyond which, so to speak, one fell off the edge. Current market prices would reflect all time horizons and people would take this into account.

Professor Hicks felt one should remember that the degree of foresight about different kinds of receipts differed greatly. For some of us, the degree to which we could take account of anything likely to happen in ten years' time was very slight, but where one had, say, exhaustible minerals, greater foresight was possible and the future could be taken account of in a calculable way. How would Professor Samuelson deal with such cases ?

Mr. Kaldor said he believed what Keynes had said, namely, that while there was a market value for capital assets, this was not because people had very definite expectations about the future. With uncertainty, unless there was a very good reason to the contrary as there would be if, for instance, one expected a resource to become exhausted on a particular date, one had to fall back on a convention. One assumed that the future would be a continuation of the past, so that valuations concerning the future did not imply definite expectations. One knew that the future was uncertain, but assets had to have prices and people knew that they must put definite values on them, despite their ignorance of those future events on which alone a rational valuation could be based. Sometimes, for example, where a man knew that he was about to retire, he did have special knowledge of the future. But the fact that capital assets had prices certainly did not mean that people were certain what the future held in store.

Professor Samuelson agreed that capital theory was intrinsically difficult when uncertainty was brought in, and this was precisely why social capital was so hard to evaluate.

Mr. Kaldor suggested that this only went to show that in economics we should avoid running into blind alleys where difficulties about uncertainty

were bound to bring us to a complete halt. The purpose of abstraction was to abstract from things one could deal with later, and *not* to assume away what was too hard to deal with at all. Otherwise theory became useless, and this was more true of neo-classical than of classical analysis. Thus in neo-classical theory one constructed models which would work only if one abstracted completely from uncertainty. When uncertainty was introduced, the model became quite useless.

Professor Samuelson hoped that by handling more manageable cases first, we might move on to deal with uncertainty later. The next stage would be to analyse a stochastic situation, dealing with risk rather than uncertainty. Operational research and the study of decision taking would probably help us in this field, and the work of von Neumann, Ramsey, Savage and Bellman had opened up the problem.

Dr. Barna was worried over the distinction between forward- and backward-looking measures of capital. Was it not true that the classical economists looked backwards ?

Professor Samuelson replied that he was dealing with physical capital and his theory was backward-looking. Future behaviour was free of the effects of history but one could go back and look at the past.

Dr. Barna pointed out, however, that the Samuelson concept did not refer to resources embodied in the past. It was not a question of chrystallized labour as Marx considered it.

Professor Samuelson replied that the value of his capital, say K, was arrived at from the properties of the production function, though it might be true that the system would never have left the ground without an initial value for K. Nevertheless, his own theory was an interesting rival to the circulating capital model of Ricardo. He had revalued capital on the basis of the present situation and of present market values. This was the reverse of what Pigou had done when he dealt with replacement costs. Other economists had based their analyses on what the asset in question was worth in the past.

Dr. Goldsmith wondered if Professor Samuelson was not getting into difficulties because he was using Professor Hicks' forward- rather than backward-looking approach.

Professor Samuelson suggested one could still say that there was a change in efficiency. He was making impeccable use of Joan Robinson's earlier technique of comparing the values of K_1 and K_2 in a case where their marginal productivities were in invariant ratio.

Professor Nakayama pointed out that in the concluding part of his paper, Professor Samuelson showed sympathy with the standpoint of Pigou. Pigou's welfare definition had been much criticized by writers of the 'new' welfare economics, but it was found operationally useful by statisticians who were more empirical in their work. Professor Samuelson's present work could be thought of as providing that realistic attitude with theoretical justification.

Professor Nakayama drew attention to what Professor Samuelson had said at the bottom of page 55 of his paper, beginning with the words

'careful reading of Professor Pigou's argument . . .' and also in the immediately preceding footnote. Professor Samuelson had pointed out that wealth could be measured in two ways, either as current consumption or as a wealth or stock item. What bothered him was that he felt there must be some limit to savings, otherwise we could depress the present level of consumption as far as we wanted in order to raise future incomes. That could not be done in a free society, so that there must be some limit to the degree of conversion between income and wealth.

Professor Samuelson thought he could rewrite Pigou's *Economics of Welfare*, though he would prefer the 1912 title of *Wealth and Welfare*. Such a re-writing would leave the argument basically intact and any changes would be very minor. Professor Samuelson agreed with Mr. Kaldor that the possession of wealth gave consumption satisfactions, so that perhaps one should impose a tax on wealth as Benham had suggested years ago in *Econometrica*. There would, of course, be operational difficulties with such a tax.

M. Barrère said that the discussion had been much concerned with the distinction between theory and practice. We had spoken of measurement and of analysis, but these two things were totally different. When we talked about measuring capital we tried to apply a purely theoretical concept of capital, yet the starting-points of the theorist and the statistician were entirely different. The analyst could not ignore the organizational complex in which the entrepreneur must operate. The statistician, on the other hand, when he measured, could take isolated and physically separate goods. The theorist's measure of capital was bound to deal with concepts quite different from those of the statistician, because the theorist had to consider each asset in relation to the whole, a thing which the statistician could not do. One big difficulty for the statistician arose from the possibility or impossibility of getting money values for assets. On income, M. Barrère pointed out that the theorist had to project into the future, something which the statistician could not allow for in his valuation. So it followed that the measures and concepts differed, and it was unrealistic to try to make the theorist and the statistician agree. Differences of a fundamental kind arose not only in measuring national capital but in trying to make the necessary qualifications to the argument in question. In the present imperfect state of science it was impossible to measure more accurately than we did, and what we should do was to admit that the statisticians could provide figures which were only an approximation to what the analyst would require to prove his theorizing correct.

Dr. Goldsmith disagreed with M. Barrère. M. Barrère had suggested that we should use two different concepts of capital. Statisticians could estimate the future earning power of assets but this was very different from what they *did* do. It was not that there were two concepts, there were several; the problem was to see how one could achieve the best measure. There was therefore a difference from the theorists' concepts of capital but it was not a difference between the theorist and the statistician. Since there could never be complete correspondence between theoretical

concepts and statistical measures, what we needed were translation rules which enabled one to say how far the one set of results approximated to the other.

Professor Fellner wondered whether the translation rules should not apply to Professor Samuelson's analysis on the one hand and to national *income* or to national wealth on the other.

Professor Domar returned to the problem of gross versus net capital. Since capital did deteriorate, a measure of the net stock of capital should be preferable to a measure of the gross in a wide range of problems ; the difficulty lay in arriving at some reasonable measure of this deterioration. But if capital was valued and revalued at current market prices, why should one bother so much about depreciation ? Why not take market valuations of both new and of old capital instead ?

Professor Samuelson agreed that, for many purposes, market prices were sufficient. But if one wanted to calculate net national product, then in his model, which followed the system through time, some measure of depreciation was necessary.

Professor Domar raised a question on the kind of problem for which a knowledge of the stock of capital was required. Presumably, an economic historian might wish to have such estimates for certain dates. In many current policy decisions, however, this need was absent, and estimates of increments to the stock, much easier to handle than those of the stock itself, would suffice.

Mr. Kaldor said that if one stuck to partial derivatives for explaining prices then one had to bring in capital. We should need to develop the Böhm-Bawerk/Wicksell theory to a point where it could be tested ; to know, for example, whether the rate of interest was within, say, 10 per cent of the calculated value of the marginal efficiency of capital, or whether the wage rate was within 5 per cent of the marginal productivity of labour. The tools of neo-classical theory were not of a kind that would permit any such empirical test.

Professor Sylos Labini said that Professor Samuelson defined wealth as the present discounted value of all future consumption, but in the footnote on page 53 he said that labour, or rather primary factors, must be included in the capitalized total. Therefore, our wealth-like magnitude included the capitalized value of labour. This point could not arise in classical economics where services were not included. Were the classical economists wrong on a purely logical plane ? Surely we were bound to acknowledge that there was a contradiction in the logical argument.

Professor Samuelson said that his L should be interpreted as a non-produced good, land rather than labour, and that we should think of land as immortal. We then had a pure Irving Fisher identity. He had never found the classical notion particularly attractive, and if the primary factor took 75 per cent of total income, we could not ignore it. There were, however, still difficulties over services. His own daughter had chosen a magnifying glass rather than a pony-ride because she knew that she could keep it for much longer.

THE DISCUSSION OF PROFESSOR FELLNER'S PAPER

The discussion was opened by *M. Malinvaud* who said it was a pleasure to introduce Professor Fellner's paper, which began with conceptual definitions and ended with an illuminating interpretation of historical phenomena.

Since he had no basic criticism to present, he would rather try to remind participants of the main basis of Professor Fellner's approach and would introduce a geometrical representation which he found useful. We could begin with a very simple model in which labour, L, and capital, K, were proportional to output, Y.

$$L = \lambda Y \text{ ; and } K = kY.$$

On a diagram similar to the one used by Dr. Barna, the above production function was represented by a single point (M) in Fig. 24a. A purely capital-saving innovation would then amount to a reduction in k without any change in λ, and the representative point would move horizontally to the left, say to N. A purely 'labour saving' innovation amounted to a reduction in λ, k remaining constant; the representative point moved to the right along the line through the origin, say to P.

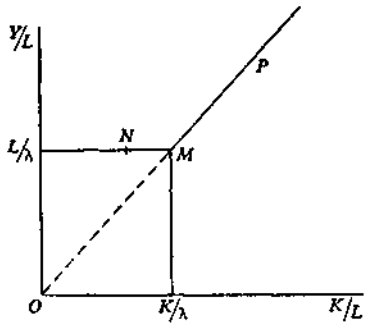

FIG. 24a

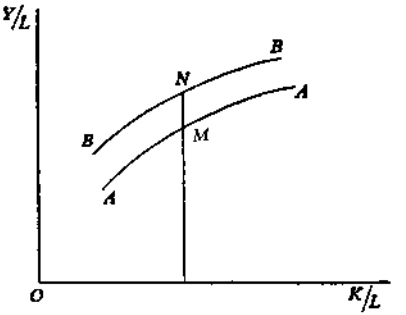

FIG. 24b

We could now consider the more interesting case in which there was some substitution between capital and labour. Assuming constant returns to scale, the production function could be written as

$$\frac{Y}{L} = f\left(\frac{K}{L}\right),$$

and was represented by a curve (AA) on the same type of diagram as in Fig. 24b. Innovations resulted in an upward shift of the curve from AA to BB. When should we say that innovations were capital saving? To do that we had to compare the two curves, which required further

hypotheses. Professor Fellner chose to limit attention to the point N on the BB curve, which corresponded to the same input combination as the actual point M on the AA curve.

One could easily see that, if innovations were purely capital saving, the tangent to BB at N would have a gentler slope than the tangent to AA at M. According to marginal productivity theory, the share of profits in income would fall if we moved from M to N. Similarly, if innovations were purely labour saving as defined above, the share of profit would increase when one moved from M to N.

Professor Fellner defined an innovation as being relatively capital-saving if it tended to decrease the share of profits, the input combination remaining the same. This definition led to some comments which Professor Fellner had made and which it might be worth restating. (1) The definition depended simply on the geometrical properties of the production functions. It could be formulated so as to avoid any reference to marginal productivity theory. (2) Even if one accepted this theory, movements in factor shares did not characterize the type of innovation which occurred. Indeed, innovations might lead to changes in input combinations so that the actual point on the BB curve might be very different from N. (3) However, knowing the time path of factor shares, output and input, one could set up an econometric model and use it to determine simultaneously some features of the production function and of innovations. Professor Solow had used this line of approach to interpret the empirical findings so fully examined in the second part of Professor Fellner's report.

Professor Marchal wanted to make three remarks about Professor Fellner's paper. The first, which was almost obvious, was that it was necessary to select practical criteria, that was to say criteria which could be used in the present state of knowledge and especially statistical information for countries experiencing problems of growth, both developed and under-developed. Second, he was a little worried about the distinction between labour-saving and capital-saving innovations. If one considered only labour, it was obvious that there was a number of types of labour, each very different from the other. It seemed to him one thing to save skilled workers or engineers and another to save unskilled labour. In under-developed countries with a surplus of untrained workers, the first might be important while the second might not. It might also be valuable to consider the amount of management ability required by any innovation. Third, perhaps one should solve the problem in a different way in under-developed as distinct from developed countries. In under-developed, as in developed countries, one was aiming at balanced growth but also at more rapid growth, and, in order that growth might be speeded up, perhaps some part of balance would have to be foregone. It also seemed to him that the different structures and the different institutions of various under-developed countries were too often overlooked by theorists; but he recognized that Professor Fellner was dealing with a situation where growth had already begun and where the major concern was equilibrium between the various factors of production.

The search for a criterion by which one could classify innovations met with several kinds of difficulty and he would like to say a few words about this. In the first place, this criterion could only characterize the variations in the social production function caused by technical advance. In practice, changes in the production function resulted both from changes in technique and from changes in factor supply. If one preferred it, one could say that each of these coefficients resulted from the interaction of changing demands and supplies. It was therefore necessary to make some initial assumptions about supply conditions. One could then arrive at a production function which took these into account and arrive at some measure of the influence of technical progress.

In the second place, when the aggregate production function changed over time, its combined variations resulted from the joint influence of technical development and changes in demand conditions. The isolation of the latter raised slightly less serious methodological problems than did technical changes, but they were nevertheless not negligible. Finally, the appraisal of the character of innovations led to difficulties if one made a dynamic approach, for then one had to deal in rates of growth and not absolute amounts.

Of these three difficulties, the first was much the most important. One could not isolate changes in demand caused by technical progress. If, on the one hand, one admitted that demand was independent of supply of factors, and if, on the other hand, it was shown how one could study the production function taking into account the autonomous evolution of supply but not technical advance and the new production function resulting from technical advance, the problem was how a single criterion could give a satisfactory comparison.

Professor Fellner's solution was to measure the character of an innovation by changes in the share of factors in the total product. The advantage of this method was its simplicity, and Professor Marchal would make three comments on it. First, Professor Fellner began by studying growth functions, that was to say, dynamic supply functions, taken as given. While it might be analytically correct, this method was difficult to operate in practice, and Professor Fellner admitted this when he said that the historical curve (K/L) was given. This amounted to admitting that at each moment there was not one supply curve for each factor but a given volume of the factor available for work. In other words, the instantaneous supply was inelastic. One must further assume at this stage that the process of accumulation depended neither on the level of the growth equilibrium nor on the character of innovations.

These different assumptions gave rise to an analysis which isolated the demand for factors as it appeared in terms of the historical behaviour of the production function. But the question one wanted answering was whether the remainder of the analysis took account of these assumptions. If we measured capital-saving innovations by changes in the share of profits and interest, the share of these, in so far as it was historically observed, resulted from the combined changes of the two ratios — the

capital coefficient and the rate of return on capital. One might say that the capital coefficient was determined solely by the production function, that was to say by factor demand ; but one could not say this about the rate of return on capital. Here demand and supply were in constant opposition. In marginal theory, the proportionality between factor prices and marginal productivities was necessary, but these were brought into line by the development of techniques as well as by changes in the behaviour of owners.

This led to the second observation. It seemed that the share of the product going to a factor constituted too composite an index to measure the over-all character of innovations. Changes in this share could reflect changes in the structure of the product-mix — an increase or decrease in the proportion of goods using much capital. Or they could represent changes in the nature of innovations, changes in the composition of factor supplies or changes in the degree of monopoly for the product or for any one of the factors. The first of these things was concerned with the demand for factors, the third was concerned with the supply and the fourth with both. How then could one isolate the particular influence of innovations ?

Professor Fellner clearly knew this difficulty for, in the historical part of his paper, he had to eliminate the influence of changes in the composition of final demand and in the structure of capital and labour resources. His silence on changes in the degree of monopoly was all the more surprising. His analysis rested essentially on assimilating marginal productivity into the price of factors ; in other words, it rested on the assumption of perfect competition. Yet it was undeniable that changes in the degree of monopoly had been very important in determining shares of the national product.

This use of relative factor shares to appraise the character of innovations led one to a final comment, namely that what one ought to use as an indicator was not the growth of the respective shares going to wages, profits and interest but the evolution of the shares going to labour and to capital and enterprise. This gave rise to a problem of imputation, not least for the income of independent business men. At present, statistical conventions led to very arbitrary results, and conclusions derived from statistics should therefore be treated with suspicion.

It seemed to Professor Marchal that the problem had to be dealt with in more general terms. One might try to characterize innovations by looking directly at the dynamic transformation of the production function in its rough state rather than after the elimination of changes in the structure of output. This would imply that, for each year, the function might be defined not by a single point but by several alternative points. The techniques of inter-industrial input/output tables and the notion of an economic budget could simultaneously constitute the extremes of such an approach and the proof of its potentialities.

Professor Fellner agreed that, since labour was not homogeneous, it was useful to distinguish various types, skilled, unskilled, etc. Perhaps

technological progress was relatively labour-saving at the two extremes, but required more semi-skilled workers. He was not quite sure how one solved this problem, but it certainly was a problem and not only in underdeveloped areas. As for the degree of monopoly, he thought that trends in income distribution did not lend themselves to interpretation in terms of changes in the degree of monopoly. Indeed, it was far from clear that such changes had altered the distribution of income in the USA very much. The crude study of time series did not suggest that they had much effect, though of course such a study was not conclusive. What came out was that trends in the average productivity of capital were more favourable than trends in profit rates, and this could not be interpreted mainly in terms of the degree of monopoly.

Professor Hicks suggested that we should take account of the influence of demand, in the sense of the distribution of demand between consumer goods. This could affect our problem. It was conceivable that people might tend to shift their demand in the direction of services as they became better off and would only buy capital-using goods if these were very cheap. Then the rate of profit would fall rapidly with progress, and progress itself would soon stop. The fact that on the whole people had been prepared to take highly capitalistic products was a valuable element encouraging economic advance.

Professor Samuelson observed it had not been thought necessary to state the theorem whether invention helped to raise the productivity of both labour and capital when one dropped a smooth marginal productivity theory. Would technical change raise output, with the amount of labour and capital fixed ? He was not satisfied with a tautology and would like to frame the matter differently. When one said that output and welfare increased, this was a sufficient criterion ; and Professor Fellner had been right in saying that smooth marginal productivity theories were not important. But one did need these strong assumptions underlying perfect competition. If one dropped them, any clever man could disprove what one said. One needed regular tastes of individuals, constant returns to scale and so on ; under this, one could observe any system in equilibrium and then offer a new option. The option might never be adopted, but if it was adopted in perfect competition then it must shift out the utility possibility schedule of the society ; it must make improvement possible for everyone. This was the same thing as saying that output had increased.

Mr. Kaldor understood that the object of the game was to show the effects of invention on income distribution. He very much agreed with Professor Fellner that one was unlikely to be able to classify investments as labour- or capital-saving in terms of observed shifts in the capital-output ratio. The purpose of the classification was to explain the effects of innovations on the share of profits and wages in the national income. On the neo-classical view, these shifts depended on the effects of innovations on the parameters of the production function. If one had a constant-elasticity function, then the kinds of shifts were classified according to their effects on the powers of the variables of that function. One could

examine the situation to see what happened to distributive shares, and one displayed a natural hesitancy because, in practice, there seemed to be no change in shares. If distribution were governed by marginal productivity the analysis was easy enough, but this assumed perfect competition and/or that the degree of monopoly had not changed. Professor Samuelson's strong assumptions were also introduced, otherwise the system had no interpretative value.

To what extent was what was now said consistent with the position where one said that capital-saving innovations in some parts of the system would induce labour saving elsewhere ? Did the rate of profit on capital reflect anything more than the innovation-mix adopted by the system ? Neutrality might be a result of the forces of adjustment and not of the peculiarities of technical progress as such.

Professor Fellner thought we ought to ask ourselves what we were trying to do with these models. Professor Hicks had suggested that we wanted to be able to use the system to see round the corner in various conditions.[1] This was not merely a game, and some kind of apparatus could help to answer, for example, Professor Hicks' question of what happened if demand shifted towards services. One would have rapidly diminishing returns to capital unless either the quantity of innovations rose rapidly or they became more labour-saving. This followed from the identity

$$\frac{P}{K} \equiv \frac{P}{Y} \cdot \frac{Y}{K}.$$

Mr. Sraffa, while he would not suggest that if one dropped marginal productivity theory innovation had no effect on distributive shares, did believe that such effects might be unpredictable. It was not that other theories said there was no effect, but merely that there was now no simple effect.

Professor Fellner said that one could avoid diminishing returns, regardless of what happened to $\frac{P}{Y}$. But, for any given capital-output ratio, the requirement of avoiding diminishing returns related to the $\frac{P}{Y}$ term.

Mr. Kaldor said he now agreed. If there was a very rapid fall of the capital-output ratio there would be a fall in the share of profits in the national income. But what happened to the rate of profit on capital was not so certain ; it might rise or it might fall.

Professor Hicks wanted to make a general remark. He could not help seeing, in Professor Fellner's paper, a close resemblance to some of his own ideas which, when he had put them forward originally, he had done in terms of a very strict marginal productivity theory. What interested him was that Professor Fellner showed that some of the same kind of apparatus still had validity, even if not in quite the same form. He would

[1] In the discussion of Mr. Kaldor's paper, which was taken before Professor Fellner's at the Round Table. (See p. 368.)

like to say that, after all, questions on distribution were not only concerned with the relative shares of labour and capital. There were problems of land even now, and many sorts of land.

If we had doubts on strict marginal productivity theory, these doubts carried over to subordinate problems. He felt fairly certain that many of the practical deductions were true. If one grew a crop on new land, this had the same effect on the relative positions of land owners and the rest of the world as classical theory suggested.

Professor Champernowne said Professor Fellner had regarded the aggregate ratio $\frac{K}{L}$ as constant. He was not clear how the two variables were measured but would like to ask Professor Fellner whether he thought it sensible to measure capital in wage-units when comparing it with labour. If Professor Fellner did accept this, and if he also accepted the criterion that innovation was labour- or capital-saving as the ratio $\frac{P}{W}$ rose or fell, then one could go on to say that progress was capital- or labour-saving when the rate of profit rose or fell. For the ratio of profits to wages in this case was only the product of $\frac{K}{L}$ and the rate of profit. So one had here a border-line state in which the rate of profit was constant in the same conditions as the ratio of total profit to total wages. This formulation avoided any marginal productivity theory, and it also made the definition of capital-saving and labour-saving innovations far more easily reconciled with the writings of, say, Mr. Harrod.

Professor Samuelson asked whether, if one held the amount of capital constant in wage-units, one could also keep a given physical quantity of capital constant.

Professor Champernowne replied that he was suggesting measuring capital in wage-units per unit of the labour force, and holding this constant. One had comparability if the price of capital per wage-unit was constant.

Mr. Kaldor said that, in other words, one had comparability if there was no technical progress in the making of capital goods, so that the purchasing power of labour in terms of capital goods was unchanged when it rose in terms of consumer goods. But both Professor Fellner and he had assumed that, at any moment, one had a given initial state of development, and a given initial amount of capital per man. If innovation raised output relatively to labour, it must also raise it relatively to the existing capital stock. Capital was 'fixed' in that sense, though in the other sense it could vary.

Professor Champernowne pointed out that the ratio of the cost of capital to wage rates need not be constant. It would be sufficient if the change in the rate of interest offset the changed productivity in the manufacture of capital goods.

Professor Solow wondered if it would not be possible to do this by considering not the rate of profit in wage-units but the real return on capital and real wages with physical capital constant.

THE DISCUSSION OF DR. BARNA'S PAPER

Dr. Barna said that, by way of introduction, he would explain that there were four possible ways of measuring capital and show why he liked his own method better than the other three. The second and third methods were those used in Professor Domar's paper, the fourth was that used by Dr. Goldsmith. His main criticism would be against Professor Domar's method; on Dr. Goldsmith's, his complaint was that there the length of life of assets was based on a conventional assumption and not measured empirically.

Dr. Barna said that just before the Conference began, he had recalculated his own direct estimate of capital in manufacturing industry in the United Kingdom, using Dr. Goldsmith's indirect technique. In section three of his paper, he had tried to assess quantitatively the deficiencies

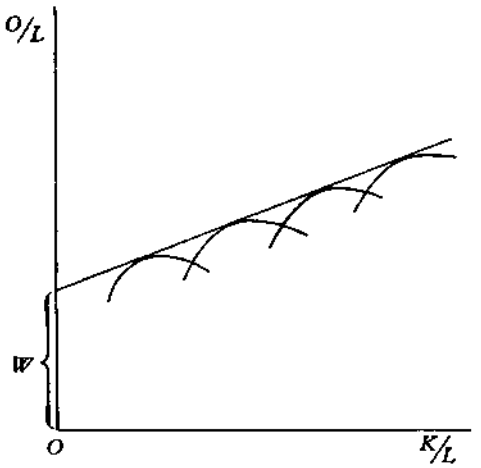

FIG. 25

of this method. His own study had been carried out by direct enquiry in industrial firms. It had suffered from the fact that the number of firms studied was rather small, but he did feel that he knew what he was measuring and that he understood the concepts lying behind the data he had collected. The fixed capital stock of an industry could be described by three sets of statistics: (i) the gross ratio between capital and labour, or capital and output, where capital was measured by gross replacement cost; (ii) mortality experience with the existing capital stock; and (iii) the falling value of an individual capital asset as it grew older. Dr. Barna said he had empirical evidence for (i) and (ii) but only a little for (iii). One could summarize the information under (i) as in Fig. 25. Economic interpretation was fairly simple and ran along the lines of Professor

Samuelson's paper. Output per man was measured on the y-axis and capital per man on the x-axis. A study of one industry gave one point on the diagram, while figures for other industries gave other points. We could assume that all statistically-measurable points lay on a straight line, whose slope represented the rate of profit in the economy, and whose vertical intercept gave wages — for reasons explained by Professor Samuelson. In fact, the empirical data were described by a straight line and the fit was quite good. The slope of the line was about 20 per cent, implying a 20 per cent rate of profit on capital, gross of income tax. Dr. Barna himself preferred the concept of capital per man as a measure of capital intensity because of the enormous variations in it. The capital-output ratio was rather less interesting. The highest capital-output ratio one got was on the extreme right; in his model it was about 5, and this was the theoretical maximum. At the other extreme, all firms must have some minimum amount of capital, if only a building. Under British conditions this minimum capital-output ratio must be at least 0·5. For logical reasons, variation lay within these limits.

On the mortality of assets, Dr. Barna said that the conclusions of his analysis had not yet been worked out. The important thing seemed to be that the mortality of assets was fairly continuous, if one took an industry in the aggregate. The conventional rule was that a proportion of the assets lasted for ten years and then collapsed; another group lasted for twenty years, after which they collapsed. Assets might last longer than expected, but the important point was that the attrition of assets started almost at birth and might perhaps be described by a straight line. A problem arose from the fact that assets changed hands after a time and, when they did this, were generally associated with different uses. On railways, for example, a main line locomotive would be turned over to shunting after a time. Dr. Barna felt that any statistical method must be preceded by a theoretical explanation, and the depreciation of these sorts of assets might be described by two straight lines with a kink where the use of the asset changed. In the first part of its life the asset would depreciate rapidly. Under a new owner it would depreciate much more slowly and might well survive for a total of 50 years. Dr. Barna suggested that the most convenient treatment was to regard oneself as dealing with two separate assets. The first user would receive a second-hand value, so that only a proportion of the investment had to be depreciated; the rest was in the nature of circulating capital. Because of this possibility of handing the asset over to an inferior use, the planning horizon of the first owner was reduced.

Dr. Goldsmith said that Dr. Barna's was the first of three papers on empirical problems, and it was a fine example of new factual information theoretically interpreted. This new material dealt with three problems, the measurement of the replacement value of fixed assets, the mortality of capital assets and depreciation in terms of age.

Dr. Goldsmith said that, on the empirical side, there was considerable argument over what the graphs were about. There was a distinction, for

example, between the services rendered by an asset and its replacement value. Dr. Barna's estimates of replacement value were based on fire insurance valuations — a time-honoured method. They had been used, for example, to estimate the value of capital in Germany as far back as 1913. Not all fire insurance valuations were available so that one could not produce an aggregate figure based on them, though this would be very useful if we had it for the United States or for the United Kingdom. Whether this value reflected replacement cost was a technical problem. Dr. Barna thought that it came fairly close to replacement cost. He himself thought it was on the high side, because those who calculated fire insurance values were thinking of the need to purchase more modern machinery, when making their estimates. However, fire insurance values represented a handy tool.

Dr. Barna had been dealing throughout with gross values and had suggested that it did not matter very much whether one was concerned with gross or net values, because the relation between the two was fairly constant. He disagreed. In the United States, on a national level, the ratio between gross and net values changed very substantially over time. Dr. Barna had suggested that net value was equal to two-thirds of gross. This seemed a very high ratio if growth of capital expenditure was exponential. The two-thirds ratio obviously implied a certain combination of length of life of asset and rate of growth. With a 30-year average life of asset, it seemed to imply an annual rate of growth of capital of the order of 8 per cent. He himself would guess that the net/gross ratio was lower, but perhaps not very much. Dr. Barna should in any case prefer the net figure, on theoretical grounds.

This led to the question how such figures compared with those for the value of depreciated capital assets. Dr. Barna's figures were 50 per cent higher than those obtained from Redfern's perpetual inventory method, and he claimed that there were three reasons for this. First, Dr. Barna said that Redfern had under-estimated the length of life of assets, though it was not clear how Dr. Barna came to this conclusion. In any case, later writers had continued to use these same lengths of life. Second, Dr. Barna said that half the difference between his and Redfern's estimates arose from the fact that Redfern had ignored government capital. Thirdly, the remaining one-fifth or so of difference arose from the fact that national income figures normally under-estimated the value of capital expenditure.

Dr. Goldsmith agreed that in the United States the gross investment figures in the national accounts were on the low side, partly because of a too-small coverage, and partly because the statisticians could not take account of increasing value resulting from repair and maintenance. Dr. Goldsmith had looked at this problem in Israel and there too the official estimates were on the low side. Any national income figures using perpetual inventory methods were likely to be too low. In his own estimates for the USA, he had therefore decided to step-up some of the figures derived from reported capital expenditures and had whenever possible used census-type checks for certain assets. If the census figures were

higher than the perpetual inventory figures, what was the cause of the understatement? Was it that the length of life had been miscalculated; or was it the result of undisclosed capital expenditure? Perpetual inventory figures were not based on conventional lengths of life as allowed by the tax authorities. The difficulty lay in the scarcity of empirical data. There was no work in other countries parallel to Dr. Barna's work on manufacturing industry, though we could construct a mortality table for housing in the USA. The life of assets was generally longer than one assumed, and this seemed to be an international phenomenon.

On the whole, Dr. Goldsmith felt that there was little disagreement between himself and Dr. Barna. Perhaps the empiricists were not so critical of each other as were the theorists. Dr. Barna's was very interesting pioneering work and would gain in value when carried out in other countries too. We should then know whether the figures were valid only for British industry in the 1950s or for all capitalist enterprise.

Professor Hoffmann wanted to add something on particular industries. We had now managed to split up aggregates for manufacturing industry. If we took other years for different countries, and cancelled out the effects of the different sizes of the various branches of industry in these various countries, he thought one would find that the relations between wages, capital per employee and value added per employee in these different branches were fairly similar. The relation between the capital-output ratios in different branches of industry for different countries was also relatively stable, provided we cancelled out the effects of changes in the structure of industry.

Mr. Kaldor thought that the British data suggested exactly the opposite, namely that capital-output ratios varied very considerably for different branches of industry, but were relatively stable for the economy as a whole.

Professor Hoffmann still felt that the relation of industries to each other — and not their absolute levels — was the important thing. If one ruled out changes in the structure of national production, the relation between wage per employee and capital stock per employee was stable. This should be true also for the capital-output ratio.

Mr. Kaldor agreed that wages and the rate of profits were similar between industries, but in Britain the rate of increase in capital per worker was significantly different between industries. In some industries the capital-output ratio was declining where the growth of output per man was fastest, and vice versa. This, incidentally, was the exact opposite of what one might expect on grounds of neo-classical theory.

Dr. Barna pointed out that, so far as fire insurance was concerned, not everybody insured the replacement cost of assets but he had picked out those firms which did. Dr. Goldsmith had criticized his comments on the difference between net and gross figures. He would have liked to have had both, but for practical reasons the net figures were not available. However, the relationship between the items considered in his graph was not necessarily affected, even if the net/gross relationship was not the same in all industries. When he said that the net figures appeared to be

about two-thirds of the gross, he did not think he had put the relationship too high, although Dr. Goldsmith appeared to feel that he had. Net investment should be more than 50 per cent of gross investment because (a) the economy was growing, (b) assets had some residual scrap value, and (c) if one took the rate of interest into account, any piece of capital half-way through its life would be worth more than half its value. Dr. Barna said the reason why he had attributed half the difference between his own figures and those of Redfern to differences in their estimates about the length of life of assets was that he had found in his empirical studies that this was the case. Indeed he had expected the difference between Redfern and himself to be even greater. Dr. Barna also pointed out that the ranking of industries according to capital invested came out approximately the same in India, the United States and in the United Kingdom.

Professor Solow did not see how one could avoid relating Dr. Barna's curve to the marginal productivity of capital, so long as one stressed that there would be a different production function for each industry. Under constant returns, this curve would give one the marginal productivity of capital.

Dr. Barna replied that one could, if one liked, call it the marginal productivity of capital but in a different sense from that used by Wicksell. All figures were in current money terms. One ought really to use a different measure in order to get marginal productivity of capital in its correct sense. One should take each industry separately, and do the calculation in real terms.

Professor Solow agreed that one ought to argue in real terms; but here one *was* dealing with one industry at a time and the market would change prices so as to give commensurate figures.

Professor Champernowne thought this connected closely with his own paper. He agreed with Professor Solow that the slope of the line represented the marginal capital-output ratio in each industry. It reflected the fact that the cost of borrowing was roughly constant for different industries. A point of theoretical interest was what would happen if the cost of borrowing changed. The line was the envelope of several curves and its slope reflected the cost of borrowing. If that fell, how could the slope of the line alter without the individual curves moving? The answer was that it could not, and that the shift would be brought about by the adjustment of relative prices under competition. So the adjustment of the economy to accumulation meant adjusting the relative prices of goods.

Dr. Barna suggested that he might have been too modest in not trying to claim that this was the marginal productivity of capital. What he had done required restricted interpretation. The reason why he had claimed that it was *not* the marginal productivity of capital was that, if capital increased, part of the extra product went towards increasing real wages.

Professor Champernowne suggested that if one made very restricted assumptions about competition, one could argue that the marginal capital-output ratio here was the same thing as economists meant by it for the whole economy.

Professor Domar asked whether the term marginal productivity of capital was used here in its conventional sense; as the partial derivative of output in respect to capital (other things remaining the same). So defined, marginal productivity of capital should be the same in various industries under the usual static assumptions of perfect competition. But the marginal capital coefficient, as it was commonly used, was not a reciprocal of this marginal capital productivity, because the definition of the coefficient did not imply that other things remained constant. More than this, did Dr. Barna use value added rather than value of output in his estimates of capital coefficients? Either procedure had its merits, and Professor Domar just wanted to be clear. In any case, he was worried about Dr. Barna using actual empirical data and interpreting them under assumptions of perfect competition.

Dr. Barna replied that his correlation showed broadly that there was competition for resources between industries as shown by the tendency to uniform profit rates.

Professor Domar replied that in the United States large differences in profits among industries had persisted for a very long time, but *Dr. Barna* held that these differences were not correlated with capital intensity.

Mr. Kaldor pointed out that the tendency for the rate of wages and profits to be the same in different industries certainly did not imply perfect competition in the usual sense. It was a necessary but not a sufficient condition.

Professor Samuelson thought there was agreement on the previous analytical point. It was very dangerous to take a fitted curve, which was an envelope, and regard it as a production function. He had never been sure that the Cobb-Douglas student was sure what he was doing when he got a production function for the whole economy.

Professor Hoffmann wanted to support Mr. Kaldor. He referred to Professor Jean Marchal's index of disparity.[1] Wage differences in the economy between equal qualities of labour were approximately zero, and the proportion of workers of different ability, etc., was changing very slowly so that relations between industries were relatively stable.

M. Malinvaud asked whether the rate of profit was calculated before taxation, and how far the result would be different if it were calculated after taxation. He also wondered why one got a lower rate of profit for housing.

Dr. Barna replied that, for companies, 50 to 60 per cent of profit went in tax. He thought the difference between industry and housing arose from the fact that risk in industry was greater, quite apart from the effects of rent control, etc.

Professor Champernowne thought there was a danger in concluding from Dr. Barna's paper that there was a constancy in the rate of profits and wages between industries, for in fact there was none. If one looked at Fig. 5, one found a wide scatter of points. He imagined that Dr. Barna

[1] J. Marchal et J. Lécaillon, *La Répartition du revenu national* (Paris, 1958), Tome i, p. 291.

would not claim any very strong agreement between wage and profit rates, only that on the whole a large amount of capital per man went with a high value-added per man.

Dr. Barna agreed. The rate of profit was not at all the same in different industries. There might be a long-term relationship between these rates, but whether there was or was not could not be proved from data for a single year. All he had shown was that the relationship appeared to be linear.

Mr. Kaldor returned to the depreciation of assets and its relation to the relative constancy of wages. One important factor in a progressive economy was obsolescence as distinct from loss of efficiency. Loss of efficiency was not very important, but obsolescence was continual in a progressive economy because wages rose more than prices, so there was a continual decrease in the profitability of operating equipment as wages rose. For that reason, in a highly progressive economy equipment was abandoned more rapidly. In Dr. Barna's paper, the first owner abandoned equipment when it was still fairly efficient technically, but could not be operated profitably. In the United States this was tremendously important, but the second owner was usually outside the country. Equipment travelled down from high- to low-wage countries, for example, from the USA to Latin America. Dr. Barna suggested that the same happened in the United Kingdom, which was intriguing. In the United Kingdom the influence of trade unions meant that wage differences between efficient and inefficient firms were not now very large. How then did Dr. Barna explain the travelling-down of equipment within the same country ?

Professor Robinson suggested that the answer was that equipment shifted out of its original mass production industry into small-scale specialist firms whose product was different. For example, in the UK, printing machinery went from the big printer to the small jobbing printer. He did not believe that the situation in the United Kingdom was different from that in the United States.

Dr. Goldsmith agreed, adding that in America one often had degradation to standby use. For instance, one had the better machinery providing the base load for electricity and the older generating equipment only brought in at peak periods.

Professor Robinson pointed out that Dr. Barna had said very little about how to revalue capital assets in order to get a measure of changes in real capital. He wondered whether, with obsolescence, one ought to write down old capital or write up new. Was one, in fact, keeping the valuation of the stock of capital constant despite the fact that this capital stock was able to yield increased services ? He wondered what one did when capital yielded a constant output over time and then there was re-investment in better equipment. Did one value this better equipment at a higher price ?

Dr. Barna said he had dodged this problem because he was not interested in time comparison. He was concerned with cross-section data and revaluation was implied. Obsolete capital had already been revalued in terms of new capital in a vague sense.

Professor Robinson replied that economists were looking to Dr. Barna to produce a time series. That was what they wanted, though he admitted it would be hard to get a meaningful one.

Professor Hicks suggested that one needed two series. He would distinguish the quantity of capital and the efficiency of capital. When the same resources became more efficient, for some purposes one could regard them as the same, but for others one could not.

Professor Robinson said that there were many separate questions to which one might want an answer and it required different statistical exercises to answer them. One might want to know what was the flow of services one might enjoy from a stock of capital in a particular year. With ageing capital, though the flow of output was constant, the value of the stock of capital in the sense of the unused services remaining in it might be declining. Which were we discussing ? Gross capital was a better index of the possible flow of services than net. Did we not want some measure of depreciation of capital not in terms of the services left in it but of the decline in its efficiency ? For some purposes, he suggested, one did not want a measure of either gross or net capital.

Dr. Goldsmith agreed with Professor Hicks and Robinson that there were these different measures. There had been some attempts to find measures of capacity from a semi-engineering point of view. Series could be built up and these would show more of an increase than the usual net measure, which would reflect changes in capacity rather than in the stock of unused services.

Mr. Kaldor said that, as he understood Professor Robinson, the latter would want to make allowance for the increased costs of operating capital assets at the same output. So far we had decided that one could either deduct something on account of mortality or on account of depreciation. The third possibility was that we might deduct something for mortality plus that part of depreciation which represented a loss of technical efficiency, *or* one might deduct increased maintenance costs.

Professor Domar pointed out that Fig. 6 of Dr. Barna's paper, on the survival of assets in engineering, showed no retirements in the first two years. Thereafter retirements proceeded along a straight line. Presumably, no capital asset lasted less than two years, by definition ; others lasted for a very long time. Was there any information about the scrapping of *particular* kinds of assets ?

Dr. Barna replied that there was not, except in a few cases. The analysis made no distinction between scrapping due to time and scrapping due to stochastic elements.

Dr. Goldsmith explained that there was very little information on this in the United States. Automobiles and ships did not show this straight-line depreciation, but one might get a straight line by combining assets of different types ; or perhaps one would arrive at a line which was curvilinear. Dr. Barna was only concerned with equipment, not structures. What kind of individual curves would produce his results ?

Mr. Thalberg wondered whether, when he was concerned with replace-

ment cost, Dr. Barna tried to make allowance for the fact that the life expectation of new capital might differ from firm to firm. *Dr. Barna* replied that he did not, but *Mr. Thalberg* wondered whether, if one did try to do this, one would get a better correlation in the chart.

Dr. Barna replied that, on mortality, he could see no analytical significance attaching to the fact that his curve came out as a straight line. The period he was studying was too short; he had information only over a period of 40 years. With data over a longer period, one would get the kind of curve Professor Hoffmann had shown. He had put all assets together because other people's calculations were generally for particular types of assets which could be easily identified, such as locomotives. In manufacturing industry, only a proportion of equipment would be accounted for by such assets; machine tools, for example, were relatively unimportant in value.

Professor Robinson wondered whether, with more specific capital equipment, there would be a change in the shape of the mortality curve. He suspected that what Dr. Barna had said about railway engines did not now apply. One could not use a modern main-line locomotive for shunting. Locomotives of 1900 had outlived those of 1920 because they could descend to uses of a lower order.

Dr. Barna suggested that one would have to study how the mixture changed in the sense of the percentage of locomotives coming down through the economy.

Professor Hoffmann said that in the discussion of differences between all industry and separate industries, we had been discussing experiences in manufacturing, which represented only 40 to 50 per cent of national product even in developed countries. Was there any evidence on the capital-output ratio for non-manufacturing sectors of the economy?

Dr. Goldsmith replied that here housing became very important. There was something like a mortality table for housing in the United States. There were also data for public utilities and for automobiles. Indeed, Dr. Goldsmith wondered whether there was not *more* information on mortality from the non-manufacturing parts of the economy.

Dr. Islam wondered whether we should spell out the purposes for which we wanted to measure capital and relate these to our methods of measurement. With a development plan, the most interesting thing was to collect engineering data on the capital-output ratio. We should have to rely on Dr. Barna's second method, especially where there was no statistical time series for replacement cost as written down.

Dr. Barna thought it would be dangerous for under-developed countries to take as their starting-point any of the relationships existing in highly developed countries. It would be better to ask the engineers to give an average coefficient.

Professor Solow wanted to make a technical remark. One would get a bias between direct and indirect measures if one took increasing survival rates. Dr. Barna attributed the difference to cyclical factors but in indirect estimates the likelihood that, for example, 1926 assets would die in 1927

was based on the number of 1956 assets dying in 1957. Any systematic change in the durability of machinery over time might lead to a bias.

Dr. Barna agreed with this. There were various explanations of the difference between curves in Fig. 6, but the main one was that in boom years scrapping was retarded and in slump years it was advanced. The statistics were consistent with this.

M. Malinvaud asked a question about the rate of profit in the economy as it applied to Dr. Barna's regression. Was there a difficulty over the qualifications expected of workers in different sectors of industries as discussed on page 84 ? Dr. Barna had said that profits were either 16 or 20 per cent, but could not a multiple regression be used to eliminate the effects of differing wage rates ?

Dr. Barna said that his report was abbreviated. In fact, he had done two correlations. The first one, between value added and capital, had shown that wages were correlated with capital. He had then done a second correlation between profits (i.e. residual incomes) and capital, and had got a figure of 16 per cent instead of 19 per cent. He did not know which was the correct figure, nor did he know what caused variations in wages with changes in capital intensity. If it was due to heterogeneity, 16 per cent was correct. If it was due to the monopoly element, 19 per cent seemed more accurate.

Professor Robinson pointed out that if the capital-intensive industries used more men relatively to women, this would affect the answer, assuming there were no considerable differences between the productivities of men and of women.

Professor Solow wondered whether the second regression of gross profits on capital passed fairly near the origin and *Dr. Barna* replied that it did.

Professor Todorović said that Dr. Barna had been speaking from the point of view of a highly-developed economy, where, when machinery was fairly old, it retained a large part of its value if demand was high. In under-developed countries, because wages were lower, comparatively old machinery was still used and had not lost value to the same extent. This meant that under-developed countries suffered from a delay in economic development. Was there a theory to support this ?

Professor Robinson wondered whether the productivity of simple capital was not *higher* than the productivity of very elaborate capital in an under-developed country. In East Africa, there was a shortage of skilled maintenance labour. Elaborate capital assets were often out of working order, and there were high maintenance costs because skilled maintenance engineers had to be imported. In such a country, one might well get higher productivity from older and simpler equipment.

Mr. Kaldor said his experience was that the use of identical equipment required different quantities of labour in different countries. In under-developed areas, exactly the same machines usually required far more labour. He had been told of the same hydro-electric power plant being in use in Norway and in Ceylon. In Norway, its operation required ten families being settled on the site ; in Ceylon, one hundred and ten.

Professor Delivanis took the view that sometimes in under-developed countries one had to use *more* elaborate machinery than in developed countries. One had to compare the cost of importing elaborate machinery which more or less ran itself, with the cost of importing people to operate the less-elaborate machinery. This was an example on the other side.

Dr. Goldsmith wondered whether an under-developed country was right in trying to attain rapidly the kind of productivity reached in developed countries. Even if older machines from developed countries were installed productivity would increase, and he could see no point in trying to reach by one sudden jump levels attained over a long period in developed countries.

Dr. Barna suggested that the existence of international trade in machinery improved the economic welfare of both sides. A developed country could get rid of out-dated machinery, while in an under-developed country a given output could be produced with a smaller amount of capital. There was therefore a general benefit, but one still found strong psychological resistance to this kind of trade.

Miss Goudis suggested that one of the problems in under-developed areas was how to introduce more elaborate technical methods or start production which required them, when technical knowledge was inadequate and technical tradition limited. Consequently it might pay, in the short run, to have second-hand machinery, and this was not just a matter of theorizing. The fact that in such countries know-how and skilled labour capable of using up-to-date machinery were lacking, should be counted among the factors holding back their economic development.

Mr. Kaldor said he would expect the same technique to involve a lower capital-output ratio in an under-developed country because lower wages were more than offset by differences in operating cost.

Professor Domar was glad that Dr. Barna's paper had brought out the question of the longevity of capital. The best contribution on this subject was made by George Terborgh. In Keynes' *General Theory* the marginal efficiency of capital was computed on the basis of a given cost of the asset, of its net revenue and of its longevity. The acceptance of the cost of the asset and its longevity as given had diverted our attention from these two most interesting questions. Yet the higher the wage level relatively to the cost of capital assets, the greater should be the incentive to replace labour by capital and also to replace worn-out assets by new ones, rather than to engage in repairs. An American factory was likely to be more efficient than many a foreign one, but not necessarily an American repair shop. Thus the wage level, the cost of the asset, and the decisions to acquire it and to replace it were mutually interdependent.

Mr. Kaldor explained that his own strictures of marginal productivity theory did not include any attack on Keynes' concept of the marginal efficiency of capital. This latter was not the same thing at all as the partial derivative of a Cobb-Douglas function, but rather like Marshall's *net* marginal product — namely, total product *less* the cost of co-operating factors. The fact that Dr. Barna had discovered a linear relation between

value added and capital per man said nothing at all either for or against the marginal productivity theory. It only showed that there was enough competition to make wages and profit rates similar in the different industries. This could also be true in a super-Ricardian world where capital and labour were strictly complementary — one could still have equality in profits and wages as between industries.

Dr. Barna replied to the discussion. The major issue, raised by Professor Robinson and Mr. Kaldor, was obsolescence. He was very concerned with this problem, but it so happened that in this particular paper obsolescence was implicitly rather than explicitly dealt with. It was clear that the way to cover obsolescence was by the second method outlined in Professor Hicks' paper, where one measured capital in terms of output. It was easy enough to work out formulae for converting old capital to new. One had to write down the output stream and the input stream and construct an index. In theory there was no problem about this, but it was very difficult to do in practice since the basic data were absent.

Dr. Barna explained that he had done empirical research on this. Many industrial companies which had to face this problem *had* revalued their assets. He had spoken to people in such firms to see whether what they did had any economic meaning. The kind of people he had approached were not very articulate in terms of economics but had at least made a brave attempt to overcome this difficulty, and had opened the way for the collection of quantitative information. Dr. Barna suggested that some firms tended to over-estimate the value of their obsolete assets and that the whole concept of gross replacement cost and gross capital value became most complicated when obsolescence came in. In fact, only net value could be computed. Gross value was a conventional figure which broke down when the capital was very old or very out of date. What firms tended to do when they came to value an old-fashioned boiler which used much fuel was to assume that it had a very short life and not bother any further. A high proportion of obsolescent assets was hardly ever found in modern industrial society. Assets were either kept up to date or thrown out.

THE DISCUSSION OF THE PAPER BY PROFESSOR DOMAR[1]

Professor Domar introduced his own paper with the comment that some economists loved capital coefficients, while others would not touch them with a barge pole. Without taking either position, he thought that capital coefficients were useful and interesting concepts. The knowledge of how

[1] Dr. Goldsmith also supplied a number of statistical tables and charts dealing with the relation between the capital-output ratio and economic growth in the USA. These statistics were not yet sufficiently accurate to warrant publication, though they are mentioned at various points in the record of the debate.

they had behaved in advanced countries like the United States should help less-developed countries with practical problems; while the realization, for instance, that housing and railroads had very high capital coefficients helped one to understand Soviet investment policies in these fields.

The number and variety of capital coefficients produced by American economists was now great, if not overwhelming, and one was not always sure which coefficient to use for which kind of problem. To mention only two distinctions, when should the gross (of depreciation) coefficient be used as against the net, and when should the coefficient be expressed in constant rather than in current prices? In many historical problems constant-price coefficients were presumably more helpful; yet practical decisions were made in current prices. Similar questions arose in estimating a country's capital formation effort. For example, in the United States prices of capital goods had increased faster than other prices. A current-price estimate would show a larger fraction of output devoted to investment as compared with the corresponding fraction in constant prices. In the Soviet Union, exactly the opposite had been the case. In current prices, the Soviet fraction of output invested had not been much higher than the American; in constant prices it had. If the comparison was made both in net (of conventional depreciation) and in constant-price terms, the Soviet advantage would be striking.

A second and more important question which Professor Domar wanted to raise dealt with the general importance of capital accumulation as a factor in economic growth. Recent studies by Abramovitz, Kendrick and Solow[1] indicated that by far the larger share of the increase in *per capita* income in the United States was attributable not to capital formation but to technological progress; in Solow's study this share reached nearly 90 per cent. Even though these studies measured not technological progress as such but the residual left over after capital accumulation had been accounted for, and, even though, in addition, one could pick methodological quarrels with their authors, the findings were striking. They were at least sufficient to raise the question whether undeveloped countries should worry less about capital formation and more about technological changes. But we did not know yet to what extent new investment served as an instrument for introducing technical progress, nor did we have an estimate of the latter's cost. Technological progress appeared in these studies as a contribution to output, without being a part of the input. Hence, we could not yet tell whether an extra dollar should be invested in capital formation or in a university or a research laboratory.

Mr. Kaldor said that in introducing the papers he would sum up his general impressions, since he was not a statistician able to criticize the techniques employed. The outstanding impression he had was of the great stability in over-all capital-output ratios. Variations, for example those in Professor Domar's Table 5, were very small indeed. If one took the lower figures, there was a rise to 1919 and a fall afterwards, the fall being more pronounced in the period 1939 to 1955. The figures now

[1] See p. 117 (footnote).

produced were more stable than those given in earlier works and, in particular, the rise to 1919 was now rather smaller, though the tendency was still evident. Mr. Kaldor also noted the similarity of capital-output ratios between countries where technology and productivity were very different, for example, between the UK and Germany. The stability he had mentioned was equally observable in all countries for which time series were available.

Mr. Kaldor thought that this stability in over-all ratios concealed much variation over time in the capital-output ratios of individual industries and particular sectors of the economy. In Table 13, on public utilities, ratios were anything but stable, and this also appeared to be true in the UK and in other countries. It was clear that capital-output ratios in any one industry varied considerably over time and this was very puzzling. Was it just accident that with individual variations so large (and on the whole he thought the statistics exaggerated them) the over-all ratio came out so stable and so similar in different countries ? Perhaps he should add that such movements in over-all capital-output ratios as did occur seemed to run counter to what one would expect on the basis of neo-classical theory. This would imply that an increase in the rate of accumulation should be followed by an increase in the capital-output ratio and combined with a higher rate of growth of labour productivity. American figures showed that in periods when the capital-output ratio was rising, the growth of labour productivity was slowing down and the ratio of investment to output, and the share of savings in national income, were *diminishing*. The opposite was also true.

Mr. Kaldor did not want to try to give a reason or a general theoretical hypothesis explaining this, but he did want to talk about Professor Domar's final section, which needed explanation. During structural change, industries with a high capital-output ratio ought to gain relatively to those with a low one, but Professor Domar thought that, in fact, things happened the other way round. Finally, technological progress increased output from given resources, and therefore must lower the capital-output ratio as compared with what it would otherwise have been. Professor Fellner had made the same point, namely that it was wrong to think of the capital-output ratio as rising, constant or falling according to whether technical progress was neutral, capital-saving or labour-saving. One could not use movements in the capital-output ratio as indicators of the character of technical progress. He was surprised that Professor Domar had not mentioned the evident point that the rise and fall in the over-all capital-output ratio had something to do with the speeding up and slowing down of technical progress. He had asked Professor Kendrick how far he thought the two were correlated and had been assured that the correlation was as he expected. Between 1919 and 1929 output per man-hour in American manufacturing had increased at 5 per cent per annum. Since 1939, partly with recovery from depression, the increase in output per head had been greater, especially during the post-war decade, than at any time since the 1800s.

Professor Hoffmann said that the American statistics in this field were the best in the world, and it was extremely useful to have such valuable material. He wondered how the series had been deflated. He also wanted to ask, so far as Table 6 was concerned, how one calculated the marginal capital-output ratio. Was the denominator the average increase over the whole period, or only from one point of observation to another ? Nor was anything said about capital movements, the import and export of foreign capital being entirely excluded. Such imports would clearly have considerable influence on figures for the UK in the nineteenth century, but perhaps they were not important in the United States.

Dr. Goldsmith had produced some deflated figures at 1929 prices, which suggested that the deflated capital-output ratio in the second half of the nineteenth century was rising. But what was happening in the various sectors of the economy ? Housing was the biggest element in capital stock, and Professor Hoffmann said he could not see any trend for the nineteenth century resulting from changes in US manufacturing capital.

Professor Hoffmann commented on the relation between the capital-output ratio and the long-term interest rate. Would it be possible, on the basis of these American figures, to put forward the hypothesis that one had a rising capital-output ratio parallel with a rising long-term rate of interest ? If this relation really existed, it was certainly very difficult to interpret. The long-term bond rate was stable in the United States between 1880 and 1925 and he would leave the problems for the theorists to sort out.

Dr. Goldsmith said that he himself assumed a conventional life of capital assets during which the whole of their value was written off. As fas as possible specific price indices had been used for deflation, but these were not easy to come by. In calculating marginal capital-output ratios, the denominator used was the difference in capital between the beginning and the end of the period. Finally, it was correct to say that only domestic capital was considered.

Mr. Thalberg asked whether Messrs. Goldsmith and Domar had tried to allow for changes in the durability of capital. Imagine two new machines with the same productive capacity. If one machine was built to give greater durability, it would be more expensive. If we used market prices, the figure for capital input would be greater in the case of the more durable machine, and the capital-output ratio therefore higher. Prices might not be proportionate to durability, but perhaps it would be possible to alter market price figures to allow for such variations in durability. At times when the durability of capital was decreasing, the capital-output ratio might appear to decline too much if we merely used market prices.

Dr. Barna was very critical of the whole effort. So far as the stability of the capital-output ratio was concerned, should one not expect this in any case ? He wondered what stability and instability really were. One could surely only argue that something was stable if it did not move very much by comparison with the possibilities for moving up and down that

were open to it. Comparisons over 10 years were not really long enough where capital was concerned, and periods of 50 or even 100 years were needed. In any 10-year period, a great part of the capital stock would be in existence both at the beginning and the end. Second, Dr. Barna said that if the component parts of a total moved up and down while the total remained unaltered, was not that to be expected on general grounds? An aggregate always varied less than any component part of it.

Dr. Barna suggested that the tendency to revalue old capital in terms of product itself led to a tendency towards stability. He had been asking himself what kind of an economy one would have if the capital-output ratio varied, and in what circumstances the capital-output ratio could vary. Here it was not only economic theory which was unhelpful because of its non-operational achievements. In their statistics, too, authors had not related their figures to what theory did exist. There was not sufficient explanation of which theoretical concept was equivalent to which figures. Dr. Barna pointed to the interesting discussion in Joan Robinson's book [1] where she showed that by defining and measuring capital in different ways, one got different capital-output ratios according to which definition of capital was being used. He did not see how one could interpret such figures without a firmer theoretical basis.

Finally, Dr. Barna pointed out that the statistical sources might have inherent defects which were themselves correlated with apparent movements in the capital-output ratio. He had long been puzzled by the inclination in the United States to accept accounting data as valid for economic analysis. In fact, they possessed various inherent defects, some of which were hard to eliminate. Though accounting data in the UK were quite as good as in the USA, nobody in the UK believed that they were sufficiently accurate to be used in this sort of analysis. Dr. Barna agreed with Mr. Kaldor that in periods of accelerated growth the capital-output ratio would fall. Any cyclical change could be explained in terms of economic theory or by inherent faults in the basic data which themselves led to cyclical movements. He thought it difficult to reach any conclusion, mainly because there were fluctuations in the degree of use of capital. One had little progress when there was considerable unemployment of resources. This did not happen in Wicksellian theory but it did in real life. Further, in so far as accountants over-depreciated assets, this might lead to a cyclical fluctuation in the relationship between the book value and the true value of assets.

Mr. Kaldor differed from Dr. Barna on his first point. He did not agree that the stability of the capital-output ratio was of no deep significance because it ought to occur in any case and because capital equipment had a long life, ten years not being long enough for big changes. He did not agree with this where capital stock per head was large and rising. If one had a small amount of capital per head, it would be true enough; but theoretically one would expect that any addition to the capital stock per unit of labour would give rise to a less than proportionate addition to

[1] *The Accumulation of Capital.*

output. It was not a matter of mere triviality that in the United States the increase in output was proportional to the increase in capital. Mr. Kaldor was a little troubled that statisticians fell into the habit of saying that if two forces were working in opposite directions it was not surprising that the net result was a constant. It was an unsupportable proposition, because there was no good reason why two opposing forces should cancel each other out. In particular individual industries capital-output ratios had risen or fallen from decade to decade in a very significant manner, and this showed that the point about long life of capital was by no means the whole story.

Professor Lutz did not share Mr. Kaldor's impression that the average capital-output ratio was stable. If one looked at Table 5 in Professor Domar's paper and the figures supplied by Dr. Goldsmith, there was certainly no stable capital-output ratio there. What did participants mean when they talked of stability ?

Professor Solow supported Dr. Barna in his view that one should not expect rapid change in capital-output ratios. Even during rapid growth, the annual increment to the stock of capital was small, and lags in adjustment were what one would expect. Second, there was a purely optical illusion of stability because if one had no solid standard for comparison, one was talking in a vacuum. The aggregate capital-output ratio was the weighted average of a number of individual capital-output ratios and, as with any aggregate, it would display less variability than each of its component parts. It was said, for example, that the distributive shares going to labour were also stable ; yet if one took what crude data were available and broke them down by industry and by sector, one found the same thing. Over time, there was an apparent lack of variability in the aggregate ; this depended on it being a weighted average of ratios in many separate sectors of the economy and required no macro-economic explanation.

Third, there was the question of the relation between progress and the capital-output ratio. If it were true that growth (in the sense of an increase in *per capita* income) was a reflection of technical progress, then one would expect the capital-output ratio to fall during periods of rapid growth. Professor Solow said that if one looked away from the stability of the capital-output ratio to its increase during the second half of the nineteenth century and the first three decades of the twentieth, then perhaps one could interpret what had happened, in a crude way, in terms of neo-classical economics. Perhaps in the early period, capital had increased more rapidly than the labour force, but not during the last few decades.

Finally, he would call attention to the fact that, if the marginal capital-output ratio was given, Professor Fellner's point had its full force. It could not be the reciprocal of the marginal productivity of capital because the labour input *was* varying over time, and constant returns to scale were washed out by dividing by the number of men.

Professor Domar agreed with Dr. Barna. He also thought that

Professor Solow was right in saying that before one propounded the view that the capital-output ratio was relatively constant, one had to have some criterion of constancy. Was a 10 per cent change large or small? Since the total capital stock did not change rapidly, it was difficult, under reasonable conditions, for the over-all coefficient to change greatly, even over a period of 10 or 20 years, unless significant changes in the degree of use of capital took place. He wondered whether such stability of the over-all capital coefficient as had been observed was caused by diverse and possibly random movements in capital coefficients in specific industries, or whether some more fundamental forces affecting the whole economy had been at work. In general, the over-all capital coefficient tended to approach the ratio between the fraction of national product invested and the latter's rate of growth. In the United States, both the numerator and the denominator of this ratio had been declining, but not at exactly the same relative rate, so that the over-all capital coefficient had risen during the last quarter of the nineteenth century and the early part of the first quarter of this century, and had declined since 1920 or so. But as his paper showed, the decline was common to a number of industries taken separately. In any case, in spite of the abundance and variety of capital coefficients now available, all of them were extremely rough measures. It was by no means clear that they could stand the weight of complicated theories which it was so tempting to try to build on them.

On Dr. Barna and Mr. Kaldor's suggestion that rapid growth was associated with a falling capital coefficient, American experience pointed in the opposite direction. During the last quarter of the nineteenth century the rate of growth of output was particularly rapid; yet the capital coefficient was rising and not falling.

Professor Champernowne went back to an earlier point, namely whether one should use ratios of volume of capital to volume of output, or work in values. And, if one did use values, should one take current or constant prices? This connected up with the point made by Messrs. Kaldor and Fellner that, while capital accumulation must raise the real capital-output ratio, technical progress must lower it, apart from the effects of capital accumulation. He thought there was a great deal of sense in the idea that technical progress might raise or lower the capital-output ratio according to whether it had a bigger effect in cheapening final output or in cheapening capital. There was an apparent contradiction here with what Messrs. Fellner and Kaldor had said, and he would try to reconcile the two views. Economists like Harrod and Joan Robinson had made use of the algebraic fact that the capital-output ratio was equal to the share of profits in income divided by the rate of profit on capital. To use the relation in this form, one had to have money values in both the numerator and the denominator of the capital-output ratio. The trick in the finding that the capital-output ratio rose or fell according to the nature of technical progress lay in the assumption that the rate of interest remained constant. Then the statement that the capital-output ratio rose or fell was equivalent to the statement that the share of profits in income was rising or falling. To make

sense of a rise or fall in the capital-output ratio according to the type of technical progress, one must suppose that investment was taking place in circumstances which allowed the rate of profit on capital to remain constant.

Mr. Kaldor said that if one assumed for the moment that he was a firm believer in the marginal productivity theory of capital, Professor Champernowne's statement could be paraphrased as follows. Technical progress would inevitably lower the capital-output ratio in terms of what it would have been in the absence of technical progress. But if the technical progress was capital-saving, as this term was defined by Pigou and redefined by Harrod and Joan Robinson, it would lower the share of profits in national income. If labour-saving innovations raised the share of capital in the national income, then the labour-saving and capital-saving character of innovations would be reflected in the changes in distributive shares. He did not believe for a moment that this happened. In other words, he did not believe that the marginal productivity of capital was in any way relevant to distributive shares in reality ; but if he *had* believed this, then what Professor Champernowne had said would be right.

Mr. Kaldor thought that Professor Solow talking about lags had turned the whole thing upside down. One could not say that lags delayed a change in the capital-output ratio. They merely meant that the impact effect was greater than the long-run effect. If there was a lag, there would be an immediate increase in capital but not in output. Therefore, the capital-output ratio in the short run would be higher than in the long run. It followed that lags increased and did not decrease the movements in the capital-output ratio.

Professor Champernowne disclaimed the notion that he had mentioned the theory of the marginal productivity of capital, and said he did not believe in it. He only wanted to show how various distinguished people could believe different things.

Professor Solow said it was clear that an increase in investment raised the capital stock and also that net investment raised output. His point was concerned with something different, namely that the ratio of the capital stock to the labour force had to do with factor proportions, and that changes in such things took time.

Dr. Barna explained that he had not said that the stability of the capital-output ratio was a trivial finding. The empirical findings of both these papers were extremely stimulating. All he was asking was what one meant by stability. It had been calculated that if the average temperature in Switzerland fell by 5 degrees centigrade, this would bring the Rhône glacier down to Geneva. It followed that a small fall in an average could imply big changes in particular sectors. Here, too, it was not clear what was stability and what was not. He wanted to point out that in the N.B.E.R. data, he had found a correlation between the rate of gross investment and the capital-output ratio. When gross investment was high, so was the capital-output ratio.

Professor Lindahl wanted to add something on the relation between

annual investment and the annual rate of growth, which was a practical problem for governments committed to maintaining growth. He held the view that a higher rate of growth needed more investment in order to sustain it. He also held that, if one had a rate of growth of 4 per cent and investment was equal to 12 per cent of the national income, one needed to invest more than 12 per cent of a *higher* national income to sustain that rate of growth over time. The explanation of this was that new investment went in an increasing degree into uses that were not directly productive. For example, an increase of capital accumulation in the form of consumer durables would not increase the rate of growth in the same way as would investment in manufacturing industry. With a rising standard of living and with more consumer capital, it was necessary to invest more and more of one's national income to maintain the rate of growth. He did not think that Dr. Goldsmith's material supported this view but he could not avoid the conclusion that in the long run the United States would reach this position.

Professor Domar said that if Professor Lindahl included durable consumer goods in capital, then the imputed services of these should also be included in income. The same held true of government capital. Otherwise one could obviously get a rising capital coefficient because there had been a relative increase both in consumer durables and in government-owned capital.

Professor Nakayama reported that in Japan there had been two general censuses on national wealth in 1930 and 1955 respectively. He had compared the capital-output ratio at these two periods and was astonished to find it very much the same at both dates despite the intervening 25 years, and all the structural changes in the Japanese economy. He would explain the stability by saying that technological improvements had been mainly responsible, but one must also take account of the changes in the labour force and in the share of national income going to labour.

Dr. Goldsmith said that in estimates made by the perpetual inventory method very few accounting data were used. Expenditure figures, and even estimates of length of life, were *not* technically included in accounting data. It was true that there were more accounting data in N.B.E.R. calculations for mining and manufacturing, but there were very few in his perpetual inventory figures. Dr. Goldsmith also thought he had tried to take account of the argument about changes in the degree of utilization for plant, by taking long periods.

Dr. Goldsmith agreed that one factual question was what we meant by stability. We clearly needed some definition and he was not sure whether doubling or halving represented a material alteration. We simply had not found the right test. He himself was less inclined to regard the capital-output ratio figures as stable than were some members of the Round Table. This led to the question whether we should use current values, or figures at base year prices and also whether one should use marginal or average ratios. Whatever one did, it would be difficult to regard the ratios as very stable. There had been big changes during the

nineteenth century, and if one worked in constant prices the decline after the turn of the century amounted to one-third or even more. Whatever one's definition of stability he did not think one could call such a change negligible. Dr. Goldsmith agreed that if one worked in current prices one might call the period from the end of the nineteenth century up to 1929 one showing stability in the sense that the figure changed less than the change that might have resulted from imperfections in the statistics. If the figures were not systematically biased one found a good deal of movement calling for explanation, though this was more true with business assets than with residential or government capital.

This led to Professor Lindahl's point. One had this same tendency in the United States, where an increasing proportion of total investment went into consumer durables, government buildings, etc., but this had not led to an increase in the proportion of gross investment to gross national product. One could say that in the long run there was no visible trend in the ratio of investment to income or in the rate of growth of output per head. Perhaps one explanation was the strong decline of the capital-output ratio in industry. Dr. Goldsmith was therefore more impressed by changes in figures for the capital-output ratio in 1929 prices. No picture of stability emerged from a study of such figures and the changes were even greater if one took marginal ratios as Professor Domar had done in Table 6. Since it was true that when one used current prices the results were substantially more stable for the period since 1890, it was important to decide which series was economically the more meaningful.

Professor Delivanis wondered if it was worth investigating the way in which capital creation was financed, for example, whether one went to the capital market and whether one ploughed back resources. He thought the latter seemed to lower the capital-output ratio because motives differed.

Professor Robinson said that Dr. Goldsmith had already dealt with some of his worries. He had sometimes thought that economists were discussing capital-output ratios without ever pausing to think how they were measuring either of the two concepts concerned, namely capital and output. When one talked about technical progress, was one referring to the net or the gross capital stock ? With rapid growth the average age of capital would be less than in a period when growth was slow. In the Goldsmith figures he noted that there had been a decline in the net capital-output ratio in US transportation since 1890. Was it true that this decline in the transportation ratio was a decline from a state where there was much new investment to a state where, on the average, assets had lived about half their life ?

Dr. Goldsmith replied that at the peak of railway building the ratio had risen. Later there was a sharp decline, caused by rapid technical progress. Indivisibilities were also important.

Professor Robinson suggested that the same lack of clarity in the definitions went over into the discussion of the results of technical progress in increasing production. Technical progress took two forms. It could

increase the efficiency of capital goods themselves, or it could increase the efficiency of other inputs. What happened here was influenced by the technique of revaluation. When the efficiency of given inputs increased, did one then measure aggregate capital in terms of outputs or of inputs? Did revaluation in 1929 prices simply reflect inputs or did it reflect the productivity of capital as well?

Dr. Goldsmith replied that it only reflected inputs. He had not even tried to cover outputs.

Professor Robinson was also worried about the effects on the statistics of cycles of under-investment and delayed replacement. How safe was one in using marginal calculations relating to abnormal periods? Was Dr. Barna's basic period in Britain one of normality? There was an outdated capital stock, and Britain's gross investment was being used to broaden capital. Now the UK apparently had true net investment, which was really overtaking arrears in the wearing-out and obsolescence of the capital stock. How far could one generalize such ratios of investment to increases in output into long-period relationships?

M. Barrère wanted to ask Dr. Goldsmith how far one could or should allow for changes of structure over a long period. He would like to take two situations, one in which there was concentration, and another in which there was nationalization. To calculate the capital-output ratio one considered each enterprise individually and reached a given result. Now suppose that there was concentration and reorganization in the use of capital, leading to a new structure. We knew that this would not only lead to a rise in productivity but also that some capital goods might no longer be used, so that there would be a decline in the amount of capital and a change in the capital-output ratio. This modification was both qualitative and quantitative and explained things which were not shown by price changes. The same kind of thing could occur with nationalization where the state took over assets. In French electricity, the capital actually being used was less because some was left idle, but productivity had increased since the nationalization of the French electricity industry. Could one allow for such things?

Dr. Goldsmith said that in the United States there could not have been much visible result from such changes; for one thing there had been no nationalization. Concentration, on the other hand, had been essentially limited to manufacturing and would not show up much in the national figures. Retirement of capital could be handled in two ways. Ideally, one should make all capital-output ratio calculations only on the basis of capital actually used and not on the basis of what was in existence. The problem was to decide when an item had been 'eliminated', since equipment was often merely put to standby use. If we could base the figures on capital actually being used, there would be no problem. The other possibility was to allow for this kind of phenomenon in the mortality assumption. Such structural changes as M. Barrère had mentioned should certainly be taken into account, and they pointed to imperfections in the methods now being used.

Professor Hoffmann wondered whether, leading on from M. Barrère's argument, fluctuations in the capital-output ratio in the United States could be explained by fluctuations in the degree of monopoly in manufacturing industry. He wondered, first, whether there was any trend in the degree of monopoly in the United States, and second whether, with monopoly, less capital was used.

Professor Robinson thought that M. Barrère's point could be illustrated from what had happened in the United Kingdom. There what had happened had been more intensive use of capital, little abnormal retirement of capital but less gross investment. All this was looked after in the capital-output ratios and represented an ordinary form of technical progress.

M. Barrère suggested that concentration might not lead to a decrease in the intensity with which the physical capital in existence was used, but might have the reverse effect. When a big firm took over many small shops, the existing capital assets of those shops might be better used, with centralized buying and so on.

Professor Sylos Labini wondered whether there were two phenomena in a concentration process. First, there might be a different distribution of bankruptcies and business failures, which might almost vanish in highly concentrated industries. It was clear that with a high degree of concentration the number of failures became very small. This was evident in the banking and oil industries, and it must affect the capital-output ratio over the cycle and, perhaps, also in the long run. It also meant that during prosperity one tended to have new investment carried on within existing firms rather than by the addition of new ones. Again, the longer concentration continued, the more possible it was to plan changes and to pursue a rational depreciation policy. One might expect the gradual disappearance of abrupt change in those sectors of the economy which were highly concentrated.

Dr. Barna thought the important issue raised by Professor Sylos Labini was that the criteria which a monopolist took into account in his investment policy were different from those of competitive private enterprise. The monopolist was perhaps more interested in vertical than in horizontal investment, but he could not see any reason for saying, on theoretical grounds, whether the capital-output ratio would increase or decrease with a change in the degree of monopoly.

Mr. Kaldor thought, first, that monopolies maintained rather more excess capacity than competitive firms, which in itself tended to *raise* the capital-output ratio. Second, monopolies were supposed to charge higher prices, which in itself would raise profit margins and *lower* the capital-output ratio. He did not himself believe the second factor had any empirical evidence to support it. Dr. Goldsmith's evidence for the United Kingdom and the United States did not suggest that monopolists added higher profit margins. In so far as the first point was important, one would expect the capital-output ratio to rise but, in real terms, Dr. Goldsmith had shown that exactly the opposite had happened since 1890,

especially in manufacturing industry. Third, concentration of production in itself increased efficiency, being one way of achieving technical progress and lowering the capital-output ratio. Concentration taking place in developed countries was lowering capital-output ratios. At any time there were always unexploited economies of large-scale production, and concentration allowed one to take advantage of these.

Professor Domar pointed out that the capital coefficient appeared to be directly related to the size of the firm. This was true not only for manufacturing as a whole in the United States, but also for 19 out of 22 subdivisions of manufacturing.

Mr. Kaldor said that, if this were so, it was difficult to explain the greatly increasing concentration of production in American industry.

Professor Samuelson thought the notion that concentration was increasing in American industry was very much open to doubt.

Mr. Kaldor replied that facts were all disputable. Certainly British figures showed that the proportion of the production of any industry made by the largest firms had substantially increased.

Mr. Little maintained that there was no evidence of continual concentration of production in the UK.

Professor Samuelson expanded his point by saying that there was some evidence of increasing concentration in America up to 1929, but that there seemed to have been a decrease after that, and he felt that the hypothesis of increasing concentration had doubtful validity.

Professor Sylos Labini said that, so far as concentration measurement was concerned, to take the five leading firms might be misleading. He had examined the figures from three censuses, considering establishments. In most industries technical concentration had increased, with firms making only one product in a given establishment but, with high concentration, the smaller establishments were specializing on different things. To Professor Domar, Professor Sylos Labini said that, while it might be true that concentration led to a high capital-output ratio in individual firms, this could not be true for the industry as a whole. When planning investment leading to concentration and the elimination of duplication, one might get an over-all increase in efficiency.

THE DISCUSSION OF THE PAPER BY PROFESSOR HOFFMANN

Professor Hoffmann pointed out that his paper was more concerned with facts than with the explanation of methods. He was interested in long time-series, studied in less detail than Dr. Barna had done, and in the changing structure of the capital stock. He would accept all the criticism that had been made of such calculations but felt that we ought to go on trying to fill empty boxes. On methods, he would say that comparing the post-1913 data with the pre-1913 data was dangerous since

there had been no allowance for changes in the area of Germany, which had altered both the *per capita* and absolute figures.

Professor Hoffmann had been asked about methods several times. For buildings in general, he had in the main used insurance statistics; in Prussia, figures were available from a tax on building capital. For agricultural buildings he had based the calculations on statistics giving the relation between the value of land and the value of buildings in Prussia. For industrial buildings tax data were used, industry in this context meaning manufacturing industry and including handicrafts, banking and insurance. Only a small proportion of the tertiary sector was excluded. It was difficult to give figures for such buildings, though there was some information on buildings in banking, insurance, etc. So far as equipment was concerned, there was a tax on equipment, and statistics were available for Baden and, for a short period, for Prussia. These statistics were used to give figures for the whole of Germany. With agricultural equipment there were census figures and sample data for agricultural machinery and total agricultural capital. Railway data were easily available.

The basic problems were those of deflation and depreciation. Data were originally at current prices, with each piece of capital valued through its life at the current price. One could therefore divide each year's capital by that year's output, though a lot of errors crept in. He had also tried to take account of the differing lives of different capital goods.

Professor Hoffmann thought his Fig. 7 confirmed what Mr. Kaldor had been saying. From 1851–1875, the capital-output ratio was slowly rising and the growth rate of total real national income was 1·7 per cent. In the second period, the rate of growth of real national income had almost doubled while the capital-output ratio had fallen. Real income per head had grown rapidly. This confirmed that a rising capital-output ratio could occur simultaneously with a small increase in income per head. What he said on page 128 was merely a tautology which explained the capital-output ratio as capital intensity multiplied by the inverse value of the productivity of labour. In the long run, one should expect big changes in labour productivity without big changes in the capital-output ratio. If one broke down total capital into different kinds of investment, building (especially residential) represented 50 or 60 per cent of the total. He had tried to explain fluctuations in residential building, and his results showed that residential building fluctuations were correlated with investment four years ahead, marriages one year ahead and population three years ahead. As a further explanation, one could use the deviation of the interest rate on mortgage bonds from the long-term interest rate on the one hand, and the relationship between investment in residential building and national income on the other. One got a fairly high correlation here too.

The international comparisons (pages 135-6) should not be taken too seriously. A specific danger was that he was not sure how far *levels* could be compared. Fluctuations and trends were correct, but the German figures were too high in terms of levels. To test the calculation he

suggested that if the figures for the UK and the USA were reliable this might be an indirect confirmation of the German figures. He had tried to make a correlation with the discount rate but got too high a correlation with a lag of eight months. The figures for the distribution of income calculated by Pareto's alpha method were reliable only for the period 1850 to 1913.

Dr. Barna said this was a wide-ranging paper, not entirely concerned with the essential topics of the conference, but still very interesting. Most of the information was unpublished and we all looked forward to more detailed publication later in the year. Professor Hoffmann was an expert on the UK and had made various international comparisons. On the sources, Dr. Barna said it was clearly impossible to give a complete list of sources in less than 60 or 100 pages, and final judgment on the reliability of the figures must await full publication. Broadly, Professor Hoffmann's capital estimates were similar to those of Kuznets. Base-year estimates were extrapolated and then deflated. The statistics were always weak, but one sometimes had the feeling that the trends were right. Sometimes the cyclical fluctuations were right but some sort of bias caused an error in the trend. He had the feeling that we must be cautious here, for the series between 1851 and 1875, which was the weakest, might include a bias in the trend, though he was not sure. One general deficiency was that the war years were left out of the income and capital calculations. It was very difficult to make long-term comparisons in Germany where the economy had been more disturbed by wars than elsewhere. But the inter-war period (1925–1939) and the post-war period (since 1950) showed relatively high rates of growth. One could not, however, say whether this was a genuinely high trend rate of growth or merely a matter of catching up with a normal backlog. So it was very difficult to say anything conclusive about the effects of the wars, though he was sure Professor Hoffmann would try to do so.

Annual rates of growth of national income were not very relevant, but were interesting for the pre-1913 period. The United States had a higher rate of growth of production than Germany, and Germany a higher rate than the UK. This meant that these three countries were ordered inversely to their state of economic development but that might not be an adequate explanation. Figures for the capital coefficient showed a great deal of stability and all the qualifications which applied to Professor Kuznets' work applied here. The picture up to 1913 was very interesting, beyond that one could say little. The process was interrupted by two wars, and the early 1930s were so abnormal that capital-output ratios did not mean very much. The size of the capital coefficient in the three major countries was about the same.

During the morning's discussion people had been belittling the constancy of the capital-output ratio in time series. This did not seem to be true of international comparisons. The data were imperfect but the stability was striking. Dr. Barna suggested that the analysis of the structure of capital was more reliable than estimates of total capital, and

that industry might be broken down further. (Some of the increase in the share of industry was caused by territorial changes, because the territories detached from Germany were the least industrialized.) He was impressed by one similar phenomenon in the UK, where one had first of all an increasing and then a decreasing share of investment taken up by railways; but the peaks were not at the same dates. In the UK in 1870, one had a high share of investment going to railways and shipping. The textile industry also accounted for a lot, but then its share declined. Further breakdown by industry might yield interesting conclusions for Germany.

Dr. Barna was puzzled by the statement that between 1850 and 1913 the stock of capital was increasing by 2·7 per cent per annum but the volume of investment was increasing by 4 per cent. On mathematical grounds he would say that if the gross volume of capital was rising at a rate of 4 per cent, then in the long run the rate of growth of capital should also settle down round about 4 per cent, whatever concept of capital one used. Why was there a difficulty here? Was there something inconsistent in the deflation methods? This should be looked into. On the correlation between the stock of capital and the long-term interest rate, Dr. Barna was not so impressed as was Professor Hoffmann. He wondered how far the high correlation in the inter-war period was the result of high unemployment; but the correlation between the inequality of the distribution of income and investment/saving was extremely interesting.

Mr. Kaldor drew attention to the similar study made in the UK by Phelps Brown and Weber,[1] where there was a closer correlation than Professor Hoffmann's between fluctuations in the capital-output ratio and in the share of profits in the UK. He felt that, if possible, Professor Hoffmann should collect information on the distribution of incomes in Germany, not in a Pareto sense, but for the share of profits and of contractual incomes. He thought there was a close correlation between profit fluctuations and fluctuations in investment, and that the changes in the Paretian curve were a reflection of changes in the profit share.

Professor Nakayama asked Dr. Barna about the continuation of figures through periods of wartime destruction. Japanese statisticians were facing the difficult problem of how to evaluate figures for such periods and he felt that one could not really say anything useful about capital structure during wartime.

Dr. Barna agreed that he did not know how to solve the problem. After a war, though capital might have been destroyed, excessive production capacity could exist because of a shortage of raw materials. At what point of time did one get back to a normal relationship? All of the papers were aiming at analysing such long-term normal relationships, and Germany was certainly not normal in 1946–1947.

Professor Hoffmann suggested that long-term equilibrium came about 1950.

Professor Domar thought that Mr. Kaldor might be right in stressing

[1] *Op. cit.*

the relation between income distribution and the rate of investment. But he would like to suggest that causality might go either way. High undistributed corporate profits might raise investment, and hence national income, which in turn might result in higher profits. The chain could start at any point.

Professor Domar wondered why (on p. 122) Professor Hoffmann identified productive capacity with the gross stock of capital. Furthermore, it was surprising that only 25 per cent was deducted from the gross stock of capital to obtain an estimate of the net stock. Considerably larger deductions were made by Professor Kuznets and his associates, in corresponding American estimates. The relation between the gross and net stocks of capital was strongly affected by the rate of growth of capital and by its longevity. The German rate of growth might have been below the American, but then the longevity of German capital might have been greater. With the two effects working in opposite directions, the resulting situations in the two countries might not have been too dissimilar. Hence, Professor Domar thought that the 25 per cent deduction was rather low.

He also wondered whether a constant deduction of this type was justified. For instance, Kuznets' American estimates suggested higher depreciation rates (as fractions of gross investment or gross stock of capital) in recent years as compared with the last quarter or so of the nineteenth century, both because the longevity of capital was declining (with the diminishing importance of construction as compared with industrial equipment) and because there was a fall in the rate of growth of capital.

Professor Domar joined Dr. Barna in wondering whether the relation between the capital coefficient and the rate of interest on government bonds was as strong or as meaningful as the paper suggested. Perhaps it supported one of Mr. Kaldor's points, that in periods of rapid growth the capital coefficient fell, while rapid growth and the prosperity that implied led to rising interest rates.

Professor Domar said that after reading the paper he found himself attacking the Harrod/Domar model. If one studied what Professor Hoffmann said, the relation between the rate of investment and the rate of growth of income was not clear, and the figures suggested that the marginal capital coefficient varied a good deal from period to period. Otherwise it was hard to understand how the rate of investment had increased while the rate of growth of income had fallen.

Professor Hoffmann said that, as he had pointed out in the last sentence of his introduction, he had many hesitations about using the Harrod/Domar model for different periods. On the last page, he had made corrections and said that if one reduced both the capital-output ratio and the investment quota by 25 per cent, because of the methods of estimation of these figures, then the relationship between the two seemed to agree with the model.

Professor Domar said it depended on the length of the period. With

short periods the results were not significant, but similarly they were not significant if periods were very long.

Professor Hoffmann said that nevertheless Professor Domar seemed to agree that there was a lot of variation in the capital-output ratio. On the 25 per cent question, he knew the work of Professor Kuznets and thought there must have been a tendency to write off assets more quickly in the USA because of the more rapid tempo of development. The 25 per cent he used had emerged from a study of the balance-sheets of German industry since 1870, but it was only a rough guess and he agreed that there might be big differences for different industries and periods.

Professor Domar pointed out that the Kuznets figures for depreciation did not represent actual depreciation charges made by firms, but were based on certain assumed longevities of capital assets. He did not know how these estimates would be affected if one took the actual life of the assets.

Dr. Barna pointed out that since in fact the whole paper ran in terms of gross capital, the 25 per cent point did not affect the analysis.

Dr. Goldsmith thought it might affect the analysis of time trends. Could Professor Hoffmann explain what his gross figure was, and how did he bring in mortality ? Were the cost figures such that assets were removed from the accounts once the accountants had written them off ?

Professor Hoffmann replied that one had to distinguish between building and equipment. We had life tables for houses which were used as a basis for tax statistics. There might, however, be some error because he had used results for one area to estimate results for the whole country.

Professor Drewnowski asked whether Professor Hoffmann would agree that more attention should be paid to the rate of utilization throughout. Otherwise all the figures might be affected because utilization varied very much in all capitalist countries. He further asked about the breakdown of the whole economy into sectors. This was very detailed, but would it not be a good idea to divide the economy so as to separate off directly productive capital from capital not yielding direct income ?

Professor Hoffmann thought it right that industry should be split up, but wondered if it would be possible for such a long period. Nor as yet had he tried to measure percentage use of capacity. He wondered if it was really very helpful to separate productive from unproductive capital ; capital was only interesting if it gave rise to income.

Professor Drewnowski suggested that a national economy needed a certain basic standard of living, after which the possibility of industrialization was open to it. This, if true, affected the capital-output ratio and would cause it to change once industrialization started.

Dr. Goldsmith said that Professor Hoffmann's was a most elaborate system, and the only one for such a long period which was not based on the perpetual inventory but was a synthetic figure. It was interesting to compare it with perpetual inventory figures or with the results of censuses. For 1913, and for the post-war period, a group in Berlin was making an estimate of German capital stock. He wondered how the two studies

would compare, because this was interesting from a methodological point of view.

Professor Hoffmann said that the work in Berlin was being carried out by Dr. Gruenig Krengel and others. He would do his best to explain differences on another occasion. The method used was mixed, and data varied from period to period, which was a danger when one used figures. One would have to try to bring different methods together, and in making such rough estimates many methods were equally possible.

Dr. Goldsmith said he was not criticizing Professor Hoffmann. The big virtue of Professor Hoffmann's estimate was that it came from many entirely different sources, but it was still interesting to compare the two studies.

Dr. Islam enquired about the composition of investment. How was investment distributed between capital and consumer good industries at various stages and how was this distribution related to the rate of growth ? Did the results alter if one took current prices for the capital-output ratio ?

Professor Hoffmann said he had figures for the capital-output ratio in current prices, but for growth analysis we needed to deflate the series and use constant prices. For deflating different kinds of capital good, various price indices had been constructed.

Dr. Barna was right in saying that because one started at a low level after 1925 this helped to give one a high rate of growth. But was it true that the rate of increase in investment and capital stock should be parallel to each other as Dr. Barna said ?

Professor Domar thought this must happen over a long period of time, such as 30 years. Hence, the result depended where one started from.

Dr. Barna thought that mathematically the period would have to be infinite. The fact that the increasing share of investment nevertheless did not yield the results one would expect seemed to support this same point.

Professor Jöhr said that if one started off from Fig. 7 of Professor Hoffmann's paper the behaviour of the capital-output ratio confirmed what Professor Domar had said ; the ratio rose until the 1920s and then fell. Professor Domar had mentioned several factors which helped to explain this. Another was surely the theory of the French economist Fourastié who divided industry into primary, secondary and tertiary. Tertiary production was a great mixture, but one might find some truth in statistical findings and in predictions that the percentage of primary output to total production would be reduced over a hundred years. Manufacturing would rise and then fall, and finally tertiary production would expand considerably. Nowadays tertiary labour was important, with doctors, psycho-analysts, workers in repair shops, advertisers, tax collectors and so on. In such tertiary industry, labour played a very big rôle and that of capital was reduced. The general rule seemed to be that the more income increased, the more the labour-intensive tertiary sector would increase too. This would have the effect of lowering the capital-output ratio.

Professor Domar said that even though the capital coefficients in the

several sectors were falling, the changing composition of output could still have resulted in a rising over-all capital coefficient.

Dr. Goldsmith said that Professor Jöhr's tertiary industry was the same as his 'other' category. The rise of the tertiary sector in the USA had had some influence of the kind one would expect, but did not show any effect on the capital-output ratio in current prices.

Professor Hague wondered whether the capital-output ratio had fallen even in manufacturing because the tertiary sector within manufacturing industry itself had expanded, and there was an increasing number of non-manual workers.

Professor Domar agreed that the ratio of persons of doubtful productivity in industry, such as in advertising, public relations, etc., was rising. In these fields, according to Leontief, capital coefficients were very low.

Mr. Kaldor thought one ought to take value added rather than output since this was the best measure of income generated by industry.

Professor Domar said that advertising by the automobile industry would appear as value added in the advertising industry, but what real difference did it make whether General Motors employed advertising men or used the services of a separate advertising agency ?

Mr. Kaldor said one was always encountering this problem where firms bought services from each other, and it was not easy to get an unambiguous definition of value added.

Dr. Barna pointed out that Professor Domar in his paper had said what Mr. Kaldor was saying now, namely that value added was a better basis.

Mr. Kaldor thought that Fig. 7 of Professor Hoffmann's paper raised the question of what we meant by stability. He knew stability was always relative and suggested that a change in the capital-output ratio should be regarded as large or small when it was compared with the movement in the capital stock over the same period. Would Professor Domar accept this as a test of stability ?

Professor Domar said that one could always ask just how stable a capital coefficient had to be in order to be regarded as 'stable'. Nevertheless, the capital coefficient did not jump all over the place, but showed a remarkable similarity among countries.

THE DISCUSSION OF THE PAPERS BY M. BARRÈRE AND MR. THALBERG

Professor Lutz said that M. Barrère used two criteria for profit maximization. The first was maximization of the difference between the present value of the revenue stream coming from the equipment and the cost of the equipment; this difference might be expressed by $V-C$. The other was maximization not of the internal rate of return, but of the

present value of the revenue stream, V, in relation to the value of the equipment, or V/C. This idea was hardly used at all in the literature. M. Barrère took the view that those entrepreneurs who had easy access to the capital market would maximize $V - C$, while those who had not would maximize V/C. Professor Lutz could not see why it followed that a firm which had difficulty in getting capital would maximize V/C; the logic of this distinction was not clear to him.

Professor Lutz pointed out that M. Barrère's Fig. 9 was open to two major criticisms. First, the operating costs should not be current operating costs, but the present value of future operating costs over the whole life of the equipment. Second, the output to be compared with these operating costs must be over the whole life of the equipment *and* reduced to current values.

The assumption that the same output stream was produced with different types of equipment required that all equipment should produce the same output stream in terms of *present* values. But, even so, the different assets might produce output streams with different time shapes. If the interest rate changed, the present value of the output stream would also change, and different output streams which, before the change in the interest rate, had the same present value need not have the same present values after the change. So one might have to line up the equipment in a different order, which meant that one could not go on using the diagram to show how changes in the rate of interest altered the values of machines compared with their operating costs. Professor Lutz suggested that, to be accurate, the definition of 'the same output stream' needed to be much narrower. One must assume at least the same durability of assets, and a physical output stream of exactly the same length and breadth.

Turning to Mr. Thalberg's paper, Professor Lutz said that, in the first model, the effect of a change in the rate of interest on investment was really based on the assumption that labour and capital were complementary, since in Mr. Thalberg's model there was only one type of machine. So a fall in the interest rate raised the demand curve for machines, and if one assumed that this effect was immediate but that an expansion of output took time, the discounted value of machines would rise above the costs (in this case wage costs) of the machine. So, output would expand to the stationary equilibrium point where the discounted value of the services of the machine was again equal to the cost of the machine. Nothing in what Mr. Thalberg had said contradicted this view; but he took up a problem which was usually not taken up, namely, the speed with which machines were produced to bring the system to the point of stationary equilibrium. This speed of reaction was given in Mr. Thalberg's equation 4, where the rate of output of machines depended on the difference between the cost of machines and the wage rate. The rate of supply of machines would be greater the higher the ratio of machine prices to wages. As capital accumulation went on, and as this ratio fell, investment would proceed at a decreasing rate until stationary equilibrium was reached. As capital accumulation proceeded, Mr. Thalberg got the result that the ratio

of capital to labour increased, but this seemed to contradict his assumption that there was only one type of machine, an assumption which left no room for substitution of capital for labour.

Mr. Thalberg said Professor Lutz had argued that when we imagined homogeneous units of capital, we must also assume a fixed proportion of capital to labour. He did not see why. He had assumed homogeneous capital to simplify the theory. Besides, it would be a very special kind of homogeneous capital if one could not employ more or less labour in collaboration with it.

Professor Hicks said he wanted to continue with the Lutz-Thalberg discussion, and to emphasize that Mr. Thalberg was assuming a Cobb-Douglas function, with α and β adding up to less than one, so that the product was not exhausted. It followed that someone must receive what was left over. He did not think this mattered in Mr. Thalberg's analysis, which was partial and not general. There was not even any reason why the labour and capital which entered the theory should be all the labour and capital there was.

Mr. Thalberg said that the residual income went to entrepreneurs.

Professor Hicks thought this was useful for general discussion, because he felt we should not be tied down to conditions of constant returns to scale. In some cases, there was the possibility that the third factor — land — might be important. In manufacturing countries, increasing returns in the form of external economies might be important, with α and β adding up to more than 1. Mr. Thalberg did mention that, in view of the assumption of diminishing returns to scale, he had to make up his mind whether his factors were complements or substitutes. Where there were three factors, it was possible that capital and labour might be substitutes, with more capital implying that less labour was used.

Professor Champernowne said it had been claimed that in trade cycle and capital accumulation models the weakest link was often the equation explaining the rate of investment in terms of the demand for capital. It was to this question that Mr. Thalberg had devoted his attention. He himself had found model two the more interesting, but his understanding of it remained imperfect. This second model set out to explain the rate of investment in terms of the difference between the equilibrium capital stock and the capital stock already existing, and also in terms of the costs of producing capital goods at different rates of output. He himself thought it convenient to use the analogy of a dog attached to his master by a long piece of elastic. The rate of approach towards the master depended on the tension and the length of the elastic. As the dog approached the master, it came more and more slowly. This seemed to apply in Mr. Thalberg's model too. So he was puzzled as to what question Mr. Thalberg was answering. How was the rate of investment determined over time ? In the real world, the rate of investment persisted through time. One must know something about the rate of increase of the equilibrium capital stock itself in order to be able to describe and understand this persistent movement. Could one hope for a satisfactory account of

demand for investment without an account of the total amount of capital needed ?

Mr. Kaldor said he had been held up at the beginning of Mr. Thalberg's paper by the factor h introduced in addition to r. This confidence factor (h) was assumed to decline gradually with the distance in time ahead. In equation 3, the factor h was treated as if it were a simple addition to the rate of interest with e^{-hT} and e^{-rT}. If this were all, then one could say that corresponding to the market rate of interest there was a rate of interest h (which included r) applicable to the entrepreneur. But this could not be done in the situation Mr. Thalberg was tackling, for here one had an r which was increasing over time, with distant future receipts discounted more heavily than less distant ones. One could incorporate a Wicksellian type of function where the marginal rate of return varied over time. One could not add it to the rate of interest, because the rate of discount was not uniform over time, but increased at an increasing rate over time. The introduction of the confidence factor was not what it originally claimed to be.

Professor Lutz wondered if h should be made a function of the period of investment.

Mr. Thalberg did not agree with Mr. Kaldor. He said Shackle and Schneider used this construction and referred to 'the subjective rate of interest'. The income from an additional asset was in his model the marginal product of capital multiplied by the expected price of the product. If prices were expected with certainty to remain constant for all future time, one could discount at the rate of e^{-rT}. But when the confidence was assumed to decline gradually one had to multiply by e^{-hT} where T represented time.

Professor Samuelson said this was a problem of behaviour and not a mathematical point. In practice, economists like Terborgh did the same sort of thing. They applied a higher instantaneous risk factor, constant per unit of time. Some did apply a high rate to the distant future and a more modest one to the near future, but one could argue that it was not necessary to be too meticulous and that a single rate would do.

Professor Solow argued that the part of the income stream which was further ahead weighed less in entrepreneur's calculations. One could write the discount factor down to zero at some point in the finite future. One could or could not behave as Mr. Thalberg suggested towards the future. As Mr. Kaldor had suggested, entrepreneurs might well use a factor which extrapolated the recent past into the future.

Mr. Kaldor pointed out that Mr. Thalberg said (p. 162) that 'The parameter h expresses something about how confidently producers believe that the price situation will remain the same'. The trouble lay in his next sentence. 'This confidence is assumed to decline gradually with the distance of time ahead.' This sentence should be left out. If there were an equal lack of confidence in estimates of prices at all future dates, one could still add h to the market rate. It was not a question of a gradual decline in confidence in expectations. Even in the short run, h was needed.

Professor Solow said that if one supposed the market rate of interest to be zero, then one dollar after 20 years would still be worth less than one dollar after 10 years. With no time discounting, the reason must be a lower degree of confidence.

Professor Samuelson thought the notion of a uniform decline in confidence over time had no meaning. He agreed with Mr. Kaldor that this part of the paper needed amending.

Mr. Thalberg noted that Professor Champernowne had said that he was dealing with a neglected side of theory. In the literature, Mr. Thalberg had often seen conclusions about the volume of investment based wholly on the demands of capital users, the behaviour and technical possibilities of the producers of capital being ignored. Professor Champernowne's story of the dog attached to a piece of elastic was relevant. Actually, one needed to take into account not only the contracts for future deliveries which were made today, but also contracts made at previous dates for machines which had not yet been delivered. If one took the production function in equation 25, the parameters depended on the deliveries which producers of capital were committed to making by previously concluded contracts. If ship builders already had many orders, this made a difference to their readiness to take new orders.

Dr. Todorović said that while he was convinced by M. Barrère's conclusions, the fact that M. Barrère mentioned, in passing, one of the problems of the French economy raised some questions. The passage in question was on page 159. 'There is a presumption that the capitalistic firm will never reach this degree of capital intensity, because the entrepreneur will wish to keep a high average rate of profit per unit of capital. But there is a type of firm which may reach this point and stay there. It is the public enterprise which, in the absence of a capitalistic profit motive, pays less regard to the average rate of profit per unit of capital. Consequently, we may conclude that the public enterprise tends towards the highest degree of capital intensity, as we have defined it. The ultra-modern and very costly equipment of the French National Railways is a case in point.'

This passage, said Dr. Todorović, raised a point of principle, because of the way in which the theory of growth on which he himself was working envisaged the problem of dynamic and balanced growth in under-developed countries. M. Barrère was concerned with combinations of productive factors, and especially capital intensity within the firm — what the Marxist would call the organic composition of the firm's capital. He concluded that the choice lay between the highest rate of profit per unit of capital and the maximization of total profits. One could not object to M. Barrère's general reasoning, but, if one tried to fit it into the specific conditions of under-developed countries which were in the process of developing rapidly, one could ask two questions.

First, would the choice of the combination of factors differ between a macro- and micro-economic decision, and, if there were a difference, which choice was economically the more rational? Second, since in his

sense, these two decisions were different, what direction should the development of the economy take to ensure the optimum rate of growth ? Dr. Todorović went on to ask whether one should leave the individual entrepreneur to make decisions about combinations of factors for himself. Was it not better to resort to a plan whose fulfilment could be ensured by State intervention ?

Dr. Todorović said he would answer his first question by saying that one would get quite different results from macro-economic and micro-economic decisions, for the motives and the aims underlying them were quite different. The individual entrepreneur was concerned only with self-interest and was therefore over-concerned with immediate objectives. That was why he chose the highest rate of profit per unit of capital ; he would use a labour-intensive and not a capital-intensive factor combination.

Macro-economic decisions, on the other hand, were directed towards long-run goals, such as the more rapid development of the whole economy. That was why they turned towards the maximization of total profits — towards more capital-intensive factor combinations. It was quite clear why, in the early stages of economic development, the important aim was a high rate of new capital formation, and not full employment, which could be achieved only after a long period of economic development. It was useless to suppose that one could resolve the problem by replacing a labour-intensive agriculture by an industry which was equally under-capitalized.

As a result, even if one assumed that entrepreneurs could not easily acquire capital, and that labour was abundant, it was necessary to aim at maximum total profit and not at the maximum rate of profit per unit of capital, in order to achieve an optimum growth rate. This might seem paradoxical, but both Marxist theory and experience in many countries showed its truth. If the ends sought after were different, then so were micro- and macro-economic decisions.

Dr. Todorović's answer to his second question followed on logically from this. The social division of labour would be very different as it depended on micro- or macro-economic decisions. Where individual entrepreneurs made the decisions, one had the development of light industry ; with macro-economic decisions heavy industry was encouraged, to simplify the problem of the allocation of resources. Although the entrepreneur would have made his choice of factor combination in a rational way, this combination would not be economically desirable, for it would spring from the different character which heavy and light industry gave to the timing and nature of economic development.

Dr. Todorović concluded that there *was* a difference between macro- and micro-economic decisions — that the behaviour of simple and complex economic subjects, especially the State, could not be reduced to a simple rule, least of all with under-developed countries. This was why priority must be given to macro-economic decisions. Theories defining the opposite point of view suffered from an inherent defect, since they were inconsistent with the optimum rate of growth.

Dr. Barna raised three points on M. Barrère's paper. First, M. Barrère had argued that in a given industry with a given technique, the capital-labour ratio was wholly determined by technological factors. This was too strong. Institutional and economic factors also played a part, for instance, shift-working. Second, there was the argument about complementarity between capital and labour. Was this the same as the linear-programming approach, which gave identical results to those of marginal theory ? Third, Dr. Barna felt that in such a highly abstract paper it was not easy to derive conclusions about anything so specific as the French Railways. For one thing, they were a monopoly ; for another, investment in railways was much less risky than in small manufacturing industries.

Dr. Barna also made several general remarks. As Professor Lutz had said in his introduction, one of the subjects before the Round Table was the theory of investment in the firm. We had only these two papers on that topic, and they covered only a very small, specialized corner of the theory of the firm. In all theories one could distinguish normative from positive economics, and both these papers were normative. Positive theory started from observed behaviour. In this field of the theory of investment in the firm, there was a substantial gap between normative and positive theories. Terborgh was one exponent of a positive theory of this kind, while another was Joel Dean. The latter's ideas on capital budgeting and rationing used very different criteria from writers dealing with normative theory.

All one could say was that a reconciliation of the two theories would require the discovery that some assumptions of normative theory were unrealistic. For example, what were we maximizing ? Joel Dean's theory implied the rationing of funds. Second, perhaps the behaviour of entrepreneurs was not rational ; or perhaps they could do better if they knew more economics. Deeper research might lead to the discovery that the entrepreneur really was doing something sensible, but was too inarticulate to give a rational explanation of his actions to economists. The restrictive thing about both these papers was that they dealt with investment as a once-for-all happening. In real life, much investment was carried out by large corporate bodies as a continuous process. Dr. Barna suggested that any theory should admit that entrepreneurs took account of the fact that in future both general and *relative* prices would change. Did they, then, want to maximize a constant real income stream — or something in terms of money ? Firms did take account of the fact that both real and money wages would rise in the future, which implied that if they were investing in durable assets, they had to allow for what would happen to wages over a long period of time — perhaps ten or twenty years.

All investors expected continuous technical progress. In the short run they expected the capital input to fall continuously in relation to output. If one expected rapid technical progress, one would invest less at present. With atomic energy projects, for example, one needed a more accurate theory to determine the correct rate of investment at present.

From the point of view of society, the best situation was one where there was a high rate of technical progress but this was not foreseen by entrepreneurs.

Professor Domar thought that Dr. Barna had made an important point on technical progress. It might very well be true that technical progress had a dual effect on investment. On the one hand, technical progress created investment opportunities in general, and called forth more rapid replacement by making existing assets obsolete. On the other hand, the fear of future obsolescence might make firms hesitate to acquire capital assets, particularly for replacement purposes, at any given moment. The length of time which had elapsed since a given asset was acquired was of major importance here.

Professor Domar was unhappy over M. Barrère's identification of funds invested in fixed assets (machinery) with the firm's worth. Fixed assets represented only one of many uses of a firm's funds. After all, from the firm's point of view the distinction between what economists called fixed capital — buildings, plant and equipment — and labour lay essentially in the timing of payments. An employee hired on a long-term contract acquired, from this point of view, the attributes of capital, while a machine which could be rented for short periods of time was similar to hired labour.

On the question of the correct magnitude to maximize, Professor Domar thought this should be discounted profits. But a comparison in terms of absolute amounts could be misleading, since a larger project could obviously have a larger present value. If market imperfections excluded free borrowing and lending, it might be more useful to assume that the firm had a given amount of funds, and tried to maximize the present value of a combination of various investments subject to this limitation.

Professor Hoffmann turned to M. Barrère's micro-analysis. One point was that capital per man differed between industries. There were many cases where capital per man-hour and wages were both high. If one studied capital per man-hour over time in different branches of manufacturing industry, one found that relations between branches were *not* changing very much. The rate of growth of capital stock per man was nearly the same in each branch of industry in Canada, in Australia, in New Zealand and in the USA, but different in the different countries. Professor Hoffmann thought that subjective decisions on capital ratios were therefore greatly determined by general technical progress.

Professor Hicks considered two questions in M. Barrère's paper. First, what did the entrepreneur maximize ? We had not got to the bottom of this question while discussing Professor Lutz's paper. In particular, did he maximize a sum or a rate ? Professor Lutz had discussed the Wicksell theory at length in terms of maximizing a rate. Thinking this over, Professor Hicks felt that Wicksell had used that approach because he was working on the assumption of constant returns to scale. Then, even in perfect competition, there was nothing for the entrepreneur to maximize but a rate ; this was a mere matter of history.

Another set of assumptions where the answer was clear, was where one had perfect competition all round, where constant returns to scale were not assumed, but the nature of the firm's resources determined its size, or at least its growth pattern. Here the entrepreneur should be maximizing the present value of his production plan — the present value of his assets. Neither of these sets of assumptions was realistic, and it was not clear whether the entrepreneur maximized either the first or the second in the real world. What he did could not be reduced to simple terms. Sometimes one aim was important, and sometimes the other. In general, the entrepreneur was trying to get to the best position he could, and that depended on the market conditions which confronted him. One could not deal with this case by either Wicksell's or Barrère's solution.

The second question was the choice of techniques. One found an unholy alliance between people who, for quite different reasons, were enamoured of the assumption of fixed techniques ; some of them because the assumption allowed them to do various calculations more easily. He did not suggest that we should go back to smooth isoquants, but he did feel that we should think more deeply. If we were concerned with long-period theory, we ought not to consider only a situation where one machine was employed with one man. This was correct enough in the short run. In the long run, however, one could alter the design of the machine. It was not impossible to alter the machine to allow for changing factor prices, and it might be maintained that there were economic forces which should cause this to come about.

In the printing industry, when one was ordering a large piece of equipment, one would make one's choice on the basis of a general specification, but after that there would be negotiation as to the details of the kind of machine that would be best, and here one had considerable opportunity for making substantial modifications. Even if the machine producers did not themselves look for possible changes, the fact that all clients were asking for the same adjustments would mean that the change got into the catalogue. This showed that, even within a particular technique, a considerable amount of adjustment was possible. This should be allowed for in our theory — one should represent each technique, not by a point, but by an isoquant for that technique. This was similar to the short-period cost curves used by Mr. Harrod, where the effective choice was along the envelope of the short-period curves. Professor Hicks felt that the classical approach made rather better sense if one allowed for shifts round about a given technique, than if one just assumed two techniques and discontinuous shifts between them. We ought to allow for this in our theory.

Professor Robinson agreed with Professor Hicks on this point, but also suggested that, even with *given* machinery, factor proportions were not too rigid. The motor industry had added simple transfer devices to standard — or even existing — machines. In the printing industry, the basic factor was the keyboard of the compositior, but what he produced depended on the quality of the copy he was given, and, with most authors, it paid to prepare the copy for the typesetter.

There were similar qualifications to the idea that a given technical system carried its own capital-labour ratio into a new economy. In Huddersfield, one had 12 looms of a particular type to each weaver; in East Africa, one had one loom per weaver and one assistant to every two of these weavers.

Mr. Kaldor replied to Professor Hicks that he did not think Wicksell considered indeterminacy. In an uncertain world, it was reasonable to assume that the size of an establishment was limited by the amount of capital owned by the entrepreneur. This justified Wicksell's approach as compared with those who assumed that firms borrowed as much as they could. Mr. Kaldor said he had never intended to suggest that capital and labour could not be used in varying proportions. What he *had* done was to suggest that the choice of techniques depended on the level of wages in terms of commodities, and only to a minor degree on the rate of interest, and to deny that in a dynamic world there was a functional relationship between the real wage and the rate of interest (or product), since productivity was not constant but rising over time.

Professor Solow agreed with Professors Hicks and Robinson. Professor Hicks' second point connected with what he wanted to say on the views of M. Barrère and Dr. Barna. Whether or not each technique required a rigid capital-labour ratio, or whether techniques could be mixed together by an entrepreneur, one could obtain an equal output curve, the concavity of which would depend on the possibility of substitution. What *was* true was that there would be times when small changes in the relative price of machines and labour would lead to sharp changes in techniques, while at other times they would not. With given techniques, variations in the proportions between capital and labour were large. First, much capital consisted of buildings. Second, in many industries a large proportion of the labour force was engaged in repairing and resetting. Professor Solow did not think it was quite clear that rapid technical progress, if anticipated, would decrease the volume of investment, though it might bias entrepreneurs in favour of investing in equipment with a short life. With technical progress, current rates of return would be higher, and the volume of investment would depend on the full context of investment decisions.

M. Barrère recalled that Keynes had said that the French economists were eclectics. We were here dealing with an abstract subject and each particular model had its own assumptions and conclusions. He agreed with those who tried to bring realism into their abstract models, for such a model was not a demonstration of reality, but a way of approaching it. So, one ought to correct the conclusions of a model by reference to reality.

It followed that his answer to Dr. Barna's question about railways was to say that ideas had preceded theory. He had tried to explain why the railways spent so much. The reason was that they were nationalized and had much money; public companies tended to use more capital. This showed how his observations could be made general by translating them into reality. M. Barrère pointed out that he assumed his entrepreneurs to behave rationally, which might lead them to change from maximizing

total profit to maximizing the rate of profit. Similarly, with long-term investment, there were many uncertainties, and one had to allow not only for technical changes but for the technical facts of the existing situation. Complementarity between labour and capital lay at the root of his reasoning and such complementarity affected the organization of firms in some ways. So, despite the limitations of his theory, entrepreneurs must behave rationally, taking the given rate of interest and rate of wages.

M. Barrère thought there had not been enough distinction between his theory and traditional ones. Traditional economists considered substitution at the margin ; they tried to explain what entrepreneurs did and not the behaviour of the firm. M. Barrère thought a more complex theory would explain more. In order to use a machine, a firm must have decided which machine to use. When a firm was actually using a machine to produce, there might be substitution ; but when it was originally bought there would not be. Professor Lutz had asked two questions. He had said that the diagram could not explain entrepreneurial conduct, because changes in the rate of interest led to a move of the whole curve and so altered total profit. But the rate of interest was given for the whole production period and one could work in terms of a rate of interest that had to equate future and present values. Professor Lutz had also asked why it was hard for entrepreneurs to get capital. The answer was that, even if it was not physically very hard when there was a very high rate of interest, the entrepreneur might not be a good risk if his firm was small.

Finally, Professor Todorović had said that in under-developed areas where capital was scarce the firm would try to maximize the rate of profit. M. Barrère agreed that one needed planning to control investment, and also with Professor Todorović's views on micro- versus macro-economics. He thought planning need not mean socialism, but that under-developed areas would develop more successfully if there were planning.

THE DISCUSSION OF MR. KALDOR'S LECTURE

Professor Hicks said he would have to begin by asking the mercy of the meeting. Since Mr. Kaldor's was a verbal contribution, he had had no opportunity for preparation. Fortunately, Mr. Kaldor had followed previously written works fairly closely, but in the circumstances he thought the best thing was to suggest how to split up the discussion. Mr. Kaldor's contribution could be divided into three parts. First, there was the model without technical progress. Second, came the characteristic theory of distribution reached by turning round the savings/income relation — a Keynesian distribution theory. Third, there was the highly original theory in which technical progress was brought in as a variable. So far as the first topic was concerned, all he would say was that he did not feel there were any marked inconsistencies with neo-classical theories ; indeed, many of the conclusions could be reached by a more neo-classical

route. So far as the distribution question was concerned, there would be an opportunity for discussing that when we came to Professor Solow's paper.

So we were left with the third topic, with technical progress as a variable, and we should concentrate on that. His own position here was especially difficult, because the exposition had come at the end of the paper, by which time we were all getting tired. He did not propose to discuss technical details, only to comment on methodology. Professor Hicks said he was nervous about what we were trying to do. We were trying to compare a growth theory of this kind with a theory of the trade cycle. With trade cycle theory, one was looking at fluctuations in the economic systems of a nation, or of the world. One could treat these as separate phenomena and try to produce generalized explanations into which one could fit observed cycles. Such fitting of theory to experience was mainly a matter of putting in qualifications. But when one was dealing with growth in general, he doubted if one had separate phenomena of the same kind. One had only a single story to explain, and the production of any alternative explanation was a job for the historian rather than the model-builder. This was not to say that theoretical models were of no help, but he was not convinced that because a model happened to fit the particular statistics, that was necessarily a good mark for the model itself. A model should tell us what *could* happen rather than what *had* happened. When dealing with a single story, he did not feel that what had happened in various countries differed much. So he wanted a theory which could tell us what was round the corner, rather than one which explained the past.

Professor Champernowne said that, in this third stage, Mr. Kaldor's diagram assumed just one rate of growth of capital per head, causing an equal rate of growth of output per head. If we called the equilibrium rate of growth of capital and output per head, t, and if, at the same time we called the rate of growth of population, l, then the equilibrium rate of growth of capital and of output would each be $t + l$. In any given economy, there would be some equilibrium capital-output ratio, depending on historical circumstances. It would be interesting to consider a case where the rate of population growth, l, was fairly small, and the growth rate of output per head, t, also small, so that the equilibrium rate of growth of capital per annum was also small. This could lead to a contradiction if the savings proportion was substantial, if the minimum rate of profit, P/K, was substantial, and if capitalists saved a large proportion, α, of their incomes. Here the minimum ratio of savings to capital might be greater than the equilibrium rate of growth of capital. In this situation, the economy would not settle down to equilibrium, but would run into a stagnation crisis.

On a different point, Professor Champernowne suggested that this model would be more satisfactory if one could reasonably suppose that the TT curve would remain still, than if it wandered. It was difficult to see what meaning could be given any points of the curve, other than that

where one actually was. It was easy to see the meaning of a point describing the present rate of growth of capital per head or output per head, but it was hard to find a meaning for any other point.

Mr. Kaldor said he agreed with Professor Champernowne. A stagnation crisis could arise for two reasons. First the P/K which was equal to $\frac{t+l}{\alpha}$ might be lower than the minimum acceptable P/K (P/K min). So the rate of profit would be too low to maintain a continuous inducement to invest (here, $\beta = 0$). Second, the equilibrium value of P/Y might be lower than the minimum rate of profit given by the existing conditions of competition. If that happened, it simply gave one the basis for a trade cycle model, where the economy had too few investment opportunities for either a high enough share of profits, or a high enough rate of profit. So the economy would run down, and unexploited ideas would accumulate. One feature of the capitalist system was that when it grew, it grew at a fairly high rate. The boom rate of growth was above the average. In a slump it grew little, if at all. If t and l were both high, any slump would be short. If they were low, the system could only grow by fits and starts.

On Professor Champernowne's second point, the whole value of the approach lay in the fact that the curve TT was reasonably stable. There was a good statistical method of testing the stability of a curve, namely, whether the rate of growth of productivity was fairly steady during periods of full employment growth — comparing successive booms. American data suggested that it *was* fairly stable, but not constant. He did not think these changes in the rate of growth of production between successive boom periods were big enough to prevent the device being useful, despite Professor Champernowne's hesitations.

Mr. Little considered the nature of equilibrium without technical progress. Supposing, in stage three of Keynesian equilibrium, one had an increase in risk or in generalized liquidity preference, capitalists would invest less, and this would require a rise in real wages to avoid a savings-investment crisis. But this would make it difficult to achieve rapid long-term growth, which required increased savings to keep up with growth of population. It followed that one must bring in a mechanism to make capitalists consume when they did not want to invest.

Mr. Kaldor said that when one reached the stage where P/K was equal to P/K min., then if the risk factor and P/K min. both increased, the economy moved 'backwards' along a Wicksellian curve, thus reducing K/Y to the extent necessary to restore P/K min. to the level required by the rate of growth. A change in the capital-output ratio had provided a way of adjusting the marginal risk premium to the ruling rate of profit. Meanwhile, one would have stagnation crises, and he would not suggest that such changes were possible without stagnation crises intervening.

Dr. Barna recalled that he had said that the fact that capital lasted a long time made the stability of the capital-output ratio less interesting. He had now convinced himself from Professor Hoffmann's paper that this was quite wrong. It was really very impressive that the capital-output

ratio had stayed at 4, while the volume of capital rose eight-fold between 1850 and 1950.

Dr. Barna suggested that part three of Mr. Kaldor's paper represented a definite advance from the point of view of model-building. We now had a 'family' of Keynesian models — and especially those of Harrod and Joan Robinson. In both of these great play was made with the idea that the flow of innovations varied in character — some being capital-saving and some labour-saving. Because of this, both said that there was inherent instability in the economy if one had changes in the flow of ideas. Dr. Barna suggested that Mr. Kaldor's model represented a great advance from the point of view of methodology. The exogenous element in the Harrod and Joan Robinson models was now brought inside the system. With any rate of flow of ideas, the process of selection by entrepreneurs was such that there tended to be balanced development. This paper argued that there was greater stability in capital development than other models had shown, because, whatever the flow of ideas, their effective adoption took place in such a way as to give 'neutral' technical progress.

However, we had a straight-line demand curve and a supply curve, and all we knew was the equilibrium point. There was still another step to be taken ; we must find how the curve itself would shift up and down. He agreed with Professor Champernowne about this, that it was logically impossible to tell whether we were measuring shifts in demand or in supply. Nevertheless, Dr. Barna thought the conclusion was rather important, and was the one stated by Professor Hicks. We needed to bring in other sciences to explain why things happened as they did. Mr. Kaldor was here saying that to explain the rate of progress one had to make sociological and psychological studies of behaviour. We needed to do something beyond economic policy to change the ideas of entrepreneurs.

Mr. Kaldor said he was trying to get away from the rigid idea that if the capital-output ratio remained constant, this was caused by a peculiarity in technical progress — by its 'neutrality'. The assumption seemed too restrictive, so he had postulated that technical progress was both capital- and labour-saving, depending on the extent to which it was exploited. There was one rate of capital accumulation at which technical progress was neutral. In some sense this was a restriction. One could say that technical progress was such as to cause the curve to shift. One could not tell whether one was concerned with the shape of the curve, or whether the whole was moving. The interpretative value of the function depended on how stable these magnitudes were in fact. As a piece of economics, this model tried to show that the ultimate causal factor was not savings or capital accumulation, but 'technical dynamism' — the flow of new ideas and the readiness of the system to absorb them. Readiness to adopt ideas varied from country to country, and might stimulate the actual flow of ideas. One could not distinguish the two, but this whole factor was the one responsible, in his view, for the fact that some societies grew faster than others.

Professor Samuelson suggested that Mr. Kaldor was in danger of over-

differentiating his product. We could have a neo-neo-classical theory which would behave in exactly the same way. The consequence of the slant imparted by Mr. Kaldor was to give it a different appearance. He himself would use certain techniques, and if he were asked to rewrite the model to fit Professor Haberler's book, he would put in alternative activities which would take most of the sting out of Mr. Kaldor's system. He would accept the challenge and be von Neumann-like enough not to use smooth partial derivatives.

Although Professor Hicks had suggested that history was a once-over affair, Mr. Kaldor's system had great consequences for policy. If we changed government policy, and ran budget deficits and surpluses, we could make some of the functions observable. His own instinct was that one could introduce many alternative activities, which, while not like those of Haberler or Knight, would give a behaviour in terms of comparative statics which turned out to be very different from that of Mr. Kaldor's wider setting. He would be surprised if the real world was like Mr. Kaldor's. He might be over-concerned with the USA, but the real world did not have these extreme-angled corners. Something like a neo-neo-classical system modified by a militant fiscal policy would give a better description of the future of the system.

Mr. Kaldor said his general difference from Professors Hicks and Samuelson lay in his preference for macro- as against micro-techniques. Those who believed in macro-theory might say that its great virtue was to exhibit the reaction mechanism of the system in terms of a limited number of parameters and variables. He was not saying that a many-variable system could not give the same results, but he did not see how one could handle the model unless one restricted oneself to a limited number of variables. Professor Samuelson was quite right that if some constant were introduced one could observe behaviour at different points on his curve. He had some experience of that in particular countries, for instance, Norway, which had the hothouse stimulation of capital accumulation after the war. There were vast government projects raising the investment/output ratio far beyond that of other economies. Before the war, the capital-output ratio in Norway was constant. As a result of post-war policy, there was a dramatic rise in capital-output ratio from about 3 to about 6, and out of all proportion to that elsewhere.

Professor Samuelson believed in using only a manageable number of variables. He had often changed his own view of macro-economics in the great depression, and would be prepared to adopt new types of macro-economic theory. But if he were forced to stick to a crude Harrod-Domar type of model with no capital deepening, then he felt macro-economics of this kind was not rich enough. What sort of blown-up micro-economic system gave the best answer ?

Mr. Kaldor said this was the real difference. We could fight traditional neo-classical theory like that of Samuelson or Solow on two different grounds. First, there were the inherent logical difficulties of defining capital, and knowing what we meant by our parameters. Second, we

could, as Professor Lutz had shown, appeal to the facts. If we lived in a world where the capital-output ratio was steadily rising and the rate of profit falling, and where progress meant a movement along a production function of a Wicksellian kind, with the rate of interest falling as more capital was used, he would agree that we ought to use this sort of approach. But we knew this was not the case. It was the shift in the curve, not the move along it, that had most importance. Mr. Kaldor said his approach was not invented out of nothing, but was forced on him by working on capital theory for over 20 years. He did not mean to imply that techniques in any individual industry did not respond to a rise of wages, or to a fall of profits, though this latter was not very important in practice. But the appearance of capital-saving inventions induced the adoption of compensatory labour-saving inventions in other parts of the economy. That could be described within the framework of his model.

So far as he could tell, the basic idea was true ; labour-saving innovations were generally adopted in those industries where technical progress was lagging. Technical progress over the economy as a whole led to a rise in real wages. Labour became constantly dearer in terms of instruments and machines, so that industries not experiencing spontaneous technical progress were forced to adopt new methods, even if these raised the capital-output ratio. This process could be perfectly easily explained without resort to any classical production function. There was no sense in concentrating on this function when we knew it was only the shift of the curve which was of any interest.

Professor Solow wondered whether Mr. Kaldor would be entertained by the idea that the technical progress curve (TT) might shift systematically with K/L. One could argue that in the nineteenth century the flow of ideas was such that labour-intensive processes were easier to improve. Recently things might have been the other way round.

Mr. Kaldor replied that this was an interesting question which required a study of the facts. The general conclusion was that any acceleration of technical progress meant a fall in the capital-output ratio, subject to one important qualification ; namely, the invention of new products. The invention of railways did not bring just an improved means of transport, but a new and highly capital-intensive product. This made it possible for technical progress to bring about a rise in the capital-output ratio.

Professor Solow said that even if one assumed a technical production function, one need not use it for distribution purposes. But the only relation giving the stable curve which Mr. Kaldor wanted was the Cobb-Douglas function. Any other production function would give a family of curves.

Mr. Kaldor agreed, but thought it quite possible that there were no Cobb-Douglas functions ; there was no way of telling.

Professor Hoffmann asked whether it was necessary to suppose that labour productivity could only be increased by an increase of capital per man. If labour productivity could be increased by better training, the capital-output ratio would also change, and this was important for under-

developed countries. Second, even if one accepted that the marginal capital-output ratio was constant, this said nothing about the average capital-output ratio, which was important for labour productivity.

Mr. Kaldor agreed that not all improvements in productivity required additional capital. But the system tended to the point where both sorts of improvements took place. Mr. Kaldor also agreed that his model did not say at what level the capital-output ratio was constant; it only implied that capital and output both increased in the same proportion. He suggested that the reason why K/L settled down at about 3 had something to do with minimum rate of profit, which governed the rate of accumulation. Firms would only invest when the prospective rate of profit was above the 'cut off' point. If the minimum rate of profit was some function of the capital-output ratio, that would make the system determinate. The higher the capital-output ratio, and the higher the ratio of fixed to circulating capital, the more risk there would be, because investment depended on expectations concerning the more distant future if entrepreneurs were to recover outlay. The longer the period, the more uncertain the expectations. This could explain why the ratio of capital to output was 3 rather than, say, 6.

Mr. Thalberg said that the figures mentioned for capital in Norway were higher than the official figures. But there were pitfalls in the official ones. The system of taxation meant that firms called investment things which were not. Also some post-war plants were built to last for a very long time, and employment increased substantially in the first post-war years, but later this growth stopped and at the same time the degree of 'overfull' employment decreased.

Mr. Kaldor did not think this disproved the general point. If the process of development were accelerated by government policy, the capital-output ratio could increase to much higher levels. In Communist countries, where investment was much accelerated, one would find higher capital-output ratios. K/Y was not determined merely by the existing state of technique.

Dr. Barna pointed out that this investment had not led to any impressive increase in Norway's output.

Professor Domar said that Mr. Kaldor parted company with Harrod by using a variable propensity to save, or, more exactly, by making the propensity to save a function of the ratio of profits to national income. Did Mr. Kaldor's model work then as follows: a high initial ratio of profits to national income called forth large investment and a high rate of growth; this in turn raised payrolls and thus depressed profits and hence investment and income?

Mr. Kaldor said this was not his mechanism. One had to distinguish between saving and investment. Business men decided to invest a proportion of turnover, say, 10 per cent of expected sales. This made the investment/output ratio 10 per cent. If the rate of profit was at such a level that the share of savings was 20 per cent, the contraction in demand, which was due to the high propensity to save, would go on to the point

where active saving equalled active investment. The representative firm would invest 20 per cent of expected value added. If the firm expected sales to grow at the rate GY and the capital-output ratio was constant, capital expenditure would be $G\frac{K}{Y}$. If *ex post* profits were less than *ex ante*, this was partly because output went down and partly because actual *ex post* profit margins were lower than *ex ante* ones.

Professor Domar wondered if faster growth was impossible because output hit a Harrodian type of a 'ceiling'. Otherwise, why should entrepreneurs' expectations not be satisfied ?

Mr. Kaldor did not think this a satisfactory explanation, in a Keynesian system. The adjustment of savings to investment was rather through prices relatively to wages than through adjustment of output. A fall in prices in relation to wage costs implied, of course, a decline in the share of profits.

THE DISCUSSION OF PROFESSOR CHAMPERNOWNE'S PAPER

Professor Samuelson opened the discussion. He said that this was an excellent paper, and it was difficult to say much about an excellent paper, once one had called attention to its quality. However, it did 'point up' some things said at earlier sessions.

First, this was what had earlier been called a neo-neo-classical model, with none of the basic technologies of the different goods obeying smooth

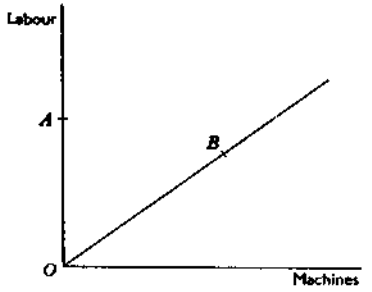

FIG. 26a

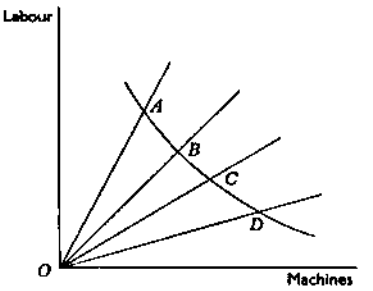

FIG. 26b

differential functions. As in Fig. 26a, one had only two activities. One could either have a certain amount of labour, as at A, or less labour and more machines, as at B. But since each of the different goods gave one a point like B, one had (as in Fig. 26b) a continuous range of points, when one measured machines in value units. Other things being equal, accumulation out of income would, as time passed, lower the rate of interest and raise national income. Machines were substituted for labour

as the current rental of their services fell relatively to the wage rate, but it was the process of accumulation — the 'deepening' of capital — which lowered the profit or interest rate to bring this about. The 'time horizontal' comparison of machine and human service costs was — in this and more general models — a pale reflection of the 'time vertical' comparison of interest and discount as capital was accumulated over time; for one had to remember that machines were producible over time.

A second point was that Professor Champernowne's continuous spectrum of goods could be replaced by a large, finite set of goods, with different factor proportions. This was similar to Hotelling's analysis of spatial competition;[1] again, Houthakker, in a study of the production function in textiles,[2] had taken a spectrum of firms and ended with a Cobb-Douglas function. It was not necessary to insist on a continuous spectrum. What one got by replacing it was a time shape of interest and of the marginal efficiency of capital, with many steps, and these could be as small as one pleased. The picture would remain essentially the same.

One could go further and assume that there were more than two technical possibilities of production in a given industry, though, again, one need not have a continuous spectrum. He had written down in Table 28 the typical case in terms of activity analysis.

TABLE 28

Outputs (+) and Inputs (−)	Activities		
	1	2	3
$M_0(t+1)$	$+1$		
$M_1(t+1)$		$+(1-u)\gamma_0$	
$N(t+1)$	$+\alpha f$	$+\alpha\beta_0$	$+\alpha\beta_1$
$O_c(t+1)$		$+1$	$+1$
$M_0(t)$		$-\gamma_0$	
$M_1(t)$			$-\gamma_1$
$N(t)$	$-f$	$-\beta_0$	$-\beta_1$
$O_c(t)$			

Outputs at the end of the week were designated by $(t+1)$, new machines by M_0, week-old machines by M_1, labour by N and consumer goods by O_c. The first activity was that of making new machines, and it could be seen that one new machine could be produced with a labour input of $-f$. As a by-product, one would have α times as much labour at the end of the period. The natural rate of exogenous growth in the labour force was $(1-\alpha)=G_a$. The remaining blanks in row 1 showed that there was no other way of making machines.

The second activity was the production of consumer goods, and the

[1] *Economic Journal*, 1929, p. 41.
[2] *Review of Economic Studies*, 1955–1956, pp. 27-31.

second column showed that one unit of consumer goods could be produced by $-\beta_0$ of labour and $-\gamma_0$ of machinery. As a by-product, labour was increased, as if exogenously, by $+\alpha\beta_0$, and the $-\gamma_0$ new machines produced $+(1-u)\gamma_0$ old ones.

The third column showed that alternatively one could produce $+1$ consumer goods with $-\beta_1$ labour and $-\gamma_1$ old machines. No new machines would then be produced. We had here an example of depreciation in Dr. Barna's sense of a deterioration in current functioning. This was shown by $\gamma_1 > \gamma_0$ or $\beta_1 > \beta_0$, more inputs being required with old than with new machines. In Akerman's model, u was equal to zero, and old machines were as good as new ones, so that $\gamma_0 = \gamma_1$ and $\beta_0 = \beta_1$.

Professor Samuelson pointed out that if one assumed a similar situation, with many commodities and alternative processes allowing a wide range of coefficients and 'factor proportions', one would obtain results similar to those of his paper.

Professor Domar wondered whether the fall in the rate of interest was the only reason why robots became cheaper. What about technical progress in the robot industry ?

Professor Samuelson thought that in this model, technical progress did not create difficulties for capital deepening, but only problems of exposition, and *Professor Champernowne* agreed.

Mr. Kaldor commented on some implications of Professor Champernowne's model. Put in words, the reaction mechanism was such that at any one time, the degree of mechanization determined the rate of interest. He agreed that the relationship would give a smooth curve so that the state of accumulation would determine the rate of interest, which in turn would give the proportion of income saved. This would determine the rate of investment, the rate of accumulation and the rate of growth.

Professor Champernowne had introduced uncertainty at the stagnation stage, by introducing a minimum level for the rate of interest. Admitting that there was a bottom stop to the rate of interest was the same as saying that there was uncertainty. At the bottom stop, the reaction mechanism took on an entirely different character. The rate of investment became the primary factor ; savings and the distribution of income both became passive. Any further accumulation depended on new opportunities, either through population growth or technical progress. With the rate of interest at the bottom stop, the system continued to grow, but there was no further capital deepening. The rate of accumulation was then determined by what could be done without further deepening. Mr. Kaldor said his own model started where the Champernowne model ended — at the point where the rate of interest had reached the minimum, so that investment took charge, and one entered a Keynesian world. Certain phenomena in modern capitalist societies indicated that they had reached this stage. The most striking feature, which was not explained by Champernowne, was that fluctuations in the rate of accumulation were reflected in changes in the distribution of income and the savings ratio. It was hard to explain fluctuations in the rate of accumulation except by

saying that without further deepening, the rate of capital accumulation depended on population growth, expectations and so on. The whole model then took on a new character in which technical relationships were displaced from a dominating rôle.

Professor Champernowne sensed some misunderstanding about the place of uncertainty and stagnation, which were dealt with on page 228. Stagnation occurred where one had done all the machine building one could do, and whether one would reach this stage depended on population growth and technical progress. One might reach a golden age first. Machines might become very expensive, and, in the absence of uncertainty, there would be unbounded opportunities for spending money because of the cost of machines.

Mr. Kaldor said that, in other words, the rate of interest approached zero asymptotically.

Professor Champernowne agreed, but with the proviso that people must not foresee the state of stagnation. The model was dominated by savings, not investment, and assumed that people always did the cheapest thing. Without this assumption, the theory would be impossibly complicated. To illustrate the real world, one would have to bring in savings and the demand for investment goods.

Mr. Kaldor suggested that one could alternatively bring in a bottom stop for the rate of interest.

Professor Samuelson said that everyone was being very conciliatory, and he did not wish to disturb the serenity, but he would like to turn to one historical phenomenon which had been mentioned, and try to find an alternative explanation. This was the well-known fact that in a recession in the USA the share of profits in income went down, and in prosperity it went up. He would hesitate to postulate a firm relationship between investment and corporate income, and regarded this as a dynamic phenomenon. Entrepreneurs paid out costs in advance of commitments, but if there were a fall of, say, 30 per cent in US activity, he thought that, in time, entrepreneurs would work off their fixed commitments so that the share of profits would rise. It was just that they were caught off their guard. They had to get down the break-even point and, given time, this could be done.

Mr. Kaldor pressed Professor Samuelson not to dispute the fact that the fluctuations in the share of profits were themselves caused by fluctuations in the rate of investment.

Professor Samuelson pointed out that we had no scatter diagram with which to do a regression analysis. Nature had not made the experiment.

Mr. Kaldor replied that he did not think one could explain short-period phenomena without a Keynesian type analysis based on expenditure decisions.

Professor Domar asked whether, in the absence of population growth, capital accumulation was the propelling force of Professor Champernowne's system.

Professor Champernowne hesitated to say which pushed and which was

pulled. Employers had savings at their disposal and used them to introduce robots which would save them from losses.

Professor Domar concluded that if savings, and hence capital formation, rose, the rate of development would also rise ; but since population was given, the economy would be gradually saturated with robots, and the system would eventually come to a halt with no further investment opportunities. What rôle did technological progress play in this system ?

Professor Champernowne replied that two cases of technical progress were easily dealt with. The first was the case where the number of robots per operative increased. The second was the situation where the cost of existing types of robots fell. Finally, he thought one could deal with the case where new industries were mechanized, and new products made with these machines.

Professor Sylos Labini considered the reaction mechanism. More machines led to a relative fall in consumer good prices, a lowering of the rate of profit, and rising real wages through the falling prices of consumption goods. The internal consistency of the model was good, but was it at all near to reality ? There was an assumption of pure competition, yet if we looked at the real world, we found quite the reverse. The prices of commodities whose output was mechanized did not fall relatively to others. Raw material prices, for instance, did not rise relatively. It followed that the ways in which mechanization spread through the system were not those described by this model. Statistical analysis showed that in the earlier history of the USA mechanization had led to some fall in the relative prices of commodities made by mechanized industries as compared with the products of the less-mechanized industries. Yet now that concentration in industry had reached a high level, and the assumption of competition was more and more removed from reality, the model became less realistic. Professor Sylos Labini said that a major difference of opinion in the Round Table had been over whether the assumption of pure competition and constant returns to scale was needed. While in Mr. Kaldor's model none of these assumptions was necessary, Professors Solow and Champernowne introduced them. But they certainly did not give realism. Differences between the models lay in their assumptions rather than in the techniques used. Yet internal consistency was only one test of models — realism was the other.

Professor Robinson wanted to reinforce what Professor Sylos Labini had said. Professor Champernowne brought in price-elasticity but not income-elasticity. These models supposed a hierarchy of possible investment schemes which might be carried out. Was it realistic to abstract from income-elasticities ? If one relaxed the assumption of constant returns to scale, then surely one had to allow for the fact that growth would occur in those industries where capital investment was profitable because income-elasticities of demand permitted expansion.

Professor Champernowne said he had tried to determine the influence of income-elasticity of demand by assuming that profit-earners had a different pattern of expenditure from wage-earners. Unfortunately, the

results were complicated. One had to take into account the correlation between the degree of luxury of each good and the complexity, and hence the cost, of the machine required to make it, so that the results were very awkward to work out. Nor had he investigated economies of large scale.

On Professor Sylos Labini's question of whether mechanization meant that manufactured goods became cheaper relatively to hand-made ones, he thought they did.

Mr. Kaldor wanted to clarify his own position. One could introduce restraints into this kind of model in different ways. One way was to bring in a minimum profit margin, which was what Kalecki did. The other was to introduce, via uncertainty and liquidity preference, a minimum below which the rate of profit on investment could not fall. The behaviour of the model differed according to the restraint used. If one introduced the former, one could not have a model with a steady rate of growth, since equilibrium involved the under-utilization of resources and less-than-full employment. One could not have a moving growth-equilibrium if output was elastic, since the accelerator would begin to operate. His own model was similar to those of Professors Champernowne and Solow in that respect. Again, if there was a limit to competition, with a level below which prices would not fall, it was necessary that this limit should not have been reached. Prices must be above the minimum, and respond to fluctuations of demand. Mr. Kaldor fully agreed with Professors Solow, Domar and Champernowne on the facts of history. The effects of monopoly on prices and on distribution had been tremendously exaggerated. Prices did come down with falling costs.

Professor Samuelson said that income-elasticity was unitary in Professor Champernowne's model. There was much evidence that this was not realistic, but the assumption did allow a causal analysis of why changes in product-mix might be neutral so far as the capital-labour ratio was concerned.

Dr. Barna wanted to clear up the dispute between Professor Champernowne and Professor Sylos Labini. Professor Champernowne was comparing mechanized output with hand-made goods; in the model, raw material production was not mechanized. Professor Champernowne's model was also a two-sector one, which did not include land, and there had been immense advances in the productivity of land in the past 100 years. So, the fact that raw material prices had not risen did not prove that the degree of monopoly was increasing.

Professor Solow said that he agreed with Mr. Kaldor on the rôle of monopoly. Even if one were constructing a general-equilibrium model with increasing returns to scale and monopoly added to the parameters, it was still not true that technology would drop out of the analysis. Monopolies were just as subject to technical constraints as competitive industries.

On the question of income-elasticity of demand, Professor Robinson was quite right. He had been surprised to discover in the Wicksell model that the real difficulty arose only when one considered more than one

consumption good. Then the whole notion of a golden age disappeared, because the process of growth and technical progress itself raised income per head and so changed the proportions in which commodities were consumed.

Mr. Kaldor wondered whether Professor Champernowne could work out a model where income-elasticities were the important things, price-elasticities only of minor importance and where the 'fixed basket' of goods varied with progress, because of income-elasticities.

Professor Champernowne did not find it possible to make such a generalization. What he could do would be to vary the assumption that each commodity had unitary price-elasticity of demand. This would reproduce some of the effects of income-elasticity, if mechanization permitted the mass-production of goods with a high elasticity of demand.

Professor Hicks thought one trouble might be that it might be possible (even probable) that one had mechanized some industries by mistake, so that some robots were unemployed, although the introduction of depreciation would alter this.

Dr. Barna suggested that the problems introduced by high income-elasticity of demand depended on the kind of commodity. There was one kind of answer for durable consumer goods, but one could proceed to take transport and laundry services instead of washing machines and cars. One could then construct the model on the assumption that, as techniques developed, transport was provided by more mechanized processes. He had in mind J. R. N. Stone's article in the *Economic Journal* [1] which showed a remarkable stability of consumer demand in rough categories over about 40 years; but with a high degree of commodity differentiation, one might have enormous shifts.

Professor Samuelson said that the Stone model was like Klein's. Once one had the basic necessities of life, any additional spending had unitary income-elasticity.

Professor Champernowne said he had already done a great deal to relax his assumptions. Machines need not be permanent, and one could allow for radioactive decay. Again, the number of workers per robot was not assumed constant. With income-elasticities, the results would be much the same with non-unitary elasticity of demand. The difficulty over income-elasticity was a technical one. He had been interested in seeing whether one could use a simpler form of production function, and once one introduced income-elasticities, the function ceased to be homogeneous of the first degree. He had not tried to use a more complicated production function, but he thought there would be great difficulties, and that the production function would not be fixed, but would vary according to assumptions about elasticities of supply, and so on.

Professor Hicks felt that one of the beauties of the model in its original form was that these peculiar assumptions had at least the virtue of ensuring that if a particular robot was profitable when it was first used, it

[1] 'Linear expenditure systems and demand analysis', *Economic Journal*, 1954, p. 511.

went on being profitable. If one dropped these assumptions, that ceased to be true ; one had to introduce expectations, and this would change the whole character of the model.

Professor Champernowne agreed. He pointed out that in his model technical progress meant that the number of operatives per robot went down — that was the easiest way of introducing progress. Another possibility would be to study the case where the relative value of robots was unaffected by technical progress.

Mr. Kaldor suggested that if technical progress made robots cheaper, then primary production would become less profitable and might ultimately cease.

Professor Champernowne asked to be allowed to answer Mr. Kaldor's last question in writing later. He subsequently submitted the following note :

'Two types of technical progress were distinguished in section III, sub-section 3 :

(i) labour-saving progress, by which λ, the number of men per robot, is steadily lowered ;
(ii) capital-saving progress by which the $f(x)$ (the robot cost functions) are steadily reduced.

In the case of progress of either type, a breakdown of the model will be caused if this progress drives outside the margin of mechanization any good previously on or inside it. Apart from this case, no particular difficulties are introduced by the labour-saving type, even if λ varies for different goods and falls at different rates for different goods. Nor does it make any difference what people's expectations may be about future technical progress *of this labour-saving kind.*

The latter type, capital-saving progress, will not cause difficulties unless different proportional rates of fall in cost are *expected* for different kinds of robots. Provided the expected (proportional) rate of fall in cost is the same, ρ say, for all kinds of robots, the effect of this expectation will merely be to drive down the money rate of interest r by an amount ρ, so that $r+\rho$ must be substituted for r in all the equations. Provided the *expected* rate of fall is thus always the same for all the cost functions $f(x)$, it will not matter if the *actual* rate of fall is at each moment different for different $f(x)$. At each moment a production function $\phi(M, N)$ and a price index p can be calculated from the levels reached at that moment by the $f(x)$, and this production function $\phi(M, N)$ will satisfy the marginal equations

$$p\frac{\partial\phi}{\partial N}=1 \quad p\frac{\partial\phi}{\partial M}=r+\rho \quad M\frac{\partial\phi}{\partial M}+N\frac{\partial\phi}{\partial N}\equiv\phi(M, N).$$

In the difficult case where *different* proportionate rates of fall $\rho(x)$ are expected for the different cost functions $f(x)$, it will still be possible to

work out a production function $\phi(M, N)$ appropriate to the level of techniques at any given moment, and this will still satisfy the equation

$$M\frac{\partial\phi}{\partial M}+N\frac{\partial\phi}{\partial N}\equiv\phi(M, N).$$

But, in general, it will *not* satisfy $p\frac{\partial\phi}{\partial N}\equiv 1$ on account of a divergence between $\rho(X)$ the expected proportionate fall in robot-cost at the margin of mechanization and the appropriate weighted average of the $\rho(x)$ for goods inside this margin. Thus wages will no longer in this case equal the value of the marginal product of labour, in this sense of $p\frac{\partial\phi}{\partial N}$.

THE DISCUSSION OF PROFESSOR SOLOW'S PAPER

Professor Solow said he would try not to cover the whole paper, because he wanted to leave room for Professor Champernowne, who had worked out a way of dealing with the model which was in many ways clearer than his own. In most respects his model followed Wicksell closely. It went one step beyond the Ramsey-Samuelson model by doubling the numbers of commodities (from one to two), and tried to keep a firm grasp on all technological factors. The price paid was that the model applied to the real world, if at all, only by analogy. There was no guarantee that there were any index numbers of consumption, capital and labour which behaved in precisely this way. In principle, one could enlarge the number of commodities, but then one could no longer see at a glance what was going on.

The model assumed that there was one consumption good and one type of machine. Consumption goods were produced under constant returns to scale by labour and machines. Professor Solow said he had taken a Cobb-Douglas function, although for many purposes this restriction was not essential. Machines were also produced by labour and machines. There was perfect competition and perfect foresight about the future. The latter assumption limited him to the special situation where the system was either stationary or else growing steadily, so that the future would be like the past.

Professor Solow said that in his model, machines could be built to have different durability, but, of course, it cost more to build a more durable machine, and entrepreneurs chose the durability of machines 'rationally'. It did not matter which of several criteria the model used, since he was dealing with free entry and long-run competitive equilibrium, so that the various criteria came to the same thing.

Professor Solow mentioned that, before he left, M. Malinvaud had told him that he had worked out some of the consequences of a different assumption about durability; namely, that when it was produced, a

machine was always the same machine. But in use (either in producing machines or consumption goods) a machine could be used up quickly or slowly, depending on how intensively it was used. In other words, with a given input of labour, one got the same output, say, with ten machines wearing out in ten years, or with eight machines if one was willing to see them wear out in, say, five years. Naturally this different assumption yielded different results, but M. Malinvaud had shown that the nature of the situation did not change.

Professor Solow explained that in his paper he had used Wicksell's assumption that machines were one-hoss shays, but he had since discovered that one got simpler results if one assumed radioactive depreciation, and he would present that form of the theory here. He had tried using the straight-line assumption of Dr. Barna, and had convinced himself that he could carry out the calculation, but that it would be the most complicated of the three.

Wicksell covered only the stationary case. If the supply of labour was fixed, and also the value of a balanced stock of machines in terms of consumption goods, what configuration of prices and interest rates would result ? And if one changed the total value of the stock of machines, what changes resulted in the other variables ? He showed, among other things, that relative shares would not change. (This result did, however, depend on the Cobb-Douglas assumption and so was not very surprising, though not obvious.)

In the radioactive case, the share of capital was

$$\frac{1-\gamma}{1+\frac{\alpha\beta}{1-\beta}},$$

where γ was the elasticity of the output of consumption goods with respect to labour input,

α was the analagous elasticity for labour expended on machines, and

β measured the difficulty of extending the durability of machines.

The share of capital depended only on γ, α, β, in other words, on technology.

One could go beyond Wicksell and imagine a 'golden age' in which relative prices, real wages and the rental of machines were constant, and R, L, and Q_c grew exponentially at rate g. The age distribution of machines was skewed towards the young end, and the output of machines must cover replacement and net investment. The only thing that happened was that the whole system expanded at rate g. Then the share of capital became

$$\frac{1-\gamma}{1+\frac{\alpha\beta}{1-\beta}+(\alpha-\gamma)\frac{g}{\rho}},$$

which depended only on α, β, γ and $\frac{g}{\rho}$.

One must also remember that, with a general production function, α, β, γ depended on g. The use of g/ρ as a parameter was an improvement suggested by Professor Champernowne. We should here note that if g could increase, leaving ρ the same, the effect on distributive shares would depend on the relative sizes of α and γ, which in fact corresponded to his neo-classical intuition. But g and ρ were not free to vary independently. There was a relation (where D was the share of property in income)

$$\sigma_\rho D + \sigma_w(1 - D) = \frac{gD}{\rho} = \frac{\text{Net S}}{\text{Net Y}}.$$

It then followed that

$$D = \frac{(1-\gamma) - (\alpha - \gamma)\sigma_w}{1 + \frac{\alpha\beta}{1-\beta} + (\alpha - \gamma)(\sigma_\rho - \sigma_w)}.$$

This introduced Mr. Kaldor's savings propensity, but also $(\alpha - \gamma)$.

If σ_ρ and σ_w were econometric constants, then relative shares depended only on $\alpha, \beta, \gamma, \sigma_\rho, \sigma_w$. A change in g would not affect them, because ρ must move in the same proportion. This was most easily seen if σ_w was equal to zero, when g/ρ was equal to σ_ρ. But if, as seemed reasonable to him, σ_ρ, the average propensity to save out of profits, was itself sensitive to ρ, the position became rather different. The same equation held for D, but if g now increased exogenously, ρ and σ_ρ would increase, and what happened to distributive shares depended on $\alpha - \gamma$. If α was equal to γ and σ_w was unaffected, there would be no change.

Professor Solow explained that he had also tried to work technical change into the model. So far he had considered only the simplest possible case, where there was uniform or neutral technical change in the consumption good industries at rate λ. This rate was expected, and affected old capital as well as new. He realized that this was not really satisfactory. For one thing, in most calculations, all it did was to replace the rate of interest ρ by $\rho - \lambda$ — it was the opposite of a risk discount. This had the paradoxical result that a more rapid rate of technical change increased the optimum durability of machines. This was because old machines gained efficiency rather than obsolesced. His own view was that it would be better to take the case where technical progress affected only new machines and left old ones as fossils of earlier technologies. He thought one could handle this. It would require a lot of integration, but he hoped to produce the results sooner or later. Professor Solow said he had also considered the case where λ was a function of g, much as in Mr. Kaldor's model, and made some calculations, but this assumption seemed too mechanical. With this special kind of technical progress, one got

$$D = \frac{1-\gamma}{1 + \frac{\alpha\beta}{1-\beta} + \frac{(\alpha - \gamma)g}{\rho - \lambda}}.$$

But with $\sigma_w = 0$, one had $g + \lambda = \rho\lambda_\rho$, and when λ increased one must calculate whether $\frac{g}{\rho - \lambda}$ increased. That depended on how g responded to λ, and one still had to take account of $\alpha - \gamma$.

Professor Champernowne opened the discussion with a detailed comment, which is reproduced here in full.

The Solow model allows only one type of machine, but ingeniously makes the rate of output of this machine variable according to the durability which it is to have. He discusses the special case where the production functions for both consumption goods and machines are of the Douglas type and where labour and machines are the only two factors of production. However, he points out that his model can easily be generalized to the case where there are several factors of production and where the two production functions are quite general apart from the restriction that each must be homogeneous of degree one in the quantities of factors employed.

It may be useful to indicate the lines on which his results may be generalized to this wider case. We will first consider the comparison of stationary states.

Let the functions giving the flows of output of consumption goods and machines respectively be

$$Q_C = f(L_C, R_C) \qquad Q_R = F(L_R, R_R, N),$$

where R_C and R_R are the quantities of machines employed in the two sectors and where N is the life of the machines being produced and where L_C and L_R denote any other factors of production : we assume both the functions f and F to be homogeneous of degree one in the L's and R's. Let f_R, F_R and F_N denote the elasticities of these functions with respect to the independent variables R_C, R_R and N.

Consider a stationary state with the instantaneous rate of interest ρ and with gross rental r per machine : then the value of a new machine is evidently

$$\frac{r}{\rho}(1 - e^{-N\rho}) = rNh(N\rho), \tag{2.1}$$

where

$$h(x) \equiv (1 - e^{-x})/x. \tag{2.2}$$

Since a small proportional increase ϵ in N causes a proportional decrease of $-F_N\epsilon$ in Q_R, we must have under perfect competition

$$-F_N\epsilon Q_R rNh(N\rho) = e^{-N\rho}\epsilon Q_R rN$$

$$\therefore \quad -\frac{1}{F_N} = {}^{N}e^{\rho}h(N\rho) = h(-N\rho). \tag{2.3}$$

This is the same as Solow's equation No. 10, but we have obtained it directly from the definition of F_N and the assumption of perfect competition.

In this stationary state, the number of machines is

$$N.\ Q_R \tag{2.4}$$

and the gross income of capitalists is accordingly

$$r.\ N.\ Q_R \tag{2.5}$$

whereas the gross income of capitalists in the machine industry must be

$$\overbrace{F_R\, rNh(N\rho)}^{\text{price of machine}}\, Q_R \tag{2.6}$$

because of the well-known property of an elasticity such as F_R that it must, under perfect competition, give the proportion of income going to the factor to which it refers. Subtracting equation 6 from equation 5 we see that the gross income of capitalists in the consumption good industry must be

$$\{1 - F_R h(N\rho)\} rNQ_R, \tag{2.7}$$

whence, by the analogous property of the elasticity f_R, the total value of consumption goods output must be

$$\{1 - F_R h(N\rho)\} \frac{rNQ_R}{f_R}. \quad \text{(This is net income.)} \tag{2.8}$$

The total value of machinery is given by

$$Q_R \int_0^N rTh(T\rho)dT = \{N\rho - N\rho h(N\rho)\} \frac{rQ_R}{\rho^2} = \frac{NrQ_R}{\rho}\{1 - h(N\rho)\}, \tag{2.9}$$

whence the net income of capitalists must be

$$rNQ_R\{1 - h(N\rho)\}, \tag{2.10}$$

so that, by comparison with equation 2.8, we see that their proportionate share is

$$\theta = \frac{f_R\{1 - h(N\rho)\}}{1 - F_R . h(N\rho)}. \tag{2.11}$$

This corresponds to Solow's equation 15.

The same reasoning may readily be extended from a stationary state to a golden age with constant growth rate g.

The number of machines is now

$$R = h(Ng)NQ_R \tag{3.1}$$

and the gross income of capitalists is

$$h(Ng)rNQ_R \tag{3.2}$$

and in the machine industry it is, as before,

$$F_R h(N\rho)rNQ_R \tag{3.3}$$

so that in the consumption goods industry, by subtraction, it is now

$$\{h(Ng) - F_R h(N\rho)\} rNQ_R \tag{3.4}$$

and the output of consumption goods has value

$$\{h(Ng) - F_R h(N\rho)\} \frac{rNQ_R}{f_R}. \tag{3.5}$$

The value of the stock of machines is now

$$Q_R \int_0^N e^{(T-N)g} rTh(T\rho)dT = \frac{rNQ_R}{\rho - g}\{h(Ng) - h(N\rho)\}. \tag{3.6}$$

The net income of capitalists is now

$$\frac{rNQ_R P}{\rho - g}\{h(Ng) - h(N\rho)\} \tag{3.7}$$

and the net national income is now

$$\{h(Ng) - F_R h(N\rho)\}\frac{rNQ_R}{f_R} + \frac{rNQ_R g}{\rho - g}\{h(Ng) - h(N\rho)\} \tag{3.8}$$

so that the proportion of net income going to capitalists is now

$$\theta = \frac{\rho f_R\{h(Ng) - h(N\rho)\}}{gf_R\{h(Ng) - h(N\rho)\} + (\rho - g)\{h(Ng) - F_R h(N\rho)\}}. \tag{3.9}$$

This equation is the same as Solow's equation 19: it shows that the proportion of net income going to capitalists depends only on the four magnitudes $\frac{g}{\rho}$, f_R, F_R and F_N since (2.3) established that $N\rho$ depends only on F_N.

This enables us to obtain fairly powerful results about the effects of the savings propensities of wage-earners and capitalists upon the distribution of income in this model. For suppose that the proportions saved out of profits and out of all other income are σ_P and σ_W, then

$$\theta\sigma_P + (1-\theta)\sigma_W = \frac{\text{Net Savings}}{\text{Net Income}} = \frac{g\theta}{\rho} \text{ from (3.7) and (3.8)}$$

$$\frac{g}{\rho} = \sigma_P - \sigma_W + \frac{\sigma_W}{\theta}.$$

$$\theta = \sigma_W \Big/ \left(\frac{g}{\rho} - \sigma_\rho + \sigma_W\right) \tag{3.10}$$

Comparing equations (3.9) and (3.10), we see that the ratio can be obtained as a function of $\sigma_P \sigma_W F_R F_N$ and f_R.

This means that the distribution of income is completely determined in the model once the two savings propensities σ_P σ_W are given and the

three technical elasticities f_R F_R and F_N. The questions whether there is much capital per head and a low rate of interest or little capital per head and a high rate of interest and whether population expansion is slow or fast are completely irrelevant to the distribution of income, or at any rate only relevant in so far as they may affect the technical elasticities or the two savings propensities: I am not clear whether this result is altogether consistent with the conclusions which Professor Solow suggests on pages 255 and following.

We may now introduce neutral technical progress of the kind discussed in the paper, such that the gross output of consumable goods obtainable from a given stock of labour and machines is expanding with the growth rate λ. The effect of this on the distribution of income turns out to depend merely on the ratio of λ to the rate of interest ρ. Let this ratio be denoted by $\bar{\lambda}$, then the only effect of this technical progress on the stationary state, if the three technical elasticities and the two savings propensities remain unaltered, is to increase the capitalists' income in the ratio $1 : 1 - \bar{\lambda}$, but to leave wage-earners' income unaltered. All the real constituents of the situation remain unaltered: the only changes are monetary ones.

To see this, one can imagine the situation where the consumption goods consist solely of lumps of sugar: we may suppose that conditions of production remain completely unaltered by the introduction of the technical progress with the one exception that the lumps of sugar produced become more numerous subject to the growth rate of λ, but at the same time each lump of sugar becomes lighter in exactly the same proportion, so that in reality everybody gets just the same amount of sugar as before, although being unobservant, they suppose that they are getting more and more according to the growth rate λ. The producers, being wise to all this, will obviously adjust their production also exactly as before, although if they are measuring in units of lumps of sugar they will reckon their machinery to be becoming worth more and more (lumps of sugar), or at any rate to be depreciating more slowly: the rate of profit, being reckoned with an eye to lumps of sugar as the units of value, will accordingly be swollen by an item allowing for this growth rate of λ in the value of the machine stock: the effect is to increase the capitalists' income as compared with the wage-earners' income in the ratio $\rho : \rho - \lambda$ which is equal to the ratio $1/(1 - \bar{\lambda})$.

The corresponding effect on the ratio θ of capitalists' net income to total net income is to increase it in the ratio $1 : 1 - \bar{\lambda}(1 - \theta)$.

It therefore transpires that this ratio θ measuring the distribution of income is uniquely determined by the six magnitudes f_R, F_R, F_N, σ_P, σ_W and $\bar{\lambda}$, the three technical elasticities f_R, F_R, F_N, the two savings ratios and $\bar{\lambda}$, which gives the ratio of the rate of technical progress to the rate of interest. Once these are known, no arguments about whether the rate of interest is high or low, or whether the amount of capital per head is high or low, can affect the distribution of income any more. This is always supposing that we are comparing two golden ages which have in fact been attained. Here, again, I am doubtful whether this conclusion entirely

squares with Professor Solow's own conclusions on pages 258 and following, but further discussion on this may be fruitful.

It is tempting to conclude that in the conditions of the generalized model such factors as the rate of population growth and the amount of capital per head have no influence on the golden-age distribution of income. This would be true if

(1) The savings propensities σ_P and σ_W were fixed.
(2) $\bar{\lambda}$, the ratio of the rate of technical progress to the rate of profit, were given.
(3) The production functions were Cobb-Douglas so that the elasticities f_R, F_R and F_N were impervious to the above factors.

The first of these three conditions may be granted without serious sacrifice of realism.

The second is more doubtful : although one might expect a high rate of profit to stimulate the rate of technical progress, one would not perhaps expect the ratio $\bar{\lambda} = \lambda/\rho$ to be so high when ρ was high as when it was low. If we suppose $\bar{\lambda}$ to be a decreasing function of ρ, then given the technical coefficients and the savings propensities, the distribution of income will be the *more* favourable to capitalists the *less* is the rate of profit. The explanation of this apparently paradoxical result is as follows. In the two golden ages compared, the ratio of capitalists' gross income to total gross income will be the same : the difference in the distribution of income will arise only from the fact that the ratio of depreciation to gross capitalists' income will be less in the golden age with the lower rate of profit ρ. The fact that it will there be lower and not higher is due to the fact that the life N of capital will there be so much *higher* as to offset the lowness of ρ and to make $N\rho$ actually higher than in the golden age with the higher ρ. That this must be so follows from the facts that $N(\rho - \lambda) = N\rho(1 - \bar{\lambda})$ must be the same in one golden age as in the other and that we assume $\bar{\lambda}$ to be higher in the golden age with the lower profit rate ρ.

The third assumption which concerns the technical coefficients f_R, F_R and F_N is the most misleading of all. In actual fact there are probably in any given state of technical knowledge some particular amount of capital per man and length of life of capital which are 'appropriate' so that the elasticities f_R and F_R are high so long as the amounts of capital per man in the two sectors are below the appropriate levels, but become low so soon as these amounts exceed the appropriate levels ; similarly there is likely to be some natural durability for the machines so that the elasticity F_N is low if the machines have a lower-than-natural durability, but high if they are excessively durable.

Directly we admit these features into our model the tail ceases to wag the dog. We now find that given the rate of population increase, the rate of technical progress and the savings propensities ; the golden age must be such as to allow roughly the appropriate amounts of capital per head and the natural durability of capital : the growth rate of capital (which must equal that of population) and the amounts of capital per head tell us

how much savings must be both per head and as a proportion of income : the distribution of income must then be such as to allow this proportion of income to be saved. Finally the elasticities F_K, F_N and f_R will depend on the resultant values of ρN and of the numbers of men and machines employed in the two sectors.

One further remark may be made about the discussion of the durability, N. The length of use of machines in the real world is likely to be limited as often by obsolescence as by physical wearing out. But the kind of technical progress allowed for in this model does not introduce any obsolescence of machines : the durability chosen for machines is thus likely to be artificially high under the conditions of the model since there is no risk of the durability being wasted by obsolescence.

Professor Hicks wanted to know whether, in Professor Solow's paper, the introduction of the savings proportion in the determination of the relative shares of factors was connected with Mr. Kaldor's theory. He himself did not think it was. In Mr. Kaldor's theory the relation between saving and the distribution of income was a result of savers' behaviour — of their propensity to save. In Professor Solow's paper it was a property of the production function, and therefore on the opposite side of the system. If he might be allowed to enlarge on this in a peculiar way, Professor Hicks said he had recently been thinking about this problem on related lines, and had proceeded rather differently. He did not want to give a new model, but some ideas from his own model were relevant to the one under discussion.

The way the behaviour of savings had been put in terms of savings by different classes was not important ; it was, in any case, difficult to know what to do with savings out of wages. Savings from wages were small, and there was the question of what happened, at later stages of growth, to wage-earners who had saved. Did they receive profits ? If one could assume that all savings were made out of profits, some of these relations could be put simply. In the so-called golden age, all elements would be expanding at the same rate, with perfect foresight, and saving equal to investment. Consequently, the income of capitalists (P) must be equal to the rate of interest, multiplied by the stock of capital, i.e. rK. Saving would be αrK. If the growth of capital were GK, it would follow directly that $G = \alpha r$. The result, in his model, and perhaps in Professor Solow's, was that, when comparing two states of steady growth, if saving were higher in A than in B, then, in A, G would be higher and r lower, assuming given general all-round productivity. This was much more general than any conclusion from a Cobb-Douglas function.

Professor Solow wondered if this relationship was the same for Messrs. Kaldor, Champernowne and Hicks. What one did with an exogenous change in one variable depended on the rest of the apparatus, but in all cases the nature of the relationship between the rate of growth and the share of profits had a strong family resemblance.

Professor Hicks explained that what he had just said held generally, and much more strongly, than any proposition on distributive shares.

All came from a straightforward consideration of the investment choices of entrepreneurs. No attention was necessary to wages, so that anything could happen to the share of wages without upsetting the model. Provided K was increasing steadily, the share of profits had to obey some rules.

Professor Champernowne enquired whether Professor Hicks had said that the share of profits was determined by the model and the share of wages could be whatever it wished.

Professor Hicks explained that he was talking about absolute shares.

Mr. Kaldor thought that what had been said by the previous speakers showed that the gap between them was smaller than he had imagined. In the light of what Professor Champernowne had said, these differences were a matter of what we regarded as the solid and the pliable elements. Professor Champernowne had a growth equation like $g = \lambda + p$ and a profit equation like $p = \frac{g}{sp}$. The difference was that Professor Champernowne did not regard p as technically determined by the elasticities α, β and γ. Instead he regarded these as pliable, as taking on values consistent with the equations. If Professor Solow would agree that β lost importance when obsolescence was taken into account, and that technical progress (λ) was the quantitatively more important element in g, then there was no real quarrel. He would himself introduce one further relation to close the system, but taking care that it was pliable enough to allow steady growth. This would be an investment behaviour equation which would say that the rate of profit could not be lower than what entrepreneurs would regard as necessary to compensate them for the risks involved. This would be equivalent to the rate of interest element, and would bring in a liquidity preference equation or, if profit differed from interest, the bond rate would be an element. There was no *unique* relationship. If there were, this would be inconsistent with the rate of profit being an independent variable. One might allow the minimum rate of profit to vary with the capital-output ratio, and with the turnover period.

So he would still suggest that if one got away from the Cobb-Douglas theorem and constant technical elasticities, then α, β and γ could drop out. Professor Solow was concerned only with 'golden age' steady growth. He himself was trying to make a model of how the system could *attain* steady growth, and how changing investment would call forth the required changes in savings.

Professor Samuelson thought that Mr. Kaldor should not be too happy with this system. He did not like the Cobb-Douglas function, but would prefer to consider α, β and γ as functions of the unknowns of the problem. That would not make the system at all to Mr. Kaldor's liking — but a model of truisms based on definitions. The exposition would use some of Mr. Kaldor's arithmetic, but the laws of causation might run very differently. It was all very well to talk of golden ages. In practice the production function was not necessarily of the Cobb-Douglas type, but depended on inputs as specified by Professor Solow and himself. One could imagine a process where the supply of labour was constant and g

was zero. Yet accumulation could go on. Here, with a gradual lowering of the rate of interest, the share of profits would be determined by mutually-interdependent pricing, and ought not to satisfy Mr. Kaldor.

Mr. Kaldor said that growth must come to an end when the rate of profit fell below the minimum at which capitalists were willing to invest.

Professor Samuelson suggested that Mr. Kaldor ought to concentrate on uncertainty.

Mr. Kaldor said we were back at playing games. He was not interested in logical reasons, but in setting up a model. He admitted that it was only because the world contained uncertainty that models had to allow for change.

Professor Solow did not think anyone would hold the idea that he believed in the Cobb-Douglas function. It would be very strange if the physics of the universe related outputs and inputs in this way. He was prepared to take production functions in whatever form nature presented them. If elasticities were not constant, as was virtually certain, then when any of the parameters of the neo-classical system changed, so would his α, β and γ. He had used the Cobb-Douglas function to lay out his model with a minimum of boring detail; to complicate it would have required more algebra. Although in some ways Mr. Kaldor's model and his own coincided, there was a hard core of difference between them. Earlier, with radioactive depreciation, it had turned out that the way in which things worked out when rates of growth changed — whatever the savings-investment process — was that the effects on distributive shares depended on the nature of the technological background. He did not think the constant elasticities were given by nature, and he was worried that Professor Champernowne had said that there were natural input-output ratios. He would be hard put to it to say what natural ratios he expected in terms of the technical nature of the model.

Professor Samuelson said this was what he was thinking. If Professor Champernowne had sharp corners in his functions, one would have indeterminacy. We should then have to go to Mr. Kaldor and ask him to provide a law to sort it all out.

Mr. Kaldor made two points. First, Professor Solow should embody in his model some mechanism to show the dependence of the rate of investment on the rate of interest. Second, in an economy like that of the USA, the rate of profit on capital, and the capital-output ratio were both remarkably steady. This was compatible with two possibilities. First, technical factors α, β and γ might be very important, with technical progress strictly neutral. So the need to keep the capital-output ratio stable meant keeping the rate of interest where it was. The other interpretation was the opposite — technical elasticities might be very low so that a large change in the rate of profit led to a small change in the capital-output ratio, for reasons not depending on α, β or γ. Mr. Kaldor wanted Professors Solow and Samuelson to work out a model with classical conditions, with a falling rate of profit and a rising capital-output ratio. Having already reached the state where the rate of profit was at the

minimum, only investment which did not reduce the rate of profit further would be possible. Considered like this, the steadiness of the rate of profit was not grounded on technical elasticities. To explain constant capital-output ratios on technical grounds, one needed very restrictive assumptions.

Professor Solow agreed strongly with Mr. Kaldor's first point; there was no discussion of investment behaviour in the model. It was built like this in order to take the simplest case of certainty and smooth development first. He had not yet built a model of investment behaviour under uncertainty. Mr. Kaldor was also right in saying that all he was concerned with was a comparison between golden ages, not divergences from them, and that to deal with the real world one needed a theory of uncertainty. This raised many of the difficulties which were often blamed on the theory of capital.

Professor Solow was less convinced of the stability of the capital-output ratio in the USA. However, if the capital-output ratio *was* stable, he thought he agreed with Mr. Kaldor that technical change was not something that happened wholly by chance. Inventions that were adopted were selected on economic grounds, but he did not pretend to know what the selection mechanism was. We should not cast aside the possibility of the existence of offsetting forces, whether exogenous or pure chance.

Professor Fellner thought that, so far as the constancy of the rate of profit was concerned, the empirical evidence suggested that the best assumption about the profit rate in the USA was that from about 1870 to some date in the twentieth century, there was a rise in capital-output ratio, and a significant one, and that there was no rise in the share of capital in total income. The data were not entirely satisfactory, but it was reasonable to say that the evidence pointed this way, and it meant that the rate of profit was falling at that time. Similar reasoning pointed to some rise in the profit rate more recently. Over the whole 80 years, there was not much decline in profit rate, but this certainly did not point to the conclusion that the profit rate was fixed.

Professor Fellner suggested that this period, from 1870 to somewhere in the twentieth century, was associated with shifts in the composition of output, which of themselves would have favoured the share of capital. These shifts seemed incompatible with the assumption of a unitary elasticity of substitution and neutral inventions. This, plus more recent experience, suggested to him that if inventions had been neutral, the elasticity of substitution was less than one. Alternatively, inventions might not have been neutral.

Professor Solow said he had not been a close enough empirical student. He thought that a propensity to save out of profits reflected the presence or absence of profitable investment opportunities, whether the firms concerned were growing or not.

Professor Domar thought the paper reached some useful conclusions. It established the interesting, and even striking, fact, that in the world of a Cobb-Douglas production function, with constant factor shares, a

constant propensity to save did not affect the long-run rate of growth of output. The latter depended only on technological progress and the growth of population. In the short run, the propensity to save was, of course, important. The small rôle it played in the long run did not prevent economists from developed countries from pressing upon their colleagues in less-developed countries the standard question about the fraction of output which the less-developed countries invested. This need not, however, be a silly question for several reasons : (1) short-run considerations were important ; (2) the real world need not be of the Cobb-Douglas type ; and (3) more specifically, technological change itself might depend on the fraction of income invested, since new capital might be a major vehicle of technical progress.

Professor Samuelson asked whether, if one had two Cobb-Douglas functions, with the same labour and capital, but in one investment equalled 100 per cent of income and in the other only 50 per cent, the systems would grow at the same rate.

Professor Domar replied that, in the long run, the rates of growth would be the same.

Professor Champernowne said that the answer was correct, but one system would have more capital and be richer.

Professor Solow pointed out that the heavily investing economy would have an income which was a constant multiple of income in the other.

Mr. Kaldor held that the proposition was true irrespective of the Cobb-Douglas function, so long as one had diminishing returns to capital. His own model implied that advanced countries had reached the point where an addition to the rate of savings, when invested, did not have much effect on rates of growth.

Professor Domar said this could be generalized, and did not require some of the specific Cobb-Douglas assumptions. So long as technological progress was ruled out when investment took place, capital accumulation was not important to growth in the long run.

Mr. Little wondered how it was that the exogenous growth rate had dropped out of the equation giving the distribution of income. Why should not higher profits go with more rapid growth ?

Professor Champernowne said that the things which were given in the model were all elasticities, and the time unit cancelled out. So one had a whole family of golden ages consistent with these variables. The rate of growth did involve time units, and the assumptions of Professor Solow's model had been so arranged as to cancel time out.

Professor Lindahl commented on the Wicksellian model on page 249. If population were constant, then each increase in capital meant an increase in durability. This was not in Wicksell's model.

Professor Solow held that Wicksell had said the same thing. The main difference between him and Wicksell was that Wicksell had axes made only by labour, and had dealt only with stationary conditions. He had never seen a copy of Akerman, and only knew of it from the Wicksell review. His theory did, however, contain an arbitrary assumption. Once

one supposed, with Wicksell, that consumer goods were produced on Cobb-Douglas lines, it followed that when capitalists chose to hold more assets with a bigger value in terms of consumer goods, then the distributive shares and the distribution of the labour force were invariant and also independent of this bigger holding.

Mr. Thalberg thought the idea of radioactive decay very unrealistic. The opposite idea, that of making decay inversely radioactive was much more realistic. In other words, depreciation per unit of time was initially very small but increased as time passed.

Professor Champernowne said he had originally introduced the idea that the three parameters α, β and γ might become variables. He was not suggesting that production functions would change, but only that the elasticities would be different in different regions of the functions. When he said there might be an appropriate amount of capital per head, and an appropriate durability, he was saying no more than that there was a steep production function, so that one had big variations in elasticities, corresponding to small changes in other variables. The correct analogy was that of Marshall's billiard balls, where the position of each was appropriate to the equilibrium of the others.

Mr. Kaldor pointed out that Professor Solow and he were aiming at different things. Professor Solow was trying to give a generalized set of equilibrium conditions for a steady rate of growth — comparative statics on a dynamic plane — and so excluded many things which he himself tried to include. Provided the output per head and capital per head were high enough, one could have profits above the minimum rate demanded, and wages above the minimum supply price. Mr. Kaldor explained that in his model he had merely followed Keynes' analysis, and his real interest had been to show how the system was tending towards a certain position and by what mechanism. All this was compatible with Professor Solow's model, but not with Professor Solow's purpose, in that he had to reduce the number of variables as far as possible, and so eliminated those variables which were the most pliable.

On a more technical point, if one assumed a minimum rate of profit to be demanded, and if one had attained the stage where this was being earned, then Professor Solow's system was over-determined, and did not necessarily give a steady rate of growth. The only solution, then, was to put in, not a single rate of minimum profit, but a functional relationship between the latter and the capital-output ratio.

THE DISCUSSION OF PROFESSOR MARCHAL'S PAPER

Professor Fellner thought the problem in this discussion was to reconcile Professor Marchal's observations with the conventional way of looking at the problem of income distribution, a task which was made more difficult

by the fact that notions of the conventional way of looking at income distribution were by no means identical. Professor Fellner believed that, for long-run analysis, marginal productivity theory gave a reasonable first approximation to the demand for the factors of production. Marginal productivity theory, which Professor Marchal neither accepted nor rejected, but whose inadequacy he stressed, was a theory of demand for factors of production. It was based on the idea of profit maximization, and on the idea that in the long run, the functions along which profit maximization was attempted could be treated as continuous. Professor Fellner was not sure which assumptions of marginal productivity theory were so violently rejected by some participants. Monopoly was usually taken care of by pointing to the difference between marginal physical and marginal value products. For analysing long-run trends, this difference became important only if the degree of monopoly changed. This theory of factor demand was neither accepted nor rejected by Professor Marchal, and his observations were concerned with something supplementary.

Professor Fellner thought it was possible to relax a good many of the assumptions of marginal productivity theory without arriving at very different conclusions. If the strict continuity assumptions of marginal productivity theory were omitted, and one moved towards activity analysis, the results obtained from the relaxed version of the theory were not essentially different. Some members of the Round Table strongly rejected this theory of factor demand, but he could not see what theory they wanted to substitute for it. Mr. Kaldor's exposition in his paper had been exceedingly illuminating, but on this one point Professor Fellner was as much in doubt as before. He could not see why, in the absence of some testable econometric model of factor demand, the assumptions of profit maximization and of long-run continuity of functions should be regarded as so poor — especially since one knew that omitting or relaxing them made so little difference.

If one looked at the matter in this way, then Professor Marchal's comments were mainly concerned with factor supply — a problem not covered by marginal productivity theory. He would not, however, say that Professor Marchal's observations related exclusively to factor supply. Monopsony problems, groups and group struggles had a legitimate place on the demand side, even if one accepted the general ideas of marginal productivity theory. But most economists would say that the monopsony problem on the demand side of factor markets was not of primary significance, even though it was quite important.

If one rejected the theory of marginal productivity on the demand side of factor markets, then presumably one concluded that Professor Marchal's considerations had great significance, not only on the supply side but also on the demand side. For the opponents of marginal productivity theory had created an empty space on the demand side which one had to fill with considerations like Professor Marchal's.

Professor Fellner thought that what Professor Marchal emphasized was that the conventional theories developed in the technical literature

were mainly concerned with *given* social-political conditions, and that we also needed an analysis which could explain what these conditions were and how they evolved and changed over time. Here, Professor Marchal put much emphasis on the fact that there had to be a two-way traffic between this complex of questions and the conventional problem of income distribution in economic theory. For the 'facts' with which 'usual' theory was concerned depended on these factors. However, the conclusions to which conventional theory led were important, because they had a significant effect on how owners of factors of production would try to change social and political relations in their society. Professor Marchal pointed out that owners of factors of production had very different ways of altering structural relations in society, and that some of these ways represented direct methods of acting on distributive processes by group action, including ways of influencing the political, administrative and legislative machinery of a country.

Again, Professor Fellner pointed out, Professor Marchal said that once we engaged in an analysis of how these socio-political relationships affected and were affected by distributive processes, the usual ways of distinguishing between different types of owners of factors of production proved quite insufficient. The courses of action open to such owners depended on the characteristics of these groups, and this was somehow missed in traditional analysis. Various groups, all of which supplied capital and labour of a very similar kind, differed greatly in location, in social and political structure, and therefore in the ways in which they could form groups and exert influences. In a sense, this expressed Professor Marchal's most essential conclusion, namely, that one was tempted to lump services together because economically they seemed similar, and yet these services were offered by groups with widely different social and political characteristics.

Mr. Kaldor noted that Professor Marchal's paper introduced various important considerations which were often ignored — the attitudes of different groups and the importance of group struggles — but had not much to say about how this fitted into the usual type of model. He himself would suggest that, in dealing with income distribution, we ought to make some distinctions according to the type of problem we were considering. First, we should distinguish between horizontal and vertical income distribution. By vertical income distribution, he meant how the social product was divided between different types of income, for instance, profits and wages. By horizontal distribution, he meant how income was divided between different sectors or industries. Mr. Kaldor's view was that group struggles had very little influence on the vertical distribution of income ; that the pressures of social groups had little to do with how much of the product went into wages or into profits. This was determined by the fundamental characteristics of the system. But he would agree that in the *horizontal* distribution between industries there was a tug-of-war for larger proportions of the national cake. So, the group struggle and countervailing pressures became crucial. The whole operation of the

wage-price spiral depended on workers in some industries getting ahead, so that countervailing pressures were very important.

Second, Mr. Kaldor considered the validity of the distinction between the primary and the subsidiary divisions. Labour was not homogeneous. There were many kinds of wage- and salary-earners, hundreds of different kinds of entrepreneurs, and many different types of property income. But he did feel that it was a valid way of proceeding to say that the primary division was between wages and profits, and that one then had the secondary problem of how the total wages bill and total profits were divided between the different types of labour and the various kinds of entrepreneur. Mr. Kaldor thought one could legitimately consider the primary problem as that of sharing-out income between wages and profits, in so far as changes in income distribution between the different types of income *within* each of these two broad groups had little, if any, effect on over-all distribution. If this was a valid proceeding, then the problems raised by Professor Marchal emerged at this second stage and not at the first stage.

Professor Fellner had reaffirmed his belief in marginal productivity as the long-run principle determining distributive shares. In other words, Professor Fellner thought that in the long run we all became bees. For in *his* 'long run', technical change had ceased, and we were in a thorough-going stationary state; hence in the long run, substitution between factors would assume primary importance. The experience of history showed that change was perpetual and not a temporary process, and he could not see how marginal productivity theory would apply in the long run if it did not apply in a world of change.

In Mr. Kaldor's view, one of the great drawbacks of marginal productivity theory was that it scored so badly in providing empirically testable hypotheses. Over long periods it was testable to a certain vague extent, and looking back over the last hundred years all the correlations seemed to go in exactly the opposite direction to what one would predict on the basis of that theory. This meant that changes in 'other factors' (excluded from the theory) were more important than the basic factors which were included. One could make the comment that if a curve was always shifting, any speculation about the shape of that curve was arid and unscientific. More than this, when it was said that so long as the degree of monopoly was constant, the marginal productivity theory held true, he would beg to differ at the 'low technical level'. This was not a matter of judgment. As Pigou showed in the *Economics of Welfare*, when there was monopoly, and some factors received less than the value of their marginal product, others must receive more. The 'degree of exploitation' or the 'degree of monopoly' became an element in determining factor shares. Indeed, on Kalecki's extreme assumptions, the whole difference between output per head and wages was explained by the degree of monopoly.

Mr. Kaldor argued that this way of posing the question pre-judged the whole issue. We did not need a theory of factor demands if we did not

believe that these determined distributive shares. He agreed that the interpretative value of marginal productivity theory depended on whether factor demands and their elasticities were the governing force in determining distributive shares; but he suggested that technical substitutability between factors of production was not a determining force over factor prices, partly because factor supplies might be elastic. For instance, the quantity of capital could not be taken as constant in the face of changes in the demand for capital goods. When factor supplies were elastic, relative factor demands might have no significant influence on factor prices.

Professor Jöhr thought Professor Marchal was right in wanting to introduce more sociological elements into distribution theory. Yet this made the construction of economic models much more difficult. Professor Marchal introduced the important distinction between action *on* the structure and action *within* the structure. The first had to be understood as a struggle between different groups to influence economic and fiscal policy so as to get a greater share of national income. Despite Mr. Kaldor, he himself thought that this kind of struggle played a great rôle. One would, of course, never get an unambiguous model which would permit exact prediction of the course of events. Nevertheless, by studying these forces we should gain increased insight into action *within* structures. Professor Jöhr thought this was covered by the theory of the market economy, though Professor Marchal wanted substantial modifications to conventional theory. This brought one back to what Professor Fellner had said, with which he fully agreed. We needed a theory of distribution to explain the remuneration of factors of production in terms of supply and demand.

On the demand side, one had to begin from marginal productivity; there was no other starting-point. Productivity was the important thing for the firm, though one could bring in qualifications later. Many participants had said they were not adherents of marginal productivity theory, but how would they replace it? If we adhered to the marginal productivity theory, we could not hold that 'profit is the share of capital', for this had been rejected by J. B. Say. Interest was the payment, not of capital, but of those who, by saving, had made the formation of capital possible. After one had deducted interest and rents earned by better techniques, what remained was only remuneration for the effort of entrepreneurs and for their uncertainty-bearing. If we thought of these different components of gross profit, it became clear that we had no right to incorporate the share of profits into our models as a causal factor.

Professor Drewnowski made one general remark. He thought the great merit of Professor Marchal's paper was that it had drawn attention to the sociological background of economic activity; this was essential if we were to develop realistic theory. But he thought that we should take yet another step and include State intervention in questions to do with capital. A theory of capital which ignored State intervention as an integral part of that theory was unrealistic and useless. Problems of

socialist economies lay outside the scope of the Round Table, but even in capitalist economies some reference to State intervention was essential. Professor Marchal had taken a first step, but it was only a first step.

Dr. Goldsmith said that on first reading this paper, he thought Professor Marchal was reopening the old question of power and economic law. But Professor Marchal disclaimed that, and explained that he was only interested in making modifications within the framework of economic theory. What was the main drift of Professor Marchal's suggestions ? Perhaps it was sufficient to distinguish two types of income, as Mr. Kaldor had tried to do by introducing his two levels of income determination. He was not sure whether this would be an acceptable interpretation to Professor Marchal, whose point was that a model which used only two forms of income was not accurate or realistic enough, though Mr. Kaldor seemed to think it was. What was the minimum number of separate kinds of income required for a realistic model ; that was the question we had to solve.

So far as he could see, Professor Marchal's categories did not take the pure forms of Mr. Kaldor's division. Professor Marchal was not concerned with the sector of origin, the form of income, or the third characteristic of the recipient, namely, his social position or position on the income scale. Professor Marchal's categories were mixed and mostly or largely sectoral ; but where did he include agricultural labourers ? How did Professor Marchal feel we should choose the optimum number or type of income recipient to use in models? International and intertemporal considerations also came in. As Professor Marchal said, his classifications were based on France, and some aspects did not apply in the USA, especially what he said on the family firm. His specific classification and the arguments for it were much more relevant to countries in a semi-developed state than to the countries we should regard as highly developed.

Dr. Todorović said that this was the only paper dealing specifically with the problem of distribution, and especially with the share of capital. Professor Marchal's paper was novel and constructive, but represented only a small part of the large and fruitful researches to which he had devoted so much time and energy. Although the paper was intended primarily for this Round Table, and was therefore mainly concerned with capital, it did deal with more general questions, and Dr. Todorović wanted to consider some methodological aspects of distribution.

There were today several clearly defined strands in distribution theory. One of these was what Professor Fellner had called 'the qualified marginal productivity theory', because it took account of big economic units — groups, the State, and so on. Another attempted to replace it by some kind of macro-economic theory. Among the macro-economic theories, he did not doubt that Mr. Kaldor's — which was both Keynesian and closely related to that of Professor Marchal — was the most realistic, and it dealt with several problems of distribution which interested him.

Dr. Todorović found Professor Marchal's approach to distribution theory macro-economic, dynamic and sociological ; it was objective and

not subjective. In particular, it studied changes in structure and their effects on income distribution. Most important of all, Professor Marchal made it clear that one could not analyse distribution thoroughly in terms of factors of production. One had to go beyond them to the individuals owning them and take account of their special characteristics and their environment.

Dr. Todorović thought this was the most important feature of Professor Marchal's paper ; that he found marginal productivity theory incomplete and so wanted to go beyond the pure observation of market events to find internal links of cause and effect in economic phenomena. In other words, marginal theory was superficial in the sense that *alone* it could not explain income distribution.

Dr. Todorović raised two queries. First, why did this struggle between social groups arise if each member of society owned the original source of his income ? Second, were there objective limits to the extent to which the incomes of the main social groups of workers and capitalists could be increased or decreased by the internal struggle between capitalists discussed in the paper ?

Dr. Todorović wondered whether these two questions were not of decisive importance for a general theory of distribution. Had not the Marquis of Mirabeau asked for the explanation of four problems of distribution when he wrote, 'it is first of all necessary to discover where the income arises, how it is distributed among the different classes of society, in which places it vanishes, and in which it is reproduced' ?

Professor Marchal had explained how income was distributed and where it went to ; what was still not explained was where the income arose and where it was reproduced. Professor Marchal had himself argued (p. 285) that constant criticism from as many sources as possible was necessary to elucidate so delicate a question. Doctor Todorović's own view was that one should not try to simplify so vast and delicate a problem, but should begin the discussion of the problem from another point of view — that of Marxian theory.

Professor Delivanis agreed fully with Professor Marchal's idea that we should rethink our models to take account of sociological factors. He felt that Professor Marchal's theory was applicable to both under-developed countries and to developed areas. Professor Marchal had considered post-war experience in some developed countries, pointing to abnormalities and drawing attention to new problems. On the distinction between agriculture, trade and industry, Professor Delivanis suggested that the differences between participants were not really so great. While it was true that land was fixed in amount, so was specialized industrial capital. With interest and rent, another factor was that capitalists who owned liquid assets could lose whole fortunes through inflation. House-owners would not.

M. Barrère said all agreed with Professor Marchal's main proposition, but many people were concerned over how to incorporate his views into traditional analysis. Was it possible to build a bridge between flow

analysis and group analysis as Professor Marchal understood it? The attempt had been made. Marx had distinguished between the capitalists and the proletariat, and had duly identified sociological distinctions with economic ones. Income, as a flow, linked these groups. The problems of finding dividing lines between groups could be solved by taking groups with similar economic calculation, whose members would therefore work in the same way.

Speaking about action *on* structures, M. Barrère said that those with similar interests would compete. There was pressure through, for example, fiscal policy, or pressure by trade unions on the public authority. So one had competition between small and big entrepreneurs, or between skilled and unskilled labour.

If one turned to action *within* structures, one could turn also to marginal productivity theory. Some entrepreneurs would be competing with some workers, since the similarity of their economic calculus would lead them to defend themselves. It might be fruitful to add to traditional theory. When Keynes said that money wages were defended, but not necessarily real wages, the group as a whole was concerned with defending the same thing. Similarly, monopolies determined the profit they wanted, and fixed employment and output on that basis. This idea that groups were determined by the similarity of members' economic calculus meant that groups were economically and not sociologically determined.

Professor Marchal thought the most important point was that of Dr. Goldsmith, who had seen that the link between his ideas and economic theory lay in his notion of categories. Yet theorists ignored this important concept of categories. In France, there were statistics which used categories, but not those of the theorists. However, one was not obliged to accept the definitions of the statistician. Dr. Goldsmith had seen that the categories for theories varied from country to country.

Another problem was the change in categories over time. About 1860, the most important category in France was the agricultural worker. Now, there were fewer agricultural workers, and one had to base oneself on industrial workers. Similarly, there were 'cadres' in French industry. The wages of such employees were not the same as for workers, and one could not give a realistic picture of what was happening in France without considering them.

Mr. Kaldor had used the words 'class' and 'group'. In France the word 'class' had a Marxian connotation, and Professor Marchal said he was now turning towards the word 'category' to avoid any unwanted implication. Mr. Kaldor had also spoken of a horizontal and a vertical division of income; Professor Marchal was inclined to be reticent here. If one analysed the French economy, the system was not two- but three-sided. One had wage-earners, including 'cadres', employers big and small, and representatives of agriculture, though one might find this situation only in a semi-developed economy. Professor Marchal wondered if Mr. Kaldor was quite sure that there was no mutual action or reaction between his horizontal and vertical processes of distribution.

Professor Marchal was grateful to Professor Fellner for asking the major question about the possible incorporation of his ideas into marginal productivity theory. He had no reason to reject marginal productivity theory, but it could not account for the whole phenomenon. Group pressures were not always easy things to analyse.

Professor Marchal was glad to hear Professor Jöhr protest against the idea of vertical and horizontal analysis as being too simple. M. Barrère had gone further than he had tried to go himself in the construction of a bridge, but his first reaction was that he was inclined to agree with the general trend of M. Barrère's ideas.