

A Conversation With Sir John Hicks About “Value and Capital”

E.E.J.: Sir John, your *Value and Capital* (1939) which most would rank along side of *The General Theory* in terms of the magnitude of its influence on the development of economics during the 20th century, will soon be fifty years old! There will, undoubtedly, be many observances during the anniversary year to assess its influence in ways which this conversation cannot hope to tackle. I should like to ask, therefore, that we focus instead on its gestation. Can you reflect for us on those important years that antedated *Value and Capital*?

Sir John: I think I may identify the turn of 1932–3 as the beginning of a whole complex of developments, professional as well as personal, that were to have a bearing on *Value and Capital*. The six years from that start, in 1932–3 to completion in 1938–9 fall, in my personal biography, into two nearly equal parts. There are three things which happened very near to the midpoint by which they are divided. The first was my move from LSE to Cambridge; the second was my marriage to Ursula; the third was the publication of Keynes's *General Theory*.

E.E.J.: What a triad of momentous happenings! Let's start with the first. Are we to understand that you consider *Value and Capital* as a “Cambridge book”?

Sir John: By no means! The actual writing of *Value and Capital* must have been largely done at Cambridge; though some of the static part must have been written in 1934 (year II of my six). Quite six months out of year IV, my first at Cambridge, were occupied in writing two papers on Keynes's theory, one of which is very well known. But, I would say that these papers had little to do with *Value and Capital*. There are indeed some references in it to the *General Theory*, but they come in as extras. The book could have been completed, with its main lines much the same, if I had never been asked to write those papers on Keynes, but had been able to put his book aside until I had finished my own job. *Value and Capital* is, in essence, an LSE book, not at all a Cambridge book. The ideas that went into it were fairly fully formed before I left LSE.

E.E.J.: So it is years I–III of your six that are particularly relevant and formative in leading up to *Value and Capital*.

Sir John: Indeed. But something must be said that relates to the previous year to serve as background. I was then teaching, in a very junior capacity, at LSE when Hayek gave his famous (or should I say infamous?) *Prices and Production* lectures as a visitor to LSE in February 1931. He came back the following September as Professor, a regular member of the department to which I was attached. But already by that February—or perhaps even earlier, in anticipation of his arrival—we were reading the Austrians, Böhm-Bawerk in particular, in German. Hayek subsequently introduced me to Wicksell's writings. There is quite a lot of Wicksell's Volume I in my *Theory of Wages* (1932).

E.E.J.: Where precisely can we identify the Wicksellian influence in that work?

Sir John: Its most Wicksellian part is the concluding chapters, in which I discuss the effect of “too high wages” in a closed economy—distinguishing, as it was useful to distinguish,

what I later called the income and the substitution effects. But since my "equilibrium" was the stationary state of Wicksell, my income effect, on profits and saving, was described, on Hayekian lines, as "capital consumption"; and this, as I remember being told by Dennis Robertson, was just wrong. "There is some excuse for Hayek" he told me "having lived in mouldering Vienna, but none for the rest of you, having lived in London and the Home Counties." (One should remember that in that dreadful year 1932 the south-east of England was the most prosperous place to the world.) I should have started with the "equilibrium" of a progressive economy, when the effect under discussion would have showed up as a retardation of growth.

E.E.J.: So Dennis Robertson's influence became relevant at that point?

Sir John: Yes. I think I may reckon that letter, which was sent to me while *Wages* was in the press, but before it was published, as the stimulus which sent me off in a new direction almost at once after *Wages* had come out. It was the direction which led to the "dynamic" parts of (III and IV) *Value and Capital*.

E.E.J.: Now, how does that tie in with what you had learned from Wicksell's writings? And with what Hayek was lecturing about at LSE?

Sir John: Hayek was mainly directing our attention to Wicksell's monetary theory which is to be found in Volume II of the *Lectures*. At that date Volume I had more of what I was searching for. Here Wicksell was linking Böhm-Bawerk with Walras, on whom I was already lecturing; so his version was easier than that of Böhm, or even of Hayek, for me to understand. I may remark, in passing, that I had a good deal to do with the English translation of that volume of Wicksell (1934). I had to correct a version in very imperfect English, taken directly from the Swedish, with nothing to help me except the German translation which I had been using, but which I have been told by better linguists than myself does not have a good repute. But it was possible to work out from these what the author was trying to say, and to put it in my own English words. The result, surprisingly, appears to have been quite acceptable.

E.E.J.: What sort of an influence did Hayek's theory—in particular about the Wicksellian monetary construction on which he thought he was building—have on your own thinking?

Sir John: I was free to start thinking seriously about Hayek's theory as soon as *Theory of Wages* was off my hands. Wicksell had just looked at the monetary consequences of disequilibrium (between the market rate and natural rate); Hayek thought there were *real* consequences also. But that made it more necessary for Hayek, than it had been for Wicksell to define what he meant by equilibrium. Hayek's disequilibrium was to be a *real* disequilibrium; a distortion of the "structure of production"; so it was incumbent on him to provide a criterion for *non-distortion*, non-distortion in real (non-monetary) terms. In his London lectures, with which I and my friends at LSE were wrestling, he had not offered any such criterion. A Wicksellian stationary state would be non-distorted, but for Hayek, as he himself accepted, that would not do. When I put my trouble to him, he showed me a paper he had written and published, in German, in 1929; here he had given a non-stationary definition—disequilibrium was disappointment of expectations. So an economy would be in equilibrium when what happened was what was expected to happen—"perfect foresight." Now a perfect foresight model is a possible economic model, having some sort of use in theoretical discussions; but it cannot

be claimed that it is a realistic model; no actual economy could ever be "in equilibrium" in that sense. It must always in that sense be in disequilibrium. So Hayek could only make use of this construction, as he did, by saying that he was going to concentrate attention on disequilibria that are due to monetary causes.

E.E.J.: What happens if one admits, as one surely ought to admit, that expectations are uncertain?

Sir John: I found that one had to introduce uncertainty before one could introduce money. That led on to a first consideration of the effect of uncertainty on the demand for money at a point of time—the "spectrum of assets." This was much better put, a year later, in my "Suggestion for Simplifying the Theory of Money," on which the greater part of my later work on monetary theory has been based.

E.E.J.: Since works published in German were accessible to you, you were surely acquainted with Gunnar Myrdal's work. What sort of influence did that have?

Sir John: The German edition of Myrdal's *Monetary Equilibrium*, which I read for review during the first part of 1934, was the beginning of my contact with contemporary Swedish scholars. This was immensely clarifying on the old Wicksell doctrine; as criticism of Wicksell it has hardly, even now, been superseded. But it was not exactly what, at that stage, I wanted. I was already clear (or was on the way to getting clear) about substitution between money and interest-bearing securities, the basis of a "point of time" or balance-sheet equilibrium, one element in a theory of economic change, which was to be an analysis or process. It was the flow aspect, the analysis of change over a period, which was holding me up. Myrdal on that was tantalising; he seemed often to be just on the point of helping me, but it did not quite come off. His frequent references to Lindahl in that book suggested he must have been drawing on discussions that had taken place in Swedish between himself and his colleagues.

E.E.J.: Was not Lindahl among the many visitors who came to the LSE at that time?

Sir John: As good fortune would have it, he was. I was just wondering whether I could venture to write to Lindahl, when I met him. He came into tea at LSE and Robbins introduced us. (There were other important visitors to LSE whom one met that way.) I was so excited that I ventured to ask him out to dinner.

E.E.J.: Do you recall anything special about that dinner conversation?

Sir John: Indeed I do. He explained that his purpose in visiting England was to find someone who could help him in an English translation of his writings; could I help him to find someone? I found a helper, a lady who had taken part in the discussions at LSE which I have been describing, who was herself a public finance specialist, so that in all that side of Lindahl's work she was particularly interested, and who was prepared to take the trouble to get a reading knowledge of Swedish. So it was that we went on working together, up to my marriage with the lady, at the end of '35, and after, until all our books came out in 1938–9: his *Essays in the Theory of Money and Capital*, her *Finance of British Government*, and my *Value and Capital*.

E.E.J.: Is there a particularly Lindahlian part that can be identified in *Value and Capital*?

Sir John: The most obviously Lindahlian chapter is the chapter on Income. This, in substance, follows Lindahl's well-known paper "The Concept of Income," which had appeared in English in 1933, so I ought to have read it before I met him, but I don't think I had done

so. My chapter re-states his argument, in a less paradoxical manner; so it seems to have been acceptable to national income people and such-like, who (I have been told) regard it as rather standard. It is however remarkable that it would be possible to cross out that chapter without making much difference to the rest of the book which was designed to proceed without any reference to Income, or to the Savings and Investment which go with it.

E.E.J.: Why did you choose to proceed in that way?

Sir John: Why did I proceed that way? It was itself a consequence of what I had got from Lindahl. The curious "week" and "Monday" assumptions which are employed throughout almost the whole of the latter parts of *Value and Capital* were designed to get the analysis as far as possible while avoiding the "ex ante—ex post" trouble on which he and Myrdal had thrown so bright a light.

Their basic point was simply that no one can decide what is to happen, only what he intends to happen and taking steps to facilitate the execution of his decision, the steps that he can take now. He can enter into contracts, which hand over the execution of the decision (or a part of it) to someone else; the contract is a promise, but no contract can offer perfect certainty that the promise will be kept. There is a vital distinction between promising that a thing shall be done, and actually doing it. My device was based upon this distinction. I was trying to find a way of bringing the behaviour of an economy, over a period, into a formal model.

E.E.J.: Wasn't there already some sense among older economists that this could readily be done?

Sir John: Many older economists had thought that this could easily be done; but in the light of what I had learned from Lindahl, I could see that it was terribly hard. Could one find a device (it would have to be an artificial device) which would help one to do it? A device which would recognize the distinction I have just been making, but which would still allow one to construct a usable model? My device, which must have arisen in conversations with Lindahl (I wish that I could check with him or with the other witness), was to think of decisions—and contracts to embody those decisions—all being made at the beginning of the "week" (my "Monday"); execution of those decisions, and of the contracts embodying them, continuing during the week, but no new decisions being taken until the following Monday. This made it possible for me to use "point of time" theory, which I thought I understood, to determine a *temporary equilibrium*—of decision-makers and between decision-makers—on the Monday, in the light of information available on that Monday; while recognizing that on the next Monday there would be new information.

So what follows in *Value and Capital* is just Temporary Equilibrium theory, in this narrow sense. It is solely concerned with what happens on the "Monday," so the methods of "point of time" theory can be used, and are used. But since the decisions concern not only the distribution of assets on the Monday, but also the flows of inputs and outputs that are planned for the week (and after) the distinction between stocks and flows is not at that stage of much importance. I got as far as I could then see my way to go in that direction; I know that I ought to have tried to go a bit further. I have been trying ever since to find ways of going further.

E.E.J.: What are some of the ways along which you have explored?

Sir John: There is one thing which has become clear to me rather recently, in these explorations, which is so much to the point that I must mention it. The problem is to get a theory of behaviour over a period (week or longer). Such a period has a start and a finish. All I had done, by '39, was concerned with the start. There ought to be a corresponding theory of the finish. It also would be a "point of time" theory, so one might think, and for a long time I did think, that it would be just like the theory of the start. But that is not correct. For there are two things which will have happened between start and finish. The first relates to informational things, the moving-on of expectations—I have nothing fresh to say about that. The second relates to actual things that have happened during the period. The things that happened before the start are by-gones; for in any analysis of the period they are data; but the things that happened *during* the period, though they have a similar place with regard to the finish of the period, do not have a similar place in the analysis of the period as a whole. They are part of what, in analysis of the period, has to be explained.

E.E.J.: What are some of the things that will be affected by this change in viewpoint?

Sir John: One major subject which this change in point of view affects is the Determination of the Rate of Interest, discussed in a chapter in Part III of *Value and Capital*. The argument there runs, of course, in Temporary Equilibrium terms. That is to say, it is just looking at the Monday, say the first Monday, in isolation. The Walrasian proof, there given, that either the demand-supply for bonds or that for money may be eliminated, is perfectly valid. But it is misleading when one turns to look at the period as a whole, comparing the end-equilibrium with the start-equilibrium. For suppose that there are no significant information, or expectational, changes between start and finish, but there has been new borrowing, an increase, we may say, in the supply of bonds. If the bonds are held as money substitutes, as Keynes (and myself in '39) must have been supposing them to be, the effect on the Rate of Interest must be a matter of Liquidity Preference (the LM curve of that old diagram). But, over the period, there is time for them to be taken up by additional saving; that is a possibility which should also be taken into account. On some occasions the one might be more important, in others the other. Thus it could be that Marshall, who was surely on this matter the "classical" theorist whom Keynes had most in mind, was mainly right in his day, while Keynes was mostly right in his. And in ours—we should not commit ourselves to either view without looking around. (I don't think that the empirical "research" which would be needed need be very arduous!)

E.E.J.: Is there more that we, as contemporary thinkers, might extract from Lindahl's work?

Sir John: On this I have just one thing to say. I am firmly of the opinion that the use of Lindahl's approach to the theory of the Income concept is by no means exhausted. It was not exhausted by what I said in *Value and Capital*. Only a few years ago I managed to write a paper on capital gains which I think went far to clearing up that elusive concept. It now appears as "The Concept of Business Income" in the Third Volume of my Collected Papers, *Classics and Moderns* (1985). In this paper I showed that there were two quite different meanings which, in practice, have got superimposed. I called them "external" and "exceptional" gains. I could not have written that paper without help, as I have acknowledged, from Lindahl.

E.E.J.: It has been most enlightening, Sir John, to have you acquaint us, first hand, with the several influences on the development of your *Value and Capital* and to enjoy your

restaging of some brief scenes from those heady days of the LSE in the 1930's. I wonder if, as a closing remark, you would venture your own assessment concerning the possible long term influence of *Value and Capital* and the place you personally accord it in your long and productive professional lifetime?

Sir John: I think we can begin by saying that it has surely been shown by experience that a good deal of what is in that book is an essential component of any teachable economic theory; though how big a place it should occupy is another matter. The things it omits from consideration are quite major. One, of course, is economies of scale; also there is nothing about government. Its traders seem to be able to get on without even having a legal system to support them; a consequence of that is that it is very sketchy on money. I have not been able to do much to remedy the former gap. I can however show that I have been very well aware of it, for I have given it much attention in the work that I have done (beginning almost at once when *Value and Capital* was finished) on the critical analysis of macro-economic concepts—which is what I have come to understand “welfare economics” is about. This work is now in *Wealth and Welfare* (1981) the first volume of my collected essays.

My quite extensive work on money has, regrettably, not been brought together; the second volume of the collection, some important recent writings, includes neither what is in my *Critical Essays in Monetary Theory* (1967) nor *Crisis in Keynesian Economics* (1974). The latter has very properly been appended to the Hungarian translation of *Value and Capital*. So I think it can be said that there are some Eastern European readers who can get a more rounded idea of my work than many get in the West. I am rather hoping that *glasnost* will last long enough for the Russians to follow that Hungarian example. And then the two might start to be put together on American reading-lists, so that the Americans do not get left behind.