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# PERSPECTIVES ON TWENTIETH CENTURY ECONOMIC THEORY\*

## Capital Controversies: Ancient and Modern

By JOHN HICKS\*\*

In what must by the rules be a fairly short paper, I cannot make a survey of all controversies about capital, from (say) Ricardo-Malthus to Joan Robinson-Solow. All I can attempt is something much more modest. What I propose to do is to take one particular point, which has figured (as I shall show) in many such controversies, and to use it as a means of pulling a part of the story together. It is a matter which would seem to be appropriate for discussion at this joint meeting, for it is an interesting illustration of the way in which the history of his subject can be of use to the modern economist.

Economists do indeed have a special use for the history of economics, something more than the general use that can be made of their own history by students of other subjects such as mathematics and the natural sciences. The history of science, certainly, is no mere antiquarianism; one is learning science when one learns in what ways scientific discoveries have been made. The history of economics has that use, and it has other uses. It has, of course, a pure historical use; the greatest economists, Smith or Marx or Keynes, have changed the course of history; they are as worthy the attention of the pure historian as Louis Napoleon or Woodrow Wilson. But

this again is not the economist's use. That is something different.

Economics is a social science, and a particular kind of social science, in that it is concerned with the rational actions, the calculated actions, of human beings, and with their consequences. This has the result that those whom we study can hear what we say. We may speak to each other in our private languages, but private conversations are no more than goods in process; while we speak only to each other we have not finished our job. The ideas of economics, the powerful ideas of economics, come from the market-place, the "real world," and to the "real world" they go back. So there is a dialogue between economists and their subject-matter. It is a dialogue in which there are important intermediaries; statisticians are one kind of intermediary, journalists another, accountants (as we shall see) another; the economist-statistician and the economist-journalist do much of the intermédiation themselves. In the course of the dialogue ideas acquire associations; they cease to be free ideas, which can be defined at choice. It is not in our power to say with Humpty-Dumpty, "When I use a word it means just what I choose it to mean"; we cannot escape the associations.

I do not mean that there is not such dialogue, and such associations, in the case of other social sciences. Clearly there is; in political science, say, as much, if not more,

\* The paper given at this session by Joseph J. Spengler of Duke University will appear in the *Journal of Economic Issues*.

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than in economics. Political ideas are indeed so rich in associations that the study of politics seems sometimes to consist of little else. Economics has (relatively) much more that is positive to offer, but we should not allow our passion for quantification to blind us to the fact that economic ideas share this characteristic of political ideas and do so for the same reason.

We cannot escape the associations, but we can try to understand them, so as to be masters of them. That is what, in my view, the history of economics is for. It is what it is for, for the economist. We need to know the history of our concepts in order to know what it is that we are handling.

The history of economics, so understood, cannot be discovered by poring over old textbooks, even old "classics." That is no more than a part of what has to be done. The books must be read against their background, the events which prompted the analysis, and what happened to the analysis when it went out into the world. All that is part of the tradition which we have inherited, and from which, if we are to do our job, we cannot escape.

I turn, I hope in that spirit, to my particular topic.

One must begin with a distinction which has been fully understood only in quite recent years. Suppose we start by saying (as many would say) that the capital of an economy is its stock of real goods, with power of producing further goods (or utilities) in the future—the stock of such goods existing in the economy at a moment in time. That, in strictness, is no more than a list—what in English, but not in American, we would call an inventory. How do we aggregate it, as for macroeconomic purposes we have to do? We cannot aggregate except by adding money values; how do we deflate that money aggregate, so as to get a measure of Real Capital? There are, in principle, two main ways.

The first, which on the analogy of other

aggregates, such as consumption, may seem the more natural, is to deflate by an index of the prices of the capital goods themselves. (It may not be easy to find a suitable index, but that is not here my concern.) It is, theoretically, a possible measure; to distinguish it from the other, to which I shall be coming, I call it Volume of Capital. It has the property, it will be noticed, that as between two economies which have capital stocks that are physically identical, Volume of Capital must be the same.

It may well be objected, and has often been objected, that Volume of Capital misses the essential fact about capital—that the utilities of capital goods are indirect. The values of capital goods are derived values, capitalized values of future net products. If these future products are valued at current prices of products, the resultant capital values should be better indicators of the true values of the capital goods than their actual market values, which are influenced by expectations of changing values of products. These "corrected" capital values could be aggregated; but since they have been built up from product prices, not capital good prices, they can only be deflated by an index of product prices, if they are to be used as a measure of Real Capital. Real Capital, in this sense, does not have the invariance property; it may be changed, without any change in the physical goods, by the mere admission of new information. I call this other measure Value of Capital.

When the distinction is expressed in this statistical (or quasi-statistical) manner, it would seem that we need have no difficulty in living with it. We can keep both in play, using one for some purposes, the other for others. We may nevertheless have some difficulty in explaining ourselves. Both, as described, are measures of Real Capital; but what is Real Capital? We cannot say that the two measures are two measures

of the same thing—as one may say that one buys a pound of something, or a dollar's worth of it, when the price of the thing is a dollar a pound. The Real Capital that is being measured is different.

If it is capital in the volume sense that is being measured, capital is physical goods; but in the value sense capital is not physical goods. It is a sum of values which may conveniently be described as a Fund. A Fund that may be embodied in physical goods in different ways. There are these two senses of Real Capital which need to be distinguished.

I do of course borrow the term Fund from the history, and to the history I now turn. I am going to maintain that the distinction is quite ancient; it divides economists, ancient and modern, into two camps. There are some for whom Real Capital is a Fund—I shall call them Fundists; and there are some for whom it consists of physical goods. It is tempting to call the latter Realists;<sup>1</sup> but since one wants to emphasize that both concepts are *real*, this is not satisfactory. I shall venture in this paper to call them Materialists. (Materialists, I mean in the sense of Dr. Johnson's refutation of Berkeley's idealism—"striking his foot with mighty force against a large stone, till he rebounded from it, *I refute it thus.*" There will be some at least of my Materialists who are worthy followers of Dr. Johnson.)

One of these was Edwin Cannan, the teacher of Lionel Robbins and the founder of the economics school of the London School of Economics and Political Science. A beautiful illustration of the opposition with which I am here concerned is to be found in Cannan's comments on capital in Adam Smith.<sup>2</sup> Cannan was convinced that Smith was in a muddle. I do not think that

he was in a muddle; he was simply a thoroughgoing Fundist, and to the Materialist Cannan the Fundist position was quite incomprehensible. Cannan: "The capital [in Smith] is often spoken of as if it were something other than the goods themselves." That is just the point.

Not only Adam Smith, but all (or nearly all) of the British Classical Economists were Fundists; so was Marx (how else should he have invented "Capitalism"?); so was Jevons. It was after 1870 that there was a Materialist Revolution. It is not the same as the Marginalist Revolution; for some of the Marginalists, such as Jevons and Böhm-Bawerk, kept the Fundist flag flying. But most economists, in England and in America, went Materialist.<sup>3</sup> Materialism, indeed, is characteristic of what is nowadays reckoned to be the "neoclassical" position. Not only Cannan, but Marshall and Pigou, and J. B. Clark, were clearly materialists. Anyone, indeed, who uses a Production Function, in which Product is shown as a function of labor, capital and technology, supposed separable, confesses himself to be (at least while he is using it) a Materialist.

What about Keynes? Keynes, of course, was brought up as a Materialist, and there are no more than slight signs, in the *General Theory*, that he had departed from the Materialist position. So it is perfectly possible to be a Keynesian and yet to be a Materialist. But the rethinking of capital theory and of growth theory, which followed from Keynes, and from Harrod on Keynes, led to a revival of Fundism. If the Production Function is a hallmark of Materialism, the capital-output ratio is a hallmark of modern Fundism. That, in the briefest of outline, is the story; how it happened I shall now attempt to explain.

<sup>3</sup> But there was at least one important American Fundist, F. W. Taussig. Irving Fisher is harder to place, since he, at least sometimes, could see both sides. But it is interesting to find that Cannan thought Fisher, like himself, to be a Materialist.

<sup>1</sup> As I have done myself in a short passage in my *Capital and Time*, p. 13. I have since become convinced that Materialist is better.

<sup>2</sup> Cannan, pp. 145-50.

Let us go back to the Classics. Why were the Classical Economists Fundists? It is not easy to see, just from looking at their works; they take their Fundism so much for granted that they do not need to justify it. Surely the reason is that it came to them from outside—from business practice, from accounting practice.

Even to this day, accountants are Fundists. It is not true, accountants will insist, that the plant and machinery of a firm are *capital*; they are not capital, they are assets. Capital, to the accountant, appears on the liabilities side of the balance sheet; plant and machinery appear on the assets side. Capital, accordingly, is a Fund that is embodied in the assets.

The origin of accounting is in the business of the merchant; accounting categories, to this day, bear the mark of their mercantile origin. It was the merchant who was the original Fundist. It is the merchant who thinks of his capital as a Fund that is invested in a stock of salable goods. It is in the Fund sense that capital "circulates"; the physical goods do not circulate, but the Fund does. It is the Fund that is "turned over." The stock of goods in the merchant's possession is one thing (the most he will admit is that it is the form that is taken by his capital at the moment); his capital, he will surely say, is something more permanent.

These were the business terms which came naturally to the Classical Economists. They had no reason to depart unnecessarily from the businessman's language. This was the system of thought that the businessmen of their time were using; they just followed it. It is true that they were thinking of the whole (national) economy, not of the single business; but this, they surely felt, made no difference. It did not need to make much difference. They could think (as Henry Thornton, in particular, most surely did think) of the

whole economy as having a balance sheet, constructed by consolidation of the balance sheets of the single businesses; in the consolidation, the liabilities of one unit would be cancelled against the assets of another, but no item would be transferred from the liabilities to the assets side. So, even when all debts and paper claims had been cancelled, there would remain on the assets side the real goods (and balance of external claims), on the liabilities side the Capital—still a Fund. It need not be thought of as a debt owed by the nation to itself; it is the same kind of thing as the Capital of the single business.

The way would thus be open for Classical Fundism if the whole economy consisted of merchants; how far, however, could it cope with businesses of other kinds? It was necessary, from the start, to deal with businesses of other kinds; but for the first of the extensions that were needed, Fundism did not do at all badly. It is often thought that the notion of capital as "advances to laborers" took its origin from observation of agriculture, so it is labelled physiocratic; and it is true that if one looks only at the economic literature it is with the French physiocrats that it seems to come in. But so far at least as the British Classical Economists are concerned, it is more convincingly interpreted as a fitting of agricultural experience into the mercantile pattern. The farmer, like the merchant, "turns over" his capital, buying the services of labor, as the merchant buys his stock in trade; selling the product of that labor when it is ready to be marketed. So the Fundist concept of capital could be carried over to agriculture, surviving the transition. It seemed, on the whole, quite a good fit.

The farmer, of course, used land as well as capital, but that land was a separate factor of production no one doubted. The rent of the land must be deducted from

the "gross" profit on any agricultural operation before the "net" profit was arrived at; it would be the rate of net profit on the Capital Fund that competition would tend to equalize.

Classical economics was three-factor economics, and we can now see that the triad had deeper roots than is commonly supposed. Labor is a flow, land is a stock (as stock and flow are used in modern economics); but capital is neither stock nor flow—it is a Fund. Each of the three factors has its own attribute, applicable to itself but to neither of the others. Labor works *on* land *through* capital, not on capital nor with capital. The place of each of the factors in the productive process is sharply distinguished.

The Classical Economists, so interpreted, are rather consistent; among their successors, in the latter part of the century, consistency disappears. Not all of those who went Marginalist went Materialist, and those who did go Materialist did so in different ways. The case of Walras, for instance, is quite peculiar. I feel fairly sure that he is to be reckoned as a Materialist; but the reason for his Materialism is his interest in particular capital goods, appearing (of course) in his work as a part of his general determination to work with an *n*-goods model. Yet his Materialism may antedate that determination, and may have been one of the things which impelled him towards it. He says that he took his view of capital from his father and Auguste Walras (writing in 1849) would certainly seem to have been a Materialist, even an extreme Materialist. "Capitals" (*capitaux*) to him are capital goods; "incomes" (*revenus*) are income goods; they are distinguished by multiple (*successive*) uses against single uses and by that alone. Carriages, carts, steam-engines are *capitals*; a glass of wine, a round of beef, a candle are *incomes*. "*Le revenu, ainsi que*

*son nom l'indique, c'est ce qui revient; or, ce qui revient, c'est ce qui s'en va.*" (pp. 53–54).<sup>4</sup>

One must yet beware, in father and son alike, of mistaking for a theoretical approach what is no more than a peculiarity of the French language, the restrictiveness, the deliberate restrictiveness, of its vocabulary. It may well be that much French Materialism is only apparent, a matter of linguistics rather than economics.

The case of Marshall is here more interesting. Marshall's Materialism is much more clearly to be explained by events—by the now achieved Industrial Revolution—the rise in importance of plant and machinery. The Classical schema, as we have seen, began with trade and was extended to agriculture; so long as stocks and work in progress were the main part of the manufacturer's physical assets it could be extended, in much the same way, to manufacture also. But when a large part of his capital became *fixed* in plant and machinery, a candidate had appeared for factor status, which did not fit into the classical triad. What was to be done?

Before considering what happened in the economics, it will be useful to turn again to the accounting aspect. The rise of the Machine had already presented the accountant with a parallel problem. What did the accountant do about it?

So long as he had nothing to consider but mercantile transactions, his task in principle was simple. For it is characteristic of the business of the merchant that it is divisible into separate units. Every bale of cotton or pound of cheese which ever forms part of his stock is acquired at a particular date and sold at a particular date; purchase, retention and sale constitute a separable transaction. (Complete

<sup>4</sup> ("Revenue [the return], as its name indicates, is that which comes back; now, that which comes back, is that which went off."—Editors.)



separation is of course not attained in practice, since there are overheads which have to be allocated; but it is so nearly attained that it sets the pattern.) So the accounts of the merchant may be regarded as a bundle of separate accounts. Purchases and sales are indeed going on continually, so that if they are set out in a time sequence, the separate accounts will overlap; it is only if an account were prepared for the whole history of the business, from first setting-up to final closing down, that a record of purchases and sales would tell the whole story. In any arbitrary, say, annual, period there must be transactions which have started before the beginning of the period, but are completed within the period; there must similarly be transactions which are begun within the period but not completed when it is ended. These, however, in the case of a mercantile business cause the minimum of trouble. They can be dealt with by the accepted rule of never taking a profit on any transaction before it is completed. The initial stock of the year will then be brought in *at cost*; and the final stock will be valued at cost in the same manner.<sup>5</sup>

But what was to be done, on these principles, with plant and machinery? The use of land, being regarded as a permanency, could be brought in as a regular charge; but the plant and machinery is not expected to last indefinitely, though its use is spread over a time which is longer than the accounting period. It is important to observe that it is the extension of the use to a duration which is longer than the accounting period which creates the difficulty. There would be no difficulty, here as in the mercantile case, if the account were drawn up for the whole life of the business,

from first setting-up to final closing-down.<sup>6</sup> It is for the annual account that there is the problem. The cost of the machine has to be set against a series of sales, the sales of the outputs to which it contributes, but some of these sales are sales of the present year, some are later and some, maybe, earlier. There is thus a problem of imputation; how much is to be reckoned into the costs of this year, and how much into the costs of other years? It is just the same problem as the allocation of overheads, and to that, as is now well known, there is no firm *economic* solution.

Neither has the accountant found a solution—only a name and a set of (essentially arbitrary) rules. The “depreciation quotas” must add to unity, but that is all that is known, at all firmly, about them. The form of the account is preserved, but only by bringing in, as the capital which is supposed to be invested in the machine at the beginning of the year, that part which has not been absorbed (by being allocated as a cost to the output of preceding years) and by reckoning as the capital invested at the end of the year that part which has not been absorbed in those years nor in this year—so that it is left to be carried forward to the future. This is in fact what accountants did (probably what they had to do) as soon as they were confronted with the problem. It is what they still do, even to this day.

We have had plenty of opportunity in the present century to understand how arbitrary the accountant’s depreciation quotas are. We have seen them battered by inflation, and we have seen them manipulated by tax authorities in the interests of fiscal policy. Late nineteenth century economists had much less of this ex-

<sup>5</sup> There is of course the qualification that an expected *loss* may be taken in advance: “cost or market value whichever is the lower.” But this does not affect the principle.

<sup>6</sup> It was the realization that the economist, unlike the accountant, need not be bound down by annuality, unless he chooses, that prompted me to write *Capital and Time*.

perience, so it is not surprising to find that they began by taking the accountant's depreciation quotas much too seriously. That is true of both schools. It is true of the surviving Fundists (such as the Austrians) who conceived of investment in fixed capital as equivalent to a bundle of investments in circulating capital. If one-tenth of the cost of a machine of ten-year life could be imputed as cost to the product of each year, the machine was equivalent to ten investments in circulating capital, one with an investment period of one year, one of two years . . . and one of ten years. It was the accountant's depreciation quotas which did the trick.

It is also true of Marshall. He also relied upon the accountant's "solution," but in a different way. It was an essential element in his concept of long-period equilibrium. In the short period, Marshall tells us, when "the producers have to adjust their supply to the demand as best they can with the appliances at their disposal," the "income" derived from those "appliances" is a *quasi-rent*.<sup>7</sup> It is called a quasi-rent by analogy with the rent of land; in all of his short-period theory Marshall has Ricardo's rent theory very much in mind. Now the rent which is determined as a surplus, in Ricardo's manner, makes no allowance for depreciation; in Ricardo, land being "indestructible," no such allowance is of course required. Marshall, however, does think of a deduction being made, *even in the short period*, though Ricardo has given him no help in determining what that deduction should be. It is true that Marshall is so reticent on the matter that he can easily be misunderstood; one has to read him quite carefully to discover whether quasi-rents are to be taken gross or net. He does however say in a definitional chapter of *Principles*:

We cannot properly speak of the

interest yielded by a machine. If we use the term *interest* at all, it must be in relation not to the machine itself but to its money value. For instance if the work done by a machine which cost £100 is worth £4 a year net, that machine is yielding a quasi-rent of £4 which is equivalent to interest at four per cent, on its original cost; but if the machine is worth only £80 now, it is yielding five per cent on its present value. [pp. 74-75]

This seems conclusive.

It is indeed quite remarkable how little there is in Marshall's book about depreciation. There is a footnote in which he recognizes that it is a problem, but the footnote just ends with a reference to an accounting textbook.<sup>8</sup> He has evidently decided that for his purposes, the accountant's solution will do. Gross can in that way be reduced to net, and it is net returns that are equalized by competition in the long period.

That is what Marshall says, but in the forty years which followed on the publication of his *Principles*, the strangest things must have happened to teaching on this point, even among his closest followers. One had heard rumors that there was a good deal of confusion between "gross" and "net" among Cambridge teachers, and a strong piece of evidence in support of them has now come to hand. In his chapter on the marginal efficiency of capital in the *General Theory*, Keynes is careful not to call his *Q*'s quasi-rents, and rightly so, since they are gross of depreciation, so they are *not* what Marshall meant by *quasi-rent*. But why are they symbolized by *Q*? It has naturally been assumed that *Q* stands for quasi-rent; so the *Q*'s are called quasi-rents in many post-Keynesian writings. We now know that in an earlier draft the *Q*'s were called quasi-rents.<sup>9</sup> Keynes made a correction, or semicorrection, in

<sup>8</sup> Marshall, pp. 354-55.

<sup>9</sup> Keynes, pp. 425-26.

<sup>7</sup> Marshall, p. 376.



the final version, but the confusion is betrayed.

The fact is that until the new wind began to blow, in the mid-twenties, very little interest had been taken in Cambridge in capital theory. Take the case of Pigou. One of the remarkable changes introduced into the later editions of *The Economics of Welfare* is a chapter entitled "Maintaining Capital Intact." There was no such chapter in the first or second edition. It makes its appearance in the third (1928), by which time the problem of defining saving, in conditions of changing prices, had been brought to light in the work of Robertson (1926). In the fourth edition (1932) it is considerably altered; and there is a further version, in an important article (1935) which looks as if it was intended as the basis of another revision.<sup>10</sup> Pigou, it is clear, was very bothered; and one can see why.

It was only a part of the problem of capital with which he thought himself to be concerned; he was trying to look at that one part in isolation. In spite of its "Welfare" coloring, the subject of his book was the Social Product (or "dividend" as he calls it): how it is measured, what makes it large or small, how it is distributed. The Social Product of one particular period ("year") is considered, almost, in isolation. Having chosen that way of posing his problem, he is led (almost inevitably) to what would nowadays be called a "Production Function" approach. Even so, he might have found himself confronting the general problem of measuring capital—for how can we make a static comparison, between two economies whose capital stocks are different, without having some means of comparing their capital stocks? Pigou did not, at first, raise this wide question, though Robertson, at much the same date,

<sup>10</sup> Pigou (1935).

could already see that it was involved.<sup>11</sup> He confined himself to what arose in the measurement of income, in the single year—the measurement of the investment component, the reduction of gross investment to net. (This, of course, already implies a problem of capital measurement—the comparison of the beginning-year and the end-year capital stocks.)

Pigou's approach is strictly Materialist. He does indeed recognize that the business concept of capital is different, but

for economics the stock of capital existing at any time is a collection of objects, the extent of which is a purely physical fact. . . . the size of the stock is not . . . affected by its value; it is exactly the same . . . whether that value is large or small. [1935, p. 235]

He goes on to draw from this the important conclusion, echoes of which are to be found in many more recent writings:

A distinction should be drawn between changes which, while leaving the element still as productive as ever, bring nearer the day of sudden and final breakdown, and physical changes which reduce its current productivity and so rentable value. With the former sort of change, until the breakdown occurs, the capital stock is, I suggest, best regarded as intact, just as it is best regarded as intact despite the nearer approach of a day that will make a part of it obsolete. [1935, p. 238]

I shall not discuss Pigou's treatment in detail; enough has been said to indicate its Materialist character. For the purpose which Pigou had in mind, it may well be defensible (as I shall show), but it can hardly be regarded as general; so it is not surprising that there were quarters where it was not well received. I think not so

<sup>11</sup> See Robertson's paper "Wage Grumbles," reprinted in Robertson (1931). I have a particular affection for that paper since it was through it that I myself first came into touch with its author.

much of the "user cost" chapter in Keynes's *General Theory*, which I regard as an unsuccessful (and for Keynes's purpose unnecessary) attempt to bridge the gap. The most direct statement of an opposing view came from Professor Hayek.

Hayek's first paper on the subject was already in preparation when Pigou's article appeared in 1935.<sup>12</sup> He further developed it in his *Pure Theory of Capital* and in a paper in *Economica* which is a reply to a further note by Pigou.<sup>13</sup> Hayek, of course, was a Fundist, but a very sophisticated Fundist, deeply preoccupied with the problems of ignorance and uncertainty which come to the fore as soon as one thinks of capital value as being determined by expectations of the future. It was the omission of this aspect which set him against the Materialism of Pigou. His striking illustration—of the machinery installed to produce a fashion good, required within the year but not afterwards, so that at the end of the year the machine which is physically intact has lost its value—is directed to show the economic irrelevance of physical identity. *In general*, so it seems to me, Hayek is right.<sup>14</sup>

I shall not attempt to carry the story further, except to suggest that we may get a useful light upon more recent differences if we look at them in terms of this interesting controversy between Pigou and Hayek. Pigou, I think one can now see, was working within a model—a rather restrictive model but a useful model, a model which for certain purposes we would most of us still wish to employ. Its central concept was the Social Product, a *measurable* Social Product. It has now become clear

that one cannot measure a Social Product of the Welfare type with which Pigou was concerned unless one assumes that wants are unchanging—or that some kind of Social Welfare Function is unchanging—and that cuts out the sort of variation over time which is the root of Hayek's example. In Pigou's world the Hayek problem could not arise. In general, of course, it does arise. There are bound to be odd cases which will not fit into the Pigou model.

I do not mean to imply that attention to odd cases is the principal reason for the revival of Classical Fundism. It is one of the reasons; there are plenty of economists who make their living by trading in odd cases, which make splendid subjects for little notes in journals. There are other reasons which are more substantial. Fundism, as I have emphasized, is the businessman's concept of capital; social accounting, which has brought economists into closer relation with business accounting, was bound to induce a revival of Fundism. So was attention to planning. Planning is forward-looking, and the Fundist concept of capital is forward-looking; they fit in. For such and such a development, how much capital will be required? It is capital in the Fund sense to which such questions are relevant.

When Fundism is looked at in this light, we can see that there may be a place for Materialism also. If Fundism is forward-looking, Materialism is backward-looking, for it is concerned with the capital goods existing at the moment, goods which have been brought into being in the past. "By-gones are by-gones," but there are still some purposes for which we have to go to the past. Our statistics, in particular, always relate to the past. One cannot measure capital, statistically, excepting in terms of its history, valuing, of course, not at historical cost, but at doctored historical cost, or replacement cost. There is a striking

<sup>12</sup> Hayek (1935).

<sup>13</sup> See Hayek (1941a) and Pigou (1941).

<sup>14</sup> In my own contribution to the same discussion (Hicks, 1942), I came down mainly on Hayek's side. The positive part of my paper is reprinted in *Capital and Time*, pp. 164–66.

example of this in the book by J. R. S. Revell, *The Wealth of the Nation* (1967), an inquiry conducted in Cambridge, England, a place that must often appear on this side of the Atlantic as a headquarters of the New Fundism. Revell's calculation of the National Capital of Britain would have appealed to Pigou;<sup>15</sup> he had to be Materialist, because as a statistician he could be nothing else.<sup>16</sup> There is nothing else that can be used in macroeconometrics; so it is macroeconometrics itself which is on trial—but that, perhaps, is as it should be.

#### REFERENCES

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- <sup>15</sup> I am aware that in his last note on the subject, Pigou (1941) dissociated himself from the replacement cost measure of capital, pointing to the case where the article to be replaced "had become impossible to produce" as a reason for rejecting it. I do not think he was well advised to make this concession. He would have done better to insist that it was one of the "odd cases" that he was leaving out.
- <sup>16</sup> For further discussion of this matter of measurement, see Hicks (1973), Chapter 13, and (1971), Appendix D.
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