The Cambridge-Cambridge Controversy in the Theory of Capital; A View from New Haven: A Review Article[1]

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Economics has long been plagued with controversies: the Keynesian-monetarist controversy, the investment function controversy (is the elasticity of substitution unity?), the liquidity preference–loanable funds controversy. Geoffrey Harcourt has extended his survey article from the *Journal of Economic Literature* (1969) into a book dealing with one of the latest of these so-called controversies, that between Cambridge, England, and Cambridge, Massachusetts, concerning capital theory. The book is more balanced and more complete than the original survey article and includes some good pieces of exposition.

The problems that I find with the book are basically problems I find with the Cambridge (U.K.) theory of which he is a partisan on one side—as, I suppose, I am on the other—and so, rather than focus on any errors and confusions which are peculiar to Harcourt, I prefer to focus on three of the major issues involved in the dispute and to suggest, in doing so, where Harcourt (and the Cambridge [U.K.] theorists) have gone astray.

I. The Determination of Savings and the Rate of Interest

We all know that different individuals save different proportions of their income, depending on their age, number of children, income, the riskiness of their job, etc. Neoclassical economists have tended to emphasize the demographic characteristics (the life-cycle hypothesis), or, alternatively, when they wish a simplified exposition for students, they simply assume savings is a constant fraction of income. The Cambridge economists have emphasized alternatively the role of institutions (corporations) in determining savings, and class behavior (whether the individual is a "worker"

[1] G. C. Harcourt, *Some Cambridge Controversies in the Theory of Capital* (New York: Cambridge University Press, 1972).

or a pure "capitalist"). One would have thought that there would be an easy way to resolve such a controversy: which hypothesis provides a more accurate prediction of savings? Unfortunately, the hypotheses are not so easy to verify. For instance, direct evidence on savings of corporations may be misleading, for it is not unreasonable to assume that individuals can see, at least partially, through the corporate veil, and that corporate savings are a substitute for individual savings. (There appears to be some recent econometric support in favor of this hypothesis. Indeed, given our tax structure, it is to be expected that investment is largely financed by retained earnings.) Flow-of-funds analysis can provide relatively little information about the "real structure" of the economy.

The question arises, however, Why the excitement over one specification of the savings behavior or another? The answer lies in the role that savings behavior plays in the Cambridge (U.K.) theory in the determination of the rate of interest in long-run equilibrium. In the simplest form, if savings are a given fraction s_π of profits, π, and all of wages are consumed, the rate of interest, r, is equal to the rate of growth, n, divided by s_π:

$$r = n/s_\pi. \tag{1}$$

It is also true that with profit-maximizing competitive firms, in long-run equilibrium where all relative prices are constant, the rate of interest is equal to the own rate of return of every capital good[2] (the marginal productivity of every capital good in terms of itself):

$$r = MPK_{ii}. \tag{2}$$

The controversy arises in the peculiar theory of causation: the Cambridge (U.K.) economists argue that the rate of profit is determined by the rate of growth and s_π, and that this in turn determines the own marginal productivity of capital goods. The reason for this view is that one can first solve equation (1) for r, without knowing anything about the production function or the factor supplies, and having solved for r, one can solve the second equation for the marginal productivity of each capital good and the capital goods–labor ratios.

There are three objections to this view: first, there is a confusion between "causation" in a temporal sense and "causation" in the formal analysis of the structure of a set of equilibrium conditions. It is only in the temporal sense that causation has any economic significance. At any moment, there is a given vector of capital goods and of labor. Under the extremely simplified models conventionally used, these endowments determine the marginal productivity of the different capital goods and the rate of interest. Given the savings behavior, this determines the change in the stocks of capital goods; eventually the economy converges to a

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[2] I.e., the value of the marginal product of the capital good divided by its price. (I am assuming differentiability, so the marginal products are defined. See below.)

state where the rate of interest is equal to the rate of growth divided by the savings propensity; still, at each moment, it is the "capital goods–labor ratios" which determine the rates of return on the different capital goods.

Second, in more general models, there is a simultaneity of the determination of the values of the variables of interest in the short- and long-run equilibrium.[3]

Third, the Cambridge (U.K.) approach cannot be extended to the determination of the relative factor prices if there are more than two factors; for example, if labor is not homogeneous, we have to have a theory which determines the relative prices of the different kinds of laborers. The marginal-productivity theory provides an explanation of all relative prices at the same time; the Cambridge theory has nothing to say except that, somehow, however the factor prices of the different kinds of labor are determined, the average wage must satisfy the "Cambridge" equation.

II. Reswitching of Techniques

In comparing economies in balanced growth, one set of techniques may be chosen at a high and a low interest rate, and another set of techniques at an intervening interest rate. This phenomenon—called "reswitching"— is the extension to a whole economy of the well-known phenomenon of multiple internal rates of return.[4] All that this implies is that the weak

[3] For instance, if there are some savings out of wages, we replace eq. (1) by $nK = I = s_\pi rK + s_w(Y - rK)$ (where K is the capital stock, I is investment, Y is output, s_w is the savings propensity out of wages), in which case eqq. (1) and (2) must be solved simultaneously. Pasinetti (1962) has shown that if there are two classes, workers and capitalists (who do no work), then even though there is some savings out of wages, eq. (1) still holds. But the result is not robust: if some capitalists save more than others, in the Pasinetti model r may be determined by the single capitalist with the highest savings rate; on the other hand, if workers save too much, then the pure capitalists disappear (in a relative sense), and eq. (1) no longer obtains. More generally, savings today will depend on expectations about the future, and these in turn will depend on the values of the relevant variables this period, which in turn depend on the level of savings.

[4] Assume we have a single consumption commodity, produced by labor and machines. Machines last T years and require 3 years to construct. Machine A requires a_1 units of labor the first year, a_2 the second, a_3 the third; machine B requires b_1, b_2, and b_3 in the first, second, and third years. Which machine will be used depends simply on the sign of $(a_1 - b_1)(1 + r)^2 + (a_2 - b_2)(1 + r) + (a_3 - b_3)$. If $a_1 > b_1$, $a_2 < b_2$, $a_3 > b_3$, then there may be two values of r, say r_1^* and r_2, such that for $r \leq r_1^*$ and $r \geq r_2^*$ machine B is employed; for $r_1^* \leq r \leq r_2^*$, A is employed; and at r_1 and r_2^* the two machines are equally profitable· In an earlier terminology, we would have said that this is an instance in which A is not an unambiguously more or less roundabout method of production than B; in present terminology, we say that A is not unambiguously more capital intensive than B. Note that consumption per man in a stationary state using A will be more or less than that in a stationary state using B as $a_1 + a_2 + a_3 \lessgtr b_1 + b_2 + b_3$. See Samuelson (1966) and the other articles in the Q.J.E. symposium on reswitching.

qualitative assumptions we conventionally make in economics—that is, convexity of the technology, with its implications of diminishing returns— do not have any strong implications for comparisons of economies in steady state: that is, reswitching establishes that the derivation of simple comparative dynamics propositions requires stronger assumptions than those required for the existence of competitive equilibrium and the derivation of qualitative properties concerning economies with given initial endowments. (In steady-state analysis, on the contrary, we let the initial capital stock be whatever it has to be to sustain the given steady state.) The conventional economic assumptions do enable us to make qualitative statements like "a decrease of a unit in the level of consumption today, c^0, will allow an increase in consumption tomorrow, c^1, of $\partial c^1/\partial c^0$, keeping consumption at all other dates constant; the one-period con- sumption rate of interest is equal to $\partial c^1/\partial c^0$ and decreases (more accurately, does not increase) with successive increases in consumption today." (If the technology is not differentiable, we must make the usual modifications involving left- and right-handed derivatives.) [5]

A comparison between the role of the "reswitching phenomenon" in capital theory and "Giffen's paradox" in consumer theory may be instructive. Competitive-equilibrium theory generally assumes convexity of preference sets, but to make qualitative propositions, even weak ones such as that demand curves are downward sloping, requires stronger assumptions. Giffen's paradox was important in the development of demand theory; the resolution of the change in demand into income and substitution effects is, most of us would agree, insightful. The reswitching phenomenon may eventually play a similar role in capital theory. But I doubt it, for two reasons.

1. Steady states are of limited interest in themselves; even the best of well-run economies never have a choice of steady states. They have a choice of consumption paths beginning with present initial conditions; conventional assumptions do allow us to make qualitative statements about such paths (economies may of course, eventually go to one steady state or another).

Moreover, it is easy to develop, using steady-state analysis, all manner of paradoxes. [6] For instance, the opening of free trade may actually lower steady-state consumption (this does not contradict the classical prop-

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[5] That is, if $(\partial c^1/\partial c^0)^-$ and $(\partial c^1/\partial c^0)^+$ are the left- and right-hand derivatives, then the one-period rate of interest lies between $(\partial c^1/\partial c^0)^-$ and $(\partial c^1/\partial c^0)^+$.

[6] Elsewhere (Stiglitz 1973b) I have discussed the concept of *recurrence*, that in the process of economic growth one technique is used, then another, and then the economy returns to the earlier technique. Recurrence can occur in technologies in which re- switching does not occur, and need not occur in technologies in which reswitching does occur.

ositions concerning gains to trade).[7] One can show that of all the feasible steady states in a life-cycle model, the one which maximizes steady-state utility is not sustainable by a competitive equilibrium (without appropriate lump-sum transfers), and conversely. (This does not contradict classical welfare propositions.)[8]

2. Though Giffen's paradox is irrelevant for most commodities, there are a few, perhaps potatoes and cheap wine, which have upward-sloping demand curves. There is thus some empirical basis for an interest in Giffen's paradox.[9] So far, no one has provided us with a corresponding empirical basis for an interest in reswitching. The reason, I suspect, is that the conditions under which it can be ruled out are very weak; for instance, reswitching cannot occur if, in only one industry in the economy, it is possible, by an increase in labor, either to reduce the input of some other factor, keeping other factors and output fixed, or to increase output, keeping all other factors fixed.[10] For reswitching to occur not only must there be essentially no substitutability in *every* industry, but also there must be large changes in the relative price for different capital goods, for it is the change in the relative price of capital goods which, in effect, leads one process to be more "capital intensive" at one interest rate (so the cost of using it rises more with an increase in the interest rate than the cost of using the other process) while it is less capital intensive than the other process at other interest rates. The question is, Are the kinds of changes of interest rates experienced in a growth process likely to be associated with such large changes in relative prices? Unfortunately, this is not a question which can really be answered, since most of the changes in relative prices which we observe have more to do with technical change, ignored in the discussion of reswitching, than with changes in interest rates; perhaps this simply testifies to the irrelevance of the reswitching controversy.

Thus, although reswitching has no implications for the validity of neoclassical distribution theory or for qualitative statements concerning the consequences of a given economy's increasing its level of consumption, it does mean that statements such as "the reason that one economy has a lower interest rate than another is that the former economy has a higher

[7] See Stiglitz (1970).

[8] Although the possibility, in the absence of money, of oversaving (rate of interest less than rate of growth) in a competitive equilibrium is an example of a competitive equilibrium which is not Pareto optimal. See, e.g., Samuelson (1958), Cass and Yaari (1967), and Diamond (1965).

[9] It is perhaps worth noting that the occurrence of Giffen's paradox depends on the level of aggregation chosen; wine is not likely to be Giffen, "cheap" wine is.

[10] Not only must there be this limited amount of substitutability, it must be "smooth substitutability"; i.e., the directional derivative must be defined. This is still much weaker than differentiability. For this and other "nonreswitching theorems," see Stiglitz (1973a).

capital labor ratio" may be seriously questioned, for it is *possible* that a given economy have a lower interest rate than another and a higher interest rate than a third, but that the two comparison economies be identical in all physical respects. But by the same token statements like "sales of the commodity were high because the price was low" may be questioned, because it is possible for the higher sales to be associated with higher prices as well. (It should be noted, however, that in comparing economies at the same time, which can trade capital goods with one another so that they have the same relative prices [approximately], these problems cannot arise.)

Some of the confusion arises because in dynamic analysis, just as in static analysis, there is a distinction between macroeconomic and microeconomic models. Although we all recognize that there should be a close relationship between the two, the models which are appropriate for answering one class of questions may not be appropriate for answering another. Thus, to answer the kinds of questions conventionally raised in general equilibrium analysis, we never need to talk about an aggregate savings function; we need only impose fairly weak restrictions on each individual's preferences. In macroeconomics, we want fairly precise qualitative (and indeed quantitative) answers to particular questions, such as the effect of a change in some policy. This necessitates making stronger assumptions. The criticism of neoclassical growth theory is really an assertion that the restrictions embodied in neoclassical macroeconomic models do not necessarily follow from the microeconomic (disaggregative) models from which they should be derived. But the same criticism could be leveled against the static macroeconomic models.

Thus, to put the matter another way, if you accept (as most participants on both sides of the controversy do) the von Neumann (Sraffa-Samuelson-Solow, et al.) dual price–interest rate inequalities and the assumption that at any prices the cost-minimizing techniques are chosen, you have accepted the full content of the (microeconomic) neoclassical analysis of steady states. Reswitching says something about what that does or does not imply for comparisons of steady states, but no more.

III. Aggregate Capital

The first major attack of the Cambridge (U.K.) economists was not concerned with the saving hypothesis or the "reswitching phenomenon" (indeed, the latter was considered more of a curiosum, or a "perversity," than a serious economic problem) but with the existence of an aggregate capital stock. But like the reswitching phenomena, this is now recognized to be a red herring—and for very much the same reasons. Neoclassical distribution theory nowhere requires the use of aggregates, and to make

qualitative statements of the kind discussed above requires only convexity of the technology.[11]

Wicksell (a neoclassicist!) was the first to discover the difficulties one encounters in making comparative dynamic statements involving the aggregate value of capital stock, consumption, and interest rates of economies in long-run equilibrium; he discovered, for instance, that changes in the consumption per unit of change in the value of the capital stock might not be equal to the rate of interest, because of changes in the relative price of capital goods.[12] I have already argued that from a theoretical point of view, the analysis of steady states is not of much economic significance and may be misleading, and that the basic qualitative properties of economies out of steady state do not in any way depend on the ability to form aggregates. From a practical point of view, economists are always dealing with aggregates: one person's labor is aggregated with another, one piece of land is aggregated with another, one kind of steel is aggregated with another, even though they all have different properties. The condition under which these aggregates can be formed, that is, under which the aggregates act as if they were homogeneous factors of production, are very restrictive; nonetheless, I believe that, under most circumstances and for most problems, the errors introduced as a consequence of aggregation of the kind involved in standard macro-analysis are not too important; nonetheless, we must always be on our guard for situations in which this is not true. The question is, Do the problems associated with the accumulation of capital in growth processes represent one area in which properly formulated aggregates (e.g., using chain indices) are likely to lead to serious error? This, I suggest, remains a moot question.

[11] In an attempt to deemphasize the role of aggregate capital, Solow (see, e.g., Solow 1963, p. 98) has emphasized an alternative "aggregate"—the consumption rate of return (aggregating over different consumption goods rather than over different capital goods). It can be shown, for instance, that if A and B are two technologies which are both viable at a given rate of interest r^* (i.e., they both make zero profits and all other technologies make losses, at the given rate of interest and the associated prices), and if the economy is in steady state in A and wishes to "switch" to steady state B, the present discounted value, at the rate of interest r^*, of the change in consumption (from that on the path remaining at A) is zero, provided that the economy fully utilizes its resources at every date. This result does not in any way depend on the ability to form aggregate capital stocks, or on the possibility of reswitching. Nor does the fact that in the steady state A there is more of a particular capital good than in steady state B imply that in the transition some of the capital good must become redundant, as Pasinetti (1969) seems to have suggested. (The conditions under which a full-employment transition is possible are far more subtle than that.)

[12] The other comparative dynamic propositions which have been shown not to be generally valid are: (a) economies with higher interest rates have lower values of steady-state consumption, and (b) economies with higher interest rates have lower aggregate capital labor ratios. The "perverse" case can occur even without reswitching, and, indeed, it can be shown that it may occur even in a mild modification of Wicksell's model. See Stiglitz (1973a).

There are other issues in capital and growth theory which are not discussed in this book, and these omissions are perhaps as revealing as the topics which are discussed. For instance, as I have noted above, most of the analysis concerns steady-state growth paths. The interesting and difficult questions concerning disequilibrium dynamics, expectation formation, and adjustment processes cannot be properly analyzed in that context.[13] Second, the models analyzed all ignore the role of the government and, in particular, of the monetary authorities in determing the growth path of the economy. Finally, the vast amount of work in normative capital theory—for example, the characterization of efficient paths and the analysis of optimal growth paths—is completely ignored. Even if one doubts the validity of the neoclassical models as a description of how competitive economies develop, the models may prove to be of some use in planned economies.

It is difficult to leave this subject without asking, How can we explain the Cambridge-Cambridge controversy? This is a question which a sociologist of knowledge might perhaps be in the best position to answer. Although "personalities" clearly have something to do with the dispute, the fact that the controversy has received such widespread interest suggests that there is more to it than just that. I suggest that there are four reasons—all well illustrated in this book—why a seemingly dry subject like capital theory is the subject of such controversy.

1. Harcourt and the Cambridge (U.K.) economists keep insisting that adherence to the alternative approaches to capital theory is a matter of ideology. As Harcourt puts it: "It is my strong impression that if one were to be told whether an economist was fundamentally sympathetic or hostile to basic capitalist institutions, especially private property and the rights to income streams, or whether he were a hawk or a dove in his views on the Vietnam War, one could predict with a considerable degree of accuracy . . . which side he would be on in the present controversies." Without examining the empirical validity of the statement,[14] one can see that it illustrates the common confusion in the Cambridge (U.K.) approach between correlation and causation. For Harcourt and the Cambridge (U.K.) economists, one cannot separate ideology from economic analysis, just as one cannot separate the determinants of the rewards to factors from the analysis of the personal distribution of income

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[13] For instance, the differences in stability properties of the Harrod and Solow models and the questions of stability of economies with heterogeneous capital goods (see Hahn 1966) are never discussed. The question of whether "savings determines investment or investment determines savings" is also a question which cannot be properly analyzed in a steady-state model with full employment, for in that context, an increase in savings must necessarily be associated with an increase in investment.

[14] As I note below, one of the chief characteristics of the Cambridge (U.K.) "methodology" is a heavy reliance on "casual empiricism" and a reluctance to subject alternative hypotheses to statistical verification.

and the patterns of expenditure of that income. Many other economists would argue that ideology plays a far less important role in their economic analysis than Harcourt would suggest. It may have some limited influence on the questions we ask, but not on how we go about answering them; for instance, the F-test is not ideological, nor is the logical consistency of the assumptions of neoclassical analysis an ideological issue.

2. There has been a remarkable absence of an attempt at empirical verification of any of the underlying hypotheses at any but the most casual level by advocates of the Cambridge (U.K.) approach. Certainly some of the underlying hypotheses should be testable (e.g., the alternative-savings hypotheses). In fact, on many of the issues, the economic data we have may not be adequate to say that one approach is much better than another; yet a proper account of the alternative approaches would have made clear what tests should be performed.

3. What makes the matter more difficult is not only the absence of any attempt at empirical verification with any but the crudest of "casual empiricism" but a fundamental misunderstanding of the role of economic theory. A model which is appropriate for illuminating one class of problems may not be adequate for illuminating another: we know that Newtonian physics is only a first approximation; that gravity does not quite vary with the square of the distance, etc. Yet for most of the questions we wish to discuss in elementary physics courses, it is perfectly adequate. Similarly, an aggregative growth model may do reasonably well in explaining long-term movements in certain macroeconomic variables.

4. There is a well-known propensity of individuals to dislike what they don't or can't understand. This book, as well as the writings of the other Cambridge economists, makes perfectly clear that they do not understand neoclassical capital theory. A modern treatise on capital theory which makes no reference to Malinvaud, to Debreu, or to Samuelson's consumption loan model is indeed striking. To a neoclassical economists (corn, money, . . .) interest rates are just intertemporal prices. If p_c is the price today of corn delivered at time $t + 1$, then the corn interest rate is just $[p_c(t + 1)]/[p_c(t)] - 1$. The relationship between prices (interest rates) and marginal products, in a competitive economy, is fundamentally the same in the capital theoretic models as it is in the static models. One can object to the competitive assumption, one can say that firms use markup pricing (without explaining how the markups are determined, except vague references to competitive forces), but these are not controversies in capital theory. Indeed, most economists would argue that the competitive profit-maximizing model is a more appropriate description for the behavior of the economy in the long run than it is in the short run; that is, there is more competition in the long run than in the short, and in the long run, firms that happen to maximize profits will

do better than firms that don't (the Schumpeterian approach). Thus, the appropriateness of the marginal-productivity theory as a theory of distribution of income among factors is completely unrelated to any of the controversies concerning double switching, savings behavior, or the aggregation of capital.

(There is one important difference between the "static" model and the intertemporal model rising from the almost complete absence of futures markets. This means that an analysis of expectation formation is crucial in the description of dynamic economies. To their credit, the Cambridge [U.K.] economists have stressed the role of expectations and stochastic disturbances, but unfortunately the role of expectation formation is obscured by focusing on "Golden Ages" [steady-state analysis] as the Cambridge economists—as well as most others—have done.)

Yet it appears that it is the confused attempt to discredit the marginal-productivity interpretation of the interest rate which imbues the topics of capital theory with their ideological interest to the devotees of Cambridge (U.K.) doctrine.

Even though Harcourt's book will not lead to a resolution of the issues, or even to a resolution of what are the real issues, by setting out in perhaps as complete and intelligible a manner as is possible one side's view of the debate, Harcourt may have performed a service.

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