NEOClassical THEORIES

On the whole, it seems to me that, having modified Wicksell’s treatment of Åkerman’s problem in order to avoid its implausible and unfounded implications, one might as well drop Wicksell’s model altogether unless one is particularly concerned with questions of variable durability (see Solow 1961). If one still wants to make use of a two-sector Austrian model, one can, I think, more profitably employ Lange’s work. While seeking better theoretical tools, economists in need of a good two-sector model for investigations of the effects of different durability of one’s capital stock may still find Wicksell useful. For other investigations, they are well-advised to pass over Wicksell’s treatment of Åkerman’s problem and turn instead to Lange’s simpler and more versatile presentation.

Table 3.1 Simulation of the Modified Wicksellian Model

<table>
<thead>
<tr>
<th>( t^* )</th>
<th>( r )</th>
<th>( L )</th>
<th>( L' )</th>
<th>( a[(k)^{t'}] )</th>
<th>( w[(k)^{t'}] )</th>
<th>( b[(k)^{t'}] )</th>
<th>( R[(k)^{t'}] )</th>
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<td>400</td>
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<td>.177</td>
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<td>546</td>
<td>454</td>
<td>78,000</td>
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Chapter Four

The Cambridge Criticisms of Neoclassical Capital Theory

The Austrian theory may be ingenious, but is it true? Can one, for example, explain in part why real wages increased steadily in the United States from the end of World War II until the late 1960s by pointing to an increase in the average period of production? Is the rate of interest determined by preferences for present consumption and the larger productivity of more time-intensive production processes? Apart from illustrative special cases, we have not yet seen any evidence. Most orthodox economists believe that there is something to the story the Austrians tell about the relations between time, interest, and capital. Theorists have from time to time articulated and defended certain aspects of the theory.¹ Yet there is little evidence for its truth, and many distinguished neoclassical economists have criticized it harshly.²

Before turning to general equilibrium models and the alternative neoclassical approach to capital and interest, I shall consider some recent criticisms of explicit theories of capital and interest like Clark’s or the Austrian theory. The issues I shall discuss are the subject matter of a now dead controversy between critics of neoclassical theory (most of whom were associated with Cambridge University) and defenders (whose chief spokespersons were associated with MIT). Thus the controversy was appropriately labeled “The Cambridge Controversy.” The main points in the controversy were settled by 1966 with the critics carrying the day, although the significance of the criticisms is still disputed.³

¹ For example Kaldor (1937), Hayek (1941), Dorfman (1959; 1959–60), Solow (1961), and Hicks (1973).
² Especially Knight (1936–37, 1938). Hayek (1941) is as much a critique as a development of the Austrian theory. Metzler (1950) and Lerner (1953) are sympathetic critics.
³ The literature on the Cambridge Controversy is by now immense. Listed below are those contributions that are most interesting or important with respect to the issues that
Beginning with Robinson (1953–54), a number of economists, most of whom have had some association with Cambridge University, have enunciated criticisms of neo-classical theories of capital, interest and economic growth. I shall not consider the theory of growth. This restriction conceals some of the motivation for economists’ concern with production functions and the theory of capital and interest, but it will not distort the theoretical issues. The Cambridge economists do not espouse a uniform doctrine. Neither do they all criticize the same aspects of neoclassical theory. Nevertheless, it is fair to identify them as a school, since they make use of similar analytical techniques and since almost all look to Marx and Ricardo for inspiration.

Some of the Cambridge critics believe that their work reveals the bankruptcy of the whole neoclassical approach to economic theory. Most neoclassical theorists believe, on the other hand, that the Cambridge critics have mistaken the inadequacies of certain parables or simplifications for flaws in fundamental theory. If the critics are right, they have largely overturned established economic theory. If the defenders are right, the critics have merely seized on superficial difficulties. To assess the Cambridge controversy, several different questions must be addressed. In this chapter I shall ask, “What flaws in explicit neoclassical theories of capital and interest like the Austrian theory have the Cambridge critics revealed?” In Chapters 5 and 6 I shall consider how theories like the Austrian theory fit into neoclassical theorizing and whether other kinds of neoclassical theory avoid the Cambridge criticisms. Showing that these other (general equilibrium) theories avoid the Cambridge criticisms does not, however, close the discussion, since general equilibrium theories have their own problems. The critics, whose work I consider in this chapter, look to Sraffa’s work as a foundation for their own. This foundation will be developed and assessed in chapters 8 and 9. Only at the end of this book will we be fully able to judge the force of the Cambridge criticisms.


The principal economists are Robinson, Pasinetti, Garegnani, Kaldor, Nuti, Harcourt, Dobbs, Eatwell, Bhaduri and Nell.

1. Outline of the Criticisms

The Cambridge critics have been dissatisfied with many different aspects of neoclassical economic theory. Robinson (1953–54) accused neoclassical theorists of misusing stationary equilibrium analysis. Her criticism was cogent and forceful. Yet few other participants in the Cambridge Controversy have been concerned with whether economists should compare stationary states. Apart from growth theory, there have, I think, been three major objects of criticism: (1) neoclassical applications of aggregate production functions, (2) neoclassical theories of the relations between capital, interest, and exchange values, and (3) neoclassical theories of the distribution of income. These three objects are not completely distinct, but the division helps in deciding where the various Cambridge critics are aiming their critical arrows and whether they hit their mark.6 One warning to avoid later misunderstanding: the controversy has focused on extremely unrealistic comparisons of different stationary states in which the production possibilities are identical, but in which, because of the differences in wages, different production processes or techniques are employed. The special case models discussed are of no practical interest. They are only important for the light they shed on economic theory.

Much of the Cambridge criticism has been directed to the use of aggregate production functions in empirical and historical studies. I have already referred to Solow’s use of such a function (1957:312–30). In an aggregate production function total output is regarded as a function of a few aggregate variables, such as land, labor, capital, or time. Robinson (1953–54:81, 90) and Pasinetti (1969) object, however, that factors of production are not easily substitutable for one another and that the proportions in which factors are available are less important in explaining output or the distribution of income than neoclassical economists suggest. One cannot use less of some malleable stuff called “capital” in production and compensate by employing more of some homogeneous stuff called “labor.” Changes in factor proportions are changes in the technology employed. They are expensive, time-consuming, and have broad economic effects. One can provide simple hypothetical examples in which such discreteness of technologies limits the validity of neoclassical results (Pasinetti 1969:520–23).

The Cambridge economists have also objected explicitly to including

6 In this chapter I shall not discuss separately criticisms of neoclassical theories of the distribution of income. Some of these criticisms follow directly from points I shall discuss. Others rely on Sraffa’s work and are best considered later in Chapters 8 and 9.
the quantity of capital as an argument in an aggregate production function. In denying that output should be regarded as a function of the quantity of capital, where the quantity of capital is a quantity of value, the Cambridge critics echo Böhm-Bawerk, Wickell, and Lange. Capital is not an input into production. Differentiating an aggregate production function with respect to the quantity of capital does not tell one what the marginal contribution of capital to production is, since capital makes no contribution at all. The apparent marginal product does not equal the rate of interest and does not help one to understand the distribution of income. On all these points the Cambridge theorists are at one with the Austrian theorists. The reasons for these conclusions differ, however. The Cambridge critics agree that capital is not an input into production whose cost is interest, because they do not believe that there are any inputs into production apart from various resources, labor, and capital goods and because they believe that interest is not a cost at all. They are thus no better disposed to an Austrian aggregate production function with time as a variable than they are to more common aggregate production functions. Austrian aggregate production functions are seldom encountered and rarely discussed, because they are not suitable for most empirical studies.

Objections to aggregate production functions thus lead directly to objections to neoclassical theories of the relations between capital, interest, and exchange values. If the Cambridge criticisms were only directed to particular aggregate production functions or with production functions which involve smooth substitutability, the criticisms would not strike deeply at neoclassical economic theory. The aggregate production function is at best a useful device to answer empirical questions. Contemporary theory can dispense with continuous differentiable production functions. Modern linear programming techniques can even replace such functions in practice. To explain interest as the cost of waiting or of capital does not require that one make use of any aggregate production functions.

Criticisms of neoclassical accounts of the relations between capital, interest, and exchange values go deeper. Both Clark's theory and the Austrian theory assert that interest is the return to a scarce element in production. Expanding on a curiosity noticed by Ruth Cohen in the early 1950s and discussed by Sraffa in 1960, the Cambridge critics have provided perfectly plausible special case models with no technological innovations in which the rate of interest—at all but its lowest values—is an increasing function of the value of capital. It seems that without technological change capital can be more abundant and yet command a higher rate of interest. This possibility implies that at least one of the following four claims must be false:

(1) Capital is a factor of production.
(2) The value of capital is a measure of its quantity.
(3) The apparent marginal productivity of capital is a decreasing function of its quantity.
(4) The rate of interest is an increasing function of the marginal productivity of capital.

If all of these claims are true, a higher value of capital must go with a lower rate of interest. Since the Austrian theorists have already challenged (1) and (2) and have shown how (4) could be false, one might wonder whether the Cambridge critics have added anything. If one examines their arguments, however, one finds that they apply equally to the Austrian theory. Indeed, the Cambridge criticisms apply to any theory that associates capital with some single aspect of production. In the next two sections I shall develop these criticisms.

2. The Wage-Profit Frontier

To discuss the Cambridge criticisms of the relations between capital, interest, and exchange values, one needs a further theoretical tool. That tool, the wage-profit frontier, or as Samuelson (1961–62) calls it, "the factor-price frontier," is ingeniously simple (see Bhaduri 1966, 1969). Suppose one has an economy with no unproduced factors of production except homogeneous, fully employed labor. There is thus no rent. One has for a given method of production, some net output, Y, and net income, YpY, which is distributed between wages, wL, and profit, rK, where K is the value of the produced means of production. For simplicity assume that there is no depreciation. Since YpY is net income, it does not particularly matter whether K involves fixed capital, circulating capital, or both. Since income is divided between wages and profits,

\[ Yp_Y = rK + Lw \]

where L is the number of workers, which is taken to be a constant. Dividing through by L and writing yp_y for Yp_Y/L and k for K/L, one obtains

\[ yp_y = rk + w. \]
Setting \( p_s = 1 \) and rearranging,

\[
(4.3) \quad w = y - rk.
\]

One may then graph (4.3) (see fig. 4.1). One must, however, interpret figure 4.1 and equation (4.3) carefully. They do not depict the true relations between wages and profits. Figure 4.1 represents the tradeoff between wages and interest if the proportions between capital and labor are held constant despite the differences in wages and interest. In reality, unless this is an absolutely fixed-proportions economy, the production process employed will differ depending on wages and interest. If capital and labor are infinitely substitutable for one another in a Clark-like smooth aggregate production function, only one point of the wage-profit line in figure 4.1 will represent an actual combination of wages and interest. If there are a finite number of techniques of production, a segment of that line will be on the wage-profit frontier.\(^6\)

The actual wage-profit relationship (excluding technological changes) will be the outer envelope of such graphs since, for a given wage, entrepreneurs will employ that technique which will maximize profits. Figure 4.1 has some convenient properties. When interest is zero the wage and the per capita net income are equal; \( k = (y - w)/r = \tan a \). If the wage-profit line is a straight line, \( k = -dw/dr \). If \( k = -dw/dr \) at \( (r_1, w_1) \), then the wage-profit line must either be a straight line or it must be tangent at \( (r_1, w_1) \) to a straight line through that point and \( (0, y) \). The importance of these last observations is the following:

\[
(4.4) \quad \frac{dw}{dr} = \frac{dy}{dr} - k - r \frac{dk}{dr} \quad \text{[differentiating (4.3)]}
\]

\[
(4.5) \quad \frac{dk}{dr} = \frac{dy}{dr} \quad \text{if and only if } k = -\frac{dw}{dr}
\]

\[
(4.6) \quad r = \frac{dy}{dk} \quad \text{if and only if } k = -\frac{dw}{dr}
\]

Thus one can say, roughly, that the rate of interest is equal to the apparent marginal product of capital if and only if the wage-profit lines, (which are by definition tangent to the wage-profit frontier at one or more points or coincide with it over some interval) are straight lines.

The economic meaning of a straight wage-profit line is simple: the line will be straight if and only if the value of capital is independent of the distribution of income between wages and interest. In that case the value of capital acts like a physical measure of capital, and one can regard the rate of interest unproblematically as the price of capital. Of course no one believes that when the production process used is held fixed, the value of capital remains the same no matter what the distribution of income is.

Let us now consider the wage-profit frontier (fig. 4.2). Since the frontier is either tangent to wage-profit lines or made up of segments of such lines and since such lines have negative slope, the slope of the frontier is everywhere negative. Both wages and interest can increase simultaneously only through the discovery of some new technique of production. Furthermore at any switchpoint between two techniques,\(^6\)

The wage-profit frontier does not necessarily represent the actual wage-profit tradeoff, because switching techniques will typically bring about or be accompanied by technological innovation, which will cause the frontier to shift. See Hicks (1975:367).
Figure 4.2

\( T_1 \) and \( T_2 \), \( k_1 > k_2 \) if and only if \( y_1 > y_2 \) and \( k_1 > k_2 \) if and only if \( k_1/y_1 > k_2/y_2 \). A higher per-capita value of capital always goes with a higher capital-output ratio and a higher per-capita income. These results match what a Clark-like theory demands. The proof of these claims is simple.

\[
\begin{align*}
\text{(4.7)} & \quad y_1 - rk_1 = y_2 - rk_2 \quad \text{two techniques are equal at a switch point} \\
\text{(4.8)} & \quad y_1 - y_2 = r \cdot (k_1 - k_2) \quad \text{from (4.7)}
\end{align*}
\]

Since \( r \) is positive \( y_1 > y_2 \) if and only if \( k_1 > k_2 \). (4.8) seems to provide a shortcut to a proof that \( r = dy/dk \) at least at all switch points and to contradict the argument that \( r = dy/dk \) if and only if the wage-profit lines have constant slope (Solow 1967:32–33). Although (4.8) is not a trivial result, it only shows that \( r = dy/dk \) if we hold wages and interest (and thus prices) constant. If prices are constant, an increase in the value of capital is truly an increase in the quantity of capital goods and \( dy/dk \) behavoes like a physical rate of return.\(^7\) (4.8) cannot serve as part of a theory of the causal determination of the rate of interest by the marginal productivity of capital; but, as already discussed above, the notion that the marginal productivity of capital \( \text{causally determines} \) the rate of interest is misleading.

Proving that \( k_1 > k_2 \) if and only if \( k_1/y_1 > k_2/y_2 \) is also simple.

\[
\begin{align*}
(4.9) & \quad k_1 \left( \frac{y_1}{k_1} - r \right) = k_2 \left( \frac{y_2}{k_2} - r \right) \quad \text{from 4.7} \\
(4.10) & \quad \frac{k_1}{k_2} = \frac{\frac{y_2}{k_2} - r}{\frac{y_1}{k_1} - r} \\
(4.11) & \quad k_1 > k_2 \quad \text{if and only if} \quad \frac{y_2}{k_2} - r > \frac{y_1}{k_1} - r \\
& \quad k_1 > k_2 \quad \text{if and only if} \quad \frac{y_2}{k_2} > \frac{y_1}{k_1} \\
& \quad k_1 > k_2 \quad \text{if and only if} \quad \frac{k_1}{y_1} > \frac{k_2}{y_2}
\end{align*}
\]

\(^7\) Pasinetti (1969:508–31) makes the same point, although more polemically. He objects to Solow's claim, which is technically correct, that "the interest rate is an accurate measure of the social rate of return to savings" (Solow 1967:30). Because of re-switching, an investment can have more than one rate of return. While this does not falsify Solow's claim, it makes Pasinetti skeptical of the significance of the equality between the rate of return and the constrained derivative of income with respect to the value of capital. See also Solow (1970:423–28) and Pasinetti (1970:428–31). Dixit (1977:20–23) accuses Wickert of a bad confusion in attempting to link what Wickert considered to be the marginal product of capital [see (3.7)] to the rate of interest. Keeping prices constant as in (4.8), the difficulties disappear: "stock appreciation should never be included...in the first place" (p. 20). "Of course we now know how they [the paradoxes] arise from mistaken concepts, and we have seen how the general framework of intertemporal equilibrium explains or resolves them" (pp. 22–23). Although the Austrian theory is unsatisfactory, it is not such a muddle as this, nor is intertemporal equilibrium theory such a successful resolution.
The wage-profit frontier enables one to depict simply the relations among wages, profits, and techniques of production. Thus far we have seen that, unless there is a technological innovation which shifts the frontier, wages and profits are inversely related. At different wage rates different techniques of production will be most profitable. At switch points between different production processes (points where two processes are, at a given wage, equally profitable), the process that has the larger value of capital and the higher capital/output ratio will have the larger output.

3. Reswitching and Capital Reversing

How is this analytical device converted into a tool of criticism? The geometry of wage-profit lines suggests the theoretical possibility of reswitching or double-switching depicted in fig. 4.3. Technique 1 is most profitable both at very high and very low values of the rate of interest, while technique 2 is most profitable at intermediate values. For \( r > r' \) technique 1 will be in use. For \( r'' < r < r' \) technique 2 will have the highest wage for a given rate of interest or the highest rate of interest for a given wage. At still lower interest rates it is profitable to switch from technique 2 back to technique 1. In itself the possibility of reswitching is not very important. What is important is what happens at point B: \( (r', w') \) on the graph. Remember that nothing is actually happening. We are only comparing stationary states. What is peculiar about figure 4.3 is that for \( r'' < r < r' \) the technique chosen has a lower per capita value of capital, a lower per capita income (consumption), and a lower capital/output ratio than does the technique in use for \( r > r' \). When technique 2 is employed, there is less capital than when technique 1 is (unless the value of capital is not a measure of its quantity), yet the rate of interest is lower than it is in some equilibrium states in which technique 1 is employed. This phenomenon is sometimes called "capital reversing." The "normal" inverse relation between the rate of interest and the quantity (value?) of capital is reversed. Reswitching is sufficient for capital reversing, but not necessary, as fig. 4.4 illustrates.

In fig. 4.4 there is no reswitching. Each technique is most profitable for only one range of wages. Yet the switch from technique 3 to technique 2 is a capital reversing switch. Technique 2 is employed at rates of interest lower than those at which technique 3 is employed, yet it has a lower per capita output and a lower value of capital; \( k_2 \), the magnitude of the slope of the straight line from \( (0, y_2) \) to \( B \), is less than \( k_3 \), the magnitude of the slope of the straight line from \( (0, y_3) \) to \( B \). The only limit on the geometry of such graphs places on capital reversing is that the switch to the technique employed at the lowest rates of interest cannot be capital reversing.
One can also give algebraic models of reswitching. Bliss provides the following simple one (1975:91). One is comparing stationary states which employ respectively the following techniques:

- **I**: $1L$ and $2x$ make $5x$
- **II**: Process 1: $1L$ and $1m$ make $5x$ and $1m$
  Process 2: $4L$ make $1m$.

From these equations, setting $p_x = 1$, one can easily calculate the two wage-profit lines: $w_1 = (3 - 2r_1)/(1 + r_1)$ and $w_2 = 5/(1 + 4r_2) \cdot (1 + r_2)$. The two curves intersect at $(.25, 2)$ and $(1, \frac{5}{3})^9$ and their graph resembles figure 4.3, except that for Process II $r$ is not bounded. There is nothing less plausible about algebraic examples of reswitching and capital reversing than there is about the sort of model that Wicksell or Lange present and discuss.

Precisely the same sort of algebraic hypothetical counterexample can be posed to the Austrian theory. Suppose that we have two methods of producing some consumption good, $x$. Let $t$ be the time when $x$ is available for consumption. One method requires the application of 3 units of labor at $t - 11$ and 10 units of labor at $t - 1$. The other requires the application of 10 units of labor at $t - 6$ and 2 units of labor at $t - 1$. Setting $p_x = 1$, the equations of the two wage profit lines are $w_1 = 1/(3(1 + r)^{11} + 10(1 + r))$; $w_2 = 1/[10(1 + r)^6 + 2(1 + r)]$. The second technique will produce $x$ at a lower cost (and thus be the technique with the higher wage or higher rate of interest for $r < 6\%$ or $r > 15\%$).\textsuperscript{10}

\textsuperscript{9} Bliss miscalculates the intersections.

\textsuperscript{10} Calculations for the Austrian reswitching model are as follows:

- **technique 1**: $p_1 = w_1[3(1 + r_1)^{11} + 10(1 + r_1)]$
- **technique 2**: $p_2 = w_2[10(1 + r_2)^6 + 2(1 + r_2)]$

at the switch points: $w_1 = w_2; r_1 = r_2$. Thus

$$w[3(1 + r)^{11} + 10(1 + r)] = w[10(1 + r)^6 + 2(1 + r)].$$

Assuming that $r \neq -1$, one gets $3(1 + r)^{10} - 10(1 + r)^5 + 8 = 0$.

$$ (1 + r)^5 = 4/3 \quad (1 + r)^5 = 2 $$

$$ r \approx 5.97\% \quad r \approx 14.9\% $$
Böhm-Bawerk suggests that we calculate the average period of production, $T$, as follows:\footnote{11}  
\[ T = \frac{\sum_{j=1}^{n} L_j f_j}{\sum_{j=1}^{n} L_j} \]

where $L_j$ is the point input of labor in the period $t_j$ periods before the one in which the output is available. $L_0$ is the most ancient labor input. If one applies this formula to the given techniques, one finds that $T_1 = (33 + 10)/13 = 43/13$. $T_2 = 62/12$. The second technique has the longer average period of production. Stationary equilibrium states for a rate of interest between 6% and 15% compared with stationary equilibrium states for $r > 15\%$ have a lower rate of interest, a higher wage, and a less roundabout method of production. Either the marginal contribution of waiting is larger when more roundabout processes are already employed, or the rate of interest is not the price of waiting.

We can also calculate the value of capital.

\[ K_1 = 3 w_1 \left[ \sum_{i=1}^{11} (1 + r_1)^{j-1} \right] + 10 w_1 \]

\[ K_2 = 10 w_2 \left[ \sum_{i=1}^{6} (1 + r_2)^{j-1} \right] + 2 w_2. \]

$K_2$ is larger than $K_1$ at both switch points. The switch from technique 2 to technique 1 at $r = 15\%$ is a capital reversing switch. The re-switching cannot be explained in terms of a reversal of the capital intensities of the two techniques at different interest rates.\footnote{12}

If one modifies Böhm-Bawerk’s measure of the average period of production, as some authors have suggested, to take account of compounding of the effects of roundaboutness, the period of production for process 2 remains longer than the period of production for process 1.\footnote{13} Modifying Böhm-Bawerk’s measure of the average period of production for process 2 remains longer than the period of production for process 1,\footnote{13} There are possibilities for disagreement concerning how to calculate the average period of production in a way which takes account of the compounding of roundaboutness. See Kuenne (1971:69) for a simple exposition. The most reasonable formula seems to me the following:

\[ T = \sum_{j=1}^{k} L_j \left[ \sum_{i=1}^{m} (1 + r)^{j-i-1} \right] \sum_{j=1}^{k} L_j \]

$k$ is the number of different point inputs of labor; $m$ is the number of periods before the output is available that the labor input $L_i$ is added. Applying this formula,

\[ T_1 = \left[ \frac{3 \left( \sum_{i=1}^{11} (1 + r_1)^{j-1} \right) + 10}{13} \right] \]

and

\[ T_2 = \left[ \frac{10 \left( \sum_{i=1}^{6} (1 + r_2)^{j-1} \right) + 2}{12} \right]. \]

For $r_1$ and $r_2 = 0$, one gets the same values as from the simpler formula that ignores interest. For $r = 5.9\%$, $T_1 = 4.2$ and $T_2 = 6$. For $r = 14.9\%$, $T_1 = 6.4$ and $T_2 = 7.4$.

Leland Yeager (1976:328–29) shows that re-switching need not always establish capital reversing, if one is willing to calculate the average period of investment in a particular way. He discusses a simple model due to Samuelson in which there are two techniques: $A$, employed at low and high rates of interest and $B$ employed at intermediate rates of interest. Let $r_1$ and $r_2$ be the rates of interest at the two switch points with $r_1 < r_2$. For $r < r_2$, the average period of investment of $B$, calculated in Yeager’s way, is less than that of $A$, but for $r > r_2$, $A$ becomes the less time intensive technique. Yeager finds this result comforting, “Preconceived insistence on measuring all factor quantities in purely physical terms clashes with the fact of reality—or, of arithmetic—that the amount of waiting, requiring in accomplishing a physically specified purpose depends on its own price.” (p. 337). I find Yeager’s conclusion unpalatable and, in any case, mistaken.

Yeager calculates the average period of investment of labor and waiting as follows:

\[ T = \frac{\sum_{i=1}^{k} L_i \left[ \sum_{j=0}^{m} (a_i - f_j)^{j-1} \right]}{\sum_{j=1}^{k} L_j \left[ \sum_{j=0}^{m} r_j \right]} \]
duction in this way makes that period a function of the rate of interest. One then wonders what role the average period of production is supposed to play in an explanation of the rate of interest.

In this algebraic special case model the fundamental claims of the Austrian theory do not hold. Contrary to (3.26) the rate of interest is not always smaller when the time-intensity of production is larger. If the quantity of waiting or the average period of production measures the increased productivity of the stored up services of land and labor (3.23) and the marginal productivity of such storing up is a decreasing function of its quantity (3.21), and interest is the cost of this storing up or waiting (3.24), then it cannot be the case that a lower rate of interest goes with a shorter average period of production. Contrary to (3.27), wages are also not always larger when the roundaboutness of production is greater. The Cambridge critics have produced a story that falsifies any capital theory which takes interest to be the cost of some input which yields diminishing returns.

4. Interpretation and Tentative Conclusions

The Cambridge critics have presented simple special case models in which certain techniques employ more roundabout methods of production and have a higher value of capital yet have a higher rate of interest than do others. If interest increases with the marginal productivity of "capital," and the marginal productivity declines with the quantity of "capital," there should be an inverse relation between the rate of interest and the period of production or the value of capital. The Cambridge critics have thus presented simple hypothetical cases in which things do not work as neoclassical capital theories say they should.

What should one conclude? The possibilities explored by the critics show that the Austrians' claims are not necessary; but mere possibilities will never prove that those claims are false. Surely one can argue

where \( k \) is the number of different labor inputs and \( n_i \) is the number of periods before the output is available so that the input \( L_i \) is added. For the example I have been considering in the text, one finds that \( T_1 = \left(3\sum_{i=0}^{n} (1 - j r^i) \right) + 10 + 10 \sum_{i=0}^{n} k_i r^i \). For \( r = .15, 10, 3.4, 13.15 \). For \( r = .15, 3.4, 13.15 \). The capital reversing does not disappear. Whatever the merits of Yeager's aspirations and of his method of calculating the period of investment, he has found no general answer to the problem of capital reversing. Comparing stationary equilibria with \( r > 14.9\% \) to stationary equilibria with \( 5.9\% < r < 14.9\% \) one finds that with a higher rate of interest a more time intensive productive technique is employed.

that, until the Cambridge critics have shown that the peculiarities of the special case models are true of the real world, they have not revealed any flaw in neoclassical capital theories. Although there is some justice in this objection, I do not think that the Cambridge criticism can be so easily dismissed. The Cambridge critics have not shown that capital reversing is a real phenomenon, but neither have neoclassical economists given us reason to believe that there is usually an inverse relation between the rate of interest and the quantity of capital. Measuring the various theoretically relevant quantities, particularly the time-intensity of production, is impractical, perhaps impossible. There is no experimental way to know whether the anomalous phenomena occur. Bruno et al. have investigated the conditions which logically rule out reswitching and have shown that they are so strong that no real economy is likely to meet them (1966, esp. pp. 543-46). The conditions which rule out capital reversing are even stronger: The Cambridge critics have not merely presented some strange possible cases; they have revealed that the neoclassical theorist has no good reason to believe what traditional capital theories assert.

There is a second consideration that reinforces the importance of the mere possibilities the Cambridge critics have discussed. The Austrian theory was not intended as a calculating device which enables economists to make predictions about the behavior of certain economic quantities when they have measured others, but was supposed to explain what capital and interest are and, to some extent, to justify the existence of interest. Although the normative and the descriptive issues are distinct, they are explicitly related. Böhm-Bawerk asserts, "In the essence of interest, then, there is nothing which should make it appear in itself unreasonable or unjust" (1888:360). If one cannot discount the possibilities revealed in the counter-models or explain them away in terms of Austrian concepts of capital and interest, one cannot claim to have explained what capital and interest are or to have justified the earning of interest.

Some neoclassical economists have argued that, even if capital reversing does occur, it is no more important to capital and interest theory than are the existence of Giffen goods and upward-sloping demand curves to the theory of demand (Blaug 1975:42-43; Stiglitz 1974:896). This argument is a mistake. It is fair to say that rough supply and demand theory states that demand curves are downward sloping, just as the Austrian theory of capital and interest states that interest is a decreasing function of the quantity of "waiting." Thus far the analogy carries one. There are, however, three important conditions that are not analogous. Demand theorists know there are few Giffen
goods. They know why there are Giffen goods. They can successfully predict that certain goods in certain economies (potatoes in Ireland, rice in China, or yams in New Guinea) are likely to be Giffen goods. Capital theorists, on the other hand, do not know whether capital reversing is common or rare. Until recently they possessed no theory which made sense of the phenomenon. The status of that fundamental theory remains, moreover, questionable. From the perspective of the Austrian theory or of Clark’s theory, capital reversing is nothing but a disconfirmation. Capital theorists are also unable to predict when capital reversing will occur. They cannot point to some feature of an economy and say, “Ah, we can see that this is one of the exceptional cases in which we should not expect our simpler capital theories to work.” There is no justification for the claim that capital reversing demands only minor qualifications in simplified capital theories.

The criticisms of the Cambridge economists, even though they involve only the construction of special case models, should not be dismissed. Yet the critics have not accomplished nearly so much as some of them think. The Cambridge critics have shown, as Wicksell did, that the value of capital may depend on the distribution of income between wages and interest, and have thus made clear that the quantity of capital, as measured by its value, cannot be regarded as an economic primitive on a par with iron ore deposits. Perhaps, they have thus refuted J. B. Clark. Yet there is little new in this refutation. I know of no theorist of the past generation who has denied that the value of capital might differ with a different distribution of income. The Cambridge critics, on the other hand, have not refuted the Austrian theory. They have revealed no logical inconsistency in it, nor have they shown that there is any error involved in using either Clark’s or the Austrians’ theories in empirical studies like Solow’s. One may lack evidence for the Austrian theory; Clark’s theory may be ultimately incoherent. Yet there is no way to prove, a priori, that nothing can be learned by employing these theories. It is preferable not to base one’s investigations on such shaky foundations, but there may be no better choice. Remember that the Cambridge criticisms do not rule out the possibility that (other things being equal) the rate of interest is in fact always smaller when the value of capital and the roundaboutness of production are larger.

The important conclusion established by the Cambridge critics is that the Austrian theory of capital is unfounded. One has no reason to believe the claims of the theory. Whatever good reasons we have to accept other neoclassical theories, those reasons do not support the Austrian theory. Since economists have no reason to believe the Austrian theory, they have no reason to believe that it explains the phenomena of capital and interest. If orthodox economists have nothing better than the Austrian theory, they cannot justifiably assert that they know why there are profits or what determines whether profits are, on average, large or small. The Cambridge critics thus raise serious questions about the ability of neoclassical economics to deal with the phenomena of capital and interest.