seems to be saying that interest is determined by the marginal product of capital; but, as mentioned previously, it is misleading to say that the price of any commodity or factor of production is determined by its marginal utility or its marginal product. From the perspective of general equilibrium theory, prices are determined by all of the given of the system of equations. This criticism, however, misses the central flaw in Clark's proposal. He is suggesting in effect that capital, as a "fund of productive wealth," be included among the constraints on the equilibrium solution. The endowment of capital (since it is measured as a quantity of value) is, however, itself a function of the exchange values for which economists attempt to solve. If one takes capital to be a fund of "productive wealth," the explanation of interest as the return to capital cannot take the same form as the general explanation of the return to factors of production. The problems of incorporating capital and interest into neoclassical value theory remain.

Unless neoclassical economists can find some way of incorporating the phenomenon of interest into the constrained balancing of marginal utility theory of exchange value, that theory of value cannot be correct. The viability of the fundamental structure of neoclassical economics depends on the success of neoclassical capital theory. Neoclassical economists must explain what capital is and how the quantity and rate of interest are determined. Perhaps the only sensible theory of exchange value is based on physical cost and distribution between wages and profits. Perhaps the whole project of explaining exchange values on the basis of constrained preferences is a mistake. The project cannot succeed until the puzzles of capital and interest are solved.

The viability of the opposing theory of value based on physical cost and distribution depends on the demonstration that physical cost and distribution can determine exchange value without bringing in utilities. Whether interest is a cost to be explained in terms of some disutility or whether interest is merely a portion of the social surplus apparently has implications for both positive and normative theory. Notice that it has not yet been established that these supposedly alternative theories of value exclude one another; perhaps interest can be regarded as both a cost in terms of a balancing of marginal utilities theory and as a portion of the social surplus in terms of a physical cost cum distribution theory.

Capital theory thus rests at the foundations of economic theory. Let us now examine and assess specific neoclassical attempts to deal with the problems that capital and interest pose.
not turn to general equilibrium theories. Instead, he worked with an aggregate production function, in which the inputs were simply capital measured by its value and labor. Interest and wages were set equal to the respective marginal products. There are many serious economic questions which theorists cannot consider unless they employ such simplified models. Although simplified theories which link capital with an aspect of production face many difficulties, I shall argue that such theories may nevertheless be informative. Even though most contemporary theorists regard the general equilibrium approach as more rigorous and fundamental, we should not simply pass over more traditional theories of capital and interest.

The traditional approach is to associate capital with some feature of production and to regard the rate of interest as the price of this feature. There are several ways to carry out this idea. I have already mentioned that J. B. Clark regarded capital as a permanent “fund” of productive wealth. Clark hoped to treat capital as an input, in many ways on a par with lathes or steam engines. Clark’s program requires that one measure this input, discover what its role in production is, and investigate its marginal productivity. The theorist should be able to explain why the rate of interest is positive by pointing to the scarcity of this factor of production (or its marginal productivity).

To speak of explaining interest in terms of the marginal productivity of capital is somewhat confused. As I have explained, marginal productivities or relative scarcities are not given to an equilibrium system, but are calculable within such systems (see Bliss 1975, pp. 33-77 and ch. 5). Clark’s work is confused, but, as I argued before, the principal difficulty does not lie in the attempt to explain wages or interest in terms of marginal productivity. Clark seeks to add another factor of production, capital, and another price, interest, to some equilibrium system. Charitably interpreted, Clark’s claim that interest is determined by marginal productivity asserts only that the initial endowment of capital (relative to that of labor or land) and the known technology are crucial factors influencing the rate of interest. Even though the rate of interest is a function of all the unknowns, it is not equally sensitive to all of them. It is reasonable to focus on particular factors.

Having said this much for Clark, his theory of capital and interest remains unsatisfactory. One cannot coherently introduce as a given into an equilibrium system a quantity of capital measured as a quantity of value. Such a quantity will obviously depend on what the equilibrium prices are. Even if the account were not thus incoherent, it would be mysterious. What sort of an input into production is a “permanent fund of productive wealth”?

One might indeed question not simply Clark’s particular theory, but any attempt to associate capital with some single feature of production. What Clark and others who have proposed simplified capital theories have attempted resembles partial equilibrium analysis. Just as one isolates for the purposes of analysis the market for shoeaces and considers demand and supply for shoeaces as (at least as a first approximation) functions only of the price of shoeaces, so one might isolate the market for “capital” and attempt to consider separately the supply and demand for capital. One has made a strong argument that interest can be explained in the same way as prices are, if the partial equilibrium tools apply to capital and interest in the same way that they apply to any other commodity and its price. The capital theorist wants, in Böhm-Bawerk’s words, to show that “The exchange...in which interest has its origin, is only a special case of the exchange of goods in general. It goes, then, without saying that the formation of price in this case is subject to the same laws as [sic] govern the formation of price in economical exchange generally” (1888:375).

Such a project seems dubious, however. The market for shoeaces is relatively isolated. A shift toward a method of producing shoeaces that requires more synthetic materials and less cotton has limited consequences for the rest of the economy. A change in the price of shoeaces is unlikely to have appreciable ramifications in many other markets. A general shift toward more capital-intensive methods of production has, on the other hand, broad repercussions and reflects general economic changes.

Despite these apparent difficulties, many neoclassical economists have attempted to give such simplified theories of capital and interest. I shall consider in some detail one particularly intelligible effort, the “Austrian” theory of capital and interest, so-called, because it was largely developed by Austrian theorists, particularly Böhm-Bawerk (1888). See also (Menger 1871, 1:4 and 3: 3, 8) and (Jevons 1871, ch. 7). The Austrian theory is more sophisticated than Clark’s work and, indeed, functions as a critique of Clark. It avoids introducing a mysterious physical something which functions as an input into production in addition to capital goods.

1. Capital and Waiting

One way of incorporating capital into neoclassical theory is to say, as Clark did, that capital earns interest because it is scarce and has a positive marginal product. To the Austrian theorists such a claim
seemed naive and unhelpful. According to them, to possess capital is simply to own, and thus to control, productive goods. Capital is not itself some productive stuff. Furthermore, regarding capital as an input leaves the crucial questions unanswered. Both capital and capital goods are augmentable. Why doesn't the quantity of capital increase to the point where profits are zero? Knut Wicksell, whose exposition I shall follow for the most part, objected that we cannot talk of the scarcity of capital without specifying the units in which we measure capital. The obvious choice of value units seems unsatisfactory. How could the scarcity of capital be a scarcity of a quantity of value? Surely competition would simply bid up the value of capital until it was no longer scarce (Wicksell 1911:146). Something physical and real must be involved.

According to Böhm-Bawerk and Wicksell, the real physical something involved is time. Capital goods can "be ultimately resolved into labor and land" (Wicksell 1911:149). Saving up labor and land and applying them indirectly increases their productivity. Capital is not itself a factor of production. Rather "capital is saved-up labor and saved-up land. Interest is the difference between the marginal productivity of saved-up labor and land of current labor and land" (Wicksell 1911:154). This difference shows the role of time or "waiting." "Waiting" is both deferring consumption and applying resources to activities that require longer intervals of time to produce consumption goods. Time or waiting makes it possible to apply land and labor in increasingly indirect or roundabout and thus (the crucial step) more productive ways (Böhm-Bawerk 1888:82–87). Interest is not the marginal product of capital. Capital is not an input and has no marginal product. Interest is a portion of the increased output that waiting makes possible paid to those who own the resources which permit the waiting. Indeed interest might be said to be the marginal product of time or waiting.¹

As accumulation proceeds, more and more roundabout processes will be employed. The marginal contribution of waiting will decline. Waiting will no longer be so scarce. The role of time in production apparently provides a unified demystified treatment of capital and interest.

Time has a second important relationship to interest, which was

¹ Böhm-Bawerk would be hesitant about this particular formulation, since he objects to theories, like Nassau Senior's which regard the capitalists as performing a sacrifice which contributes to production when they abstain from consuming their wealth (1888:123).
tivity of more roundabout production processes with longer periods of production. We have a sketch of an intelligible theory of capital and interest.

2. Wine and Grape Juice

The above comments provide only a fragmentary sketch of a theory of capital and interest. Much more is needed. How is apparent capital accumulation related to the time-intensity of production? How can one measure the time-intensity of production? How can one find out whether the value of capital and the length of the average period of production do generally increase together? How can one test the claims that with increased roundaboutness come higher returns? How do market forces relate the rate of interest to the period of production?

Wicksell does surprisingly little to answer these questions. His presentation of the Austrian theory takes a different course. After merely sketching the general claims (1911:162–63, 184) he turns immediately to the construction of models of hypothetical simplified economies. He then employs these models to illustrate, develop, and vindicate his general claims concerning capital and interest.

Wicksell’s discussion of simplified hypothetical economics raises difficult philosophical questions. What roles can discussions of hypothetical cases play in science? Can such discussions serve as evidence for or against theories? What structure do such “models” have? These questions are important ones in the philosophy of economics, because a large part of theoretical economics employs similar simplified models.

Let us begin by examining Wicksell’s simplest model (1911:178–81). The notation used here is mine, not Wicksell’s. One has an industry or a sector of an economy in which firms buy grape juice and store it as aging wine. The industry is competitive, and each firm attempts to maximize its profits. Since there are many firms, none can influence the price of grape juice or the rate of interest. To simplify my discussion, I shall assume (unnecessarily) that each firm buys a fixed amount of grape juice. The value of the stored wine (x) is a given increasing function of its age. The only choice a firm has is how long to store the wine.

There is thus a point input of grape juice of value \( w \), which the firm takes as fixed. The task is to determine the equilibrium state of such a wine sector. In the equilibrium state there will be equal quantities of wine stored of every age up to and including the equilibrium period of storage, \( t^* \). Notice that this model is of an economy in stationary equilibrium. Wicksell talks of “changes” and discusses the derivative of, for example, the value of grape juice with respect to equilibrium time of storage. This talk can be confusing. One is considering stationary equilibria only and is unable to say anything about changes. All Wicksell is doing is comparing stationary states. As Joan Robinson points out (1953–54:81–106), confusion on this point is one of the pitfalls of stationary equilibrium models.

The profit of any firm, \( P \), is the value of the wine sold, \( x \), minus the cost of the grape juice, \( w(1 + r)^t \), where \( r \) is the rate of interest and \( t \) is the period of storage. The compound interest factor, \( (1 + r)^t \), must be included because during the time period involved firms could have been earning interest on the money they invested in buying grape juice. If one allows a period to be extremely short one can approximate \( (1 + r)^t \) by \( e^{rt} \) where \( r \) is now the momentary rate of interest and \( e \) is the base for the natural logarithms. Thus one has:

\[
(3.1) \quad P = x - we^{rt}
\]

Since this is a perfectly competitive sector with no uncertainties or risks, there can be no profits in equilibrium. Another way to say the same thing is that each firm will earn only the normal rate of profit, which in this special case is equal to the rate interest on a secure loan. Wicksell treats “normal profits” as interest. There must be no other profit. Hence, for the equilibrium storage period \( t^* \) and the equilibrium value of wine \( x^* \):

\[
(3.2) \quad x^* = we^{rt^*}
\]

Each firm attempts to maximize profits. Taking the derivative of (3.1) with respect to \( t \) and setting it equal to zero (with \( w, r \) as constants) one gets in equilibrium

\[
(3.3) \quad r = \frac{1}{x^*} \left( \frac{dx}{dt} \right)^*
\]

The rate of interest is equal to the proportional increase (in the neighborhood of the equilibrium storage period) of the value of wine with respect to time. (Compare Jevons 1871:241.) One cannot know whether (3.3) gives a condition for the maximization of profits until one examines the second derivative of (3.1). Wicksell specifies that the second derivative of \( x - we^{rt} \) is everywhere less than zero:
(3.4) \[ \frac{d^2x}{dt^2} - \frac{1}{x} \left( \frac{dx}{dt} \right)^2 < 0 \]

This is a reasonable stipulation. All that Wicksell requires is that the value of wine increase less than geometrically with its age. One can thus reasonably expect that profits can be maximized. Letting \( k \) be the equilibrium value of capital, one gets finally:

(3.5) \[ k = w \int_0^t e^{rt} \, dt = \frac{x^* - w}{r} \]

(3.6) \[ x^* = rk + w \]

(3.6) says that the value of the wine is equal to the payments for the grape juice plus the interest payments.

There are five variables in the model: \( x^* \), \( w \), \( r \), \( t^* \), and \( k \), but only three independent relations besides the unspecified equation relating the value of the wine to its age. There is thus a degree of freedom and no account of what determines the rate of interest. J. Hirschleifer suggests that the system should be closed by introducing time-preference (1916–71:191–99). Wicksell draws a graph (Fig. 3.1).

Wicksell has plotted the log of the value of the wine and of the compounded value of the grape juice against time. (3.4) says that the log \( x \) curve is concave. (3.2) asserts that for any equilibrium period of storage that the log \( x \) and the log \( w + rt \) curves intersect. (3.3) demands that the two curves be tangent at the point of intersection [recall that \((d\log x/dt) = (1/x)(dx/dt)\); \( k \) is an increasing function of the trapezoid \([0, \log w], (t^*, x^*), (t^*, 0), (0, 0)\].

One can now determine directly from the graph that a longer equilibrium period of storage, \( t^* \), goes with a higher price of grape juice, \( w \), and a larger equilibrium value of wine, \( x^* \), a larger value of capital, \( k \), and a smaller rate of interest, \( r \). The greater productivity of roundabout processes benefits the sellers of grape juice (workers and landlords). \( x^* \) reaches a maximum when \( r = 0 \). The model does not specify whether for any finite \( t^* \) \( r \) will be zero and \( x^* \) reach a maximum. It is, for example, consistent with (3.1)–(3.6) to add the equation \( r = 1/t^{*2} \) or the equation \( r = (1/t^{*2} - 1)/10,000 \).

Wicksell can now examine the relations between the apparent marginal product of capital, \( dx/dk \) and the rate of interest. Differentiating (3.6) with respect to \( k \), he finds

(3.7) \[ \frac{dx}{dk} = r + k \frac{dr}{dk} + \frac{dw}{dk} \]

The apparent marginal product of capital is equal to the rate of interest plus the increased value of grape juice minus the decreased interest payments. In this model the rate of interest is larger than the apparent marginal product of capital.\(^2\)

The relations in this simple model are as the Austrian theory says they should be. Let us briefly review and classify the assumptions of the model. Most are common in neoclassical or "equilibrium" models (see ch. 6, §2). Wicksell assumes that there are competitive conditions, that agents are price takers, that markets clear, that capital is mobile,

\(^2\) Differentiating (3.2) with respect to \( t^* \) and using (3.3), one finds that \( dw/dt^* = -w't^*/d\). Differentiating (3.6) with respect to \( t^* \) and employing this last result, one finds, instead of (3.7) that \( dx/dk = r + (k - wt^*)/dr/dk \). We can see from (3.5) that \( k \) is larger than \( wt^* \); \( dx/dk \) is thus smaller than \( r \).
that commodities are infinitely divisible and that agents are perfectly informed. Furthermore, he assumes the “laws” that equilibrium is reached and that firms attempt to maximize profits. There are only three special assumptions: (1) The input of grape juice is fixed. (2) $dx/dt > 0$. (3) $(d^2 \log x/dt^2) < 0$. (2) is a particular instance of Böhm-Bawerk’s “law of roundableness” that increasing the waiting in production increases the value of the product. If we regard time as an input, (3) is a special instance of the “law” of diminishing returns to a variable input into production with other inputs held constant. (1) is just a simplification. The model shows that these assumptions entail (i) that $r$ is the marginal product of the period of storage, (ii) that $r$ is smaller while $k$ and $w$ are larger when that period is longer, (iii) that capital is not an independent factor of production, and (iv) that the apparent marginal product of capital is not equal to the rate of interest. The last implication can be disputed. It will be discussed further in chapter 4, §2.

The assumptions and theorems of the model should now be clear. What is not clear is what, if anything, Wicksell has accomplished by presenting the model. To understand the significance of the model demands some careful philosophical discussion.

3. Models and Theories in Economics

What has Wicksell accomplished? He has written a mathematical fairy tale incorporating the claims of the Austrian theory of capital. As fairy tales go, his is unexciting. What is the point of the exercise?

My answer will be roundabout (and thus, I hope more productive). Wicksell has developed a particular kind of model. To understand precisely what he has done, we must first understand what economic models are. There is, however, no way to understand the structure and significance of economic models without a definite conception of scientific theories. To appreciate the peculiarities of economic models we need to understand what theories are and what roles they play in science.

My view of theories and models is controversial. One way to evaluate my philosophical analysis is to employ it in case studies and to see how well it “works.” As we shall see, my view of theories and models not only helps one to understand directly and simply how economists theorize, but it also refines and clarifies philosophical questions about this theorizing. I shall not argue that my view of theories and models is in general the only correct one. I claim merely that this conception simplifies and clarifies this philosophical inquiry into theories of capital and interest. The distinctions I shall need can be made in different terminology. Other conceptions of theories and models could have been employed, but they would have been further removed from economists’ conceptions of their own work and they would have complicated the discussion.

My analysis of theories and models relies on the conception of scientific theories suggested by Patrick Suppes (1957, ch. 12). See also Sneed (1971) and Stiglmuller (1979). In outline Suppes proposes that we regard scientific theories as predicates. The empirical claims of science consist of assertions that such predicates are true of actual systems. Suppes insists that scientific theories are set-theoretic predicates, because he hopes to provide formal restatements of scientific theories. Since I am doubtful about the prospects and merits of this program, I do not insist that theories be set-theoretic predicates. Once one drops the talk of set theory, the core of Suppes’ conception of scientific theories can be presented very simply. Giere (1979, ch. 5) provides just such a simplified presentation.

Giere takes scientific theories to be definitions of predicates, not to be predicates themselves, a difference not important here. Newton’s laws of motion and his law of gravitation define, for example, what Giere calls “a classical particle system.” The predicate, “is a classical particle system,” is true of something if and only if Newton’s laws of motion and of gravitation are true of it. On Giere’s view of scientific theories, the basic claims of the Austrian theory of capital define a new predicate, “is an Austrian economy,” or a new kind of system, “an Austrian economy.” An economy is (at a time) an Austrian economy if and only if the basic assumptions of the Austrian theory of capital are true of it at that time.

On this view of scientific theories, there is no point to asking whether the claims of a theory are true or whether a theory provides reliable predictions. Mere predicates cannot be true or false or provide any predictions. Definitions are trivially true but also do not enable us to make any predictions.

Science, of course, does more than provide definitions. The proposing of scientific theories is, on this view, only one part of the theoretical enterprise. The other crucial part is the proposing of theoretical hypotheses, which assert that the new term is true of some actual system. In Giere’s view, Newton’s laws of motion and his law

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3 For a formal application to neoclassical economics of Sneed’s conception of scientific theories see Händler (1980).
of gravitation only define a "classical particle system." Newton, however, did more. He also offered the theoretical hypothesis that the solar system is a classical particle system. The Austrian theorists similarly offered the theoretical hypothesis that actual competitive economies are (approximately) Austrian economies. We accommodate the conviction that theories say something about the world by dividing "theories" in some intuitive sense into theories (predicates or definitions of predicates) and theoretical hypotheses which employ these new predicates.

Although this account of scientific theories may appear cumbersome, it is useful in understanding the theoretical work of economists. First, however, I shall introduce a crucial terminological change. What Giere and Suppes call a "theory," I shall call a "model." I shall then use the term "theory" for something else. It is generally a bad idea to change terminology in this way, but I think in this case that the change is worth the dangers of confusion: I shall not only be better able to match the usage of economists, but I shall also be able to capture the intuition that theories say something about the world. Suppes in fact admits that scientists frequently use the term "model" as I shall, to mean what he means by "theory" (1957:254). Giere argues that the term "model" is in one of its senses more or less a synonym for "theory" (1979:81). I think that it clarifies matters to reserve the term "theory" for another use. Models define kinds of systems. The sentences in models, which I shall call "assumptions," are merely clauses in such definitions. It is a mistake to ask whether an assumption in a model is true or whether a model itself is true. We test theoretical hypotheses, not models.

Economists use the term "model" in many ways (Machlup 1960:569). Although some economic models are also models in other senses of the term, I know of none in theoretical economics which cannot be characterized as a predicate or as a definition of a predicate. Taking models as definitions permits one to develop an interesting and cogent interpretation of the nature and significance of economic models. Note that in philosophical and mathematical discussions of this century the term "model" has a different meaning. Philosophical discussions of models in this other sense of the term are of little use to economists or to philosophers interested in economic models.

The following two definitions summarize the discussion thus far:

A model is a predicate or a definition of a predicate (or of a kind of system). It is made up of assumptions.

A theoretical hypothesis states that the new predicate defined by a model is true of something.

Using models and theoretical hypotheses, economists can make claims about the world. In defining "an Austrian economy" by means of the claims of the Austrian model of capital and interest and offering the theoretical hypothesis that the United States has always been an Austrian economy, one can conclude, for example, that in the United States more roundabout production processes have always been more productive. One can infer from a theoretical hypothesis what I shall call "closures" of the assumptions of the model. The model Giere calls a "classical particle system" contains, for example, the assumption that any two bodies attract one another with a force inversely proportional to the square of the distance between them. The assumption does not say what it applies to. From the theoretical hypothesis that the solar system is a classical particle system, one can infer the "closure" of the assumption—that any two bodies in the solar system attract one another with a force inversely proportional to the square of the distance between them. In the closure of the assumption the domain is specified and in some cases the interpretation of the predicates in the assumption is limited or sharpened. From a theoretical hypothesis one in a sense "recovers" the assumptions of the model. A theoretical hypothesis entails the closures of all the assumptions of the model. Closures of assumptions are genuine statements which may be true or false.

Some theoretical hypotheses state that a particular real-world system, like the solar system, belongs to the extension of the predicate defined by the model. When a theoretical hypothesis is such a singular statement, I shall call the resulting set of closures of the assumptions of the model an applied theory. It is a theory of the particular individual hunk of the world referred to in the theoretical hypothesis. Philosophers are sometimes attracted to the predicate view of models (called in the literature "theories") because they seek to avoid judging general theoretical claims to be either true or false. On the predicate view of models one can apparently avoid judging whether Newton's law of gravitation, for example, is a universal law. Instead one considers whether it is true of particular ensembles of bodies.

Although one might employ a predicate view of models in attempting to avoid judging the truth of general theoretical statements, one need not use the predicate view of models in this way. Someone who claims that we do not need to consider whether Newton's law of gravitation, for example, is truly a universal law invites two critical questions. Why do scientists confidently employ the law in domains in which they have not tested it? How can one rationalize the general judgments scientists make concerning at least the usefulness of the purported law? I find
these criticisms persuasive. In my view scientists can offer not only singular theoretical hypotheses ("The solar system is a classical particle system") but general theoretical hypotheses ("Everything is a classical particle system"). The distinction between singular and general theoretical hypotheses does not, however, capture precisely the distinction between general laws and a regularity in an individual system only, since the latter distinction is not a formal one. There are many difficult questions here that deserve further philosophical discussion. The only point I insist on is that theoretical hypotheses are not restricted to singular claims about individual systems and that the closures of assumptions they imply may be general laws. Adopting a view of models as predicates or as definitions of predicates does not itself commit one to any thesis concerning the aims of science or whether general theoretical claims may be true.

A theory is thus a set of assertions systematically related to one another which is derivable from a theoretical hypothesis. Equivalent reformulations of the assertions do not count as theory changes. If the theoretical hypothesis mentions only a particular system, we have an applied theory. If our theoretical hypothesis is universal (and our model does not itself mention spatiotemporal particulars), we have a pure or a general theory.

Let me summarize schematically. A model consists of a set of assumptions, $A_1, \ldots, A_n$. These assumptions define a new predicate, $M$. A theoretical hypothesis, $H$, is a statement like "$X$ is an $M$" where $X$ refers to something and $M$ is the predicate defined by the model. From $H$ one may deduce the set of statements $A_i^H, \ldots, A_n^H$, where $A_i^H$ is the closure by $H$ of the $i$th assumption of the model. The set of statements $A_1^H, \ldots, A_n^H$ is a theory.

4. Special Case Models

Given my definition of a "model," we can see that Wicksell's story about aging wine is a rather special kind of model. The sort of system it defines, an Austrian wine sector, is quite specific and extremely unrealistic. Any theoretical hypothesis asserting that a certain portion of an economy is an Austrian wine sector will be false. Yet, it is not only the falsity of theoretical hypotheses that distinguishes Wicksell's model from more usual scientific models: It is also an example or a description of a special case.

Special case models, like general theoretical models, can be regarded as predicates or as definitions of predicates. They have, however, their own peculiarities. Whether a model is a special case model depends on its relations to others and on how it is used. In special case models, various features of a more general theory or model are simplified and made less general and more vivid. The resulting model is used particularly for illustrating or evaluating the more general model. Special case models can be used both to support and to contest general models. A set of assumptions constitutes a special case model only if it simplifies and specifies the features of some more general model. In presenting a special case model, economists sometimes invite us, as Wick- sell implicitly does, to believe that the assumptions of the model are true in some possible economic circumstances.

It is especially appropriate to call special case models "models," because they resemble descriptions of the actual physical models that scientists and engineers often build. Just as one can illustrate, develop, teach, and test general claims about the properties of airplanes by means of scale models, so one can illustrate, develop, teach, and test general claims about properties of economies by means of special case economic models.

To claim that economists can use special case models to illustrate, develop and teach general principles should not be controversial. All sciences employ models in these heuristic ways. Unless, like Duhem (1906, ch. 4), one sees this practice as evidence of a weak English mind, one should not object to the heuristic use of special case models. This role is of great importance. Integrating the Austrian theory of capital with the general neoclassical (equilibrium) approach to economics is quite difficult. Models like Wicksell's are a beginning. The implications of the claim that increased roundaboutness increases the product are not obvious. One can employ simple models like Wicksell's to see what the implications would be were its assumptions true. Examples like aging wine help one to appreciate the plausibility of the suggestion that "waiting" contributes to production. Wicksell's model certainly possesses heuristic virtues.

Philosophers have, however, contested the claim that special case models can be used to test or to provide evidence for theoretical claims. Since models are only predicates or definitions of predicates they are not themselves tests or evidence or anything. Special case models can, however, be used as part of reports of imaginary experiments. If one asserts that the assumptions of the model are true in some possible economic circumstances or that they could be true, one can regard the assumptions of the model and their implications as describing results
of an imaginary experiment. When employed to report the results of an imaginary experiment, can special case models help one to test or to confirm general theoretical claims?

Carl Hempel, for one, thinks not. He points out that perfectly plausible, but entirely misleading imaginary experiments have been used in the past. He concludes, "Such experiments, then, cannot provide evidence pertinent to the test of sociological hypotheses. At best, they can serve a heuristic function: they may suggest hypotheses, which must then be subjected, however, to appropriate objective tests" (1965:165). By parity of reasoning, scientists ought never to rely on actual experiments, because they have been misleading, too. That imaginary experiments may mislead does not show that they have no evidential force.

Can one apply special case models to provide evidence that theoretical hypotheses employing the relevant general model are true? This question is particularly important for our purposes, since theoretical discussion and debate concerning capital and interest constantly employ special case models (see Dewey 1965, ch. 1). J. E. Cairnes, in contrast to Hempel, is quite unreserved about evidential applications of special case models. "There is, however, an inferior substitute for this powerful instrument [experiment] at his [the economist's] disposal, on which it may be worth while here to say a few words. I refer to the employment of hypothetical cases framed with a view to the purpose of economic inquiry. For, although precluded from actually producing the conditions suited to his purpose, there is nothing to prevent the economist from bringing such conditions before his mental vision, and from reasoning as if these only were present, while some agency comes into operation . . . the economic character of which he desires to examine" (1888:90).

Can we agree with Cairnes? Is it ever rational to rely on such special case models to support general hypotheses? I think so. We can at least grant, as Popper does (1968:442–56), that such models may have a critical function. One can use them to demonstrate that some more general model is inconsistent or that a theoretical hypothesis employing it conflicts with prior beliefs. One can employ special case models to point out possibilities previously overlooked. Furthermore, it seems to me that applications of such models may confirm theoretical hypotheses. The theoretical hypothesis that Wicksell's wine and grape juice model is true of some possible economic sector bears much the same relationship to the Austrian theory of capital that descriptions of experimental results bear to theoretical hypotheses. If one is prepared to assert that some possible economic sector is roughly a wine and grape juice sector, one has found in this possibility, in this imaginary experiment, an instance of Böhm-Bawerk's "law" of roundaboutness. One sees that the claims of the Austrian theory match in this instance what economists already expect. If, like many economists, one believes that in simple economic circumstances a larger value of capital will (other things being equal) go with a lower rate of interest, one can use Wicksell's model to test Böhm-Bawerk's law against this belief. One is not acquiring new beliefs about the world in the course of having new experiences. Instead one is learning about the logical relations between prior beliefs, simplifying assumptions, and a new hypothesis. It is thus misleading to speak of such applications of simple case models as a substitute for experiments. They are quite different.

The special case model is being used to show that the implications of the theory being considered match the economist's prior beliefs. Economists can use such models to check the consistency of the propositions of the theory with accepted beliefs.

Although we can thus see that it is rational to invoke special case models to help criticize or confirm general theoretical claims, we can also understand why this practice makes philosophers like Hempel uneasy. So long as one contests or defends theories in only this way, one's theorizing is not directly subject to control by experience. Empiricist philosophers and economists want more than consistent beliefs. They want beliefs that are supported by observations and experiments.

Empiricists do not, however, need to reject the use of special case models when assessing theories. When economists measure their theories against their prior beliefs, they may be confronting their theories indirectly with the results of observations. The prior beliefs may be well confirmed. If the relations between the prior beliefs and the observations which support them are very simple, the theoretical hypothesis applying the special case model would virtually be an account of past experiments. One's belief that Wicksell's assumptions are true of some simple cases is not, however, directly related to observations or experiments. One thus has grounds to be skeptical concerning the evidential force of theoretical hypotheses applying special case models like Wicksell's. Yet, insofar as one is justified in believing that such a model does sometimes apply, one has evidence for the theory under consideration. We shall have to return to this topic later when we consider what role ideology has played in capital theory. The accepted beliefs economists rely on in using special case models for theory assessment may carry ideological distortions.
5. Wood and Axes: The Basic Model

Wicksell also developed a more complicated model in his treatment of Åkerman’s problem (1911:274f). A point input of labor produces a capital good. The combined use of labor and the capital good during the lifetime of the capital good yields a continuous output of the consumption good. With two commodities and two sectors Wicksell’s treatment of Åkerman’s problem allows one to consider more of the difficulties of capital theory. I shall not, however, present Wicksell’s model here. A much simpler, more versatile, and more interesting model proposed by Oskar Lange (1935–36:159–92) captures the important features of Wicksell’s two-sector model (see the appendix to this chapter).

Lange’s model is important to my exposition. In discussing general equilibrium theory in chapter 5 and Sraffa’s work in Chapter 8 I will present special case models which are variants of Lange’s model. I will use the various special case models to clarify the relations between the different theories of capital and interest. I will thus be proposing and using special case economic models to discuss general economic models. The function of the models in this book are expository and illustrative. I shall not rely on any of the features peculiar to the special cases to argue for or against any general theoretical hypotheses concerning capital and interest.

In Lange’s model there are two commodities: a capital good (axes) and a consumption good (wood). Axes are used both in the production of axes and in the production of wood, while wood is not used in production at all. Both the production of axes and of wood takes one period (year). Although the period of production is not a variable in either sector, the roundaboutness of production is reflected in this simple model. Let \( x, m, \) and \( m' \) denote respectively the wood output and the quantity of axes used in the two sectors. \( m + m' \) is the ax output. Lange assumes that all axes last exactly one year. Lange’s model has two production functions, which are homogeneous of degree one with positive first partial derivatives, and, after a point, negative second partial derivatives.

\[
\begin{align*}
(3.8) & \quad x = f(m, L) \quad \text{(production function for wood)} \\
(3.9) & \quad m + m' = h(m', L') \quad \text{production function for axes}
\end{align*}
\]

where \( L \) and \( L' \) are the respective quantities of labor used in each sector. There is a constant and fully employed labor force. Thus:

\[
\begin{align*}
(3.10) & \quad L + L' = L^* \\
(3.11) & \quad p_m = f_m \\
(3.12) & \quad w = f_L \\
\end{align*}
\]

where \( p_m \) and \( f_m \) are the wage and price of axes and \( f_m \) and \( f_L \) are the values of the partial derivatives of \( f \) with respect to ax and labor inputs when profits are maximized. Maximizing profit in the ax sector, one can derive

\[
\begin{align*}
(3.13) & \quad h_m = 1 \\
(3.14) & \quad w = h_L p_m
\end{align*}
\]

where \( h_m \) and \( h_L \) are the values of the partial derivatives of \( h \) when profits are maximized.

There are thus as many equations as unknowns and the system appears to solve. Lange does not himself set \( p_x = 1 \). He treats \( p_x \) as a variable and the model itself as having a degree of freedom (1935–36:172). It simplifies matters to take \( p_x \) as numeraire. Notice that there seems to be no room for interest (1935–36:172–73). Time preference is thus necessarily zero.

Interest enters Lange’s model through a limitation on what Lange calls “money capital.” Suppose that in the wood sector \( mp_m + wL \) are in this stationary state limited for some reason, perhaps time preference, to some amount of \( x = k^* \). Subject to the limitations on money capital \( k^* \) and \( (k^*)^* \) in the ax sector, one can again express the conditions for the maximization of profit:

\[
\begin{align*}
(3.15) & \quad p_m = \frac{f_m}{1 + i} \\
(3.16) & \quad w = \frac{f_L}{1 + i} \\
(3.17) & \quad (1 + i') = h_m \\
(3.18) & \quad w = h_L' \cdot \frac{p_m}{1 + i'}
\end{align*}
\]
where \( i \) and \( i' \) are Lagrange multipliers. (3.15)–(3.18) are the direct result of setting the partial derivatives of the two Lagrange functions equal to zero. If one assumes that there is a unified money market and just one economy-wide limit on money capital, \( K^* = (k^* + k^**) \), then, with the equalization of the rate of interest, \( i = i' = (f_mh_L - f_L)/f_L \), which is the rate of real interest or the real rate of return over the cost of transferring labor from the wood to the ax sectors. Notice that the interest is compounded on indirect labor: \( w = h_L p_m/(1 + i) = h_L f_m(1 + i)^2 \). Given the last equation, labor will be relatively cheaper in producing wood and more than the optimum quantity will be used. Thus the marginal productivity of direct labor and the wage will be lower because of the shortage of money capital.

Notice that, despite the fixed periods of production, Lange’s model does support the Austrian view that capital is not an independent factor of production and that indirect processes are more productive. When money capital is scarce, less indirect and more direct labor per unit wood output will be used. Thus wood production is less time intensive than it is when money capital is abundant. The rate of interest is a steadily decreasing function of the time intensity as so construed. All the other relationships are as in Wicksell’s treatment of Åkerman’s problem. If time intensity is larger, \( wp_m, K^*, m, L' \) and \( x \) are larger, while \( r \) is smaller. Furthermore (1935–36: 184), \( x = rK^* + w(L + L') \).

Thus

\[
\frac{dx}{dK^*} = r + L^* \frac{dw}{dK^*} + K^* \frac{dr}{dK^*}
\]

The apparent marginal product of money capital divides itself into the rate of interest plus the marginal increase in wages minus the marginal decrease in interest payments. Thus one has the same relation as (3.7) above. The rate of interest is not equal to the marginal product of capital; it is not necessarily always larger or smaller than the apparent marginal product of money capital. Calculating a period of production is possible but messy, since axes are not made by unassisted labor. (See Dorfman 1959: esp. 361–65).

A couple of words need to be said about demand and time preference. In the above I have tacitly assumed that one is dealing with a closed economy in which workers consume their wages and capitalists consume their profits. If profits are reinvested and there is no time preference, the only stationary equilibrium is the state of maximum production and zero interest. Unless some quantity of profits is consumed, interest can only be a temporary phenomenon that reveals the shortage of money capital (and thus of waiting). Once one includes the consumption of capitalists and the variable factor of time preference, the shortage of money capital may be permanent. One can have an absolutely stationary state (like Marx’s simple reproduction, 1967, vol. 2, ch. 20) with positive profits equal to the consumption of the capitalists.

6. The Austrian Theory

In order to assess the theory of capital sketched above, it is essential to state this theory explicitly. The following formulation is not refined, but it will serve. We can think of the basic statements of the theory as the following:

(3.20) Some general formulation of neoclassical equilibrium theory that does not itself provide any account of capital.

(3.21) Storing up the current services of land and labor increases their productivity at a decreasing rate.\(^5\)

(3.22) Individuals prefer present consumption to future consumption.\(^6\)

(3.23) The quantity of capital is measured by the quantity of services of land and labor stored up and the time during which they are stored. Capital is not an independent factor of production.\(^7\)

(3.24) Interest is the return to this storing up or waiting.

(3.21) and (3.22) seem to be the fundamental “laws,” while (3.23) and (3.24) define “capital” and “interest.” The following are three important derivative claims:

(3.25) Interest is not the marginal product of capital.

\(^5\) “On the whole it may be said that not only are the first steps more productive, but that every lengthening of the roundabout process is accompanied by a further increase in the technical result; as the process, however, is lengthened the amount of product, as a rule, increases in a smaller proportion” (Böhm-Bawerk 1888:84).

\(^6\) Perishable commodities available only in certain seasons are the best known counter-examples.

\(^7\) “Capital is an intermediate product of nature and labor, nothing more. Its own origin, its existence, its subsequent action, are nothing but stages in the continuous working of the true elements, nature and labour. They and they alone do everything from beginning to end in bringing consumption goods into existence” (Böhm-Bawerk 1888:96).
(3.26) The rate of interest is smaller when the time-intensity of production is larger. 
(3.27) The quantity of wages and/or rent is larger when the roundaboutness of production is greater.

Remember that in the above formulations we are comparing stationary states. (3.20)–(3.24), regarded as assumptions, define an Austrian competitive economy. The Austrian theorists believed that real economies are, to some degree of approximation, Austrian competitive economies. (3.20)–(3.24) can thus also be regarded as empirical generalizations true or false of actual economies.

The assumptions and implications of assumptions listed above are part of or consistent with the two models discussed. As roughly formulated above, the Austrian theory need not insist that waiting is precisely a factor of production, nor that interest is its marginal product, but only that larger stores of productive services involving more waiting are more productive and that interest results from the increased productivity to which waiting gives rise. The rate of interest is positive, because individuals prefer present consumption to future consumption and because roundabout processes of production are more productive. Although I have not presented anything like a formalization of the Austrian theory of capital, its main claims and logical structure are clear enough that they can be assessed.

APPENDIX TO CHAPTER 3: Wickell’s and Lange’s Austrian Models

Wickell’s treatment of Åkerman’s problem (1911:274f) is designed to show that the claims of the Austrian theory of capital remain valid when one shifts to a more complicated time structure of production. In the model Wickell presents, a point input of labor produces a capital good, axes, from which a continuous output of the consumption good, wood, is produced with the assistance of labor. The durability of axes is a function of the quantity of labor time it takes to produce them. Their efficiency is constant over their lifetime. Through some stipulations concerning cost and production functions and the labor force and some imprecise, but correct mathematical manipulations, Wickell provides almost a complete specification of the simplified hypothetical wood and ax economy. One degree of freedom remains, but that might be eliminated, as J. Hirschleifer (1966–67) suggests, by including some role for time preference. Wickell’s model makes no reference to utility functions or to demand.

As a result of his various manipulations, Wickell shows that, in stationary equilibrium states in which the equilibrium durability of axes, \( t^* \), is larger, the labor input per ax, \( a \), the output of wood, \( x \), the wood wage, \( w \), and the wood value of capital, \( K \), are all larger. The rate of interest, \( r \), and the (wood) rental cost for axes, \( b \), are smaller. One thus finds, as the Austrian theory asserts, that increased roundaboutness goes with increased output, a higher wage, a lower rate of interest, and a larger value of capital. In this two-commodity model, we still find the so-called “Wickell Effect.” Part of the increase in capital is absorbed in higher wages; only part of it increases output. The rate of interest is generally larger than the marginal product of capital, although Wickell notes the puzzling possibility that they may diverge in the opposite direction (1911:292–93). As in his simpler wine and grape juice model, Wickell illustrates, elaborates, and perhaps even supports his general theory of capital by means of a mathematical model.

Yet certain features of the model are disquieting. With a larger equilibrium durability of axes, the distribution of the labor force between sectors and the distribution of income between interest and wages remain constant. Moreover, with a given technology and labor force, there is neither a maximum output nor a maximum feasible lifetime for axes. These results seem counterintuitive. One wants to know whether they are an implication of the Austrian theory for two-sector models generally. If so, it seems that Wickell’s wood and ax model would count against the Austrian theory of capital that he presents. Moreover, Wickell’s model has a feature that is of ideological importance: there is no possible stationary equilibrium in which the rate of interest is zero. Is it in fact impossible for interest to disappear?

None of the above counterintuitive or contestable conclusions can be derived when one replaces the particular cost function Wickell employs with a more reasonable one. The model that results has im-
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important affinities to the model discussed in §5 and helps to see precisely the relationship between Lange's work and Wicksell's.

In a model that has one degree of freedom, we can consider the various economic variables as functions of some parameter, z. To simplify matters, Wicksell treats \( t^* \), the lifetime of the axes produced in the given static equilibrium, as itself the parameter on which the other economic variables depend. What Wicksell does, which shortens the exposition at the cost of some mathematical clarity, is to take \( a \), the input of labor per ax as a function of \( t^* \), the equilibrium lifetime of the axes. The particular function he proposes, about which more will be said later, is:

\[
(3.28) \quad a = k \, t^{*v}
\]

where \( k \) and \( v \) are positive constants with \( v \) less than 1; \( t^* \) is being used as a surrogate for some parameter upon which the static equilibrium depends, but \( a \) does not depend upon any specific equilibrium or, indeed, even on the existence of an economy. Let \( t \) be the lifetime of some given ax. Then (3.28') \( a = k \, t^{*v} \) expresses a technological fact wholly independent of any other features of the economy. If the given ax happens to have the lifetime \( t^* \), then the value of \( a \), for \( t = t^* \), is \( k \, t^{*v} \), but \( a \) is function of \( t \), not of \( t^* \). Similarly the discounted value of the rentals of an ax over its actual lifetime \( t \) is \((b/r) (1 - e^{-rt})\), where \( b \) and \( r \) are functions of the parameter \( t^* \).

If the lifetime of the given ax, \( t \), is equal to the equilibrium lifetime, \( t^* \), then the price of an ax is \((b/r) (1 - e^{-rt})\). Zero profits in competitive equilibrium tells us that

\[
(3.29) \quad \frac{b(1 - e^{-rt})}{r} = wa.
\]

Maximizing profits, \((b/r) (1 - e^{-rt}) - wkt^v\), with respect to \( t \), we get as the first order condition:

\[
(3.30) \quad be^{-rt} = wkt^v - 1.
\]

For \( t = t^* \), we have (3.30') \( be^{-rt^*} = wkt^v - 1 \).

From here on we can investigate the parametric dependence of the various variables on \( t^* \). Although in stationary equilibrium the longevity of axes is taken as the parameter upon which the variables depend, the difference between a change in the longevity of axes and a change in the parameter upon which the variables depend should be kept clear. The same issue arises in the simpler wine and grape juice model, where it is important to distinguish between the parameter which determines which equilibrium one is in (which is taken to be the equilibrium storage period) and the time interval during which a certain batch of wine has aged.

The basic equations of Wicksell's model are the following eight:

\[
(3.28) \quad a = kt^{*v}
\]

\[
(3.29) \quad \frac{b(1 - e^{-rt})}{r} = wa
\]

\[
(3.30) \quad e^{-rt} = 1 + \frac{rt^*}{v}
\]

\[
(3.31) \quad L + L' = L^*
\]

\[
(3.32) \quad x = CL^* \left(\frac{L't^*}{a}\right)^d
\]

\[
(3.33) \quad b = \frac{dxa}{L't^*}
\]

\[
(3.34) \quad w = \frac{cx}{L}
\]

\[
(3.35) \quad K = \frac{L'b(e^{-rt^*} - 1 + rt^*)}{ar^2}
\]

(3.30') is a consequence of (3.29) and (3.30'). It implies that \( rt^* \) is a constant, which is a great mathematical convenience. \( L \) is the quantity of labor devoted to wood production, \( L' \), the quantity devoted to ax production. \( L^* \) is a constant. The wood production function is a Cobb-Douglas production function with \( c \) and \( d \) positive constants adding to 1. \( C \) is another positive constant. The wood and ax production processes require negligible time intervals. According to (3.33) and (3.34), ax rentals and wages are equal to the marginal products of axes and labor respectively in producing wood. The value of capital is obtained by integrating the value of the stock of axes of each age.

If we do not include the price of wood, which is taken as the numeraire (equal to one), there are nine variables, \( a \), \( x \), \( w \), \( b \), \( r \), \( t^* \), \( L \), \( L' \) and \( K \). There is thus one degree of freedom. Varying \( t^* \) paramet-
ically, the results summarized earlier can be obtained straightforwardly. Those results support the general claims of the Austrian theory of capital.

Two special features of Wicksell’s treatment of Åkerman’s problem are primarily responsible for the more disquieting conclusions mentioned earlier: the Cobb-Douglas production function for wood and the particular cost function for axes. I shall not discuss the virtues and pitfalls of the Cobb-Douglas production function here. More interesting is Wicksell’s cost function for producing axes, \( a = \frac{k}{N - t^*} \). Wicksell argues that this function should be considered as a general form of a cost function for capital goods (1911:288), but it is in fact quite counterintuitive. It specifies that more labor per ax is needed to produce more durable axes, but that as axes become more durable (no matter how durable they already are), it becomes easier (requires less additional labor) to make them last still longer. There is no limit to the durability of an ax, given a large enough labor input. I suggest replacing (3.28) and (3.28') above with

\[
(3.28a') \quad a = \frac{k}{N - t^*}
\]

\[
(3.28a) \quad a = \frac{k}{N - t}
\]

\( N \) here is a constant which is the limit of the possible durability of an ax with finite labor input; \( k \) is, as before, a positive constant. I make no general defense of (3.28a) as a cost function for capital goods, except to point out that it is more plausible than Wicksell’s. Notice that \( da/dt \) is positive and that, until \( t = t^* = \frac{1}{2}N \), \( t^* \) increases more than proportionally to \( a \) and thus that there would be a tendency for \( t^* \) to be larger in static equilibria in which there is a larger wood output. Equations (3.29) and (3.31)–(3.35) remain as before. (3.30') changes as follows: Since \( t^* = t \), one can derive from (3.28a') and (3.29)

\[
\frac{b}{k}(1 - e^{-\alpha}) = \alpha k(N - t).
\]

Maximizing profit and substituting \( t^* \) for \( t \), one gets,

\[
be^{-\alpha t^*} = \alpha k(N - t^*)^2.
\]

Substituting \( wke^{-\alpha t^*}(N - t^*)^2 \) for \( b \) in the first expression and rearranging, one derives:

\[
(3.30a) \quad e^{\alpha t^*} = 1 + \frac{\alpha}{\alpha} (N - t^*).
\]

Although the derivations involve some tedious calculus, it can be shown that, just as in Wicksell’s model, \( x, K, w, \) and \( a \) are all larger when \( t^* \) is larger (provided \( t^* \) remains less than \( \frac{1}{2}N \)), while \( r \) and \( b \) are smaller (Hausman 1978:98–103). On the other hand, there are three new results. When one compares static equilibria in which \( t^* \) is greater than \( \frac{1}{2}N \), not all of the above relations still hold. With a given fixed

\[
(9) \quad \frac{du}{dn} > 0 \text{ if } n > 2
\]

\[
(10) \quad n > 2 \text{ if and only if } t^* < \frac{1}{2}N
\]

\[
(11) \quad \frac{dn}{dt^*} < 0
\]

9 The derivation in Hausman (1978:101–2) that purports to show that \( K \) must increase with \( t^* \) contains an error. A valid derivation follows. I shall not repeat the proof that \( ddt^* \) is positive, even though I use the result, since it is tedious and available in my (1978:100).

(a) First I shall show that \( d(\alpha t^*)/dt^* \) is negative for \( t^* < \frac{1}{2}N \).

\[
(1) \quad \text{let } u = \alpha t^*, n = \frac{1}{a}
\]

\[
(2) \quad e^{\alpha t^*} = 1 + au(n - 1)
\]

\[
(3) \quad \frac{du}{dn} = \frac{u}{e^{\alpha t^*} + 1 - u}
\]

\[
(4) \quad \frac{du}{dn} = e^{\alpha t^*} + 1 - (1/u)(e^{\alpha t^*} + u - 1)
\]

\[
(5) \quad \frac{du}{dn} = \frac{u^2}{(ue^{\alpha t^*} - e^{\alpha t^*} + 1)}
\]

\[
(6) \quad \text{let } g(u) = ue^{\alpha t^*} - e^{\alpha t^*} + 1
\]

\[
\frac{dg}{du} > 0 \text{ if } u > 0
\]

\[
\frac{dg}{du} > 0 \text{ if } u > 0
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } u > 0
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } u > 2
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } n > 2
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } n > 2
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } n > 2
\]

\[
\frac{dn}{dt^*} > 0 \text{ if } n > 2
\]
labor force and a given technology there is, as one would expect, a maximum output which is reached when capital goods are plentiful and the rate of interest is zero. This is the case when \( t^* = \frac{1}{2}N \). Second, \( L^* \) (the labor used in producing capital goods) increases with \( t^* \). The proportion of the worker population employed in the consumer goods industry is smaller when capital goods are more durable (and capital or waiting is more abundant). As \( t^* \) approaches \( \frac{1}{2}N \), \( L/L^* \) approaches \( \frac{1}{2} \). Third, the distribution of income between wages and interest is no longer constant. \( rK/wL^* \) is a monotonic decreasing function of \( t^* \), reaching zero when \( t^* = \frac{1}{2}N \). Thus the existence of a Cobb-Douglas production function for consumption goods does not itself insure constant factor shares. A simulation is provided at the end of the appendix.

The modified Wicksellian model bears a close relationship to Lange's model presented in §5 and clarifies the relationship between Lange's and Wicksell's work. The major relations among the parameters are the same. The only striking differences between Lange's model and the modified version of Wicksell's are that in the latter axes are still produced by unassisted labor and are of variable durability. There are three points to be made concerning these differences.

1. Money capital is abundant in Lange's model when it is equal to the value of the maximal products of both sectors, while in the modified Wickellian model, the total capital when \( r \) is zero (if one assumes, as in Lange's model, a one-year production period for axes and wood) is the maximum total product plus \( L^*t^*p_m/2a = L^*t^*p_m^2/2a \) —the value of the stock of axes. Much more "accumulation" is needed before \( r \) is zero. Lange's model understates the contrasts between stationary equilibria where money capital is scarce and those where it is abundant.

2. If there is any factor which upset the equilibrium position, adjustment appears more difficult and time-consuming in the modification of Wickell's two-sector model than in Lange's.

3. The greater realism of Lange's model—its avoidance of the use of unassisted labor in the production of capital goods—is a considerable advantage. Realism here comes with a bonus rather than as a cost in terms of applicability and simplicity.

\[
\begin{align*}
10.
\frac{d(t^*)^2}{dt^*} &< 0 \text{ if } t^* < \frac{1}{2}N \\
11.
\text{Now to show that } \frac{dK}{dt^*} > 0 \\
12.
K &\approx \frac{L^*b(e^{-\alpha t^*} - 1 + rt^*)}{ar^2} \\
b &\approx \frac{dx}{L^*t^*} \\
13.
\frac{1}{d x} &\approx \frac{e^{-\alpha t} - 1 + u}{ru} \\
14.
D_x \left( \frac{1}{d x} \right) &\approx \text{differentiating (15) and rearranging} \\
15.
r(1 - e^{-\alpha t - ae^{-\alpha t^*}}) &\approx \frac{u(1 - u - e^{-\alpha t})(e^{\alpha t} + 1)}{dr^*} \\
16.
\frac{dr}{dt^*} &\approx \frac{e^{\alpha t} + 1}{N} \frac{du}{dt^*} \\
17.
\frac{dr}{dt^*} &\approx \frac{e^{\alpha t} + 1}{N} \frac{du}{dt^*} \\
18.
\frac{du}{dt^*} &\approx \frac{u(1 - u - e^{-\alpha t})(e^{\alpha t} + 1)}{dr^*} \\
19.
F &= Nr^2aD_x \left( \frac{1}{d x} \right) \\
+ &\frac{u(1 - u - e^{-\alpha t})(e^{\alpha t} + 1)}{dr^*} \\
20.
F &= (e^{\alpha t} + 1 + (1 - e^{-\alpha t} - ae^{-\alpha t^*}) \\
+ &\frac{u(1 - u - e^{-\alpha t})(e^{\alpha t} + 1)}{dr^*} \\
21.
F &= \sum_{j=1}^{\infty} e^{k+2j} \frac{1 - 4j - 4j^2}{(2j + 2)!} \frac{du}{dt^*} \\
22.
F &= 2 \sum_{j=1}^{\infty} \frac{e^{n+2j} + 4j^2}{(2j + 2)!} \frac{du}{dt^*} \\
23.
F &> 0 \text{ for } t^* < \frac{1}{2}N \\
24.
\frac{dx}{dt^*} &> 0 \text{ for } t^* < \frac{1}{2}N \\
25.
\frac{dK}{dt^*} &> 0 \text{ for } t^* < \frac{1}{2}N \\
26.
Hausman 1978:100.
\end{align*}
\]
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 twosector 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>
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<tr>
<th>$t^*$</th>
<th>$r$</th>
<th>$L$</th>
<th>$L'$</th>
<th>$a(k^{d/C})$</th>
<th>$w(k^{d/C})$</th>
<th>$b(1/C(k))$</th>
<th>$K(1/C(k))$</th>
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<td>.89</td>
<td>600</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. Yet there is little evidence for its truth, and many distinguished neoclassical economists have criticized it harshly.

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 spokesmen 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.

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 (1959) 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