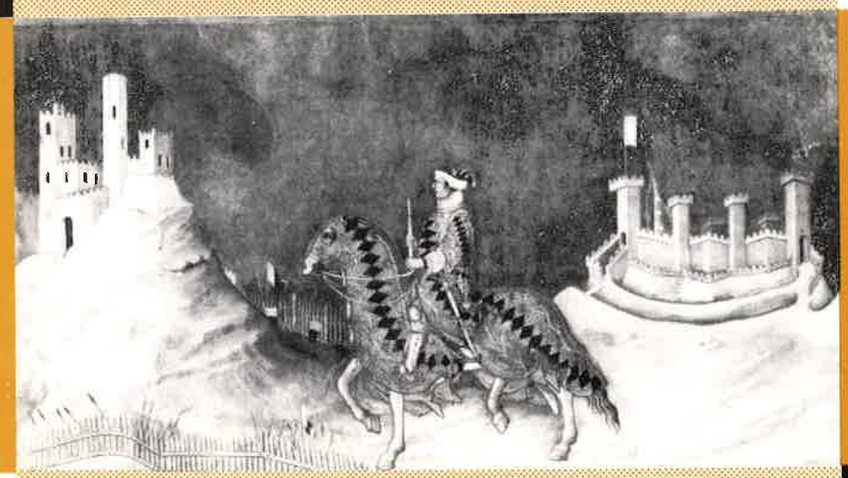
UNIVERSITA' DEGLI STUDI DI SIENA
Facoltà di Scienze Economiche e Bancarie

QUADERNI DELL'ISTITUTO DI ECONOMIA

Fabio Petri

THE MEANING OF THE CAMBRIDGE CONTROVERSIES ON CAPITAL THEORY: AN ATTEMPT AT COMMUNICATION



QUADERNI DELL'ISTITUTO DI ECONOMIA

Alla presente Collana "gialla" di quaderni l'Istituto di Economia affianca una Collana "celeste" di monografie. I numeri pubblicati a tutt'oggi sono i seguenti:

1) GIUSEPPE DELLA TORRE, Istituzioni Creditizie ed Accumulazione del Capitale in Italia (1948-81), 1984.
2) SANDRO GRONCHI, Tasso Interno di Rendimento e Valutazione dei Progetti: una Analisi Teorica, 1984.
3) ALDINO MONTI, La Proprietà Immobiliare a Bologna in Età Napoleonica (1797-1810), 1984.
4) GIULIO CIFARELLI, Equilibrium and Disequilibrium Interpretations of Inflation, 1985.

• Redazione: Istituto di Economia della Facoltà di Scienze Economiche e Bancarie - Piazza S. Francesco, 17 - 53100 Siena - tel. 0577/49059

• La Redazione ottempera agli obblighi previsti dall'Art. 1 del D.L.L. 31.8.45 n. 660

• Le richieste di copie della presente pubblicazione dovranno essere indirizzate alla Redazione

• I Quaderni dell'Istituto di Economia dell'Università di Siena vengono pubblicati dal 1979 come servizio atto a favorire la tempestiva divulgazione di ricerche scientifiche originali, siano esse in forma provvisoria o definitiva. I Quaderni vengono regolarmente inviati a tutti gli istituti e dipartimenti italiani, a carattere economico, nonché a numerosi docenti e ricercatori universitari. Vengono altresì inviati ad enti e personalità italiane ed estere. L'accesso ai Quaderni è approvato dal Comitato Scientifico, sentito il parere di un referee.

Fabio Petri

THE MEANING OF THE CAMBRIDGE CONTROVERSIES ON CAPITAL THEORY: AN ATTEMPT AT COMMUNICATION

Siena, ottobre 1986

CONTENTS

1. Introduction	p. 1
2. Deficiencies of the temporary equilibrium method	p. 4
3. The long-period method	p. 8
4. The critique of long-period marginalist theory	p. 13
5. Post-Walrasian theory and the microfoundations of neoclassical macroeconomic models	p. 26
6. The demand curve for labour	p. 33
7. Investment	p. 36
8. Summary and Conclusions	p. 38
Notes	p. 42
References	p. 49

Fabio Petri è professore associato di Economia Politica nella Facoltà di Scienze Economiche e Bancarie dell'Università di Siena

1. Introduction

After a period of intense discussion in the 1960's and early 1970's, a majority of the economic profession appears to have decided that the "Cambridge criticisms" of neoclassical, or, as I prefer to call it, marginalist capital theory do not constitute a sufficient reason for abandoning the neoclassical, or marginalist, approach to value, distribution and employment[(1)].

The present essay is an invitation to reconsider the issue, addressed at all those who do not feel convinced by the claims of the "Cambridge critics" or have doubts as to the opportunity of dedicating time and energies to a study of the debate, but who feel nonetheless puzzled by the obstinacy with which a number of highly qualified economists keep claiming that those criticisms have shown that the entire marginalist approach is untenable on *logical* grounds. Matters of logic are usually rapidly settled: how come on this issue the neoclassical side is unable to convince the other? Could it be that there is something to the critics's claims, which has so far been imperfectly grasped by the mainstream majority?

I have long felt that there in fact exist, in this debate, great communication difficulties, which need, in order to be overcome, an effort from both sides. The present essay - based on the experience gained in the numerous occasions on which, in the last 8 years, I have presented and tested my first effort at communication, Petri (1978) - results from the persuasion that these difficulties can be surmounted, and the issues at stake made plain, with the use of no advanced models; but that a comprehensive, organic discussion of many interconnected issues is required, and hence a long paper, and some patience on the reader's part.

The main persuasions motivating the exposition adopted in this essay are the following.

A) The point which nowadays is most in need of clarification is the relevance of the "Cambridge criticisms" (centred on the inconsistencies of the notion of 'aggregate capital') to the modern versions of neoclassical general equilibrium (GE) theory. The defenders of neoclassical theory have in fact

usually conceded that it is not possible to treat heterogeneous capital as a single, homogeneous factor of production, but have added that neoclassical theory has no need for such a treatment of heterogeneous capital: different capital goods, it is argued, can be treated as so many different factors of production, and *are* so treated in the 'rigorous', i.e. post-Walrasian general equilibrium, versions of neoclassical theory[2]. To this the "Sraffians"[3] have replied that these post-Walrasian versions (exemplified by the Arrow-Debreu model) of GE theory have achieved their *apparent* immunity from the Cambridge critique by changing the notion of equilibrium so as to make it economically worthless (Garegnani 1976, Eatwell 1978, Petri 1978, Harcourt 1976; why the immunity is only apparent will be made clear in the sequel). But at this point the debate has come to a complete halt; to the best of my knowledge, no reply has so far been advanced against this contention.

B) The reason for the stalemate appears to be a diffuse lack of familiarity with the traditional notion of long-period equilibrium, or more generally of long-period, or normal, positions[4] and with the method of explanation of economic facts based on that notion, which makes it difficult to understand what the critics are arguing. A number of recent papers and books (Milgate 1982; Eatwell 1982; Eatwell and Milgate 1983; De Vivo 1984; Schefold 1985) have addressed this issue, but the communication difficulties seem to be still there. The present essay tries to surmount them by insisting on one basic difficulty which modern neoclassical value theory encounters, when used to explain and predict the behaviour of real economies: a difficulty deriving from the impossibility to assume an instantaneous adjustment to equilibrium in the real world. The roots of the difficulty will be traced in Chapter 2 to the *data* of the various post-Walrasian notions of equilibrium, in particular, to the data relative to the capital goods endowment of the economy (the meaning of the term 'post-Walrasian' will be clarified later). It will then be easier to understand why earlier economists had adopted a different, and, it would seem, better method of explanation, based on the notion of "normal" positions, towards which the economy is continually gravitating. Some examples of that method at work

are useful, and will be given in Chapter 3. Above all, one must insist that the marginalist approach to value and distribution was born (and was able to become dominant) *within* an unquestioned acceptance of that traditional method, and that is why all founders and early developers of marginalist theory tried to determine an equilibrium characterized (just like the normal positions of the Classical economists from Smith to Marx) by a uniform rate of profits on the supply price of capital goods - the distinguishing element of a "normal" competitive position.

C) These clarifications about the method make it easier to understand the nature of the change undergone by the marginalist approach with the shift to post-Walrasian notions of equilibria. Here the model already used in Petri (1978) remains useful in order to clarify the change in the nature of the data, and the importance of Walras in the transition to the new notions of equilibrium. With the help of that model, Chapter 4 will make it clear that within the marginalist approach the determination of a "normal position" (i.e. of what, after Marshall, would be called a long-period equilibrium) requires the treatment of capital goods as embodying various amounts of "capital", a *single* factor of production. The meaning of Sraffa's critical contribution - which will be briefly summarized - can then be more easily appreciated. One will also be able better to appreciate the relevance of the thesis that the abandonment of the traditional method was due, not to any intrinsic deficiency of that method, but rather to the difficulties that *marginalist theory* encountered when trying to determine a long-period position (Garegnani 1976, Milgate 1979).

D) All the points under B and C are only a necessary premise to resuming, in Chapter 5, the task announced under A. Here the central claim is that, contrary to what is commonly believed, the explanatory and predictive validity of post-Walrasian GE theories *requires* - instead of being itself a support for - the validity of neoclassical *macro* theories; and that the validity of the latter theories rests in turn on the validity of *long-period* marginalist theory. In other words: those neoclassical macro theorists, who send the reader back to neoclassical value theory, i.e. to micro (post-Walrasian) textbooks, for a confir-

mation of the validity of many assumptions of their models, are in fact (and have no choice but that of) relying not on current post-Walrasian equilibrium (or disequilibrium[5]) theories, but rather on the traditional long-period marginalist analyses, based on long-period forces and needing, in order to be acceptable, that concept of "aggregate (or rather, homogeneous) capital" which is generally conceded to be invalid. This will be argued in particular with reference to the labour market and to the savings-investment market, in Chapters 6 and 7. The "Cambridge criticisms" will thus be shown to invalidate mainstream macroeconomics, and with it the whole of the marginalist/neoclassical approach.

E) A critique of an entire approach to value, distribution and employment is only half a critique if at least the outline of a promising alternative is not also offered. Accordingly, in a concluding Chapter, it will be briefly indicated that a different, non-neoclassical framework within which the fruitful method of normal positions can be developed exists already, and appears to offer more promising prospects for advance than the neoclassical research programme[6].

2. Deficiencies of the temporary equilibrium method

Given the aims of this essay, I assume the reader to be reasonably well acquainted with the general structure of modern GE models, as expounded e.g. in Arrow and Hahn (1971). Two main categories can be distinguished: the intertemporal and the temporary equilibrium models. Here one preliminary observation is necessary. It is universally conceded that forward (futures) markets are very rare indeed, and that therefore intertemporal equilibria with complete futures markets are not, prima facie, the appropriate models to explain and predict the behaviour of actual economies. Obviously it cannot be a priori excluded that analysis might show that a market economy in fact behaves as if futures markets existed, and that therefore to assume that they exist does not yield empirically wrong predictions; but this result should emerge as a theorem, not be a postulate one starts from. It is therefore not surprising that much more effort should have been applied in recent years to the study of temporary equilibria and their sequences[7]. Still, it is puzzling that, for

so many years, the energies of neoclassical value theorists should have rather gone to develop the intertemporal equilibrium model. The recent survey by Weintraub (1983) might be interpreted as suggesting that part of the reason lies in the fact that mathematical studies on the existence of equilibrium were always de facto concerned with capital-less economies (equilibria with production and exchange but only non-reproducible factors of production). The realization that such equilibria could simply be reinterpreted as referring to dated commodities must have appeared to many as a satisfactory extension of the model to include capitalistic production. But it is easy to point out one major shortcoming of this 'reinterpretation': unless one makes the ridiculous assumption of futures markets extending indefinitely far into the future, an intertemporal equilibrium model must have a finitely far horizon; now, it would be senseless to assume that in the last period of the equilibrium there will be no production of capital goods; therefore the last time period of the model must include investment and savings decisions just like a temporary equilibrium; then the formal equivalence with a capital-less model can no longer be obtained, because all the complications of a temporary equilibrium (e.g. expectation functions) must be faced. In view of the very low number of futures markets in reality, it would then appear that one might as well stick to one-period temporary equilibria only, and their sequences. In the sequel, only these will be discussed, although much of the argument which will occupy the remainder of this Section could be extended to intertemporal models without difficulty.

Consider a competitive capitalist economy without auctioneer or perfect foresight or complete futures markets. Consider this economy in a short time period, a Hicksian "week". Then, contemporary neoclassical theorists would argue, under certain assumptions there exists a set of (current) prices capable of making all agents' current actions compatible, i.e. such as to simultaneously clear all markets in this economy. The assumptions necessary to this result might be criticized as unrealistic[8], but this is not my purpose here. I rather want to analyze some implications of the fact that this set of prices cannot be expected to be hit upon at once on the Monday of the "week", because the

agents cannot generally know the equilibrium in advance, and transactions take time, and cannot generally be reversed, and production and consumption go on in the meanwhile. So, in the period under consideration, the economy will not behave as the equilibrium (let us assume that it is unique[9]) would have it behave. It will be in disequilibrium, with "false price" transactions and productions.

The following period, the economy will generally have different data from those of the previous period (the endowments of the various kinds of capital goods, if nothing else, will have changed), so the equilibrium will be different too; this makes it just as implausible as in the previous period that the agents be able correctly to guess the equilibrium prices and quantities; therefore, again, the economy will be in disequilibrium. And the following period too, and so on. Even if, at a certain instant, the economy happened to hit precisely that instant's equilibrium prices, i.e. the current prices the equilibrium would determine were it to be established at that instant, no one would realise it, since it takes time to make sure that demand is equal to supply; and since, already the next instant, equilibrium would generally require different prices, these almost certainly will no longer coincide with the actual prices. The economy cannot be expected to behave exactly as the temporary equilibrium prices would imply for more than a fledging instant, and then purely by a fluke. And a succession of flukes is not possible. So the economy must be expected to be in disequilibrium, to all relevant effects, all the time.

Let us then distinguish the actual path of this economy from the equilibrium paths which, depending on initial conditions, would be determined by the sequence of temporary equilibria. Now, it is well known that the determination of sequences of temporary equilibria encounters grave problems owing to the non-uniqueness in general of temporary equilibrium (when it can be assumed to exist) and to the largely arbitrary nature of the assumptions about how expectations change from one equilibrium to the next, so that a solid accusation of indeterminacy can be advanced against this type of value theory[10]; but here I want to concentrate on a different criticism, which would apply even

if this indeterminacy could somehow be surmounted; I assume therefore, for the sake of argument, that the sequence exists and is unique. Clearly, it is the sequence of equilibria rather than the single equilibrium which can teach us something, if it can teach us anything, about real economies, since each single equilibrium must necessarily refer to such a short time period that only those characteristics which are going to last for a number of equilibria can be of interest. But all we get is an equilibrium path, which we can be certain will not coincide with the actual path of the economy even in the first period, for the reasons indicated; from the new data (which are off the old equilibrium path) one could start a new equilibrium path, but again only to be sure that the actual path will go off that path too. Therefore, it would seem that the knowledge of an equilibrium path can be of use to explain and predict the actual path of an economy only if one can argue that the equilibrium path is a good approximation to the actual path, i.e. that there is no significant initial divergence, nor rapid accumulation of deviations, of the actual from the equilibrium path (Garegnani 1976).

But any argument of this kind appears to require a theory *of the actual path*: a theory of equilibrium paths is not such a theory, and the frequent assumption that there is an auctioneer who causes the actual path to coincide with the equilibrium path is only a fairy tale.

Interestingly, this assumption of the actual existence of an auctioneer is not to be found in Hicks's *Value and Capital*, the most influential single work in the shift away from the traditional concern with long-period positions; Hicks tried to justify his assumption of instantly reached temporary equilibrium by arguing that the deviations from it of the actual economy would be small, because one could generalize to the whole economy Marshall's analysis of temporary equilibrium in a single market. Now, Marshall's conclusion is that the market will usually finish up very close to the equilibrium price; but it is based on the assumption that, either both buyers and sellers accurately predict the equilibrium price (which must mean that their past experience is a very good guide to the present), or the marginal utility of money of each agent in the

market is constant (an assumption only legitimate in a partial equilibrium context); so Marshall himself is ready to concede that it is a conclusion devoid of general applicability (Marshall 1970, p. 279). Furthermore, Hicks needed to show, not that the final price will be very close to the equilibrium price, but rather that "the transactions which take place at 'very false' prices are limited in volume" (Hicks 1946, p. 129): and Marshall clearly says that this will not be generally true (1970, p. 278). Hicks's assumption, we may add, is particularly arbitrary for the savings-investment market, where, as the studies of economic fluctuations produced by that time by marginalist authors had repeatedly argued, even within the marginalist approach it had to be admitted that the existence of banks and other intermediaries would make the response of the interest rate to discrepancies between the supply and the demand for savings particularly slow.

Thus, Hicks's argument is not satisfactory; and Hicks himself grew more and more dissatisfied with the approach of his 1939 book, up to defining it "nonsense" in one of his latest writings (Hicks 1977, p. 7; also see Hicks 1965, pp. 73-74, and 1983 p. 51 to be quoted later). But afterwards, to the best of my knowledge, no better argument has been proposed[11]. Thus there is still a void in post-Walrasian writings on the question of the connection between equilibrium paths and actual path, owing to the lack of a theory of the actual path.

3. The long-period method

Eatwell 1982, p. 222, has appropriately noticed that, given the framework, it is only natural that GE theorists should try to develop analyses which attempt to embody more and more of the complications which prevent an actual economy from following an equilibrium path: imperfect information, rigid prices, etc. But the precise consequences of the implementation of disequilibrium transactions appear to be impossible to model. It would be necessary (but it will never be possible) correctly to predict each single price and single quantity at each moment of time - actually, each single transaction, since out of equilibrium

in a real economy one cannot even assume the 'law of one price'. Even a consumer does not himself know in advance how much beer he will feel like buying the next day. To say nothing of accidents, deaths, etc. Too much of these events depends on minute and accidental causes which one cannot hope ever to be able to explain and predict in detail. Nor is this a reason to despair and give up the aim of scientific explanation: a physicist does not hope to predict (nor is interested in predicting) the movement of each single molecule in a fluid, nor a biologist the moment each leaf will fall from a tree, etc.

Precisely the reference to the concern with average results in other scientific disciplines may help one to understand the traditional method in value theory, i.e. the one to be found in Smith, Ricardo, Marx, as well as in Marshall, Böhm-Bawerk, Wicksell, J.B. Clark, Robertson, Pigou, etc., even (although with some ambiguities and inconsistencies) in Walras. All these theorists believed that, although it is impossible correctly to describe the forces determining the details of each single transaction, it is still possible to explain and predict the trend of the average of each price or quantity, because the actual path of a price or of a quantity, although unpredictable in its details, will tend to gravitate around and towards definite values or "centres of gravitation", which can be characterized independently of the details of the gravitational process itself. Changes of this "centre of gravitation", caused by changes in the data determining it, could then be used to explain and predict the changes in the trend of the actual path of the variable under consideration; while the existence of this gravitation made the prediction of each single transaction unnecessary.

The idea can be perhaps illustrated at its simplest, in a neoclassical framework, by referring to an imaginary exchange economy where, period after period, the same agents bring to the market the same amounts of goods to be exchanged. Let us assume this economy to be in equilibrium; period after period, everything is repeated in unchanged fashion. If now the equilibrium (assumed, for the sake of argument, to be unique) were one day changed by, say, a once-for-all change in tastes, it would be unrealistic, in the absence of an auctioneer, to expect that the new equilibrium will be reached immediately; it will be

reached, if at all, only after a number of periods during which there will be disequilibrium and price changes; but during this time the new equilibrium remains unchanged, and, assuming the disequilibrium processes to converge towards it, it can be viewed as a 'centre of gravitation' attracting towards itself the actual behaviour of the economy. Owing to a variety of accidental causes, the economy may never actually reach the equilibrium; but, if the gravitation exists, the equilibrium will give a good indication of the trend, or average, behaviour of the economy; and its changes can be used to explain and predict the changes in the actual trends of the observed, disequilibrium magnitudes.

At the risk of being boring - but the issue is absolutely central - I reproduce from Petri (1978) a further example, still internal to the marginalist approach, but now allowing for produced goods. Let us try to imagine how e.g. Marshall or Wicksell or J.B. Clark would have explained the usefulness of equilibrium theory for the analysis of price changes in an economy with no capital goods, only one type of labour and only one type of land, these two factors being hired by entrepreneurs to produce a variety of consumption goods. Let us imagine that the stationary equilibrium of this economy is one day disturbed by immigration: the supply of labour undergoes a once-for-all increase. Marginalist theory predicts that the wage rate will go down until a new full employment equilibrium is reached. Yes, but it will take time. Unemployed immigrants, in the town where they first arrive, will offer to work for less than the employed workers. They will be hired, perhaps causing some of the previously employed workers to lose their jobs. The lower wage will take time to spread throughout the economy. Furthermore, where wages have gone down first, the entrepreneurs, when they sell their products at the old prices, will make 'net profits'; then other entrepreneurs will start bidding up the rents of those lands whose use is so profitable; so rents will start changing too, and this process again will require time to spread throughout the economy. Product prices and quantities will also be changing, and time will be required for them to tend towards the new equilibrium. As in the previous example, the (theoretically defined) new equilibrium is not affected by the trial-and-error processes of

adaptation of supply and demand: it is the "centre of gravitation" to which the economy approaches closer and closer. Thus, the comparison of the old and the new equilibrium allows an understanding of a process of change in real time.

The same authors would have agreed that, if production uses produced means of production, a very similar gravitational process will tend to establish a uniform rate of return over the supply price of the different capital goods. In this case too - the argument would have run - entrepreneurs will move about, entering and leaving industries in their search for positive "net profits". This process will alter the rentals of the capital goods, and hence the rates of return to be earned by buying these capital goods and lending them to entrepreneurs. The prices of the capital goods yielding higher rates of return will go up, owing to the high demand for them, stimulating increases in their supplies (new enterprises will be set up in the industries producing them). But their endowments having increased, their rentals will go down, and, with them, the rates of return obtainable by buying them. These adjustments will go on until the rates of return become equal. Only then prices stop changing. Therefore the prices which qualify as "centres of gravitation" must be associated with a uniform rate of return over the supply prices of the capital goods.

One important aspect of this traditional opinion must be stressed. The *processes* establishing the gravitation around and towards such "centres" were conceived of as real processes happening in real time, and involving mistakes, imperfect information, the implementation of out-of-equilibrium decisions, etc.; as a consequence, the data determining these "centres of gravitation" had to be such as not to be affected by the day-by-day disequilibrium production and consumption activities of the economic agents. The endowments of each type of capital goods could not, therefore, be included among such data, since they would be rapidly altered by the processes of adaptation of capacity and of intermediate products to demand. Even Marshall's short-period analyses, where the fixed plant (not, anyway, the intermediate products) is taken as given, do not contradict this requirement, because Marshall always thought

of them as only partial-equilibrium analyses; he knew that the lenght of the short period could not be taken to be the same for all industries, so that in each given time period in an economy there would be both short-period and long-period adjustment processes going on (where by long-period adjustment processes it is meant processes of alteration of the type and number of plants in an industry), and never attempted to conceive of a general equilibrium with respect to short-period adjustments only. As a consequence, the only positions qualifying as 'centres of gravitation' in an economy-wide analysis were those where the productive capacity in each industry had completely adjusted to demand so as to leave no inducement for capital transfers and hence for price changes. The associated prices - the prices around and towards which day-by-day prices would gravitate - were therefore to be the prices which cover the costs of the means of production and of labour (at the cost-minimizing techniques) and, besides, guarantee a uniform rate of profits *on the supply price* of the capital goods: Ricardo's "natural prices" or Marx's "prices of production" or Marshall's long-period normal values. These were the prices that analysis tried to determine: hence the name of long-period analysis or "method of long-period positions"[12]. The same process was thought to cause relative quantities to gravitate around and towards the quantitaties demanded at the long-period prices[13].

Thus, the concern with the determination of the normal prices - the prices associated with a uniform profit rate on supply price - remained central to the theory of value and distribution across the change in theoretical framework associated with the abandonment of the Classical approach in favour of the marginalist one (Garegnani 1976). Short-period analyses were only concerned with the study of deviations from long-period positions; the determination of the latter was therefore logically prior to short-period analyses. This is particularly evident in the marginalist theories of economic fluctuations, which were seen as deviations - due essentially to fluctuations of the state of confidence or to the power of banks not to adjust their supply of monetary loans to the actual savings - from the long-period equilibrium, which real forces

tended to establish, and which was one of full employment (Milgate 1982, Ch. 2).

It would seem that this traditional method is still now the generally adopted one in applied economic studies (even the neoclassical ones), where usually only the trends and averages of relative prices and quantities constitute the objects of attempted explanations; and where the relative proportions of the capital goods in existence are usually (except perhaps for some very durable, and slow to build, fixed plants) considered *determined*, rather than determining, magnitudes.

It is the concern with the determination of "normal positions", or long-period equilibria, in which the composition of capital is among the variables to be determined, which ultimately explains why traditional marginalist authors had to conceive of "capital" as a *single* factor of production, capable of assuming diverse "forms" without changing in "quantity". A contrast with a Walrasian treatment of capital will clarify the issue.

4. The critique of long-period marginalist theory

It has been said that the attempt to determine a "normal position" would be methodologically contradictory if it included, among the data determining that position, the relative endowments of the different kinds of capital goods in the "normal position" itself[14]. But this contradiction is precisely what one finds in Walras's own formulation of GE - as distinguished from the much later models which I call post-Walrasian. It is useful to dedicate a few pages to this issue, because the methodological contradiction reflects itself in an analytical difficulty faced by Walras, which by contrast clarifies many issues: the role of "aggregate capital" in the long-period versions of marginalist theory, the implications of the "Cambridge critique" for those versions, and the analytical origin of the post-Walrasian developments.

It can be shown that, in the determination of a marginalist GE, the condition (accepted by Walras) of a uniform rate of profits on the supply price of capital goods is generally mathematically incompatible with (what Walras

also did) taking as given the endowments of each kind of capital goods. This result was first shown by Garegnani (1960), and recently discussed (without naming Walras) by Hahn (1982, p. 365). It will be briefly reviewed here with the help of a simple model, not quite as simple as Hahn's, but having the advantage of separating consumption from investment in a clearer way, and of allowing a comparison of a Walrasian and a (marginalist) long-period treatment of capital, and also a discussion of the issue of stationariness.

The model describes an economy with three products. A pure consumption good, c, is produced by labour l, land t, and two circulating capital goods, m and n. The latter two goods are produced, for simplicity, by labour and land only. For all three goods, the production cycle is of the point-input, point-output type, and takes a year; and factors are paid at the end of the year. The capital goods are circulating capital goods, in the sense that, when employed, they are used up entirely in the course of one production cycle. The production functions are differentiable and exhibit constant returns to scale. Factors are paid their value marginal products (the consumption good is the numéraire); it is assumed that there is enough substitutability to allow us to dispense with inequalities. Factor rentals are indicated as w_l, w_t, w_m, w_n; one may imagine (with Walras) that savers buy themselves the capital goods and lend them to entrepreneurs; the rate of interest (or of profits) r is then given by

$$r = (w_m - p_m)/p_m \qquad \text{or} \qquad r = (w_n - p_n)/p_n$$

the two having to be equal in equilibrium (p_m, p_n are the selling prices of the capital goods). It is not necessary to write down the equations referring to each agent, the moment one assumes rigid factor supplies L, T, M, N and a rigid, uniform (gross) saving propensity s: then the existence of only one consumption good eliminates all consumer choice, and Walras's law ensures that whatever does not go to savings goes to consumption. The Walrasian equilibrium equations are then the following:

TABLE 1

1	$c = c(l_c, t_c, m_c, n_c)$
2	$\partial c/\partial l_c = w_l$
3	$\partial c/\partial t_c = w_t$
4	$\partial c/\partial m_c = w_m$
5	$\partial c/\partial n_c = w_n$
5.b	$w_l l_c + w_t t_c + w_m m_c + w_n n_c = p_c c = c$ (i.e. $p_c = 1$)
6	$m = m(l_m, t_m)$
7	$p_m \cdot \partial m/\partial l_m = w_l$
7.b	$p_m \cdot \partial m/\partial t_m = w_t$
8	$p_m \cdot m = w_l l_m + w_t t_m$
9	$n = n(l_n, t_n)$
10	$p_n \cdot \partial n/\partial l_n = w_l$
10.b	$p_n \cdot \partial n/\partial t_n = w_t$
11	$p_n \cdot n = w_l l_n + w_t t_n$
12	$l_c + l_m + l_n = L$
13	$t_c + t_m + t_n = T$
14	$m_c = M$
15	$n_c = N$
16	$(w_m - p_m)/p_m = (w_n - p_n)/p_n$
17	$r = (w_m - p_m)/p_m$
18	$p_m m + p_n n = S$
19	$S = s(c + p_m m + p_n n)$

Equations [5.b], [7.b] and [10.b] can be derived from the others because of Euler's theorem and of the assumption that $p_c = 1$; therefore the in-

dependent equations are 19, as many as the unknowns: c, l_c, t_c, m_c, n_c, w_l, w_t, w_m, w_n, m, l_m, t_m, p_m, n, l_n, t_n, p_n, r, S. Nonetheless, an economically significant solution will not generally exist (Tosato 1969). Only an informal argument will be provided here. *If* the composition of investment, m/n, were given, then there would clearly be overdeterminacy: the model *without* equation [16], but with the additional equation

$$m/n = \text{a given constant},$$

would already have as many equations as unknowns. The same would be true it there were no gross savings, s = 0 (which is the hypothesis made by Hahn 1982)(15). It is therefore crucial, for the possibility of a solution to exist, that there be positive gross savings and that the composition of investment be a variable. Walras's idea, translated into the terms of the present model, was in fact the following: if e.g. $w_m/p_m > w_n/p_n$, there will be an incentive to invest more in good m, i.e. to increase m/n, which will result in an increase of the rental of the factor which is used in higher proportion in the production of m relative to that of n, and hence in an increase of p_m/p_n, which decreases the inequality between w_m/p_m and w_n/p_n. But - even leaving aside other possible difficulties(16) - it is evident that the possibility of satisfying equation [16] through the variation of m/n depends on the amount of total savings. If s=0, the model without equation [16] is already determinate and therefore w_m/p_m and w_n/p_n are determined and generally unequal. If s is positive, the possibility of varying m/n from m=0 to n=0 ensures some variability of the two rates of return which become functions of m/n. As s is increased, if other troubles do not arise the minimum absolute value attainable by $r_m(m/n)-r_n(m/n)$ decreases and, for a sufficiently large s, it may become possible to ensure the equality of the rates of return r_m and r_n. But this means that, for values of s lower than a certain positive value, no solution will exist (De Vivo 1976).

Furthermore, Walras's idea that the composition of investment would be determined endogenously by the process described is also unacceptable: it would

rather be determined by expectations relative to the subsequent period, and these expectations could not be taken to be static expectations, in a model like Walras's where prices can vary drastically from one period to the next owing to the possible rapid variations of the relative endowments of capital goods.

What is the interest of this discussion of a peculiar theory, Walras's own, which nowadays no one defends anyway? In the first place, it makes it possible to see that Walras's problem would not arise if the composition of capital, in our model M/N, were a variable. Thus the analytical difficulty points in the same direction as the observation that the tendency to a uniform rate of return operates by quickly altering the relative proportions of the amounts in existence of the various types of capital goods, so that those proportions should be considered as determined, rather than determining, magnitudes. We are now ready to appreciate the role of the consideration of capital as in some sense a *single* factor of production, to be found in all the other economists who brought the marginalist approach to dominance(17), and who saw better that Walras the implications of the hypothesis of an uniform rate of profit on supply price for the treatment of the relative proportions of capital goods in the equilibrium.

Let us in fact drop the treatment of M/N as a datum. Obviously, both M and N must be treated as variables - taking only one of them as given would make no economic sense. But then the model acquires two degrees of freedom: these would be k, if there were k different types of capital goods. How would traditional marginalist authors 'close' the model? It must be said that not many of them got down to writing the equations of such a long-period general equilibrium: Wicksell (1934, vol. I) was in fact the only one; still, from their analyses one can clearly reconstruct the way they would have gone about it. Of the k degrees of freedom, k-1 derive from the fact that the composition of investment (in our model, m/n) is so far indeterminate: from a mathematical point of view, it might be taken as arbitrarily given, but the logic of long-period equilibrium appears to require that entrepreneurs be satisfied with the capital

they are employing, i.e. do not wish to alter it as long as prices do not change; therefore, both the amount and the composition of investment must be such as not to alter the existing (price-determining) stock of capital (Marshall 1970, p. 315 note 1). In fact, in order to determine the long-period equilibrium corresponding to a given economy, traditional neoclassical authors generally assumed that the economy be *static*, i.e. that tastes be such that net savings be zero (Robbins 1930); then, in our model, equation [19] would change to equation

$$[19'] \qquad S = p_m M + p_n N$$

and the composition of investment would be determined by the additional equation

$$[20] \qquad m/n = M/N,$$

or more generally by k-1 equations (if there were k different types of capital goods) imposing an unchanging composition of capital[18].

There is left one degree of freedom, and it clearly derives from the fact that, so far, the capital endowment is indeterminate. It may now be clearer why the traditional marginalist conception of the various capital goods as each embodying a certain amount of "capital" - a single, in some sense homogeneous factor of production, capable of changing "form" (i.e. physical composition) without changing in "quantity" - played an essential analytical role in their attempt to explain distribution by supply and demand[19]: it made it possible to 'close' the model by adding one equation imposing the equality between supply and demand of this single factor "capital", while leaving the composition of capital to be determined by the uniform-profit-rate condition. Again, the analytics pushed in the same direction as intuition, which suggested that, since (at least in competitive conditions) there must be a distinct homogeneous factor of production behind each tendentially uniform rate of payment to factors (e.g. different types of land behind different rates of rent, and a different type of labour behind each rate of wages), there must be a *single* factor of

production behind the uniform rate of profits. The problem was, of course, in which units to measure both the endowment of, and the demand for, this homogeneous "capital". Ultimately, this had to be measured in value units[20]; and so we find e.g. Wicksell imposing the condition that "in equilibrium the sum total of capital shall have a certain exchange value" (Wicksell 1934, p. 204). In our model, this would mean adding an equation such as

$$[21] \qquad p_m M + p_n N = K,$$

where K is the given (value) endowment of "capital" of the economy.

One may then start better to appreciate the targets and the relevance of the "Cambridge criticisms" of the notion of aggregate capital. These criticisms (see, for a survey, Harcourt 1972) were often misunderstood as aimed only at the legitimacy of aggregate production functions, and perhaps some responsibility for the misunderstanding lies with some of the critical contributions themselves. As a matter of fact, those criticisms were, or should have been, more generally aimed at the legitimacy of conceiving heterogeneous capital goods as embodying amounts, definable independently of prices and hence of distribution, of a single factor "capital". One of the targets of the criticisms should now be clear: the legitimacy of the addition to a marginalist long-period (disaggregated as well as aggregate) equilibrium model, in order to 'close' it, of an equation imposing the equality between supply and demand for "capital", a value magnitude. Marginalist economists were always somewhat uneasy on this point; Wicksell, probably the most rigorous, even wrote: "But it would clearly be meaningless - if not altogether inconceivable - to maintain that the amount of capital is already fixed *before* equilibrium between production and consumption has been achieved. Whether expressed in terms of one or the other, a change in the relative exchange value of two commodities would give rise to a change in the value of capital." (1934, p. 202). But then, as Garegnani (1960, p. 181) points out, it is not possible to determine the equilibrium if one does not know the amount of "capital", and it is not possible

to determine the amount of "capital" if one has not already determined the equilibrium. The theory is indeterminate, i.e. it is no theory.

Hahn (1982) does not seem to grasp the relevance of this point. He uses a two-commodities model in which each good is both a consumption good and a capital good. Having made the assumption of zero gross savings (the products are demanded anyway because they are also consumption goods, so their prices are well-defined, while in my model of Ch. 3, if zero gross savings are assumed, the capital goods are not produced, and their prices must be deduced from the costs of production they would have if they were produced in infinitesimally small amounts), he gets a general equilibrium model where, when the endowments of capital goods are not given and there is a uniform rate of profits on supply price, there is only one degree of freedom (the degrees of freedom relative to the composition of investment have been made to disappear by abolishing investment; the unreality of this assumption has been criticised in Chapter 2 in connection with a different problem, but here it is essentially harmless since it is the remaining degree of freedom which raises the problem under discussion now). He then sees that the model could be 'closed' by adding an equation imposing that the value of the capital goods employed in the economy (the 'demand' for "capital") be equal to a given number, for which he uses the symbol C (our K of equation [2!]); but he denies that this equation would entail arguing in a circle. He does add, though (p. 369): "The problem is the sense to be made of C being given from outside." One might have hoped that he tried to discuss this problem instead of leaving it at that, in view of its importance: taking C as "given from outside" was in fact the universally adopted procedure (with the only exception of Walras) during the rise to dominance of the neoclassical approach.

Hahn might have then seen the following fact. The economic meaning of the equation would be as follows. For any real economy, one could collect data as to its technology, its endowments of labour and land, and the value of its capital stock. On the basis of these data, one might build a long-period general equilibrium model including that equation, and determine the equilibrium

income distribution. But since, in the real economy, the value of the existing capital stock depends on distribution, and would have been different had income distribution been different even if nothing else (neither technology nor the composition of production) had been different, the equilibrium one gets comes to depend on what the observed income distribution is. Thus an explanation or prediction of the trend of the observed income distribution on the basis of the one thus theoretically determined would indeed be arguing in a circle, as Hahn himself appears to admit a few pages earlier (1982, p. 358).

The traditional treatment of capital as a single factor of production, measured as an amount of value, appears to have been based on a deep underestimation of the importance of the dependence of normal relative prices on distribution - as if one could assume relative prices not to vary as distribution varies. In fact, no satisfactory analysis of that dependence existed before Sraffa's *Production of Commodities by means of Commodities* (1960). But once the analysis was there, the results could be seen to go against what marginalist authors had believed. Sraffa has confirmed, to start with, that, as the rate of profits is made to vary, normal relative prices will vary (except in the case of 'equal organic composition' already noticed by Marx - in which case prices are proportional to embodied labour). He has also made it possible to arrive at an extremely simple graphical representation of how the value of capital will change (in terms of whatever numéraire is chosen) with changes in distribution when neither technology nor the composition of production is varied. I will restrict myself here to the case of absence of joint production and of scarce natural resources, annual production cycles, and wages paid at the end of the year. Then, as is well known, the normal price equations are given, in matrix terms, by $pA(1+r)+wl=p$, where A is a Leontief-Sraffa technological matrix, l the (row) vector of labour inputs, p the (row) vector of prices, r the rate of profits, w the wage rate. Having normalized prices and the wage rate by choosing a numéraire, e.g. $p_1=1$, the price equations define w as a function of r, and it can be demonstrated that, for viable economies, the function is downward sloping and its graph in the positive orthant, the w-r frontier

or 'wage curve', exists and intersects both axes (see Fig. 1). Let W

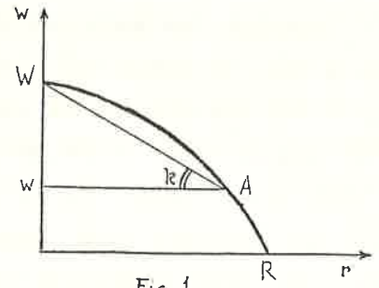


Fig. 1

indicate the vertical intercept (r=0), and R the horizontal intercept (w=0). Let us choose as numéraire the net product of the economy per labour unit, y, a row vector of commodities: py=1. Then the vertical intercept W is necessarily equal to 1=py, since, when r=0, wages appropriate the entire net product. For $w < W$, i.e. $w < 1$, the difference W-w or 1-w is the profits per labour unit; having fixed $r < R$, and indicating with k the corresponding value of capital per labour unit in the economy as a whole, and with w the corresponding wage rate, the following relation must be true:

$$rk = 1 - w$$

i.e.

$$k = (1-w)/r$$

which geometrically is the tangent (taken as positive) of the angle wAW in Fig. 1. It is then immediate, and shown in Fig. 2, that k will increase, decrease or stay constant as r varies, depending on the shape of the w-r frontier: it will remain constant only if the w-r frontier is a straight line, which will be the case only if relative prices do not change with distribution - the pure labour theory of value case. It has been shown that in general the w-r frontier can be concave, convex or alternate concave and convex sections. Thus the dependence of the value of capital on distribution is demonstrated.

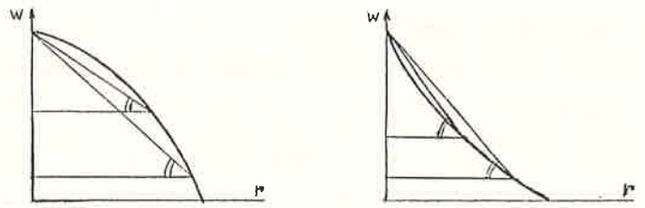


Fig. 2

Sraffa has further shown that, given the productive techniques, when the rate of profits is increased the price of a commodity relative to another commodity may first rise, then decrease (see Fig. 3). This result, Sraffa comments, is incompatible with *any* notion of "capital" as a single factor of production measurable independently of distribution (1960, p. 38). It is perhaps useful to expand on this point.

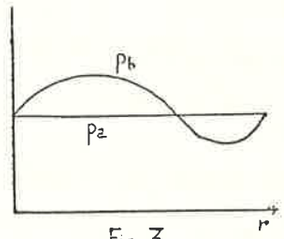


Fig. 3

Let us imagine two goods, say corn and meat, produced by labour and land only, with different, fixed coefficients; and let corn be the more labour-intensive of the two. The relative price of corn to meat, p_c/p_m, will then be an increasing function of the wage of labour relative to the rent of land, w_l/w_t. This is a fundamental result for marginalist theory, as it is the foundation of the mechanism of 'indirect' factor substitution through which consumer choice helps establish the existence of decreasing demand curves for factors of production: if e.g. there is labour unemployment and the real wage goes

down, the more labour-intensive consumption goods will become relatively cheaper, inducing (apart from 'perverse' cases of - it would usually be argued - little empirical relevance) a shift in the composition of consumer demand in their favour, and thus an increase in the average labour-land ratio in the production processes in the economy, and hence, given the land employment, an increase in the demand for labour. But if now we imagine corn and meat to be produced by labour and heterogeneous capital goods, and thus distribution to be between wages and profits instead of between wages and rents, then we might observe that, as the real wage increases and the rate of profits decreases, the relative price of corn to meat first increases and then decreases. If we wanted to see corn and meat as produced by labour and "capital" we would be obliged to conclude that corn is more labour-intensive than meat for certain values of the rate of profits, but more *capital*-intensive for other values, in spite of the fact that no production process has changed. Thus no measure independent of distribution of the "capital" intensity of production processes exists, which might allow "capital" to be seen as a factor of production analogous of labour or land: Böhm-Bawerk's belief that the 'period of production' could be such a measure is thus revealed to have been based on highly restrictive assumptions.

If the critique of circularity to the given K in equation [21] highlights the inconsistencies of the long-period marginalist equilibrium from the side of the *supply* of "capital",the results just summarized also constitute a critique to the *demand* side. They show, first of all, that the 'indirect' factor substitution mechanism may work in a direction opposite to that postulated by marginalist economists: however one wanted to define (independently of distribution) the capital-intensity of a consumption good, it might happen that a decrease of the profit (interest) rate decreased the price of the *less* capital-intensive goods, impeding the functioning of the postulated adjustment mechanism. Further, what was just said about corn and meat might be re-intepreted as referring to two different processes to produce the *same* commodity; the relative price of the commodity when produced with one process to itself when produced

with the other will then indicate which one of the two processes of production is cheaper and therefore tendentially imposed by competition. The possible reversals in the direction of the movement of the relative price mean that it may happen that one process is the cheaper for more than one interval of values of the rate of profits, while the other is the cheaper in between: this is the so-called 'reswitching of techniques' (see the 1966 Symposium in the *Quarterly Journal of Economics*), which confirms that it is not possible to order production techniques according to relative "capital" intensity independently of distribution. Any criterion for thus ordering techniques would oblige one to admit that a decrease of the rate of profits may result in the adoption of *less* capital-intensive techniques, implying an upward-sloping 'demand' curve for "capital". Particularly important is that this may also happen for the value of capital: as shown in Fig. 4 utilizing the graphical device described previously, one may obtain what has been called 'reverse capital deepening', a decrease of the rate of profits which causes the adoption of techniques having a *lower* value of capital per labour unit.

Thus, *even if* it were not illegitimate to include among the data of the equilibrium the amount of "capital" measured as an amount of value, still the theory would not have plausibility because the relationship between rate of profits and value of capital per labour unit cannot be presumed generally to yield a stable[21] nor an unique intersection with the "supply" curve (see e.g. Garegnani 1970). We will later see the importance of this result for macroeconomic models.

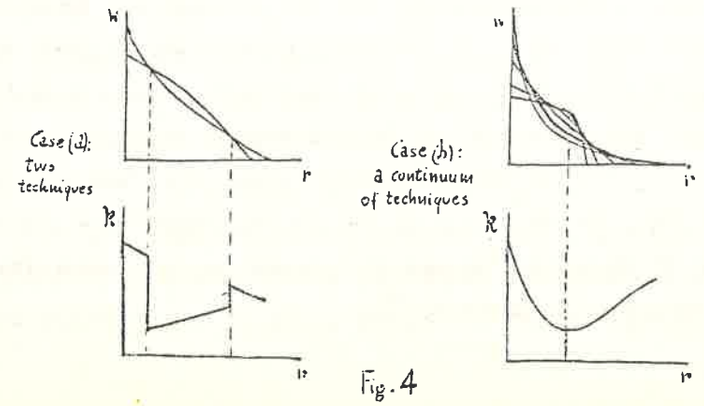


Fig. 4

But the fact that the marginalist approach is incapable of determining long-period equilibria in a logically consistent way does not mean that long-period, i.e. normal, prices cannot be determined. Sraffa's positive contribution has been to show that they *can* be determined once we go back to the Classical framework. Thus there *is* a logically consistent approach to value within the traditional method. Something more on this question will be said in the concluding chapter of this essay. Now let us go back to the post-Walrasian approach.

5. *Post-Walrasian theory and the microfoundations of neoclassical macroeconomic models*

It was said above that Walras was the only one, among the founders and early developers of the marginalist, or neoclassical, approach to value and distribution, who did not treat the capital endowment as a single magnitude; but, as explained in the previous chapter, he ended up with an inconsistent model. It can be argued (Garegnani 1976, Petri 1978, Eatwell 1978, Milgate 1982) that the post-Walrasian models can be seen as the logical outcome of the attempt to avoid Walras's inconsistency in the treatment of capital while keeping to the approach to value and distribution as determined by supply and demand schedules - but without going back to the treatment of capital as a homogeneous factor. The post-Walrasian way out consists in fact of: 1) maintaining the given endowments of each kind of capital good, but 2) abolishing the uniform rate of profit on supply price, i.e. abolishing the condition that the capitalized value of the existing capital goods be equal to their cost of production, and 3) substituting in its place the condition that the profit rates to be earned (in the future) on the *newly* produced capital goods be equal: expected profit rates, in the temporary equilibrium versions, or profit rates derived from futures markets, in the intertemporal equilibrium versions[(22)]. The result is the very-short-period equilibrium models which have been criticised earlier on. Thus, the difficulties of Walras's own model were circumvented at the cost of abandoning the method of normal positions which Walras was -inconsistently - trying to adhere to[(23)].

It has also been convincingly argued that the shift to such post-Walrasian notions of very-short-period equilibrium was mainly motivated, and not only in the influential case of Hicks's *Value and Capital*, precisely by a perception - albeit not very clear - of the difficulties inherent in the treatment of "capital" as a homogeneous factor measured in value terms (Garegnani 1976, pp. 133-138; Milgate 1979).

The full implications of the change in the notion of equilibrium do not seem to have been widely appreciated, though, as witnessed by the imperfect (at times, completely lacking) awareness that such a change has occured. The analytical point stressed above about the treatment of the composition of capital as given or not is a useful way of sharpening the issue. Many different aspects of the present situation of value theory, and not only of value theory, become easy to understand with that change in the nature of the data of the equilibrium in mind. For instance, the very frequent assumption in GE models of a *given* number of firms becomes at least not totally nonsensical (although still highly criticisable) in view of the extremely short time period for which the data of the model can be considered unchanging. Also, the high dependence of some of the data on the instant chosen to (hypothetically) collect them and the consequent very-short-period nature of the decisions based on such data make it natural, when money is introduced, to include the amount of money held by each agent among the data affecting his decisions and hence among the data of the equilibrium; in traditional analyses, on the contrary, the distribution of money (or at least, of the amount of money to be used for transaction purposes) among the agents was endogenously determined[(24)]. Consistency then appears to require that the money debts of each agent be also taken into account, which means that the possibility of bankruptcies can no longer be left out of the determination of the equilibrium; with additional difficulties for the demonstration of its existence.

Above all, it becomes easy to understand why, notwithstanding frequent recognitions of the insufficiencies of the analysis, post-Walrasian stability analyses are confined either to models without reproducible means of production

or with only one capital good, or else to unreal adjustment processes happening outside real time: the adjustment processes considered cannot be such as to change the data of the model; therefore, if there is heterogeneous capital, no production must take place before the equilibrium is reached, otherwise the endowments of the various types of capital goods might change considerably, and the original equilibrium would no longer be there to be reached. The difference is radical from traditional analyses based on long-period positions: these positions, being based on data independent of the gravitation itself[25], could be considered the centres of gravitation of real, time-consuming processes of trial and error involving production and consumption[26].

Thus, if one reads the attempts to explain economic events, fluctuations, crises etc. of the old marginalist economists such as Marshall, Wicksell, Pigou, Lavington etc. one cannot help being struck by how *sensible* their analyses were, and how strict the connection was between theory and interpretation of real events, when compared with the situation nowadays: what contemporary mainstream value theory offers for an understanding of reality, according to no less than Hahn himself (Hahn 1973), is little more than negative recriminations to the effect that unfortunately in real economies there is no auctioneer, no complete futures markets, no perfect information, no perfect competition, etc....so that reality does not behave like an Arrow-Debreu world. No wonder that there should be increasing dissatisfaction with the state of value theory! One also wonders whether post-Walrasian value theory would have become so dominant, had not the ground been prepared by the total acceptance of the marginalist approach - originally in its versions internal to the traditional method - to the point of making the existence of the marginalist mechanisms of supply and demand an unquestioned faith.

Be that as it may, one might wonder why it was necessary to have such a long digression on the old, long-period versions of marginalist theory, explicitly based on a conception of "capital" as a single factor of production, which would no longer be defended nowadays. Why not concentrate exclusively on the shortcomings of marginalist, or neoclassical, value and distribution theory as expounded and practiced nowadays?

The reason is, I will contend, that long-period modes of reasoning have never really been abandoned - when trying to understand reality - by mainstream economists, who often wrongly believe that those modes of reasoning can be supported by referring to post-Walrasian value theories, while exactly the opposite appears to be the case: it is the usefulness of the latter theories which appears to require the legitimacy of long-period marginalist reasonings, i.e. of the traditional conception of "capital" as a factor of production.

As argued in Chapter 2, without some supplementary argument the knowledge of a post-Walrasian equilibrium path tells *us nothing at all* about the actual path an economy might follow. But now let us suppose that an independent argument existed, to the effect that real economies exhibit a persistent tendency towards the full employment of labour (and obviously also a tendency of investment to be determined by savings). One could then argue that the divergencies discussed in Chapter 2 between actual and equilibrium paths are going to be of secondary importance only, since the sequences of temporary equilibria will differ from the actual path of the economy only because of frictional divergencies of employment from the equilibrium one, and because of mistakes in the composition of output and purchases relative to those required by the equilibrium path: divergencies of secondary importance only - one could argue - for the macropolicy questions which are those usually requiring a general, rather than a partial, equilibrium framework.

(The same argument could be advanced for the case of given real wages; then the tendency would have to be towards the level of employment associated with the given real wage, depending on the economy-wide marginal-product-of-labour curve, a curve implicitly assumed not to change drastically in short periods).

If this is, as I suspect, the reason why many think that a representation of an economy's evolution as a sequence of temporary equilibria or as an intertemporal equilibrium is not illegitimate, the problem remains that an *independent* argument for the tendency to full employment (or to the level of employment

associated with a given real wage) must be provided.

It would seem that the only such argument around is found in mainstream macroeconomic theory; but, I will try to argue, it relies on mechanisms, which are implicitly based on traditional, long-period marginalist analyses (i.e. analyses where capital goods are seen as embodying "capital", a single factor), and therefore it falls under the "Cambridge criticisms".

It is necessary to turn to macroeconomic theory because, in the present division of teaching and theorizing in economics into micro and macro, it is only in the macro courses that the existence of a tendency to full employment in real economies is discussed. Let us leave "heterodox" schools of thought (Marxian, Kaleckian, etc.) aside, since, in these, one does not find any argument for a spontaneous tendency to full employment. The other contending schools: 'neoclassical- synthesis' Keynesians, monetarists, 'new classical economics', supply-siders, all share a certain core of theoretical beliefs as to the real forces at work in actual economies: by and large, the core which was shared both by Keynes and by the orthodox economists he was criticizing. It is this core which makes it possible to argue that, were it not for the Keynesian disturbances (if one admits them), a competitive market economy without wage rigidities would gravitate around a full employment situation. The fundamental elements in this core appear to be:

1) the idea that there is a decreasing (and only slowly shifting) aggregate demand curve for labour;

2) the idea that there is a supply-and-demand mechanism which - unless inhibited from working properly - brings investment into equality with the savings forthcoming at any given level of employment[(27)].

(These two ideas have in fact in neoclassical analysis a common root, the marginalist postulates about long-period factor substitution; but in order to make my point clearer, I will treat them separately).

In the following two chapters, largely basing myself on Garegnani (1976, 1978-9), I will argue that both ideas need a recourse to long-period marginalist analysis (the context in which they first emerged) for their plausibility.

This claim is likely to sound bizarre to most readers, because the standard macroeconomic models are usually seen as determining short-period equilibria where, following Keynes - or so it is claimed -, the capital equipment is given in physical terms; hence there would appear to be no need to conceive of capital as a homogeneous factor capable of changing "form" without changing in "quantity".

But, I will try to argue, behind such views there lie some confusions as to the meaning of short period and the logic of the analyses based on macroeconomic models.

It seems, first of all, necessary to distinguish the post-Walrasian notions of equilibrium from the Marshallian and Keynesian notion of short period: in the latter two authors the short period equilibrium is still a state towards which the economy (in Marshall, the single industry) gravitates owing to real-time adjustment processes; this seems to entail that in Keynes's equilibrium the endowments of circulating capital goods, as well as the short-period expectations, cannot be included among the data of the short-period equilibrium (Petri 1984). Most subsequent authors have, on the contrary, followed Hicks in interpreting his own IS-LM formalization of Keynes within the framework of *Value and Capital*, i.e. as a temporary equilibrium (e.g. Lange 1942, Patinkin 1956, Arrow and Hahn 1971); and probably, owing to this interpretation, the applicability and fruitfulness of Keynes's analysis and more generally of analyses based on macroeconomic models have supported the faith in the importance of post-Walrasian equilibria. It is therefore particularly interesting that, recently, Hicks himself should have admitted that this interpretation, as an interpretation of Keynes's analysis, is not defensible: "Keynes's (he said) was a 'short-period', a term with connotations derived from Marshall; we shall not go far wrong if we think of it as a year. Mine was an 'ultra-short period'; I called it a week. Much more can happen in a year than in a week; Keynes has to allow for quite a lot of things to happen. I wanted to avoid so much happening, so that my (flexprice) markets could reflect propensities (and expectations) as they are at a moment... a very artificial device, not (I would think now) much to be

recommended" (Hicks 1983, p. 51). He then goes on to argue that a temporary-equilibrium interpretation of the IS-LM model would make little sense, because within the single "week" of a temporary equilibrium: "The actual outputs of products, and probably also the actual input of labour, would be largely pre-determined" (p. 55); while in the IS-LM model (as well as in Keynes) the equilibrium output and employment are endogenously determined.

He might have added that macroeconomic theorists, when they use their models for analyses and predictions of real economic events, clearly assume that the equilibrium of the model adopted can be considered a good approximation to the actual behaviour of the economy, because the economy gravitates towards it, and that the position of the curves (the shape of the functions) determining it does not change rapidly over time, so that comparative static exercises can be used for predicting the effects of changes of parameters: so he might have concluded that the practice of macroeconomists too (including the practice of Keynes) would be at odds with the interpretation of the IS-LM model as a temporary equilibrium.

It would seem, then, that the equilibrium determined by macro models must be seen as a centre a gravitation of real adjustment processes, requiring time, during which time the composition of capital can at least partly change (e.g., in the year suggested by Hicks, it could change quite considerably). The fact that macroeconomists nonetheless speak of a given K strongly suggests that they are, in essence, leaving out of the analysis those variations of the stock of capital brought about by *accumulation*, but as for its composition they are treating it as largely endogenously determined; and nonetheless they are believing one can in some meaningful way say that the factor endowment of the economy is given, i.e. they are still going about the treatment of capital in the same way as the traditional marginalist authors who believed in "capital". Nor should the survival of such modes of reasoning be found surprising, in view of the fact that until the "Cambridge controversies" such a treatment of capital was generally found unobjectionable, and, what is perhaps even more important, not in contradiction with the different, post-Walrasian treatment to be found

in the researches in pure value theory. Thus e.g. Milton Friedman felt he needed not distinguish Wicksell from post-Walrasian theories when writing:

> Thanks to Wicksell, we are all acquainted with the concept of a "natural" rate of interest and the possibility of a discrepancy between the "natural" and the "market" rate....This analysis has its close counterpart in the employment market....The "natural rate of unemployment", in other words, is the level that would be ground out by the Walrasian system of general equilibrium equations...." (Friedman 1968, pp. 7-8).

And Solow, after co-authoring the famous *Linear Programming and Economic Analysis* (where equilibrium theory is clearly seen in the post-Walrasian framework), went on to write the even more famous articles on neoclassical one-good growth models.

These general interpretative observations will now be corroborated by a more detailed discussion of the decreasing demand curve for labour and of the investment function, in the next two chapters.

6. *The demand curve for labour*

In the old marginalist authors, it was clear that the notion of an economy-wide marginal-product-of-labour curve to be used in the explanation of wages required, in order to be defined, that the unchanging quantity of capital changed 'form' as the amount of labour varied, so as to take the 'form' best adapted to the amount of labour available (see e.g. Clark 1902, pp. 114-115; Robertson 1931, p. 227). We can again rely on Hicks to see why. In *The Theory of Wages* he wrote that, when there is a change in wages, since

> "one of the co-operating factors - capital - is, at any particular moment, largely incorporated in goods of a certain degree of durability... the change in conduct which follows from the change in relative profitability cannot immediately be realized... In the short period, therefore, it is reasonable

to expect that the demand for labour will be very inelastic, since the possibility of adjusting the organization of industry to a changed level of wages is relatively small... Since the whole conception of marginal productivity depends upon the variability of industrial methods, little advantage seems to be gained from the attempt which is sometimes made to define a "short period marginal product" - the additional production due to a small increase in the quantity of labour, when not only the quantity, but also the form, of the co-operating capital is supposed unchanged. It is very doubtful if this conception can be given any precise meaning which is capable of useful application" (Hicks 1932 pp. 20-21)[28].

In other words: in order to be able to assume that the demand curve for labour has the elasticity necessary to give plausibility to the theory that the wage is determined by the interplay of supply and demand[29], one must allow the "form" of "capital" to change as the wage level changes, the industrial methods adopted becoming the ones best suited to the varying amount of labour employed. If the "form" of the existing capital stock were kept fixed as the amount of employment were varied, the "short period marginal product" curve for labour would, realistically, be constant or increasing (owing to the usually increasing efficiency of production up to the level of production for which the plants were conceived) up to a point, after which it would fall very sharply owing to the existence of "a reasonably definite limit to the quantity of labour that can be employed with a given productive equipment" (Garegnani 1976, p. 62, n. 48). A decrease in the real wage could therefore be expected to bring about an increase in the demand for labour of any significance only by changing the capital goods in use so as to make them appropriate to a higher level of employment. Thus each point on the labour demand curve must be understood as being associated with a different "form" of capital. (An additional point Hicks appears to make in the passage quoted is that some change in the capital goods associated with a changing amount of labour *must* be admitted: one cannot produce more steel without using up more iron one etc.; thus an element of

arbitrariness, depending on the length of the period chosen, would necessarily be introduced by any definition of the short-period marginal product of labour, without doing away with the need to conceive of capital as, at least partially, changing "form").

The need to conceive the "form" of capital as a variable can be seen also by considering that, if the composition of capital were a datum of the equilibrium, it would be illegitimate to consider it as unaffected by all the changes associated with the change in wages, and then the change in the composition of capital might (e.g. owing to a change in the amount of raw materials available) cause a considerable shift in the position of the schedule of the marginal product of labour, depriving comparative-static exercises of validity; for the same reason, the possibility of a zero marginal product of labour at full employment could not be easily dismissed, depending on whether there were enough stocks of intermediate goods.

Neoclassical theory must then rely on its traditional conception of capital as something capable of being treated as a single, homogeneous factor of production susceptible of changing "form" (i.e. composition) without changing in "quantity" in order to be able to argue:

a) that it makes sense to conceive of the quantity of capital in the economy as not having changed simply because relative prices (and hence technical and consumption choices and hence the composition of capital - remember we are not in the timeless auctioneer world) have changed consequent upon the change in the real wage;

b) that the change in optimal techiques, induced by the decrease in the real wage, will be such as to induce entrepreneurs to employ, with the unchanged "quantity" of capital (albeit in a different "form"), a greater amount of labour.

It is not clear how else one could argue that, after the change in prices and in capital composition, a lower real wage should bring about a higher labour demand.

One may then better appreciate the importance of the critiques of neoclassical capital theory, briefly remembered in Chapter 4. These have shown

the lack of foundation for both propositions a) and b); they have shown, in other words, that the neoclassical postulates about long-period factor substitution are without foundation, and, with them, is without foundation the faith in the existence of decreasing demand curves for factors.

7. *Investment*

The diffusion of post-Walrasian modes of thought appears to have obscured the issues in the theory of investment even more than for the labour demand curve, so that there appears to be no consensus even among neoclassical economists on how to treat investment(30). Among those temporary equilibrium theorists who do discuss the problem, the number of ad-hoc assumptions is quite high (e.g. Bliss 1983 introduces further fabulous agents besides the auctioneer, in order to get round the problem that when expectations are not uniform the most optimistic agent will push all other competitors out of the market for savings thus endangering the hypothesis of competition); and concern has generally been with existence only, no one, as far as I know, discussing the stability of the savings-investment market outside a one-capital-good world. Because of the lack of any general discussion of the question, it seems to be broadly correct to assert that the adaptation of investment to savings is simply *assumed* by temporary equilibrium theorists, on the basis of a little-discussed belief that this very usual assumption can be extended to temporary equilibria. But can it?

One has to turn to discussions of economic policy to find out what justifies such an assumption - and one then finds that the reason is still the one advanced by the long-period neoclassical theorists, namely, the role of the rate of interest as the price capable of bringing investment into equality with savings. And why so? This is not usually explained in these discussions; clearly, it is considered an accepted fact. Now, accepted facts are explained in textbooks; but when one turns to these, one finds that the explanation relies on the traditional, long-period reason: the belief, shown to be false by the recent results in capital theory, that the demand for capital (treated as a homogeneous factor)

is a decreasing function of the rate of interest(31).

What is perhaps not easy to grasp immediately is that this is - it would seem - the only possible justification for the belief that investment is a regularly decreasing function of the interest rate. Although there undoubtedly is room for further research on this topic, the argument to that effect by Garegnani (1978) appears to be already quite persuasive:

> "Indeed, if we were to take literally the claims of these theories, and to confine ourselves to considering the capital goods as physically specified elements of a given productive equipment... it would be difficult to see how we could ever provide any theoretical basis for the notion, plausible as it may seem, of a demand function for investment elastic with respect to the rate of interest... we would be faced by a multiplicity of factors, each of which may influence the demand for investment... the disproportions between available equipment and the level of demand for products in each industry; the age structure of existing equipment and the connected irregular replacement requirements, etc. Above all, the hypothesis of a given productive equipment... not adapted to the state of demand for products, would force us to attribute a decisive role to the expectations which the entrepreneurs entertain about future changes... The attempt to determine the effects on investment of changes in the rate of interest on such indefinite grounds would seem liable to dissolve into casuistry concerning the influence of these changes on the expectations of entrepreneurs. And this influence would differ from situation to situation, thus making impossible any general and unambiguous conclusion concerning direction and intensity of the effects of interest on investment..." (Garegnani 1978, pp. 36-37).

For instance, the price changes induced by changes in the interest rate might shorten the economic life of old fixed capital still in use, and thus induce an increase in investment, both for sufficiently large *increases* as well for *decreases* of the interest rate. Or, if a decrease in the interest rate induced

of the interest rate. Or, if a decrease in the interest rate induced an expectation of further decreases, borrowing and investing would be postponed, i.e. investment would decrease.

The belief in the possibility of reaching general and unambiguous conclusions relative to an inverse relation between rate of interest and investment therefore clearly points to an implicit reliance on the persistent forces presumed to cause the demand for "capital" to be a decreasing function of the rate of interest - a reliance, therefore, on the traditional, long-period neoclassical analyses.

8. Summary and Conclusions

At this point it is possible to summarize the implications of the above arguments as follows.

It is illegitimate to send one back to post-Walrasian analyses for the rigorous justification of the neoclassical or IS-LM macro models which are used to explain and predict real-world events. Those simple models summarise a "vision" of the forces at work in a capitalist economy, which must rest on the traditional, long-period versions of marginalist theory, with their indefensible treatment of capital; without this theoretical basis, those forces have no plausibility; but without those forces, there is no tendency towards equilibrium, and then post-Walrasian analyses have no connection with the actual path of a market economy. Thus the defensive détour attempted by neoclassical theorists, who have abandoned the long-period method in order to try to dispense with homogeneous capital without abandoning an explanation of prices and distribution in terms of demand and supply schedules, comes out to have been all the time a false trail, since it presupposes, in order to make sense, the existence of those long-period tendencies it should have validated.

Now, faith in those tendencies came into existence, with the founders of marginalism, on the basis of long-period formulations of their theory. These long-period versions are nowadays no longer defended, and the result is that neoclassical analyses of real-world economies have no longer any foundation; only the imperfect understanding of the change which has occurred in the notion

of equilibrium prevents neoclassical economists from grasping the disastrous state of their theory, by making them illegitimately believe they can still use reasonings originating in the long-period versions of their theory in spite of their abandonment of those versions. Going back to what was said in the *Introduction* of the present paper, one may conclude that the critique of traditional neoclassical capital theory undetermines not only the long-period versions, but rather all versions, and with them the whole "vision", of the marginalist or neoclassical approach to value and distribution.

This internal critique reinforces and, in a sense, may help understand the dissatisfaction which in numerous fields of applied economics is frequently voiced vis-a-vis the marginalist approach, which many economists find incapable of accounting for important aspects of the working of labour markets, of investment, of international trade, of underdevelopment, of industrial structure, etc. The internal critique gives decisive support to the thesis that the fundamental forces at work to determine value, outputs and distribution in a market economy cannot be those envisaged by the marginalist tradition; it is then only to be expected that grave difficulties should often arise when trying to reconcile the marginalist approach with what reality shows.

In the face of this crisis of the dominant approach, the "Sraffian" proposal is that the economic profession should fully re-adopt the fruitful traditional method based on the notion of gravitation around long-period positions (with its implication that the composition of the capital stock should be explained via the composition of demand and technical choices rather than vice-versa - a method of reasoning which appears never to have really been abandoned in applied studies); that the neoclassical approach to value and distribution should be abandoned, as logically indefensible; and that one should turn instead to - as a starting point for a reconstruction of economic analysis still largely to be accomplished - the alternative approach of the Classics: Adam Smith, Ricardo, Marx (reinterpreted in the light of Sraffa's work of clarification, see Garegnani 1983, 1984; Eatwell 1977, 1978; Tucker 1960), as improved by Bortkiewicz, Sraffa and others on the question of price formation, and by the

principle of effective demand of Keynes and Kalecki on the explanation of outputs and employment. What is absent in all these authors[32], when they are compared with the marginalist tradition, is the idea of "factor substitution" which is the foundation of those decreasing demand schedules for factors of production, on which the whole marginalist edifice rests, and which have been shown generally not to exist. Now, the role of those decreasing demand schedules for factors was to give plausibility to the existence of a tendency towards the full employment of factors, which would at the same time cause a tendency of their rates of remuneration towards the full employment levels: thus they allowed the simultaneous determination of distribution, technical choices, prices and quantities. If one discards demand schedules, one is left in need of a theory of distribution, and of a theory of outputs and employment, but not of a theory of relative prices, because, as shown by Sraffa, the universally accepted existence of a tendency of competitive prices towards 'costs of production', i.e., in Classical terminology, the gravitation of the rates of return to capital in the different industries towards uniformity, will suffice to determine technical choices and long-period or 'normal' competitive prices, once the quantities produced, and either the real wage or the rate of profits, are determined. Imperfect competition, barriers to entry, etc., can then be added in order to explain persistent profit rate differentials.

As to distribution, as well as outputs and employment, one is far from starting from scratch. On the contrary, the probable basic elements of a reconstructed Classical approach appear to be already now quite popular with large numbers of economists.

For the theory of output and employment, we are now in a much better position than the Classics because we have grasped, thanks to Keynes and Kalecki, the principle of effective demand, i.e. the tendency of savings to adjust to investment via variations of the level of output; what is needed most is an analysis of what determines investment, i.e., in Classical terminology, a theory of accumulation; here Smith, Ricardo and Marx can offer many insights on the importance of the division of labour, the extension of the market, the

role of profits, the factors affecting the pace of technical progress, the possibility of crises, etc.; and many further non-neoclassical elements can be derived from Aftalion, Tugan-Baranowsky, J.M. Clark, Schumpeter, Harrod, Steindl, Goodwin, the empirical surveys on the determinants of investment, etc. As for income distribution, the Classical idea that it is determined by a complex process of class conflict and bargaining in which the rate of unemployment is only one, although a very important one, of the elements affecting the relative bargaining power of the parties (another crucial element worth mentioning here being the government policies, e.g. monetary policy, and attitudes to the labour movement), appears to be a very fruitful starting point for historically specific analyses which will have to take into account e.g. the possible recourse to inflation, to incomes policies, to anti-unions deflationary policies, to 'social contracts', to the threat of insurrections or of dictatorships, to institutional peculiarities, etc.[33]

Thus probably economists will have to renounce the dream of explaining nearly everything on the basis of very few universal principles, and accept to dirty their hands with history and institutions much more than is on average the case now. Such an approach, if perhaps less aesthetically appealing than the marginalist simultaneous explanation of everything in one single theoretical edifice, promises to be much more fruitful, both because of its being free of logical inconsistencies, and because of its flexibility, whick makes it compatible with different explanations of real wages or of accumulation, etc., thus allowing a better understanding of a diverse and historically changing world.

Notes

(1) Revised text of a talk delivered at seminars at the Universities of Massachusetts (Amherst), Harvard, Bremen, Siena, Catania, and Paris X (Nanterre) between February 1984 and May 1986. Helpful comments from the attendands to the seminars, and in particular from M. Di Matteo, H. Kurz, L. Punzo, J. Kregel, C. Bidard are gratefully acknowledged. Thanks go to the Italian Ministero della Pubblica Istruzione and to the Italian Consiglio Nazionale delle Ricerche for financial support.

(2) See, e.g., Blaug (1980), p. 202; Hahn 1974, p. 14; Bliss 1975b, p. 117, n. 22.

(3) The term "Sraffians" has been preferred here to "neo-Ricardians" because the latter term is often used to stress a supposed anti-Marxian nature of the analyses of Sraffa, Dobb, Garegnani, Bharadwaj, Eatwell, H. Kurz, Schefold etc. which is just not there (see Garegnani and Petri 1982, Garegnani 1984). It must be added, though, that some authors who consider themselves Sraffians, e.g. Pasinetti, Nuti, Mainwaring, do not appear to share the views summarised in the present paper as to the relevance of the "Cambridge critique" for the GE versions of neoclassical theory. "Neoclassical", by the way, is also highly misleading in its suggestion that the approach to value and distribution so labelled is a renewal of the Classical one, rather than a drastic departure from it (Garegnani and Petri 1982); terms like "marginalist" or "supply-and-demand" are more correct and will be often preferred in this paper.

(4) The term "long-period positions" has been proposed by Garegnani (1976) to cover both the marginalist notion of long-period equilibrium and the Classical notion of "natural prices"; these two notions share a common analytical element, the presence of an uniform rate of profits on the supply price of the capital goods, but are otherwise different enough to warrant a new term to cover them both. Hayek, Lindahl, Hicks were the originators of the move away from the traditional notion of long-period equilibrium, in the years around 1930 (Milgate 1979). But only with Hicks's *Value and Capital* (1939) and Lange's "Say's law: a restatement and criticism" (1942, but an early version was read at an Econometric Society meeting in 1938) did the new notion of equilibrium start gaining widespread acceptance; and already in Lange what was clear enough in Hicks, i.e. that it was a *new* notion of equilibrium, is no longer said; as argued below in this paper, the change in the notion of equilibrium appears to have gone unnoticed by the greater part of the economic profession, even by Arrow 1959.

(5) So-called 'disequilibrium' fix-price models (as developed by Drèze, Benassy, Malinvaud, Grandmont etc.) share with the other GE models the treatment of the capital endowment, and are therefore, for the argument of this paper,

included among the post-Walrasian models; also see below, note 26.

(6) See some indications of disappointment with the latter program by GE specialists in Hahn 1973, 1982b, p. 1; Fisher 1976; Weintraub 1979, pp. 124-5; and, more generally, Phelps Brown 1972, Worswick 1972, Blaug 1980. No one, though, traces the roots of the unsatisfactory state of neoclassical theory to the reasons suggested in the present paper.

(7) See, e.g., Grandmont (1977), Fitoussi (1983).

(8) As is well known, the introduction of expectations and of money causes serious difficulties to the proof of the existence of an equilibrium, see Arrow and Hahn 1971, Grandmont 1977; also see the beginning of Chapter 7 below in the text.

(9) This assumption is made only for the sake of discussion. It is worth stressing, anyway, that a theory of value, in order to be acceptable, should be capable, it would seem, of yielding uniqueness except in isolated cases of either very low likelihood or recognized correspondence to observed events: and that neoclassical general equilibrium theory does not satisfy these requirements.

(10) For a similar argument and a contrast with the treatment of expectations in the traditional method, see Garegnani 1976, p. 140.

(11) On the contrary, one finds occasional recognitions of the problem, but to no avail. Thus Bliss has writen. "Does is not take time to establish equilibrium? By the time equilibrium would be established will we not have moved on to another 'week' with new conditions, new expectations, etc.?" (Bliss 1975, p. 210) but has continued working on temporary equilibrium. Morishima 1977, pp. 80-81 clearly sees the non-coincidence of the actual and the equilibrium paths, and quotes Walras to the same effect, but does not draw any critical conclusion from his observations. Hahn 1984, p. 4 writes: "... imposing the axiom that the economy is at every instant in competitive equilibrium simply removes the actual operation of the invisible hand from the analysis", but does not seem to notice the problem discussed here. Duménil and Lévy (1985) criticize neo-Walrasian equilibria along lines similar to those of the argument of this paper, but fail to notice that those critiques do not apply to all versions of neoclassical theory, since the 'gravitation' method which they incorrectly ascribe exclusively to Classical economics was in fact shared by the founders and developers of neoclassical theory for at least 50 years. Also see note 26 below.

(12) For synthetic descriptions of the method based on long-period positions one may consult Garegnani 1976, Eatwell 1982, Petri 1978, Milgate 1982. But a full grasp requires a study of at least some instances of the method at work. One might do worse than start from Marshall and the writings of the Marshallian school on the trade cycle; Ricardo is even better.

(13) The differences between the Classical and the supply-and-demand approaches to value and distribution also entailed different approaches to the determination of these quantities, see Garegnani 1984.

(14) Nor can a long-period position be determined as the asymptote of an intertemporal equilibrium converging - as the recent researches on the equivalence of intertemporal equilibria and turnpike paths suggest may happen (Bewley 1982) - to steady growth, because the economy cannot be expected to follow the equilibrium path, so the problem of the divergence between equilibrium path and actual path remains. Besides, the assumptions necessary for such a convergence are quite unrealistic (e.g. no scarce natural resources), and, finally, a steady growth path determined by the growth rate of population is closer in its nature to a secular than to a long-period equilibrium (see note 18 below) and as such answers different questions from those which a long-period equilibrium addresses.

(15) If s=0, then $S=m=n=l_m=t_m=l_n=t_n=0$. Hence $l_c=L$, $t_c=T$ and equations [1] to [5] plus [12] to [15] form a self-sufficient system of 9 equations in the 9 variables c, i_c, t_c, m_c, n_c, w_l, w_t, w_m, w_n. Owing to the constant-returns-to-scale assumption, p_m is anyway well-defined by equations [6] to [8] (although it is now the *virtual* cost of producing one unit of m) because the optimal l_m/t_m ratio is determined by w_l/w_t independently of scale; and the same is true of p_n. Thus for s=0 all magnitudes appearing in equation [16] are already determined, so equation [16] cannot be satisfied except by a fluke.

(16) As m/n is varied, w_m/w_n will also change because l_c and t_c will change; and it may happen that w_m/w_n changes in the same direction as p_m/p_n. If the production of capital goods used capital goods as inputs too, then this would tend to happen if each capital good used itself as an input in a higher proportion than it used the other.

(17) See, for a demonstration, Kregel 1976; also Garegnani 1960, 1970.

(18) Garegnani (1960, Appendix E; 1976, pp. 134-5), following Robbins 1930, has stressed that a static long-period equilibrium should not be confused with what Marshall would have called a *secular* stationary equilibrium, in which the constancy over time of the quantities of factors is the result of an equilibrium of the forces tending to alter the factor endowments: a state far removed from real economies. On the contrary, if the marginalist conception of "capital" as a single factor and the associated analysis of substitution mechanisms had been correct, then it would seem that Marshall, Wicksell, J.B. Clark etc. would not have been wrong in thinking that the *static* hypothesis would not prevent the conclusions of the analysis from being relevant to non-stationary economies too: see, for an example, the discussion of the usefulness of the marginalist long-period investment function in Petri 1986. It is worth adding that, owing to the different theoretical structure, no analogous need to assume stationary

conditions appears to exist for the "normal positions" studied in the Classical approach to value and distribution: in the latter, e.g., accumulation need not change production techniques nor distribution.

(19) "Supply and demand" are terms to be found in the Classical authors too, but there they do not refer to the same concepts as in neoclassical theory; they are not schedules, but rather single quantities, see Garegnani 1983.

(21) See Garegnani 1960 Pt. II. The measurement of the "capital" endowment as an amount of value was never something neoclassical authors felt at ease with. The ultimate need for a value measure arises from the fact that, in equilibrium, amounts of "capital" receiving the same amount of remuneration - and having therefore the same value - must, by the logic of the theory, be equal: any measure of the amount of "capital" embodied in specific capital goods must therefore be proportional to their value. The value measure is also naturally suggested by the treatment of capital as homogeneous with savings, i.e. with income, see Garegnani 1978 p. 33.

(21) "Stable" here means compatible with the traditional informal arguments about the stability of realistic adjustment processes.

(22) The introduction in Walras's model of inequalities in place of the equations specifying the uniformity of the rates of return on the supply price of the capital goods (Morishima 1964, Note to Chapter 3) is the simplest way to overcome Walras's inconsistency and go over to the temporary equilibrium framework with, implicity, in Morishima's case, static expectations - a quite debatable assumption in this framework, owing to the speed with which the data of the equilibrium may change.

(23) Various elements in Walras's analysis suggest that he - no less than Böhm-Bawerk or Wicksell - was trying to determine a long-period equilibrium: e.g. the uniform rate of return on the supply price of capital goods; the implicit assumption of static price expectations; the absence of net entrepreneurial profits; the absence of a fixed factors/variable factors distinction; the absence of initial endowments of firms; the absence of all consideration of a debt structure of firms; even, if well understood, the treatment of the money endowment (Petri 1982). He seems to have simply been less clear than those other authors on the implications of that notion of equilibrium for the treatment of the capital endowment.

(24) The imperfect awareness of the shift to a post-Walrasian notion of equilibrium is the reason for the duration and the confusions of the so-called "Classical Dichotomy Controversy" in monetary theory associated with Lange, Patinkin, Archibald and Lipsey, Clower, etc., see Petri 1982.

(25) For a contrast of the data of the Classical and the marginalist approaches

to value and distribution, see Eatwell 1977, Garegnani 1984. One point may deserve further discussion here. In a long-period (marginalist) GE, although the notion of capital as a single factor which may change "form" makes it possible to conceive of its total amount as unaffected by the disequilibrium actions of agents in the gravitation towards the equilibrium, the *distribution of the property* of the factor endowments among the agents might be affected by these disequilibrium actions; it might therefore be argued that even a long-period GE does not have data unaffected by the gravitation towards it - even leaving aside the problems connected with the measurement of capital -. But, it would seem, the degree of indeterminancy of a long-period GE due to this cause would not be so great as to endager its role as a centre of gravitation: the changes in the distribution of the property of wealth - long-period marginalist theorists could have plausibly argued -, in order to affect to any significant extent the equilibrium, would in all likelihood need to be much greater than might result from the mistakes of agents. The problem is analytically different from, and much less grave than, the problem which arises with the total endowments of each type of capital goods: if these endowments are given, a long-period equilibrium will generally not exist.

(26) Reading e.g. an often-cited survey such as Weintraub 1979, I think one cannot avoid a feeling of unease at how GE theorists invent one unreal "scenario" or "story" after another to discuss the adjustment towards the equilibrium - recontracting, B-processes, and what not - in order to avoid having the data of the equilibrium altered by the adjustment process. F.M. Fisher (1983), in the first neoclassical attempt so far to deal with this problem, concedes that the alteration induced in the data of a post-Walrasian equilibrium by any realistic adjustment process "makes the calculation of equilibria corresponding to the initial state of the system essentially irrelevant" (p. 14), and tries to study the stability of disequilibrium processes actually taking time and involving production and consumption. He is able to prove that the economy will tend to some Arrow-Debreu equilibrium only by assuming, among other things, the existence of futures markets for the infinite future, and what he himself admits to be an unrealistic assumption of No Favourable Surprises, i.e. that during the disequilibrium process things never turn out to be better, for any agent, than he had expected.
This may be the place to note that, while the criticism advanced in Chapter 2 also applied to so-called 'disequilibrium' (fixprice) Drèze-Benassy-Malinvaud-Grandmont equilibria and their sequences (see, for a survey, Grandmont 1977; and see Fitoussi 1983 p. 15: "The theory of 'disequilibria' had been presented at the beginning as the theory of imperfectly co-ordinated systems, but the models... are all founded on tatonnement processes, which ensure a perfect co-ordination through quantities... (the) critical remarks concerning the assumption of the instantaneous adaptation of prices apply, pari passu, to the case of instantaneous adaptation of quantities"), these latter models appear to be even weaker on this score, owing to the presence of one more element (the given prices) whose evolution over time introduces an element of arbitrariness

in the modelling of the sequence of equilibria and a cause of possible divergencies between actual and equilibrium path.
This is not the place for a full discussion of the attempts to circumvent this problem by assuming rational expectations. Briefly, the assumption that individuals use all their information in the best possible way cannot be identified with the assumption that the equilibrium is reached instantaneously.

(27) Both are necessary: the second without the first can only ensure that aggregate demand will be sufficient to absorb aggregate output, but not that the national product will automatically gravitate towards the full-employment level; the first without the second cannot guarantee that decreases in money wages will bring about decreases in real wages, and hence increases in employment, because if the increased savings, normally associated with an increased employment and output, are not matched by an increased investment, an attempt to increase employment following a decrease in money wages will cause price deflation in step with the money wage deflation, and employment will be reduced again.

(28) See Garegnani 1976 p. 137 n. 16. Also notice how clearly these lines from Hicks reflect the conception of capital as a single factor capable of changing "form" without changing "quantity", which was at the time the generally accepted one.

(29) A very low elasticity of the demand curve for labour would deprive the theory of plausibility: e.g. very small shifts of the labour supply curve would have to cause, according to the theory, enormous variations of the real wage, contrary to what experience shows (small contractions in the labour supply would cause the reduction of profits to zero). The full employment assumption would also lose plausibility, the normal situation being one of either labour or capital structural unemployment; the assumption of an indefinite downward flexibility of factor prices would then also lose plausibility, and it would then be natural to look elsewhere for the forces determining income distribution.

(30) As an example, see Bliss 1975b ch. 13, who denies validity to Haavelmo's problem about the speed of adjustment to the optimal capital stock, and also (incorrectly, see Petri 1986) argues that Solow's famous 1956 article on neoclassical growth theory is wrong in believing that the rate of interest can at the same time bring to equality both the supply and demand for capital and the supply and demand for savings.

(31) No neoclassical macro textbook that I know of, with the only exception of Nagatani 1981, goes so far as *reporting* the possibility of 'reverse capital deepening'. This seems to me to be a good example of how a 'paradigm' may simply refuse to take troublesome counterexamples seriously.

(32) With the exception of Keynes, where the principle that savings are brought

into equality with investment by variations of the level of income - a principle in no way dependent on the marginalist approach to value and distribution - coexists with marginalist elements (the most important being the decreasing marginal-product-of-labour schedule and the decreasing marginal-efficiency-of-capital schedule), giving rise to problems of theoretical consistency, see Garegnani 1979 p. 59 n. 44.

(33) It might be thought that the above conclusions leave the existence of the post-Walrasian equilibrium untouched: so that one might still argue that there exists at each moment of time a full-employment price vector, although the market mechanism is unable to find it. But the conclusion, that the forces determining value, distribution and outputs in the real world cannot be those envisaged by the marginalist tradition, suggests that there are other forces and mechanisms at work: the implication (in agreement, it would seem, with many empirical studies) appears to be that the neoclassical description of the economic agents' choice sets is defective. Three examples on the issue of price-taking behaviour may suffice here. The widespread existence of product differentiation suggests that it must often appear rational to a producer to differentiate his product if he can: the assumption of price-taking behaviour then appears to contradict the assumption of rationality in a world where product differentiation is possible. Again, in labour markets the common experience is one of (often institutionalised) collusive behaviour on both sides; one should conclude that it must at least *appear* rational to the agents to collude (and that it may actually be rational becomes more plausible if it is concluded that there is no reason generally to believe that a wage decrease brings about an increase in emplyment); the assumption of price-taking behaviour in labour markets is then, again, probably in contradiction with the assumption of rationality. Thirdly, for investment decisions the assumption of price-taking behaviour (relative to the expected price of the product one intends to produce) is clearly ridiculous the moment there are constant returns to scale since then the expectation of extraprofits would induce an infinitely large investment in the given industry and entrepreneurs must know that this will affect the product price. Thus, it would seem, the neoclassical notion of prices sufficing to determine choices is highly implausible; already on this ground alone, the view enunciated at the beginning of this note appears therefore indefensible.

References

K.J. Arrow (1959), "Toward a theory of price adjustment", in M. Abramovitz ed., *The allocation of economic resources*, Stanford: Stanford University Press.

K.J. Arrow and F.H. Hahn (1971), *General competitive analysis*, Edinburgh: Oliver and Boyd.

T. Bewley (1982), "An integration of equilibrium theory and turnpike theory", *Journal of Mathematical Economics*.

M. Blaug (1980), *The methodology of economics*, Cambridge: Cambridge University Press.

C.J. Bliss (1975), "The reappraisal of Keynesian economics: an appraisal", in M. Parkin, A.R. Nobay eds., *Current Economic Problems*, Cambridge: Cambridge University Press.

C.J. Bliss (1975b), *Capital theory and income distribution*, Amsterdam: North-Holland.

C.J. Bliss (1975), "Capital theory in the short run", in Brown, Sato and Zarembka 1976.

C.J. Bliss (1983), "Consistent temporary equilibrium", in Fitoussi 1983.

M. Brown, K. Sato, P. Zarembka eds. (1976), *Essays in modern capital theory*, Amsterdam: North-Holland.

J.B. Clark (1902), *The distribution of wealth*, New York: Macmillan.

G. De Vivo (1976), "Sul sistema di equilibrio economico generale di Walras", *Economia Internazionale*.

G. De Vivo (1984), Review of M. Howard, *Profits in economic theory*, in *Contributions to Political Economy*.

G. Duménil, D. Lévy (1985), "The classicals and the neoclassicals: a rejoinder to Frank Hahn", *Cambridge Journal of Economics*, December, 327-345.

J. Eatwell (1977), "The irrelevance of returns to scale in Sraffa's analysis", *Journal of Economic Literature*.

J. Eatwell (1978), *Theories of value, output and employment*, London: Thames Polytechnic, repr. in Eatwell and Milgate 1983.

J. Eatwell (1982), "Competition", in I. Bradley, M. Howard eds., *Classical and Marxian Political Economy*, London: Macmillan.

J. Eatwell and M. Milgate eds. (1983), *Keynes's economics and the theory of value and distribution*, London and New York: Duckworth and Oxford University Press.

F.M. Fisher (1976), "The stability of equilibrium: Results and problems", in M. Artis, A.R. Nobay eds., *Essays in economic analysis*, Cambridge: Cambridge University Press.

F.M. Fisher (1983), *Disequilibrium foundations of equilibrium economics*, Cambridge: Cambridge University Press.

J.P. Fitoussi (1983), "Modern macroeconomic theory: an overview", in Fitoussi ed., *Modern macroeconomic theory*, Oxford: Basil Blackwell.

P. Garegnani (1960), *Il capitale nelle teorie della distribuzione*, Milan: Giuffré.

P. Garegnani (1970), "Heterogeneous capital, the production function and the theory of distribution", *Review of Economic Studies*.

P. Garegnani (1976), "On a change in the notion of equilibrium in recent work on value and distribution", in Brown, Sato and Zarembka 1976, as repr. in Eatwell and Milgate 1983.

P. Garegnani (1978), "Notes on consumption, investment and effective demand: Part I", *Cambridge Journal of Economics*, as repr. in Eatwell and Milgate 1983.

P. Garegnani (1979), "Notes on consumption, investment and effective demand: Part II", *Cambridge Journal of Economics*, as repr. in Eatwell and Milgate 1983.

P. Garegnani (1979b), "A reply to Joan Robinson", *Cambridge Journal of Economics*, as repr. in Eatwell and Milgate 1983.

P. Garegnani (1983), "The classical theory of wages and the role of demand schedules", *American Economic Review*, May.

P. Garegnani (1983b), "Two routes to effective demand", in J.A. Kregel ed., *Distribution, effective demand and international economic relations*, London: Macmillan.

P. Garegnani (1984), "Value and distribution in the classical economists and Marx", *Oxford Economic Papers*.

P. Garegnani and F. Petri (1982), "Marxismo e teoria economica oggi", in *Storia del Marxismo*, Vol. IV, Turin: Einaudi.

J.M. Grandmont (1977), "Temporary equilibrium theory", *Econometrica*.

F.H. Hahn (1973), "The winter of our discontent", *Economica*.

F.H. Hahn (1974), "Back to square one", *Cambridge Review*.

F.H. Hahn (1982), "The Neo-Ricardians", *Cambridge Journal of Economics*.

F.H. Hahn (1982b), *Money and inflation*, Oxford: Basil Blackwell.

F.H. Hahn (1983), "Stability", in K. Arrow, M. Intriligator eds., *Handbook of Mathematical Economics*, vol. II, Amsterdam: North-Holland.

F.H. Hahn (1984), "Introduction", in id., *Equilibrium and macroeconomics*, Oxford: Basil Blackwell.

G.C. Harcourt (1972), *Some Cambridge controversies in the theory of capital*, Cambridge: Cambridge University Press.

G.C. Harcourt (1976), "The Cambridge controversies: Old ways and new horizons - or dead end?" *Oxford Economic Papers*.

J.R. Hicks (1946), *Value and Capital*, 2nd ed., Oxford: Clarendon Press.

J.R. Hicks (1965), *Capital and Growth*, Oxford: Oxford University Press.

J.R. Hicks (1977), *Economic perspectives*, Oxford: Oxford University Press.

J.R. Hicks (1983), "IS-LM: An explanation", in Fitoussi 1983.

J.A. Kregel (1976), *Theory of capital*, London: Macmillan.

O. Lange (1942), "Say's law: a restatement and criticism", in O. Lange and others eds., *Studies in mathematical economics and econometrics*, Chicago.

E. Malinvaud (1977), *The theory of unemployment reconsidered*, Oxford: Basil Blackwell.

A. Marshall (1970), *Principles of economics* (8th ed.), London: Macmillan.

M. Milgate (1979), "On the origins of the notion of intertemporal equilibrium", *Economica*.

M. Milgate (1982), *Capital and employment*, London: Academic Press.

M. Morishima (1964), *Equilibrium, stability and growth*, Oxford: Clarendon Press.

M. Morishima (1977), *Walras's Economics*, Cambridge: Cambridge University Press.

K. Nagatani (1981), *Macroeconomic dynamics*, Cambridge: Cambridge University Press.

F. Petri (1978), "The difference between long-period and short-period general equilibrium and the capital theory controversy", *Australian Economic Papers*.

F. Petri (1982), *The Patinkin Controversy Revisited*, Siena: Quaderno n. 15 dell'Istituto di Economia.

F. Petri (1982b), "Ancora sulla teoria del capitale di Wicksell", *Rivista di Politica Economica*.

F. Petri (1984), "The meaning of short period equilibrium in Keynesian economics", unpublished paper read at the Siena Colloquium "Domanda effettiva, prezzi e distribuzione", May 1984.

F. Petri (1986), "Professor Morishima, Say's law and the investment function", forthcoming in Proceedings of the Colloquium "Systemes des prix de production", Paris 1986.

E.H. Phelps Brown (1972), "The underdevelopment of economics", *Economic Journal*.

L. Robbins (1930), "On a certain ambiguity in the conception of stationary equilibrium", *Economic Journal*.

D.H. Robertson (1931), "Wage-Grumbles", in id., *Economic Fragments*, London: P.S. King & Son Ltd., as repr. in *Readings in the theory of income distribution* (ed. by the AEA), London: G. Allen and Unwin, 1950.

B. Schefold (1985), "Cambridge price theory: special model or general theory of value?", *American Economic Review*, May.

P. Sraffa (1960), *Production of commodities by means of commodities*, Cambridge: Cambridge University Press.

D. Tosato (1969), "Sur la theorie Walrasienne de la capitalisation", *Economie Appliquee*.

G.S.L. Tucker (1960), *Progress and profits in economic theory*, Cambridge: Cambridge University Press.

E.R. Weintraub (1979), *Microfoundations*, Cambridge: Cambridge University Press.

E.R. Weintraub (1983), "The existence of general equilibrium: 1930-1954", *Journal of Economic Literature*.

G.D.N. Worswick (1972), "Is progress in economics possible?", *Economic Journal*.

K. Wicksell (1934), *Lectures on Political Economy*, 2 vols., London, Allen and Unwin.

Elenco dei Quaderni pubblicati

n. 1 (febbraio 1979)
MASSIMO DI MATTEO
Alcune considerazioni sui concetti di lavoro produttivo e improduttivo in Marx.

n. 2 (marzo 1979)
MARIA L.RUIZ
Mercati oligopolistici e scambi internazionali di manufatti. Alcune ipotesi e un'applicazione all'Italia

n. 3 (maggio 1979)
DOMENICO MARIO NUTI
Le contraddizioni delle economie socialiste: una interpretazione marxista

n. 4 (giugno 1979)
ALESSANDRO VERCELLI
Equilibrio e dinamica del sistema economico-semantica dei linguaggi formalizzati e modello keynesiano

n. 5 (settembre 1979)
A. RONCAGLIA - M. TONVERONACHI
Monetaristi e neokeynesiani: due scuole o una?

n. 6 (dicembre 1979)
NERI SALVADORI
Mutamento dei metodi di produzione e produzione congiunta

n. 7 (gennaio 1980)
GIUSEPPE DELLA TORRE
La struttura del sistema finanziario italiano: considerazioni in margine ad un'indagine sull'evoluzione quantitativa nel dopoguerra (1948-1978)

n. 8 (gennaio 1980)
AGOSTINO D'ERCOLE
Ruolo della moneta ed impostazione antiquantitativa in Marx: una nota

n. 9 (novembre 1980)
GIULIO CIFARELLI
The natural rate of unemployment with rational expectations hypothesis. Some problems of estimation

n. 10 (dicembre 1980)
SILVANO VICARELLI
Note su ammortamenti, rimpiazzi e tasso di crescita

n. 10 bis (aprile 1981)
LIONELLO F. PUNZO
Does the standard system exist?

n. 11 (marzo 1982)
SANDRO GRONCHI
A meaningful sufficient condition for the uniqueness of the internal rate of return

n. 12 (giugno 1982)
FABIO PETRI
Some implications of money creation in a growing economy

n. 13 (settembre 1982)
RUGGERO PALADINI
Da Cournot all'oligopolio: aspetti dei processi concorrenziali

n. 14 (ottobre 1982)
SANDRO GRONCHI
A Generalized internal rate of return depending on the cost of capital

n. 15 (novembre 1982)
FABIO PETRI
The Patinkin controversy revisited

n. 16 (dicembre 1982)
MARINELLA TERRASI BALESTRIERI
La dinamica della localizzazione industriale: aspetti teorici e analisi empirica

n. 17 (gennaio 1983)
FABIO PETRI
The connection between Say's law and the theory of the rate of interest in Ricardo

n. 18 (gennaio 1983)
GIULIO CIFARELLI
Inflation and output in Italy: a rational expectations interpretation

n. 19 (gennaio 1983)
MASSIMO DI MATTEO
Monetary conditions in a classical growth cycle

n. 20 (marzo 1983)
MASSIMO DI MATTEO - MARIA L. RUIZ
Effetti dell'interdipendenza tra paesi produttori di petrolio e paesi industrializzati: un'analisi macrodinamica

n. 21 (marzo 1983)
ANTONIO CRISTOFARO
La base imponibile dell'IRPEF: un'analisi empirica (marzo 1983)

n. 22 (gennaio 1984)
FLAVIO CASPRINI
L'efficienza del mercato dei cambi. Analisi teorica e verifica empirica

n. 23 (febbraio 1984)
PIETRO PUCCINELLI
Imprese e mercato nelle economie socialiste: due approcci alternativi

n. 24 (febbraio 1984)
BRUNO MICONI
Potere prezzi e distribuzione in economie mercantili caratterizzate da diverse relazioni sociali

n. 25 (aprile 1984)
SANDRO GRONCHI
On investment criteria based on the internal rate of return

n. 26 (maggio 1984)
SANDRO GRONCHI
On Karmel's criterion for optimal truncation

n. 27 (giugno 1984)
SANDRO GRONCHI
On truncation "theorems"

n. 28 (ottobre 1984)
LIONELLO F. PUNZO
La matematica di Sraffa

n. 29 (dicembre 1984)
ANTONELLA STIRATI
Women's work in economic development process

n. 30 (gennaio 1985)
GIULIO CIFARELLI
The natural rate of unemployment and rational expectation hypotheses: some empirical tests.

n. 31 (gennaio 1985)
SIMONETTA BOTARELLI
Alcuni aspetti della concentrazione dei redditi nel Comune di Siena

n.32 (febbraio 1985)
FOSCO GIOVANNONI
Alcune considerazioni metodologiche sulla riforma di un sistema tributario

n. 33 (febbraio 1985)
SIMONETTA BOTARELLI
Ineguaglianza dei redditi personali a livello comunale

n. 34 (marzo 1985)
IAN STEEDMAN
Produced inputs and tax incidence theory

n. 35 (aprile 1985)
RICHARD GOODWIN
Prelude to a reconstruction of economic theory. A critique of Sraffa

n. 36 (aprile 1985)
MICHIO MORISHIMA
Classical, neoclassical and keynesian in the Leontief world

n. 37 (aprile 1985)
SECONDO TARDITI
Analisi delle politiche settoriali: prezzi e redditi nel settore agroalimentare

n. 38 (maggio 1985)
PIETRO BOD
Sui punti fissi di applicazioni isotoniche.

n. 39 (giugno 1985)
STEFANO VANNUCCI
Schemi di gioco simmetrici e stabili e teoremi di possibilità per scelte collettive.

n. 40 (luglio 1985)
RICHARD GOODWIN
The use of gradient dynamics in linear general disequilibrium theory.

n. 41 (agosto 1985)
M. MORISHIMA and T. SAWA
Expectations and the Life Span of the regime.

n. 42 (settembre 1985)
ALESSANDRO VERCELLI
Keynes, Schumpeter, Marx and the structural instability of capitalism.

n. 43 (ottobre 1985)
ALESSANDRO VERCELLI
Money and production in Schumpeter and Keynes: two dichotomies

n. 44 (novembre 1985)
MARCO LONZI
Aspetti matematici nella ricerca di condizioni di unicità per il Tasso Interno di Rendimento.

n. 45 (dicembre 1985)
NIKOLAUS K.A. LAUFER
Theoretical foundations of a new money supply hypothesis for the FRG.

n. 46 (gennaio 1986)
ENRICO ZAGHINI
Una dimostrazione alternativa dell'esistenza di equilibri in un modello di accumulazione pura

n. 47 (febbraio 1986)
ALESSANDRO VERCELLI
Structural stability and the epistemology of change: a critical appraisal

n. 48 (marzo 1986)
JOHN HICKS
Rational behaviour: observation or assumption?

n. 49 (aprile 1986)
DOMENICO MARIO NUTI
Merger conditions and the measurement of disequilibrium in labour-managed economies

n. 50 (maggio 1986)
SUSAN SENIOR NELLO
Un'applicazione della Public Choice Theory alla questione della riforma della Politica Agricola Comune della CEE.

n. 51 (maggio 1986)
SERENA SORDI
Some notes on the second version of Kalecki's Business Cycle Theory

n. 52 (giugno 1986)
PIERANGELO GAREGNANI
The Classical Theory of Wage and the role of Demand Schedules in the determination of relative prices

n. 53 (luglio 1986)
FRANCO BIRARDI
Crescita, sviluppo ed evoluzione: l'affermazione dei nessi strutturali sistematici e la combinazione di scienza, tecnologia ed economia nelle prime industrializzazioni

n. 54 (agosto 1986)
M. AMENDOLA - J.L. GAFFARD
Innovation as creation of technology: a sequential model

n. 55 (settembre 1986)
GIULIO CIFARELLI
Exchange rates, market efficiency and "news". Model specification and econometric identification

n. 56 (settembre 1986)
ALESSANDRO VERCELLI
Technological flexibility, financial fragility and the recent revival of schumpeterian entrepreneurship