

Controverse Garegnani/Bliss
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Capital in the Neoclassical Theory.

Some Notes *

Pierangelo Garegnani

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I. Two preliminary observations

1 I shall start with two preliminarily observations on neoclassical theory which will be at the basis of my arguments . Section II will then deal with the apparent early realization by Hicks that capital could not be consistently defined as a single magnitude, and with the way out of the problem he attempted in *Value and Capital*. These preliminary considerations will pave the way for a discussion in section III of the post-war capital controversy, taken when —in its later stages, after the recognition of phenomena like the reswitching of techniques and reverse capital deepening — the defence of neoclassical theory came to be conducted in terms, essentially, of the reformulations of the theory proposed in *Value and Capital* three decades before. Finally, in section IV, reference will be made to the argument I developed elsewhere (Garegnani, 2003) according to which those reformulations also ultimately depend on the notion of capital as a single magnitude, the same which had been found indefensible, at the level of pure analysis, after the early stage of the controversy.

* Thanks are due to numerous colleagues with whom parallel versions of this paper have been discussed and, in particular, to Dr. Saverio Fratini of the University of Rome 3, who has also been precious with interpretations of contemporary literature.

2 The first of my two observations is of both a historical and a logical nature. It concerns the two approaches which, at the cost of severe simplifications, can be said to have dominated the theory of distribution and relative prices in succession since its systematic inception in the 18th century.

The earlier approach is the classical one of the Physiocrats, Smith, Ricardo and Marx. It was founded, essentially, on the notion of the surplus which a community can dispose of, over the part of its product which must be put back into the production process to ensure its repetition on an unchanged scale. This part was taken to include, besides the consumed means of production, also the subsistence of the workers employed broadly identified with their wages: the incomes accruing to the other classes in society were then traced to the surplus. The prior, thus separately determined real wage allowed, on the other hand, for a determination of relative prices similarly separate from that of outputs, engendering a simple analytical structure deeply different from that of the later theory, and open — through the institutional determination of the wage and the flexibility of the separate determination of outputs — to an essential role for broader social, political and historical forces in the working of the economy.¹

The later approach is instead that which after several decades of transition from classical analysis, crystallized in the last quarter of the 19th century around the twin concepts of marginal utility and marginal productivity and has dominated since. It is founded on the conception of a substitutability between ‘factors of production’, and on the demand and supply functions for factors and commodities which descend from that substitutability. It arose essentially out of the classical theory of the rent of land extended to the division between wages and profits, thereby replacing

¹ Cp. e.g. Garegnani (2002a, 250; 2007, 186).

the notion of surplus by which, we saw, that division had been explained by the classics.

Now, and here we come to our first preliminary observation, one element can be argued to have been decisive in allowing for the passage from the earlier to the later approach. This element is the use in an essential role of the conception of capital as a single magnitude. We shall presently be back to clarify and justify this statement which, I know, is likely to be highly controversial, but to do so we must first proceed to our second preliminary observation.

3. This second observation concerns a basic feature of capital goods in a market economy. In the eyes of their owners physically heterogeneous capital goods, are perfect substitutes in proportion to their values. As Walras had lucidly pointed out nearly a century and half ago, capital goods are demanded by savers as elements of a single commodity, ‘perpetual net income’.² That is indeed the single commodity whose existence we imply when we assume competitive arbitrage to be sufficient to realize a uniform ‘effective’ rate of riskless return on the price of such goods : the reciprocal of that rate is the price of that Walrasian commodity. And little changes if, more in keeping with contemporary intertemporal general equilibrium, its finite horizon and changing prices, we refer to ‘income for next year’, whose gross unit³ has the price given by the discount factor $(1 + r_t)^{-1}$, r_t

² Walras (1954) par. 242, pp. 275-76

³ It is the price of a unit of *gross* income, because an amount $\left(\frac{1}{1+r_{t+1}}\right)$ out of that unit will have to be set aside at the beginning of period $(t + 1)$, if a similar unit of gross income is to be had in $(t + 2)$.

being the ‘effectively’ uniform rate of return for the year in question⁴ numerically expressed in terms of numeraire. In what follows we shall generically refer to this commodity as ‘future income’.

4. We can now return to the first of our observations, and contend that it is this single commodity, or ‘quantity of capital’, rooted in the experience and practice of wealth owners,⁵ that has suggested in the first place the idea of a generalization of the classical theory of rent to the division between wages and profits. That extension is in fact founded on assuming a variability of the proportion of capital to labour (and land), analogous to the classical one of labour (plus capital) to land. It is a variability understood to descend from either the alternative between the methods available for producing (directly or indirectly) the same consumption good, or from the methods for producing outputs of alternative consumption goods. Now, the fact is that in both cases the alternative production processes involved differ, generally, by the *kinds* of capital goods used, rather than by the *proportions* to labour in which each kind of them is employed. A variability of the ‘proportion of capital to labour’ in the economy could therefore have hardly been conceived for

⁴ The *adverb* ‘effectively’ is used above in order to remind the reader that this kind of uniformity of returns on capital is quite compatible with, and indeed *requires*, a ‘nominal’ difformity of the commodities own rates of interest, once changes in relative prices over the period of the loan are considered in the equilibrium (below n. 7). In the latter case it is only the *numerical expression* of that uniform effective rate that will differ depending on the numeraire adopted.

⁵ Thus, *e.g.*, Bliss (1975, 8) rightly notes that capital ‘cries out to be aggregated’. He does not however notice the essential reason for that: the homogeneity of capital goods for savers.

neoclassical theory, unless the heterogeneous capital goods required by the alternative methods, or by the alternative outputs, had been viewed as embodiments of quantities of the same homogeneous value commodity demanded by savers.

5. That, however, is far from being all about the role of the single savers’ commodity in the origin of neoclassical theory. Another role, even more important from a strictly analytical point of view, lies in that only such a commodity allowed expressing the capital endowment, — a datum in marginal theory, like land or population for classical rent — in a way compatible with the homogeneity of capital goods for savers.

I am referring here to the tendency, under free competition, towards a uniform effective rate of riskless return on the capitals’ supply prices — *i.e.*, if we prefer, the tendency to an equality between the capitals’ demand prices (or prices simply) and their supply prices (costs of production).⁶ It is the tendency to what, following Marshall, used to be called the ‘long period equilibrium’, in which “plant” has adjusted to outputs in each industry. Indeed that uniformity — the traditional one of the competitive ‘rate of profits’ — assumes the physical composition of the capital endowment to be fully adjusted to the techniques adopted and outputs

⁶ Simple arbitrage, over however short a time, will be sufficient to realize a tendency to uniformity of the (riskless) effective rate on the *demand prices* of the capital goods, by lowering those prices below the respective supply prices in the case of relatively abundant capital goods. That is of course a quite different phenomenon from the tendency to the uniformity on their supply prices of which in the text above, which requires changes in the physical composition of the capital stock.

produced.⁷ It assumes, therefore, an *endogenous* determination of that physical composition. Now — and here we are back to the need for capital as a single magnitude in neoclassical theory — this endogenous determination is compatible with the basic neoclassical treatment of the capital endowment as a datum, only if the latter is again conceived as a fluid susceptible of taking any physical form.⁸ Without this uniformity of effective returns, on the other hand, the position of the economy as determined by the theory would have been no more *persistent*⁹, under free

⁷ The uniformity of rate of return on capitals' supply prices of course excludes, as is generally done at the level of abstraction of a normal price, the presence in the capital endowment of 'obsolete' capital goods — pertaining, that is, to methods of production presently dominated by other methods at all possible levels of the distributive variables. More embarrassing for a theory in which the capital endowment is a datum is the fact that the same uniformity of returns also excludes the presence in the endowment of kinds of capital goods which are not 'obsolete' in the sense above, out do pertain to methods of production other than those dominant in the equilibrium considered. (The question does not arise in Walras who assumes that all methods require the same kinds of capital goods though in different proportions: but it reflects the general case, and it re-enforces the neoclassical need for capital as the single magnitude which can take the form of any concrete capital good).

⁸ Cp. *e.g.* Hicks 1932a, pp 20-21, on the need to refer to a marginal product of labor for which the 'quantity', but *not* the 'form', of the 'co-operating capital remains unchanged.

⁹ "It is to this *persistence* of the influences considered, and to the time allowed for them to work out their effects that we refer when contrasting Market and Normal price" (Marshall, 289, V, III, 6; our italics). The question is discussed in Garegnani (2002b).

competition, than any position of the economy with, say, different wages for labour of the same quality, or with prices of products differing from their costs of production, effects strictly analogous to those we have for capital when the above uniformity of returns does not hold.

But the question then is: why this *persistence*, leading neoclassical theory towards the troublesome notion of the given capital endowment as a single magnitude? Indeed to such a persistence had long been attributed, nothing less than the possibility of ensuring correspondence between theory and observation in economics. It was the role played — even across the deep divide between classical and neoclassical theory — by what we may indicate here as the 'normal' price or, more generally, the 'normal' position of the economy — the basis of economic analysis since Adam Smith's notion of the natural price as "the central price, to which the prices of all commodities are *continually* gravitating". (Smith 1776, Bk. I, ch VI, p.51) The persistence of the normal price, as warranted under competition, by the above uniformity of the rate of return, was in fact thought to allow for a *repetition of transactions*, which by occurring on the basis of nearly unchanged data, would be generally sufficient to compensate the temporary 'accidental' deviations from it of the actual price: in that way persistence would allow for a correspondence between the theoretical variables and some average of the corresponding magnitudes in the actual economy.

6. It was thus not a matter of accident, or of mere convenience, that the capital endowment, given as a single magnitude, characterized with varying degrees of explicitness, and with the single partial exception of Walras, all mainstream expressions of neoclassical theory up to a few decades ago, and to the events we are going to discuss below. On that idea there rested in fact two key points of neoclassical theory: the plausibility of

the notion of factor substitutability lying at its basis and, with the possibility to determine a ‘normal position’, that of a correspondence between theoretical variables and observable magnitudes — both of which are essential, it would seem, to prevent the theory from slipping into an intellectual game.¹⁰

It seems indeed possible to say that, *once* the neoclassical demand and supply framework is adopted, the nature of capital as the single saver’s commodity carries with it the need for the treatment of capital as a single magnitude. The quantity of the commodity in terms of which savers take their demand decisions cannot, then, be absent from the system any more than the quantity of any other commodity on which individuals take their demand decisions. This need, which we have just traced with respect to the normal position goes, we submit, beyond it. We shall recall in section IV below, my contention in (2003) that such a single commodity can be retraced with its difficulties in the contemporary formulations of the theory, which have done away with it at the level of factor endowments.

As we could expect from the homogeneity of capital goods for the savers which cannot but be reflected in any theory of the market economy — also the classical economists tended to treat capital as a single magnitude. However the essential fact in this respect is that their different ‘surplus’ theory of distribution and relative prices — *i.e.* the absence of the demand and supply framework — exempted them from the above two needs for treating capital as a single magnitude. As shown, for example, in Sraffa’s contemporary revival of classical theory, a vectorial notion of capital suffices for its determination of prices and distribution¹¹, which

¹⁰ It is the risk which Malinvaud appears to detect when he writes ‘the risk seriously exists that economics loses touch with real problems and develops on its own into a scholastic’ (1991, p. 66).

¹¹ Cp. *e.g.* Garegnani (1960, p. viii) and (*e.g.* 1990, p. 2).

only needs capital among the determinants for expressing the technical conditions of production.

II The Neoclassical problem and Hicks’s “Value and capital”

7. We are over with our two preliminary observations and we can get closer to the post-war capital controversy which will concern us in section III by introducing here the question central to the controversy.

The neoclassical need for capital as a single magnitude raises a problem which had been simmering since the very origin of that theory. The ‘quantity’ of that special factor of production had to be measured independently of the distribution and relative prices which it was brought to determine, just as the classical quantities of labour and land had to be so measured for determining rent. However the commodity demanded by savers clearly was not *directly* measurable in any such independent terms, since its primary expression for savers lay in the *value* of the capital goods in terms of some numeraire. A basic problem of the new theory was, therefore, how to measure capital, the single magnitude in terms which would be both independent of distribution, as the value of capital goods is not, and at the same time appropriately related to the value quantity on which savers do take their decisions. The ‘average period of production’ over which labour and more generally non-produced factors have to remain invested under each method of production of the commodity and its direct and indirect methods of production, was the road along which a conciliation of the two requirements was attempted, from Jevons and Böhm Bawerk onwards. As is known, the attempt foundered on (i) the necessity to consider the compound rate of interest on the advances for

non-produced factors; (ii) on the plurality of such non-produced factors, and (iii) on fixed capital.¹

The impossibility of consistently defining a concept as basic for the intended generalization of classical rent, as we just argued the ‘quantity of capital’ was, might conceivably have led to the abandonment of the attempt in favour of some return to classical analysis (as had in fact happened when the ‘wages fund’ theories, the progenitors of neoclassical theory, were abandoned around the middle of the 19th century). However, the principle of factor substitution and the ensuing demand and supply explanation of distribution, had apparently taken roots too deep for them to be abandoned — while Marshall’s interpretation of Ricardo and the classical economists as primitive exponents of a demand and supply theory of prices, simplified by an assumption of constant returns, had succeeded in largely cancelling the traces of the alternative analysis that had dominated economic thought for more than a century before the ‘marginalist revolution’. Thus, when it began to be recognized that capital could not be ultimately treated as a single magnitude, the reaction was instead to apply the principle of substitution to each kind of capital good taken as a distinct ‘factor’, with little explicit consideration of the drastic difficulties such a change would raise for the theory.

8 Hicks (1939) was probably the main influence for bringing into mainstream theory this tentative way out of the difficulty. After basing his *Theory of Wages* (1932) on normal positions and therefore on the usual notion of a ‘quantity of capital’ as essentially the value magnitude,² he

¹ Cf. Garegnani 1960, Part I, ch. III; and Part II, ch. IV; also 1990 pp. 23-31 for, respectively, the notion of the average period of production, and its shortcomings from the viewpoint of marginal theory.

² Cf. Garegnani (1976, n. 12).

appears to have come to an early perception of the fact that the notion could not be made ultimately viable and had to be replaced.³ Thus in *Value and Capital* (1939), he came to treat the given capital endowment of the theory as a vector of capital goods. It was indeed the conception that Walras had advanced as early as 1877,⁴ having initially failed to realize its inconsistency with the uniformity of returns on capitals’ supply prices pertaining to the ‘normal position’, — the position which, like all his predecessors and contemporaries, he had originally intended to determine.⁵

The necessary, if implicit, recognition of Walras’s inconsistency meant however that Hicks had to accompany the adoption of that conception of capital with the abandonment of the normal position and hence, inevitably, with basic changes in the notions of equilibrium of the theory. The competitive tendency to a uniform rate of profits could but be powerful and quick in bringing about appreciable changes in the composition of a capital stock unadjusted to the most profitable methods and outputs, thus causing appreciable changes in the prices of productive services and commodities. The persistency which justified the determination of the normal position while abstracting from the changes the latter is undergoing, could no longer be assumed. The analysis had to attempt remedying that by considering the effect of future conditions on the markets for current commodities and productive services.

³ On the effect on Hicks of Shove’s remarks on capital in his review of Hicks’ (1932a) cp. Garegnani 1976, n. 13

⁴ *Théorie Mathématique de la Richesse Sociale*, Paris 1877 pp.568-69 reproducing the paper Walras delivered in July 1876 at the Société Vaudoise des Sciences Naturelles. The year 1877 is also the one in which Walras published the second instalment of the first edition of the *Elements* (1974 and 1977) containing his theory of capital formation

⁵ Garegnani (1960, Part II chapters 2 and 3, also 1990 pp. 13-19).

This consideration of future conditions was done in either of two ways. The first was by introducing ‘price expectations’ in the ‘temporary equilibria’ resulting from the Walrasian capital endowment. It was the way taken by Hicks himself in *Value and Capital*. Alternatively, in the search of something less volatile than expectations on which to found a theory, the analysis could be expanded to imagine present markets for future commodities and factors, so that future prices could be envisaged coming into actual existence. In the limit the assumption of future markets could be extended to all commodities and factors over the whole assumed life of the economy, as was done in the general intertemporal equilibrium of Arrow and Debreu (1954) and then Debreu (1959).

Both those drastic reformulations of neoclassical theory may in fact be seen to have been allowed into mainstream theory, largely by *Value and Capital*, since also the possibility of the second alternative, — intertemporal general equilibrium — had emerged in that book (1939 *e.g.* 136 ff.) and, above all, it was developed later essentially by ‘dating’ the commodities of the ‘static general equilibria’ of Parts I and II of *Value and Capital*.

These reformulations of neoclassical theory, which we may thus call ‘Hicksian’ for short, were however affected by two deficiencies which were the mirror image of the two reasons for which, we contended in section I above, capital as the savers’ single commodity had been at the origin of marginal theory. They were (i) the absence of substitutability between ‘productive factors’ when capital goods are conceived in the Walrasian way⁶: (ii) the just recalled lack of persistency, and therefore

⁶ With the factors as conceived in Walras, substitutability could be claimed to enter the system *over time* with gross investment, as new more profitable capital goods replace the existing ones and therefore the savers’ commodity (capital) can enter in its direct form of object of the saving

lack of potential correspondence with observation. And this affected both the ‘temporary equilibria’, and also those equilibria of each ‘date’, which constitute, so to speak, the bricks of general intertemporal equilibrium.⁷

These were, presumably, the difficulties which, variously perceived and expressed, had underlain the remarkable fact that, despite the fame of

investment flows. However the fact remains that (i) substitution would be almost entirely absent in the initial equilibria with the existing physical capital and, in the logic of the theory, this would be bound to deeply affect all subsequent positions; and (ii) the limited substitutability that would thus emerge would again appeal to capital the single magnitude confirming its necessity for factors substitution.

⁷ It is interesting to note that the two difficulties of the Walrasian conception adopted are not mentioned in *Value and Capital*. This is so, despite the fact that the difficulty regarding factor substitutability had been prominent in a 1932 debate between Hicks and Robertson (*cp.* Hicks 1932b, Robertson 1931), when both authors stressed the necessity that the ‘capital’ endowment be allowed to change ‘form’ in order to give rise to marginal products and, more generally, sufficient substitutability between factors. The point returned with force in the *Theory of Wages* (1932a, 20) where *e.g.* Hicks contrasts the ‘full equilibrium’ marginal product of labour with a ‘short period’ one, where the ‘form’, as well as the ‘quantity’, of the capital is said *not* to change: the latter is then dismissed as something which ‘it is very doubtful if [it] can be given any precise meaning which is capable of useful application’. This regards primarily the difficulty of factor substitution, but the contrast drawn here between “short period”, and “full equilibrium” marginal product of labour appears to also imply awareness of the second deficiency of the vectorial conception of capital, *i.e.* the non-uniformity it entails in the effective returns on capitals’ supply prices.

its author, and the well known difficulties of the alternative notion of a single magnitude, Walras's own conception of capital had failed to enter the mainstream during the six decades which had elapsed between its first 1876 formulation and its revival by Hicks in 1939. Hicks himself in 1932, not many years before *Value and Capital*, had sceptically commented

“[Walras and Pareto's theories of capital] are the last part of their work which one can consider as final or accept without the most careful consideration” (1932b, 297)

Not unlike Walras's, Hicks's notion of capital and the associated 'dynamic theory' of *Value and Capital* appear in fact to have initially had little impact on mainstream theory. The notion drew, for example, little or no attention, in what was then the centre of economic theorizing, the Cambridge of Pigou, Keynes, Robertson. It is even conceivable that its influence might have remained confined to groups of mathematical economists⁸ on the margin of the mainstream, as had indeed been the case before, had it not been for the emergence of some striking results incidental to Piero Sraffa's work on the classical approach to prices and distribution over the same decades. The phenomena of the reswitching of techniques and of 'reverse capital' deepening, advanced “in preview” in the 1950s by the hands of Joan Robinson,⁹ were indeed sufficient to soon render untenable the notion of capital as a single factor at the level of pure theory. The way to dominance was then open for what was essentially the only alternative that would keep within the premises of neoclassical theory: capital on Walrasian lines, and the necessary reformulations of the

⁸ Indeed the Walrasian conception of capital had been used long before Hicks, by mathematical economists with little notice of it being taken in the mainstream literature at the time. See e.g. Wald [1936].

⁹ Cp. J. Robinson, 1970, pp. 144-5 about her use of the Sraffa results.

conception of equilibrium on Hicksian lines — marking, so to speak, a deep 'Hicksian divide' in the evolution of neoclassical theory.

III The capital controversy and Hicks's "Value and Capital"

9. With that we have reached the heart of the postwar capital controversy, and have joined it at what we indicated above as its later stages when the defence of neoclassical theory was conducted essentially in terms of its Hicksian reformulations.

We might have therefore expected that, at such stage, the difficulties of those reformulations would, if not take the centre of the scene, at least emerge with sufficient clarity to be debated. However the way in which the reformulations had been introduced in *Value and Capital* nearly thirty years before made it difficult for the controversy to achieve clarity and focus on these questions. We must therefore turn back to *Value and Capital* for those aspects of Hicks's argument which are important for what, I would submit, has been the inconclusiveness of those later stages of the controversy.

10. Despite its title, what we find in the foreground of *Value and Capital* is not the problem of capital, but, rather, the claimed need for a 'dynamic theory', accompanied by a critique of what is there called the 'static theory' of 'the economists in the past' (1939, 115).¹ However, what is striking is that when we come to a description of what that 'static theory' consisted of, we do not find the normal position which was in fact

¹ Cp. Garegnani 1976, 31-36 for the traces of the deeper line of Hicks's (1939) criticism of previous theory, concerning capital (cp. also n. 4 below).

the mainstay of those economists. What is attributed to them, in the forms we shall presently see, are instead two kinds of equilibria which, though showing some features in common with the normal position, are definitely not it.

The first of those two kinds is, Hicks says, what the static theory would be if stated in a ‘strict’, consistent way” (1939, 115). It is represented by the equilibria analysed in Parts I and II of *Value and Capital*, those by which Hicks in effect replaces by a Walrasian given vector of capital goods the previous notion of the capital endowment as, essentially, a single magnitude. Hicks has however to admit that those equilibria cannot be taken to represent the thought of the economists of the past as it actually was.

We must therefore look for the second representation Hicks gives of those equilibria of the past (1939, 116). And here we find the stationary position: the one, that is, where the incentive to net savings has disappeared. However, this again is a notion quite different from the neoclassical normal position. It resembles it in one respect, namely the constancy of prices assumed in the definition of the equilibrium. But even for that respect the two positions radically differ. In the normal position the constancy of the capital endowment, and hence of the relative prices, is merely an abstraction from the changes which they are admitted to effectively undergo in the economy — an abstraction founded on the persistency of the position due to the comparative slowness of the changes in its data, in particular of the only endogenous such change, that in the capital endowment.² In the stationary position instead the same constancy

² As *e.g.* Marshall wrote “if we are considering [...] the whole of a large country as one market of capital, we cannot regard the aggregate supply of it as altered quickly and to a considerable extent by a change in the rate of interest”. (Marshall 1920, VI, ii, 4).

of capital is the endogenous result of an equilibrium condition of zero net saving, so that the capital endowment is an *unknown* of the equations and not the *datum* it is in the normal position.³ And the same is true for the proportion of capital to labour of the ‘steady state’ that since the post-war period has become the commonest form of stationary state contemplated in the analysis.

11. The paradox of *Value and Capital* is then that in its account of the “usual course of economists in the past”, we do not find the hallmark of that “usual course” down to Hicks’s own *Theory of Wages* (1932): namely the normal position.

That disappearance of the normal position entailed, then, a second and even more striking paradox : it is that we do not find in *Value and Capital* any specific criticism of the normal position of those economists — the very position which Hicks proposes there to replace by his ‘dynamic theory’. The only criticism remains the generic one of the lack of realism of assuming the constancy of prices in the definition of an equilibrium

³ Hicks’s apparent replacement of the normal position by a stationary one was made easier by the frequent use of the term ‘stationary’ to also indicate the normal position, because of its abstraction from changes in relative prices. However Lionel Robbins (1930) had already lucidly clarified that ambiguity by his distinction between ‘static’ and ‘stationary’ positions of the economy. Hicks’ attribution of a proper stationary state, and not of a normal position, to ‘the economists of the past’ is on the other hand made entirely clear when he writes that, in the stationary position of those economists, the “quantity of intermediate products — the quantity of capital — will be determined through the rate of interest [...] fixed at a level which offers no incentive for net saving or dissaving” (Hicks 1939, 118).

(1939, 116-17) — a criticism which would have been more convincing if ‘previous theory’ had in fact rested, as it clearly did not, on either Hicks’s ‘stationary states’, or on the fleeting-equilibria of Part I and II of *Value and Capital*.

As a matter of fact, the dependence of current prices on future prices, was all but overlooked by those economists — starting from Adam Smith’s dichotomy between ‘market’ and ‘natural’ prices, and then down to Walras, Marshall, Pigou, Wicksell, etc. To the extent in which the expected prices reflected merely ‘accidental’ circumstances, or the undoing of those circumstances, their effects could be ignored because they would be averaged out in the normal price through the repetition of transactions allowed for by the persistency of the normal position. And to the extent in which the expected prices expressed instead changes in the *data* of the position, they would be dealt with by the comparison between the corresponding two normal positions.

The real point behind this alleged past oversight of price changes — a point which remains implicit in *Value and Capital*⁴ — was however to be that the *persistency* allowing for the *abstraction* from those changes, had been made possible by the treatment of the given capital endowment as a single magnitude susceptible of adjusting its physical form. And this is

⁴ Except perhaps for the indication of it which may be read in the passage quoted in Garegnani (1976, p. 32)

‘Of course people use to be able to content themselves with the static apparatus because they were imperfectly aware of its limitations. Thus, they would often introduce in their static theory a ‘factor of production’ and its price interest, supposing that capital could be treated like the static factors [...]. That some error was involved in their procedure would not have been denied [...] (1939, 116 n.). We are not however told by Hicks what that ‘error’ actually was.

just what the Hicks of (1939), as distinct from that of (1932), knew could *not* be done. Leaving aside the obviously unrealistic stationary states, the normal positions had therefore to be replaced by the static equilibria of *Value and Capital*, whose fleeting character made the remedy of dated prices and quantities all but inevitable. In other words the need of a “dynamics” was the *effect* rather than the *cause* of the change in the conception of capital, contrary to what Hicks might be taken to imply in his (1939) foreground argument.

12. The disappearance of the normal position from Hicks’s (1939) argument was to weigh heavily on the controversy of thirty years later. Because of the strong direct or indirect influence of *Value and Capital* by that time, the eclipse of the normal position had a series of effects on the controversy which, I submit, converged in obscuring the basic terms of the question of capital in neoclassical theory. We may perhaps try to summarize these effects under four main points.

That disappearance meant first of all the disappearance of the most transparent form of dependence of neoclassical theory on capital as a single magnitude, namely its ultimate use as a datum for determining the position. On the one hand, that made the use of the notion in previous neoclassical theory a confused bone of contention rather than the simple historical fact it was.⁵ On the other hand, it made the role of the conception of a ‘quantity of capital’ at the very origin of the analytical

⁵ Cp. *e.g.* “It seems to me impossible (as a matter of intellectual history) to maintain that the possibility of perfect capital (or labour) aggregation is a neo-classical doctrine (Hahn 1982, 354). It is however difficult to envisage an intellectual history in which, say, Böhm Bawerk, J.B. Clark, Pigou etc. could use an ‘aggregation’ of capital whose possibility they did not admit.

structure of the theory, much more difficult to discern and understand. As a result, and most importantly, it made it difficult to understand the continued dependence on that conception in the reformulations of neoclassical theory that were being advanced — a continuing dependence of which more will be said in the next section.

Secondly, the disappearance of the normal position went together with that of its key condition: the uniformity of effective returns on the capitals' supply prices or costs (*i.e.* the equality of their demand prices with the respective supply prices), ensuring the persistence of the position and the possibility of its correspondence with observation.⁶ Thus when that condition was referred to from the critical side in order to explain the rationale of the normal position, and its neoclassical dependence on the capital endowment as a single fluid fund, that rationale was generally not understood and the condition was even confused with the altogether different condition of a uniformity in the commodity own rates of interest, a mere synonym of assuming constancy of prices in defining the equilibrium.⁷

⁶ It is significant and again somewhat paradoxical that Hicks's revival of Walras's theory of capital in Parts I and II of *Value and Capital* went together with the total disappearance there of Walras's own equations of 'capital formation' (Walras 1954, Lesson 23) which contained the condition of uniformity of returns, as well as the relation equalizing the demand and supply of 'net perpetual income' (par. 3 above), *i.e.* savings and investment, in today's terms. That disappearance left a serious gap in the 'static theory' of Parts I and II of Hicks (1939), into which we cannot however enter here.

⁷ For an example of this confusion see the discussion in Garegnani 2003, 153-54, of a passage in Hahn 1975 in which — using the above uniform

Thirdly and most importantly the misunderstanding of the normal position as a stationary or steady state, led many of the participants in the controversy to take for granted the Hicksian charge that 'previous theory' was inapplicable to the 'real world (1939, 215)'. We have already mentioned the paradoxical character of that charge, but we may now stress how, by putting the alternative method based on normal positions out of sight, that charge helped turning a blind eye on the real undermining of the applicability of the theory, the one due to the impermanence of the new equilibria and the resulting need of a dynamic theory. It was indeed the undermining which Hicks himself had implicitly admitted when, in a little quoted passage of *Value and Capital*, he wrote that he assumed "the economy to be always in equilibrium" (1939, 131),⁸ an assumption which

rate of return (referred to by critics) in order to characterize a 'Special neoclassical case' to which Sraffa would refer — he sees it as one in which

"the equilibrium price of a good for future delivery in terms of the same good for current delivery will be the same for all goods" (Hahn 1975, 360)

clearly the case of uniform own commodity rates of interest, *i.e.* constant prices, quite compatible logically with any divergence between rates of return on capitals' supply prices, and which the effective uniformity of those rates must in fact contradict whenever price changes over time are considered in the equilibrium.

⁸ Samuelson appears to seriously underestimate the difficulty of tracing the actual path of the economy (what Hicks's passage reported in the text implies) when in the *Foundations* (1947) he draws the analogy of a "cannonball [which] can be held to be in equilibrium at each point on its path". The dominant forces acting on the cannonball at each instant of time are in fact comparatively few in number and their effects on the position of the cannonball are accordingly calculable with a degree of

should have shocked the readers of *Value and Capital*: no economist had ever supposed before the economy to actually be in an equilibrium or more generally a position of rest except by a fluke⁹: gravitation around it and

approximation sufficient to establish a correspondence between the theoretical and the actual position of the ball at that instant. Given instead the numberless forces of analogous strength which affect the economy at each instant of time, the actual instantaneous position of an economy cannot even in principle be determined with any approximation: only averages of observable positions, reflecting the effects of the few most persistent among those forces can be determined. And, the cumulation of the errors would seem to make the path of the economy even less calculable in such terms, than its instantaneous position by itself. This, it seems, is what prompted Marshall to write

“dynamical solutions in the physical sense of economic problems are unattainable. And if we are to adhere to physical analogies at all, we must say that statical solutions afford starting points for *such rude and imperfect approaches to dynamical solutions* as we may be able to attain to” (1898, 38-9, my emphasis).

the ‘approaches’ in question being, essentially, the comparison of normal positions.

⁹ This assumption, to which Hicks is in effect led by the abandonment of the normal position, is similar to that we find in Bliss when he writes

“it may seem more sensible to simply assume that equilibrium will prevail and to thus confine our investigations to the equilibrium state. We could regard the object of our investigations not as ‘the economy’ but as ‘economic equilibrium’ [...]. This approach may seem more attractive, if only because more tractable than the Herculean programme of constructing a complete theory of the behaviour of the economy out of equilibrium” (Bliss, 1975, 28)

Bliss is here, so to speak, touching with his own hand the implications of that abandonment of that normal position, where the “Herculean task” was largely left to itself by the simple Smithian device of the ‘centre of

not achievement of it, had always been what was thought relevant for the position of the economy object of the analysis — the only form of correspondence with observation which, it was held, economic theory could attain.¹⁰

And, fourthly, the mist in which the discussion had been moving because of these unclarified misunderstandings, was made thicker by a tendency to see the neoclassical dependence on the notion of a ‘quantity of capital’ as pertaining to the empirical construct of an ‘aggregate production function’ purporting, that is, to represent the output of the whole economy as a single homogeneous aggregate, produced by a ‘capital’ homogeneous with it. Used for Solow’s 1956 simplified neoclassical answer to the long period problems raised by Keynes and aggregate demand, that notion was an initial target from part of the critics. Taken in isolation the target was however misleading as it risked turning an inconsistency at the foundations of the idea of a generalized ‘factor substitutability’ (par 4 above) into difficulties pertaining only to an admittedly unrigorous approximation, and therefore presumably absent when the several productive sectors are distinguished in a general equilibrium system. It was thereby overlooked that the inconsistency is there, whichever the number of sectors which we might wish to distinguish into the economy. In fact the essence of neoclassical problem of capital is not aggregation versus general

gravitation’ *i.e.* by the concentration of the analysis on persistent forces. Those implications appear here to have in fact led to an impasse, such that the way out comes close to assuming away reality. (Cp. in this respect the passage by Malinvaud in section I n. 10 above.)

¹⁰ As Denis Robertson wrote with admirable simplicity and lucidity

“It seems to me that anybody who rejects these two ideas, that a system can move towards equilibrium and that it may never get into

equilibrium, but, if anything, one about *two kinds of general equilibria* : the traditional one based on normal positions, exemplified by, say, Wicksell (1906), or even by Walras (as far as his original intentions went) versus the Hicksian one that renounce such positions in the attempt to avoid the single magnitude.

13. Thus, in conclusion, the later stages of the controversy appear to have been marred by multiple misunderstandings which, largely, are still waiting to be cleared. Thanks to the unambiguous phenomena of reswitching and reverse capital deepening, the first stage was conclusive in discarding from pure theory the traditional version of the theory. Subsequently, however, when the implications of those phenomena and the reformulations of the theory inevitably became the main object, those misunderstandings prevented decisive progress and led to what is in fact an inconclusive phase of the discussion. A certain unpreparedness on the critical side to extend the critique to the Hicksian reformulation of the theory advanced from the opposite side, was also relevant. Many of the critical authors had their roots in a Cambridge which in its majority, had essentially ignored the relevant part of *Value and Capital*, judging it unnecessarily complicated and unlikely to bear serious fruit. In that, as it turned out, such a majority had underestimated the real potential of the Hicksian proposal, which was defence and not construction. The lack of a sufficiently known and well tested alternative like that of the classical economists ¹¹ contributed to what I see as the half way pause of the critical course.

it — has made it extremely difficult for himself to interpret the course of events in the real world” (1957, pp. 144-5).

¹¹ Elements of an alternative theory — partly influenced by the classical theory brought again to light by Sraffa — were advanced in the initial

It is this complex of circumstances, I submit, which has left space for the credence that, whatever its methodological deficiencies, the equilibria which became dominant with the ‘Hicksian divide’ are immune from the inconsistencies on capital of previous marginal theory. That in turn has left space, I believe, for a second no less unwarranted consequence: a feeling that since the ‘Hicksian’ reformulations and in particular general intertemporal equilibrium would confirm at the level of pure theory the essential validity of the general demand and supply framework, they would also provide some validation for the admittedly imperfect previous concepts — foremost that of a ‘quantity of capital’ — as workable approximation in more applied work.

IV The problem of capital is still with us

14. I think that neither of those beliefs is well founded. I have in fact argued elsewhere (2003) that intertemporal equilibrium does not avoid the dependence on the notion of the capital as a single magnitude. Though it no longer occupies its highly visible position as a *fund* among the factor endowments, the homogeneous commodity ‘future income’ demanded by savers, can be shown to emerge as a flow, with the respective demand and supply functions and the corresponding market. They are respectively what, after Keynes, we are used to

stages of the controversy by Robinson, Kahn, Kaldor and other authors on the critical side of the controversy. The concern to rapidly fill the huge area of long-run problems which Keynes “left covered with fragments of broken glass” (Robinson 1956, p. v) may, however, have prevented deriving from the revival of the classical approach all it could offer including, in my opinion, a consolidation and extension to the long period of Keynes’s own achievements on aggregate demand (Cp. *e.g.* Garegnani 1978-79 and 1992).

call (gross) savings supply, (gross) investment demand, and saving-investment market. The implications of the inconsistency of that notion of capital - the same implications which enforced the abandonment of the traditional analysis in pure theory - are still there to be faced.¹²

The discussion on the matter proceeding. The question however may already be asked: should we not begin to recognise that those difficulties are but the expression of a theory originally inspired by the concept of capital as an independently measurable single productive factor, which we now all agree does not exist?

Professor Bliss is however correct when in introducing a collection of reprints of articles on capital he writes

“ultimately, only new theory beats old theory however bad” (Bliss 2008, xvi),

but he continues,

“there really is not a well worked out alternative long run capital theory to set against the various orthodox models”

where, we may however note, the reference to a *long run* theory seems to limit the expected novelty of the alternative by implying that we already have a satisfactory short run theory. And a similar limitation appears to underlie his reference to the alternative theory as a *capital theory* to be set against orthodox models: but the question, as I have attempted to argue in this essay, is wider than is carried by the qualification of ‘capital theory’. It

¹² On the specific notion that the adjustment between savings and investment in an intertemporal equilibrium raises no more problems than do adjustments to relative demands for contemporary commodities, and the associated idea that the equilibrium in the markets for the future consumptions that correspond to today’s saving, would take care of the equilibrium between today’s saving and today’s investment, cp. Garegnani (2003,130-32; also 2005, 495-96)

more properly concerns an alternative between theories of distribution and relative prices.

If we start thinking in those wider terms we must first of all set aside Marshall’s interpretation of Ricardo and the classical economists as representing an early simplified version of later demand and supply theory. Demand and supply in Ricardo and the early classics had never been *determinants* of normal prices, rather than merely movers, so to speak, of ‘actual’ or ‘market’ prices (Smith 1776,49 bk. I, ch. vi) towards a ‘natural price’ or ‘price of production’ determined independently of them, by forces governing the division of the product between wages and profits, alien to any principle of factor substitution, and, therefore, to the ultimate source of the neoclassical problem of capital.

Once that clarification is accomplished we may find there the alternative we are looking for, free from the difficulties of capital. It is a theory well tested in its long tradition. Its contemporary revival, initiated by Sraffa (1960), has involved, with a reconstruction of its analytical structure by means also of comparison with neoclassicism, the development of the theory with respect *e.g.* to the basic question of the stability of its normal positions. It has also involved dealing with special problems like the treatment of joint production, fixed capital or land rents. But it has above all regarded work intended to include the Keynesian analysis of aggregate demand for both the short run and, most importantly, the long run.¹³

When the vision of the forces governing the economy underlying that classical analysis will be grasped, it will be found, I believe, to be surprisingly close to that inspiring much present theorising aiming to bring out the basic role of institutions in the economy. Classical analysis may then perhaps allow to develop and refine that work, providing it with a

¹³ Cp. n. 11 above

solid analytical framework, free from the straightjacket of a generalized principle of factor substitution - undermined by the results of the capital controversy and, even independently of that, hollowed out by the reformulations which that controversy has brought about in neoclassical theory.

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Comment on "Capital in Neoclassical Theory: Some Notes"

by Professor Piero Garegnani

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Equilibrium with Capital

A leading idea of the paper is the correct observation that capital, which is fluid for the individual investor, who can sell one type and buy another, is not fluid for the economy as a whole. Keynes lays great emphasis on this point in his General Theory. Two questions flow from this observation. First, is there a formal equilibrium model that resolves this problem? Secondly, what are the implications of the tension between demand-fluidity and supply-rigidity for a less formal, more realistic, view of the world? Garegnani notes correctly that the key to a resolution of this problem is to be found in price dynamics, but his discussion of those dynamics does not include the most powerful specification. A lot has happened in Economics since pre-war John Hicks.

The key insight is the old observation that a capitalist economy in an idealized equilibrium behaves like a planned economy that is following an optimized path. What do the dynamics of an optimized economy with many capital goods look like? That question was answered long ago. Dorfman et als (1958) does it nicely for an economy with linear processes. See also Bliss (1975), Chapter 10. Particularly useful is Hahn (1968). Hahn showed that in general, equilibrium conditions for the various capital goods, equal net returns for each good, depend upon the rate of change of prices, not just on the prices alone. Then the intertemporal dynamic equilibrium of the system is defined by a set of simultaneous differential equations. What can we say about those equations? The easiest route to the answer comes from the theory of optimal growth, as with a many-capital-good Ramsey

model. From there we obtain dynamic equilibrium conditions (Euler equations), but these do not suffice by themselves. We need to add transversality conditions. The dynamic equations are of the saddle-point variety. Only transversality ensures that the system converges to a particular asymptotic state.

It is transversality that takes care of the possibility of multiple equilibrium, including multiple steady-state solutions. We have these already in a simple two-sector model with aggregate capital, so we certainly cannot exclude them with many capital goods. If multiple equilibrium is the end of neoclassical theory, then it is dead and buried long ago. But if we want just some equilibrium solution, then we can stay with the idealized equilibrium dynamics for a while. It is a rock-solid theory, but how useful is it? Its price dynamics are essentially the dynamics of correctly-foreseen prices. Hicks's elastically-adjusted expectations will not do. We do not live in a world where price movements are accurately foreseen. Just look at the gyrations in the oil market in recent months to confirm the point.

Unfortunately it gets worse. Not even price movements accurately foreseen in the short or medium term will do. As a non-optimized dynamic equilibrium is just like the necessary conditions for an optimized system, there will be infinitely many "equilibrium" paths for prices, but most of these will crash into the sign-constraint barriers of the race track. A successful capitalist economy needs to have the right idea of where it should go in the long-run. Real life capitalist economies are extremely bad at knowing where they are going in the long-run, and judging that destination correctly. Recent financial crisis, and similar crises in the past, remind us that capitalism is often driven by unsound expectations and foolish intellectual fashions.

Please forgive me. I have strayed into writing about the real world. A striking feature of the school to which Piero Garegnani belongs is its seeming lack of interest in the real world. Its target is

formal abstract neoclassical economic theory. And it insists on taking on that theory on its own long-past ground. This is not a well-chosen target.

Some Closing Remarks

Professor Garegnani is completely confident that his opinions are the right and the true. He is so certain that he has discovered the key to what is wrong with orthodox economics that he can hardly contain his disdain for anyone who declines to follow him into the new dawn. Not even Hicks is spared his condescending attitude. If that is his view, then so be it. But to him I would say this. Despite some refinements, the argument is the same as he has been putting about for the last 30 years. The impact of this argument has been notably limited. Not even in Italy does a significant proportion of the profession know his case and take it seriously, and in the world at large the impact is negligible. There is of course no guarantee that a crude majority will not be terribly mistaken, but there is equally a possibility that the intellectual loner, or his small school, has lost the plot. If you believe in your cause you have to learn to communicate effectively, and to engage with outsiders in the contemporary world. Our world is changing rapidly and in ways that demand economic analysis of what is happening. A backward-looking negative view of current economics will not do.

General acceptance of a particular view can never be the final test. What always matters is intellectual productivity. Over the last 30 years so-called neoclassical economics has been extraordinarily productive. And from Akerlof to Stiglitz (and Hahn cited above) it has been its own most rigorous critic. What has been the contribution of the post-Sraffa school in the same period? Nothing at all as far as I can see. This has been an exceptionally sterile approach. Where are the new ideas? Where are the illuminating insights into what is happening today? What about China's economic miracle, or the pensions crisis that results from aging

populations? Would David Ricardo have considered such questions unworthy of his consideration?

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3

A Reply to Professor Bliss's Comment

Piero Garegnani

1. Professor Bliss has not responded to the arguments. Thus in the comment I find no word on the role which capital, the single magnitude, played in allowing for a neoclassical normal position and the possibility of a link between theoretical variables and observable magnitudes. Pages of equations will not remedy the need of bridging over the myriad of accidental circumstances acting on the observable magnitudes at each instant of time and making it impossible to determine them and their path in time. These are indeed the myriad circumstances which made Bliss himself refer in the past to the intractable "Herculean programme of constructing a complete theory of the behaviour of the economy out of equilibrium" — a programme made necessary "by the fact that, even if equilibrium were to be stable, there might not be enough time within the space of a week for prices to adjust to equilibrium". (Bliss 1975, p. 28)

Nor do I find any answer on how we are going to take care of substitution between productive factors within equilibria, like the initial ones of Bliss's dynamic paths, where a physically given capital endowment should provide for alternative processes of production which generally require capital goods specific to each of them. The result can only consist of rigidly determined equilibrium outputs and methods of production with abundant zero (or indeterminate) factor prices.

I do understand that these questions are not those which neoclassical scholars would generally set themselves in their routine of problems solving on bases passed down by their elders. But Bliss is one of those elders and we would like to know whether he has now any answer different from that he gave at the time to his doubts above, *i.e.* "confine the investigations to the equilibrium state" and "regard as the object of our investigations not the 'economy' but 'economic equilibrium' (Bliss, *ibid.*) — *i.e.* renouncing an answer on the vital question of the relation between 'equilibrium' and the 'economy'.

Professor Bliss sees me and like minded colleagues as taking for their criticism neoclassical theory "on its long past ground". As he puts it "a lot has happened in economics since pre-war John Hicks". Yes, certainly a lot has happened, including the capital controversies, but where, before *Value and Capital*, did we find in mainstream theory the 'dynamical' methods of which the paths of Bliss's comment are an instance? And those paths are of today, not 'long past'. Before Hicks we had instead statements like the following

"dynamical solution in the physical sense of economic problems are unattainable [...] statical solutions afford starting points for such rude and imperfect approaches to dynamical solutions as we may be able to attain to (Marshall 1898m, 38-39, P.G. 21 n.8).

It is thus to ‘long past’ Hicks that we have also to turn in order to criticize *e.g.* today’s Bliss.

2. My commentator admits however one basic fact: that of the homogeneity of the capital goods for savers, seen by them as constituents of the single Walrasian commodity ‘future income’ (PG, 3)¹, and its conflict with the vectorial measurement of the given initial capital endowment of contemporary neoclassical theory. But when he envisages the conflict as a tension inherent in economic reality he overlooks two important elements.

The first is that neoclassical theory was originally conceived in a way thought to solve the problem: *i.e.* with capital as a fluid as much on the side of the supply (the given capital endowment), as from that of the demand.

The second is that in the alternative classical surplus theory of distribution, no such ‘tension’ exists between a ‘fluid demand’ and a vectorial ‘supply’ since, as we saw (P.G., 8), capital as a determinant of prices and distribution emerges there only for defining the technical conditions of production. Thus, the theory of the Physiocrats, Adam Smith, Ricardo or Marx — to which Bliss fails to refer in his comment² — would apparently provide the “formal [...] model [...] that resolves the problem” (Bliss p. 1).

When we keep in mind these two elements it becomes clear that the ‘tension’ in question results not from economic reality, but from insisting at explaining distribution and the general functioning of a market economy in terms of a substitutability between factors of production, and resulting demand and supply forces, even in the face of the admitted failure of basic concepts on which the theory was built.

¹ References to my paper are indicated simply by the initials “PG” and by “Bliss” simply the references to the comment.

² In (2008, 6) Bliss writes: “I formed the impression that Sraffa knew that his great life’s project had run into the ground : that he could not come up with a complete alternative to the classical [clearly a misprint for ‘neoclassical’ P.G.] theory he disliked”.

However, Sraffa contended in the (1960) Preface, that he was taking in his book the standpoint which is that of the old “classical economists from Adam Smith to Ricardo [...] submerged and forgotten since the advent of the marginal method” (Sraffa 1960, V) and, as has been repeatedly argued in these years, that approach does provide a complete consistent alternative to neoclassical theory. Used by those economists for a century or more, completion, as distinct from rediscovery, was hardly much of a problem. The qualification of a ‘prelude’ in the subtitle of the (1960) book, related explicitly to the “critique” of dominant theory, and not to the proposed underlying alternative.

3. Indeed even a quick consideration of the dynamical theory Bliss outlines in his comment does, I believe, reveal the marks of the above complex origin, rather than those of a physiological advance and complement of previous analysis.

We start (Bliss 1) from an ‘idealized equilibrium’, based apparently on the theory of general intertemporal equilibrium. But, to begin with, this equilibrium will be one in which, as just noted, substitution between factors is essentially absent, and when substitutability will appear at later dates it will be founded on capital, the single magnitude, in the form of the traditional ‘free capital’ of the investment flow. Then, in addition to the consequences of the likely, multiple zero factor prices of the early equilibria, we shall have the ‘capital paradoxes’ with the resulting possible multiple, unstable and zero-prices equilibria.

Bliss writes that if multiple equilibria were the end of neoclassical theory, then the theory “is dead and buried long ago” (Bliss, 2). Certainly Marshall and the other founders of neoclassical theory worried a lot about multiple equilibria. It was, and it is, not easy to find in the centuries of history of market economies facts suggesting the possibility of multiple or unstable equilibria, validating a theory asserting such a possibility. My commentator seems in fact to mix multiple equilibria deduced from the essential assumptions of neoclassical theory — maximization of profits and utility, as is the case for reverse capital deepening and reswitching — with altogether different multiplicities resulting merely from dispensable additional hypotheses made in specific models. It is to the latter that Bliss refers, when *e.g.* he mentions the possible multiplicity of steady states with proportional savings and a single capital good, and concludes that the ‘aggregation of capital is not the crucial difficulty’ (Bliss 2008, p. 21). By mixing the comparatively innocuous with the lethal, one can indeed make the lethal look less lethal.

My opponent feels however that he can proceed on such an insecure basis and try to demonstrate that neoclassical theory can conceivably say something on the long run tendencies of the economy — a demonstration which he seems to think constitutes the essence of a solution of the neoclassical problem of capital.³ Apparently basing himself on the

³ *E.g.* Bliss 2008, 12-13. My commentator also implies there that the argument reported in the text answers a question I put him decades ago about the impossibility for neoclassical theory to give a long period theory of the rate of return on capital. But what I meant was that, with the capital endowment given as a physical vector, it was impossible to determine a ‘normal position’ with its uniform effective rate of return on the capitals’ supply prices (P.G., 5-7). My question related therefore to the inability of the post-Hicksian pure theory to respect the criterion of potential correspondence with observation which had become established in economics, and not to the

obviously not very general assumption of a single ‘representative agent’, he utilizes the properties of a competitive equilibrium in order to identify the possible paths followed by such a representative agent, and therefore by the economy, with those developed out of Ramsey’s (1928) famous article, and which are defined once the time preferences of the agent are given. That path might then bring the economy to a stationary or steady state which would give definiteness to the long run tendencies of the economy. On the way to try to show that, Bliss meets the additional obstacle of the ‘saddle’ character attributed to that path, which should be overcome by “transversality conditions” further restricting, apparently, the possibility that such a Ramsey path to steady states be ever one which the economy can walk.

But the above restrictions are yet far from constituting the whole of the deficiencies which Bliss himself sees in this construction, when taken as a representation of reality. At the end of his argument, he asks himself:

“how useful is it? The price dynamics [of that equilibrium] are essentially the dynamics of correctly foreseen prices [and] we do not live in a world where price movements are accurately foreseen.”

He then falls back on “non optimized dynamic equilibrium” paths of the economy of which, he states, there will be “infinitely many”.

In fact Bliss is just experiencing here, and in the first person, some of the reasons for which, as we quoted above, Alfred Marshall had stated :

“dynamical solutions in the physical sense of economic problems are unattainable”

That firm statement by what has probably been the strongest head ever on the neoclassical side, and the main author of the original acceptance and dominance of that theory, should alone be sufficient to give some doubts to Professor Bliss as to whether he really is, to use his expression, on the long run ‘plot’ of science. Indeed that plot is often very long run : my opponent mentions that, a generation only elapsed between Galileo and Newton, (Bliss 2008, p. 24), but there were two generations between Copernicus and Galileo.

Bliss seems however to think that he has a way out of Marshall’s denial. We might have expected that the negative answer he gives on the usefulness of his equilibrium dynamics would be followed by some recognition of shortcomings of the theory. He takes instead the *theory* as impregnable (‘rock solid’, Bliss, 3) and blames the failure on *reality* (‘Real life capitalist economies are extremely bad at knowing where they are going in the long run and judging that destination correctly’: Bliss, *ibid.*). But,

abstract possibility to frame an analysis of long run tendencies of the economy by a sequence of the new equilibria. The doubt in the latter case is on the significance of the analysis (potential correspondence with observation), not its possibility.

knowing where the economy goes, should it not be the task of the economists, rather than that of “the economy”?

4 Soon after that exposition of what contemporary neoclassical theory has to say for the long run of the economy — *i.e.*, it appears, that the latter can follow any of the infinitely many ‘non optimized equilibrium paths’ — my commentator reproaches the school of which I would be part, of a “seeming lack of interest for the real world” (p. 3).

Certainly, after what we saw, the reflection comes natural that if the school to which Bliss instead belongs has a strong interest for the real world, it is not served well by its own theory. As for the critics, it would certainly be odd if authors devoted to the revival and development of the theory which has been that of Adam Smith, Ricardo or Marx should have little interest for the real world — and even odder if their theory did not lend itself to serve those interests. Of course some of the authors to whom Bliss refers have contributed to the task of carrying the critique of neoclassical theory into the field of intertemporal equilibrium,⁴ and it was on the other hand necessary to clarify this alternative classical theory in both its historical roots and its logical structure.⁵ But certainly their ultimate purpose has never been to replace the Ramsey model by anything similar to it, but rather by *e.g.* a discussion of whether the fall of the rate of growth in advanced European capitalist economies from the average of 4-5% of 1950-73 to an average of 2%, and even below in later years, might not have had to do with reactions to social movements, like those of the French May of 1968, not unconnected with a regime of full labour employment that had lasted for an entire generation *e.g.* (Cavalieri, Garegnani and Lucii 2004).

Thus if the doubt expressed by the word ‘seeming’ in that ‘seeming lack of interest’, had stimulated Bliss to some search about those authors’ writings, he would soon have discovered that much work has been done by them for absorbing Keynes into a classical framework where aggregate demand fits well with the characteristic broad separation between determination of outputs and determination of distribution and prices of the classical economists— and, above all, where any long period role of

⁴ Cp. *e.g.* Petri (2004) and a recent debate on *Metroeconomica* (2005) to which Schefold and Garegnani have contributed and which had been stimulated by a review article of works by them (Mandler 2002), or, for a rapid information on the matter, the General discussion at a Siena Conference of 1999 on general equilibrium, reported in Hahn and Petri (2003).

⁵ Cp. *e.g.* Bharadwaj (1989), and debates. like that stimulated by Samuelson (2000), with Garegnani (2007a), Samuelson (2007); Garegnani (2007b); or that on Blaug (1999) with Kurz Salvadori (1999) and Garegnani (2002).

Keynesian aggregate demand finds no obstacle in a theory of distribution and relative prices based on the full employment of productive resources.⁶

When combined with the Ricardian, and more generally classical, admission of long period labour unemployment as a normal phenomenon in a market economy⁷ — that analysis of aggregate demand brings to light the great potentialities of growth which exist when labour is available as it generally is. A compound rate of additional growth becomes in fact possible whenever aggregate demand allows for an initial increased use of existing capacity and then, over time, for use of the new productive capacities associated with the cumulative savings made possible by the initial increase. Though largely invisible when not utilized, this growth potential can be rapidly mobilized when conditions of aggregate demand are favourable.⁸ The possible effects of that on received principles of economic policy, (beginning from the idea that free competition leads to Pareto optimalities) can be imagined and have begun to be considered.⁹

It seems thus singularly odd that Professor Bliss should raise as example of that school's lack of interest for the problems of the contemporary world¹⁰, the China 'economic miracle'. Clearly, a theory which reveals the existence of such potential resources usable or wastable, would seem much more likely to explain the China 'miracle', or the Korean 'miracle', or the German post-war 'miracle', etc., than Bliss's economy analyzed by means of the 'Ramsey model' and steady growth. Indeed if I may refer to a personal experience, the development of those ideas was certainly stimulated by the Italian post war "miracle", and the possibility it lent to make clear that growth and the absorption of the high labour unemployment, required consumption to be encouraged, and not discouraged, contrary to the neoclassical view dominant in the Italy of the time.

5. That work, not to mention that on the stability of classical prices,— which appears to have so far broadly confirmed Adam Smith's old positive

⁶ Cp. e.g. Garegnani (1978-79) and the essays in Eatwell, Milgate (1983).

⁷ On long run labour unemployment in the classical economists cp. e.g. Stirati (1994). Garegnani (2007a).

⁸ As for aggregate demand in growth and capital accumulation cp. Ciccone (1990); Garegnani (1992); Palumbo and Trezzini (2003).

⁹ Cp. e.g. the Round table (2007) in *Review of Political Economy* with contributions by Foley, Levrero, Garegnani, Pivetti, Vianello.

¹⁰ Classical theory and the role of aggregate demand have in fact been applied to the pension crisis referred to in Bliss 3 (e.g. Cesaratto 2006), just as they have been more generally applied to problems of fiscal policy (e.g. Ciccone 2002).

conclusions¹¹, or that on classical wages and their different mechanism of adjustment (cf. e.g. Garegnani 2007a, 213-15) would not seem to reflect any lack of interest in the real world — nor indeed reveal the absence of new ideas of an 'exceptionally sterile' approach (Bliss, 3).

How, then, Bliss's contrary allegations? Evidently he has not chanced to come across those works in the literature he reads. This suggests that in the future he might use his influence in making access to the Journals he frequents easier for the critics. He and like minded colleagues might then effortlessly gain information of what goes on in that camp by direct reading, rather than by what they see as 'impact'. There, I would add, it is not common to conclude that in each given situation "there will be infinitely many equilibrium paths" which the economy may follow.

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¹¹ Cp. e.g. Garegnani (1997) and the conference on the subject held in Siena in April 1990, the Acts of which are published in *Political Economy; Studies in the Surplus Approach* (1990).

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