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General Equilibrium Theory and Professor Blaug

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Abstract - In a series of recent papers professor Mark Blaug accuses the Formalist Revolution of the 1950s of having greatly damaged economic science by burying the conception of 'competition as a process' in favour of a conception of 'competition as an end-state', incompatible with realistic studies of stability. The paper argues that the criticism is convincing and in fact addressed at the Arrow-Debreu conception of equilibrium, which is a very-short-period conception due to the Walrasian treatment of the capital endowment. Given the unreality of the resulting model, which assumes complete futures markets and can study stability only under the auctioneer, the question arises of why the Arrow-Debreu model and its conception of equilibrium were so successful, given that traditionally the dominant conception of equilibrium had been a long-period one (even in Walras: new evidence is adduced in support of this last thesis). The answer is found in the problems of neoclassical capital theory. The conclusion is that professor Blaug's criticisms point to a necessity to return to the long-period method, but this requires abandoning the marginalist or supply-and-demand approach to value and distribution, because it was precisely the inability of the latter approach satisfactorily to determine long-period positions that motivated the switch to the sterile modern versions of general equilibrium.

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I. Introduction. Professor Blaug has a point, but this raises a question.

1. In a series of stimulating papers professor Mark Blaug (1997, 1999, 1999b, 2002, 2002b, 2003, 2003b) has accused modern economics of having undergone a "formalist revolution" with very negative consequences on its scientific fruitfulness:

> The metamorphosis of economics in the late 1940s and 1950s is aptly called a "formalist revolution" because it was marked, not just by a preference, but by an absolute preference for the form of an economic argument over its contents. (Blaug 2003: 145)

> ...modern economics is sick; economics has increasingly become an intellectual game played for its own sake and not for its practical consequences; economists have gradually converted the subject into a sort of Social Mathematics in which analytical rigor as understood in math departments is everything and empirical relevance (as understood in physics departments) is nothing… To paraphrase the title of a popular British musical: "No Reality, Please. We're Economists". (Blaug 2002: 36)

The present paper contends that professor Blaug's criticism points at very important problems and sets us on the right track in the search for the roots of these problems; the result of this search – which brings us to reconsider Walras too – is a very useful perspective on the state of economic theory, and a proposal on how to surmount the deficiencies of modern value theory.

The reason why professor Blaug gives great importance to the 'formalist revolution' is partly because it causes a waste of intellectual energies, channeling them too much into showing an ability to manipulate mathematical models[1], but above all because he considers it responsible for the sterility of modern value theory, i.e. of modern general equilibrium theory. The 'sickness' of which he accuses modern economics is essentially a

[1] This point is developed mainly in Blaug (2002: 35-6), but not particularly insisted upon elsewhere.

sickness of general equilibrium theory, and it consists of having expunged from economic theory the study of 'competition as a process', in favour of an exclusive concentration on 'competition as an end-state':

> The Formalist Revolution made the existence and determinacy of equilibrium the be all and end all of economic analysis ... What is little understood about the Formalist Revolution of the 1950's is precisely that the process-conception of equilibrium was so effectively buried in that period that what is now called neoclassical orthodoxy, mainstream economics, consists entirely of static end-state equilibrium theorizing with little attention to the stability of equilibrium (Blaug 2003: 146)

> In short, what is missing in GE theory and hence in Neowalrasian microeconomics is, quite simply, competitive rivalry between transactors in actual markets. (2003b: 401)

The result, Blaug accuses, is that modern microeconomics obscures the way competition actually works, i.e. through rivalry and Schumpeterian/Marxian continuous innovation. The same disregard for the dynamical processes of actual choice characterizes the game-theoretic notion of Nash equilibrium, Blaug notices; but he makes it clear that the developments in general equilibrium theory were more important, cf. especially Blaug (2003: 149-50). The real target of his criticism is crystal clear in the final sentence of one article: "The best way *not* to learn how markets function and how a competitive economy actually works is to study general equilibrium theory" (Blaug 2003: 154).

According to professor Blaug a special role in the spread of the Formalist Revolution must be attributed to the Arrow-Debreu famous 1954 article:

> the centerpiece [of the publications of the 1950s embodying the Formalist Revolution] is (surely?) the Arrow-Debreu proof of the existence of general equilibrium. It neatly exhibits the worst features of formalism, which is not just the application of mathematical techniques to economics, but rather reveling in mathematical modeling as an end in itself (Blaug 2003: 146)

> If we can date the onset of the illness at all, it is the publication in 1954 of a famous paper by ... Kenneth Arrow and Gerard Debreu; it is this paper that marks the beginning of what has since become a cancerous growth in the very center of microeconomics. (Blaug 2002: 36)

Why can the 1954 paper by Arrow and Debreu on existence of general equilibrium be accused of having started “a cancerous growth in the very center of microeconomics”, i.e. a growth of papers more interested in showing mathematical prowess than in furthering the understanding of economic reality? Because based on

> assumptions which clearly violated economic reality; for example, that there are forward markets for every commodity and for all conceivable contingencies in all future periods... Even so, Arrow and Debreu did not manage to prove that such a general equilibrium is stable in the sense that it is actually attained from whatever position at which we start... Unfortunately, this paper soon became a model of what economists ought to aim for as modern scientists. In the process, few readers realized that Arrow and Debreu had in fact abandoned the vision that had originally motivated Walras. For Walras, general equilibrium theory was intended to be an abstract but nevertheless realistic description of the functioning of a capitalist economy and he was therefore more concerned to show that markets will clear automatically via price adjustments in response to positive or negative excess demands – a property which he labelled "tâtonnement" – than to prove that an [*sic*] unique set of prices and quantities is capable of clearing all markets simultaneously. By the time we get to Arrow and Debreu, however, general equilibrium theory has ceased to make any descriptive claim about actual economic systems and has become a purely formal apparatus about a virtual economy... blatantly and even scandalously unrepresentative of any recognizable economic system. (Blaug 2002: 36-7)

It is not easy to reject the claim that a model that assumes complete futures markets is “unrepresentative of any recognizable economic system”. Professor Blaug is, indeed, in excellent company in finding that there is something basically wrong with modern general equilibrium theory. Strong dissatisfaction with that theory was expressed by one of its most highly esteemed practitioners already in 1981:

> I have always regarded Competitive General Equilibrium analysis as akin to the mock-up an aircraft engineer might build....theorists all over the world have become aware that anything based on this mock-up is unlikely to fly, since it neglects some crucial aspects of the world, the recognition of which will force some drastic re-designing. Moreover, at no stage was the

> mock-up complete; in particular, it provided no account of the actual working of the invisible hand. (Hahn 1981: 1036)

Has the situation changed in more recent years? It would seem not. A number of respected specialists have kept expressing serious reservations on the usefulness of general equilibrium theory. For example:

> ...we are at a turning point in economic theory. Much of the elegant theoretical structure that has been constructed over the last one hundred years in economics will be seen over the next decade to have provided a wrong focus and a misleading and ephemeral idea of what constitutes an equilibrium. (Kirman 1999: 8)

Out of many other possible references expressing similar misgivings, I would like to remember three which are possibly less well known, Shubik (1993), Fisher (2003: 91) and Howitt (1996: 76)[2].

The further claim implicitly advanced by Blaug, that the unreality of the assumptions of the Arrow-Debreu paper significantly helped the rise of a formalistic style too little concerned with the empirical relevance of the assumptions made, again has considerable plausibility. Once a model based on highly questionable assumptions becomes praised by the profession, then other economists feel authorized to do the same, and journals feel authorized to accept their papers. Thus, for example, the assumption of complete futures markets (or perfect foresight) has been extended to an *infinity* of future periods in the study of intertemporal general equilibrium over the infinite future: an extension not without motivations, given the illegitimacy of postulating that the economy ends after a finite number of periods; but so absurd an extension (what about the tastes of consumers yet to be born? or future technical progress?), that it should have induced economists to ask

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[2] The issue has also attracted the attention of methodologists. In 1989, in a contribution in the *Journal of Economic Perspectives* meant to give a general picture of the field of economic methodology, Daniel Hausman, after expressing some skepticism at the traditional central concern of methodologists − theory appraisal −, proceeded to list some issues on which, he suggested, the energies of methodologists might be more usefully employed. The first one was: "The role and significance of general equilibrium theory are still not entirely clear" (Hausman 1989: 51). Food for thought, isn't it: after decades of centrality in research and teaching, the recognized foundation of the dominant theory of value and distribution was found of still unclear role and significance by a scholar with excellent credentials for the role of impartial observer.

where the wrong choice had been made, whose inner logic obliged the theorist to make such an extreme assumption.

But if professor Blaug appears to have a point with both his claims relative to the Arrow-Debreu model, then an important question is thereby raised: what is it that induced Arrow and Debreu to propose, and the economic profession to accept, so patently unrealistic a model as the Arrow-Debreu intertemporal general equilibrium model as the foundation of the theory of value and distribution? Surely the answer, if a convincing answer can be found, will also help explain how general equilibrium theory can maintain its privileged place in textbooks as *the* rigorous formulation of the theory of competitive value and distribution, when so many doubts are expressed about its validity.

Professor Blaug does not supply a clear answer. In one article he suggests an influence of Bourbakism, the French project of recasting the whole of mathematics as deductions from axioms (Blaug 2003: 148, 150). But such an influence can perhaps explain Debreu's style, not the proposal of the conception of general equilibrium as an intertemporal complete-markets equilibrium, nor the success of that conception with the majority of the profession (anyway surely little touched by those tendencies in pure mathematics). In an earlier article, after explicitly posing the question of why the Formalist Revolution happened, Blaug had suggested a sociological explanation:

> As mathematics became the order of the day, the Young Turks found mathematics a perfect tool to disenfranchise the older generation and in a rapidly growing system of higher education after World War II some such device was useful as a way of gaining an edge over one's rivals in the academic market-place...the trigger for the entire process is nevertheless the steady growth of mathematical skills increasingly perceived as the entry ticket to an academic career. (Blaug 1999b: 276)

Considerations of this type can contribute to explain why mathematical competence became more and more important to the point, probably, of overestimation, but they cannot explain why the Arrow-Debreu conception of general equilibrium as an intertemporal complete-markets equilibrium was accepted: the acceptance of that notion of equilibrium (as distinct from admiration for the mathematical feat of the existence proof)[3] did not require

[3] Once a problem requiring advanced mathematics for its solution is shown to have economic relevance, the use of advanced mathematics for its study cannot be

high mathematics, as we can clearly see now that this notion of equilibrium is explained in second-year textbooks using no maths beyond elementary calculus. And since "When Arrow and Debreu employed game theory and the Nash equilibrium to prove existence of general equilibrium in the 1950s, the Formalist Revolution was still in its early stages" (Blaug 2003: 149-50), economic theorists could not be at the time already so deformed by formalism as to be ready to accept nearly anything as long as it was mathematically elegant.

Supplying a more satisfactory answer is the central purpose of the present paper.

The essential clue, I will argue, lies in the problems of neoclassical capital theory. The present paper intends to show that the reconstruction, started by Garegnani (1960, 1962, 1976, 1990) and then integrated by several other authors (e.g. Milgate (1982), Petri (1991, 2004)), of how these capital-theoretic problems have conditioned the development of neoclassical theory, supplies the needed answer. This answer has implications that extend beyond issues of historical reconstruction: it yields indications, that will be briefly pointed out at the end of the paper, on how to surmount the deficiencies of the Arrow-Debreu model that professor Blaug insists upon.

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criticised. And it is only normal that mastery of powerful mathematical tools at the level of good mathematicians should earn respect. Trouble only starts when these tools are wasted on irrelevant problems and the waste is not recognized as such. But assessment of a line of inquiry as relevant or wasteful is a theoretical issue, that normally, in economics, does not require the mastery of very advanced mathematics. Thus I would suggest that, in economics, employment of vast mathematical energies in a field only persists if economic theorists consider the field important. This is not to deny that there may be systemic reasons inducing economists to prefer problems requiring a high mathematical formalization, with a danger of misallocation of scientific effort: for example the greater comfort, and smaller cost, of doing armchair theorizing relative to collecting and processing historical and statistical information; the barriers to understanding and criticism introduced by the use of more and more advanced mathematics that only very few people can follow; the greater ease of ascertaining 'technical competence' (mastery of tools) rather than capacity to understand reality, in candidates to academic posts; and perhaps also the greater distance from the frequent ugliness of brutal facts, whose unprejudiced study might sometimes create problems to one's conscience, if not to one's career (let us remember what happened to the Israeli historian Pappe). These issues, important as they are, and largely amenable to analysis with the economist's tools (one example is Pagano 2002), fall however outside the scope of the present paper, which is not centrally concerned with the reasons for the mathematization of economics, but rather with the reasons for the success of the Arrow-Debreu model.

2. It is professor Blaug himself who brings us to issues discussed in recent debates on neoclassical capital theory. He particularly stresses two negative aspects of the Arrow-Debreu approach to general equilibrium: first, that it needs an assumption of complete futures markets; second, that it gives up any attempt to prove that the equilibrium is stable, and concentrates exclusively on existence. This second criticism, when well understood, is a useful entrance key to the question. Blaug accuses Arrow-Debreu of having abandoned Walras' attempt "to show that markets will clear automatically via price adjustments in response to positive or negative excess demands" (Blaug 2002: 37). But Arrow, shortly after the article with Debreu, co-authored with Hurwicz and Block two articles on the stability of equilibrium. So what Blaug must mean is that the modern studies of the stability of Arrow-Debreu equilibria do not really ask the same question Walras was asking. And why so? Because the studies of the stability of Arrow-Debreu equilibria, based as they are on a fairy-tale tâtonnement in a 'suspended activity' situation in which no exchanges nor production nor consumption are permitted until an equilibrium is reached, cannot pretend to describe, in Hahn's words, "the actual working of the invisible hand" and therefore cannot be considered studies of the stability of actual market processes[4]. But then even if it could be shown to be tâtonnement-stable, an Arrow-Debreu equilibrium would still offer no guarantee of being a situation toward which "the actual working of the invisible hand" pushes a market economy, even leaving aside the continuous innovation brought about by rivalrous Schumpeterian/Marxian competition. Thus Blaug can conclude that Arrow and Debreu have "ceased to make any descriptive claim about actual economic systems". However, on this issue of the study of stability, at least the Walras of the last two editions of the *Eléments*, where the tâtonnement is based on the new device of 'bons' and therefore excludes the implementation of disequilibrium productions (in the first three editions the tâtonnement involved actual disequilibrium productions and sales of factor services), is as liable as Arrow and Debreu to such a charge[5].

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4 That this is what actually Blaug means emerges when he writes (2002: 40) that "general equilibrium theory solved the stability-of-equilibrium question by ruling out disequilibrium trading"; cf. also the next footnote.

5 Blaug (2002b: 24-25) knows it well, and in (2002: fn. 4, p. 51) he confirms that he views the studies of stability through the tâtonnement based on 'bons' as not concerned with real stability, by writing that the later Walras shifted his focus from the stability problem to the existence question, and citing in support Walker (1997), and elsewhere Walker (1996), evidently interpreting Walker's criticism of the

One is then induced to ask, is there any analytical element common to the later Walras, and to Arrow-Debreu, that may explain the peculiar absence of realism of their analyses of stability? And might not the same element be also responsible for the other obvious absence of realism of the Arrow-Debreu model, the complete intertemporal markets assumption? If such an element can be found, then the explanation of the success of the Arrow-Debreu model will have to look for the causes of the acceptance of this element, in spite of the lack of realism it engendered.

3. The common analytical element we are looking for can indeed be found: it has been pointed out by Pierangelo Garegnani (1976, 1990) and insisted upon in Petri (1991, 2004). It is the treatment of the equilibrium's endowment of capital goods as a given *vector*, in other words, the inclusion in the equilibrium's data of a given endowment for each capital good.

The founders other than Walras of the marginalist[6] approach to income distribution had on the contrary treated capital as a single quantity of variable 'form' (composition), crystallised at any moment in a certain vector of heterogeneous capital goods but capable of changing 'form' without changing in 'quantity' through the utilization of the resources, which might reproduce the worn-out capital goods, for the production of different capital goods. When writing down (in equations or in words) the conditions determining the general equilibrium with capital goods, Jevons, J. B. Clark, Böhm-Bawerk, Wicksell all took as given the quantity of capital, a single number, and left its 'form' (i.e. its composition, and hence the endowments of the several capital goods) to be determined *endogenously* by the long-period condition of a uniform rate of return on the supply price of capital goods[7].

The difference this makes to the study of the stability of equilibrium is evident. Traditional marginalist authors, by conceiving capital as a single factor whose 'quantity' was only altered by net accumulation, not by changes in its 'form', could treat its endowment as given with the same legitimacy as for the endowments of (the several types of) labour or land, and could therefore allow for time-consuming disequilibrium adjustments including production and exchanges; these adjustments would adapt the 'form' of capital so as to reach a uniform rate of return on supply price, while in the

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unreality of the tâtonnement based on 'bons' (cf. e.g. Walker 1997: 116), as meaning an accusation that Walras had actually given up on stability.

6 'Marginalist' is used in this paper as synonymous with 'neoclassical'.

7 For Marshall, who shied away from an explicit formulation of the complete system of equations of the general equilibrium with capital goods, cf. Garegnani (1978).

meanwhile net savings would only marginally affect the total ‘quantity’ of capital which could therefore be treated as given. (The speed of variation of the endowment of capital so conceived could indeed be argued to be of the same order of magnitude as the speed of variation of population, or of the supplies of the several types of labour[8]: i.e. much slower than the speed of variation of the composition or ‘form’ of capital; this difference in relative speed rendered it legitimate to assume that the ‘form’ of capital was endogenously determined while treating its total endowment as given.) If, to the contrary, the data of equilibrium include given endowments of *each* capital good, then stability can only be studied on the basis of processes that do not cause changes in those data, and therefore on the basis of ultra-fast adjustments with recontracting, based on 'bons' or analogous fairy tales that exclude production during the adjustment.

The connection with the assumption of complete futures markets is also easily grasped. When the initial composition of capital is given, one can no longer assume, as on the contrary is possible in long-period analyses, that endogenous changes in equilibrium relative prices are slow enough as to make it legitimate to neglect them. The arbitrary given initial endowments of capital goods may be very far from what firms wish to have at their disposal, so they may be quickly changing with consequent relevant changes in relative prices. It becomes therefore necessary, in determining the actions of agents at the time the equilibrium is established, to take into account the changes that relative prices are undergoing. This can be done either by assuming the simultaneous determination of equilibrium current and subsequent prices (intertemporal equilibria), or by introducing expectations of price changes and determining temporary equilibria with expectation functions among the data. The complete futures markets assumption is the necessary way to take into account the relevant non-constancy of relative prices in these very-short-period equilibria, when one does not wish to consider temporary general equilibria with their problems deriving from subjective unobservable and non-uniform expectations.

4. The question to be answered is then, why did the Walras-Arrow-Debreu treatment of the capital endowment as a given vector finally come to

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[8] Precisely because of the slowness with which they can be assumed to change, the endowments of the different types of labour could each be treated as given in the determination of a long-period equilibrium, and therefore, contrary to a frequent misunderstanding, the need for 'aggregation' which, in long-period equilibria, arises for capital does *not* arise for labour, cf. Petri (1999: 45-6).

be preferred to the originally dominant treatment of capital as a single factor of endogenously determined 'form'?

A satisfactory answer must first explain why the Walrasian treatment of the capital endowment remained for many decades minoritarian. The reason, it will be argued, is that, at least until the 1940s, adherence to the traditional *long-period method* prevailed; as the next section points out, Walras was no exception, and was simply contradictory because his treatment of the capital endowment was incompatible with that method (Garegnani 1962, 1990); it is then not surprising that his treatment of the capital endowment could only become widespread when the basis of the neoclassical theory of value and distribution shifted to *very-short-period* equilibria. Section 3 goes on to explain why this shift occurred; Section 4 shows that the shift was accompanied by confusion and uncertainty; Section 5 pulls together the argument and enunciates the answer to the question why Arrow-Debreu was so successful; Section 6 briefly discusses the implications of this answer for the best way forward for value theory.

II. Walras as a long-period theorist.

5. The distinction between long-period and very-short-period notions of price and of equilibrium is fundamental, and can help bring clarity to the debates on the interpretation of Walras too. Walras, no less than the other founders of the marginalist approach, aimed at determining prices yielding a uniform rate of return on supply price: the prices called natural prices by Adam Smith, prices of production by Marx, long-period normal prices by Marshall, and simply equilibrium prices by Wicksell.

This notion of long-period price is of course what students are introduced to in any economics textbook, in the chapter illustrating the partial-equilibrium analysis of the tendency (in competitive conditions with free entry) of the short-period price of a product toward the long-period price corresponding to zero 'pure profits', owing to changes in the number and/or dimension of firms in the industry([9]). In these analyses the long-period price

[9] Zero 'profits', in the marginalist sense of what is left of revenue after paying all costs including interest (gross of a risk allowance) on the capital employed. The Classical authors did not include interest among the costs to be subtracted from revenue in order to obtain profits, so that the term 'profits' has a different meaning: the tendency to zero 'profits' in the marginalist sense is expressed by the Classical authors as the tendency of profits to become the normal ones i.e. to guarantee the normal 'rate of profits' (the same rate of return on the capital employed as in other industries - once account is taken of risk).

of a good, i.e. its minimum average cost, is determined on the basis of *given* input prices. But the moment capital goods are admitted among the inputs of the good in question, the same tendency must be admitted to be simultaneously at work for *their* prices, and (by altering their quantities) to be influencing the rentals to be paid for their use and thus the average cost of the good in question; so, a consistent determination of the long-period price of a product requires the simultaneous determination of the long-period prices and rentals of all capital goods directly or indirectly entering its production, i.e. the determination of the uniform-rate-of-return-on-supply-price (URRSP) relative prices I have been speaking of.

Adam Smith, Ricardo, Jevons, Marshall, Wicksell, Robertson, J. B. Clark, in spite of profound differences in the theory of income distribution and employment, did not differ on the central role of these long-period prices. These were the prices that analysis had to determine in order to be able to explain and predict the trend of the average day-by-day market price of a product.

Thus marginalist authors like Marshall, Robertson, Wicksell or J. B. Clark took it for granted – just like Smith, Ricardo, or Marx – that it is not only uninteresting, but also impossible, fully to describe the forces determining the details of each single transaction, or production decision; but they shared with the classical authors the belief that it was possible to explain and predict *the average* of each price or quantity, because the actual path of a price or of a quantity, although unpredictable in its details, would tend to gravitate around and towards definite values or "centres of gravitation", independent of the details of the gravitational process itself. The existence of this gravitation made the prediction of each single transaction unnecessary (and uninteresting). Changes of this "centre of gravitation", caused by changes in the data determining it, could then be used to explain and predict the *trend* of the actual path of the variable under consideration, a trend determined by the tendency to gravitate toward the new (or the shifting) centre of gravitation. The differences between the classical and the marginalist theories of distribution entailed that the centres of gravitation were determined differently in the two approaches, but the distinction between market and normal magnitudes, the latter being the centres of gravitation of the former, is found in both groups of theories[10].

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10 Marshall had a more articulated analysis and thought that one could also conceive, in certain cases, of more short-period centres of gravitation of market prices in partial-equilibrium analyses (although in those cases his analysis did not reach the definiteness of results achievable in long-period analysis, cf. Ciccone,

6. So the role traditionally assigned to equilibria by marginalist authors was the very concrete and relevant one of determining the average or trend of the observed prices and quantities of economies *admitted to be continually in disequilibrium*[11]. Walras is no different in this respect:

> It never happens in the real world that the selling price of any given product is absolutely equal to the cost of the productive services that enter into that product, or that the effective demand and supply of services or products are absolutely equal. Yet equilibrium is the normal state, in the sense that it is the state towards which things spontaneously tend under a régime of free competition in exchange and in production. (Walras 1954: 224-5)

> Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping, and, before the goal is reached, it has to renew its efforts and start over again, all the basic data of the problem, e.g. the initial quantities possessed, the utilities of goods and services, the technical coefficients, the excess of income over consumption, the working capital requirements, etc., having changed in the meantime. Viewed in this way, the market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. But whereas there are days when the surface of a lake is almost smooth, there never is a day when the effective demand for products and services equals their effective supply and when the selling price of products equals the cost of the productive services used in making them. The diversion of productive services from enterprises that are losing money to profitable enterprises takes place in various ways, the most important being through credit operations, but *at best these ways are slow*. (*ibid.*, p. 380, emphasis added)

These passages are present from the first to the last edition of Walras' *Eléments*.

1999). But the variables impounded in the ceteris-paribus conditions in those analyses, and especially the distributive variables, were not based on *short-period* equilibration between supply and demand, but rather on long-period forces.

[11] For some examples of application of the long-period method, cf. Petri (2004: ch. 1).

An important implication of these admissions is the following. If one wants to determine the "centre of gravitation" of time-consuming adjustment processes for an economy with produced means of production (capital goods), then the need to conceive these processes as taking time and involving the implementation of out-of-equilibrium decisions *makes it impossible to include a given quantity of each capital good among the data determining the "centre of gravitation"*, because disequilibrium processes can and will quickly alter these quantities, which must therefore be conceived as determined by, rather than determining, the 'centre of gravitation'[12]. As a consequence, the positions qualifying as "centres of gravitation" in a general equilibrium analysis must be such that the relative amounts of the several capital goods in existence have themselves reached an equilibrium and are therefore endogenously determined by the equilibrium itself. In those marginalist authors who recognized this logical necessity of their analysis, the force which would tend to adjust the stocks of capital goods to the demands for them was the same as in the classical authors: the 'mobility of capitals' in the search for the highest rate of return, i.e. the tendency of investments to be directed to the purchase prevalently of the capital goods offering the prospect of a higher rate of return – the same process which was seen as responsible for the tendency of rates of return on supply price toward uniformity.

Here too Walras is no different; he too writes that in equilibrium this process must have completed its operations:

> Capital goods proper … are products and their prices are subject to the law of cost of production. If their selling price is greater than their cost of production, the quantity produced will increase and their selling price will fall; if their selling price is lower than their cost of production the quantity produced will diminish and their selling price will rise. In equilibrium their selling price and their cost of production are equal. (Walras 1954: 271; unchanged from the second to the last edition of the *Eléments*).

Consistently with this quotation, in his general equilibrium equations Walras assumes the equality between cost of production and "selling price" (his term for *demand price*, the present value of the future rentals to be earned by the capital good), i.e. he assumes the uniformity of the rate of return on supply price for all capital goods. This is the distinctive mark of long-period analysis. The above quotation furthermore admits that it is *changes in the*

12 The *very* durable produced means of production (e.g. dams), once built, are more appropriately seen as analogous to natural resources.

relative endowments of the several capital goods that bring this equality about: the reason why an increase in "the quantity produced" brings about a lower "selling price" for a capital good can only be the decreased scarcity of that capital good, brought about by the increase in its endowment. Again, this is fully traditional. But then he, like e.g. Wicksell, should have treated the relative endowments of the several capital goods as variables, that adapt so as to guarantee, for all capital goods, the equality between cost of production and capitalized value of future rentals. On the contrary, he takes the endowments of the several capital goods as given.

7. The explanation would appear to be that he did not initially realize that he was being contradictory. He seems to have initially thought that he could assume the initial endowments of capital goods to have already adapted so as to bring about a uniform rate of return on supply price, in spite of their treatment as data, i.e. arbitrary givens, of the equilibrium. This is what the passage from Walras (1954: 271) quoted above suggests. As shown in the Appendix to this paper, this interpretation is also supported by Walras' wholehearted endorsement of an 1890 article by Bortkiewicz. And then there is the fact that, up to the third edition, Walras supports the URRSP assumption on the basis of the argument that the equality of rates of return on the supply prices of the several capital goods will be reached because of two reasons: when a higher-than-average rate of return induces an increased production of a capital good, the increase in the production of that capital good will, first, slightly ('légèrement') increase the rentals of the factors employed in its production and thus will increase its supply price ('prix de revient')[13], and second – and this is what interests us here – *it will appreciably decrease its demand price* ('prix de vente', Walras 1988: 396, § 253 of the second edition; also cf. Walras 1954: 594, Jaffé's collation note [n] to §257), a decrease only attributable to a decrease of the rental of that capital good, due to an increase in the *endowment* of the capital good.

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13 With a strange contradiction, Walras writes that this 'slight' increase in the factor rentals will *appreciably* ('sensiblement') increase the supply price of the capital good: he does not seem to realize that if all rentals increase 'légèrement', the total cost of production increases 'légèrement' too. It may also be noticed that the increase in the supply price of the capital good will entail a decrease of the rate of return on supply price only if the rental of the capital good does not increase even more, which requires that the production of the capital good does not utilize the services of that same capital good as an input in a higher-than-average proportion – a problem for Walras' argument first pointed out by Garegnani (1962: 14-15), cf. Petri (2004: 142, fn. 11).

8. Only between the third and the fourth edition of the *Eléments* Walras realized that his previous reasoning was illegitimate. Strikingly, the editors of the recent variorum edition of the *Eléments* (Walras 1988: 812) do not include the changes in the theory of capitalization among the important modifications introduced in the 4th edition. On the contrary, these changes are extremely important because they show that Walras finally reaches a better appreciation of the implications of the given capital endowments. The change most relevant for our interpretation is the new justification of the tendency toward a URRSP, which is now exclusively based on the first of the two reasons mentioned in §**7**: all reference to an influence of an increase in the production of a capital good upon its demand price has disappeared (Walras 1954: 292-3; 1988: 399).

Thus now the increase in the production of a capital good causes a decrease in its rate of return on supply price only because of the increase in its cost of production, and no longer also because of the inadmissible increase in its endowment. This radical change in the reasoning shows that Walras has finally realized that, since he is including the endowments of capital goods among the data of the equilibrium, he cannot let them change as part of the process bringing equilibrium about. This new awareness and the consequent changed justification of the tendency toward a URRSP are what probably make him also realize that in fact a uniform rate of return cannot be generally reached (Walras 1954: 294, 308; 1988: 401, 430-1), another very important novelty of the fourth edition – an admission of radical inconsistency of the equations of the model, in fact – neglected by the editors of the 1988 edition.

9. Walras' new awareness that he cannot let the equilibrating process change the endowments of capital goods is also, most probably, the explanation of the introduction of the tâtonnement with 'bons' in that same fourth edition. There is general consensus that up to the third edition the tâtonnement is described by Walras as entailing actual disequilibrium productions (with the difficulty, unnoticed by Walras, that the endowments of capital goods would change during the tâtonnement); there is, on the contrary, disagreement on the reasons for the introduction of 'bons' in the 4th edition. Archival research has been so far unable to discover unpublished notes by Walras capable of shedding more light on this issue. One has to rely on his published writings, and these suggest that, without the new awareness that the equilibrating process must not change the endowments of capital goods, there would have been little reason for Walras to change his previous description of the tâtonnement of the production and of the capitalistic economy. As

confirmed by the Bortkiewicz article discussed in the Appendix of the present paper, by the time of the second edition of the *Eléments* Walras had concluded (and all other marginalist economists would have agreed with him) that in the production economy (i.e. in the model where capital goods were supposed either non-existent or identical to lands, i.e. eternal and not currently produced) he had the right to consider the data of the equilibrium as unaffected by the path taken by the tâtonnement (1988: 308; 1954: 590 [*f*]), because the amounts of factors would not be affected, and because the tastes of consumers could be assumed given. Thus, in order to be induced later to change his description of the tâtonnement, he must have discovered some new problem.

There is no evidence of any such discovery until 1899, when in the memoir *Equations de la circulation* Walras tried to include in the equilibrium equations the determination of equilibrium amounts of inventories of produced goods too, again on the basis of data including for each consumer the ownership of given endowments of *all* factors, even of inventories[14]. It is here that the *bons* appear for the first time. The high variability of inventories must have made it particularly evident to Walras that the adjustments toward equilibrium had to be conceived as excluding, not only actual exchanges, but also actual productions, which would have altered endowments. Thus Walras ended the memoir with a Note where he suggested, still very tentatively, that the adjustments "pourraient être supposés fait sur *bons*" (Walras 1899(1993): 581), adding that "Peut-être, au moyen de cette hypothèse, distinguera-t-on plus nettement" a first phase of 'preliminary tâtonnements' from a second phase of actual exchanges and productions of the equilibrium quantities and from a third phase of "*équilibre dynamique*" with change in the data and establishment of new equilibrium. Immediately afterwards he explicitly stressed that "il doit être bien entendu" (it must be clearly understood) that the amounts of newly produced capital goods determined by the equilibrium equations only start being utilized in a subsequent period, "ne fonctionnent que dans la troisième phase" (ibid., 581-2) – a warning pointing precisely to the problem arising with his previous description of the tâtonnement as entailing actual productions.

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14 "...nous supposons une société établissant cet équilibre *ab ovo* pour une période de temps déterminée, pendant laquelle il n'y aura pas de changements dans les données du problème. C'est pourquoi aussi nous dotons nos propriétaires fonciers, travailleurs et capitalistes consommateurs de quantités quelconques de capitaux circulants et de monnaie, comme nous les avons dotés précédemment de quantités quelconques de capitaux fixes: fonciers, personnels et mobiliers" (Walras 1899(1993): 566). 'Capitaux circulants' is Walras' term for inventories.

In the 4th edition of the *Eléments* the tentative tone disappears: evidently further reflection had shown to Walras that his previous description of the tâtonnement was *incompatible* with the data of his equations, and therefore the *bons* were not simply a way 'more neatly to distinguish' the equilibrium from the adjustment toward it, but rather were the *sole* way to achieve consistency between the data of equilibrium and the description of the adjustment towards it. In this edition Walras reproduces the explicit warning that new capital goods only start functioning in a subsequent period (and therefore do not alter the equilibrium's endowments) (§274; Walras 1954: 319, 1998: 447), and adds to it the observation, when discussing the tâtonnement in the theory of capitalization, that the tâtonnement based on *bons* prevents the production of disequilibrium quantities of new capital goods (§251; Walras 1954: 282, 1998: 377).

10. This interpretation is more convincing than some alternative ones.

Bridel and Huck argue that even in the production economy Walras had a serious problem with "distributional effects" (redistributions of the endowments among individuals), and they attribute the introduction of 'bons' to the need to preserve "the distributional neutrality of *tâtonnement*" (Bridel and Huck 2002: 521), a need that had already induced Walras in the second edition of the *Eléments* to assume no exchanges at disequilibrium prices in his exchange model. They advance two arguments in support of their claim: first, in order to determine the selling prices of the given quantities produced at each round of the tâtonnement in the production economy, Walras assumes given demand functions for the produced goods, while disequilibrium transactions would alter them; second, as long as the prices of produced goods differ from their costs of production, entrepreneurs make profits or losses and this alters their wealth, hence it cannot be assumed that the wealth of each individual remains unaltered during the tâtonnement. But the first argument founders on Walras' opinion, expressed in the discussion of the exchange economy, that once the quantity of a product to be exchanged or sold on a market is given, the market is usually able to find the equilibrium price very quickly and that therefore his assumption of no disequilibrium transactions in the exchange economy is *realistic*[15]. As to the second argument, the profits

15 "The rapidity and reliability of the practical solution leave no room for improvement. It is a matter of daily experience that even in big markets where there are neither brokers nor auctioneers, the current equilibrium price is determined within a few minutes, and considerable quantities of merchandise are exchanged at that price within half or three quarters of an hour" (Walras 1954: 106; 1988: 93). No analogous appeal to realism could be advanced in the 4th edition for the elimination

and losses of entrepreneurs are no more important than other possible redistributions of wealth during the tâtonnement, e.g. due to sales and purchases of lands at disequilibrium prices, and Walras, like all other marginalist economists, justifiably neglects these changes as of secondary importance, indeed, he never mentions them as a possible problem for his theory.

Donzelli (2005) admits that the change in the capital endowments is one of the two main difficulties of the tâtonnement of the second and third edition, the other one being the possibility that the disequilibrium productions assumed to happen in the tâtonnement without 'bons' be in fact impossible because entailing factor demands different from (I suppose Donzelli means, in excess of) factor supplies; and he argues that it is this second difficulty "which in the last analysis explains both Walras' twistings in the first three editions of the *Eléments* and his final change of course in the fourth one" (2005: 35, fn. 23). However Donzelli is unable to produce any textual evidence that the introduction of 'bons' was motivated by this difficulty[16].

The absence of textual evidence for these interpretations contrasts with the explicit admissions by Walras, noticed at the end of §**9**, that the 'bons' prevent the production of disequilibrium quantities of new capital goods and prevent the tâtonnement from altering that part of the data of the equilibrium consisting of the endowments of capital goods. These interpretations also suffer from not relying on some *new* difficulty discovered by Walras between the 3rd and the 4th edition. It must be remembered that the tâtonnement of the second and third edition is in full accord with Walras' description, quoted earlier (§**6**), of the adjustments toward equilibrium as "slow" and entailing disequilibrium productions, a description that he never retracts. The new tâtonnement based on 'bons' is hardly reconcilable with that description; it is also in contradiction with Walras' earlier admission, in a letter to Barone dated 1895, that the production function is not known a priori to the entrepreneur, and must be found out by experimentation (Walker 1987: 771). In order to resign himself to a description of the tâtonnement contradicting so sharply his views of the actual working of the economy, Walras must have

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of disequilibrium productions and the neglect of the time required for production.

16 If, as Donzelli appears to suggest, this difficulty motivated the role of foreign markets in the description of the tâtonnement in the production economy in the first edition of the *Eléments*, then the disappearance of the role of foreign markets in the second edition must have been due to some reflection on this difficulty, which brought Walras to conclude that he could neglect it; so if Donzelli were right, Walras should have afterwards changed his mind on this issue, but there is no indication that he did.

been motivated by the discovery of some grave new problem. The new awareness of the implications of the given endowments of capital goods for the tâtonnement and for the tendency to a URRSP stands out as the sole plausible candidate I am aware of; no other analogous discovery is revealed by the fourth edition of the *Eléments*.

III. Why the dominance of long-period notions of equilibrium was subverted after some decades.

11. The long-period character of Walras' formalization emerges not only in the URRSP assumption but also in several other elements (Petri 2004: 147). For space reasons I only remember the total neglect of the changes that relative prices may undergo over time: e.g. the determination of the value of land as equal to the rental divided by the rate of interest, which implies that Walras thought that equilibrium prices could be treated as if unchanging for very long periods. This confirms that Walras' conception of equilibrium was a long-period one, like that of everybody else at the time; then it is not surprising that Walras' treatment of the capital endowment remained isolated among the founders of the marginalist approach: the other founders were clearer than Walras on the need to leave the composition of capital to be determined endogenously by the equilibrium. What we must now explain is why the situation was reversed some decades later.

The answer lies in the gradual recognition of grave difficulties with the conception of the several capital goods as embodiments, crystallizations, portions of a single factor 'capital' of variable 'form', which is the conception of capital that one finds in the other founders of the marginalist approach, and which rendered it possible to formulate long-period general equilibria where the endowments of the several capital goods were endogenously determined[17].

This conception of capital was fundamental to the plausibility of

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17 The presence of this conception in the generality of marginalist authors up to the 1930s, with the sole exception of Walras and of a few economists more directly influenced by him, is universally admitted and I need not dwell on it. (Walras was apparently unable to grasp the possibility of such a conception of capital.) Nowadays a widespread misunderstanding identifies this conception of 'capital' as a single factor with the use of aggregate production functions. But the traditional long-period equilibria were fully disaggregated general equilibria and nonetheless needed the conception of capital as a single factor of variable 'form', because they left the composition of capital to be determined endogenously by the equilibrium. This is made especially clear by Wicksell (1934). Cf. Petri (2004: ch. 3).

marginalist analyses. One can distinguish a supply-side from a demand-side role of this conception. Its supply-side role was to make it possible to leave the endowments of the several capital goods as variables to be endogenously determined by the equilibrium; thus the endowments of the several capital goods were *not* data of the equilibrium: had they been so treated, this would have deprived the equilibrium of the *persistence* required to conceive it as the centre of gravitation of day-by-day magnitudes[18]. The persistence of the data made it possible to extend to economies with capital the conception applicable to economies with only non-produced factors, i.e. the conception (adopted by Walras too before the introduction of 'bons') of disequilibrium processes as taking time and involving the implementation of disequilibrium production decisions.

On the demand side, this conception considered capital as analogous to labour or land in the factor substitution mechanisms central to the theory, thus permitting the conclusion that the demand for capital was a decreasing function of its price, i.e. of the rate of interest. One can usefully distinguish *two* aspects of this demand-side role. First, the postulate that 'capital'-labour substitution worked much like land-labour substitution made it possible to assume that the substitution mechanisms worked in the 'right' direction; the rate of interest could then be viewed as the price bringing into equality the supply of and the demand for 'capital', and therefore also the supply of, and the demand for, savings or loanable funds. Thus the decreasing demand curve for 'capital' was the basis for the acceptance of "Say's Law", i.e. of the tendency of investment to adapt to savings rather than vice-versa[19]. The

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18 Persistence does not mean total absence of change; it only means that the changes in the data can be taken to be sufficiently slow, relative to the presumable speed of tendency toward the equilibrium, as to render the (slowly moving) equilibrium position a good indication of the trend of market prices and quantities. An example is population changes. Drastic once-for-all changes in the equilibrium's data were of course to be studied via comparative statics.

19 The discovery of reverse capital deepening undermines this basis. In the classical authors who, like Ricardo, accepted Say's Law (i.e. accepted that all savings would translate into investment so that 'general gluts' could be excluded), the reason was *not* an equilibrating role attributed to the rate of interest on the basis of a decreasing demand curve for capital or for loanable funds (Garegnani 1978). The absence of the notion of decreasing demand curves for factors in these authors shows up in the fact that Say's Law was not thought to imply the full employment of labour, and that unemployment created by labour-saving technical innovations was thought to require, in order to be absorbed, capital accumulation, not a change in capital-labour proportions at the given stage of accumulation as on the contrary argued by marginalist theory.

second aspect was that the variable ‘form’ of ‘capital’ made it possible to assume a *sufficient* substitutability between ‘capital’, and labour or land; such a sufficient substitutability was impossible to conceive for the single capital goods, as openly admitted e.g. by Hicks (1932: 18-21) or Robertson (1931).

12. But this factor 'capital' had to be conceived as a quantity of *value*, or at least a quantity of a substance proportional to value, because in equilibrium heterogeneous capital goods receive net rentals proportional to their values (their costs of production) and if these rentals are to be seen as reflecting the contribution of a common factor ‘capital’ crystallized in them, then necessarily the amount of capital embodied in them must be proportional to their net rentals and therefore also to their values.

This conception entails therefore that, whatever substance it is conceived as being made of, the 'capital' embodied in different capital goods must be proportional to their equilibrium relative values; but then any change in distribution, by altering the relative values of commodities, implies a change in the relative amounts of 'capital' contained in different capital goods; thus the 'quantity of capital' embodied in any given vector of capital goods depends on the choice of numéraire and on the prices (and hence on the income distribution) assumed to be ruling at the time of measurement; therefore the 'capital' endowment of an economy changes as relative prices change, *even* when the capital stock remains unchanged as a physical vector; it is therefore impossible to take the endowment of 'capital' of an economy (a single number) as given without arbitrariness when relative prices are what must be determined. A long-period marginalist general equilibrium is accordingly impossible to determine: the datum relative to the endowment of 'capital' is logically indeterminable.

13. After some decades, this problem started to be admitted by marginalist economists. Knut Wicksell, the first economist to attempt the writing down of the complete system of equations of a long-period disaggregated general equilibrium, grew clearly uneasy with the need for an endowment of 'capital' measured as an amount of value; indeed, he 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" (Wicksell, 1934, p. 202), and he admitted a few lines later that this implied an

"indeterminateness" of the endowment of capital[20]. Therefore it is not surprising that, later, his pupil Lindahl openly admitted that the notion of a 'quantity of capital' was indefensible because indeterminable independently of relative prices (Lindahl 1939: 316-17). Lindahl turned to the treatment of the capital endowment as a a given vector, and formulated in 1929 the notions of intertemporal equilibrium (with perfect foresight) and of temporary equilibrium. Friedrich Hayek developed a similar rejection of value capital in about the same period (Milgate 1982), and he too turned to a vector specification of the capital endowment, vigorously criticizing the conception of capital as a single factor in a series of articles culminating in Hayek (1936)[21]. Around 1935 John Hicks too became unhappy with capital as a 'fund', under the impact of Shove's harsh objections to his uncritical use of that notion in *The Theory of Wages* (cf. Garegnani 1976), and under the influence of Hayek and Lindahl he turned to temporary equilibria. His *Value and Capital* was very influential[22]. Later, one finds for example Friedrich Lutz writing: "the subsistence fund, in the sense of a given value magnitude, cannot be taken as a datum but is itself one of the unknowns, so that the system of these writers [Lutz is referring to Böhm-Bawerk, Wicksell, and other 'Austrian' authors, *F.P.*] lacked one equation for determining the equilibrium" (Lutz 1967, p. 69). Lutz too concluded by opting for the treatment of each capital good as a separate factor with its given endowment.

14. But one also finds in 1963 a treatise on general equilibrium by Robert E. Kuenne, which, after discussing Walras in considerable detail, concludes that the nature of capital and of the origin of a positive rate of interest are best examined in terms of stationary economies, i.e. with an endogenously determined composition of capital (only to be then unable to

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20 In spite of this admission, in the immediately following formulation of the general equilibrium equations Wicksell takes as given "the total exchange value of the capital employed" (1934, p. 204); but he does not explain why he considers such a procedure acceptable in spite of what he has just written.

21 Blaug (1999b: 260-1) argues that the controversy between Knight and Hayek, of which Hayek (1936) is part, resulted in "the categorical rejection of any operational metric of capital expressed in the dimension of time", thus suggesting that what was rejected was only the Austrian approach to capital. In fact Hayek forcefully rejected not only the average period of production but also any conception of capital as a single quantity, a 'fund', a conception present in J. B. Clark, in Knight, and in the Marshallian school as much as in the 'Austrian' authors.

22 Interestingly, Blaug (2002, fn. 3, p. 51) states that "the rot goes back to Hicks (1939)", but he does not stop to clarify the roots of the rot in the adoption of a very-short-period notion of equilibrium.

choose between the different views of Clark, Böhm-Bawerk, Fisher, Knight, Wicksell, Metzler – an indirect confirmation of the difficulty of making the notion of capital as a single factor logically consistent).

Anyway Kuenne is able to avoid a given endowment of capital (the value magnitude) only because he, like Hicks (1939) before him, mistakenly interprets the traditional stationary state assumption as referring to a *very*-long-period, or '*secular*', stationary state, i.e. a state in which accumulation has come to an end because the capital endowment has become so large, and the interest rate so low, that net savings become zero (the capital endowment is then endogenously determined not only in its composition but also in its 'quantity'): the stationary state assumption of J. B. Clark or Wicksell was on the contrary concerned with determining the income distribution of a given economy with its *given* quantity of capital, and only had the purpose to leave aside the complications connected with the existence of net savings so as to make the reasons for the level of the rate of interest as clear as possible (Garegnani 1976; Petri 2004, ch. 4). Thus Wicksell (1934: 204) had been crystal-clear on the need to include among the data of a long-period equilibrium a *given* value endowment of capital. But after Hicks's *Value and Capital* long-period equilibria are more and more confused with steady-growth (i.e. 'secular') equilibria[23].

Actually, in the 1940s and 1950s there appears to have been a striking loss of familiarity with older marginalist analyses, to the point of no longer understanding the long-period nature of the equilibrium that older marginalist authors had in mind[24], and often of being content with the general-equilibrium model of the *acapitalistic* production-and-exchange economy, as if capital did not pose any new analytical problem relative to land-labour

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23 Hicks used this identification of long-period equilibrium with secular stationary equilibrium to argue that long-period equilibria were useless because unable to deal with real economies, which are not stationary; his argument is refuted in Petri (2004: ch. 4). More generally, it is nowadays common to attribute the shift to very-short-period equilibria to a supposed applicability, of analyses where changes of normal relative prices are neglected, only to steady states. This is a misunderstanding that forgets that long-period analyses do not need the strict constancy of relative prices through time, but only that endogenous changes in long-period prices (i.e. in normal costs of production inclusive of the normal rate of return) be slow relative to the presumable speed of convergence of market prices to costs of production.

24 As I argue in Petri (2004: Appendix 5A1), persuasive evidence that already by the 1940s the loss of familiarity with the older, long-period notion of equilibrium was enormous comes from the Patinkin Controversy (or Classical Dichotomy Controversy) in monetary theory.

models[25].

IV. The new problems arising with the shift to very-short-period (neo-Walrasian) general equilibria.

15. It would seem therefore that with the 1930s a period of confusion and uncertainty ensued on how to insert capital goods into the supply-and-demand approach to value and distribution. The more and more widely perceived illegitimacy of a value endowment of capital pushed toward the adoption of a vector endowment of capital, but, it would seem, with little clarity as to the implications of such a shift to very-short-period equilibria. The new problems posed by such a shift were left nearly unmentioned.

It is useful briefly to recall those, of these new problems, more clearly responsible for much of the current dissatisfaction with general equilibrium theory in its modern formulations (Garegnani 1976, 1990; Petri 1991).

The *impermanence problem* is nowadays widely recognized in one form or other (but generally its gravity is not fully appreciated). The equilibrium's data relative to the endowments of capital goods are insufficiently persistent, so before agents can learn and correct disequilibrium mistakes, the equilibrium may have changed considerably, thus affording little guide to the behaviour of the economy. (This is the reason for the restriction of stability studies to fairy-tale tâtonnements with 'bons'.) Among neoclassical economists, the gravity of this problem has been fully recognized almost only by Franklin M. Fisher, who has admitted that the moment one accepts that disequilibrium involves actual productions, then disequilibrium actions change the economy's endowments, and this "makes the calculation of equilibria corresponding to the initial state of the system essentially irrelevant" (Fisher 1983: 14). Where realistically conceived disequilibrium adjustments will take the economy becomes in fact *a totally open question*, on which modern general equilibrium theory has nothing to say because it is silent on what happens when disequilibrium decisions are implemented; it gives no reason to presume that the actual path taken by the economy will not

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25 Weintraub and Gayer (2001), although their aim is different, provide evidence in support of this statement by showing that between the end of the 1940s and the end of the 1950s the main textbooks in value theory used in graduate courses in the USA (Sidney Weintraub; Stigler; Henderson and Quandt) only presented the *acapitalistic* general equilibrium model, without discussing how that model could accommodate capital goods. Lange (1942), the article at the origin of the Patinkin controversy, is also based on an acapitalistic general equilibrium model.

be considerably different from the equilibrium path. The theory's silence on the economy's behaviour in realistically conceived disequilibrium means that modern general equilibrium theory tells us *nothing at all* as to how actual economies behave (Petri 1999: 49-50). Professor Blaug would appear to be close to the same conclusion.

The *substitutability problem*, less widely recognized nowadays but not less important, consists of the fact that alternative productive methods generally require different capital goods, not the same capital goods in different proportions, so only the conception of capital as a single factor of variable 'form' can give plausibility to the assumption of extensive factor substitutability; with a Walrasian treatment of the capital endowment the absence of substitutability will generate implausibly high numbers of zero rentals and a highly inelastic labour demand curve resulting, with high likelihood, in implausible levels of the equilibrium real wage (as admitted e.g. by Hicks in 1932: 18-21). The substitutability problem further reinforces the relevance of the impermanence problem because it also implies that small changes in the composition of capital can drastically change many capital goods' rentals as well as the equilibrium real wage.

The *price-change problem* arises due to the fact that, as already noticed (§**3**), it becomes necessary to take into account the changes that relative prices are undergoing. This is done either by assuming the existence of complete futures markets and the simultaneous determination of equilibrium on current and on subsequent markets (intertemporal equilibria), or by considering temporary equilibria with expectation functions among the data. The dilemma then arises between the absurd assumption of complete futures markets or perfect foresight, and an *indefiniteness problem* arising in temporary equilibria: subjective unobservable expectations render the results of the analysis largely arbitrary, depending on unverifiable assumptions about expectations and their change over time.

16. The older versions of marginalist theory relying on the conception of capital as a single factor of variable 'form' did not suffer from these problems: the long-period nature of the equilibrium left the composition of capital to be determined endogenously, thus avoiding the impermanence problem; the variable 'form' of capital avoided the substitutability problem; expectations could be assumed to have the time to be corrected by experience on average, thus falling out of the data of the problem. (In the case of Walras, the overabundance of mathematics obscured the fact that his treatment of the capital endowment was incompatible with his declared aim of determining a long-period equilibrium.)

It should now be clearer why the marginalist approach at its birth could not be accused of extreme abstraction or unreal assumptions, and could propose itself as a more satisfactory determination of what the theory of value and distribution had always attempted, the determination of long-period positions[26]. Indeed, when looking for the reasons for its success, it must not be forgotten that the marginalist approach became dominant on the basis of the (mistaken) faith that it was able to determine *long-period* equilibria, capable of describing the trend of economies realistically conceived as continually in disequilibrium[27]. At the end of the 19th century it was nearly impossible to believe otherwise: the specialist works on this issue, Walras's *Eléments* in its first three editions (1874-1896), and Wicksell's *Uber Wert, Kapital und Rente* (1893), the sole attempts at the time to treat the problem of capital within a general equilibrium setting, had both claimed to have shown that the approach was able to determine a long-period equilibrium. Nor was it easy to realise, later, that the changes in Walras's 4th edition of his *Eléments* (1900) implied defeat in this respect: Walras did not openly admit it, and to the best of my knowledge the thing became clear only with Garegnani (1960). As to Wicksell, his rejection in the *Lectures* (1901-1928) of the average period of production and hence of the 1893 analysis, and reticent admission of problems with determining the endowment of capital, became available in English only in 1934. The writings of Lindahl and Hayek that question capital the single factor come out in the same decade. If we consider 1880 as a date when the marginalist approach was already very influential, then we must conclude that for fifty years at least, the faith in the marginalist approach was based on a myth: that it was able satisfactorily to determine long-period equilibria. The history of economic theory might easily have taken a different turn, if Walras' and Wicksell's admissions of difficulties had happened earlier and had become common knowledge before the supply-and-demand approach had had the time thoroughly to permeate the economists' minds.

17. It can also be doubted that the approach would have been able to impose itself, if it had been presented from the start in versions needing an assumption of instantaneous equilibration. The shift to the very-short-period

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26 This term is used to encompass also the natural prices and normal effectual demands that classical value theory aimed at determining.

27 Thus professor Blaug's identification (particularly clear in Blaug 1999b: 267) of what he calls "the end-state conception of competition" with the neo-Walrasian notion of equilibrium is misleading: long-period equilibria were end-states too, but compatible with time-consuming disequilibria, and the same holds for the classical long-period positions.

versions could only be accomplished after decades of total dominance of the approach, when it had become so deeply ingrained into the economists' minds that its conclusions tended to be taken as obvious aspects of reality (rather than as the results of a complex chain of deductions whose validity became doubtful once one of its pillars was recognised to be untenable); the replacement of long-period equilibria with sequences of very-short-period equilibria could then be considered of minor importance, since the faith in the tendency toward a situation of full employment, with each factor receiving its marginal product, was by then solidly entrenched, and the sequences of very-short-period equilibria were sequences of full-employment situations, with factors receiving their marginal products.

It seems clear, in fact, that the rejection of the traditional treatment of the *endowment* of capital as a single factor in general equilibrium theory did not entail a questioning of the traditional *conception* of capital as in some sense a single factor (whose desired proportion to labour was a decreasing function of the interest rate), nor of the conclusions based on such a conception. Hayek's rejection of all conception of capital as a single factor, a 'fund', should have raised doubts on all analyses based on that conception and on the associated notion of capital-labour substitution responding to changes in income distribution; the question, for example, whether a decrease in the rate of interest could be expected to increase the demand for loanable funds – a thesis based on an assumed desire by firms to employ more capital per unit of labour – should have been reconsidered from scratch, the moment it was admitted that capital goods could not be seen as embodiments of a single 'fund' whose quantity was independent of distribution. But at the time that conception of capital was so universal, so instilled into the mind from the first contact with economics, that it continued to be accepted in fact. For example, no one doubted that the long-period demand schedule for labour (the one derived from giving time to firms to change the capital goods they employ) was a decreasing function of the real wage, indeed, a more elastic function than the short-period schedule. Analogously, the negative elasticity of the capital-labour proportion (and thus of aggregate investment) vis-à-vis the real interest rate was not doubted. Borrowing a term from Garegnani (2000: 443), the shift to very-short-period equilibria was essentially a cosmetic operation; the working of the economy was still believed to be the one emerging from the older marginalist analyses relying on capital the single factor of variable 'form'.

A striking confirmation is provided by Hicks who, after advocating with *Value and Capital* the temporary equilibrium method as a way to avoid the conception of capital as a single factor (a 'fund'), later had second thoughts

and admitted grave problems in the temporary equilibrium method, but was not induced by this to doubt the entire supply-and-demand approach and on the contrary reaffirmed in 1963 his faith in the possibility of conceiving capital as a single quantity *in some physical sense* (Hicks 1932: 345; cf. Petri 1991), although never daring to go back to treating capital as a single factor K in his formal analyses. Professor Blaug brings further confirmation to this thesis when in (1999b: 261) he mentions the survival in Hayek's *Pure Theory of Capital* of an inverse relationship between rate of interest and capital intensity of production processes, and then in the same place (*ibid.*, fn. 6) he quotes Hayek as declaring in a 1945 interview that "Böhm-Bawerk was fundamentally right". And the generality of marginalist authors in those years shows, for example with the development of the theory of international trade, and later with the Tobin-Solow-Swan theory of growth, that the conclusions based on treating capital as a single factor were still believed to be valid.

However in the 'rigorous' presentations of the theory of value and distribution, i.e. in the theory of general equilibrium, there could be no going back to an endowment of capital as a single factor, an amount of value: after Hayek's (1936) forceful attacks against such a procedure, the inconsistency was too blatant. It was inevitable, if the approach was not to be entirely abandoned, to turn to the Walras-Lindahl-Hayek-Hicks treatment of the capital endowment as a given *vector*.

V. The rise to dominance of the Arrow-Debreu model

18. The observations of the previous Sections yield the essential premise for an explanation of the rise to dominance of the Arrow-Debreu model.

This model was a reformulation of the Wald model, in turn a generalization of the so-called Walras-Cassel model of an *acapitalistic* production-and-exchange general equilibrium. This helps one explain the initial unproblematic reception of the model: given the traditional 'three-stage' presentation of the marginalist theory of value (pure exchange; exchange and production; introduction of capital), the study of pure-exchange models and of acapitalistic general equilibrium models was considered legitimate and important as first steps toward the complete theory, and therefore an acceptance of the *intertemporal*, very-short-period equilibrium, reinterpretation of the Arrow-Debreu model was *not* necessary in order to appreciate the feat of the authors, and probably it was not initially widespread (on this, more research would be necessary).

But what we must explain is the subsequent acceptance of the

intertemporal reinterpretation of the model, with its necessary assumption of complete futures markets or perfect foresight, and with its very-short-period nature that obliged stability studies to assume the fairy-tale tâtonnement based on 'bons'[28].

The answer here is that the neoclassical approach could not remain without a theory of value capable of including capital goods. Long-period equilibria, with their need for a value endowment of capital, could no longer be defended; the void had to be filled somehow. The sole other possible treatment of the capital endowment compatible with a supply-and-demand approach was to treat each capital good as a separate factor with a given endowment[29]. This might have meant temporary equilibria, but their rigorous formalization needed a treatment of expectations, with complications that only many years later it was found possible to examine analytically (serious difficulties then emerged in the formalization of the equilibrium and in the demonstration of existence; I suggest that these difficulties, together with the indefiniteness problem, explain why temporary equilibria have never supplanted the Arrow-Debreu model as the foundation of neoclassical value theory). The notion of intertemporal equilibrium had already been proposed by Lindahl and Hayek; its formalization as simply a re-interpretation of the acapitalistic model had the great advantage of familiarity. The a-priori faith, discussed in §**17**, in the correctness of the traditional marginalist theses rendered the debatable aspects of the model (e.g. the finite horizon) less relevant: the model could be seen as a first approximation, whose implausible aspects would be removed by the further progress of analysis, and in the meanwhile it yielded the results that one believed to be correct anyway: full employment, each factor receiving its marginal product, the first welfare theorem, etcetera.

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28 The equilibrium of the exchange-and-production model with only nonproduced factors could on the contrary be interpreted as a long-period equilibrium, reached by time-consuming adjustments, because its data could be treated as unchanged by disequilibrium actions, cf. §**9**.

29 We have here the reason for what Blaug describes as "the curious trade-off that seems to prevail between existence proofs and stability proofs in GE theory" (Blaug 2002b: 25). In order to formulate a system of equilibrium equations admitting capital goods but not including among its data a value endowment of capital, it was necessary to adopt the Walrasian treatment of the capital endowment, which precludes the consideration of realistic disequilibrium adjustments. No such 'trade-off' would arise in a long-period GE model, e.g. Wicksell's model, if one accepted to take the capital endowment as given as an amount of value (existence of equilibrium would be demonstrable for such a model as easily as for neo-Walrasian GE models).

19. Another aspect that must have contributed to the acceptance of the intertemporal interpretation of the Arrow-Debreu model is the confused state of capital theory at the time, in particular the little familiarity with previous attempts at including capital goods into the supply-and-demand approach. It has been noticed above that for many economists in those years general equilibrium theory, i.e. the rigorous presentation of value and distribution theory, appears to have consisted of the acapitalistic general equilibrium model only; the presence of a third 'stage', a specific treatment of capitalistic production in earlier authors, e.g. in Walras or in Wicksell, was largely forgotten. To those economists, the intertemporal reinterpretation of the acapitalistic model must have appeared a novel and interesting extension, finally permitting an inclusion of capital goods into general equilibrium theory.

One wonders whether at the time this was not the case also with Arrow and Debreu. When they propose the reinterpretation of their model as an intertemporal model, they do not accompany the proposal with any comment on the previous attempts to include time and capital into general equilibrium theory. It is indeed striking that, as far as I know, no author of the Cowles Commission group (the group, including Arrow, Debreu, Koopmans, Gale, Hurwicz, and others, out of which came the Arrow-Debreu model) felt any need to discuss why the approach to capital of Walras or of Wicksell was inferior to the one they were proposing.

However one must not think that the rise to dominance of the Arrow-Debreu intertemporal model was quick. Here too more research is needed, but some observations can already be advanced. We have noticed above that international trade theory and growth theory show the persistence in the 1950s of the faith in the legitimacy of the treatment of capital as a single factor at least in applied analyses. I have also mentioned Kuenne (1963); later still, the survey of general equilibrium models by Bent Hansen (1970) presents a series of 'Austrian' general equilibrium models, and one of them (in ch. 17) is based on a given value endowment of capital. It would seem then that at least in the 1950s and early 1960s the Arrow-Debreu model was far from having won the field as *the* foundation of neoclassical value and distribution theory.

In this situation of uncertainty and lack of clarity, the decisive push most probably came from the Cambridge debates on capital theory; the impossibility of a measurement of aggregate capital in units independent of income distribution was again and again repeated in that debate, and accepted also by the neoclassical side; the results on reswitching permitted a logically unassailable attack on any notion of capital as a single factor; all this

compelled neoclassical economists to claim that their theory did not need the conception of capital as a single factor, and to turn exclusively to the very-short-period versions as the foundation of their approach, as admitted e.g. by Christopher Dougherty: "Since then [the mid-1960s] the general equilibrium model has been the undisputed core of neoclassical capital theory" (Dougherty, 1980: 3).

20. So, why did the Arrow-Debreu paper become a model of scientificity? why wasn't the unreality of the model, when interpreted so as to encompass production with capital goods, and therefore as an intertemporal equilibrium suffering from the implausible assumption of complete futures markets and from the incompatibility with time-consuming disequilibrium adjustments, soon met with destructive criticism?[30] The basic answer I have suggested is: because the root cause of the lack of realism, the neo-Walrasian treatment of the capital endowment, was the sole way to remain within a supply-and-demand approach to value and distribution once the treatment of the capital endowment as an amount of value was recognized as untenable. In order not to abandon the supply-and-demand approach, value theorists had to convince themselves that the basis of the approach could be shifted to very-short-period equilibria and sequences of such equilibria, in spite of the new difficulties thus emerging such as the impermanence problem and the price-change problem. The survival, more or less explicit, of the faith in the adjustment mechanisms based on the traditional conception of capital as a

30 That the mathematical feat deserved some praise cannot be doubted; that the aim was worthwile cannot be doubted either (if it had been shown that equilibrium generally does not exist for the acapitalistic economy, this would have been very important); what requires explanation is why this praise was not so much watered down by recognition of the unreality of the model when interpreted so as to make room for capital goods, as to bring a majority of readers to the conclusion that the mathematical effort was inconclusive because only applicable to the acapitalistic model. Thus the problem with GE theory is not correctly characterized as one of excessive rigour, as Blaug appears to suggest in several places (1999b: 272; 2002b: 27) and very clearly already in (1992: 167): "Alas, there is a trade-off in economics (and not only in economics) between rigor and relevance". The problem would rather seem to be one of *insufficient* rigour, because theoretical (as distinguished from mathematical) rigour is exercised in finding out to which world the equations can possibly apply, but not in asking what the roots are of the patent unreality of the resulting world, nor in asking - on the basis of this result - whether the equations do satisfy the question originally motivating them: the characterization of the situation toward which the interplay of supply and demand realistically conceived is believed to push the economy. It is a poor conception of rigour, that only requires that consequences be correctly drawn from a model, and not also that the model's structure be well justified in the light of the aims of the enquiry.

single factor made this shift easier, because it was believed to make little difference.

It took decades for the sterility due to the impossibility to study adjustment problems in more realistic ways to start raising deeper doubts. The Cambridge controversies no doubt had an important role, by pointing out in irrefutable terms the indefensibility of the traditional conception of capital-labour substitution: this made it harder for neoclassical value and distribution theory to run with the hare and hunt with the hounds by claiming that GE theory did away completely with any notion of capital as a single factor, while at the same time claiming GE theory as the foundation for theses originally derived and only derivable from that notion of capital[31]. Since then, general equilibrium theorists have more and more often had to admit that GE theory, owing to the unrealistic assumptions needed in order to assume that market economies reach an Arrow-Debreu equilibrium, does not give support to the neoclassical theories that dominate macroeconomics, international trade, etcetera.

VI. Back to Marshall?

21. The important implication of the argument advanced in this paper concerns how to surmount the sterility of modern general equilibrium theory and make room for the more realistic conception of competition that professor Blaug rightly considers essential to an understanding of concrete economies.

Once again we can rely on professor Blaug, who implicitly indicates the way when he accuses modern GE theory of preventing

> consideration of all dimensions of competitive rivalry other than price, such as availability, quality of product, quality of delivery, quantity and quality of information about the product, etc.; in short, all aspects of non-price competition because those take place sequentially in real time. This is precisely what competition meant to Smith, Ricardo and Marx and, even after Cournot, this is what it meant to Marshall. (Blaug 1999b: 266)

The obvious suggestion emerging from this quote is that one should return to the way of approaching the study of market economies of Smith, Ricardo, Marx, and Marshall. What these authors had in common was the

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31 Petri (2004: chs. 7, 8) shows that such is indeed the case for the negative elasticity of aggregate investment vis-à-vis the rate of interest, and for the negative slope of the demand for labour.

method we have called ‘of long-period positions’, based on the distinction between *market* prices and quantities, and *normal* (or long-period) prices and quantities: a method that permits the consideration of phenomena taking place “sequentially in real time”. Clearly, we should recuperate that method.

But, among these authors, Marshall presents a problem. The analysis of this paper shows that we cannot go back to notions of ‘centres of gravitation’ of market prices and quantities, while retaining the marginalist, or supply-and-demand, approach to value and distribution. It was precisely the decision not to abandon that approach in the face of its inability to determine sufficiently persistent ‘centres of gravitation’, that prompted the shift to neo-Walrasian general equilibrium theory. The root of the problem is the supply-and-demand approach to distribution itself, which comes out to be unable satisfactorily to treat capital. Both possible treatments of the capital endowment in this approach run against insurmountable difficulties (Petri, 1991): the long-period treatment needs an indeterminable endowment of a quantity of value; the very-short-period treatment runs into the three methodological problems illustrated above, and in addition, if the traditional conception of capital is truly abandoned (as rendered inevitable by the discovery of reswitching and reverse capital deepening), no basis is left for the thesis that investment adjusts to savings, nor for the decreasing labour demand schedule (Petri 2004: chs. 7 and 8).

Thus, the ‘back to Marshall’ appeal, implicit in much of the literature that deplores the excessive formalism and lack of realism of modern neoclassical value theory[32], can only be accepted in so far as it means ‘back to realistic analyses that admit time-consuming adjustments’; but on issues such as, what determines income distribution or the level of employment, Marshall was fundamentally a marginalist theorist and his analyses, in so far as they rely on the marginalist approach, cannot be resumed.

Fortunately there remain Smith, Ricardo and Marx. The weaknesses of the classical determination of relative prices (the shortcomings of the labour theory of value) have been overcome by the modern developments of the theory of long-period prices (prices of production). As argued in Petri (2004: ch. 9), the resumption of the classical approach and its union with a Keynesianism freed of neoclassical elements allows not only a full recuperation of the traditional and fruitful method of long-period positions, but also a better reconciliation of economic theory with a number of phenomena that the supply-and-demand approach has always found it difficult to explain, for example persistent unemployment (e.g. Europe since

32 E.g. Blaug (1999: 674).

1975), persistent underutilization of capacity (e.g. Japan since 1990), or the insensitivity of investment to the rate of interest. The greater role given in the classical approach to institutions, to political and sociological elements, and to historical specificities can also help correct the excessive importance given today to mathematical competence vis-à-vis knowledge of how real economies actually function, another point on which it is difficult to disagree with professor Blaug.

APPENDIX - WALRAS AND BORTKIEWICZ

This Appendix points out that the interpretation advanced in §**7** of Walras' views on capital before the 4th edition is supported by a so far neglected evidence. In September 1889 Edgeworth published in the English journal *Nature* a review of the second edition of Walras' *Eléments*, where, after an initial praise of Walras as co-discoverer of the role of marginal utility, he harshly criticized several aspects of Walras' work. Walras did not reply directly, but immediately set to persuade the young Ladislaus Bortkiewicz to write a reply. I rely on Marchionatti (2003) for the following useful information:

> In early December Walras received Bortkievicz's paper and was very satisfied of it: 'I found a man capable of reading me attentively and understanding me perfectly, and capable of defending my point of view as well as I can, if not better' ('*J'ai trouvé un homme capable de me lire attentivement, de me comprendre parfaitement et de défendre mon point de vue aussi bien, sinon mieux, que je pourrais le faire moi-même*') (letter of 8 December 1889). He sent it (with a few changes) to Gide, the editor of the *Revue d'économie politique*. He wrote: 'I am sending you an excellent paper that offers an exact idea of my work in the form of a rejoinder (incontestable, according to me) to Edgeworth's criticism' ('*Je vous envoie sous ce pli séparé un article excellent qui, sous forme d'une réponse (tout à fait irréfutable, selon moi) aux critiques d'Edgeworth, donne une idée parfaitement exacte de mon ouvrage*') (26 December). Bortkievicz's paper was published at the beginning of 1890. On 20 February 1890, Walras wrote proudly to Edgeworth: 'This is the answer to your critiques' ('*Voici la réponse à vos critiques*'). (Marchionatti 2003: 4)

Now, Bortkiewicz's 1890 article (actually signed Bortkévitch) contains the following passage on p. 84-85, whose relevance for the interpretation of Walras' views on capital before the 4th edition appears to have escaped attention so far:

> "M. Edgeworth ne distingue pas nettement le marché des produits du marché des services ou, ce qui revient au même, l'équilibre de l'échange de l'équilibre de la production. Ce n'est pas le moindre mérite de M. Walras d'avoir insisté sur cette distinction importante.
>
> Mais M. Edgeworth ne distingue pas mieux l'équilibre de la capitalisation de celui de la production qu'il ne distingue l'équilibre de la production de celui de l'échange. Il croit qu'il ne sert à rien de traiter spécialement le problème de l'utilité *maxima* des capitaux neufs, vu que, «le prix du capital étant déterminé par concurrence, il résulte de la théorie générale de l'offre et de la demande que l'utilité *maxima* de toutes les parties intéressées se réalise dans le même sens que dans les autres marchés» (p. 435, col. 1 et 2). On peut objecter au critique anglais : 1^0 que le concept de l'utilité des capitaux n'est pas le même que celui de l'utilité

> des produits consommables, l'utilité des capitaux étant en quelque sorte dérivée de celle des revenus auxquels les capitaux donnent naissance; 2^0 **que la théorie de la capitalisation s'occupe du problème relatif aux quantités fabriquées des capitaux neufs, tandis que ces mêmes quantités sont considérées comme données dans la théorie de la production**. Voilà donc un troisième problème tout nouveau qui ne saurait être traité comme un cas particulier d'aucun des problèmes résolus dans les chapitres précédents du livre de M. Walras. Il devient évident que M. Edgeworth n'a pas du tout saisi la corrélation existante entre les trois parties du système des *Éléments d'économie politique pure* -- Dans la théorie de l'échange, il s'agit de déterminer les prix des produits, étant données les quantités fabriquées de ces produits. -- **Dans la théorie de la production, ces quantités de produits figurent à titre d'inconnues qui se déduisent des quantités données de capitaux fonciers, personnels et mobiliers.** Quant aux premiers (les terres), leurs quantités sont toujours des donnees du problème et non des inconnues. Les facultés personnelles des hommes ne dépendent pas non plus du mouvement de la production industrielle, mais de celui de la population (*Éléments***,** p. 266). -- **Restent les capitaux mobiliers (artificiels), ou capitaux proprements dits, dont les quantités peuvent être considérées comme des inconnues; il s'agit de démontrer comment elles [p.85] se déterminent, et c'est là l'objet propre de la théorie de la capitalisation.**"

The sentences that I have highlighted in bold strongly suggest that, according to Bortkiewicz, Walras' theory of capitalization aims at determining the quantities *in existence*, i.e. the *endowments*, of capital goods: "Dans la théorie de la production", in the theory of production, Bortkiewicz writes, the quantities of capital goods (capitaux mobiliers) are "given quantities" (quantités données) on a par with the quantities of lands and of labours, while "the proper object of the theory of capitalization" is to show how these quantities of capital goods, which now "can be considered as unknowns", are determined. The same identification (cf. "ces mêmes quantités") of the quantities produced of new capital goods with the endowments of capital goods occurs in the sentence following "2°" halfway through the quotation. Thus, Bortkiewicz writes as if the determination of the quantities produced of new capital goods amounted to also determining their endowments. Now, we have seen that Walras wrote (and there is no reason to think that he was lying) that Bortkiewicz' article offered "a perfectly exact idea of my work".

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