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None so deaf as those that will not hear: on Garegnani's contributions to the capital-theoretic critique and the resistances to accepting them

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Abstract: In this paper I will discuss the usefulness of Garegnani's contributions to the capital-theoretic criticism of marginalist/neoclassical theory trying also to indicate possible avenues for fruitful further work. Three main groups of critical contributions by Garegnani on capital theory are distinguished: 1) on the structure of the marginal approach to value and distribution and its differences from the classical or surplus approach; 2) criticisms of the traditional versions of the marginal approach; 3) criticisms of the neo-Walrasian versions. The content of the three groups of contributions is briefly remembered. It is suggested that Garegnani's arguments are increasingly accepted among historians of economic thought, and remain unaccepted among mainstream economists mainly because of a conscious decision not to study them: a disconcerting non-scientific attitude due to dogmatism and to political-ideological pressures. On Garegnani's 2000-2003 criticism of intertemporal equilibrium theory it is better to wait for a thorough examination of his unpublished manuscripts.

Keywords: Capital theory, traditional marginalist theory, neo-walrasian versions, aggregate production functions, perfect foresight.

JEL classification: B13, B21, D24, D33, D51, E11, E13

INTRODUCTION. THREE GROUPS OF CRITICAL CONTRIBUTIONS BY GAREGNANI

The Bulletin's initiative is timely and useful. The questions are important, and would require volumes for exhaustive answers. I will restrict myself to discussing only the usefulness of Garegnani's contributions to the capital-theoretic criticism of marginalist/neoclassical theory, and even so I am afraid I will be too long: the issues are many and complex. I will try as I proceed to indicate possible avenues for fruitful further work.

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I distinguish three main groups of critical contributions by Garegnani on capital theory: 1) on the structure of the marginal approach to value and distribution and its differences from the classical or surplus approach; 2) criticisms of the traditional versions of the marginal approach; 3) criticisms of the neo-Walrasian versions.

The first group of contributions is in my view the most important one, because it shows:

(i) that there were excellent reasons for the traditional marginalist treatment of capital as a single factor of variable 'form';

(ii) that, in the light of those reasons, the abandonment of that treatment of capital with the shift to the neo-Walrasian versions must have created very serious problems to the supply-and-demand approach;

(iii) that it is possible to analyse the capitalist economy without demand curves, for goods or for factors, so that the rejection of the supply-and-demand approach does not leave us in a desert.

THE DIFFERENCES BETWEEN VERSIONS OF THE MARGINAL APPROACH ARE STILL NOT SUFFICIENTLY CORRECTLY PERCEIVED

It is useful to distinguish two aspects of Garegnani's views in the first group of contributions:

1A: which is the central idea of the marginal or supply-and-demand approach i.e. where does it centrally differ from the classical approach?;

1B: the differences between versions of the marginal approach.

On 1B, the central point is the distinction between the 'traditional' marginalist authors (J. B. Clark, Marshall, Wicksteed, Böhm-Bawerk, Wicksell, Fetter, Knight etc.) who conceive of capital as a single factor and its composition as endogenously determined by the equilibrium (which is a long-period equilibrium aiming at determining prices which are the marginalist equivalent of Smith's and Ricardo's natural prices), and the neo-Walrasian authors who follow Walras in considering the equilibrium endowments of the several capital goods to be all given, part of the data of equilibrium.[1] The distinction is of course associated with the claim that the 'traditional' marginalist authors were internal to the same 'method of long-period positions or normal positions' that one finds in Adam Smith and the other classical economists, the method that distinguishes market price from normal price, and considers the normal price (associated with a uniform rate of profits or of return on the supply prices of capital goods) to be what value theory can and should determine, because it is impossible to predict all details of the determination of market prices which depends on a myriad of accidental and transitory elements, but owing to the *gravitation* of the market price toward the normal price due to the tendency of the rates of return on investment toward uniformity, the normal price indicates the average and the tendencies of the market price. The conception of equilibrium as indicating the average produced by *time-consuming gravitation processes* obliges to conceive the amounts of the several capital goods as endogenously determined (adjusting to the demand for them at normal prices), and requires then a given 'total quantity of capital' to constrain these amounts, which otherwise would be left undetermined by the equilibrium conditions, the condition of uniform rate of return on supply price being only capable of determining their relative proportions but not their abundance relative to labour. This analytical point remained little understood during the Cambridge debates and afterwards too (I felt it was necessary to make the point explicit in my 2004 book, ch. 3, and then again in my talk in Australia, Petri (2020)); this helps to explain the frequent misinterpretation of the presence of a 'quantity of capital' in traditional marginalist authors as indicating they were using an aggregate production function, and the difficulty that even a historian of economic thought like Mark Blaug had in grasping that J. B. Clark's and Wicksell's equilibria were general equilibria as much as Walras's or Debreu's, differing not in the degree of disaggregation but in the determination of the capital endowment as a quantity of a single factor of variable composition, rather than as a given vector, which is why their analyses could admit time-consuming adjustments and, with them, a realistic conception of competition as rivalry. General equilibrium for Blaug was only Walras and neo-Walrasian models, and in Petri (2014, fn. 15) I noticed that Blaug was also unclear on the difference between neo-Walrasian equilibria and Walras, and considered the proof of existence of general equilibrium by Arrow-Debreu as "the fulfillment of Walras's dream", evidently not realizing that that proof refers to the model without 'capitalization'.

So clearly Blaug would have had difficulties also with Garegnani's inclusion, among the versions that aimed at determining long-period prices, of Walras, described as simply not realizing for many years that his taking as given the endowment of each capital good was incompatible with that aim, and finally trying with the fourth edition to surmount the contradiction with hurried modifications of little consistency. The issue is important because Garegnani's interpretation of Walras, totally correct in my view, means that the idea that value theory must first of all try to determine long-period prices (that is, a uniform rate of return on the supply prices of capital goods) was *universally* accepted by the founders of the marginal approach, in full accord with the earlier classical economists, and was finally abandoned only because of the problems it raised in the marginal approach owing to its requiring an indefensible given quantity of value capital, not because of intrinsic weaknesses of the notion of long-period position. Also, and very importantly, this interpretation clarifies the reason for Walras' shift in the 4th edition of the *Eléments* to the conception of the tâtonnement as virtual, based on 'tickets' or pledges, while in the earlier editions it was an actual time-consuming process of productions and adjustments: this fairy-tale picture of how equilibrium might be reached is revealed by Garegnani's interpretation to be an act of desperation by an unclear mind, and this revelation should undermine the disconcerting subsequent facility with which this picture has been uncritically accepted, with a disastrous loss of sight of the importance of admitting time-consuming adjustments.

All this confirms the central importance of a correct interpretation of the evolution of marginalist theory. It seems to me that Garegnani's views on this issue are more and more accepted among critical economists, whose number is growing, and possibly also among open-minded historians of economic thought, if I can generalize on the basis of the positive reactions to my 2019 talk to the HETSA (History of Economy Thought Society of Australia) in Sydney (Petri 2020).[2] Also, the 2009 article by Bloise and Reichlin shows that at least some general equilibrium specialists now read the critics' writings and admit the existence of a marginalist tradition based on suppy and demand for capital not because of the assumption of an aggregate production function, rather because of a belief in the value of *heterogeneous* capital goods as measuring the quantity of a factor for which one can derive demand and supply functions. One can hope that the time is perhaps arriving when the dogmatism and wilful ignorance of a Christopher Bliss will appear an isolated regrettable exception. Still, ignorance of Garegnani's theses is still widespread even where one would not expect it, for example I was shocked by Cohen and Harcourt (2005), where again the mistake is repeated that there are only two groups of versions of the marginalist/neoclassical approach, the disaggregated ones (by which the authors mean the neo-Walrasian ones) and the aggregate-production-function ones. A quick look at some histories of economic thought reveals that Garegnani's views on which I am insisting are not only absent in those written by economists of mainstream formation (Backhouse, Landreth and Colander, Sandmo…) but are not clearly presented even in some of those written by notoriously 'Sraffian' economists. In the Screpanti-Zamagni history of economic thought (3rd Italian ed., 2017), which is analytically more advanced, detailed, and correct than most, the assessment of modern general equilibrium theory is merciless, but the difference in the specification of the capital endowment as between the long-period theorists and the neo-Walrasian models (and Walras) is not stressed: on one side it is not made clear that the long-period nature of the traditional equilibria obliges J. B. Clark, Böhm-Bawerk and Wicksell to specify the equilibrium's given endowment of capital as a quantity of exchange value, on the other side it is not pointed out that neo-Walrasian equilibria are incompatible with the idea that adjustments take time and imply therefore the abandonment of the tendency toward equilibrium as a time-consuming gravitation (nor is it pointed out that the 'tickets' in Walras have the origin indicated above); so the reader does not learn that the difficulties with proving the stability of neo-Walrasian general equilibria are even worse than admitted by the specialists because the auctioneer does not exist, and since no time-consuming disequilibrium adjustments are compatible with the data of those equilibria, on the effects of disequilibrium time-consuming adjustments in real economies neo-Walrasian equilibrium theory is silent; so if one does not assume continuous equilibrium (which is ridiculous once one admits there is no auctioneer) one derives simply nothing from neo-Walrasian theory about how the economy behaves. Things are no better with Roncaglia (2019) where there is the same absence of clarification of the above points, especially Walras is treated way too quickly.[3] The problem, which is a consequence of the lack of persistence of the data of neo-Walrasian equilibria and therefore can be subsumed under the tag 'impermanence problem', certainly cannot be perceived from reading more 'mainstream' treatises such as Mas-Colell *et al.*, Kreps, Hildenbrand and Kirman, and it is also not noticed by Ingrao and Israel nor in the specialist articles and essays on general equilibrium by mathematical economists; it seems then important to constrain neoclassical theorists to face the problem. Of course in certain cases dogmatism is too strong, for example Bliss is not unaware of the problem, he admits that "even if equilibrium were to be stable there might not be enough time within the space of a 'week' for prices to adjust to an equilibrium", but avoids any further analysis of the problem by incredibly concluding that "In the face of all the foregoing problems it may seem more sensible to simply assume that equilibrium will prevail" (Bliss 1975: p. 28), that is: since to justify the validity of my theory is very difficult, I will simply *assume* that the theory is valid (and hope that this assumption will be shown correct some time in the future, and meanwhile avoid all discussion of the difficulties). This assumption is actually a certitude in Bliss, as made evident by his subsequent arrogant interventions on capital theory and on Garegnani (Bliss 2005, 2010). But not everybody is so dogmatic; in many cases, we can presume that the problem is simply not perceived because never raised in the environment in which one works. And without consciousness of the impermanence problem it seems impossible to grasp the state of modern mainstream macroeconomics and growth theory, in particular the sleight-of-hand of considering neo-Walrasian general equilibria as authorizing the use of one-good Ramsey growth models when in fact they do not, because silent on disequilibrium and hence unable to argue that the path of the actual economy does not considerably diverge from the intertemporal equilibrium path.

It would seem therefore that an effort to stimulate more debate on Garegnani's views on the evolution of the marginal approach, and to involve also economists little familiar with them, appears important and potentially very useful. For example it seems clear that Garegnani's views on Walras were not familiar to the editors of the 1988 variorum edition of Walras' *Eléments*, who do not mention Walras' admission in the 4th edition of the inability of his equations to determine a uniform rate of return on supply price, in spite of its having been clearly indicated as very important in Garegnani's *Il Capitale Nelle Teorie della Distribuzione*, translated into French in 1980. In particular on Walras I suspect that it will not be easy to induce the profession to accept Garegnani's interpretation, Walras is now a symbol, considered (with gratitude!) the father to modern general equilibrium theory because mistakenly identified with the acapitalistic general equilibrium model, so all criticisms of Walras are immediately perceived as attacks on mainstream economic theory - which indeed they are! - and easily the mental shutters are lowered even before listening to the arguments. Against dogmatic rejections of criticisms not much can be done. But the increasing recognition of a tradition of long-period marginalist equilibria should help the open-minded to realize the correctness of Garegnani's thesis that Walras was contradictory, aiming, at least originally, at determining a long-period equilibrium; this should be helped by the availability in Petri (2004: p. 140; 2020) of a clearer list of the many aspects of Walras that confirm it; my 2016 paper on Bortkiewicz' review of Walras has brought, I think, further support; and Garegnani's PhD thesis will soon be out, reinforced on Walras by Garegnani (2008). On this issue and more generally on the theses of this group 1B of contributions, I think that Garegnani is extremely persuasive if one seriously studies him, the danger is that he may remain less known than he deserves, so some initiative promoting discussion of his views capable of involving non-'heterodox' economists too would be of great help.

ON THE DIFFERENCE BETWEEN CLASSICAL AND MARGINAL APPROACHES: MORE CONCRETENESS IS NEEDED ON WAGE DETERMINATION

Turning to aspect 1A, I am not sure that it is possible to hold the same cautious optimism. The resistance is clearly strong to admitting that the approach of Smith, Ricardo and Marx was radically different (lacked demand curves for factors) and - the thing that greatly motivates the resistance - offers a potential alternative to the marginal approach. Samuelson's insistence that distribution cannot but be determined by supply and demand for factors is well known: "Until factors cease to have their rewards determined by bidding in quasi-competitive markets, I shall adhere to (generalized) neoclassical approximations in which relative factor supplies are important in explaining their market remunerations" (Samuelson 1966: p. 444). Neoclassical growth theory testifies to the same certitude. Again it is unclear, to me at least and no doubt because of my ignorance, whether Sraffa's and the Sraffians' reconstruction of the classical approach has or not considerably penetrated among the many economists who teach in less prestigious universities and colleges; sometimes I suspect that the 'mainstream' (especially the follies of real business cycles and infinite-horizon macro models) is much less generally accepted than the name would suggest, and it is the barriers to the emergence of non-mainstream opinions in the more widely read journals that hide this fact. (It might be worth trying the following: one year all non-mainstream economists join the American Economic Association and vote for heterodox candidates to the top positions and take over the AEA and change the editorial boards of its journals. Someone should study whether this might succeed.)

But the resistance is perhaps also due to some weakness of the proposal of a classical-Keynesian alternative. Perhaps the difficulty of Samuelson and others, in so far as the difficulty is honest and not motivated by dogmatism or non-scientific interests, is partly due to a difficulty with imagining what can determine wages if it is not supply and demand. After all, there is some apparent plausibility in the implicit monetarist argument that if the unemployed workers are *involuntarily* unemployed then if rational they *will* offer to work for a lower wage, and therefore if wages do not decrease in the presence of unemployment it must mean that the unemployed are *not* involuntarily unemployed.

It may be because of my ignorance, but I am not aware of refutations of this argument so satisfactory as to be capable of surmounting objections. Efficiency wages or the fairness/gift-exchange approach of Akerlof clearly indicate that the need is felt for a theory of wages other than supply and demand; but these appear to me to be weak theories, which do not explain the level of wages. For example efficiency wage theory does not explain why the *same* level of unemployment can be associated, after a few years, with a *higher* level of real wages: certainly the explanation cannot be that the workers' tastes have mysteriously so changed as to shift the efficiency wage as observed. But the terms generally used to point out the difference of the classical from the neoclassical approach to wages: 'subsistence, custom, social and political influences, institutions, bargaining power', are way too vague.

Take for example this passage by Garegnani in his (excellent) 2007 reply to Samuelson: «the circumstances determining what we have described as the 'intermediate data' … were seen to include broadly institutional and historical factors, which, because of their complexity and variability according to circumstances, prevented deducing the corresponding variables from a few basic principles as was done for prices and profits in the 'core'.» (p. 186). Garegnani becomes more concrete later in the article, see pp. 193, 217 fn 48, 219-222, but still insufficiently so: the argument, that since wage decreases would not ensure increases of employment then society *must* have developed conventions and rules and laws capable of preventing wages from falling indefinitely otherwise it would be impossible for an orderly economic life to continue, is *prima facie* convincing, but needs to be made concrete by persuasive illustrations of these institutions and of why they do not contradict a presumption of rationality of agents – an illustration capable of refuting arguments such as Olson's.

My textbook *Microeconomics for the Critical Mind* remains just as vague in the chapter on labour (it is particularly poor on unions). I will try to improve this chapter in the second edition, but "di buone intenzioni è lastricata la strada dell'inferno" (the road to hell is paved with good intentions). A starting point may be what has been elaborated by Elinor Ostrom and more generally by collective action theory - namely, that "a theory of boundedly rational, norm-based human behaviour is a better foundation for explaining collective action than a model of maximizing material payoffs to self" - but this theory does not seem to have been much applied yet to labour markets and class struggle more generally. A very interesting little publication unfortunately not publicized by the author and not easy to trace, Piccioni (2022), that stresses the inability of neoclassical theory to deny the plausibility and rationality of 'combinations', will also deserve careful study and development. The aim should be to render obvious even to a modern mind familiar with game theory and with Olson (and therefore asking for proof that collective cooperative behaviours are not irrational) that we have no need for supply and demand curves for labour in order to understand what determines wages and their changes. Not a one person's task.

Before passing to the second and third groups of comtributions by Garegnani, two final observations on the reconstruction of the evolution of the marginal appraoch. First, there is a point on which there isn't a complete accord even among Sraffian economists broadly defined. Is the zero-net-savings assumption (the static stationary-state assumption) of Clark or Wicksell only a simplifying assumption, or is it indispensable to the determination of a consistent neoclassical long-period position in which relative prices are treated as constant? I agree with Garegnani on the first answer (there is material on this in Garegnani's unpublished manuscripts too), but as far as I understand not everybody agrees, and indeed the question is not simple, so I look forward to some reconsideration of the issue.

TWO POSSIBLE AREAS OF FURTHER RESEARCH IN THE EVOLUTION OF THE MARGINAL APPROACH

Second, as the Bulletin's questions indicate strong interest in possible areas of further research, let me hazard an indication of two possible such areas, hoping that I am not simply revealing a dismal ignorance of the literature. One is, where to locate Irving Fisher in the distinctions between long-period vs. Walrasian treatments of the capital endowment, and between acceptance vs. neglect of the role of equilibrium as centre of gravitation of time-consuming adjustments (an assessment of Hirshleifer and of his approach to investment would be a natural extension of this research). Another one is, how can we explain the rapid acceptance of Hicks' proposal in *Value and Capital* to abandon traditional equilibria in favour of neo-Walrasian models. This acceptance is strikingly rapid, the notion of normal long-period equilibrium has totally disappeared in Jacob L. Mosak's 1941 PhD on temporary equilibrium and international trade [4] (which might suggest an acceptance of the temporary equilibrium approach by Mosak - and by his supervisor? - even antedating Hicks' book!), in Lange (1942), in Debreu's and Malinvaud's early articles, in the Patinkin controversy (which involved dozens of economists) at least up to Archibald and Lipsey. Perhaps the reason indicated by Garegnani for this disappearance, namely Hicks's mistaken identification of long-period equilibria with (secularly) stationary equilibria, is an insufficient explanation. The effect of Hicks's error can hardly have been so quick. (Also, there was Robbins' 1930 article to indicate the identification was mistaken.) The terrain, one can suspect, had been prepared by lack of clarity in the 1920s and 1930s on capital theory and on normal prices: Pareto's *Manuel* presents only the general equilibrium of exchange and production without capitalization, Cassel too presents equations only for that case, the economists in the Vienna Circle too, who with Wald were to be the starting point of Arrow-Debreu's proof of the existence of equilibrium, make no attempt to go beyond the acapitalistic general equilibrium, where there is no room for the notion of normal prices associated with a uniform rate of return on supply price. I know next to nothing on the Walrasians in Italy and France but certainly the tâtonnement with 'tickets' cannot have helped them to be clear on the issue of gravitation and normal prices. So perhaps by the time Hicks writes *Value and Capital* many economists are unclear on the issue of gravitation and even less clear on capital, and as a result are well disposed to jump to temporary equilibria, or to the intertemporal reinterpretation of the acapitalistic general equilibrium model, as at last offering a way forward on how to treat production with capital goods.

This might help to explain the origins of the Cowles Commission group. Perhaps economists like Malinvaud and Debreu never became familiar with the notion of normal or long-period position and with the treatment of capital as a single factor of variable 'form'; perhaps they learned value theory from Pareto only in the form of the acapitalistic general equilibrium, and when they learned from Maurice Allais the notion of intertemporal equilibrium as a way to introduce production of capital goods into the model, they did not have the tools to see the problems of such an approach, and did not understand the reasons for Allais' backtracking. So their neo-Walrasianism, and possibly that of the entire Cowles Commission, may have been born out of sheer lack of familiarity with the long-period marginalist tradition, rather than out of a rejection of it. There seems to be room here for a PhD thesis.

However my suggestion, that Hicks' identification of long-period equilibrium with secularly stationary equilibrium is only part of the story, does not imply that Hicks' error did not have important consequences. Certainly it helped the subsequent identification of all analyses where relative prices are treated as not changing with secular stationary equilibria or steady growth paths. There has even been an attempt (by Tom Kompas, a pupil of Hollander, in 1992) to argue that the static equilibrium of Clark or Wicksell was indeed a secularly stationary equilibrium. With Hicks' help modern neoclassicals have become blind to the fact that '*the speed with which the composition of capital can change is of a higher order of magnitude than the speed of the changes induced by accumulation or population growth, so the latter changes can be neglected for the determination of the tendential result of the faster adjustments that determine the composition of capital and normal relative prices*.' (Petri 2020: p. 10). I mentioned this argument to professor Foley in 2002, and revealingly he found the argument new to him and deserving reflection. The argument could not reach Bliss, Stiglitz, Hahn who after 1975 stopped reading the critics' contributions and remained blind to the role of capital the single factor in traditional marginalist authors. But even Mark Blaug was unable to grasp the critics' arguments (why? here is another possible research topic in the history of thought). But perhaps things are starting to change, if one is to judge from Bloise and Reichlin.

RESWITCHING AND AGGREGATE PRODUCTION FUNCTIONS

The second group of Garegnani's contributions has used Sraffa's results, in particular reswitching, to develop (i) a criticism of the negative interest-elasticity of investment, which undermines the 'neoclassical synthesis' with its 'Keynes effect'; and (ii) a criticism of aggregate production functions (1970).

On criticism (i), based on reswitching and reverse capital deepening, a frequent argument is that these are too unlikely to have relevance, and now the main representative of this argument is professor Schefold. My criticism of Schefold, in a Centro Sraffa WP in July 2021 and now in revised version in *Contributions to Political Economy* (Petri 2022b), has recently received a 48-pages answer by Schefold; he has also published two further articles on the topic, and I have barely started to study these three papers; so for the moment I have nothing to add to what I have already written.

Let me only remember one argument which I consider important in my criticism of Schefold and seems to be seldom utilised. If (as proved by Sraffa) no logically satisfactory definition of capital as a factor of production is possible, then this factor *does not exist*; then it is impossible to talk of a demand curve for, or of an endowment of, this factor, and then it is impossible to determine a long-period *labour* demand curve since it is impossible to specify the amount of capital kept fixed when one tries to determine the demand for labour.[5] But without a labour demand curve there is no equilibrium real wage, the explanation of wages *must* have recourse to something other than a supply-demand equilibrium. This argument holds independently of the empirical likelihood of reswitching.

On (ii), after Garegnani's proof of how restrictive the conditions are for viewing distribution as corresponding to the marginal products of an aggregate production function, the problem of course is, how come aggregate productions continue to be used. My answer will become clear as I proceed. But let me remember another argument, usually not advanced in debates on capital theory, and yet relevant to the issue: empirical evidence confirms Garegnani on the great flexibility of capacity utilisation in response to demand, not only downwards but also upwards; the indication by firms' managers that on average firms generally use only about 80% of 'full production' implies that nearly all firms would have no problem - actually, would be very glad - with producing 20% more if only they could sell it, which means, to be on the safe side, that it is generally perfectly possible via stimuli to aggregate demand to increase employment in the short period even by 10% *with no need to reduce real wages*. At least in advanced nations, capital is not scarce, unemployment could be reduced and the refusal to act in this direction is a conscious political choice; the experience of so-called neocorporatism shows that inflation is not a danger. (Balance-of-payments constraints can become relevant of course, so how to surmount them in a classical-keynesian framework is clearly an important topic for research; there is an old Keynesian literature on the topic that deserves recuperating.) The relevance of the above for the debate on aggregate production functions is, that even if approximate production functions were determinable as Schefold contends, still this would tell us nothing on what determines employment and income distribution - but here I must refer the reader to my paper on Schefold, Petri (2022b, Section *II*.4) since I could only verbatim repeat what I have written there.

But anyway the use of aggregate production functions in macroeconomics and growth theory is not defended on the basis of Schefold-type arguments, actually it is not defended at all, except by appealing to the fact that one-good neoclassical models do correctly grasp the qualitative tendencies that complex economies cannot but exhibit because neoclassical theory is no doubt valid. Behind this view there is a continuing faith in traditional capital-labour substitution i.e. in capital the single factor (on which more later), supported by the feeling that everybody agrees on this (apart from the dissenting economists of course, but these must be treated as non-existing, as in Charles Jones' textbook on growth theory). And why this continuing faith?

Garegnani's answer is in the second paragraph of his last paper (2012). He argues that the lack of understanding of the roles of capital the single factor in traditional marginalist authors authorizes the unwarranted belief that

> the [neo-Walrasian] reformulations of neoclassical theory ... which have become dominant after the first stage of the capital controversy, are immune of the inconsistencies affecting previous theory on the conception of capital. This has in turn left space for a second, no less unwarranted, consequence: a feeling that since those reformulations, and in particular general intertemporal equilibrium, would confirm at the level of pure theory the essential validity of the neoclassical demand-and-supply apparatus, they would also provide some validation for the admittedly imperfect previous concepts—foremost that of a 'quantity of capital'—as workable approximations in applied work.

After initially accepting it, I have come to have doubts about this answer. In the Cambridge controversy, intertemporal equilibrium theory was no doubt proudly presented by Stiglitz (1974) and Bliss (1975) as not only "immune of the inconsistencies affecting previous theory" but also confirming the essential validity of the supply-and-demand apparatus. Perhaps such a faith in neo-Walrasian equilibria existed in the 1960s, when temporary equilibria had not yet been abandoned, the novelty of intertemporal equilibria may have looked fascinating, and perhaps consciousness of the absurdity of its assumptions had not had time to become widespread by 1975. But Hahn (1982) is already much less sure, admitting problems with the stories supposedly indicating convergence to equilibrium; he admits that because of reswitching "various neoclassical adjustment theories ... are certainly at risk ... marginal productivity theory ... concerns an economy in full neoclassical equilibrium which, I have repeatedly argued, has nothing to fear from anything in Sraffa's work. But on the manner in which such an equilibrium is supposed to come about, neoclassical theory is highly unsatisfactory." (p. 373) A year earlier he had written:

> 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: p. 1036)

And in (Hahn 1982b, p. 746) he admits that because of the assumption of no actual trading or production in disequilibrium "it is obvious that it [the tâtonnement] is incapable of providing a satisfactory answer to the stability question." Since then, numerous general equilibrium theorists have warned about the little applicability of their theory to the real world; the assumption of perfect foresight, initially not paid much attention to, has been recognized as essential to the determinability of intertemporal equilibria and is universally admitted to be totally unrealistic; admissions of the unreality of the tâtonnement are everywhere. So I contend that now only a fool can believe that general intertemporal equilibrium theory *confirms* "the essential validity of the neoclassical demand and supply apparatus": belief in that validity, I contend, is prior to any faith in the validity of general equilibrium theory, the latter theory is now only a *smokescreen* to hide a continuing belief in the traditional marginalist mechanisms. I argue this point in a little more detail later.

So we must turn to another sentence by Garegnani later in the same article, intended for why the doubts about capital entertained by Lindahl, Hayek, Hicks did not induce a shift in the 1930s to a completely different theory: "the principle of factor substitution and the ensuing demand-and-supply explanation of distribution had apparently been rooted too deeply in mainstream economic theory for them to be extirpated" (p. 1424). This extirpation continues to be very difficult; why? My answer is extra-scientific reasons; I will come back on this.

NEO-WALRASIAN EQUILIBRIA

The third group of contributions by Garegnani has criticized neo-Walrasian (intertemporal and temporary) general equilibrium, on the basis (i) of methodological arguments, and (ii) of the claim that capital-theoretic problems connected with reverse capital deepening arise in neo-Walrasian equilibria too. The arguments under (i) are well known to the 'Sraffians': impermanence problem, price-change problem, substitutability problem imply that general equilibrium is totally silent on the behaviour of economies without auctioneer and without perfect foresight. Certainly most mainstream theorists are not familiar with these criticisms,[6] but it is impossible that none of them has come across them or some variant of one or more of them formulated by other economists. There has even been the admission by an important mainstream economist, Franklin M. Fisher, that because there is no actioneer and there are disequilibrium tradings and productions, "the set of equilibria is path dependent … [This path dependence] makes the calculation of equilibria corresponding to the initial state of the system essentially irrelevant" (Fisher 1983 p. 14). Essentially irrelevant! And it was seen above that the unreality of assuming an essentially instantaneous adjustment to equilibrium was admitted by Bliss. So it is simply not possible that there aren't at least a few neoclassical theorists conscious of the problems stressed by Garegnani (a bit less so, perhaps, for the substitutability problem). And yet these criticisms have so far received no reply, and the reason, clearly, is that no reply is possible; so we are in a scandalous situation, of a general equilibrium theory claimed to be the microfoundation of all macroeconomics and yet silent on how actual economies function.[7]

So let me briefly repeat now the argument I have advanced several times, that contends that intertemporal general equilibrium theory is now only a conscious *smokescreen* used to hide behind impressive mathematics a continuing faith in the traditional, time-consuming marginalist factor substitution mechanisms based on capital the single factor (a *cosmetic* change only, Garegnani wrote in 2000). In order for intertemporal equilibrium paths to claim they indicate the behaviour of actual economies in spite of the fact that actual economies are *not* continuously in equilibrium (as economists know well), one would need a proof that the actual path remains close to the equilibrium path, a proof intertemporal theory cannot provide because it is silent on what happens in disequilibrium. This is remedied by a *previous* belief in the approximate correctness of Solow-type growth paths, derived from a continuing belief in *traditional* marginalist forces. Then the intertemporal equilibrium path can be attributed explicative/predictive relevance because it is qualitatively similar. Neoclassical economists may be ignorant but are not stupid, they know that this is how they in fact reason.

One cannot expect to find *explicit* confirmation for this interpretation, since economists, who have rejected the Cambridge criticism by arguing that 'rigorous' neoclassical theory has no need for capital the single factor, are not going to admit they still rely on capital the single factor; but their persuasion that one has the right to reason as if that traditional conception of capital were valid emerges whenever the analysis tries to be applicable to the real world.[8] Thus Lucas admits that rational expectations need time to be learned; this implies that most of the time expectations are mistaken and the economy is not in equilibrium, so the rational-expectations equilibrium path can be a guide to the actual path only if the latter gravitates around and toward the RE path, and this means not only stability but also that the RE path must not be altered by disequilibrium actions, must be persistent:

which requires the traditional conception of capital, which is indeed the way capital is treated in all these models. Particularly revealing is the recent article by Guzman and Stiglitz (2020) which essentially considers Arrow-Debreu as only an ideal benchmark far from how real economies function, criticizes recent equilibrium models as lacking all proof of stability, accepts the need for fiscal policy in crises, *but* accepts that 'in normal times', as the authors put it, wage reductions increase labour employment, and that except 'when there is too much uncertainty' the elasticity of aggregate demand to the interest rate is negative and significant – the old theory.

So the criticism of *traditional* marginalist analyses relying on capital the single factor is as relevant as ever, because it is on traditional capital-labour substitution that neoclassical macro in fact continues to rely.[9]

I consider this conclusion highly clarificatory, and to support it I have added in Petri (2022a, p. 13) a further argument that confirms a radical inability of intertemporal equilibrium theory to tell us anything on the behaviour of actual economies: the reason is that *intertemporal equilibria are indeterminable, hence non-existent, a myth*, owing to the impossibility of an ex-ante coordination of future choices once one admits that there will be unpredictable novelties and therefore perfect foresight is impossible. For space reasons I must refer readers to that article for a detailed presentation of the argument.

It is to be hoped that there will be neoclassical reactions to this radical criticism. If the present situation of no neoclassical reaction to criticisms continues, some initiative will have to be taken: perhaps a public challenge, signed by numerous economists, to answer Garegnani's methodological criticisms, a challenge full of disdain toward the neoclassicals, accusing them of ignorance, and also of conformity and cowardice unless they try to answer Garegnani's criticisms. Some collective action of this type seems necessary, since the resistance can only be strong to accepting to discuss whether one's lifelong intellectual efforts have been wasted on a wrong theory; and younger economists are wary of going against the strong preferences of the leaders of their academic subgroup. Also, the passage from the neoclassical rosy picture of capitalism to the conflictual picture derivable from Adam Smith, Marx and Kalecki can easily encounter a visceral resistance because contradicting conservative ethical/political persuasions and political pressures, and in economics these are very influential. No doubt visceral resistances and academic conveniences bear strong responsibility for the refusal even to read the critics' writings. Here one finds the answer to why there is still so much use of aggregate production functions: none so deaf as those who refuse to listen. Misunderstandings persist, but largely because of the refusal to confront the critics' arguments, which by now are available in very clear presentations. Of course if people willingly ignore the criticisms, they cannot understand what is judged wrong in their theory.

So it is important to find ways to surmount the obstacles to a wider diffusion of non-neoclassical ideas (one way is to make these ideas more convincing). As argued by Chomsky, the intellectual battle is very important, we tend to underestimate the extent of the 'regimenting of the public mind'.[10]

Lastly, I come to Garegnani's attempt in 2000-2003 to prove that reverse capital deepening creates problems of non-uniqueness and instability in intertemporal general equilibria too, in particular questioning the adjustment of investment to savings. The validity of this complex attempt is an open question and will benefit from a completion of the archival work on his manuscripts. The attempt raised perplexities even among economists very close to Garegnani, for example myself; since 2003 I communicated to him increasingly strong doubts on the feasibility of what he was attempting, nor was I the only one; finally I criticised him in print in Petri (2011), unfortunately too late to benefit from his comments. No doubt these disagreements stimulated Garegnani to further reflections, of which however little is known. Also, it is unclear whether in 2000-2003 Garegnani was familiar with the theorem on which Mandler was basing his disagreement. This theorem states that if one assumes that aggregate consumer demand obeys the Weak Axiom then the intertemporal equilibrium (over a finite number of periods, and assuming monotonic utility functions, i.e. that capital goods too yield utility) is essentially unique [11] and tâtonnement stable. Mandler used it to argue that the presence of produced means of production cannot be a cause of problems for the uniqueness and stability of equilibrium, problems can only derive from consumer heterogeneity and income effects. It would seem, from personal communications, that later Garegnani did dedicate some time to this theorem; now, Garegnani's was a very profound mind, so probably he developed reflections of great interest on this theorem, to be searched for in the unpublished manuscripts of his last years. So I think that it is premature to try to assess now this contribution of Garegnani; it is better to wait for an examination of Garegnani's manuscripts, which will clarify his views and offers hope for new stimuli on this issue, on which reflection seems to have largely stopped in recent years.

Professor Parrinello (2011, 2022) is the exception, he has continued to try and prove that an explicit consideration of the savings-investment market in intertemporal equilibria (over a finite number of periods) introduces possibilities of instability absent in the standard tatonnement. So far I remain unconvinced by his arguments but it would take a full seminar to explain why.[12]

I have again and again come back on the issue, but have been able to prove only two things which do criticize intertemporal equilibrium theory but do not prove what Garegnani intended to prove. First, one should not concede the assumption of a finite number of equilibrium periods after which the economy *ends*; there is always a next period, so in each period there are decisions to produce capital goods that will yield returns in subsequent periods, decisions which can only be based on reasonable guesses (perfect foresight is nonsense); then even the Weak Axiom does not guarantee the uniqueness of equilibrium (Petri 2011). Second, in disequilibrium the spendable income of consumers cannot be assumed equal (as the tâtonnement assumes) to the value of factor supplies independently of whether these find purchasers or not, more realistically this income must be assumed equal to the value of that portion only of factor supplies that does find purchasers (that is, as Hicks and Keynes found obvious, unemployed workers have no income): then the tâtonnement cannot proceed, because investment is not determined by savings but determines savings, and therefore is indeterminate unless a theory of investment is supplied, and intertemporal equilibrium theory does not supply it (Petri 2017). It emerges that the adjustment of aggregate investment to full-employment savings is simply *assumed* by intertemporal equilibrium theory, with no supporting argument: a conclusion converging with the one by Garegnani, although differently based.

In spite of my disagreements with Garegnani (2000, 2003), my sensation is that *there is something* to Garegnani's basic intuition (particularly clearly stated in his last rejoinder to Mandler, in 2005), and that sooner or later a way will be found to clarify it and at least partially to confirm it. However, having said this, I cannot help wondering whether the topic is worth the great efforts it requires, since intertemporal equilibrium theory must be discarded anyway, because it is total nonsense. An examination of Garegnani's unpublished manuscripts will also perhaps help to understand how come he decided to attempt an internal critique of intertemporal equilibria after having written: "complete 'futures' markets not only do not exist, but cannot ever be thought to exist. It is impossible to imagine that we can now make all contracts relating to production and consumption over the entire future, and expect them to be fulfilled. The necessary foresight regarding the tastes of the individuals of future generations, future endowments of original factors, and the future technical conditions cannot evidently be assumed." (Garegnani 1990: p. 53) The enormous force of the social pressures on economic theory is confirmed by the fact that in spite of the obvious truth of this statements, Garegnani felt, and we too feel, that we cannot simply laugh the absurdities of intertemporal equilibrium theory out of our science.

Notes

1. I am too ignorant on Irving Fisher to be able to decide where to allocate him.
2. Also not contested, among those who accept the distinction between traditional and neo-Walrasian neoclassicals, seems Garegnani's contention that in the traditional versions one must distinguish (i) the Clark-Marshall versions where value capital is treated as an input analogous to labour or land even in the production functions of individual goods, (ii) the Böhm-Bawerk Wicksell versions based at least initially on the period of production, where the mistake of production functions with value capital as one of the inputs is avoided. In both groups of traditional versions capital is a single factor of variable 'form', measured as a quantity of exchange value, the value of capital goods.
3. Strangely, both Screpanti-Zamagni's and Roncaglia's references do not list the publications by Garegnani where these points are more clearly explained, Garegnani (1990, 2012).
4. Mosak publishes in 1944 a Cowles Commission monograph drawn from that Ph.D. dissertation, see Mosak (1944).
5. A Marshallian short-period demand for labour is no way out because of lack of substitutability as admitted by Hicks, plus insufficient persistence of the productive structure, plus other problems of a Marshallian short-period approach noted both in my paper on Schefold, Petri (2022b), and in my recent paper on Bloise and Reichlin, Petri (2022a). A neo-Walrasian demand curve for labour is totally indeterminable just like neo-Walrasian equilibria more generally, as argued in Petri (2022a) and briefly mentioned below in the main text.
6. This situation is not helped by the absence of a presentation of these criticisms, by now over 30 years old, even in histories of economic thought where one might expect to find them mentioned.
7. There seems to be room for a PhD thesis for those with an interest in methodology, on how neo-Walrasian economists have defended their theory in spite of the unreality of the auctioneer and of the absurdity of complete futures markets or perfect foresight.
8. A less succinct version of the argument of this paragraph, with some additional examples, is in Petri (2022a: p. 11 of the advance online version).
9. Then the continued use of aggregate production functions becomes easier to

understand, it is seen by their users as correctly illustrating the qualitative broad tendencies of market economies, the only problem is the quantitative correctness of numerical predictions and ex-post explanations. Actually here we find another case of refusal to consider the critics' arguments, the refusal to pay attention to the criticism advanced against Solow's econometric estimations already in 1957 by Phelps Brown and in 1958 by Hogan, then taken up and developed by Shaikh, Simon and, in numerous articles, by Felipe and McCombie (see e.g. Felipe 2003). The argument is that the good statistical fits often obtained with aggregate production functions prove nothing about the forces actually at work, because resulting from the degrees of freedom in the specification of the form and the parameters of the aggregate production function, which are such that an aggregate production function can always be perfectly fitted to an empirical time series of output and income distribution, if the way the parameters of the function vary in time owing to technical progress is derived ex post; less than perfect fits only result from constraints - of unclear justification - on the allowed parameter variations, e.g. from assuming that the coefficient α in $Q=A(t)\cdot K^{\alpha(t)}L^{1-\alpha(t)}$ does not change with t, which will yield a non-perfect fit if the shares of profits and wages in output are not constant. The critics have shown for example that the growth rate of Total Factor Productivity "is simply a weighted average of the growth rates of w and r (with the income shares as weights), whatever the determinants of those growth rates" (Petri 2004: p. 339). But no notice is taken of these articles by the neoclassical side.

10. " 'Prevailing doctrines could hardly survive were it not for their contribution to regimenting the public mind every bit as much as an army regiments the bodies of its soldiers,' to borrow the dictum of the respected Roosevelt-Kennedy liberal Edward Bernays in his classic manual for the Public Relations industry, of which he was one of the founders and leading figures. Bernays was drawing from his experience in Woodrow Wilson's State propaganda agency, the Committee on Public Information. 'It was, of course, the astounding success of propaganda during the war [World War I] that opened the eyes of the intelligent few in all departments of life to the possibilities of regimenting the public mind,' he wrote. His goal was to adapt these experiences to the needs of the 'intelligent minorities,' primarily business leaders, whose task is 'The conscious and intelligent manipulation of the organized habits and opinions of the masses.' Such 'engineering of consent' is the very 'essence of the democratic process,' Bernays wrote shortly before he was honored for his contributions by the American Psychological Association in 1949. ... Meanwhile the business world warned of 'the hazard facing industrialists' in 'the newly realized political power of the masses,' and the need to wage and win 'the everlasting battle for the minds of men' and 'indoctrinate citizens with the capitalist story' until 'they are able to play back the story with remarkable fidelity'; and so on, in an impressive flow, accompanied by even more impressive efforts, and surely one of the central themes of modern history." (Noam Chomsky, 'Market Democracy in a Neoliberal Order: Doctrines and Reality' (1997), available over the Internet.)

11. Actually equilibrium need not be unique in prices but all equilibrium price vectors form a convex set.
12. Still, Parrinello's 2011 analysis has produced an interesting result: it has uncovered a striking incompetence of the mathematical economists, Arrow included, who studied the stability of the 'Samuelsonian' tatonnement and did not realize that the way they were formulating it violates the homogeneity of demand functions, with an inconsistency that can be surmounted via simple economic considerations, as Parrinello shows. This suggests to me that there is a general overestimation of how much respect one should have for the big names of neoclassical mathematical economics.

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