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Capital, macroeconomics and perfect foresight:
throwing down the gauntlet

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The publication of Garegnani’s PhD thesis, besides making at last available in English a rare example of rigour and depth in the study of economic theory from which all economists can learn a lot already at the level of method, will help the profession to become better acquainted with a line of criticism of the marginal or supply-and-demand approach which Garegnani did not insist upon in subsequent years, for reasons that the study of his unpublished papers may clarify. He preferred not to publish the thesis in the most important language, English; so, for the criticisms — rather different from those raised during the Cambridge controversy — that the thesis advances against the marginal/neoclassical approach, a majority of economists had to wait for the much more succinct exposition in ‘Quantity of Capital’, 1990.

It was a pity, I think, that the thesis remained unavailable in English except in the library of Cambridge University during the years of the Cambridge controversy. My study of the Patinkin Controversy or Classical Dichotomy Controversy in monetary theory (Petri, 2004, ch. 5 Appendix 1) suggests that already in the 1950s familiarity with the long-period method, and consciousness that the founders of the marginal approach aimed at formulating *long-period* general equilibria, were absent in the vast majority of the profession. The confusions were enormous, and persisted in subsequent decades. An early publication of the thesis in English, with the comments and debates it would no doubt have stimulated, might have considerably redressed the situation.

The general misunderstanding, that the treatment of capital as a single factor in traditional authors meant the use of an Aggregate Production Function, would have been harder to maintain in front of the thesis’ discussion of Wicksell’s equilibrium as disaggregated and yet needing the single factor ‘capital’ because of the endogenous determination of the composition of capital. I felt this point was still badly in need of clarification in the mid-1970s, pushing me to publish my first article in 1978 on the

difference between long-period and neo-Walrasian equilibria, not because I felt I had anything original to say — I had learned it all from Garegnani's supervisions (yes I was lucky, I had two Cambridge-like supervisions with him in Firenze in 1973-4) — but because the point was not being made clear by anybody in the Cambridge controversies, not even by Garegnani (in published writings).

The thesis will help redress a tendency to underestimate the importance of the problems with the conception of capital as a single factor. There is in fact a tendency, testified for example by several papers by Professor Schefold on what remains of the Cambridge critique, by the 2009 article by Professors Bloise and Reichlin, and by other writings too, to reduce the Cambridge critique to the argument that reverse capital deepening questions the negative interest elasticity of the investment function.

But the roots of reswitching and reverse capital deepening — the inevitability of the measurement of capital as a quantity of exchange value, that depends on prices — also have supply-side destructive implications, which were in fact the ones on which the Cambridge debate at first concentrated: namely, the impossibility to *measure*, i.e. to define, this supposed factor 'capital' in a way consistent with the logical structure of the supply-and-demand approach. *Hence* the impossibility to define production functions with capital as one of the factors, and the impossibility to treat the economy's total endowment of capital as given in the determination of a long-period general equilibrium. Sufficient reasons, these, for Garegnani to conclude, in the thesis, that the marginal approach had to be abandoned, without needing for this conclusion the support of reswitching and reverse capital deepening.

Garegnani for many years did not insist on this impossibility of a satisfactory definition of capital as a factor, and preferred to argue that even if one neglected this deficiency the theory would remain indefensible, because of the possibly anti-neoclassical shape of the capital demand function and hence of the investment function. Thus in the 1970 paper 'Heterogeneous capital...', in order to criticize the 'traditional "demand function" for capital (saving)' (p. 425), Garegnani grants the assumption he calls (*d*), that 'as r and w change, with systems of production and relative outputs changing accordingly, net savings realized in the economy can still be meaningfully defined, and can be measured—however broadly—by the difference between the K of the final and that of the initial situation.'

In an attached footnote he immediately adds that this assumption is 'highly questionable', but he does not explicitly say that a given capital endowment K in the initial or in the final situation is indeterminable, hence is an illegitimate notion, he remains on the indeterminability of net savings: 'in an economy with heterogeneous capital goods ... [t]he possibility of referring to physical increments of the capital

stock will fail, and with that will fail the possibility of any meaningful notion of "net saving", not to mention "net saving" in terms of K' (1970 p. 425)

Of course the illegitimacy of a given capital endowment is implicit in these observations, but it is not explicitly stated, and one cannot expect a neoclassical reader easily to have realized it.

An analogous absence of explicit rejection of a given capital endowment emerges in the short 1983 paper "Two Routes to Effective Demand: Comment on Kregel", where Garegnani derives standard decreasing 'employment curves' for labour and for corn-capital (Fig. 2a.1, p. 70) in an economy where corn is the sole output, and then argues that 'when the theory is applied not to the *imaginary* corn economy, but to the *real* economy, with "capital" as a value magnitude, there is no reason to suppose that the employment curves should resemble those of Figure 2a.1, rather than those of Figure 2a.2. For the reasons we have seen this is sufficient to deny plausibility to the traditional argument about *a long period* tendency towards the full employment of labour.' (1983 p. 73).

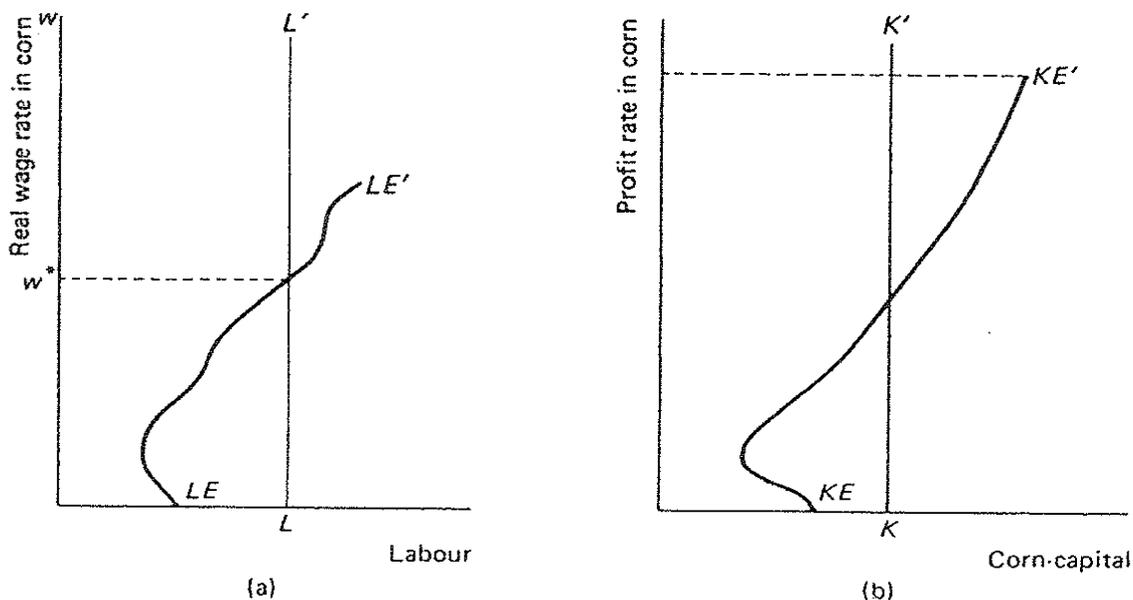


FIGURE 2a.2

(reproduced from Garegnani, 1983, p. 72. An employment curve indicates the long-period employment of a factor as a function of income distribution when the other factor is fully employed. It can be interpreted as a demand curve for that factor only if the equilibrium is stable, which is plausible only if employment curves are downward-sloping.)

What strikes me in this argument is the absence of any discussion of the legitimacy of drawing an ‘employment curve’ for *labour* when capital is a value magnitude, as if the amount of value capital *could* be treated as given as the real wage changes.

Things change only with the 1990 paper ‘Quantity of capital’, where Garegnani insists on the inconsistency of the value measurement of the endowment of capital; and in his last paper on the capital controversy the very last lines read ‘the concept of capital as an independently measurable single productive factor, which we now *all* agree does not exist’ (2012, p. 1431).

Unfortunately in the last decades in applied neoclassical analyses, for example in macroeconomics, the prevalent view seems to be that although a rigorous definition of capital as a single factor may be impossible (I think ‘may be’ is the correct description, because I doubt many neoclassical economists are clear on the issue), still *it is unnecessary*, because general equilibrium theory authorizes the use of simplified models where capital can be treated in the traditional marginalist way. Indeed the so-called DSGE models are not general equilibrium models at all, they are essentially variations on (‘sand thrown into the wheels’ of) the one-good Solow-Ramsey growth model, which no one dared call a general equilibrium model. (In some DSGE models the misunderstanding of the nature of capital extends to excluding intermediate goods from capital, a frequent mistake that reveals profound ignorance, but here I will neglect this aspect.)

The task of developing a solid alternative to the neoclassical picture is of course essential, and it deserves a majority of the energies of non-neoclassical economists, but criticism of the mainstream mistakes should not be abandoned, and one mistake definitely deserving criticism is the view which I have just described. The criticism will be helped by Garegnani’s thesis, because the latter shows in rich detail that the supply-side problems of the traditional neoclassical conception of capital suffice to reject it, and with it the entire supply-and-demand approach, in its traditional versions to start with, but then, I will argue, in its modern neo-Walrasian versions too. With the help of the thesis it will be easier to go back to the problems with measuring i.e. *defining* capital as a single factor, and to point out the logical implications of the non-existence of a satisfactory definition of this factor compatible with the requirements of the marginal approach.

One evident implication is that it is impossible to take the endowment of capital as given; and this has in turn important implications that are not always fully grasped. Rigour requires admitting that without a given endowment of capital a labour ‘employment curve’ cannot be determined,

- hence a labour demand curve is indeterminable,

- hence it cannot be stated that lower real wages imply more labour employment

- hence an equilibrium real wage is indeterminable,

- hence a different theory determining the real wage is indispensable.

Of course the implications of the non-existence of the factor ‘capital’ extend beyond the specification of the *endowment* of capital, to all instances in which one talks of capital as a factor; production functions with capital as one of the factors are illegitimate; the notion of capital-labour substitution must be abandoned (so the use of capital-labour isoquants to derive the demand for labour and for capital from a given expected output is illegitimate too; this is relevant for the Dornbusch-Fischer approach to investment, discussed in Petri 2013, 2015); all books and articles using the notion of capital as a factor must be considered thoroughly unacceptable, and all journals that publish such articles must be considered as respectable as a mathematics journal which regularly published papers containing patent mathematical mistakes – i.e., not respectable at all.

Another implication is that, since the tendency toward the full employment of labour cannot be argued, investment becomes subject above all to a dependence on expected demand, which opens the door to further possibilities of instability as I have stressed in my 2013 and 2015 papers. The entire neoclassical edifice crumbles.

The *entire* edifice? But what about neo-Walrasian general equilibrium, and the macro models which claim to have it as their microfoundation? Yes, it is the entire edifice that crumbles, because *neoclassical theory in its concrete applications, as well as the attribution to neo-Walrasian general equilibrium theory of explicative and predictive capacities, rest on traditional capital-labour substitution.*

This can be solidly argued and I see insistence on this point as an important necessity in the present situation of neoclassical economic theory. We have the last paper of Garegnani (2012) as an excellent explanation of what seems not to be understood by neoclassical economists. It behoves those who have understood Garegnani to insist on this point.

I hereby fling down the gauntlet, challenging neoclassical economists to attempt to refute the argument I will now put forward.

The argument is that neo-Walrasian general intertemporal equilibrium theory is not the true microfoundation of current applied mainstream analyses. The assumption that the behaviour of the economy is sufficiently well indicated by the intertemporal general equilibrium path rests and can only rest on an implicit appeal to the traditional marginalist long-period adjustments, only justifiable on the basis of traditional capital-labour substitution.

I have already advanced this argument in previous papers (e.g. Petri, 1999, 2017; Dvoskin and Petri, 2017), so some of my sentences below are repetitions of things I have written in these papers, but here I will add references to the literature to confirm my argument, and I will also add a further argument concerning the absurdity of perfect foresight.

The argument as advanced in my previous papers rests on the impermanence problem, stressed by Garegnani in 1976 and 1990. The point is that, even assuming the determinability of a well-defined intertemporal equilibrium path corresponding to a given economy (in fact this assumption is unsustainable, I will argue later), still intertemporal equilibrium theory supplies no credible reason why the economy should be on that path or reach it or tend to it or remain close to it.

(I leave aside temporary equilibria (without perfect foresight) as these seem to have been completely abandoned; the theory of temporary equilibrium is no longer mentioned in textbooks, even the advanced ones; the only exception is my textbook *Microeconomics for the Critical Mind* where anyway the aim is only to show that there were excellent reasons to abandon that theory, and I was lucky to obtain Professor Ravagnani to explain the thing to students much better than I could have done, I am very grateful for this.)

The auctioneer-guided instantaneous tâtonnement is only a fairy tale, it tells us nothing about how disequilibrium adjustments really operate; but intertemporal equilibrium theory cannot be combined with more realistic, hence time-consuming, adjustment processes because then the endowments of heterogeneous capital goods are altered by disequilibrium actions; the original equilibrium is no longer there to be reached and is in fact ‘essentially irrelevant’ as honestly admitted by Franklin Fisher (1983, p. 14), and one does not know where disequilibrium actions take the economy.

This inability to discuss actual disequilibrium means that intertemporal equilibrium theory can give no indication of forces acting in disequilibrium and capable of correcting or compensating the inevitable deviations of the actual path from the equilibrium path. Unless one assumes the economy to be continually perfectly in equilibrium, the theory is totally silent on the actual behaviour of the economy. One consequence is that, already for this reason alone, the theory is unable to *justify* the adjustment of investment to full-employment savings via realistic adjustments, it can only *assume* it.

Neoclassical economists are certainly aware of this difficulty. They cannot ignore that in real economies there are disequilibria, and that the theory has the right to assume continuous equilibrium only if the actual path remains close to the equilibrium path, something the theory is unable to prove. I suspect this has been at least as important as the Sonnenschein-Mantel-Debreu results in causing the

scandalous disappearance of all discussion of the stability of equilibrium from recent treatises and advanced textbooks. But then, how can these economists go on attributing to disaggregated intertemporal equilibria the role of indicators of the tendencies of actual economies?

I can see only one explanation. These economists must be persuaded that the qualitative trend indicated by the intertemporal equilibrium path sufficiently corresponds to the trend of the actual economy *in spite of* the absence of the auctioneer or of instantaneous adjustments. Evidently these economists continue to believe in a tendency of firms to employ more labour if the real wage decreases, and to invest more if the rate of interest decreases; and in a tendency of the real wage and of the rate of interest to respond to excess demands in the respective markets. From these mechanisms based on traditional capital-labour substitution they derive a tendency toward the full employment of resources and toward an income distribution determined by marginal products; this tendency is taken to imply that *on average* the economy evolves in the way described by the traditional neoclassical picture, that is (in simplified form) by the Solow growth model.

The explanatory-predictive capacity of intertemporal equilibria is then *derived* from the fact that the paths these equilibria determine are qualitatively similar to the traditional neoclassical picture of trends, which is believed correct. That is, it is the similarity between the trend described by Solow-Ramsey models, which is believed correct on average, and the path generated by general intertemporal equilibria (a full-employment path too, with income distribution determined by marginal products and investment determined by savings) that authorizes the belief that *the assumption that the economy is in continuous intertemporal equilibrium does not entail gross mistakes about the average trend the economy will follow.*

I conclude that these economists still believe in the traditional marginalist adjustment mechanisms, which means they still ultimately believe in capital-labour substitution as traditionally conceived; the traditional notion of capital has been only apparently abandoned.

One cannot expect to find *explicit* confirmation for this interpretation. Economists who are aware that the Cambridge criticism of the traditional conception of capital as a single factor was dismissed by claiming that no such conception is needed by ‘rigorous’ neoclassical theory, who accordingly declare Arrow-Debreu to be the microfoundation of their macro analyses, and then must assume that prices and quantities ‘are taken to be always in equilibrium’ (Lucas, 1980, p. 709) — these economists cannot be expected explicitly to admit that they have not given up on traditional capital-labour substitution.

But the persuasion that they have the right to reason as if that traditional conception of capital were valid — let me indicate it as the Persuasion — implicitly emerges whenever these economists discuss concrete economies. The Persuasion emerges in Lucas' little-noticed admissions that the learning of rational expectations *takes time* (see Dvoskin, 2014, for quotes and excellent comments); these admissions imply that much of the time expectations are mistaken, so prices and quantities cannot always be at their equilibrium values, the equilibrium can at most indicate averages and trends, and only if it is the centre of gravitation of the actual path: which requires that the equilibrium be conceived as not only stable but also *persistent*, hence defined on the basis of persistent data, i.e. that the changes the composition of capital inevitably undergoes owing to disequilibrium actions do not alter the equilibrium; which requires the traditional conception of capital.

The same Persuasion also emerges in Lucas' declaration in his Cambridge lecture that capital can be treated as 'a force, not directly observable, that we postulate in order to account in a unified way for certain things we can observe', for example 'that the production of these [capital] goods enhances labor productivity in future periods', things whose observation gives 'aggregative theorists a sense of having "microeconomic foundations"' (Lucas, 1988, pp. 35-36): a contorted way to say that this 'force' is correctly grasped by the (neoclassical) models, including of course Lucas' own, which like Solow's growth model treat capital as a single good homogeneous with output.

The Persuasion again emerges in the interventions on policy issues of such general equilibrium specialists as Malinvaud (2003) or Bliss (1983), where adjustments in actual economies are clearly *not* assumed to be instantaneous and yet the admission is explicit that, in the absence of price rigidities or liquidity traps, downward wage flexibility would raise employment. Here the Persuasion is revealed by the fact that these brilliant general equilibrium specialists should know well that if, as they admit, the wage change causes time-consuming disequilibrium adjustments and hence changes in the composition of the capital stock, then they cannot derive *any* effect of wage changes on employment from Arrow-Debreu equilibria: definite comparative-statics conclusions become impossible; so they are in fact treating capital as the traditional single factor. (Bliss (1999) is also very indicative: in this article that discusses the credibility of the Solow and the Ramsey growth models for the explanation of the very-long-run rate of interest no doubt at all is raised on the treatment of capital as a single factor homogeneous with output.)

The Persuasion again emerges in a recent article by Guzman and Stiglitz (2020 OREP) kindly brought to my attention a few weeks ago by Professor Bloise, very interesting because it admits instabilities, grave weaknesses of the Arrow-Debreu

model (bankruptcies, information costs, herd behaviour...), the absence of any proof of the stability of equilibrium in current equilibrium models, the presence of involuntary unemployment and the need for fiscal policies in grave downturns; *but* also argues that in the absence of serious disturbances — ‘in normal times’ as the authors put it — wage reductions *would* be capable of bringing about increases in labour employment (*ibid.* pp. 624; see also pp. 642, 645-6), and that except ‘when there is too much uncertainty’ the elasticity of aggregate demand to the interest rate is negative and significant (*ibid.* p. 661): that is, standard capital-labour substitution is assumed to be operative, although sometimes blocked by uncertainty, imperfect information and various destabilizing influences.

This evidence strongly confirms that capital-labour substitution is still accepted, and is in fact the real microfoundation of the claimed validity of intertemporal equilibrium theory as a positive theory, not the reverse.

It is then largely a mistake to criticize DSGE models on the assumptions of a single representative consumer or of continuous equilibrium. These can be argued to be only simplifications which do not grossly misrepresent the average trend of the economy if this is correctly grasped by the Solow-Ramsey model (perhaps modified by the addition of imperfections and frictions).

The criticism must be directed at the neoclassical theory of distribution embodied in the Solow model itself:

- first, at the pretence that the neoclassical theory behind the model is justified by modern neo-Walrasian general equilibrium theory;

- second, once that pretence is refuted, at the inability to justify that theory on the basis of traditional marginalist factor substitution mechanisms.

I see little difficulty with this second step, fortified as it can be by more insistence on the non-existence of the factor ‘capital’. This is why I am now concentrating on the first step, the criticism that general equilibrium theory is unable to tell us anything on how actual economies behave and therefore is no support for Solow-type models. I will now strengthen the criticism with a second argument.

This second argument focuses on the need of intertemporal general equilibrium theory (extended to include contingent commodities), IGE theory for short, to assume perfect foresight not only of future prices but also of future economic conditions. The need to assume perfect foresight of future prices arises because the reaching of equilibrium for all periods at the initial date on the basis of complete markets in ‘futures’, the ‘all-at-once market structure’ as Kreps 2013 calls it, is impossible, it would require the presence at the initial date of yet-to-be-born consumers; so the equilibrium can only be a Radner equilibrium — or Equilibrium of Plans Prices and Price Expectations (EPPPE) — , a sequence of spot market equilibria (plus some

ways to transfer purchasing power across dates and states), which reaches the same allocation as the complete-markets Arrow-Debreu equilibrium because of correct foresight of the prices that will rule (because equilibrium prices) at each subsequent date-state combination, in case that state occurs at that date.

The unreality of perfect price foresight is admitted by any economist to whom the question is posed, so we have here one more reason why intertemporal equilibrium theory cannot be the real microfoundation of neoclassical macro theory. Consider in particular: ‘My guess would be that not a single proponent of general equilibrium theory holds that perfect foresight accurately describes agents’ expectations. Perfect foresight models are not designed to deliver descriptive accuracy’ (Mandler, 2005, p. 487). Mandler’s admission is particularly interesting. The only alternatives to perfect foresight in general equilibrium theory are the impossible ‘all-at-once market structure’, or temporary equilibria without perfect foresight which have been unanimously abandoned as hopeless; therefore Mandler’s statement means that there is *no* version of neo-Walrasian value theory designed to deliver descriptive accuracy.

But the unreality of the perfect foresight of *future economic conditions* is a much less discussed weakness of the notion of IGE, and it questions the determinability itself, and hence the existence, of intertemporal equilibria, so it deserves special attention. This weakness is briefly mentioned in the last lines of the following passage in ‘Quantity of capital’:

“The influence of future prices would in fact be susceptible to an objective treatment, if we could assume the existence of complete markets for future commodities. However, such 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)

These considerations should have sufficed completely to discard the notion of IGE. But this has not happened. (Clearly there is room here for a PhD dissertation, on how the notion of intertemporal equilibrium came to be accepted in spite of its evident absurdity, and on what attempts at justification have been advanced, if any, to surmount Hicks’s dismissal of extensive forward trading in *Value and Capital*, 1946, pp. 136-9.) So let me try to make the absurdity of the IGE notion more evident to

neoclassical economists, by reformulating Garegnani's rejection of 'the necessary foresight' in terms of the problems the IGE notion encounters because of the occurrence of *novelties*.

Novelties are happenings, events, occurrences that are totally unexpected in the sense that no one could have predicted them, be it because the scientific knowledge is still missing that might have predicted their occurrence; or because the novelties concern areas of reality not explored yet (we cannot predict, for example, what new dangerous viruses will be discovered in bats); or because they are logically impossible because resulting from creative brain processes, and a brain (a human brain as much as an artificial intelligence) cannot predict the new results of its thought processes before obtaining them: which means that new inventions, new scientific theories and theorems, and more generally original ideas, innovative laws, new artistic creations, new writings (e.g. the present paper) cannot be predicted, their prediction is *logically impossible*.

This last reason implies that the future evolution of preferences cannot be precisely predicted either: I cannot know which future ideas my brain will develop and how this will affect my future preferences.

So the notion of EPPPE requires *two* kinds of perfect foresight: of prices for each date-state combination, and of which future date-state combinations are *possible*. This second kind of perfect foresight assumes correct prediction of all possible new events, new discoveries, new ideas, in sum *perfect foresight of possible future novelties*, which however would destroy their nature of novelties and therefore is impossible. So the IGE assumption of complete knowledge of the set of possible future states of the world is unacceptable.

After reaching this conclusion, I was glad to discover it had been reached by Guzman and Stiglitz too:

'The assumption of complete markets requires that market participants have full knowledge of the space of states, from now on *ad infinitum*. In a deep sense, it assumes an economy in which there are no innovations. Everything that can ever happen is considered as a possibility from the moment when the market is completed. One has to have a market for the creation of atomic energy before the concept has been *conceived*.' (Guzman and Stiglitz, 2020, p. 628).

What remains unclear in this article is the consequences of this observation. What are the implications of dropping the assumption of complete knowledge of the set of future possible states of the world? Clearly, if the possible futures are not known, then no *ex ante* intertemporal co-ordination of agents' actions can be reached,

since again and again agents will have to face a novelty and what will then happen cannot be predicted since the nature of the novelty is unpredictable. That is, no general intertemporal equilibrium can be conceived capable of also covering situations following a novelty with relevant economic effects.

But relevant novelties are quite frequent: think of the dissolution of USSR, the euro, the Internet, smartphones, bitcoins, the 2008 crisis, Covid.... And no doubt small novelties are happening all the time, for example gradual changes in preferences: no one is able to predict them except vaguely and tentatively, and occasionally they can generate relevant novelties too, e.g. unexpected results of elections, or rapid changes in certain habits (e.g. importance of social media). After a few years the cumulative effect of novelties would render any path without novelties clearly far removed from how the actual economy is behaving.

But this is not all. The breathtaking assumption that from the moment equilibrium is established there are no more novelties is never made explicit, and perhaps for this reason it escapes most economists; so it also escapes attention that intertemporal equilibria *are internally contradictory*.

It is contradictory to endow agents with amazing powers of correct prediction of prices as an EPPPE needs to assume, and yet to assume they ignore that novelties will certainly occur: this contradicts the assumed rationality of agents. Price forecasts based on the assumption of no novelties would be *known* to be generally mistaken, the more so the farther in the future the period to which they refer, owing to the cumulation of novelties. People would know, for example, that the *search* for novelties (in science, in firms, in the entertainment business, in the military apparatus...) is certainly not going to stop. Not by chance, no one has dared explicitly to propose such a notion of intertemporal equilibrium without novelties. And yet this absence of novelties is what the theory needs in order for intertemporal equilibria to be determinable, but it means an indefensible specification of how people take their intertemporal decisions.

So, consistent intertemporal general equilibria are indeterminable: they are clearly not based on correct foresight and hence contradictory if ‘no novelties’ is assumed, and are indeterminable if novelties are admitted (as they must be).

Conclusion: the idea, that one can associate to any given economy at a certain date a definite intertemporal equilibrium path that starts at that date, dissolves into thin air; it comes out to be only a fantasy. *There are no intertemporal equilibria.*

The above gives greater precision to the intuitive feeling, shared I am sure by neoclassical economists too, that the future is not predictable in all economically relevant details, not even in the sense of a list of *possible* futures. The idea of an

equilibrium over the infinite future is confirmed to be nonsense upon stilts, as Jeremy Bentham would have put it (and Joan Robinson too on one occasion, I think).

So we have here a second reason, additional to the impermanence problem, to judge intertemporal equilibria unable to tell us anything about the actual path a market economy will follow. Neoclassical macro models derive no justification at all from IGE theory. The more this will be admitted, the more it will become clear that it is the traditional versions of the marginal approach that these models actually rely on, and then the untenability of the factor 'capital' and the implications of reswitching will be impossible to escape.

The considerations advanced to explain the neglect of the impermanence problem appear useful here too: perhaps the problems caused by novelties are not clear to most neoclassical economists, but perfect price foresight *is* admitted to be totally unrealistic whenever the question is posed. Again, the way to explain the readiness to accept IGE as indicative of the economy's behaviour in spite of knowing that there is no perfect price foresight in actual economies appears to be that, *because of a prior certainty that the neoclassical general picture cannot be wrong, it is believed that to assume perfect price foresight does not cause gross mistakes about the trend the actual economy will follow, because the economy will anyway follow a neoclassical trend roughly similar to a Solow growth path.*

It is this prior certainty that must be criticized, after having made it clear that it is not at all supported by intertemporal general equilibrium theory.

However, the article by Guzman and Stiglitz suggests that the faith in the traditional substitution mechanisms can be so strong, that it can actually do largely without a reference to IGE as the microfoundation of macro analyses and policies. In this article the possibility of non-existence of an Arrow-Debreu equilibrium owing to bankruptcies, the absence of any proof of the stability of equilibrium, the problems caused by the equilibrium's inability to deal with novelties are not considered a reason for worry, they are taken to mean only that markets do not always operate in the ideal way assumed by IGE theory. A consciousness seems to be totally missing that the adjustment mechanisms justifying the two basic pillars of the neoclassical picture, the tendency toward the full employment of labour and the adjustment of investment to savings, cannot be assumed without a theoretical foundation, and if neo-Walrasian general equilibrium theory is admitted not to support them, then (since the traditional versions of the marginal approach based on the factor 'capital' have been rejected in favour of neo-Walrasian theory, see Stiglitz 1974) no theoretical foundation is left for them and they can only be judged unscientific dogmas.

So there are many confusions that need dispelling. I think that an advanced macro textbook which introduced in a user-friendly but rigorous way to

macroeconomics in the way Garegnani might have liked, carefully explaining the history of macro, the need for basing macro theory on a theory of distribution, and the untenability of the marginal approach, dispelling the confusions one finds for example in Stiglitz, and debunking DSGE models as deprived of any foundation both theoretically and empirically, would be very useful.

I had a quick look at a number of alternative textbooks, Lance Taylor, Foley and Michl, Shaikh, Hein and Stockhammer, and I think one can do better, these textbooks do not help surmount the confusions I have pointed out. The energies and capabilities to write such a textbook seem to me to be available, especially with the arrival of so many new young energies, which for example have cooperated with Professor Stirati on a series of useful contributions which have not been afraid of econometrics. I do not see myself as part of such a project owing to my limited knowledge of the recent macro literature and to my econometric incompetence, but I think that the start of such a project would be an excellent homage to the memory of Garegnani.

Thank you

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