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# Should Sraffian economics be dropped out of the post-Keynesian school?*

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*Un certain nombre de post-keynésiens prétendent que la théorie postkeynésienne se porterait mieux si on pouvait en exclure la branche sraffienne. L'auteur rappelle les liens historiques étroits qui existent entre les sraffiens et les autres postkeynésiens et il présente les arguments en faveur de l'intégration de l'école sraffienne avec les autres branches postkeynésiennes, montrant, exception faite du concept de gravitation autour des prix de production, que les sraffiens et les autres postkeynésiens partagent de nombreuses notions théoriques.*

*A number of post-Keynesians have recently argued that post-Keynesian economics would be more coherent if the Sraffian strand were excluded from it. The paper recalls the historical tight links between the Sraffian and the other strands of post-Keynesian economics and it presents arguments in favour of the integration of Sraffian economics into post-Keynesian economics, showing that, except on the notion of the gravitation towards prices of production, Sraffians and other post-Keynesians hold very similar positions.*

* Paper prepared for the Conference at the University of Roma Tre, 2-4 December 2010, *Sraffa's Production of Commodities by Means of Commodities 1960-2010, Critique and reconstruction of economic theory*. I am grateful for the comments provided by Geoff Harcourt. This paper is dedicated to my former teacher, Tom Rymes, who passed away in May 2011. Rymes and I had three common interests: tennis, Sraffian and Keynesian economics. Among other things, he compiled *Keynes's Lectures, 1932-35, Notes of a Representative Student*, published in 1989.

The title is purposefully provocative for an assembly of distinguished economists, gathered to honour the 50th year of the appearance of Sraffa's *Production of Commodities by Means of Commodities*. Let me say right away that my answer to the question – should Sraffian economics be dropped out of the post-Keynesian school – is a clear negative one. So I am throwing away the punch line: Sraffian economics is an important component of post-Keynesian economics, defined as a large tent.

Before I move on to more formal writings, allow me to point out that my relationship with Sraffa goes back in time deeper than I thought. Until recently, I assumed that I had two nearly simultaneous initial encounters with Sraffa in 1975: One in a reading course, where Dean Gilles Paquet forced me to read and comment on all kinds of classic papers, including Sraffa's 1926 paper; the second, when T.K. Rymes introduced the Cambridge capital controversies in his Honours Seminar. All the students in the class wondered why Rymes bothered to discuss these controversies when he seemed to have such a hard time conveying their meaning. It is only a year later, when I was in England to visit my parents, that I realized, browsing through shelves in a London bookstore, that he had written a whole monograph about it [Rymes (1971)], with a foreword by Joan Robinson. Later, I decided to do the same, devoting more than half of my first book to a pedagogical presentation of these controversies [Lavoie (1987)]. But coming back to my parents, just a few months ago, when cleaning up my office, I fell upon my father's MA dissertation and discovered that he had written a few lines about Sraffa's 1926 paper. So I was fated to get interested in Sraffian economics.

Enough gossip. Here is a brief outline of the paper. I will start by recalling the tensions that have been existing between Sraffians and the economists belonging to the other branches of post-Keynesianism. In particular, I will present the various arguments, most of which based on methodology, that have been put forward to argue that Sraffian economics ought to be removed from post-Keynesian economics. I will then contend that this move, in my opinion, would be a mistake, both for historical reasons and also because Sraffians and the other post-Keynesians share several theoretical positions. Finally, I will claim that the reluctance of post-Keynesians to take Sraffian economics aboard is tied to a caricature of the dominant Sraffian strand, and that except for one issue, there is no true incompatibility between Sraffian economics and the rest of the post-Keynesian school. Before moving on, I should provide a word of warning: the object is the present paper is not to find out whether Sraffa's writings are compatible with those of Keynes and Kalecki or those of the economists who knew them. Rather the purpose is to check whether contemporary post-Keynesians fit with each other.



## SRAFFIAN ECONOMICS SHOULD BE PURGED OUT OF POST-KEYNESIAN ECONOMICS

### The Trieste partial failure

Before we go any further, it should be pointed out that despite their relatively small numbers, it is possible to subdivide the post-Keynesians and the Sraffians in various strands or sub-schools. The post-Keynesians, following the well-known distinction of Hamouda and Harcourt (1988), are said to be made up of Fundamentalist Keynesians, Kaleckians and Sraffians, to which we can add Kaldorians and Institutionalists à la Galbraith [Lavoie (2011)]. Roncaglia (1991) has distinguished Marxian Sraffians (e.g., Pierangelo Garegnani and John Eatwell), Ricardian Sraffians (e.g., Luigi Pasinetti), and Smithian Sraffians, which he associates with Sylos Labini but which should certainly be associated with Roncaglia himself.[1]

Arena (1987) identifies exactly the same three Sraffian strands. At least from a sociology of knowledge point of view, he argues that the Fundamentalist Keynesians and the Marxian Sraffians, which he calls the surplus approach, were the two *dominant*, or *constituted*, strands in the 1980s [Arena (1987, 1992)]. The members of the other post-Keynesian and Sraffian strands were thus *dissidents* relative to the dominant views. Arena bases his assessment from his observation of the several Trieste summer schools and conferences that he attended, noting that the viewpoints associated with Fundamentalist Keynesianism and Garegnani's surplus approach dominated the debates at the schools. As is well-known, these schools were held each year from 1981 to 1990 in a hotel next to the Italian Adriatic sea. The school was run by a triumvirate made up of Garegnani, Jan Kregel, and Sergio Parinello. The purpose of the school, besides bringing established and young researchers together, was to promote a fruitful alternative to

[1] I am not fully at ease with these names. I think it would be best to call them the Garegnani, Pasinetti and Roncaglia sub-schools. For instance, I have argued that Pasinetti (1981) is the last attempt at defending a Marxist-kind of labour theory of value, since his natural prices are such that the price of a consumption good are exactly proportional to the sum of direct, indirect and hyper-indirect labour. One could also argue that Pasinetti's approach is Smithian, because of his focus on technical progress.

neoclassical economics that would be based on a synthesis of the Keynesian monetary production economy and the Sraffian surplus approach. But as summarized by Edward Nell (2005, p. 150), a frequent participant to these summer schools, " no agreement was ever reached ".

The evolution of the relationship between Sraffian economics and post-Keynesian economics has been well summarized by Gary Mongiovi (2003, p. 318): " Sraffian economics was generally regarded not only as compatible with Post Keynesianism, but as an important branch of it.... By the end of the 1980s, however, this view had largely given way to the presumption that the two frameworks are distinct, and, in at least some key respects, incompatible ". This is confirmed by John King (forthcoming, p. 18), who says that " almost no-one today regards Post Keynesian-Sraffian economics as a single coherent school ". Mongiovi explains this development as both the cause and the effect of the apparent failure of the Trieste summer school to achieve " a unified methodological and analytical foundation for the Post Keynesian project ".

As usually portrayed, and as reported by Arena (1987, 1992), the disagreement mainly involved on the one hand Fundamentalist Keynesians (also called Post Keynesians by Dutt and Amadeo (1990)) — those concerned with fundamental uncertainty as well as monetary and banking relations, along with instability and disequilibrium dynamics, such as Paul Davidson and Hyman MInsky ; and it involved on the other hand the Marxian Sraffians – led by Garegnani and John Eatwell, who emphasized stable convergence or gravitation towards long-period positions. " Sadly the dispute between rival schools of thought became more and more intense. Indeed the conflict between Fundamentalist Keynesian and Sraffians ... seems to have crystallised in Trieste. The growing antagonism between adherents to these two points of view came to be reflected both in divisions in the organizing committee and in acrimonious conflicts in the classroom " [King (2002), p. 158]. As a student participant to the 1984 Trieste summer school, despite the overall easy-going atmosphere, I have observed first-hand the arrogance of some of the protagonists as well as the pressure to score points to impress followers during well-rehearsed debates.

The rising degree of annoyance can be observed by reading the proceedings of the 1981 Udine conference [Kregel (1983)] and those of the 1985 Florence conference [Bharadwaj and Schefold (1990)], comparing, at a four-year interval, the comments made by Fundamentalist



Keynesians on papers presented by Sraffians. In the latter conference, comments lose their varnish of sympathy and reflect impatience. What Pasinetti (2007, p. 38) said about his Cambridge days, applies perfectly here: " There was, then, a personal and temperamental component that exacerbated the situation ". Indeed, the Magnani (1983) paper, published in the book edited by Eatwell and Milgate (1983) which constituted a kind of manifesto for the surplus approach, probably put gas on the burner, as Magnani accused the Fundamentalist Keynesians of being no different from the neoclassical Keynesian imperfectionists – the worse accusation that could be made against Davidson — claiming that involuntary unemployment in his model arose from nominal wage rigidity.

Conflicts between strong personalities, as well as excessive emphasis over the positioning claims made by these individuals, as suggested by Sheila Dow in an interview with John King (1995, p. 163) would then explain, at least in part, the failure of the Trieste experiment as well as the feeling that the synthesis experiment was not successful.[2] This would help to justify Mongiovi's (2003, p. 320) view about the rift: " The tensions are largely unnecessary, and at least some Post Keynesian resistance to the Sraffian view is based on a misunderstanding of it. There are of course genuine differences of perspective, but these do not render the two traditions incompatible with each other ". Somehow the members of the *dissident* strands, who, as pointed out by Arena (1987, 1992), were much likely to agree on some synthesis or compromise, did not manage to draw or retain followers and to get their message across. Indeed, as was the case with Alfred Eichner (1987) – an icon among those trying to put together a coherent heterodox view that would integrate Sraffian concerns with interdepence in pricing – some of the dissidents were never invited to Trieste. Again, in my opinion, this does not have to do with the validity of their arguments, but rather with the fact that those that hold important institutional positions, tied to journals or to financial resources, tend to have the upper hand; and also with the fact that the most vocal academics, usually also taking the most unambiguous stances, tend to attract the

[2] Dutt and Amadeo (1990, p. 155) also believe that the quarrels had to do with ideology, with Sraffians being generally more distrustful of the efficacy of the market system than were Fundamentalist Keynesians, some of which seemed to give credence to Keynes' well-known claim that once the economy was back to full employment, no objection could be raised against neoclassical analysis. But as pointed out by Harcourt (1996-97), these differences now appear minor when heterodox views are compared to the free-market ideology and market-clearing assumptions of the profession mainstream.

attention of fellow researchers. Those who take less clear-cut, eclectic, positions, which the dissenters did, do not seem to attract as many devoted followers.

With the end of the Trieste school in 1990, the attempt to bring together the Sraffians and the other post-Keynesians seems to have been terminated. The Post Keynesian Summer school that started off in Knoxville in 1992 mainly brought together Kaleckians, Kaldorians and Fundamentalist Keynesians. As Dow points out in her interview, she felt at the end of the 1980s that " Sraffians and other Post Keynesians were not talking to each other so much " [King (1995), p. 162]. My last recollection of a major post-Keynesian meeting attracting Sraffian economists was at the occasion of the conference held in 1992 at the Levy Economics Institute in honour of Tom Asimakopulos – a frequent participant to the Trieste schools despite being quite skeptical of achieving the synthesis objective. After two days of rather abstract theoretical presentations, the organizers had felt that the sponsors of the event – the Levy brothers, who were mostly concerned with forecasting – would be showing signs of annoyance, and so two special round tables on economic policy had been added to the program with little advance notice. The evening round table was made up entirely of Sraffians, thus giving them the impression that they had been purposefully trapped, to demonstrate to all present that Sraffians were unable to discuss policy issues. If my memory serves me well, Heinz Kurz and Bertram Schefold passed the test with flying colours, giving very inspiring talks on European economic and monetary policy, but the anecdote serves to show that the relationship between Sraffians and other post-Keynesians was getting ever strained. Trust, which is so important in science, was fading away among the members of the two dominant and apparently rival strands.

Still, later on in the text, I will try to show that Mongiovi is right in claiming that the rift between the post-Keynesian and Sraffian subschools is *mainly* due to a misunderstanding, even when authors from the respective dominant strands are considered. But before we do so, I will explain why and how the prospects of an integrated Keynesian and Sraffian project lost even more momentum after the end of the Trieste school in 1990.

## The role of methodology

In my view, the rift between Sraffians and other post-Keynesians was enlarged by the growing importance accorded to methodology



within post-Keynesian economics starting in the late 1980s. Fontana and Gerrard (2006) have called this the Age of Uncertainty, both because post-Keynesians became less sure that some alternative synthesis would ever be possible and also because methodologists and philosophers nearly took over the field, emphasizing the radical uncertainty content of Keynes's writings.[3] These methodologists, in general, tended to be more favourable to the point of view defended by Fundamentalist Keynesians, and did not do modelling themselves, or if they did, relied on standard supply and demand curves, not fully aware of their interdependence. As Fontana and Gerrard (2006, p. 63) rightfully pointed out, " the increasing concern with the methodological originality of Post Keynesian economics served to exacerbate the disputes between and within the different strands of Post Keynesian economics ".

The clearest example of the proposed break is from Gerrard himself, a Fundamentalist Keynesian. In his book, devoted to what he calls a " post-classical synthesis " – a term I have used myself [Lavoie (1992a)] to designate a mixture of Keynesian economics, radical economics and the surplus approach – Gerrard (1989, p. 181) proposes an ABS synthesis, that is an All-But-Sraffians synthesis. He writes that " the neo-Ricardian influence has been an entirely negative one....The neo-Ricardians have gone off on the completely wrong track, ignoring or dismissing much of Keynes's concern with the implications of the dynamic and uncertain context of behaviour in the capitalist economy ". A similar, but more circumspect opinion was being offered at the same time by Sheila Dow (1988, p. 7), claiming that while the surplus approach " could be incorporated as part of a more general Post Keynesian approach, its presentation as a complete approach, complete in itself, may render it methodologically incompatible with the rest of Post Keynesianism ". Dow attributed this to an exclusive focus on the long period and the exclusion of the role of money and uncertainty.

While recognizing common elements, such as the theory of effective demand, Dow thus thought that there was a methodological incompatibility between Sraffian economics and the rest of post-Keynesianism. She concluded by saying that " it becomes doubtful as to whether much of neo-Ricardian analysis can be classified as Post Keynesianism ". Dow's (1988, p. 13) main complaint was that " the pre-

[3] This was accompanied by a plethora of studies on history of economic thought among all strands of post-Keynesian analysis, as noted by Peter Reynolds (King 1995, p. 131), a fad that did not spare Sraffian economics according to Nell (1998, p. 720).

sentation of a general, analytically complete long-period system, excluding Post Keynesian short-period analysis as irrelevant to the long-period outcome, is methodologically distinct from the partial use of equilibrium in the rest of Post Keynesian economics ". As is clear from her interview, Dow attributed this incompatibility to rigid Sraffian positions: Sraffians " seem to be putting forward their ideas as being exclusively correct and the rest of Post Keynesianism as misguided " [King (1995), p. 162]. So in Dow's opinion, the Sraffians were at fault here.[4]

The influence of methodologists was also strongly felt through the study group around Tony Lawson at Cambridge. One of Lawson's ideas is that proper economics should be based on critical or transcendental realism and " open " systems. The association between open systems and post-Keynesian economics can already be found in Brown (1981), under the expression of " multiple-exit decision situations ". In her assessment of post-Keynesian methodology in the *New Guide to Post Keynesian Economics*, Dow (2001, p. 16) claims that " Post Keynesians understand the economy as an open system ", that is, a system in which " one does not know all the relevant variables ". This is a statement that is fully endorsed by Stephen Dunn (2008, p. 42), who says that " the commitment to open-system theorising is the methodological glue that binds Post Keynesians together ". Thus whether a model is open or not becomes the litmus test to find out whether the model is compatible or not with post-Keynesian economics. Stephen Pratten (1996, p. 437) has devoted an entire article to assess whether or not Sraffian models are open systems, concluding, after having read Garegnani, Eatwell and Bharadwaj, and despite a few restrictions here and there, that " neo-Ricardianism, in its method, remains tied to closed systems ". In his final concluding remarks, Pratten (1996, p. 439), despite being a former PhD student of Harcourt, declares that as a consequence of his methodological analysis, " if Post-Keynesianism is to be a consistent project its link with neo-Ricardianism appears to be untenable ". My four-page single-line comment did not generate any substantial change in the paper, not even my argument that since the profit rate in Sraffian economics is considered to be a variable which is not determined within the core system, it is hard to see how the typical Sraffian model can be considered to be closed, a point that had also

[4] Reading the text of Paul Davidson (2003-04), devoted to getting the record straight on the history of post-Keynesianism and to a discussion of who is truly a post-Keynesian economist, one wonders who was the most rigid in arguing about the validity of one's ideas.



been made earlier by Dutt and Amadeo (1990, p. 57) when considering the separate determination of prices and outputs in the Sraffian system. In support of this view and in opposition to Pratten, Pasinetti (2005, p. 846) believes that the Sraffian model is an open theory that does not offer a unique solution, made up of objective relations that contain " many degrees of freedom that are left open ".

The " open systems " criterion to judge whether a model can be given a post-Keynesian stamp of approval has however given rise to strange attributions. In his realist appraisal of post-Keynesian pricing theory, Paul Downward (2000, p. 218) agrees with Pratten that Sraffian pricing models " are inconsistent with an open-system perspective and a critical-realist-defined post-Keynesian economics ". Downward however goes even further, claiming that while the pricing theories of Kalecki, Andrews and Means should be acceptable to post-Keynesians, those of Asimakopulos, Eichner, Fred Lee and Lavoie (and by extension the historic full-cost pricing view of Wynne Godley and the target-return pricing formula of Lanzillotti (1958)) are imbedded in a closed-system framework, and hence " cannot provide the constructive basis of an alternative account of pricing " [Downward (2000), p. 222]. In this he gets full support from Dunn (2008, p. 153). Thus, no formal modeling, if we are to listen to methodologists or to economists influenced by methodologists, passes the test and ought to be considered as part of post-Keynesian economics. Thus we are back to the debate between dominant versions of post-Keynesian economics: how far can we go in discussing extreme statements?

Methodological arguments were also in the forefront of Walters and Young's (1997) critique, pointing to the lack of inner coherence of post-Keynesian economics, arguing that for this reason it could not be considered as " an alternative school of economic thought ", in contrast to neoclassical, Austrian or Marxian economics. Again, the seven-page single-line appraisal that I sent to Walters and Young had no influence whatsoever on the thinking and the contents of the published paper. Their article brought responses from Arestis, Dunn and Sawyer (1999a, 1999b). Walters and Young (1999) point out that Arestis *et al.* (1999a) do not include Sraffians among their strands of post-Keynesianism. Still, just a few years before, Arestis and Sawyer (1993) had given a definition of what they considered to be the political economy tradition, which included Sraffian economics among its four strands (the fourth one, besides Keynes and Kalecki, being Institutionalists). However, already in Arestis's (1996) paper on the coherence of post-Keynesian economics, the Sraffian strand is gone from the definition of the major post-Keynesian strands, although it should be pointed out that Sraffian economics is very much imbedded in that article, since the section on pricing and production is mainly devoted to Leontief and Sraffian modelling.

Obviously, Arestis (1996, p. 112, fn 4) was somewhat hesitant about the room that should be devoted to Sraffian economics inside the post-Keynesian school. What he seems to reject is some rigid interpretation of long-period centres of gravitation, in other words he is willing to accept some strands of Sraffian analysis while rejecting what seems to be the dominant strand: " The assumption that there are persistent forces that drive the economy toward a normal or long-period position when the world is characterized by uncertainties, nominal contracts, and path dependency sits rather uncomfortably with the general thrust of Post Keynesian economics " [Arestis, Dunn and Sawyer (1999b), p. 544]. But this issue will be precisely the subject of the next section.

In the meantime, let us conclude with a discussion of the paper written just a year later by the co-author of Arestis and Sawyer – Stephen Dunn (2000). Making a reference to the critique of Walters and Young, Dunn tackles the relationship between Sraffians and post-Keynesians square on, and it is worth quoting him extensively:

> It is this dawning realization, that Sraffians are methodologically incompatible with other traditions within Post Keynesianism, that has led to the exclusion of the Sraffians from the core of the Post Keynesian program (see Walters and Young, 1999). The inclusion of the Sraffian School under the Post Keynesian umbrella is highly contentious, given its closed system methodology. The time has come for Post Keynesianism to formally part company with the Sraffians and to jettison the burdensome accusation of negative critique that is chiefly identified with their contribution.... The coping stone of Post Keynesianism is its commitment to open systems reasoning. As such, any attempts to develop (coherent) links to other approaches must be within this framework. [Dunn (2000), p. 350]

Dunn's diatribe did not generate any response.[5] As far as I know, this is where the debate stands in the minds of several post-Keynesian or heterodox economists: Sraffians ought to be purged out of post-Keynesian economics to achieve coherence. Sraffian theory is nowhere to be found in the *New Guide to Post Keynesian Economics* [Holt and Pressman (2001) and in a recent paper about what is post-Keynesian economics [Holt (2007)]. The methodologists have won the day. Sraffians are said to assume forces that push the economy towards a long-period position – a feature of closed system modeling as Dunn and Pratten would say – whereas post-Keynesianism is built on open-world features. This would explain the tensions that existed during the Trieste Summer School experiment, and it would explain why the tensions have never been resolved. The project to build a fruitful alternative to neoclassical economics that would be based on a synthesis of the Keynesian monetary production economy and the Sraffian surplus approach is dead. Dunn (2000, p. 350) claims that even dissident Sraffians have given it up, writing that " Roncaglia (1995) has called for the abandonment of the project to integrate Sraffian and Post Keynesian analysis ". Dow (2001, p. 18) makes an identical attribution, saying that " it has even been suggested that attempts to identify the Sraffian approach with Post Keynesianism should be discontinued ", citing the same Roncaglia (1995) paper in support of her claim.

[5] There is some irony in noting that Eatwell and Milgate (1983, p. 12) wish to develop a theory that "captures the persistent and systematic forces", while Dunn (2008, p. 35), who objects to Sraffian analysis, proposes to "move beyond surface phenomena and develop abstractions that are appropriate ... and essential". What is the difference?



But is this really the case? Has Alessandro Roncaglia thrown the towel in building bridges between Sraffians and other post-Keynesians? Is it true that the Sraffian approach is methodologically inconsistent with the rest of post-Keynesian analysis?[6] We shall give a negative answer to all these questions, arguing that Sraffian economics ought to remain within post-Keynesian economics.

## THE CASE FOR AN INTEGRATION OF SRAFFIAN ECONOMICS WITH THE REST OF POST-KEYNESIAN ECONOMICS

There are two forces at work right now among heterodox economists. On the one hand, there are centripetal forces. As a minority in danger, some heterodox economists try to find commonalities among the various heterodox schools. This is a search for a large-tent approach. On the other hand, the huge amount of published work and the inherent specialization associated with this intellectual output makes it more and more difficult to keep track of the work being performed in other fields. This, plus the tendency of some economists to differentiate their product and go for a small-tent approach, reinforces the centrifugal forces. As observed by Dutt and Amadeo (1990, p. 154), " there exists a natural tendency in minorities to look for their roots and find ways to establish their identities ". This tendency can

[6] One could say that Bhaduri and Robinson (1980) certainly set out to show that a Sraffian model was not incompatible with the study of the problem of the realization of profits as emphasized by Marxists and Kaleckians.

lead to a dangerous path, as can be observed in the writings of Holt (2007) and Davidson (2003-04, 2005): first one starts by excluding Sraffians, then Kaleckians are weeded out because they are overly deterministic, and finally Fundamentalist Keynesians that don't exactly fit the bill of one's preferred theory get dismissed. Where does this all end?[7]

In the present section, I will briefly recall the numerous commonalities between the theories espoused by Sraffians and other post-Keynesians. I will then tackle the crucial issues, that of the role of long-period positions and their relationship with path dependence.

## Obvious links

The relevance of keeping a tight link between Sraffian economics and the rest of post-Keynesian economics can be addressed from several angles. First, Sraffians are intimately linked with post-Keynesian analysis by tradition and by history. To exclude Sraffians would render incomprehensible part of post-Keynesian history and evolution. This is also the opinion of a long-time participant to the Trieste Summer School, when writing about post-Keynesian thought: " Increasingly, commentators tend to put the so-called neo-Ricardians in a class of their own. I do not agree with this tendency. The Sraffian critique of neoclassical economics and Sraffa's positive contributions are integral parts of both the historical and the logical developments " [Harcourt (2001), p. 275]. Indeed, Tiago Mata has argued in great detail that the Cambridge capital controversies, closely associated with the Sraffian approach, were a defining moment in the construction of the post-Keynesian identity. He identifies " the 1960s capital controversies as the point where Post Keynesians part ways with the mainstream and emerge as a separate body " [Mata (2004), p. 257]. In addition, Trigg (2008, p. 133) contends that " post-Keynesians can demonstrate the strength and the generality of their approach by embracing Sraffian multisectoral foundations ".

Second, there is some wide agreement about policy issues and the need for government intervention. Fontana and Gerrard (2006, p. 51) present what they call the " three interconnected characteristic Keynesian propositions ": there can be involuntary unemployment; output

[7] The tendency for exclusion can hit all schools. At a meeting of the History of Economics Society held in Toronto some 20 years ago, Garegnani confided to a group of us that, retrospectively, he wished the journal that he had edited, *Political Economy: Studies in the Surplus Approach*, had been less intransigent in its choices.



and employment variations play the key role in macro adjustments; economic policy is effective and will stabilize the economy. Certainly, Sraffians would agree with all three of these key Keynesian propositions.

Third, as pointed out a long time ago by Dutt and Amadeo (1990), and this is why they call them neo-Ricardian Keynesians, Sraffians are in close agreement with other post-Keynesians on crucial issues such as the causality between investment and saving, the role and importance of the principle of effective demand both in the short and the long run, the importance of money and credit in allowing effective demand to break the stranglehold of saving [Garegnani (1983), p. 78], the endogeneity of the money supply and the possibility for the central bank to set short-term interest rates at levels of their choice – the rate of interest being essentially a convention enforced by the central bank. All post-Keynesians attach great importance to income distribution and its effect on effective demand, economic activity and financial instability. The concept of capital, as viewed by Sraffians and post-Keynesian authors such as Kaldor and Joan Robinson, and even Harrod, is also remarkably similar, as shown by Rymes (1971). Kaleckian growth models also seem to derive some acceptance from Sraffian authors, if one sets aside the precise form of the investment function. I have tried to demonstrate all this in previous work [Lavoie (1992a, 1992b, 2003, 2006)].

Although this may be somewhat contentious (see Blankenburg (2011) for a recent review), several authors see a tight relationship between Sraffian prices and administered pricing, as found in cost-plus pricing or benchmark pricing [Earl (1983), p. 30; Nell (1998), p. 394]. Sraffian price theory can be also seen as an idealized administered pricing theory, which abstracts from imperfect information, past disequilibria, non-uniform profit rates or target rates of return, debt structures, etc. Those who are interested in the study of relative prices can then introduce these complications at will. Finally, it is often claimed that Sraffians do not take into account financial and monetary factors. But what has been the contribution of the other post-Keynesians in this regard, when analyzing pricing and relative prices? The Sraffians were the first to claim that relative prices and real wages are being affected by the trend level of the rate of interest, through its proportional impact on the normal profit rate, that is, the target rate of return imbedded in the pricing markup [Pivetti (1985), (Panico) 1985]. Furthermore, Trigg (2008, p. 137) shows that Sraffian price equations can be interpreted in terms of national accounting and thus made consistent with a monetary economy.

Except for the topic of long-period positions, to which I shall return later, it would seem that the major disagreements between Sraffians and Fundamentalist Keynesians arise more from their criticisms of neoclassical analysis than from their potential positive contributions. Sraffians and Post Keynesians seem to disagree most about the fatal flaws of mainstream theory. For Sraffians the flaws of neoclassical theory are mostly due to their adoption of continuous downward demand curves for investment or for labour, based on diminishing marginal productivity. All Sraffians (Eatwell, Garegnani, Kurz, Mongiovi, etc.) complain of Keynes because he kept an excess baggage of neoclassical theory, a complaint even made by Herbert Simon (1997, p. 14). For several other post-Keynesians, the flaws are to be found in the neoclassical school's avoidance of fundamental uncertainty, the instability of expectations, and the non-neutrality of money. Both schools agree however on the fact that flexible money wages, or the elimination of market imperfections, would not automatically bring back the economy to full employment in the long period. Indeed both schools reject the relevance and operation of the neoclassical principle of substitution. Members of both schools also generally agree on the independence of investment from savings, and on the argument that output variations provide the adjustment mechanism that equates saving and investment, rejecting the neoclassical view that variations in interest rates will bring investment to the level of full-employment saving. In addition, and this may seem quite ironic to some observers, both dominant Post Keynesians and dominant Sraffians recognize that a lack of deterministic results can prevail: Sraffians express this by referring to the lack of clear results that can be obtained outside the core (i.e., outside the determination of normal prices), while Fundamentalist post-Keynesians refer to radical uncertainty and non-ergodicity. Thus in contrast to what has been claimed repeatedly, Sraffians and other post-Keynesians are brought together more than by their dislike of neoclassical economics. This dislike, as reflected in their critique of neoclassical economics, is more a source of tension than a source of unison. Despite appearance to the contrary, Sraffians and other post-Keynesians are brought together by the similarities in their positive contributions.

Thus, in contrast to what is often claimed by methodologists, the links between Sraffians and other post-Keynesians go much beyond the unified belief that free market forces will not bring in full employment by themselves or the motto that " the enemy of my enemy is my friend ". Attempting to build a synthesis between Sraffians and other post-Keynesians is not a matter of sacrificing theoretical coherence for political coherence, in opposition to what is proclaimed by Dunn (2000, p. 350).



But if so, what then is the reason for this malaise between Sraffians and other post-Keynesians? We must now tackle the controversial notion of long-period positions – the cause of all the troubles.

## The weakest link

In my opinion, the cause of the mistrust between Sraffians and other post-Keynesians is that the dominant Sraffian strand, that of Garegnani, has been subjected to a caricature, which can be summarized by the following elements.

- Prices of production are normal prices, that is, prices that incorporate a normal profit rate, which is uniform throughout;
- Market prices, or actual prices, gravitate towards prices of production through some demand and supply mechanism;
- Persistent forces will push the economy towards long-period positions; these are long-period centres of gravitation;
- Long-period analysis is important, while short-period problems are not;
- The core of economic analysis is the study of relative prices and the distribution of the social product; the other fields are not capable of precise analysis and hence have little importance;
- Long-period positions are situations with normal prices at normal output, or with rates of capacity utilization equal to their normal level: these are also called fully-adjusted positions;
- Long-period positions are based on a trend which is determined ex ante, and which is independent of the short-period and of any short-run variable such as finance.

Reading these seven points, it is obvious why the other post-Keynesians would feel compelled to reject any possible integration of Sraffian analysis within the post-Keynesian school, as other post-Keynesians usually focus on short-period output and employment issues, question the strength of the competitive process that would bring about a uniform profit rate, argue that short and long period issues are tied together, and reject the existence of a trend independent of short-run events.

Now it must surely be the case that what I call a caricature is not entirely false. There must be some papers by some authors who have taken the position that I just described. Even observers as keen as Arena (1987) and Roncaglia (1990, 1995) seem to have accepted at least some elements of this caricature, so there must have been some statements made by members of the dominant Sraffian school that have justified this perception. I have myself argued in the past that it was better to forget the dominant strands and keep track only of the dissident strands in attempting to build a synthesis between the Sraffians and the other post-Keynesians [Lavoie (1992a, 1992b)]. I have now partially reconsidered this view. I am now of the opinion that there is only one remaining obstacle to the achievement of a synthesis between the Sraffian approach, including the Garegnani dominant Sraffian strand, and the other post-Keynesian schools. This obstacle is to be found in the first three of the seven statements given above that pertain to caricature the dominant Sraffian strand. I believe that the last four statements are misrepresentations. Let us start by examining the first three statements, those that are problematic for the synthesis.

Garegnani's followers do believe that competitive forces bring about a tendency towards an equality of normal profit rates throughout industries, and that such a tendency operates through a reaction of produced quantities to discrepancies between the actual profit rate and the normal profit rate. Now, as summed up by King (1995, p. 246), there is " little enthusiasm for any notion of long-period " prices of production' as centres of gravitation towards which short-period or market prices are supposed to tend. The unreconstructed " neo-Ricardians ", of whom Pierangelo Garegnani is the most resolute example, are increasingly isolated on this question ".

Classical economists believed that market prices converged towards production prices, and so did the Sraffians such as Garegnani. It took some time for conditional proofs of such a process to be put forward. Duménil and Lévy (1990) have presented a neat summary of the various kinds of convergence processes that have been proposed. The first convergence models, proposed mainly by French authors, assumed that market prices in a given period were clearing prices, prices that equated demand to the given supply of the period. The discrepancy between the market price and the natural price, or rather between the actual profit rate and the uniform normal profit rate, then generated capital mobility and hence quantity adjustments. As these modern " classical " models of convergence sounded rather walrasian, they were progressively replaced by cross-dual models, with clearing prices being replaced by prices being based on their value in the previous period and on the observed disequilibria in quantities (excess demand).



These cross-dual models were then supplemented with various mechanisms, including a Keynesian increase in output in the case of excess demand.

The problem with these convergence models is that they undermine their own foundations. This is pointed out by Bertram Schefold (1984, p. 1), who argues that " classical economists sometimes adopt neo-classical conceptual tools for the analysis of supply and demand in individual markets (e.g. in the analysis of the gravitation of market prices towards prices of production) and are then led towards models of at least superficial similarity with neo-classical general equilibrium in consequence of the attempt to analyse the interdependence of the various markets ". It follows that " some of the work by modern classical economists on the convergence of market prices towards prices of production tends to undermine its own foundations by generating systems in which supply and demand are dominating forces " [ibid, p. 2]. This is quite obvious with convergence models based on clearing market prices, while non-supplemented cross-dual models must rely on substitution effects in demand to achieve convergence – a rather ironic feature. These criticisms are also made by Boggio (1986, p. 84), when he mentions that " the whole disequilibrium process " of classical convergence models is " dominated by price reactions to excess demand and by consumption reactions to price changes ", as it would in mainstream models. In addition, Boggio (1990, p. 56) says, several of the models assume that economic agents know the long-run or equilibrium values of prices and quantities, a rather unKeynesian assumption since this implies that the long-run position is known before actual short-period values occur. These formalized models also imply that quantities are brought back to their " normal " levels or to their normal rate of capacity utilization. Finally, convergence is always conditional.

Thus, if one wishes to connect Sraffian economics with the other strands of post-Keynesian economics, one needs to examine production prices in a different light, not as long-run or long-period centres of gravitation to which market prices tend. This alternative view, as I see it, is tied to the contributions of Adriano Boggio (1980, 1986, 1990, 1992). His relative price models are full-cost models. He assumes that firms set output prices on the basis of wage costs and the commodity prices of the previous period, with a markup designed to achieve an exogenously given target rate of return (the normal profit rate).[8] These models are very robust, in the sense that they converge to a steady set

[8] See also Nisticò (2002), who seems to endorse similar views.

of relative prices without the need to impose nearly any restriction. One can assume from the start that the target rate of return is the same in each sector; or we can assume differential target rates of return, in which case one might wish to add a slow reaction process, perhaps akin to the excess demand mechanism described by the Sraffian dominant strand, that would explain the evolution of these target rates (Boggio 1986).

In my opinion, Boggio's full cost prices are cousins of Roncaglia's interpretation of Sraffa's prices. For Roncaglia (1995, p. 114), " Sraffa's 'outputs' should not be identified with those actually observed at any point in the historical development of the economy ". Instead, costs ought to be computed, " not for current output levels, but for a 'normal' degree of capacity utilisation " [ibid, p. 115]. This is consistent with statement of Joan Robinson (1978, p. 16) when she says that " each firm is assumed to reckon its costs on the basis of a standard ratio of utilization of its plant ". It is also consistent with the notion of target return pricing, as described by Lanzillotti (1958) following his survey of pricing practices of large firms, whereby unit costs are assessed on the basis of some standard rate of capacity utilization which is only roughly related to actual rates of capacity utilization. Some two-sector models, similar in their structure to simple Sraffian models, with a consumption-good and an investment-good sector, have been built on this basis, making use of full-cost pricing with target rates of return and standard rates of capacity utilization, analyzing the traverse towards new long-period positions [Lavoie and Ramirez-Gaston (1997), Kim (2006)].[9]

There is thus a need to reconsider what are production prices, adopting a point of view which is closer to that of the dissident Sraffian strands. For authors such as Pasinetti, Roncaglia or Schefold, in a sense, production prices arise both in the short and in the long period. Prices set on the markets by oligopolistic firms are *quasi* production prices [Lavoie (1987), p. 111]. Leading firms administer prices, taking into account costs assessed at the normal rate of capacity utilization, with some target rate of return. Inequalities between supply and demand are mainly resolved through changes in stocks or in the rate of utilization, not by changes in market prices. Of course, in the real world, administered prices are not exactly equal to prices of production. But the fact that they are not equal has to do with various frictions

[9] In an email that was sent to me, Boggio told me that he considered these models to be the quantity equivalent of his models that focused on changes in relative prices.



(incorrect information, past disequilibria, non-unique prices, debt structures, abnormal rates of utilization of capacity, differentiated profit rates, incompatible claims on the social product, etc.), rather than the discrepancy between supply and demand. Actual prices are not market prices which would clear out excess demand at each period [Arena (1987), p. 105]. Actual prices are imperfect prices of production, the level of which is administered by firms according to normal costs. If one avoids the so-called process of gravitation, it then becomes clear that production prices and cost-plus prices are compatible with each other and part of the same conceptual framework. The following quote by a well-known Sraffian tries to make the point:

> The mark-up on unit costs will have to be such that normal profits corresponding to the prevailing rate of profit are obtained at the normal level of capacity utilisation. This approach allows to give a rationale for the rule of full cost pricing, but only in a very simple case. The merit of this application is to clarify the conditions under which full cost pricing is consistent with a given rate of profit in a classical long period position (Or, in an obvious extension, there may be a hierarchy of such rates of profit ...). One may say that normal prices are here calculated on the basis of a given normal utilization of capacity, and that changes of capacity are used to adapt supply to demand at unchanged prices .... Actual prices are therefore equal to prices of production but utilization fluctuates around a normal level [Schefold (1984), p. 4].

A similar interpretation was offered by John Hicks (1990, p. 102) when he argued that Sraffa's prices were " based on costs, not of actual outputs but of 'normal' outputs ". This, according to Hicks, helped to explain the mystery of the " (apparently uniform) rate of profit in Sraffa's system ", which had to be considered " as a mark-up, established by convention " [ibid, p. 100]. Hicks (1985, p. 306) made an earlier similar statement: Sraffa's prices " seem to be prices which are set upon products, by their producers, according to some rule. Now it is perfectly true that we are nowadays familiar with that method of prices-fixing, by 'mark-up'; but when that method is used, the rate of profit that is used to establish the mark-up is conventional. Now it may be that Sraffa wants us to think of his rate of profit as being conventional; and that the uniformity of the rate of profit throughout his system, of which he makes so much, is just a uniformity of conventions ".[10] Thus here Hicks interprets Sraffian prices as cost-plus prices, where adjustments occur through overutilization or underutilization of capacity.

[10] This quote is taken from Sinha and Dupertuis (2009, p. 496), who argue strongly against the concept of a center of gravitation.

## Misrepresentations versus corrected statements

The conception of production prices – as represented by the first three points mentioned above – is, in my view, the only remaining obstacle in the attempt to integrate Sraffian and Keynesian analyses. The other four points which are said to characterize the dominant Sraffian strand are being incorrectly attributed to Garegnani and his followers, and hence they cannot be an obstacle to the synthesis. The last four statements that truly represent Garegnani's views and those of (most) Sraffians, are instead as follows:

- Both long-period and short period analyses are important;
- Elements outside of the core of relative prices are just as important, perhaps even more important for economics;
- Long-period positions are situations with normal prices, computed on the basis of some normal output; in general, actual output will not be equal to normal output, or rates of capacity utilization will not be equal to their normal level; fully-adjusted positions will not be achieved;
- The trend that describes long-period positions is determined ex post, and is dependent on actual short-run sales and actual rates of capacity utilization; in other words, there is path dependence.

Let us start with the easiest point, the claim that long-period analysis is important while short-period problems are not. This is a claim that was first made by Fernando Carvalho (1984-85, p. 220) when he wrote that " for Post Keynesians such as Garegnani and Eatwell the only objects of economic analysis are the 'long run positions' of the system. Short run behaviours are not considered amenable to analysis, and therefore, are irrelevant ". Now Eatwell and Milgate (1983, p. 12) are careful to point out that their " arguments should not be mistaken for the advocacy of what has been referred as ' long-period analysis' to the exclusion of everything else ". Similarly, Garegnani (1988, p. 252) makes clear, in a footnote which constitutes an explicit response to Carvalho, that he does not " in the least believe " that long-run positions ought to be the only object of analysis and that short-run theory is irrelevant ". If this is true, the current emphasis on stock-flow consistency put forth by post-Keynesians enamoured with the work of Wynne Godley shows some convergence. It becomes ever more recognized that post-Keynesian analysis has to go beyond the short period, and beyond the Marshallian/Keynesian method of looking at parts of the economy in sequence, abstracting from what is going on elsewhere [Dos Santos (2006)]. Although present stock-flow consistent models



(Godley and Lavoie 2007) focus primarily on monetary and financial issues, they clearly show that the short-period is not the exclusive focus of the other post-Keynesian strands.

Similarly, the notion of a core of economic analysis, devoted to relative prices and value theory, has generated a lot of anxiety among post-Keynesians, even Dutt and Amadeo (1990, p. 165) who are rather sympathetic to the Sraffian views. Garegnani's argument is that, as mentioned above by Carvalho, he believes that matters related to the theory of value are more amenable to a general analysis and are closer to reality than are analyses deployed outside the core, such as in the determination of employment or the rate of accumulation. Pratten (1996) is quite critical of this distinction, although he recognizes that Garegnani is careful to point out that fields outside of his core are just as important and relevant. Roncaglia (1990, p. 145) is also annoyed by the distinction, arguing that the " dichotomy between the 'core' and the rest of economic analysis, as stated by Garegnani, goes too far ", adding that the relations studied in the core should not be considered to be superior or necessary to other kinds of analyses. Garegnani (1990, p. 151) confirms that he does not believe that the analysis of the core is in any sense superior or causal to the other fields. But he hangs on to his belief that the analysis of relative prices requires less assumptions to be realistic than the analysis of the Keynesian multiplier for instance. Well, this is an idiosyncratic belief, and I do not see how, by itself, it could hold up the creation of a Sraffa-Keynes synthesis.

To the above two points, the importance of the core and of long-period analysis, post-Keynesians critical of Sraffian economics usually add a host of complaints about the lack of interest shown by Sraffians with regards to monetary variables, finance, liquidity preference, uncertainty, and so on. This is certainly the case, although some Sraffians have devoted quite a lot of space to monetary relations or have tackled differentials in interest rates, with these analyses being quite compatible with other post-Keynesian work. But it is rather difficult to confront all problems at once. The same complaints are voiced by Fundamentalist Keynesians against Kaleckians. What is important, for instance in the case of fundamental uncertainty, is to recognize that the agents of the models cannot be granted a capacity to foretell the future or to know equilibrium values.

We now move on to what I consider to be the most two important caricatures. First, does the dominant Sraffian strand believe that long-run positions coincide with actual output being equal to normal output; or in other words, do Sraffians assume that there is gravitation towards normal rates of capacity utilization? Second, does the dominant Sraffian strand believe that long-period positions are based on a trend which is determined ex ante, and which is independent of the short-period and of any short-run variable? My answer is a clear no to both questions. However, one may wonder why so many people seem to have been fooled into believing that the answers are positive.

As pointed out earlier in the discussion of the gravitation towards prices of production, several models of convergence do entertain the result that in the long run actual output equates normal output. So this may be responsible for some of the confusion. Furthermore, Eatwell and Milgate (1983, preface) define a long-run position as a position such that " the structure of capacity has been adjusted to the structure of demand, and hence in which there is a uniform rate of profit in each line of production ". This, they continue, " requires that the market for produced commodities " clear'.... ". Eatwell (1983, p. 271) elsewhere provides a similar definition: " Long-period positions of the economy are defined with respect to the uniformity of the general rate of profit and ... requires that the scale and composition of output and the size and composition of capacity are adjusted one to the other. Thus the long-run position of the economy must embody a conception of the relation between output and capacity as a corollary of the conception of the normal relation between prices and distribution ". Eatwell (1998, p. 599) is even more precise in his later writings, despite his current interest in monetary economics and financial instability, writing that " a long-period *normal* analysis of the formation of natural prices must be accompanied by a long-period *normal* analysis of output ", meaning that " the theory of output must be the natural, or normal, level of output, itself the centre of gravitation of the transitory forces which affect output at any time ". Thus, it is possible to attribute to Eatwell the belief that Sraffian prices are associated with normal outputs or with normal rates of capacity utilization.

If these statements are still unconvincing, Vianello (1985, p. 71) in his critique of Joan Robinson's model of accumulation, provides a clear definition of long-period positions. He defines " fully adjusted situations " as positions such that " productive capacity is normally utilised and a uniform rate of profits prevails ". He then associates fully-adjusted positions to the normal degree of utilisation of capacity. Other Sraffians such as Committeri (1986) and Serrano (1995) have also insisted that long-period positions require actual rates of capacity utilization to equate their normal or target value. Indeed, the necessary association of long-period equilibrium with the realization of the normal rate of capacity utilization can be found primarily among authors with a Marxist background, such as Duménil and Lévy (1993) and Anwar Shaikh (2009) and even among some post-Keynesians such as Peter Skott (2010). In any case, several observers, even friendly ones, are convinced that long-run Sraffian positions are fully-adjusted positions, with actual output being equal to normal output. Sraffians would presume " that fluctuations in capacity utilization tend to converge or gravitate to the long-period positions of normal capacity utilization " [Lee and Jo (forthcoming), p. 20].



But is this the view of Garegnani and of the other members of the dominant Sraffian strand? The answer is that it is not. Vianello (1989) himself did backtrack, arguing that he agreed with Ciccone (1986), that is, he agreed that actual rates of capacity utilization may diverge for long periods of time from their normal level, even though actual prices may be equal or very close to production prices. Thus, according to Ciccone, realized profit rates can diverge from normal profit rates over long periods of time. There is thus a great degree of similarity with the arguments advanced by Kaleckian economists, who claim that the actual rate of capacity utilization is endogenous even in the long run (Hein, Lavoie and van Treeck 2010, 2011). The main difference is that Kaleckians contend that the actual profit rate would have an impact on the rate of accumulation set by entrepreneurs, whereas Sraffians maintain that there is no such role, insisting instead that the normal rate of profit is the key variable determining investment in new capital.

This argument is modelled by Kurz (1991). In an otherwise Kaleckian model, his investment function depends on the rate of capacity utilization and the real wage rate obtained by workers. Seen from another angle, it implies that accumulation is speeded up by a higher normal rate of profit. This is very similar to the famous Bhaduri and Marglin (1990) model, where accumulation depends on capacity utilization and the share of profits. Both models obtain a rich range of possible growth regimes. But the key point here is that in Kurz's model, the actual rate of capacity utilization does not converge to its normal value. Kurz (1994: 408) is very clear about this: " There is no reason to presume that productive capacity will be exactly utilized at that level which, in conditions of free competition, cost-minimizing producers desire to realize and which will be called the " normal degree of utilization' ". And later he writes: " It is virtually impossible for the investment-saving mechanism, as it is conceptualized in this section along non orthodox lines, to result in an optimal degree of capacity utilization. It is, rather to be expected, that the economy will generally exhibit smaller or larger margins of unutilized capacity over and above the difference between full and optimal capacity " [Kurz (1994), p. 414]. In other words, the actual degree of capacity utilization is likely to be different from its normal (optimal) level.

It is interesting to note that in the French version of his paper, Kurz (1993, p. 69) mentions that his work was inspired by a paper first drafted by Garegnani (1992) for the Trieste Summer School in 1982. It would then seem to follow that Garegnani himself would endorse the notion that long-period positions are not characterized by normal rates of capacity utilization, and that this was a position that he held as early as 1982. And indeed this is what Garegnani (1992, p. 59) says: " Even correct foresight of future output will not eliminate average capacity at levels other than the desired one ". And indeed, the fact that dominant Sraffians do not assume fully-adjusted positions is confirmed in various later articles [Garegnani and Palumbo (1999); Palumbo and Trezzini (2003); Trezzini (1998)].

There is some irony in noting that Ciccone's and Vianello's papers started out as a critique of Robinson's *Accumulation of Capital* while Kurz's paper was first designed as a critique of Kaldor's income distribution mechanism. All these papers make the case that there was something odd with the growth models of these Cambridge Keynesian authors, as they seemed to assume that demand variations would induce output changes in the short-run while also assuming that the economy would run at full or normal use of capacity in the long run. In other words, it is Kaldor and Robinson that used the notion of fully-adjusted positions in their models, not the modern Sraffians! Vianello (1985) in particular was very much concerned about how Robinson attempted to describe the traverse leading from some short-run change in the degree of utilization towards the new fully adjusted position in the standard two-sector model with a consumption good and an investment good. Indeed, the traverse is nowhere to be found in Robinson!

Despite all this, the Sraffian approach is still interpreted by many post-Keynesians as assuming that long-period positions are associated with fully-adjusted positions. For instance, Halevi and Kriesler (1991, p. 86) write that " the neo-Ricardians argue that ... variations in the degree of capacity utilization are seen to occur only in the short run. However, to maintain this position, they must postulate a long-run adjustment of capacity to demand so that the actual rate of utilization tends towards the desired one ". This, they continue, is unacceptable " until some coherent dynamic adjustment process is specified which can describe the " traverse' from one equilibrium position to another, without the traverse itself influencing the final equilibrium position, that is, without the equilibrium being path determined ".



This statement introduces the last point of caricature, as it implies that Sraffians in general, or at least those of the dominant Garegnani strand, reject the possibility of path-dependence, believing that long-period positions are based on a trend which is determined ex ante, and which is independent of the short-period and of any short-run variable. The belief that Sraffians reject path dependence is widespread. Chick (1995, p. 27) for instance writes that " the system is path-dependent, in sharp contrast to neoRicardian theory ". Similarly, Dutt and Amadeo (1990, p. 157) write that the neo-Ricardian approach " analyses long-period position *independently* of the short-period behaviour of the economy ". This is contrasted to the standard post-Keynesian view, which assumes that " the long period is not independent of the short period ", meaning either that " the long-run trend is but a slowly changing component of a chain of short-period situations " [Kalecki (1971), p. 165] or that long-run determinants of growth and capacity creation are influenced by short-run events. This point has been made repeatedly by Asimakopulos (1983, 1988) in his encounters with Sraffian authors.

Now, at least as early as 1981, at the Udine conference, Garegani (1983, p. 80) recognizes, in a footnote however, that the trend may be determined by short-run deviations from normal capacity utilization. This idea is developed however in the notes prepared for the 1982 conference, published in Garegnani (1992) and to which I have already referred. There, Garegnani describes how a demand-induced increase in the rate of capacity utilization may generate a completely different path in the values taken by future capacity and output. His usual post-Keynesian critics (Asimakopulos, Minsky) did not seem to realize that Garegnani was already moving towards the Keynesian-Kaleckian point of view, claiming in his response to Asimakopulos that " aggregate demand controls the speed of capitalist accumulation and not just the temporary underutilization of productive capacity characteristic of the trade cycle " [Garegnani (1988), p. 258]. Despite this, Asimakopulos (1988, p. 261) sees no convergence in their views, writing that " the difference with Garegnani is that he appears to see the trend as independent of the cycle ".

Mongiovi and Rühl (1993) present an enlightening illustration of what is at stake, inspired from the overly brief discussion that Garegnani (1983) provided at the Udine conference. They draw a trend line representing the evolution of output through time when this trend line is independent of cyclical fluctuations. However they recognize that short-run fluctuations may alter the data set that governs the gravitational process and create new trends. As they say, " here the trend itself is governed by fluctuations; it becomes, in other words, an ex post notion, an entity which has no existence independent of short-run fluctuations. Under these circumstances, the long- and the short-run cannot be kept analytically distinct when the development of the system over time is under discussion " [Mongiovi and Rühl (1993), p. 99].

This seems to be no different from the arguments presented by Roncaglia in favour of a Sraffian approach that would be compatible with the rest of post-Keynesian economics. Roncaglia (1995, p. 119) writes: " In general, the actual degree of capacity utilisation influences the path of productive capacity and output: whenever the current and the normal degree of capacity utilisation differ, *ex post* realised profits will be affected, and this will affect financing conditions, which in turn may affect investment expenditures, and hence the expansion of productive capacity, as well as technology...; on the other hand, the current level of investment expenditure will affect aggregate demand, and hence the current level of capacity utilisation ". Roncaglia thus provides an illustration of why short-run fluctuations may modify the long-run growth path, or in the terms of modern macroeconomics, why the natural rate of growth is likely to become endogenous to the actual rate of growth and to actual economic activity.

## CONCLUSION

Dunn (2000) and Dow (2001) have both said, like several other methodologists, that post-Keynesians ought to purge Sraffians out, both authors asserting that even long-standing advocates of a Sraffa-Keynes synthesis, such as Roncaglia (1995), had given up on this project. But what Roncaglia (1995, p. 120) has actually said is that " the interpretation of Sraffa's outputs as " long-period centres of gravitation' ... is therefore an obstacle to the integration of Sraffian and Keynesian analyses, and should be abandoned. However ... a different interpretation of the conceptual framework underlying Sraffa's analysis is possible.... A solid stream of non-neoclassical economics is already available, integrating not only Keynes's and Sraffa's analyses, but also the contributions of a wide group of economists ... ". Roncaglia does believe that Sraffians and other post-Keynesians are compatible bedfellows.



One of the lessons of the present study is that the interpretation of Sraffa's outputs as actual *normal* outputs has already been given up by all strands of the Sraffian school. There is no clash here between Sraffians and post-Keynesians: they all recognize that both in the short and in the long period, rates of capacity utilization are likely to be different from their normal level.[11] Even more surprising, many Sraffians accept the possibility of path dependence, a characteristic which other post-Keynesians take to heart as it exemplifies Joan Robinson's historical time and the presence of radical uncertainty.

What is instead at stake is whether dominant Sraffians are ready to give up the concept of the gravitation towards production prices. This in my view is the crucial issue, as recognized earlier by Frederic Lee, who, despite his sympathies with Sraffian economics and the surplus approach as well as his belief in a " Post Keynesian-Sraffian tradition ", rejects the notion of prices of production as centres of gravitation [King (1995), p. 195], even concluding some years later that " with the long period method problematical, it appears that the Sraffian social surplus approach is a dead end " [Lee and Jo (forthcoming), p. 21]. Arestis, who explicitly supports the position taken by Roncaglia that was stated at the beginning of this conclusion, is just as anxious, claiming that " once we have got rid of long-run centres of gravity, we may be able to demonstrate that Sraffian and Post Keynesian economics have much in common " [King (1995), p. 205].

I would argue that due to specialization and the outpour of literature, Sraffian and other post-Keynesian economists have grown somewhat apart since the 1990s. However, all strands of post-Keynesian economics, when dealing with similar issues, have converged towards each other. As claimed by Nell (2009, p. 18) recently, " today the distinction between them do not appear to be as sharp as they once did ". Thus efforts to provide a synthesis of the various strands of post-Keynesian economics ought to be maintained and should keep track of Sraffian economics.

11 Of course, there are some Sraffians, just as there are some other post-Keynesians, who argue that there are strong forces pushing actual rates of utilization towards their normal levels.

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