How can Marshall be still alive after Sraffa's 1925/1926 devastating critical appraisal?

ANDREA SALANTI[1]

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ABSTRACT

1. Introduction

The ultimate purpose of Sraffa (1925) and (1926) critiques of Marshall's partial equilibrium theory, according to the 1930 often quoted words of their author[2], should have been anything but the abandonment of Marshallian theory. However, one century after those Sraffa's seemingly devastating critiques, Marshall's partial equilibrium demand-and-supply-curve famous diagram[3] is still an essential part of the analysis of the functioning of a single market, together with some exercises in comparative statics, in every microeconomic textbook.

According to traditional normative approaches in the philosophy of science, such as logical empiricism or Popperian falsificationism[4], this should have prompted us to cast doubt on the alleged scientific character of economics.

Present-day epistemology, however, underwent a process of 'naturalization' (see Rysiew, 2021), which ended up influencing studies of economic methodology too. In this respect, it has been argued (Davis, 2007) that a 'third revolution' in economic methodology occurred, characterized by the tendency to leave aside any normative or prescriptive qualification of economic discourse in favour of a descriptive methodology, in other words a 'reflection

[1] *Correspondence Address*: andrea.salanti@guest.unibg.it. Paper prepared for presentation at the ASSA 2026 annual meeting. I wish to tank Marco Dardi for useful suggestions.

[2] 'I am trying to find what are the assumptions implicit in Marshall's theory; if Mr. Robertson regards them as extremely unreal, I sympathise with him. We seem to be agreed that the theory cannot be interpreted in a way which makes it logically sell-consistent and, at the same time, reconciles it with the facts it sets out to explain. Mr. Robertson's remedy is to discard mathematics, and he suggests that my remedy is to discard the facts; perhaps I ought to have explained that, in the circumstances, I think it is Marshall's theory that should be discarded.' (Sraffa 1930, p. 93)

[3] Actually, even if the diagram generally carries Marshall's name, he was not the first to make use of it, but he was nonetheless decisive in its diffusion (see Humphrey, 1992).

[4] With reference to economics see, respectively, Hutchison (1938) and Blaug (1980).

without rules' (Hands 2001). This does not necessarily imply adherence to one of the radical versions of such an attitude, with their rejection of any methodological prescription. Rather, it suffices to recognize the advantages to be gained from paying more attention to the actual scientific practices. In other words, it is enough to recognize some merits in what Wade Hands (2001, p. 132) has called 'reformist naturalism,' a perspective that 'employs science to reform epistemology; traditional epistemological questions remain the same, science simply provides a new set of answers/solutions.' After all, looking at what economists actually do may well suggest some hints about why they do that way.

It is from this perspective, therefore, that in this paper I intend to answer, at least tentatively, the question I put forward in the title. In other words, if still now the majority of economists – in teaching and research – continue to rely on partial equilibrium models there must be, after all, some (good) reasons.

2. Sraffa's 1925 and 1926 criticisms

Several appraisals of Sraffa (1925 and 1926), usually maintain that the two papers had a common purpose, that is a sharp criticism of the Marshallian theory of (perfectly) competitive markets in the long run, and that the first part of the 1926 paper summarizes the content of the 1925 one. Mainly Italian scholars have dealt with the 1925 paper, for the obvious reason of better acquaintance with the Italian language,[5], while the 1926 article in the *Economic Journal* is better known at the international level. For the scope of this paper, all things considered, I will therefore limit myself to referring to Sraffa (1926).

At first sight Sraffa's assessment of Marshall's account of competitive firms and markets points at its inner logic in order to show its weakness between the Scylla of perfect competition and the Charybdis of partial equilibrium. Actually, however, the main target of Sraffa's criticism appears to be the legitimacy of partial equilibrium analysis. Indeed, as Sraffa (1926, pp538-539, italics added) explicitly states:

> The really serious difficulties make their appearance when it is considered to what extent the supply curves based on the laws of returns satisfy the conditions necessary to enable them to be employed in the study of the equilibrium value of single commodities produced under competitive conditions. This point of view assumes that the conditions of production and demand for a commodity can be considered, in respect to small variations, as been practically independent, both in regard to each other and in relation to the supply and demand of all other commodities.[…]
> But, … the assumption becomes illegitimate, when a variation in the quantity produced by the industry under consideration sets up a force which acts directly, not merely upon

[5] See, for example, Becattini (1986), Freni (2001), Freni and Salvadori (2013), Maneschi (1986), Panico (1991), Panico and. Salvadori (1994), Roncaglia (1991), Salvadori and Signorino (2024), Signorino (2001), and Sylos Labini (1985 and 1990).

> its own costs, but also upon the costs of other industries; in such a case the conditions of the 'particular equilibrium' which it was intended to isolate are upset, and it is no longer possible, *without contradiction*, to neglect collateral effects. *It unfortunately happens that it is precisely into this latter category that the applications of the law of returns fall, in the great majority of cases.*

In order to justify such a conclusion, it must be shown that in both cases (of decreasing and increasing returns), the same type of problem arises: that is, variations (even small ones) in the quantity supplied imply non-negligible variations in the costs of firms in other industries. In the case of diminishing returns (i.e. increasing costs) this happens in all those cases in which a significant portion of a factor available in a fixed quantity[6] (or increasable, but at increasing costs) is used to produce a particular good: the consequent increase in cost will also affect all those firms in other industries which use the same factor, albeit to a variable extent given the different intensities in the use of that factor. This will also lead, due to the resulting change in relative prices, to changes in the equilibrium of other markets due to shifts in demand curves. It follows that the only case of partial equilibrium analysis compatible with the (perfect?) competition assumptions turns out to be that of a good in whose production the total available quantity of the fixed factor is employed.

About the (less controversial) case of increasing returns (i.e. decreasing costs) it is immediately noted that continuously decreasing costs for the individual company are incompatible with the competitive hypothesis since they would lead to monopoly situations (today we would say 'natural' monopoly), while decreasing costs at the industrial level would be due to external economies such as to re-propose the problem of simultaneous effects on other industries. It follows, therefore, that the only economies of scale compatible with a partial equilibrium analysis should be 'external' to the firm and 'internal' to industry: a category which does not seem to be much relevant in reality.

It could be concluded that in this way Sraffa rigorously defined the domain of Marshallian competition theory, that is, the set of cases with respect to which it is legitimate to apply the method of partial equilibria. The scope of Sraffa's criticism would then lie in having demonstrated how this set is extremely limited and in a certain sense practically empty from an empirical point of view, given the unrealism of the necessary assumptions[7]. However, that is not all. From Sraffa's perspective, this critical operation is functional to the re-proposition of an approach based on the assumption of constant costs, in which case the influence of the

[6] If the share of that factor used by the industry in question is 'small', the cost increase would be negligible (giving rise to the case of constant costs) and in any case common to all industries that use that factor

[7] Other attempts, as for instance Samuelson (1971), to reconstruct the Marshallian theory of value in a logically consistent way in order to determine the exact boundaries of its empirical domain end up reaching similar conclusions.

cost of production in determining the price would defeat the explanation of the price as resulting from symmetry of the forces of supply and demand. In the words of Sraffa (1926, pp. 540-541):

> Reduced within such restricted limits, the supply schedule with variable costs cannot claim to be a general conception applicable to normal industries; it can prove a useful instrument only in regard to such exceptional industries as can reasonably satisfy its conditions. In normal cases the cost of production of commodities produced competitively – as we are not entitled to take into consideration the causes which may make it rise or fall – must be regarded as constant in respect of small variations in the quantity produced. And so, as a simple way of approaching the problem of competitive value, the old and now obsolete theory which makes it dependent on the cost of production alone appears to hold its ground as the best available.

However, even this last conclusion is not free from problematic aspects in the context of a theory of competition, because, if the individual firm experiences constant average costs, these will be equal to the marginal costs so that the equilibrium of the firm will be undetermined. This led Shackle (1967, p. 19), somewhat going beyond the textual evidence of Sraffa's 1926 article, to argue that:

> … if we allow ourselves to speak in modern terms of perfect competition, and mean by this that prices of both product and factors to the individual firm are independent of its output, then the conclusion of Mr Sraffa's argument at this stage is the failure of perfectly competitive assumptions to show any equilibrium of the individual firm. For both the demand curve for its product and the curve relating unit cost to output would be horizontal straight lines. This indictment of the perfectly competitive assumptions is Mr. Sraffa's first objective.

Although this issue was not explicitly addressed in Sraffa (1926), some passages from Sraffa (1925) show that the author was well aware of the problem[8]. Be that as it may, in the second part of the 1926 article, the one commonly credited with having paved the way for or developed a theory of monopolistic competition, we find precisely the proposal 'to abandon the path of free competition and turn in the opposite direction, namely, towards monopoly' (Sraffa 1926, p. 542). Apart from a surprising mistake[9], promptly detected by Richard Kahn

[8] Sraffa (1925, pp. ??? in the English translation): 'A perfectly possible case is that in which the individual marginal cost is, for some or even for all the amounts of production, constant. For such amounts the marginal cost curve would coincide with the average cost curve, and within these limits the equilibrium will be indeterminate, given the definition of competition that we have followed up to now. […] Under these conditions, if the unit cost curve displays constant costs over a certain range, equilibrium will be reached at the point which corresponds to the maximum quantity that can be produced at that cost: and it will no longer be possible to allow that the curve may display constant costs throughout its length, for this would lead to monopoly on the part of the firm considered.'

[9] 'The only case in which it would be possible to speak of a general price would be that of a trade in which the productive organisation of the different undertakings was uniform, and in which their particular markets were alike as regards the nature and attachment of the customers. In that case … the general price of the product, through the independent action of a number of firms, each of which is prompted only by its individual interests, would tend to reach *the same level as that which would be fixed by a single monopolistic association in*

as early as in 1929 in the Chapter 7 of his fellowship dissertation (now available as Kahn 1989), the 1926 article puts forward a series of intuitions which would have been confirmed a few years later. As Joan Robinson acknowledges in her foreword to Robinson (1933, p. xiii of the second edition):

> Mr. Sraffa's [1926] article must be regarded as the fount from which my work flows, for the chief aim of this book is to attempt to carry out his pregnant suggestion that the whole theory of value should be treated in terms of monopoly analysis.

For the purpose of this article, however, it is a different feature which is worthy to call attention to, that is the reasons Sraffa gives in support of his proposal to 'abandon the path of free competition'. These appear to have to do not so much with theoretical concerns, but rather with considerations of an essentially empirical nature. The following quote is quite long, but it deserves to be reported in full (Sraffa 2026, pp. 542-543):

> These two points in which the theory of competition differs radically from the actual state of things which is most general are: first, the idea that the competing producer cannot deliberately affect the market prices, and that he may therefore regard it as constant whatever the quantity of goods which he individually may throw on the market; second, the idea that each competing producer necessarily produces normally in circumstances of individual increasing costs. Everyday experience shows that a very large number of undertakings – and the majority of those which produce manufactured consumers' goods – work under conditions of individual diminishing costs. Almost any producer of such goods, if he could rely upon the market in which he sells his products being prepared to take any quantity of them from him at the current price, without any trouble on his part except that of producing them, would extend his business enormously. […]
> Business men, who regard themselves as being subject to competitive conditions, would consider absurd the assertion that the limit to their production is to be found in the internal conditions of production in their firm, which do not permit the production of a greater quantity without an increase in cost. The chief obstacle against which they have to contend when they want gradually to increase their production does not lie in the cost of production – which, indeed, generally favours them in that direction – but in the difficulty of selling the larger quantity of goods without reducing the price, or without having to face increased marketing expenses.

Passages like these undoubtedly bring some support to Kurz and Salvadori (2011) who, contra Blaug (2009), deny the existence of a trade-off between rigor and relevance inside Sraffa's implicit method (and within economics in general, for that matter). They may be right in maintaining that Sraffa would not have tolerated any trade-off between logical rigor and empirical relevance, and that he would have been nonetheless equally uneasy with a theory divorced from 'facts'[10]. Indeed, his famous criticism of Marshallian economics can be read as an attempt to reconstruct in a logically consistent way Marshallian partial equilibrium models,

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accordance with the ordinary principles of monopoly. This result, far from being conditioned by the existence of an almost complete isolation of the individual markets, requires only a very slight degree of preference for a particular firm in each of the groups of customers. (Sraffa 1926, p. 549, italics added)

[10] See also, in this respect, Signorino (2000), Kurz and Salvadori (2005), Salvadori and Signorino (2007).

in order to single out the logically admissible accounts of empirical situations to which those models could be applied and those situations to which they could not.

What is troublesome is that, when reading the 1925/26 articles in their parts devoted to reconstructing the logically admissible cases in partial equilibrium analysis à la Marshall, it is difficult for the reader to escape the conclusion that there actually is a trade-off between logical correctness and empirical importance (given the resulting tininess of the empirical domain of such admissible cases). Whether this can be said or not of the entire body of economic theory is a question that goes beyond the scope of this paper; in this respect, let refer the reader to Salanti (2014).

3. Sraffa's criticisms disappeared

As a matter of fact, however, if we search for accounts of Sraffa's 1925/1926 criticisms in modern textbook on microeconomics, we virtually find no trace of them. Actually, as we all know, since the pioneer of modern textbooks in economics (*i.e.* Samuelson 1948), Marshallian partial equilibrium analysis of a single market, together with the connected series of exercises in comparative statics, has become part and parcel of any introductory or intermediate manual dealing with microeconomics. Usually such a choice is somewhat taken for granted with no particular justification being offered to the reader. At most, we may find some words of caution about the necessary assumption of some kind of *ceteris paribus* clause.

If such a position could even be tolerated for this kind of 'literature', given its intended purposes, one might well expect to find more accurate accounts of this matter in advanced textbooks, but such an expectation would be doomed to remain unfulfilled. Indeed, in spite of their different purposes, and apart from one notable exception, even in these more challenging expositions of advanced microeconomic theory we find, more or less, the same kind of attitude[11].

As far as the notion of 'partial equilibrium' is concerned, we may find either no specific attention devoted to it (as in Varian 1992[12], Jehle and Reny 2011, and Saha 2025), or some

[11] In other words, as Freni Salvadori (2013, p. 191) have complained: '[…] almost all microeconomics textbooks portray the Marshallian partial equilibrium model of competitive markets as the benchmark model. Moreover, the same textbooks either fail to mention Sraffa's criticism (a criticism, it is worth stressing, that has never been refuted), or devote to it just a brief footnote or a special appendix as if it were a *curiosum* in which only the most pedantic students could be interested (as an example consider Kreps, 1990, Section 3 of Chapter VIII, where Sraffa is not even mentioned as the author of the criticism).

[12] For this authors it seems to be enough to alert the reader that 'The single-market story described above is a partial equilibrium model in that all prices other than the price of the good being studied are assumed to remain fixed. In the general equilibrium model *all* prices are variable, and equilibrium requires that *all* markets clear. Thus, general equilibrium theory takes account of all of the interactions between markets, as well as the functioning of the individual markets.' (Varian 1992, p. 313).

meagre portrayal of it. We come across, for instance, the following accounts:

> [O]n partial equilibrium, we implicitly assume that (1) such a good represents a small proportion of the economy, which guarantees that changes in the price of that good do not significantly affect equilibrium conditions in markets for other goods, and (2) the budget share that individuals spend on the good we analyze is relatively minor, and thus its wealth effects are negligible (which allows us to use the change in consumer surplus as a relatively accurate measure of welfare change). (Munoz-Garcia 2017, p. 411)

> The partial equilibrium approach, which originated in Marshall (1920), envisions the market for a single good (or group of goods) for which each consumer's expenditure constitutes only a small portion of his overall budget. When this is so, it is reasonable to assume that changes in the market for this good will leave the prices of all other commodities approximately unaffected and that there will be, in addition, negligible wealth effects in the market under study. (Mas-Colell, Whinston and Green 1995, p. 311)

Note that problems with partial equilibrium, according to both of these two authors, mainly arise from the substitution and income effects as involved in the standard theory of consumer, while there is no mention at all of the supply side and of what is entailed by the assumption of constant factor prices.

The only advanced textbook which I have found devoting more attention to the pitfalls of partial equilibrium analysis is Kreps (1990) where in section 8.3, titled ‘What's wrong with partial equilibrium analysis?’ on pages 279-283, the reader is made aware that

> [I]n a partial equilibrium analysis … we hold ‘fixed’ various things left out of the model. It is typical in a partial equilibrium analysis of the market in a single product to say that one is holding fixed the prices of all commodities not in the market. [...] And it would be typical to analyze how demand for the commodity in question changes, holding fixed the price of other commodities and holding fixed the incomes the consumers have to spend. […]
> If you are analyzing the market in a commodity where changes in its price (and the corresponding level of demands) won't much change the prices of other goods, then a demand curve computed under the hypothesis that other prices don't change at all won't be too far wrong. But unless this condition holds for the commodity you are interested in, you will want to think about whether there might be other prices that move substantially as the price of the good in question moves, with feedback to the demand for the good in question. If other prices can reasonably be expected to move with movements in the price of the good in question, then you must analyze more than the single market, if reasonable predictions are to emerge.
> The same sort of consideration arises on the supply side of the market.[…] We might think to justify this assumption [that the prices of factors of production don't change] by appealing to the notion that many firms are in the industry, and so each is a price taker in all the factor markets. But this is no justification at all. As we change industry supply levels, we aren't looking at changes in demand for a given factor caused by a single firm; instead we are looking at changes in demand for the factor caused by changes in the activities of many firms in the industry. If this factor of production is used by many firms in the industry, if demand for this factor by firms in the industry is a large part of the demand for the factor, and if supply of the factor is not perfectly elastic, then it is wrong to build *industry* supply curves based on the supposition that the prices of the factors don't change with changes in the level of *industry* supply. (Kreps 1990, pp. 279-280, italics in the original).

Kreps (1990) is also the most accurate concerning the supply curve of a competitive industry in the long run. In addition to the standard result of an infinitely elastic supply curve, which is obtained assuming increasing cost for the single firm, all other prices given and free entry[13], the author provides an interesting example of a rising long-run supply curve, due to the absence of free entry in the industry producing one of the two inputs.

4. A tentative answer in guise of conclusion

The time has come to attempt to provide some kind of response to the question posed in the title of this paper. Answering a different, but tightly intertwined, question might provide us with a hint: what would we be forced to give up if we accepted Sraffa's negative assessment of the Marshallian theory in its entirety?

I put the question in these terms because we know, if only from Kuhn (1970, p. 77), that:

> As in manufacture so in science – retooling is an extravagance to be reserved for the occasion that demands it. The significance of crises is the indication they provide that an occasion for retooling has arrived. […]These hint what our later examination of paradigm rejection will disclose more fully: once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature.

In this respect the problem is not so much the notion of perfect competition, which we all know is a completely unrealistic representation of real markets. Indeed, the literature that has followed about imperfect (or monopolistic, if you will) competition and oligopoly tends to provide more realistic representations of actual markets. However, the notion of perfect competition, as an ideal situation, is still useful, as it serves as a benchmark against which to measure variations in welfare/efficiency compared to Pareto-optimal situations[14].

What would be very difficult, if not impossible, is to replace the partial equilibrium method. Indeed, *the answer to the question posed in the title of this paper could be, in a nutshell, that we cannot do without the partial equilibrium method*, and we cannot do without it for a number of reasons. In a way, this is more true today than, say, fifty years ago, due to changes both in the construction of economic theories[15] *and* in the field of applied economics[16].

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[13] Cf. Kreps (1990, pp.267-279). The same result is reported, with more or less details, in Varian (1992, pp. 220-221), Mas-Colell, Whinston and Green (1995, 334-341), Jehle and Reny (2011, 168-170), and Saha, (2025, pp. 266-267).

[14] A propos of welfare measurement in terms of Marshallian surplus(es), note that it allows, in a partial equilibrium setting, an immediately intuitive evaluation – for instance – of the welfare effects of government interventions such as taxes, subsidies, tariffs, price controls, and so on.

[15] In Akerlof (2003: pp. 1-2, italics added), for instance, we find the following open acknowledgment of such a change of attitude: 'At the beginning of the 1960s, standard microeconomic theory was overwhelmingly based upon the perfectly competitive general equilibrium model. By the 1990s the study of this model was just one branch of economic theory. Then, standard papers in economic theory were in a very different style from now,

Concerning the former, as Rodrik (2015) has forcefully maintained, the belief that there is not a 'right model', but rather many potentially useful context-specific models among which economists must choose the most fitted to the task at hand, has become quite widespread. In a sense, the strength of economics would definitely reside in her wide collection of models, which allows its practitioners to deal with a broad array of economic observable fact. This implies the abandonment of any falsificationist rhetoric just because 'facts' are no more regarded as potential falsifiers of 'theories' which are meant to 'explain' them. Rather they are seen as possible sources of suggestions for building new models. Rodrik (2015, p. 67), goes as far as to say that:

> In economics, context is all. What is true of one setting need not be true of another. […] This reliance on multiple models does not reflect the inadequacy of our models; it reflects the contingency of social life. Knowledge accumulates in economics not vertically, with better models replacing worse ones, but horizontally, with newer models explaining aspects of social outcomes that were unaddressed earlier. Fresh models don't really replace older ones. They bring in a new dimension that may be more relevant in some settings.

This may happen because economic systems are continuously changing over time, but also for the reason – suggested by the recurring comparisons with Newtonian mechanics or other branches of natural sciences – that in economics (and in other social sciences, for that matter) we never got something equivalent to the (parallelogram) rule for calculating the resultant of different forces acting together on an object, so that every new 'friction' or novel situation necessitates a new model for appropriately dealing with it. Consequently, we must rely, according to the various circumstances, upon many different *ceteris paribus* clauses[17]. Take, for example, the notion of equilibrium in a perfectly competitive market: of course all of us immediately think of the usual 'market clearing' definition, but if we introduce the assumption of asymmetric information about the quality of the traded good, what happens?

Given the previously referred change of attitude among economists, we might wonder which part of Marshall's original vision stands out compared to Sraffa's strictures of a century ago. To paraphrase a statement by Milton Friedman, which is usually remembered in his first part

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where *economic models are tailored to specific markets and specific situations*. In this new style, economic theory is not just the exploration of deviations from the single model of perfect competition. Instead, in this new style, the economic model is customized to describe the salient features of reality that describe the special problem under consideration.'

[16] Cf. Backhouse and Cherrier (2017).

[17] It is unlikely that we will discover 'economic laws' with the same degree of generality as we may find in the so-called hard sciences. As pointed out by Sutton (2000, p. 4): 'Why, Marshall asked, should the 'laws of economics' be less predictable and precise in their workings than the law of physics (contrary to Hume's confidence)? The key of his answer lies in his claim that economic mechanisms work out their influences against a messy background of complicated factors, so that the most we can expect of economic analysis is that it captures the 'tendencies' induced by changes in this or that factor.'

only[18], one might say that we are all Marshallians now, but at the same time no one is any longer Marshallian. *Pace* Sraffa's 1925/1926 critical appraisals, the current literature is full of partial equilibrium analyses of short and long run both in theoretical models and in applied research. On the other hand, in partial acceptance of Sraffa's 1930 criticisms, the notion of a representative firm and the related worries about increasing returns have been completely abandoned.

Finally, let me note that the intention of some advocates of a Marshallian attitude seems, in general, to be their willingness to escape what Josef Steindl (1965, p. 18), for example, reports as 'an old epigram of Professor Kalecki in his characteristic vein (quoted without permission): 'Economics consists of theoretical laws which nobody has verified and of empirical laws which nobody can explain'.' As an antidote to such a somewhat cynical perspective, the following passages from (Becattini 1986, p. 56, my translation) may well serve as a final remark:

> It depends on what we are looking for. If we seek a thought technique that - despite its recognized historicity and therefore 'compromise' with the world that produced It - helps us to untangle… the social phenomena that we face today, in order to make the fewer possible mistakes in our conscious interventions, then it is to Marshall-Keynes (the pairing must be taken with all the necessary qualifications) that we must, for the moment, turn. If, on the other hand, our thirst is not quenched except with truths that 'have the nature of mathematics', as Ricardo writes, it is to Sraffa (and not only to him, of course) that we must turn. But these are two different things and it is not helpful to mix them up.[19,20]

[18] Friedman (1997, p. 6): 'Some years ago, I remarked to a journalist from *Time* magazine, "We are all Keynesians now; no one is any longer a Keynesian." In regrettable journalist fashion, *Time* quoted the first half of what I still believe to be the truth, omitting the second half.'

[19] In the same vein Blaug (2009, pp. 241-242), in defence of partial equilibrium (contrary to general equilibrium) analysis contends; 'Is there no alternative to either Debreu or Sraffa? Of course, there is. I am not advocating loose thinking as a replacement for rigor but rather Marshallian partial equilibrium analysis that tackles local questions in a subset of economic relationships, taking account of as many interdependencies in the economy as possible but always favouring practical results rather than logical generalities, keeping close to what can be quantified and measured.

[20] A different way of proceeding with a 'rehabilitation' of Marshall, which is beyond the scope of this paper but deserves to be mentioned nonetheless, is based on a critique of the notion of equilibrium in favour of a revival of Marshall's 'economic biology'. A good presentation of this point of view is Hart (2012).

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