Marshall’s Partial Equilibrium Analysis : a Methodological Note*

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Introduction

There are at least two grounds for possible complaints about the bulk of existing literature on Marshall’s partial equilibrium analysis and its underlying methodology. First, that part of Marshall’s (or, more exactly, Marshallian) economics that still finds a place in current textbooks (in the face of an increasing influence of the neo-Walrasian approach) depicts partial equilibrium analysis as a ‘special’ case whose legitimacy is severely limited by the well-known criticism raised by Sraffa as early as in 1925/26. In this representation of Marshallian analysis, however, some deep methodological differences between the two approaches end by going completely unnoticed.

Secondly, even when such a misleading version is set aside and ‘Marshall’s method’ is explicitly addressed, with a few exceptions’ attention is focused on his own methodological claims rather than on a possible independent methodological assessment of his economics. Favorable topics for comment are Marshall’s predilection for ‘biological’ rather than ‘mechanical’ analogies, his suspicious attitude towards the use of mathematics, and how his methodological pronouncements stand up to those presented at the same time in the classical treatise by J.N. Keynes’s [1891].

The purpose of this paper is to show that, in contrast with what the referred literature seems to suggest, we may usefully consider Marshall’s method of partial equilibrium with reference to his economic analysis (instead of to his methodological judgments) and to some pieces of current research in the growing field of economic methodology. More specifically, it will be argued that Marshall, though always concerned about the ‘realism’ of his analysis, endorsed a notion of causal relation which may now be regarded as typical of causal instrumentalism. He consequently availed himself of the wide analytical articulation made possible by the extensive use of ceteris paribus clauses within partial equilibrium analysis in order: i) to capture as much of the economic phenomena he perceived as relevant in everyday life; ii) to identify causal connections by means of the distinction between exogenous and endogenous variables; iii) to avoid the embarrassing question ‘Are there causal relations among dependent variables?’ that would have inevitably arisen within a general equilibrium analysis; iv) to ‘isolate’ those questions he considered to be (analytically) tractable with some degree of precision.

On Marshall’s ‘Realism’ : Some Preliminary Remarks

It should be clear that the outlined argument can be better understood with regard to what Marshall ‘practised’, rather than to what he repeatedly ‘preached’ on methodological matters. Therefore, it is in books III and V of Principles, rather than in Marshall [1898], [1919, App. A] or [1961, App. C], that we must look for the peculiar features of his methodology.

Those who have followed this route (cf., for example, Moss [1984] and Dooley [1985]) have been invariably impressed by Marshall’s ‘realism’, enhanced by his constant concern with combining analytical precision and explanatory significance. The conclusion, which is almost invariably reached in such, is that Marshall (unlike Walras) was interested in the need to adapt the ‘theory’ to ‘facts’. Consequently, the criticism directed at him on the ground of logical consistency would be based on a lack of understanding of the ultimate aim of his theoretical efforts.

Though at first glance it seems very attractive to critics of Walrasian theory, such a conclusion only raises a number of difficult problems without really contributing to the solution of any of them. Apart from the fact that ‘logical consistency’ is not (contrary to what is usually believed) easily distinguishable from ‘empirical adequacy’, conclusions such as the above suggest that the two features are necessarily mutually exclusive. Though they might be found to be irreconcilable in a particular case, in order to establish a case of general validity we need to identify as precisely as possible the reasons within economic theory that make it generally impossible to reconcile this ‘logical coherence’ with ‘empirical realism’ (or, if you prefer, ‘analytical rationality’ with ‘intelligibility’). However, in the light of the current trend of epistemological thinking, a convincing justification of this impossibility is not available, nor is it likely to be in the near or more distant future.
However, I do not wish to deny here that Marshall adopts a realist perspective, as can be inferred from both his constant concern with the realism of assumptions and the complete absence in his writings of any statement implying an instrumentalist view of the role of theory. What I should like to stress is that Marshallian realism has less obvious (and, in my opinion, more interesting) consequences than the more or less high price to be paid in terms of analytical precision.

Indeed, in the Principles there is an important exception (which at first sight could be taken as a manifest contradiction) with regard to a completely realist attitude. I refer to the famous (for reasons different from those discussed below) appendix I on Ricardo’s (and, though not explicitly mentioned in the title, Jevons’s) theory of value. Here Marshall, in order to show how the ‘old’ and ‘new’ theories of value were much closer than maintained by Jevons, criticises the latter’s claim that ‘Cost of production determines supply. Supply determines final degree of utility. Final degree of utility determines value’ and identifies the most serious fault in Jevons’s position in that:

"[I]t does not represent supply price, demand price and amount produced as mutually determining one another (subject to certain other conditions), but as determined one by another in a series. It is as though when three balls A, B, and C rest against one another in a bowl, instead of saying that the position of the three mutually determines one another under the action of gravity, he had said that A determines B, and B determines C. Someone else however with equal justice might say that C determines B and B determines A." (Marshall [1961, 818])

As the reader can easily see, Marshall’s view on the causal ordering among variables of a model focuses on the distinction between exogenous and endogenous variables and excludes the possibility of causally interpreting any relation between endogenous ones. As is well known, it is precisely the instrumentalist conception of causality that entrusts to the formal structure of the model the task of distinguishing causal connections from interdependent relations between variables. However, tempting though it may be, the use of formal models does not necessarily force one to endorse such a conception of causality. After all, a realist could claim that, when a model implying some (causally interpreted) relations between exogenous and endogenous variables is well-confirmed, this is not because its structure establish the existence of causal relation, but because a model would not be so well-confirmed (except by mere chance) if it failed to take into account such causal connections (cf. Hausman [1983]).

As usual, the problem here is that in economics controlled experiments or observations do not really exist. Every economist who (like Marshall) holds realist beliefs, has therefore to identify (not for epistemological reasons, but for contingent needs) a research strategy that allows him to preserve his own epistemological convictions (which, in Marshall’s case are accompanied by a deep ethical concern) even though he can neither resort to experiments nor give much credit to his own observations.

Causal Realism and ‘Ceteris Paribus’ Clause

It is my contention here that Marshall’s extensive use of ceteris paribus clauses can be regarded as a means to overcome the above-mentioned difficulties. It goes without saying that I do not claim that Marshall was fully aware of all the intricacies of the notion of causality, rather I would like to show that his solution can be here read in this perspective. In this respect, it may be of interest to refer to an example suggested by Hausman [1983] (see also Zamagni [1983]) that demonstrates the inconsistency of causal instrumentalism.

Let us consider the statement “Tom eats a lot of potatoes and little meat because he is poor”. Since it would be ludicrous to claim that if Tom ate few potatoes and a lot of meat he would be richer, it is more sensible to suppose that, assuming a reasonable structure of Tom’s preferences, if he were not so poor he would change his diet, and it is plausible to interpret the above statement in a causal sense. Indeed, this conclusion would be confirmed by representing such a situation through the traditional theory of the consumer’s choice, where income and prices are ‘given’ (i.e. exogenous) and the consumed quantities represent dependent (i.e. endogenous) variables, and by interpreting possible variations in equilibrium solutions corresponding to variations in one of the exogenous variables as causal connections.

Otherwise, if we consider the same situation through a non-recursive model of general equilibrium, since both prices and income would be included in the independent variables, the existence of a causal connection between Tom’s income and his diet should be excluded. The somewhat paradoxical conclusion brought about by this argument should therefore be:

"[...] that poverty is the cause of the high consumption of potatoes in the consumer’s model, while it ceases to be such in the model of general equilibrium: the causal assertions therefore are ‘model-dependent’.
Only if one of the two models should prove to be disconfirmed, and therefore be considered invalid, could we escape from a similar conclusion. But this will not occur because, despite all the critiques, both models are constantly being used by economists. (Zamagni [1983, 130]).

Although all this does not oblige us to endorse the thesis of causal instrumentalism, we need some sort of justification for not doing so. In the case in point we could remark that an increase of Tom’s ‘initial endowment’ would only negligibly influence all the variables concerned, except for the quantities he himself consumes. In this way we might preserve a ‘causal’ interpretation of the phenomenon even within a model of general equilibrium. Not always, however, is it so easy to find a similar way out: when aggregate relations are considered (e.g., an Engel’s curve or a Marshallian demand curve) this possibility is precluded. This is still more true if – as is the case with Marshall – we feel obliged to justify our causal judgments through models in which causal connections can be represented by relations between exogenous and endogenous variables.

‘Ceteris Paribus’ and Marshall’s Time Analysis

It is widely known that in Marshall [1961] (especially in Book V and corresponding appendices) ceteris paribus clauses are primarily exploited to specify the various ‘periods’ within which the supply price and market equilibrium are to be analysed. If the previously envisaged interpretation of the methodological foundations of such a research strategy may appear unusual, this is, in my opinion, because of two aspects which at this point deserve a more detailed comment.

The first can be traced back to Marshall himself, i.e., to his insistence on the limitations of the ‘static method’. Warnings that

"In breaking it up, he segregates those disturbing causes, whose wanderings happen to be inconvenient, for the time in a period called Ceteris Paribus. The study of some group of tendencies is isolated by the assumption other things being equal: the existence of other tendencies is not denied, but their disturbing effect is neglected for a time. The more the issue is thus narrowed, the more exactly can it be handled: but also the less closely does it correspond to real life." (Marshall [1966, 366], italics added).

are repeatedly set forward in defence of the need to proceed by successive approximations when tackling difficult problems. Such admissions surely bear out Marshall’s intellectual honesty when justifying his own ‘abstractions’, but they might also suggest misleading conclusions about the realism of his analysis.

Moreover, the same admissions may have prepared the ground for the subsequent interpretation that Marshall’s time analysis refers to an ‘operational’ time identified, on each separate occasion, by that particular version of the ceteris paribus clause intended to specify the time horizon of the analysis. According to a corollary of this interpretation, it would therefore be misleading to try to interpret the various Marshallian ‘periods’ with reference to clock time.

In the light of what has been previously said, this last conclusion should undoubtedly be revised. Indeed, it is not difficult to find in Marshall [1961] ample evidence of the inadequacy of such a conclusion. Constantly concerned with the realism of his own theoretical framework, Marshall consciously avails himself of the ample articulation made available through the diversified use of the ceteris paribus clause, to attempt to capture in his analysis as much of the economic phenomena which he perceives as important in ‘practical life’. Were it not so, it would be difficult to explain time references in the following passage

"As regards market prices, Supply is taken to mean the stock of the commodity in question which is on hand, or at all events 'in sight'. As regards normal prices, when the term Normal is taken to relate to short periods of a few months or a year, Supply means broadly what can be produced for the price in question with the existing stock of plant, personal and impersonal, in the given time. As regards normal prices, when the term Normal is to refer to long periods of several years, Supply means what can be produced by plant which itself can be remuneratively produced and applied within the given time." (Marshall [1961, 378-9], italics added)

or those, even more precise, concerning the 'corn market in a country town' or the 'fish market'. Note that his analysis of the market period (which has now almost completely disappeared from textbooks) highlights just how the same time lapse may require different specifications of the ceteris paribus clause, simply in consideration of the different perishability of corn and fish. Indeed, while in the case of fish, the supply curve would be vertical (possibly starting from a minimum price dictated by the fear of 'spoiling the market'), with regard to corn we should in any case trace a positively-sloped supply curve (starting
from a minimum price, in this case somehow determined also by sellers' short-run expectations).

The interpretation which would remove from the Principles any reference to the time in which 'the ordinary events of life' take place, besides being arguable from a 'philological' point of view, appears to be pointless misleading: it could lead, for example, to endless discussions on the 'realism of assumptions'. In my opinion, the special use Marshall makes of the ceteris paribus clause can be interpreted more usefully:

1. on an epistemological level, as an attempt to provide a solution for the two problems (already identified by Mill) regarding the need to tackle an elusive topic as the object of economic study: defining its boundaries (i.e. justifying the 'separateness' of the discipline) and representing its fundamental properties through 'inexact' laws;

2. on a methodological level, as an attempt to represent, through the exogenous vs. endogenous variables dichotomy, causal connections (interpreted, to be sure, from a clearly realistic point of view) which would be otherwise difficult to identify within a general equilibrium framework.

These are problems for which every 'school' in economics has put forward a different solution. There will always be room for explanatory work on the appropriateness of the different solutions in relation to their own methodological premises. However, if only to avoid unnecessary quarrels, there is nothing to be gained from proposing interpretations of different approaches based on methodological premises alien to them.

Final Remarks

What has been said so far could be also read as a discussion of one aspect - actually rather important one since it involves the whole semantics of the interpretation of the model - of the irreducibility of the Marshallian view to the (neo)Walrasian one.

In this regard, mention should be made of all the attempts aimed at incorporating typically Marshallian hypotheses in models of general equilibrium. What such a literature (see the comprehensive review by Novsheck and Sonnenschein [1987]) can prove is the possibility of taking into account U-shaped cost curves and free entry for new firms (instead of assuming convex technology and a given number of firms). However, those aspects of Marshallian analysis on which we have focused our attention here (i.e., the time specification of the notion of equilibrium and relative causal explanations) are inevitably engulffed by the analytical structure of the neo-Walrasian approach. This should not come as a surprise. In fact, all the various methodological justifications for general equilibrium analysis share a strong appreciation of the 'generality' of the model (seen as the ability to 'endogenise' all the a priori relevant variables). In contrast, the conventionalist tradition appreciates the applicability of the same theoretical principles to as wide a class of phenomena as possible.

Unfortunately, I have some doubts as to whether the importance of this type of argument can be adequately recognised in the current theoretical climate. When the final word goes to mathematical formalism (an attitude, by the way, about which Marshall never tired of cautioning his students and readers), it is inevitable that the 'reducibility' of one theoretical approach to another will be interpreted on the basis of the possibility of incorporating in the models of one approach those hypotheses of the other which are compatible with it. How much of importance is lost in operations of this kind, however, is something that each reader should decide for himself.

Notes

* This paper is a shortened version of one presented at the 9th International Congress of Logic, Methodology and Philosophy of Science, Uppsala, August 7-14, 1991. An earlier Italian version was delivered at the annual meeting of the Società Italiana degli Economisti, Rome, November 2-3, 1990.

I would like to thank Stefano Zamagni for helpful comments and continuous encouragement. Financial support from M.U.R.S.T. is also gratefully acknowledged.

1. As is well known, 'Marshallian economics' is due to Pigou [1928], Harrod [1930], Viner [1931], Joan Robinson [1933] and others, rather than to Marshall himself.

2. Even in these cases, however, the possible vantage point of the current debate on economic methodology is not fully exploited: see, for instance, Loasby [1989], Rogers [1989, chap. 8], Currie and Steedman [1990].


4. By this, I do not mean either to deny the possibility of reconciling Marshall's methodological judgments with his own theoretical framework or to underrate such study (to be sure, I believe that in this respect Marshall should be included among the most 'coherent' economists, as does, for instance, Hammond [1991]). I simply prefer to take a (different) perspective which, according to what is emerging from the most recent work in economic methodology (cf., for example, Caldwell [1989], Hausman [1989] and Mäki [1990]), places strong emphasis on the necessity of paying more attention to the effective practice of economics. Although approaching economic methodology in this way raises some problems not yet completely solved (see Salanti [1989a]), it makes possible a better understanding of the problems encountered in economic research.

5. For a comprehensive and stimulating discussion of the pervasiveness of the "method of isolation" in economics, see Mäki [1991].


7. Take, for instance, the repeated observations regarding the
irreversibility of the supply curves or the reasons given by Marshall [1961, n. XII b in the Mathematical Appendix] to justify the hypothesis of an additive utility function.

8. According to the distinction between 'realism' and 'realistness' as set forth by Mäki [1989], the latter term would be more appropriate. In the case of Marshall, however, we can follow the usual terminology without giving rise to any serious misunderstanding.


10. For an example of the same kind of argument with reference to the question of whether, within the marginalist theory of distribution, it is possible to claim that marginal productivity of work 'determines' equilibrium wages, see Schumpeter [1954, 941-44].

11. More precisely, we should speak of causal connections only with reference to events. However, because nothing prevents us from representing as an event a change in the value assumed by a certain variable, the common practice of talking about causal relations between variables, when correctly understood, can be accepted.

12. Note that Marshall consistently rejected naive inductivism. In Marshall [1961, 774], for instance, we read: "It must then always be remembered that though observation or history may tell us that one event happened at the same time as another, or after it, they cannot tell us whether the first was the cause of the second. That can be done only by reason acting on the facts."

13. Note that, as Persky [1990] has recently pointed out, it was Marshall himself, probably referring to Cairnes [1857], who made this expression popular with economists (an expression, by the way, not of classical Latin origin). It should also be noted that evidence of a realist conception of causality (when not dealing with equation systems) abounds in his writings. One quotation should suffice: "It is sometimes said that the laws of economics are 'hypothetical'. Of course, like every other science, it undertakes to study the effects which will be produced by certain causes, not absolutely, but subject to the condition that other things are equal, and that the causes are able to work their effects undisturbed." (Marshall [1961, 301]).

14. Note that according to a widespread interpretation such a result, far from being regarded as perplexing, should be actively pursued. In this vein, for instance, Schumpeter [1934, chap. 1] maintained that the economists' task ends whenever the 'identified' causes do not pertain to the domain of economics because at that point economists must call upon other disciplines. What actually happens, however, is that "Notwithstanding their often expressed desire to cooperate with the adjoining disciplines, economists have more often than not developed their own brand of psychology, their special versions of sociology and their particular 'laws' of technology." [Leonieff, 1953, 14].


16. This can be reformulated in more rigorous terms, stating that in a market with a 'continuum' of traders (as in Aumann [1964]) firm is not 'affecting equilibrium' prices. In this way it is therefore possible to highlight a causal connection between income and diet without referring to a model of 'partial' equilibrium.

17. At this point it should be clear which interpretation of Marshall's method of partial equilibrium analysis I intend to advance here; the question will be dealt with in the next section with reference to his analysis of supply. The claim that such an interpretation as I make here can in fact find support also in Marshall's analysis of demand involves a more formalised discussion and is examined in some detail in Salanti [1991a].

See also, for instance, Marshall [1961, 379-80n, 460-1, 501, App. H].

19. Cf., for example, Blaug [1985, chap. 10]. One of the first and the clearest discussions on the subject can be found in Opie [1931].

20. On the different epistemological status of the economic 'laws' compared to those in (some) natural sciences, see Zamagni [1987]. For a convincing analysis of the Millian methodological arrangement, see Hausman [1981].


22. What is suggested here is, in other words, a definite preference for approaches which are possibly critical, but 'internal' to the methodological perspective underlying each theoretical line. For a discussion of the distinction between 'internal' and 'external' criticism in methodological appraisals, let me refer to Salanti [1989c].

23. Extensive references to the various positions with regard to the explicative value of general equilibrium analysis can be found in Salanti [1991b].

24. For a clear instance of this way of tackling the problem of the reducibility of one model to another, see Hahn [1982].

References


