**On the Right Side for the Wrong Reason: Friedman on the Marshall–Walras divide**


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## Abstract


The aim of this article is to assess Friedman's claim, put forward in his 1949 article on the Marshallian demand curve, that there is a methodological divide between the Marshallian and Walrasian approaches. Friedman's argument will be critically examined and compared with the views he expressed in other articles written around the same time. My evaluation will lead to a mixed conclusion. Positively, Friedman must be hailed for having brought the Marshall–Walras divide to the forefront. In a more critical vein, I will suggest, first, that Friedman's argumentation in the 1949 paper is definitely wanting. A better account of the differences between Marshall and Walras is to be found in his 1955 review of Jaffé's translation of Walras's *Élements d'économie pure*. Second, I will claim that Friedman's real target in his 1949 article was imperfect competition theory *à la* Chamberlin and Keynesianism *à la* Lange, which he wrongly associated with Walrasian theory. Third, present-day proponents of the divide will be criticised for having naively echoed Friedman's argumentation instead of having tried to improve it. The paper finishes with an outline of the criteria that should be used for giving the Marshall–Walras divide a stronger grounding.


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## 1. Introduction

In 1949 Milton Friedman published an article on the Marshallian demand curve (Friedman ([1949] 1953) which is still frequently quoted, less for its main argument than for its short final section entitled "Alternative conceptions of economic theory" in which he launched a fierce attack on Walrasian theory. Drawing a contrast between the Marshallian and the Walrasian approaches, Friedman expressed his regret as to the latter having superseded the former. In his oft-quoted words, "We curtsy to Marshall but we walk with Walras" (Friedman [1949] 1953: 89).

Eminent Friedman scholars, such as Roger Backhouse (1997), Mark Blaug (2003), Michael Bordo and Anna Schwartz (2004), Dan Hammond (1996, 2003), Abraham Hirsch and Neil de Marchi (1990), Kevin Hoover ([1984] 1990) and Tom Mayer (1993, 2003), have endorsed Friedman's claim about the Marshall–Walras divide.[1] The following quotation from Hammond summarises the gist of their argument:

> The hallmark of Friedman's methodology is Marshallianism. With this label he sets his view apart from the Walrasians'. In the 1940s the differences in the two methodologies were recognized, and as we have seen, they were not Friedman's creation. As time passed, the Walrasian approach ascended to a position of dominance in both micro and macroeconomics, with the virtues of the general equilibrium approach and mathematical qualities of elegance and generality taken for granted by most economists (Hammond 1996: 43).

The aim of this paper is to assess the validity of Friedman's claim, first by studying the argument offered in Section V of his 1949 article and, second, by comparing it with his other methodological papers — his reviews of Robert Triffin's *Monopolistic Competition and General Equilibrium* (Friedman 1941) and Oscar Lange's *Price Flexibility and Employment* (Friedman ([1946] 1953), his comment of Christ's assessment of Klein's econometric model (1951), his famous "Methodology of Positive Economics" essay (1953) and finally his review of William Jaffé's translation of Walras's *Élements d'Économie Pure* (Friedman [1955] 1993).

As to my motivation for writing this paper, it is simple. In recent papers (De Vroey 1999a, 1999b, 2003, 2004), I have argued that the Walrasian and Marshallian approaches are alternatives rather than complements. Hence my desire to check whether Friedman was my ally in this respect! The conclusion of my inquiry is mixed. Positively, Friedman must be hailed for having brought the Marshall–Walras divide to the forefront. Negatively, he must be indicted for his the faults in his argument. Although rhetorically powerful, Section V of his 1949 article is rambling and reveals a poor understanding of Walrasian theory. I surmise below that its real target was less Walrasian theory *per se* than monopolistic competition and Keynesianism *à la* Lange, two streams of thought that Friedman criticised in his other papers and which he wrongly associated with Walrasian theory. By contrast, his 1955 review of Jaffé's translation is more balanced and reveals a better understanding of the Walrasian project.

## 2. The Marshall–Walras divide in Friedman's early writings

### 2.1. Friedman's 1941 review of Triffin

Triffin's *Monopolistic Competition and General Equilibrium* (1940) was based on his doctorate, written under the supervision of Chamberlin. As far as can be judged from its introduction and first chapter, its aim was to integrate monopolistic competition and Walrasian theory:

> Monopolistic and imperfect competition theories have evolved in the United Sates and in England alike along the lines of the theoretical tradition dominant in both countries: the particular equilibrium of Alfred Marshall. What we might well do now is to restate the whole problem in terms of the Walrasian, general equilibrium system of economic theory, so much more influential in economic thought on the continent of Europe. We shall find with happy surprise that monopolistic competition begins to bridge the canyon which has for years separated two schools of theoretical thought (Triffin 1940: 3).

However, going beyond rhetoric, not much of this programme ended up being realised. While Triffin's book contains many scattered references to Walrasian theory, they concern particular points rather than Walras's general system. No model of a general equilibrium incorporating imperfect competition is present. The bulk of the book rather consists of a comparison of

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[1] Nonetheless the prevailing view, which can be traced back to Hicks (Hicks 1934), asserts that the Marshallian and Walrasian approaches are complementary.

different imperfect competition models. Still, one claim that could not but attract Friedman's attention, stands out, namely that as soon as product differentiation is introduced the notion of an industry ceases to be relevant. Only the notions of the firm, on the one hand, and the economy as a whole, on the other, remain relevant.

In his two-page long review of Triffin's book, Friedman (1941) did not evoke the Marshall–Walras opposition. Actually, Walras's name was not mentioned. Friedman's main complaint was that "Marshallian tools have been thrown away too light-heartedly" by imperfect competition theorists (1941: 390). He had in mind the notion of industry. While, like Triffin, he considered the notions of monopolistic competition and industry antagonistic, he drew the opposite conclusion — for Friedman, the notion of monopolistic competition had to be abandoned and that of industry maintained. The underlying reason was his enduring concern for practical issues. The latter, he believed, were to be grasped at the level of the industry instead of that of the firm or the economy as a whole (Hammond 2003).

### 2.2. Friedman's 1946 review of Lange's *Price Flexibility and Employment*

The aim of Lange's (1944) book, whose inspiration was Hicks' (1946) *Value and Capital,* was to examine the Keynesian issue of whether a decrease in the money wage could restore full employment in the face of involuntary unemployment. Lange's interest lay in the persistence of under-employment, rather than in the study of the logical existence of a state of equilibrium with involuntary unemployment. Not surprisingly, his conclusion was that Keynes had been right:

> Only under very special conditions does price flexibility result in the automatic maintenance or restoration of equilibrium of demand for and supply of factors of production (Lange, 1944: 83).

Lange's book started with a rejection of partial equilibrium analysis on the grounds of its narrow range of validity. Although Lange did not refer to the industry notion, it is clear that he had no sympathy for it. This was certainly not music to Friedman's ears. Yet his criticism was not directly addressed to this point. Rather Friedman wanted to indict Lange's analysis, for consisting of "empty tautologies" or being mere "taxonomic theorising" ([1946] 1953: 277). He concluded that Lange was just "enumerating theoretical possibilities, not describing the real world" ([1946] 1953: 271) and that his work lacked any empirical anchorage — no fact-gathering at the initial stage of the inquiry, no refutable predictions at its end stage.

> Lange largely dispenses with the initial step — a full and comprehensive set of observed and related facts to be generalised — and in the main reaches conclusions no observed facts can contradict. His emphasis is on the formal structure of the theory, the logical interrelations of the parts. He considers it largely unnecessary to test the validity of his theoretical structure except for conformity to the canons of formal logic. His categories are selected primarily to facilitate logical analysis, not empirical application or test. (Friedman [1946] 1953:283).

This was the very criticism that Friedman was later to address to the Walrasian approach.[2] Yet at this juncture, no reference was made to Walras.

A further, yet less emphasised, reason for Friedman's dissatisfaction with Lange's work concerned its policy conclusion. In Lange's paper, general equilibrium theory was used as a vehicle for making a Keynesian case. That general equilibrium might serve the purpose of dismissing the working of the invisible hand did not bode well for it in Friedman's eyes.

### 2.3. Friedman's 1949 paper on the Marshallian demand curve

Like the Triffin review, Friedman's 1949 paper on the Marshallian demand curve was a complaint about the turn taken by economic theory.[3] Here Friedman's object of dissatisfaction was the standard interpretation of the Marshallian demand curve. He felt that it was unsatisfactory and needed to be replaced by an alternative interpretation introducing some interdependency across markets. Although Friedman did not use this terminology, and always referred to the "current interpretation", this should more aptly be called the Hicksian conception (or, following Knigh, the "Slutsky school" conception).[4]

The difference between Hicks' and Friedman's interpretations bears on what must be put in the *ceteris paribus* category. According to the Hicksian interpretation, this should include the money income and the price of every other commodity on top, of course, of tastes and preferences. According to Friedman, it should be the real income and the price of all close

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[2] Referring to Jaffé in his 1955 review, Friedman wrote: "In his final sentence, Jaffé speaks like a true Walrasian in methodology. One first constructs a pure theory, somehow on purely formal considerations without introducing any empirical content; one then turns to the 'real' world, fills up the empty boxes, assigning numerical values to constants, and neglects 'second-order' effects at this stage. As I have argued extensively elsewhere, this seems to me a basically false view" ([1955] 1993: 29, note 6).

[3] This theme is also visible in Friedman's 1940s lecture notes for his Economics course at the University of Chicago (Hammond 1996: 31).

[4] Cf. Yeager (1960: 313).

substitutes for and complements of the commodity considered. Thus, Friedman assumed that whenever a change in the price of the good considered occurred, the average level of prices of unrelated goods would change in such a way as to freeze the purchasing power of money. This position amounted to isolating the substitution effect while eliminating the income effect. Another consequence of his stance was that the "Friedman–Marshall" demand curve was necessarily negatively sloped while this was not so for the Hicksian conception when the good in question was a Giffen good.

In defence of his new interpretation Friedman claimed that it was more modest ("because it does not try to take account of all prices in the system, not even by keeping them constant" (Yeager 1960: 54)) and more "useful" than its rival when it came to practical problems.

> A demand function containing as separate variables the prices of a rigidly defined and exhaustive list of commodities, all on the same footing, seems largely foreign to this [Marshallian] approach. It may be a useful expository device to bring home the mutual interdependence of economic phenomena; it cannot form part of Marshall's "engine for the discovery of concrete truth". The analyst who attacks a concrete problem can take explicit account of only a limited number of factors; he will inevitably separate commodities that are closely related to the one immediately under study from commodities that are more distantly related (Friedman [1949] 1953: 57).[5]

Friedman went to great lengths to convince his readers that his interpretation was more faithful to Marshall than the Hicksian one. In effect, most of the paper is devoted to this task.[6] My own interest, however, is less in the demand curve discussion than in the paper's final section, to which I now turn.

For all its brevity, Section V develops several distinct themes. To introduce them, let me quote directly from Friedman's text.

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[5] See also Friedman ([1949] 1953: 85 and 91).

[6] My aim here is not to assess the validity of the change proposed by Friedman. Let me nonetheless give a quotation from Aldrich that is rather dismissive: "Friedman's "Marshallian Demand Curve" 1949 carried forward the revolution in esteem. It took a different fragment and argued that Marshall anticipated the full application of modern theory to the circumstances of a single good analysis. Commentators just had not made the right connection with Slutsky. Friedman's essay did not generate new economic theory but it established Marshall's demand theory as a topic for historians of economic thought. Its bold thesis and its detailed attention to what "Marshall really meant" were provocative. It set a pattern: the subject text is tortured until a confession is produced that can be corroborated by modern theory" (1996: 212).

*A difference in purpose and method between Marshall and Walras*

> The important distinction between the conceptions of economic theory implicit in Marshall and Walras lies in the purpose for which the theory is constructed and used. To Marshall — to repeat an expression quoted earlier — economic theory is "an engine for the discovery of concrete truth". … Economic theory, in this view, has two intermingled roles; to provide "systematic and organised methods of reasoning" about economic problems; to provide a body of substantive hypotheses, based on factual evidence, about the "manner of action of causes". In both roles the test of the theory is its value in explaining facts, in predicting the consequences of changes in the economic environment. Abstractness, generality, mathematical elegance — these are all secondary, themselves to be judged by the test of application. The counting of equations and unknowns is a check on the completeness of reasoning, the beginning of the analysis, not an end in itself. Doubtless most modern economic theorists would accept these general statements of the objective of economic theory. But our work belies our profession (Friedman [1949] 1953: 90).

*Friedman's indictment of the Walrasian approach*

According to Friedman, the Walrasian approach must be indicted on several grounds:

(a) Means — i.e. "abstractness, generality, and mathematical elegance" — have become ends in themselves (Friedman [1949] 1953: 91).

(b) The Walrasian approach is flawed because it gives precedence to the realism of assumptions over their predictive ability:

> Facts are to be described, not explained. Theory is to be tested by the accuracy of its "assumptions" as photographic descriptions of reality, not by the correctness of the predictions that can be derived from it (Friedman [1949] 1953: 91)

(c) It eliminates the industry notion and takes the monopolistic competition line, a blind alley:

> From the " Walrasian" viewpoint, to take another example from recent developments in economic theory, it is a gain to eliminate the concept of an "industry", to take the individual firm as the unit of analysis, to treat each firm as a monopoly, to confine all analysis to either the economics of the individual firm or to the general equilibrium analysis of the economy as a whole. From the Marshallian viewpoint this logical

> terminus of monopolistic competition analysis is a blind alley. Its categories are rigid, determined not by the problem at hand but by mathematical considerations. It yields no predictions, summarises no empirical generalisations, provides no useful framework of analysis. (Friedman [1949] 1953: 91-92).

*The difference between the two approaches is not a matter of partial versus general equilibrium*

> The distinction commonly drawn between Marshall and Walras is that Marshall dealt with "partial equilibrium", Walras with "general equilibrium". This distinction is, I believe, false and unimportant. Marshall and Walras alike dealt with general equilibrium; partial equilibrium as usually conceived is but a special kind of general equilibrium analysis — unless, indeed, partial equilibrium analysis is taken to mean erroneous general equilibrium analysis. Marshall wrote to J. B. Clark in 1908: "My whole life has been and will be given to presenting in realistic form as much as I can of my Note XXI". Note XXI, essentially unchanged from the first edition of the *Principles* to the last, presents a system of equations of general equilibrium (Friedman [1949] 1953: 90).

*Praising Keynes*

Section V ends with Friedman praising Keynes on the ground that he was a real Marshallian:

> Of course, it would be an overstatement to characterise all modern economic theory as "Walrasian" in this sense. For example, Keynes's theory of employment, whatever its merits or demerits on other grounds, is Marshallian in method. It is a general equilibrium theory containing important empirical content and constructed to facilitate meaningful prediction (Friedman [1949] 1953: 92)

**2.4. Friedman's 1951 Comments on Christ's article**

In a paper presented at a NBER Conference on Business Cycles, Christ (Christ 1951) set himself the task of testing Klein's econometric model. The conclusions he reached were rather negative.

Friedman's comment on it is interesting for my purposes, because he took the opportunity to plead against the use of using simultaneous equation models aiming at representing an entire economy to make short-term predictions. In Friedman's eyes, Christ has shown that Klein's model was a failure. This was music to his ears. He then raised the question of what to do next. "Does it not then follow that despite the unsatisfactory results to date, the appropriate procedure is to continue trying one after another of such systems [of general equilibrium] until one that works is discovered?" (Friedman 1951: 112). No, was his answer, for "the probability that such a process [of constructing a model for the economy as a whole] will yield a meaningful result seems to me almost negligible" ((Friedman 1951: 113). Instead, his line was to return to the study of industries:

> The direction of work that seems to me to offer most hope for laying a foundation for a workable theory of change is the analysis of parts of the economy in the hope that we can find bits of order here and there and gradually combine these bits into a systematic picture of the whole. In the language of model builders, I believe our chief hope is to study the sections covered by individual structural equations separately and independently of the economy ((Friedman 1951: 114).

Again, the names of Walras and Marshall were absent from Friedman's comments, yet their background presence was obvious. Studying the individual structural equations separately was nothing else than doing Marshallian industry analysis, while Klein's attempt at having a grasping the economy as a whole suggested a Walrasian affiliation.

### 2.5. The 1953 "Methodology of Positive Economics" Essay

While the Marshall–Walras opposition was not mentioned in Friedman's celebrated 1953 paper, two related themes were nonetheless broached. First, Friedman engaged in a criticism of empty tautologies, taking up the theme developed in his Lange paper ([1946], 1953). Second, as in the Triffin review (1941), he crossed swords with imperfect competition theory. It is to the latter that he resorted when criticising the realism of assumptions view:

> The relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic" for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether its yields sufficiently accurate predictions. … The theory of monopolistic and imperfect competition is one example of

> the neglect in economic theory of these propositions. The development of this analysis was explicitly motivated, and its wide acceptance and approval largely explained, by the belief that that the assumptions of "perfect competition" or "perfect monopoly" said to underlie neoclassical economic theory are a false image of reality. And this belief was itself based almost entirely on the directly perceived descriptive inaccuracy of the assumption rather than on any recognised contradiction of predictions derived from neoclassical economic theory (Friedman, 1953: 15).[7]

Friedman's choice of imperfect competition as an example was hardly innocuous. In effect, according to Leeson, "his methodology of positive economics was formulated as a "reaction" to Robinson and Chamberlin" (2000: 62).[8]

### 2.6. Friedman's 1955 review of Jaffé's translation of Walras's *Elements*

Clearly, Friedman's tone regarding Walrasian theory is more positive here than in his Marshallian demand curve article, as witnessed by the following statement: "The *Elements* is a great work which marked an important step forward in the development of economics as a science" (Friedman, [1955], 1993: 19). His review started with the recognition that Walras was pursuing an aim different from Marshal's — to "give a bird's eye-view of the economic system as a whole" instead of trying to solve concrete issues — yet of no lesser importance.

> Walras solved a different, though no less important, problem. He emptied Cournot's problem of its empirical content and produced a "complete and rigorous" solution "in principle", making no pretence that it could be used directly in numerical calculations. His problem is a problem of form, not of content: of displaying an idealised picture of the economic system, not of constructing an engine for analysing concrete problems. His achievement cannot but impress the reader with its beauty, its grandeur, its architectonic structure; it would verge on the ludicrous to describe it as a demonstration of how to calculate the numerical solution to a numerically specified set of equations (1955: 23-4).[9]

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[7] Hammond mentions a paper by Stigler in the same vein where the argument is "that Triffin's emphasis on 'realistic' assumptions leaves the theory incapable of explaining the problems encountered in the real world" (Hammond 1996: 37).

[8] On this, see also Hammond (2003: 13 and *seq*.).

[9] The same more balanced view is to be found in his 1974 "Comments on the critics" (Friedman, 1974: 145-6). Amongst the different assessments of Walrasian theory that were written on the occasion of Jaffé's translation

Walras was thus to be credited with having given economists "a framework for organising their ideas". Nonetheless, it was time, Friedman claimed, to return to the more serious business of "meaningful theory" (1955: 27).

> Substantive hypotheses about economic phenomena of the kind that were the goal of Cournot are an essential ingredient of a fruitful and meaningful economic theory. Walras has little to contribute in this direction; for this we must turn to other economists, notably, of course, Alfred Marshall. … All these [factors] have combined to favour the Walrasian emphasis on form, to make it seem not only an essential part of a full-blown economic theory, but that economic theory itself. This conception — or misconception — of economic theory has helped to produce an economics that is far better equipped in respect of form than substance. In consequence, the major wok that needs now to be done is Marshallian rather than Walrasian in character — itself a tribute to Walras's impact. (Friedman, 1955: 27).

In writing the above, Friedman fell back on Stigler's opinion as expressed in *Production and Distribution Theories*:

> Indeed the general equilibrium theory has contributed little to economic analysis beyond an emphasis on mutual dependence of economic phenomena: the problems are far too complicated to be grasped *in toto*. Yet this particular theory describing the nature of general equilibrium was essential: such an idea had to appear before rigorous study could proceed. It was Walras's greatest contribution — one of the few times in the history of post-Smithian economics that a fundamentally new idea has emerged (Stigler, 1941: 242).

### 2.7. Hammond's summary of Friedman's viewpoint

Finally, to close this survey, let me give a last quotation, no longer drawn from Friedman but from Hammond, which aptly summarises Friedman's standpoint:

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(Bridel 1996), some took a harsher position than Friedman. Harrod's review is a striking example: "… Almost all those general qualities that made Marshall's *Principles* a great classic, despite the fact that its original contributions to pure theory are admittedly limited, are lacking in Walras. He had little regard for the actual phenomena of our economic life, no comprehensiveness, few reservations and little nuance or subtlety. The presentation is extremely clumsy" (Harrod, 1956: 311).

> Marshallian theory is problem oriented in the following sense: 1) that it is focused on actual problems from the world of experience; 2) that one begins analysis of a problem well-armed with observed and related facts; 3) that the structure of analysis is dictated by the specific problem one is dealing with; 4) that real world institutions are accounted for and dealt with; 5) that definitions of terms are problem specific; and 6) that mathematical considerations do not take a dominant place in the analysis. The Walrasian approach is to be more concerned with generality: to make theory more abstract, and less connected with problems of policy or experience and institutions, to check the theory or otherwise resort to empirical evidence only after the theory has been worked out; and to emphasise logical consistency and mathematical elegance (Hammond 1992: 226)

### 3. Friedman's subsequent views

There is little doubt that Friedman remained faithful to his earlier views on the existence of a divide between Marshall and Walras. Two testimonies suffice to support this view. The first comes from Friedman's interview with Snowdon and Vane, one of several interviews in which he was invited to develop his current views on economics,

> *Question*: Kevin Hoover has drawn a methodological distinction between your work as Marshallian and that of Robert Lucas as Walrasian. Is that distinction valid?
>
> *Answer*: There is a great deal to that. On the whole I believe that it is probably true. I have always distinguished between the Marshallian approach and the Walrasian approach. I have always been personally a Marshallian. That doesn't mean that the Walrasian approach is not a useful or appropriate approach. Peoples' temperament and attitudes are different I guess. (Snowdon and Vane: 1997: 202).[10] [11]

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[10] Lucas concurred with such an assessment as his own interview by the same authors testifies: "*Question [to Lucas]*: You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you? *Answe*r: I like mathematics and general equilibrium theory. Friedman didn't.... *Question*: His methodological approach seems more in keeping with Keynes and Marshall. *Answer*: He describes himself as Marshallian, although I don't know quite what it means. Whatever it is, it's not what I think of myself (Snowdon and Vane 1998, 132).

[11] Unfortunately, when asked to delve into the reasons why he initiated the Marshall-Walras divide, Friedman's testimony has been of little help as the following extract from Hammond's interview makes clear:
"*Hammond*: In 'The Marshallian Demand Curve" you echo Marshall's description of economic theory as an 'engine for the discovery of concrete truth'. You compare the Marshallian conception of economic theory with

The second testimony is the well-known discussion between Friedman and his critics organised by Gordon (Friedman 1974). Section 3 of Friedman's "Comments on the Critics" is entitled "What explains the difficulty of communication?" (between Tobin and him). His answer to this question was that "the difficulty is a different approach to the use of economic theory — the difference between what I termed a Marshallian approach and a Walrasian approach in an article I wrote many years ago" (1974: 145). Friedman went on quoting extensively from his 1949 article. Later on, when discussing Patinkin he returned to the same theme of the contrast between Marshall and Walras.

> … Patinkin, even more than Tobin, is Walrasian, concerned with abstract completeness, rather than Marshallian, concerned with the construction of special tools for special problems (1974: 159).

All this strongly suggests that Friedman kept defending his earlier standpoint. [12] One element might point to the opposite: Friedman's definition of the natural rate of unemployment given in his Presidential Address to the American Economic Association introducing the expectations-augmented Phillips curve model (Friedman 1968).

> The "natural rate of unemployment" is the level that would be ground out by the Walrasian system of general equilibrium equations, provided there is embedded in them

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the Walrasian conception of what theory should be and should do… Do you have a sense of how you first came to make this distinction and how or why you saw it as important?
*Friedman*: I don't really have the sense of how I first came to make the distinction or why I said it was important. I haven't thought about the question, and offhand, in thinking about it, I really don't know. It is a distinction I made fairly early on..
*Hammond*: You made it early, and I've come to think that it may be one of the keys to your methodology and perhaps…
*Friedman*: I suspect that came from Burns. That's my guess; but I really couldn't document it – because he was so imbued with Marshall. You see, he was very much a disciple of Marshall on the one hand, and Wesley Mitchell on the other. And Wesley Mitchell would have impelled in him aversion to the pure abstract Walrasian, while Marshall would have impelled in him his problem-seeking approach. I suspect that that's where it comes from, but I really can't say. That's just pure rationalisation (Hammond 1992: 226).

[12] Incidentally, in this piece Friedman reiterated his praise of Keynes in even stronger terms than in his 1949 article, insisting on the fact that Keynes's approach was a far cry from the Walrasian. "Keynes was no Walrasian seeking, like Patinkin and to a lesser extent Tobin, a general and abstract system of all-embracing simultaneous equations. He was a Marshallian, an empirical scientist seeking a simple, fruitful hypothesis. And his was a new, bold, and imaginative hypothesis, whose virtue was precisely how much it could say about major problems on the basis of so little. Of course, his assumptions were not in literal correspondence with reality. If they had been, he would have been condemned to pedestrian description; his whole theory would have lost its power. Of course, he could be wrong. There is no point to any scientific theory that cannot be. The greater the range of evidence that, if observed, would contradict a theory, the more precise are its predictions and the better a theory is, provided it is not, in fact, contradicted. I believe that Keynes' theory is the right kind of theory in its simplicity, its concentration on a few key magnitudes, its potential fruitfulness. I have been led to reject it, not on these grounds, but because I believe that it has been contradicted by evidence: its predictions have not been confirmed by experience. The failure suggests that it has not isolated what are 'really' the key factors in short-run economic change" (Friedman 1974: 134).

> the actual structural characteristics of the labour and commodities markets, including market imperfections, stochastic variability in demands and supplies, the cost of gathering information about job vacancies and labour availability's, the cost of mobility, and so on (Friedman 1968: 8)

This passage may be taken as meaning that Friedman ended up considering Walrasian theory as the natural framework within which to embed his concept of the natural rate of unemployment.[13] All in all, however, Friedman's definition should not be taken in earnest, as many commentators have argued (Hall, 1979: 154; Dixon, 1995: 64; Rogerson, 1997: 76). It should rather be viewed as a mere ecumenical gesture serving the purpose of rallying as many people as possible around his conception.

## 4. A criticism of Friedman's Section V argument

Section V of Friedman's 1949 article stands out as a brilliant defence of the Marshall–Walras divide. Yet, it raises a host of questions. How does this last section of Friedman's paper relate to the previous ones? Had Friedman's claim that Walrasian theory dominated the profession any plausibility at the end of the 1940s? Did Friedman mean that the Marshallian and the Walrasian approaches differed in purpose or in their implementation of a common purpose? Was Friedman right when stating that the divide was not a matter of partial versus general equilibrium? Can his portrait of the Walrasian approach be considered fair and well-documented? Finally, how does his 1949 article relate to his other pieces?

### *The link between the first four and the fifth section of the 1949 article*

As Section V of Friedman (1949) is its last section, one would have expected it to offer a generalization of the analysis made in the earlier sections. That is, Sections I to IV would provide a comparison between the Walrasian and the Marshallian interpretation of the demand curve while Section V would deal with the Marshall–Walras contrast at a broader level. Yet, this is hardly the case. The discussion in the first four sections of the paper remains within the bounds of Marshallian theory, since it consists of a confrontation of two distinct interpretations of the Marshallian demand curve, the Friedmanian and the Hicksian ones.

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[13] For example, commenting on Friedman's model, Leeson states that "the natural rate of unemployment is a Walrasian concept" (2000: 10).

Could it be true that the Hicksian–Marshallian and the Walrasian demand functions are one and the same thing? Although Friedman does not make such a claim explicitly, it may well be the "subtext" of his paper.[14] Friedman seems to think that the fact that every commodity composing the economy is individually listed in these two demand functions makes them similar. In Yeager's words:

> The Hicksian approach, in regarding all prices as kept constant except the price of the particular commodity to which the demand curve refers, reflects the Walrasian notion that the quantity demanded of any particular commodity depends on all prices in the economic system (1960: 54).

However, this similarity does not allow us to draw a conclusion of equivalence. First, in the Hicks–Marshall story, the prices of the other goods are constant parameters while they are variables in the Walrasian setting. Second, the Marshallian scenario is about monetary prices while money is absent from the core Walrasian models. *Numéraire* prices and monetary prices should not be confused. Third, in the Marshallian approach it is assumed that trade takes place in two stages. During the first of them, households supply their services in the input markets, thereby gaining an income. When the goods markets open in a second stage, income is an exogenous quantity. In contrast, in the Walrasian case, all trade takes place at one stroke. No need for the income category exists in this framework. The central concept is the budget constraint, which is absent from the Marshallian story.

As soon as the Hicks–Marshall and Walrasian demand functions are considered different, no link can be traced between the two parts of Friedman's article. Actually, the two parts of the article hardly need each other. They could be split without harming the argument in either.

This in turn raises the question of what pushed Friedman to supplement his analysis of Marshallian theory with a section on the Marshall–Walras contrast. My surmise is that he felt that the Hicksian version of the Marshallian demand curves had been contaminated with the Walrasian vice of privileging abstractness and mathematical elegance over empirical relevance.

> From this viewpoint [privileging abstractness, generality, and mathematical elegance] the current interpretation of the demand curve is clearly the better. It is more general

[14] This is the step taken by Hirsch and de Marchi: "In comparing two demand curves, one of which he calls Walrasian (the conventional one) and of which he refers to as Marshallian (constant real income) Friedman gives us an example of how he applies the Marshallian–Walrasian distinction in a specific case" (1990: 23).

> and elegant to include the price of every commodity in the universe in the demand function rather than the average price of a residual group. Any price may affect any other, so a demand equation including every price is a more accurate photographic description. Of course, it cannot be used in discovering "concrete truth" as it contains no empirical generalisation that is capable of being contradicted – but these are Marshallian objections (Friedman [1949] 1953: 91).

Unfortunately, these remarks do not stand up to scrutiny. Actually, they are a typical example of Friedman's powerful yet shallow rhetoric. For all its appeal, his argumentation hardly withstands scrutiny. Its second sentence, "It is more general…", is a mere truism. What does "higher elegance", "photographic description" really mean? Finally, one is at a loss to see why the Hicksian interpretation might witness more abstractness, generality, and mathematical elegance than Friedman's own.

*The alleged dominance of the Walrasian approach*

Considering the time he was writing (the end of the 1940s), Friedman's fierce attack on the Walrasian approach is surprising. "We curtsy to Marshall but we walk with Walras" is definitely a powerful image, yet is it a correct account of what was going on? I doubt it.

The *Elements* had not been translated into English at this time. Arrow and Debreu had not started their collaboration. Only a few sources (e.g. Stigler (1941) or the works of Cassel and Wicksell) were available to economists who wished to become acquainted with Walrasian theory without being able to read French. Admittedly, a revival of Walrasian theory, triggered by Hicks' *Value and Capital*, was effectively on its way. If there was a place in the US where its effects might be felt, it must have been the Cowles Commission with which Friedman was acquainted. Yet there is a tremendous gap between being alert to the revival of Walrasian theory and asserting that it has pervaded the majority of economists' everyday practice.

Hence Friedman's claim cannot be taken literally. What he must have had in mind is the milder view that some traits typical of the Walrasian approach had been imported into Marshallian economics – reminiscent of the contamination theme evoked above. Yet it all remained quite vague. Moreover, formalisation, if this is what it was all about, was still at an infant stage.

Furthermore, on reading Section V it is unclear whether Friedman had in mind that the Marshallian and the Walrasian approaches were pursuing different purposes or whether he indicted the Walrasian approach for having lapsed from some purpose that would have been common to both of them. He started by asserting that they pursued different purposes. Yet, when it came to documenting this difference, he surreptitiously shifted towards denouncing abstractness, mathematical elegance and means becoming ends, all of which suggests that Walrasian theory had derailed from the supposed common purpose of economic theory.

Even this derailment claim is poorly supported. Friedman's opposition between mathematical elegance and relevance has little grounding. While mathematical elegance has indeed been a distinctive feature of neo-Walrasian theory — which, remember, had not seen the light of day at this time — Walras's own work can hardly be characterised in this way. If anything, Walras's mathematics was, as noted by Harrod (1956), "clumsy and tortuous".

*A fair portrait of the Walrasian approach?*

The Lausanne economist would hardly have recognised his brainchild in Friedman's sketchy portrait. Take the statement that to Walras "facts are to be described, not explained". Assuming its meaning is clear, which is questionable, I do not see how it relates to Walras's methodology. Likewise, having Walras ranked as a defender of the realism of assumptions looks odd. Ascribing to him the view that a "theory is to be tested by the accuracy of its "assumptions" as photographic descriptions of reality, not by the correctness of the predictions that can be derived from it" (Friedman [1949] 1953: 91) indicates a total ignorance of his methodological principles. It is true that, unlike Friedman, Walras did not believe that economic theory needed to be gauged by its predictive capacity. Yet, he never viewed the realism of its assumptions as the criterion by which to assess it. Finally, another indication of Friedman's ignorance of Walrasian theory is his claim that Walras adhered to monopolistic competition and treated firms as monopolies (see point (c) on p. 8). In fact, nothing was more alien to Walras than such a view.

The question must then be raised of what exactly Friedman understood by Walrasian theory in his 1949 article. I doubt that it was might be expected, i.e. the theoretical corpus to be found in Walras's *Elements of Pure Economics*. To be bold, my conjecture is rather that, to Friedman, Walrasian theory was a catchword for all that he rejected, any model taking the economy as a whole as its object, any theory severed from empirical verification, Keynesianism *à la* Lange, and monopolistic competition models!

*Friedman's claim that the Marshall–Walras divide is not a matter of partial versus general equilibrium*

Can Friedman be taken at his word when claiming that Marshallian analysis is full-fledged general equilibrium? This question is hard to answer because of a lack of agreement about the proper definition of general equilibrium analysis.

My own definition is that a model can be considered as belonging to general equilibrium analysis as soon as its object of study is an entire economy rather than a fraction of it. All models that fail to meet this criterion must be considered as partial equilibrium models. The joint study of two markets (for example the market for a given good and the markets for its inputs) still constitutes partial equilibrium. As soon as this definition is adhered to, the matter is settled: Marshallian analysis *à la* Friedman, centred on the study of an industry, is partial equilibrium.

Unfortunately, supporters of Marshall and Friedman will probably disagree with my definition. In private correspondence, Kevin Hoover cast the Friedman-Marshallian point of view as follows:

> For Friedman, partial equilibrium refers to a market in isolation. But he he often wanted to consider a partial analysis of general equilibrium models (which is not the same as partial equilibrium). Such an analysis for Friedman involves focusing on one aspect of the system, while not ignoring the rest (to ignore it would be partial equilibrium), but instead summarizing it compactly using, for example, some portmaneau variables.

To get out of the impasse, we may to Friedman's comment on Christ, where he pleaded for industry studies as opposed to studies that try to grasp the economy as a whole, may help us getting out of the impasse. This divide actually comes close to my own way of putting the alternatives, except for terminology. As aptly argued by Friedman, industry analysis has advantages of its own that are lost when the economy as a whole becomes the object of study. Why wanting to to brand it as the general equilibrium analysis as if the lack of engaging in the latter type of work was an unbearable stigma? Nothing but confusion is to be gained by such a semantic move.

Friedman's claim that Marshall's Mathematical Note XXI generalised his partial equilibrium into a general equilibrium analysis breaks down as soon as the line between general equilibrium and non-general equilibrium is drawn along my line. His note is concerned with the phenomenon of joined and composite supply and demand. Clearly an element of interdependency is involved. Yet widening the scope of the analysis from a single market to a group of related markets cannot be viewed as tantamount to analysing the entire economy. Likewise, the fact that Marshall wrote to Clark that all his life had been devoted to giving flesh to this note should not be taken for more than it was, a declaration of intention. Authors must be judged on their theories rather than on their meta-theoretical comments.

*The relationship between the different articles surveyed*

Let me finally relate the 1949 article to Friedman's other, contemporary, articles surveyed above. A divide can be de drawn between the Triffin review, the Lange review, the Christ comment and the 1953 article, on the one hand, and the 1949 article and the Jaffé review, on the other. While no mention is made of Walrasian theory (nor, for that matter, of the Marshall–Walras divide) in the former articles, the latter two devote substantial space to this topic. However, when the question is raised as to whether a new criticism, that specifically addresses Walrasian theory, is present in the 1949 article, the answer is in the negative. That is, in the 1949 article Walras may well enter the picture, and be criticised, yet no new grounds are presented compared to the argumentation found in the other articles. The only difference is that this argumentation is now explicitly addressed to Walrasian theory rather than to imperfect competition theory *à la* Chamberlin or to Keynesian theory *à la* Lange.

Next compare the 1949 article with the 1955 review. While the former is virulent, the latter, although still dismissive, is milder in tone. In particular, the Jaffé review improves on the 1949 article by bringing out the difference in purpose between Marshall and Walras more satisfactorily. Walras is no longer assessed through a Marshallian lens. The specificity of his project is recognised.

Two related factors may be evoked to explain the difference between the 1949 and the 1955 articles. The first is that, when writing his 1949 article, Friedman took it for granted that imperfect competition, Walrasian theory and Keynesian theory were closely associated. Triffin had claimed that a deep affinity linked imperfect competition and Walrasian general equilibrium, to the effect that a synthesis between them was within reach. Lange had argued that Walrasian theory was the proper language in which to embed the Keynesian claim that market forces could be unable to maintain full employment. Friedman did not question Triffin's and Lange's co-optation of Walrasian theory. Saddled with such bedfellows, Walrasian theory could hardly look congenial to him. Moreover, if imperfect competition, Walrasian theory and Keynesian theory were all different aspects of the same approach, they could be criticised in the same way.

However, what Friedman failed to see was that the synthesis between Chamberlin and Walras evoked by Triffin was just wishful thinking. Actually, Triffin's thesis had hardly made the first step of constructing such a synthesis. The same was true for Lange's bridge-building between Walras and Keynes. With the benefit of hindsight it has turned out that they were both wrong. In other words, Friedman built his 1949 Marshall−Walras divide on the false premise that Triffin and Lange were right in arguing that Walrasian theory, on the one side, and either imperfect competition or Keynesian theory, on the other, were compatible.[15]

The second explanation is that, in 1949, Friedman had only an indirect knowledge of Walrasian theory while the works against which he felt it necessary to react were claiming a Walrasian affiliation. Professor Blaug's testimony can be called in support of this view, as he has written to me that Friedman told him that the first time he read Walras was when he reviewed Jaffe's translation. Once Walras's *Elements* became available in English, Friedman realised that Walrasian theory did not carry the associations which Triffin and Lange had wanted to impose upon it. It was still not his cup of tea, but at least he recognised its specificity and was ready to give it due credit.

## 5. Giving the Marshall−Walras divide a stronger foundation

Although this paper has thoroughly criticised Friedman's views, it should not be considered a piece of "Friedman-bashing". On the contrary, Friedman should be hailed for having blazed the trail of the Marshall−Walras divide. Contrary to the prevailing opinion that the difference between the two approaches was well recognised at the time, I think that he initiated such a recognition. Surprisingly, no elaboration of his insights have seen the light of day. As stated

[15] An extract from Hammond (2003) is worth quoting in this respect for it reveals that Friedman's mistake is still being made: "So the impetus of Friedman's discussion of methodology with George Stigler was the challenge posed by *Walrasian monopolistic competition* to Marshallian industry analysis" (2003: 16; my emphasis). Treading in Friedman's footsteps, Hammond takes it for granted that Walrasian monopolistic competition

in the introduction, several present-day economists have endorsed Friedman's view of the Marshall–Walras divide. However, instead of trying to strengthen his arguments, they have usually contented themselves with quoting a few of Friedman's telling passages as if he had clinched the issue. It is almost as if they had learned their Walras exclusively from Friedman!

My own view is that the Marshall–Walras divide provides an important clue to understanding the development of economic theory in the twentieth century, but that the matter is much more complicated than Friedman thought. To draw the full contrast between the Marshallian and the Walrasian research programmes, a wider range of criteria needs to be considered: the purpose of the inquiries and the methodological strategies adopted, the underpinning conception of equilibrium, the assumptions made about trade technology and, finally, the depiction of agents' decision-making processes, in particular the information set that is supposedly available to them. Friedman, for his part, touched only on the first of these criteria, and too vaguely.

At this late stage, I cannot go beyond their brief evocation. As to purpose and method, two aspects should be separated. The first concerns the aim ascribed to theory. Here Friedman was broadly right in his 1955 article. The Marshallian approach aims at solving concrete puzzles, while the Walrasian approach is concerned with matters of principles, of a more philosophical nature – questions such as "Is a decentralised economy logically conceivable, viable, efficient, etc?" This is not a question that would have interested Marshall! The second aspect, also touched on by Friedman, concerned what Hicks called the "general equilibrium strategy". Walras's stroke of genius was to realise that it was necessary to take an entire economy at once as the object of study in order to achieve his aim. Simplifications were, of course, to be introduced, yet they needed to pertain to the characterisation of the economy rather than to fraction it into separate sub-entities. Thus, Walras started his analysis with the most rudimentary economy possible, a two-good economy, where the two goods between them constituted the entire economy. Marshallians take a different road. Theirs is a two-step methodology. Its first stage consists of the study of an industry. The underlying rationale is clear: the choice of a more limited object of analysis allows it to be studied in more depth. The piecing together of the separate industries is left for a further stage of the research programme, possibly to be postponed indefinitely. Friedman's comment on Christ is an apt

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existed and raised a challenge to Marshallians. But the truth is that such a theory was non-existent. There was no reason for putting the Walrasian label on monopolistic competition.

defence of this strategy. Oddly enough, he did not refer to this difference in research strategy when contrasting the Marshallian and the Walrasian approaches.

As to equilibrium, my claim is that the Marshallian and the Walrasian approaches rest on different conceptions of equilibrium.[16] The Marshallian approach belongs to the traditional stationary equilibrium concept, where equilibrium is viewed as a state of rest or centre of gravity. Walras more or less unwittingly inaugurated a new approach, which was further developed by Hicks in *Value and Capital,* and which is currently espoused by Lucas. The fundamental difference lies in the assumption made about the data characterising the economy or the industry under study. The stationary equilibrium conception is concerned with the study of the economy over a given time span, the "period of analysis" during which the fundamental data of the economy or the industry are supposed to remain unchanged. Only temporary and reversible changes in non-fundamental data are admitted. Thus, the world depicted is one of basic immutability. This constraint of unchanged data is removed in the Walrasian–Hicksian conception, which is therefore better equipped for the study of a changing world.

A third criterion, underlined by authors such as Clower and Leijonhufvud ([1975] 1984) and Colander (1996), concerns trade technology, i.e. the assumptions about the institutional set-up through which markets work. For lack of space I cannot enter here into a description of the way in which this issue is solved in the two approaches, yet it is my contention that the two scenarios are significantly different.[17]

As to the informational dimension, I argue elsewhere (De Vroey 2003) that, while both the canonical Marshallian and the canonical Walrasian models were characterised by the perfect information assumption, this assumption has a different meaning in each of them. Somewhat surprisingly, perfect information à la Marshall involves a higher informational requirement than perfect information à la Walras, the reason being that in the Marshallian set-up the absence of the auctioneer needs to be compensated by agents' omniscience as to market conditions.

These are the main criteria that, in my opinion, should be considered when drawing the full contrast between the Marshallian and the Walrasian approaches. Note that I disregard the point on which Friedman insisted the most, namely the contrast between a formalised and a non- or less formalised approach. The passing of time has shown that this is a superficial

[16] Cf. De Vroey (1999b, 2002).
[17] On this, see De Vroey (1999a).

opposition. Friedman's view that formalism and empirical relevance are necessarily on a collision course has been counter-evidenced. On the one hand, themes that should be labelled "Marshallian" because they concern concrete issues have gradually been addressed in a highly formalised way. On the other, for better or worse, with the growth of the real-business-cycle tradition, Walrasian theory has stepped down from its Olympus of abstractness to come to grips with empirical work.

Finally, there is one further point on which I would plead for a change in attitude. Until now, most authors who have ventured to defend the Marshall–Walras divide have done so as a reaction against the development of the neo-Walrasian approach, which they perceived as an unfortunate evolution. While I doubt whether this approach has ever become as dominant as was claimed by Friedman and his followers, the point that I want to make here is different. To these authors, bringing this divide to the fore amounted to pleading for the superiority of what they saw as the underdog, the Marshallian approach. My own view is that the position that there is a Marshall–Walras divide can be held without having to defend the superiority of either. Of course, economists may have their personal preferences. Yet there is no reason for stating that one approach is better than the other. They are just different. To use present-day labels, why should industrial organisation be superior or inferior to macroeconomics?

## 6. Concluding remarks

My aim in this paper has been to assess the validity of Friedman's claim, made in his 1949 article, as to the existence of a Marshall–Walras divide. I have shown that Friedman's arguments do not stand up to scrutiny, and have advanced the view that his real target when criticising the Walrasian approach was imperfect competition *à la* Chamberlin and Keynesianism *à la* Lange rather than Walrasian theory *per se*. The change in tone between Friedman's 1949 article and his 1955 review of Jaffé's translation of Walras's *Elements of Pure Economics* has also been brought out. I have claimed that on reading Walras's work directly, rather than on seeing it through the eyes of Triffin or Lange, Friedman must have realised that Walras was not really keeping the bad company he had attributed to him. As a result, Friedman ceased to feel the need to attack him. While keeping his Marshallian affiliation, he followed Stigler to a position combining praise for Walras's achievement with the conviction that it was not the right line to take. Thus, to my mind, the 1955 article is a better embodiment of Friedman's definitive view as to the relationship between Marshall and Walras than the 1949 article.

My judgement that Friedman's justification of the Marshall–Walras divide was unsatisfactory should not be taken as meaning that he was wrong in believing that the two approaches were poles apart. On the contrary, he should be credited for having blazed the trail for the recognition of this divide, which in my opinion constitutes a leading thread for understanding the development of economic theory over the last century. The task ahead is to provide a stronger foundation for Friedman's insight.

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