On the Right Side for the Wrong Reason: Friedman on the Marshall–Walras divide [ϕ]

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(revised version)

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.

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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a contrast between the Marshallian and the Walrasian approaches, Friedman expressed his regret as to the latter having superseded the former.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 contemporary 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 Lawrence 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).

Most of these papers pursued a critical aim. As will be seen, Friedman had many foes at the time – the concept of indifference curves, Hicksian demand analysis, Henry Schultz's, work aiming at measuring interrelated demands, macro-econometric models *à la* Klein, monopolistic competition, Keynesianism *à la* Lange. To Friedman, the common thread in all

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

these disliked models was their connection to Walrasian theory. Hence his attack on the latter – and the commentator's difficulty in assessing whether his target was Walrasian theory *per se* or the models he associated with it. His positive hero was Marshall, whose insights, he felt, had been unduly discarded, and his indictment of the 'Walrasian' models rested on their alleged departure from the Marshallian agenda.

The questions that I wish to address in this paper ensue from these observations. Was Friedman right in wanting to draw a contrast between the Marshallian and the Walrasian approaches? Did he characterise it in an apposite way? Should we follow him when he claims that the Marshallian programme was good and the Walrasian bad instead of simply opposing them as alternatives? What did he actually understand by the 'Walrasian' modifier? Why was he so strongly anti-Walrasian in his 1949 article and less so in his 1955 piece?

A complete discussion of these issues must evolve at two levels, concerned respectively with the 'politics' underlying Friedman's stance and the contents of his arguments. The first is a tale of a group of Chicago economists (Milton Friedman, George Stigler, W. Allen Wallis and Aaron Director) gradually taking the reins of the University of Chicago community of economists, with the aim of implementing a defence of free-market economic liberalism's agenda.[2] A twofold offensive, external and internal, was implied. The external fight opposed Chicago and Harvard and concerned the relevance or otherwise of the monopolistic competition theory put forward by Chamberlin.[3] On the internal front, one enemy was Walrasian theory as upheld within the Department first by Henry Schultz (who died in 1938) and later by Oscar Lange. Adhesion to the Walrasian approach was also the hallmark of some prominent economists (such as Tjalling Koopmans, Jacob Marschak and Lawrence Klein) on the Cowles Commission (which had been relocated in Chicago in 1939). While they were only residents in the University's Department of Economics, their presence might be viewed as a threat to the laisser-faire agenda since, in the wake of the debate on the possibility of socialism, Walrasian theory was associated with collectivism, or at least with social-democracy. So, when studying what was going on in Chicago in the 1940s and 1950s at the level of ideology and politics, a possible shorthand answer to the question 'What did Friedman mean by 'Walrasian'?' is 'socialism'.[4]

While, for all its bluntness, I may sympathise with this interpretation, it does not provide a complete assessment of the issue. Whatever Friedman's motivation for entering into the fray and attacking Walrasian theory, his objections ended up being expressed as theoretical

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[2] Cf. Stigler (1988: 150).
[3] See Leeson (2000).
[4] As stated by Mirowski and Hands, " In the minds of many at Chicago, Walrasian mathematical theory became conflated with socialism, crude numerical empiricism, and politically naïve welfare economics" (Mirowski and Wades, 1998: 268).

statements whose intrinsic validity needs to be examined. The shortcoming of Mirowski and Hands work (Mirowski (2002) and Mirowski and Hands (1998)), which provide vivid narratives of the contextual-historical dimension, is that they fail to discuss this validity. The aim of my paper is complementary to theirs, in that its exclusive concern is an assessment of Friedman's theoretical arguments.

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 States 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 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' *Value and Capital* (1939; second edition 1946), 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 be to please Friedman. 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. 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, a regret as to Marshallian tools being thrown away too airily.[5] The object of Friedman's dissatisfaction was the mutation that Marshallian demand theory had undergone. He felt that the newly prevailing interpretation of the Marshallian demand curve was unsatisfactory and needed to be replaced by an alternative more faithful to Marshall's interpretation.

> The currently accepted interpretation can be read into Marshall only by a liberal – and, I think, strained – reading of his remarks, and its acceptance implicitly convicts him of logical inconsistency and mathematical error at the very foundation of his theory of demand. More important, the alternative interpretation of the demand curve that is yielded by a literal reading of his remarks not only leaves his original work on the theory of demand free from both logical inconsistency and mathematical error but also is more useful for the analysis of most economic problems. (Friedman [1949] 1953: 48).

While some readers may be at a loss to see who Friedman had in mind when speaking of the 'current interpretation,' his target was actually well defined: the ideas developed by Hicks in his analysis of demand in part I of *Value and Capital* and expanding on Hicks and Allen (1934), on the one hand, and Henry Schultz's work (his 1935 article his book, *The Theory and Measurement of Demand* (1938)), on the other. Both Hicks and Allen, and Schultz had independently rediscovered views that had been put forward by Slutsky's in a 1915 *Giornali degli Economisti* article.[6] Hicks's views soon gained wide recognition. Although Hicks and Schultz shared the same insight about building demand theory on the apparatus of indifference curves, they pursued different purposes. While Hicks was interested in pure theory without aiming at measurement, Schultz's aim, in contrast, was to make the new demand theory amenable to empirical testing. Friedman wrote his master's dissertation at the University of Chicago under Schultz's supervision. He was also Schultz's research assistant for a year. Yet at the end of the day he barely adhered to Schultz's views.[7] He was not alone in this respect. Critiques of Hicks's conception had already been delivered by Stigler (1939) and Knight (1944),[8] and in the 1942 Festschrift edited by Lange *et al.* in memory of Schultz,

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[5] This theme is also visible in Friedman's lecture notes for his 1940s Economics courses at the University of Chicago (Hammond 1996: 31).
[6] For a study of the relationship between these authors, see Chipman and Lenfant (2002) and Moscati (2004).
[7] Hammond (1992: 223) gives a vivid account of Friedman's opinion of Schultz. See also Stephen Stigler (1994: 1200).
[8] Knight introduced his paper as follows: "The objective of this paper is largely negative — to criticise certain recent innovations in the treatment of demand which have been generally hailed as representing an advance but which, in the writer's opinion, constitute a movement in a backward direction. The particular reference is to the

Friedman himself had already written an article (jointly with Wallis) criticising the indifference curve notion (Wallis and Friedman, 1942).

Hicks' aim was to re-build the Marshallian theory of demand while disposing of the idea of diminishing marginal utility. Beginning with the case of two goods, Hicks studied the effect of the variation in the price of X on the demand for Y, the nominal income of the agent under study being fixed. The renown distinction between the substitution effect and the income effect ensued. The analysis was subsequently extended to the case of a great number of goods.

The contrast drawn by Friedman between the Hicksian reconstruction of Marshallian theory and what he believed to be its proper interpretation is as follows. In Hicks' model the money income and the price of every other commodity is fixed, on top, of course, of tastes and preferences. The real income is changing due to the eponymous effect. In contrast, Friedman argues that the real income should be considered fixed as well as the price of all close substitutes for and complements of the commodity considered that. Whenever a change in the price of the good considered occurs, the average level of prices of unrelated goods needs to be changed in such a way as to freeze the purchasing power of money. Adopting Friedman' s stance amounts to eliminating the income effect, thus leaving only the substitution effect to be active. Therefore, the Marshallian demand curve *à la* Friedman is necessarily negatively sloped, what is untrue fro the Hicksian demand curve.

Friedman was critical of the indifference curve apparatus in general, but his precise target was its use by Hicks, i.e. employing the substitution effect while keeping utility constant – that is, the consumer is compensated by just enough money to bringing him or her back to his or her initial indifference curve. Friedman dismissed this view on the grounds that indifference curves were not an observable phenomenon. In contrast, he endorsed Slutsky's formulation, where the substitution effect gives the agent enough money to get back to his or her initial level of consumption. Slutsky was thereby tilted towards Marshall's side. [9]

In defence of his 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

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treatment of demand and utility by J.R. Hicks, pioneered by E. Slutsky, and also followed, and more or less independently worked out, by Henry Schultz and many others" (1944: 289).

[9] "On my interpretation, Marshall's demand curve is identical with the constructions introduced by Slutsky in his famous paper on the theory of choice, namely, the reaction of quantity demanded to a 'compensated variation of price', that is, to a variation in price accompanied by a compensating change in money income. Slutsky expressed the compensating change in money income in terms of observable phenomena, taking it as equal to a change in price *times* the quantity demanded at the initial price" ([1949] 1953: 52-3).

> [Marshallian] approach. It may be a useful expository device to bring home the mutual interdependence of economic phenomena; itcannot 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).[10]

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. While I doubt that Friedman was able to make the point, I will shrink from entering into this issue, and turn at once to the paper's penultimate section.[11]

For all its brevity, Section V develops several distinct themes. Instead of trying to paraphrase what Friedman wrote, I find it preferable to quote at length from this section.

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:

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[10] See also Friedman ([1949] 1953: 85 and 91).
[11] Let me nonetheless give a quotation from Aldrich that is rather dismissive: "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) 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 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 proposed 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).

Friedman might aptly have evoked the names of Walras and Marshall in this discussion. In effect, studying the individual structural equations separately was nothing else than doing Marshallian industry analysis, while Klein's attempt at grasping the economy as a whole suggested a Walrasian affiliation. Yet he did not do so.

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.

[12] 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).

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

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).[13]

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[12] According to Leeson, "his methodology of positive economics was formulated as a "reaction" to Robinson and Chamberlin" (2000: 62). On this, see also Hammond (2003: 13 and *seq*.).

[13] 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 (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

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 work 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).

Note finally that, in spite of his praise to Walras, Friedman still stuck to his 'empty tautologies' indictment, as the following excerpt makes clear:

> 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).

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phenomena of our economic life, no comprehensiveness, few reservations and little nuance or subtlety. The presentation is extremely clumsy" (Harrod, 1956: 311).

3. Friedman's subsequent views

Two main claims were made in Friedman's 1949 article, first that there existed a better alternative to Hicks' account of demand theory, second, that the Marshallian and the Walrasian approaches were poles apart. Since Friedman continued to contribute to economic theory for almost a century, it worth raising the question as to whether he stuck to his earlier views. The answer is yes.

As to demand theory, it suffices to turn to the first chapter of his *Price Theory* essay (1976, second edition; first edition 1962). It starts with the exposition of the 'old' Marshallian conception of demand and includes the different concepts which Hicks wished to ban. As in the 1949 article, the 'ordinary' demand function is criticised 'because changes in real income are not rigorously excluded' and contrasted with a favoured alternative 'in which the real income, in the sense of money income divided by the purchasing power of income, is held constant' (1976: 29).

A concession to orthodoxy, Friedman broaches the notion of indifference curves – an apparatus fiercely criticised in Wallis and Friedman (1942) on the grounds of its separating preferences form prices and income. Yet when it comes to deriving demand functions from indifference curves, he falls back on the wedge between Slutsky and Hicks, with the former being the 'good' and the latter the 'bad guy! Thereby, the earlier contrast between a conception of demand that is somewhat loose yet lends itself to empirical work and another, which for all its possible higher rigor fails to do so, turns up again:

> The advantage of the Slutsky measure, even though in one sense it is an approximation which the Hicks measure is not, is that it can be computed directly from observable market phenomena and behaviour, namely prices and quantities purchased. The Hicks measure cannot; it requires knowledge of the indifference curves. (1972: 50)

Likewise, there is little doubt that Friedman remained faithful to his earlier views as to 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).[14] [15]

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).

4. A criticism of Friedman's Section V argument

Friedman's 1949 paper section V raises a host of questions. 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

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[14] Lucas concurred wirth 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).

[15] Unfortunately, when asked to delve into the reasons why he endorsed 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 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).

approach be considered fair and well-documented? Finally, how does his 1949 article relate to his other pieces?

The alleged dominance of the Walrasian approach

Considering the time at which 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, but is it a correct account of what was going on? I doubt it.

At the time, Walras's *Elements* had not been translated into English. Arrow and Debreu had not started their collaboration. Only a few sources (e.g. Stigler (1941) and the works of Cassel and Wicksell were available to economists who wished to become acquainted with Walrasian theory while being unable to read French. Admittedly, a revival of Walrasian theory, triggered by Hicks's *Value and Capital*, was effectively under way. A few works of mathematical economics had seen the light of day, led by authors such as Moore, Hotelling and Schultz. Their aim, which could not but have looked odd to Walras, was to render the Walrasian conceptual apparatus statistically operational. Thus, their perspective was poles apart from the line pursued by Arrow, Debreu and Mackenzie, which I for one view as being more faithful to Walras's own methodological precepts. It is also true that Friedman was in direct contact with some of the few Walrasian American economists and enjoyed many opportunities to confront his views with them when attending seminars at the Cowles Commission.

Yet, all in all, these economists were a definite minority in the profession. Therefore, Friedman's image is totally inappropriate. At the time, the majority of economists were not walking with Walras.

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. In fact, nothing was more alien to Walras than such a view.

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 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".

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 he claims that Marshallian analysis is a fully fledged general equilibrium theory? I do not think so.

The most plausible definition of general equilibrium analysis consists of stating that a model can be considered as 'general equilibrium' as soon as its object of study is an entire economy rather than a fraction of it. The immediate implication is that 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) constitutes partial equilibrium even it is concerned with an interdependency phenomenon. 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, Friedman refused this characterisation. The underlying reason, I believe, is a matter of territorial claims. The term 'general equilibrium' has positive connotations of completeness, while the term 'partial equilibrium' suggests an admission of incompleteness. Hence the struggle to stop the Walrasian camp having a monopoly over the positively connoted field

As soon as it is accepted that general equilibrium must be equated with the analysis of an economy as a whole, Friedman's stance can be ascertained unequivocally: he is against it. It suffices to return to his comment on Christ's assessment of the Klein model, where he argued that industry analysis has advantages of its own that are lost when the economy as a whole becomes the object of study. Why wanting to brand the study of an industry as 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.

As to Friedman's claim that Marshall's Mathematical Note XXI generalised his partial equilibrium into a general equilibrium analysis, it breaks down as soon as the line between general equilibrium and non-general equilibrium is drawn along the above line. Marshall's 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 John Bates 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.

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Three main lessons can be drawn from my survey. First, it is surprising to see how numerous Friedman's foes were at the time. The concepts or works that he disliked and felt the need to attack ranged widely: indifferences curves, Hicksian demand theory, Schultz's statistical demand curves, econometric models aiming at representing the economy as a whole, monopolistic competition and Lange's Keynesianism. All this conveys the image of Friedman as an outcast, or at least as somebody fighting against the tide. Positively, he had one hero, Marshall, and to him this meant three associated features: (a) the search for concrete truth and an understanding of the ordinary business of life; (b) the need to focus on the study of particular industries; and (c) the need to resort to concepts that were directly operational, i.e. measurable. The second lesson is that Friedman seems to have believed that all the models he disliked were 'Walrasian'. The third lesson is the difference in tone between Friedman's criticism of Walrasian theory in his 1949 and 1955 articles. In the latter, he is still dismissive of the Walrasian approach, but in a far less vehement way.

Two tasks ensue from these observations. The first is to assess whether there are good grounds for characterising all the above models as Walrasian. That is, what might the 'Walrasian' modifier have meant for Friedman, and does his understanding of it stand up retrospectively? The second is to elucidate his change in tone between 1949 and 1955.

5. Friedman's understanding of the 'Walrasian' modifier

As I see things, Friedman's characterisation of Walrasian theory can be summarised in the following four points:

1) Walrasian theory aims at discovering fundamental principles governing abstract economies.
2) Walrasian models comprise theoretical concepts that have no direct measurable counterparts.
3) Empirical measurement and prediction-making and testing are no part of the research programme.
4) Walrasian models are concerned with the economy as a whole rather than with a part of it, such as an industry.
5) The Walrasian approach is formalised; whenever necessary, relevance will be sacrificed to rigour.[16]

On all these points, the Marshallian approach is supposed to stand in sharp contrast to the Walrasian. Moreover, to Friedman, bringing out a contrast between the Marshallian and the Walras approaches was tantamount to pleading for the superiority of the former over the latter. So all the above features were considered by Friedman as flaws and reasons to condemn any model which adopted them.

The question must be asked as to whether such a characterisation stands up to a retrospective test – would a present-day defender of the Marshall-Walras divide agree with Friedman's way of putting the issue? As expected, the answer is not clear cut. Positively, Friedman's characterisation of the scope and aim of Walrasian theory is acceptable. Negatively, three critical observations come to mind. First, Friedman insists too much on broad methodological traits such as those above, failing to realise that the contrast between the two approaches is also a matter of narrower methodological choices, the effect of which is to put the theoretical construction on divergent tracks. These bear on the notion of equilibrium that is adopted, the role of money, the trade organisation and information assumptions, the treatment of time, etc.[17] Second, the fifth criterion, on which Friedman insisted, has failed to pass the test of time. Accepting that game theoretical and industrial organisation models can be considered neo-Marshallian, it has turned out that the Walrasian approach no longer has a monopoly over

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[16] Some further traits mentioned in Section V of Friedman (1949), such as the monopolistic nature of Walrasian theory, have been dropped because they are patently mistaken.

[17] On this, see De Vroey (1999a, 1999b, 2003).

formalisation. Finally, Friedman's manicheist stance must also be questioned. In my opinion, no one approach should be considered superior to another. They are rather alternative research programmes, having each its pros and cons.

In order to qualify as Walrasian, a given model must abide by the above criteria.[18] Let me then ask whether there are good reasons to consider the models criticised by Friedman as Walrasian according to his own definition.

Hicks' demand theory

Unlike Friedman (and myself), Hicks saw no fundamental reason for opposing Marshall and Walras. To him, these two authors held complementary approaches (Hicks ([1934] 1983). He viewed the study of the Marshallian market as a first step in the analysis of the economy as a whole.[19] Be that as it may, separating the elements of Hicks's *Value and Capital* book that are Marshallian from those that are Walrasian is quite easy. The very fact that Part II of the book is entitled 'General equilibrium analysis' indicates that Part I, entitled 'The theory of subjective value', where Hicks develops his demand analysis, is not a general equilibrium theory. In effect, Hicks's recurrent reference in the chapters composing Part I of *Value and Capital*is Marshall. So Hicks's demand theory analysis belongs to the Marshallian part of the book rather than to its Walrasian part.

It may be argued that the fact that every commodity composing the economy is individually listed in the Hicksian demand theory makes it Walrasian.[20] Yet this is not a sufficient condition. For example, the category of income as understood by Hicks in these chapters – a quantity of money that agents have at their disposal before entering the goods markets and which accrued to them from their antecedent sale of inputs – has no place in a Walrasian framework.

Finally, comparing Hicks's demand theory with the set of conditions above, it turns out that it abides by conditions (1), (2) and (3), but definitely not by condition (4).

To conclude, it is dubious whether the Hicksian demand theory can be characterised as specifically Walrasian. Friedman's dissatisfaction with it may well rest on a trait that he associates with the Walrasian approach – basing the analysis on concepts having no observable counter-parts – yet this association is far from compelling. The dispute between

[18] Henceforth, I will consider only the first four conditions, eliminating the fifth one.
[19] "If a model of the whole economy is to be securely based, it must be grounded in an intelligent account of how a single market functions' (Hicks 1965: 78).
[20] 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).

Hicks and Friedman is one that takes place within the Marshallian partial equilibrium framework rather than opposing the Walrasian and Marshallian approaches. As a result, Friedman's transition from the discussion of demand theory in Sections I to IV of the 1949 article, where he discusses demand theory, to Section V, devoted to the broader Marshall-Walras contrast, is far from straightforward. While there is a link between these two parts of the article, they are hardly part and parcel of each other. In fact, they could be split without harming the argument in either.

Schultz

Schultz was an author who claimed to be part of the Walras-Pareto lineage while pursuing the aim of making Walrasian concepts statistically measurable, a project that Walras would have refused to consider as his. Be that as it may, Schultz cannot be considered Walrasian according to Friedman's criteria. His agenda fails to abide by conditions (1) and (3), and possibly also condition (2). Friedman might have been expected to credit Schultz with having breached the Walrasian reluctance to enter into empirical work, but this was not the case. In Friedman's eyes, something more than merely engaging in empirical work was needed, namely that the empirical results exerted a feedback on the theoretical structure.[21]

Moreover, Schultz's work shows that there is many a slip 'twixt cup and lip. To achieve his aim of measuring Walrasian concepts, Schultz had to go through many twists as, to give just one example, his transmogrification of the Walrasian notion of endowment into the Marshallian notion of monetary income witnesses. While the theoretical and empirical parts of Schultz's paper and article are excellent contributions on their own, the link between them is virtually non-existent.[22]

To conclude, there is more to Friedman's dislike of Schultz's work than its Walrasian character. Moreover, Schultz should not be considered a fully fledged Walrasian economist.

[21] This is evidenced in a letter that Friedman wrote to Edwin Wilson in 1946 in which he criticised Schultz in the following terms: "Schultz took the theory as fixed and given, and tried to measure what he thought were essential functions in the theory. He imposed extremely high standards of care and thoroughness on the measurement process — but he nowhere attempted what seems to me the fundamental important task of reformulating the theory so that it would really generalize the observable data; he also tried to wrench the data into a pre-existing theoretical scheme, no matter how much of a wrench was required" (Stigler 1994: 1200).

[22] This has led Mirowski and Hands to conclude that "it is seldom recognized that [Schultz's] book is essentially a swan song for Walrasian economics: Schultz bravely reported the empirical debacle in detail and then produced a litany of excuses why things had not worked out as hoped" (1998: 264).

Lange

Price Flexibility and Employment was an attempt to combine Walras and Keynes, two authors to whom Friedman was strongly opposed. Was there then some domino process at work, Friedman's dislike of Keynes spilling over to Walras? Actually, the matter is more complicated than that. Recall Friedman's hailing of Keynes as a genuine Marshallian in Section V of the 1949 article. In other words, while Friedman was opposed to Walrasian theory because of its methodological principles, his opposition to Keynes had no such groundings. Keynesian theory was to be rejected, not, like Walrasian theory, because of its faulty methodological basis, but rather because it had been contradicted by empirical evidence.[23] So, Friedman's target in his criticism of Lange's book is less the Keynesian than the Walrasian element of the proposed Keynes−Walras alliance.

The irony here is that, while Walras's name does not appear in Friedman's review of Lange, of all the authors attacked by Friedman he was the only one who really deserved the Walrasian label. And, in effect, his theory abides by the criteria set up to this effect.

Triffin

Two reasons may explain Friedman's hostility to Triffin's project. First, it amounted to shelving the study of industries. The second objection has a more ideological nature. Triffin claimed that a deep affinity linked imperfect competition and Walrasian general equilibrium, to the effect that a synthesis between them was within reach.. This could not but displease Friedman on the ground of a bedfellows argument – any friend of any enemy must also be an enemy. Friedman did not question Triffin's co-optation of Walrasian theory. Saddled with such a bedfellow, Walrasian theory could hardly look congenial to him. However, what he failed to see was that the synthesis between Chamberlin and Walras evoked by Triffin was just wishful thinking. Triffin's thesis hardly took the first step towards constructing such a

[23] The following extract from Friedman's *Theoretical Framework for Monetary Analysis* makes this clear: "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).

synthesis. If the Marshall-Walras divide makes sense, it would be better to put monopolistic competition on Marshall's side (as argued in De Vroey, 2004).

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The general lesson to be drawn from this quick assessment is that, with the exception of Lange, the reasons for Friedman's dissatisfaction with the models he rejects actually have little to do with their alleged Walrasian character.

6. The relationship Friedman 1949 account of the Marshall-Walras divide and his review of Jaffé's translation of Walras' *Elements*

Finally, let me 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 factors may be evoked to explain the difference between the 1949 and the 1955 articles. A first one is the bad company explanation. Yet, as seen, it played only for the imperfect competition/ Walrasian theory connection. In 1949, Friedman might still have believed that Triffin was right in arguing that Walrasian theory and imperfect competition were compatible.[24] At the time, he 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.[25] 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.

The second factor relates to the political dimension evoked in the introduction. On the one hand, the Cowles Commission had left Chicago. On the other hand, the socialist debate had

[24] 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 wrongly takes it for granted that Walrasian monopolistic competition existed and raised a challenge to Marshallians.

[25] This point was confirmed to me by Professor Friedman in private correspondence.

petered out, and on reading Walras's *Elements*, Friedman must quite rightly have discovered that no plea in favour of socialism was to be found in it.

7. 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 in favour of it stand up to scrutiny only partially. I have also put forward the view that when he criticised specific models for their Walrasian flaws he might have had in mind other targets for which the Walrasian label served as a proxy. The change in tone, from a vehement to a softer anti-Walrasian stance, 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. 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 clue for understanding the development of economic theory over the last century. He was right in perceiving the need to draw the Marshall-Walras divide yet unable to give a solid argumentation as to its grounding. The task ahead is to fill in this lacuna.

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