Robert Solow's Non-Walrasian Conception of Economics

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ABSTRACT

The neoclassical synthesis has been defined as a bridge between Keynesian theory and Walrasian general equilibrium theory. The aim of this article is to show that founders of the neoclassical synthesis were not homogenous in their appraisal of the importance of Walrasian theory. To do so, we focus on Solow's contributions as a case study and examine the history of his lifelong criticism of what he called "axiomatics." According to Solow, the axiomatic approach aims at founding economics on one general and complex model based on first principles or axioms. In contrast, Solow advocated the use of a diversity of simple and partial models, which have practical utility, are realistic in their crucial assumptions, consider institutions and the evolving nature of the economy, and rely on common sense microfoundations. We conclude by suggesting that Solow can be characterized as Cournotian.

JEL CODES: B22; B31; B41.

KEYWORDS: Friedman; Marshall; MIT; Solow; Walras.

> I could define macroeconomics as non-Walrasian economics… Perhaps it would be enough to define macroeconomics as applied – or at least applicable – non-Walrasian economics. (Solow 1983a, 2).

1. Introduction

Robert Solow belongs to the generation of economists who elaborated and promoted the Post-War Keynesian mainstream.[1] Among these economists, prominent figures considered the IS-LM model – the cornerstone of that mainstream – as a Walrasian model. Oskar Lange introduced that idea in a 1938 article that was as important as John Hicks's 1937 article "Mr. Keynes and the Classics" in the training of young American Keynesians in the 1940s (Rubin 2016). The view of IS-LM as a Walrasian model was then developed by Franco Modigliani (1944), Lawrence Klein (1947) and Don Patinkin (1947; 1956). These economists also took some inspiration from Hicks's *Value and Capital* (1939) and Lange's *Price Flexibility and Employment* (1945), books that tried to translate Keynes's (1936) insights in the language of general equilibrium theory.

As a result, the Keynesian mainstream after 1945 – or 'neo-classical synthesis' after Paul Samuelson's expression in the third edition of his textbook *Economics* (1955, 212) – is often considered as an attempt to establish a bridge between Walrasian theory and Keynesian

[1] Robert Solow received a Ph.D. in economics from Harvard in 1951 – under Wassily Leontief's supervision – and joined the Massachusetts Institute of Technology (MIT) in 1949. In 1987, he received the Nobel Prize in Economics for his contributions to the theory and empirics of economic growth (Solow 1956b; 1957).

theory. As Wade Hands put it: "The neoclassical synthesis was a two-way street with influence between Walrasian and Keynesian ideas flowing both ways" (2012, 95).[2]

In retrospect, the importance American Keynesians gave to the Walrasian model might appear curious. Indeed, as is well known, Keynes was a Marshallian and one of the characteristics of the Marshallian approach is its opposition to the Walrasian approach (Groenewegen 1995, 778; De Vroey 2009a; 2012; Hoover 2006a-b). As Keynes put it in a 1934 letter to John Hicks, "I shall hope to convince you some day that Walras' theory and all the others along those lines are little better than nonsense!" (cited in Clower 1975, 4 f.n. 17). Keynes's immediate disciples shared that negative appreciation of Walras's contributions. For instance, in his review of Walras's *Elements* edited and translated into English by William Jaffé (Walras and Jaffé 1954), Roy Harrod (1956) wrote that "Marshall has far greater scope and depth than Walras" (315-6).

The aim of this article is to show that we can distinguish between Walrasian enthusiasts and Walrasian skeptics within the seminal authors of the Neoclassical Synthesis. To do so, we focus on Robert Solow's criticism of the Walrasian approach.[3] As we shall see, Solow did not

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[2] The quest for a bridge between Keynesian macroeconomics and Walrasian microeconomics was instrumental for the developments in macroeconomics. One of its results was the development of the fixed-price models of Robert Barro and Herschel Grossman (1971), Jean-Pascal Bénassy (1975), and Jacques Drèze (1975) (see De Vroey 2016). Another was the contributions of Robert Lucas (1972; 1975) who was inspired by his reading of Patinkin's 1956 *Money, Interest, and Prices* (Lucas 2004, 15). Finally, dynamic stochastic general equilibrium macroeconomics can be considered as a by-product of the debates between the disequilibrium school and Lucas's New Classical school in the 1970s (Backhouse and Boianovsky 2012).

[3] Though the focus of this article is Solow's contributions, we acknowledge that studying other prominent characters of the neoclassical synthesis – especially, of course, Paul Samuelson – would be interesting but would also deserve another whole article (see Backhouse 2017, chap. 18 and 24).

reject the Walrasian model itself (De Vroey and Duarte 2013) but the attempt to found all of macroeconomics on it.

We show that Solow's criticism of the Walrasian approach – or what he calls "axiomatics" – goes beyond ideological divergences and can be explained by his methodological commitments. Doing so, we fill a gap in the recent contributions to the history of Solow's contributions that have not examined – or not in a systematic way – the history of his criticism of the Walrasian approach (Assous 2015; Boianovsky and Hoover 2009, 2014; Goldfarb and Ratner 2009; Halsmayer 2014; Halsmayer and Hoover 2016; Thomas 2014). De Vroey and Duarte's (2013) can be considered as an exception. They study the evolutions of the definitions of the expression "neoclassical synthesis" and show how it gradually stabilized on the interpretation of a bridge between Keynesian and Walrasian economics. De Vroey and Duarte note Solow's criticism of the Walrasian approach and characterize him as a "side-by-side" economist, in the sense that Solow does not aim at building a bridge between Keynesian and Walrasian economics. Nevertheless, they do not document Solow's criticism of the Walrasian approach, nor study his favored methodology.

Before proceeding, we must clarify our historiographic approach. In what follows, we purposely refrain from defining the expression "Walrasian approach." Instead, we follow in Solow's footsteps to see how *he* defined it, and how and why he opposed it. Our approach is similar to the one adopted by Hirsch and de Marchi's (1990) in their book on Milton Friedman. Hirsch and de Marchi note that:

> When [Friedman] says something of a methodological sort he is trying to tell us about the way he does economics and believes that it should be done, usually in opposition to some prevailing alternative, and usually in some specific context. The resulting observations are therefore concrete, directed, partial. To try to make them into general principles may well be attempting to put on them a burden they were not meant to carry and will not bear. (153-4)

We believe that this quotation also aptly applies to the study of Solow's methodological views. Indeed, Solow also defends a specific way of doing economics and most often in opposition to the Walrasian approach. Solow has made explicit that his research rests on specific methodological commitments: "I have in the back of my mind a picture of the sort of discipline economics ought to be – or at least the sort of discipline I wish it were" (Solow 1985a, 328). [4] Moreover, as is also the case for Friedman, Solow's methodological views can be found in different types of contributions – academic articles, books, speeches – targeting a variety of audience in different periods. We thus apply to Solow what Hirsch and de Marchi apply to Friedman, that is, we examine what Solow "is trying to tell us about the way he does economics and believes that it should be done." The last sentence of Hirsch and de Marchi's above quotation is also useful in underlining the limitations of our historiographic approach: in our study of Solow's criticism of the Walrasian approach, we collect, group, and label the commonalities we can identify in Solow's arguments and that will be reflected in the titles of our sections. However, Solow himself did not necessarily use the same labels as ours. We thus offer in this article a reconstruction of the – often implicit – methodological principles that have guided Solow's research during more than half a century and which explain his opposition to Walrasian macroeconomics.

[4] Characterizing Solow as an anti-Walrasian economist also raises the issue of the characterization of his early works on multisectoral models with Wassily Leontief and Paul Samuelson and his works on linear programming leading to the influential volume by Dorfman, Samuelson and Solow (1958). Should all this be seen as a serious engagement with Walrasian economics? We postpone the discussion of this issue to future work on the *origins* of Solow's view on methodology.

2. Opposition between two conceptions of economics in Solow's contributions

Starting in 1956, Solow used different labels to distinguish two conceptions of economics: an approach related to Walrasian general equilibrium theory and his preferred method.

In 1956a, Solow published a review of Jaffé's translation of Walras's *Elements* (Walras and Jaffé 1954). At first sight, Solow seems to defend Walras from Friedman's attacks in his review of the book (Friedman 1955). Indeed, at the end of his review, Solow criticizes the idea that Marshall's contributions would be superior to Walras's:

> Walras is supposed to be endowed with all the defects of his virtues, to be a naive, primitive, clockwork economist with no feeling for anything beyond counting equations and unknowns (and even *that's* not a safe activity any more), to be perhaps the original bloodless abstractionist. The usual statement is that Marshall knew everything Walras knew and more, and had no defects. This impression of Walras is just wrong. (88)

Solow makes the target of those lines clear by referring in the same paragraph to "Friedman's verdict that Walras' sole preoccupation was with 'abstractness, generality and mathematical elegance' in distinction to Marshall's search for 'an engine for the discovery of concrete truth'" (89). In the same review, however, Solow criticizes Walras with arguments that are very similar to Friedman's arguments. Indeed, Solow notes that "Walras worked mainly formally" (88) and stresses that Walras's theory of capital cannot lead to a "usable theory of investment" or that his monetary theory "never gets off the ground." Solow also writes that "the famous *tâtonnements*" are "a swindle" (88).

In other words, like Friedman, Solow emphasizes the limited empirical scope of Walras's theory. To prove, nevertheless, that "Walras was an ancestor well worth having" (87), Solow suggests that Walras would have been perfectly aware that his general equilibrium method was not suitable to examine empirical issues: "[Walras] remarks somewhere in the *Elements* that *naturally* when one comes to discuss particular aspects of the economic system there is no point in simply repeating that everything is interrelated. Instead one picks out the crucial variable or two in each relation and concentrates on that" (88). Thus, when it came to applied matters, Walras would have known that a different approach was required. That approach was not much different from the one called Marshallian by Friedman and the one Solow defended in his later writings.

The first explicit instance of Solow's distinction between the Walrasian approach and his own approach is found in his 1958 review of Koopmans's 1957 *Three Essays on the State of Economic Science*. In his review, Solow differentiated two types of economists, the "axiomatic" and the "heuristic":

> There are people [the axiomatic types] who are neither happy nor productive unless they have everything tied up in a self-contained and watertight package. There are others [the heuristic types] who thrive on the partial theory and the stray idea, and let the postulates fall where they may. (1958, 179)

The context of the above quotation suggests that Koopmans would be the archetypal representative of what Solow also calls the "axiomatic method in economic theory" (179), while Solow considered himself as a "heuristic" economist. Solow relates the axiomatic method to the economists who support the "descriptive and normative logic of competitive general equilibrium, an area whose cultivation began with Walras and Pareto, was revived in the thirties by Wald and von Neumann, and is today most thoroughly tilled by Arrow, Debreu, McKenzie, and Koopmans himself" (178). In other words, according to Solow, the axiomatic method is directly related to the Walrasian tradition.

In a 1969 document titled "Microeconomic Theory" found in his archives, Solow argues again that there are two different methodologies in the field, which he labels "system-building" and "problem-solving." System-building economic theory is "generally concerned with tidying up the foundations of economic theory, checking the consistency of assumptions, extending the generality of conclusions, and elucidating the most general properties of abstract economic systems" (1). Solow adds that "General equilibrium theory is system-building *par excellence*" (6). The problem-solving methodology corresponds to applied microeconomics, it is "most often carried on with a particular applied problem, and sometimes even a particular body of empirical data, in view" (2). Solow also notes in that text that the problem-solving approach is in the majority: "Most of the square-footage of what is commonly called economic theory fits under the rubric of problem-solving" (2).

The distinction between the "system-building" and "problem-solving" approaches can also be found in Solow's correspondence with his friend Frank Hahn in the early eighties. In 1982, Hahn presented a conference titled "Economic Theory and Policy" as his Shell Lecture at City University (London). In this lecture, Hahn explained the virtues of "serious theory," which according to him, means a theory deduced from "first principles" or "axioms" (1982, 329). In a 1982 letter to Hahn, Solow presented critical comments on Hahn's contribution:

> If by 'serious theory' you mean axiomatic theory, as you sometimes say, then we know it can hardly ever tell us anything truly interesting – that's the import of Sonnenschein's theorem: it limits you to the implications of continuity, homogeneity, and Walras's law. (Solow to Hahn - April 12, 1982, Robert Solow Papers (RSP), box 13, David M. Rubenstein Rare Book and Manuscript Library, Duke University)

Most importantly, later in his letter, Solow defended "simple theory" as an alternative to "axiomatic theory." At this time, Solow defined a simple theory as one "applicable with due care to a world not ridiculously unlike ours" (ibid.).

In a 1985 article, Solow discussed again what he called "the attempt to construct economics as an axiomatically based hard science" (1985a, 328) and sought to explain why "the interests of scientific economics would be better served by a more modest approach" (1985a, 328). That "more modest approach" was a definition of economics as applied science.

In addition, he came back to his 1969 argument in a 1997 article. In the later, Solow examined the evolution of economics in a fifty years' timespan (1997a, 39) and challenged the idea of a formalist turn in economics. To do so, Solow distinguished between "formalist economics" and the activity of "model building." His definition of "formalist economics" echoes Hahn's definition of "serious theory": "One starts with a few axioms, as close to 'self-evident' as they can be…and then tries to work out all the logical implications of those axioms" (Solow 1997a, 42). Solow then emphasized that "formalist economics" is a minority practice and adds: "To tell the truth, not many more pay any attention at all to formalist theory. Generally speaking, formalists write for one another" (43). In contrast, Solow argued that the dominant practice is "model building," which, he contended, is much different from the formalist approach.

3. Solow's criticism of the axiomatic conception of economics

Solow thus distinguished between two kinds of practice or subfields. We shall call them "applied economics" and "axiomatics." According to Solow, both approaches are useful and important – even if axiomatics is a minority practice. In other words, Solow does not reject axiomatics in itself or its results like the Arrow-Debreu model. What he consistently criticizes is the idea that "axiomatics," in particular general equilibrium theory, could be considered as the necessary foundation of the whole discipline. For Solow, the erroneous character of this conception is both a matter of fact and principles.

3.1 Axiomatics *does not* provide the foundations of economics

In his review of Koopmans's 1957 book, Solow (1958) relates "axiomatics" or the "postulational method" (179) to a specific understanding of progress in economics. As we noted, Solow claimed that the supporters of the axiomatic approach want to have "everything tied up in a self-contained and watertight package" (179). By "everything," Solow means economic theory. In other words, we interpret this definition as saying that "people of the axiomatic type" aim to provide the foundations of economic theory. What Solow pointed to Koopmans in his review is that historically, that is not how economics developed:

> I would differ from him on grounds not of the logic of science but of its history. Science does not seem to advance in this sedate and majestic way, paving its highway as it goes. It seems to push ahead in much more helter-skelter fashion, with later generations left to clean up the ground over which theory and observation have already passed. (179)

The second sentence of this quotation may be interpreted as saying that the "heuristic conception of economics" based on "partial theory" and "sheer opportunism" is the primary engine of progress for the discipline. Axiomatics could only be useful to "clean up the ground" in a secondary stage.

As we have seen in the preceding section, in his unpublished 1969 text on "Microeconomics" Solow also points to the fact that "system-building" or axiomatics is concerned with the "foundations of economic theory." He also notes that "it is the job of systematic theory" to provide "some sort of unifying framework within which separable problems can be placed in perspective and then tackled at the appropriate level of generality" (3). Does this mean that the "system-building" approach provides the foundations on which the "problem-solving" approach is based? Solow insists that "even in cases where a connection between theory and applied work would seem natural, it is often lacking" (3). He then stresses that if the two approaches are complementary, they are often in open conflict:

> There remains a tension between the two theoretical styles. System-builders observe that problem solvers isolate what cannot in principle be isolated, neglect interactions that may not in fact be negligible. Problem-solvers observe that system-builders can never actually use their elegant theories to answer any interesting questions. (3)

Although Solow never explained precisely what the use of axiomatics according to him was, he clarifies his views in texts that refer to its "heuristic value":

> One cannot expect there to be any simple and straightforward mapping from Walras to macroeconomics. Those are ways of looking at economics with different goals in mind, and correspondingly different methods. It would be strange, however, if each did not contain lessons for the other. My suggestion is that *those lessons are best conceived heuristically and informally.* (2011, 101 – our emphasis)

Solow's 1969 text can thus be interpreted as saying that general equilibrium theory helps applied economists to have a clearer understanding of the limits of their "simple models" (see also Solow 1982, 19 and 26). That is why, according to Solow, it is still essential to teach economists what a system of interrelated markets is (1990, 29-30).

3.2 Axiomatics *cannot* provide the foundations of economics

For Solow, not only economics is not entirely derived from clear axioms, but providing such axioms would be impossible or unnecessary. Solow supports that argument in a text on the relationship between economics and history: "I suspect that the attempt to construct economics as an axiomatically based hard science is doomed to fail" (1985a, 328, see also Solow 1980, 2). For Solow, economics is applied in the sense that its main objective is the explanation of observations from real economic life – where "explanation" means, for Solow, isolating the specific causal mechanisms behind the observed phenomena.[5] Axiomatics cannot be the foundation for such a definition of economics because the models it produces are too complex and too general and because it looks for a unique model serving as a benchmark for all applied models. We now review Solow's argument supporting his idea that the axiomatic approach cannot provide the foundations of economics.

Complex versus simple models

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[5] On this point, and even if he does not refer to his works, Solow is very close to Marshall's methodology, especially as laid out in the latter's 1885 lecture on the "Present Position of Economics" – a contribution that was also at the basis of Friedman's methodological reflections in his 1949 contribution (see Hoover 2009, 309).

In his unpublished 1969 text, Solow argues that the gap between "system-building" and "problem-solving" results from the "conditions of applied works in economics" (2). The difficulty comes from the "small number of observations" that excludes a priori the possibility of testing too complex models: "The data rarely contain enough information to test subtle many-parameter hypotheses about economic behavior" (2). Solow already considered complexity as a defect of axiomatics in his review of Koopmans's 1957 book in which he opposed to Koopmans's view of "economic theory as a sequence of conceptual models that seek to express in simplified form different aspects of an always more complicated reality" (Koopmans 1957 cited in Solow 1958, 179) the contention that "the test of a model is not just how much it describes but the economy with which it does so" (179). Axiomatic models also simplify reality, but they aim at describing an increasing number of aspects of reality and end up lacking in parsimony. This can be related to Walras's approach in his *Pure Elements of Political Economy* where he started with a two commodities and pure exchange economy and progressively added more commodities, production, capital, and money. The problem that such complexity raises for empirical work was also stressed in later texts in connection with the Walrasian model. It is the basis of Solow's 1985a conclusion that reconstructing economics on axiomatic grounds is "doomed to fail." We may quote him at some length here:

> A modern economy is a very complicated system. Since we cannot conduct controlled experiments on its smaller parts, or even observe them in isolation, the classical hard-science devices for discriminating between competing hypotheses are closed to us. The main alternative device is the statistical analysis of historical time-series. But then another difficulty arises. The competing hypotheses are themselves complex and subtle. We know before we start that all of them, or at least many of them, are capable of fitting the data in a gross sort of way. Then in order to make more refined distinctions, we need *long* times-series observed under *stationary* conditions. Unfortunately, however, economics is a social science. …To express the point more formally, much of what we observe cannot be treated as the realization of a stationary stochastic process without straining credulity. (1985a, 328)

We can reconstruct Solow's argument in the following way: the economic system is complex; trying to account for the whole system implies complex assumptions or complex models; there are many possible assumptions, but not enough data to discriminate between them because the historical time-series are not stationary. Solow supported the same argument much later: "No one expects to implement a Walrasian general equilibrium model empirically; the necessary detail is beyond knowing" (2011, 98).

Solow's answer to the problem of complexity is to rely on what he calls "simplicity." As he put it: "I am addicted to simple models" (Solow 1996 quoted in McAleer 2002, 295). One definition of simplicity (in the case of microeconomics) is suggested in his 1969 unpublished manuscript:

> There is a premium, therefore, on theoretical conclusions that can be embodied in relatively simple few-parameter behavior-equations, which can be estimated successfully from the available data and which may yet prove to have substantial explanatory power. (2)

In contrast to axiomatic models aiming to encompass all the complexities of reality, with "subtle many-parameter hypotheses" (2), applied models aim at explaining a particular set of data with a more limited set of parameters. Another definition of simplicity à la Solow is a focus on a limited number of causal mechanisms identified on the basis of observation or data: economists should focus on "*one or two* of the macroeconomic mechanisms that might be operating out there" (Solow 1999, 282 – emphasis added; see also Solow 1982, 26; 1997a, 43; 2001, 111).

General versus simple models

According to Solow, another drawback of axiomatics is a consequence of what Solow calls its "drive for generality" (1969, unpublished, 2). The result of this quest is that axiomatic models lack the specific or contingent implications that applied economics seeks to explain. The idea is suggested in his 1969 unpublished text in the case of general equilibrium theory: “naturally, from so general a setting, one gets only the most general results” (4). Solow goes further and stresses the limited results of that research: “the theory of general economic equilibrium is far from complete. Indeed, only sketchy results are known” (5). All this explains why “system-building” cannot “answer any interesting question” (3). In later writings, Solow refers to the Sonneschein-Mantel-Debreu results to develop this point concerning general equilibrium theory. These economists showed that general equilibrium in the Arrow-Debreu framework was neither unique nor stable. For Solow, the only aggregate implications of this framework are the conditions for the existence of equilibrium: continuity, homogeneity of degree zero in price of net demands, and Walras’ law. Nevertheless, this weak set of restrictions is not sufficient to build a model with empirical relevance. This is the point he makes in his 1982 letter to Hahn quoted above (see also 2008, 244; 2011, 98). Again, simple models are the solution. Simple models do not try to derive their conclusions from very general assumptions. According to Solow, they are built the other way around, starting from specific observations or specific problems: “problem-solving theory is most often carried on with a particular applied problem, and sometimes even a particular body of empirical data” (1969, unpublished, 2). Simple models account for a specific causal mechanism that can explain a particular phenomenon: “I am making a much weaker point, that there is a lot to be said in favor of staring at the piece of reality you are studying and asking, just what is going on here? Economists who are enamored of the physics style seem to bypass that stage, to their disadvantage” (1997a, 56)

Unique versus multiple models

In addition, Solow criticizes that the axiomatic approach seeks to unify the discipline by establishing a unique model as the foundation for all the others. For Solow, this wish is inappropriate given the heterogeneous nature of the real economy. It is impossible to understand a diverse reality starting from a single model since markets vary in time and space. Economists thus need to rely on different (classes of) models. Solow especially criticizes the idea that there would be a "one true model" in the following passage:

> My impression is that the best and brightest in the profession proceed as if economics is the physics of society. There is a single universally valid model of the world. It only needs to be applied. You could drop a modern economist from a time machine…at any time, in any place, along with his or her personal computer; he or she could set up in business without even bothering to ask what time and which place…We are socialized to the belief that there is one true model and that it can be discovered or imposed if only you will make the proper assumptions and impute validity to econometric results that are transparently lacking in power. (1985a, 330)[6]

The implicit target of this quote is probably New Classical Macroeconomics – more on this later – but the axiomatic approach is the explicit target which Solow defined two pages earlier than this passage. This idea is also present in Solow's 1982 letter to Hahn (quoted in the second section) when he asks him if by "serious theory" he means axiomatic theory, thus the Arrow-Debreu model which is a single model that would be the benchmark of all economics.

To the quest for a unique foundational model, Solow opposes the changing nature of the economy due to the evolution of institutions. As Solow put it, "all narrowly economic activity is embedded in a web of social institutions, customs, beliefs, and attitudes. Concrete outcomes are indubitably affected by these background factors, some of which change slowly and gradually, others erratically" (1985a, 328, see also the letter of January 26th 1954, to journalist and author Godfrey Blunden cited in Halsmayer 2014, 230). That is why, according to Solow, economic theories can cease to account for the facts, leading to the development of new

[6] Kevin Hoover as drawn our attention to the fact that the attitude described by Solow goes beyond economics. The philosopher of science Paul Teller, in particular, has named this conception of the relation between the model and the world as the "Perfect-Model model" (Teller 2001, see also Hoover 2019, 1).

theories: “if a theory that worked well in the 1950s and the 1960s goes sour in the 1980s, that does not necessarily mean it was wrong in the 1950s. It may just have stopped being right” (Solow 1982, 24, see also Solow 1983b, 65-6; 1997a, 56; 1999, 282). This argument also explains the lack of stationary of time series that prevents the testing of complex models mentioned above. Solow’s conclusion is straightforward:

> In this scheme of things, the end product of economic analysis is likely to be a collection of models contingent on society’s circumstances – on the historical context, you might say – and not a single monolithic model for all seasons. (1985a, 329)

If models vary in time, variety at one point in time is also required since models are “partial,” that is tailored to explain particular aspects of the current economy. Indeed, institutions can vary from one country to the other or one market to the other. Even a unique aspect of reality, like the persistence of unemployment, requires “a whole catalog of possible models,” models that are not “mutually exclusive” (1980, 8).

To sum up, according to Solow because the axiomatic approach seeks to establish a complex, general, and unique model, it cannot provide the foundations for a science that is mostly applied and has to base its conclusions on the incomplete data obtained from a complex and changing reality.

4. Axiomatics and macroeconomics

Solow’s criticism of axiomatics applies to all attempts to base macroeconomics on general equilibrium theory. The issue is also raised in his 1983 text for the conference about “Cowles and the Tradition of Macroeconomics.” Macroeconomics is an applied field and deals with phenomena that are excluded from “Walrasian general equilibrium theory”:

> Common observation then leads to the realization that the assumptions of Walrasian theory and the accompanying Arrow-Debreu equilibrium concept are not very suitable tools for understanding many important macroeconomic events. (1983a, 2)

> Macroeconomics has been a practical subject from its beginning. Perhaps it would be enough to define macroeconomics as applied – or at least applicable – non-Walrasian economics. (2)

In this 1983 conference, Solow distinguishes two existing strategies to apply the axiomatic approach to macroeconomics. The first strategy was developed by economists trying to reconcile Keynesian ideas with the Walrasian approach. The second strategy was that of the New Classical economists. In this text, Solow also stresses the importance given to microfoundations in Lawrence Klein's early works on macroeconometrics. We review Solow's distinction between those two strategies by examining his thoughts about the new classical macroeconomics, disequilibrium theories, and macroeconometrics.

4.1 New Classical Macroeconomics

Solow started to campaign against new classical macroeconomics in the 1970s and developed his criticism of the new theory of economic fluctuations when real business cycle theory and Dynamic Stochastic General Equilibrium (DSGE) models appeared. For Solow, the real business cycle and DSGE approaches both attempt to found macroeconomics on general equilibrium theory, meaning the Arrow-Debreu model. The models used by real business cycle and new classical macroeconomists are nevertheless not complex but simple. Thus, Solow's criticism of axiomatics based on complexity does not apply to real business cycle and new classical macroeconomics. Solow's argument is summarized in a 1997 article:

> The goal of Real Business Cycle Theory was the same [as the goal of New Classical Macroeconomics]: to show the everyday experience of economic fluctuations could indeed be accounted for within the framework of formal general equilibrium theory, without the 'impure,' 'ad hoc,' 'Keynesian' violations of standard principles. In doing so it proceeded to abandon formalism in all but name by canonizing one very simple, very special, and very maneuverable version of competitive general equilibrium – in fact, by adopting a highly specific model…The generality that is the hallmark of formalism is gone. (1997a, 51-2)

Two main points should be noted.

Solow first argues that the real business cycle approach failed because its models would not abide by the standards of general equilibrium theory that they pretend to introduce in macroeconomics. To account for macroeconomic phenomena, real business cycle theorists developed a simplified version of the general equilibrium theory based on the representative agent. For Solow, the representative agent assumption contradicts the "drive towards generality" that is the hallmark of axiomatics. As he suggests in another article, it is part of the "dodges" (1983a, 2) used in the New Classical Macreoconomics, a way of solving the unsolved problems of general equilibrium theory concerning aggregation, uniqueness, and stability. For him, what is left from the "axiomatic" quest for rigor is "mostly advertising" (2005, 96).

Second, if real business cycle and DSGE macroeconomists do not use models that are too complex or too general, they nevertheless fall under the "one true model" argument. Solow (1999) repeats that argument in an interview by Snowdon and Vane: New Classical and Real Business Cycle theorists "want to establish a *canonical* model, and then answer whatever question they are interested in by using that model…They think that there ought to be a *true model*, and then you just spin out its implications" (282 – emphasis added). The same baseline model, a neoclassical growth model with a representative agent and stochastic shocks, is used in modified forms to examine all issues in various time and space.[7] Also, Solow considers the basic assumptions of that basic model (perfect competition, identical agents and rational

[7] The relevance of that criticism can be illustrated by quoting from Edward Prescott: "Macroeconomics has progressed beyond the stage of searching for a theory to the stage of deriving the implications of theory. In this way, macroeconomics has become like the natural sciences" (2006, 204) and "[Macroeconomics] is now that branch of economics in which applied dynamic equilibrium tools are used to study aggregate phenomena. The study of each of these aggregate phenomena is *unified under one theory*. This unification attests to the maturity of economic science when it comes to studying dynamic aggregate phenomena" (229 – our emphasis).

expectations) and its implications (optimality of fluctuations, absence of involuntary unemployment, importance of expectational errors) as "implausible" or not consistent with his understanding of the real economy.

4.2 Disequilibrium theories

Solow was a Keynesian economist strongly committed to full employment fiscal and monetary policies. This explains the strength of his criticism against New Classical macroeconomics. But what about the Keynesian quest for general equilibrium microfoundations before the rise of DSGE models? In *Value and Capital* (1939), Hicks introduced the idea that the Keynesian macroeconomic model was properly justified only if its properties could be derived from a disaggregated general equilibrium model (see also Hoover 2012, 21; Rubin 2011). This implied that aggregate demand and supply curves could be derived from agents' intertemporal optimization programs. This approach inspired the contributions of Mosak (1944) and Lange (1945) and those of two pillars of the Keynesian mainstream in the postwar period, namely Modigliani (1944) and Patinkin (1956). In the 1970s, this approach was developed by disequilibrium theoreticians at the macroeconomic and microeconomic levels (Backhouse and Boianovsky 2012; De Vroey 2016). That line of research was clearly identified by Solow in his conference for the fiftieth anniversary of the Cowles Commission:

> A natural reaction for a rigorous economist would be to go back to the microeconomic foundations and try to reconstruct system-wide theory in a way that could give a reasonable account of those everyday pathologies. There have been some attempts in that direction, associated with the names Drèze, Bénassy, Malinvaud, and others. (1983a, 2)

What is Solow's position concerning this version of the axiomatic approach to macroeconomics? As the above quotation illustrates, Solow was not openly critical about this approach. Indeed, in some articles (see Solow 1979b, 79), he was laudatory about the contributions of some disequilibrium theorists, mainly Edmond Malinvaud (1977). This can be explained by the fact that Solow supported the goal of disequilibrium theory, i.e., the Keynesian understanding of the market economy. He also shared their opposition to Monetarism and Lucasian macroeconomics. Besides, at the macroeconomic level, what Malinvaud (1977, 1980) developed were simple three commodities models that conformed to the methodological views of Solow.[8] However, even if he did not write it explicitly, Solow was probably skeptical about all works trying to provide a Keynesian alternative to the Arrow-Debreu model. We interpret the little controversy with Frank Hahn, documented by their correspondence, as evidence of this skepticism.

During the 1970s, Frank Hahn tried to develop non-Walrasian general equilibrium models to ground Keynes's "vision of the economy."[9] One needed to introduce money in "sequential" general equilibrium models and to allow for a distinction between demand and effective demand to account for the possibility of involuntary unemployment. The philosophy behind Hahn's theoretical works is shown at the end of his *Money and Inflation*:

> The Lucasians have the advantage of a well-worked theory of competitive equilibrium. This theory at the end of the twentieth century can at best be regarded as scaffolding and not as the building…Honest economists will be engaged on the building…Above all…let the

[8] Why Solow was so supportive of Malinvaud and so critical of Prescott is a problem given that they both relied on simplified general equilibrium models with representative agents. But Malinvaud's agents were less sophisticated than the agent of Prescott. In Malinvaud's works, the household do not engage in intertemporal optimization and do not follow rational expectations. Moreover, Malinvaud assumes that prices were fixed in the short term and that their variations could be defined by rules derived from empirical studies and not from theoretical principles. Finally, Malinvaud supports the idea that the economy could remain in a Keynesian unemployment equilibrium.

[9] "Keynes, as you know, was a very careless writer, very sloppy. I think the *General Theory* contains nothing which we would recognize as a proof of any proposition. Nonetheless, Keynes had a certain vision of the economy, a vision I share, and I think it is very relevant to monetary theory" (1980, 164).

> scaffolders be silent on public affairs while the building is nowhere in sight. (Hahn 1981 [1983], 106)

This essay was mainly a criticism of Lucas's 1972 article. Hahn concurred with Solow in arguing that *Walrasian* general equilibrium theory was not the appropriate framework to deal with macroeconomic issues. Like Solow, Hahn believed that this theory was incomplete, a "scaffolding" and not a "building." For Hahn, Lucasians act as if the building would already be complete.[10] Nevertheless, Hahn departed from Solow's research strategy. For Hahn, completing the building was the priority for "honest economists." A defense of this strategy can also be found in his 1982 lecture "Economic Theory and Policy," in which Hahn argues in favor of what he calls "serious theory." In their correspondence about this lecture that same year, already quoted in section 2, Solow rejected "serious theory" defined as the Arrow-Debreu model. Solow then added:

> I fear there is also a sense in which axiomatic theory is almost stuck with perfect competition, an unattractive assumption. To do axiomatics, you need an unspecific – categorical – sort of assumption. Competition is such a one. (Solow, letter to Hahn, April 12, 1982)

This quote represents an in-depth criticism of Hahn's approach. In the 1970s, Hahn tried to go beyond the pure competition assumption with a concept of conjectural equilibrium (Hahn 1977; 1978). Here Solow is telling him that axiomatic theory is stuck with perfect competition. In other words, it cannot go beyond the unsatisfactory Arrow-Debreu model. Then he refers to his taste for simple models. Hahn's answer is clear in the published essay: "I agree that simple theory can be serious theory; I doubt that it can be sufficient theory" (1982, 332). And in the correspondence, he wrote: "My view is that the mess we are in is particularly due to this fact,

[10] In this respect, Hahn's criticism of the New Classical economists is close to Kevin Hoover's criticism of the "eschatological justification" (Hoover 2015, 704).

ie. we have not hit on the uniform primitive principles, without them we have chaos" (Hahn to Solow; April 19, 1982).

Hahn shared with New Classical macroeconomists the belief that macroeconomics had to be founded on a general equilibrium theory. However, whereas New Classical macroeconomists were satisfied with existing general equilibrium theory, Hahn believed that a non-Walrasian general equilibrium theory was required. Solow's letter of 1982 is evidence of his skepticism. His approach to microfoundations was different (see section 5).

4.3 Macroeconometrics

Large-scale macroeconometric models were considered by Lawrence Klein – their leading architect in the US – as applied Walrasian models (Hoover 2012, 39; Pinzon-Fuch, 2016; Rubin 2016, 302-4). Solow discussed these models in 1983a and 1985b. His position is consistent with his assessment of the Arrow-Debreu model. Like the latter, but for different reasons, macroeconometric models are a useful addition to the economist toolbox. Nevertheless, they cannot claim to reign over macroeconomics as the only valid type of model. Overall, Solow's views are quite critical.

Solow's 1985b contribution is about Otto Eckstein (1927-1984), founder with Donald B. Marron of Data Resources, Inc. Solow considers Eckstein as "the natural successor to Jan Tinbergen and Lawrence Klein in the illustrious family tree that forms the ancestry of the large-scale complete macroeconomic model of today" (79). In the introduction of his text, Solow refers to his ambivalence about "big econometric models" and adds that although Eckstein did not share his "doubts about macroeconometrics" that "didn't keep [them] from being friends and allies" (80).

The doubts that Solow mentions again echo his arguments against axiomatics. Macroeconometric models are continually modified to fit the data (82) hence they do not fall under his criticism concerning the quest for generality. The main drawback of these models is their complexity. This raises two problems that we identified earlier and that Solow stresses in that context:

> One of my unhappinesses with the large all-inclusive macroeconometric model is that it, too, has a way of burying the causal connections in a vast exfoliation of regression coefficients, many of which are inevitably clinging to statistical significance by their fingernails, if at all, and then only by virtue of some particular choice of functional form, sample period, or other casual decisions. (82)

Large macroeconometric models are inadequate because causal mechanisms are "buried" hence they are not a good tool to explain facts. Besides, their relations are not robust. This is the lack of data problem already stressed in other texts. And given the complexity of these models, the tricks used by the model builder to improve his empirical results are more difficult to understand: "with big models, however, it is harder for anyone to see what is going on" (80). To this approach, Solow opposed again his emphasis on partial models:

> My own inclination is always to want to narrow the scope, to try to understand one relation at a time, to stick to the few strong stylized facts that will likely survive any change in angle of vission [*sic*], to use evidence from any source, even casual observation, and not only from econometric routine. (83)

One can also find in this 1985b text an echo of the "one true model criticism." Large-scale macroeconometric model builders tend to understate the complexity of the world and think that they can capture all phenomena with one model:

> The large econometric model responds to a frame of mind that thinks the real economic world is not fundamentally very noisy. In this view a good model would explain nearly everything that happens. (80)

Solow also expressed his skepticism concerning the possibility of deriving macroeconomic models from rigorous microfoundations in the context of macroeconometrics. In his conference for the fiftieth anniversary of the Cowles Foundation, Solow notes that Klein

(1950) wanted to "base operational macroeconometric models on a rigorous microeconomic foundation" (1983a, 7). Solow then describes the failure of works on aggregation during the 1940s and ironically notes that Klein claims that his aggregate consumption function, dependent on real disposable income, is derived from "the theory of household behavior" (8) whereas, in a general equilibrium context, it should only depend on relative prices.

If "the large macroeconometric model is an indispensable tool" (1985b, 81), according to Solow, that is not because it succeeds in identifying the structural relations organizing a whole economy. Macroeconometric models are useful because they are continuously adjusted to fit the data and because they reflect the judgment of the economists that produce them. Otto Eckstein was a good economist; hence, his model provided useful information about the state of the economy and the possible effects of various shocks: "the main function of econometric modelling is rather to provide very sophisticated descriptive statistics" (81). Hence, these models ought to co-exist with the simple models defended by Solow:

> I have no doubt that it will be found, after detached study, that the successful coexistence of large coordinated research enterprises, like the one presided over by Lawrence Klein, and federations of small-scale handicraft operations, like the Cowles Foundation, is a rational, maybe even efficient, response to differences in technology, tastes, and, of course, the presence of transaction costs. (1983a, 16). [11]

Large-scale macroeconometric models were developed by economists belonging to Solow's side in the academic and political battles of his time. This may explain the understated nature of the criticisms he nonetheless addressed to their approach. Macroeconometric models are based on a careful study of data and, from this point of view, they are useful. However, they are also the result of the (for Solow, wrong) belief that the economy is simple enough to be

[11] "[F]ederations of small-scale handicraft operations" refers to the contributions of James Tobin and his associates.

explained by one big model. They are too complex to be robust and to explain how the economy works.

5. Whatever Works or Common Sense Microfoundations

Solow discussed the issue of microfoundations in several contributions, rejecting the dominant approach advocated by real business cycle and DSGE economics. As we have seen, however, Solow's position on this matter implies a broader rejection of the tradition of microfoundations descending from Hicks's 1939 *Value and Capital*. In contrast, Solow criticized the idea that a macroeconomic model should be explicitly derived from agents' optimization programs. As an alternative methodology, he supported an "opportunistic," "common sense," or even "loosely abstracted" approach to microfoundations:

> My choice would be to model the main components of aggregate demand more or less opportunistically. By 'opportunistically' I mean that whatever works (empirically) works. (Solow 1997b, 231)

> One possible methodological decision is to insist that a valid macro model must be an exact aggregation of the corresponding microeconomy…As an alternative, I would be quite content with a macro model that could be described as 'loosely abstracted' from particular micro assumptions. (Solow 1998, 32-3)

> [The representative agent] is not needed with the common-sense approach to microfoundations. (Solow 2004, 659)

Those quotations could suggest that Solow rejected any discipline concerning microfoundations. That is, however, not the case. Solow argued that the idea of microfoundations was always present in macroeconomics. For instance, when explaining James Tobin's way of formulating a model – which he endorsed – Solow wrote:

> The first thing you will notice about "A General Equilibrium Approach" [Tobin 1969] is that its basic building blocks are net-asset-demand functions…There are no optimizing consumers who maximize the expected present discounted values of infinite utility streams,

> no Euler equations. So where are the 'microfoundations?' The answer is that they are embedded in those common-sense restrictions on partial derivatives…Every author tries to make his behavioral assumptions plausible by talking about the way that groups of ordinary economic agents might be expected to act. (2004, 659; see also Solow 1997a, 51; 1998, 9)

Solow considered in the above quotation that aggregate relations in a macroeconomic model must be justified by reference to the real behavior of individuals – a weak requirement. We should also note that, in the above quotation, the justification for the "net-asset-demand functions" remains *outside* the model. This point towards the first characteristic of Solow's approach to microfoundations, which is that a macroeconomic model can be grounded on already accepted – Solow's criteria of "plausibility" – behavioral principles not developed within the model under study.

Solow's contributions from the end of the 1970s to the middle of the 1990s illustrates his conception of microfoundations. In 1979(b), Solow claimed that the most important line of research in macroeconomic theory could be represented by the work of Malinvaud (1977) or what can be called fixed-price models. According to Solow, the problem with that approach was the lack of justification or microfoundations of wage and price rigidities (Solow 1979a, 346). From the Walrasian standpoint, this shortcoming would require the incorporation of imperfections or constraints in a general equilibrium model to explain how agents' choices lead to wage inflexibility and unemployment. This was the line followed by Drèze (1975) and by the more recent literature on DSGE models. Solow's path was different. As mentioned above, he developed several partial equilibrium models based on standard microeconomics to explain why the real wage did not decline in case of excess supply on the labor market. Solow's aim was not to obtain ingredients worth integrating into the final general equilibrium macroeconomic model. Many different mechanisms were at play in the labor market, each of them requiring a specific model (Solow 1980, 3 and 8; Hahn and Solow 1995, 102).

Hahn and Solow's 1995 book *A Critical Essay on Modern Macroeconomic Theory* clarifies this conception of microfoundations. From the end of the 1980s, Hahn and Solow collaborated to counter the influence of new classical macroeconomics. We interpret the result, their 1995 essay, as Hahn's abandonment of the ambitious research project he defended until the early 1980s and his adoption of a more pragmatic and Solowian approach. In Chapter 5 of their 1995 book, Hahn and Solow present a detailed version of a model already discussed in Solow's 1990 book on the labor market. This partial equilibrium model based on game theory explains why workers have no incentive to undercut each other's wages when unemployed, resulting in wage rigidity.

In Chapter 6, Hahn and Solow (1995) developed a macroeconomic model, assuming that wages are exogenous and fixed. This assumption is not *ad hoc* but grounded on the partial equilibrium model they discussed in the preceding chapter. What Hahn and Solow defended is the Old Keynesian approach: macroeconomic models with apparently ad hoc specifications but justified by partial microeconomic studies of consumers' choices, investment decisions or portfolio decisions, and a heavy dose of econometric studies to define the parameters of the behavior functions. That is what Solow called the "micro-rationalization of macroeconomic relationship":

> Of course macroeconomics can hardly just tread water while more realistic micro-foundations are being worked out, taught and tested. In the meanwhile, the older rough-and-ready approach may be the best we can do, and not intolerable. I mean the informal micro-rationalization of macroeconomic relationships with all of its infuriating reliance on stylized facts, partial econometric analyses, appeals to common sense, and even amateur sociology. (Solow 1986, 197)

Solow's approach to microfoundations also emphasized the "plausibility" of a model's assumptions (1998, 9). "Plausible" means conforming to our knowledge of empirical data, whether individual or aggregate. It is mainly on this basis that Solow rejects the representative agent models. His argument was about the fact that agents in the real economy are heterogeneous, a fact that real business cycle and DSGE models would – according to Solow – discard:

> a commitment to representative agent models is a serious mistake. Many of the frictions and occasional flip-flops that characterize macro-behavior seem to stem from the heterogeneity of agents, with respect to underlying beliefs, expectations, market power, access to capital, and so on (Solow 2000, 155; see also 2004, 659; 2008)

In the same vein, Solow rejected rational expectations in the short run, arguing that expectations are "best handled ad hoc, that is, in a commonsense way" (Solow 1997b, 231).[12]

The last point we must discuss concerns choice theory. Again, Solow rejected the consumer theory used in the standard business cycle and growth models. Solow (1994, 49) recalled that this approach was not conceived to have a descriptive content but, instead, to represent the choice of an ideal policymaker. Moreover, Solow considered the assumption of a "single immortal family with perfect foresight" carrying out an "infinite-horizon optimal plan" as "highly specific" (1997a, 51), and the related assumption of rationality as "narrow" (54). Meanwhile, Solow did not reject the discipline imposed by standard choice theory. Solow's commitment to standard choice theory is also apparent in his 1980 article "On Theories of Unemployment" in which he noted: "I am generally stodgy about assumptions, and like to stay as close to the mainstream framework as the problem at hand will allow" (3). Moreover, in a later article, he noted that the (simplifying) assumption of constrained optimization is part of the art of model building: "We have to assume that this person does the best he can to satisfy his tastes for leisure and for the goods that his after-tax income can buy" (1997a, 44). In the

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[12] In this respect, Solow departs from Keynes who accepted perfect expectations in the short run but rejected them in the long run (see Hoover 1997).

same vein, he later noted: “I have no objection to the assumption, at least as a first approximation, that individual agents optimize as best as they can” (2008, 244).

Nevertheless, what Solow defends is microfoundations that incorporate his views concerning the needs to be plausible and to adapt the working hypothesis to the object of study. This may lead the economist to look for inspiration in the works of sociologists and psychologists and to incorporate social norms or beliefs in the models:

> The program of constrained maximization has to rest on a careful statement of what is being maximized and what the constraints are. Mainstream economics takes a narrow view of both; some hardy souls would like to try out a wider range of assumptions. They look to sociology and social psychology as a source of alternative ideas. (Solow 1997a, 54-5)

Thus, Solow does not criticize the idea of microfoundations *per se*, but he criticizes the *kind* of microfoundations generally used: “Many varieties of macro models can be constructed that satisfy those basic requirements [that excess demands be continuous, homogeneous of degree zero in prices and satisfy Walras’ Law] without imposing anything as extreme and prejudicial as a representative agent in a favorable environment” (2008, 244).

To sum up, according to Solow economists should not derive all parts of an all-encompassing model from a single, general representation of the economic agent. A macroeconomic model rests on exogenous ingredients and behavior functions that are justified by other (partial) models. This allows, according to Solow, avoiding excessively narrow and implausible assumptions as those used in representative agent models.

6. Solow’s Growth Model is Not Walrasian

Solow’s 1956 neoclassical growth model is a general equilibrium model (Solow 1956b). Boianovsky and Hoover (2014, 200) have argued that Solow’s methodology in this classic article epitomizes the MIT style of economics. If our reconstruction of Solow’s methodology is correct, we should be able to show that Solow's growth model reflects not only the MIT style but also his opposition to the Walrasian or axiomatic approach. That is what Solow suggested in a 2008 article on the state of macroeconomics:

> I have often described that model [Solow 1956b] as a miniature general equilibrium. I will make three exculpatory observations. First, I restricted the applicability of the model to tranquil trajectories without stormy intervals. Second, I deliberately avoided recourse to the optimizing representative agent and instead used as building-blocks only aggregative relationships that are in principle observable. Third, I immediately warned the reader of the possibility of aggregative short-to medium-run supply-demand imbalances that would not fit into the model. (244)

The non-Walrasian character of Solow's 1956 growth model is indeed apparent. At the beginning of his 1956 article, as is sell-known, Solow made clear that he based his contribution on Harrod's and Domar's Keynesian contributions.[13] This can explain why, in Solow's model, the behavior functions of households are not consistent with a Walrasian approach. Indeed, in the Walrasian approach, agents decide simultaneously the quantities they supply and demand in each market based on their utility function and budget constraint. As a result, agents' behavior functions must depend on all prices. In contrast, in his article Solow characterized the behavior of the "community" by aggregate behavior functions that do *not* consider prices. The saving function, in particular, comes from the Keynesian apparatus (where aggregate saving is a share of aggregate income) and is representative of Solow's "common sense" approach to microfoundations.

More broadly, the style of Solow's 1956 growth article is non-Walrasian. A comparison with Don Patinkin's approach in *Money, Interest and Prices* (1956) is enlightening. Patinkin

[13] Halsmayer and Hoover (2016) have shown that Solow's reading of the "Harrod-Domar" model is misleading in many respects, while those deficiencies can be explained by Solow interpreting "Harrod along the lines most congenial to the practices of the emerging community of economic modellers in the 1950s" (591).

considers his IS-LM model as a simplified Walrasian model (see Rubin 2004, 197-200). He begins by defining the four markets composing the model and the three prices that clear them. He also identifies the agents populating the economy and writes down their budget constraints to derive Walras's law. Then, he details how the *tâtonnement* process allows the system to find its ways towards an equilibrium. By contrast, Solow's growth model focuses on "output as a whole" and the process of its determination. The section of the article that introduces the relationships of the model does not contain a single reference to firms or households, nor to their budget constraints. The markets of the economy are not identified, and when Solow discusses the determination of prices, he refers to four prices including the money rate of interest– even though there is no money in the model. The "composite commodity" is the *numéraire* of the model, but Solow prefers writing that the price of real output is given or constant. Moreover, Solow never refers to a *tâtonnement* process. Furthermore, he regrets that monopolistic competition remains difficult to introduce in aggregative models, but this form of competition is not compatible with a Walrasian framework (Solow 1956b, 79 fn.7).

Finally, Solow's growth model conformed to his notion of partial theory. Indeed, Solow's model isolated the growth mechanism of the economy, leaving out market imperfections and the role of the government necessary to attain the full employment growth path (Assous 2015):

> All the difficulties and rigidities which go into modern Keynesian income analysis have been shunted aside. It is not my contention that these problems don't exist, nor that they are of no significance in the long run. (Solow 1956b, 91)

In other words, Solow's methodological stance in what can be considered as his most famous article already reflects his criticism of the "axiomatic" or Walrasian approach and illustrated his favored approach to macroeconomics.

7. Chicago meets MIT: Friedman and Solow on Methodology

Other evidence that Solow's criticism of the Walrasian approach goes beyond the mere rejection of market clearing and its political implications are the similarities between his methodological views and that of Milton Friedman.

Friedman is well known for his criticism of the Walrasian approach and his adoption of the Marshallian approach (Friedman 1946; 1949; 1955). For instance, in his review of Jaffé (Walras and Jaffé 1954), Friedman noted – echoing Harrod – that "Walras has little to contribute in this direction [fruitful and meaningful economic theory]; for this we must turn to other economists, notably, of course, to Alfred Marshall" (1955, 908). We argue in this section that the similarities between Friedman's and Solow's methodologies can be explained by their shared opposition to the Walrasian approach.

Friedman's 1953a essay "The Methodology of Positive Economics" is widely considered as the most influential contribution to the methodology of economics in the postwar era (Hausman 1992, 162). Thomas Mayer (2009, 133) has notably argued that Solow's 1987 Nobel Lecture (see Solow 1987 [1970]) would be in line with all the criteria Mayer identifies as constituting the core of Friedman's methodology (as expounded in the latter's 1953b book), including Friedman's support of the Marshallian approach and criticism of a Walrasian one. In his 1953a famous introductory essay, Friedman suggested using different criteria when appraising the relevance and the scientific quality of economic theories. Specifically, Friedman argued that the requirements of simplicity and fruitfulness are more important than logical completeness and consistency when choosing between "alternative hypotheses equally consistent with the available evidence" (Friedman 1953a, 10). Friedman defined the criteria of simplicity and fruitfulness as follows:

> A theory is 'simpler' the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more 'fruitful' the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests. (10)

As we have seen, Solow also emphasized the criteria of simplicity and one can recognize in his contributions the criteria of fruitfulness – in the sense of further developments of a theory – although he did not use that specific word. Indeed, in his book *Growth Theory, an Exposition*, Solow (1987 [1970]) noted that the macroeconomic theory of growth "is a theory with a fairly simple skeleton…capable of a quite surprising amount of elaboration" and added that "we are dealing with a drastically simplified story, a 'parable'" (1). In the same vein, for Solow, a model is a "deliberately simplified representation of a much more complicated situation" (1997a, 43) and a good model is one which makes the "right strategic simplifications" (Solow 2005a, 92). Importantly, Friedman argued that the criteria of simplicity and fruitfulness are at the heart of Keynes' Marshallian approach. As Friedman put it, "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…I believe that Keynes's theory is the right kind of theory in its simplicity, its concentration on a few key magnitudes, its potential fruitfulness" (1972, 908). In other words, according to Friedman, the criteria of fruitfulness and simplicity are on the Keynes-Marshall side and in opposition to the Walrasian approach. Solow would undoubtedly recognize his favored "heuristic" approach in Friedman's description of Keynes' and Marshall's methodologies.

Moreover, Friedman's quotation strikingly resembles Solow's criticism of the Walrasian wish to develop the "one true" model of the economy. Friedman's criticism of the general equilibrium approach and of the idea that one could develop a unique model for the whole economy can also be found in his 1951 comment on an article by Christ presented at a

National Bureau of Economic Research Conference on Business Cycles in which Christ tested Klein's econometric model. Friedman emphasized that: "Until we can develop a simpler picture of the world, by an understanding of interrelations within sections of the economy, the construction of a model for the economy as a whole is bound to be almost a complete groping in the dark. The probability that such a process will yield a meaningful result seems to me almost negligible" (Friedman 1951, 112-3).

Hoover (2009) has characterized Friedman's methodological stance as causal realism, defined as "the view that the object of scientific inquiry is the discovery through empirical investigation of the true causal mechanisms underlying observable phenomena" (305). Friedman's arguments regarding the unimportance of the "realism" of the assumptions of a theory can thus be interpreted as a "way of referring to the desirable property that a theory captures the essence of a deep relationship" (313). As we have seen, Solow also emphasized the necessity to focus on a limited number of causal relationships. As he put it in 1985(a), one of the functions of analytical economics is to "tell plausible – sometimes even convincing – causal stories with the help of a few central principles" (329). In the same vein, in a definition of the notion of model he offered in 1997a, Solow noted that "The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact" (43).

Also, like Solow, Friedman emphasized the importance of institutions. For instance, in his 1947 review of Abba Lerner's 1944 *The Economics of Control* – a book that "incorporated the entire Walrasian framework" (Düppe 2011, 159) – Friedman lamented that "The institutional problems are largely neglected and, where introduced, treated by assertion rather than analysis" (1947, 405). In other words, both Friedman and Solow criticized the Walrasian approach for not considering – or doing it inappropriately – the influence of institutions.

Finally, Friedman and Solow converged on their appreciation of the so-called "Cournot problem" (Hoover 1984; 1988). The latter refers to the French economist and philosopher Antoine Augustin Cournot's (1801-1877) abandonment of the wish to develop a general equilibrium approach in his 1838 *Recherches sur les principes mathématiques de la théorie des richesses* – a book translated into English in 1897 as *Researches into the Mathematical Principles of the Theory of Wealth* (hereafter, *Mathematical Principles*). Friedman considered that Walras did not solve Cournot's problem. For Friedman, Walras "emptied Cournot's problem of its empirical content and produced a 'complete and rigorous' solution 'in principle,' making no pretense that it could be used directly in numerical calculations. His problem is the problem of form, not of content: of displaying an idealized picture of the economic system, not of constructing an engine for analyzing concrete problems" (1955, 904). Hence, Friedman would actually treat Cournot's approach not as a problem but a s a virtue, and his opposition to the Walrasian approach is also clearly apparent in that quotation. We shall see below in more details in our conclusion that Friedman and Solow converge in their appreciation of the "Cournot problem", Solow supporting Cournot's giving up in his quest of a general equilibrium approach.

Again, that is not to claim that Solow's methodology was the same as Friedman's. As only one instance, Friedman supported the argument that the test of the validity of a theory lies in its predictive power. Solow rejected that argument, considering it as a "widespread but misleading belief in our profession" (1985b, 80) – Solow did not, however, argue in favor of another criterion. In other words, it is because of their criticism of the Walrasian approach that Solow and Friedman – who oppose on the side of economic policy – both favored focused, fruitful, and simple models which should also consider the role of institutions.

8. Conclusion: Solow as a Cournotian Economist

According to Solow, the Walrasian or axiomatic approach seeks to capture the complexity of the economy within the bounds of one general and complex model derived and developed from general principles, a general model that would found all the other models used by economists. Solow rejected this approach for two main reasons. First, one of the main goals of economics is the explanation of economic phenomena by which he means the isolation of a few causal mechanisms. This explanation is not possible with complex and general models where causal mechanisms are too numerous and impossible to disentangle or too abstract and detached from specific real-world phenomena. Second, according to Solow, a unique class of models all derived from the same overarching framework cannot explain a diverse and changing reality.

Solow concluded that the Walrasian or axiomatic approach could not and should not dominate economics. This led him to advocate the use of a diversity of simple and partial equilibrium models, which have practical utility, consider institutions and the evolving nature of the economy, and rely on commonsense microfoundations. Solow's contributions thus illustrate that the American Keynesians of his generation were not homogeneous in their appraisal Walrasian economics.

We conclude by suggesting – this would thus need further study – that Solow's methodology can be described as Cournotian. As the title of his 1838 book reveals, one of the methodological landmarks of Cournot's approach to economics is the use of mathematics. Solow first read Cournot's *Mathematical Principles* (English version) when he was a graduate student. In Solow's recollection, Cournot's contributions to economics were at the time "widely acknowledged and discussed" (Solow 2005). Solow's interest in – and knowledge of – Cournot's contributions are also shown by his becoming president of the French *Centre Cournot pour la recherche en économie* (Cournot's Centre for Economic Research) since its inception in 2000. In his introductory speech at the 2005 Cournot Centre Conference on "Economic Models and Rationality," Solow (2005) quoted from Debreu who stated in his 1983 Nobel lecture titled "Economic Theory in the Mathematical Mode" that:

> If a symbolic date were to be chosen for the birth of mathematical economics, our profession, in rare unanimous agreement, would select 1838, the year in which Augustin Cournot published his *Recherches sur les Principes Mathématiques de la Théorie des Richesses*… Cournot stands out as the first great builder of mathematical models explaining economic phenomena. (Debreu 1984, 267)

Solow agreed with Debreu's statement, claiming that Debreu's "words are perfectly true today [2005] as they were in 1983," testifying to Solow's high esteem for Cournot's contributions.

Solow's view of Cournot, however, involved more than high esteem. We contend that Solow was a supporter of Cournot's way of doing economics. At the 2005 conference we just quoted from, Solow (2007) discussed Chapters eleven and twelve of Cournot's *Mathematical Principles*. In those chapters, Cournot relaxed his partial equilibrium approach and sought to examine a system of many markets. In other words, Cournot "points clearly ahead to Walras and the theory of general equilibrium" (Solow 2007, 107). Solow first notes that Cournot was led to assume that the effects of the interactions of the many markets were negligible, and he failed to develop a general equilibrium model. As Solow put it, "a purist would turn up his nose and say that, after those fine Walras-sounding words…Cournot has simply abandoned the whole idea of general equilibrium. From a puristic point of view that is so" (2007, 109).

Nevertheless, and this is the crucial point, Cournot's abandonment of the general equilibrium approach is at the same time the reason why Solow favorably praised Cournot. Following the previous quotation, Solow adds:

> a sophisticated and pragmatic macroeconomist might cheer Cournot on. He has found his way to the simplification that makes macroeconomics possible. To treat the economy as if it were a one-commodity (or perhaps two-commodity) world is to hope that changes in relative

> prices, though they will change relative quantities, may have only second-order effects on aggregates such as the social income. No one believes that such good luck could hold all the time, even in non-extreme cases. But it may hold often enough to provide a basis for macroeconomics. (ibid.)

Cournot's methodology in his *Mathematical Principles* thus eventually boiled down to a combination of a pragmatic and partial equilibrium approach couched in mathematical form. If we had to find an ancestor to Solow's methodology, Cournot would thus undoubtedly be an appropriate candidate.

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