Perspectives
on Epistemology of Economics

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Essays on Methodology of Economics

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PREFACE

Since 1995 the Centre of Research on Epistemology of Economic Sciences, School of Economic Sciences, University of Buenos Aires has uninterruptedly organized the Annual Meeting of Epistemology of Economic Sciences. In light of the recent economic and social changes, as well as the crisis undergone by the social sciences – the economic sciences among them –the members of the Centre deem it necessary to undertake a deep philosophical and epistemological debate that allow the academic community to guide and critically discuss their contributions. Our Annual Meetings have proved to be a promising tool for that purpose.

In those academic events a broad range of issues are tackled. Among them one can highlight the following: Economics Epistemology, Social Technologies (Methodology of Administration and Accountancy), Mathematics and Artificial Intelligence, History of Economic Thought, Historical Methodology and Pedagogy of the Economic Sciences. The different research interests of the Meeting's participants have turned our event into a privileged field for the academic debate and exchange of ideas from an interdisciplinary perspective, which chiefly contribute to enhance the interest on these research areas.

The present book of essays comes out of the latest of our Meetings that took place in Buenos Aires in October 2011. The editors have selected a series of works presented at that Meeting in order to show the current diversity of approaches that exist in the economics epistemology. The research topics covered in the chapters range from traditional methodology to the present state of the field of the economics methodology, to social ontology, to mechanistic literature. Both editors have tried to keep the selection of the present essays as most balanced as possible, reflecting the complexity of the issues that epistemology presently deals with.

In the first chapter contributed to the book, *Orthodox and Heterodox economics in recent Economic Methodology*, **Wade Hands** explores a three-way relationship between orthodox economics, heterodox economics, and economic methodology during the last three decades or so. He shows there how work in economic methodology related to orthodox and heterodox theory during the period 1975-2000 and then how this relationship has changed in recent years, characterized as is known by the economic crisis which also questions the economic discipline. Hands argues that there has been a lot of expansion and change within the field of economic methodology: it changed its general philosophical focus from universal rules borrowed from the shelf of scientific philosophy to local practical advice grounded in the interests of particular sub-fields. Also it has changed its domain of inquiry from neoclassical and heterodox economics in general to the more pluralistic microeconomic approaches. Thus his main conclusion is that changes in the economic methodology will contribute to the growth of the field, although he warns us by saying that this is not necessarily the certain outcome.

Luis Mireles Flores sheds light on the practical value causal knowledge has for policy making. *Causality, pluralism and economic policy making* explores the consequences of causal pluralism for economic policy making. According to causal pluralism, the notion of causation can have a variety of distinct meanings. The author argues that if this is the case, then economists and policy makers should ensure that the proper interpretation of scientific causal knowledge employed for policy purposes be explicit and properly understood before any recommendation is offered. After presenting a distinction between pluralism about causal theories and pluralism about causal concepts, research on unemployment by the OECD is used as an illustration of how, in practice, economic policy recommendations are formulated on the basis of causal claims that are left open to rather ambiguous interpretations.

In *Economics as a separate science: a critical review*, **Eduardo Scarano** explores a long controversial issue in the economics methodology that is the separability of economics from other disciplines. In this chapter the author takes issue in the contemporary debate on the notion of economics and its methodological projection into other social disciplines. With that purpose in mind, Scarano goes back to J.S. Mill's approach to discuss the first arguments raised for the separability in the economics science; then Friedman, Hausman and Maki's position on the separability issue are dealt with. The main conclusion arrived at in the paper is that separability in itself does not forbid any type of testing, predictability, or articulation of the theory with the facts. By an extensive review of the main author's works on this issue, the chapter clearly argues that sciences can or cannot be separate, and that the science of a theory is not directly related to the presence or absence of this property.

Mechanistic literature is a hot topic in the field. **Leonardo Ivarola** and **Gustavo Marqués** explore in *Expectations-Based mechanism – An interventionist account*, a processual and dualistic account of mechanisms in order to examine a particularly relevant case of economic mechanism: the so-called *Keynes Effect*. They provide a specification of its structure, and throw light on the way in which its elements relate to each other, and an account for how the mechanism can generate its results as an example of a broader class of social mechanism referred as Expectations-Based Mechanisms (EBM). Characteristically, an EBM shows a connection between the information that individuals receive from the relevant economic context, the expectations they form, and the activities they perform (which may modify the preexisting context). The chapter provides an outline of the way in which authorities' interventions may contribute to a convenient change in agents' expectations (decisions), helping to produce some targeted economic phenomena.

Following the mechanistic literature, particularly their recent development of causal mechanism explanations, in *A teleological causal mechanism for economics: socio-economic machines* **Ricardo Crespo** proposes to combine Nancy Cartwright's conception of capacities and nomological machines with Amartya Sen's capabilities in order to enact a causal mechanism for economics. He defends that economics should go beyond technical reason, and reincorporate theoretical and practical reason. Cartwright's and Sen's approaches suit this target. He claims that "we must build a socio-economic machine and the corresponding model to define and determine capabilities (theoretical and practical reason) and look for the best means to attain them (technical reason). The socio-economic machine will produce these wished-for goals."

In *On Economics and the impossibility of its reduction to Physics,* **Ricardo Gómez** discusses the meaning of reduction by exploring the logical neo-positivist tradition in philosophy. He discusses in some detail the contributions of Carnap on the issue at stake but concludes that the it is not possible a strict reduction of psychology and biology to physics, and consequently of economics to physics because in the chain of reductions economics was supposed to be reduced to individual psychology and physics, something not achievable on the neo-positivist agenda. One of the most challenging conclusion in Gómez' chapter is that there is one error in the attempted analogy of economics to physics, since it presupposes not only a physics that never was, but, in addition, a science that never was. Indeed, the author argues, physics was understood as providing the methodological model to imitate because it allegedly was the paradigm of objectivity and this happened because it was supposedly, as it should be all science, objective, in the sense of being value neutral.

The ways of scientific representation: models, maps and reality, by **Diego Weisman and Germán Thefs**, considers the problem of scientific representation in the light of the recent epistemological turn from theories to models. Recovering models as carriers of scientific knowledge about the world rise questions about the specific manner in which that knowledge is delivered. In other words, in which way scientific models represent reality? How do we know that a scientific model represents faithfully their "real" target system? Lacking elaborated answers, many (mainstream) economists use to say "models represent as maps do". The paper analyzes the meaning and the links with a *realist* metaphysical framework in those economists which work with *unrealistic* models.

In *A Critical Look at Critical Realism* **Agustina Borella** tries to show the main difficulties that emerge in Tony Lawson's critical realism. Agustina explains in detail what critical realism is and what Lawson's philosophical assumptions of the mainstream economic theory are. One of the most important issues discussed in the chapter is the realism of models. In this regard the author argues that Lawson does not claim exactly more complex models, but models that are capable of capturing the mechanisms that operate behind the events and, in this way, transform the social world. The conclusion is that it is necessary to adhere to critical realism and manage to reorient economics and transform reality, to adhere to his social ontology, and apply transcendental realism to the social world. Without this look at the social realm, economics will go on in the sad, unfortunate and unhealthy state that Lawson diagnoses. Yet, Borella claims that if Lawson's ontology is not shared, what room is left for dialogue with the mainstream economic theory's proposal?

In the last chapter of the book, *Mill, Hausman and the traditional method in neoclassical economics,* **Andrés Lazzarini** attempts to indentify the links between the traditional approach in methodology proposed by Mill and the neoclassical theory. However, the author clearly distinguishes two branches within this theory and clarifies that Mill's method can *only* be compatible with what the author calls the traditional versions of the neoclassical theory. In this connection the chapter will argue that the recent 'return to Mill' as proposed by Hausman's works in the economics methodology cannot be invoked for a defense of equilibrium theory *if* by this we mean the intertemporal or temporary general equilibrium models of the Arrow-Debreu type. The conclusion is that only within the traditional versions of neoclassical theory will one be able to abstract in a plausible manner from what Mill called the 'perturbing causes' affecting the actual equilibrium, while pursuing the same method of abstraction turns out to be implausible for the neo-Walrasian general equilibrium models inspired by Arrow and Debreu.

*ORTHODOX AND HETERODOX ECONOMICS IN RECENT ECONOMIC METHODOLOGY**

Douglas Wade Hands*

Myself when young did have ambition to contribute to the growth of social science. At the end, I am more interested in having less nonsense posing as knowledge. Frank Knight, 1956

1. Introduction

Thirty-five years ago, as I was starting graduate school, there was no real 'field' of economic methodology. There were of course methodological writings by influential economists e.g. Robbins 1932, 1952; Friedman 1953; Samuelson 1964, 1965, but these works were seldom of the same intellectual quality as the research that had made these economists famous as economists. There were also brief discussions of economics in influential books on the philosophy of science e.g. Hempel 1965, Nagel 1961, but they focused on general problems associated with the human and social sciences, rather than with specific issues concerning economics. There were two recently published case studies in the philosophy of economics written by philosophers – Hausman 1981 and Rosenberg 1976 – but in general the field was almost as unpopular among philosophers as it was among economists. Finally, and perhaps most importantly, there was beginning to be a

* Lecture prepared for XVII Meeting on Epistemology of the Economic Sciences School of Economic Sciences University of Buenos Aires, Buenos Aires, Argentina October 6-7, 2011. I would like to thank John Davis for helpful comments on an earlier version of this paper. Errors and omissions of course remain solely my responsibility.

collection of dedicated books on economic methodology – Blaug 1980a, Boland 1982, Caldwell 1982, Hutchison 1981, Latsis 1976, Wong 1978 and a few others – but it was a relatively assorted collection of texts with little to suggest that these books would end up being the foundational texts for the inchoate field of economic methodology. All in all, thirty-five years ago there seemed to be very little to encourage a young scholar thinking about an academic career in economic methodology or the philosophy of economics.

But thirty-five years is a long time, and I am happy to be able to report that the situation today is much improved. There are now dedicated journals such as *The Journal of Economic Methodology* and *Economics and Philosophy*, as well as numerous journals specializing in the history of economic thought that frequently publish methodological research. There is also a growing number of research institutes and professional societies dedicated to the intersection of economics and philosophy around the world. It is now possible for a young scholar to specialize in research connecting economics and philosophy without necessarily feeling like they are jeopardizing the possibility for a successful academic career. Of course, this does not mean that such careers are easy, or that all is well within the field – i.e. 'better' certainly does not imply 'good.' Particularly in the United States, the economics profession still seems to have little or no interest in elevating economic methodology to the status of a legitimate a field of inquiry within the discipline of economics. The financial crisis and the associated questioning of the methodological foundations of macroeconomic theory, seems to have initiated a momentary warming the relationship between mainstream economics and economic methodology, but who knows how serious the overtures are or how long they will last. Also, it is probably not a good sign that the profession considers economic methodology to be an inferior good in the traditional microeconomic sense: that is, one that economists consume more of when incomes fall.

The last twenty or so years have also witnessed a significant change in the traditional relationship between 'orthodox' and 'heterodox' schools of thought within economics. For most of the second half of the 20th century the economic mainstream, the orthodoxy, consisted of neoclassical microeconomics combined with some version of macroeconomics it was IS-LM Keynesian theory during the immediate post WW II period, and new classical macroeconomics and real business cycle theory later. On the other hand, the periphery of the discipline was divided into a small number of self-consciously heterodox schools of thought: Institutionalist, Marxist, Austrian, Post-Keynesian, and others. There were two key features to this half-century long equilibrium in economic theorizing. First, there was a dominant orthodoxy based on neoclassical principles – prediction and/or explanation of economic phenomenon in terms of the coordinated equilibrium behavior of rational self-interested agents – and those principles were strictly enforced. If there were no maximizing agents in the model, then it was not mainstream, and for the majority of the profession, not scientific, economics.[1] And second, those outside of the mainstream tended to be self-conscious members of some particular heterodox school. It was not simply a matter of there being a dominant mainstream and a disparate group of outsiders – not just the discipline's 'insiders' and the 'others' – there was a dominant neoclassical school and a number of different, but distinct and self-consciously identified, heterodox schools in the periphery. Very few economists were engaged in theorizing that was outside of the mainstream and yet also outside of any of these clearly-labeled heterodox groups.

This relationship seems to have changed during the last few decades. On one hand, many of the most important recent developments within economics have occurred within fields such as experimental economics, behavioral economics, evolutionary economics, and neuroeconomics. These are fields that are not

[1] The maximizing agents were explicit in microeconomics; in macroeconomics there were always ongoing efforts to find "microfoundations" – ways of grounding the macro-theoretical concepts on neoclassical principles. Although it is clearly recognized that the new classical macroeconomics that became dominant at the end of twentieth century was motivated by the desire for microfoundations, it is less well-recognized that even during the immediate post WW II period when Keynesian ideas dominated macroeconomics, there were also ongoing efforts to "ground" Keynesian ideas like the consumption function, liquidity preference, and the marginal efficiency of capital in individual maximizing behavior. The relevant "microfoundations" were defined more broadly during the Keynesian than the New Classical period, and perhaps the latter was more successful than the former in reaching its microfoundational goals, but the profession's preference for grounding macroeconomic concepts on neoclassical microeconomic principles was clearly revealed even during the Keynesian period.

necessarily ‘orthodox’ in the strict neoclassical sense – they produce anomalous results that conflict with standard neoclassical theory and they characterize choice in very non-neoclassical ways – but they are also not ‘heterodox’ in the traditional sense either; they are not Marxist, or Institutionalist, Austrian, etc. For some of the economists working in these new research programs, their research provides a radical new non-neoclassical approach to the prediction and explanation of economic behavior, but even among those who are less radical – those who believe that some version of neoclassical theory will eventually be able to subsume these new developments – there still seems to be a consensus that the problems and anomalies these fields have identified are real and deserve the profession's attention. This is very different than had been the case for many of the criticisms traditionally raised by heterodox economists. The Marxian concern with the exploitation of the working class by the capitalist class, or the Veblenian distinction between business and industry, were for most mainstream economists, not real issues that deserved the attention of the discipline. This is very different from, say, the mainstream's response to the endowment effects, reference dependency, and irreversibility of preferences, identified in more recent work by Daniel Kahneman, Amos Tversky, Richard Thaler, and others Kahneman 2003; Kahneman, Knetsch, and Thaler 1991; Kahneman and Tversky 2000; Thaler 1980; Tversky and Kahneman 1991, etc..[2] These concerns matter to mainstream economists in a way that most traditional heterodox concerns did not.[3]

There may also be changes underway within macroeconomics – changes initiated by what many see as the discipline's failure to predict, explain, or offer effective solutions for, the recent and on-going world financial crisis – but I will focus primarily on microeconomic developments. There are a number of reasons for

[2] One argument for the acceptance of these issues might be that some of these problems were recognized by the neoclassical economists of the ordinal revolution early in the 20th century. I have written in detail about this (Hands 2006, 2010 2011a), but it cannot be an argument for the recognition of these problems by the neoclassical mainstream because there is essentially no recognition by contemporary economists – either neoclassical or behavioral – that these some issues were also raised by economists during the ordinal revolution.

[3] See Sent (2004) a discussion of why this might be the case.

this. First, as I will argue later, microeconomics – individual choice theory in particular – is where much of the recent methodological research has been done – it is where the methodological action is, so to speak – and recent methodological research is the main focus here. Second, it is not at all clear at this point how, or if, macroeconomics will change. The changes taking place in microeconomics – whether they end up being revolutionary or reformist – have been ongoing for at least two decades and came mainly as a result of internal forces: the available laboratory evidence, new tools and ways of gathering data, and so forth. In the case of macroeconomics, the forces of change have been external – in the economy, not in economics – and have come quite quickly. The current crisis may end up having a profound impact on future macroeconomic theorizing in the way that the Great Depression did, but at this point that is not clear. Finally, given the particular features of the current crisis, <u>if</u> mainstream macroeconomics changes, it is possible that it will change back in the direction of Keynesian theory: not a new theory or a new methodological approach, but a revival of an earlier, and at least on some readings of Keynes once dominant, framework for macroeconomic analysis. This is quite different than in recent microeconomics where experimental and behavioral economists are now making it possible to do that which every influential methodological writer from John Stuart Mill, to John Cairnes, to Neville Keynes, to Lionel Robbins, to Milton Friedman, said was totally impossible – that is, experiments – and where neuroeconomics is adding new technology to render the previously immeasurable, now measurable.[4] It is useful also to note that this broadening of the base of acceptable approaches within mainstream microeconomics has occurred commensurate with a decline in the number of economists self-identifying with the traditional heterodox schools. This is not to say of course that Institutionalist economics, or Marxist economics, or other heterodox schools have completely disappeared, but simply that while there are many economists critical of mainstream neoclassical practice, those who

[4] Although it is certainly possible to combine developments in experimental and behavioral economics with an analysis of the macroeconomic crisis (e.g. Heukelom and Sent 2010).

are, seem to be focused on particular problems, applications, and tools, rather than self-identifying with any general heterodox school of thought.[5]

My talk will explore this three-way relationship between orthodox economics, heterodox economics, and economic methodology during the last few decades. I will begin by characterizing how work in economic methodology related to orthodox and heterodox theory during roughly the period 1975-2000 and then turn to how this relationship has changed in recent years.

2. Orthodox and Heterodox in Economic Methodology: 1975-2000

Unlike most fields within economics, economic methodology does not have a standardized framework for inquiry; there are a wide range of approaches, styles, tools from philosophy and elsewhere, as well as a wide range of goals what it is the methodological research is supposed to 'do'. Given this, how can I, in the time available, do justice to the methodological literature of the period 1975-2000? The truth is, I cannot, and for those interested in a detailed discussion of this literature I suggest a survey such as *Economic Methodology: Understanding Economics As A Science* 2010 by John Davis and Marcel Boumans or my own *Reflection Without Rules* 2001. My focus here will be much more modest. I will focus on the relationship between orthodox and heterodox economics in the work of two influential economic methodologists during the second half of the 20th century: Mark Blaug and Terence Hutchison.[6] There were many other doing very different types of methodology during this period, but these two authors seem to be representative of the most influential work in the field at least the work written by economists.

The first thing to notice about the methodological literature of this period is that it was based on what I have elsewhere called the 'shelf of scientific philosophy' view

[5] See Dow (2010) or Lee (2009) for an alternative reading of the current situation in heterodox economics.

[6] A non-exhaustive list of their important contributions to the methodological literature includes Blaug (1976, 1980a/1992, 1990, 1994, 2002, 2003) and Hutchison (1938, 1981, 1988, 1992, 2000, 2009).

of economic methodology Hands 1994, 2001. Ideas from the assumed given and stable shelf of scientific philosophy were simply taken off the shelf and 'applied' to the science of economics without reconfiguration or with much sensitivity to the peculiarities of the discipline. In the case of both Blaug and Hutchison, the relevant philosophical shelf was Popperian – based on Karl Popper's philosophy of science 1959, 1965, 1994 – and according to Popper in order to qualify as a real science a discipline needed to make bold falsifiable, non ad hoc conjectures and subject those conjectures to severe empirical tests.[7] Blaug and Hutchison both argued that while most economists claim to be engaging in this type of scientific activity, they in fact fail to do so: economists do not practice what they preach. Instead, economists are engaged in what Blaug called 'innocuous falsificationism':

> I argue in favor of falsificationism, defined as a methodological standpoint that regards theories and hypotheses as scientific if and only if their predictions are at least in principle falsifiable, that is, if they forbid certain acts/states/events from occurring ... In addition, I claim that modern economists do in fact subscribe to the methodology of falsificationism: ... I also argue, however, that economists fail consistently to practice what they preach: their working philosophy of science is aptly characterized as 'innocuous falsificationism.' (Blaug, 1992, p. xiii).

Such Popperianism offered tough standards – standards that Blaug and Hutchison argued economists could have, and should have, lived up to, but seldom actually did. It was an economic methodology that demanded economists clean up their act.

[7] Although it should be noted that neither Blaug nor Hutchison were entirely consistent about the substantive details of what a Popperian approach to economics (or any science) would entail. For example, Blaug was notorious about moving unapologetically between advocacy of Popperian falsificationism and advocacy of Imre Lakatos's Methodology of Scientific Research Programs (MSRP). Although both approaches are broadly "Popperian," they are quite different in detail with Lakatos sharply differentiating his view from falsificationism, and Popper denying that MSRP was in any way Popperian.

There are of course many well-documented problems associated with Popperian falsificationism – in general, as well as when specifically applied to economics – but that is not my topic here.[8] The task here is not to evaluate these positions, but simply to try to characterize the general tone/attitude of the methodological discussion of this period as represented by the work of Blaug and Hutchison and relate it to orthodox and heterodox economics.

So what did the methodology of Blaug and Hutchison have to say about heterodox economics, or the relative scientific standing of orthodox and heterodox economics? On the face of it quite a lot. Even a cursory examination of the methodological work of Blaug and Hutchison reveal that they directed a substantial amount of critical attention to heterodox theory of all persuasions: Marxian, Institutionalism, Post- and Fundamentalist-Keynesianism, Neo-Ricardian/Sraffian, Austrian, URPE-type late-1960s radical economics, and others. Blaug began his career with a methodologically-inspired historical study of Ricardian economics Blaug 1958 and he frequently criticized later Ricardians like John Stuart Mill for relying on introspection, ignoring the empirical facts of the mid 19th century British economy, and constructing various 'immunizing strategies' to insulate Ricardian economics from empirical falsification Blaug 1980a/1992. The Sraffa-based neo-Ricardians of the second half of the 20th century were also criticized on the same grounds, as well as for succumbing to 'formalism' Blaug 1990, 2009.[9] Blaug spent a substantial amount of time criticizing the labor theory of value and tendency laws such as the falling rate of profit in Marxian economics for not being falsifiable Blaug 1980b, 1990 and noted Popper's own remarks about the unfalsifiability of the Marxian system Popper 1976. Not to neglect the other side of the political spectrum, Blaug also had harsh methodological words for Austrian economists, particular Ludwig von Mises Blaug 1980a/1992. Similarly, Hutchison's first book Hutchison 1938 was primarily a methodological critique of Lionel Robbins's *Nature & Significance* 1932/1952, but it focused on the Austrian influence in Robbins's work. Hutchison

[8] See Hands (2001, pp. 275-304) or Hausman (1988).

[9] See Garegnani (2011) and Kurz and Salvadori (2011) for recent critical responses to Blaug on Sraffian economics.

continued to criticize Austrian economics throughout his life Hutchison 1981 and while, like Blaug, the main methodological villain was von Mises, he included others such as Friedrich Hayek as well Caldwell 2009. Hutchison criticized Marxian economics on grounds similar to Blaug's Hutchison 1981 as well as the Cambridge-fundamentalist version of Keynesian economics Hutchison 1981, 2009.

Based on all these criticisms, one might assume that Blaug and Hutchison used their Popperian methodology to defend the neoclassical mainstream against heterodox criticism. But that was not really the case. Both Blaug and Hutchison were just as critical of work in the neoclassical mainstream because it also was in conflict with the Popperian principles of bold conjectures and severe empirical tests. In particular, the formalist revolution which started during the 1950s and ended with the Arrow-Debreu abstract Walrasian general equilibrium theory that dominated microeconomics until quite recently, was harshly criticized by both Blaug 1980/1992, 1997, 2002, 2003 and Hutchison 1992, 2000. For example, Blaug called 1954 paper on the existence of competitive equilibrium by Kenneth Arrow and Gerard Debreu 'a cancerous growth in the very centre of microeconomics' Blaug, 1997, p. 3 and Debreu's 1959 *Theory of Value* 'the most arid and pointless book in the entire literature of economics' Blaug, 2002, p. p. 27. Hutchison was only slightly more positive in his appraisal, calling general equilibrium theory the substitution of 'fantasy content for realistic, or relevant, content' Hutchison, 2000, p. 18. But the criticism of neoclassical economics did not stop at the abstract Arrow-Debreu version of the theory. In fact, Blaug's survey of economic methodology 1980a/1992 was a veritable litany of criticisms of various aspects of the dominant neoclassical theory, with the eight chapters of Part III going topic by topic through standard neoclassical theory from consumer choice, to production theory, to general equilibrium, to international trade, etc., pointing out in each case how the theory failed to meet Popperian standards for scientific adequacy and/or progress. The only aspect of the mainstream theory of the day that Blaug seemed to give a positive nod was Keynesian economics, and even there he was critical of the 'Mickey Mouse versions of Keynes in the 1950s'

1980a, p. 221 as well as the fundamentalist Cambridge versions of Keynesian theory. Hutchison was generally not as aggressive in his critical stance, but he too was critical of the formalism and lack of relevance of much of the dominant neoclassical theory 1981, 1992, 2000. Like Blaug, he was not very clear about exactly what kind of economics would meet the tough Popperian standards, but he was clear that both the neoclassical mainstream and heterodox theory were methodologically problematic.

The bottom line is that the Popperian ‘shelf of scientific philosophy’ methodology of Blaug and Hutchison set the epistemic bar so high than essentially no economic theory could pass the scientific test. Although both Blaug and Hutchison probably favored the orthodox theory of the day – at least in its more applied, non-Arrow-Debreu, formulations – over various heterodox alternatives, it was a weak and frankly not very well-articulated preference since according to the methodological standards they endorsed, almost all economic theory was either unfalsifiable or false, and even the most serious empirical work was ‘like playing tennis with the net down’ Blaug, 1980a, p. 256. The shelf of scientific philosophy approach was often defended as a ‘tough’ approach to methodology, because it demanded compliance with a relatively strict set of methodological standards. For that reason it was often endorsed by those who sought to use it as a way to attack economic theories they did not support, but such a strategy was only effective as long as the critical fire was not turned on one's own position which, of course, it always could be. The toughness was explained as a kind of ‘tough love’ because even though it was strict, it was ostensibly done in the interest of helping the economics profession be epistemologically all that it could be. Unfortunately, since no economic theory, orthodox or heterodox, really passed the test, the discipline was left without any template for how particular fields or models might be improved, or how the discipline's cognitive value could be increased at the margin.

The literature on economic methodology expanded significantly during the period 1975-2000 – and for that we should be grateful since it helped establish economic methodology as a legitimate field – but it expanded in a way that prevented it from engaging in much constructive criticism, or in playing any significant role in the actual practice of economic theorizing, or in allowing orthodox theory to respond to the criticisms of heterodox economists or vice versa in any meaningful way.

3. Orthodox and Heterodox in Economic Methodology: the Recent Literature

John Davis, my co-editor of *The Journal of Economic Methodology* and others, have suggested that the mainstream of disciplinary economics is no longer neoclassical: that the once dominant neoclassical framework has been replaced by a new, more pluralistic, mainstream which is more open to psychology, less individualistic, accommodates various types of path-dependencies, and allows for a much broader class of modeling strategies and tools Colander 2000; Colander, Holt, and Rosser 2008; Davis 2006, 2008, Santos 2011. As David Colander, Richard Holt, and Barkley Rosser put it: 'Economics is moving away from a strict adherence to the holy trinity – rationality, selfishness, and equilibrium – to a more eclectic position of purposeful behavior, enlightened self-interest, and sustainability' 2008, p. 31. The most important piece of evidence for this change is the type of research that is currently being published in the most highly ranked economics journals: the *American Economic Review, Quarterly Journal of Economics, Economic Journal,* and even although perhaps to a lesser extent in the *Journal of Political Economy*. Another piece of evidence for this is that thirty years ago, most of the various specialty areas of research and teaching – labor economics, environmental economics, public finance, managerial economics, international economics, etc. – were simply particular 'applications' of the standard neoclassical utility and profit maximizing framework. Now each of these fields is more likely to employ particular tools and conceptual frameworks that are indigenous, and in some cases endemic, to the particular subfield. International economics is now more than Walrasian general equilibrium theory with countries A and B replacing individuals A and B, environmental economists now need to actually know something about the relevant biological science, and so forth.[10] Of course much of economic education – particularly undergraduate education – is still dominated by the neoclassical framework, but defenders of the 'neoclassical is dead' thesis have tried to explain this in terms of lags and the institutional structure of the discipline Davis 2006.

It is also important to note that the work identified with the new more pluralistic mainstream is not only not strictly neoclassical, it is also <u>not</u> heterodox either. Although many of the issues and anomalies identified in this recent literature have also long been identified by economists working within the heterodox tradition – think of the Institutionalist critique of neoclassical choice theory or the Institutionalist emphasis on evolutionary change, or the Post-Keynesian or Austrian emphasis on path-dependency and hysteresis – the economists working in these new fields do not generally self-identify with heterodox schools of thought. For example, the histories of behavioral economics produced by practitioners e.g. Camerer and Loewenstein 2004 often note Herbert Simon, James Dusenberry, and a few others from the middle of the 20th century, but do not generally cite any authors from the traditional heterodox literature. So too for earlier precursors. Behavioral ideas have been traced to Adam Smith Ashrof, Camerer, and Loewenstein 2005, David Hume Sugden 2006, Jeremy Bentham Kahneman, Wakker, and Savin 1997, and William Stanley Jevons and Francis Edgeworth Bruni and Sugden 2007, but not to authors such as Karl Marx, Friedrich List, J. A. Hobson, or Thorstein Veblen. If there is a new more pluralist mainstream forming, it is neither neoclassical nor heterodox.

Although I am not as convinced as many commentators that the mainstream is no longer neoclassical, I do think the trend is clearly in that direction, and more importantly here, I definitely believe that a substantial change has taken place

[10] As anecdotal evidence for this, at one point early in my teaching career I agreed to teach (undergraduate) international economics and public finance even though I never had graduate training in either of these fields. My thought was that I was well-trained in Walrasian general equilibrium theory and that was all I needed to teach any type of economic theory (at least at the undergraduate level). I would not agree to this today, but that is probably because I am older, wiser, and generally less accommodating to my department, but my point is that I doubt any of my junior colleagues would agree to such teaching today. The discipline is indeed different.

within economic methodology. In my 2001 book *Refection Without Rules* I argued that economic methodology was moving away from the 'shelf of scientific philosophy' and more in the direction of naturalism, context-specific inquiries, and research that draws on a wider range of intellectual resources than just the philosophy of natural science. That process was ongoing at the time and has surely continued, but what was not clear a decade ago is how changes in economics itself have also initiated changes in the way that economic methodology is done. The bottom line is that almost all of the real 'action' within contemporary economic methodology is in precisely the fields that Davis and others point to as elements of the new, more pluralistic, mainstream: neuroeconomics, experimental economics, behavioral economics, evolutionary economics, and research employing new tools such as complexity theory, computational economics, and agent-based modeling. Neoclassicism may not be dead, but it is no longer the focus of the cutting edge of methodological research – but then nor is heterodox economics. Neither Neoclassicism nor Heterodox economics are the main focus of recent methodological inquiry.

To provide some evidence for this claim about the recent methodological literature, let me just note a few of the most-discussed books on economic methodology during the last few years. A non-exhaustive list of such books would be Bardsley et. al 2010, Guala 2005, Reiss 2007, Ross 2005, and Santos 2010. Notice that the vast majority of these books focus on experimental economics, but they all examine the economic research in one or more of the new more pluralistic microeconomic fields. Also notice that they all focus on either one particular field, or a small set of fields, within areas of economics that are neither heterodox nor strictly neoclassical. From a methodological perspective they are relatively close-focused studies: only certain aspects, authors, and applications within a field or small set of fields. These are also books with a normative philosophical focus – they are not at least primarily historical or sociological; they are philosophical – but again, it is a local or micro-philosophical focus, not the universal 'one rule fits all science' approach of earlier methodological work like that of Blaug and Hutchison.

As another piece of evidence for this tendency, John Davis and I recently assembled a collection of papers by some of the most important contributors to the recent methodological literature. The book is *The Elgar Companion to Recent Economic Methodology* 2011 and it will appear in print later this year. The book has six sections: a section on methodological issues in contemporary choice theory, with papers on experimental economics, behavioral economics, and neuroeconomics; a second section on welfare economics, with many of the papers focusing on the economics of happiness and neo-hedonism; a third section on complexity, computational economics, and agent-based modeling; a fourth section on evolution and evolutionary economics; a fifth section on recent macroeconomics; and a final shorter section on the profession, the media, and the public. Notice that four sections out of six are dedicated to the areas of economics associated with the new pluralist mainstream in microeconomics. The last two sections are motivated in part by the recent macroeconomic and financial crisis and its impact on the profession and the public's perception of the profession. The point is that when we attempted to put together a collection of papers that represented the best work in the most active research areas within recent economic methodology, we ended up with no papers on traditional neoclassical or heterodox topics.[11] This is not to say that none of the authors offered a methodological defense of neoclassical economics – a few did – but it was never the main subject. To me this is a nice example of the fact that not only has pluralism of intellectual resources replaced the once-dominant 'shelf of scientific philosophy' within economic methodology, a new more pluralist mainstream has replaced the 'neoclassical shelf of scientific economics' as the dominant domain of inquiry regarding the important questions and concerns for methodological inquiry.

As a final bit of evidence for these recent methodological trends, it is useful to look at what seems to be the most influential methodological research by economic practitioners, that is economists who are not also contributors to the

[11] The possible exceptions, depending on how one defines orthodox and heterodox, are the four papers in the macroeconomics section.

general methodological literature[12]:Caplan and Schotter 2008.[13] Again, as with the methodological literature previously discussed, this book focuses on new pluralist areas like experimental economics, behavioral economics, and neuroeconomics. The volume contains the controversial ‘mindless economics’ essay by Faruk Gul and Wolfgang Pesendorfer 2008 and a series of comments on that paper by economists who are practitioners in the relevant, or closely related, fields.[14] The Gull and Pesendorfer paper has been much discussed and elicits a wide range of responses, but it and the commentaries on it exhibit many of the same features as the recent literature from within the methodological community: the focus is on the new pluralist fields within microeconomics, it has a normative – but narrowly targeted – philosophical focus, and it exhibits a pronounced disinterest in most of the traditional methodological questions associated with either neoclassical or heterodox economics. Two of the published responses from within the methodological community – Hausman 2008 and Ross 2011 – are quite different. Hausman is quite critical of not only Gul and Pesendorfer's methodological thesis, but also the revealed preference approach to choice theory on which it is based; while Ross is sympathetic to the revealed preference framework, but argues their methodological position needs to be strengthened in various ways.[15] Although the main subject of the Gul and Pesendorfer paper is behavioral and neuroeconomics, they end up defending what they call standard neoclassical economics although they define neoclassical in a very idiosyncratic way. This said – and even though they are defending a view they consider neoclassical – their work, like the commentaries on it, and most of the recent research from within the methodological community, demonstrates that the ‘hot’ methodological topics are in these relatively new microeconomic fields. The bottom line is that one does not need to be completely convinced that neoclassical economics has been displaced from its dominant position within the mainstream

[12] For example the various authors of Bardsley et. al (2010) are all practitioners in experimental and behavioral economics, but since many of the authors are also regular contributors to the methodological literature I listed this book as recent economic methodology (not practitioner's commentary).

[13] Another example is Smith (2009), but it explores a much wider range of topics.

[14] Only one of the contributors to the volume was a regular contributor to the methodological literature, the philosopher Daniel Hausman.

[15] My own critical preferences are closer to Hausman (Hands 2011b, 2011c).

to recognize that the most interesting and important methodological questions are no longer about either traditional neoclassical or heterodox economics, but rather, are about precisely the fields most often identified as representing a new more pluralistic mainstream.

This recent methodological literature is certainly less universalistic and more local, more naturalistic, and more sensitive to the particulars of the subfield within economics under investigation than the methodological literature of the period 1975-2000. Blaug's book *The Methodology of Economics* 1980a/1992 provided a methodological assessment of various areas within economics, but the Popperian assessment tools were exactly the same for every single area. Do they make bold empirical conjectures and attempt to falsify them? If yes, then it is good science, and if no, then it is bad science full stop. This is not the approach that is taken in most of the recent literature. A second point about this recent literature is that while it does exhibit the tendency to move away from the universalistic, and toward the particularistic, it is important that this movement does not imply an absence of philosophical rigor, a lack of normative assessment, or imply that anything goes. This was a claim often voiced in the earlier period; the argument was that once you give up on the strict universal rules for good scientific practice provided by the shelf of scientific philosophy, then one must end up with sociology, or science studies, or something other than real philosophy. Although I would note that science studies and these other fields provide perfectly legitimate approaches to the study of economic knowledge, such work does not validate the type of philosophical justification or normative appraisal that comes from the philosophy of science. My point is that the recent work in economic methodology, although much more particularistic, is in fact normative philosophy. Not having a single narrow standard – what Deirdre McCloskey 1994 aptly called 3" x 5" card philosophy of science – does not mean having no philosophical standards at all. Again all of works mentioned earlier are good examples of this.

4. Conclusion

It is probably useful to conclude by summarizing the various parts of the argument I have presented. The earlier methodological literature like the work of Blaug and Hutchison was aggressively normative in it style, and negative in its assessment. The message was 'this is what economists must do in order to produce scientific knowledge about the economy and economic behavior, and you either neoclassical or heterodox are not doing it.' And yet the methodological rules it endorsed were offered at such an abstract and universalistic level, and so insensitive to the interests and concerns of the economists actually working in the various specific subfields within economic science, that it had essentially nothing to offer either neoclassical or heterodox practitioners about how disciplinary practice might be improved. There were very general injunctions to 'test more' and 'be more realistic,' but there was no practical guidance to a group of economists working in a particular subfield struggling to extract as much knowledge as possible from the models and the data at their disposal while facing a wide range of subfield- and context-specific constraints. This is very different from the vast majority of the methodological literature of the last decade. For most of the recent research the domain of inquiry is neither neoclassical nor heterodox economics in general, but rather the many currently expanding subfields in microeconomics I have been discussing. In addition, it is not based on grand universalistic philosophy of science; it is applied philosophical inquiry aimed at the practical methodological issues of practitioners within specific subfields and sensitive to the issues, challenges, and constraints they face. It is important to note that while this more recent methodological work is local and close-focused, it is often critical – constructively critical – and it is philosophy-based. The argument that was often made in the earlier literature – Blaug 1994 is a good example – was that if one stepped down even a few steps from grand universalistic and 3" x 5" card rules for how all science must be done, one was necessarily on a slippery slope and doomed to doing mere history, or sociology of science, or science studies, or some other type of inquiry that was not grounded in the normative philosophical justification of scientific knowledge and practice. Of course, I believe that history, science studies, and sociological or anthropological studies of science including economics are interesting and important intellectual endeavors, but they do in fact have different goals, issues, and concerns than work grounded in normative philosophy. The point is that the recent literature in economic methodology clearly demonstrates that the entire slippery slope argument was an illusion. One can do local, subfield- and context-sensitive, studies in economic science that are philosophy-based and critical of current practice. Not only does one not need to give up on normative issues and philosophical justification, but one can produce work that actually offers the practicing economist some ideas about how knowledge production within specific subfields might be improved.

To conclude: there has been a lot of expansion and a lot of change within the field of economic methodology during the last thirty-five years. During these years the field has changed its general philosophical focus from universal rules borrowed from the shelf of scientific philosophy to local practical advice grounded in the interests and concerns of particular sub-fields; and it has changed its domain of inquiry from neoclassical and heterodox economics in general to the more pluralistic microeconomic approaches at the edge of the current research frontier. Since interests always matter in the developmental path of any research program – within a particular science or within the study of a particular science – these changes will, and to some extent already have, contributed to the re-alignment of interests behind the field of economic methodology. My guess is that these changes will contribute to the steady growth and increased health of the field, but one never knows. Economic theorists have recently re-discovered path-dependency and the significance of context; we should not forget that these things matter to the future of economic methodology as well.

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CAUSALITY, PLURALISM, AND ECONOMIC POLICY MAKING [1]

Luis Mireles-Flores

1. Introduction: on the practical relevance of causality

According to John Stuart Mill, causation 'is co-extensive with the entire field of successive phenomena' because 'every fact which has a beginning has a cause' (Mill 1874 [1843], 235, 236). While elaborating on these ideas, Mill makes a brief remark about the practical relevance of causal knowledge:

> Of all truths relating to phenomena, the most valuable to us are those which relate to the order of their succession. On a knowledge of these is founded every reasonable anticipation of future facts, and whatever power we possess of influencing those facts to our advantage" (Mill 1874 [1843], 235).

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The idea that knowledge about causal relations can be exploited for the attainment of practical goals seems uncontroversial: knowing that exposure to asbestos is a cause of cancer can be used to prevent cancer; knowing that oxygen is a cause of fire can be used to extinguish fire; knowing that poverty is a cause of social uprisings can be employed to avoid insurrections; knowing that excess demand is a cause of price increases can help determine whether it is lucrative to sell (or to buy) certain commodities; or knowing that education is a cause of economic growth can be used to guide national policy and personal decisions.

This insight about the practical usefulness of causal knowledge has been commonly taken for granted in the literature on causation. For instance, Nancy Cartwright has explicitly justified her probabilistic account of causality with a formalised version of this basic intuition. In "Causal laws and effective strategies" (Cartwright 1979), she writes that there is "a natural connection between causes and strategies that should be maintained: if one wants to obtain a goal, it is a good (in the pre-utility sense of good) strategy to introduce a cause for that goal" (p. 431). By 'the pre-utility sense of good' she means "effective" (p. 420). That is to say, the way causal knowledge can be exploited for practical purposes takes the form: if 'X *causes* G' is true, then bringing about X "will be an effective strategy for G in any situation" (p. 432).

Bert Leuridan, Erik Weber, and Maarten Van Dyck (2008) have labelled this position the 'standard view on the practical value of causal knowledge' (p. 298). According to them:

> [The standard view stands for] the thesis that the practical value of causal knowledge lies in the fact that manipulation of causes is a good way to bring about a desired change in the effect (Leuridan, et al. 2008, 299).

As these authors also point out, many philosophers working on causation have simply taken the *standard view* as given (pp. 298-299). This view is also one of the key motivations behind the scientific aim of distinguishing *genuine* causes from *spurious* ones, since claims about genuine causation 'are needed to ground the distinction between effective strategies and ineffective ones' (Cartwright 1979, 420). Accordingly, the standard view is (somewhat silently) embedded in most of the admittedly large literature on methods for causal inference (e.g., Simon 1954; Suppes 1970; Spirtes, et al. 1993; Scheines 1997; Glymour 1997; Pearl 2000; Hoover 2001; Shadish, et al. 2002; Steel 2004; Guala 2005).

The standard view is often illustrated with some evocative example involving smoking and lung cancer more or less as follows: suppose that 'S is a variable that codes for smoking behavior, Y a variable that codes for yellowed, or nicotine stained, fingers, and C a variable that codes for the presence of lung cancer' (Scheines 1997, 188). Suppose further that the actual causal structure among these variables is: S *causes* C, S *causes* Y, but C and Y are not causally related to each other, and all three variables regularly obtain together. The practical relevance of a genuine causal claim in contrast to a spurious relation is then shown to follow automatically: avoiding smoking would be an effective strategy for avoiding lung cancer and yellowed fingers, but steering clear of nicotine stained fingers (say, by wearing protective gloves while smoking) would be a plainly ineffective strategy for avoiding lung cancer.

As a result of the unquestioning acceptance of the standard view on the practical relevance of causation, little attention has been paid to the details of what it actually amounts to. What kind of practical power is causal knowledge in fact capable of conferring? Is it the same kind of practical power present in all forms of causal knowledge? How exactly is one meant to exploit or make use of causal knowledge for the effective attainment of practical goals in specific situations? Could there be any general methodological guidelines concerning the use of causal knowledge for practical purposes?

Instead of providing detailed philosophical answers to these types of questions, authors interested in causality have primarily directed their attention to some other (no less important) types of inquiries, for instance, conceptual or semantic questions such as 'what does *cause* mean?' (e.g., Russell 1912-1913; Ducasse 1926; Lewis 1973) or 'what is the logical form of causal claims?' (e.g., Davidson 1967);

ontological questions like ‘are there causal relations in the real world?’ (e.g., Salmon 1980; Menzies 1989; Dowe 2000); and epistemological questions such as ‘how can one distinguish, find, or learn about causal relations?’ (e.g., Simon 1954; Suppes 1970; Spirtes, et al. 1993).

Insofar as philosophy of science is meant to study not only the philosophical aspects of the historical and theoretical development of science, but also its practical benefits and applications, it seems appropriate to devote a fair amount of intellectual effort to the investigation of how scientific knowledge is, can, and should be employed to support, guide, or implement practical decisions and policy recommendations.[2]

To be clear, I do not want to claim that the standard view is mistaken. It can be taken as a starting point for more substantial philosophical investigations of the practical relevance of causal knowledge. It can also remain as a general motivation for the investigation of causality and the methodology of causal inference. However, the apparently simple and unproblematic idea presented in the standard view leaves in the darkness the concrete features of the process by which causal knowledge can be used to do things. My suggestion is simply to proceed and investigate such features more systematically and in more detail. A better understanding of such practical features of causation need not be of interest exclusively to philosophers, but might also help policy makers engage with the tasks of formulating, evaluating, and implementing reliable policy prescriptions on the basis of available scientific knowledge.

In this article, I will explore one particular aspect of causation that is entirely ignored by the standard view: *causal pluralism*. In the first part (section 2), I will elaborate on the idea that causation is a plural notion, i.e., that there are distinct theories of causation and distinct causal concepts which need not be extensionally

[2] As a reaction to this longstanding gap in the philosophical research, there have been a few authors who have openly suggested that philosophy of science could and should play a much more significant role in the investigation of how science is actually applied and used for practical (and policy) purposes; see, e.g., Cartwright 1974; Suppes 1984; Kitcher 2001; Douglas 2009; Mitchell 2009. My position here can be seen as an attempt to take this suggestion seriously and contribute to explore its possibilities.

equivalent, and as a result causal claims can have a variety of meanings and interpretations. Then I will present (in section 3) a generic schema to analyse the kind of causal claims that are commonly employed in the process of policy making. If causal pluralism is true, economists and policy makers should ensure that the different meanings of scientific causal claims that are to be used for policy purposes are unambiguously communicated and understood. Vague or plainly mistaken interpretations of a causal claim could lead to ineffective or counterproductive policy recommendations. In the final part (section 4), I will present an example from economic policy research that illustrates how causal claims are used to support policy recommendations despite the fact that their meanings are not made explicit and hence remain unclear. The example consists of policy recommendations based on unemployment research done by the Organisation for Economic Co-operation and Development (OECD).

2. Causal pluralism and practical relevance

There are several accounts in the philosophical literature proposing and exploring a number of varieties of causal pluralism (e.g., Hitchcock 2003, 2007a, 2007b; Godfrey-Smith 2009; Reiss 2010; De Vreese 2010). In particular, I will refer here to two forms of causal pluralism which have implications for policy-oriented science: pluralism about theories of causality and pluralism about causal concepts.

2.1. Pluralism about theories of causality

It is possible to identify at least five main traditional theories of causality available in the philosophical literature. Each of these theories characterises causation in terms of an alternative notion, allegedly more primitive than causation, which in turn allows one to define some comprehensive criteria for what counts as causal. The primitive notions put forward in the five theories are, respectively: law-like regularities, probabilistic relations, counterfactual relations, physical processes, and potential manipulations. The gist of each of these theories of causation goes as follows:

Regularity theory: X *causes* Y if and only if there is a regular connection between the occurrences of X and the occurrences of Y (see, e.g., Hume 1975a [1739]; 1975b [1777]; Mill 1874 [1843]; Davidson 1967; Mackie 1974).

Probabilistic theory: X *causes* Y if and only if the occurrence of X increases the probability of an occurrence of Y (see, e.g., Suppes 1970; Cartwright 1979; Skyrms 1980; Eells 1991).

Counterfactual theory: X *causes* Y if and only if both X and Y have occurred and had X not occurred, then Y would have not occurred (see, e.g., Lewis 1973; Swain 1978).

Process theory: X *causes* Y if and only if both X and Y have occurred and there is a physical process from X to Y (see, e.g., Salmon 1980; Dowe 2000).

Manipulationist theory: X *causes* Y if and only if bringing about or affecting the occurrence of X brings about or affects the occurrence of Y (see, e.g., Gasking 1955; Menzies and Price 1993; Woodward 1996; 2003).

These theories were (at least originally) intended as accounts of *in virtue of what* a relation between X and Y is causal, and presented in terms of necessary and sufficient conditions. Nevertheless, it is recognised by now that these attempts to capture the 'fundamental nature' of causation with a universal approach are all defective (see, e.g., Hitchcock 2003; Cartwright 2004; Campaner and Galavotti 2007; Reiss 2009a). There is a huge literature that presents and discusses counterexamples designed to challenge either the necessity or the sufficiency criteria for each of the posited universal accounts.[3] As a consequence, the quest for a univocal account of causation has been gradually substituted by a pluralistic view which can be described as follows.

[3] For instances of discussions about these counterexamples, see the articles included in Sosa and Tooley 1993; and in Collins, et al. 2004.

> ***Pluralism about theories of causality*** is the position that there is no univocal theory providing all necessary and sufficient conditions for causality, but rather each of the general theories might capture one aspect or another (i.e., regular connections, probability raising, counterfactual dependence, processes, or interventions) of the nature and meaning of being causal (see, e.g., Longworth 2009; Psillos 2010).

Accordingly, for a claim "X *causes* Y" to be considered as genuinely causal it is sufficient that it fulfils the conditions of at least one of the available general theories of causality. This form of pluralism is entirely compatible with the fact that some relations which one theory of causation takes as causal do not count as causal under some other account. Once the goal of finding the ultimate all-encompassing theory of causation is abandoned and a pluralistic position is adopted, the counterexamples to each of the theories become harmless (see Longworth 2009).

Now let us think of economics for a moment: is it at all clear which theory or theories of causality are endorsed when formulating and testing causal relations in economics? Is it obvious (to economists and to the users of economics) according to which theoretical account the established causal claims should be interpreted? These questions are fundamental since adopting one or another theory of causality to analyse the meaning of particular causal claims can lead to different practical implications.

For instance, if 'X *causes* Y' means that the occurrence of events of type X increases the probability of the occurrence of events of type Y, one can perhaps forecast the *likelihood* of events of type Y after observing an occurrence of X. In contrast, if the meaning of the causal claim is that there is a deterministic physical causal process from X to Y, then one could confidently *produce* Y by triggering X and by ensuring that the causal process is not disrupted. Or one might even be able to *replicate* or *reproduce* the causal process as it would be convenient for attaining a desired practical goal. In the case one also knows that the causal process linking X and Y is invariant to a wide range of interventions, then it might be possible to generate *fine grain variations* of Y (see, e.g., Woodward 2010), or perhaps even to elaborate a *detailed mapping* about how certain precise manipulations of X would generate definite changes in Y.

Again, the story can change a great deal if what one means by 'X *causes* Y' is that had X not occurred, then nor would Y have occurred. Knowing that such a claim is true could be useful to ascribe some *causal responsibility* to factor X, given that one observes that Y obtained.[4] But knowing that an instance of X has been causally responsible for a particular occurrence of event Y provides almost no grounds to infer much about future occurrences of Y. Counterfactual dependence was in fact originally considered mainly appropriate for cases of so-called 'singular causation', cases in which X and Y represent particular occurrences and not general types of events (see Lewis 1973; Sober 1985; and Eells 1991, chapter 6). In this sense, it is difficult to see how claims about singular causation could be practically useful, say, to generate *reliable forecasting* of future occurrences of Y.

The fact that there are several theories that can be used to interpret causation suggests a practical consideration: it seems recommendable that scientists and the users of scientific causal knowledge are clear about the theory (or theories) of causation used to analyse the causal claims that are to be subsequently employed for policy purposes. Still, even if everybody agrees on the theory of causality being used, there are some further distinctions that have to be made explicit in order to properly disambiguate the meaning of causal claims. To see this, let us consider the second type of pluralism.

2.2. Pluralism about causal concepts

This form of pluralism is different from the previous in that it is not concerned with the variety of theories of *in virtue of what* a posited relation from X to Y is causal. Instead, it is concerned with *different ways* in which the causal influence

[4] To adjudicate causal responsibility seems to be a significant practical goal in some sciences, such as history, law, archaeology, and the like, in which an event that has already occurred has to be established as either causal or not. For a detailed elaboration on the particular roles of counterfactual causation in law and history, see Hart and Honoré 1985; and Reiss 2009b.

from X to Y can obtain relative to the causal structure in which it occurs. I will describe and illustrate in more detail the idea of different ways of 'causing' bellow (in section 4). But to give you a basic grasp, suppose there is a genuine causal relation between X and Y in a causal structure that includes some known additional factors Z = $\{Z_1, Z_2, \ldots, Z_n\}$, then the causal influence from X to Y can obtain in at least two ways: either through a single path from X to Y, or through more than one path, with each of these paths including one or more different members of Z (Hitchcock 2001b). Alternatively, the causal influence from X to Y can be either sufficient, but not necessary to produce or affect Y (since perhaps Y can also be caused by some Z_i alone), or necessary, but not sufficient for Y (since perhaps some Z_i is always required to interact with X in order to cause Y). Then again, in some other cases, the causal influence from X to Y takes the form of X preventing or interrupting the occurrence of Y (Dowe 2001). The characterisations and labels for these and other distinct ways in which genuine causation can obtain are commonly referred to as "causal concepts" (see, e.g., Hitchcock 2003, 2007b; Godfrey-Smith 2009; Reiss 2010). Thus the second type of causal pluralism can be described as follows.

> ***Pluralism about causal concepts*** is the position that there are different causal concepts, each one corresponding to a distinct type of causal influence occurring in a causal structure. These concepts are all causal, and none of them is privileged as the main or the most basic concept of causation (e.g., Hitchcock 2001b; Hall 2004; Cartwright 2004; Reiss 2009a, and 2010).

Accordingly, given that a certain claim is deemed to be genuinely causal (in line with one, some, or all general theories of causality), it can have different meanings depending on the causal concept that properly captures the type of causal influence singled out by the claim. Some examples of different potential meanings for the claim "X *causes* Y" are:

X is a *net cause* of Y

X is a *contributing cause* of Y

X is a *sufficient cause* for Y

X is a *necessary cause* for Y

X is a *preventative* for Y

These propositions have distinct meanings because they refer to different ways in which X causes Y. Notice that, in principle, the different causal concepts appearing in the propositions above could all be characterised by any one of the general theories of causality (see Hitchcock 2001b). Hence, conceptual causal pluralism does not entail pluralism about theories of causality. Analogously, it is possible to hold a monistic position about causal concepts (i.e., to argue that there is only one causal concept) and also accept that different theories of causality capture different aspects of what is to be causal (see, e.g., Russo and Williamson 2007; Williamson 2008; Casini 2012). Hence, pluralism about general theories of causality does not entail conceptual pluralism.[5]

The implications for the practical relevance of causal claims should be obvious. If different interpretations of the claim "X *causes* Y" correspond to different causal concepts, and thus refer to different ways in which the causal influence from X to Y obtains, then the same causal claim can have different practical potential and be useful for policy in different ways depending on which interpretation is taken as its actual meaning. Different ways of 'causing', denoted by distinct causal concepts, need not be practically relevant in exactly the same way for the attainment of a particular goal. And therefore, at least for policy purposes, the specifics of the causal concepts employed in scientific claims should be made explicit.

[5] Some of the new pluralistic approaches (both about causal theories and about causal concepts) have become more epistemologically rather than metaphysically motivated, and hence have moved from questions about what is the nature of causation to questions about what are the most useful ways of investigating and learning about causal relations. Along the same line, causal accounts such as Woodward's or Pearl's—which can be taken as pluralistic in the two forms I have described—are not anymore in search for the best theory about the nature of causation, but rather use elements of several theories of causality (probabilities, counterfactuals, interventions, and so forth) in order to investigate and characterise different causal concepts, and to illuminate various issues of causal inference and causal explanation (see Woodward 1996, 2003; and Pearl 2000).

For instance, the practical relevance of a causal claim could differ, on the one hand, when it means that X is a *preventative* of Y, and on the other, when it means that X is a *contributing cause* of Y. For if X is a preventative of Y, then one knows that a causal interaction between X and a causal process that results in Y can preclude the occurrence of Y, and that if X does not occur, then the causal process will actually result in Y (see Dowe 2001). In such a case, X is sufficient to prevent Y. Whereas if X is a contributing cause of Y, then all one knows is that X has a component causal influence on Y along one particular causal path (see Hitchcock 2001b, 374; Woodward 2003, 57). Given that there could be various paths going from a cause to an effect, component causal influences need not be sufficient for producing their posited effect. From this comparison, it seems that preventatives can have a higher practical power than contributing causes.

As another example, knowing that X is a *net cause* of Y, i.e., that the causal influence from X to Y includes all relevant causal paths to Y, and thus knowing that that X is sufficient for affecting Y in a certain causal structure (see Hitchcock 2001b, 369-373) offers a different practical power from knowing that X is an INUS condition (an insufficient but necessary part of a set of conditions that together are unnecessary and sufficient) for Y (see Mackie 1974, chapter 3). If the goal is to produce, or to predict as accurately as possible an occurrence of Y, then knowing that X is a net cause of Y confers a more reliable practical power than knowing that X is an INUS condition, which in fact—without having also the appropriate information about the rest of the causal factors that together with X are at least sufficient for Y—would only offer a limited and somewhat unreliable practical power.

These rough illustrations are only meant to give a broad impression of the different ways in which scientific causal knowledge can be exploited for practical purposes depending on its different causal interpretations. Different causal concepts need not have the same practical relevance, and hence conceptual causal pluralism has direct consequences on how one would interpret and be entitled to use causal knowledge to design or implement policy recommendations. Notice that this is a crucial feature of the practical relevance of causation about which the *standard view* remains completely silent.

If the ideas presented in this section are correct, then clarifying or disambiguating the meaning of causal claims is of utmost importance before using them as the basis for any recommendation or implementation of a policy. It is still to be seen whether this step is completed or bypassed in practice, i.e., whether the causal claims that are used to guide policy have an accurate and unambiguous meaning. The example in the following sections exposes just how complicated it can be in practice to get a clear-cut meaning for certain policy-oriented causal claims, thereby leaving the door open for many not entirely justified interpretations and casting doubt on their reliability for policy use.[6]

3. Causal claims in economic policy making

The sort of causal claims generated and employed in policy making are in most cases 'causal generalisations' (see Hitchcock 2001a). A classic example of this type of claim is 'smoking *causes* lung cancer'. Philosophical discussions of this case commence by demonstrating how theoretical and empirical evidence accumulated over the years and transformed the claim from a highly contentious hypothesis into a well-established causal generalisation (but see Hausman 2010). Given the eventual scientific consensus on the validity of this generalisation, recommendations about reducing smoking in order to diminish the incidence of lung cancer have then been presented as direct (and somewhat obvious) practical implications of the truth of the causal claim, more or less without qualifications.

Similarly, in economics, causal generalisations are the main kind of causal claims investigated, established, and considered in order to support practical recommendations. The research on the institutional determinants of unemployment done by the Organisation for Economic Co-operation and

[6] The example presented in what follows is based on work in progress in collaboration with François Claveau from EIPE, Erasmus University Rotterdam. For details on our joint endeavour, see Claveau and Mireles-Flores 2011. For additional insights concerning other methodological aspects of the OECD research on unemployment, and of the economics of unemployment more in general, see Claveau 2011; 2012.

Development (OECD) in the 1990s illustrates this. The initial motivation for this research came from the persistence of high unemployment in most OECD member countries throughout the 1980s and into the early 1990s. The ministers of these countries then required the secretary-general “to initiate a comprehensive research effort on the reasons for and the remedies to the disappointing progress in reducing unemployment” (OECD 1994a, 1).

The major result of this effort was the 1994 *OECD jobs study*, which was presented in two parts: a ‘scientific’ report, subtitled ‘evidence and explanations’ (OECD 1994b), explicitly put forward as the evidential base for a subsequent ‘policy-oriented’ report that was subtitled ‘facts, analysis, strategies’ (OECD 1994c). The transition between these two reports was mainly done through the formulation of causal generalisations. In this case, the process whereby scientific causal knowledge was used to come up with effective remedies to unemployment can be thought of as following three general stages: first, some scientific evidential base is gathered and investigated; second, causal generalisations about unemployment are established on account of the evidential base; and third, policy recommendations are proposed on the basis of these scientific causal claims.[7] Some examples of causal generalisations presented in the OECD research on unemployment are the following:

> **1**. The lack of labour market flexibility of the OECD economies is the principal cause of high and persistent unemployment (OECD 1994b, p. vii).
>
> **2**. More generous unemployment benefits cause higher unemployment (OECD 1994b, pp. 19, 29, 38, 50).

[7] To be fair to the OECD, one can find here and there statements in the reports that make the narrative more complex—for instance, by raising the possibility of contextual interferences affecting certain results. Nonetheless, what seems beyond doubt is that, after the publication of the report, its causal generalisations and the associated policy recommendations became established recipes for the relevant expert community with all caveats stripped and until very recently entirely forgotten. For discussions of the impacts of the OECD study on the expert academic community, see, Freeman 2005, 131-132; Blanchard 2006, 51-52; Boeri and van Ours 2008, 1-2. For a more recent and revised perspective on unemployment by the OECD, see OECD 2006.

3. Short-time work schemes help preserve permanent jobs (OECD 2010, p. 68).[8]

Ignoring for the time being the first and the second stages of the policy making process (investigating the evidential base and using that base to infer some causal truths), then the main question is: Are the meanings of these causal generalisations definite enough so as to confidently base policy recommendations upon them? To explore possible answers to this question, let us take the following schema as capturing the generic form of a causal generalisation:

$$(\text{For } P),\ X \hookrightarrow Y \qquad\qquad \text{(schema CC)}$$

In this schema, the symbol '$\hookrightarrow$' is a connective that refers to a generic causal relation where the causal influence goes from X to Y (in principle signifying any conceivable causal concept and analysable by any theory of causality), 'X' and 'Y' stand for the causal relata,[9] and the clause 'For P' specifies the relevant population for which the causal claim is meant to be true, where P is composed by individual units u_i, such that $P = \{u_1, u_2, \ldots, u_n\}$. Some potential sources of ambiguity in the meaning of the causal generalisations above can be identified by analysing the possible meanings of the causal relation '$\hookrightarrow$'in schema CC in the context of the OECD policy-oriented research.

4. Potential meanings of the causal relation

As was mentioned above (in subsection 2.1), the problems with each of the main univocal theories of causality have lead philosophers to develop less ambitious and more plural accounts that desist from reducing causality to any primitive notion, and rather intend simply to characterise causal relations in a clear and tractable manner. In economics, it is seldom explicit the theory of causality with

[8] Short-time work schemes are public schemes inciting employers to temporarily reduce the number of working hours of their employees instead of laying them off.

[9] Following a strong trend in the philosophy of causation (e.g., Spirtes, et al. 1993; Pearl 2000, Hoover 2001; Hitchcock 2001a, Woodward 2003; Hausman 2005), and in conformity with general usage in economics, upper-case italics (X and Y) are variables, and lower-case italics (x and y) represent specific values of these variables.

which one is supposed to understand economic causal relations. However, existing work on causality and causal inference suggests that certain non-reductive versions of the manipulationist approach are indeed suitable for analysing economic causal claims (see, e.g., Hausman 1998; Pearl 2000; and in particular Hoover 2001). Whether this is actually the right approach for economics in relation to the plurality of causal theories is a topic that deserves further attention. Yet, for simplicity reasons, in what follows I will exclusively focus on the consequences of the second form of pluralism—i.e., *conceptual causal pluralism*—for the meaning of the OECD causal generalisations.

To begin, let us consider the potential meanings of the causal relation '↪' in a claim like that represented by schema CC, but with only one single unit (u_1) as the relevant population referred to in clause 'For *P*'. In the context of the OECD research, units in the population are individual countries. To restrict the analysis to "single-unit causal claims" is useful for at least two reasons: first, it helps us focus on what happens when distinct causal concepts are picked out by the causal relation '↪', without having to deal with additional semantic ambiguities that arise from having different kinds of population compositions (e.g., causally heterogeneous populations). And secondly, the clarification of the meaning of claims with multi-unit populations can be subsequently approached by asking which single-unit causal claims are actually entailed by a posited causal generalisation.

This latter issue is especially significant in policy-oriented sciences in which causal generalisations are meant to be useful for the production of effects in particular members of *P*, and not necessarily in the population as a whole. For instance, policy makers aiming at reducing unemployment can be thought of as being interested not only in reducing the general incidence of unemployment for the totality of OECD countries, but mostly in how *one particular unit* (i.e., a specific country) can reliably reduce *its* unemployment. This surely is the primary interest from the perspective of the government of any particular OECD member country.

There are at least two distinctions among causal concepts that are relevant to the meaning of '↪' in the case of the OECD generalisations: (1) *net* versus *component* causal effects, and (2) *necessity* versus *sufficiency*.

4.1. Net versus component causal effects

This distinction comes from the fact that in some cases X can affect Y through multiple causal paths in a given causal structure. Then, as it was already hinted at (in subsection 2.2), a claim about a net effect means that X affects Y taking into account all the existing causal paths, while a claim about a component effect means that X affects Y only along a particular causal path. Following Hitchcock's (2001b) version of this distinction:[10]

> **Net effect**: X has a net effect on Y if and only if Y varies as X is varied while holding fixed other appropriate factors, including common causes of X and Y, but excluding factors intermediate between X and Y (Hitchcock 2001b, p. 372).[11]
>
> **Component effect**: X has a component effect on Y along a particular causal route if and only if Y varies as X is varied while holding fixed other appropriate factors, including factors that are intermediate between X and Y along other routes (Hitchcock 2001b, p. 374).[12]

Consider a causal structure for some particular unit, which can be represented with the following structural equations:

[10] The terminology employed by Hitchcock is explicitly meant to be "theory-neutral" with respect to any existing theory of causality (see Hitchcock 2001b, 369). To Hitchcock the distinction between these two causal concepts is meant to hold "without presupposing any one theoretical perspective [about causation]" (p. 369). Notice this is also in agreement with the two distinct forms of pluralism presented above (in section 2).

[11] The notion of a 'net effect' is also sometimes called 'total effect' (Pearl 2000, pp. 151-152, 164; Pearl 2001) or 'total cause' (Woodward 2003, pp. 50-51).

[12] This causal concept, which Hitchcock (2001b) calls 'component effect along a causal route', is essentially the same that Woodward (2003, pp. 50, 57) calls 'contributing cause', and fairly similar to what Pearl (2001) defines as 'path-specific effect'.

$X = E_X$

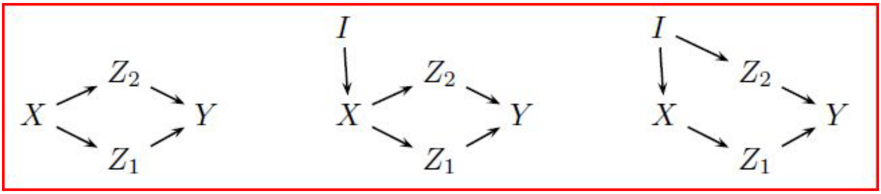
$Z_1 = \alpha_1 X + E_1$

$Z_2 = \alpha_2 X + E_2$

$Y = \beta_1 Z_1 + \beta_2 Z_2 + E_Y$

In these equations, X and Y are variables standing for the causal relata, Z_1 and Z_2 represent causal factors that are intermediate between X and Y along two distinct causal paths, and the Es represent all other (uncorrelated) relevant causal factors. The causal graph for this system is shown in Figure 1, Graph 1.

Figure 1



Graph 1 **Graph 2** **Graph 3**

Accordingly, the *net* effect of X on Y (denoted here as '$Net\Delta y$') can be interpreted as the change in Y due to an intervention I that varies the value of X from x_0 to a new value x_1 (see Figure 1, Graph 2). Thus the *net* causal effect of the change in X ($\Delta x = x_1 - x_0$) will be $Net\Delta y = (\beta_1\alpha_1 + \beta_2\alpha_2)\Delta x$.

The interpretation of the *component* causal effect of Δx on Y along one causal route (here denoted with the label '$Comp\Delta y$') can be similarly obtained, yet by introducing a more complex intervention. To consider only the causal effect along the causal path passing through Z_1 (as shown in Figure 1, Graph 3), the required intervention involves changing the value of X from x_0 to x_1, but also breaking the influence of X on Z_2 such that Z_2 remains fixed with the value z_0—i.e., the value it would normally have when X is set to x_0, even though the value of X is now set to x_1. The component effect along the causal path of Z_1 will then be $Comp\Delta y=\beta_1\alpha_1\Delta x$.

Now consider the OECD causal generalisation: ‘generosity of unemployment benefits (B) *causes* unemployment (U)’. As Daniel Hausman suggests, let us call ‘causal role’ whether the causal influence of a cause on its effect is positive or negative (Hausman 2010, 48). The causal role from B to U is commonly believed to be positive due to evidence of certain mechanisms in the labour market, such as the ‘job search effect’, i.e., that as generosity of benefits increases, jobless individuals covered by an unemployment benefit program tend to search for jobs less intensively (see Boeri and van Ours 2008, 11.2.1). Exclusively taking into account this component effect, then an increase in B would be said to cause an increase in the unemployment rate U. The corresponding policy recommendation would be to reduce and keep under strict control the generosity of unemployment benefits “at levels that do not discourage job search excessively” (OECD 2006, p. 21), which is indeed what the OECD study recommends.

However, this would be oversimplifying the matter, since some economists argue that there is also a countervailing ‘entitlement effect’ that negatively connects the generosity of unemployment benefits to the unemployment rate. As the story goes, if B increases, then the jobless individuals currently ineligible for unemployment benefits would have a stronger incentive to get employed soon, because they will be entitled to higher unemployment benefits in case they lose their jobs in the future (see Boeri and van Ours 2008, 11.2.2). Thus, exclusively taking into account this component effect (i.e., the entitlement effect), an increase in B would be said to cause a decrease in the unemployment rate U, and more generous unemployment benefits would then supposedly be an effective policy recommendation to reduce unemployment.

As a matter of fact, the ‘job search effect’ is believed to dominate the ‘entitlement effect’, thus according to the extant literature, the net causal effect from B to U is normally said to be positive. Yet, even when the net effect is well known in general, making explicit all the potential component effects can still be quite useful for policy purposes. For instance, before implementing any concrete change in the generosity of a system of unemployment benefits, it might be wise that policy makers first evaluate whether the 'entitlement effect' could possibly counterbalance the other component effects with an opposite causal role in the particular country in which the policy will be implemented.

4.2. Necessity versus sufficiency

With respect to the notions of necessity and sufficiency, the single-unit causal claim '(For u_i), $X \hookrightarrow Y$' could mean four different things: First, that for unit u_i, bringing about a change in X is *necessary* and *sufficient* to induce a change in Y. Second, that for u_i, a change in X is *necessary* (but not sufficient) for a change in Y. Third, that for u_i, a change in X is *sufficient* (but not necessary) for a change in Y. And fourth, that for u_i, a change in X is an *insufficient* but *necessary* element of an *unnecessary* but *sufficient* set of causal factors, i.e., an *INUS* condition, for a change in Y (see Mackie 1974, chapter 3). This fourfold distinction can be illustrated employing some simple equations representing each of these types of causal connections.

First, an instance in which a change in X is *necessary* and *sufficient* for changing Y can be represented by:

$Y=\alpha X$ (with $\alpha \neq 0$)

Alternatively, an example of a causal system in which a change in X is *necessary* (but not sufficient) for a change in Y can be given by:

$Y=Z*X$ (where Z is a variable for which $D_Z = \{0, 1\}$)

Notice that, in this latter expression, the multiplication of the variables indicates that it is a causal interaction between X and Z what is required to affect Y, rather than a change in X (or in Z) alone (see Woodward 2003, 44-45). Whereas a case in which a change in X is *sufficient* (but not necessary) for a change in Y is given by:

$Y = \alpha X + E$ (where E stands for other relevant causal factors for Y)

In contrast to the previous expression, the linear addition of variables implies that there are no interactions among the causal factors, i.e., a change in any independent variable (in this case either X or E) is sufficient for a change in Y. Finally, by combining some components of the previous equations, it is possible to get an example in which a change in X is an *INUS* condition for a change in Y:

$Y = Z * X + E$

In this last case, a change in X is a necessary element of a set of conditions (but not sufficient, since Z also would have to take the value of 1) which are jointly sufficient to generate a change in Y, yet also jointly unnecessary (since a change in Y can alternatively be the consequence of a change in E).

These distinctions are mainly intended to clarify two points: First, that each of the four causal notions depicted above refers to four different ways in which a change in X can bring about a change in Y for a given unit u_i. The main difference among these concepts is that each one imposes different requirements on the changes in X in relation to a causal structure, so that a change in Y actually obtains. Thus, for policy purposes it would seem indispensable that the different requirements of sufficiency or necessity for any causal claim are made explicit and that the particular causal structure of the actual system in which a policy is going to be implemented is properly checked to conform to such requirements. In other words, knowing that 'X *causes* Y' is true is not enough for policy purposes without also knowing the specifics of the meaning of '$\hookrightarrow$' in terms of its necessity and sufficiency requirements in relation to the causal structure of application.

Secondly, when a causal relation is presented in the form of an equation—or alternatively when some structural equation is given a causal interpretation—the functional specifications of the equation presuppose the choice of a particular causal concept in terms of sufficiency and necessity requirements. This point is especially relevant to policy-oriented social sciences such as economics in which most of the empirical research conducted to build an 'evidential base' for causal generalisations employs some version of structural equations analysis.[13] In general, the regression literature in economics employs linear specifications to establish constant (and homogeneous) causal effects.[14] A typical regression equation looks like:

$$\boxed{Y = \alpha X + \sum_i \delta_i Z_i + U_Y}$$

From this linear functional form it is clear that empirical studies which employ this basic type of equations to estimate causal effects assume that a change in *X* is *sufficient* for a change in *Y*, regardless of any change in the values of the other variables Z_i.[15]

Issues of necessity and sufficiency could lead to some semantic ambiguity in relation to policy-oriented scientific claims. These can be exposed by probing the correct interpretation of the causal claim 'generosity of unemployment benefits (*B*) *causes* unemployment (*U*)'. Given that the OECD research on unemployment recognises that distinct causes can affect *U* through paths that are independent from *B*, then any interpretation of *B* as a *necessary* cause can safely be ruled out, which leaves us with two alternative interpretations: *B* is either a *sufficient* cause or an *INUS condition* for a change in *U*.

The OECD research uses regression methods extensively to study the labour market institutions, and hence there is some indication that the causal claim can be interpreted as 'a change in *B* is *sufficient* to bring about a change in *U*'.

[13] Some insightful accounts on the use of regression analysis as the primary tool in empirical economics are: Pearl 2000, chapter 5; Hoover 2001, chapter 7; Morgan and Winship 2007, chapter 5; Angrist and Pischke 2009.

[14] This sentence in fact refers to two assumptions common to regression analysis, 'linearity' and 'homogeneity', yet the focus in this section is only on the consequences of the former. The consequences of the latter assumption on establishing causal effects in heterogeneous populations for policy purposes are extremely significant, since in heterogeneous populations it is not straightforward whether generalisations that are true for the population as a whole would also be true for individual units of the population. For some discussions of this topic, see Hitchcock 2001a; Steel 2008; Hausman 2010.

[15] The regression equation could let go this assumption of sufficiency by allowing interaction terms like $X*Z_i$, yet this would again imply a quite specific form of interaction between the regressors.

Nevertheless, closer inspection of the narrative in the OECD research reveals some recognition of causal interactions among distinct factors that affect unemployment, such as interactions between *B* and changes in the GDP in the member countries. Therefore it might be more appropriate to interpret *B*, the GDP, and any other relevant causal factor as INUS conditions. This seems to be the OECD position in some parts of the reports:

> The hypothesis here is not that unemployment benefits (or other structural factors) may cause movements in unemployment independently of movements in GDP, but rather that they may contribute, for example, to the unexpected depth or prolonged nature of a recession (OECD 1994b, p. 171).

Accordingly, since the relevant causal factors are thought to interact with each other when they affect the unemployment levels, the OECD recommends that the policy interventions should consist of a package of various interactive interventions directed concurrently to the relevant causal factors:

> The Restated OECD Jobs Strategy has four pillars. All countries need to ensure that each of the four pillars is solid. However, within each pillar there may be scope for individual countries to use different policy combinations to achieve successful outcomes, taking into account policy interactions and country circumstances and objectives. Indeed, *there is no single successful approach*; what matters is that the policy package be coherent (OECD 2006, p. 20; emphasis in the original).

This means that, for a given country, the effect of a policy depends not only on intervening on the right cause, but also on having the right set of conditions in place, and that multiple interventions together are recommendable in order to bring about a desired effect on *U*. Still, if this is the correct reading, then the OECD oscillates between a *sufficiency* interpretation (implicit in the empirical research methods employed) and an *INUS* interpretation of some of the proposed causal generalisations.

5. Conclusions

The practical value of causal knowledge goes far beyond the intuition 'that manipulation of causes is a good way to bring about a desired change in the effect'. The standard view points in the right direction, but still tells us nothing about how causal knowledge is meant to be useful in actual cases. In this essay I have presented only one aspect of such a complex topic. Since causation is a plural notion, the practical relevance of causal knowledge can vary depending on the precise meaning of the causal relations postulated in scientific causal claims. Disambiguating the meaning of claims that are intended as the basis for policy—such as economic causal generalisations—turns out to be a priority in the process of policy making.

The OECD generalisations and policy recommendations on unemployment are a good example to illustrate these ideas. The causal generalisations in the OECD research can have different meanings depending on which causal concept they refer to. Nevertheless, such generalisations seem to be left with an ambiguous (or at least not entirely definite) meaning in the way they are presented in the OECD study, which in turn is explicitly meant to guide policy decisions by authorities in member-countries. More communication and collaboration between economists, philosophers, and policy makers can help make causal notions explicit and clear so that scientific causal knowledge can have a more useful and effective role in the solution of practical socio-economic problems.

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ECONOMICS AS A SEPARATE SCIENCE: A CRITICAL REVIEW

Eduardo R. Scarano

1. Introduction

The notion of economics as a separate science has been important in the development of classical economic theory and mainstream economics.

Intuitively, it means that a separate science is self-sufficient; it does not have to appeal to the concepts of other sciences to solve the problems of its own field. Economics does not need to appeal to political, sociological or psychological notions to formulate its principles nor to explain the phenomena of its domain.

The ideal of economics as a separate science had already been found with Aristotle, who, by means of the incommunicability of genera[1], assigned to each science a domain perfectly separated from the remaining ones due to their ontological properties, a different cognitive capacity to know each class of objects, and a proper method to investigate the domain of each discipline.

The incommunicability of genera was overcome in Rebirth and, especially, in the Scientific Revolution, but its resonances continued and still continue today with

[1] 'In the demonstration one is not able then, to pass from one gender to another: one is not able, for example, to prove a geometric proposition by Arithmetic. The arithmetic demonstration always has the gender of the specimen in which the demonstration is carried out and it is the same thing for the remaining sciences.' (Aristotle, I, 7, 75a35-75b5; our translation).

the demand of separability of a discipline, as a mark of its explanatory and predictive power, of its self-sufficiency, and sometimes, also with a hierarchical character.

No doubt, in social sciences, this has an additional meaning. If a theory, for example, of economics, is not separate, studying its own field seems to depend more clearly on political concepts or positions about the society.

A historical vision of the problem is not what interests us, but what does is the contemporary debate on the notion of economics and its methodological projection into other social disciplines. Undoubtedly, we will have to go back to John Stuart Mill who highlighted the problem by means of *homo economicus* and gave it a very peculiar bias with the famous abstract characterization of Political Economy.

In part 2 this article examines the separability of economics according to J. S. Mill. Then, in part 3, we analyze some contemporary positions: we will begin with M. Friedman, who exhibits a compatible position with the current methodology of the Senior-Mill-Cairnes tradition. Then, we will present the position of D. Hausman who agrees to having taken it from Mill. We examine to what extent their separability notion is sustained and the consequences of its definition. We consider, briefly, the observations of U. Mäki on Hausman's developments. In part 4, we briefly present several counterexamples to the separability idea in economics, both in the consumer's theory, basically through experimental results, and in the theory of the firm, by means of alternative concepts. Finally, in part 5, we will evaluate the different positions about the separability and the possible directions of development of economic theory and social disciplines.

2. John Stuart Mill: economics as a separate science.

J. S. Mill has shown a remarkable precedent of the current discussions about the separability of economics.[2] To affirm the separability of economics or to qualify a

[2] He shows it especially in two works, *A System of Logic Ratiocinative and Inductive* (1843) and *Essays on some Unsettled Questions of Political Economy* (1844).

body of knowledge with some characteristics implies, above all, a fundamental distinction of the type of certain knowledge. This can be basically science or art. Although economics has to do with both fields, it mainly investigates its characteristics as a science.

Science and art are distinguished whether they refer to analyzing the facts or to getting actions, as they look for truths or rules, or rely on laws or means to an end,

> These two ideas differ from one another as the understanding differs from the will, or as the indicative mood in grammar differs from the imperative. The one deals in facts, the other in precepts. Science is a collection of *truths;* art, a body of *rules,* or directions for conduct. The language of science is: This is, or, This is not; This does, or does not, happen. The language of art is: Do this; Avoid that. Science takes cognizance of a *phenomenon,* and endeavors to discover its *law;* art proposes to itself an *end,* and looks out for *means* to effect it. (Mill, 1844, p.312; cursive in the original)

Art should be based in science; when it lacks support it is simple experience or opinion.

There are many characteristics that distinguish economics from other sciences; one of them consists of it being a *separate science*. Intuitively, this means that the terms in which the solutions to economic problems are formulated, id. est. the economic hypotheses, are restricted to exclusively economic terms.

This first approach is highly unsatisfactory for several reasons. In the first, and fundamental, it excludes the use of logical terms in formulation of the hypotheses, which simply prevents formulating them. It is obvious that we should allow the inclusion of logical terms, and the characterization of the notion of a *separate science* should make this explicit.

The previous delimitation is not enough as will be seen next. It seems unavoidable to make reference only to purely economic aspects. This way, when one speaks of volume of production, period of time, the surface of the earth, etc., physical qualities of goods are mentioned. The characterization of the goods themselves cannot usually be made without reference to different material properties that they possess, and which individualizes them. Consequently, the inclusion of other terms should also be allowed, not only logical ones.

Mill presents this problem clearly and gives a good explanation, ahead of its time. He considers two big classes of sciences, *physics* and the *moral* or *psychological* (Mill, 1844, p.316, point 3). Economics is not in charge of determining the laws of movement and physical structure of bodies; it simply supposes them and appeals to that knowledge when needed. The same goes for chemical characteristics, or agronomic or biological aspects of the earth, for example. Economics is in charge of the study of certain characteristics that some phenomena exhibit, although this study cannot be carried out without physical, chemical or agronomic knowledge. As we would say nowadays, the economic level is rooted in inferior levels, although it is not reducible to these.

> Political Economy, therefore, presupposes all the physical sciences; it takes for granted all such of the truth of those sciences as are concerned in the production of the objects demanded by the wants of mankind; or at least it takes for granted that the physical part of the process takes place somehow. It then inquires what are the phenomena of *mind* which are concerned with the production and distribution of those same objects; (...) and inquires what effects follow from these mental laws, acting in concurrence with those physical ones. (Mill, 1844, p.318)

Implicitly, Mill is sketching a theory about levels of reality, although he does not always maintain this concept and superimposes it on a more classic vision that distinguishes the types of knowledge by the classes of objects that are studied; that is, a classification of science by its object[3].

[3] In spite of statements like the following; "The authentic distinction between political economy and physical science should be looked for in something deeper than the nature of its object of study."(Mill, 1844, p.316, our translation). This, in fact, postulates the difference among both for the type of laws or

Economic science needs, then, to incorporate terms and laws of other physical sciences. It excludes, however, terms and laws of other moral or psychological sciences; the same argument would be valuable for other moral or psychological sciences. If they are sciences, in a strict sense, they only consider properties and bonds proper to the discipline and of *any other of the same level*. Although this development is coherent, however, it is not the one that Mill follows because it trips up with an inconvenience. He does not recognize the psychological level as an autonomous level between the physical and the moral. In it, moral sciences take root like he does in the physical.

Why does this inconvenience take place? Because Mill includes the *pure philosophy of the mind*, which contemporarily denominates psychology, within moral sciences, and it recognizes that the explanation of an economic agent's actions should appeal to this knowledge, like the physical. This situation also happens when, instead of considering man individually, the bonds with other individuals that cooperate with each other to achieve a common *objective*, are studied; Mill denominates it *political science or speculative politics or social economy*.

> This branch of science, whether we prefer to call it social economy, speculative politics, or the natural history of society, *presupposes* the whole science of the nature of the individual mind; since all the laws of which the latter science takes cognizance are brought into play in a state of society, and the truths of the social science are but statements of the manner in which those simple laws take effect in complicated circumstances. Pure mental philosophy, therefore, is an essential part, or preliminary, of political philosophy. The science of social economy embraces every part of man's nature, in so far as influencing the conduct or condition of man in society. (Mill, 1844, p.320; our cursive)

methods. However, in other parts of their work, he goes back to the traditional distinction of sciences by their objects.

Particular moral sciences presuppose psychology, or the Pure philosophy of the mind, and the laws common to man in society; social economics or political science. The latter branches of science share with the particular sciences the general relationship of the moral level with the regional level.

Now, we can define Political Economy: It consists of a branch of political science that studies the consequences of individuals' actions that pursue, exclusively, the desire to be wealthy, or otherwise said, it analyzes the most efficient use of the means to reach it. It is the very clear definition of the *homo economicus*. The analysis of the desire to be wealthy is enough[4] to characterize economic behaviors; we do not need other non-economic aspects, except the exceptions pointed out in the note, to explain this behavior. One can affirm that economics, in this sense, is a separate science; that is to say, detached or isolated from other disciplines of the same level like sociology, anthropology and so forth. In Mill's words,

> It is concerned with him solely as a being who desires to possess wealth, and who is capable of judging of the comparative efficacy of the means for obtaining that end. It predicts only such of the phenomena of the social state as take place in consequence of the pursuit of wealth. *It makes entire abstraction of every other human passion or motive*; except those which may be regarded as perpetually antagonizing principles to the desire of wealth, namely, aversion to labour, and desire of the present enjoyment of costly indulgences. (...) Political Economy considers mankind as occupied solely in acquiring and consuming wealth; and aims at showing what is the course of action into which mankind, living in a state of society, would be impelled, if that motive, except in the degree in which it is checked by the two perpetual counter-motives above adverted to, *were absolute ruler of all their actions*. (Mill, 1844, p.321-22; our cursive)

[4] He makes the exception that just two desires of another nature, the aversion to work and the immediate enjoyment of expensive goods, intervene "as an impediment" to the determination of behaviors of the economic agents.

For the thesis of economics as a separate science to be at least commendable, the definition of its field of validity is still lacking. The laws that compose the domain of economic science can be affected by two orders of interactions: the first, for reasons that are not wealth; the second, for economic antecedents not subjected to laws.

The first type refers to non-economic laws, for example, sociological, anthropological, etc.; if they interact with those of the economic subsystem they modify the agent's purely economic behavior. Mill himself, after the text presently mentioned, claims that the concrete act of human beings is guided by multiple reasons; economic and non- economic (cfr. Mill, 1844, p.322-23). It would be absurd to suppose anything else, Mill adds. To be able to study this complex situation, a cognitive strategy separated the multiple causes of behavior and, once we knew them, tried to integrate them so as to give a more complete explanation. That was the successful strategy of the progress of natural sciences.

The second type of interaction refers to the actions that produce effects, but which are not subjected to laws, because we do not know them or because they do not exist. A leaf that falls is subjected to the physical laws of movement, but the breeze that interferes with it modifies its trajectory and may turn the place where that leaf will fall unpredictable. There are no laws that allow advancing when, and in what measure, the breeze will affect the fall of a leaf. Similarly, the same occurs with other human aspects. Dealing with this class of facts is more difficult, both as difficult as is generalizing about them.

Mill denominates *disturbing causes* to both classes of interactions.

He claims the study of economic facts as a science, considering them as if they were only motivated by the motivation of wealth, that is to say, without considering the disturbing causes. This way,

> (...) there are also certain departments of human affairs, in which the acquisition of wealth is the main and acknowledged end. It is only of these that Political Economy takes notice. The manner in which it

> necessarily proceeds is that of treating the main and acknowledged end as if it were the sole end; which, of all hypotheses equally simple, is the nearest to the truth. (...)This approximation is then to be corrected by making proper allowance for the effects of any impulses of a different description, which can be shown to interfere with the result in any particular case. (Mill, 1844, p.323)

Due to this approach, he characterizes political economy as *abstract*, with the advantage of allowing the obtaining of laws, and general truths about economic facts. The science of political economy, which is generated by the abstraction of disturbing causes, consists of a body of general truths about the causes of economic phenomena that possess the same type of certainty as physical truths. Due to the nature of these truths, obtained by abstraction, they have their own method, the *a priori* method. In spite of the confusion that the term can cause, it does not mean typical logical truths, but rather, that they are not obtained by induction like in physical sciences. Their truth is determined by introspection, and from them, the other truths of political economy are deductively generated, and strictly, in the same way one proceeds in any moral science (cfr. Mill, 1844, pp.325-26). If reality only consisted of this type of cause, moral sciences would make exact and accurate predictions. However, another type of cause acts, being the disturbing cause, consisting of an economic cause, or from other fields that add to those of the respective sciences, and they then perturb the laws of abstract science, collaborating to obtain the result.

To understand and predict the economic phenomena of reality, we should take into account all the concurrent causes that impact on that phenomenon. Economic reality presents two sources or facets; the group of abstract truths, the *science*, and the *application* to the reality of economic science (Mill, 1844, p.325). Applied science takes care of other operating causes and possesses their own method, the inductive one starting from specific experiences. These inductions verify the scientific truths.

The application of science should not be confused with art. Although the latter are based on science and its applications, they differ in its objective, method and type of enunciations. The objective of science and its applications is cognitive, the *a priori* and inductive method, and their enunciated statements. Art has an eminently useful objective; its method is experience through opinion, and its enunciations are prescriptive, consisting of rules for action.

The thesis of economics as a separate science cannot always be sustained in the scientific field, but only in the restricted field of economic science in a strict sense, that of abstract economics or Political Economy.

3. Contemporary Positions

The contemporary positions that we selected referred to the separability in economics and we chose just a few ones that are linked to the previously exposed position.

The positive/ normative economics of Friedman

One of the basic distinctions of Mill, within economic science and other related aspects urgent in his time, was to differentiate between science and the debates among different theoretical positions of their applications in parliamentary disputes, economic policy discussions, etc. This had already been done before and continued after him. This point of view is usually denominated by the Senior-Mill-Cairnes tradition.

Contemporarily, Milton Friedman, for almost identical reasons, recaptures this tradition in an explicit way in a classic text of economic methodology, *Essays in Positive Economics*. Tacitly, it is included in his characterization of positive science that economics is a separate science.

Discussions about economics usually occurred as much among experts as among non-experts, since they are considered very important matters. The impulse to separate the different aspects linked to economics leads Friedman to distinguish between *positive economics and normative economics*. The second cannot be independent from the first because political economy and other ‘applications’ would lack a scientific basis, but neither is there, among both, a direct relationship; any other way it would make no sense to distinguish between both.

Normative economics is related to ethical or normative trials, in short, determining goals, ends or objectives. Positive economics is a theory or group of hypotheses able to carry out certain and significant predictions about economic phenomena that seek to explain. The predictions also constitute one of the best ways to justify or check the validity of the hypotheses. The methodological fundamental principle is ‘that a hypothesis can only be proven by means of the conformity of its deductions or predictions with the observable phenomena.’ (Friedman, Conclusion, p. 23). The election among supposed rivals is carried out on the same basis, as well as other methodological principles such as simplicity, clarity and precision, etc. (See Friedman, p. 24 Conclusion).

In the context of our discussion we can summarize Friedman’s position as follows: Economic science exists; it is necessary to distinguish it from normative economics. Economic science is separate; it does not need political, sociological or other non-economic terms, except those of inferior levels, to explain and predict the phenomena of its domain.

In Friedman, we find a clearer methodological position than in Mill. The former picks up a methodological tradition in use; opposed to this, the other author proposed a very detailed methodology that neither the economists nor other social scientists use in their investigations.

Does Hausman return to Mill?

Daniel Hausman has taken two methodological ideas from Mill, the inaccuracy and the separability of moral sciences, and has tried to illuminate the task and the methodological strategy that economics has previously had.[5] His main interest is

[5] He has carried it out mainly in his article *J.S.Mill's Philosophy of Economics* (1981) and in his book, *The Inexact and Separates Science of Economics* (1992).

not the reconstruction or the interpretation of Mill's thought. As he points out, 'I hope rather to provide an interesting and accurate philosophical reconstruction of Mill's remarks – to translate and interpret his views that may be of use in current discussions.' (Hausman, 1981, p.363). It is important to highlight this declaration since his interpretation of separate science is problematic from our point of view.

In Hausman, we find two sides of the notion of separate science. In the first, there is only a group of immediately decisive causal economic factors of the phenomena; which implies that there is a unified science[6]. In the second, economics is complete.

Science unifies what is to be considered a problem and how it is solved starting from a reasonably well-known group of 'causes', which is, in a sense, very similar to Kuhn's paradigms, or to the hard core of the lakatosian programs for which a theory is able to define its domain.

Rarely someone states that a theory unifies the field that it studies, when giving resources to solve problems by means of 'causes' and principles, and determining what belongs or does not belong to that theory. You can consider the inverse; which theory does not unify? All scientific theory, if it has a certain minimum grade of development, is able to unify and, its development in extension and depth is a permanent task.

We have to admit that a unified science is compatible with a non-separated science in Mill's terms. The unification does not imply whether it takes, or not, the terms and resources of other disciplines.

Let us return to one of his original statements, 'that a single set of causal factors are 'immediately determining' for 'one large class of social phenomena.' (Hausman, 1981, p.376) How do we interpret a 'unique' group? This could be understood in the sense that there are not two or more theories where each could explain part of the theory's domain. Economics would not be unified if it did not

[6] Cfr. Hausman (1981) p.376. In (1992, pp.90-1) he explains it through four theses but the basic idea is the same.

reduce the macro to the microeconomics; physics was not unified when the theory of heat was an independent theory of existent physics. This concept implies that if there are two theories in a domain, and each one explains a disjunct subset of that domain, they are not separate; although they are in a traditional sense (each one has a group of different terms and is not shared with the remaining disciplines).

Also, as we pointed out before, the causes could be gathered because they are the only ones that allow us to explain certain phenomena, although they do not belong to the same class from an ontological point of view (they could mix economic and political causes). Or they could be the only ones, due to their homogeneity from a certain philosophical point of view of the entities that the theory discusses.[7] In summary, the existence of only one theory in a domain is not incompatible with the separability usually understood.

Hausman details the proposal more in (1992, pp.90-95), however it is not clearer and we do not have to change anything previously said.

Let us now consider the *completeness* of economics. This seems less problematic than the previous because its meaning coincides with the intuitive one; 'The separate science is the conviction that economics is, within its own domain, complete. No explanatory or predictive purpose of economists would be served by fusing economics with any other science.' (Hausman, 1981, p.377)

His comments in order to clarify this meaning, and the reference he quotes from Cairnes, contribute, however, to darken the meaning.

In (1992, pp.93) he affirms, 'Since the laws of the major causes are joined together within economic theory and are thought to be reasonably well-known, economic theory is regarded as *complete*' That is to say, if one reasonably knows the group of economic causes, all the phenomena of that class can be explained according to them only and the specification of the *ceteris paribus* clauses. This excludes, like explanatory factors, those belonging to other sciences and also, the presumed

[7] At some given time the physical terrestrial, and celestial phenomena were considered two different kinds; unified, then philosophical and explanatorily by means of the common laws of Newton.

explanations that are not carried out in terms of the theory. For example, in this way, non-individualists' explanations, the experimental anomalies of the theory of expected utility, macroeconomics, the marginal propensity to consume, etc, are excluded. (1992, pp.94-5)

If a theory is unified because it determines its domain it is also complete in the sense that it potentially explains all that falls within that theory. The rest of the phenomena belong neither to the theory nor to its domain. There is no need for two different concepts; they are the same. The idea of explaining, using a group of terms that do not belong to other disciplines, is different. The (traditional) separability is an independent requirement of the previous one. A discipline can be unified and complete and not be separated in the traditional sense.

For this reason the conceptual displacement proposed for separability does not seem very fruitful.

Lastly, does our author return to Mill? Or does he use him freely to develop his own methodological ideas? If our demonstration is correct, he chooses the second alternative.

Mäki's critiques of Hausman

Uskali Mäki in his article 'Two Portraits of Economics' (1996) examines several of Hausman's statements and dedicates one section, (9. The separateness of economics, pp.26-29), to his notion of separate science. For Mäki there are two ideas that Hausman evaluates very differently. The first is the deductive method with which economists evaluate theory. The second is the structure and strategy of the development of economic theory that is closely related to economics as a separate science. For Hausman the first idea is commendable and compatible with the concept of the contemporary method of testing theories, but the excessive attachment to the second idea leads sometimes to dogmatism.

The deductive method consists of, following Mill, deducing predictions of the principles of economics, or of obtained generalizations, by specifying *ceteris* *paribus* conditions. The predictions are contrasted, and if they are not correct, they are compared with alternative explanations. Although the rebuttal of principles is not forbidden, the economists do not use it to revise the principles, but the remaining components.

The second, he characterizes in the way described in the preceding paragraph; according to this economic theory it gives a unifying and complete explanation of its domain. Mäki asks himself, why denominate 'separate' to this characteristic?

> ‘Hausman also talks about the familiar phenomenon of economics being separate from other disciplines such as psychology, but this seems to be a derivative feature relative to the above most fundamental characteristic. These more fundamental characteristics constitute economics as a science which subscribes, not the separateness directly, but the ideal of theoretical and explanatory unification, the pursuit of maximal scope employing a parsimonious set of fundamental claims.’ (Mäki, p.27)

Mäki notices the conceptual displacement which the term 'separate' has suffered, but he does not examine it, because he is only interested in criticizing the independence of Hausman’s two notions of inaccuracy and separability. This distinction allows him to defend the plausibility of the deductive method for theory evaluation, and to criticize the unreasonable dogmatism to which the development’s strategy of economics sometimes leads, especially its commitment to economics as a separate science. (cfr. Mäki, p.28).

4. Objections to the thesis of economics as a separate science

In this section we will take into account some objections or very well-known counterexamples, as much in the consumer's theory as in the theory of the firm, to the thesis of economics as a separate science that implies the introduction of non-economic characteristics, to understand and to explain anomalies in economic behavior as described by standard theory.

We will begin with those related to the consumer's theory.

Theory of the decision: Simon's limited rationality

The most frequent decisions in real economy are the decisions at risk or uncertain. In spite of their importance, there was not a satisfactory theory until the appearance of *Theory of Games and Economic Behavior* (1944) of von Neumann and Morgenstern.

Herbert Simon, in his book, *The Administrative Behavior* (1970), originally published in 1946, subjects it to early critiques. His analysis supports the basis of an alternative theory that will be known later on as the *theory of limited rationality*. It also includes, as a basic component, a psychological concept widely applicable in other contexts; the *satisfaction* as opposed to that of maximization which he usually refers to also as *levels of satisfaction*. (cfr. Simon, 1979, pp.502-3) This notion is fundamental in the treatment of psychological motivation (Simon, 1959, pp.262-63), in which the reasons to be acted are explained by impulses, that when satisfied, the action concludes. The conditions of satisfaction are not fixed, but rather, are changing based on experience.

The satisfaction of an objective, instead of its maximization, cannot be conceived of, just like the objective of firms, also of the decision of individuals. This way, 'Models of satisfying behavior are richer than models of maximizing behavior, because they treat not only of equilibrium but of the method of reaching it as well.' (Simon, 1959, p. 263).

Prospect Theory: the emotional factors in the aversion and propensity to risk

Kahneman and Tversky (1979) have widely studied, in diverse experimental ways, different anomalies in the theory of expected utility. The first we will discuss consists of choosing between A (4000$ .80) and B (3000$). In experimental situations, most people choose B (80%) regarding A (20%). However, when they are asked to choose between C (4000$, .20) or D (3000$, .25) they choose C (65%) and D (35%), even though both choices are not consistent.

Now then, when they are asked to choose between losing 4000$ with high probability or losing 3000$ with security, they invert the previous election. That is to say, A' (-4000 .80) or B' (-3000); they chose A' (92%) and B’ (8%).

How do we interpret these results? The violation of prospective utility is explained in the first case by the ‘certainty effect ‘, that is to say, because of the aversion to risk the sure option is chosen instead of a prospectively superior gain. When the signs are inverted representing losses, there is a ‘reflective’ effect; the aversion to risk in the domain of earnings is replaced by the search for risk in the domain of losses. This shows that the preferences seem to be determined by attitudes or emotions before earnings and losses, and the change from aversion to propensity toward risk implies a reference point, that is, for agents; the variations of wealth are more significant than the levels of it, which is very common in perceptive phenomena. (cfr. Kahneman, pp.191-195)

The frame effect of Tversky-Kahneman

Tversky and Kahneman present an interesting effect denominated *frame*. It consists of two descriptions of options with small differences but without extensional variation and yet, the agents decide in a different way. The following example illustrates this phenomenon. The flu A that will affect our country this winter will cause the death of 600 people. Two alternative methods with the following consequences are proposed to deal with it: If one chooses treatment A, 200 people will survive; if one chooses treatment B, there is the probability that a third of those 600 people will survive and two thirds will not. Most people choose alternative A.

A part of those that responded previously are then randomly selected and now they are asked to decide based on this presentation: If one chooses treatment A', 400 people will die. If one chooses treatment B' there is the probability of a third of survivors and a probability that two thirds of that 600 will die. With this formulation a great majority has now chosen B'. The affective reaction involved in the decision is evident, the certainty of saving lives is highly attractive and the aversion to accepting the certain death of people is disproportionate (see

Kahneman, 2003, p.197-199). Given the lack of canonical ways to formulate the options, the intervention of contextual factors in the making of decisions is unavoidable.

Next we will examine three cases related to the theory of the firm.

Coordination without the market: Coase's costs of transaction

At the beginning of the 1930's, Coase noticed the inadequacy of market mechanisms to understand the firm; to concentrate on what happens in the market, in the determination of price, without applying the internal arrangements of the firm, reduced the field of economics and darkened the understanding of this as a study object. The theory of the firm is in charge of what happens between the purchase of the production factors and the sale of goods produced by these factors and the rest is ignored. Most of the resources in contemporary economics are employed within the firm and their use depends on administrative decisions - not on market decisions, 'the efficiency of the economic system depends to a very considerable extent on how these organizations conduct their affairs.' (Coase, p.714) And, in order to understand commodity exchanges, one has to examine the institutional arrangement in which market phenomena occur, and although their importance goes beyond markets. The latter should be conceived as a part within the former.

Why do other coordination mechanisms exist, as those previously referred to, if, according to standard economics, the system of prices is enough? For Coase, using the system of prices has a cost through the negotiations that should be carried out, the contracts that should be made, the solution of the disputes, etc. Those costs are known as transaction costs, and, 'Their existence implies that methods of coordination alternative to the market (...) may nonetheless be preferable to relying on the pricing mechanism, the only method of coordination normally analyzed by economists.' (Coase, p.715).

Additionally, this explanation gives reasons for the existence of the firm and its role in the assignment of resources for administrative decisions. The firm should plan to continue existing because like this, it gets a lower cost than if the transactions were made by the market or by another firm. Efficient markets are only obtained if there is a firm of appropriate size that contains planning departments. The transaction costs make the firm emerge. (cfr. Coase, p.716)

Coase points out, as an obvious derivation from his position, that in an economy with transaction costs, the legal system becomes very important. The economists suppose that most of what is traded in the market are physical objects. On the contrary, in a system with a transaction cost they are right to carry out certain actions that are established by the legal system. If the legal system did not exist, of course, two individuals could negotiate their differences, but it would be extremely expensive and maybe, because of these barriers, it would prevent general production. (Coase, p.718).

The theory of the rights of property

Societies have always had to solve conflicts derived from alternative uses of scarce resources. They have done it by means of very varied mechanisms that go from forced employment to making the decision via elections. Economics textbooks propose that three questions should be answered to consider how a society will be organized: what, how and who will produce goods. However, for Alchian and Demsetz, two conspicuous agents of the rights of property school, it is more appropriate to consider the society, 'as relying on techniques, rules, or customs to resolve conflicts that arise in the use of scarce resources rather imagining that societies specify the particular uses to which resources will be put.' (Alchian and Demsetz, p.16) In a capitalist society, and in many of the precedents, the resolution of conflict by means of the rights of property is central. The most basic aspect in any type of economic exchange is the exchange of the rights of property of the goods.

A property right can be defined as the socially valid right to use an economic product, 'a property right is the exclusive authority to determine how a resource is used whether that resource is owned by the government or by individuals'.

(Alchian, p. 105) Another characteristic of the property right is that of obtaining a benefit from the goods and, lastly, that of alienating it total or partially.

According to how these three aspects are combined and specified, the different forms of property are defined. The right of *private property,* besides allowing its holder to determine the use of the product, also determine the exclusive rights to the services that it affords (for example, the rent or the usufruct) and finally, the right to delegate, to rent or to sell part, or all of the rights (at a price or as a gift) (cfr. Alchian, pp.105-6). C*ollective property* is characterized because the use of a product is determined by a group of agents by means of a procedure of collective decisions. Other forms of property rights like *common property*, *communal*, *mutual*, and other historical forms, are so defined.

In fact, the theory of property rights is not restricted to an economic theory; it is a genuine theory of society and institutions.[8]

The notion of efficiency X

Liebenstein (1966) points out that neoclassical theory only recognizes denominated 'assignment efficiency' as efficiency. A perfectly competitive market assigns, for the economy as a whole and in an optimal way, the factors between firms and sectors, and inside the individual firm when it is individually considered. This author asks himself if this only form of efficiency is enough to explain the differences of economic productivity within firms. One can observe without difficulty that similar firms, that is to say, firms that possess the same productive labor and the same technology, produce, however, very different results with respect to productivity and product quality. This author's solution to the outlined enigma resides in the organization type of each of the firms, that is to say, the explanation does not reside with a factor taken into account by the neoclassical theory, and that it is not a transaction object in the market. Two

[8] Coriat and Weinstein (2011, p.77) describe it as an alternative version of the neoclassical theory that conserves the four fundamental principles: the analysis of individual behaviors, the method of equilibrium, stable preferences, and perfect rationality; it overcomes the standard theory introducing imperfect information and transaction costs.

similar firms can buy *n* work units in the market; however, it is not guaranteed that both firms obtain the same productivity with those units. There exists a factor, the X factor, not considered by the neoclassical theory of the firm that explains efficiency or inefficiency: the organizational factor.

The neoclassical theory supposes that firms are in an optimal situation, that is to say, they take the optimum from their factors. On the contrary, for Liebenstein the typical situation is that firms, due to the X factor, are in a suboptimal state. The idea that firms automatically get an optimal state, an optimal employment of their resources is a simplifying and comfortable fiction, but at the cost of not recognizing the importance of the intervention of key organizational factors that, in certain circumstances, can help to obtain the optimal employment of resources.

To promote the optimum state or feasible nearest one, the firms use different mechanisms like remuneration strategies or non-monetary incentives as motivation, or career plans. These are necessary because labor contracts can never be complete, they can never foresee all that will be demanded of a wage earner.

One of the ways to look at the above-mentioned is, to expose it like a replica to the motto, 'the best resource allocator is the market,' and in light of the previous analysis, to confirm that, 'the best allocator of resources is the organization.'

5. Conclusion

The separability notion in scientific knowledge has, as we have pointed out, a long tradition that goes back to Greek reasoning. The answer to the meaning of this notion implies ontological, cognitive, general philosophical questions and, of course, methodological ones. Lastly, and perhaps most important, assuming the evaluation of the methodology that describes or proposes separability will always be present, will it give an account of scientific practice? Does it guide it in its development?

The pre-eminence of ontology in ancient thought implied that, in order to justify scientific knowledge determining the kinds of things that exist, how each other was known and adjusting the methodology to them was critical. Therefore, the separability of sciences reflected the separations in reality, those 'natural classes' of things that are in the universe. The domain of each of the disciplines (and in consequence their specific methodology for their different natures) was, or should be, previously well adjusted for those natural classes. The establishment of this delimitation was the first step toward developing scientific knowledge. It is urgent that today we do not recognize them; our conceptions of the world and methodology are very different nowadays.

At present and, at least, in modern times, it is not a stigma that a science is not separate. One of the easily demolished concepts in Aristotelian conception was the recognition of the mathematization of disciplines.

What is the Mill's basic motivation for proposing economics as a separate science? There are two different motivations. The first is influence on some aspects of methodological traditional positions. Their tenacious deductivism is to characterize the theoretical structure of political economy and other sciences in a strict sense, and their statements on the classification of sciences by the objects to which they refer. As we point out, in spite of the fact that his thought oscillates from, and sometimes states, other criteria, there are two manifestations of Aristotelian conception of science. Although this conception is not predominant in his thought, however, it works to support the separability of economics and moral sciences in a strict sense.

The second motivation is to distinguish between economics and other concomitant knowledge that are based on the first, like that of legislators. They use economic knowledge, and many other classes of knowledge, to argue about reality or to achieve certain goals. This is the difference between art and science sustained by Mill and previously by Senior, and which was set as the Senior-Mill-Cairnes tradition. Friedman reprocessed it with the same motivation: many care for economics but for different uses and on different bases. The separability of economics is a radical way to distinguish economics from other uses that imply it and, also, provides a recovery of its scientific quality. It is not just autonomous, but also, the more compactly constituted of the social sciences.

The separability of economics, from a sociological point of view, played a role in the demand and consolidation of the professional identity; the base to build it on and, at the same time, to distinguish it from others that use it as do politicians or legislators.

Separability carries out a similar function in the empirist's rhetoric to consider economics. Economists usually consider it with a status similar to physics because it is highly formalized, and is able to face reality through contrasts and predictions.[9] They are not limited to demanding scientific status or cognitive importance; they can prove it. It is not ideological arguments that prevail but the tribunal of facts, exactly opposed to the other social sciences. This distance in science and consolidation is manifested, at least partially, by means of separability.

In section 4, we illustrated by means of some representative few examples, the current objections to the thesis of separability in economy, and how negative testing results are solved by introducing psychological factors. This is typical in the case of Tversky and Kahneman, Simon or Liebenstein, and more thoroughly, in the case of Coase or Alchian who go from legal to organizational aspects, which are non-reducible, to purely economic (in terms of standard theory) dimensions.

A question, even more important than that of the separability, is still open and it relates to the conceptual change that is implied by the incorporation of terms of other disciplines. The changes that Tversky and Kahneman make to the standard theory of decision are smaller in comparison with those of Simon, and still are much smaller compared to those of Coase or Alchian. At this point, it seems

[9] In the same sense, see the description of Rosenberg (p.17) from which we select this statement, "It seems evident that if forced to, most neoclassical economists would endorse an empiricist account of knowledge, which makes the proximate goal of science the successful testing of its claims by experience and, more specifically, by prediction."

natural to speak of a remake of standard theory; of a ‘paradigm’ change. In this case, it would be necessary to tinge it a lot if we placed ourselves at the other end.

The illustrations that we consider, constitute excellent examples to study the moments of changes in theories, that is, alternative explorations that take place when theory systematically fails as regards to important predictions (the case of Prospect Theory and a very important part of Behavioral Economics), or when it cannot solve problems that are the responsibility of the theory (for example, the inadequacy of the Theory of the Firm in the standard conception).

These problems are independent of separability or not of a discipline, although this property can be involved in both cases.

Separability has to do basically with forms of seeing the world, how they are considered; in short, natural types and partial pictures of the world that we carry out with the concepts. Two kinds of phenomena that are separate from a conception of the world (terrestrial phenomena and celestial) can then be part of one kind in another conception of the world; or a group of phenomena that is excluded from economic or physical phenomena, then being included in another conception making them cease to belong to the previous ones (circular movement of planets, or the objective value of classic economists).

Separability in itself neither forbids nor allows any type of testing, predictability, or articulation of the theory with the facts. As we have insisted, and none of the reviewed authors have questioned, sciences can or cannot be separate; the science of a theory is not directly related to the presence or absence of this property.

When the separability of any discipline, and therefore in economics, is harmed, a natural question arises: where is it going? Economics would remake itself as an interscience, incorporating terms of other disciplines that are an essential part of the laws of economics; or the changes could be vast and embrace many of the social disciplines and a new (social) intradiscipline may appear. Economists will decide in whatever direction it goes. The important thing to highlight in relation to separability is that, undoubtedly, and independently of the result, with a new interscience, an intrascience, or almost a new definition of economics or a new definition of a natural kind, the vision of the world underlying the standard theory will not be the same.

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EXPECTATIONS-BASED MECHANISMS – AN INTERVENTIONIST ACCOUNT

Leonardo Ivarola
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I. Introduction

During the last decade the mechanistic movement played a crucial role both in the philosophy of science and in the social sciences, albeit a bit less in the latter. This movement supports the idea that a vast variety of phenomena in the world is the result of the operation of mechanisms (Glennan, 2008). Thinking in terms of mechanisms is attractive because it allows avoiding the use of the controversial notion of *laws*: on one hand, the main characteristics of laws – non-temporality, universality, etc. – usually do not maintain themselves in reality (and, it is argued, even less in social sciences). On the other hand, the notion of law has a very limited usefulness in characterizing the operation of both mechanisms (Woodward, 2002) and activities involved in mechanisms (Machamer, Craver and Darden, 2000, MDC in short.) This idea is closely related to the principle of "precision" stated by Hedström and Swedberg (1998b): not looking for universal laws in social sciences, but aiming at more limited range regularities.

Different accounts have defined what a mechanism is (MDC, 2000; Glennan, 2002b; Woodward, 2002; Hedström and Swedberg, 1998b; Bunge, 2004; Darden, 2006; Bechtel and Abrahamsen, 2005; etc.) Despite some differences in content, all of these authors share the view that mechanisms are entities composed of *parts*. There is however some differences about how to characterize both mechanisms and the nature of their components.

As regards mechanisms, there are two main views about their nature. On one hand, they are thought of as interrelated sets of entities operating in a range of time and space (Woodward, 2003; Glennan, 2002b) On the other hand, they are conceived of as processes (Bunge, 2004; MDC, 2000) We will refer to them, respectively, as "interactionist" and "processual" view of mechanisms.

Though many authors assume a monist position according to which mechanisms are composed of entities interacting in a stable way (e.g., Glennan, 2002b); other philosophers like MDC (2000) propose a non-reducible dualist account that depicts mechanisms as conformed by entities and activities. These activities usually require that entities have specific types of properties[1].

Notwithstanding, no process is considered a mechanism. Mechanisms are a *particular type of processes* characterized by a *stable* behavior. It is precisely this stability which allows separating processes that are mechanisms from those that are just sequences of events. Elaborating on this point Glennan (2002b) distinguishes between

a) Fragile processes (sequences that have particular (occasional) configurations)
b) Robust processes (sequences whose configurations are stable)

[1] With regard to this, MDC (2000, p.3) provides an example of the chemical neurotransmission mechanism:

> *a pre-synaptic neuron transmits a signal to a post-synaptic neuron by releasing neurotransmitter molecules that diffuse across the synaptic cleft, bind to receptors, and so depolarize the post-synaptic cell... The neurotransmitter and receptor, two entities, bind, an activity, by virtue of their structural properties and charge distributions.*

The successive stages that constitute sequences may or may not be connected to each other in a stable way. For instance, as Glennan (2002b) has pointed out, the succession of events that led to his first meeting with his wife was rather unique. These kinds of processes are what he calls 'fragile.' Fragile sequences are not regular; even small changes in the precedent conditions could result in unanticipated events. The process that starts with the hitting of a ball and ends with a broken window after impacting many intermediate obstacles is not a stable set of elements. It does not exhibit the kind of behavior that we designate as regular. Only robust sequences have a fixed (stable) structure and may therefore be considered mechanisms.

The paper incorporates a processual and dualist account of mechanisms in order to examine a particularly relevant case of economic mechanism: the so-called *Keynes Effect* (in short KE). A specification of its structure, the way in which its elements relate to each other and an account of how the mechanism generates its results will also be provided. In this respect three points deserve to be pointed out.

To begin with, it is not our purpose to offer a *general* characterization of what an economic mechanism is. On the contrary, our approach provides a characterization of a particular type of economic mechanism – a mechanism based on peoples' expectations, although we believe that this account could be particularly relevant for other disciplines belonging to the social sciences.

Secondly, these mechanisms characteristically show a connection between the information that individuals receive from the relevant economic context, the expectations they form and the activities they perform (which may modify the pre-existent context). The whole process may be represented this way:

Context1 → Information → Expectations → Activities → Context2

These mechanisms are stable processes whose regularities depend on agents' expectations and will be called *Expectations-Based Mechanisms* (EBM).

Finally, the information that influence agents' expectations may be altered through selective *interventions* carried out by the authorities, who may perform the changes basically in two ways: either by manipulating some "objective" component from the context (for instance, creating regulations or institutions, or modifying the actual state of an economic variable), or by indirect means able to influence the interpretations that individuals have of the changes suffered in context. These indirect means could also be rhetorical in nature (for instance, gestures and proclamations of the authorities directed to gain people's confidence). Intervening in the available information the authorities may contribute to generate those expectations that are judged as convenient and in this way, manipulate agents' activities and decisions, helping to produce some targeted economic phenomena. The ability of intervening in order to influence a particular arrangement of expectations is a central feature of the sort of mechanisms analysed in our paper, which is absent or insufficiently considered in the standard accounts of mechanisms.

II. Economic mechanisms: the case of the *KE*

Let us now consider a particular economic mechanism: the KE. The idea involved in a mechanism is that once triggered (i.e. the initial stage is activated), and assuming no interferences in its development, the process continues in a firm and stable way; in order to reach the *final stage* only one intervention is required. Apparently, KE satisfies this condition:

$$+\Delta M \rightarrow\rightarrow -\Delta i \rightarrow\rightarrow +\Delta I \rightarrow\rightarrow +\Delta N \,/\, +\Delta Y$$

where the expressions $+\Delta X$ $(-\Delta X)$ means, respectively, a positive (negative) change in a variable X. Returning to the KE, we can assert that when the money supply (M) is increased by monetary authorities, a decrease in the interest rate (i) will take place (stage I). This change will stimulate investment (I) (stage II) and consequently employment (N) and production (Y) (stage III). We call K the "typical connection", because it is the connection which normally prevails.

As will be shown, the connection between the variables is made by the crucial participation of a human agency. In order to get a more detailed explanation of this point, we separate the analysis into four stages. The first stage (from M to i) is started by an expansive monetary policy – specifically, an increase in money supply. Acknowledging that an increase in the quantity of money has taken place, people tend to demand more bonds[2], which increase their price and reducing the interest rate. To understand the second stage (from i to I) we assume that when a firm is to invest, it may use proper funds or even solicit a loan. In the latter case, the cost of the loan depends on the interest rate. More importantly, firms invest according to their expectations of selling their commodities in the future. Thus, it is often said that firms invest according to their estimation of the marginal efficiency of capital: "the rate of discount which would make the present value of the series of annuities given by the returns expected from the capital asset during its life just equal its supply price" (Keynes, 1936, p.121). The signal that the interest rate is lower has a positive impact on firms' investment projects, which become cheaper, promising greater benefits. In the final stage (from I to N-Q) it is necessary to introduce Keynes' distinction between primary employment in the investment industries (N2) and total employment (N). Thus, let us suppose that there is an increase in investment that brings about an increase in employment in the investment industries (N2). Through the Kahn' multiplier, the increase in N2 will mean a higher increase in N [3].

[2] The standard model assumes that peoples' wealth is composed by bonds and money.

[3] Kahn's multiplier (also called employment multiplier) shows how much the total employment (N) increases when N2 increases. What is more, the change in N is always superior – in absolute value – to N2, because of the idea of the multiplier. In addition to this, there exists a direct association between employment multiplier and investment multiplier. In this juncture, if there is no reason to expect any material relevant difference in the shapes of the aggregate supply functions for industry as a whole and for the investment industries respectively, Keynes deduces that both multipliers are equal.

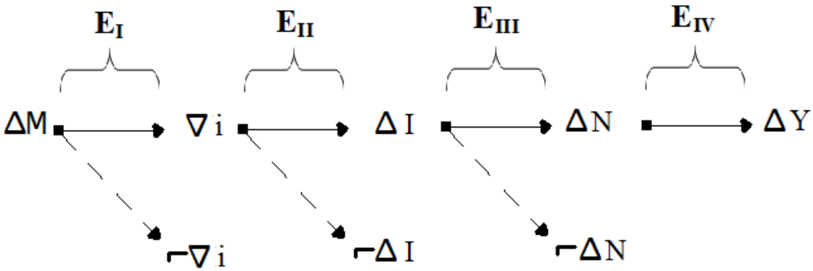
Deviations from the typical road

The KE mechanism described above is not isolated, but is part of a broader mechanism provided by the General Theory, which consists of a set of interrelated sub-mechanisms. Therefore, KE prevails as long as a ceteris paribus clause – including all the remaining relevant factors – is accomplished. Hence, the normal prevalence of KE means that changes in those factors are not significant enough to impede that the sequence of events - described by KE – is accomplished. However, these changes may be sometimes significant. As a consequence, agents modify their course of action, which alters the normal behavior of KE. In Keynes' words:

> We have now introduced money into our causal nexus for the first time, and we are able to catch a first glimpse of the way in which changes in the quantity of money work their way into the economic system. If, however, we are tempted to assert that money is the drink which stimulates the system to activity, we must remind ourselves that there may be several slips between the cup and the lip. For whilst an increase in the quantity of money may be expected, cet. par., to reduce the rate of interest, this will not happen if the liquidity -preferences of the public are increasing more than the quantity of money; and whilst a decline in the rate of interest may be expected, cet. par., to increase the volume of investment, this will not happen if the schedule of the marginal efficiency of capital is falling more rapidly than the rate of interest; and whilst an increase in the volume of investment may be expected, cet. par., to increase employment, this may not happen if the propensity to consume is falling off. Finally, if employment increases, prices will rise in a degree partly governed by the shapes of the physical supply functions, and partly by the liability of the wage-unit to rise in terms of money. And when output has increased and prices have risen, the effect of this on liquidity-

preference will be to increase the quantity of money necessary to maintain a given rate of interest. (Keynes, 1936, p. 155).

This situation may be represented through the following schema:



where the horizontal arrows denote the KE process, and where the diagonal arrows (dotted lines) denote possible exceptions which impede KE to continue its process until the final state. In what follows, we explain the deviations of the KE-mechanism through its respective stages, specifying the conditions in which it is possible to take alternative sides from the standard process. It is argued that these deviations have their origin in the information obtained from the context, which significantly influences agents' expectations.

First deviation: no change in interest rate

As Keynes stated above, let us suppose that despite the fact that an expansionary political economy is applied, the liquidity-preference of the public is increasing more than the quantity of money. If so, then the monetary policy will have no consequences in the interest rate, as people are not going to use that surplus of money to buy goods nor bonds. An interesting example of this is the "liquidity trap"; let us assume that the interest rate is quite low. In this case, agents are waiting for an increase in the interest rate. This is equivalent to saying that they are expecting a decrease in the price of bonds. Therefore, they will not end up buying bonds. Instead, they will prefer to keep their surplus of money (precautionary motive). Hence, an increase in money supply will not bring about significant consequences in the interest rate. It seems that people's reactions are sensitive to two relevant kinds of signals: those coming from an increase in money supply and those coming from the context (different values of interest rates bring about different people's reactions).

Second deviation: no change in investment

At this stage we must assume that the increase in money supply has successfully reduced the level of interest. Nevertheless, let us suppose that the marginal efficiency of capital is falling more rapidly than the rate of interest (Keynes, 1936). If so, firms will be reluctant to invest. We analyze this case through two examples. In the first one, let us suppose that there are no good expectations about future sells. Ceteris paribus, there is a decrease in the marginal efficiency of capital. If this decrease is superior to the decrease in *i*, then although credits are cheaper, this signal will not have a consequence on the amount of investment. This is due to bad expectations in future sells, which has an important effect on the profitability of projects. In the second example, let us suppose that agents differ about the future behavior of the interest rate. If most of them think that it will go down for a while, then they will not invest, because new entrepreneurs will be able to benefit from even lower interest rates, increasing their profitability.

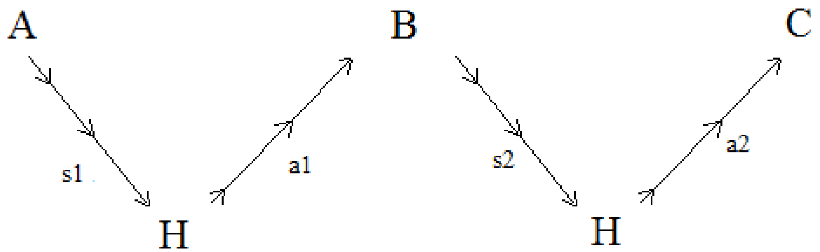
Third deviation: no change in total employment

The expectations formed in this step not only depend on the information that N2 has increased but also on the estimation that the consumption sector has about the marginal propension to consume. Specifically, total employment will increase as long as this sector does not expect a drop in the marginal propension to consume. In this sense, let us assume that the marginal propensity to consume decreases – for instance, as a result of propaganda in time of war in favor of restricting individual consumption. In such a case, firms producing goods for consumption will receive on one hand, a signal of higher employment in the investment industries (an increase in N2), but on the other hand, an imminent reduction in consumption, which could negatively affect their expectation of future sells. Consequently, they could find no incentive for incorporating additional workers.

III. The underlying structure of KE: The Expectations-Based Mechanism

To approach our subject let us first see how human action is generally involved in social mechanisms. As has been already said, our characterization of social mechanisms takes into account some contributions made by mechanistic literature, particularly its dualistic and processual approach. This view allows us to rethink the role of entities and activities in both social and economic realms. Particularly, one of the essential features that distinguish a social from a *natural* mechanism is that the activities involved in a social mechanism are intimately connected to human action. As Hedström and Swedberg (1998b, p.24) said, a mechanism "is not built upon mere associations between variables but always refers directly to causes and consequences of individual action oriented to the behavior of others."

We claim that social mechanisms involve, at the very least, two kind of *entities*: that which transmits information (for instance, the actual state of economic variables or the changes they show), and the *human* entity (economic agents), both receiving and interpreting the information sent by the transmitter entity. More importantly, agents perform *activities*, which are the agents' "material" reactions to the information they receive. Such reactions usually bring about changes in other economic variables. Thus, the basic ontology in social mechanisms has three main components: economic entities, agents, and activities. The process that links together all these pieces is outlined in the following picture:



which means that the actual state (or a change in state) of an entity A – conceived as a starting condition – provides information (s1) for agents (H), who receive it, interpret it, and consequently react, developing an activity (a1), which generates a change in the state of another entity, B. This change functions as new information (s2) for agents (not necessarily the same agents who generate the latter activity), who receive it, interpret it and consequently react, developing a new activity (a2), which modifies the state of the entity C. This change in C would represent the final stage of the process.

The KE mechanism fits fairly well into this schema. A simplified representation of the underlying structure of the first stage of K is this:

$+\Delta M \rightarrow\rightarrow H \rightarrow\rightarrow -\Delta i$

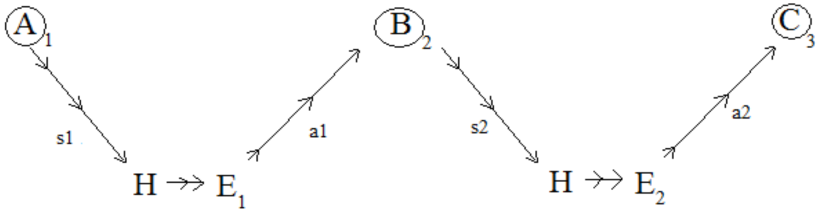
Here, we identify three main components of the mechanism: *changes in economic variables* (in this case an increase in money supply), *individuals* (who receive this information), and the *activities* they perform (which contribute by generating a change in another economic variable: the interest rate). Individuals are *active* in two different senses: first, they receive signals from changes in variables and interpret them; second, based on the information received, they react, adopting some decisions of economic relevance. The arrows drawn at both sides of H represent this complex nature of human action in a social mechanism. To simplify the exposition we will take information as given and will design, as an activity, the reactions (decisions) made under its influence.

However, the situation is a little bit more complex. The significance or meaning that individuals attach to changes in economic variables depends on the specific *contexts* in which they take place. The information that carries with it an increase in money supply is different under full employment than in conditions in which unemployment is high. The same change in a variable (say a reduction of 1% in the interest rate) sends a different message to individuals in different contexts. This is why fiscal policies are ineffective under full employment but successful when unemployment is high. Thus, the notion of context must be understood in the broadest sense; it means an economic background X where a change in some economic variable Y is generated. Such a background is relevant for the interpretation that agents assign to changes in Y. In other words, the information that individuals receive comes from the *joint action* of X and Y (or, better, from changes in Y once context X has been taken into account).

Other crucial components of economic mechanisms are the *expectations* that individuals form about future changes in some relevant economic variables. They are formed under the guide of the information received[4]. Expectations and activities are strongly related to each other: once individuals form their expectations they make decisions on this basis. Thus, we can say that *activities* developed by economic agents are triggered by expectations.

Taking all this into account, we can make the mechanism schemata developed in picture XX more explicit:

[4] More importantly, both kinds of signals appear to be quite important in the formation of expectations. For example, Lucas' thesis about the irrelevance of monetary policy, asserts that after receiving the signal of an increase in money supply, people may expect an increase in the general level of prices. Although the increase in money supply seems to be the only relevant signal, Lucas' model shows that the degree of effectiveness of such a policy depends on the historical background in which it takes place.



where A, B, and C represent economic variables, and the circles which enclose each variable represent the context in which each variable takes place (these contexts have been enumerated in order to clarify that changes in variables that may occur in different contexts). A certain change in (A) in context 1 sends a signal (s1) to the individual (H). Using this information he forms expectations (E1) which plays a crucial role in determining the activity (a1). In turn, (a1) contributes to an alteration of (B), and so on.

IV. Discussion

Given the discomfort that the academic audience feels regarding laws, the mechanism approach is an appealing notion that promises to be useful for understanding scientific practice, especially in social and economic contexts. Our notion of EBM incorporates some useful properties to approach some key economic situations, but also allows some interesting questions about them to be asked. In the remaining part of the paper some remarks about the peculiarity of economic mechanisms, their stability, in what extent authorities' interventions could be successful and what can be concluded from the basic structure of EBM on behalf of the project of building a more open and interdisciplinary worked economics, will be advanced.

Intervention – manipulation

Agents' expectations have a decisive role in EBM. On one hand, expectations are the key targets that should be intervened in to assure the working of the mechanism as was anticipated. In a natural mechanism the intervention usually consists of modifying certain aspects in the initial conditions; this works as a triggering factor of the mechanism, which continues its "processual road" until the so-called final condition is reached. EBM are less automatic and more demanding; they require that interventions take place not only upon their starting conditions (some economic variables), but also in context, providing an informational frame that prompts people to form those expectations which enable authorities to reach their goals. For example, an economic policy can be accompanied by some modifications in certain institutions and also a cluster of rhetorical devices, designed to generate a well calibrated context in the economic system, which is able to affect agents' expectations, and consequently the activities they develop, in the desired way.

The presented analysis also sheds light on one of the functions of the EBM: identify which expectations are relevant in each case. Once the mechanism is triggered the relevant points of intervention are the arrangement of expectations the analysis reveals. To the extent that some specific arrangement of expectation that leads from a change in an economic variable to a change in another variable is known, the pertinent interventions will be addressed to guarantee a background of information that promotes the arrangement of those expectations.

Two different kinds of knowledge sustain this kind of intervention. Firstly, *theoretical* knowledge is needed in order to know which economic variables have to be manipulated (although it is also important to know *how* to do it). Nonetheless, *practical* knowledge is also needed in order to operate on expectations, so that agents' activities are performed in the desired and expected way. In the absence of a better term we label this kind of knowledge as *management knowledge*. Let us take the example of KE: the fact that the final goal (an increase in employment) is achieved spontaneously is not expected, but such a goal is a result of a set of interventions in each stage of the mechanism, as well as through a set of marketing politics, a firm is expected to increase its sales. In this juncture it is propitious to recall the difference between knowing 'what' and 'how'. In politics, not only do you need to know *what* to do, but also *how* to do it. The necessary skills for an adequate intervention combine both types of knowledge. For instance, it is recognized that in order to increase investment both the interest rate has to be lowered and uncertainty in firms ought to be dissipated. Reducing the interest rate is a step that can be done without difficulty. However dissipating the uncertainty is something more difficult to reach, because it depends on a complex set of expectations. In particular, it presupposes a kind of knowledge that is not properly speaking, a scientific knowledge. On the contrary, it presupposes knowing how to manage peoples' expectations. Thus, more than an automatic mechanism KE can be seen as a process *controlled* by the intervening authorities. Analyzing the EBM make us understand that no economic knowledge is enough to control some economic variables. It is also necessary to know how to handle peoples' minds.

KE is involved so that in order to modify the interest rate the Authorities may manipulate the money supply. However, as Mill has pointed out, a causal connection between X and Y, which depends on the validity of a certain structure G that "enables" the connection, would be self-defeating if the activation of X also results in an alteration of G. In such a case an argument against the benefits of intervening could be sustained. Consider the following two levels:

1) $+\Delta M$ produce $-\Delta i$
2) Enable arrangement: G

In the case in which $+\Delta M$ not only operates on i, but also on G, eliminating the "enabling conditions", the causal connection between M and i collapses. We doubt, however, that Mill's argument could be successfully applied to EBM. Note first that in our case G consists basically in configurations of (favorable) expectations. Even if a deliberate increase in M had a negative impact on expectations, this is not the only resource that the Authorities may use to influence agents' expectations. It was argued that a whole battery of measures could be adopted to reorient expectations in a desired way, counteracting the initial negative impact, in case it existed. This is what we mean by an appropriate *management* of the results.

Stability

The purpose of intervening on economic variables is based on the idea that it leads to a desired goal in a regular way. However, this kind of intervention presupposes that the intervened-on mechanism involves a stable process. Particularly, the more stable the process is, the more chance there is to reach the goal. As a matter of fact, we asserted in the introduction that stability is a key feature in the mechanistic approach; no one would call mechanisms too fragile or unique processes.

Yet, taking into consideration that the KE can be stopped very easily, one could ask if this process belongs to the category of stable or fragile processes, and consequently, if it is suitable to label it a “mechanism”. We claim that it would be an error to consider it a fragile process. It is true that economic mechanisms are less stable than biological, chemical or physical mechanisms. But it is also true that, on one hand, social mechanisms involve human action, and on the other hand, KE is a kind of mechanism where their activities depend on expectations of the future, which is uncertain. Both the human capability of “producing” activities and the existence of an uncertain future contribute to a *not-unique*[5] behavior. However, this does not imply that it is an unstable or fragile process. It is very likely that the entities and activities involved in a fragile process do not occur several times. Let us take the example of how Glennan (2002b) met his wife. He claims “that event, like many events in which people meet, involved a confluence of events that were not to be expected and will not be repeated” (Glennan, 2002b, p. S350). On the contrary, we believe that despite the fact that different sequences may occur in the KE (a tree of possibilities), the courses of action are very limited and are usually identified by scientific research, which makes what kind of expectations produce particular reactions explicit. As long as only the

[5] This means that people do not act like machines or robots.

typical sequence described in KE is desired, it can be reached if pertinent expectations are generated.

Specificity and explanatory capacity of EBM

Mechanisms allow analyzation of the connection among variables, enlarging the available information. For instance, let us suppose the causal relation between a positive change both in money supply and employment

$+\Delta M \rightarrow\rightarrow -\Delta N$

can be decomposed in the following way,

$+\Delta M \rightarrow\rightarrow -\Delta i \rightarrow\rightarrow +\Delta I \rightarrow\rightarrow +\Delta N$

This analysis ‘opens’ the connection between M and N in terms of the same *kind* of macro-entities. Nonetheless, such analysis may be made in terms of new entities of a lower level. For example, the first stage of the KE can be analyzed in terms of people’s expectations and preferences,

$+\Delta M \rightarrow\rightarrow H \rightarrow\rightarrow (E) \rightarrow\rightarrow -\Delta i$

The last analysis leads us to an interesting point: each scientific community identifies a set of specific entities which constitute its own theoretic basement. This means irreducibility to other entities. Following MDC (2000, p. 13) we say that each community *bottoms out* in that set of elements:

> Nested hierarchical descriptions of mechanisms typically bottom out in lowest level mechanisms. These are the components that are accepted as relatively fundamental or taken to be unproblematic for the purposes of a given scientist, research group, or field. Bottoming out is relative: Different types of entities and activities are where a given field stops when constructing mechanisms. The

> explanation comes to an end, and description of lower-level mechanisms would be irrelevant to their interests. Also, scientific training is often concentrated at or around certain levels of mechanisms.

This topic poses interesting problems for economics, because it not only allows discussion of the set of economic entities that bottom out, but also to which point this concept is crucial in identifying the specificity of economic mechanisms. Particularly, it seems to be that economics bottoms out in peoples' preferences and expectations. Nevertheless, these preferences and expectations are not about entities of any kind. In the case of preferences, it is assumed that peoples pursue a specific goal, expressed in terms of optimization under some constraints. And in the case of expectations, it is usually considered that they refer to a selected pool of economic variables: GDP, money, demand, interest rate, etc. In the KE we have seen that signals people receive are related to economic variables, in the sense that they are created both on the basis of some previous theoretical economic knowledge and on the present state of particular economic variables. Thus, the conduct of some (macro) economic variables could be understood in terms of peoples' expectations about the behavior of the same kind of variables.

On the other hand, mechanisms are intimately associated with the idea of *explanation*. Let us see why. Suppose we have a lawful connection that asserts that when Y always X. Taken by itself, it tells us nothing about *why* and *how* the consequence is generated. The claim that we may explain X in terms of Y by just pointing out to a relation of this sort is somehow unfounded and it is usually labeled as a kind of *black-box* explanation. A *mechanistic-based explanation*, instead, *opens* the black box, showing "how the participating entities and their properties, activities, and relations produce the effect of interest" (Hedström and Ylikoski, 2010, p. 51). As a result, it is considered that in order to be a satisfactory explanation of a given phenomenon, a scientific theory must provide the description of a mechanism responsible for it.

In this juncture, the idea of bottom out is important because it imposes constraints on what a scientific discipline considers a genuine explanation. Following MDC (2000) a scientific discipline usually considers that the phenomena analyzed are explained, as long as the entities of the mechanisms used by such scientific discipline belong to a well-defined set (that in which the discipline bottoms out). Therefore, the explanatory capacity of an economic argument no longer depends on its logical structure, but on its ability to link those phenomena that has to be explained with the basic entities in which the discipline bottoms out.

Interdisciplinarity in Economics

Traditionally, economics has been characterized by its explicative (causal) factors (those where the discipline does bottom out), not by the phenomena that should be explained. Interestingly, in economics, the two domains largely juxtapose. Many factors – like consumption, demand, saving and investment - are both explanatory as well as explanation-demanding factors. More importantly, non-orthodox economic theories have included as explicative causal factor mechanisms that incorporate psychological or ethical notions, like mental accounts, loss aversion or fairness. These new incorporations allow us to rethink the problem of the specificity of the economic mechanisms and the value of the interdisciplinary approach in economics.

We actually repair to an important debate in relation to the different modes of investigation in economics. On one hand, some heterodox theorists emphasize the necessity of interdisciplinary knowledge, in order to improve the explanatory and predictive capacity of economic theories. In this regard, many works from Behavioral Economics – for instance, *Prospect Theory* (Kahneman and Tversky, 1979), *The Behavioral Life Cycle Hypothesis* (Shefrin and Thaler, 1988), *Myopic Loss Aversion and the Equity Premium Puzzle* (Benartzi and Thaler, 1995), and many others, support their models on the basis of psychological principles. On the other hand, economists like Gul and Pesendorfer (2009) consider that psychological and neuroscientific knowledge is irrelevant for economics, because predictions from these disciplines are not associated with economic agents' actions. Apparently, we can see that these different points of view bottom out in different hierarchies. This makes interdisciplinary research more difficult to carry out, and raises new challenges in elaborating an agreed-upon comprehension for the concept of explanation.

V. Conclusion

Our analysis of EBM focused on one case of economic mechanism which is relevant and rich enough to obtain interesting philosophical results, which might be extended to many other economic mechanisms of the same kind. The KE fits the main features that contemporary mechanistic literature assigns to mechanisms. It is made up of two kinds of parts: entities (economic variables and individuals), and activities (the decisions made by individuals). Those activities in which agents are engaged depend crucially on their expectations (which may be traced back to the relevant information they received). KE is a stable process in which macro-economic variables are linked with one another by agents' participation (based on preferences and expectations), which in turn may be affected by external intervention.

Even if we benefit from the mechanistic literature we do not endorse what Reiss (2007) called "new mechanist perspective" (NMP), which is a restricted methodological view that attributes just one (or mainly one) function to mechanism: explanation. Although we think that explanation through mechanisms is a good strategy and a desirable target, in our paper a case is made in favor of using mechanisms for intervention and transformation. In fact the purpose of "control" is privileged in our account. Particularly, our analysis is relevant to question what Reiss thinks is one of NMP's theses: that aims like control are either unattainable, or rather, "uninteresting because how to reach them is already well understood" (Reiss, 2007, p. 173). We provided reasons against Reiss' argument and advanced some suggestions on how economic control may be acquired provided an expectations-based mechanism is identified and management skills for manipulating expectations are available.

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A TELEOLOGICAL CAUSAL MECHANISM FOR ECONOMICS: SOCIO-ECONOMIC MACHINES

Ricardo F. Crespo

1. Introduction

In the last years, given the problems of the so-called 'received view', a new kind of explanation has appeared in the realm of the philosophy of science, the causal mechanism explanations. This kind of mechanism has been applied to different disciplines. In the area of social science we can mention the works of John Gerring (2008, 2010) and Colin Wight applying it to politics. The paper proposes to combine Nancy Cartwright's conception of capacities and nomological machines with Amartya Sen's capabilities in order to enact a causal mechanism for economics.

On the one hand, Cartwright, professor at the LSE, holds that what she calls "capacities" are real causes of the events. She also maintains that when causes combine in a stable way they produce patterns of behavior in nature we can explain. She proposes calling this arrangement of stable causes a 'nomological machine' (NM). On the other hand, Sen speaks about 'capabilities', as freedoms or possibilities of the human persons. Both Cartwright and Sen relate the terms capacities and capabilities to closely related Aristotelian concepts. Thus, this relation between capacities and capabilities suggests that we can combine these concepts to achieve certain results of interest to us in life.

The introduction of capacities and capabilities implies a revision of the epistemological and anthropological assumptions of current economics. The capabilities of Sen are the capacities of Cartwright in the human realm; human capabilities are the real causes of events in economic life and should be seen as the basis of their explanation. Institutions, moreover, are like 'socio-economic machines' that allow us, through our use of practical reasoning, to appraise, deliberate upon and guide our decisions about capabilities (Cartwright's capacities in the human world). Institutions thus embody practical reason and insert certain predictability in human affairs.

2. Socio-economic machines

Cartwright speaks about complexity, reflexivity and lack of control as causes of additional difficulties in explaining causes in the social realm (2007, p. 42). She also speaks about the derived nature of social capacities. They depend on social, institutional, legal and psychological arrangements that give rise to them, i.e., underlying structures that can be altered. Thus the social field entails a special kind of NM, a socio-economic machine (Cartwright 2001 and 2002). These socio-economic machines, given the nature of the economy, should be highly local: they are associations 'generated by particular social and economic structures and susceptible to change by change in these structures' (Cartwright 2002, p. 141). Referring to one example that she provides, she asserts:

> Each of the countries studied has a different socio-economic structure constituting a different socio-economic machine that will generate different causal relations true in that country and concomitantly different probability measures appropriate for the quantities appearing in these relations (Cartwright 2002, p. 143).

For Cartwright (2002, p. 143), we need arguments both at the phenomenological and theoretical level to achieve knowledge of those local particularities. Models are blueprints of those socio-economic structures (Cartwright 2002, p. 150). On the one hand, these blueprints must maintain a close relation to the specific situation they aim to explain. On the other hand, the greater the scope of the related institutions, the greater will be the universality or scope of the socio-economic machine.

This story, however, does not end here. I propose we deepen Cartwright's concept of a NM. What kind of reality is it? It is a real configuration of stable causes, 'a system of components with stable capacities' (1999, p. 49). However, there is a nuance in Cartwright's concept of NM when it refers to the social field. In these cases, rather than an established arrangement that is 'there outside' and that is only explained, a machine is a system built by us as a way of producing a result. Consider the following passages:

> In building the machine we compose causes to produce the targeted effect (1999, p. 65). ...you give me a component with a special feature and a desired outcome, and I will design you a machine where the first is followed by the second with total reliability (1999, p. 72). ... [W]e always need a machine (...) to get laws – (...). Sometimes God supplies the arrangements –as in the planetary systems– but very often we must supply them ourselves, in courtrooms and churches, institutions and factories (1999, p. 122).
>
> Just as the science of mechanics provides the builder of machines with information about machines that have never been constructed, so too the social sciences can supply the social engineer with information about economic orders that have never been realised. The idea is that we must learn about the basic capacities of the components; then we can arrange them to elicit the regularities we want to see. The causal laws we live under are a

> consequence –conscious or not– of the socio-economic machine that we have constructed (1999, p. 124).

That is, while in subjects such as physics we have one kind of machine, another kind of machine that could be labeled as ‘practical’ is more suitable for technical and practical fields. This is an arrangement meant to achieve a particular result. Thus, the machine suitable for the physical field may be called natural machine in the sense that it stems from a natural arrangement and naturally produces its effect, without intervention of outsiders, and is a ‘theoretical’ machine in the sense that we know it without intervening or trying to change it. ‘Practical’ machines are especially relevant for Cartwright. She stresses the importance of the *construction* of regularities (see, e.g., 1989, p. 182). As she states in the Introduction to *the Dappled World*, ‘I am interested in intervening’. So the question is: ‘how can the world be changed by science to make it the way it should be?’ (1999, p. 5). In the Introduction to *Hunting Causes and Using Them* (2007, p. 1) she adds that the three questions, what are our causal claims, how do they know them, and what use can we make of them, play a central role.

In this second kind of machines, i.e., practical, with its correspondent design, there are roles for theoretical, practical and technical reason. By using theoretical reason we ‘learn about the basic capacities of the components’ (1999, p. 124) of the practical machine, and about the relationships among them:

> We must develop on the one hand *concepts* [...] and on the other, *rules for combination*; and what we assume about each constraints the other, for in the end the two must work together in a regular way. When the concepts are instantiated in the arrangements covered by the rules, the rules must tell us what happens, where *regularity* is built into the demand for a rule: *whenever* the arrangement is thus-and-so, what happens is what the rule says should happen.

> Developing concepts for which we can also get rules that will work properly in tandem with them is extremely difficult, though we have succeeded in a number of subject areas (1999, p. 56).

These concepts and rules are known by theoretical reason. We also make use of technical and practical reason to design rules. Both uses of reason are implied in the quoted statement: 'how can the world be changed by science to make it the way it should be?' (1999, p. 5). We have to define how the world should be – practical reason's role– and how this can be achieved –the task of technical reason in combination with practical reason in the way we organize productive actions.

How do we design these practical machines? Their design starts with their blueprints. For Cartwright, theory is not enough:

> The theory gives purely abstract relations between abstract concepts. For the most part, it tells us the capacities or natures of systems that fall under these concepts. (...) No specific behavior is fixed until those systems are located in very specific kinds of situations. When we want to represent what happens in these situations we will need to go beyond theory and build a model, a *representative* model. And (...) if what happens in the situation modeled is regular and repeatable, these representative models will look very much like blueprints for nomological machines (1999, p. 180).

This kind of model, Cartwright holds, 'provide precisely the kind of information I identify in my characterization of a NM' (1999, p. 53):

> All these models provide us with a set of components and their arrangements. The theory tells us how the capacities are exercised together (1999, p. 53). In a nomological machine we need a number of components with fixed capacities arranged appropriately to give

> rise to regular behaviour. The interpretative models of the theory give the components and their arrangement (1999, p. 187).

In the formulation of models, theoretical reason also has a key role. We must take into account all the relevant factors and their relationships. As Cartwright argues:

> The situation must resemble the model in that the factors that appear in the model must represent features in the real situation (...) But it must also be true that nothing too relevant occurs in the situation that cannot be put into the model (1999, p. 187).

Models can have explicative (theoretical) or productive (practical) roles, depending on their subject. Practical and technical reasons intervene in the design of the latter category of models. For Cartwright, in economics, we often use these latter models:

> Models in economics do not usually begin from a set of fundamental regularities from which some further regularity to be explained can be deduced as a special case. Rather they are more appropriately represented as a design for a socio-economic machine which, if implemented, should give rise to the behavior to be explained (2001, p. 278).

One task of economics is the explanation of economic events. Another is the prescription of individual or economic behaviors in order to reach a goal, a normative task. This normative character may be practical (related to ends) or technical (related to means). Hence, we might postulate different types of socio-economic machines and models: theoretical and practical machines and models. Practical models have two tasks: determining and prescribing ends and means. Theoretical reason provides the concepts and knowledge of causal links for both kinds of machines. Practical and technical reason enters into the second kind of machine and model.

Human and social ends are not simply data but a task to be performed. Thus, they are normative. We can assume that man is rational, but he is also often irrational. As an empirical postulate, rationality often fails. This is why socio-economic theoretical models will frequently fail. Instead, we can always use rationality as a normative postulate.

Practically-designed machines are also local but they share some common principles. They are of two types: 1. a few general anthropological constants of human beings that are capacities, and 2. some capabilities that can be assumed as ends in practically-designed socio-economic machines. These capabilities are in themselves capacities and, in addition, they are capacities of the human realm. We need to look for specific derived principles for each situation.

In sum, socio-economic machines assume general principles but need to be local, adapted to the conditions and institutional arrangements of each situation. As mentioned, the broader the institutions, the more universal in their applicability, because, in fact, institutions are practically-designed devices that insert predictability into the realm of hazard and freedom. We need theoretical reason to know their specific natures and conditions that affect their working. A specific economic policy, for example, is a design of a socio-economic machine. It defines goals and means to attain them. Both the goals and the means may coincide or not with social and individual goals. Then, disturbing causes may interfere. The alignment of policy and people goals is the difficult task of practical reason; once achieved, the road of technical reason is more straightforward. This alignment of goals and design of the way to attain them is the work of a practical model.

Practically-designed socio-economic machines are the work of practical reason concerning ends and of technical reason concerning means, also using theoretical concepts. The contingency of the practical field is overcome by designing it. Institutions may manage and provide legitimacy to this work of theoretical, practical and technical reason. Institutions actually are socio-economic machines.

3. Capabilities are human capacities

Let us briefly consider the connection between these two theoretical concepts, Sen's concept of capabilities and Cartwright's concept of capacities. Essentially, Sen's capabilities are what Cartwright regards as capacities in the human world. This might be argued in two ways. First, it is suggestive that both authors, Cartwright and Sen, employ very similar concepts (capacity/capability) and that both authors link these concepts to closely related Aristotelian concepts, i.e., nature (*physis*) for capacities and potentiality (*dynamis*) for capabilities. Thus Cartwright asserts with respect to *physis*:

> Still, I maintain, the use of Aristotelian-style natures is central to the modern explanatory program. We, like Aristotle, are looking for 'a cause (*aitia*) and principle (*arché*) of change and stasis in the thing in which it primarily subsists' [the definition of nature (*physis*) in Aristotle's *Physics* II, 1, 192b22], and we, too, assume that this principle will be 'in this thing of itself and not *per accidens*' (1992, p. 47; 1999, p. 81).

Nature, as Cartwright holds with Aristotle, is a stable –not *per accidens*– principle or cause. This is why she indiscriminately speaks about natures or capacities (which are for her stable causes), and in her book *Nature's Capacities and their Measurement* (1989) maintains that capacities or natures are powers. Sen asserts with respect to *dynamis*: 'the Greek word *dynamin*, used by Aristotle to discuss an aspect of the human good (sometimes translated as 'potentiality'), can be translated as 'capability of existing or acting' (...)' (1993, p. 30, footnote 2; see also 45 footnote 41).

The meaning of the Aristotelian concept of potentiality (*dynamis*) is capacity, faculty or power. For Aristotle, potentiality is a principle of change (*arché*; *Metaphysics* 1046a 4-6). Potentiality or capacity (*dynamis*) is the dimension of nature related to the source of its actuality. Aristotle also distinguishes between two types of capacities (*dynameis*): not rational and rational. Rational capacities imply the intervention of deliberate decisions of agents (*Metaphysics* 1048a 7-15).

Sen's capabilities are rational capacities in themselves: capacities of, e.g., being free from hunger and undernourishment, achieving self-respect and social participation. This is a first sense in which capabilities are capacities.

There is a second sense in which capabilities are capacities. When Cartwright speaks about explanation in terms of causes in science she refers to the four Aristotelian causes (1989, pp. 219-224). Final cause triggers the action of the other causes. This can be said of all effects but is especially clear in the human realm. People have reasons to act. Thus capabilities are also capacities, because they are the final causes or reasons to act in personal and social actions. I agree with Nuno Martins (2006, p. 672) when he interprets Sen's notion of capabilities as a specification of the ontological category of causal power. He asserts: 'Sen's approach is not just the 'capability approach to *welfare* economics', but the *capability (or causal powers) approach to economics* as a science, an approach where the emphasis is on potentiality, freedom and openness' (2006, p. 680). Similarly, John Davis (2002, p. 490) maintains that 'in Sen's framework, capabilities can be thought of as powers that individuals can develop.' According to Smith and Seward (2009, p. 216) 'capabilities *are* causal powers (a '*power to*') that provide the *potential* to realize particular functionings.' They also argue that they are like tendencies that do not act deterministically. These characteristics fit with the nature of the practical realm and with Cartwright's conception of causes.

To summarize, Cartwright's capacities are then internal powers of things acting as stable causes, and Sen's capabilities are Cartwright's capacities in the sense of being faculties or possibilities but also in being rational and free causes of the human realm.

4. Back to socio-economic machines

The CA has three essential characteristics: the heterogeneity of persons and their capabilities, the incompleteness of the ordering of those capabilities, and thus the need for practical reason or public discussion to deliberate about our capabilities and their hierarchy. This situation stems from human freedom and diversity, and can be managed by a reflective agents exercising practical reason.

We should add that institutions are a way of giving a material embodiment to the outcomes of practical reason thus stabilizing the relevant causal relationships. In this sense, the link established in the previous section between capabilities and capacities can be very useful. The idea that capabilities are capacities reinforces the idea of building socio-economic normative machines. These machines would overcome the problems raised by the social world: they insert stability and thus predictability into the world. In this way they secure the work of practical reason.

We manage practical affairs by building models which originate in normative policies. These policies would shape socio-economic normative machines. The objectives of these policies would be capabilities chosen with the aid of practical reason. Capabilities as final causes thus provoke adequate arrangements to achieve them. Thus, these socio-economic machines will be the embodiment of the effective work of practical reason.

Human freedom inserts, by definition, a factor of unpredictability. Although we can have complexity in the physical realm, human complexity includes this unpredictable factor. Additionally, the human realm is a realm of reflexivity and lack of control, as Cartwright (2007, p. 42) argues.

The only way to manage the human future, subject as it is to these characteristics, it is to transform the practical (free) aspects of human or social action in technical way, fixing ends and means, and calculating the best allocation of the latter into the former. This has been an ancient desire of human beings. The earliest testimony to this ambition is expressed in Plato's dialogue *Protagoras*. He looks for a procedure of choice that would save us from the contingency of 'luck'. Aristotle realized that customs and routines are means that help to consolidate a predictable tendency (see, e.g., *Nicomachean Ethics* VII, 10, 1152a 26-7). Social pressure, laws and organizations produce predictable behaviors. All these means are often gathered under the label of 'institutions' in a broad sense. Institutions are then socio-economic machines that produce the intended results.

The alignment of qualitatively different ends is facilitated by the reduction of their different qualities into a common quantity. Numbers are homogeneous and pragmatic. As Theodore Porter (1995, p. 86) asserts, 'numbers are the medium through which dissimilar desires, needs, and expectations are somehow made commensurable.' Expressing realities in numbers facilitates decisions. Porter (1995, p. 8) also states, 'quantification is a way of making decisions without seeming to decide.' How, then, could we reduce choice about qualitative features to a quantitative calculation? This is the question raised by Plato. He asked: what science will save us from the unpredictable contingency? and he answered: 'the science of measurement' (Protagoras, 356e). Human beings strive for security, and measurement helps to promote it. Martha Nussbaum accurately notes that:

> What we need to get a science of measurement going is, then, an end that is single (differing only quantitatively): specifiable in advance of the techne (external); and present in everything valuable in such a way that it may plausibly be held to be the source of its value (Nussbaum 2001, p. 179).[1]

Institutions apply standards, procedures and measurement devices. Once the crucial step of making practical definitions is advanced, institutions establish technical processes to achieve them. Given that often these technical aspects impact on practical aspects, the process of designing technical proceedings is not accomplished directly but requires further adjustments.

Among these technical tools, index numbers provide a straightforward homogeneous representation of multiple factors. This homogenization, however, has its limits. However, we have to reach a middle ground position: although the reduction of qualitative concepts to quantitative measures will always be imperfect, we need these measures. Numbers conceal complex realities, and relevant meanings are lost in the process of commensuration, but numbers are still very useful.

Note, then, that when making these reductions to numbers, we must recall, for example, that ends are plural and incommensurable, and entail values that can only temporarily be hidden. As Sen (1999, p. 80) contends, 'the implicit values

[1] See also Elizabeth Anderson (1993, 3.1).

have to be made more explicit.' Quantitative reasoning is not enough, and thus Sen also stresses the need for using practical reason to scrutinize the ends we aim for (2002, pp. 39 and 46). Alain Desrosières (2008, p. 10) expresses this well, remarking that to quantify implies attaining a consensus on how to measure ('convenir et mesurer'). He adds that 'to postulate and to build a space of equivalence allowing quantification and thus measurement is at the same time a political and a technical act' (2008, p. 13).

Ends –capabilities in Sen's words– are the causes of human and social actions. They can be known by theoretical reason, without making value-judgments. However, as I have explained, this is the realm of unpredictable disturbing causes. The consequence is that, previously, we need to normatively establish and consolidate those ends. The way to achieve this is to build a practical socio-economic machine.

Designing a model of the socio-economic normative machine must include the practical work of discovering or deciding on its ends or goals. Institutions, Sen recently wrote (2009, p. xii), 'can contribute directly to the lives that people are able to lead in accordance with what they have reason to value.' Nobody wants to act in order to attain a set of ends that has not been chosen by him/her. Nobody wants to be an automaton. Every person should participate in a reasoned definition of shared goals, or at least should be informed about them and be free to adhere to them or not. One of the objectives of every policy is freedom itself. That is, there is a field of consensus about objectives and another field of deliberate freedom. Once the work of practical reason is done, we need to define the kinds of institutions needed to accomplish the resulting capabilities/ends, and also try to reduce them to a quantitative measure. This quantitative measure will be a first approximation for the particular situation. A thorough analysis will need to then return to the qualitative capabilities that compose the common measure.

5. Conclusion

Social science and more specifically economics need to reincorporate theoretical and practical reason. An exclusively technical approach leads to a partial analysis that is far from being relevant and unable to explain real phenomena without distorting them. Nancy Cartwright's argues that capacities are real stable causes that configure NMs, and theoretical reason has a primary role in producing knowledge of these capacities and their relations. Sen is not satisfied with a merely quantitative evaluation of poverty, equality and development. He urges us to take into account the heterogeneity of human persons, their situations and goals. Given that capabilities are the ends of persons and societies and that they are the causes of their actions, they are known and determined by practical reason. In this way, this later use of reason also re-enters into social science. 'How do we combine capacities and capabilities and work to achieve certain results of interest to us in life?' My proposed answer is: 'We must understand how practical reason is institutionalized in the world in the sense of being embedded in practices and procedures that allow people to solve practical problems that require the exercise of practical reason.' We must build a socio-economic machine and the corresponding model to define and determine capabilities (theoretical and practical reason) and look for the best means to attain them (technical reason). The socio-economic machine will produce these wished-for goals. The construction of this machine calls for a model of it. The HDI of the UNDP is an example of this kind of models. In the HDI we need to define concepts, to discover or deliberate on capabilities (which are the ends that are determined as dimensions to be considered) and their rules of combination, in order to technically combine them. That is, the HDI uses theoretical, practical and technical reasons. Cartwright's conception of capacities and machines, and Sen's capabilities (that are Cartwright's capacities in the human and social realm) are combined in this model and in the machine that it tries to produce and represent.

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ON ECONOMICS AND THE IMPOSSIBILITY OF ITS REDUCTION TO PHYSICS

Ricardo J. Gómez

1. Introduction

We believe that it is initially crucial for this paper to clarify what we mean by reduction. No one as the logical neo-positivist tradition in philosophy of science was more precise and rigorous obsessively thereon.

Carnap especially devoted part of his work to that notion in order to show that scientific knowledge was ultimately reducible to knowledge provided by physics. With characteristic analytical subtlety, Carnap distinguished between terminological reduction, methodological reduction and reduction of laws. By terminological reduction, he understood that the terms of all science, in a phased manner, starting with the reduction of the terms of biology and psychology to the terms of physics, were introduced from observational predicates that, in turn, were the result of the intersection of the terms of the common language and the language of physics (body, heavy, light, red, bitter, etc). In turn, the theoretical terms of physics were entered from observational predicates, e.g. 'electric charge', was introduced through sentences talking about rods of glass in rubbing, attraction of pieces of paper, etc.

That way of speaking presupposed the questionable distinction between theoretical and observational terms where the latter referred to directly observable properties, red, hard, etc. whereas the former were just not observational ones. Moreover, in order to avoid the intrusion of empirically meaningless terms that, according to the neo-positivists were, for example, 'the absolute', 'essence', etc. into science, the theoretical terms were required to be introduced starting from observational ones. Thus it arose the famous thesis of terminological physicalism, according to which all the terms, including the ones from physics were introduced from certain observational terms. The latter constituted the 'reduction basis' from which could be successively introduced the terms of any science.

The reduction of economics to that basis was allegedly achieved by reducing the terms of the social science to the terms of psychology which in turn were reduced to the terms of biology which were then introduced by reducing them to physical terms already reduced to the reduction basis. It must be emphasized that the terms of the social sciences were introduced by reducing the terms referring to social groups to terms about individuals; ontological individualism: the totalities are mere aggregations of individuals interrelated through law-like correlations between them; this ontological individualism grounded methodological individualism stating that all the propositions about social groups were reducible to propositions about individuals. In other words, the properties of the collective were reduced to the study of the properties of their parties or individuals and their interactions.

However, this reductionist program was a failure, just from the beginning. Since the first attempt in 1936, Carnap clearly saw that the theoretical terms are not explicitly definable from observational ones. This would imply that every theoretical term is replaceable by observational ones with the consequent elimination of theoretical terms. To introduce them, he used chains of reductive sentences making that the theoretical terms have always an incomplete, partial and never definitively complete meaning because it is always possible to add a reduction sentence to the chain. The problem is that in that way the theoretical terms become dispositional terms expressing the disposition of something to behave in a certain way given certain conditions. But Carnap himself perceived that not all scientific terms are dispositional, for example, space-time, or economic structure. Later, in two works Carnap (1939; 1962), he tried again using other resources such as e.g. introducing the theoretical terms via correspondence rules, but difficulties always pervaded. Finally Carnap had to acknowledge that he had been unable to solve the problem of interpreting theoretical terms relating them to observational ones. The obvious conclusion is that it is not possible an strict reduction of psychology and biology to physics, and consequently of economics to physics because in the chain of reductions economics was supposed to be reduced to individual psychology and physics, something not achievable on the neo-positivist agenda.

Terminological reduction was a necessary prelude to the reduction of laws. The laws of T2 are reducible to T1 laws if they can be deduced from the principles of T1. Carnap acknowledged, however, that until then such deductibility had not been achieved, but believed that it was not impossible to do so once the terminological reduction were attained. None of this has happened, especially, as we shall see for reasons of ontology.

Carnap also spoke of methodological reduction: all the sciences have the same method of justifying the acceptance-rejection of hypothesis, i.e. the inductive method. There are other methodological reductionist versions which postulate a method for all the sciences. Nagel, for example, but not limited to the inductive method. It is this reducibility that more influence has had on economics because what has been tried, especially since the end of the 19th century, is to show a certain analogy between the method of procedure of physics and economics generally based on huge and controversial ontological presuppositions.

As a matter of fact, one should not speak of reducibility, but more vaguely but prudently of *analogy*.

2. Procedural analogy and its ontological pseudo-justification

There is a long list of distinguished economists of the past two centuries that are archetypal examples of the attempt of analogizing the method of economics to the method of physics. In all of them is taken for granted a certain kind of quasi-ontological reductionism.

Thus, for Walras, economics is a mathematical science, as well as mechanics and astronomy because ‘pure economic theory... appears in every physical respect Science Mathematics’. Pareto, in turn, argues that ‘pure economics is a sort of mechanics’ because ‘pure economic theory rests on a fact of experience... the combination of quantities of goods....in which the individual remains indifferent’ so that economic theory ‘acquires the rigor of rational mechanics’. He believed that there is a strong analogy between the equations of physics and economics. Consequently, the equilibrium of an economic system is very similar to that of a mechanical system.

Some critics, as Mirowski (1989), point out that there are some problems in the analogy between mechanics and economics. Thus for example, he says that the individual is identified with a particle that only manifests itself through her psychology, through his preferences. But there is no equivalent to the concept of ‘mass’, there is not a principle of conservation of energy, which in this case would amount to the fact that the sum of income and utility should be a constant , something that for him was an irreparable default.

Of course, even in economics, not everyone adhered to the reductionism or to trying to analogize the method of physics. Keynes rejected such an idea, again, for ontological reasons because the material to which economics applies is not homogeneous in time. Accordingly, the search for invariant statistical laws destroys the validity of the economic model and the loss for economics of being a ‘moral’ science.

Others propose that classical mechanics refers to systems of material points on which operate directional forces which work according to the laws of motion. This makes impossible the presence of qualitative changes. Furthermore, that implies that the behavior of particles allows to determine the status of the aggregates. None of this is exported to economics because in the social formations in which they operate the economic proposals do not have their properties as an additive result of the properties of individuals. Qualitative changes are an inseparable component of the reality of economic systems and human beings are damaged by that kind of reduction that makes them act mechanically, driven exclusively by economic motivations.

The first reason to reject all ontological reduction grounding any attempt to analogize the methods of economics and physics is that there is a crucial difference between the open nature of economic systems and the closed systems of physics.

The more systematic defense of the irreducibility of open to closed systems was performed by Bhaskar, and applied to economics by Tony Lawson.

According to Bhaskar (1978), the world is constituted not only by events and our experience of them, but basically by structures, mechanisms, powers and trends that underlie and govern events. The purpose of knowledge and scientific explanation is to deal with the structures, mechanisms, powers and trends that generate and govern the phenomena. Bhaskar distinguishes between natural and social structures. In the latter, its elements are the individual agents in such a way that the structures do not exist regardless of the knowledge that those agents have of them.

The natural world, according to the empiricist conception of it, is composed of atomistic facts and their constant conjunctions. This constant conjunction is what makes that they are closed systems, governed by a strong determinism, where the future is contained in the present and where given a description of the present and being known all natural laws, everything can be determined with certainty. It is a system of regularities without novelties: the same type of events has the same sort of causes and vice versa. The uncertainty is possible if the state of the system is not properly known, but it is always in principle avoidable through the full possession of that knowledge.

‘Closed’ here involves the presence of closure that requires: the isolation of the system with regard to external influences or the invariance of them, the absence or the constancy of the internal structure, in which the individuals are always conceived atomistically, and the additive character of the system, i.e. individuals can be described in terms of the behavior of their parts. Do closed systems exist in nature? Indeed, in nature, closed systems has to be established experimentally in the laboratory where the scientist artificially close the system isolating it by using ceteris paribus, making possible constant conjunctions, etc. Instead, the natural and social reality is composed of open systems, where the laws designate structures and generative mechanisms, regardless of any pattern of pre-determined or artificially created events. It must be clear then that as the closing must occur artificially, the closure could not be universal. Therefore, one cannot deny the existence of open systems; all the opposite, it is hard to build truly closed systems.

The central problem of the closures is that they lead to alter the phenomena under study in the social sciences Bhaskar (1978). Studying non-homogeneous environments in which not everything is a conjunction of events in which each cause has not always the same effect, where the relationships are not between atomistic facts and the system is not isolated from external influences, individuals have structure internally, and where, in particular, the behavior of everything cannot be described by the behavior of its parts.

The latter is fundamental. In social systems the individuals are not what they are regardless of the system to which they belong. Therefore, the whole is not a mere aggregate of individuals with its specific properties that are what they are regardless of their membership to the whole.

Besides, in the social systems, in contrast to closed systems, mechanisms tend to be unstable, among other reasons, by the fact that the activity of human processing which is linked to human behavior is subject to feedback or learning.

Tony Lawson (1995) establishes a fundamental relation between Bhaskar's work and his own. He accepts Bhaskar's view and emphasizes that to explain, in closed systems based on the conjunctions of events, is to infer a statement about an event from universal laws of constant conjunctions and initial conditions nomological deductive model that is extrapolated from the natural to the social sciences. Of course, Lawson, as Bhaskar, objects to such extrapolation for reasons of ontology: If the realities are basically different, the a-critical transfer of the method of study of one to the other is objectionable. It is said that the implementation of the deductive method implies to adhere to the metaphysical-atomist-determinist ontology. But this is exactly what could not be extrapolated to the social world. Then, the tranference of the hypothetic-deductive method to the social sciences, and therefore to economics, has no ontological foundation; on the contrary, it is a forgetful transgression of the crucial difference of the realities under study. The result is unacceptable, because the universal implementation of the deductivist method of the natural sciences to the social sciences distorts the very nature of social facts.

But then, if deductivism in the social sciences, and therefore in economics is not recommended, is the existence of economic laws proscribed? Lawson argues not for the impossibility of economic laws. In certain spatio-temporal regions, certain mechanisms can dominate others and play to generate partial regularities, semi-regs, that can be observed. In these cases, relations might change, not allowing the establishment of precise unique predictions, but repetitions of a degree warranting reliable explanations and anticipations of patterns of events do occur. Accordingly, as it happens with Hayek to comment later, prediction in the strict sense cannot be basic as a parameter for evaluating theories. Instead social theories are evaluated in terms of their explanatory power in terms of structures, mechanisms, etc., and their potential for deducing other consequences as well as accounting for the capacity of the theory for explaining the existence of the mechanisms, their mode of reproduction and the conditions that they generate.

Lawson criticizes the neoclassical economic program just for committing the sin of assuming the closed nature of the systems studied in economics and hence the deductivist-empiricist nature of its methodology. This is so because such economics assumes an individualist view whereby the elements of the model are legal atoms, each of which has a separate, numerically measurable, independent and invariable effect, and can be treated as a separate case, individuals are isolated from all exogenous influence by establishing clauses ceteris paribus that guarantee the constancy of the repetitions of events. Its method of theorizing is the deductive method, and consistent with that it postulates the axiom of rationality which reduces human rationality to just means-end rationality leaving out any possible concern about the rationality of the goals.

Of course, none of the assumptions undergoes empirical testing – another feature of all empiricist version who enthrones empirical testing, but keeps out of it anything that it is convenient to preserve, according to that view.

For Lawson, the most important shortcoming of the matemathical-deductivist method applied to economics are: .1. the impossibility of matching the economic model and the world of facts, because the empirical data comes from an open system, while the method adopted presuppose closed and atomistic systems, .2. it ignores the problem of the generative structures and the causal mechanisms that govern economic events by reducing them to mere appearances of sequences of data, .3. it isolates the object of study by conceiving it as a closed system, .4. it assumes the one-dimensional atomicity of agents operating purely in terms of rational decisions where rationality is reduced to instrumental rationality ,.5. it reduces man to *homo economicus* governed exclusively by purely economic standards. That makes Pareto to claim that ‘all other property is excluded from his study’ which is why ‘real men governed only by economic interests does not exist’.

Moreover, it does not exist in the world, or it has not been found yet, the reference for the concept of equilibrium. It is assumed that it usually comes the state in which all actions, plans, expectations, are mutually compatible, which makes the system of equations of the model be consistent. However, that does not reflect a quality of the phenomena under study, but it just stresses the determinability of the system.

We are therefore, in empiricists approaches in general, and in the neoclassical in particular, before a distorting analogy, without ever indicating rigorously that we are facing just an analogy between a distorting model and a more complex reality . But there is something much more unfortunate still. The analogy is always to a physics that never was.

3. The analogy to a physics that never was

The main problem is attempting to establish an analogy between economics and a poorly understood Newtonian physics.

More clearly: everything that happened when trying to analogize economic methodology to the method of physics was the result of a false analogy between economics and a distortion of Newtonian physic. It was believed, erroneously, that physics was an infallible predictive tool without taking into account that its predictive capacity depended on adopting certain restrictions that were not taken into account to shape economics. Then, that economics was never successful because actually it could not have been successful because it was built on a false analogy.

Economists proceeded without realizing that such Newtonian physics was not able to solve the problem of the three bodies. Newtonian physicists adopted a huge ceteris paribus clause whereby all happened as if the influence of a third body, to explain the movement of one body around another centre of the movement, were considered expendable. As indeed it was not, to consider the real influence of the third body were introduced corrections to avoid obvious deviations from the orbit anticipated by the calculations including the restrictive clause.

From the historical point of view, this generated a serious problem to Newton, because it was highlighted that his planetary system could not represent the real movement of all the planets of the solar system in a single algebraic equation. Leibniz considered this as an insurmountable limitation of the Newtonian view, because he demanded that Newton should show the intelligibility of the system and its creator, i.e. that the system should deploy the regularities observed, something that it cannot be done if it were not obtained a formal proof of the solvability of the three bodies problem for all the planets. We must remember that if one enters a third body (planet) besides the sun and any planet, the equations proposed by Newton were not more algebraically solvable.

In 1889, Henri Poincare in a brilliant display of management of the mathematics of the time 'On the problem of three bodies' and of the equations of Dynamics showed that there was no way to solve, even with all mathematical resources available, to solve the three-body problem. In addition, he showed that in a world where many objects moving freely under gravitational attraction may occur unpredictable critical collisions. Thus, the full prediction is impossible in the real world of physical movements.

None of this was considered by the economists of the time. That is why the economic theoretical proposal with a capacity for prediction analogous to a supposedly unrestricted prediction of physics was doomed to failure. This economics was never what was expected of it. It could not be so because the physics that the economists imitated was modeled on a physics that never was.

This was a regrettable mistake of, inter alia, neo-classical Economics, and remains today in some economic circles as an ideal that we now know cannot be achieved Everything discussed before is a report of another episode over a long and unfortunate history of attempts of methodological reduction of the humanities to the natural sciences. Economics was often a paradigmatic example of such failed reductionism.

4. On the probability of our predictions

We must not confuse the previous difficulty with the impossibility of certainty in our predictions. Poincare, among others, recognizes that all our predictions in science are merely probable, although in certain cases, this probability can be considered as virtually equivalent to the certainty [but] is only a probability.

No law , according to Poincare, will ever be more than approximate and probable. Scientists of all disciplines never ceased to recognize this; i.e., that this is true both for economics and for physics as well as for astronomy. Poincare adds that every scientist believes that any law could be replaced by another more likely. In all cases, every law is only provisional, but the replacement for new and most likely conjectures may be always continued indefinitely. Then, such a process will approach indefinitely to the accuracy as much as the one desired by the scientist. That is why Poincare asserts that condemn such calculation would condemn the whole of science. Of course, certainty is ruled out.

This would be achievable in principle, if nature were essentially simple. But 'this is what we have no right to do'. For example, 'the simplicity of Kepler's laws is only apparent.' This does not preclude that they apply very closely to all systems similar to the solar system; but it prevents them from being strictly accurate. The predictions of any discipline, regardless of the particular problem of the three bodies, will have this unavoidable limitation. But it is manageable, because it is always possible in principle, to assume a particular error as recommended by the circumstances. In addition, it is inevitable to do so, since scientists seek to obtain predictions. However, if an economic situation is analogous to the Newtonian one by ignoring the influence of a third item then it is committed a grave sin of omission which will lead anyone to grossly erroneous predictions not improved by the proposal of new legislation if it is still proceeding as if the third item were irrelevant.

Another inescapable limitation of all science is related to the construction of models. None of them can be a faithful reproduction of the modeled situation. Therefore, any hypothesis or law of such a model is always approximate. But again, such approach is always, in principle improved, with corresponding progress in the representative and predictive capacity of the hypothesis or model laws. Again, such a limitation is of a totally different order to the difficulty posed by the problem of the three bodies. This last refers to an analogy with a physics that never was, while the other one is an inescapable part of all science.

5. Leibniz-Newton and some of their philosophical disagreements

We must now clarify something important for the plausibility of our proposal. The very strong accusation of Leibniz to Newton on the impossibility of an algebraic solution of the problem of the three bodies, even though it is part of a long polemic between the two, has a notable difference from other aspects of the controversy.

Disagreements between both monumental thinkers were of two types: .1. on the primacy of the Calculus, and .2. about essentially philosophical issues. Disagreements of the first type are not relevant for the purposes of this work. Those of the second type cover a vast theme. But such philosophical disagreements accelerated the process of generating a more and more hot controversy even for the primacy of the Calculus. In *Theodicy* Leibniz expressed its rejection of the Newtonian concept of action at a distance. In 1702 Leibniz said that those who believe that bodies attract each other , actually did leave the appeal to natural causes, and appeal instead to miracles. Newton replied by emphasizing the experimental nature of his position: he was not trying to teach the causes of phenomena unless the experiments revealed them. But his critics believed that it was a lack of Newton not to offer any hypotheses about the cause of gravity.

In the new edition of *Principia* Newton added a new rule of reasoning according to which all the qualities of the bodies being discovered as belonging to all bodies within the scope of our experiments, must be estimated as universal qualities of all bodies. Therefore, if one sets by astronomical observation that all the bodies close to Earth gravitate towards her, among them the Moon, and planets do among them, while planets do towards the Sun, then that rule allows to claim that all bodies are fitted with a principle of mutual attraction. Newton appealed further to the numerical accuracy of the predictions made by applying the law of universal gravitation in order to legitimate their position.

Leibniz never denied such quantitative accuracy. His objection was strictly philosophical: the almost miraculous nature of the concept of action at a distance without further search of the mechanical causes of such action. It is well known that Newton attempted to find such causes and never found a satisfactory answer. It is also known how Newton excused himself from this flaw in the General Scholia to *Principia* written at the end of his active intellectual life: 'I have not been able to discover the cause of these properties of gravity from phenomena, and I not *fingo* hypothesis...'. But for Leibniz, such a stance was radically non-scientific because it constituted a betrayal of the very recent conquest that all genuine explanation of natural phenomena should be in mechanical terms and a setback to the merely verbal explanations of medieval scholastic philosophers.

Leibniz always accused Newton of professing a strange philosophy that was very difficult to defend. Leibniz believed, in opposition to Newton, that it would be impossible to empirically prove the existence of the vacuum. He objected even more emphatically the attribution of a sensorium to God, and he mocked Newton for making God create a world in such an imperfect way that it needed to be eventually repaired. On the other hand, Leibniz thought that ‘God has anticipated everything, has provided a remedy for everything in advance. There is in his works already pre-planned a harmony and beauty’. A radical difference existed between the two about the philosophical status of the Newtonian system. Such a system was, according to Newton, the rationality of its creator and ruler of all things. But, what perfection could involve such rationality if required go correcting any imperfections? According to Leibniz, the impossibility of an algebraic solution to the problem of the three bodies with the corresponding need to go addressing one by one each planet movement making any corrections to empirically take into account influences from other bodies constituted another final test of the unacceptable limitations of Newtonian motion.

Regardless of the acceptability of the critique of Leibniz, Poincaré reaffirmed the impossibility of an algebraic three-body problem solution although, of course, refrained from interfering in the historical dispute Leibniz-Newton. We have no doubt, however, that Leibniz would have felt very happy to hear the conclusions of Poincare’s view.

6. The Austrian School and the rejection of the ontological-methodological reducibility of economics.

Not all approaches to economics took for granted the parallelism with physics. A notable example of this was the Austrian school and, in particular, its most famous representative, F. Hayek, who in his studies of philosophy, politics, and science , emphasized the impossibility in economics of the kind of predictions of the exact sciences.

According to Hayek, economic phenomena have a higher degree of complexity than natural phenomena. Therefore, what we can predict aren't unique facts, but patterns of fact. Thus, the set of simultaneous equations which Leo Walras - one of the most important mathematicians of the neoclassical version of economics - used to establish the general relationships between prices, on the one hand, and the quantities of goods sold and purchased, on the other, cannot predict specific prices. It only allows us to anticipate a certain pattern of facts. Moreover, the prediction of a pattern as 'if we knew all the parameters in Walras' equations, we could know the prices' depends on certain assumptions as 'most of the people get involved in trade to obtain an income', 'people prefer a high-income over a low one' and 'people are not prevented from trading'. These assumptions determine the range of the variables, but do not determine the particular values of them.

This sort of inevitable complexity of social phenomena prevents to speak, according to Hayek, of such sciences as being possible to be reduced to physics. Although the method of all of them is analogous of conjectures and refutations, what varies due to the different degrees of complexity of the studied phenomena, is what can be achieved with them. While in some sciences the prediction of singular facts will be possible, in others it can only be achieved the anticipation of the recurrence of certain patterns of facts.

Hayek concludes that 'rather than prediction it is better to talk of *guidance*.' We cannot predict unique events, but we can orient ourselves. We will have little power to control future developments, but our knowledge of what kinds of phenomena can be expected and what types cannot will help us to make our action more effective. And he adds that we can talk about cultivation, in the sense that a farmer cultivates its plants, insofar and as soon as he only can control some of the decisive circumstances, but not all. Hayek emphasizes that, as a result, to pay a price in predictability must inevitably also pay a price in falsifiability. The assumptions used to anticipate what will happen in the future become less falsifiable. Therefore, we can propose neither the appeal to experience nor crucial experiments to decide between competitive theories. This does not happen because we are dealing with an immature science, but because of the nature of the phenomena that we are dealing with. The more we know the complexity of the studied phenomena, the more we're going to be convinced that we have to make concessions to the falsifiability of our hypotheses. Then, to handle the complexity of the phenomena we have to use simplifying formal models.

But the falsifiability of hypotheses is a demarcation requirement sacred to someone as Hayek who has said: ‘I derived my epistemological position and many ideas from the work of K. Popper’. Economics then, strictly speaking, was never falsifiable in the unambiguous way that it was always assumed by Popper and Hayek for the physical sciences.

Something similar happens with the other famous ideologue of neoliberalism. According to Friedman, what the economist can do when the empirical evidence falsifies a certain hypothesis, is to reduce the domain of applicability of that hypothesis and correspondingly of the theory to which it belongs. In this case, the hypothesis or theory can be kept because the falsifying consequences would be outside of such new domain of applicability. This strategy, regardless of being honest with regard to the real practice of neoliberal economists, leads to the extreme Hayek’s thesis for the decrease of the falsifiability of the hypotheses or theories in economics. It will ultimately lead to unfalsifiable hypotheses and theories.

To make such a reduction of a domain of applicability, there is no decision-method, i.e. there is no algorithm allowing us in a finite number of steps to conclusively decide what to do. Friedman recommends that, in such cases, we should rely on the opinion of experts, who, of course, will recommend something favoring the interests of those financing them. We now envision another trait of an economics that never was. It did not proceed, because it could not do so, for the acceptance or rejection of their hypotheses or theories, by using only a mere algorithm in terms of good logic and reliable empirical evidence.

But, then, there is something more important for an economics that never was. It was never, something that Toulmin also recognizes. It was never value-neutral, that is, it was never purely descriptive.

7. Objectivity and scientific rationality in an inevitably normative economics

Economics was never truly value neutral. As we have pointed out on several occasions, this is due especially to the fact that all economic discourse assumes a series of ontological, epistemological and ethical presuppositions making certain systems of values be constitutive of economics, for example, neoliberal economics presupposes the validity of the principle of economic rationality, that the market is the Supreme locus of such rationality, that freedom is the supreme value to respect , etc. All these permeate the economic decisions, especially in the context of justification, and makes the presence of values unavoidable.

On this occasion, thereon, we prefer to concentrate on some other author. For example, Toulmin, emphasizes that any decision in the context of the acceptance or rejection of a hypothesis and economic theories as well as in the assessment of future alternatives, always involves a situational component, i.e. interests and unavoidable conflicts. The best we can do often is therefore to handle the situation by moderating the conflicts without adding new difficulties.

This highlights that in economics it is impossible the total estrangement which requires the classical notion of objectivity, today already in crisis. What must be considered, however, is the need for awareness of the interests involved, as well as the values used in a decision in order to achieve a more open, honest and not utopian objectivity. That is why Toulmin asserts that in the social sciences as in any other place, the problem of achieving objectivity is to learn how to counteract our own biases and distortions. This requires to make explicit the interests and values that we bring to our research. As a result, Toulmin thinks that impartiality and objectivity are general rules that can acquire specific force, only when they are understood as embodied in particular classes of situations and cases.

Therefore, the unavoidable evaluative burden present in any decision related to economics, does not preclude objectivity, now understood in a practical, realistic sense. This of course also covers trials of *good* or *bad* economic results, goals or objectives to achieve and global judgments about groups or society as a whole. Rather than be able to predict a future as unavoidable, we can discuss the future that we can, in principle, realize. We try and do the best to create the conditions that will help us to move in a better direction rather than adopting worse alternatives.

In Toulmin's terminology, when we reason to work properly within the scope of the practical taking into account the situation in which people operate, the history and peculiarities of agents that intervene in it, we must move from talking about *rationality* (rigorous, formal, inevitable) to *reasonableness* taking into account the situational circumstances. This is precisely what makes necessary to appeal to the opinion of Friedman's expert. This reasonableness is requiring an indispensable place for *prudence*, which necessarily involves avoiding the pedantry of assuming that decisions about behavior of human agents are predictable in the same way and with the same rigor as we predict the paths of the planets.

More clearly: the requirement that the social sciences are objective does not entail that their appraisal is value neutral, and consequently that it does not imply that any ethical consideration is left aside. The reasonableness-rationality pervading economics is an inseparable and unavoidable ethical-practical dimension of it.

As the Nobel Prize in economics Amartya Sen pointed out, there is a reciprocal relationship between rationality and freedom. On the one hand, the concept of a reasoned choice plays a crucial role in the concept of freedom. On the other hand, rationality depends on freedom, because without some kind of freedom of choice, the idea of 'rational choice' would be empty; the concept of rationality must also accommodate the diversity of reasons that can determine a choice.

The concept of self-interest and the reduction of rational decision to the maximization of it dissociates the individual conduct from values and ethics, because it eliminates another reason for choosing diminishes freedom and distorts rationality. In addition it is also distorting in relation to the prediction of many actions in which, in fact, we pay attention to the demands of cooperation.

Scientific rationality, according to Sen, involves, then the requirement of the reasoned self-scrutiny of our goals and values. It is not a rationality limited by reduction to a mere calculative instrumental rationality as in the case of neoclassical and neoliberal economic rationality. It does not accept, without reasoned discussion, goals prefixed even by tradition. It allows the recognition of objectives that are not reduced to our own well-being. This kind of reason is used, therefore, not only to rationally pursue specific objectives and values, but also to investigate and criticize the objectives and values themselves. As a corollary, part of such rationality is to recommend the use of those values and objectives for making systematic choices that has been accepted only after a critical discussion .

In short: economics was never merely descriptive economics. I.e. it was never what teachers and partisans of neoclassical and neoliberal economics claimed to believe, a science that as such should be value neutral. By contrast, all economics as a science about human agents acting in freedom and using reasons to choose between objectives and decide between means of achieving them is pregnant of a practical dimension that makes it always fundamentally normative.

Therefore, there is one error in the attempted analogy of economics to physics. It presupposes, not only a physics that never was, but, in addition, a science that never was.

Physics was understood as providing the methodological model to imitate because it allegedly was the paradigm of objectivity and this happened because it was supposedly, as it should be all science, objective, in the sense of being value neutral. Why ethics was removed from economics? Mainly because, according to empiricism, ethics was not objective due to the presence of values in it or, more emphatically, because ethics is fundamentally about values.

It is here where we arrive to the core that links science with objectivity and the latter with value neutrality. To make a science objective it should be value-neutral and physics was the archetypal example of this assumption. Then, to be truly scientific, it must be objective and, in doing so, it must remove all evaluative dimension.

However, science is not value neutral; even in the context of justification it is necessary more than good logic and empirical evidence to decide whether to accept or reject hypotheses and theories. There is, as has already been said even by the logical positivist Philipp Frank, that there is always present a variety of reasons for assessing what are the actual factors and standards for the acceptance of a particular hypothesis or theory.

Science was never value neutral. However, that does not preclude objectivity, because, as wisely Robert Nozick (1997) stated ‘science is objective because of the values which is infused’. To forget it leads to a wrong conception of science and to paralyzing imitative claims for the sake of a science that never was.

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THE WAYS OF SCIENTIFIC REPRESENTATION: MODELS, MAPS AND REALITY

Diego Weisman
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Traditionally, *theories* have been considered the carriers of information about the world. The so-called *Syntactic Conception* constituted common scientific thought up until the late sixties, and provided the framework for the 'theory centrism' that prevailed until few years ago[1]. According to this vision, the fundamental objects of analysis for understanding science are *theories* which are viewed as logically analyzable linguistic structures, capable of offering real-world information through correspondence rules. In this framework, *models* were broadly relegated to secondary elements with, at most, heuristic value for science.

The popularity of this posture began to change around the 1960's. The reassessment of models in relation to theories and the fall of Syntactic Conception held one thing in common: growing popularity of the *Semantic Conception*, associated with the work of Patrick Suppes[2]. Under this second conception,

[1] Or alternatively, as 'axiomatic vision' of theories (Rosenberg 2005, cap.4)

[2] See Suppe (2000) for a qualified account and fundamental references. Contessa (2011) considers this denomination incorrect and proposes the label 'Vision of Models' in its place. Undoubtedly, its nomenclature is more transparent, but we decide to conserve 'Semantic Vision' for its widespread conventional use.

theories are understood as sets of models, or rather, formal definitions joined with theoretical hypotheses about the theories' adequacy to the world.

Today, philosophers of science widely agree about the main role of *model*s as source of empirical knowledge. Undoubtedly, the displacement of the axis of analysis makes necessary reconsider problems that philosophy of science has traditionally addressed, as realism, scientific change, reductionism, scientific relativism, among others. For example, *realism,* associated with the Semantic Conception, encouraged the contemplation of the relationship between theories and reality in the secular Aristotelian tradition. In this tradition, theories are (sets of) statements, and statements are defined as sentences that can be labeled as true or false. Therefore, theories, too, can be true or false, depending on if their statements do or do not correspond with reality. However, if we accept that the carriers of knowledge are *models* instead of *theories*, then, considering the non-linguistic nature of models, the problem becomes explaining the way in which scientific models relate to reality.

Faced by perplexities raised by the relation between models and reality, and a lack of clear questions, many scientists and philosophers of science have created analogies. One of the most widespread maintains that models represent reality in the same way that maps do. In effect, we could say that maps are not in themselves true or false, but nevertheless, in some sense, *represent reality*. In the same exact way, scientific models would represent real systems. The model of quantum levels would do it with the disposition of electrons around an atom, and a model of billiard balls would do it with particular movements, none of those being descriptively adequate in all senses, nor being genuine statements in their component parts that are capable of carrying a value of truth.

Of course, this topic is not foreign to economists, as demonstrated by extensive literature on the problems of unrealism of assumptions (Mäki 2009). In relation to this controversy, one of the major justifications of common thought among economists can be summed in the following statement: 'Don't expect descriptive exactitude from our (irrealist) economic models; after all, models are like maps.' As such, this analogy must be philosophically elaborated and explored in order to make it susceptible to a critical discussion, one that sheds light on its strengths, inconveniences, and limitations. Undergoing that labor is one of the objectives of this work.

The aforementioned turn of philosophy of science towards models coincides, incidentally, with the academic *practice* of economists, whom certainly work more often with models than with theories or laws. But is not so clear in which ways mathematically sophisticated models, overflowing with heroically 'unrealistic' assumptions could be able to represent the complete economic world in which we live, or even if they do represent something at all.

Talking about models raises further consequences. In analogous form with what occurred in philosophy of science, certain debates in economic methodology also end up restructured. Referring back to the previous example of the traditional view of theories as systems of statements, for some authors, the discussion about *realism* in economics involves discerning if the postulated entities exist or not[3]. Meanwhile, the anti-realist position argues that the scientific enterprise consists merely in the creation of useful instruments of prediction, and theories have no truth-value at all, nor represent anything. In this discussion, the displacement of interest towards models as units of analysis would seem to tip the balance towards an instrumentalist stance, but this appearance is mistaken. As we will see, even though a large part of the referenced authors consider models to be instruments, they only consider them to be *instruments* in as much as they *represent* something. This fact distinguishes models from the conventional instruments as defined by anti-realism, such as hammers, knives, etc.

The work is structured in the following manner. In the first section, the problem of representation is presented. The second section postulates the metaphor of maps as a usual way in which the relationship between models and reality is

[3] But see Mäki (1998d)

posed. The third section distinguishes between three meanings of 'representation,' and claims their use for resolving issues with representation. The fourth section explores the answer that the structuralist conception of theories forged to understand the representational capacity of models, an answer constructed around the concept of *isomorphism* between the model and the represented system or the target system, revisiting their criticism. Then, examine the introduction of the pragmatic conception as an attempt for solution, from the basis of the purposes of individuals. The fifth and sixth sections examine some objections in the suitability of maps as analogies of scientific models and offer some preliminary conclusions while inquiring as to the limits of the theory of scientific representation for the understanding of the role that models play in relation to our knowledge of reality.

1. The problem: How do scientific models represent?

The traditional philosophy of science has been settled in two convictions: scientific knowledge is incorporated in scientific theories, and theories consist mainly in systems of statements. These assumptions do not appear mistaken. This is partly because it coincides with an academic tradition which takes for granted that knowledge is inseparable from language, and partly because it opens the doors for the use of tools already available in the arsenal of philosophy, logic in particular. This second fact helps to minimize the cost of entrance of philosophers to the scientific field. With those tools, the Synthetic Conception analyzed the structure of theories, initiating a research program that lasted successfully for a large part of the 20th century. The linguistic nature of scientific *theories* converts them into clear candidates for being the carries of knowledge about the world. However, scientific *models* do not share this nature due to the fact that many of them are not sets of statements, but actually material objects, such as diagrams or drawings, and even if they are considered a set of statements, they lack rules of correspondence. As an object cannot be in and of itself either true or false, this brings about another problem of *how to relate models with reality*. In what way do scientific models give us knowledge about the world, if not as linguistic entities does, i.e. being true or false? A very extensive answer maintains that models relate with reality by representing it, much like maps. The question as to how a determined model represents its target system is usually denominated in literature as 'the problem of scientific representation,' and it has increasingly been the focus of philosopher's attention in recent years.

Despite enormous intellectual efforts that have been dedicated to clarifying the aforementioned problem of the relationship between scientific models and reality, a large degree of confusion persists. In the words of Contessa (2007):

> In the last decade or so, the problem of scientific representation has increasingly attracted the interest of philosophers of science. Unfortunately, the increase of interest in so-called scientific representation has not been accompanied by a comparable increase in our understanding of how models represent systems in the world. The lack of progress, I suspect, is mainly due to the fact that not only is not clear what the possible solutions to the problem exactly are, but is not even clear what the problem to be solved exactly is (Contessa 2007, p.9).

To avoid this issue, we take Contessa's proposal[4] and distinguish three meanings of 'representation': *denotation, epistemic* representation, and *faithful epistemic* representation. We also distinguish between two different questions– how models represent and how they do this adequately. However, before advancing with the development of rethinking this problem and its solutions, we pause to present a special class of the vehicles of representation that interest us especially: maps.

2. How do maps represent things?

The map metaphor has its own tradition in scientific philosophy (Van Fraassen 1980, 2008; Kitcher 2001; Giere 2006). In this point, we are going to call back the analogy once more in order to extract some useful analogies to clarify the use of

[4] See also Suárez (2004)

models in general and economic models in particular. The economist's standard defense against the accusations of unrealism has implications worth shedding light on.

Maps are *physical objects*. They are not statements, and for the most part it is not possible to say they are true or false in a strict sense. More than true or false, we habitually label maps as good or bad, referring to maps that carry out the task they are meant for and those that do not. Nonetheless, the category of *progress* does not seem to be foreign to cartography. For the most part, when we compare a 17th century map with a current one, the differences are notable and we find ourselves tempted to say that the current map is *more realistic* or perhaps *better* than the older one. Nonexistent entities have been omitted in the modern versions and those that really do exist have been added, as well as any relations between the postulated entities, like the distance between two cities, making it seem closer to reality.

Without a doubt, maps have become better in this sense, but the notion of an ideal or *perfect map* remains a myth. For an example of a 'good' map, take one offered by Google Maps. The service permits us zoom in and out between a large variety of maps of the same zone and to see different levels of detail within a limit. Yet, even the map that represents Earth's surface with the highest degree of precision is not 100% precise in any way (later we will discuss a special case), nor does it reveal all possibilities[5]. The map, after all, is not the territory. The complete or universal map is not only a practical or theoretical impossibility, but is also, as in Borges' tale[6], an undesirable finality.

[5] For a systematic defense of these limitations in direct reference to models, see (Teller2001)

[6] 'In that Empire, the Cartography of Art achieves such Perfection that the Map of only a Province occupies an entire City, and the Map of the Empire, an entire Province. With time, those Excessive Maps were not satisfactory, and the Schools of Cartographers came up with the Empirical Map that had the Size of the Empire and punctually coincided with it. Less addicted to the Study of Cartography, the Next Generations understood that this extensive Map was Useless, and they handed it over with no impiety to the Sun's and Winter's inclemency. In the Western Deserts, torn Ruins of the Map remain, inhabited by Animals or Beggars; in the entire Country, there is no other relic of the Geographic Disciplines.' Suárez

A hugely surprising variety of maps exists– road maps, subway maps, geographic maps, physical maps, maps of campsites, of airline routes, of stars, etc. Their diversity of uses allows us to note the role of *conventions* in the cartographic representation. A subway map, for example, could represent the stations with colored circles, each subway line with a distinct color, and the connections with dotted lines. It is clear the *real* stations are not circles, nor are the actual subway lines distinctly colored, but nobody expects a subway map to offer this information and nobody criticizes the map for these discrepancies. Rather, to understand the map as it is, is to know that there are irrelevant representations that are mere simplifications or conventions –stations shown as circles, connections shown as dotted lines, etc. – and central representations– in this case, the *order* of the stations. Of course, a good subway map must adequately represent the order of the stations, but not their shapes. In general, any map has representations that are relevant and others irrelevant. Users know this, and make inferences about properties of the real system over the basis of the map' *relevant* representations. Yet, what determines which elements are those necessary to be represented? How is decided what representations are relevant and what irrelevant?

2.1. The relativity of representation to purpose

Previous knowledge and purposes are fundamental for distinguishing characteristics on a map that are relevant from those which are not. Rather than the area represented, it is the aim who define the specific shapes of maps. And the aim of a map is also an important component when separating relevant inferences on a map from those which are not reasonable. In a similar way, the purpose makes possible to trace a line between valid and invalid inferences. In other words, a valid inference (Eg: 'Agüero station is connected to the Bulnes station') distinguishes itself from an invalid one ('Agüero station is a two-dimensional circle') by the undertone of the map's purposes.

Miranda: Travels of Prudent Men, short story, cap. XLV, Lérida, 1658. *Of scientific rigor*. Included in The Universal Story of Infamy.

Here, *purposes* are not a psychological state of mind. Independent from the psychological state of those who use them, subway maps are designed with an *objective* purpose that transcends the particular ones each user has – to get from one station to any other on the system. One could even say that knowing such general aims is what allows us to understand and identify a map as such. A nautical map or a map that shows a mountain's paths is nothing more than a 'rhapsody of sensations' to those who do not possess the minimum knowledge about how to use it or what it is used for. Such intersubjective purposes (and for that manner, objectives) will also allow the user to evaluate the good qualities of one map over another.

The fit to the purpose traces the line between which sectors or elements of reality can be deformed or directly erased, and which of them must be kept. The aim of get from one station to another determines both the necessity of representing the *order* of stations[7], and that the stations themselves can be represented in an arbitrary and unrealistic way, as green circles. No one that understands the subway map protests such simplifications.

It is not the geographical territory that determines the shape of the map. Consider a map of Patagonia made for backpackers and let us compare it with another made for cyclists. The components included in each differ. In the first one, we can find camp sites, paths, and price information, while in the second one we can find information about heights, authorized areas, waterlogged steps, etc. As *objects* they have very different shapes one from another, even though one could say they represent the same area: what varies is, once again, the *purpose* that determines which entities and relations are included and under what aspects and precision levels.

Up to here we have talked mainly about the *practical* purposes that guide the construction and utilization of maps. In that sense, the discussion would seem to

[7] A digression: here we face a *special case* in which the map's given purpose is representing an *ordinal* system. Here representation can be perfectly *exact* with respect to this purpose (but indefinitely inexact in relation to the remaining details).

be going towards an instrumentalist conception of maps, but the appearance is tricky. Maps, unlike other instruments, are capable of satisfying a purpose because *they are representative*. A hammer does not need to represent something in order to hit a nail, but if a backpacker's map is representing its territory systematically wrong, it becomes incapable of fulfilling its purpose, because the user would make awfully mistaken inferences.

2.2. From maps to models

The next step in the map metaphor is to assume that, in some relevant senses, *scientific models are like maps*. Models are usually physical, concrete objects, just like maps, or they are at least linguistic objects that lack correspondence rules. In the same way that it is impossible to say that a map alone is true or false (after all, a map is usually a piece of paper and not a statement), a model cannot, on its own, be true or false. Nevertheless, both are capable to give rise to conjectural statements over the territory or target system that can be true or false. Ultimately, models and maps derive their use from the ability to represent with a purpose that guides its construction and evaluation.

Now the map metaphor, as used by economists, appears clearer. To affirm that an economic model is unrealistic and nothing more because it deforms entities would be like saying that a subway map is unrealistic because King's Cross Station is not a Euclidean circle. It is very possible that the map designers know *that*, just as economists know that men are not completely rational, or physicists know that the planets are not points lacking mass. In terms of the present work, inferences of this class are *invalid*, as will be seen in the following section. It cannot be prohibited from a logical point of view that someone come to these sort of conclusions, but undoubtedly, to understand a subway map as such pragmatically disables the *relevance* of those inferences. Of course, the purpose of a map is to orient oneself and not to describe King's Cross Station. The map metaphor illustrates in what way the field of relevant criticisms of scientific models is bounded to the model's aims.

Maps and models are epistemic representations that, once established, easily show before our eyes the similarities between the vehicle and the represented system. However, what is later evident may not be at first. In other words, the similarities that a successful representation highlights do not serve as *criteria* for selecting certain vehicles as more accurate than others. This is not the only confusion that can arise from using the polysemic term ‘representation’. Moreover, the assertion ‘this map represents’ might have many faces. This brings us to the next question.

3. The many faces of representation: Denotation, Epistemic Representation, and Faithful Epistemic Representation.

The assertion ‘models represent’ can refer to at least three different questions. In the first place, under some sense of representation, a dice could be used to *represent* fortune; or the chair that holds the reader, to *represent* the ire of Achilles. The only requisite is putting one object in the place of another. A second sense of representation is more demanding. In this case, not all objects can just represent anything. We represent molecules with letters for atoms, and use lines or points for ionic and covalent bonds, respectively. Even though there is some degree of liberty for modifying the representations, there is an element of similarity with the object represented that should be kept at first. The arbitrariness of the first sense of representation disappears in the second. Finally, *some* of the representations of the second type are *faithful* in the sense that they give reliable information about their target system. The fact that the train lines of a country are represented adequately by the diagram is central for using it as an indicator of what we can expect at a geographic level.

If one accepts that the three senses of representation are different, it is surprising that there hasn’t been, except very recently, a systematic work carried out for separating them. Contessa (2007) makes a luminous point in this sense, by noting three different interpretations in which the term *representation* is usually understood as: denotation, epistemic representation, and reliable epistemic representation. The difficulties surrounding the subject could be originated on a semantic confusion.

The fundamental trait of denotation, the first interpretation, is its *arbitrary* character (in the conventional sense). One can use any object to represent any other object in the world. The employed object is denominated the 'vehicle' and the represented one, the target system. For example, we take in the course of a conversation a plastic glass to denote China, and later we crush it with our hand to demonstrate the changes that the principles of the communist economic system have undergone under the pressure of capitalism. The arbitrary part of this representation is a characteristic that exalts the purely *conventional* aspect of denotation. Practically any element that we choose can serve to denote any other element, and there does not seem to be limits for this. (Of course, it is simpler to denote concrete objects through the vehicle of our glass; with abstract concepts like 'the audacity of the cat' or 'Socrates' love for Alcibiades,' further conventions may be necessary)

A second interpretation of representation, more relevant for the scientific realm, is *epistemic* representation. Elements continue to be conventional, and usually there is comparatively less arbitrariness, or alternatively, the requisites that the vehicle must fulfill are stricter. Often, we do not only aim to denote, but also to bring about *conjectures* about the target system from a vehicle. For example, we can construct, for the comfort of philosophers trained in the Popperian tradition, a very rudimentary model of our solar system, in which the sun is represented by a lamp, while the Earth and the moon are represented by plastic, colored balls, with the aim of explain in a classroom the way in which solar eclipses are produced. Our rudimentary model permits us not only to denote, but also to *make inferences* about the target system. We could surmise, for example, that if orbits had dissimilar inclinations with respect to a common plane they would produce eclipses in our 'real' solar system, or that eclipses occur when the moon interposes between the sun and the Earth, etc. This ability to utilize the vehicle as a surrogate for making inferences about the real world involves the second sense of representation, *epistemic representation*. Of course, denotation is a necessary but insufficient component of epistemic representation.

From our model we *may* infer, also, that the earth is made of some polymer or that the power source of our sun, like the lamp that we use in our model, is made of large alkaline batteries. From a logical point of view, this is possible, but clearly there are some inferences that would make us think that whoever formed them simply did not understand the model (we could say even that understand a model is *impossible* without understand its purpose). Let's call this set of logically possible but pragmatically irrelevant, *invalid inferences*. Of course many of the valid inferences will also be *correct* (like for example, that if the moon interposes between the sun and the earth, there will be an eclipse), and others will be incorrect. The difference of validity or invalidity depends on comprehension of the aim of the model; the correctness or incorrectness is a *purely empirical matter*.

Finally, it is convenient to separate those vehicles that lead (mostly) to correct inferences and those that do not. As an illustration of this, let us suppose that we have constructed by error a representation of our solar system in which two bodies which denote the moon exist, and therefore there are two moons instead of one. Compared to our standard model, this one will lead to a set of conjecture that, though valid, will be *incorrect*. For example, it could result in an unusually high number of lunar eclipses over the course of a calendar year. In our terms, the first design is a *faithful* epistemic representation, while the second is not. More broadly speaking, models that constitute faithful epistemic representations lead to a set of inferences referring to real systems that end up being, generally, correct.

Denotation, epistemic representation, and faithful epistemic representation are three categories which one can utilize with large advantages to analyze the problem of representation. In relation to the question about the way in which models represent reality, it is easy to see now that an ambiguity exists. Some authors seem to understand the question as one of denotation, others as epistemic representation, and a third group attempts to resolve the problem of faithful epistemic representation. No wonder the state of disarray surrounding the problem of model's representation.

In the following we aim to disentangle some problems taking advantage of this taxonomy. For that, it would be useful distinguish between two questions: how do models represent? (In the sense that what make representation possible), and how do models represent reliably? (Or how can we know that a vehicle is reliable). Let's see the traditional answers to the first question.

4. How do models represent? The (too strong) answer of *isomorphism*

Under the representational account of models, models relate with reality by attempting to represent it. There are two branches: *structural* conception (in which the link between models and world relies in some kind of 'x-morphism', such as isomorphism), and the *intentional* conception (in which the nexus is provided by some sort of similarity)[8]. By way of introduction, we can anticipate that the first one claim the existence of some sort of direct and logical relation –for example, isomorphism- between the structure of reality and the structure of models, whilst for the second one, the representative relation is clearly weaker than a strict logical relation –for example, similarity- and can only be established and evaluated in the light of the user's objectives.

Following Frigg (2006) we can say that one of the questions that semantic theories of scientific representation must solve is the so called 'puzzle of representation.' Why are models capable of representing? The answer that structural conception offers declares that models are reducible to structures, capable of representing their target systems due to the fact that both –the structure of model and the structure of the target system- maintain an isomorphic relationship (or partially isomorphic or homomorphic in more refined versions).

[8] Due to the abbreviated character of this exposition we omit considering the possibility that both positions have an unintentional variant and an intentional variant (Suárez 2002, pt.1), nor more sophisticated interpretations of representation by similarity or isomorphism (Contessa 2007)

That is to say, scientific models could be one of the poles in a dyadic relationship formed by very different entities that share a certain structure at the same time. In more formal terms:

- M represents T if and only if M and T are structurally x-morphic.

Where M is the vehicle of representation (for example, a scientific model) and T is the target system, or objet to be represented.

This conception implies that in order to recognize the representation, all that is necessary is to analyze certain properties of both poles and verify that the relationship holds. Ideally, a privileged observer could examine the vehicle on one hand and the target system on the other hand and decide from this point if structural isomorphism exists or not. Of course only in the first case does the model represent in an epistemic manner, and therefore, is it capable of offering relevant information about the target system.

If the outlined version is not too unjust, it would appear that isomorphism is a sound answer only for certain subclasses of models, scaled models. The scaled model of a commercial airplane is constructed to resemble the real airplane in a way so that here isomorphism is clear. As fans know, the greater the degree of detail, the better for the scaled model, and such isomorphism can be determined simply by observing the two poles of the relationship.

Nevertheless, as a *general* answer to the question about representation in scientific model, this version is undoubtedly problematic. Tree basic criticisms have been insistently repeated against the structuralist vision of models and their variants (Suarez 2003, Frigg 2002). The first of them is formal in character and involves challenging the idea that scientific representation can be explained in terms of isomorphic relationships. Isomorphism is a symmetrical and reflexive relationship, while representation is characterized by asymmetry and non-reflectivity. Put in illustrative terms, a hypothetical photo is the representation of a face and not the other way around (asymmetry), and that the face is not a representation of the face, but simply the face itself (non-reflectivity).

The second criticism puts in question what can determine the existence of an isomorphic relationship between models and their target systems. The central argument is that neither reality nor the models exhibit their structure *per se* (at a very minimal point, it is necessary describe them). What's more, assuming that this description could be possible, there are no reasons at first to think it would be univocal. That is to say, if it is feasible to describe structures of reality and of models, then the mere possibility of different descriptions delineates different structures- not isomorphic structures- of both poles ban the relevance of isomorphism as a relation for establishing representation.

Finally, the practice of scientific modeling is not foreign to idealization, deformation, exaggeration and trimming the target system, all well-known and heavily discussed aspects, but not yet able to be incorporated in a structural vision.

4. How do models represent? The (weak) solution of *similarity*

The increasing consensus of the relevance of *purpose* for explaining the representation of scientific models has led to a second wave of studies that pay particular attention to the pursued aims of a model. Within the representation vision, there are remarkable attempt to exceed this obstacle appealing to revalue the pragmatic activity of representing in order to resituate the discussion. In brief, the *intentional* version, developed by Giere, introduces novelties to the picture. On one hand, he relocates the scientific *practice* en the center of the scene, introducing explicitly both agents and their purposes. This way, the general scheme 'X represents W' transforms into 'S uses X to represent W with the purpose of P.' On the other hand, he maintains that the relevant relationship between models and reality is not about isomorphism, but rather is about *similarity* and only similarity in appearance and with a grade of precision determined by the intentional framework.

To briefly take a look on those points, we begin with a new scheme of representation that the aforementioned author denominates ‘Intentional Conception of Scientific Representation’ In it:

- an agent proposes to utilize a model M to represent a part of the world W with some objective, O.

This scheme attempts to solve the problems of symmetry and the necessity of a univocal description of the reality that fall over the structuralist conception. The agent’ objective introduces *asymmetry* and specifies aspects of reality to adequately represent the objective, O, and also gradients of required similarity between them and the model, M, negating simultaneously that an *intrinsic* relation of representation exists between these things.

By way of synthesis, in the Pragmatic Conception a model is in most cases *similar* to a real system, and is this only in appearance and with a grade of precision established by the user in function of its purposes. Therefore, in this vision, the representation depends on the specific aims of the agents, and nothing too general can be said about the representation itself (Knuuttila & Boon 2011). This has driven to a progressive abandon of the question of scientific models’ representational capacities (considered as a criterion). If a criterion independent of similarity does not exist, then all that we say when we affirm that a monkey is a good model for a man for the purpose of X is that, later, the monkey resulted an effective model for certain purposes. Therefore, some similarity should exist between a man and a monkey. Otherwise, it is just lip service rather than the genuine explanation. This has been recognized for supporters of the pragmatic focus (R. Giere 2010; Teller 2001) and in the extreme, it assumes a minimalist version of representation which – in light of old pretensions- says nothing substantial of it, (Suárez 2004), no intends to reduce it to isomorphism, similarity, (Giere 2004), or resemblance (Mäki 2009).

5. The dilemma in the theory of representation

Structural Conception possesses the merit to provide a clear explanation of models as artifacts that bring us knowledge about reality: as far as a structural isomorphic relationship exists, the models represent and hence, they are genuine carriers of knowledge. Critics, nevertheless, turn this vision into something unacceptable. Meanwhile, the Pragmatic alternative presents candidates which are certainly weaker (similarity or resemblance), that do not appear to improve the outlook substantially. The problem, expressed in all its magnitude, is the following: it cannot be argued that a scientific model offers knowledge about its object *because* it represents it, but without having a representative criterion that is independent from the model, the whole explanation appears to be a word game. This brings us to what we can call the representation dilemma: either we explain the representation using the base of the properties of both a model and its object, and then we are susceptible to criticisms of the Structural Conception, or we adopt a minimal conception of representation in pragmatic or inferential terms, but in this case we renounce offering a substantive response to the epistemic question of how scientific models give us knowledge (or we say 'by similarity,' and quickly declare the concept unanalyzable)[9].

In case of being accepted, the dilemma turns 'representation' a notion of suspicious or superfluous value, and in both cases the option remains open to abandon the category and with it, the analogy of maps used by economists becomes obscure or unjustified.

[9] 'What has so far escaped notice in the discussion on scientific representation is that the pragmatic approach to representation has, in its minimal guise, rather radical consequences for how we conceive of models. Namely, if we accept the minimalist approach to representation, not much is established in claiming that models give us knowledge because their represent their target objects. Thus while it may be the case that the pragmatist account offers most that can be said about representation at a general level, it makes the representational approach hopelessly minimal as an explanation of how we can gain knowledge through models' (Knuuttila and Boon, 2011, p. 3)

Nevertheless, the distinction between epistemic representation and faithful epistemic representation permits us to see the dilemma in better light. It is clear that isomorphism is too strong *if taken as criteria* for determining *a priori* if a vehicle is a faithful epistemic representation, or if it attempts (a) to determine *a priori* that some vehicle is a 'good model' (in the sense of being a faithful epistemic representation) of its target system. This is a question that only supports *empirical* answers[10]. Indeed, how does one know beforehand that a model will be successful in its application in the domain of reality?

Isomorphism can answer other questions, nevertheless. If a model is successful, how can we explain such success later? The answer (because a model represents, or has some structural properties, or is similar) can appear *trivial* if what is looked for is a criterion, but not for those who have a genuine *metaphysical* interest. The difference between providing a criterion and responding to a metaphysical matter is relevant because it permits cutting back on the field of the application of the theory of representation. While the theory is not capable of determining if a vehicle is a faithful epistemic representation, it can explain for what reason the vehicle is a faithful epistemic representation. That is a matter of metaphysics, not a criterion, and so isomorphism as similarity can provide, if adequately understood, an answer. And metaphysics, from a post positivistic point of view, has meaning. If this argument is sound, representation concept play a role in positing a *realist* metaphysical attitude on those economists engaged with the construction or highly *irrealist* economic models.

6. The analogy of maps as a scientific realist stance

Despite its wide utilization, and the fact that it possesses a status of its own within science, the maps metaphor finds itself limited when it comes to showing relevant aspects of scientific practice. After all, when we work with scientific models, we don't have the possibility of evaluating the terrain on one hand and the

[10] It is possible that many critics of neoclassic models confuse both questions. 'Unrealism' in the sense of an absence of similitude between the model and reality, does not permit *anticipate* the model's failure, but does permit an explanation to those who seek *for what reasons* a model fails empirically.

representation on the other, as we have with maps. With a map we can interpret a preexisting terrain: this drawing refers to such and such level of ground elevation; that blue line refers to a certain river, etc. Scientific models don't fit with this picture (see Knuuttila and Boon, 2009), because we can't hold them with one hand while we appreciate with the other the represented object. The impossibility of accessing the structure of a scientific model's target system in a direct way doesn't make the category of representation null. It is true that you can no longer talk about similarity or isomorphism independently from the model's success, but representation remains necessary to sustain a realistic point of view.

The analogy of maps reveals a strong realistic imprint. It suggests that the usual economist's defense against accusations if unrealism, is neither a retreat to instrumentalism ('our models are mere instruments, and like maps they are just useful for orienting ourselves'), nor a calling to abstain from making metaphysic links ('our activity is like cartography, a technique independent from any kind of metaphysic'), but rather an appeal to *realism* and an assignment to a certain *metaphysic*.

If that is true, 'unrealistic' economic models try to represent something from reality, and the fact that they are useful – as surrogates, meaning they constitute faithful epistemic representations- allows the assertion (following the theory of representation) that they *actually* represent that something, which means their success is not the consequence of a miracle (Putnam, 1975), but of representational capacity.

On another hand, there is a *particular* function of representation somehow set aside by contemporary literature. Back to maps, besides their *practical* purposes, like getting from one point to another, or traveling from one city to another, they seem to have a *theoretical* purpose as well, which is getting to know *how the world is*. That is why we appraise recent maps over older ones, and why we can speak of progress in cartography. The prevalence of theoretical objective in the Western culture exposes the fact that we are not wondering what actually makes the contemporary world map better than the older one. In other words, we could direct another question at the map that has nothing to do with our practical necessity of getting from one point to another: the question about if it is *representing correctly* the world, or in other words, if it is giving us accurate information about the structure of reality. This second question is the one that allows us to speak about *progress*. It is the difference between a cartoon and a picture: they can both be 'models' suitable for certain purposes, and for some purposes the cartoon can be even preferable to the picture. However, in the light of the question of *which of those representations is the most realistic*, most people would pick the picture. The 'external' purpose, the reflection of reality, shows a possibly deeper reason by which the analogy of maps cannot be interpreted as a defense aligned *tout court* with scientific instrumentalism.

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A CRITICAL LOOK AT CRITICAL REALISM

Agustina Borella*

Tony Lawson, founder of The Social Ontology Group and The Realist Workshop of Cambridge, has proposed critical realism to reorient economics.

The transformation of the social world that Lawson tries, emerges from the adherence to critical realism, this is, from taking the transcendental realism of Roy Bhaskar to the social realm.

With the purpose of deepening the criticisms to this movement, we will specify what is critical realism, and which are the philosophical assumptions of the mainstream according to this author.

We will set out the criticisms on: a) the notion of mainstream economics, b) the possibilities of economics based on social ontology, c) the realism of economic models, and d) the notions of isolation and abstraction.

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1. What is critical realism?

In order to specify what critical realism is, we will distinguish scientific, empirical and transcendental realism.

According to Lawson, realism holds the existence of some kind of entity. He affirms that with attending scientific realism, the objects of scientific investigation exist mainly independently from their research.

> Now the conception of realism I want to argue for is closely and explicitly bound up with *ontology* or “metaphysics”, i.e. with enquiry into the nature of *being,* of *existence,* including the nature, constitution and structure of the objects of study. Lawson (1997, p.15) (The italics are from the original.)

> In identifying my project as realist I am first and foremost wanting to indicate a *conscious* and *sustained* orientation towards examining, and formulating *explicit* positions concerning, the nature and structure of social reality, as well as investigating the nature and grounds of ontological (and other) presuppositions of prominent or otherwise significant or interesting contributions. Lawson (1999a, p. 271)

He adds that empirical realism understands that reality consists of objects of experience or impression that constitute atomistic events. According to transcendental realism, the world is composed not only by events or states of affairs and our experiences or impressions, but also by structures, powers, mechanisms and tendencies underlying that exist, and govern or facilitate the events (Lawson, 1994). Attending this realism three domains of reality exist: empirical (of experience and impressions), actual (of the events themselves or state of affairs plus the empirical), and the real (of the structures, powers, mechanisms and tendencies, added to the previous).

In accordance with Lawson´s critical realism, in social phenomena underlie mechanisms or causal powers and science ought to ‘illuminate’ those mechanisms.

Lawson adheres to realism because he thinks this orientation can facilitate a more relevant economics. Critical realism is constituted by transcendental realism in the context of social realm (Marqués, 2003).

Bhaskar´s transcendental realism, that influences Lawson, opposes the new realism or the empirical realism. Critical realism conceives the social world as structured, differentiated and changing. According to this position, we can understand the social world only if we identify the structures that generate the events. This is possible by the theoretical and practical work of the social sciences (Bhaskar, 1989).

For Lawson, critical realism is not the way in which economists think about their discipline, but it is the way in which they should think about economics.

In this concept of the social world, the power of complex things depends on their structures. The objects that make up the world are structured in the sense of irreducible to events of experience, and intransitive in the sense that they exist and act independently of their identification.

> The conception I am proposing to defend is of a world composed in part of complex things (including systems and complexly structured situations) which, by virtue of their *structures*, possess certain *powers* – potentials, capacities, or abilities to act in certain ways and/or to facilitate various activities and developments. Lawson (1997, p. 21) (The italics are from the original.)

1.a. Social reality

For Lawson, the social realm is constituted by those phenomena that exist depending on human activity as intentional. Lawson (2001, p. 173) states ‘What is first of all the social realm? It is typically defined as that domain of all phenomena whose existence depends, at least in part, on intentional human agency.’

Social reality is a dynamic and complex net, formed by human action, the structures and the context of action, that interrelate and are in constant flux.

> The conception of the social world to be sustained is of a network of continually reproduced inter-dependencies. That is, social reality is conceived as intrinsically dynamic and complexly structured, consisting in human agency, structures and contexts of action, none of which are given or fixed, and where each presupposes each other without being reducible to, identifiable with, or explicable completely in terms of, any other. Lawson (1997, p. 159)

The social structure as rules, positions and relations is a precondition for intentional action. The structure cannot be considered fixed.

> Social reality is an emergent realm dependent upon, though irreducible to, inherently transformative human agency, and consisting of stuff that is intrinsically dynamic, i.e. everywhere a process, highly internally related and often relatively enduring, amongst much else. Lawson (2003, p. 44)

The powers that things have are in virtue of their structure. Investigating their structure we can know their powers. The structures act through their mechanisms. Lawson (1997, p. 21) says ‘A *mechanism* is basically a way of acting or working of a structured thing.’ These mechanisms are formed by causal powers that act like mechanisms that determine the phenomena. And he adds, (1997, p. 21) ‘Structured things, then, possess causal powers which, when triggered or released, act as generative mechanisms to determine the actual phenomena of the world.'

Lawson characterizes the social realm in the following way:

a. internally related.

> A social system can be recognised as a structured process of interaction; an institution, as already noted, as a social system/structure that is relatively enduring and perceived as such; a collectivity as an internally related set of social positions along with their occupants, and so forth. Lawson (2003, p. 58)

The social realm is what it is, in virtue of the relationship in which each one places himself in respect to the others.

Lawson distinguishes internal and external relations. Two objects are externally related if neither of them is what it is because of its relation to the other. For example, bread and butter, coffee and milk, among others. And they are internally related if they are what they are in virtue of their relation with the other. For example: owner and tenant, teacher and student, employer and employee (Lawson, 1997).

A distinctive characteristic of the social world is the omnipresence of internal relations that make up what he calls 'organic wholes.'

b. holistic

c. open

> Now I take the social realm to be that domain of phenomena whose existence depends at least in part on us (a realm which, I take it, includes [but I suspect is not exhausted by] Popper´s world 3). And according to the conception I defend social reality is (found to be) in a fundamental sense *open* (...) Lawson (2002, pp. 2-3)

According to Lawson many economists share the intuition that human agents have the real capacity to choose. This implies that if the capacity is real, then, men could have always acted differently. This assumes that the world is open and that the events do not occur necessarily. As this, the intentionality of the agents is related to the knowledge they possess.

> Now if choice is real any agent could always have done otherwise, each agent could always have acted differently than he or she in fact did. Clearly, a necessary condition for this is that the world, social as well as natural, is open in the sense that events really could have been different. Put differently, if under conditions *x* an agent chose in fact to do *y*, it is the case that this same agent could really instead have done not *y*. Choice to repeat presupposes that the world is open and actual events need not have been. But the possibility of choice not only presupposes that events could have been different. It also entails that agents have some conception of what they are doing and wanting to achieve in their activity. That is, if choice is real then human actions must be intentional under some description. Intentionality in turn is bound up with knowledgeability. Lawson (1994, p. 269)

Social structure can only be present in an open world. (Lawson, 1994). Lawson distinguishes closed and open systems. A closed system is the one in which constant conjunctions of events are presented. The author understands that in deductivism underlies a concept of the social world as a closed system, and because of this, he rejects it. The aim of science is not to make predictions of events, but to identify those structures, mechanisms, causal powers that underlie phenomena, to enable to reorient economics and transform the social world, essentially open. These mechanisms are identifiable by the method of contrast explanation and abductive reasoning. By this method, a causal explanation of the mechanisms underlying phenomena is intended (Lawson, 2009c). Although how we arrive at a specific cause of a certain phenomenon is, with this author, problematic.

d. structured.

Lawson points out that behind the events and states of affairs that form social reality are structures, powers, mechanisms and tendencies that make them possible.

> However, it is a further feature of the conception I defend that social reality is (found to be) not only open in the manner described but also *structured*. That is, it comprises not only actualities such as actual events and states of affairs (some of which we may directly experience) but also deeper structures, powers, mechanisms, and tendencies, etc., which produce, facilitate, or otherwise condition these actualities. Lawson (2002, p. 3)

According to the structure certain powers that are actualized by mechanisms, are possessed.[1] The powers and the structure are kept even when they are not exercised.

> Consider an aspirin. In virtue of its intrinsic chemical structure it has certain powers, most obviously to relieve a headache (or pain in general). Or consider a bicycle. Because of its physical structure it facilitates rides. Now the powers of aspirins, bicycles, and anything else, can exist unexercised; the aspirin may remain in the bottle, the bicycle in the garden shed. When powers are exercised they work by way of mechanisms or processes. Lawson (2001, p. 172)

e. processual

[1] Even though mechanisms, tendencies and powers have a fundamental role in Lawson´s social ontology, it is necessary to point out that along his work, these terms do not appear sufficiently explained. In many opportunities he highlights the central role they play, though he uses these terms as if they were primitive.

The nature itself of the social realm is the process. Change is intrinsic to the way of being of social reality.

> Rather social items such as markets and political systems must be understood as processes, as reproduced structures of interaction, with change recognized not as an external happening, the result of an external or exogenous stock, but as an integral part of what the system or object in question is. Lawson (1994, p. 279)

The characterization of the social realm typical of the social ontology of Tony Lawson establishes restrictions to the possibilities of isolation in the social world, and is key in the evaluation that this author makes of mainstream economic theory.

2. The philosophical assumptions of the mainstream

Lawson criticizes the philosophical assumptions of mainstream, deductivism and empirical realism (Marqués, 2004).

2.a. Deductivism:

Regarding deductivism, what he criticizes is the conception of law on which depends deductivist explanation. For Lawson this conception of law is formulated in terms of constant conjunctions of events or states of affairs. They are laws that connect results at the level of events. They express regularities of the form 'whenever the event *x*, then the event *y*'. The theories that are constructed with this conception of law are what Lawson calls deductivism.

> By deductivism I simply mean the collection of theories (of science, explanation, scientific progress, and so forth) that is erected upon the event regularity conception of laws in conjunction with the just noted principle of theory assessment. Lawson (1997, p. 17)

Such constant regularities of events, expressed in those laws, refer to systems in which those constant conjunctions of events emerge, this is, to closed systems. To apply deductivism, a closed system is necessary.

2.b. Empirical realism:

As we have shown, according to empirical realism, that he rejects, only events and individuals that register them by their senses exist, and the laws express constant conjunctions of events. It considers positivism as the knowledge that consists of sensations or impressions. The relation of these with instrumentalism appears. Here economic theories are useful or efficient, but not true or false. Lawson (2001, p. 158) explains 'I understand by instrumentalism the thesis that theories are to be interpreted merely as practical tools or instruments for some purpose other than causal explanation'. According to Lawson the realist is busy explaining the world. And he adds (Idem, p. 167) '(...) a scientific realism, asserts that there are ultimate objects of scientific investigation, and that these exist, for the most part quite independent of, or at least prior to, our investigation of them.'

The typical characterization of mainstream by Tony Lawson, that assumes deductivism and empirical realism, makes it impossible that by its models we may access the social world, according to the social ontology of critical realism.

The problem about the ontology present in Lawson, is not adhering or not to the presence of causal mechanisms operating behind the phenomena, since it is not necessary to be a critical realist to support this. Beyond the transfactuality of the mechanisms that is not restrictive of Lawson´s realism, the difficulty is in the characterization of social reality, and its stratification. To this, social ontology is added as prescriptive, which makes the possibilities of accessing the social realm difficult, without adhering to this ontology.

> Lawson is clearly engaged in *prescriptive metaphysics*; he wants economists to change the way they think about necessity and being.

> According to Lawson, there is a right ontological approach (critical realism), and a wrong ontological approach (empirical realism), and he wants to convince economists to move from the latter to the former. Hands (2001, p. 328)

3. Criticism on critical realism:

3.a. The notion of mainstream economics

Different criticisms have emerged in relation to what Lawson understands by mainstream. To begin with, a more accurate description of what the mainstream is seems to be asked for.

Vromen disagrees with the idea that the mainstream economic theory assumes an ontology of closed worlds of isolated atoms (Vromen, 2009).

Davis points out that the object of Lawson´s criticism is diffuse (Davis, 2009). He understands that the mainstream theory is more heterogeneous than that which Lawson explains. Regarding the two fundamental criteria he uses to distinguish what he calls mainstream: 1) the insistence on formalistic methods and 2) the presupposed ontology; in some cases he seems to refer only to the second, which is the most relevant. In other opportunities he maintains the two of them. The first criterion is rather epistemological, even though he sustains that he does not do epistemology.

Lawson opposes to the insistence in the use of the methods of mathematical modelling, that he understands as essential to mainstream economic theory. Although he sustains (in opposition to what is attributed to him by his critics) that he does not reject mathematics.

> *My argument is not all and anti-mathematics one; and it never has been.* I have only ever criticised the way (certain) mathematical methods tend to be used in modern economics. Indeed it is

> precisely the belief that mathematics ought not to be applied without due care and consideration, coupled with a conviction that in modern economics it too often is so, that explains the direction of much of my writing. If you like, my concern is that much of economic modelling appears somewhat analogous to a violin being used as a drumstick. To suggest that this may be "bad practice" is in no way to devalue the violin, or to deny it a place in the orchestra. Lawson (2009a, 228, note 12) (The italics are from the original.)

According to Lawson, the methods of mathematics used by economists are tools. But he understands that the conditions under which these tools are useful do not occur frequently in the social world. As we have pointed out, he criticizes the insistence on the use of the methods of formalistic modelling; although he does not reject them, he does not specify which would be those occasions in which those methods would be useful.

As to which is the state of the mainstream, there are also difficulties (Hodgson, 2009). Lawson points out that the state of the mainstream is 'unhealthy', and to the question 'which is the illness of the mainstream?' he answers, 'deductivism'. From the latter he infers a way of understanding reality, different to the one underlying critical realism.

Lawson adds that the situation of mainstream economics is sad and unfortunate. However, Deichsel sustains that on the evaluation that Lawson makes of modern economics, that he takes as a point of departure from his ontological proposal for reorienting economics, there is no agreement (Deichsel, 2011).

> First, although there are many economists who agree with Lawson that there is something badly wrong with mainstream economic theorising and practice, there are others with very divergent beliefs in this regard. Many believe that orthodox economics is doing perfectly well and has shown itself to be highly successful _both in

> its general predictions and as an explanatory guide to policymakers. Hodge (2007, p. 23)

3.b. The possibilities of economics founded in social ontology

According to Lawson, the ontological investigations convey methodological implications. The most important ontological difference in this sense is the existence of transfactuals.

> A transfactual statement is not a counterfactual, i.e. it does not express what *would* happen if the conditions were different. Rather it refers to something that *is* going on, that is having an effect, even if the actual (possibly observable) outcome is jointly co-determined by (possibly numerous) other influences. Lawson (1999b, p. 5)

Hausman points out two reasons at Bhaskar and Lawson´s criteria to accept transfactuals (Hausman, 1999a):

1. Without them it is not possible to theorize in open systems.
2. The knowledge of transfactuals allows to explain and provides a guide for politics when there are no regularities available.

Hausman rejects these two reasons because he indicates that just as the ceteris paribus assertions do not tell what will happen when other things do not remain the same, the transfactuals do not tell what will happen in open systems. In his opinion, there is nothing that can be done with transfactuals that cannot be done with counterfactuals, except convince themselves mistakenly that the knowledge of non-empirical activity allows to explain and do politics without knowing anything about real results. It is a criticism on the explanatory value of the transfactuals.

The transfactuals have a central role in Lawson´s critical realism. The distinctive is, to Hausman´s criterion, the thesis of transfactuality of mechanisms, that apart from being real, they are always active (once triggered) under the phenomena (Hausman, 1999b).

The defence of transfactuals allows the laws to be applied when in open systems appear counterexamples or when the ceteris paribus conditions do not occur. According to transcendental realism the law is true if it describes correctly the operation of a generative mechanism and the mechanism is really operating in that instant (Bhaskar, 1978).

Hausman points out two problems around transfactuals:

1. How do you justify the operation? Because the mechanisms can belong to the essence of the thing, and because of that you can suppose it is maintained from one context to another.
2. To suppose that ‘x tends to do y’, given that there can be other operating mechanisms that intervene (Hausman, 1999b).

However, is it possible to know which particular mechanism is operating here and now? If economics cannot predict in Lawson´s discretion, facing these difficulties, it could not explain in a strong sense, either, (this is, to know which particular mechanisms are acting in a determined place and time). So, what is left is a ‘how possible explanation’, but this is also done by the mainstream.

How is it possible to explain in a strong sense? Lawson´s proposal implies that we know which is the mechanism, that if it were functioning, it would make these events happen. But, in general, there is more than one mechanism, and mechanisms that overlap, so how can we distinguish which mechanism is operating? If we cannot answer this, is it possible to explain?

According to Bhaskar transfactuals can explain what occurs in open systems. For this he proposes a procedure that consists in:

1. Breaking down the causal components of the phenomenon.
2. Describing the cause and the effect in terms of the theoretical knowledge of one of the relevant mechanisms.
3. Identifying the possible causes.
4. Eliminating the alternative causes (Hausman 1999b).

As a result of this procedure the transfactual knowledge of the mechanism allows to explain the phenomenon.

3.c. Realism of economic models.[2]

As regards the unrealistic assumptions of economic models that 'turn them' false, Hodgson (2009, pp. 175-188) holds that it is not about constructing more complicated or realistic models. From more complicated or realistic models can emerge similar results. This is, more complex models are not a guarantee of better results. Even though Lawson agrees on reality being complex, he disagrees with that because of that we ought to construct simple models. Lawson attacks the idea that from the complexity of the world, one can infer that our analysis should distort reality knowingly (Hirsch-DesRoches, 2009).

Given the complexity of social reality, models are not isomorphic, this is, they are inevitably distorsions. This implies an important difficulty to obtain true models.

Hodgson understands that even though Lawson´s position and the mainstream´s are opposite, both have assumptions. Mainstream economists assume that models are sufficient to represent the world, and interpretation can be left aside. Lawson assumes, in turn, that models suppose a way of understanding the nature of reality. Hodgson holds that the assumptions of both (positions) are false. Hodgson (2009, p. 182) adds that no mainstream economist would deny that the world is open, and (would not sustain) that any formal model would suppose that other causal mechanisms that have been omitted in the model, do not exist.

Lawson understands that the comprehension of social reality is independent of the construction of models, and that modellers are uncomfortable with the mathematical formalistic models because they are unrealistic.

[2] Note that, curiously, Lawson says he has not taken part in the debate realism-anti-instrumentalism of philosophy of science, in which the debate on realism of economic models of the philosophy of economics can be located, nor in the debate realism-anti-realism of metaphysics, and nevertheless he makes contributions to both debates.

> Obviously, modellers are uncomfortable with the charge of irrelevance, so attempts will be made to render models as realistic as possible, real insight will be tagged on wherever feasible. But as I say, I believe the real insights are typically independent of, and indeed achieved prior to, the construction of the mathematical model. Lawson (2009a, 229, note 20)

The deductive formalistic mathematics of the mainstream economic theory assumes, according to this author, an ontology of closed systems of isolated atoms. Given the nature of the social world, the method of theoretical idealization typical of the isolation, cannot provide insight into social reality (Lawson, 2011).

The unrealistic mainstream theories consist of explanations of isolated atoms, this is, entities that have independent and invariable effects whatever the context is. The ontology of isolated atoms implicit in deductive mathematic models is, according to Lawson, inconsistent with the way social reality is.

> This blind faith in the appropriateness of always using mathematical-deductivist methods is a problem just because the implicit ontology of isolated atoms, that such methods presuppose, is inconsistent with the way social reality is found to be. For it is easy enough to show (via philosophical-ontological analysis) that social reality is open structured, processual, highly internally related, characterised by meaning, value and so on. Lawson (2009b, p. 167)

Hodgson believes that for Lawson 'more realism' means 'more richness or complexity' of the models. But Lawson does not hold to what is attributed to him. For him, more realism means incorporating the mechanisms that 'we know' work in reality. Lawson´s criticism of the increasing complexity of the econometric models illustrates this point.

Deichsel also questions the thinking that an economics that describes better, in detail the social world, is going to be better than the mainstream theory. The more detailed description may not be a useful base to theorize.

> Second, for the sake of Lawson´s argument let us accept that an economics that depicts the inherent dynamics and openness of social systems fits better into the totality of our current beliefs than the mainstream mechanistic picture. This fit is surely not an absurd standard for "realism" in economics. But is it a helpful normative guideline to improve this fit? I have my doubts. Also at the methodological level more realism may not be helpful, because the increased detail of research based on "social ontology" is not likely to be a useful basis for theorising, because the emerging picture is too "messy" for that. While a deterministic picture of humans as rational agents may be false, it can be fruitfully so. Deichsel (2011, p. 14)

More realism does not imply models that correspond with reality, as the correspondent notion of truth. They are models that fit not as detailed correspondence, but models that adequate to the ontological conditions of the social world, according to the social ontology of Lawson´s critical realism. Such models would be realistic ones within the framework of critical realism.

His engagement with social ontology is decisive for his position towards the use of mainstream economic models, according to the characterization that this author makes of the mentioned economic theory.

3.d. The notions of isolation and abstraction.

Abstraction allows, at Lawson´s discretion, to investigate closed and open systems. To abstract implies focusing on certain aspects of something and leaving others aside.

> I interpret abstraction, here as always, according to its traditional meaning of focusing on certain aspects of something to the (momentary) neglect of others. It is a process of focusing on some feature(s) of something(s) while others remain in the background. Lawson (2009a, pp. 203-204)

Also through abstraction possible causal mechanisms that may cause those phenomena are selected (Hodge, 2007).

In Lawson, isolation is made by abstraction that does not necessarily imply idealization. To abstract is not to idealize. Even though to isolate implies to abstract, to abstract does not necessarily imply to idealize. Lawson does not accept isolation since it implies idealization and omission.

His defence of critical realism is not supported in the use of isolations by false idealizations. The place of isolation is connected to the relation of Lawson with mainstream economics, to reorient economics, moving it away from simplified, idealized models, that have nothing to do with social reality.

His ontology is very important to show his rejection of isolation. Social entities are totalities, and in accordance with this, isolation is not viable, because this would imply a split.

> Clearly abstraction, but not theoretical isolation, will be relevant wherever the whole is not just the mechanical sum of parts. Composers, surgeons, artists as well as social theorists deal with internally related wholes. As such abstraction, not theoretical isolation, will be the appropriate method of analysis. Lawson (2009a, p. 205)

To abstract is to identify a set of aspects that are essential to the phenomenon. But it does not consist in pointing out which of those aspects is more general (Lawson, 1997).

To abstract is not to make closures either. This is according to the notion of abstraction that we have explained and agreeing that a closure is a system that sustains event regularity.

> Clearly, abstraction can be applied to all types of systems, to those that support strict event regularities, to those that support partial ones and equally to those seemingly not supporting any. It can be applied to matters that are real or fictitious. If I talk only about the horn (or white colour, or the horn of a billy goat-beard or lion´s tail, or cloven hoofs) of a unicorn, I am abstracting in the context of discussing a fiction. To say of the social system, or of any specific part of it, that it is fundamentally open is to abstract. To suggest that abstraction presupposes closure is simply to misunderstand one or other or both of the two terms. Lawson (2009a, p. 207)

There exists a relation between to abstract and to model, as far as modelling is concerned you leave aside the rest of the world.

> Economies are “modelled” as closed in the sense that the rest of the world does not exist, uncertainty is all but banished, as are becomings, and “begoings”, mortalities and (systematic) mistakes, conflicts and crisis, internal relations and transformations. In the name of abstraction all features of social reality that prove inconvenient to deductivist modes of reasoning are ultimately assumed away. Lawson (1997, p. 235)

Hodgson (2009, pp. 175-188) criticizes Lawson that in his proposal there is a vague distinction between isolation and abstraction.

Hodgson points out that Lawson realizes that there are no theories without a certain degree of abstraction, given that it is impossible to consider all elements at the same time. But if abstraction is necessary, and it implies a limitation of what is going to be considered, to exclude additional forces, etc., this also implies the assumption of a closed system.

However, Lawson maintains that to abstract and to isolate are different. To abstract is to focus on certain aspects of something leaving aside, momentarily, others. Focusing on some characteristics of something, while others are left "in the back". To isolate is to treat those aspects that are not focused on as if they did not exist.

> To abstract is to focus on aspects of something whilst *not* assuming the non-existence, or non-impact, of features not focused explicitly upon (that are abstracted from). To isolate theoretically is precisely to treat those aspects not focused upon as non-existent, or at least as sealed off, as having no systematic influence. Lawson (2009a, p. 204)

Lawson sustains that it is different to leave aside something momentarily than to treat it as if it did not exist, as it is done when isolating. Abstraction is, according to Lawson, indispensable to science. Its aim is to individualize a component or an aspect of something concrete to understand it better (Lawson, 1997).

The process of abstraction allows us to illuminate social reality, essentially open.

Abstraction, in Lawson´s criteria, does not imply closure, given the definition of abstraction indicated, and the consideration of what we understand for closure: a system that holds a regularity of event.

> It is true that I argue that regularities (real or imaginary) of the form "whenever event (or state of affairs) x then event (or state of affairs) y" (or stochastic near equivalents) are a necessary condition if formalistic deductivist methods of the sort economists seek are to be utilised. Systems in which these regularities occur I refer to as closed. Lawson (2009a, p. 194)

Abstraction can be applied to every kind of system, those that hold strict event regularities, partial ones, or none. It can be applied to real or fictitious questions.

Abstraction is relevant when the whole is not the mechanic sum of its parts, as in Lawson´s proposal. He explains that even if these methods are different, they are not alternative. The complexity of the world makes abstraction to be always involved. The method of isolating, by contrast, has conditions that are very restricted under which it is useful or relevant.

A theoretical isolation is a thought experiment. It is the process of imagining what will occur if a physical isolation could be reached.

To explain how the social world is, is not about isolating. There is a pre-eminence of the ontological over the theoretical. It makes no sense for Lawson to separate what in reality cannot be separated. Even though reality is complex, the method proposed by this author is not to simplify it through isolations, but to abstract.

The models reached by isolations do not adjust to the social world.

Lawson thinks that Hodgson´s aim is to persuade that the methods Lawson defends, especially abstraction, have the same problems of the mainstream. Hodgson holds that formal methods can be more useful than what Lawson considers.

> I think Hodgson´s goal is to persuade that the sorts of methods that I advocate (and more especially abstraction) face essentially the same problems as those confronting the mainstream. In other words, Hodgson seems to be working on two fronts. On the one hand he wishes to suggest that the formalistic methods can be more useful than I allow. On the other hand he wishes to convey the impression that any alternative methods that I have advocated share any difficulties that can be associated with formalism. Lawson (2009a, p. 202)

For an explanation to be successful it is necessary to maintain the distinction between abstraction and isolation.

Facing Lawson´s criticism that the distinction between to isolate and to abstract is insufficiently precise, Lawson intends to show that both methods are irreducible between themselves.

4. Conclusion

In this work we have tried to show the main difficulties that emerge in Tony Lawson´s critical realism. In order to do this we explained what is critical realism and which are the philosophical assumptions of the mainstream economic theory according to this author.

We pointed out the critical aspects related to the notion of mainstream economics; the possibilities of an economics founded in social ontology; on the realism of economic models; and the notions of isolation and abstraction.

In the former, there are difficulties to define mainstream economic theory and distinguish if Tony Lawson makes an adequate characterization of it and of the state of modern economics.

On the possibilities of an economics founded in social ontology, it was posed that there exists certain disagreement on that Lawson's ontological proposal may allow to make a better explanation of the social world. (Especially if there is disagreement around the state of mainstream economics)

On the realism of models, what this author "claims" is not exactly more complex models, but models that are capable of capturing the mechanisms that operate behind the events and in this way transform the social world.

It is still necessary to specify a bit more of what are transfactuals, and in particular the notion of mechanisms, central to his social ontology. Especially, if what is expected is that illuminating those mechanisms, we will be able to reorient economics.

As regards the difference between abstraction and isolation, he clearly distinguishes between them because he attributes to those concepts different ontologies. Isolation requires empirical realism opposite to Lawson´s critical realism. Isolation, far from bringing us closer to the social world, moves us away from it, and stops us from explaining it and transforming it.

Finally, it is necessary to adhere to critical realism and manage to reorient economics and transform reality, to adhere to his social ontology, and apply transcendental realism to the social world. Without this look at the social realm, economics will go on in the sad, unfortunate and unhealthy state that Lawson diagnoses.

However, if Lawson´s ontology is not shared, what room is left for dialogue with mainstream economic theory´s proposal?

Is there room left for modelling and that modelling does not imply a commitment with an ontology of closed systems? Does modelling imply necessarily an ontological commitment? Is it not possible that modelling is only a tool that we use to understand something of an essentially open world? Which is the concrete economic theory for open systems alternative to mainstream economic theory? Is it possible to do economics without adhering to Lawson´s prescriptive ontology? Is Lawson really pluralist? Does a possibility of a meeting between Lawson´s heterodoxy and mainstream orthodoxy exist?

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MILL, HAUSMAN AND THE TRADITIONAL METHOD IN NEOCLASSICAL ECONOMICS

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1. Introduction

In the present essay we argue that the economics methodology originally developed by John Stuart Mill (1836, 1843) – who stated that economic science can only be defined as *inexact* since the theorist is only aware of the main causes of economic phenomena while abstracting herself from the myriad of infinite causes that also operate upon them – could be compatible *only* with certain versions of neoclassical economic theory[1] which from now on we shall refer to them as *traditional versions*, but not with the general inter-temporal or temporary equilibrium models inspired by the works of Hicks (1946) and Arrow and Debreu (1954). The discussion here proposed is, we believe, quite relevant since it takes issue with Hausman's particular proposal of a 'return to Mill' in economics methodology (Hausman, 1992; 1998). In these works Hausman argues that such a 'return' could be used for a defence of what this scholar has called equilibrium theory (*i.e.* the neoclassical economic theory). However, as will be argued in this essay, Hausman overlooks the fact that the neoclassical theory

[1] In order to avoid any misunderstanding I mean by neoclassical theory the school of economic thought that was born in the late XIX century with the works of Walras (1900), Menger (1871) and Jevons (1871). I shall also deliberately use the adjective marginalist to refer to this school.

underwent a deep turning point in its analytical structure – and so in its method – after the so-called “formalist revolution” which took place in the late 1940s (Hicks, 1946) but thoroughly took root in the profession in the mid-1950s along with the models proposed by Arrow and Debreu (1954) and related work published in the successive decades. We shall, therefore, argue that only those versions of the neoclassical theory whose premises taken as data allow determining a persistent and stable equilibrium of supply and demand – so the latter is apt to be examined as a tendency – are compatible with Mill’s methodology.[2] Moreover, only within these traditional versions of neoclassical theory will we able to abstract in a plausible manner from what Mill called the ‘perturbing causes’ affecting the actual equilibrium, while on the other hand pursuing the same method of abstraction turns out to be implausible for the neo-Walrasian general equilibrium models inspired by Arrow and Debreu models so the latter’s equilibrium cannot be conceived as being compatible with any notion of tendency as instead can the equilibrium determined by the traditional versions of the marginalist theory (e.g. Marshall, 1920; Wicksell, 1904). Besides this introduction, the present paper consists of four sections. In Section 2 we shall show what the so-called ‘Mill’s problem’ is about and discuss its relationship with traditional economic theory. Then, in Section 3, we take issue with the position held by Hausman in regards of Mill’s problem, its criticisms and its solution. Section 4 will introduce the analytical elements which distinguishes the two different versions of neoclassical economic theory which, as the present author shall argue, are key for a proper appraisal of mainstream economic theory in the light of Mill’s methodology. Finally in Section 5 a conclusion will be advanced.

2. Mill’s problem

John Stuart Mill was probably the most important XIX century economist trained in the classical political economy (see Mill, 1848) who was chiefly concerned about the problem of definition, nature and method in the economic science. For

[2] Of course the classical theory of prices and distribution (e.g. Ricardo, 1821, [1951]) is also apt to be examined in the light of Mills’ methodology but in the present essay we shall exclusively deal with the neoclassical (or marginalist) theory.

Mill political economy is an abstract science whose method to be applied is the *a priori* method (see below) and, although it is not the science of speculation or politics, belongs to a branch of the latter since it 'does not treat of the whole of man's nature as modified by the social state, nor of the whole conduct of man in society. It is concerned with him solely as a being who desires to possess wealth' (Mill, 1836 [2000], p.97). In this sense Mill underlines that political economy as a scientific discipline will deal with the examination of men's capabilities to distinguish the most efficient means to obtaining wealth and to raising it, by making abstraction of any other behaviour, passion or will that might have an influence upon men's conduct. Thus,

> [u]nder the influence of this desire, [political economy] shows mankind accumulating wealth, and employing that wealth in the production of other wealth; sanctioning by mutual agreement the institution of property; establishing laws to prevent individuals from encroaching upon the property of others by force or fraud; adopting various contrivances for increasing the productiveness of their labour; *settling the division of the produce by agreement, under the influence of competition* (competition itself being governed by certain laws, which are therefore *the ultimate regulators of the division of the produce*); and employing certain expedients (as money, credit, etc.) to facilitate the distribution. All these operations, though many of them are really the result of a plurality of motives, are considered by Political Economy as flowing solely from the desire of wealth. The science then proceeds to investigate the laws which govern these several operations, *under the supposition that man is a being who is determined, by the necessity of his nature, to prefer a greater portion of wealth to a smaller in all cases*. (Mill, 1836 [2000], pp. 97-98, emphasis added)

For Mill, therefore, competition acts as the regulating force of the behaviour of the distribution of the social product and of the economy in general, under the assumption that mankind will always tend, to use modern economic language, to maximise (minimise) benefits (costs).

At this juncture, however, Mill acknowledges that in society one can find a broader group of phenomena that has an influence on mankind's conduct and that goes far beyond the desire for wealth. For example, there are reasons of political nature or even of personal and sociological nature that affect the decision on whether undertaking a given economic action. Of course, as Mill also stresses, no economist ever did assume that the only one men's motivation was wealth and its increment; there exists indeed a myriad of phenomena that can impinge on men's economic behaviour. That is why the economic principle that every man (or society as a whole) will search for the highest wealth is subject to the clause known as *ceteris paribus*. Still, Mill acknowledges that those motivations that are put aside by economic analysis might interfere in the validity of the economic principles in such a way that the *expected results according to the theory in question* could not be verified in the actual world (Marqués, 2000, p. 247). In other words, Mill envisaged the existence of the validation problem in the economic theory he himself adhered to (*i.e.* classical theory) according to the empirical standards of scientific knowledge. Following related literature, this conflict we call 'Mill's problem' (Hausman, 1998; Marques, 2006) and comes up from the contrast between the theoretical basic economic principles and the difficulty to prove them. How was it possible to get round this problem?

Mill himself (see Mill, 1836 [2000]) settled this conflict through the incorporation into economic theory of the assumed premises (which in the case of classical theory are justified on the grounds of the current actual experience). For Mill, these premises or principles not only are the starting point but also are *true abstractions* from which one can obtain *true outcomes* (Marqués, 2000, pp. 247-248). The problem, as said above, is that Mill is aware that the theoretical predictions cannot be proved in the actual reality so in order to overcome this issue Mill suggests examining the nature of economic science. According to Mill (and arguably according to Ricardo himself) the world is far too complex as to capture and incorporate into the theoretical apparatus the whole aims and motivations; indeed a myriad of causes that ultimately affect phenomena are usually abstracted from by the theorist when she postulates the *ceteris paribus* condition. Thus the economic science must, therefore, incorporate certain major or fundamental causes while leaving aside those of secondary or minor importance. By thus limiting the domain of application of the theory, Mill concludes that economic theory bears the distinguishing character of *inexactness*. Since the theoretical postulates have an inexact character, and so predictions turn to be precise *only approximately*, the method suggested by Mill considers that the laws derived by the theory can only exert their influence approximately as well and so they themselves can only be conceived as *tendencies*. This has important implications, as we shall see, for the methodological appraisal of economic theory. According to these features of Mill's methodology, the economic science develops and confirms itself by examining simple and enclosed domains that rule the causal factors. Let us for example take the case of neoclassical theory (which is the theory we are chiefly concerned with in this essay); assuming one consumption good, and given the endowment of productive factors and the alternative methods of production for a given technological knowledge, producers – who according to everyday experience will seek to minimise their costs of production[3] – will chose the more (less) labour intensive productive methods whenever the wage rate be relatively lower (higher) to the other factors' prices. From the statistical data the observer cannot get an appropriate conclusion as to, for example, the relationship (stable in time) between low wages and employment. Likewise, experience will show that wages have never been nil. On these assumptions the theorist will have *reasons* to conclude that a drop in the wage rate will lead – as a tendency – to adopting the 'more labour intensive' production techniques and, therefore, to an increment in the employment level, confirming in this way the former postulate.

[3] Note that since we are assuming free competition, this behaviour also implies the uniformity of the rates of returns on the different capital goods. This is so because when producers tend to adopt the most efficient techniques of production, they will also be deciding the different capital goods which will be involved in those techniques so that in the long period production of capital goods will be allowed and investors in those capital goods will tend to produce those most profitable (e.g. because they are initially relatively scarce). This process will give rise to a competition among investors and the tendency to produce the most profitable capital goods will make other investors to withdraw from some other capital goods' industries and getting in the former, and in this way rendering the different returns to be uniform.

From this analysis it can be derived the law of labour demand with respect to the wage rate.[4] This example can thus be understood as an application of Mill's *a priori* method for the derivation of the law of factor demand (of labour in this case) in the neoclassical theory. Mill also shed further light on the meaning of this method and its relationship with the empirical evidence:

> By the method *a priori* we mean reasoning from an assumed hypothesis; which is not a practice confined to mathematics, but is of the essence of all science which admits of general reasoning at all. To verify the hypothesis itself *a posteriori*, that is, to examine whether the facts of any actual case are in accordance with it, is no part of the business of science at all, but of the *application* of science. (Mill, 1836 [2000], p. 101, emphasis in the original)

Mill at the same time has recourse to a broad vision of the concept of experience, since the principles (or postulates, or premises) of economic theory – which, in abstract, are true – are justified by both the introspective experience[5] and the everyday experience[6] realised in turn by any cautious observer under the form of

[4] Neoclassical theory must assume that the other factor's supply (capital) is kept constant in order for the deductive reasoning we have considered to be operational.

[5] Introspective experience is a kind of intuition picked up from the broad experience that ensures us the access to the fundamental principles without questioning their character of truth. For example, within classical theory, the fact that commodities' production prices (or supply prices) will have to cover the costs of production that are necessary for their reproduction in the successive stages of the economy according to the most profitable current techniques, is a manifestation of the principle that there exists an operating force behind these phenomena that is competition. Of course, in the reality there will be firms operating with production prices both higher and lower than the necessary costs of production, but their *tendencies* in due time will confirm as approximations to the level defined by those costs.

[6] Lionel Robbins (1932 [1945], pp. 78-79, emphasis added) adheres to and deepens further the Millian methodological approach in regards of the role of the everyday experience for justifying the economic postulates: 'The propositions of economic theory, like all scientific theory, are obviously *deductions from a series of postulates*. And the chief of *these postulates are all assumptions involving in some way simple and indisputable facts of experience* relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. *These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity*: they are

tendencies that operate on the phenomenon in question. In other words, the premises are true in abstract since the theorist abstract herself from the group of secondary causes that affect the phenomenon and that in fact might interfere with the verification of the former; however this does not mean that the premise be false, in fact it is true because there exist sufficient causes to consider it as such since it is being felt persistently so that its effects can be examined as *tendencies*. That is to say, the premises are true insofar as the perturbing causes are abstracted from when analysing an economic phenomenon. These perturbing causes, in fact, do not invalidate theory but *defer* the full operation of the theoretical postulates (Mill, 1843, book III, ch. X).[7] Mill explains the character of the true premises in the abstract:

> [I]f the assumption is correct as far as it goes, and differs from the truth no otherwise than as a part differs from the whole, then the conclusions which are correctly deduced from the assumption constitute *abstract* truth; and when completed by adding or subtracting the effect of the non-calculated circumstances, they are true in the concrete, and may be applied to practice.
>
> Of this character is the science of Political Economy in the writings of its best teachers. (...) The conclusions correctly deduced from these assumptions, would be as true in the abstract as those of mathematics; and would be as near an approximation as abstract truth can ever be, to truth in the concrete. When the principles of Political Economy are to be applied to a particular case, then it is necessary to take into account all the

so much the stuff of our *everyday experience* that they have only to be stated to be recognised as obvious'.

[7] For instance, the fact that the density of a balloon when is in the atmosphere makes it possible that the balloon itself not fall at the same speed as does any other body, does not invalidate the gravitation theory but 'defers' the complete operation of the gravitational force. That is why in the theory the abstraction from the perturbing causes operating upon the phenomenon would be justified. The perturbing causes will, therefore, not prevent the tendency to fall of the bodies within a gravitational field. On this matter, Mill in his System of Logic affirms: 'All laws of causation, in consequence of their liability to be counteracted, require to be stated in words affirmative of tendencies only, and not of actual results'. (Mill, 1843, p. 445).

> individual circumstances of that case; ... which not being common to it with any large and strongly marked class of cases, have not fallen under the cognizance of the science. These circumstances have been called *disturbing causes*. And here only it is that an element of uncertainty enters into the process – an uncertainty inherent in the nature of these complex phenomena, and arising from the impossibility of being quite sure that all the circumstances of the particular case are known to us sufficiently in detail, and that our attention is not unduly diverted from any of them. This constitutes the only uncertainty of Political Economy. (Mill, 1836 [2000], pp. 105-106, emphasis in the original)

In this long quotation Mill shows that the theory proceeds by assumed premises and that true conclusions can be obtained through deduction. The theoretical derivations are thus conceived as *approximations* to the confirmed truths by reality, hence the *inexact* character of economic science. Yet, Mill is quite aware in stressing that the application of the theory to concrete phenomena requires us to take into account the perturbing causes (from which the theorist abstracts herself by having recourse to the *ceteris paribus* clause) for analysing the specific phenomenon. But even for the specific cases the theory will never be able to take into account *all the perturbing causes* in full detail as would be required by a *complete* science and this is so not because Mill (or the economic theorists Mill is thinking of) ignored the laws ruling those minor causes but basically because economic phenomena turn to be infinitely more complex than other scientific domains (*e.g.* in physics). Therefore, it will never be possible to incorporate into the theory *all* the causes affecting any phenomenon and therefore the element of uncertainty will never be overcome by any abstract science. As Marqués (2000, p. 251) has also pointed out in this connection, 'the *ceteris paribus* clauses are indispensable. (...) the inexactness [of the premises in economics] ultimately originates in the application domain of economics'. [8]

[8] Following Marqués (2000, pp. 250-252) it is important to emphasise that Mill is concerned with the 'practical economics' where, besides the perturbing causes, one has to analyse the socio-political institutions, making the job of the 'practical economist' far more difficult, although richer in content and research, due to the changing nature of the social environment. Despite this issue goes beyond the aims of

To sum up, since economics (formerly political economy) deals with too complex phenomena which change in time, then economics must limit its purview of examination to those factors that persistently exert their influence on the underlying economic motivations that aim at the increment of wealth and to the choice of the most efficient economic means to obtain it, and put aside those factors of transitorily or fleeting character. Economics, therefore, by introducing the *ceteris paribus* clause, limits its domain exclusively to such causes, so that its verification will never be exact. The theoretical result will thus represent an approximation of the *tendencies* of the empirical evolution of the phenomena under scrutiny.

3. Mill's problem according to Hausman

Mill's methodology was dominant in the discipline well until the 1930s, which can partially be understood since the redefinition of the scope and aims of economics carried out by Lionel Robbins in 1932 (see note 6 above). This redefinition entailed an important change in the traditional methodology since the place that wealth exerted in Mill's approach was now replaced with choice theory. This paramount change entailed on this discipline that it was no longer defined as the study of problems related to production, distribution and exchange, but as 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses' (Robbins, 1932 [1945], p. 16). Unlike the classical political economy, the neoclassical theory unified into their theoretical system certain subjective factors, beliefs and counterfactual premises. However, Robbins, like many other influential economists of the time (*e.g.* Knight, 1940), *reinforced* Mill's original idea according to which economic science is an *inexact science*. But, as Hausman (1998) holds in his reconstruction, towards the 1950s the Millian approach to economics methodology failed to get a good reception among the influential methodologists that otherwise would have supported it. In

the present paper, we want to point out that the methodology followed by Mill was also that pursued by the classical economists.

fact many critiques of Mill's methodology arose in those years such as Hutchinson (1938) among others. Since then, the instrumentalist approach, which had been inspired by the seminal work of Friedman (1958), the Popperian falsacionism and Lakatos' scientific research programmes have all attempted to overcome the problem of how to test neoclassical theory in order to validate it on the grounds of such proofs. However, as Hausman (1992) has argued, none of these alternatives seemed to have overcome Mill's problem. The paramount issue here is that the standard validation method (*i.e.* the hypothetical-deductive method) finds insurmountable obstacles to interpreting the assessments of the hypotheses on the grounds of the tests' results. Let us briefly see the structure of the hypothetical-deductive method:

A. A hypothesis is *formulated*.
B. A prediction is *deduced* from the hypothesis and other postulates.
C. Prediction is *tested*.
D. The hypothesis is *assessed* on the grounds of the tests' results.

Since economic laws are *inexact* in nature, and since even for specific applications it will never be possible to take into account the whole (and complex in nature) set of operating factors, then point D turns to be difficult to appraise. Owing to the infinite perturbing causes operating upon economic phenomena, if the test turns out to be positive then there will not be sufficient reasons to increase the confidence of the given hypothesis (and *vice versa*). In other words, Mill's problem raises important concerns about how the economist can interpret out of experience. According to Hausman (1992, p. 123), the alternative approaches to the Millian methodology could not overcome that problem and the same solution proposed by Mill himself 'still appears to dominate methodological practice'.

As seen in Section 2, Mill's methodology implies that the theory chooses those fundamental factors by abstracting from the rest, and so the laws (either fundamental or derived laws) turn out to be inexact and the mechanisms described by them are left subject to the *ceteris paribus* clause. In this way the theorist adopts the view that generalisations are also inexact laws independently of whether apparently adverse evidence could be found. The evidence within this methodological approach has a rational utilisation which serves as an inductive, indirect ground for accepting the derivations of the premises or postulates. The question we presently explore is: Does neoclassical theory satisfy these typical conditions of Mill's methodology? According to Hausman (1992, p. 149), it is uncontroversial that the basic proposition of what this author calls *equilibrium theory* (*i.e.* neoclassical theory of prices, output and distribution) satisfy those conditions 'in some contexts (...) and that the simplifications used can be given an analogous defense'. In order to understand what contexts this author refers to, one will have to go back to a previous work of his (Hausman, 1981), where the different *versions* of the neoclassical theory of capital and interest are properly examined. Although Hausman aptly distinguishes the *traditional versions* of the theory (*i.e.* those versions which take the specification of the capital endowment in value terms among the data or premises of the theory) from the Neo-Walrasian versions of inter-temporal or temporary general equilibrium models (in which the specification of the capital endowment among the data is in physical terms), it is yet unclear which versions he refers to when he states that 'the laws of equilibrium theory do not satisfy the excusability condition' (Hausman, 1981, p. 135), which must be satisfied for a rational use of the *ceteris paribus* condition and so for the rationalisation of inexact laws. Basically this condition consists in being able to enumerate those causal factors that, owing to their condition of having a secondary importance, must be put aside but that might at the same time have on certain occasions some influence on the phenomenon concerned, determining in this way that certain laws or generalisations could be falsified. The issue at stake is that, for Hausman, certain 'interferences' can be more known than other ones (*e.g.* the existence of *Giffen* goods) so that relying on *anomalies* would justify the ongoing use of the laws of equilibrium theory. The problem, however, is that economics theorists will generally not waste their time, if facing adverse evidence yet not previously acknowledged, in inquiring about what kind of anomaly or interference was involved in the examination of the phenomenon that would deny the laws or generalisations. At this juncture, however, clarity in

Hausman's most analytical arguments is wanting. Indeed, for this scholar 'in some cases' qualified generalisations with the *ceteris paribus* clause would satisfy the justifications of inexact laws,[9] while 'the other basic general statements of equilibrium theory cannot be regarded as qualified universal laws [*i.e*. they do not comply with the justifications of the inexact laws]' (Hausman, 1981, p. 135). These strong qualms Hausman has about the circumstances under which the laws of equilibrium theory would comply with the conditions that justify the inexact laws, are due to the unhappy though actual fact that economists are seldom concerned about examining empirical *anomalies* (Hausman 1992, p. 149).[10] The reasons for this, according to Hausman,

> lie in their *commitment to equilibrium theory as a separate science* and in the pragmatic virtues of equilibrium theory (...) In my view, questions about whether it is reasonable to regard the postulates of equilibrium theory as inexact laws should be regarded as questions ... ultimately about *the strategy of economic theorizing*. (Hausman, 1992, pp. 149-150, emphasis added).

For Hausman, then, the fact that the economics theorist goes on doing her job (theorising) as if she deals with vaguely true, inexact laws subject to the *ceteris paribus* clause is ultimately due to a pragmatic commitment to neoclassical theory that apparently would be legitimised by the normal experience of the practitioner. For Hausman (1992, p. 150), it would be thoroughly worthless to inquire about how far equilibrium theory laws satisfy the conditions that must be met in inexact science *if beforehand the use of the inexact postulates are being criticised as being illegitimate* – as has been the case in the history of economics methodology since the end of the 1930s up today. It thus seems to be the case that Hausman would be defending Mill's approach against the various attacks it received in the second half of XX century; however, several objections can be raised to Hausman's particular defence of equilibrium theory. First, we think it

[9] *Cf*. Hausman (1992, pp. 140-141).

[10] Moreover, Hausman (1981, pp. 134-135) earlier in the same work asserts that 'Economists (...) expect their generalizations to fail from time to time'.

would not be necessary so sophisticated a defence as is Hausman's to fence in the apparently *mainstream* methodology that is employed by most economics theorists (and even more by those practitioners involved in empirical research). We rather believe that in order to assess how far equilibrium theory satisfies the conditions that must be met in science *it is not necessary* to take either a critical stance of Mill's approach or the use of inexact propositions. Secondly, Hausman is not clearly enough to pinpoint the distinct versions of equilibrium theory (*i.e.* neoclassical theory) when he sets out to defend that economic theory by calling upon Mill's methodology (although in Section 5 below we shall briefly see those parts of Hausman's 1981 work in which such distinctions are made). In our opinion, therefore, a proper examination of the nature of equilibrium in the different versions of neoclassical theory can be of some help to understand why Mill's approach, in which the main economic causes are conceived as tendencies, the premises are taken as partially true, the laws are inexact by the nature of economic phenomena themselves, and the *ceteris paribus* clause must be brought in to derive vague generalisations, can be defended (and be of substantive meaning) *only* for the traditional notion of equilibrium, *i.e.* the kind of equilibrium traditional versions of neoclassical theory strove to determine. On the other hand, as will be shown below, neo-Walrasian versions of the neoclassical theory, which determine a different kind of equilibrium in contrast with the traditional one, cannot be defended by relying on Mill's approach to economics methodology. This examination will also make clear the different versions of mainstream neoclassical theory Hausman refers to in his works.

4. Neoclassical economic theory: Traditional versions vs. Neo-Walrasian versions

In this section we set out to describe the salient features of the equilibrium determined, on the one hand, by the traditional versions of neoclassical theory and, on the other, by the so-called inter-temporal and temporary general equilibrium models, also known as neo-Walrasian versions. The aim here is to show the incompatible nature of both notions of equilibrium and to argue that a defence of Mill's approach can only be held for the traditional versions of neoclassical theory.

Neoclassical economics has been traditionally (*i.e.* since the last quarter of XIX century until the 1950s decade) concerned about determining a supply-and-demand equilibrium for economies with production of heterogeneous capital goods with the following characteristics: [11]

i) Equilibrium is conceived as a centre of gravitation of market prices, so economic theory must necessarily encompass in its determination an explanation of the underlying mechanisms that allow that equilibrium position to be reached. In general, it will be necessary long periods of time in order for the variables involved (prices, output quantities) to reach an equilibrium position.

ii) The variables defining the equilibrium must be sufficiently *persistent* – that which does not mean they will remain necessarily unchanged – in such a way that in equilibrium those variables can determine the *tendency* of the evolution of their actual empirical counterparts which *are not substantially perturbed* by disequilibrium accidents that occur in the real world.

iii) Equilibrium prices of the physically heterogeneous capital goods will generally determine a uniform rate of return which entails their costs of production being covered while the physical stock composition is being determined *endogenously*.[12]

iv) The physical composition of the capital stock, which is endogenously determined, will imply that neoclassical economics needs a specification of the capital factor in value terms among the data or premises theory takes as given, as

[11] The first two characteristics are common to both the neoclassical and the classical theories; that is why equilibrium in the former approach and the 'normal position' in the latter are both here considered as traditional. On the other hand, the remaining two characteristics are proper of the neoclassical or marginalist theory.
[12] *Cf.* note 3, above.

long as the theory attempts to explain long-period equilibrium prices in terms of the *factor substitutability principle*. This entails that the outcome of the mechanisms at work (the factor substitution processes), *i.e.* the equilibrium prices as indexes of relative scarcity, turns to be plausible *only if* capital as a factor of production is specified in value terms among the data of the theory.

This *traditional* equilibrium was indeed the aim pursued by such neoclassical writers as Wicksell (1901), Böhm-Bawerk (1891), Clark (1899), Marshall (1920), and also Walras (1900). Arguably this notion of equilibrium was dominant in pure neoclassical theory until the 1950s or maybe even the 1960s (cf. Dewey, 1965). In the case of Walras, however, his economic model did *not* consider capital as factor in value terms but specified it as the vector of the physically heterogeneous capital goods – that which must be *endogenously* determined in the traditional versions. The problem in Walras's procedure is that, as Walras himself acknowledged in his fourth edition of his *Elements* (1900), the rate of return of the capital goods *is not uniform*. As is well known, neo-Walrasian models (Arrow-Debreu, 1954; Hicks, 1946) consider capital in physical terms so they must deal with the same problem that worried so much Walras. The consequence was that the kind of equilibrium these versions are able to determine has salient features that render it incompatible with the traditional notion of equilibrium. Let us now see these specific characteristics of the neo-Walrasian equilibrium.[13]

Unlike the traditional notion of equilibrium, the equilibrium derived from models that consider capital as a factor of production in physical terms will be lacking *persistence* and, in the best of the cases, will be a resting point for a short (*actually, very short*) period of time. This is one of the key problems underlying the 'Arrow-Debreu' equilibrium to which one has to add the problem of *contingence* for future markets in the inter-temporal models. As can already be perceived, these problems have important implications for giving this notion of equilibrium a useful operational meaning:

[13] The discussion that follows in this section is based on Garegnani (1990).

- Substitutability problems: the specification of capital in physical terms prevents the supply-and-demand equilibrium from determining the physical form adopted by the capital goods when the rate of return is rendered uniform through competition. This will be the case because the equilibrium in the new versions depends on the initial and arbitrary configuration of the data involving the economy's capital, which will not necessarily lead the system to determining a uniform rate of return on the several capital goods and therefore to a proper position of rest in capital goods' markets.[14] This problem arises both in the temporary (Hicks, 1946) and in the inter-temporal models (Debreu, 1959). Therefore, the *non uniformity* of the rate of return (and hence of the general rate of interest) will lead investment decisions to *permanent changes* so the *general equilibrium* thus determined will, in the best of the cases, be able to reflect a very-short-period equilibrium, which *prevents itself from being considered as the persistent position* towards the variables would *tend* for sufficient long periods of time, as in the traditional versions.

- This kind of equilibrium is *non-persistent* in nature: even under the very peculiar conditions that guarantee both uniqueness and stability of the Arrow-Debreu equilibrium (see Kirman, 1989), it prevents the theory from conceiving the necessary adjustments in the face of any disequilibrium situation since any change in the equilibrium will immediately lead to a change in the data which will in their turn shift the former position to any new position of the system that becomes however theoretically implausible to predict since nothing guarantees that this new position will persist for a long time. Under this notion of equilibrium it would turn to be illegitimate to call upon the *ceteris paribus* clause to analyse, *e.g.* which will be the tendency of a determined equilibrium good price if its production technique is changed, because this fact will stimulate change in the rest of the relevant variables

[14] For instance, the more scarce capital will yield a higher return, whereupon a rise in their production will be expected. However, a rise in their production will lead to a change in the data from which analysis started so that equilibrium will have, if at all, a very short-period (fleeting) duration.

which will at the same time make feel their effect on the former variables under examination (the price of the good concerned). In this case, since exogenous variables and equilibrium are both non persistent in nature, the *ceteris paribus* clause lacks a proper justification. Therefore, the neo-Walrasian equilibrium *fails to become a theoretical guide of real variables* and will only refer to the vector of quantities and prices that only formally clear all markets.

- Arrow and Debreu (1954) have shown that under certain conditions (convex technology and preferences) it is possible to demonstrate that there exists at least one vector of quantities and prices that formally clear all markets. Still, it is well known that these conditions are sufficient yet not necessary, so it might well be the case that there exists equilibrium even if those conditions themselves are not verified.

- Indefiniteness of equilibrium in the case of the temporary general equilibrium models based as they are on subjective expectations functions over agents' future variables.

5. Conclusions

If we take into full account the salient characteristics of both notions of equilibrium the different versions of neoclassical economics respectively determine, then we will be able to better inquire which versions of his 'equilibrium theory' Hausman's (1992, 1998) thesis refers to in his defence of mainstream economics by relying on Mill's approach. More specifically, owing to the entire incompatibility of both notions of equilibrium underlying the two versions of the neoclassical theory of general equilibrium (*i.e.* the traditional and the neo-Walrasian versions) we can conclude that Mill's solution *cannot be proposed as a defence of the equilibrium notion of the neo-Walrasian versions*, and so of the basic postulates intended to work in those specific models of modern theory. These versions, unlike the dominant versions of neoclassical theory up until the 1950s or 1960s, determine a short or very short-period equilibrium *that renders itself incompatible with the inexact theoretical implications* as those entailed in Mill's methodology. In the inter-temporal general equilibrium models, there is no room for out-of-equilibrium exchanges, making them inappropriate to be employed according to universal yet qualified economic principles conceived as *tendencies*. The neo-Walrasian equilibrium thus becomes in an instrument of little useful application for the studying of real economies, a fact that not only Mill or Ricardo but also Marshall, Walras, Clark or Wicksell would have never unquestionably accepted.

Even if we just only considered perhaps the only one positive prescription derivable from the Arrow-Debreu equilibrium, the conclusion would also be negative. Indeed, such positive prescription involves employing the equilibrium as a *benchmark*. According to neo-Walrasian scholars, like Frank Hahn (1973) for instance, the new conception of equilibrium, which abstracts itself from studying the system's tendencies in the long-period, will still retain a connection with the empirical world insofar as the new equilibrium would perform the task of showing, as clearly as possible, under which circumstances – if these are actually verified in the real world – a new Arrow-Debreu equilibrium could be expected to be found.[15] However, as argued in Section 4, because the conditions for the existence of equilibrium are sufficient yet not necessary, the idea of a *benchmark* fades away since it is not necessary that those conditions be verified for the existence of equilibrium.[16]

[15] See, *inter alia,* Hayek (1941).
[16] This problem may even turn out to be more serious. If the sufficient conditions are verified, at least one equilibrium will exist. But it might well happen that there exist other equilibria in which market clearing conditions are not verified. Particularly, it might happen that the economy is in equilibrium along with unemployment (a situation in which agents are unemployed but at the same time they do not deem optimal to push down their supplied wages). In this way we can well query: What is the *benchmark* against which one can compare, and understand, the real world? In fact, it might well happen that Arrow-Debreu conditions are not met and yet there is equilibrium in any case – or, even worse, that those conditions are met and owing to the multiplicity of equilibria the economy is found in a different equilibrium position from that postulated by Arrow-Debreu.

A further problem worth highlighting in the neo-Walrasian versions of general equilibrium is that:

> The first important point to understand about this construction [modern general equilibrium theory] is that it makes no formal or explicit *causal claims at all.* (Hahn, 1973, p. 7)

Therefore the attempt to strongly call upon Mill's solution in economics methodology, which centres around inexact laws subject to the *ceteris paribus* clause, for a defence of the modern versions of general equilibrium models, as seems to be the case in Hausman's position, is at least not fully grounded.

Still, in his 1981 work Hausman seems to acknowledge that 'real economies are not approximately in inter-temporal general equilibrium' (Hausman, 1981, p. 134) and that the theoretical hypotheses asserting otherwise are false, that which would push Hausman closer to the position held in the present essay. However,

> one can assess implications of such theoretical hypothesis indirectly. Some of the implications may be true even if the theoretical hypothesis is false. The economic agents referred to in applied general equilibrium [i.e. neo-Walrasian theory] *are the same agents referred to in microeconomic theories.* Since qualified generalizations concerning preferences and motivation are reliable in many microeconomic applications, *one has good reasons to rely on these generalizations in general equilibrium theories as well.* (Hausman, 1981, p. 134).

For this author it seems that one can rely on Mill's methodology for a defence of the neo-Walrasian versions of general equilibrium theory, since there appears to be an instrumental reason inasmuch as the elements involved in these models are to be found in the models of other versions of neoclassical theory. However, we still believe there are analytical reasons within the theoretical system of neo-Walrasian models that render Hausman's position groundless as long as one acknowledges that the kind of supply-and-demand equilibrium extraordinarily differs from the traditional role it has had in economic analysis. It is precisely this change in the notion of equilibrium in neoclassical economic theory that renders the method to be applied in economics cannot be the traditional one so that Mill's postulates on method cannot be posited for a soundly logical defence of the modern versions of mainstream economic theory.

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