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Cahiers de Recherche

Série « Décision, Rationalité, Interaction »

Cahier DRI-2015-01

Philosophy of Economics

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Cahiers de recherche de l'IHPST
Série « Décision, Rationalité, Interaction »
Sous la responsabilité scientifique de Mikaël Cozic et Philippe Mongin

Philosophy of Economics[1]

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Résumé : Le présent chapitre (à paraître dans A. Barberousse & ali., *Philosophy of Science*, OUP) est consacrée à la méthodologie de l'économie. Le fil directeur en est donné par le "problème de Mill généralisé": l'économie (positive) obéit-elle aux canons méthodologiques de la science empirique ? L'étude est organisé en deux parcours. Le premier ("Thèmes milliens") présente et discute les thèses classiques de J-S. Mill et de ses héritiers contemporains (Hausman, Cartwright). Le second ("Thèmes néo-positivistes") aborde les conceptions que l'on peut rapprocher du néo-positivisme et de l'empirisme logique. Ce parcours s'achève avec les discussions actuelles soulevées par l'économie expérimentale, l'économie comportementale et la neuroéconomie.

Mots-Clés : philosophie de l'économie, méthodologie de l'économie, modèles économiques, rationalité, équilibre, préférences révélées, lois ceteris paribus, économie expérimentale, économie comportementale

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Abstract: This chapter (to appear in A. Barberousse & ali., *Philosophy of science*, OUP) is devoted to the methodology of economics. It is organized around the "generalized Mill's problem": does (positive) economics obey the methodological standards of empirical science? The study is divided in two parts. The first part ("Millian Themes") expounds and discusses J-S. Mill's classical thesis and the one of his contemporary heirs (Hausman, Cartwright). The second part ("Neo-positivists Themes") tackles views that can be associated to neo-positivism and logical empiricism. This part ends with actual debates concerning experimental, behavioral and neuro-economics.

Keywords : philosophy of economics, methodology of economics, economic models, rationality, equilibrium, revealed preferences, ceteris paribus laws, experimental economics, behavioral economics

Classification JEL: B41.

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[1] I heartily thank Philippe Mongin for his commentary and advice on two successive versions of this chapter. I also thank Jean Baccelli, Denis Bonnay and Bernard Walliser for their comments. The content is in large part taken from the notes of my class "Philosophie de l'économie" given, in collaboration with Philippe Mongin, at the Ecole Normale Supérieure of Paris (2007-2010), and I would like to express my gratitude to the students who followed this somewhat unusual class and facilitated its improvement. Part of this work was done at the Institut d'Etudes Cognitives (Paris, ENS Ulm) with the support of the ANR-10-LABX-0087 IEC and ANR-10-IDEX-0001-02 PSL* grants.
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1. Introduction
1.1 The Philosophy of Economics

Economic science occupies a large area in our daily life: the concepts, statistics, predictions, if not the very economic theories themselves, are all transmitted to the public at large and contribute significantly towards economic and political behavior. Still, the epistemological status of economics never fails to spark debate. For example, economics will be criticized for hiding its inability to predict or advise behind sophisticated mathematical constructions[1], for basing itself on an inadequate understanding of man and society[2], or indeed for surreptitiously propagating a questionable ideology. It is certain that economic science is *singular*, particularly amongst the other social sciences from which it seems, in method, so different. This in part explains why the philosophy of economics (and notably the methodology of economics) is practically as old as the discipline itself and has punctuated its whole development. Economics raises a diverse array of philosophical questions. Three main fields can be distinguished within philosophy of economics (Hausman, 2008c). (1) Like all scientific disciplines, economics is the object of epistemological and methodological discussions; this first field is generally referred to as the *methodology of economics*. (2) Then, to the extent that among the fundamental theories of contemporary economics is to be found, in some guise or another, the assumption that economic agents behave in a rational manner, economics raises questions belonging to the *theory of action and rationality*. (3) Finally, to the extent that economics provides concepts and principles for the appraisal of institutions, states and economic processes, its questions are also part of normative philosophy and, more specifically, *moral and political philosophy*. These three fields make up the subject matter of the international journal reference point that is *Economics and Philosophy* (Cambridge UP), founded by D. Hausman and M. MacPherson in 1985. Methodology is the specific subject of the *Journal of Economic Methodology* (Routledge) created in 1994[3].

1.1 "Positive" Economics

The present chapter is dedicated to the methodology of economics, conceived of as the branch of philosophy of science dedicated to economics. Numerous economists participate in the evaluation of policies and socio-economical institutions. If we approach economics with a philosopher of science's eye it is because a part of economists' goals, attitudes and contributions seems at first glance to obey an epistemic regime similar to that of the sciences. The assumption, generally implicit, upon which most economic methodology rests is that these goals, attitudes and contributions of economics are sufficiently *separable* from its normative dimensions for us to evaluate and analyze them using the tools and criteria of philosophy of science. This assumption is closely linked to that famous and still widespread distinction between *positive economics* and *normative economics*: it is positive economics that is the philosopher of science's preferred subject. The distinction dates back to the trichotomy between "positive science", "normative science" and "art" introduced by Keynes senior (1890/1917): the first is a "body of systematized knowledge concerning what is", the second a

[1] On the history of the mathematization of economics, see Ingrao and Israel (1990) regarding the general equilibrium theory.

[2] See the article "Rational Fools" in the collection Sen (1987): "The *purely* economic man is indeed close to being a social moron. Economic theory has been much preoccupied with this rational fool decked in the glory of his *one* all-purpose preference ordering. To make room for the different concepts related to his behavior we need a more elaborate structure."

[3] See also Davis *et al.* (1998) *The Handbook of Economic Methodology*. See also Davis *et al.* (1998) *The Handbook of Economic Methodology*.

"body of systematized knowledge relating to criteria of what ought to be", while the third is a "system of rules for the attainment of a given end".

In making the assumption of separability explicit, we do not wish to suggest that questions attached to the distinction between positive and normative in economics are already solved or even easy to solve, nor that the assumption itself is self-evident. The distinction between positive and normative is, in the literature, inextricably linked to the role of economists' value judgments, and in particular to the question of *axiological neutrality*: is it possible, or is it desirable, that economists "*qua* economists" refrain from asserting value judgments (the formulation is Mongin's 2006a)? Robbins (1932/1935), who is largely responsible for introducing the distinction between facts and values into economic literature, responds in the positive to both parts of the question (chap. VI). Opposing this, others have maintained that

(T1). Economics cannot (in any of its domains) be axiologically neutral.

From this point of view, even economic contributions ordinarily qualified as "positive" would be run through with value judgments. In supporting (T1), one asserts that economics (and, generally speaking, other social sciences and humanities, see Martin and McIntyre (1994) Section VII) is run through with value judgments in a manner, or to extents, which differentiate it from the natural sciences. Thus (T1) directly threatens the working hypothesis upon which the core of the methodological literature is based.

What prompts such an argument is that economics is concerned with elements to which we spontaneously attach value judgments - think, for example, of revenue distribution or poverty. On the basis of this analysis, which is difficult to contest, a partisan of (T1) such as Myrdal (1958) can develop his position by concluding (a) that the economist's value judgments are *inevitably* expressed through (i) the selection of questions posed, (ii) the kinds of answers given and (iii) the evaluation of these answers. He can also conclude (b) that economic concepts *necessarily* carry an evaluative dimension. Conclusion (a) leads doubly to confusion. For one thing, it combines heterogeneous phenomena. The fact, for example, that the economist's values guide him in (i), the selection of the questions which he will attempt to resolve, does not imply that these questions (and the answers they call for) are not "factual". For another thing, (a) doesn't do justice to the distinction between the *assertion* of value judgments and the *influence* of value judgments on the formation and evaluation of factual judgments[4]. As for conclusion (b), it is, according to Mongin (2006a), a false generalization of a partially correct truth. The economist's conceptual toolbox contains many evaluative concepts, starting with the concept of rationality, but it also contains genuine non-evaluative concepts.

For the reasons which have just been indicated, and for others beside, the (T1) thesis is difficult to uphold. Rather, the focus of discussion is on the examination of the different components of the axiological neutrality thesis. The claim of neutrality presupposes that judgments of fact and value judgments can be easily and unambiguously distinguished. The philosophical examination of this presupposition is closely linked with contemporary debates on "fact-value entanglement" (see for example Putnam, 2002) and requires a thorough conceptual analysis of judgment categories and of their linguistic expressions. Certainly, this examination is one of the important tasks on the current philosophy of economics agenda[5] ,

[4] Hausman & McPherson (2006, chap.3) contains two examples of interference between value judgment and positive economics.

[5] Mongin (2006a) attempts something of this sort. The author pleads in favor of a position of "weak non-neutrality" according to which (i) the economist can (and must) assert value judgments and (ii) these value judgments are abundant and difficult to distinguish, in principle and in practice, from factual judgments. See also Reiss (2008) who equally exploits the idea that certain concepts simultaneously carry an evaluative and a non-

and one of the most grueling, as it demands the establishment of communication between abstract philosophical considerations and an economic tradition which has independently developed its own reflexive tradition.
Economics, sometimes referred to as the "dismal science" (Carlyle), is often poorly understood and little loved amongst philosophers. Before beginning our methodological reflections, we will very briefly present some notions of economics. Often, one looks to the 18th century when locating the birthplace of modern economic science, particularly to the works of Cantillon (*Essai sur la nature du commerce en général*, 1730), Hume, and, above all, Adam Smith (*An Inquiry into the Nature and Causes of the Wealth of Nations,* 1776). It is relatively easy to name the kind of things which have been of priority interest to economics since that time: production, consumption and the exchange of goods, revenues, currency, employment, etc. In contrast, it is more difficult to give a less extensional, more general characterization. Certain attempts nevertheless remain influential[6]. Mill (1848) discusses the idea, dominant in the 19th century, according to which

> (T2). Economics is the science of wealth.

where, by wealth, is to be understood any thing which has a use or is pleasant, and which has an exchange value. (In the same order of ideas, economics is sometimes defined as the science whose object is material welfare.) For Mill, that definition is not restrictive enough since, in principle, it includes all disciplines that deal with diverse forms of wealth and the factors which influence them (agronomics, meteorology, geology...). So Mill (1836) proposes defining economics as "the science which traces the laws of such of the phenomena of society as arise from the combined operations of mankind for the production of wealth, in so far as those phenomena are not modified by the pursuit of any other object". Economic science, from among all individual motivators, would take only the desire for wealth into account, disregarding all others. It doesn't rely on any thesis saying that this motivation is the *only* one, but its purpose is to study the social effects of this motivation without considering the other ones. We could summarize this idea as follows:

> (T3). Economics is the science of the effects of the desire for wealth considered in and of itself.

The favored objects of economics, which we have already mentioned (production, consumption and exchange of goods, etc.), are, in this perspective, phenomena of which the desire for wealth is, one supposes, the overriding factor. Often this "substantial" definition of economics is contrasted with the "formal" (and no less influential) definition given by L. Robbins (1932/1935): according to him, the science of economics owes its unity and specificity to the fact that it studies certain types of behavior, *choices* under constraint. The agent choosing has limited means at his disposal and he must distribute these across several end goals, consequently he must *sacrifice* the fulfillment of some of these goals to the benefit of others. Thus,

> (T4). "Economics is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses."

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evaluative dimension, as well as Sen's distinctions (1970, chap.5) regarding value judgments, notably "basic" and "non-basic" judgments.

[6] For a historical contextualization of the definitions of economics, consult Backhouse and Medema (2009).

This definition has been reused frequently up to the modern day, for example in Stiglitz & Walsh's manual (2000). It intrinsically links economics to the theory of choices made by economic agents. This implies that economics has a scope which, in principle, greatly surpasses the subjects it traditionally favors. (T4), latterly, has sometimes been specified with the addition of the assumption stating that, in these choice situations, agents behave rationally ("instrumental" rationality), and potentially coupled to the assumption stating that they form beliefs about their environment rationally ("cognitive" rationality).

Economics is marked by the existence, alongside a dominant or orthodox orientation, of heterodox schools. The *divisions* of dominant economics are relatively well defined. In general we distinguish (i) *macroeconomics* from (ii) *microeconomics.* (i) Macroeconomics, which in its separate form is often traced back to Keynes' *The General Theory of Employment, Interest and Money* (1936), deals with the national output, unemployment rate, inflation, balance of trade, etc. (see for example Blanchard, 2003). Thus, it deals with *economic aggregates* and is interested notably with the way in which economic policy (fiscal and monetary policy) can influence the properties of these aggregates. The macroeconomic *theory* typically proceeds by making assumptions about the relationships between these aggregates; for example, by supposing that the aggregate consumption C of a national economy is an (increasing) function of aggregated disposable income Y_D, which is equal to the total income Y from which we subtract taxes T. The hypothesis thus obtained is $C=C(Y-T)$, which, in the Keynesian theory of the "multiplier", we specify linearly: $C=c_0 + c_1\ (Y-T)$ where c_1, taken between 0 and 1, is called the marginal propensity to consume. (ii) As for microeconomics, its starting point is the behavior of economic agents (typically, firms and consumers) and, on the basis of assumptions regarding this behavior, it proposes to explain and predict the resulting collective phenomena (see for example Mas-Colell *et al.*, 1995). (iii) Sometimes an extra branch is added to these main two areas, *econometrics*. Appearing in the 1930s, it is dedicated to the statistical estimation of micro- and macroeconomic relationships - for example, the estimation, for a given type of good and population, of the manner in which that population's demand for that good varies according to its price – as well as to the testing of models coming from these two branches. On the basis of work in macroeconometrics, it leads to the economic forecasting of national aggregates and the simulation of the effects of public policies.

The methodological discussions which follow will have a dominant, though not exclusive, application in microeconomics. Microeconomics proceeds from a method characteristic of the contemporary economic approach which grants a central position to mathematical theories and models and relies primarily on two fundamental assumptions: (a1) the *rationality* of economic agents, and (a2) the *equilibrium* of the system formed by their interactions. We will clarify both of these assumptions in turn.

(a1) Economics begins with *agents* who evolve in a certain material and institutional environment and who, generally speaking, are not designated individuals but categories: the consumer (in fact, the household) who buys the goods on the markets, and the firm who produces the goods which it sells to the consumers. Economic models start with assumptions about the agents' behavior; these are supposed to *specify*, for a given class of agents and for the environment in which they evolve, the general assumption of rationality. Hence, consumer theory makes the following assumptions:

> (c1) The agent has transitive and complete preferences between various "bundles of goods", represented by vectors $x=(x_1,\ldots,x_N)$ where x_1 is the quantity of good 1, …, and x_N the quantity of good N. Transitivity and completeness are stated thus: for any x, y, z, if the agent prefers x to y and y to z, then she prefers x to z; for any x, y, she either prefers x to y or y to x.

(c2) The set of all bundles of goods between which the agent can *choose* is determined by her wealth w and by the standard prices of each good $p=(p_1,...,p_N)$: the total price of a bundle of goods must be less than or equal to w, that is to say $x_1.p_1 +\ldots+x_N.p_N \leq w$.
(c3) The consumer chooses for herself and demands the market for the basket of goods which she prefers among those which fall within the budgetary constraints defined in (c2).

Assumption (c3) determines the consumer's *demand* x=x(p,w) on the basis of her preferences and of the constraints (price and resources) that she encounters. For every good *n*, the consumer demands a quantity x_n(p,w) of that good. Assumption (c3) justifies our speaking of "optimization" or "maximization" models of behavior. Let us note that optimizing models are not found exclusively in microeconomics: contemporary macroeconomics has massive recourse to it and, through borrowing, they have spread also to other social sciences.

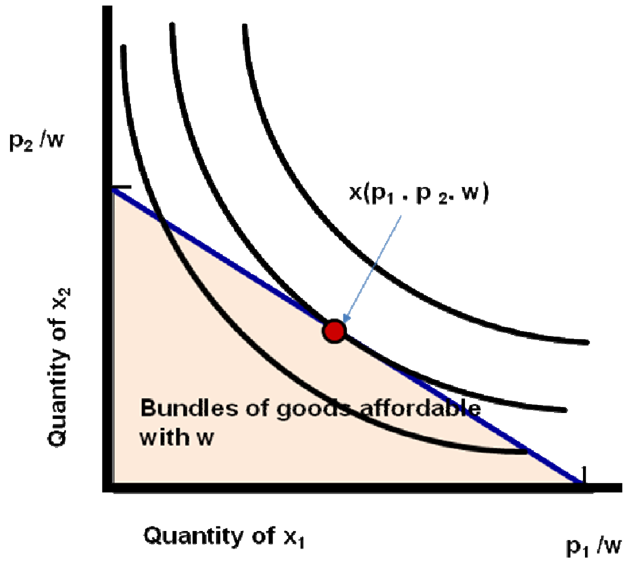

Figure 1: Graphic representation of the consumer's choice. Given a budget w and the prices p_1 and p_2, the affordable bundles of goods (the "consumption set") form the colored triangle closed by the budgetary line. So-called indifference curves, generally supposed to be convex, join up the bundles of goods between which the consumer remains indifferent. The optimal choice $x(p_1, p_2,w)$ is the meeting point of the budget line and the tangential indifference curve.

(a2) Once these assumptions about the economic agents have been made, the question of their interaction arises. At this stage, the assumption of equilibrium is introduced to assure *compatibility* between the behaviors of the different agents. For example, when we consider the market for a certain good *n* produced by certain firms and bought by certain consumers, assuming perfect competition, the concept of equilibrium means a state of equality between supply and demand for this good, this coordination taking place by means of the good's price: p_n is such that the sum of the individual demands for *n* is equal to the sum of all supplies of *n*. The *existence* of an equilibrium is not obvious, particularly when there are numerous goods and numerous agents on the market; one of the traditional fields of research in microeconomics involves the theory of general equilibrium and specifically studies the conditions for the existence of equilibria in such a situation. Models relying on the assumption of equilibrium generally remain silent in regards to the *mechanism* leading to equilibrium, and they typically deploy their predictions and explanations by determining the way in which states of equilibrium are affected from the outside. For example, they will look at the way in which the introduction of a sales tax, which alters the demand of a good, will modify the price and the equilibrium quantity of that good, and for that they compare the states of equilibrium from before and after the introduction of the tax. *Comparative statics* is the name given to the exercise which consists in studying the effect of an exogenous change on the resulting equilibrium (Samuelson, 1947, p.8; see Figure 2). Economic theory also has more and more recourse to the notions of equilibrium elaborated by game theory, which is a general theory of strategic interactions, that is, individual actions which are rationally determined relative to the actions of other agents. The fundamental notion is the *Nash equilibrium*: the actions of each individual are such that it is in no one's interest to change their action unilaterally; in other words, other individuals' actions being determined, one's own action is optimal.

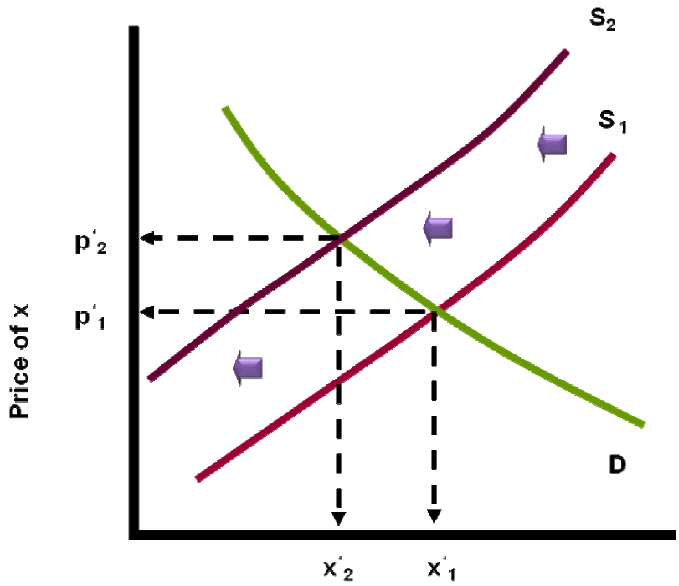

Figure 2: Graphic representation of the market equilibrium for good x. Curve D represents the demand aggregate. Curve S_1 represents the initial supply aggregate. The intersection (p^*_1, x^*_1) of the two curves is the point of equilibrium. If, following the increase in price of some factor of production for example, the supply curve moves to S_2, a new equilibrium obtains (p^*_2, x^*_2). Thus the quantity exchanged decreases while the price increases.

The relationship between micro- and macroeconomics is itself the subject of important methodological discussions that we will not elaborate on in this chapter (see Walliser and Prou, 1988, chap.6). Many of these revolve around the question of the *microfoundation* of the macroeconomy, i.e. around the question of knowing whether it is possible or desirable to *reduce* macroeconomics to microeconomics (see in particular, Malinvaud, 1991, and Hoover, 2001b, chap.3). That question is partly related to the question of methodological individualism in social sciences (see the chapters "Philosophy of the social sciences" and "Reduction and emergence" of the present volume).

1.2 The methodology of economics

Modern development of economics has been constantly accompanied by reflections regarding the discipline's method, object and scope[7]. Methodology today is largely the concern of specialists and the impact of epistemological assertions on economic research is less than it may have been in past decades. Economists are not always kind to professional "methodologists" (Samuelson, 1992, p.240: "Those who can, do science; those who can't, prattle about its methodology"); those who did take an interest, sometimes actively, in methodology, left themselves open to similar kindness in return, (Hausman, 1992b: "If one read only their methodologies, one would have a hard time understanding how Milton Friedman and Paul Samuelson could possibly have won Nobel Prizes"). Methodological discussions still have important current relevance, as witnessed by the lively debates concerning so-called behavioral economics and neuroeconomics (see subsection 7.3).

It is difficult to present economic methodology in an analytic fashion, markedly differentiating the principal questions of contention: indeed, these questions are very closely linked to each other. For that reason we will follow the dominant trend, which consists of approaching the field by way of the principal *doctrines* which animate it. Nevertheless, we will attempt to work out a question or preoccupation common to the area of methodology. This question dates back to Mill who, according to Hausman (1989), posed himself this problem: how can an empiricist methodology be reconciled with the manner in which economic science is built and practiced? In particular, how can empiricism be reconciled with the apparent falsity of the assumptions of economic theories and the small importance which *seems* to be accorded to the confrontation between the theories and the empirical data? *Mill's problem* ends up being more general even than the author's own empiricism, spreading beyond empiricism as a philosophical position: when we ponder on the realism of economic assumptions, on economists' sensitivity to empirical data or, further still, on the progress of economics, it is often because we wonder whether economics obeys the methodological standards of an empirical science - supposing that such standards exist. This *generalized problem* of Mill's is at the very heart of a large number of the reflexive discussions on economics. It explains the particular interest, in philosophy of economics, for some of the "great" questions of general philosophy of science, like the demarcation between the sciences and the non-sciences, the relationship between theory and experience, the nature of scientific progress, etc.

The formulation of Mill's generalized problem could lead one to believe, incorrectly, that economic methodology consists of comparing a would-be unified and homogeneous discipline, economics, to methodological standards which would be the object of a consensus and which would characterize what it is to take on a field of study scientifically. This is clearly not the case. For one thing, though economics does perhaps have a stronger discipline identity than other social sciences, it is still marked by significant disagreements and by the existence, alongside one dominant or orthodox orientation, of heterodox schools, Marxist or institutionalist, for example. Secondly, as could have been expected, the degree of consensus is still far lower on the side of philosophy of science. The hope, legitimately harbored during the middle of the twentieth century, of providing simple, consensual and universal criteria for scientific methodology or for scientific progress has been largely abandoned today. Resulting from this evolution, analyses of Mill's problem can vary noticeably from one methodologist to another, and broad intuitions regarding the nature of scientific method are taken on with more flexibility and less certitude than they may have been in the past.

We won't deal with these two complications symmetrically: we will give room for expression to a diversity of epistemological points of view, but, as is often the case in methodological literature, we will concentrate first and foremost on what we have called dominant or

[7]Elements of the history of economic methodology can be found in Blaug (1980/1992, section II) and Hausman (1992a).

orthodox economics (even if we do tackle distinct programs of research at the end of the chapter).
Two lines of approach will be pursued: the first ("Millian themes") will address those positions we can relate to the ideas of J.S. Mill, pioneer of economic methodology and representative of the English empiricism of the 19th century. We will begin with Mill's famous deductive method and with his Anglo-Saxon successors (section 2) to discuss their current ramifications and, in particular, current neo-Millian views (sections 3 and 4). In the second approach ("Neo-positive themes"), we will broach the methodological views close to certain epistemological trends, in particular neo-positivism and logical empiricism, which held center stage in the philosophical scene between the 1920s and 1950s, and which, in economic methodology, ousted the Millian tradition during the 1930s. Section 5 is dedicated to the contributions of P. Samuelson and to refutationist ideas, section 6 to M. Friedman's famous theses. We complete our review by reviewing contemporary discussions on experimental economics, behavioral economics and neuroeconomics (section 7).

Section I: Millian themes

2 Mill's deductivism

2.1 The deductive method

The *deductive* conception of economics has its origin in the methodological writings of J.S. Mill (1836, 1843), and is later found (with some differences of greater or lesser importance) in Cairnes (1857, 1875)[8], in J.N. Keynes[9] (1890/1917) - even if Keynes often presents himself as seeking to reconcile deductivists with their adversaries - and maybe even in L. Robbins (1932/1935). We present it in detail not only because it dominated economic methodology for close to a century, but also because certain modern economic philosophers, like D. Hausman (1992a), claim considerable allegiance to it.
Mill (1836) distinguishes two principal methods in empirical sciences: the *a posteriori* (or inductive) method, and the *a priori* (or deductive) method. The first essentially consists in detecting regularities in the empirical data and then proceeding by generalizing inference[10]. The data in question *directly* concerns the proposition to be established; in the simplest case, if the proposition has a universal conditional form ("All *P* are *Q*"), these findings could be positive instances of this (an entity or an example which is simultaneously *P* and *Q*). The second method consists in reasoning *deductively* on the basis of prior assumptions. The procedure breaks down into three steps (1843, III, XI):

> (s1) The assumptions are firstly formulated and established *inductively*.
> (s2) The consequences of these assumptions are extracted by deduction.
> (s3) These consequences are compared to the available empirical data (see above).

One fact must be insisted upon; the assumptions forming the starting point of the reasoning are themselves established by generalizing inference (or else deducted from other assumptions, these having been established by generalizing inference). The term "*a priori*", which, since Kant, more frequently refers to the property of certain propositions to be justifiable independently of experience, can thus be misleading. The *a priori* method is, in

[8] On the differences between Mill and Cairnes, see Hands (2001), p. 27.
[9] John Neville Keynes (1852–1949) is the father of John Maynard Keynes (1883-1946).
[10] Which, in philosophical jargon, is also called "enumerative induction". See also Cairnes (1857/1875), p. 41.

reality, an *indirect* method of induction. In contrast with the *a posteriori* method, the target propositions are not established by generalizing inference. But induction plays an indirect role since it is by this means that are established the assumptions on the basis of which the propositions of interest are deduced. In the case we are occupied with, the assumptions are the fundamental propositions of economic science. Mill is quite evasive about their exact content. He mainly evokes the psychological law according to which a greater gain is preferable to a lesser gain (1843, VI, IX, §3, p.901), all the while affirming that economics considers mankind as "occupied solely in acquiring and consuming wealth" (1836, p.382). Alongside other commentators from classical economics, Cairnes mentions the efficient search for individual advantage as well as the law of diminishing marginal return to the soil (1857/75, p. 41). With Robbins, a reference not from classical but neo-classical economics, the first fundamental assumption is that agents are able to arrange their options according to their preferences; the second is the law of diminishing returns, which could, he says, be reduced to the assumption stating that there exists more than one factor of production (on the justifications for the law of diminishing returns, consult Mongin (2007) who criticizes them all as being inaccurate).

2.2 Why resort to the deductive method?

The deductive method is not unique to economics - according to Mill, it is this method which we employ, for example, in mechanics. It imposes itself on the economist because

(T5). The *a posteriori* method is not viable in the economic domain[11].

(T5) means that induction cannot directly establish the propositions targeted by economics. The inapplicability of the *a posteriori* method comes from two fundamental characteristics of economics: it is a *non-experimental* science of *complex* phenomena. The empirical data of economics emerges essentially from *observation* and not from experimentation[12]. For the deductivists, such data does not generally allow one to proceed inductively (or *a posteriori*) because of the intrinsic *complexity* of the phenomena in question[13] : too many factors interact simultaneously for us to ever hope to directly extract any robust regularities or causal relations[14]. If, for example, we wanted to establish that restrictive and prohibitive commercial legislation influenced national wealth, what would be needed in order to apply what Mill calls the "Method of Difference" would be to find two nations which were identical in everything except their commercial legislation[15]. If we wish to proceed via direct induction, only experimentation is capable of untangling the complexity of the economic phenomena, but this is excluded[16]. So we cannot hope to justify economic propositions *a posteriori*.
For disciples of the deductivist approach, the fundamental assumptions are established, inductively, by introspection (Mill, 1836, p.56) or by observation raised to the level of induction. These are the "indisputable facts of experience"[17] which require no supplementary

[11] Mill (1836), p.50; Keynes (1890/1917), p. 13
[12] Mill (1836), p. 51 ; Keynes (1890/1917), p. 85-8 ; Robbins (1932/1945), p. 74
[13] Cairnes (1857/75), p. 43; Keynes (1890/1917), pp. 97-8.
[14] Mill (1836), p. 55 ; Keynes (1890/1917), p. 98
[15] Mill (1843), VI, VII, §3.
[16] Mill (1843), VI, VII, § 2 and Cairnes (1857/75), pp. 43-4. One is struck by the similarity between these Millian stances and those of contemporary economists. See, for example, Malinvaud (1991), pp. 346-7.
[17] Robbins (1932/1935), p. 78. It is doubtful, on this point, that Robbins' position be equatable to those of Mill's or Cairnes': von Mises' *apriorism* wields considerable influence over Robbins. Robbins (1938) brings up an interesting perspective: he seems to want to preserve a kind of neutrality between apriorism and empiricism. For

empirical investigation[18]. Thus, for Cairns, contrary to the physicist, "the economist starts with a knowledge of ultimate causes" (1857/75, p.50). So confidence in economic theory results from the confidence its assumptions inspire, a particular kind of confidence, as it is put in the following characteristically deductivist assertion:

> (T6). The propositions of economic theory are true only hypothetically or abstractly, or else in the absence of disturbing causes[19], or, finally, *ceteris paribus*[20].

The propositions of economic theory are not true *simpliciter*. Here we have something which contrasts, in appearance at least, with the preceding statements about the obviousness of economic assumptions. There are two ways to resolve that tension. (i) The first consists in restricting (T6) to the *conclusions* of economic theory, which Cairns does[21]. The objection which we can formulate in this case is that, if the reasoning were deductively correct and if the premises were true *simpliciter,* then, equally, the conclusions would be too. Cairnes, however, maintains that this may not be the case[22] since the premises, even if they are true, are nevertheless *incomplete*: they don't describe *all* the factors which could affect the phenomena in question. To develop an analogy with mechanics: the parabolic movement of a body allows itself to be "deduced" from the laws of movement and gravitation, which are true; however, the movements of bodies do not necessarily trace a parabola - frictions with the air, for example, disturb the trajectory. In this way we could pass deductively from propositions which are true *simpliciter* to others which are not. The analogy is not very convincing: to deduce the parabolic form of the trajectory, the assumption that gravity is the *only* force acting must be made, but this last assumption is false. When approaching the issue in this way, the deductivists tend to mix the logico-semantic and the causal registers, the latter being essentially thought of in analogy with *forces* and their combination through vector addition in classical mechanics. Mill himself (cited by Cartwright, 1989, p. 173) seems to see reasoning in mechanics as causal and *non-monotonic* reasoning: what lends itself to being inferred from an assumption will not necessarily lend itself to being inferred from this same assumption joined to another. Rather than saying that assumptions that are true *simpliciter* can result in false conclusions, they should say that premises that correctly describe some factors but assume that no other is present can be false (when some of these other factors are in fact present). (ii) The second way to resolve the tension, the only tenable one in our view, consists of applying (T6) for *all* economic propositions, including the assumptions. According to this interpretation, economics is a thoroughly *inexact* science. Whichever theoretical option they choose, deductivists nevertheless agree on the fact that, hypothetical or not, the premises retained in economic theory are not arbitrary[23]. First of all, they describe authentic factors[24] which influence economic phenomena. Secondly, these selected factors must be among the most important[25].

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him, the most important is that the two grant a very high level of certitude to the fundamental propositions of economics.

[18] Robbins (1932/1935), p. 79 ; Keynes (1890/1917), p. 13.

[19] Keynes (1890/1917), p. 14.

[20] Keynes (1890/1917), p. 101.

[21] Cairnes (1857/75), p. 39: "...the conclusions of economic policy do not necessarily represent real events."

[22] Cairnes (1857/75), pp. 38 and sq.

[23] Even so, see Mill (1836), p. 46.

[24] Keynes (1890/1917, p.104) speaks of *verae causae*.

[25] Cairnes, p. 31 speaks of "*leading causes"*. See Mill (1836), p. 38, Mill (1843), p. 901 and Keynes (1890/1917) p. 60.

2.3 Theory and experience according to the deductive method

From here on let us concentrate on the most contested step in the deductive method, step (s3), the comparison between the conclusions of the theory and the empirical data. Divergences between the two must be expected: even if the premises of economic reasoning deal with the *principal* causes of economic phenomena, they do not mention *all* the causes which can perceptibly influence them. For example, deductivists mention customs and moral or religious convictions as factors which could interfere with the desire for wealth. So the question is the following: which attitude must be adopted when the conclusions resulting from theory diverge with the empirical data? The deductivist replies that comparison with experience lets us know whether we have omitted any important disturbing causes[26].

The nature of this reply varies from author to author. For Mill, taking account of these "disturbing causes" belongs to the domain of applied economics and not to economics *stricto sensu*. For Keynes, on the other hand, disregarding all factors apart from the desire for wealth allows us to deliver a "first approximation" which can at times be first rate. But "neither the conception of the economic man nor any other abstraction can suffice as an adequate basis upon which to construct the whole science of economics"[27]. To resolve many economic questions, the simplistic *homo economicus* theory must be enriched and opened to the other social sciences. This difference has perhaps limited epistemological importance in comparison to the strong and debatable assertions which unite deductivists:

> (T7). The divergences between empirical data and economic theory should not incite rejection of the fundamental assumptions.
> (T8). Every proposition that is false *in concreto* can be transformed into a true proposition which does take the disturbing factors omitted by the first analysis into account.[28]

(T7) seems justified by the fact that the fundamental assumptions would *already* be justified by induction. (T8) is difficult to enlighten as it mixes the logico-semantic and causal registers. It is once again mechanics, more precisely the *vector sum of forces*, which serves as a model: if a force was omitted from the initial description, that description must be rectifiable by adding the omitted force to those that were mentioned. In contrast, for Mill, chemical phenomena do not obey this composition of causal factors. Economic phenomena are thus more related to mechanical phenomena than to chemical phenomena: they are phenomena where the "Composition of Causes" applies (Mill, 1843, III, VI, §1), this being a generalization of the composition of forces in mechanics. Neither of these two positions is self-evident. (T7) seems excessively conservative from the perspective of contemporary epistemology as it definitively immunizes the basic economic assumptions against empirical questioning. As for (T8), it is clearly weakened by the absence of analogy between mechanics and the social sciences, where there is not really any principle of composition of causes similar to the vector sum of forces. The justification that Mill gives for this in asserting that "human beings in society have no properties but those which are derived from, and may be resolved into, the laws of the nature of individual man" (1843, VI, VII, §1) is too rushed and imprecise to be efficient (see Hausman, 2001). Even if a law like the "Composition of Causes" did exist for social phenomena, economics is not destined to use it systematically: it is occupied with phenomena where the causal factors it traditionally retains among its

[26] Mill (1836), p. 64.
[27] Keynes (1890/1917), p. 61.
[28] Mill (1836), p. 47.

assumptions (like the desire for wealth) are dominant. Contrary to other scientific domains, the deductive method in economics is *partial*[29].

3 Economics as an inexact and separate science

Among the works which have dominated economic methodology for the last twenty years or so, *The Inexact and Separate Science of Economics* (1992a) by Daniel Hausman features incontestably. The author formulates and defends a neo-Millian view of contemporary microeconomics which he calls the "theory of equilibrium"[30]. The "theory of equilibrium" hangs on a handful of fundamental laws: the laws of consumer theory, theory of the firm, and also on the assertion that markets arrive rapidly at a situation of equilibrium (where the prices of goods are such that the aggregated supplies and demands balance out). For Hausman, the fundamental assumptions of that theory (for example, the transitivity of consumer preferences, or the maximization of profit for firms) are *inexact* laws. The economic analysis is developed essentially through exploration of their consequences and confidence in the implications of the theory comes more from the confidence placed in the assumptions than from empirical testing.
Hausman's exact position is relatively complex, notably because it combines elements of Millian exegesis, descriptive methodology of contemporary economics, and also normative methodology. We can describe it by indicating the main ideas he identifies in Mill: the position stating that economic laws are inexact; his defense of the deductive method; and the idea that economics is and must be "separate" from the other social and human sciences. Hausman's idea is made up of three components which we will examine one by one: an *enrichment* of the inexactness of economic laws thesis, a *revision* of the deductive method, and a *rejection* of the separation thesis.

3.1 Enrichment of the inexactness position

The assumptions of (micro-)economic theory do not, according to Hausman, have the same status as the fundamental laws of nature: rather they are inexact laws[31]. Thus, he proposes a semantic *and* epistemological analysis of inexactness which breaks down into (1) an analysis of truth conditions and (2) the justification conditions of the *ceteris paribus* propositions. In his view, an economic assumption like the transitivity of consumer preferences must be understood as "*ceteris paribus*, consumer preferences are transitive" (1992a, chap. 8).
(1). Let us consider propositions of the form

Ceteris paribus, all *P* are *Q*.

A semantics for propositions of this form must authorize exceptions to the proposition which is within the scope of the clause: it must be possible for an entity to be *P* without being *Q*, and

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[29] Hausman (1992a), pp. 145-6. Note that among the assumptions on behavior that are made in contemporary economics, one may distinguish between, on the one hand, those pertaining to agent's rationality (e.g., the transitivity of preferences), which are studied by decision theory, and, on the other hand, those making specific hypothesis on the content of preferences (e.g., that agents prefer larger commodity bundles or larger sums of money). In conventional models, this second set of assumptions typically captures the idea that agents are self-interested. Depending on one's view of the respective entrenchment of these two sets of assumptions in economics, one obtains distinct senses in which economics can be seen as partial. In the strongest sense, it studies the influence of rationality *and* self-interest.
[30] The place in economics of what Hausman calls the "theory of equilibrium" is the subject of an informed examination in Backhouse (1998), chap. 17. Incidentally, the article also lets us position the two fundamental assumptions (rationality and equilibrium) exposed in subsection 1.2.
[31] This does not preclude that, in certain branches of the natural sciences, including physics, laws just as inexact as those in economics can be found.

for "*ceteris paribus*, all *P* are *Q*" to be nevertheless true. The natural idea, taken up by Hausman, is that the *ceteris paribus* clause expresses a domain restrictor (implicit and context-dependent). Let us suppose that we can explicitly formulate that restriction with the predicate *S*: then "*ceteris paribus*, all *P* are *Q*" is true iff

All *P* and *S* are *Q*.

is true. The compatibility of this analysis with the deductive method is not self-evident, as Hausman in essence remarks: if the restrictors can vary depending on which proposition they are applied to, then the application of deductive reasoning to a set of propositions does not lend itself to easy interpretation; in other words, the *logic* becomes singularly complicated[32]. Why, in these conditions, should one interpret economic proposition by adding implicit *ceteris paribus* clauses? Hausman's reply hangs, in large part, on what we can call his *nomocentrism*:

> "Theorists use basic economic 'laws' to try to explain economic phenomena. They cannot regard them as mere assumptions, but must take them as expressing some truth, however rough. Otherwise, their attempts to use them to explain economic phenomena would be incomprehensible" (p.139; see also Hausman, 2009).

In other words, laws are required to account for the *explicative* ambitions of economics.

(2). Let us now move on to the *epistemology* of the *ceteris paribus* propositions: in what conditions are we justified in believing of a *ceteris paribus* proposition that it is a law? This is not a trivial matter: for some, these clauses are suspect as they allow one to indefinitely keep falsifiers of the propositions they concern at bay. If we consider a proposition like "*ceteris paribus p*", Hausman puts forward the following four necessary conditions of justification:

(j-i) proposition *p* (unmodified by the clause) must be *lawlike*. In philosophical literature, the term *lawlike* (or *nomological*) is used to speak of a proposition which has all the characteristics of a law, except perhaps that of being true[33]. This condition is natural when we take into account the preceding semantic analysis and the commonly envisaged criteria for characterizing something as lawlike.

(j-ii) *p* must be *reliable,* i.e. largely true in its field of application once certain precise interferences have been taken into account.

(j-iii) proposition *p* must be *refinable,* i.e. we must be able to add qualifications to it which make it more reliable, or reliable in a wider domain.

(j-iv) the proposition must be *excusable,* i.e. the major interferences which allow us to explain the instances when *p* is false must be known.

According to Hausman, the propositions which make up the "theory of equilibrium" are candidates for the title of inexact law. And certain amongst them, like the assumption of diminishing marginal rates of substitution in consumer theory, or that of diminishing returns in the theory of the firm, would be *good* candidates[34]. It is useful to point out that this is not the case for all propositions to be found in theories of the domain. For example, the proposition stating that goods are infinitely divisible is not lawlike. Hausman calls these

[32] For example, if the restrictions vary from one proposition to another, then we cannot conclude, in total generality, "*ceteris paribus*, all *P* are *R*" from "*ceteris paribus*, all *P* are *Q*" and "*ceteris paribus*, all *Q* are *R*". Indeed, it is incorrect to conclude "All *P* which are *S''* are *R*" from "All *P* which are *S'* are *Q*" and "All *Q* which are *S* are *R*".

[33] Hence, all laws are nomological, and every true nomological proposition is a law.

[34] Let us remember that, in contemporary microeconomic theory, the first of these has given way to the assumption of convexity of consumer preferences, and the second to the assumption of convexity of production sets. See, for example, Mas-Colell et al. (1995), pp. 44 and 133

unlawlike falsities *simplifications* and proposes a series of acceptance conditions for them analogous to, but distinct from, (j-i)-(j-iv)[35].

3.2 Revision of the deductive method

According to Hausman, if economists do subscribe to a method, it is not exactly Mill's one: they don't accept the (T7) thesis, according to which divergences between empirical data and economic theory should never incite the rejection of economic theory (nor any part of it). In other words, economists, perhaps despite appearances, distance themselves from the dogmatism of the original deductive method. It is nevertheless true that they are reticent, when faced with empirical anomalies, to question their theories. Yet they often have good reason not to. On the one hand, the essential part of their empirical data comes from uncontrolled observation and is not easily compared to the *ceteris paribus* propositions. On the other hand, economic theory, to make empirical predictions, resorts to numerous auxiliary assumptions, assumptions in which economists often have far less confidence than they do in the fundamental assumptions, and that they are thus more inclined to reject. In these conditions, in the case of a conflict with empirical data, it is not unreasonable to blame one or the other of the auxiliary assumptions rather than one of the fundamental ones. This situation makes fundamental assumptions poorly falsifiable from a methodological point of view. Hausman proposes a revision of the deductive method which is supposed to be at once methodologically acceptable and compatible with economists' practices (1992a), p. 222):

> (s1') Formulate credible and convenient *ceteris paribus* generalizations concerning the operation of the relevant causal factors.
> (s2') Deduce from these generalizations, initial conditions, and simplifications, etc., predictions concerning the relevant economic phenomena.
> (s3') Test the predictions.
> (s4') If the predictions are correct, consider the whole as confirmed[36]. Otherwise, attempt to explain the failure by comparing the assumptions based on explanatory success, empirical progress and pragmatic virtues.

3.3 Rejection of the separation thesis

Are we to conclude from what precedes that, in economics, all's well that methodologically ends well? Hausman replies in the negative. Indeed, in his view it is another important component of economists' practice that is at fault; the idea that economics should be conceived of as a *separate science*. According to that conception, (1) economics is defined by the causal factors it accounts for, (2) its domain is the one where its causal factors predominate, (3) the laws of these factors are already reasonably well known, and (4) it accounts for its domain in an inexact yet unified and complete fashion (1992a, pp. 90-1). From this point of view, economics would be a unified and general science of economic phenomena which borrows nothing from other disciplines.

Some important methodological consequences follow on from the conception of economics as a separate science: among them, the idea that particular intervening assumptions are only legitimate if these assumptions (in the best of cases) derive from fundamental assumptions, or

[35] Hausman (1981a), p. 142.

[36] This part of (s4') is a reflection of the Millian inspiration: it is the initial confidence in the fundamental assumptions which justifies considering the whole as confirmed. A liberal Popperian, who would accept the *ceteris paribus* clauses, would further demand independent tests. We are indebted to Philippe Mongin for this remark.

are at least *compatible* with them. If this is not the case, then these assumptions are readily considered as *ad hoc*. This, according to Hausman, is what drives economists to a form of dogmatism. The statement is justified notably by the study of economists' reaction to the famous phenomenon of *preference reversal*. At the beginning of the 1970s, the psychologists Slovic and Lichtenstein conducted the following experiment: when two subjects are asked to directly give their preferences between two monetary lotteries *H* and *L* (for example: *H* gives a 99 in 100 chance of winning €4 and a 1 in 100 chance of losing €1; *L* gives a 1 in 3 chance of winning €16 but a 2 in 3 chance of losing €2), the majority states a preference for *H* over *L*. Yet when the subjects are asked to assign *minimum selling prices*, the majority assigns a higher minimum selling price to *L* than to *H!*[37] Hausman's interest is in economists' reaction to preference reversal. This reaction was to quite quickly admit that this was a case of authentic empirical anomaly for preference theory, though without going so far as to question the theory's central role. The alleged reason for this hangs on an attachment to economics as a separate science. Grether & Plott (1979), for example, assert that, "No alternative theory currently available appears to be capable of covering the same extremely broad range of phenomena." Hausman judges this assertion to be characteristic of partisans of the separation thesis.

In summing up what precedes, we can compare the perspectives of Mill, economists (in Hausman's view), and of Hausman himself concerning Mill's three principal ideas about economic methodology: (a) all agree on the inexact nature of economic laws; (b) Hausman and the economists accept a revised version of the deductive method which authorizes the modification of the fundamental assumptions relative to the empirical data; (c) Mill and the economists are attached to economics as a separate science, something Hausman criticizes. There seems to be a certain tension in Hausman's attempt to defend the economists' methodological practice while also criticizing their conception of economics as a separate science. Hausman (1997) recognizes this tension and delimits the precise part of the economists' methodological practice with which he agrees: the usual empirical data has connections too distant from economic theory for them to maintain decisive relations of confirmation or disconfirmation.

3.4 Discussion

The importance Hausman accords to *ceteris paribus* propositions found echoes in philosophy of the special sciences during the 1990s and 2000s[38]. His position, and other analogous positions, were discussed and contested. Before getting to these criticisms, it is indispensable to point out that the philosophers of science participating in these discussions *interpret* the propositions of some special science as *ceteris paribus* propositions without the representatives of the discipline having openly affirmed the corresponding *ceteris paribus* statements beforehand. Economics is special: *ceteris paribus* clauses have been explicitly used there for a long time, going back at least to Petty's *Treatise of Taxes and Contributions* (1662, quoted by Reutlinger & al. 2014). It was greatly popularized by A. Marshall. In his *Principles of Political Economy* (1890/1920, see notably V, 5, §2), he makes use of them to signify that, in studying a phenomenon, certain factors can be deliberately put aside. Marshall is interested, for example, in the demand function $x_n(p_n)$ for a particular good *n*, this function being constructed to depend only on the price p_n of this good, as it occurs on the market[39]. But an individual's demand obviously depends on more factors than just the price of the good in question, be it on his resources, on the price of other goods, etc. These supplementary factors

[37] We recommend the collection Lichtenstein & Slovic (2006) for more on this fascinating phenomenon.
[38] For an introduction to this literature, see Reutlinger & al. (2014).
[39] See Figure 2, *infra*.

are thus considered to be fixed while we authorize the variation of the price *n*. Economists' use of *ceteris paribus* clauses has itself been the subject of methodological discussions (see Hausman, 1992b, chap.11), notably because, along with those just mentioned, supposedly exogenous variables (like resources) are mixed with supposedly endogenous variables (the price of other goods to *n*). A more general theory of demand than Marshall's would take into account the interdependence of prices by contradicting the assumptions stating that the prices of the other goods do not vary.

However, we leave these questions aside to come back to the interpretation of economic propositions as being implicit *ceteris paribus* propositions. Woodward (2002) criticizes this kind of view for its latent nomocentrism. He rejects the idea that laws are necessary to the scientific legitimacy of a discipline or to its explanatory capacities. Following in the steps of Earman & Roberts (1999), he also criticizes the analyses of the truth conditions of *ceteris paribus* propositions such as the one proposed by Hausman. These analyses would be at risk of trivialization: if the system studied is determinist, then it must always be possible to find a condition *S* which is by itself nomologically sufficient for *Q* the conditions expressed by *S,* and therefore such that "All *P* and *S* are *Q*" is true. Refining the analysis by demanding that neither *P* nor *S* be individually nomologically sufficient for *Q* may lead to consequences which are no less counter-intuitive.

The possibility of confirming or disconfirming *ceteris paribus* propositions, which Hausman defends and analyzes with the conditions (j-i)-(j-iv), is often challenged, for example by Earman & Roberts (1999) and Earman, Roberts & Smith (2002). They assert, in essence, that when conditions like (j-ii) and (j-iv) are satisfied, we learn the nature and the limits of a statistical relationship without there necessarily being convincing reasons to infer the existence of a law. Besides this, if Hausman is conscious of the "danger of trivialization" present in the conditions (j-ii) and (j-iv), an abusive use of which could lead to the justification of "laws" which clearly should not be laws, this danger could be judged to be too great. This is particularly true of condition (j-iv) which demands an explanation for the counter-examples only *a posteriori*.

Revisiting the major arguments of his 1992 publication, Hausman (2009) considers that his work may have been marked by the potentially exaggerated role he accorded to laws. It seemed to him that the primary task of philosophy of economics was to understand if, and in what sense, the fundamental propositions of economic theory could be analyzed as laws. Influenced by the recent work of J. Woodward and others on causality[40], Hausman now intends to organize his methodological contributions on the basis of this latter concept: it is preferable to conceive of economic generalizations as causal claims rather than as inexact laws.

3.5 *Ceteris paribus* clauses, folk psychology and progress in economics

Before moving on to other works inspired by Mill but which start by placing causality and causal powers at the center of their analysis, it is worth pausing on the ideas of A. Rosenberg. Last in a long series of publications dedicated to economics, *Economics - Mathematical Politics or Science of Diminishing Returns* (1992) accepts both the Millian position of inexactness and its contemporary re-reading, by Hausman, in terms of implicit *ceteris paribus* clauses. We will nevertheless see that, in other regards, he paints quite a different picture of economic science.

Rosenberg's first contribution to philosophy of economics was his book *Microeconomic Laws. A Philosophical Analysis* (1976). It speaks about the nature of the general propositions

[40] See the chapters "Explanation" and "Causality" of the present volume.

of microeconomics[41], and, more precisely, discusses whether or not those propositions that deal with agent behavior can be assimilated to the laws (or nomological propositions) of the natural sciences. Rosenberg's central argument, novel at the time, is that the concepts brought into effect by microeconomic generalities, and the explanatory role that these can play, bring them closely alongside folk psychology, that is, the way in which we habitually explain actions in terms of beliefs and desires[42]. As philosophers of action have highlighted, one of the essential characteristics of our common explanation of action is that the *explanans* appear as a *reason* to undertake the *explanandum.* Going against a tradition often associated with the writings of Wittgenstein and once influential in philosophy of action and the social sciences, Rosenberg maintains that this characteristic does not prevent microeconomic propositions from being causal. Thus, he subscribes to the position, known as causalist and notably maintained by D. Davidson (1980), according to which the reasons for an action can be its causes (Rosenberg, 1975, sec.II; 1976, chap.4 and 5). Another important argument of Rosenberg's (1976) hangs on the assertion that microeconomic propositions are not only causal but also *nomological*. Indeed, they satisfy the generality, the regularity and the necessity which are supposed to be the particularity of laws. According to Rosenberg's view, "there [is] no conceptual obstacle to microeconomic theory's status as a body of contingent laws about choice behavior, its causes and consequences" (1992, p. xiii).
Between the end of the 1970s and the beginning of the 1990s, Rosenberg developed supplementary theses which present that conclusion in a less favorable light:

(T9). Economics does not manifest significant predictive progress in the long term.

Rosenberg considers it an epistemological empiricist commitment that a scientific discipline *must* manifest predictive progress in the long term (1992, p.18), without which its "cognitive status" as an empirical science becomes problematic. He defends this demand and thinks it is accepted by many economists. But (T9) asserts that it is not satisfied in economics, which is different[43]. The discipline would essentially produce "generic predictions", in other words, "predictions of the existence of a phenomenon, process, or entity, as opposed to specific predictions about its detailed character" (1992, p. 69). The problem, in his view, is not that economics produces generic predictions, but that it seems *incapable* of producing anything else. Why, despite real efforts, does it find itself in this situation? Rosenberg's response is once again based on the bringing together of the conceptual arsenal of microeconomics and the "folk" explanation of action. The two domains share a recourse to *intentional states* (or "propositional attitudes" in philosophy of mind terms), such as beliefs and desires. According to Rosenberg, "the intentional nature of the fundamental explanatory variables of economic theory prohibits [an] improvement [of its predictive power]" (1992, p. 149); in other words,

(T10). The reason for the failure of economics as an empirical science lies in the recourse it has to intentional states.

The same supposedly crushing reason leads Rosenberg to uphold an even stronger proposal: economics *cannot* truly improve its predictive power. Economics as an empirical science,

[41] The analysis of these propositions is the subject of discussions approaching from other angles than their nomological properties; Mongin (2006b, 2007) discusses their status with regard to the distinctions between analytic and synthetic, and *apriori* and *aposteriori*.
[42] Economics certainly borrows from other fields of expertise, be they scientific or otherwise. We can reconcile this to Rosenberg's view by formulating the hypothesis that it is the borrowing from folk psychology which calls for philosophical clarification.
[43] See the counter-examples proposed by Hoover (1995), pp. 726-7.

therefore, does not suffer due to a conceptual problem but because it rests upon a false hypothesis which it shares with folk psychology, according to which, "the categories of preference and expectation are the classes in which economic causes are to be systematized" (1983). These categories "do not describe 'natural kinds', they do not divide nature at the joints". This is manifest in the "problem of improvability": if one views the theory of choice on which economics rests as a set of nomological propositions relating intentional states and behaviors and if these intentional states can only be measured through the observation of behavior with the help of this theory, it is hard to see how to improve our predictions in this framework – be it by improving our measurement of intentional states or by considering a better theory. This is how (T10) explains and justifies (T9).
Rosenberg paints an unsparing and contested (see, for example, Hoover, 1995) portrait of economics: its predictive failure is such that the discipline lends itself better to being conceived as a kind of "formal political philosophy" (1992, chap.7) or applied mathematics (1992, chap.8). Though not accepting this reduction, Hausman shares a part of Rosenberg's pessimism[44]. The reasons for economics' mitigated success are not to be found in its psychological roots but in the fact, already highlighted by Mill, that economic phenomena are *complex and unpredictable*[45].

4 Tendencies, capacities and idealizations in economics

4.1 Tendencies and capacities

Hausman is not the only contemporary philosopher of science to claim allegiance to Mill. Cartwright (1989) defends an idea of causality, influential today in philosophy of the natural sciences, which she reads in his work. For Mill, the fundamental assumptions of economics are tendency laws: not in the sense that they would be generally speaking true, but in the sense that what they express is at work even when other causes disturb their effect:

> (T11). A causal law doesn't only describe what is happening *in the absence* of disturbing factors; it says what *tends* to happen regardless of the disturbing factors which may be present.

The introduction of tendencies notably allows the preservation of the laws' universal scope. Nancy Cartwright brings them down to what she calls *capacities*. The capacity of a system or device is the property they have to produce certain characteristic results. Thus gravity is a capacity of attraction that bodies have in virtue of their mass and which results in characteristic movements. According to Cartwright, many causal statements, scientific or otherwise, are attributions of capacities: "[…] the laws of electromagnetic repulsion and attraction, like the law of gravity, and a host of other laws as well, are laws about enduring tendencies or capacities". This holds not only in the natural sciences: social sciences typically presuppose the existence of capacities too. For example, what would justify resorting to idealizations, the importance of which is widely recognized in modern science, is the assumption that the capacities at work in the ideal cases are *also* at work in the real situations. As for the economic sciences, much of the work in econometrics would rely on the assumption, implicit or not, that some factor (let's say, price) influences, in a *stable* and *measurable* manner, some other factor (let's say, demand). Generally speaking, econometrics

[44] In his view, "[…] scientific methods have not worked very well for economists and… they are unlikely to work well… The best methods of knowledge acquisition… have their limits and… one should not expect much of economics." (1992b, pp. 99-100).
[45] Rosenberg (2009) later goes back on his own arguments

occupies an important place in Cartwright's work (1989) because of its philosophically "refined" procedures of causal inference. If Millian economic methodology inspires Cartwright's general philosophy of science, it is still difficult to draw a systematic conception of economic science from her writings, and this despite the enduring interest she displays for the subject (2007, 2009).

4.2 Economic models and idealizations

These recent contributions concern the function of theoretical economics models and, more precisely, the persistent problem of their unrealism (see also section 6 on M. Friedman). Economists recognize and claim a fundamental role for these models[46], whose lack of realism is manifest. Economists are sometimes accused of studying the imaginary worlds that the models describe rather than the real world itself. Economic methodology converges towards contemporary discussions, very much alive in general philosophy of science, around that concept (see Frigg and Hartmann, 2009 and chapter 5 in this volume).
Cartwright thinks that physics models lack no less realism than economic ones and that the above objection is not the right one. Economic models are situated, at first glance, amongst the methodologically respectable family of *Galilean idealizations*[47] (McMullin, 1985): procedures by which, theoretically or experimentally, a cause is isolated from other causes which could disturb the effect that it produces. For Cartwright, Galilean idealization allows a capacity to be fully exercised and consequently allows the scientist to understand the causal contribution it brings *in general*. From this point of view, the lack of realism is not a *problem*, but rather a *means*: "frequently, what we are doing in this kind of economic theory is not trying to establish facts about what happens in the real economy but rather, following John Stuart Mill, facts about stable tendencies" (2007, p.221). Which we can reword thus:

> (T12). An essential part of economic modeling is destined to isolating causal factors so that their effects can be studied separately.

It is a position defended in a different philosophical setting by U. Mäki (see Mäki, 2009c).
For a partisan of (T12), the question to be asked is whether economic modeling *succeeds* in this enterprise of isolation. Cartwright (2007, 2009) gives a reserved response. Indeed, many idealizations present in economic models are not Galilean but instead consist in supplementary assumptions on the "structure" of economics. This claim is illustrated by contemporary macroeconomic models like Lucas' (1972)[48]. In such a model, individuals live for two periods, are of equal number in each generation, all produce goods that cannot be stocked, cannot pass the goods they possess to the following generation, etc. According to Cartwright, the economist needs these supplementary assumptions because the fundamental principles on which these models rest, typically specifications of assumptions (a1) and (a2) (rationality and equilibrium), are too few to result in interesting conclusions. But as a result, the guarantee that the conclusions could be exported to other circumstances - as Galilean idealization would have it – is lost. Economic models would thus be "overly-constrained".

[46]See the letter of July 4th 1938 from J.M. Keynes to Harrod: "Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world." More recently, Krugman (2009, p.18) affirms: "The only way to make sense of any complex system, be it global warming or the global economy, is to work with models — simplified representations of that system which you hope help you understand how it works."

[47] Discussion of the properties of economic models permanently enlists the notion of idealization. For a classification of the different types of idealization, see Walliser (2011), chap. 3, sec.2.

[48] Lucas R. E. Jr. (1972), "Expectations and the Neutrality of Money", *Journal of Economic Theory*, vol. 4, pp. 103-24.

The situation would be far more favorable in physics where one can rely on fundamental principles in far higher number[49]. To sum up, with economic models, "the worry is not just that the assumptions are unrealistic; rather, they are unrealistic just the wrong way" (2009, p. 57)[50].

4.3 Discussion: models as "credible" worlds

The question of knowing whether, and how, models such as those found in economics allow us to acquire knowledge about relevant aspects of the world is particularly debated in philosophy of economics today. For example, according to R. Sugden (2000, 2009), special theoretical models[51] like Akerlof's "market for lemons" (1970)[52] do not aim at disregarding causal factors which supposedly exist. More generally, they don't have the ambition of providing firmly grounded knowledge about the capacities at work in these phenomena. Instead, they should be seen as counter-factual worlds which, by virtue of their similarities with the real world, can convince us of the plausibility of certain conjectures concerning it. For example, the market for lemons model makes plausible the proposition stating that, all other things being equal, an asymmetry of information about the quality of the goods being exchanged tends to reduce the volume exchanged (see Figure 3).

The automobile market brings the members of two groups into play. The members of group 1 possess N cars whose quality x is uniformly distributed between 0 and 2. Their utility function is given by $U_1 = M + \Sigma^{n}_{i=1}\, x_i$ where M stands for the consumption of other goods and x_i is the quality of car i. The members of group 2 do not have cars. Their utility function is given by $U_2 = M + \Sigma^{n}_{i=1}\, 3/2\, x_i$. (Thus, members of group 2 attach more value to these cars and it is expected that some trade will take place.) The respective revenues of the two groups are noted Y_1 (which includes any possible revenue made from the sale of cars) and Y_2. All agents maximize their expected utility. The price (unique) of the automobiles is p while the price of the "other goods" is 1. The information is asymmetrical: the members of group 1 have knowledge of the cars' quality, those of group 2 know only their average quality μ.
According to these assumptions, the members of group 1 will be inclined to sell a quantity $S(p) = p.N/2$ of cars if $p \leq 2$ and the average quality of the cars exchanged will thus be $\mu = p/2$. In these conditions, the global demand $D(p, \mu)$ will be null and no automobile will be exchanged: the members of group 2 knowing μ, they are only inclined to buy at the price ¾ p.

[49] The contrast between economics and physics would, in reality, demand much deeper examination. It is not obvious that in physics the fundamental principles are sufficient for avoiding the "overly-constrained" problem once we move away from the discipline's "central core". We thank B. Walliser for his remarks on this point.
[50] For an elaboration of this idea, see also Reiss (2013, chap. 7) who proposes several dimensions along which differ truly Galilean idealizations and those found in economic models.
[51]The models Sugden is interested in belong to those which, in an article which anticipates current discussion on models, Gibbard and Varian (1978) call "caricatures". These are simple models which are applied to economic situations in a "casual" way: they must "explain aspects of the world that can be noticed or conjectured without explicit techniques of measurement", in contrast to the models which are applied in an econometric way. Gibbard and Varian's central argument is that these models are not conceived to be approximations of the economic reality, but as *deliberate exaggerations* of certain of its characteristics.
[52] Akerlof, G. (1970) "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism", *The Quarterly Journal of Economics*, 84(3), pp. 488-500.

(Intuitively: owners of high-quality cars won't have any interest to sell and given that only low-quality cars are available, buyers are not willing to pay *p*.)
If, on the other hand, the members of group 1 also only have knowledge of the average quality of the automobiles, i.e. if the information is imperfect yet *symmetrical*, then equilibria will exist where the volumes exchanged will be non null.

Table 1 : Akerlof's market for lemons (1970)

Sugden particularly puts the accent on the *abductive* use of economic models: logical exploration of the model shows that in the counter-factual world it describes, some factor *F* (the asymmetry of information) induces some economic phenomena (e.g., low volumes exchanged). If the model presents relevant similarities with the real world, and if analogous economic phenomena are observed in the real world, then the model makes plausible the explanation of these phenomena by a factor analogous to *F*. The inductive force of these kinds of reasoning, according to Sugden, lies in the similarity between the real world and the worlds described by the models: these must be *credible* given what we believe about our real world. In this view, "[...] the model is not so much an abstraction from reality as a parallel reality. The model world is not constructed by starting with the real world and stripping out complicating factors: although the model world is simpler than the real world, the one is not a *simplification* of the other" (Sugden, 2000)[53].

Section II: Neo-positivist themes

Millian deductivism is largely defensive: its intention is to explain *and* justify the epistemological particularities of economics. In its original form, it immunizes the fundamental assumptions of economic theory, since the comparison between empirical data and theoretical predictions would not be engaged in evaluating them. This view has always aroused unwillingness, even stretching to economists' way of doing things, in so far as they seemed to conform to the deductive method. It is not within our remit to trace the history of its decline through the 1930s and 1940s. Two factors undoubtedly played an important role, factors which can be considered in either a disciplinary or more conceptual manner. From the side of economics, it appeared doubtful, notably in theory of the firm, that fundamental assumptions like the maximization of profit should enjoy the obviousness that certain Millians were crediting them with[54]. In this way, confidence in the propositions of economic theory becomes difficult to rationalize if we suppose that it results primarily from confidence in these assumptions. Moreover, on the side of philosophy of science, this period saw the diffusion of ambitious and demanding visions of scientific knowledge, notably those from within neo-positivism, which ousted the older views like Mill's. The Millian solution to Mill's "generalized" problem revealed itself to be inadequate: it assessed economics through the lens of defective methodological standards, standards that *in any case* economics couldn't manage to satisfy. The second section of our chapter is dedicated to a methodological tradition which we can liken, but only to a certain extent, to the neo-positivist views. It is not just a matter of *variants* of neo-positivism, since refutationism will be included which, in its Popperian version, was vigorously opposed to the Vienna Circle. Rather it is a matter of notions, directly influenced or otherwise, which take up certain fundamental positions, starting with the

[53]Hoover (2001a) also discusses Cartwright's ideas about economics and its models. The angle of attack varies to the one we have presented here and favors macroeconomics and econometrics.
[54] See what we say further on about the historical context of M. Friedman's *Essay*.

determining importance for theory evaluation of the comparison between its predictions and empirical data[55].

5 Paul Samuelson, revealed preference theory and refutationism

5.1 Revealed Preference Theory

We will start with the methodological views vindicated or implemented by Paul Samuelson during the 1930s and 40s (from the revealed preference theory to the *Foundations of Economic Analysis,* 1947). On Samuelson's methodology, see Mongin (2000a, section III), to whom this section is deeply indebted, and also Wong (1978/2006). Samuelson certainly didn't "apply" neo-positivist ideas to economics. But many of his methodological options or convictions relate to them. We will give just one example, which we shan't come back to: Samuelson was attached to the ideal of the *unity of science*, as witnessed by his Nobel Prize acceptance speech which he dedicated specifically to the unifying role of maximization, in economics as among the sciences. We will concentrate our study on two of Samuelson's major projects, which also happen to be closely linked: revealed preference theory and the search for "operationally meaningful theorems" of economics.

Revealed preference theory is the result of a research program on the microeconomic consumer theory, launched by Samuelson at the close of the 1930s, and that many (including Samuelson) consider to have been completed by Houthakker (1950). With Samuelson (1938a), the objective attached to this program is to allow economics to do without the "residual traces of the concept of utility" found in contemporary consumer theory, developed on the basis of the concept of preferences (or ordinal utility, see Hicks and Allen, 1934). Hicks and Allen (1934), in the wake of Pareto's arguments, had proposed replacing Marshall's consumer theory, which relied on a notion of cardinal utility[56], with a theory which would make do with ordinal utility (or with preferences, to use more recent terminology):

> "It is necessary, in any theory of value, to be able to define just what we mean by a consumer of 'given wants' or 'given tastes'. In Marshall's theory (like that of Jevons, and Walras, and the Austrians) 'given wants' is interpreted as meaning a utility function, a given intensity of desire for any particular collection of goods. This assumption has made many people uncomfortable, and it appears from Pareto's work that it is not a necessary assumption at all. 'Given wants' can be quite adequately defined as a given *scale of preferences*; we need only to suppose that the consumer has a preference for one collection of goods rather than another, not that there is ever any sense in saying that he desires the one collection 5 per cent more than the other, or anything like that." [57]

However, the concepts of utility and preference are considered as psychological *and* non-observational, in contrast to choice behavior, which is supposed to be observable. For Samuelson, a consumer theory based only on behavior, thus "more directly based upon those

[55] Popper (1963/1989, p. 54) formulates and defends "the principle of empiricism which asserts that in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories". It is this kind of principle which unites the ideas developed in this second part of the paper.

[56] Simply put, a numerical function on options is an ordinal utility function if it represents only the way in which the individual ranks her options in terms of her preferences; it is cardinal if it also represents the *intensity* of these comparisons.

[57] Hicks (1939, pp. 17-8). Certain economists nevertheless think that the two notions of preference and ordinal utility do not coincide: it would also be possible to "cardinalize" preference (see d'Aspremont and Mongin, 1998).

elements which must be taken as *data* by economic science", is "more meaningful in its formulation" (1938a, p. 71).

These initial motivations of revealed preference theory seem to belong to a kind of timid eliminationism with regard to non-observational concepts: to rely on a theory formulated exclusively in terms of observational concepts is (all things being equal) *a* progress, not a *sine qua non* condition to the field's scientificity. The approach is not always understood in this way. For example, for Malinvaud (1972/1985), who is not one of its defenders, it belongs to a stronger eliminationism which he puts like this: "the scientist must not introduce non-operational concepts into [her] theories which do not lend themselves to objective observation." The discipline's history itself decided on this by creating a coexistence between the consumer theory of Hicks and Allen and the study of behavioral properties put forward by Samuelson.

5.2 Discussion of revealed preference semantics

Revealed preference theory call for other, less historical, remarks.

(1) The theory's eliminationist motivations underwent a similar fate in economic methodology to that of eliminationism in general philosophy of science: the elimination of theoretical concepts is considered as neither desirable nor, more often, practicable. Economics has the particularity that, for some of its central theories (such as consumer theory), elimination does seem possible: it can be shown that Hicks and Allen's version, which contains theoretical concepts, and Samuelson's version, which contains only observational (or supposedly observational) concepts, are in fact equivalent. As Mongin (2000b) underlines, this epistemic situation is not without its advantages since the theory formulated in observational language allows not only for the characterization of all the testable consequences of the initial theory, but also for the containment of any potential refuters of that theory.

(2) Moreover, revealed preference theory can be associated with a *semantics* for the concept of preference which largely exceeds the theory itself: in that perspective, preferring option x to option y *signifies* choosing x rather than y when the two options are available. Despite regular warnings from economic philosophy dating back to Sen, economists persist in incorrectly distinguishing that vague and dubious semantics from the theory which, as we have just seen, is precise and tenable. In contrast to the latter, which is barely discussed at all any more, the former continues to play an important methodological role; in particular it inspires Gul and Pesendorfer's (2005/2008) hostile anti-neuroeconomics manifesto. The open defenders of revealed preference essentially maintain that

> (T13). The only legitimate or necessary notion of preference in economics is the notion of revealed preference.

Sen (1973) was the first to distinguish himself by rejecting (T13). First of all, it wouldn't be tenable to see in revealed preference theory an attempt to *eliminate* the concept of preference: if we completely deprive ourselves of it then we also lose any possible source of justification for the assumptions of the new theory. If we dismiss that first interpretation, we are still left with the revelation hypothesis which states that preferences are directly expressed in choices. Yet, again according to Sen, an individual's choices are not rigidly linked to her preferences; in forming an assumption of this sort we run the risk of muddling the preferences revealed by choices, the genuine individual preferences, and other motives which also influence choices all into one and the same concept. Sen was followed by d'Aspremont and Mongin (1998) and Hausman (1992, 2000 and 2008), who maintained that "economics cannot function without a subjective notion of preference, which does not and cannot stand in any one-to-one relationship with choices" (2008, p.132). Hausman imagines several objections. (a) The first is that preferences, in the usual sense, are not expressed in choices except by means of assumptions about the agents' beliefs. (b) Economics doesn't only relate preferences to objects of choice, nor even to hypothetical choices. It borrows from game theory, where preferences relate to possible *consequences* of the interaction between agents, as well as from social choice theory where, according to the model established by Arrow (1951), preferences relate to abstract states of society. Concerning game theory, we can think of the elementary predictive task of game theory as being the prediction of *choices* between feasible strategies on the basis of beliefs and preferences about the possible *consequences*. (c) Finally, the theoretical apparatus of economics and decision theory would lose its explanatory power if we adopted the semantics of revealed preference: it would be a matter only of recording the behavioral generalizations without once looking at the causal factors responsible for this behavior.

5.3 Samuelson's "operationally meaningful theorems"

As Houthakker (1950) was already pointing out, Samuelson doesn't always attach his theory to an exclusive methodological motivation. In Samuelson (1950), it is no longer a question of *eliminating* the residual traces of the utility concept of consumer theory but of obtaining the "full empirical implications for demand behavior of the most general ordinal utility analysis". One of the objectives of the *Foundations of Economic Analysis* (1947) is precisely to derive what the book calls "operationally meaningful theorems". This is a "hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions"[58]. Samuelson wants to show that economics, and consumer theory in particular, do indeed entail operationally meaningful theorems[59]. For example, if a consumer obeys conventional theory (in terms of preferences), then she must conform to the Weak Axiom of Revealed Preference (WARP) according to which, for all price vectors p, p' and budgets w, w':

(a) if the consumer doesn't choose the same basket of goods in conditions (p,w) and (p',w') (i.e. $x(p,w) \neq x(p',w')$), and

(b) if she can buy the basket of goods x(p',w') in conditions (p,w),

then she cannot buy x(p,w) in conditions (p',w') - in other words, x(p,w) exceeds the budget w' when prices are p'.

[58] See Samuelson (1970): "From the beginning I was concerned to find out what *refutable* hypotheses on the observable facts on price and quantity demanded were implied by the assumption that the consumer spends his limited income at given prices in order to maximize his ordinal utility."

[59] Two answers are given to the question of knowing which pressures on consumer behavior are implied by the theory. (i) Slutsky's substitution matrix must be symmetrical, negatively semi-defined, and the demand function homogeneous to degree 0 relative to prices and to revenue. (ii) The demand function must obey the Strong Axiom of revealed preference. The second answer is the result of revealed preference theory.

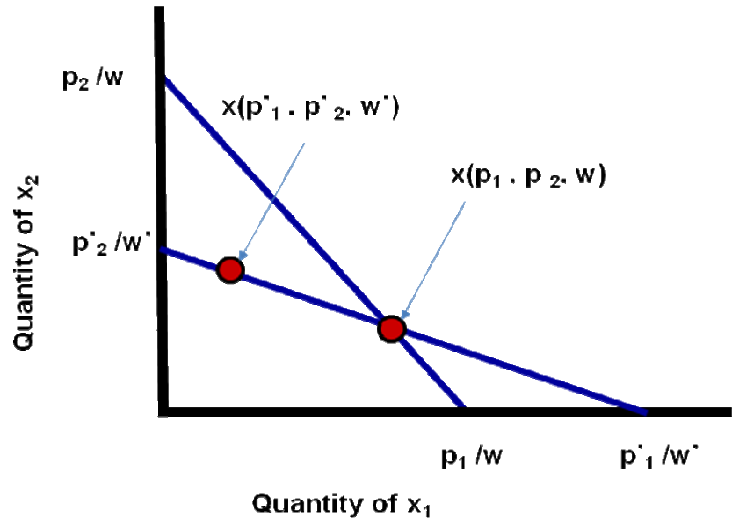

Figure 3: Violation of the Weak Axiom of Revealed Preference. The consumer does not choose the same basket of goods in conditions (p_1, p_2,w) and (p'_1, p'_2,w); she can buy x(p'_1, p'_2,w) in conditions (p_1, p_2,w); but she can also buy x(p_1, p_2,w) in conditions (p'_1, p'_2,w) .

The axiom is better understood if we introduce the concept of preferences on top of that of choice: if the consumer doesn't choose the basket of goods chosen for (p',w') in conditions (p,w), even though she has the means to, this means that she *prefers* the basket she chooses, and the choice observed in conditions (p',w') must be compatible with that same preference; so x(p,w) must not be affordable given her budget. Often the relation "x is *revealed preferred* over y" is *defined* by the property that the consumer demands the basket of goods x even though both the prices and her budget allow her equally to demand y. Thus the Weak Axiom comes down to demanding that the relation "... revealed preferred over ..." be asymmetrical. These refutable consequences give birth to what economists call the non-parametric tests of consumer theory (see Varian, 1982 and 1992, chap. 8 and 12). It is important to point out that we are dealing with an idealized notion of refutability. What we can directly observe at a given moment *t*, is at the very most a consumer's demand (given the prices and her budget). For the demands of the consumer x(p,w) at *t* and x(p',w') at *t'* to conflict with the Weak Axiom, we must suppose that the consumer's preferences, or her demand function, remain *stable* between *t* and *t'*. If we really want to conduct tests with natural data, then hypotheses about the identification of the consumers, the identification of the goods, the separability of present and future demands, etc., must also be made, and account must also be taken for the fact that this data is finite, whereas the demand function x(p,w), by definition, covers a continuum of situations (see Chiappori, 1990).

5.4 Refutability and refutationism

The determination of the refutable consequences of theories plays a crucial role in a refutationist approach to science. Refutationism wielded great influence over economic methodology with Samuelson's *Foundations*, but already it had inspired the strictly methodological work of Hutchison, *On the Significance and Basic Postulate of Economics* (1938), and it finds a rebirth through the seminar, "*Methodology, Measurement and Testing in*

Economics" (M^2T) at the London School of Economics (Archibald, Lancaster, Lispey)[60]. The work of M. Blaug (1980/1992) is the current methodological manifestation of this. Unlike Samuelson, whose philosophical sources are poorly identified, all these authors are influenced by the Popperian version of refutationism which makes refutability the criterion of demarcation between science and non-science, and makes refutation the means by which our scientific theories are evaluated.
At the meeting point of Samuelson's research program and Popperian ideas, several members of the M^2T, during the 1960s, explored the refutable consequences of various contemporary economic models (see Mongin, 2005). It was already appearing from *Foundations* that, following the ordinary distinction of what is observable from what is not, the refutable consequences of economic theory were to be found in qualitative comparative statics: the interest, then, is on the sign of variation of an endogenous variable when an exogenous parameter varies. It turns out that the variables and parameters have to maintain very particular relationships for the signs of variation of the former to be unequivocally determined by the variations of the latter and so that, consequently, refutable consequences can be reached. Archibald (1965) arrives at the conclusion that, "It seems unfortunately to be the case that the general qualitative content of maximizing models is small, if not trivial". For a refutationist wanting to turn refutability into a criterion for scientificity while holding on to parts of the economic theory in question, this conclusion is discouraging. The question of the refutable consequences of economic theories has an interest which exceeds refutationism, on top of which we would like to add some elements concerning more recent microeconomic models[61].

(1) After the second World War, theoretical economics progressively adopted the model of expected utility as a reference point for individual decision made under uncertainty, that is, when the decider is not in a position, for all possible actions, to know what the consequence of that action will be. According to this model, an action's value is the sum of the products of all the values of the action's possible consequences multiplied by the probability of their occurring. Thus, when uncertainty is already probabilized, the options can be identified with probability distributions (economists speak of "lotteries") and the model posits that the decider prefers lottery P over lottery Q if and only if

$$\sum_{c\in C} P(c).u(c) \geq \sum_{c\in C} Q(c).u(c)$$

We have noted $P(c)$ the probability of obtaining consequence c if lottery P is chosen, and $u(c)$ the utility the agent attaches to c. This model imposes a property of "independence", according to which option P is preferred over option Q if and only if the probability mixture of P with some other option R is preferred over the probability mixture of Q with the same R, and in the same proportions[62]. This proposition is considered to be refutable, and, in certain situations, individuals *seem* to violate the axiom of independence[63]. The reservation is important, as the situation resembles a Duhemian problem, see Mongin (2009). The expected utility model is thus refutable and, according to the general view, *refuted* too. A vast program of collective research among economists and psychologists, still ongoing, allowed for the elaboration of decision models for uncertainty that are compatible with the observed

[60] See Lipsey (2008). Klappholz and Agassi (1959) can be associated with the same group.
[61] For the sake of space, we leave aside Lakatos' influence on economic methodology.
[62] By definition, the α–mixture of lotteries P and R, written $\alpha P \oplus (1 - \alpha)R$ assigns the probability $\alpha P(c) + (1 - \alpha)R(c)$ to a consequence c. It is easily verified that $\alpha P \oplus (1 - \alpha)R$ is also a lottery.
[63] These cases of alleged refutation match well known paradoxes, such as Allais' paradox (1953).

anomalies. For the moment, the most convincing models are typically *generalizations* of the expected utility model, which make one lose in refutable content what is gained in empirical validity. As such, refutationism is safe only on first analysis (again, see Mongin, 2009).

(2) A second innovation of contemporary economics, even more recent, is the massive reliance on game theory. The question arises, once again, of knowing whether the theory is refutable or not. Several economists and philosophers of economics have looked into this question in recent times (Weibull 2004, Hausman 2005, Guala 2006). Game theory works by constructing "solution concepts" which select, for a set I of participants and for a given strategic configuration G, certain action profiles noted $S(G) \subseteq \times_{i \in I}(A_i)$ where A_i is the set of actions available to the individual i. At first glance it seems easy to think up a situation which would be disadvantageous for such a solution concept: (a) we observe individuals interacting according to G; (b) the actions $\underline{a} \in \times_{i \in I}(A_i)$ selected by these individuals do not belong to $S(G)$. Hence, it is often considered that Nash's equilibrium (recalled in subsection 1.2) is jeopardized in situations which reproduce the Prisoner's Dilemma: experimentally, individuals tend to "cooperate" rather than "defect"[64].

		Player 2	
		Cooperate	Defect
Player 1	Cooperate	(3,3)	(0,4)
	Defect	(4,0)	(1,1)

Figure 4: The Prisoner's Dilemma. Each player has the choice between cooperating and defecting. To each action profile corresponds, in the grid, the vector of utilities for the two players. Thus, the profile where each player cooperates incurs a utility of 3 for each player.

In this perspective, the refutability of game theory seems to pose no particular problem. Moreover, it would be variable according to the games since, with some of them, the solution concept involved is incompatible with many action profiles, something which is not the case with others. Several remarks are necessary, however.

First let us note that we have supposed game theory lends itself to the customary game of scientific assumptions, even though it is not obvious that this be the case when it is proposing solution concepts. For many specialists, it thus more defines *norms* for comparison with observed actions and not strictly speaking assumptions. It is only within certain applications that the theory appears to want to expose itself. Here, straight away, we see a difference with the theory of individual decision. But let's pursue anyway, supposing an empirical interpretation of the theory.

We must then bring attention to the fact that our provisional conclusion, according to which the refutability of the theory seems unproblematic, is based on the assumption that the individuals do indeed take part in game G. What is open to testing then, is simultaneously (ai) the assumption that, in situation G, individuals obey the solution proposed by game theory, and (aii) the assumption that they play game G. This second assumption cannot be directly assessed, if only because the individuals' preferences, supposed to be unobservable, take part in the definition of what a game is. Consequently, when it is observed that the profile of selected actions $\underline{a}$ is incompatible with $S(G)$, we can, in principle, point the figure at (aii)

[64] It is easy to verify that the action profile (defect, defect) is a Nash equilibrium: the best option for a player, presuming that the other player is defecting, is to do the same. Moreover, this equilibrium is unique.

rather than (ai), that is to say, we can question whether the individuals are really playing game *G*. Let’s suppose, for example, that the subjects are put in the following situation: each of them have the choice between two possible actions and, according to the actions chosen, they obtain the vectors of *monetary gain* given in Figure 5.

		Player 2	
		Cooperate	Defect
Player 1	Cooperate	(€3, €3)	(€0, €4)
	Defect	(€4, €0)	(€1, €1)

Figure 5

Figure 5 doesn’t describe a game, as the individuals’ preferences are not specified. If the subjects do not defect, it will be possible to save the theory by maintaining that they didn’t play the game described by Figure 4. It could, for example, be maintained that the preferences of a subject *i* are not increasing functions of her monetary gain. This (natural) idea, inspires much work in experimental game theory which associates situations like those described in Figure 5 with games where individuals’ preferences take the monetary gain of the other players into account.

Coming back to the general discussion, the essential difficulty resides in the fact that it is awkward to test (aii) independently. One could finish by concluding, like Hausman (2005), that “economists can often learn more by using game theoretic anomalies to study the factors influencing preferences rather than by treating them as disconfirming game theory”. Guala (2006) recognizes these methodological difficulties, but maintains that constraints from decision theory on the revelation of the players’ preferences impose certain limits on the flexibility of game theory which, as a result, is refutable - *and refuted* by certain recent experiments.

6 Milton Friedman and the “realism” of assumptions

6.1 The context.

The most famous contribution to contemporary methodology certainly remains “The Methodology of Positive Economics” (1953) by Milton Friedman. This article was read and widely discussed, not only by philosophers of economics, but also by economists themselves. Commentaries are legion and continue to multiply: Nagel (1963), Simon (1963), Mongin (1988, 2000a), Musgrave (1981), Blaug (1980/1992), Hausman (1992b), Mäki (2009a). Friedman’s essay was interpreted in many ways: refutationist, conventionalist, instrumentalist, realist, etc. In fact it is doubtful whether the *Essay* presents any single coherent epistemology[65]. The article can be seen as an attempt at reconciling economic methodology and philosophy of science, as they were practiced at the time. It was largely taken to be a *defense* of economic practice in the face of the most tenacious objections it encounters and, in particular, in the face of the objection, which we have already discussed in reference to Mill, according to which the theory behind it is based on excessively *unrealistic*

[65] Mongin (2000a), Mäki (2009b).

assumptions. So we shouldn't be surprised that Friedman's theses were favorably received by certain economists[66].

Before looking at these theses, it is worthwhile placing them in their historical context. Indeed, the article follows after one of the major internal controversies of the discipline, the marginalist controversy in the theory of the firm which had developed just after the second World War. The theory of the firm that we know today took root progressively during the 1930s (see Mongin, 2000a). At the end of this time period, several researchers attempted to test its fundamental assumption - of profit maximization - independently of its consequences, by directly posing questions to company heads. The results of these questionnaires, in regards to methods of price fixation and hiring, seemed to utterly contradict that assumption. If, as the Millian tradition would have it, we consider that confidence in economic theory stems from confidence in its assumptions, the situation becomes problematic. Friedman proposes another way of thinking about the assessments of the theory of the firm and of economic theories in general, a way of thinking which, ultimately, would enable their defense against objections based on the implausibility or falseness of its assumptions.

6.2 Friedman's theses.

Many reconstructions of Friedman's theses are available. We will opt for this one:

> (T14). A (scientific) theory must be primarily judged by the correctness of its predictions (pp. 4, 9-10, 15, 31)
> (T15). A theory must not be judged by the "realism" of its assumptions (pp. 14, 41)
> (T16). A theory affirms that everything happens *as if* its assumptions were true (pp. 17-19, 40)
> (T17). If a theory is important and significant, then its assumptions are not "realistic" (p.14)

Theses (T14) and (T15), baptized "F-Twist" by Samuelson (in Archibald *et al.*, 1963), are the most important, we won't really discuss the other two. Nagel (1963) and Mäki (2009b) highlight the ambiguity of the "*as if*" in (T16), the latter showing that, in certain passages (pp. 19-20), use of the phrase is clearly instrumentalist, while, in others (p. 40), the author draws on realism by suggesting that everything happens *as if* certain ideal conditions were satisfied. As for (T17), the thesis is particularly elaborated by Mongin (1988) who discerns in it both a banal interpretation and an unreasonable interpretation with the help of the neo-positivist definitions of scientific theories.

The first thesis (T14) rests upon a notion of prediction that Friedman sees in an epistemic and not temporal fashion: P is the prediction of a certain theory at a moment t if P follows on from the theory, potentially enriched by auxiliary assumptions, and if P is an empirical condition *whose truth value is not yet known at t*. Consequently, P can just as well concern a future phenomenon (prediction in the strictest sense) as a phenomenon which has already occurred (retrodiction). Friedman seems to see only a pragmatic difference between prediction and explanation, i.e. to explain is to predict something we know has occurred[67]. In reality, he limits the field of prediction by adding that a "theory is to be judged by its predictive power for the class of phenomena which it is intended to 'explain'"[68]. In other words, the theory's

[66]To take just one example, the introductory manual of Stiglitz & Walsh (2000, p.123) rejects criticisms of consumer theory's lack of psychological realism in much the same way as Friedman.
[67] See the chapter "Explanation" of the present volume.
[68] p. 8.

surface domain, that to which it seems to apply, must be distinguished from its *target domain*, that which matters in its empirical evaluation; and (T14) becomes:

> (T18). A theory must be (primarily) judged by the correctness of its predictions relative to its target domain.

To the question of knowing what economic theory's target domain is, two main responses are conceivable: (a) The first, which corresponds with Friedman's examples, consists in maintaining that it includes the behavior of economic agents, but not their mental states or processes. Certainly, the best illustration is to be found in the article that Friedman wrote with Savage in defense of expected utility theory and which it is worth quoting at length:

> "The hypothesis does not assert that individuals explicitly or consciously calculate and compare expected utilities. Indeed, it is not at all clear what such an assertion would mean or how it could be tested. The hypothesis asserts rather that, in making a particular class of decisions, individuals behave *as if* they calculated and compared expected utility and *as if* they knew the odds. The validity of this assertion does not depend on whether individuals know the precise odds, much less on whether they say that they calculate and compare expected utilities or think that they do, or whether it appears to others that they do, or whether psychologists can uncover any evidence that they do, but solely on whether it yields sufficiently accurate predictions about the class of decisions with which the hypothesis deals."[69].

(b) The second response consists of maintaining that the target domain contains only aggregated variables, such as prices or the quantities of goods exchanged. This interpretation dates back at least to Machlup (1967), for whom the target domain would be made up of "*mass behaviors*". He matches this interpretation to a restriction limiting exclusively to predictions of comparative statics (see *infra*), a restriction not read in Friedman.

The first thesis, (T14), modified in (T18), serves as a foundation for the second, (T15), directly pointed against objections to economists' practices. The scope of the response depends on the notion of "realism" employed, something which is far from unequivocal with Friedman. Several commentators have sought to clarify this[70]. The two most common interpretations are: (i1) realism as exhaustivity (in this sense a set of assumptions is unrealistic when it doesn't say *everything* about relevant objects); (i2) realism as truth (in this sense a set of assumptions is unrealistic when some of the assumptions are false), or with very high probability of being true.

The premise of Friedman's argument in favor of (T15) is that a set of scientific assumptions is *necessarily* unrealistic. The question to be asked then is whether this set is realistic *enough*, despite everything else, to satisfy the economist's epistemic objectives. It is here that (T18) intervenes: the only standard we possess for judging the previous question is the empirical correctness, relative to the target domain, that the hypotheses authorize. There is no intrinsic criterion for deciding whether a set of assumptions is a "good approximation" or not. Just as it is pointless to abstractly debate the realism of the law of falling bodies - this depends on the kind of context in which predictions of the law are expected -, so it is pointless to criticize the central assumptions of economic theory for the reason that they do not accurately describe economic agents' reasoning, or even their individual behavior. The strength of the argument will obviously depend on the meaning given to the notion of realism. If we take (i1), then the premise is trivial, as Nagel (1963) remarks, and the part of the conclusion dealing with

[69] Friedman and Savage, 1948, p. 298
[70] Notably Nagel (1963), Musgrave (1981), Mäki (2000).

unrealism will be too. On the other hand, if "realism" is to be understood in the (i2) sense, the premise is far more contestable.
Perhaps, to obtain a non-trivial methodological argument, the sequence must be understood in still a different way. In essence, Hausman (1992b) proposes passing by the intermediary conclusion (C) which differs subtly from (T18):

> (T18). A theory must be (primarily) judged by the correctness of its predictions relative to its target domain.
> (C). The only test for judging a theory consists of directly determining whether it provides correct predictions relative to its target domain.
> (T15). A theory must not be judged by the "realism" of its assumptions.

6.3 Discussion

It is difficult to give an overview of the objections which have been brought against Friedman's theses. We will concentrate on Hausman's (1992b) which develops on the basis of the thesis recalled above. The passage from thesis (T18) to the intermediary conclusion (C) is not, in his view, legitimate. Indeed, let's consider the parallel thesis about the purchasing of a second-hand car:

> (T18'). A good second-hand car is reliable, economical and comfortable.
> (C'). The only test for knowing whether a second-hand car is a good second-hand car consists in directly determining whether it is reliable, economical and comfortable or not.
> (T15'). Everything that can be found out by opening the hood of the second-hand car and inspecting its various component parts is irrelevant to its evaluation.

The conditions mentioned in (T18') must be understood to be necessary and sufficient conditions in assuring the parallel with (T18). This last thesis would be convincing were it possible to know all the road performances, past and future, of a second-hand car. Then we wouldn't need to "look under the hood". In the same way, for somebody who, like Friedman, accepts (T18), if all the empirical performances, past and future, of a theory could be known, we would have everything necessary for its evaluation. But the point that Hausman puts forward is that *we are not* in such an epistemic situation. The inspection of a theory's "components" can be a first resort resource when, for example, we want to extend the theory to new situations, or when we have to react to empirical difficulties.
It is not certain, however, that Hausman's objection quite does justice to a strong intuition discernible behind Friedman's proposals and theses and which consists in giving prominence to the *division of labor* between the special sciences (specifically, economics and psychology). For example, it would have the consequence, in the case of microeconomics, of defending the stylization of psychological description by justifying it with the fact that a keener description is a job for psychologists, while the economist must concentrate on the consequences for collective phenomena. It is not surprising then, neuroscientists that theses of a Friedmanian bent frequently reappear in current methodological discussions about behavioral economics and neuroeconomics (see above) which raise, implicitly at least, the question of division of labour between economists, psychologists.

7 Experimental economics, “behavioral” economics and neuroeconomics.

7.1 Experimental economics and its objectives

For a long time, the dominant view was that economics was exclusively a science of *observation*, and not an *experimental* science. But since the last forty years or so, experimental economics[71] has been progressively developing. The Sveriges Riksbank Prize in Economic Sciences (known as the “Nobel Memorial Prize”) 2002, bestowed on the experimenters D. Kahneman and V. Smith, bears witness to this development and to its recognition by the economics community. The number and variety of experimental work is now considerable, as shown by the *Handbook of Experimental Results* by Smith and Plott (2008) or the *Handbook of Experimental Economics* by Kagel and Rott (1995). Indeed, the experiments are just as much about individual decision and the markets as they are about strategic interactions. Moreover, they can either be laboratory *or* field experiments. In laboratory conditions, subjects evolve within a context (set by the task they must accomplish, the information they may receive, the goods they consider, etc.) that is largely artificial, while in the field, we approach a natural environment[72]. We can also make distinctions amongst field experiments. Harrison and List (2004) distinguish those which are “framed”, where the context is natural in one or several of its aspects and where subjects know they are participating in an experiment, from those which are truly “natural”, where they do not have such knowledge. They also distinguish field experiments from *social experiments*, where a public institution, in its action, undertakes a rigorous statistical procedure so as to understand the effects of certain factors it can control, and from *natural experiments*, where one *observes* variations which occur without the experimenter’s intervention, but whose structure approaches that of the controlled variants.
The experiments may pursue differing objectives. We can distinguish at least three[73]:

> (oi) Testing a preexisting theory - for example, we have already mentioned the experimental tests of expected utility theory.
> (oii) Discovering unknown phenomena.
> (oiii) Exploring questions of economic policy[74].

In the past, experimenters themselves have often put the accent on objective (oi), that is, the test of economic theories. Today, more emphasis is put on the partial autonomy of experimentation with regard to economic theory: experimenters often introduce variations relative to factors which economic theory does not take into account, and allow themselves to be guided by local and informal hypotheses regarding the importance of such and such a parameter (see for example, Guala 2005, p. 48).

7.2 Methodological questions.

The methodological questions raised by experimental economics are many, and have recently been the object of some monographs (Guala, 2005; Bardsley *et al.*, 2010)[75]. Some of these

[71] Readers can initiate themselves on experimental economics with Friedman & Sunder (1994).
[72] Furthermore, subjects of laboratory experiments are very often students of the universities where they take place.
[73] See Roth (1995), pp. 21-2 and Sugden (2005).
[74] Thus, in 1993, when the *Federal Communications Commission*, an American government agency, was wondering by which mechanism it would be wisest to allocate new telecommunications permits, experimenters were called upon to test various propositions (see Guala, 2005, chap.6).

questions concern the specifics of experimental economics, like the systematic use of financial motivations, which differentiates it from other experimental human sciences like psychology. In market experiments, which concern the coordinating role of that institution, financial motivations serve to experimentally *control* certain individual characteristics like the value assigned to options. Smith's "induced value theory" (1976)[76] is the canonical formulation of this use.

As we have already recalled, one of the objectives commonly assigned to experimentation is the *testing* of those economic theories which lend themselves to it. What is highlighted then is that the experimental approach makes possible empirical testing whose results are far more unequivocal than those that could be obtained from natural data. The *confirmational impact* of experimental data is, however, not easily gauged, and this divides economists. Economic theories are, indeed, largely thought of as seeking to predict and explain "real" phenomena. From this point of view, the relevance of their empirical adequacy in artificial contexts is in no way obvious: why, for example, should a theory which is confounded by experimental data suffer the same fate if applied outside of the laboratory? The way in which we conceive the confirmational impact of experimenting depends on two factors: (1) on the *domain* assigned to the economic theories, and (2) on the response given to the question of *external validity* or parallelism (see notably Starmer, 1999b; Guala, 2005, Section 2, Bardsley *et al.*, 2010), that is to say, the question of knowing what is allowed to be inferred concerning real economic phenomena on the basis of experimental phenomena. If we go as far as including laboratory behaviors in the domain of economic theories, then regardless of the actual response given to the question of external validity, the confirmational impact of the experimentation will already be notable: a theory confounded by experimental data will be a theory confounded in its domain. Experimental economist Ch. Plott's point of view can be read in this way:

> "General models, such as those applied to the very complicated economies in the wild, must apply to simple special cases. Models that do not apply to the simple cases are not general and thus cannot be viewed as such…
> Theories that predict relatively poorly in the laboratory are either rejected or refined. Models and principles that survive the laboratory can then be used to address questions about the field." (Plott, 1991, p.905)

Inversely, if experimental phenomena are excluded from the domain of economics and if it is thought that there are large differences between real and laboratory behaviors, then the confirmational impact of the data resulting from the laboratory will be extremely limited. We will now add a few separate remarks about the domain of economics and about external validity.

(1) The positions concerning the question of what belongs to the domain of economic theories cannot be reduced to the opposition between those who exclude laboratory behavior and those who don't. Thus Binmore (1999) limits relevant experiments to those where (a) subjects are faced with "simple" problems, (b) their motivations are "adequate" and (c) the time given to them to adjust their behavior to the problem is "sufficient". Symmetrically, he also limits the application of economic theories *in the field* to those situations which obey analogous conditions. This is not self-evident: among the phenomena generally considered to be relevant to the domain of economics feature situations which are complex or whose stakes are low or which offer little opportunity for learning (Starmer, 1999a). Moreover, it is not obvious that all economic theories must maintain the same relationship with experimental data. It can, for

[75] See also the special edition "On the Methodology of Experimental Economics" of the *Journal of Economic Behavior and Organization*, 73(1), January 2010.
[76] See Friedman and Sunder (1994), pp. 12-15 for a synthesis presentation.

example, be considered that if consumer theory's job is to account for behavior in the field, and not in the laboratory, then the abstract theory of decision has a more universal scope and experimental data *must* be involved in its evaluation. The very notion of domain quite certainly calls for clarification. A first effort in that direction was carried out by Cubitt (2005) who distinguishes

(i) the *base domain*: the set of phenomena to which the theory applies without ambiguity[77],
(ii) the *intended domain*: the set of phenomena that the scientist intends to explain or predict with her theory[78], and
(iii) the *test domain*: the set of phenomena which can be legitimately considered for the testing of the theory.

Cubitt maintains that these three domains should not coincide and, in particular, that the test domain not be limited to the intended domain. Specifically in the situation with which we are interested, one may recognize that the experimental situations do not belong to (ii) while maintaining that some of them at least belong to (iii). This assertion will not get a detailed argument but can be justified by calling on the external validity of the experimental phenomena, to which we now turn our attention.
(2) In which conditions can one "export" results obtained in the laboratory to the field? Guala (2005) asserts, in essence, that the inference from laboratory to field must take place on a case by case basis, and by a rigorous accounting of information about the experiments and about the natural domain of application. The objective is to ensure that the two contexts have enough relevant causal factors in common to allow for reasoning out, by analogy, from the laboratory to the field. According to Guala, it is essentially with a view to exploiting the analogy that these experiments have an interest for economists: the experimental situations are not so much *components* of the specific domain of economics (natural economic phenomena, what Cubitt calls the intended domain) as they are *representations* of the domain which enable it to be understood, along with models or simulations. Borrowing from contemporary literature on models, Guala sums up his idea by affirming that experiments are "mediators" between the domain of economics and the hypotheses we can form about it (pp. 209-11).

7.3 On the border between economics and cognitive science: behavioral economics and neuroeconomics

Experimental economics is often associated with two other movements, both of which also make extensive use of experimentation: (1) so-called behavioral economics and (2) neuroeconomics.
(1) The qualifiers "experimental" and "behavioral" are often used in an interchangeable fashion, but perhaps wrongly so. While experimental economics consists of studying economic phenomena with the help of controlled experiments, behavioral economics defines itself as a project that "increases the explanatory power of economics by providing it with more realistic psychological foundations" (Camerer and Loewenstein, 2004). This project involves much experimentation, but it also relies on taking into account natural data and

[77] For example, we can consider expected ("objective") utility theory to apply unambiguously to choices between bets on the color of balls randomly removed from various boxes, the proportion of balls of each color in each box being known.
[78] For example, we can consider the purchase of insurance policies as belonging to the domain targeted by expected utility theory.

revisiting the psychological and behavioral assumptions which orthodox economics rests upon. Decision theory, game theory and the auxiliary assumptions which economists often rely upon (like the assumption stating that individual preferences increase with monetary gain) are subject to particular attention. This project is largely motivated by dissatisfaction with regards to orthodox economics and by the anti-Friedmanian working hypothesis:

> (T19). An improvement in the assumptions made concerning economic agents will result in a significant improvement in economic science.

Behavioral economics typically proceeds by generalization or modification of received assumptions and in this sense it constitutes a "soft" heterodoxy. Hypothesis (T19) is empirical and behavioral economics is undoubtedly too fragmented for us to be able to evaluate it at this stage. If it does seem to go against the Friedmanian thesis (T15), which states that a theory must not be judged by the realism of its assumptions, the conflict may be only apparent. Some of its followers have the paradoxical ability of remaining loyal to the thesis grounding (T15), thesis (T18), according to which a theory must be judged by the correctness of its target domain predictions, all the while considering that an improvement in the psychological realism of economic theory is the *means* of obtaining the best predictions. Others, on the contrary, reject (T18) and consider that economic theory *must* be founded on plausible psychological principles, whether or not that results in a significant predictive improvement. (T19) can thus mask different epistemological motivations. Moreover, the reference to psychology and to psychological realism is not free of ambiguity. Certainly, the disciples of behavioral economics are opposed to the separation of economics and psychology, of the sort that Robbins (1932/1935), for example, defended[79]. But if we judge on the basis of the most striking works of behavioral economics, it is not a question of applying or being inspired by a preexisting cognitive psychology of decisions, nor even of approaching economic behavior through the use of the concepts and methods of cognitive psychology. Nor is it a question, generally, of opening the "black box" of mental states and processes that traditional economics, in its timidness, would leave closed: numerous hypotheses or theories within the domain are neither more nor less "psychological", in this sense, than the traditional theories in economics. What more certainly unifies various work within the domain is the conviction that, in many situations, the models used by traditional economics in describing agents' behavior is systematically erroneous. The call for "psychological realism" largely consists of *taking account of* these empirical anomalies through theoretical revision. This attitude has consequences for the discipline which are still difficult to evaluate. In defending the recourse to assumptions which, sometimes significantly, move away from the standards of rationality, behavioral economics also disturbs the traditional organization of economics, and particularly the communication between positive economics and normative economics, which to a considerable extent rests upon agents' individual rationality, understood in the traditional fashion.

(2) Neuroeconomics, born at the beginning of the 2000s, has the objective of exploring the neural bases of economic behavior. To do this, it employs the methods and tools of contemporary neuroscience, notably functional magnetic resonance imaging (see Glimcher & Fehr, 2008/2014 for an encyclopedic state of the art). For example, in McClure *et al.* (2004) subjects have to choose between two options with varied delayed monetary rewards. The first option (*sooner-smaller*) yields the sum R after a delay d, and the second (*later-larger*) the sum R' after a delay d', with $d < d'$ (where d is today, in two weeks, or in one month) and $R <$

[79] Robbins (1932/1935), pp. 83 and sq. For this reason Robbins criticizes the attitude of Gossens, Jevons or Edgeworth. Bruni and Sugden (2007) trace the divorce between scientific psychology and neo-classical economics back to Pareto.

R'. The authors show that (a) the limbic system is preferentially activated when the first option involves an immediate reward (d = today), (b) the parietal and prefrontal cortex is engaged uniformly by the task (irrespective of the value of d), and (c) greater activity of the parietal and prefrontal cortex is associated with choosing the second option rather than the first.

In seeking to enlighten the study of certain social phenomena through neurobiology, neuroeconomics cannot help but raise questions linked to the reductionism dealt with in the chapter "Philosophy of the social sciences". Methodology's first interest is in what the neurosciences could bring to economics, and particularly in the more specific question of the relationship between neural data and models of choice, taking the following affirmation from F. Gul and W. Pesendorfer as its target:

> (T20). Neural data can neither confirm nor disconfirm the models of decision that economics makes use of.

Gul and Pesendorfer develop several arguments to support their statement (see Hausman, 2008). If some in the field rely more particularly on the semantics of revealed preference, all highlight the fact that the traditional models of decision are *silent* on the cognitive side of things (see Cozic, 2012) and that, consequently, these models imply no testable restriction on the direct observations we could collect of individuals' deliberative processes. As the defenses and objections collected by Caplin and Schotter (2008) attest, there is today a striking absence of consensus regarding (T20) and the arguments which are supposed to justify it. These debates explain why, though economists may not doubt the interest of neuroeconomics for the cognitive neurosciences, they are often more skeptical regarding its richness in the treatment of the traditional questions of economics (see Camerer, 2007, Bernheim, 2009).

8 Conclusion

We have placed our chapter on economic methodology under the banner of *Mill's generalized problem*: does economic science obey the methodological standards of an empirical science? This question, implicitly or explicitly, has oriented a large portion of epistemological reflection about the discipline since the beginning of the nineteenth century. It is time now to see what can be learned from the principal responses this problem gave rise to.

- The judgments concerning economics as an empirical science cover an extremely wide spectrum: some defend the core of neo-classical economics' achievements, others are of the opinion that economists are guilty of insufficiently testing their theories, still others that the conceptual framework in which they work is doomed to failure.
- Unequivocally unfavorable diagnostics of economics are today, it seems, rather in the minority among specialists in economic methodology. The dominant impression is that these are based on either overly rigid epistemological foundations or overly partial considerations of the discipline's accomplishments. Unequivocally favorable diagnostics are not abundant either. A majority would no doubt be in agreement that several episodes or tendencies of contemporary economics were manifestations of excessive dogmatism.
- There is virtual unanimity *against* the methodological view which is certainly the most frequently cited by the economists of the last few decades, this being Friedman's view.
- Recent evolutions in economics, which assign increased roles to theoretical diversification, to interdisciplinary openness and to attention to empirical data (experimental or otherwise), are positively welcomed by most methodological analyses of the discipline.

Let us come back to the status of economic methodology. We said it in the introduction: methodological standards do not exist today which could be the object of a consensus among philosophers of science and whose mere application to economics would suffice in bringing out its scientific legitimacy. The methodological contributions we have chosen to present draw no radical conclusions, in the sense that they reckon a normative reflection about economics as an empirical science to still be possible[80], even if, with the most recent of them, their way of reaching that reflection encounters significant reorientation, in accordance with an increased attention to the economist's actual procedures and a larger distance with respect to the doctrines which animated general philosophy of science in the twentieth century. This is not the only reaction possible. The absence of consensus argument sometimes joins forces with an absence of expertise argument which states that it falls on the experts (the economists), and not on the methodologists, to judge their own work[81], to nourish a skepticism with regard to any normative dimension in methodological inquiry. In response, some, particularly strict about the application of philosophy of science to economic methodology, have abandoned the normative project and sometimes even the conceptual tools of philosophy of science. This is the case with studies belonging to the "rhetoric of economics" school of thought initiated by McCloskey (1985/1998)[82] which propose studying, armed with the tools of rhetoric and literary criticism, the way in which economists *persuade* themselves[83]. In our view, and that of many philosophers of economics, these absence of consensus and absence of expertise arguments have limited reach[84], even if they call (and rightly so) for the methodologist to consider her abstract principles as fallible and to base her judgments on a deep and charitable understanding of the economist's work. Moreover, in the remarkably active domains such as philosophy of physics, of biology or of cognitive science, the specialists carry on the project of critical reflection of the object of their discipline.

Let us finish by allusion to some tendencies and some gaps within current economic methodology[85]. These will be easier brought out if we pursue a comparison with the other domains of philosophy of the special sciences. These other domains grant less importance to Mill's generalized problem and perhaps greater attention to their own objects. They often seek to clarify the fundamental concepts and principles of their discipline by testing their coherence, as much within the discipline as with the rest of our knowledge. In this way these reflections take on an allure which is (a) more specialized and (b) more *ontological* than the majority of the contributions we have presented. (a) The tendency towards specialization is

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[80] This attitude is, for example, openly declared by Rosenberg (1992, pp. xii-xiii) or Hausman (1992a, sec. 14.3).

[81] See McCloskey (1985/1998), p. 139.

[82] McCloskey (1985/1998, chap. 9) paints a particularly severe picture of the "modernism" in epistemology, which more than sufficiently covers the themes we have labeled "positivist".

[83] See Hands (2001, pp. 257-258) for a detailed bibliography of these studies. These meta-methodological questions exceed the boundaries of philosophy of economics and are at the heart of the rivalry which exists between philosophy of science and what is known as "science studies".

[84] See notably the criticisms of Blaug (1992), Hausman (1992a, pp. 262-269), Rosenberg (1992), Hoover (1995). Other meta-methodological elements of discussion can be found in Hands (2001), Kincaid et Ross (2009b), Guala (2009). The latter defends an "instrumental" normative methodology which is supposed to constitute a middle road between a "categorical" normative methodology, which evaluates scientific activity from the perspective of abstract and supposedly universal prescriptions, and the abandoning of normative methodology. The idea is to have the methodologist evaluate the scientific practices he is studying from the perspective of the objectives the scientist pursues. In this way, Guala defends the idea that the normal practices of experimental economics are justifiable *if* the objective pursued is the discovery of robust causal relations (rather than universal laws).

[85] Kincaid and Ross (2009b) give another point of view, more controversial, about recent tendencies in economic methodology which they name the "new philosophy of economics". In their view, this differentiates itself favorably from the works that dominated economic methodology from the 1970s to the 1990s, those of Blaug, Hausman and Rosenberg in particular, which attached themselves too exclusively to the theoretical core of neo-classical economics and which approached it with philosophical concepts which are now obsolete.

already at work in methodology of economics. Section 7, dedicated to experimental economics, behavioral economics and neuroeconomics, will certainly have made the reader aware that, though the most recent debates often remain tied to Mill's problem, they do move towards more specific questions which are dealt with in a more autonomous fashion. In this regard, we must admit that, because of length constraints, we were not able to do justice to questions like those of causality in economics[86], of econometric reasoning[87], or regarding the links between micro- and macroeconomics[88]. (b) Philosophy of economics as a positive science is, on the contrary, still widely dominated by methodological preoccupations[89]. It could be considered that it would be advantageous for it to develop its ontological inclination, all the more so that economics internally communicates as many infra-individual properties (the mental states of actors) as supra-individual entities such as organizations or institutions. It could benefit, on the one hand, from the progress of philosophy of mind and the cognitive sciences, and, on the other hand, from recent studies in philosophy of social sciences which are interested in the status of collective entities and properties. As an example, the clarification of a concept as fundamental to economic analysis as "the market" is far less straight-forward than it may seem. Finally, there are two characteristics of contemporary economics which call for a supplementary effort of analysis: the considerable development of its theoretical apparatus, and the internal coexistence of positive *and* normative preoccupations. On the one hand, we certainly haven't reached a satisfactory degree of clarification of the norms and objectives which have required this development of the theoretical apparatus of economics. This is the case, for example, with the status of the theory of general equilibrium. Advances on this question probably necessitate a better understanding of the general nature of *theoretical progress*. On the other hand, the juncture between positive economics and normative economics, and notably the role of individual rationality in the communication between the two types of research, still largely remains to be clarified.

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[86] See Hoover (2001a), Reiss (2008, chap. 7-9).
[87] For an introduction, see Reiss (2013, chap. 10). See also Hendry (1980), Malinvaud (1991, chap. 12 and 13), Spanos (2006, 2012).
[88] See Kirman (1992), Hoover (2001b, chap. 3, 2009).
[89] This domination is also visible linguistically: "methodology of economics" in fact refers to the collected researches falling under the category of philosophy of economics as a positive science.

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