Pareto and Political Economy as a Science: Methodological Revolution and Analytical Advances in Economic Theory in the 1890s

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It is argued that Schumpeter's widely accepted judgment that Pareto's work is "completely rooted in Walras's system" constitutes a misreading of Pareto. In fact, already during the period 1892–1900, Pareto traces the methodological outlines of an economic science profoundly different from that of Walras. It is maintained that his methodological contribution represents an attempt to define the conditions for political economy to be a science. In this context, Pareto examines the theoretical premises of economic theories and questions the hypotheses of perfect foresight and rationality. The ordinalist hypothesis is also shown to be a consequence of these methodological reflections.

> Professor Walras' great contribution to economic discussion was his discovery of a general system of equations to express the economic equilibrium. I cannot, for my part, sufficiently admire this portion of his work, but I must add that I entirely disagree with him on what he has to say in his work entitled *Études d'économie sociale*. Professor Walras thinks it possible to draw certain economic deductions from metaphysical principles of jurisprudence. This opinion is worthy of respect, but I am unable to accept it. I am a believer in the efficiency of *experimental methods* to the exclusion of all others. For me there exist no valuable demonstrations except those that are based on facts. [Vilfredo Pareto 1897, p. 491; italics added]

This paper is written in the centenary of Pareto's *Cours d'économie politique* and his article in this *Journal*, offering the American public "a brief exposé" of the "new economic theories." We would like to thank Giovanni Busino and the two referees of this *Journal* for valuable comments made on a previous version of this paper.

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I. Introduction

Most modern economic theorists regard Vilfredo Pareto as one of the founders of economic science. This reputation is, however, based on only a limited part of his actual work, primarily his purely formal-theoretical contributions: the analysis of competitive equilibrium, the notion of ordinal utility, and, above all, the concept of optimality. Moreover, the judgment of Joseph Schumpeter that Pareto's work is "completely rooted in Walras's system" (1954, p. 860) is still widely accepted. Within this context, the methodological distinctions of Pareto, though acknowledged, have generally been underestimated[1] and certainly not considered significant enough to question the essential homogeneity between Pareto's and Walras's works, which from the point of view of the mathematical analysis of equilibrium does undeniably exist.

We believe that a full appreciation of Pareto's analytical work is possible only starting from his actual methodological approach: the adoption of the "experimental" method (*méthode expérimentale*)[2] in political economy. It is from methodological reflection that Pareto, in the last decade of the nineteenth century, traces the outlines of an economic science profoundly different from that of Walras, one that anticipates later works in the theory of value and may, in this sense, even be defined as "modern." And this was done before Pareto wrote his best-known book, *Manuale di economia politica* (Italian ed. 1906; French ed. 1909), which represents rather the account of his methodological and analytical innovations of the preceding years.

This paper examines Pareto's methodological and analytical contributions to economics during the period 1892–1900. In 1892 he published a series of articles in the Italian *Giornale degli Economisti* concerning the principles of pure economics, that is, the theory of price determination under free competition. These articles were followed by *Cours d'économie politique,* in 1896–97, and "The New Theories of Economics" published in this *Journal* in 1897. In 1898 Pareto

[1] An exception is Tarascio (1968, 1973). See also Kirman (1987).

[2] We translate the French word *expérimentale* as "experimental" as Pareto himself did in "The New Theories of Economics" (1897) to distinguish his method from purely descriptive-quantitative work (which Pareto termed "empirical"). However, it should be said that the French word *expérimentale* (like the Italian term *sperimentale*) has a broader meaning than the word "experimental" in English. The English word is largely confined to the meaning of artificial or controlled experiments carried out by laboratory scientists. Referring to the "experimental method," Pareto essentially had in mind, as will be seen, the empirical testing of theoretical models, though not of course the experimental methods developed by the branch of economics known today as experimental economics.

began a critique of the theory presented in the *Cours,* which first appeared in the paper "Sunto di alcuni capitoli di un nuovo trattato di economia pura del Prof. Pareto" (Summary of some chapters of a new treatise on pure economics by Professor Pareto), published in the *Giornale degli Economisti* in 1900, and introduced the new theory of value later fully developed by Hicks and Allen (1934).

In our view, the adoption of the experimental method in economics enabled Pareto to ascertain the necessary conditions for economics to be a "science." And in so doing, Pareto offers not only an excellent example of the application of the scientific method to economics but also a deep examination of the foundations of economic theory, pointing out some questions of relevance today.

II. Methodological Issues

In his preface to the fourth edition of *Eléments d'économie politique pure* (1900), Walras writes that "it is already perfectly clear that political economy, like astronomy and mathematics, is at once both an experimental [*expérimentale*] and a rational science" (p. xx; our translation).[3] Yet, as is written in the third lesson of *Eléments,* since pure economics is "a physical-mathematical science like mechanics or hydrodynamics, . . . economists should not be afraid to use the methods and language of mathematics" and, moreover, "mathematical method is not an *experimental* [*expérimentale*] method; it is a *rational* method" (1954, p. 71). The task of experience is rather to provide the real "types," which the sciences idealize:

> This much is certain, however, that the physico-mathematical sciences, like the mathematical sciences, in the narrow sense, do go beyond experience as soon as they have drawn their type-concepts from it. From real type-concepts, these sciences abstract ideal type-concepts which they define, and then on the basis of these definitions they construct *a priori* the whole framework of their theorems and proofs. After that they go back to experience not to confirm but to apply their conclusions. [P. 71]

The Walrasian analysis is informed by a rationalist philosophical point of view. This is explicit in *Etudes d'économie politique sociale,* published in 1896, where Walras outlines a "realistic utopia," a society that represents a synthesis of liberalism and socialism. Such an idea

[3] In Jaffé's version of *Eléments* the term *expérimentale* in Walras's preface to the fourth edition is translated as "empirical" (1954, p. 47). The reasons for using this term and not "experimental" are probably those referred to in n. 2.

logically precedes the elaboration of pure theory presented in *Eléments*. According to Jaffé (1980, p. 530), the *Eléments* "was designed to portray how an imaginary system *might* work in conformity with principles of 'justice' rooted in traditional natural law philosophy."

Walras's rationalism does not fit well in the presence of the positivism of Irving Fisher and Pareto, just as his insistence on the rational over the experimental method alienated him from the scientific community, whose approbation, it is known, he highly regarded.[4] The importance of the adoption of the scientific experimental method in economics was asserted by Fisher in the first part of *Mathematical Investigations* (1892): "The laws of economics are framed to explain facts" (p. 11), he writes, though this statement is not developed further.[5] It is Pareto, rather, who actually advanced the methodological aspects of the new economic theories. Economics is a "natural science," he writes in the *Cours,* "founded exclusively on facts" (Pareto 1896–97, vol. 1, §1), a concept already expressed in his earlier writings on the fundamental principles of political economy in 1892–93. The immediate consequence is that political economy "has not to give us precepts: it studies first the natural properties of things, then solves the problems attendant on the question: given certain premises, what will the consequences be?" (§1). The second consequence is that political economy uses the experimental method peculiar to the natural sciences.[6]

[4] Porter (1994) writes that Walras's general equilibrium theory was too abstract to interest the engineers: "To physicists in the era of Kelvin and Helmholtz, a theory was only meaningful if its terms were susceptible to measurement. Such views were especially common among those who were close to engineering and who wanted to see physics put to use. But it was also a moral ideal, an ideal of discipline, restraint, and humility. . . . While neoclassical economists may have derived much of their mathematical theory using analogies with physics, they were very far from accepting the prevailing standards of physics as a practice. That practice was and is strongly associated with experimental qualification, and by no means first of all with mathematical theory" (p. 157). Porter says that there were scientists such as Vito Volterra who approved of the work of neoclassical economists, but there were very few: Volterra's point of view seems atypical to him. Yet Porter does not seem to realize that Volterra referred principally to Pareto's work, never mentioned in Porter's article, and not to Walras's.

[5] It does not appear necessary to Fisher to emphasize the experimental side of scientific discourse, probably because, as a student of the mathematician I. Willard Gibbs and a friend of Simon Newcomb, an astronomer and mathematician with an interest in economics, he felt it was obvious. Moreover, at the time of his doctoral thesis, Fisher had a limited knowledge of economic debate (see Fisher's preface and Allen [1993]).

[6] "Hypothetical facts are derived from observed facts," and not from a priori principles ("whether from theological or teleological principles or supposed natural rights or metaphysical considerations"), Pareto says. This is, Pareto asserts, "the method followed, apart from a few deviations of scarce importance, by classical political economy, from Adam Smith onwards" (*Cours,* §574). On this matter Pareto, in a letter (undated) to Felice Vinci, declared, "I am closer to Adam Smith than Walras" (quoted in Sensini [1948]).

For Pareto, therefore, the economic method was different from how Walras characterized it. His methodological dissent from Walras—whom Pareto openly recognized as his teacher in the field of pure economics—is clear from his first article in the *Giornale degli Economisti* in 1892. There he affirms that "there is a tendency in the new school to subordinate experience to theories" and "this may represent its greatest danger" (Pareto 1892, p. 406), that is, providing ammunition for those who accuse the theorists of restoring metaphysical abstractions. Though not openly declared in the *Cours*, Pareto's critique is repeated again in 1897 in "The New Theories."

In Pareto's view, the experimental method is a combination of the rational (or deductive) method and the empirical (or quantitative-descriptive) one. The experimental method, borrowed from the natural sciences, according to Pareto, is the correct method for economic science to progress: as he says in "Comment se pose le problème de l'économie pure,"

> Progress in the natural sciences is the result of the combination of two methods: the rational method, in accordance with which, starting from certain hypotheses, consequences are deduced; and the empirical method, according to which those consequences are compared with the facts. If they agree, hypotheses are accepted; if not, hypotheses are rejected. . . . It is the correct method for the new economic theories. [(1898) 1966, p. 102]

It is, as Pareto indicated several times, the same method that John Stuart Mill called "concrete-deductive." It consists of an initial induction from observed phenomena, from whose common characteristics consequences are deduced (theoretical deductions, logical or mathematical); subsequently, these deductions are compared to the real facts (via observation or experience) in order to identify "the agreements and differences." They in turn produce new inductions, which demand modification, addition, and completion in order to obtain new ideal schemes of the characteristics common to the observed facts: "and so on indefinitely." The scientific principle of truth, Pareto says, following Mill, is the ability "to attack them [theories] freely" (*Cours*, §657), to be able to demonstrate that they may be wrong, or, as we might more commonly say today, to refute them.[7]

[7] In the article of May 1892 Pareto acknowledges his indebtedness to Mill's *System of Logic*. Discussing the limits of purely descriptive empirical work, in a footnote, Pareto says that "on this point, there is nothing I can add to Mill's brilliant exposition in his Logic, Book VI, Chapter VII, on the use of the experimental method as applied to social sciences. I also agree with what he writes in Book VI, Chapter IX, about the 'concrete deductive' method which in his view should alone be used in these sciences" (1892, p. 393, n. 2). We should remember that in Pareto's ter-

The experimental nature of economic science does not, however, imply that theory exactly represents the real world. It enables us only to "approximate reality." The vital criterion in building a scientific theory is, in effect, that of "successive approximations" (*approximations successives*). In the *Cours,* following considerations earlier developed in his essay of May 1892 in the *Giornale degli Economisti,* Pareto writes that

> We do not know and can never know any real phenomenon in all its characteristics; we can only know ideal phenomena which increasingly approach concrete phenomena. Let us take a very simple example. . . . Imagine a study of the earth's shape. . . . Such a surface is approximately spherical [first approximation]. . . . Analogously, pure economics indicates the general "shape" of the phenomenon. [§35]

Pure economics offers a first approximation to a social phenomenon, defining the general conditions of economic equilibrium.[8] The subject of such pure economics is the abstract concept *homo oeconomicus,* the "perfect hedonist" who "acts only as a result of economic forces" (*Cours,* §592, n). However, as Pareto emphasizes in his 1892 essay, the greatest care must be taken in the use of abstractions:

> This method is logically faultless, provided that each time we return to the real world we do not forget to demonstrate that laws found for abstract men do possess a real value. . . . It is appropriate that we look closely at what are the implicit and explicit postulates of the new political economic reasoning, in order to know the precise value in the real world of its laws. [P. 409]

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minology, the "empirical method" corresponds to Mill's "experimental method" and Pareto's "experimental method" corresponds to Mill's "concrete-deductive method." (We are ourselves indebted to one of the referees for this valuable clarification.) Consequently, in this methodological statement, Pareto clearly follows Mill. But we should also note that in the same footnote Pareto stresses a difference with Mill, regarding the use of mathematics (see also nn. 8 and 9 below): "It seems to me that Mill did not give due value to the use of mathematics. Neither can we accept what he says in another part of his *Logic,* on the use of the calculus of probability. . . . It seems that the true nature of that calculus escapes him" (p. 393, n. 2).

[8] In order to solve this problem, it is necessary to use mathematics. According to Pareto (and of course Fisher), mathematics represents a kind of logic to which we should resort "to have an exact and complete conception of the relations of economic equilibrium among various phenomena" (*Cours,* §584). And in "The New Theories," Pareto writes that "the problem of pure economics bears a striking likeness to that of rational mechanics. Now, in point of empirical fact, men have as yet not succeeded in treating the latter problem without the aid of mathematics. It therefore appears quite legitimate to appeal also to mathematics for assistance in the solution of the economic problem" (1897, p. 490).

> It is not enough to assign the characteristic of perfect hedonist to man. . . . It is also necessary to decide what qualities of foresight and reason and so on are to be granted him. These qualities . . . are to some extent assumed to be implicit. This does not appear correct to us: postulates should always be declared explicitly. [Pp. 410–11]

Hypotheses such as foresight and perfect rationality have a limited significance, concludes Pareto:

> We consider incorrect the proposition that scientific principles have an absolute value, even if only for perfect hedonists. At best they could have an absolute value for the actions of perfectly foresighted and perfectly rational perfect hedonists. Now, where economic phenomena are concerned, we believe that to suppose men perfect hedonists is not so far removed from the real world, but this is not so if we suppose them perfectly foresighted and rational. [P. 418]

Abstractions are legitimate, Pareto repeatedly says, as long as they do not lose their "purely experimental" character; that is, they remain hypotheses whose consequences have to be verified. It is interesting to note a certain similarity between Pareto and Alfred Marshall on this issue: in appendix C of *Principles of Economics* (1920), concerned with the aim and method of economics, Marshall emphasizes the limitation of deductive reasoning unsupported by specific experience.[9] But perhaps more significant is the similarity between Pareto's thinking and that of the eminent French scientist Henri Poincaré, in contrast to Walras. In reply to Walras's letter of September 10, 1901, written to request an opinion about the *Eléments,* Poincaré initially writes that "I am not hostile to the application of mathematics to economic science, on condition that the correct limits are not exceeded" (September 16, 1901; cited in Jaffé 1965, vol. 3, letter

[9] The continuity between Pareto and Marshall includes also their caution in using mathematics. Thus Pareto writes that "the use of this method [mathematics] . . . should never be separated from caution. The more a series of deductions grows, the more reasoning becomes a mechanical operation, as happens with algebraic symbols, where the probability of errors increases due to the uncertainty of the premises. . . . We can assert that logically one proposition follows another, but we do not know if we are moving too far away from reality. Now it is important to note that every science approaches reality without fully embracing it. The phenomenon studied by science is always an ideal representation. . . . From this is born the necessity to compare, as often as possible, our deductions with experience or observation. . . . The correct conclusion is the need to proceed cautiously, returning to experience and observation every time" (1892, p. 400).

1494). Two weeks later, explaining what he meant by "correct limits" (*juste limites*), Poincaré writes that

> I think that every mathematical speculation begins with hypotheses. If this condition is forgotten then you exceed the correct limits. In mechanics, for example, friction is frequently not considered under the assumption that bodies are perfectly smooth. You consider men perfect egoists and perfectly clear-sighted. As a first approximation, the first hypothesis may be accepted, but I have some reservations about the acceptance of the second. [Cited in Jaffé 1965, 3:164–65]

It is clear that "experience and observation" are the correct measure of reasoning. This is the essential ingredient of "good method," Pareto says in the preface of the *Cours:* "Good method is recognized by the result [*a bonne méthode, comme le bon ouvrier, se reconnaît à l'oeuvre*]. . . . In our opinion there is but one criterion of truth: experience. Any theory that explains facts, and enables the forecasting of new ones, may be admitted, at least provisionally. Any theory unsupported by facts has to be rejected" (p. iii). It is in this context that we should interpret Pareto's often manifested "indifference" for the actual method used, or his methodological eclecticism. In his essay of 1892 he writes that

> The debates on the method to be adopted in a science seem to us of little use. Only from experience is it possible to know the advantage that can be obtained from the use of a particular method. You may adopt the method of reasoning you prefer, you may request help from history, physics, mathematics, you may accept or reject the theories of evolution, and if the old logic does not satisfy you, you may use the new mathematical one. Everything is permissible, everything is good, as long as you are able to discover new truths, shed new light on what is already known, rectify errors; in short, increase the quantity and the quality of human knowledge. [P. 390]

In his correspondence with the Italian philosopher Benedetto Croce, published in the *Giornale degli Economisti* between 1900 and 1901, Pareto reexpounds his methodological proposal: "I am the most nominalist of nominalists. For me the only objective cases are concrete cases. Their classifications are man-made and are therefore arbitrary, unless we establish—always arbitrarily—the ends of a classification. In this case the latter can be deduced logically from those ends" ([1901] 1953, p. 203). This is the result of the fact that as

Croce observed, so Pareto says, "no science has ever been able to give a precise definition [of any science], issuing [as you say] 'from the nature of the thing [itself]'" (p. 203). Pareto, "as a good nominalist-empiricist," declares himself to be ignorant of this "nature" or essence. He compares his own and Croce's positions to those of Plato and Diogenes:

> I remember the gist of an observation by Diogenes who said to Plato that he could see the table but not *tableness*. You are playing the part of Plato and I can only play that of Diogenes. . . .
>
> . . . Just as you say, consider that where both the facts of man's activity and physical facts are concerned, "we can only observe regularity and deduce consequences therefrom, without ever penetrating into the inner nature." . . . I only study facts and concrete cases and try to find out what regularities and analogies they present. . . . All those who set out to give an absolute basis to our knowledge [are metaphysicians]; whereas we, call us empiricists or experimentalists, as you please, know nothing beyond the relative. [Pp. 203–4]

To study facts and, after, look to combine them by means of hypotheses, without ever seeking the "intrinsic nature" of the things: for Pareto, this is the method that political economy should follow and he intended "to stay closer to": "I am looking for something very different from the metaphysical reason of value, I look for a theory which may include and present economic facts" (p. 207). The emphasis on the necessity of a factual foundation to economics led Pareto to the analytical advances that are usually identified as his particular contribution to the theory of value, that is, the introduction of the concept of ordinal utility.

III. Conceptual Innovations: From Cardinal Utility to Ordinal Utility—a New Theory of Value

Economists are not interested in the reasons why a good is useful, only in the fact that it is useful. This statement, which separates the concept of utility from the hedonistic premises that were initially at its basis, is asserted for the first time by Fisher and Pareto. If the problem to be resolved by economists is "to explain economic facts," as both authors think, then they must not look beyond what is useful to this end. Initial caution and subsequent rejection of hedonism constitute a common basis for the two authors.

Utility should be defined without reference to metaphysical entities, Fisher writes in *Mathematical Investigations:*

> The economist need not envelop his own science in the hazes of ethics, psychology, biology and metaphysics.
>
> Perhaps utility is an unfortunate word to express the magnitude intended. Desirability would be less misleading, and its opposite, undesirability, is certainly preferable to dis-utility. "Utility" is the heritage of Bentham and his theory of pleasures and pains. For us his *word* is the more acceptable, the less it is entangled with his *theory*. [1892, p. 23]

The first to introduce the notion of preference as the basis of utility, he continues:

> The conception of utility has its origins in the facts of human preference or decision as observed in producing, consuming and exchanging goods and services. . . .
>
> . . . Whether the necessary antecedent of desire is "pleasure" or whether independently of pleasure it may sometimes be "duty" or "fear" concerns a phenomenon in the second remove from the economic act of choice and is completely within the realm of psychology.
>
> We content ourselves therefore with the following simple economic postulate: *Each individual acts as he desires*. [P. 11]

Fisher rejects as irrelevant to economics the traditional question of what a unit of pleasure or utility is: "This foisting of Psychology on Economics [by Edgeworth, Gossen, and Jevons] seems to me *in*appropriate and vicious" (p. 5).

At the beginning of the 1890s, Pareto originally accepted the hedonistic hypothesis as a first approximation. This derived from his rigorous methodological acceptance of experimentalism: he was interested in the empirical implications of the utility theory, and in this he distinguished his position from that of his contemporaries, as has been noted by Stigler (1950) and Chipman (1976): according to Stigler, he was "the great and honorable exception" (p. 395). In the 1892 essay, after considerations regarding method, under the title "Value," Pareto writes that "the real facts that we are able to observe are the sales of certain goods at certain prices. The object of a theory of value can only be to explain these facts, combine them, and make them appear as the consequences of one or a few principles" (pp. 405–6).

As already said, the theory of value of the new political economy derives from the consideration of the perfect hedonist, *homo oeconom-*

icus. Is such an abstraction permissible? The starting point of such an investigation is the Edgeworthian hedonistic theory, which by means of the utilitarian calculation examines

> the equilibrium of a system in which each force works for the maximum utility of everyone. . . . This concept is impressively simple and grand at the same time. In our opinion it contains a lot of truth, and for this reason we should proceed with caution in order to avoid conclusions which, when found contrary to experience, could cause the rejection of both the good and the bad contained in the new theories. [P. 410]

It is with this intention that Pareto considers the concepts of total utility and final degree of utility. He asks if utility has "a real existence," is it "present in the conscience of *homo oeconomicus* or is it simply an abstraction invented by economists?" (p. 412). Examining the question of total utility from its mathematical aspect, that is, considering its integral, Pareto notes that "normally this integral is considered as starting from zero." This, however, generates two problems: the integral may easily become infinite;[10] and "considering the elements of the integral corresponding to zero quantity of economic good makes the integral increasingly more abstract" (p. 413). In fact, says Pareto, "How many men in a town are really aware of the suffering caused by the absolute lack of nourishment? Yet when the integral is thought of as starting from zero we are considering the utility generated by the removal of those sufferings" (p. 414). He goes on: of the two ways in which it is possible to formulate the "fundamental principle of the hedonistic calculation," when one refers to total utility and marginal utility, though analytically the same, the second is preferred:[11]

> If we concentrate on concrete facts, there is this difference between the two: with the first [total utility] it may be supposed that the individual has knowledge of the total utility of an economic good. We believe that this occurs very rarely. No one has a clear conception of the total utility gained from eating, drinking, clothing oneself or owning

[10] With regard to this, Pareto presents the following example: "This integral can easily become infinite. For example if $j_a(q) = a/q$, we obtain $\int_h^d \varphi(q)\,dq = a \log(d_a/h)$ which for $h = 0$ becomes infinite" (p. 413).

[11] Pareto would go on to analyze the question of integrability in the *Manuale* of 1906 and more deeply in the French edition of 1909, on the basis of the suggestion of an article by the Italian mathematician Volterra in the *Giornale degli Economisti* in 1906 (see Chipman 1976).

> a house. Rather we only feel the advantages of small variations in the quantity of these goods: our mind can only understand the concept of the final degree of utility. [P. 415]

Pareto's dissatisfaction with the hypothesis of the additive utility function, which he considered acceptable only as a first approximation, is already evident in his 1892 essay. Additive utility functions were used by Walras. Fisher also used them in the first part of *Mathematical Investigations,* but in the second, more innovative part of the work, he adopts utility functions having as variables all the *n* goods.

In the *Cours,* Pareto reasserts the analysis contained in his articles of 1892–93. Because of his dissatisfaction with the term utility, he substituted the word "ophelimity" (*ophélimité*):

> Utility has generally been understood by those authors who have developed the new theories as a relation of convenience between a thing and an individual. However, seeing that in ordinary language useful is also the opposite of harmful, and that a lot of misunderstanding is derived from these different meanings of the same term, we have to resign ourselves to giving a new name to utility. . . .
>
> We will use the term ophelimity.[12] [§§4, 5]

This action of cleansing the utility concept of hedonistic implications put under discussion utility as a measurable quantity. Walras had argued that utility was not measurable in practice but nevertheless remained quantifiable like physical quantities, such as temperature and mass.

Fisher and Pareto thus faced the question of the measurability of utility. Their antihedonistic position initially led them to define the field of application of utility measurement along Walrasian lines. In what sense was it possible to affirm that utility, a totally subjective quantity, could be measured? As Pareto writes in the *Cours* (§§19–21),

> There are degrees of ophelimity: it is a quantity, and is therefore subject to the general rules established by the science of quantity. . . . Utility . . . is also a quantity. . . . Two distinct issues should not be confused here: one involves the existence of a quantity and the other is concerned with the practical possibility to measure it. The distances of the

[12] Fisher contested the use of the term "ophelimity" as a substitute for "utility." Pareto replied in a letter of January 11, 1897, defending his position (see Allen 1993).

> stars from the earth are quantities whose existence is established. But only some of those distances have been measured, and even then very grossly. From only the existence of a quantity, important consequences may be deduced.

Both Fisher and Pareto found ways of indirectly measuring utility. Fisher solved the problem satisfactorily, in the case of the independence of marginal utility between various quantities of goods: given a certain quantity of a good, its marginal utility is defined as a unit of utility (util); if an individual indicates his preference between two quantities, it becomes possible to construct a utility chart for a given good. In his 1892 essay, Pareto shows how, observing economic phenomena, one can indirectly obtain the value of utility with a method similar to the one used to determine the length of luminous waves from the observation of optical phenomena.

However, from Pareto's methodological point of view, the hypothetical possibility of measuring utility was a totally different thing from the real possibility that individuals were able to accurately measure the pleasure they obtained passing from one set of goods to another. Such measurement demands absolute precision, and this, according to Pareto, is not attainable by the individuals nor empirically observable. His reluctance to go further and break away altogether from the concept of utility was essentially due to the fact that the hypothesis of measurability appeared necessary to explain consumer behavior. Pareto made it very clear in 1900:

> Until now, in order to establish economic doctrines we went back to choice. Choices have been explained as man's aim to achieve maximum pleasure. Between two things, man chooses the one that provides more pleasure. The point of equilibrium is obtained by expressing the conditions mathematically which enable the individual to enjoy the maximum pleasure compatible with the obstacles he meets. . . . The use of this point of view forces us to consider pleasure as a quantity. And this is what the economists who have established pure economic theories have done, and what we ourselves have done in the *Cours:* but we must admit that this is not a thoroughly rigorous method. [1900*b*, p. 221]

Yet how can this hypothesis be replaced? The decisive theoretical step was made by Pareto alone, providing, in the words of Eugen Slutsky's famous article of 1915, "the most rigorous concept of utility" (p. 1) available at that time; the most rigorous because it is purely formal and independent of any psychological and philosophi-

cal hypotheses. In 1898, Pareto recognized that the measurability of utility is not necessary to explain consumer behavior. In "Comment," Pareto asks, If it is not possible to measure pleasure exactly, what kind of science is it that bases itself on such a measurement? He answers that, "in order to examine general economic equilibrium, this measurement is unnecessary. It is sufficient to ascertain if one pleasure is larger or smaller than another. This is the only fact we need to build a theory" (1898, p. 108). This idea is developed further in the "Sunto" (1900*b*). There Pareto offers an explanation that, as Chipman (1976, p. 75) says, "was not matched again for its clarity until Hicks' exposition 39 years later."[13] Pareto writes that

> In reality and in the most general way, pure economic equations simply express the fact of a choice, and can be obtained independently of the notion of pleasure and pain. This is the most general point of view and also the most rigorous. . . . For us, it is sufficient to note the fact of individual choice, without investigating the psychological or metaphysical implications of such a choice. . . . We do not inquire into the causes of men's actions: the observation of the fact itself is sufficient. . . . Pure economic equations and their consequences exist unchanged whether we start from

[13] Frisch (1932) says that Fisher, in an attempt to give utility a rigorous definition, should be considered the real founder of the theory of choice. In Frisch's view, Pareto developed only the original ideas. It is undeniable that Fisher anticipates the line of reasoning that Pareto fully develops: we may accept Schumpeter's judgment that Fisher "ended up with results (incompletely restated in §8 of Chapter IV) that go far towards the suggestion to do without any kind of utility at all: what is left is a concept that lacks any psychological connotation and contains the germs of all the pieces of apparatus that were to emerge in Pareto's wake" (Schumpeter 1948, p. 221). This "germ" does not fully "germinate" with Fisher, but will grow with Pareto. Fisher's reflection on the concept of utility as a measurable quantity—assumed as a first approximation—is still of fundamental importance. Certainly the considerations contained in the short chap. 4 of *Mathematical Investigations,* concluding Fisher's dissertation, move in a direction that Pareto would develop some years later: to attribute a significance to the ratio between the individual utilities is neither necessary nor "pertinent" to the study of prices and distribution, says Fisher, while it should be relevant for ethical investigations. In effect, Fisher continues, "if we seek only the causation of the *objective facts of prices and commodity distribution* four attributes of utility as a quantity are entirely unessential, (1) that one man's utility can be compared to another's, (2) that for the same individual the marginal utilities at one consumption-combination can be compared with those at another, or at one time with another, (3) even if they could, total utility and gain might not be integratable, (4) even if they were, there would be no need of determining the constants of integration" (p. 89). Fisher's considerations may be considered additional rather than alternative: it is Pareto who is fully conscious of the theoretical innovation. As the French mathematician Wladyslaw Zawadzki, author of an important and thorough investigation of the new economic theories, writes about Fisher: "his point of view is very close to that of choice theory, but the American economist was unable to resolve the difficulties definitively" (p. 142).

> the consideration of pleasure as a quantity, or we limit our investigation . . . exclusively to the fact of choice. [Pareto 1900*b*, pp. 221–24]

To develop his theory of utility on the basis of this point of view, Pareto made use of the indifference curves introduced by Francis Edgeworth in 1881. As distinct from Edgeworth, however, who assumed a measurable utility function from which he derived indifference curves, Pareto starts from the indifference curves themselves, "provided directly by experience." He obtained, "directly from real facts," the same results achieved with the hypothesis of measurable utility.[14]

In his mathematical formulation, Pareto associates a different index with each indifference curve, with an increasing algebraic value for combinations of goods chosen in preference to the initial one: "Among all the index systems there is only one which gives the measurement of the pleasure, all the other systems are only pleasure index systems; that is to say, two equal indexes indicate an equal pleasure, an index greater than another indicates a pleasure greater than that corresponding to the last index" (p. 521). Pareto assumes that "at every point on the plane XY we draw perpendiculars of a length equal to the index of the point considered: the set of the points thus obtained constitutes a surface whose contour lines represent the lines of indifference" (p. 519). In this way he obtains a surface in three-dimensional space, called the "index surface," whose height does not represent utility but is rather only an index of preference in the choice.

An analogy with Fisher's formulation in *Mathematical Investigations* is evident. There, Fisher supposes it possible to create a function that, by means of a monotonic transformation, enables the height on the plane to represent the total utility that an individual can obtain from consumption of the combination of two goods. He obtains a surface equal to Pareto's index surface but substantially different with regard to the initial hypothesis. Pareto's position is more analogous to (and, in contrast to what the literature maintains, precedes) the one affirmed by Poincaré in his letter to Walras of September 30, 1901.[15] In it, the French mathematician points out the limits of

[14] It should be emphasized that Pareto does not exclude the possibility of the measurement of utility; he just does not consider it necessary to elaborate the pure theory of the consumer. In chap. 4 of *Manuale,* he offers a criterion for the measurability of utility that would later be taken up by von Neumann and Morgenstern (1944) (see Chipman 1976, p. 78).

[15] Edgeworth would be the first to recognize the similarity between Pareto's and Poincaré's positions: "Professor Pareto is therefore in very good company [i.e., of Poincaré and of the French economist Etienne Antonelli] when, scrupling to designate utility as a function (say u) of quantities of commodities (say x, y . . .), he

the utility function used by Walras. He asserts that satisfaction is not a measurable quantity though it can be examined mathematically. He introduces the notion of preference as fundamental to that of utility: "I can say that one pleasure is greater than another, because I prefer one to the other. But I cannot say that one pleasure is twice or three times as much as another one. This has no meaning. Only an arbitrary convention could give it meaning" (cited in Jaffé 1965). Later Poincaré affirms the possibility of defining satisfaction by means of an arbitrary mathematical function. This is an ordinal function, inasmuch as the numbers that we can, arbitrarily, associate with it are only representative of the individual's order of preference.[16] As Edgeworth ([1915] 1925, p. 473) notes,

> Poincaré's ruling is in accordance with the view now generally prevalent among mathematicians, that the capacity of numbers to express the results of counting and measuring "may be regarded as a secondary property derived from the more fundamental one of expressing order. Natural numbers form a series with a definite order, and the expressions 'greater than' and 'less than' mean 'more advanced' and 'less advanced' in this order." These are the words of another eminent mathematician, Professor Love.

Pareto, as he would say in 1918 in the article "Economia sperimentale," established a link between his new theoretical position and that of Antoine-Augustin Cournot: "We can obtain more directly from the facts the same results we obtain from the hypothesis of ophelimity. Among the paths which we may take, one was pointed out by Cournot, and another by myself with the indifference curves" (p. 8).

Whereas Pareto's theory obtained the concept of ophelimity directly from facts, Cournot's theory originated "from the facts of the prices of bartered quantities" (p. 8). The Cournot view had been followed by Gustav Cassel in his 1899 essay: there the Swedish economist suggests the thesis that demand functions can be used directly, without passing through those of utility. There is a great difference between assuming that only an ordinal preference scale is needed

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contemplates a family of successive *indifference-curves* . . . in the plane x, y (or corresponding hyper-surface); such that advance from any one indifference-locus to the next in succession affords an *index*, rather than a measure, of the advance in satisfaction, or as Professor Pareto prefers to say, *ophelimity*" ([1915] 1925, p. 473).

[16] As a consequence of Poincaré's observations, Walras would adopt a substantially ordinalist formulation (see "Economie et mecanique" [1909]). But, as Ingrao and Israel (1990), probably correctly, think, Walras's acceptance of ordinalism does not seem fully conscious.

to explain market behavior and Cassel's position that no preference scale is needed at all.[17] Nevertheless, from the point of view of the history of economics, we believe that, despite originating from different assumptions, Cassel's and Pareto's contributions to economics—at the end of the nineteenth and the beginning of the twentieth century, respectively—mark a distinct break in the evolution of the neoclassical theory of value, with the abandonment of extra-economic assumptions in the explanation of economic facts.

IV. Methodology Again: The Significance of Mechanical Analogy in Economics

Of recent and wide discussion among historians of economic thought (Mirowski 1989; Mirowski and Cook 1990) is the meaning of the frequent use of mechanical analogy in economics by marginalist economists. Reference to concepts and models derived from theoretical physics, particularly mechanics, was not so unusual among economists at the end of the nineteenth century. In the fifth book of *Principles,* as a first approximation to the study of biological equilibrium, Marshall uses a concept of mechanical equilibrium corresponding to that "of a stone hanging by an elastic string, or of a number of balls resting against one another in a basin" (1920, p. 323). Walras, too, affirmed that there is an analogy between the classical mechanics of Isaac Newton and Pierre-Simon Laplace and economic equilibrium.

This use of mechanical analogy should not be surprising: at that time mechanics enjoyed the leading role in the sciences. As the Italian mathematician Volterra (1901) pointed out: "Among the physical sciences one has always been the leader, guiding the others whilst they imitated and used it as an example. That science is mechanics, and, together with geometry, it constitutes if not the most brilliant, then the soundest part of the knowledge that glorifies human kind" (p. 443). Fisher went further, attempting to offer "a systematic representation in terms of mechanical interaction of that beautiful and intricate equilibrium which manifests itself on the 'exchanges' of a great city but of which the causes and effects lie far outside" (Fisher 1892, p. 24). Chapter 3 of the second part of *Mathematical Investigations* is entitled "Mechanical Analogies": here there is the famous

[17] Cassel's position has found little favor in economic literature, probably because it has understood that, as Samuelson (1950) wrote, "Cassel . . . rejected utility in favour of demand functions and nothing else, but was never fully aware of what he was thereby assuming or denying about empirical reality" (p. 366, n.) (see also Samuelson 1993).

table of comparison between mechanical and economic equilibria. The analogy is very deep, perhaps too much so, as though mechanics could actually represent a conceptual structure able to describe the market.[18] Fisher writes: "Energy [work] is force times space, just as gain is marginal utility times commodity" (p. 85). Immediately afterward, he introduces the table of comparison (see the Appendix).

The use of mechanical analogy in Pareto is different from that used by Fisher: abstractions and mechanical analogies in Pareto's work have only the function of an instrument, in accordance with his methodological position. From his very first essay on political economy in 1877, "Della logica delle nuove scienze economiche" (On the logic of the new economic sciences), Pareto does not hesitate to say that "economists have gone too far in the comparison of economics to mechanics" (p. 7). Certainly in the *Cours,* Pareto makes wide use of mechanical analogy. But the reason is that "since we have a deep knowledge of the equilibrium of a mechanical system, this system of equilibrium may be used to understand the economic one. In turn, the latter will help us to have an idea of the social one" (§589). The motivation for the reference to the mechanical model in the construction of a new theory is thus apparent. Pareto, however, emphasizes that

> in arguing from analogies there is a stumbling block best avoided. The use of analogies is justifiable only to better explain the sense of a statement; such a procedure may generate serious errors, however, if analogies are used to demonstrate the statement itself or establish some favorable presumption. Examples and analogies should only be employed to clarify a theory. When the terms of the problem are clear, the facts, and only the facts, are capable of demonstrating if the theory is correct or not. [§590]

In a footnote to section 592 of the *Cours,* Pareto outlines, by means of a table (shown here in the Appendix), the analogies between mechanical and social phenomena. Nevertheless, he writes that

[18] Fisher designed and built a machine that mechanically solved the determination of market equilibrium. Barone (1894) writes positively of Fisher's book (as had Edgeworth in the *Economic Journal* in 1893) and the machine: "The originality of this remarkable contribution consists essentially in the fact that, for some problems of pure economy, the author has imagined—and actually built—an apparatus that mechanically provides the solution. This apparatus is neither merely a scientific curiosity nor solely an effective means of illustrating the system of equations which, for these problems, economics employs: it is a real instrument of investigation, because it makes evident what, *caeteris paribus,* are the effects of some causes, and also provides an approximate measurement" (p. 413).

> given a society, we study the relations of production and wealth between men, abstracted from other circumstances. We obtain thus a study of political economy. The science of political economy is divided into two others. If we consider *homo oeconomicus,* who acts only as a result of economic forces we obtain a pure political economy, which studies in abstract terms ophelimity. . . . From pure political economy comes applied political economy, which does not consider solely *homo oeconomicus* but also other models of man closer to reality. Men have also other characteristics which are studied by other particular sciences. . . . In reality, men who are subject only to pure economic stimuli do not exist. . . . Sometimes syntheses are attempted. An attempt has been made to find the cause of all phenomena in utility. . . . An attempt has been made to explain all phenomena in terms of biological evolution. These are all interesting studies. But we must resist these hypotheses and not go far from the solid basis of experience.

Pareto's mature position with regard to the use of mechanical analogies in economics is expressed in the debate with Croce:

> Let us go back to the equations which determine the equilibrium. In seeing them somebody—and it might be the writer—made an observation and said: "These equations do not seem new to me, I know them well, they are old friends. They are the equations of rational mechanics." This is why pure economics is a sort of mechanics or akin to mechanics.
>
> Clearly they are not identical; they are similar: this was even more so with the hedonistic principle, because there was something in pure economics representative of the concept of force in mechanics. With the fact of choice, this "something" disappears. Mechanics can also be studied without the use of force. Essentially, none of this is important. If there is someone who does not wish to hear talk of mechanics, let us avoid to mention the resemblance and concentrate directly on our equations. [1953, p. 185]

In a footnote to section 928 of the *Cours,* Pareto attempts to set down the general conditions of the dynamics of an economic system as an analogy to Jean d'Alembert's principle. However, his assurance of finding a theoretical foundation to the mechanical analogy progressively diminishes. In private conversation, in 1914 (see Vinci

1956), he actually said: "I have made many attempts to find a principle analogous to D'Alembert's, without any result. In trying to find such a principle, we are moving away from reality instead of getting closer to it" (p. 5).

Pareto did believe that the dynamics of an economic system could be studied with the method of "successive equilibria" (*une série d'equilibres statiques*; in Italian, *equilibri successivi*) as a first approximation (which corresponds to Walras's method of variable equilibria). However, in studying "the movement of economic phenomena" without dividing the dynamics of the economy into discrete intervals, Pareto considered the mechanics of the general economic equilibrium insufficient (see book 2, "The Economic Organism" [*L'Organisme économique*] of the *Cours*).

V. Conclusion

Pareto's methodological reflections during the period 1892–1900 offer a new epistemological precept to the "new economic theories," based on the experimental method. As has been shown, his epistemological reflections generated the analytical innovation of the ordinalist hypothesis, the adoption of which undermined the old theory of value and generated the theoretical revolution that would be concluded by Hicks and Allen in the 1930s. In other words, ordinalism is not the logical result of the difficulties of measurement but rather the consequence of a methodological revolution: the adoption of the experimental method, which means the self-conscious attempt to define clearly the conditions for political economy to be a "science." It is not by chance that Pareto felt, like the scientist and epistemologist Poincaré, that a fundamental problem of the new economic theories was the justification of their theoretical premises. Unjustified abstractions exceeded the "correct limits" of the scientific discourse in economics: this was the case with the crucial hypotheses of perfect foresight and rationality, statements that lack a necessary empirical basis.

At the end of the nineteenth century, Vilfredo Pareto examined, more consciously and more lucidly than his contemporaries, the problem of the theoretical premises of economics in an attempt to account for political economy as a science. In this context, instead of simply being interpreted within the circumstances of the construction and development of marginalism, Pareto's contribution should be considered a critical examination of that marginalism, and one perhaps not so irrelevant to the current debate on method in economics.

Appendix

Fisher's and Pareto's Tables of Comparison between Mechanics and Economics

FISHER'S TABLE

In Mechanics		In Economics
A particle	corresponds to	An individual
Space	" "	Commodity
Force	" "	Marg[inal] ut[lity] or disutility
Work	" "	Disutility
Energy	" "	Utility
Work or Energy = force × space.		Disut[ility] or Ut[ility] = marg[inal] ut[ility] × commod[ity].
Force is a vector (directed in space).		Marg[inal] ut[ility] is a vector (directed in com.).
Forces are added by vector addition.		Marg[inal] ut[ilities] are added by vector addition.
Work and Energy are scalars.		Disut[ility] and ut[ility] are scalars.
The *total work* done by a particle in moving from the origin to a given position is the integral of the *resisting forces* along all space axes (resisting forces are those directed toward the origin) multiplied by the distances moved along those axes.		The *total disutility* suffered by an individual in assuming a given position in the "economic world" is the integral of the *marg[inal] disut[ility]* along all commod. axes (marg[inal] disut[ilities] are directed toward the origin) multiplied by the distances moved along those axes.
The "*total energy*" (the work done upon the particle) may be defined as the like integral with respect to *impelling forces.*		The *total utility* enjoyed by the individual is the like integral with respect to *marg. utilities.*
The *net energy* of the particle may be defined as the "total energy" less the "total work."		The net ut[ility] or *gain* of the individual is the "total utility" less the "total disutility."
Equilibrium will be where net energy is maximum; or equilibrium will be where the impel. and resist. forces along each axis will be equal.		Equilibrium will be where gain is maximum; or equilibrium will be where the marg[inal] ut[ility] and marg[inal] disut[ility] along each axis will be equal.
(If "total energy" be subtracted from "total work" instead of vice versa the difference is "*potential*" and is minimum.)		(If "total ut[ility]" be subtracted from "total disut[ility]" instead of vice versa the difference may be called "*loss*" and is minimum.)

SOURCE.—Fisher (1892), pp. 85–86.

Pareto's Table

It may be useful to define a table of analogies between mechanical and social phenomena. These analogies have no value as demonstrations of a theory. They only better explain some statements which must be verified by experience.

Mechanical Phenomenon	Social Phenomenon
Given a certain number of solids, we study their relations of equilibrium and movement abstracted from the other properties. We obtain thus a study of mechanics.	Given a society, we study the relations of production and wealth between men, abstracted from other circumstances. We obtain thus a study of political economy.
The science of mechanics is divided into two others. If we consider inextensibly connected material points we obtain a pure science, rational mechanics, which studies in an abstract way the forces of equilibrium and movement. The easiest part is the science of equilibrium. D'Alambert's principle, considering the forces of inertia, enables the reduction of the dynamic problem to a static one.	The science of political economy is divided into two others. If we consider *homo oeconomicus* who acts only as a result of economic forces we obtain the political economy, which studies in abstract terms ophelimity. The only part of this which is well known is static equilibrium. There may be a principle for economic systems analogous to D'Alambert's, but at present our knowledge is very poor. The theory of economic crisis offers an example of dynamic study.
From rational mechanics comes applied mechanics, which is a little closer to reality, considering elasticity, friction, etc.	From pure political economy comes applied political economy, which does not consider solely *homo oeconomicus,* but also other models of man closer to reality.

PARETO'S TABLE (*Continued*)

Mechanical Phenomenon	Social Phenomenon
Real solids not only have mechanical properties. Physics studies the properties of the phenomenon caused by light, electricity and heat. Chemistry studies other properties. Thermodynamics, like other sciences, studies some of these properties in detail. All these sciences constitute the physical-chemical sciences.	Men have also other characteristics which are studied by other particular sciences, such as law, religion, aesthetics, the organization of society and so on. Some of these have reached quite a high level of development; others, on the contrary, have not. As a whole, they constitute the social sciences.
If we wish to consider a concrete fact, all these sciences must be taken into account because they have been separated through a process of abstraction.	
In reality, solids with only mechanical properties do not exist. It is a mistake to assume the existence of a concrete phenomenon subjected only to mechanical forces, abstracted from chemical ones, just as it is to assume that a concrete phenomenon may be subtracted from the laws of rational mechanics.	In reality, men who are subject only to pure economic stimuli do not exist. It is a mistake to assume the existence of a concrete phenomenon subject only to economic motivations, abstracted from other considerations, just as it is to assume that a concrete phenomenon may be subtracted from the laws of pure economics.
The practice differs from the theory precisely because practice must take into account a quantity of secondary characters which are not studied in the theory. The relative importance of primary and secondary characters is not the same from the general point of view of science and from the particular point of view of a practical operation. Syntheses have sometimes been attempted. An attempt has been made to find the cause of all phenomena in:	
the attraction of atoms. An attempt has been made to reduce all physical and chemical forces to a fundamental unity.	utility, of which ophelimity is simply a type. An attempt has been made to explain all phenomena in terms of biological evolution.
These are all interesting studies. But we must resist these hypotheses and not go far from the solid basis of experience.	

SOURCE.—*Cours*, §592. Our translation.

A Note on the Tables

The difference between the two tables is quite evident. Fisher's table is designed to provide, point by point, a correspondence between mechanics and economics, perhaps too deeply so. On the other hand, Pareto's table concentrates on defining the limits of pure and applied economic science. It is important to note that Pareto considers the analogies worthless "as demonstrations of a theory"; they only "better explain some statements which must be verified by experience." As Varian (1991, p. 595) writes, "it is hardly surprising that some economists, educated in the mathematics of the nineteenth century [and hence] familiar with classical mechanics, energy conservation law, etc. . . . might try to utilize the mathematical techniques developed in physics to clarify concepts in their own science." In this context, a critique, such as that of Mirowski (1989), of the mechanical analogy in neoclassical economics seems largely unhelpful and based on a misunderstanding.

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