2019 | LIBERTAS: SEGUNDA ÉPOCA. VOLUMEN 4. NÚMERO 2.
ISSN: 2524-9312 (online)
www.journallibertas.com

ECONOMIC SCIENCE VS. MATHEMATICAL ECONOMICS: PART I[i]

Juan C. Cachanosky

The greatest claim that can be made for the mathematical method is that is necessarily leads to good economic theory.
George Stigler

[The mathematical method] is an entirely vicious method, starting from false assumptions and leading to fallacious inferences.
Ludwig von Mises

1. Introduction

As we can see in the quotations of these two prestigious economists, Stigler and Mises, the difference in opinion regarding the use of mathematics in economics is not precisely a matter of nuances. Even though this discussion has been going on for over a hundred years, these two positions differ, to put it in mathematical terms, by 180 degrees.

If we were able to infer economic theorems either with mathematics or prose, this topic would not be so relevant; each individual would choose the approach he finds more comfortable. However, the problem is deeper; some mathematical economists have sustained that certain economic theorems can only be demonstrated with the use of mathematics.[1] Other "rhetorical" economists, in particular those of the Austrian school, sustain that mathematics cannot explain the market process.[2] The debate is important because what is being questioned is not the logical rigor of mathematical deductions versus rhetorical analysis, but the *possibility* of using one or another method in economic science.

In this article, I will try to demonstrate that the use of mathematical methods is *impossible* in economics if what the economist wants to do is develop valid pragmatic theories. Of course, anyone is free to practice mental gymnastics by developing unrealistic mathematical models, but this practice should be part of pure mathematics rather than economic science.

Despite Stigler's statement that the mathematical method "necessarily leads to good economic theory," there is a large number of mathematical models that lead to different results; to peruse the journal *Econometrica* is enough. If we follow Stigler, we should conclude that all of them are "necessarily" good theories. A specific study of each one of these models would require a treatise, or maybe a series of volumes, rather than a brief article.

The problem is similar to that of economic central planning. It could be said that there are as many "plans" as planners. To demonstrate the errors of central planning, it is useless to criticize each plan. A new planner can always appear, arguing that "his" plan is different. The criticism, to be effective, needs to address the *essence* of economic central planning, meaning that which is common to all plans. Similarly, there is nothing to be gained by objecting to this or that mathematical model of the economy; therefore, I address the essence of the argument.

This article is divided into three long sections. The first one covers a brief review of the history of mathematics in economics, intended to show how, after more than a hundred years, mathematical economists themselves doubt the validity of their own models. The second one is about the impossibility of applying the same research method in the natural and social sciences. It will be argued that the construction of mathematical

[1] For instance, Stigler (1949) sustains that "without mathematics, one can give only an intuitive proof of complicated relationships, such as those expressed by Euler's theorem, Slutsky's equation, the theory of general equilibrium, and certain theorems in the theory of games."
[2] "The problems of process analysis, i.e., the only economic problems that matter, defy any mathematical approach" (Mises, 1949, p. 356).

models of the economy is equivalent to applying the hypothetical-deductive model used in the natural sciences, which is not viable in economic science. Finally, the purpose of the third section is to show the differences that exist between a verbal and a mathematical deduction and the consequences that those differences have for economic theory.

The two first topics will be discussed in this first part. The third topic is discussed in part 2 (the next article in this issue).[ii]

2. The Evolution of Mathematical Economics

Since the dawn of political economy (whether its founding father is considered to be Adam Smith, Cantillon, or Xenophon) until the last quarter of the 19th century, economists would infer their theorems in prose; very few would use mathematics. Nevertheless, from the late 19th century until today, mathematical economics started to gain a presence to the point where we could say that it is rare to find an economics book that does not use mathematics.

Jevons's (1871) *The Theory of Political Economy* has two appendices (V and VI) that list mathematical economics books covering the 1711–1888 period, the period of low popularity. This list is incomplete[3] and also includes economists that were explicitly opposed to the use of mathematics in economics, such as John S. Mill and Carl Menger.[4]

Despite Jevons's long list, popular precursors of mathematical economics are few. Among these, we first find Daniel Bernoulli (1700–1782), a Swiss mathematician that developed the concepts of marginal utility and decreasing marginal utility with derivatives in an article published in 1730. Thomas Perronet Thompson (1783–1868) published in 1826 an article in the *Westminster Review* (of which he was one of the founders) applying differential calculus to define the maximum profit. In Germany, the mathematician Johann Heinrich von Thünen (1783–1850) also uses calculus in his *The Isolated State in Reference to Agriculture and the National Economy* to develop the idea of marginal product. In France, the two most distinguished precursors were Antoine Cournot (1801–1877) and Jules Dupuit (1804–1866). In 1838, Cournot published his book *Research into the Mathematical Principles of the Theory of Wealth*, where he makes significant use of mathematics and graphs and, like his predecessors, he emphasizes the use of calculus. On the other hand, Dupuit develops the concept of a demand curve with apparent independence from Cournot. His book *On Utility and Demand* (1803) does not include as many equations as Cournot's, but it does include enough of them to consider it a forerunner of mathematical economics.

The discredit that the use of mathematics in economics had at the time can be seen in the following quote by Cournot (1838, Chapter 2, as translated in 1927):

> But the title of this work sets forth not only theoretical researches; it shows also that I intend to apply to them the forms and symbols of mathematical analysis. This is a plan likely, I confess, to draw on me at the outset the condemnation of theorists of repute. With one accord they have set themselves against the use of mathematical forms, and it will doubtless be difficult to overcome to-day a prejudice which thinkers, like Smith and other more modern writers, have contributed to strengthen.

There is no doubt that the most renowned economists of the time have raised objections to the use of mathematics in economics. For instance, regarding those who employed mathematics in economics, Jean-Baptiste Say (1880, p. xvii fn.) argued the following in his *Treatise of Political Economy*:

> Such persons as have pretended to do it, have not been able to enunciate these questions into analytical language, without divesting them of their natural complication, by means of simplifications, and arbitrary suppressions, of which the consequences, not properly estimated, always essentially change the condition of the problem, and pervert all its results;

[3] See Schumpeter (1954, p. 955 n. 3).
[4] In the preface to the second edition of his book (1879), Jevons adds brief comments on the work of mathematical economics precursors.

> so that no other inference can be deduced from such calculations than from formula arbitrary assumed.

We can find similar opinions in Senior (see Bowley, 1937, pp. 52–65), J. S. Mill (1844, Essay V), and John Cairnes (1857, Lecture V). There is no doubt that the use of mathematics in economics is not the central epistemological issue that worried these economists. What these authors wanted to show is that the method used by the natural sciences is inappropriate for the social sciences;[5] the use of mathematics in economics was a byproduct of this topic.

According to Cournot (1838, p. 2), the opposition to the mathematical method is due, in part, to the "false idea which has been formed of this analysis by men otherwise judicious and well versed in the subject of Political Economy, but to whom the mathematical sciences are unfamiliar." This criticism is not very fair. In any case, the opposite conclusion can be reached. Given their education, these "judicious and well versed" men had a better understanding of the natural sciences and mathematics than the understanding that mathematical economists had of economics. After all, we should not forget that Adam Smith, for instance, wrote on the history of astronomy, physics, logic, and metaphysics. These economists had a knowledge broad enough to not confuse the nature and method of the natural and social sciences.[6] Despite all the errors that one may want to point to in the classical economists' work, they produced significant contributions to economic science with the use of prose alone. In the 1870s, the mathematical economists start to gain presence, but, as we will see, they fail to say much more than what the classical economists have already said in a more rigorous form.[7]

In 1871, Jevons published his book *The Theory of Political Economy* in which he makes significant use of mathematics for his time. In the introduction, he defends the mathematical character of economics. Moreover, in the preface to the second edition, he states that "all economic writers must be mathematical so far as they are scientific at all, because they treat of economic quantities, and the relations of such quantities, and all quantities and relations of quantities come within the scope of the mathematics" (Jevons, 1871, p. xx).

This passage shows that Jevons did not have a clear concept of what the object of study of economics was. He thought that "the object of Economics is to maximize happiness by purchasing pleasure, as it were, at the lowest cost of pain" (Jevons, 1871, p. 23). Even though Jevons (1871, pp. 11–12) admits that it was difficult, or even impossible, to directly *measure* the happiness or utility of a person, he thought that it could be measured through its quantitative effects that, according to him, are prices.

This fallacy of prices being the measure of utility is interesting because he was induced to this conclusion through his own mathematical application.[8] The "literary" economists of the Austrian school reach a different conclusion: prices can *never* reflect the marginal utility of the parties in an exchange. The price is the result of utility *disparity*. If the marginal utility of the good that each individual gave up were the same as that of the good he received, then there would be no reason to exchange, and therefore there would be no prices.[9]

It is worth recalling that Jevons did not have good mathematical training, as he himself would have admitted. His knowledge did not go beyond elemental differential calculus. Both Marshall and Cairnes pointed out the problems that this limitation gave Jevons.[10] This comment does not pretend to diminish Jevons, whose position in the history of economic thought is well deserved; it is intended to show that the forerunners of mathematical economics were not mathematical experts but individuals with superficial knowledge of the topic.

Jevons's book did not reach high popularity, in part because he was defying the classical economists, who by that time had reached great reputation with Mill, and in part because of the use of mathematics, which, as we have seen, was not well-regarded by the profession at that time.

---

[5] In the second part of *The Counter-Revolution of Science*, Hayek (1952a) recounts how the idea of applying mathematics to economics was born.

[6] A similar statement to that of Cournot is offered by López Urquía (1972, p. ix) in the prologue to the Spanish edition of Yamane's (1962) *Mathematics for Economists*: "The progress of the Scientific Method has definitely ended the issue of the use of mathematics in Economic Science (an issue in which the opponents of such utilization are usually suspiciously uneducated in mathematics)." Naturally, the knowledge of mathematics is never enough for the economist. However, as we will see below, if we understand the essence of the economic problem and its difference from the natural sciences, then we will need enough knowledge of mathematics to object to its applications in economics as an obstetrician needs to object that babies are born from cabbages. [TN: López Urquía translation is my own.]

[7] Obviously, this statement is the opposite of the one offered by the advocates of mathematical economics.

[8] Jevons (1871, pp. 98–100) reached the conclusion that the relative price of two goods equals the inverse ratio of their marginal utilities.

[9] For a more detailed discussion, see Mises (1912, chap. 2).

[10] See Stigler (1941, pp. 14–15 n. 4).

Marshall had much better mathematical skills than Jevons, to the point that he is referred to as one of the best mathematicians of his generation. However, he is not as categorical as Jevons is regarding the use of mathematics in economics. In a letter to A. L. Bowley, on February 27, 1906, he states that[11]

> a good mathematical theorem dealing with economic hypothesis was very unlikely to be good economics: and I went more and more on the rules—(1) Use mathematics as a shorthand language, rather than as an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life. (5) Burn the mathematics. (6) If you can't succeed in 4, burn 3. This last I did often. (Marshall, 1925, p. 427)

Marshall's *Principles of Economics* reached great popularity. At the end of the 19th century and at the beginning of the 20th century, the *Principles* were like the Bible. Economics was Marshall (Robinson, 1960). Regrettably, he committed some serious mistakes that still make their presence in microeconomic textbooks,[12] and, in some sense, he represents a step backward by defending Ricardo's theory of value (Marshall, 1890). However, the most relevant issue is that he paved the way for future writers to deploy their enthusiasm for the mathematical methods, ignoring his own warnings.

Because Marshall's *Principles* focuses on the analysis of the behavior of economic "agents" (i.e., the consumer and the firm), it gives particular room to what is known as partial equilibrium. The more important developers of this line of analysis were Edward Chamberlain, Joan Robinson, and the Italian Piero Sraffa. This school of thought is known as the Cambridge school because Marshall and his students taught in that particular city.

The fame gained by the Cambridge school almost completely eclipsed another English school of thought that was being developed in London by Edwin Cannan, which kept its exposition in prose and continued with the type of analysis developed by the classical economists, namely, how market forces produce a "spontaneous" order. The most important developers of this school of thought were Lionel Robbins and William H. Hutt. In the 1930s, Hayek joined this group, inserting insights from the Austrian school at the London School of Economics. As we will see later, Hayek had a significant influence on the thought of one of the most renowned mathematical economists, John R. Hicks.

It seems that the most relevant advocate of mathematical economics has been Leon Walras, through what is known as the theory of general equilibrium. This is not because of the success of his *Elements of Pure Economics* (1874) which, as happened with Jevons's and C. Menger's books, was not well received, but rather due to his redemption by later economists.

Walras (1874, p. 47) advocates for the use of mathematics in economics in the preface of his *Elements* and attacks those in opposition.

> As for those economists who do not know any mathematics, who do not even know what is meant by mathematics and yet have taken the stand that mathematics cannot possibly serve to elucidate economic principles, let them go their way repeating that "human liberty will *never* allow itself to be cast into equations" or that "mathematics ignores frictions which are *everything* in social science" and other equally forceful and flowery phrases. They can never prevent the theory of the determination of prices under free competition from becoming a mathematical theory.

In this passage, Walras is not only sustaining that economics is an exact science,[13] he is also, like Cournot, complaining that those who object to his approach are uneducated on mathematics. However, this seems to be more a problem of Walras than of the literary economists. Like Jevons, Walras was not well trained in mathematics. He applied twice to the famous *École Polytechnique* but failed due to his lack of mathematical skills (Jaffé, 1977b). He finally entered the *École des Mines* as an engineering student, which he did not enjoy

[11] Compare with Stigler's statement at the beginning of this article.
[12] See Rothbard (1962, pp. 304–306).
[13] To avoid any doubt, he continues: "In any case, the establishment sooner or later of economics as an exact science is no longer in our hands and need not concern us. It is already perfectly clear that economics, like astronomy and mechanics, is both an empirical and a rational science" (p. 48).

and soon abandoned to dedicate himself to literature. His father, Antoine-Auguste, who was an economist and was worried about the bohemian character of his son, finally convinced him to study economics.

His economics training was similar to his mathematics training. In economic theory, he had only one professor: his father, and in other topics he was self-trained (Jaffé, 1977a). Just as he believed that without proper math skills his knowledge on the subject was superior to those of the literary economists, he also believed his economic knowledge was superior: "I am not an economist, but I know economics better than economists do" (Jaffé, 1935, p. 196).[14]

Walras's general equilibrium theory is built on systems of lineal equations where prices, quantity of goods and quantity of services used in the production of each good (technical coefficients of production) are the unknowns.[15] Walras proceeds to show that the number of *independent* equations is the same as the number of unknowns, but neither he nor his disciples provide, from a mathematical point of view, a rigorous proof of their ideas. They thought that there was a solution because the number of equations and unknowns were the same, which is false. The equal number of equations and unknowns is neither necessary nor sufficient for the system of equations to have an equilibrium.

Walras's argument not only had this issue of lack of mathematical rigor, it also had shortcomings in his economic theory. The assumptions in his "model" are those of perfect competition. There is an atomistic market, perfect divisibility and mobility of goods and factors of production, there is no time (everything happens instantaneously), products are homogeneous, and economic agents have perfect information. In summary, because the assumptions are unrealistic, so are his conclusions.[16]

It seems that Walras was aware of these issues. In a letter to mathematician D'Ocagne, he says: "[…] I consider my work both from the economic and the mathematical points of view simply an incomplete sketch. I hope that in the near future it will be superseded by other work more complete and better done" (Jaffé, 1935).

Until the 1930s, the economists of the Lausanne school did not introduce major changes. Everyone continued to be satisfied by calculating equations and unknowns and maintaining the unrealistic assumptions of perfect competition. Furthermore, in some cases there was a frank theoretical step backward. At least Walras had used the theory of marginal utility to derive the demand equations; Gustav Cassel, by contrast, omitted this step, which is of major importance. However, neither of them reached conclusions different than the classical economists. And if we take into account that the purpose of a theory is to allow us to comprehend how part of reality works, then the classical economists, despite all of their shortcomings and inconsistencies, were superior to Walras and his disciples. The classical economists had put the emphasis on explaining the "process" of market adjustment; the Lausanne school, on the contrary, due to its unrealistic assumptions, remained in equilibrium analysis. Walras's theory of *tâtonnements* [tatonnement] is nothing more than a repetition of what Adam Smith had already explained in a more clear and realistic way.[17]

The general equilibrium theory starts to be enhanced from the mathematical point of view in the 1930s, but there are no advances with respect to its realism. These enhancements are achieved through two groups, one born in Austria and the other one in England. The first one concerned itself with demonstrating the existence and uniqueness of equilibrium in Walras's equilibrium system. The second one studied the problems of stability and comparative statics.

In Austria everything started with the work of three authors—F. Zeuthen (1932), H. Neisser (1932), and H. von Stackelberg (1933)—that, independent from each other, showed that the determination of equilibrium requires something more than an equal number of equations and unknowns. However, it was Karl Schlesinger,

---

[14] Walras's craving for recognition and the depression this caused is well known: "[…] when he found out that W. Stanley Jevons had surpassed him in the discovery of marginal utility he was discouraged. Walras honorably recognized Jevons's priority […] However, his jealousy continued to bother him until he discovered that the German economist Gossen, already deceased and therefore without a chance to become a rival, had anticipated Jevons in 1854 as much as he did himself" (Jaffé, 1977b) [TN: Translation is my own.]

[15] For a simple mathematical exposition, see Hansen (1970, chap. 3).

[16] The next section discusses this issue in more detail.

[17] For Adam Smith, the market price made demand equal supply even if it was different from its natural or final equilibrium. In Walras's *tâtonnement* theory, this is not a clear point. For this economist, if prices differ from those of equilibrium, there is a difference between demand and supply. Therefore, while for Smith the market force that makes the market converge to its final equilibrium is the difference between the natural price and the market price, for Walras it is between demand and supply. This point is of great significance because Smith's point of view, even if he did not see it because of his faulty theory of the value of exchange, allows us to better comprehend the market process and function of prices. Walras's mistake is still present today among the general equilibrium theorists. See, for instance, the "rules" of analysis that K. J. Arrow and F. H. Hahn give in their *General Competitive Analysis* (1971, p. 39). [TN: page number corresponds to the Spanish edition by Fondo de Cultura Económica)]. The error is more notorious in the case of Nikaido (1978, pp. 343–344) because he introduces Smith's famous "invisible hand" to explain the adjustment of prices in Walrasian terms.

who took part in Ludwig von Mises's private seminar, who saw the mathematical complexity of the issue (Arrow, 1974, p. 319). However, his mathematical skills were not enough to solve this problem. Oskar Morgenstern, another member of Mises's private seminar, put him in contact with Karl Menger,[18] who was a famous mathematician who directed a mathematics colloquium in Vienna, and with Abraham Wald, a student of K. Menger.

Schlesinger modified Walras and Cassel's system of equations assuming a surplus of some factors of production, which transformed some of the equations in the Walras-Cassel system into inequalities. This way, the argument built on the number of equations and unknowns became more complicated. It was Wald (1933, 1934, 1936) who showed, in a series of articles, the existence of equilibrium in alternative systems.[19]

Wald's proofs marked a new era in mathematical economics. According to K. Menger:

> […] with Wald's work we bring to a close the period in which economists simply *formulated* equations, without concern for the existence or uniqueness of their solutions or at best, made sure that the number of equations and unknowns be equal (something that is neither necessary nor sufficient for solvability and uniqueness). In the future as the economists formulate equations and concern themselves with their solution (as the physicists have long done) they will have to deal explicitly with the deep mathematical questions of existence and uniqueness. (cited by Weintraub, 1983, p. 9)

Wald's contribution was limited to the mathematical rigor of the general equilibrium model, yet, the unrealism of the theory remained. The two main assumptions are: (a) that the axiom of revealed preferences holds, and (b) that all goods are substitutes. Both assumptions are enough to invalidate the theory in terms of reality and reduce it to a good mental exercise of pure mathematics.

An alternative proof to Wald's was given by John von Neumann. This famous mathematician used mathematical instruments from game theory, developed by himself in 1928 (Neumann, 1928) and jointly expanded with Oskar Morgenstern in 1944 (Neumann and Morgenstern, 1944), and applied them to the theory of general equilibrium in an article on balanced economic growth (Neumann, 1937).[iii]

Von Neumann's proof is based on a generalization of Brouwer's fixed point theorem.[20] A few years later, another mathematician, Kakutani (1941), simplified von Neumann's theorem and in 1950, Nash, a mathematician from Princeton, generalized the Neumann-Morgenstern game theory to the case of n players and n strategies (Nash, 1950). This generalization uses Kakutani's fixed point theorem to prove that a game with n players has an equilibrium.

At the same time as the rise of game theory, models of linear programming start to be developed, which will eventually play an important role in the problem of the existence of equilibrium. In mid-1949, there was a linear programming conference in Chicago. The works presented at this conference were published by Tjalling Koopmans in 1951 in the famous book *Activity Analysis of Production and Allocations*. Given that game theory is equivalent to a problem of linear programming, both approaches were complemented.

Simple and more general proofs on the existence of equilibrium were developed on these foundations. These authors were McKenzie (1954, 1959, 1961), Arrow and Debreu (1954), Debreu (1959, 1962), and Nikaido (1956).

An area of study independent from general equilibrium developed in the Anglo-American world: comparative statics and equilibrium stability. John R. Hicks (1939) is considered to be the founder of this trend with his book *Value and Capital*. The book was a great success, because it was written in the typical clear "prose" of Oxford economists (Weintraub, 1983, p. 20). This gave Hicks a great advantage over the work coming from K. Menger's colloquium that, besides being written in German, had much less prose and more mathematical formalizations. The problem of "communication" played an important role.

In April 1941, Paul A. Samuelson offered a new contribution with an article on the stability of equilibrium, and a few years later he published his book, *Foundations of Economic Analysis* (1947). Neither the article nor the book mentions the problem of the existence of equilibrium that captured the attention of the Vienna group.

---

[18] Son of Carl Menger, founder of the Austrian school of economics.
[19] Wald (1936) was translated to English and published in *Econometrica* (Wald, 1951).
[20] This is a theorem from a branch of mathematics known as topology, developed by the renowned Dutch mathematician L. E. J. Brouwer (1881–1966).

This line of research was further developed by Mosak (1944), Metzler (1945), Negishi (1962), Hahn and Negishi (1962), and Uzawa (1962). However, the Vienna group, mostly formed by mathematicians rather than economists, looked at the Anglo-American group with disagreement over their use of mathematics, which is reflected in the following comment from von Neumann to Morgenstern:

> You know, Oskar, if these books are unearthed sometime a few hundred years hence, people will not believe that they were written in our time. Rather they will think that they are contemporary with Newton, so primitive is their mathematics. Economics is still simply a million miles away from the state in which an advanced science is, such as physics (Morgenstern, 1976, p. 810).

Yet, we should also remain skeptical about the limited or "primitive" economics knowledge in the Vienna mathematicians, whose "models," as we have already said, implied a step backward with respect to some classical economists. The assumptions of these models distort the practical validity that every theory should have. In particular, the assumption of perfect information that these models use changes the "nature" of economics' object of study. "Any approach," says Hayek (1948, p. 91), "such as that of mathematical economics with its simultaneous equations, which in effect start from the assumption that people's *knowledge* corresponds with the objective *facts* of the situation, systematically leaves out what is our main task to explain."[21]

Koopmans (1957, pp. 146–147, italics are mine) acknowledges this problem present in the general equilibrium theory.

> To my knowledge no formal model of resource allocation through competitive markets has been developed, which recognizes ignorance about all decision makers' future actions, preferences, or states of technological information as the main source of uncertainty confronting each individual decision maker, and which at the same time acknowledges the fact that forward markets on which anticipations and intentions could be tested and adjusted do not exist in sufficient variety and with a sufficient span of foresight to make presently developed theory regarding the efficiency of competitive markets applicable. *If this judgment is correct, our economic knowledge has not yet been carried to the point where it sheds much light on the core problem of the economic organization of society: the problem of how to face and deal with uncertainty. In particular, the economics profession is not ready to speak with anything approaching scientific authority on the economic aspects of the issue of individual versus collective enterprise which divides mankind in our time*.

This statement is from 1957, which means that since Walras wrote his *Élements* in 1874, eighty-four years went by in which the mathematical economists were preoccupied with studying the existence, uniqueness, and stability of a useless model. If, as Koopman's last sentence implies, he believes that the dilemma between individual or collective enterprise can be clarified with general equilibrium models, he has erred. As Hayek (1948, p. 104) stated: "The argument in favor of competition does not rest on the conditions that would exist if it were perfect."[22]

Once the stage of searching for the existence, uniqueness, and stability of equilibrium ended, a second stage began in which they tried to adopt more realistic assumptions by adding uncertainty. The Grunberg and Modigliani paper, "The Predictability of Social Events" (1958), played an important role in this subject. In general, these models try to explain expectations as an extrapolation of past data. An alternative approach is provided by the hypothesis of rational expectations, which assume that economic agent expectations are

[21] Hayek's article is on the subject of imperfect knowledge in economics. James M. Buchanan (1964, p. 213) offers a good analysis of this topic, where he protests, saying that economists "[…] should, I think, face up to their basic responsibility; they should at least try to know their subject matter."

[22] Hayek's statement can be contrasted with Cornblit's (1984, p. 224) [TN: Translation is my own]: "[...] the development of a system such Walras's was a step forward in the direction of giving plausibility to a liberal economic system." We should also remember that some socialist economists used Walras's model to try to reply to Mises, which brought back the idea of the possibility of economic calculation in a socialist society. See, for instance, Lange and Taylor (1938). Schumpeter (1954, p. 989 n. 12) agrees with Lange and Taylor. It seems, then, that the Walrasian model is ambivalent, that it can be used to defend either a competitive or a socialist economy. In summary, it seems to be a useless model to resolve the dispute.

formed taking into consideration the interrelation of the variables according to the appropriate economic theory (Muth 1961). The theory of rational expectations starts from assumptions as unrealistic as those of the general equilibrium: (1) all agents are predefined as smart and (2) markets are in continuous equilibrium.[23] Both assumptions set aside precisely the problem to be explained: resource allocation in a market of imperfect information.[24]

General equilibrium theorists tried to introduce uncertainty in their models. This research starts with Debreu (1959, chap. 7), two years after the above statement by Koopmans. Twelve years later, Arrow and Hahn published their *General Competitive Analysis* (1971), where they had to admit on several occasions that they did not incorporate uncertainty into the alternative models they analyzed.[25]

Hahn (1981, p. 126, italics are mine) seems to arrive at the same conclusion as Koopmans:

> […] General Equilibrium Theory is an abstract answer to an abstract and important question: Can a decentralized economy relying only on price signals for market information be orderly? The answer of General Equilibrium Theory is clear and definitive: One can describe such an economy with these properties. *But this of course does not mean any actual economy has been described*. An important and interesting theoretical question has been answered and in the first instance that is all that has been done. This is a considerable intellectual achievement, *but it is clear that for praxis a great deal more argument is required*.

On the other hand, Morgenstern (1972) offers a critique of contemporary economic theory that, because it belongs to a member of K. Menger's mathematical colloquium and Mises's private seminar, is of special significance. Morgenstern's thirteen points are enough to question the validity of quite a few mathematical models in economic theory.

We see, then, that general equilibrium theorists themselves question the practical validity of their models and, if we recall that the purpose of any theory is to explain reality, then what is being questioned is their theoretical validity; in other words, the consistency and logical rigor of the model does not imply theoretical validity.

For decades, the Austrian economists Ludwig von Mises and F. A. Hayek have "verbally" analyzed the logical implications of human behavior under real conditions that, it could be said, are the opposite of the general equilibrium models. This method let them develop theories of high explanatory power. We are not going to offer a defense of Austrian theory here;[26] but it is interesting to quote a paragraph from one of the general equilibrium theorists, J. R. Hicks (1973, p. 12, n. 1), where he retracts his previous position and highlights the superiority of the Austrian school economists:[27]

> In my [book's] sub-title, and in the text of Chapter I, I have proclaimed the 'Austrian' affiliation of my ideas; the tribute to Böhm-Bawerk, and to his followers, is a tribute that I am proud to make. I am writing in their tradition; yet I have realized, as my work continued, that it is a wider and bigger tradition than at first appeared. The 'Austrians' were not a peculiar sect, out of the mainstream; there were in the main stream; it was the others who were out of it.

---

[23] Willes's (1981) defense that "rational expectations and equilibrium modeling are unrealistic" but yet produce realistic results is similar to Friedman's (1953) position in *The Methodology of Positive Economics*. The problems of this epistemological position will be studied in the next section.

[24] The idea of rational expectations is hardly novel. See, for instance, Popper (1957), who points out that the idea is a very old one and suggests "Oedipus effect" as its name.

[25] After developing the problem of temporal equilibrium, Arros and Hahn (1971, p. 151) observe: "Of course, we are neglecting uncertainty. This is a more serious problem than we might think […]." In chapter 12, they study the qualitative properties of prices in *tatônement*, but remind the reader that "[…] the formal structure of our economy is ill-suited in its present form to accommodate what we would regard as a sensible theory of money. There is no uncertainty, and above all, we have given the economy no time sequence of transactions" (p. 302). It should be noted that in the section on equilibrium under uncertainty, the use of mathematics is significantly reduced. Nonetheless, the issue is treated in such a marginal way that it hardly represents a new development.

[26] For such a defense see Kirzner (1973, 1979), Lachmann (1977, parts II and III), O'Driscoll (1977), and Rizzo (1979). We should also remember that Knight (1921) offered a brilliant critique to perfect competition models in his *Risk, Uncertainty and Profits.* Regrettably, that future representative of the Chicago school would forget Knight ideas, in particular Friedman's *Price Theory* (1962) and Stigler's *Theory of Price* (1942).

[27] Hicks (1979, p. 63) says somewhere else: "[…] I think I have shown why I now rate Walras and Pareto, who were my first loves, so much below Menger. I hope I have shown how much I have got from him, and from thinking about him."

As we can see, there are a number of important general equilibrium economists that expressed their misgivings with respect to the validity of these mathematical models. Of course, we should not take this as "proof" that mathematical economics is useless. The conclusion we can draw from this first section is that the history of mathematical economics has had thinkers with weak knowledge of mathematics that were opposed to its use in economic science, such as C. Menger and David Novick (1954), and others that advocated for it, such as Jevons and Walras. On the contrary, there have been thinkers with a solid knowledge of mathematics that have defended its use in economics, such as Koopmans (1954) and Samuelson (1952), and others that opposed it, such as Keynes (1936, p. 264) and Eugenio Frola and Bruno Leoni (1977). We could also cite a skilled mathematician such as Thom (1984, p. 141), who expressed his skepticism regarding mathematical economics:[iv]

> In physiology, in ethology, in psychology, and in social sciences, mathematics are hardly present if not in the form of statistical recipes whose own legitimacy is suspect; there is only one exception: mathematical economics, with the Walras-Pareto models of economic exchange, which leads to posing interesting theoretical problems but, its applicability to the real economy is more than suspect.

We see then that there are as many experts as nonexperts in mathematics that oppose or support the use of mathematics in economics. Yet, the doubts that some mathematical economists have raised regarding the practical validity of their models diminish the strength of the argument in favor the superiority of the mathematical method over deduction in prose. On the other hand, mathematical economics has popularized a number of grossly false theories; just to name a few, we have: (a) the investment multipliers, (b) the acceleration principle, (c) the turnpike theorem, (d) the theory that perfect competition is more efficient than monopoly at equal cost, (e) indifference curves, (f) the theory of revealed preferences, etc. The problem seems more serious when instead of producing "new" false theories, what is done is to resurrect errors that are more than a century old, as in the cases of Samuelson (1951) and Georgescu-Roegen (1951), who reach the conclusion that in certain cases technology determines relative prices totally independent of demand, which implies not only returning to the classical economist's theory of the value of exchange, but furthermore to not having fully understood the theory of marginal utility.

No doubt Chiang (1977, pp. 16–17) has been quite correct when stating that "the economist with mathematical training is exposed to two temptations: (1) to limit himself to the problems that *can* be solved mathematically, and (2) to adopt inadequate economic assumptions for the sake of mathematical convenience."[28, v] It seems that the second temptation has won over the mathematical economists to the point where any resemblance of their models to reality is by pure chance. Mathematical economists have ended up pursuing an object of study that would adapt to the use of mathematics instead of looking for the proper method of economics.

3. The Method of Economics

Most economists believe that economic science should use the same method as the natural sciences. For instance, Samuelson (1952, p. 61) states that there "are not separate methodological problems that face the social scientist different in kind from those that face any other scientist," and Friedman (1953, p. 10) believes that even though the economist cannot perform lab experiments, this "does not […] reflect a basic difference between the social and physical sciences." This is an important mistake we will try to analyze in this section.

The elaboration of a theory consists in a mental ordering and relation of facts in a way that certain events can be inferred from other events. A theory is good when events occur in accordance with the relationship

[28] Champernowne (1954, p. 369) offers a similar opinion: "Unfortunately for the cautious theorist, his economic models will be judged according to the degree which they appear to be relevant to the real world; so that in avoiding the appearance of being wrong, he may yet appear silly by publishing a long article whose relevance to any practical issue seems to be superficial. This danger of manufacturing mere 'toys' is especially great since the assumptions which are most convenient for model-building are seldom those which are most appropriate to the real world."

established in the theory. A theory is bad when it loses explanatory power, namely, when events occur in a way different from that predicted by the theory.

The elaboration of a theory starts when we face an event whose occurrence we do not understand. To explain it, we try to relate it to another one that we generally call the "cause,"[29] such that every time the cause is present, so will be the observed event. To find the relationship one must begin suggesting some type of connection between two events. These suggestions are called *hypotheses*, which are nothing more than tentative explanations. When proposing a hypothesis, the scientist has to choose some facts that he considers significant and reject others that he thinks are irrelevant. For instance, the color of my desk does not seem to be significant in explaining the velocity at which an object falls.

In the formulation of a hypothesis, there are no fixed rules for selecting significant effects; previous experience and analogies[30] can play an important role, but, above all, what counts is the genius of the scientist in relating two factors that no one had thought of putting together before.

The formulation of a hypothesis implies some rational process; the mind is not a *tabula rasa* upon which reality implants knowledge, as the English empiricists used to believe. We always approximate reality with some preconceived "theory"—that is, with a hypothesis. Even those who consider themselves "pragmatic" have a definite theory of what they do. As J. S. Mill (1844, p. 142) said: "[…] those who disavow theory cannot make one step without theorizing."

The following paragraph by Morris R. Cohen (1931, p. 17) clearly summarizes this idea:

> Observation unillumined by theoretic reason is sterile. Indeed, without a well-reasoned anticipation or hypothesis of what we expect to find there is no definite object to look for, and no test as to what is relevant into our search. Wisdom does not come to those who gape at nature with an empty head. Fruitful observation depends not as Bacon thought upon the absence of bias or anticipatory ideas, but rather on a logical multiplication of them so that having many possibilities in mind we are better prepared to direct our attention to what others have never thought of as within the field of possibility.

This point of view is the opposite of the one that predominated in the philosophy of science in the mid-19th century. At that time, it was believed that scientific investigations started with a prejudgment-free observation. Universal laws should be inductively inferred from this observation. Karl Popper has insisted on the error of this inductivist position.[31] It is impossible to make a general induction without a prejudgment; there is a "prejudgment" from the moment of choosing some variables and ignoring others. One is suggesting that the selected variables may be the "cause" of the event that is to be explained. It is curious (or maybe not) that still in 1956 Samuelson (p. 57) sustained the primitive idea that "[e]very science is based squarely on induction—on observation of empirical facts."

Once a scientist makes a suggestion about the cause of an event, i.e., formulates a hypothesis, the second step consists in testing it empirically. The function of the empirical test is to determine if the hypothesis is refuted by the facts. There are four possibilities: (1) Every time factor A is present so is factor B,

[29] For positivists, the term "cause" includes some primitive animism. Modern physics prefers to talk about relationships between variables because it is impossible to know which is the cause of which. Even though this is true, causality is still a useful mental category used by men to order the facts of reality.

[30] On the role that analogies played in the natural sciences, see Nagel (1961, pp. 109–118). Also Cohen and Nagel (1934, pp. 40–41).

[31] Popper (1935). See also Cohen (1931, pp. 115–116), in particular the following paragraphs: "If induction is generalizing from a few instances or [a] sample [...] the character of the whole class, the following is a clear case of induction. I observe that X, Y, and Z, suffering from pneumonia, have all been cured by serum A, and I infer that all pneumonia patients will be so cured. Here the conclusion will obviously be true only if X, Y, and Z are typical samples of the whole class of pneumonia patients and do not form a special class having some distinctive trait that makes the serum effective only in their case. The generalization therefore that the serum is effective in all case[s] of pneumonia assumes that the class of pneumonia patients is, in this respect, homogeneous. If this is made explicit, we have the orthodox syllogism:

(a) A cure for X, Y, and Z is a cure for all pneumonia patients.
(b) Serum A is a cure for X, Y, and Z.
(c) Therefore serum A is a cure for all pneumonia patients.

The fact that the first premise is not usually made explicit is an important linguistic and psychological fact. It is not, however, relevant to the question as to what premises will logically warrant the conclusion.

If an inductive inference is valid, it must conform to the condition of all valid inference[s]. If the latter is called deduction, induction is not its antithesis but a special form of it. The fact that induction and deduction are separate words does not prove that they must be antithetic."

abbreviated (AB); (2) every time A is present, B is absent ($A\cancel{B}$); (3) every time A is absent, B is present ($\cancel{A}B$); and (4) every time A is absent, B is also absent ($\cancel{AB}$). If, for instance, we want to show that A is the cause of B, then, in empirical tests or experiments we have to observe (1) and (4), and (2) and (3) should not occur. If there is no way to test a hypothesis, then it cannot be scientific.[32]

In the 1930s, it was already clear that empirical tests are only useful to refute but not to "prove" a hypothesis (Cohen, 1931, p. 82).[33] This is because, from a strictly logical viewpoint, we cannot affirm that a hypothesis is true because its conclusions agree with experience; in fact, to do so would be a logical fallacy. Take, for instance, the following syllogism: "If A is true, then B is true; A is true, therefore B is true." To conclude that B is true we need to affirm that A is true (*modus ponendo ponens*): then the conclusion will be logically valid. But, what happens if we change the syllogism in the following way? "If A is true, then B is true; B is true, therefore A is true." Instead of affirming the antecedent A to be able to affirm the consequent B, we are proceeding backwards; we are sustaining that the antecedent is true from the affirmation of the consequent. Yet, this is not valid logical reasoning. See, for instance, the following example: "If that liquid is blood, then that liquid is red"; "that liquid is blood," therefore, "that liquid is red." If we affirm the antecedent "that liquid is blood," we can affirm with logical necessity the consequent "that liquid is red." On the contrary, the following syllogism is not valid: "If that liquid is blood, then that liquid is red"; "that liquid is red," therefore, "that liquid is blood." Here we are affirming the antecedent from the consequent, but there is no logical necessity. We cannot conclude that a liquid is blood because it is the color red.

What we can do with logical necessity is *deny* the antecedent from the *negation* of the consequent (*modus tollendo tollens*). Thus, the following reasoning is logically valid: "If A is true, then B is true, B is *not* true, therefore A is *not* true." In our example: "If that liquid is blood, then that liquid is red; that liquid is *not* red, therefore that liquid is *not* blood." In this way, we can conclude that if we find a liquid that is not red, we can *refute* that it is blood, but if we find one that it is red, we can only conclude that there is a *probability* that is blood. In summary, we can say that there is a logical necessity in the refutation, but not in the verification [of a hypothesis].

When scientists propose a hypothesis from which they deduce conclusions with the purpose of explaining a certain phenomenon, they are formulating a syllogism of the type: "If A, then B." The antecedent A is the hypothesis and the consequent B is the conclusion. In the natural sciences in general, hypotheses are not directly verifiable; that is, the scientist cannot affirm the antecedent. For instance, the atom, electrons, [and] waves are not observable objects; the hypotheses of Newtonian laws or of Einstein's theory of relativity are not directly verifiable.[34]

The way to empirically test a hypothesis is indirectly, by observing if its conclusions are verified or not by reality. However, because of what was mentioned earlier, if the conclusions agree with reality then the hypothesis is not "verified" or "proven," as we cannot confirm the antecedent (hypothesis) from the affirmation of the consequent (conclusion). Instead, if the conclusions are not verified in reality, then the hypothesis (that is, the antecedent) can be negated or refuted with logical necessity invoking the *modus tollendo tollens*. The scientist cannot prove a hypothesis, he can only reject it.

One hypothesis replaces another when it is more general, i.e., when it explains a larger number of cases. Advancement in the natural sciences happens when facts are found that refute an established hypothesis (Cohen, 1931, pp. 83–88; Popper 1963, chap. 1). When this happens, scientists are obligated to substitute or perfect the hypothesis. The new hypothesis should be able to explain the old and new phenomena; therefore, it is more general. The history of physics allows us to see the continuous reformulation of hypotheses; perhaps the most revolutionary examples are Einstein's theory of relativity and Planck's quantum mechanics.[35]

The fact that two or more hypotheses are logically possible means that neither of them can be rejected through rational analysis. Therefore, the experiment or observation is crucially important to refute a

[32] For a detailed exposition, see Popper (1963, chap. 1).

[33] Also, Mises (1933, p. 31) sustained that "[…] a theory that does not appear to be contradicted by experience is by no means to be regarded as conclusively established." And Popper: "[…] I shall certainly admit a system as empirical or scientific only if it is capable of being *tested* by experience. These considerations suggest that not the *verifiability* but the *falsifiability* of a system is to be taken as a criterion of demarcation" (p. 18). And "[m]y proposal is based upon an *asymmetry* between verifiability and falsifiability: an asymmetry which results from the logical form of universal statements" (p. 19).

[34] Even when the hypothesis is observable, we cannot infer an apodictic certainty of the hypothesis because it is impossible to know if we are taking into account *all* the relevant variables. If, for instance, someone observes that every time he has beer then he has a headache, he could conclude that beer is the cause of the headache. However, the headache may be due to some chemical component in the beer.

[35] For a detailed exposition of the history of physics, see Holton (1952) and Gamow (1961, 1966).

hypothesis. Without experimentation or observation, the natural sciences would be impossible, because there would be no way to reject alternative hypotheses. As M. R. Cohen (1931, p. 82) said, "[…] though no number of single experiments and observations can prove an hypothesis to be true, they are necessary to decide as to which of the two hypotheses is the preferable as showing greater agreement with the order of existence."

J. S. Mill (1843) developed five experimental methods. He thought they were useful to *discover* and *prove* cause-and-effect relationships. However, as we have already said, we cannot discover relationships through mere observation; the researcher always observes reality with some hypothesis in mind. We also saw that hypotheses cannot be proven, only refuted. Even though Mill's methods are not useful for the objectives that he proposed, they are useful for rejecting alternative hypotheses. The five methods are the following: (1) agreement, (2) difference, (3) agreement and difference, (4) residue, and (5) concomitant variation.

A brief analysis of these five methods of experimentation will help us to better comprehend why it is only possible to refute a hypothesis and not to prove it. It will also highlight the importance of isolating variables in experiments and observations.

1. The method of direct agreement: *If two or more cases of the event under investigation have only one common factor, then this factor is the cause (or the effect) of the event.*

Table 1. Direct method of agreement

Case number	Event	Observed factors				
1	s	A		C	D	
2	s	A	B		D	E
3	s	A	B		D	
4	s		B	C	D	E
5	s	A		C	D	

In this example, the cause of the event s could be factor D.

The hypothesis can be accepted until we find a case in which event s is present but factor D is absent. If this happens, then the hypothesis is refuted and the investigator would have to look for a "hidden" factor that is present in all cases. If he finds one, we will be in the presence of a new hypothesis that will be valid until it is refuted by a new factor that forces us to look for another factor.

It is clear in this method that the scientist has to be able to control the experiment to isolate the factor under study; or, at least, the observation, when there is no possibility of control, should allow us to study the event with only one factor in common. If in all observed cases there are two or more common factors and it is not possible to observe each one separately, then the direct method of agreement is not useful, since there is no way to know which common factor is the actual cause.

2. The method of difference: *If in two cases, one in which the event under study is present and one in which it is absent, we observe that all factors except one are common, then the noncommon factor is the cause (or the effect) of the event.*

Table 2. The method of difference

Case number	Event	Observed factors				
1	s	A	B	C	D	E
2	s	A		C	D	E

Here, the possible cause of event S is factor B. The hypothesis may be accepted until we run into a case where event S occurs but factor B is absent, which will force a search for the hidden variable. Like in the previous method, the researcher has to be able to isolate or observe the factor under study in isolation; that is, it should be the only factor present and absent when event S does and does not occur, respectively. If this isolation is not possible, and various factors are present and absent at the same time, then the method is not useful.

3. The joint method of agreement and difference: *This consists of the simultaneous use of the two previous methods and is useful when neither by itself allows us to isolate the factor under study.*[vi]

Table 3. The joint method of agreement and difference

	Method of agreement					Method of difference				
Case number	Event	Observed factors				Event	Observed factors			
1	s	A	B	C	D	s	A	B	C	D
2	s	A		C				B	C	

In this example, the method of agreement, by itself, allows us to reject factors B and D, but it does not allow us to decide between factors A and C. With the method of difference, we can reject factors B and C, but we cannot decide between factors A and D. However, both methods together allow us to conclude that the possible cause is A. Obviously, this joint method also requires that the phenomena can be controlled or observed such that all but one factor can be rejected.

4. The method of residue: *If we subtract from any event the part that we know is the effect of certain parts of the event, then the residue of the event is the effect of the remaining parts.*

Table 4. The method of residue

Event	Observed factors
s = a + b + c	A, B, C

If we know that event s is composed of a, b, and c, and if we also know that B is the cause of b and C is the cause of c, then A is the cause of a. This method differs from the previous ones in that it does not require the observation of two or more events: one [observation] is enough. Similar to the previous methods, there is the risk of a hidden variable. In addition, if, after subtracting the known part, the residual is composed of more than one factor, then this method is not useful.

These four methods are all *qualitative*, i.e., they are limited to observing the joint presence or absence of events and their potential causes. Furthermore, one must keep in mind that the given examples assume that the hypothesis, i.e., the cause, is observable. Yet, as we have already said, the most important hypotheses of the natural sciences, physics in particular, are not observable. One of the most cited examples is Einstein's prediction that the light from a star would be deviated by the gravitational force of the sun; he even calculated the maximum deviation. In this case, there is no way to control [all] the variables and, furthermore, the hypothesis that the sun has gravitational force is not observable in itself, but only through its effects (or logical consequences). However, even though it was not possible to control for [all] the variables, Einstein's prediction could be subjected to an empirical test during a solar eclipse from certain points on Earth were the eclipse would be total. The observation was done during an eclipse that occurred on March 29, 1919, which confirmed the German physicist's prediction. In this case, the hypothesis is considered good, even though it could not be directly observed, because its logical implications went through a successful empirical test against reality. It is worth reiterating that the hypothesis was not *proven*, but it became probable.

This is also an interesting example to show that it is not necessary that the researcher himself be able to *control* the experiment to test a hypothesis. What is relevant is that the minimum required conditions for the empirical test are present in the observation. Only when these conditions are not met it is necessary to be able to control the experiment; otherwise, the hypothesis is not testable.

5. The method of concomitant variation: *When there are variations in an event, positive or negative, every time there is a variation in a given component, then there exists a causal relationship between the two of them.*

Table 5. The method of concomitant variation

Direct relationship		Indirect relationship	
Event	Component	Event	Component
S	A	S	A
$S + AS$	$A + AA$	$S + AS$	$A - AA$
$S - AS$	$A - AA$	$S - AS$	$A + AA$

Different from the previous methods, this one is quantitative and, therefore, it is more dangerous in terms of the possibility of making a causal association of two variables that in reality are unrelated. Any statistician knows that, with a little patience, it is possible to find variables that show a high degree of correlation. As M. J. Moroney (1951, p. 304) says: "The best advice we can give to the man who finds a correlation and starts to say, 'It's obvious,' is: Think again. Ten to one there's a catch in it."

Even with a 100 percent correlation, this would not be proof that there is a causal relationship between the variables under study. Furthermore, such correlation cannot ensure anything outside the time period under examination. A rule of variation may stop being valid beyond certain intervals. The best example of this is Einstein's theory of relativity, which made it clear that Newtonian mechanics is a special case within a more general problem and that, therefore, outside certain conditions, its correlations of velocity, mass, time, and space are not valid. Another example, cited by Cohen and Nagel (1934, p. 263), is: "The period of a pendulum is proportional to the square root of its length if the arc of the swing is small. When the arc of the swing is increased, the period (theoretically and approximately) is still related in this way to the length; nevertheless, the factor of air resistance must now be considered, so that the period can no longer be rendered by that simple formula."[36]

Like the previous ones, this method is also valuable because of its capacity to discard hypotheses, but the danger of "building" false causal models is much larger because it is always possible to find variables that move in the same or opposite directions. This method can become totally arbitrary if it is not accompanied by one of the qualitative methods in which it is possible to isolate the studied variables. Let us assume that someone finds a high degree of inverse correlation between ice cream sales in Buenos Aires and the temperature in New York, i.e., when the temperature in the North American city rises, the sale of ice cream in Argentina falls, while someone else finds a high degree of positive correlation between ice cream sales in Buenos Aires and the temperature in the same city. To demonstrate that the second correlation is more likely [capturing a causal effect] than the first one, we need to resort to a different empirical test. The way to end the dispute [between the two correlations] is to control the variables. If we could maintain constant the temperature in New York and vary it in Buenos Aires, we could empirically demonstrate the error of the first hypothesis. Qualitative methods are more conclusive. Statistical correlations cannot prove causality and, even if qualitative methods hardly prove a hypothesis, they make the construction of false models less likely.[37]

Given that hypotheses are probable propositions but not necessary ones, empirical methods are indispensable as much for choosing among alternative hypotheses as for advancing science through the falsification of established hypotheses. Without these empirical tests, the hypothetical-deductive model of the natural sciences would be impossible. Why, then, are empirical tests possible? The answer is: because in natural phenomena, there is *regularity* (i.e., the relationship between variables is deterministic). The researcher can experiment because, when the relevant conditions are the same, given certain stimuli or changes, the observed behavior is the same. That is, they react with regularity. Without regularity, not only would experimentation

[36] The following passage by Cohen (1931, p. 92) is also illustrative: "In general a statistical correlation even of 100 per cent. does not prove any causal relation between the phenomena or factors correlated, because (as indicated before, apropos of simple enumeration) we need evidence to support the belief that this correlation is permanent and not temporary. Statistical information obviously cannot prove anything beyond the time of observation. To prove the invariant relations implied in laws of nature we need in addition rational considerations regarding relations of identity. These relations are generally revealed by analysis."

[37] On this topic, the following distinction by the Frenchman Levy-Leblond (1984, pp. 84–85) between theoretical physics and mathematical physics is illustrative: "Theoretical physics discovers and applies laws; develops and puts into practice physics concepts under the control of and in interaction with experimental physics. There are different levels that can go from the interpretation of a certain and specialized experimental result, through known physic laws, to the search of new fundamental laws […]. The work of mathematical physics could be described as removing scaffoldings, picking up the ruins and making it patent in plain light the internal structure of the edifice, the nature and strength of its foundation, as well as its weak points. It is about, then, an activity that necessarily has as an object theories and concepts *already developed and established*' (italics are mine). [TN: Translation to English is my own.]

be impossible, but also the action of all men. There would be no way to know the relationships between different events. Men would not be able to develop any plan of action because they could not know which means to use to achieve their ends.[38] If under the same conditions the same cause would not always produce the same effect, then the empirical verification would be impossible. Regularity is essential to perform empirical tests in the natural sciences. It is easy to see how experimental methods become useless in the absence of regularity. It would be impossible to draw conclusions from an observation such as the following one:

Table 6. Events without regularity

Case number	Event	Observed factors				
1	s	A	B	C	D	E
2	s	F	G	H	I	J
3		K	L	M	N	O
4	s	P	Q	R	S	T
5	s	U	V	W	X	Y

The determinism of the natural sciences was questioned at the beginning of this [the twentieth] century with the rise of modern physics (i.e., the theory of relativity and quantum mechanics). Some important physicists, such as Werner Heisenberg and Niels Bohr, concluded that subatomic physics is not deterministic.[39]

However, this is not true. A causal proposition can take three forms: (1) a null proposition, that is, nothing is known about the cause of the phenomenon, (2) a complete proposition, which allows us to say: "given certain conditions, A causes B, and (3) an incomplete proposition, which only allows us to state: "given certain conditions, A causes B in x percent of cases." In this last case, the scientist knows only *one* of the factors that produce an event and ignores the other part. Only when all of the phenomena are known are we prepared to formulate a complete proposition. The fact that in subatomic physics there are interrelationships between the observed object and the observer and that there is no [known] way to isolate them does not imply nondeterminism, it implies ignorance. We are at a level of knowledge where we do not know how to observe without affecting the result of the experiment, nor do we know which measure is being affected by the observer.[40] If one does not conclude that the probabilistic character of an event is due to the existence of other unknown factors that are affecting the [experiment's] result, then there is only one possible conclusion: that the observed object, e.g., an electron, has the capacity to choose, just like a human being, its own behavior. Determinism and purposeful behavior are mutually exclusive concepts; there is nothing in between them. Now, to conclude that the objects of nature have purposeful behavior would imply going back to primitive animism.

[38] In the words of Mises (1949, p. 22): "Man is in a position to act because he has the ability to discover causal relations which determine change and becoming in the universe. Acting requires and presupposes the category of causality. Only a man who sees the world in the light of causality is fitted to act. In this sense we may say that causality is a category of action. The category *means and ends* presupposes the category of *cause and effect*. In a world without causality and regularity of phenomena there would be no field for human reasoning and human action. Such a world would be a chaos in which man would be at a loss to find any orientation and guidance."

[39] According to Heisenberg (1930), in subatomic physics "[…] the interaction between observer and object causes uncontrollable and large changes in the system being observed because of the discontinuous changes characteristic of atomic processes. The immediate consequence of this circumstance is that in general every experiment performed to determine some numerical quantity renders the knowledge of others illusory, since the uncontrollable perturbation of the observed system alters the values of previously determined quantities" (p. 3) and "[t]his indeterminateness of the picture of the process is a direct result of the indeterminateness of the concept 'observation'—it is not possible to decide, other than arbitrarily, what objects are to be considered as part of the observed system and what as part of the observer's apparatus" (p. 14). Also, according to Bohr (1934, p. 54): "[…] the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation." Both passages are taken from Nagel (1961, p. 274).

[40] For instance, Holton and Brush (1979, p. 733) sustain that "Heisenberg's principle could be interpreted as a simple restriction of our knowledge of the electron, taking into consideration the limitations of actual experimental methods without rejecting, because of this, the belief that the electron has a definite position and speed. The expression 'uncertainty principle' would then be appropriate, but keeping in mind that the principle applies to the knowledge of the observer and not to nature." For a defense of determinism in the natural sciences also see Nagel (1961, chap. 10) and Mises (1957, pp. 87–88). [TN: The quote from Holton and Brush is misspelled as Bollan and Brush in the original Spanish text. The citation is referenced as *op. cit.*, but no previous citation is given in the original paper. There is also a mention to his own added italics, but the citation has no italics (I did not add them in my translation). The citation seems to refer to Holton and Brush (1979), but the original English text was unobtainable. I offer my own translation back to English.]

In a certain way, when an event refutes a long-established hypothesis it becomes an incomplete causal proposition. From this moment on, the hypothesis does not explain 100 percent of cases, which compels scientists to look for the error and find a hypothesis of a more general character, that is, one that can provide a complete explanation.

It is precisely at this point, determinism versus nondeterminism, where the difference between the natural and social sciences appears: while there is determinism in the former, there is not in the latter. The discovery of the marginal theory of subjective value implied "much more than the substitution of a more satisfactory theory of market exchange for a less satisfactory one" (Mises 1949, p. 3). With the marginal revolution, it was clear not only that all market phenomena are logical consequences of subjective valuations, but also that there clearly remain epistemological differences between the natural and social sciences. The epistemological ideas of Say, Senior, Cairnes, and Mill gained new momentum and a more solid ground.

The crucial point is that subjective valuations, which are the fundamental basis of economic theory, are not determined by factors external to the mind. Man, different from objects and animals, can choose his own behavior. *Free will* means that ideas are generated in the human and are not *determined* in the same way that natural phenomena are. The presence or absence of a certain factor can affect in different ways the valuations of each person and of the same person at different times. Of course, this does not mean that external factors do not *influence* our thinking and valuations and, therefore, the behavior of individuals; undoubtedly, they do. What is meant by nondeterminism of thinking is that these external factors do not influence in a *singular* way the final ideas and valuations.[41] So, we can conclude that, while the phenomena of the natural sciences are *causal*, those of the social sciences are *teleological*, i.e., human behavior is not guided by causes (past), but by objectives or ends (future) (Ballve 1956; Mises 1962, p. 7). The "causes" of the ends being either x or z are nondeterministic; they depend on the individual's free will.

The behavior of men, then, is influenced in a nondeterministic way by the surrounding conditions in a given moment and place. Therefore, the facts of social sciences are the result of complex phenomena; they reflect only unique events that respond to a nonrepeatable and specific time, place, circumstances, and valuations (Hayek, 1967, chap. 2; Mises, 1933, p. 12, 1949, p. 31, 1962, p. 74). The French Revolution, the Bolshevik Revolution, the crisis of 1929, etcetera, are singular events, i.e., unrepeatable.

The fact that social events are complex and singular phenomena makes it impossible to empirically test a hypothesis. Let us assume that someone maintains that the industrial development of the United States was due to a certain degree of protectionism. The only thing we can do to contest this argument is to demonstrate *rationally*, i.e., without drawing on experience, that American industry flourished *despite* protectionism and that without said protectionism a different and more efficient American industry would have flourished. However, we cannot repeat history to demonstrate the error of the protectionist thesis. The error can neither be shown by taking as an example a country with a higher degree of protectionism than the United States and low industrial potential, such as Argentina. The protectionist can argue that the *conditions* were not the same, because in Argentina there were *other factors* that more than canceled out the effect of the protection. Moreover, a totally free country could end up having a lower level of industrial development than an interventionist one if the values that predominate in a free society favor leisure and meditation over the production of "material" goods and services. In summary, without a *ceteris paribus* observation, i.e., one with controlled variables, it is impossible to refute, let alone prove, propositions. This, in turn, makes it possible for false theories to emerge and to keep going more easily over time.

It is very common to find theories that incorporate the *post hoc, ergo propter hoc* logical fallacy. We have, for instance, the case of the theory that sustains that, in some cases, inflation is caused by increases in wages, gas prices, or general costs. Without variable controls, it is impossible to demonstrate, *empirically*, the error of the cost theory of inflation.

It is for these reasons that history is subject to multiple interpretations. Each historian or economist explains the events of a given period using different cause-effect relationships. For instance, Keynes, Friedman, and B. M. Anderson attribute different causes to the crisis of the 1930s, and more than half a century after this event, it is still subject to different interpretations. These differences would be resolved if it were possible to

[41] There are, of course, those who deny that men behave freely and rather sustain that their behavior is predetermined. Examples of this position are Marx's polylogism, according to which the social class to which an individual belongs determines his ideas. And in psychology we have conductism (or behavioralism), which sustains that human conduct is determined by the influence of the external environment on the individual. For a criticism of these theories, see Mises (1957, 1962, pp. 28–33, 57–59), Hayek (1952b, 1967, chap. 3), Popper (1972), Popper and Eccles (1977), and Zanotti (1985).

control the variables and if the behavior of individuals was determined, that is, if there were regularity in the stimulus-response relationship. If these two conditions held, it would be enough to recreate certain historical periods, change the desired variables, control the other variables, and study the consequences of each scenario.

However, if experience and observation of the social sciences allows us neither to prove nor refute economic theories, we should ask, is economics a science? How do we know which theory is true and which is false? The answer is that social sciences, and therefore economics, are sciences, but unlike the natural sciences, they are not hypothetical-deductive but aprioristic (Mises, 1933, pp. 12–17, 1949, pp. 32–36, 1962, pp. 17–19). In fact, as we have seen, the scientist of the natural sciences starts from a hypothesis, i.e., a *probable* premise, to explain a certain event. Given that a hypothesis is one of the many possible explanations of an event, the empirical test plays a crucial role. In economics, the premises from which deduction starts are not probable, they are *a priori* propositions, i.e., necessarily true. An *a priori* proposition is one whose negation is unthinkable: it would be absurd to the human mind (e.g., it is unthinkable that the part could be larger than the whole, or that A could occur at the same time as not-A). Observation can neither prove nor refute an *a priori* proposition because it is a self-evident mental category. Cohen (1931, p. 145) presents the idea in the following way: "[the *a priori*] denotes propositions which must be true absolutely or unqualifiedly and which never can be refuted by anything that has happened or will happen anywhere or at any time."

Theorems deducted from *a priori* propositions, if there are no errors in the chain of reasoning, are necessarily true and are not subject to empirical tests. Any difference between the conclusion of the theorems and the empirical observation has to be attributed to an observation error. For instance, if an individual knows he has seven apples and he also knows that he was given five more apples, it is not necessary for him to count the apples to know that he now has twelve. But, if he decides to count them and does not find twelve, no one would take this as proof that seven plus five is not equal to twelve; it is necessary to resort to another type of explanation to justify the difference.

Even though Say, Senior, Mill, Cairnes, and Carl Menger had already demonstrated the aprioristic character of economic science,[42] the most systematic and concise foundation was provided by Mises (Kirzner 1976; Mises 1933, 1962; Rothbard 1976). This economist has shown that economics belongs to a more general science: praxeology, and that all praxeological theorems, and therefore economic theorems as well, can be deduced from the category of action. "All the concepts and theorems of praxeology are implied in the category of human action. The first task is to extract and deduce them, to expound their implications and to define the universal conditions of acting as such" (Mises 1949, p. 64). But, since the purpose of all theory is to know reality, the second step consists in circumscribing the research to the study of action within the real-world conditions imposed on men. For instance:

> The disutility of labor is not of a categorial and aprioristic character. We can without contradiction think of a world in which labor does not cause uneasiness, and we can depict the state of affairs prevailing in such a world. But the real world is conditioned by the disutility of labor. Only theorems based on the assumption that labor is a source of uneasiness are applicable for the comprehension of what is going on in this world.

This appeal to experience does not change the aprioristic character of economics. Its function is simply to restrict the field of the study of human action to those problems that are considered of interest for practical reasons.

If economic theorems could be not be reduced to their ultimate foundations, i.e., to *a priori* propositions, then economic theory would be impossible, because the lack of regularity and control of variables make empirical testing impossible. Not only do mathematical models of the economy try to start from assumptions or premises that are not aprioristic; they are also unrealistic. If the model starts from realistic and probable premises—that is, there could be other, just-as-realistic premises that give rise to other models—then an empirical test is needed to decide between the alternative models. But, as we have seen, this is impossible in the social sciences. In this way, mathematical models of the economy do not go beyond being nonverifiable hypotheses; we can never decide between one model or another. It is worth reiterating that even when we have a high degree of statistical correlation between the model's variables, this does not prove that the causal

[42] See Rothbard (1979, pp. 45–56).

relationships are real. To be able to construct mathematical model of the economy that is both realistic and true, the starting premises must necessarily be true, i.e., *a priori*.

However, when the mathematical economist formulates his model, he is forced to introduce unrealistic assumptions that the same use of mathematics requires of him. This is because verbal propositions cannot always be translated to mathematical propositions without changing the meaning and nature of the proposition. There are economists for whom the introduction of unrealistic assumptions is not an issue as long as these allow the development of theorems with predictive capacity.[43] There does not seem to be much difference between the theoretical explanations that this epistemological position has to offer and those who consult a gypsy to predict the future by reading cards or their hands, or with astrology predicting people's future by studying the movement and position of planets. The objective of science is to study the "real" relationships of phenomena and not to make predictions. Obviously, to the extent that we know the causes of an event, it will be possible, although not in every case, to make predictions. But to sustain that what is important is to make good predictions, regardless of the means used, i.e., even if they are unrealistic, implies reducing the theory to the level of a myth.

On this subject, it is worth quoting Moroney (1951, p. 321):

> Fashions change in nonsense and superstition no less than in ladies' hats. There was a time when popes and kings had astrologers to help them plan for the future. Nowadays government departments have statisticians for the same purpose. One day they will be relegated to the Sunday newspapers to displace the astrologers from their last refuge. I can well understand the cult of the astrologers at court. After all, the astrologer was an astronomer, and if a man has success in predicting eclipses by star-gazing why should he not have equal success in predicting the course of more mundane matters by taking cognizance of the position of the heavenly bodies? But for much of the statistical work that is done by government departments I can see little excuse.

The disrepute that economic predictions have achieved through econometric models in our time is quite significant and has been confessed by the advocates of making predictions. For instance, Martin Feldstein, of Harvard University, who served as president of Ronald Reagan's Council of Economic Advisers, sustained that "[o]ne of the great mistakes of the past 30 years of economic policy has been an excessive belief in the ability to forecast" (Greenwald, 1984). Friedman (1953, p. 40) himself concedes, "we have tried to do more than what we can do."[vii] Another important economist, Wassily Leontief (1982), complains that "economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions." L. R. Klein, coauthor with R. M. Young of *An Introduction to Econometric Forecasting and Forecasting Models*, took the defensive by arguing, "At least econometric models can give you a quick on-line response to any major event like an embargo or a change in fiscal and monetary policy" (Greenwald, 1984).[44] It should be sufficiently clear that to predict *big* changes, an econometric model is not necessary. For instance, the *big* changes in fiscal and monetary policy depend on the specific and concrete actions of those who regulate these variables, and the only way to predict them is by knowing, or by trying to guess, what the bureaucrat in charge at the time thinks about an issue and what chance he has that the parliament will approve the required tax reforms. An econometric model has little or nothing to say on this issue.

On the other hand, R. E. Lucas Jr., (1976, p. 20), a known figure of the rational expectations school of thought, sustained that the big econometric models used to make simulations "provide no useful information as to the actual consequences of alternative economic policies."

This recognition of failure in predictions is not, as Stigler (1949, p. 41) thinks, because "[e]conomics is still a primitive discipline as compared with the more advanced of the natural sciences [...]."[45] The inability to make

[43] See Friedman (1953, chap. 1) and Willes (1981). For a refutation of this position see Nagel (1963).
[44] There were attempts to apply catastrophe theory (a branch of mathematics) to big changes. But this approach, at least so far, has not been very popular.
[45] For a refutation of this point, see Machlup (1956). According to Machlup, there is a close analogy between economists who consider economics a primitive science and the excuse given by those who are beginners in a game or sport: "If they admit they are old practitioners of the game or sport, their poor performance may be attributed to lack of intelligence or talent; but for 'novices' they are not doing so badly.

predictions exists because social events are the consequence of individuals' actions and these actions, as we have already mentioned, are nondeterministic. Individuals' actions are not "mathematical functions" of anything. Good prediction consists in anticipating the actions of individuals, but economic theory does not tell us anything about this; what it does allow us to know is the *consequences* of actions. Economic theory tells us that if the action is A, then the consequences will be $A_1, A_2, A_3, \dots, A_n$; that if the action is B, then the consequences will be $B_1, B_2, B_3, \dots, B_n$, etcetera. The crucial problem is knowing what the concrete action will be and how, in turn, the consequences of that action will influence the future actions of other individuals. We should remember that the larger a society is, the more complex the resulting social phenomena will be because, say, in moment 1 there will be millions of nondeterministic actions whose consequences will also influence in a nondeterministic way millions of actions in moment 2, and so on.

Entrepreneurial activity consists precisely in trying to anticipate and quantify those individual actions that translate into prices. But this activity is an art because it is based on a *subjective* judgment of what is relevant.[46] The nondeterministic character of human action is what makes impossible a *rational* modeling of expectations and, therefore, of actions as well. Any modeling of expectations is necessarily arbitrary; it is based on subjective assessments that we call "understanding."[47]

We can group the errors in economic predictions into two types: (1) comprehension errors and (2) theoretical errors. In the first case, prediction failure occurs because the subjective judgments of the one who made the prediction were false, e.g., he thought that the relevant individuals would follow course of action A but they followed course of action B; the logical consequences (that is, theoretical) in each case were different and therefore the prediction failed. In the second case, the subjective judgment may be correct, but it is possible to be wrong about the consequences of the action.

The first type of error, which is characteristic of the social sciences, introduces an additional problem to empirical tests because a false prediction may be due to erroneous subjective judgments and not due to a bad theory. But then it is not possible to decide with logical necessity if the error is due to an error in the subjective judgments or to a theoretical error. If there is no control of the variables and it is impossible to control the individuals' valuations and behavior, there is also no way to make a decision; any explanation can be logically possible. Therefore, prediction is not a suitable means to decide among alternative theories. It is impossible to apply the hypothetical-deductive method in the social sciences and, therefore, it is also impossible in economics.

It is important to add that besides this *natural* problem there is a series of practical problems related to the veracity and precision of social and economic statistics (Morgenstern, 1950). The way in which price indices, gross domestic product, etc., are calculated does not comply with the minimal requirements of statistical estimation theory.

> […] it ought to be clear *a priori* that most economic statistics should not be stated in the manner in which they are commonly reported, pretending an accuracy that may be completely out of reach and for the most part is not demanded. […] Unemployment figures of several millions are given down to the last 1,000s (i.e. one-hundredths of one percent "accuracy"!), when certainly the 100,000s or in some cases perhaps even the millions are in doubt. (Morgenstern, 1950, pp. 8–9)

Lies also play an important role. Morgenstern (1950, p. 23, italics are my own) reminds us in the following way:

> When the Marshall Plan was being introduced, one of the chief European figures in its administration (who shall remain nameless) told me: "We shall produce any statistic that we

Thus, 'Excuse me, I'm just a beginner', is an often-heard apology from participants in sports and games who have a feeling of inferiority. This is what is probably behind the social scientists' pronouncements emphasizing how young the social sciences really are: 'Please do not think we are stupid; we are merely beginners" (p. 162).

[46] For a more detailed exposition, see Mises (1949, chap. XV, 1957, chap. XIV) and Kirzner (1973).

[47] "Understanding," says Mises (1949, p. 118), "by trying to grasp what is going on in the minds of the men concerned, can approach the problem of forecasting future conditions. We may call its methods unsatisfactory and the positivists may arrogantly scorn it. But such arbitrary judgments must not and cannot obscure the fact that understanding is the only appropriate method of dealing with the uncertainty of future conditions."

> think will help us to get as much money out of the United States as we possibly can. Statistics which we do not have, but which we need to justify our demands, we will simply fabricate." These statistics "proving" the need for certain kinds of help will go into the historical records of the period as true descriptions of the economic conditions of those times. *They may even be used in econometric work!*

Moroney (1951, p. 324) seems to be quite right when, referring to statistical calculation, he says, "Economic forecasting, like weather forecasting in England, is only valid for the next six hours or so. Beyond that it is sheer guesswork."

To avoid confusion, it is important to clearly distinguish between economic theory and economic facts. Economic theory is a set of theorems that start from necessarily true premises and, therefore, if there is no error in the syllogisms, these theorems are not only true, they are also *exact*. The fact that economic theorems are exact does not mean they can predict; these theorems explain the logical consequences of different types of actions, but they do not tell us anything (nor can they) about which concrete actions individuals will take, nor how the consequences of those actions will affect the behavior of other individuals. These theorems also tell us nothing about the *magnitude* of the consequences.

The usefulness of economic theorems is very limited for predictive purposes, but instead is of great importance in knowing which means should be used to achieve an efficient allocation of resources. Through economic theorems we can know, for instance, that a protectionist policy, progressive taxation, and price controls produce an inefficient allocation of resources. The *aprioristic* character of economic science allows us to conclude that this is *necessarily* so even if we cannot empirically prove it.

We can summarize this point in the following way: (1) When a science starts from probable premises it is hypothetical-deductive. (2) Given that the premise is probable but not necessary, the empirical test becomes a crucial factor to subject a hypothesis to a test. (3) The hypothesis can never be *proven*, only refuted; in this sense it can be said that even when the hypothesis has passed many empirical tests, that doesn't mean it is definitively established. (4) Empirical testing is possible in the natural sciences because the relationship between its elements is deterministic, i.e., in identical circumstances, a certain stimulus always produces the same response; therefore, in these sciences, there is regularity. (5) Given that man is free to choose his behavior, in the social sciences there is no determinism and therefore there is no regularity. (6) The absence of regularity makes it impossible to empirically test a hypothesis, because social facts are characterized by being unique, that is, nonrepeatable. (7) Mathematical models of the economy start from nonnecessary and, in the majority of the cases, also unreal premises; therefore, they are hypotheses. The impossibility of empirically testing hypotheses in the social sciences, in turn, also makes mathematical models unverifiable. (8) When a hypothesis is nonverifiable, then it is not scientific. (9) Economics, however, can be a science because its theorems can be reduced to a premise of a *necessary* rather than a probable character. In this sense, economics is, like mathematics and logic, an *a priori* science. Economic theories are, therefore, necessarily true and do not require empirical verification.

4. References

[1] Arrow, K. (1974). Equilibrio Economico. *In Enciclopedia Internacional de las Ciencias Sociales*. Volume 4. Aguilar.

[2] Arrow, K., & Debreu, G. (1954). Existence of Equilibrium for a Competitive Economy. *Econometrica*, 22, 265–290.

[3] Arrow, K., & Hahn, F. (1971). *General Competitive Analysis*. San Francisco: Holden-Day.

[4] Ballve, F. (1956). On Methodology in Economics. In M. Seenholz (Ed.), *On Freedom and Free Enterprise*. D. Van Nostrand Co.

[5] Bohr, N. (1934). *Atomit Theory and the Description of Nature* (2011 ed.). Cambridge University Press.

[6] Bowley, M. (1937). *Nassau Senior and Classical Economics*. London: George Allen and Unwin.

[7] Buchanan, J. M. (1964). What Should Economists Do? *Southern Economic Journal*, 30(3), 213–222.

[8] Cairnes, J. E. (1857). *The Character and Logical Method of Political Economy* (1888 ed.). London: Macmillan and Co., Limited.

[9] Champernowne, D. G. (1954). On the Use and Misuse of Mathematics in Presenting Economic Theory. *The Review of Economics and Statistics*, 36(4), 369. https://doi.org/10.2307/1925890

[10] Chiang, A. (1977). *Métodos Fundamentales de Economía Matemática*. Amorrortu Editores.

[11] Cohen, M. R. (1931). *Reason and Nature: An Essay on the meaning of Scientific Method* (1953 ed.). Glencoe: The Free Press.

[12] Cohen, M. R. (1934). *An Introduction to Logic and Scientific Method* (2007 ed.). Hughes Press.

[13] Cohen, M. R., & Nagel, E. (1934). *Introducción a la Lógica y al Método Científico* (1977 ed.). Amorrortu Editores.

[14] Cornblit, O. (1984). Laissez-faire, realidad y modelos económicos. *Libertas*, 1.

[15] Cournot, A. (1838). *Researches into the Mathematical Principles of the Theory of Wealth* (1897 ed.). London: Macmillan and Co., Limited.

[16] Debreu, G. (1959). *Theory of Value*. Wiley & Sons.

[17] Debreu, G. (1962). New Concepts and Techniques for Equilibrium Analysis. *International Economic Review*, 3, 1962.

[18] Friedman, M. (1953). *Essays in Positive Economics*. Chicago: The University of Chicago Press.

[19] Friedman, M. (1962). *Price Theory* (2017 ed.). Routledge.

[20] Gamow, G. (1961). *Biography of Physics*. New York, Evanston, and London: Harper Torchbooks.

[21] Gamow, G. (1966). *Thirty Years that Shocked Physics: The Story of Quantum Theory*. New York: Doubleday & Co. Inc.

[22] Georgescu-Rogen, N. (1951). Some Properties of a Generalized a Generalized Leontief Model. In T. C. Koopmans (Ed.), *Activity Analysis of Allocation and Production*. New York and London: Wiley & Sons.

[23] Greenwald, J. (1984, August 27). The Forecasters Flunk. *Time*.

[24] Hahn, F. (1981). General Equilibrium Theory. In D. Bell & I. Kristol (Eds.), *The Crisis in Economic Theory*. New York: Basic Books.

[25] Hahn, F., & Negishi, T. (1962). A Theorem of Non-tatônnement Stability. *Econometrica*, 30, 463–469.

[26] Hansen, B. (1970). *A Survey of General Equilibrium Systems*. McGraw-Hill Book Company.

[27] Hayek, F. A. (1948). *Individualism and Economic Order* (1958 ed.). Chicago: The University of Chicago Press.

[28] Hayek, F. A. (1952a). *The Counter-Revolution of Science: Studies on the Abuse of Reason* (1979 ed.). Indianapolis: Liberty Press.

[29] Hayek, F. A. (1952b). *The Sensory Order* (1976 ed.). Chicago: The University of Chicago Press.

[30] Hayek, F. A. (1967). *Studies in Philosophy, Politics and Economics* (1978 ed.). London: Routledge & Kegan Paul.

[31] Heisenberg, W. (1930). *The Physical Principles of the Quantum Theory* (2015 ed.). Martino Publishing.

[32] Hicks, J. (1939). *Value and Capital* (2001 ed.). Oxford: Oxford University Press.

[33] Hicks, J. R. (1973). *Capital and Time*. Oxford University Press.

[34] Hicks, J. R. (1979). Is Interest Rate the Price of a Factor of Production? In M. J. Rizzo (Ed.), *Time, Uncertainty, and Disequilibrium: Exploration in Austrian Themes*. Lexington, Toronto: Lexington Books.

[35] Holton, G. J. (1952). *Introduction to Concepts and Theories in Physical Science* (1973 ed.). Addison-Wesley.

[36] Holton, G. J., & Brush, G. (1979). *Introducción a los conceptos y teorías de las ciencias físicas*. Editorial Reverte, S. A.

[37] Jaffé, W. (1935). Unpublished Paper and Letter of León Walras. *Journal of Political Economy*, 43(2), 187–207.

[38] Jaffé, W. (1977a). Translator's Foreword. In *Elements of Pure Economics*. Augustus M. Kelley.

[39] Jaffé, W. (1977b). Walras, Leon. In *Enciclopedia International de las Ciencias Sociales*. Volume 10. Aguilar.

[40] Jevons, W. S. (1871). *The Theory of Political Economy* (1888 ed.). London and New York: Macmillan and Co., Limited.

[41] Kakutani, S. (1941). A Generalization of Brouwer's Fix Point Theorem. *Duke Mathematical Journal*, 8, 451–459.

[42] Keynes, J. M. (1936). *Teoría General de la Ocupación, el Interés y el Dinero* (1976 ed.). Fondo de Cultura Economica.

[43] Kirzner, I. M. (1973). *Competition and Entrepreneurship*. Chicago: The University of Chicago Press.

[44] Kirzner, I. M. (1976). On the Method of Austrian Economics. In E. G. Dolan (Ed.), *The Foundations of Modern Austrian Economics*. Kansas City: Sheed & Ward, Inc.

[45] Kirzner, I. M. (1979). *Perception, Opportunity and Profit*. Chciago: University of Chicago Press.

[46] Knight, F. H. (1921). *Risk, Uncertainty and Profit* (1964 ed.). New York: Augustus M. Kelley.

[47] Koopmans, T. C. (1954). On the Use of Mathematics in Economics. *The Review of Economics and Statistics*, 36(4), 377. https://doi.org/10.2307/1925893

[48] Koopmans, T. C. (1957). *Three Essays on the State of Economic Science*. McGraw-Hill.

[49] Lachmann, L. M. (1977). *Capital, Expectations, and the Market Process*. (W. E. Grinder, Ed.). Kansas City: Sheed Andrews and McMeel.

[50] Lange, O., & Taylor, F. M. (1938). *On the Economic Theory of Socialism*. (B. E. Lippincott, Ed.) (1948 ed.). The University of Minesota Press.

[51] Leoni, B., & Frola, E. (1977). On Mathematical Thinking in Economics. *The Journal of Libertarian Studies*, 1(2), 101–109.

[52] Leontief, W. (1982). *Academic Economics. Science*, 217(4555), 104–107. https://doi.org/10.1126/science.217.4555.104

[53] Levy-Leblond, M. (1984). Física y Matemáticas. In *Pensar la Matemática*. Tusquets Editores.

[54] López Urquía, J. (1972). Prólogo. In *Matemática Para Economistas*. Barcelona: Ediciones Ariel.

[55] Lucas, R. E. (1976). Econometric Policy Evaluation: A Critique. In K. Brunner & A. H. Meltzer (Eds.), *The Phillips Curve and Labor Markets* (Carnegie-Rochester Conference Series on Public Policy). New York: North Holland Publishing Company.

[56] Machlup, F. (1956). The Inferiority Complex of the Social Sciences. In M. Sennholz (Ed.), *On Freedom and Free Enterprise: Essays in Honor of Ludwig von Mises*. D. Van Nostrand Company, Inc.

[57] Marshall. (1890). *Principles of Political Economy* (1946 ed.). Macmillan and Co., Limited.

[58] Marshall, A. (1925). *Memorials of Alfred Marshall* (1956 ed.). Augustus M. Kelley.

[59] McKenzie, L. W. (1954). On Equilibrium in Graham's Model of World Trade and Other Competitive systems. *Econometrica*, 22, 147–161.

[60] McKenzie, L. W. (1959). On the Existence of General Equilibrium for a Competitive Market. *Econometrica*, 27, 54–71.

[61] McKenzie, L. W. (1961). On the Existence of General Equilibrium; some Corrections. *Econometrica*, 29, 247–248.

[62] Metzler, L. A. (1945). The Stability of Multiple Market: The Hicks Conditions. *Econometrica*, 13, 277–292.

[63] Mill, J. S. (1843). *A System of Logic* (1882 ed.). New York: Harper and Brothers.

[64] Mill, J. S. (1844). *Essays on Some Unsettled Questions of Political Economy* (1874 ed.). London: Longmans, Gree, Reader, and Dyer.

[65] Mises, L. von. (1912). *The Theory of Money and Credit*. (1981 ed.). Indianapolis: Liberty Fund.

[66] Mises, L. von. (1933). *Epistemological Problems of Economics* (2003 ed.). Auburn: The Ludwig von Mises Institute.

[67] Mises, L. von. (1949). *Human Action* (1996 ed.). Irvington-on-Hudson: The Foundation for Economic Education.

[68] Mises, L. von. (1957). *Theory and History* (2001 ed.). Auburn: The Ludwig von Mises Institute.

[69] Mises, L. von. (1962). *The Ultimate Foundation of Economic Science*. Princeton: D. Van Nostrand Company, Inc.

[70] Modigliani, F., & Miller, M. H. (1958). The Cost of Capital, Corporation Finance and the Theory of Investment. *The American Economic Review*, 48(3), 261–297.

[71] Morgenstern, O. (1950). *On the Accuracy of Economic Observations* (1963 ed.). Princeton: Princeton University Press.

[72] Morgenstern, O. (1972). Thirteen Critical Points in Contemporary Economic Theory: An Interpretation. *Journal of Economic Literature*, 10(4), 1163–1189.

[73] Morgenstern, O. (1976). The Collaboration Between Oskar Morgenstern and John von Neumann on the Theory of Games. *Journal of Economic Literature*, 14(3), 805–816.

[74] Moroney, M. J. (1951). *Facts from Figures* (1965 ed.). Baltimore, Maryland: Penguin Books.

[75] Mosak, J. L. (1944). *General Equilibrium Theory in International Trade*. Principia Press.

[76] Muth, J. F. (1961). Rational Expectations and the Theory of Price Movement. *Econometrica*, 29, 315–335.

[77] Nagel, E. (1961). *La Estructura de la Ciencia* (1981 ed.). Ediciones Paidos.

[78] Nagel, E. (1963). Assumptions in Economic Theory. *The American Economic Review*, 53(2), 211–219.

[79] Nash, J. F. (1950). Equilibrium Points in N-Persons Games. *Proceedings of the National Academy of Science*, 36, 48–49.

[80] Negishi, T. (1962). The Stability of a Competitive Economy: A Survey Article. *Econometrica*, 30, 635–669.

[81] NeisserH. (1932). Lohnhöle und Beschäftigungsrad im Marktgleichgewicht. *Weltwirtschaftliches Archiv*, 30, 415–455.

[82] Neumann, J. von. (1928). Zur Theorie der Gesellschaftsspiele. *Mathematische Annallen*, 100.

[83] Neumann, J. von. (1937). Uber ein ökonomisches Gleichungssysteme und eine Verallgemeinerung des Brouwerschen Fixpunkdsatzes. *Ergebnisse Eines Mathematischen Kolloquiums*, 8, 78–83.

[84] Neumann, J. von, & Morgenstern, O. (1944). *Theory of Games and Economic Behavior* (2001 ed.). Princeton: Princeton University Press.

[85] Nikaido, H. (1956). On the Classical Multilateral Exchange Problems. *Econometrica*, 8, 135–145.

[86] Nikaido, H. (1978). *Métodos Matemáticos del Análisis Económico Moderno*. Editorial Vicens-Vives.

[87] Novick, D. (1954). Mathematics: Logic, Quantity, and Method. *The Review of Economics and Statistics*, 36(4), 357–358.

[88] O'Driscoll, G. P. J. (1977). *Economics as a Coordination Problem: The Contribution of Friedrich A. Hayek*. Kansas City: Sheed Andrews and McMeel.

[89] Popper, K. R. (1935). *The Logic of Scientific Discovery* (2002 ed.). London: Routledge.

[90] Popper, K. R. (1957). *The Poverty of Historicism* (2002 ed.). New York: Routledge.

[91] Popper, K. R. (1963). *Conjectures and Refutations* (2002 ed.). London: Routledge.

[92] Popper, K. R. (1972). *Objective Knowledge: An Evolutionary Approach* (1979 ed.). New York: Oxford University Press.

[93] Popper, K. R., & Eccles, J. C. (1977). *The Self and Its Brain*. (1983 ed.). Routledge.

[94] Rizzo, M. J. (1979). *Time, Uncertainty, and Disequilibrium: Explorations of Austrian Themes*. (M. J. Rizzo, Ed.). Lexington, Toronto: Lexington Books.

[95] Robinson, J. A. (1960). *Collected Economic Papers* (1960 ed.). Basil Blackwell.

[96] Rothbard, M. N. (1962). *Man, Economy, and State* (2004 ed.). Auburn: The Ludwig von Mises Institute.

[97] Rothbard, M. N. (1976). Praxeology: The Methodology of Austrian Economics. In E. G. Dolan (Ed.), *The Foundations of Modern Austrian Economics*. Sheed & Ward, Inc.

[98] Rothbard, M. N. (1979). *Individualism and the Philosophy of the Social Sciences*. Cato Institute.

[99] Samuelson, P. A. (1951). Abstract of a Theorem Concerning Substitutability in Open Leontief Models. In T. C. Koopmans (Ed.), *Activity Analysis of Allocation and Production*. New York and London: Wiley & Sons.

[100] Samuelson, P. A. (1952). Economic Theory and Mathematics--An Appraisal. *The American Economic Review*, 42(2), 56–66.

[101] Say, J. B. (1880). *A Treatise on Political Economy* (1971 ed.). New York: Augustus M. Kelley.

[102] Schumpeter, J. A. (1954). *History of Economic Analysis* (1994 ed.). Oxford: Oxford University Press.

[103] Stackelberg, H. von. (1933). Zwei kritische Bemérkungen zur Preistheorie Gustav Cassel. *Zeitschrift Für Nationalökonomie*, 4, 456–472.

[104] Stigler, G. J. (1941). *Production and Distribution Theories: The Formative Period*. New York: The Macmillan Company.

[105] Stigler, G. J. (1942). *The Theory of Price* (1987 ed.). Prentice Hall.

[106] Stigler, G. J. (1949). *Five Lecture on Economic Problems*. London School of Economics and Political Science.

[107] Thom, R. (1984). Matemática y Teorización Scientífica. In R. Apéry (Ed.), *Pensar la Matemática*. Tusquets Editores.

[108] Uzawa, H. (1962). On the Stability of Edgeworth's Barter Process. *International Economic Review*, 3, 218–232.

[109] Wald, A. (1933). Uber die eindentige positive Lösbarkeit der neuen Produktions-gleichungen. *Ergebnisse Eines Mathematischen Kolloquiums*, 6, 12–20.

[110] Wald, A. (1934). Uber die Produktionsgleichungen der ökonomische Wertlehre. *Ergebnisse1*, 7, 1–6.

[111] Wald, A. (1936). Uber einige Gleichungsysteme der mathematischen Ökonomie. *Zeitschrift Für Nationalökonomie*, 7, 637–670.

[112] Wald, A. (1951). On Some Systems of Equations of Mathematical Economics. *Econometrica*, 19(4), 368–403.

[113] Walras, L. (1874). *Elements of Pure Economics* (2010 ed.). Routledge.

[114] Weintraub, E. R. (1983). On the Existence of a Competitive Equilibirun: 1930-1954. *Journal of Economic Literature*, XXi, 1–39.

[115] Willes, M. H. (1981). "Rational Expectations" as a Counterrevolution. In *The Crisis in Economic Theory*. Basic Books.

[116] Yamane, T. (1962). *Mathematics for Economists: An Elementary Survey* (2012 ed.). Literary Licensing, LLC.

[117] Zanotti, G. J. (1985). El Libre Albedrío y sus Implicancias Lógicas. *Libertas*, 2.

[118] Zeuthen, M. F. (1932). Das Princip der Knappheit, technische Kombination und ökonomische Qualität. *Zeitschrift Für Nationalökonomie*, 4, 1–24.

---

[i] TN: I thank Amy Fontinelle for translation assistance.

[ii] TN: The original publication, in Spanish, was published as a two-part article: the first in *Libertas* (2) and the second in *Libertas* (4). This sentence is modified to reflect that the English translation of part 2 is also published in this volume.

[iii] TN: This sentence is unclear in the original Spanish version. My translation reflects the intended meaning of the original article.

[iv] TN: Translation to English is my own.

[v] TN: My translation. It seems this passage is present in an early edition of Chiang's book (unavailable), but that it has been omitted in later editions.

[vi] TN: It seems that the tables that accompany this explanation in the original Spanish version are incomplete. I revised the table to match its explanation.

[vii] TN: I could not find the original passage in the provided reference.