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

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4. Prose and Mathematics: Clarity

There are a variety of arguments in favor of mathematics and against prose as a superior method to perform economic analysis. These arguments can be summarized as saying that mathematics makes the economist more rigorous in the assumptions he uses and in his deductive reasoning. Let us examine, for example, the following passages. (1) L. R. Klein (1954, p. 360): "Non-mathematical contributions to economic analysis often tend to be fat, sloppy, and vague" and "[c]larity of thought characterizes mathematical economics." (2) William Baumol (1966, p. 94): "One of the most widely recognized advantages of the mathematical methods is that their use requires the explicit listing of all premises employed in the analysis. The rules of the game prevent us from switching assumptions in mid-argument or from the employment of a premise which is unstated and perhaps even unrecognized by us when we use it. It must be granted that here is a major weakness of literary analysis, into which it is often easy to sneak new assumptions as they become convenient, and thereby arrive at whatever conclusions are desired." And (3) George Stigler (1950, p. 40); "[...] the mathematically trained economist states his concepts more clearly, on average, then the unmathematical economist." Let us show the *clarity* attributed by Klein and Stigler to mathematical exposition with a paragraph from the well-regarded mathematical economist Gérard Debreu:[ii]

The private property economy $\varepsilon = \left(\left(X_i, \underset{i}{\lessgtr} \right), (Y_i), (\omega_i), (\theta_i) \right)$ has an equilibrium if:

for all i (a) X_i is closed, convex, and has a lower limit for $\leqslant$,
(b-1) there is no satiety consumption in X_i,
(b-2) for all x_i' in X_i, the sets $\left\{ x_i \varepsilon X_i \middle| x_i \underset{i}{\gtrsim} x_i' \right\}$ and $\left\{ x_i \varepsilon X_i \middle| x_i \underset{i}{\lesssim} x_i' \right\}$, are closed in X_i,
(b-3) if x_i^1 and x_i^2 are two points in X_i, and if t is a real number in $]0,1[$, then $x_i^2 \underset{i}{\gtrsim} x_i^1$

implies $tx_i^2 + (1-t)x_i^1 \underset{i}{\gtrsim} x_i^1$,
(c) there is an x_1^0 in X_i such that $x_i^0 \ll \omega_i$; for all j $\qquad$ (d-1) $O \varepsilon Y_j$;
(d-2) Y is closed and convex,
(d-3) $Y \cap (-Y) \subset \{O\}$;
(d-4) $Y \supset (-\Omega)$.

If I am not wrong, "The private property economy [...] has an equilibrium if" would be the only *clear* passage for the majority of the readers. For another smaller group, familiar with set theory, the quote would be a little more clear, because they would understand the meaning of the words *closed*, *convex*, *lower limit*, etc., within the context in which they are used. However, only those who understand what Debreu means by ε, X_i, Y_i, ω_i, Ω_{ij}, etc., would be able to understand the full passage.

The clarity of what is being communicated is related to (1) the quantity of individuals that understand the meaning of the employed symbols to transmit the ideas, and (2) the degree of ambiguity in the employed symbols. Let us examine each one of these.

If I write "$pytaqui$," probably no one would understand what I am trying to say, and thousands of interpretations could be given if I used it in a sentence such as "last night I contemplated for three hours the outlines of $pytaqui$." Unless I change this symbol for another one that other individuals would understand, it would be impossible to transmit the message. If I replace the symbol "$pytaqui$" for "$Uranus$" the clarity of what

I want to communicate increases significantly because a much larger number of individuals understands what is meant when they read "*Uranus*."

A less extreme example is that of different languages. Some languages use words and symbols that are more universally known than other languages do. For instance, a book written in English would be more comprehensible for many more individuals than a book written in Japanese.

The use of mathematics can be useful to facilitate the use of inference rules through symbols, usually letters, that represent variables and constants. However, loose letters, in general, are meaningless. For instance, I can write: *TC = VC + FC,* whose meaning is less clear than writing "total cost equals the variable cost plus the fixed cost." Both ways to write this proposition are equivalent once we know the meanings of *TC*, *VC*, and *FC*. However, in order for both representations to have equal capacity in transmitting the message of what is meant by *TC*, *VC*, and *FC* to other individuals, these symbols have to be understood by the same number of individuals who understand the meaning of the symbols for *total cost*, *variable cost*, and *fixed cost*. But, if this occurs, then *TC*, *VC*, and *FC* become words. For instance, *TC* would be a synonym for *total cost.*

Ultimately, words are symbols that individuals associate with concepts. Without these associations, communication would be impossible. It is contradictory, then, to sustain that the use of symbols devoid of meaning allows us to put forth the concepts more clearly "on average" than prose does, as Stigler and Klein believe.[1] Of course, part (or maybe a great part) of the communication problem is due to the fact that mathematical symbols have no meaning on their own. Nevertheless, whatever the cause of the problem may be, the conclusion should be the opposite to that of Stigler: "on average, the mathematical economist tends to present his ideas in a less clear way than the non-mathematical economist."

The second factor related to the clarity of communication is the ambiguity of terms.[2] In many cases, words can have more than one meaning, that is, one symbol can be associated with more than one concept. When this happens, the message or proposition of the author can give way to disparate interpretations and, therefore, the reader will understand something different from what the author meant to transmit or, if the reader notices the ambiguity, he will not be able to know with precision what it is that the author wants to say. The typical ambiguity example is that of Croesus, King of Lydia, who before going to war with Persia inquired of the Delphic oracle to be sure that victory would be his. The oracle's answer was: "If Croesus starts a war against Persia, he will destroy a powerful kingdom." Croesus, excited about the oracle's response, started the war, but was defeated by the Persians. To his complaint, the Delphic priests replied that the prediction of the oracle was correct: Croesus destroyed a powerful kingdom: His own! Copi's (1961, p. 76) example about the English definition of "anthropology" is also illustrative: *The science of man embracing woman*.

In these two examples, "a powerful kingdom" and "embracing" are ambiguous terms; they can be associated with more than one concept. Obviously, the use of prose to reach conclusions can give rise to false interpretations and conclusions if ambiguous words are used, but this is a problem that is not solved with the use of mathematics. As pointed out by Henry Seager, there is no "sovereign virtue in mathematical modes of thought" (Fetter, 1977, p. 237) that would protect the mathematical economist from error; rather, as we will see, the opposite occurs. The way to avoid ambiguity and fallacies is through the proper definition of terms.

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[1] This communication problem of mathematical economists has been recognized in a number of articles published in *The Review of Economics and Statistics*, vol. 36, 1954. Duesenberry (1954, p. 362): "Economics seems to have suffered for a long time from 'too many chiefs and not enough Indians'—or more accurately, from a disastrous lack of communications between the Indians and would-be chiefs." Tinbergen (1954, p. 368): "[…] certain of them [economists] express themselves 'so mathematically' that they cannot be understood." Solow (1954, p. 373) "That there is a communication problem I freely agree." Dorfman (1954, p. 376): "An amelioration of the communication difficulty lies with translators or popularizers who undertake the ungrateful task of trying to grasp the meaning of the mathematics and restate it with tolerable accuracy in English." Koopmans (1954, p. 379): "[…] such communication is urgently needed to overcome what seems to be the more serious consequence of the present 'language difficulty': rather than being impressed, the discerning economist may tend to overlook the significance of contributions made by mathematical analysis in economics, because he has no real opportunity of absorbing and evaluating their contents."

It should be remembered that something similar occurs in the natural sciences. According the famous physicist Gamow (1961, p. 149): "[…] the fact is that, for conceiving a theoretical picture of a puzzling physical phenomenon, a knowledge of the intricate mathematics is often quite unnecessary and sometimes even harmful. The explorer may easily be lost in the jungle of complicated formulas and, as a Russian proverb says, 'cannot see the forest for the trees'."

[2] Sometimes *ambiguous* and *vague* are differentiated. The former is the case of a word with more than one meaning. The latter is about the limits to the "extent" of the concept and, therefore, it is an issue in limited cases. For instance, if a political leader states that "we are going to give houses to the poor," then the term *poor* is *vague*; we need to precisely define who is and is not poor. For our analysis, the important issue is that the word should not have more than *one* interpretation. Therefore, *vague* terms are included inside *ambiguous* terms.

Obviously, we cannot define a term using symbols empty of meaning; only with words can we avoid ambiguity through definition.

The context in which an idea is written is in many cases enough to eliminate ambiguities, even if the literal reading of the text is subject to ambiguous interpretations. If, for instance, when entering a building, we find a sign with the note, "Because there are stairs, ownership takes no responsibility for the use of the elevator," no one would doubt the type of stairs the sign is referring to. If the building does not have stairs that connect all the floors, it will be difficult in the case of an accident for ownership to allege no responsibility because there is a painter's ladder somewhere in the building. A statement such as "I'm sunbathing" is also clear; no one would interpret this sentence as if I were trying to clean myself with sun instead of water, even if the word "bathe" could be interpreted in this latter sense.

However, there are other cases in which the group of symbols is clearly ambiguous. For instance, in Spanish the word "muñeca" can either mean a doll or the wrist. If we say "the child broke her *muñeca*" (in Spanish), this could mean that she broke the articulation between her hand and her forearm or that her toy got broken. Cases like this require more information (i.e., more meaningful symbols to avoid the ambiguity). Whoever is writing needs to be attentive to avoid these sorts of ambiguities.

The same applies to punctuation. For instance, we can write: "The economists (,) who believe that the use of mathematics makes theory more rigorous (,) are wrong."

Whether or not we add a comma after "economists" and after "rigorous" would significantly change the meaning of the sentence. The absence of the comma suggests that the reference is to *some* economists; on the other hand, the presence of the comma suggests that the reference is to *all* the economists.

Obviously, these types of ambiguities and imprecisions can occur when prose is used to deduce theorems, but this does not imply that the use of mathematics solves or minimizes this problem. Therefore, it is advisable that the economist, before becoming an expert in mathematics and statistics, become well versed in the fundamental notions of logic in order to avoid fallacies in his reasoning—and learn how to write properly to transmit his ideas as clearly as possible.

At his conferences in Glasgow, Adam Smith would insist with great eloquence that the way to improve the use of language is through precision. The perfection of the writing style, said Smith (1983, p. 55), "consists in Express[ing] in the most concise, proper, and precise manner the thought of the author, and that in the manner in which best conveys the sentiment, passion or affection with which it affects or he pretends it does affect him and which he designs to communicate to this reader. This you'll say is no more than common sense, and indeed it is no more."

The statement that mathematical symbology is, on average, more clear than prose does not seem to have the minimum logical foundation and cannot be taken to be more than an emotional statement.

5. Prose and Mathematics: Thoroughness

Samuelson (1952, p. 56) has said that "[i]n principle, mathematics cannot be worse than prose in economic theory; in principle, it certainly cannot be better than prose. For in deepest logic—and leaving out all tactical and pedagogical issues—the two media are strictly identical." This is similar to Fisher's statement: "There is no place you can go by railroad that you cannot go afoot" (cited by Samuelson, 1952, p. 56).

Fisher's and Samuelson's statements are only half-true. Not everything that is said in words is translatable into mathematics, but any mathematical formulation can be translated into words. The mathematical symbology as we know it today is the outcome of a very long and slow historical evolution. Different symbols were progressively added for reasons of arithmetical convenience. For instance, it is possible to multiply "four thousand five hundred twenty three" times "thirty thousand four hundred fifty two" using only words. However, the calculation is obviously much less cumbersome if we "translate" the words into numerical symbols. Not only the "visualization" but also the mechanics of the calculation become significantly easier.

The best example of this is algebra, name that is derived from a Persian book called *al-jabr w'al nuqâbalah* and whose author was al-Khowârizmî.[3] In this book, which is believed to have been published around 800 BC,

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[3] It seems that his complete name was Mohammed ibn Musa Aby Djefar al-Khowârizmî. The last part is a reference to his place of birth. See Rouse Ball (1908, p. 121).

al-Khowârizmî developed two methods to solve quadratic functions of the form $x^2 + px = q$ without introducing a single mathematical symbol. Prose was the way to do algebra for more than 500 years. Only around 275 AD would a famous Greek mathematician, Diophantus of Alexandria, have used for the first time "an algebraic symbolism worthy of its name" (D. E. Smith, 1925, p. 382).[4] The equal sign (=) was introduced in 1557 by the English mathematician Robert Recorde, and the multiplication sign (×) around the year 1600.[5] As you can imagine, people multiplied and worked with equalities before these signs were introduced.[6]

Mathematical symbology is nothing other than a translation of words with the end of facilitating calculus and deductions of *some* concepts. Mathematical symbology makes its prose equivalent neither more nor less rigorous. For instance, the words "equals," "plus," "minus," "times," and "divided" are neither more nor less precise than symbols =, +, −, ×, and ÷; and the words "two," "three," and "four" are neither more nor less thorough than the symbols 2, 3, and 4. Furthermore, since =, +, −, ×, ÷, 2, 3, and 4 have meaning, we could say they are synonymous.

As we saw in part I, the renowned mathematician and son of the founder of the Austrian school of economics, Karl Menger, was one of the most important promoters of mathematical economics in Vienna. He pointed out the following (Menger 1973, p. 39):

> The sentence:
> (I)   Any real number increased by 1 is equal to 1 increased by that number
> Is perfectly equivalent to the assertion
> (I') $x + 1 = 1 + x$ for any real number x

The expressions "two plus two equals four" and "$2 + 2 = 4$" are also "perfectly equivalent." The difference is that the second expression saves space and allows for a faster reading and, in the case of longer and more complex algebraic expressions, the use of mathematical symbols is much more convenient and concise. To write in words an expression such as $y = x^2 + 8^3x - 24z + 8.81w$ is quite inconvenient. However, the difference between the two ways to write this expression is a mere issue of space. To read this expression in words would take more time, *but in no way would it be less exact or less rigorous*.

The use of mathematical symbology in place of words becomes even more convenient when making calculations. It would be quite tedious to add polynomials using words, the solution would take much more time, and, once again, this would not make it less exact or rigorous.

Now, these expressions maintain a perfect equivalence between their verbal and symbolic representations because the nature of what is being said allows it to be expressed one way or the other. The nature of the problem is that it refers to *quantities* and their relationship, or, put differently, to variables that are susceptible to being *quantified*. As mentioned by Keynes (1936, p. 40): "To say that net output to-day is greater, but the price-level lower, than ten years ago or one year ago, is a proposition of a similar character to the statement that Queen Victoria was a better queen but not a happier woman than Queen Elizabeth—a proposition not without meaning and not without interest, but unsuitable as material for differential calculus."

The great mistake of mathematical economists is to try to translate verbal propositions to equations without taking into consideration when making such a translation if what is being said is "exactly" equivalent to what was said in words. This is a crucial point: if the mathematical formulation is not a perfect equivalent, it can lead to more or less logical implications that the use of words would allow.[7]

Let us see some examples, starting again with a quote from K. Menger (1973, p. 41):

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[4] There is an interesting reproduction of the first page of al-Khowârizmî's book on page 383.

[5] For a detailed explanation of the evolution of numerical signs and the calculation methods of addition, subtraction, multiplication, and division, see D. E. Smith (1925) and the entry "Mathematics, History of" in the *Encyclopedia Britannica*.

[6] It is worth reproducing here al-Khowârizmî's passage that D. E. Smith (1925, p. 447) cites to show how the solution of a quadratic equation would be presented in prose: "You halve the number of the roots, which in the present instance yields five. This you multiply by itself; the product is twenty-five. Add this to thirty-nine; the sum is sixty-four. Now take the root of this, which is eight, and subtract from it half the number of the roots, which is five; the remainder is three. This is the root of the square which you sought for; the square itself is nine."

[7] Note that the opposite is not possible: Any mathematical expression has its "exact" equivalent in words, but it is not true that any verbal expression has an "exact" mathematical equivalent. Dorfoman's (1954, p. 375) statement that "[…] many economists forget the converse: there are some things which can be said in mathematics but which cannot be paraphrased with tolerable accuracy in ordinary English" does not have the slightest support. When Dorfman explains his hypothetical models to his students, unless he communicates with hand signs, he will have to explain them in words. If he cannot explain with "words" what his symbols mean "exactly" I do not see how his students (or readers) "exactly" understand what his equations mean.

> As far as *precision* is concerned, for example, the statements
> (2) *To a higher price of a good, there corresponds a lower (or at any rate not higher) demand*
> (2') *If p denotes the price of, and q the demand for, a good, then*
> $q = f(p)$ and $\frac{dq}{dp} = f'(p) \leq 0$
> Those who regard formula (2') as more precise or 'more mathematical' than the sentence (2) are under a complete misapprehension just as are the textbook writers who consider the formula (1') as more precise or 'more algebraic' than the verbal sentence (1). The only difference between (2) and (2') is this: since (2') is limited to functions that are differentiable and whose graphs, therefore, have tangents (which from an economic point of view are not more plausible than curvature), the sentence (2) is *more general*, but it is by no means less precise; it is *of the same mathematical precision* as (2').

As K. Menger says, expression (2') is "limited to functions that are differentiable." However, most goods depict noncontinuous functions. Even though we can think of the price as a continuous variable, this assumption is harder to accept for the "quantity" variable. Most goods are not divisible (cars, suits, houses, desks, horses, etc.) Yet, even if they were divisible, individuals do not make decisions on infinitesimal quantities.[8] Now, if formula (2') refers only to some cases (maybe so few that they would be marginal), then it is a less general expression than sentence (2); both formulations are not "exactly" equivalent. Therefore, to translate sentence (2) into expression (2') makes the message less rigorous. Note that if we translate expression (2') to words then we have exactly what this expression says, but we would not obtain sentence (2). Instead, we will get something such us: "quantity demanded (q) is a function of its price (p), whose derivative with respect to (p) is not positive." Of course, one could dispense with the continuity assumption, but I include it given its popularity in today's economic theory. To open any microeconomics textbook is enough to verify its popularity.

Another widely used example is the consumer utility optimization problem. According to mathematical theory, a consumer has to choose between two goods, x and y, and maximizes his utility when the condition is satisfied that the marginal rate of substitution between x and y equals the relative prices of x and y. The marginal rate of substitution between x and y is defined as the number of units of y that has to be given up for each additional unit of x such that the utility level remains constant. This is equivalent to saying that the marginal rate of substitution equals the ratio of the marginal utilities of x and y.[9] This is why consumer utility maximization is usually written as $\frac{UMg_x}{UMg_y} = \frac{p_x}{p_y}$ or $\frac{UMg_x}{p_x} = \frac{UMg_y}{p_y}$. If $\frac{UMg_x}{p_x} > (<) \frac{UMg_y}{p_y}$ then the consumer would transfer resources toward buying x (y), which would make $UMg_x \left(UMg_y\right)$ decrease and $UMg_y(UMg_x)$ increase.

This way of expressing the consumer utility optimization implies that marginal utilities are measurable (the unit of measure is usually referred as "utils") and that, therefore, these quantities can be added, subtracted, multiplied, and divided (like in this case)—all of which is false.

The Austrian economists, using prose, arrived at different formulations and conclusions. *Strictly* speaking, the consumer does not maximize his welfare when the marginal utility of the last dollar spent *equals* the marginal utility of the last good bought, but when he *buys up to the last unit where decreasing marginal utility exceeds the decreasing marginal utility of the good given up in exchange (for instance, money).* The difference is clear when the problem is presented as an "ordering" rather than as a measure of utilities (as the indifference curve assumes). Let us consider the following example. Assume the scale of initial values of an individual is given in the first column of table1 (parentheses denote the goods that the consumer does not have, and lack of parentheses denotes the goods that the consumer *does* have).

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[8] As Rothbard (1962, pp. 307–308) points out: "If it is a matter of indifference for a man whether he uses 5.1 or 5.2 ounces of butter for example, because the unit is too small for him to take into consideration, then there will be no occasion for him to act on this alternative. He will use butter in ounce units, instead of tenths of an ounce. For the same reason, there are no infinitely small steps in human action. Steps are only those that are significant to human beings; hence, they will always be finite and discrete."

[9] If total utility (U) is a function of x and y, then $U = U(x, y)$ where $\partial U/\partial x$ and $\partial U/\partial y$ are the marginal utilities of x and y, respectively. Given that when substituting one good for another we assume that total utility does not change, then, $\partial U/\partial x \cdot dx + \partial U/\partial y \cdot dy = 0$, from where $-dy/dx = (\partial U/\partial x)/(\partial U/\partial y)$, where dy/dx is the marginal rate of substitution.

Table 1. Consumer value ranking

		1	2	3
First	1	(cow)	cow	cow
First	2	horse	horse	horse
Second	3	(cow)	(cow)	cow
Second	4	horse	horse	horse
Third	5	(cow)	(cow)	(cow)
Third	6	horse	horse	(horse)
Fourth	7	(cow)	(cow)	(cow)
Fourth	8	horse	(horse)	(horse)

The ranking of values is an "ordering" in which we locate at the top the goods for which the consumer assigns a higher marginal utility and we continue in decreasing order. In our example, the marginal utility of the first cow is superior to the marginal utility of the first horse, the marginal utility of the first horse is superior to that of the second cow, etc.

Given the initial condition (column 1), the individual could improve his situation if he can give a horse in exchange for a cow, given that the marginal utility of the fourth horse is less than the marginal utility of his first cow. That exchange would put him in column 2; instead of having four horses and no cows, he would have three horses and one cow. Given his preferences, this is a better combination than the first one. Furthermore, the situation in column 2 can be improved if this person can exchange another horse for a second cow, because the marginal utility of giving up a second horse is less than the marginal utility of acquiring a second cow. This exchange would put him in column 3.

In these successive exchanges, the marginal utility of the cows decreased, while the marginal utility of the horses increased. Once this person is in the situation depicted in column 3, the individual cannot perform any more exchanges that would improve his utility. Still, even though he has maximized his utility, it cannot be concluded that the marginal utility of the last good bought equals the marginal utility of the last good sold. What's more, as long as the analysis is based on an "ordinary" scale of values, then the marginal utilities can *never* be equal in an exchange. This should be obvious: exchange occurs when each one of the individuals values more what he receives than what he turns over and never when the valuations are *equal*. In our example, the second cow is the last one, whose decreasing marginal utility exceeds the decreasing marginal utility of the good given up (in this case, the second horse). To buy a third cow would imply giving up one horse whose marginal utility is superior.

To treat this issue in terms of continuous function and derivatives does not make the analysis more thorough; it makes it less thorough. Even if it is true that marginal utilities tend to match and that derivatives explain borderline cases, this does not mean that the conclusion that the consumer maximizes his welfare when marginal utilities are equal is *less* precise than the prose conclusion that the Austrians arrive at. This loss of thoroughness is due precisely to the misapplication of mathematical analysis.[10]

The acclaimed mathematical thoroughness of mathematical economics seems to have a big hole in one of the most widely spread theories: the inefficiency of a monopoly with respect to a market of perfect competition. What is interesting is the inconsistency of the conclusion despite the graphs and derivatives used.
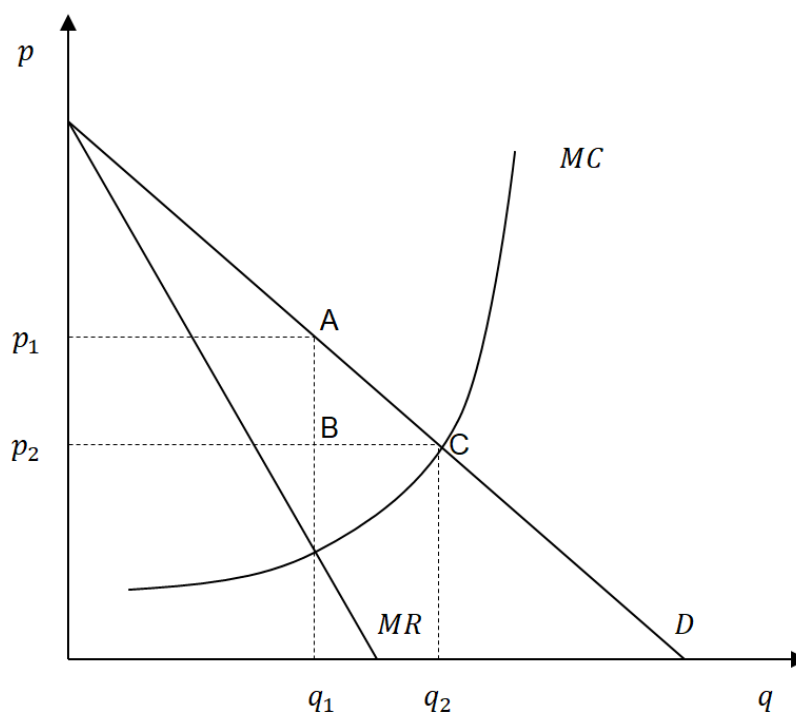
Under perfect competition, a firm has no effect on the market price when it expands or contracts its output. This means that the price is given to the firm, which behaves as a price "taker." Any microeconomics textbook presents this postulate, but, to put it in the words of an economist of repute, let us cite Stigler (1949, p. 149): "The individual will treat the current market price as given […]: no individual buys or sells enough of a commodity to exert a perceptible influence on its price. The supply curve appears […] as a horizontal line."[iii] From here it is concluded that for a firm, the market price equals the marginal revenue (e.g., for each additional unit that the firm sells, its revenue is incremented by the sale price). From this it follows that profits are maximized at the output level whose "marginal cost equals price" (p. 150).[11]

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[10] For a more detailed analysis, see Rothbard (1962, chap. 4).
[11] This point is subject to the same criticism raised by the consumer utility maximization problem.

The monopolist, however, is not a price taker. When he changes the level of output, it affects the market price. He is facing the whole market's demand curve, which, of course, has a negative slope. Now, when the demand line is no longer "perfectly" elastic, then it is no longer true that the market price equals marginal revenue. The marginal revenue line has a steeper slope than the demand line. It is to the monopolist's advantage to increase output when the marginal revenue is more than the marginal cost, and he would maximize his profits when the marginal revenue (not the price) equaled the marginal cost. This implies a suboptimal situation with respect to the case of perfect competition. The following graph shows this conclusion.

Figure 1. Monopolist

Under perfect competition, all firms together would produce q_2 at price p_2. At this point, the price equals the marginal cost (MC). In the case of the monopolist, however, the producer would produce q_1 (which is less than q_2) at price p_1 (which is higher than p_2). Therefore, under equal costs, a pure monopoly is less optimal than perfect competition because it produces less output at a higher price. The ABC triangle measures the welfare increase that could be achieved if production were raised to the competitive level.[iv]

This conclusion about the superiority of a competitive market over a monopoly has been very convenient because, as Stigler says, "[...] antitrust laws were created to approximate competition [...]."[v] However, so many graphs, equations, and derivatives seem to have led to the wrong conclusion.

In fact, one of the main differences between the monopolist and a firm under perfect competition is that the former faces the whole of market demand, while the latter faces only a small portion of it. What does not look consistent is to say that the demand line that the monopolist faces is downward sloping while the one that the competitive firm faces is horizontal or "perfectly" elastic. Given that these two are the same market (good), this is not mathematically possible. If each competitive firm faces a "perfectly" elastic demand line, then the total market demand *cannot* be "a conventional downward sloping line";[vi] the slope of a function composed of various functions is the summation of the slope of the functions that compose it. Therefore, it is not possible to obtain an aggregate demand function with negative slope from a summation of demands that make quantity q equal a constant p and therefore have a slope of zero. The aggregation of demands with slope zero is an aggregate demand also with slope zero—that is, horizontal as well.

Now, if aggregate demand is as horizontal as the one facing the competitive firm, then it is not true that under *equal* costs a pure monopolist produces less than the aggregation of the competitive firms because the monopolist would also face a horizontal demand line.

However, if the demand line is a conventional one—that is, with negative slope—it cannot be true that in an atomized market each individual firm has "no" influence over the price. To be *rigorous*, the individual demands have to have an infinitesimally small slope; the slope "tends" to zero, but "is not" zero. If a firm under perfect competition does not face a horizontal demand, but one with an infinitesimal slope, then it is no longer true that for this type of firm the price "equals" the marginal revenue. There will be an infinitesimal difference between both. Therefore, the competitive firm would maximize profits at the point in which the marginal revenue "equals" the marginal cost; but at this point the price would also exceed the marginal cost. If we aggregate the behavior of the individual firms, the result has to be the same as the one we get with a pure monopolist (because of the equal cost assumption).

In fact, there is no reason at all to assume that a monopolist *per se* is an "imperfection" of the market. The market does not only fix prices and quantities, it also sets the size and number of firms. In the absence of legal barriers of entry to competition, the emergence of a monopolist is nothing other than that said monopolist is more efficient than a number of firms in competition.[12]

These examples were chosen because, in general, almost all mathematical economists agree with the conclusion that a consumer maximizes his utility when the marginal utilities of the exchanged goods are equal and that, under equal costs, perfect competition produces more output at a lower price than a monopolist does. Therefore, I think these examples are, to put it in statistical terms, "representative samples" of the errors and internal inconsistencies that the use of mathematics in economics has produced. Of course, this does not mean that the use of prose would free the economist of errors and inconsistencies; recall that the literary economists of the classical school committed a number of errors and logical inconsistencies. Despite this, the use of prose has allowed economists to reach much more fertile conclusions to understand the mechanics of the market than the use of mathematical models has. I think this is because of two factors: (1) the use of prose does not require the economist to make unrealistic assumptions, and (2) to force a "translation" to mathematics opens the door to false logical implications that the use of prose would not allow (e.g., matching marginal utilities in the consumer optimization problem), or to avoiding conclusions that the use of prose would not allow (e.g., the role of the entrepreneur in the market, which cannot be explained in a model such as Walras's). As a general rule, we should doubt any mathematical translation that is not saying "exactly" what was said in words.

Let us now move to the argument from mathematical economists, the one that sustains that in the literary analysis we would constantly inject arbitrary assumptions for pure convenience. At the beginning of this second part, we quote Baumol (1966, p. 94) saying that "[i]t must be granted that here is a major weakness of literary analysis, into which it is often easy to sneak new assumptions as they become convenient, and thereby arrive at whatever conclusions are desired." Unfortunately, Baumol does not give an example of what he means. It is not always the case that the introduction of additional assumptions is incorrect; what is important is that the additional assumption does not erode the validity of the conclusions. The same mathematical economists continuously add mathematical theorems as assumptions that help them reach the conclusions they want.

It is precisely at this point where we can counterargue. Who, other than mathematical economists, has introduced assumptions that are arbitrary to the point of making their models unreal, and all for pure

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[12] The following passage from Nikaido (1978, p. 170; except "data", italics are mine) is a good reflection of this incoherence: "The firm (under perfect competition) cannot exercise any *significant* influence over the market through artificial measures such as price manipulation because its size is tiny with respect to the whole market. Therefore, this firm takes the prices as given *data* that *cannot alter* by its own means, that is, with restrictions to its output plans. In fact, as the firm increases its size, its influence in the market would also be more significant." Nikaido first sustains that there is an influence on prices, but this is *insignificant*; second, he says that the firm cannot influence the price at all (or, at least, that is what the firm thinks, which contradicts the assumption of perfect information), and third, he says that the firm can change prices. One has the right to wonder: Which one is it? With respect to the market as a whole, the difference between these statements implies a *big* difference.

Other authors have recognized this inconsistency in the assumptions and introduced another artifact, the auctioneer, who is in charge of changing the prices. For instance, Arrow and Hahn (1971, p. 325) sustain that "in a production economy, if every firm faces a horizontal demand curve (or thinks that it faces such a curve), it is not easy to visualize any firm changing the price at which its product is sold. What is happening now is that, having decided on one idealization (perfect competition), we run into what must be taken to be logical difficulties unless we import a further idealization: the auctioneer." This reflects more the functioning of a socialist economy, as this was proposed by Lange and Taylor (1938) in "*On the Economic Theory of Socialism.*"

As Mises (1922, p. 385) says, "[n]o other part of economic theory has been so much misunderstood as the theory of monopoly."

For a critical Austrian analysis, see Mises (1922, pt. III.II), Kirzner (1973, chap. 3), and Rothbard (1962, chap. 10).

convenience? Weren't the mathematical economists the ones who invented a world feasible to be explained in terms of equations? What is the foundation of assuming continuous functions? Is this not an assumption for pure *convenience* to demonstrate the existence of a Walrasian equilibrium? For instance, Dorfman, Samuelson, and Solow (1958, p. 367), in what they call a *rigorous* proof on the existence of solutions, say that they "follow Wald in requiring our demand functions to be continuous. This is a minimal assumption of mathematical regularity which can hardly be dispensed with. Without it one could hardly hope to prove the existence of a competitive equilibrium […]." Given that in reality demand curves cannot be continuous, the theorem does not go beyond an interesting but impractical mental exercise—that is, is not scientific. Science requires more from a theorem to be logically *rigorous*; it also needs to explain facts as they are happening in reality. Baumol's observation, we may say, fits with great precision what mathematical economists do: accommodate the world to explain to the *convenience* of their own methodology.

Another argument in favor of the use of mathematics in economics sustains that formalization allows economics to advance faster. For instance, Fisher says that the use of mathematical symbology is like crossing the United States by train, while to use verbal formulation is like a transcontinental walk. We find a similar position in Whitehead (1911, p. 61): "[…] by the aid of symbolism, we can make transitions in reasoning almost mechanically by the eye, which otherwise would call into play the higher faculties of the brain."

Since the argument is about "velocity" rather than degrees of precision or ambiguities, this is a secondary argument. Yet, it is worth pointing out that the advance of "literary" economics has been faster than that of mathematical economics. Furthermore, it could be said that mathematical economics is an imprecise way of saying what literary economists have previously said. Maybe the clearest example of this would be that of expectations and imperfect information that mathematical economists started to include in their models almost twenty years after the literary economists of the Austrian school.

I think the following passage by Mises (1949, p. 354, italics are mine) explains why verbal analysis is always ahead of the mathematical one (at least in economics):

> The deliberations which result in the formulation of an equation are *necessarily* of nonmathematical character. The formulation of the equation is the consummation of our knowledge; it does not directly enlarge our knowledge. Yet, in mechanics the equation can render very important practical services. As there exist constant relations between various mechanical elements and as these relations can be ascertained by experiments, it becomes possible to use equations for those solutions of definite technological problems. Our modern industrial civilization is mainly an accomplishment of this utilization of the differential equations of physics. No such constant relations exist, however, between economic elements. The equations formulated by mathematical economics remain a useless piece of mental gymnastics and would remain so even if they were to express much more than they really do.

In fact, the formulation of an equation is in principle no more than a hypothesis born from a nonmathematical previous reasoning. An economist who says that the quantity demanded of a good x depends on its price, disposable income, and the price of good y gains nothing by writing $D_x = f(p_x, Y, p_y)$.[13] To see an advancement in knowledge we need to find the way in which these variables are concretely related to each other, which in economics is not possible given the reasons explained in section 3 (part I). In general, mathematical economists limit themselves to representing models with linear equations for convenience. Yet, since it is also impossible to test the hypothesis that D_x depends on p_x, Y, and p_y, also for the reasons presented in section 3 (part I), the model does not go beyond being a nonverifiable hypothesis of relations between variables.

Paraphrasing Hazlitt (1959, p. 102), we could say that is possible to formulate *hypothetical* mathematical functions and deduce from them *hypothetical* relations that allow us to reach *hypothetical* conclusions. "We can, in short, assert that a general hypothetical relationship implies specific hypothetical relationships," he writes. As we saw in part I, none of them is empirically testable.

To conclude this point, we can say that only a few would doubt the logical *rigor* of mathematical models to extract certain conclusions from their premises and assumptions (yet, as we saw with the case of the monopoly, there could be exceptions to this alleged internal consistency). However, I have the intuition that among

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[13] Not even space and ink, since words are needed to explain what each symbol means.

mathematical economists a belief prevails that scientific rigor is synonymous to a theorem perfectly demonstrated from a logical point of view. Even more, it seems that some of them are convinced that if a theorem is not mathematically formalized, then it is less scientific. In the meantime, the theory's potency to explain the real world seems to have been displaced to the background, even if the scientist should formulate logically consistent theorems that explain how variables are related in the real world. Lack of logical thoroughness is as sufficient as lack of realism to invalidate a theory from a scientific standpoint.

In this sense, the obsession with formulating economic theory in mathematical terms has caused said theory to have a very low level of realism that, as we saw in part I, has been admitted by mathematical economists themselves. As Mises (1933, p. 127) pointed out, "[t]hus far, the use of mathematical formulations in economics has done more harm than good" because it has led economic theory on a path that moved it away from its final objective, which is to explain how real markets work.

6. Logic, Mathematics, and Consistency

The "literary" economist resorts to the rules of logic to extract valid conclusions from his premises. The mathematical economist translates words into equations and applies mathematical rules to extract his conclusions. Are the rules of logic different from those of mathematics? What is the difference in their inference mechanisms? To answer these questions, we need to start asking what the object of study of logic and mathematics is, or if they are one and the same thing.

There seems to be agreement that the objects of study of logic are the methods and principles that allow us to distinguish a correct reasoning from an incorrect one (Copi 1961, p. 4). Logic has no interest in whether the premises are true or false, but in whether or not the conclusion *necessarily* follows from the premises.

The object of study of mathematics has become less clear since the development of non-Euclidean geometries. At the beginning, there was clarity in the object of study: mathematics was the science of quantities. However, today, it seems mathematicians themselves cannot agree on what it is that they study, even though on this they are not alone, since economists have the same issue.

As we will see, there is a group of mathematicians that, after distinguishing between pure and applied mathematics, reached the conclusion that the first one is identical to logic; this way logic and mathematics would be one and the same science. For instance, Peirce (1881, p. 97) argues that "[m]athematics is the science which draws necessary conclusions." According to Whitehead (1898, p. VI): "Mathematics in its widest signification is the development of all types of formal, necessary, deductive reasoning." Russell (1903, p. 3) says: "Pure mathematics is the class of all propositions of the form 'p implies q,' where p and q are propositions containing one or more variables, the same in the two propositions, and neither p nor q contains any constants except logical constants." Also, Nagel and Newman (1958, p. 12): "[…] the sole question confronting the pure mathematician (as distinct from the scientist who employs mathematics in investigating a special subject matter) is not whether the postulates he assumes or the conclusions he deduces from them are true, but whether the alleged conclusions are in fact the *necessary logical consequences* of the initial assumptions."

This raises a semantic problem because if these statements are true, then logic and mathematics become the same thing. When "literary" economists claim that the use of mathematics is inappropriate to explain market phenomena, to which mathematics do they refer? Without being an expert on the topic, I will try to briefly explain how this association between mathematics and logic came to be, what it is that other schools or currents of mathematics think,[14] and what the implications might be for mathematical economics.

As is well known, Euclid is known for having reduced to five all geometry postulates. Yet, what he actually did was to unify the works of other thinkers into one systemic work (Eves and Newson 1958, p. 14; Rouse Ball 1908, p. 54). For the next two thousand years, Euclid's five postulates were taken as true because they were evident by themselves, except the fifth one. The five postulates are:

1) Any two points can be joined with a single straight line;
2) A straight line can always be prolonged;

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[14] For a more detailed exposition of this issue, see Eves and Newson (1958), Kneebone (1963), Nagel and Newman (1958), Dou (1974), and Crossley et al. (1972).

3) There is only one circumference with a center and a given diameter;
4) All right angles are equal; and
5) If a secant cuts two lines forming interior angles on one side whose sum is less than two right angles, then the two lines must intersect if they are prolonged enough.

The fifth postulate is an indirect reference to parallels; it is equivalent to say that through an external point of a straight line, which is in the same plane as the point, there is only one parallel line that can be drawn to said line. This happens when the sum of the internal angles equals 180 degrees.

Because of the way the fifth postulate was presented, it was not as self-evident as the other four. Some mathematicians thought it could be deduced from the other four. Only in the 19th century was the independent of the fifth postulate proven with the birth of non-Euclidean geometry, whose precursors were Lobachevsky, Riemann, and Bolyai.

In 1826, Lobachevsky developed a new geometry by changing Euclid's fifth postulate and assuming that more than one parallel can be drawn from an external point to the line which is in the same plane, which leads to the addition of the internal angles being less than 180 degrees. Lobachevsky named his geometry "imaginary geometry." More or less at the same time, Bolyai reached results similar to those of Lobachevsky.

In 1854, Riemann developed another type of non-Euclidean geometry in which he assumed that through the external point to the line, there is no parallel in the same plane. This way, two different non-Euclidean geometries were created: (1) Lobachevsky-Bolyai, or hyperbolic geometry, and (2) Riemann or elliptic geometry.[15]

The birth of non-Euclidean geometry revolutionized mathematical foundations, because it seemed that it had been demonstrated that it was possible to develop a set of consistent axioms that were contradictory to our senses. From this discovery, it unfolded the idea that the work of the pure mathematician is to deduce theorems from arbitrary axioms and that, as a mathematician, he should not be concerned whether or not the axioms are true or not in the real world.

However, this new idea came with a problem that, until then, had been ignored: Are arbitrary axioms consistent, or can they lead to contradictory theorems? Euclidean geometry axioms were accepted as true because they were corroborated in the space in which man moves. No one ever thought about the possibility of reaching contradictory theorems from Euclid's postulates.

The case of non-Euclidean geometry is different. At the beginning, its axioms were considered clearly false with respect to our real space and, therefore, doubts about their consistency were raised. The fact that the theorems developed so far did not contradict each other was not a solution to the problem. It was possible that some future theorem would introduce said contradiction.

It was not possible to definitively conclude that non-Euclidean geometry was a valid alternative to Euclid's geometry until this problem was solved. The fundaments of mathematics had to be revisited.

In 1869, Beltrami found a meaning to Lobachevsky's hyperbolic geometry, and he did so over a Euclidean model. Lobachevsky's geometry lines and curves can be represented over a non-Euclidean surface with a saddle shape. Another interpretation was given by the famous mathematician Poincaré, also over the basis of a Euclidean figure: in this case, a circle. Reimann's lines and curves can also be represented over the Euclidean surface of a sphere.[16] This way, the consistency of both non-Euclidean geometries was demonstrated.

Yet, this demonstration has a problem because the consistency of the new geometries is demonstrated by appealing to the consistency of the old Euclidean geometry. In other words, non-Euclidean geometries are consistent because the Euclidean geometry is consistent. Unsatisfied with this situation, mathematicians and philosophers continued to look for an absolute demonstration.[17]

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[15] It seems that C. F. Gauss was the real founder of non-Euclidean geometry but did not write any book about it, although he influenced Lobachevsky, through a mutual friend called Bartels, as much as he did Bolyai through his father, a friend of Gauss. See D. E. Smith (1925, pp. 336–337). Saccheri had made some steps in this direction in 1773, but he dropped the project thinking that it would lead him to contradictory theorems.

[16] As Nagel (1961, p. 235) says: "It follows from this that if 'straight line' (in Euclid) and 'straight line' (in Riemann) are *corresponding* terms to which the same interpretation should be given it would be logically impossible to give them an interpretation that would satisfy *both* systems. Evidently, therefore, there is a consistency in Riemann's system, not taking 'straight line$_E$' and 'straight line$_R$' as corresponding terms of the two systems, but looking at *another* Euclidean term (for instance, 'the arc of the maximum circle of a sphere') as the corresponding term of 'straight line'."

[17] The demonstration of the consistency of a geometry based on the consistency of another geometry is a relative demonstration.

This crisis on the foundations of geometry and mathematics gave rise to three epistemological schools: (1) formalist, (2) logicism, and (3) intuitionism.

Hilbert was the founder of the formalist school. He tried to develop "absolute" proofs, for which he differentiated mathematics from metamathematics. He sustained that formal systems developed by mathematicians from given axioms fall into the domain of mathematics, while the theoretical speculations over these systems belong to the field of metamathematics.

With this distinction between mathematics and metamathematics, Hilbert believed that it was possible to exhaustively examine the structural characteristics of a mathematical system and demonstrate that they cannot lead to contradictory theorems.[18] We will see below that Gödel invalidated Hilbert's proposal. For the moment being, we can add that Hilbert's method is only applicable in the case that the alternative systems to study are finite. Otherwise, an exhaustive analysis would be impossible. Furthermore, it is implied that mathematics is reduced to a sort of game in which the mathematician designs the rules and postulates of the system and the metamathematician studies its consistency.

The logicism school was fostered by Frege and continued by Peano, Whitehead, and Russell. According to these thinkers, all mathematical concepts can be defined in terms of strictly logical ideas and all mathematical axioms can be deduced from a small quantity of verifiable and logically true propositions.

At the time, Russell and Whitehead's book, *Principia Mathematica*, seemed to be the ultimate solution to the problem of consistency in mathematical axioms, and in particular in arithmetic. Because the consistency of mathematical axioms is built on the consistency of logical axioms, logicism also seems to be a relative proof. Russell and Whitehead start their book as if it were a treatise on logic and move on with deductions until they get to all of the foundational elements of mathematics. They reach the conclusion that it is impossible to draw a clear line of separation between logic and mathematics; such demarcation would be completely arbitrary.

Now, to be able to reduce mathematical concepts to logical propositions, Russell and Whitehead must take as true three axioms, one of which, called reducibility, allows them to take certain steps that otherwise would not be allowed or would take them to different conclusions.

What significantly weakened logicism is that this axiom was quite difficult to justify. The authors themselves have to admit (in the second edition) in *Principia Mathematica* that

> [o]ne point in regard to which improvement is obviously desirable is the axioms of reducibility […]. This axiom has a purely pragmatic justification: it leads to the desired results, and to no others. But clearly it is not the sort of axiom with which we can rest content. On this subject, however, it cannot be said that a satisfactory solution is as yet obtainable. (Whitehead and Russell 1910, p. xiv)

The other two axioms also led these authors to some setbacks. These issues led the mathematician Weyl to comment that in the *Principia* "mathematics is no longer founded on logic, but on a sort of logician's paradise" (cited by Kleene 1952, p. 45). Also Gödel (1990, p. 120), maybe the most important logician and mathematician of the twentieth century, sustains that "[i]s it to be regretted that this first comprehensive and thorough-going presentation of a mathematical logic and the derivation of mathematics from it so greatly lacking in formal precision in the foundations (contained in *1–*20 of *Principia*) that it presents in this respect a considerable step backwards as compared with Frege."

The intuitionism school was fostered by L. E. J. Brower, whose followers were Kronecker, Poincaré, Bore, Weyl, and Kleene. The intuitionists are closer to a Kantian position; they sustain that mathematics is a certain type of mental construction. The intuitionist mathematician considers that the starting point of mathematics is an intuitive comprehension of natural numbers. These are not logically deduced, as the logicists sustain; "they are immediately constructed in the mind of the mathematician and its objective value or truth is directly based on evidence from intuition," writes Dou (1974, p. 117; TN: Translation is my own). With this school, we get

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[18] The typical example to illustrate this issue is the game of chess. Given the quantity of pieces, slots, and rules of the game, chess is similar to a mathematical formalized system. A meta-chess statement would be, for instance, that given certain position of the pieces on the board and being white's turn to move, this would give check in two moves. This statement is possible because we can exhaustibly analyze all the alternative possibilities. For more details, see Nagel and Newman (1958).

back to the position discussed at the beginning: the foundations of geometry and mathematics are, in general, self-evident.

Summarizing what has been said so far, we have that for the formalists, mathematics studies the logical implications of any set of axioms; for the logicians, mathematics is a branch of logic; and for the intuitionists, mathematics is the study of the logical implications of the intuitive concept of numbers, which seems to recover the definition of mathematics as the study of quantities.

In 1931, the Austrian logician Kurt Gödel published a famous article, probably the most important in the history of logic. This article shows the impossibility of Hilbert and Russell-Whitehead's programs. In the words of Gödel (1986, p. 195): "[...] it can be proved rigorously that in *every* consistent formal system that contains a certain amount of finitary number theory there exist undecidable arithmetic propositions and that, moreover, the consistency of any such system cannot be proved in the system." In other words, the problem of proving the consistency of axioms that contradict (and do not contradict) the senses raised by non-Euclidean geometries cannot be solved inside the system itself. To appeal to another formal (mathematical) system only takes the problem one step back.

According to Hayek (1967, p. 62):[19]

> It would thus appear that Gödel's theorem is but a special case of a more general principle applying to all conscious and particularly all rational processes, namely the principle that among their determinants there must always be some rules which cannot be stated or even be conscious. At least all we can talk about and probably all we can consciously think about presupposes the existence of a framework which determines its meaning, i.e., a system of rules which operate us but which we can neither state nor form an image of and which we can merely evoke in others in so far as they already possess them.

What is important for us is that mathematical economic models are, in some way, inserted in the general problem of mathematical consistency. In part I, we saw that it is impossible to empirically test economic theories because the facts of the social sciences are singular and complex phenomena—that is, nonrepeatable. Even some advocates of mathematical economics pointed out that the purpose of their models was just analytical and that they did not intend to find the parameters of their equations.

But, if mathematical economic models cannot be empirically tested, or the purpose is not to find the real parameters, then this model seems to be located more in the field of pure than applied mathematics. The only thing being studied is the logical implications of the model's premises regardless of their realism. Now, given that mathematical economic models are characterized by their lack of realism in their premises, they seem to be in the same situation as non-Euclidean geometry; there is no guarantee of the internal consistency of the model. And, according to Gödel's theorem, we could conclude that said consistency cannot be demonstrated mathematically, let alone by an economic model. It seems, then, that the paradox of mathematical economics' rigor is that it cannot be certain of its own internal consistency.[20]

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[19] It seems that Friedman (1953, chap. 1) arrived at a similar conclusion.

[20] Cohen (1931, chap. 112) arrived at the same conclusion: "While strict mathematical deduction from explicit hypothesis helps us to eliminate contradictions and facilitates the process of discovering new truths, it offers us no conclusive proof that our assumptions are free from all implicit contradictions. If the meaning of our assumptions is to be found in all their possible deductions, the full recognition of which is an interminable task, what proof have we that the future will not reveal any implicit contradiction between some of our assumptions?"

It should be pointed out that Cohen's statement that mathematics "facilitates the process of discovering new truths" is limited to the natural sciences. In another section of his book, he states: "In general, social situations are networks in which one cannot change one factor without affecting a great many others. It is, therefore, difficult to determine the specific effect of any one factor. Moreover, social elements seldom admit of simple additions. The behavior of some individuals in a large group will not in general be the same as their behavior in a smaller group. This makes it difficult to apply the mathematical methods which have proved so fruitful in the natural sciences. For those mathematical methods depend upon our ability to pass from a small number of instances to an indefinitely large number by the process of summation or integration."

7. Conclusion

Undoubtedly, Koopmans (1954, p. 378) is right when he says:

> [...] the justification for mathematical economics depends merely on whether the logical links between the basic premises economists have been led to make and many of their observable or otherwise interesting implications are more efficiently established by mathematical or by verbal reasoning.

The problem is that Koopmans seems to have misunderstood what the object of study of economics is; he then adds this (p. 378, italics are mine):

> I cannot help adding that only recently the non-contradictory character of the premises of the theory of competitive economic equilibrium was fully established, and that this was done through a sequence of studies using the tool of topology which had hitherto not been used in economics. *Is there any topic more basic in contemporary economic theory?* Is there any tool less familiar to literary and mathematical economists alike?

In the first place, it is worthwhile to remember that what was demonstrated is the existence and uniqueness of equilibrium, but not the "noncontradictory" character of the premises. As we previously saw, Arrow and Hahn have recognized "logical difficulties" in the theory of perfect competition. On the other hand, said demonstration was done with so many unrealistic assumptions for the sake of mathematical *convenience* (as Baumol would say, or, in other words, to *get* the desired result) that the theory does not go beyond a good mental exercise with no capacity to explain the real functioning of the market.

In the second place, and more importantly, there are topics that are more basic. The object of study of economics is the market "process" and not market "equilibrium"; the latter takes a secondary place. It is worth reproducing, despite its length, the following reflections from Mises (1949, pp. 355–356, italics are mine):

> Both the logical and the mathematical economists assert that human action ultimately aims at the establishment of such a state of equilibrium and would reach it if all further changes in data were to cease. But the logical economist knows much more than that. He shows how the activities of enterprising men, the promoters and speculators, eager to profit from discrepancies in the price structure, tend toward eradicating such discrepancies and thereby also toward blotting out the sources of entrepreneurial profit and loss. He shows how this process would finally result in the establishment of the evenly rotating economy. *This is the task of economic theory. The mathematical description of various states of equilibrium is mere play. The problem is the analysis of the market process.* [...]
> The problems of process analysis, i.e., *the only economic problems that matter*, defy any mathematical approach. The introduction of time parameters into the equations is no solution. It does not even indicate the essential shortcomings of the mathematical method. The statements that every change involves time and that change is always in the temporal sequence are merely a way of expressing the fact that as far as there is rigidity and unchangeability there is no time. The main deficiency of mathematical economics is not the fact that it ignores temporal sequence, but that it ignores the operation of the market process.

As we can see, Mises's citation contrasts with Koopman's (which can be considered representative of mathematical economists). What for Koopmans is "basic" (general equilibrium), for Mises is "mere play."

When we study the market process, we are also analyzing human action and its logical implications and this is not translatable into mathematical equations; rather, it has to *necessarily be* "explained" in words (symbols with meaning). As we saw in part I, human action is not *determined* by any external factor. Obviously, external factors *influence* the behavior of men, but not in a mechanical or deterministic way. Therefore, human action is not a mathematical function of anything, and neither are the logical implications of said actions.

When Baumol (1966, p. 92) says that the "kindergarten clichés that 'Economics is not an exact science' or that 'You can't put human nature into an equation' need hardly be considered," he is minimizing what is

precisely the crux of the problem because we "can't put human nature into an equation" and he is trying to do so incorrectly from a scientific point of view. For instance, explaining the behavior of the consumer with indifference curves is, and will be, one of the most absurd theories produced by mathematical economics.

The development and dissemination of mathematical economics has created the myth that verbal exposition is ideological quackery while the mathematical approach is "scientific." Even if an educated one, this is no more than a superstition. The success of mathematical economics in the natural sciences does not imply that the same method should be applied in social sciences to reach the rank of science. On the contrary, the use of mathematical formulations in economics has slowed down its progress. On this subject, Hayek (1952, pp. 15–16) has suggested the terms "scientism" and "scientistic" to describe the imitation of the natural sciences method by social "scientists":

> […] in the sense in which we shall use these terms, they describe, of course, an attitude which is decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed. The scientistic as distinguished from the scientific view is not an unprejudiced but a very prejudiced approach which, before it has considered its subject, claims to know what is the most appropriate way of investigating it.

This deformation in the profession is very influenced by the education that universities give to economics students, and its outcomes are not only epistemological: they also affect economic policy. From their first steps, economics students are introduced to mathematical economics, and from there, they are subjected to an intense reading of papers and books that present different mathematical models according the current fashion and latest "advances." By the time he finishes his studies, he has become a "model" of a central planner economist. Those who get to a relevant public position will try to apply some of the unrealistic mathematical models to reality (the more bold ones will create their own models). Then, there begins a manipulation of interest rates, exchange rates, tariffs, reserve requirements, prices, wages, etc., according to what their models predict will occur (with some standard deviation). The degree of regulation will vary from those who believe all variables need to be controlled to those who believe only one variable should be controlled (e.g., money supply given a "rule" recommended by the model.) In addition, inside the same model, the degree of regulation can change the "type" of intervention; some may think variables A, B, and C should be controlled while others think variables W, Y, and Z should be controlled. The combination of all degrees of regulation and types of intervention results in a large number of "experiments" to put in practice.

When each one of these experiments comes to its inevitable failure, technocrats look for a solution in the wrong place. Instead of considering that the error lies in the act itself of regulating economic variables, they think that what needs to be done is to perfect the model and diligently work on inventing a new chimera. The education received at the university prevents them from thinking of another alternative. Mathematical economics moved them away from the problem that any economist should know: how markets work in reality. There are only a few economists who stop a minute to think what it is that they are doing; the use of "mathematics" is in fact not open to questioning. However, like any error, it is condemned to disappear in due time.

8. References

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

[2]   Baumol, W. J. (1966). Economic Models and Mathematics. In S. R. Krupp (ed.), *The Structure of Economic Science: Essays on Methodology*. Englewood Cliffs, NJ: Prentice Hall.

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

[4]   Copi, I. M. (1961). *Introduction to Logic*. Macmillan.

[5]   Crossley, J. N., Ash, C. J., Brickhill, C. J., Stillwell, J. C., & Williams, N. H. (1972). *What Is Mathematical Logic*? New York: Dover.

[6]   Dorfman, R. (1954). A Catechism: Mathematics in Social Science. *The Review of Economics and Statistics*, *36*(4), 374. https://doi.org/10.2307/1925892.

[7]   Dorfman, R., Samuelson, P. A., & Solow, R. (1958). *Linear Programming and Economic Analysis*. New York: Dover.

[8]   Dou, A. (1974). *Fundamentos de la Matematica*. Editorial Labor S. A.

[9]   Duesenberry, J. S. (1954). The Methodological Basis of Economic Theory. *The Review of Economics and Statistics*, *36*(4), 361. https://doi.org/10.2307/1925887.

[10]   Eves, H., & Newson, C. V. (1958). *An Introduction to the Foundations and Fundamental Concepts of Mathematics*. New York: Holt, Rinehart and Winston.

[11]   Fetter, F. A. (1977). *Capital, Interest and Rent*. Sheed Andrews and McMeel.

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

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

[14]   Gödel, K. (1986). *Collected Works: Volume I*. Oxford University Press.

[15]   Gödel, K. (1990). *Collected Works: Volume II*. Oxford University Press.

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

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

[18]   Hazlitt, H. (1959). *The Failure of the "New Economics."* D. Van Nostrand.

[19]   Keynes, J. M. (1936). *General Theory of Employment, Interest and Money* (1964 ed.). San Diego, New York, London: Harcourt.

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

[21]   Kleene, S. C. (1952). *Introduction to Mathematics* (1974 ed.). Wolters-Noordhoff.

[22]   Klein, L. R. (1954). The Contributions of Mathematics in Economics. *The Review of Economics and Statistics*, *36*(4), 359. https://doi.org/10.2307/1925886.

[23]   Kneebone, G. T. (1963). *Mathematical Logic and the Foundations of Mathematics*. D. Van Nostrand.

[24]   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.

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

[26]   Menger, K. (1973). Austrian Marginalism and Mathematical Economics. In J. Hicks & W. Weber (Eds.), *Carl Menger and the Austrian School of Economics*. Oxford: Clarendon Press.

[27]   Mises, L. von. (1922). *Socialism*. (J. Kahane, Trans.) (1962 ed.). New Haven: Yale University Press.

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

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

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

[31]   Nagel, E., & Newman, J. R. (1958). *Gödel's Theorem*. New York University Press.

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

[33]   Peirce, B. (1881). Linear Associative Algebra. *American Journal of Mathematics*, *4*(1/4), 97. https://doi.org/10.2307/2369153.

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

[35]   Rouse Ball, W. W. (1908). *A Short Account of the History of Mathematics* (1960 ed.). New York: Dover.

[36]   Russell, B. (1903). *The Principles of Mathematics* (2nd ed.). New York: W. W. Norton.

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

[38]   Smith, A. (1983). *Lectures on Rhetorics and Belles Letters*. (J. C. Bryce, Ed.) (1985 ed.). Indianapolis: Liberty Classics.

[39]   Smith, D. E. (1925). *History of Mathematics: Volume II*. New York: Dover.

[40]   Solow, R. (1954). The Survival of Mathematical Economics. *The Review of Economics and Statistics*, *36*(4), 372. https://doi.org/10.2307/1925891.

[41]   Stigler, G. J. (1949). *The Theory of Price*. New York: Macmillan.

[42]   Stigler, G. J. (1950). *Five Lectures on Economic Problems*. Macmillan.

[43]   Tinbergen, J. (1954). The Functions of Mathematical Treatment. *The Review of Economics and Statistics*, *36*(4), 365. https://doi.org/10.2307/1925889.

[44]   Whitehead, A. N. (1898). *A Treatise on Universal Algebra*. Cambridge: Cambridge University Press.

[45]   Whitehead, A. N. (1911). *An Introduction to Mathematics*. London: Williams and Norgate.

[46]   Whitehead, A. N., & Russell, B. (1910). *Principia Mathematica: Volume I* (1927 ed.). Cambridge University Press.

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

[ii] TN: There is no citation for Debreu's passage in the original Spanish version.

[iii] TN: I could not find the exact passage in the English version of Stigler's book. The difference may be due to translation or a revision in a different edition. I use the closest passage to the one provided in the Spanish version of the article. Even though the wording is a little different, the message is the same.

[iv] TN: I could not find the exact location of Stigler's references on this point.

[v] TN: I could not find the exact location of this passage in Stigler's book.

[vi] TN: I could not find the exact location of this passage in Stigler's book.