

Epistemology In Neuro - Management

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“The human being makes decisions in a context of limited rationality, subject to biases and noises that lead him to behave sub optimally, from the point of view of what Neoclassical Economics prescribes. Behavioral Economics has been showing this phenomenon for decades, with the nominees Simon, Kahneman and Thaler as main banners. However, in recent years, the disruptive confluence of Cognitive Neuroscience, Psychology and Economics, has built a hybrid field called Neuroeconomics, which with methods different from the traditional is building, at accelerated pace, a unified theory on human decision making. Throughout this work, we illustrate the main advances of this novel field called Neuroeconomics, as well as the enormous epistemological possibilities of this new approach, giving rise to the debate on possible changes in the dominant research program.”

..... Sebastian

INTRODUCTION

At this stage, it is already quite clear what neuromanagement is, what neuropsychological techniques it uses to do its research, its latest findings on factual human decision-making rationality (not that of neoclassical management), and conclusions of concrete studies being made in this embryonic field. But researchers still need to delve into a matter of extreme importance: how far will these new findings impact the way management theory is done? Or to put it in epistemological terms, will there be a paradigm shift in managerial science? And if the answer is affirmative, will it be a change of paradigm to Kuhn (profound changes) or to Lakatos (the hard core of science almost does not change)? And if there is any change in the current paradigm, will Friedman's ‘irrelevance of assumptions’ paper continues to be important, or will this scientific jugglery lose meaning, in the words of Paul

Samuelson? In short, many important questions still to be answered, on which we are going to try to cast a mantle of light in this paper.

But first let's start by defining what a paradigm is. The term paradigm was defined by Thomas Kuhn -it is fundamental within his argument-, as ‘universally recognized scientific relationships that, for a certain time, provide models and solutions to a scientific community’. An alternative definition, from Kuhn himself, indicates as scientific paradigm ‘the complete constellation of beliefs, values, techniques, etc., shared by the members of a given community’. That is, within Kuhn's argument, scientists belonging to a certain school of thought (for example the ‘new classics’ in management) are ascribed to a certain paradigm (we might call it the orthodox in macro management), which fulfills the function to frame or standardize the methodology used in their research, assumptions from which their models start, etc., since it allows new researchers to adapt to past scientific achievements, achievements that some particular scientific community has been recognizing, for some time, as a foundation for their practices. And continuing with the contribution of the aforementioned philosopher, when the number or magnitude of the anomalies are excessive and the restlessness arises among the researchers, a scientific revolution or change to a paradigm superior to the preceding one takes place. And while Kuhn's ideas have been criticized - Lakatos, another relatively contemporary philosopher of science - is more accepted, his concept of paradigm is generally accepted and in this paper we will use it as a basis.

Science has its own deities. Aristotle, Galileo, Newton, or Darwin are the rock stars of the scholarly world and rightly so. They inspired the rest of us to have an inquisitive mind and they challenged conventional wisdom at critical junctures for humanity i.e., when this was needed

the most. For this reason, we, mere mortals who avail of their legacy and try to publish, remain eternally indebted to their genius. However, what would those early giants have to say if they realized that the gist of their own scholarly work is the new conventional wisdom? That the epistemological scaffolding that their work has spawned acts as an unproductive bedrock against a more nuanced understanding of the new world we inhabit?

.... Konstantinos Poulis

In what follows, and before turning fully to deal with the effects that neuromanagement could cause in the current paradigm in economic science, we will analyze the way in which that current paradigm was built, where we will first explain general concepts about epistemology and then, in a more detailed way, of epistemology applied to economic science. In this paper we will mainly follow, among several consulted, the wonderful work of Mark Blaug, perhaps the highest authority in epistemology of management in recent decades.

Notion

The birth of Neuroeconomics has created one of those rare historical moments in which economists stop to reflect on the fundamental questions of our science: the interdisciplinarity of the approaches to the economic problem, the frontiers of the economy, its objectives, questions of validity or refutation of theories, among others. The debate is hot today, and is on the agenda of many important economic congresses around the world, and the waters are partly divided, although generally more inclined in favor of this novel research program. Although it is not intended, at all, an exhaustive treatment of a topic of great complexity such as the epistemological, in order to justify the dominant methodological position today among economists (which is key to understand the current paradigm) will be useful an introductory reference to the subject, which will be stopped at those most relevant milestones of the last two Centuries, a period during which management has taken a separate entity as a science. To give a simple definition, epistemology (from the greek, ἐπιστήμη or episteme, 'knowledge'; λόγος or logos, 'theory') is the study of the production and validation of scientific knowledge. It deals with problems such as historical, psychological and sociological circumstances that lead to its obtaining, and the criteria by which it is justified or invalidated.

Empiricism

The selfish and utilitarian side of human beings, that of Adam Smith's famous "invisible hand," is the basis of traditional economic theory.

But this is a partial approach to the complex human dimension, being necessary to incorporate the emotional side to the economic models, to include the passions that often cloud reason, the empathy and trust generation, the collaborative and cooperative spirit, the psychological biases that make markets fall into bubbles, overreactions, panics, along with our powerful "unconscious rationality", which dominates much of daily decisions

It is a philosophical current that exerted a considerable influence on scientists, whose name comes from the Greek *Empireia*, which means experience. According to the empiricists, human knowledge begins in sensory experience, and what is more, for many of them sensory knowledge is the only valid type of knowledge. This movement flourished in England during the seventeenth and eighteenth centuries, in some way as a form of opposition to continental-inspired rationalism. Francis Bacon is a prominent forerunner of empiricism, especially in his defense of the procedure of observation and induction as a valid method for science; Bacon himself gave much more relevance to practical knowledge than to speculative nature. Hobbes, Locke, Berkeley and Hume are the main representatives of British empiricism. Especially the contribution of Locke stands out, which denies the existence of innate ideas, in clear opposition to the Cartesian philosophy. David Hume argues that causality can be interpreted as the mere temporal succession of two phenomena, while John Stuart Mill goes so far as to ground more abstract knowledge, such as mathematics or logic, in sensory experience and in induction. Due to the influence of the aforementioned authors, the most common vision of scientific research in the mid-nineteenth Century is to defend that science must start from the observation of facts, carried out in a free and without prejudice. Then the inductive inference is applied, so that it goes from the particular to the general and universal laws are formulated about these facts. That way, induction is again applied in order to obtain theories or arguments endowed with a greater degree of generality. Finally, it is contrasted whether the laws and theories are true or not by comparing their empirical implications with the observed facts.

In short, it is clear that the empiricists were eminently intuitivist, and as we shall see shortly, quite influential among some classical economists such as Adam Smith; however, their methodology little by little was losing sustenance as a way of doing science, especially thanks (or because of) the emergence of the hypothetical -

deductive method, much more abstract to do science, and therefore, much more powerful in its scope. This last method is with which management took the form that even today maintains; it is the method of mathematical modeling, of the assumptions far from reality, of the hypotheses, that although they are not entirely accurate, they are always logically well deduced. Below we detail this method a little more.

Hypothetical - Deductive Method

“First, we must determine the nature of knowledge; that is, what does it mean to say that someone knows, or fails to know, something? This is a matter of understanding what knowledge is, and how to distinguish between cases in which someone knows something and cases in which someone does not know something. While there is some general agreement about some aspects of this issue, we shall see that this question is much more difficult than one might imagine. Second, we must determine the extent of human knowledge; that is, how much do we, or can we, know? How can we use our reason, our senses, the testimony of others, and other resources to acquire knowledge? Are there limits to what we can know? For instance, are some things unknowable? Is it possible that we do not know nearly as much as we think we do? Should we have a legitimate worry about skepticism, the view that we do not or cannot know anything at all?”

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Following the philosophy level, in the 2nd half of the 19th Century the empiricist intuitivism began to decay to give rise to deductivism, under the influence of authors such as Mach, Poincaré and Duhem, and more late (early twentieth Century) due to the growing strength of the logical positivism of the Vienna circle, the latter born around the figure of physicist and philosopher Moritz Schlick, who progressively gave rise to a new philosophy, logical positivism. Among its most prominent members are Carnap, Feigl and Karl Menger (son of the well-known economist). And while they disintegrated as a group in the late 1930s, logical positivism exerted considerable influence on twentieth-Century philosophy of science. Some characteristics of the Vienna circle and its thinking were:

- Received the intellectual heritage of Comte's positivism (opposition to metaphysics, faith in reason, methodological monism and debugging of normative considerations of positive science);

- with certain influences of English empiricism (just explained) and relativism;
- Advocated that the intuitivist methodology described above (English empiricism) be replaced by a procedure based on two principles: the hypothetic-deductive method and verification.

At that time (late nineteenth Century and early twentieth Century), the current epistemological paradigm required a science, to consider it 'serious', the following:

- Use the hypothetic-deductive method: the logical structure of the scientific explanation should be the following: first, the formulation of a universal law and relevant initial conditions, which were the premise or explanans. The starting point of this universal law is not induction but certain conjectures (as for example that the human being is eminently optimizing). Then the explanandum was deduced by deductive logic. The universal law could consist of a proposition similar to the following: 'when a happens, b happens'. In turn, it could be deterministic or statistical, in which case the universal law would be qualified in the following way: 'when a happens, b happens with probability p'.
- Use the principle of verification: to understand it, it is necessary to make a classification of the judgments in analytical or synthetic. Analytical judgments are those that contain a truth in the definition of their own terms ('a triangle has three sides') while synthetic judgments provide a truth thanks to practical experience ('swans are white'). However, the synthetic judgments, according to logical positivism, had meaning if and only if they were susceptible of empirical verification, and this principle was used to eliminate from the sciences those statements that could be qualified as metaphysical, such as 'the paintings of Velázquez are beautiful.'

In short, experimental science was conceived as the only valid mode of access to reality. Surely the reader, at this point of the story must already have realized -especially if he is an economist- the enormous influence that this epistemological school has had on economic science, especially because it was decisive at the time when management was written by the neoclassicals (Jevons, Walras, Pareto, Later Marshall, etc.), creators of the theoretical trunk that even today is almost intact in our science. In fact, it is at this time when management begin to use profusely the mathematical tools (derived, integral, differential equations, etc.) To formulate the

hypotheses and hence apply the deductions (hence the hypothetical deductive method), that although they forced ‘a little’ the real human psychology - there were no developments in neuroscience that there is now, it must be recognized - they gave scientific rigor and predictive power to our nascent science. Later in this paper we return to the subject.

Lógica Deductivism

“Having narrowed our focus to propositional knowledge, we must ask ourselves what, exactly, constitutes knowledge. What does it mean for someone to know something? What is the difference between someone who knows something and someone else who does not know it, or between something one knows and something one does not know? Since the scope of knowledge is so broad, we need a general characterization of knowledge, one which is applicable to any kind of proposition whatsoever. Epistemologists have usually undertaken this task by seeking a correct and complete analysis of the concept of knowledge, in other words a set of individually necessary and jointly sufficient conditions which determine whether someone knows something. “

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Both empiricism (intuitivism) and logical positivism (deductivism) had an impact on the way of doing economic theory (the method), the intuitivism more in some authors of the classical school (like Adam Smith), while deductivism (called verifications) more among the neoclassical, that is, the economic theory that has survived to this day.

The classical economists, from the end of the s. 18th and early 19th Century, they did not discuss the methodological issues in great detail, but it can be said that, in general, some advocated the use of the inductive method in management, which was providing so many successes in the natural sciences, but others became quite side of the hypothetical deductive method. The work of Adam Smith (perhaps the most influential economist of all times), integrates diverse influences and, for that reason, its methodology is a complex mixture of many factors, but in the last analysis, it can be described as inductive.

In general, most of smith's work is an example of the use of the methodology of the so-called Scottish historical school. According to Blaug, it is not easy to characterize the methodology of this school, because neither Adam Smith nor any of its other members used many words to define it: ‘in any case, such a method seems to consist, on the one hand, of a firm belief in the historical stages, based on the relationship between defined modes or types of economic

production and certain principles of human nature; and on the other hand, on a deep commitment to simplicity and elegance as absolute priority criteria of an adequate explanation, both in the field of physical and social sciences. Both his theory of moral sentiments and the wealth of nations can be considered deliberate attempts by smith to apply the Newtonian method (highly successful in physics, until the appearance of quantum physics), first to ethics and then to management.

David Ricardo, in a certain contrast to smith, used deduction and abstract modeling to a greater extent in his works. Another outstanding figure among the classics is Nassau Senior, who in 1827 publishes introductory lecture on political economy and in 1836 outline of the science of political economy. In his writings, senior establishes a distinction between management as a science and as art, the first what is now considered a positive management while the second what we now call normative management. Senior is credited with the argument that management rests on very general propositions (the desire to maximize wealth with the least possible effort, the Malthusian principle that the population grows faster than the means of subsistence, and the existence of diminishing returns in agriculture), which are the result of observation and from which certain conclusions are obtained, always according to senior. That is, somehow, from senior, among others, the deductive method is gaining strength among economists, from assumptions not necessarily entirely true, such as maximizing rationality.

John Stuart Mill, in his famous principles of Political Economy (1848), takes these ideas and gives them their own added value. In the first place, ‘they are seized’ against methodological monism, recommending that management employ the deductive method, since the inductive method would be harmed by the concomitance of several causes that affect the same phenomenon. In particular, according to J.S.Mill, the economist must begin his research from psychological premises, which are reached by introspection; next, it is necessary to elaborate a theory from the premises, and finally the theories must be contrasted by empirical procedures. That is, with mill continues to take strength in management that useful technique of modeling forcing assumptions about our psychology, from introspection, obviously because there was no way to ‘get inside the black box of the human mind’, as now you can, thanks to Neuroimaging and Transcranial Magnetic Stimulation, among other neuro techniques. Besides, the optimizer was a rationality very easy

to mathematize, and in addition, the postulates of the psychologists of that time were not scientifically strong enough to impose themselves 'per se'.

Stuart Mill highlights his concept of homo economicus, although it was already exhaustively dealt with in paper 2, where we analyze the evolution of the concept of rationality in management. For Mill, although there is a part of human behavior where obtaining wealth is not the main objective, there are other departments of human affairs where the acquisition of wealth is the main purpose: management deals with this second category, of way that abstracts from all human passions and motives except the desire for wealth and the aversion to work. That is, Mill knows for certain that the man thus described is a fictitious man, in fact he is aware that the economic sphere is only a part of human behavior; nevertheless, he recommends that management proceed to abstract and work with that fictitious man, who seeks to obtain 'the greatest possible amount of wealth with the minimum possible work and self-denial.'

John Elliot Cairnes, another classical economist, in consonance with J.S. Mill, emphasizes that political management is a hypothetic-deductive science, and that the use of a methodology of these characteristics by a science indicates its maturity. In fact, Cairnes affirms that management must be based on real premises, undoubted facts about human nature and the world (affirmation with which we are in agreement), but that are obtained not by induction but by introspection (there no longer we agree, but of course, at that time there was no neuromanagement) as, for example, adds Cairnes, the desire to obtain wealth with the minimum sacrifice, or the Malthusian principle on the population. In short, quite a coincidence between the arguments of Cairnes and Senior; but we repeat, introspection as a scientific mechanism in social sciences is not the best tool, and in management introspection has been used in excess, even far from that which could be justified, based on an ad-hoc psychology, built by economists, not psychologists.

And to make his thinking even clearer, Cairnes does not consider verification as a test of hypotheses, in order to find out if they are true or false, but rather as a method to establish the frontiers of application of the theories, since verification helps to corroborate deductive reasoning. For example, for Cairnes, if a certain theory has been correctly deduced, it will be true. If discrepancies between the facts and the theories are observed, however, it can be attributed to

disturbing causes that obscure the theory and show that it has been applied incorrectly, but the theory itself will be true if it has been rigorously obtained through the deductive process. These ideas, which had already been pointed out by Stuart Mill, and expressed more forcefully in Cairnes, contradict what came later in the field of economic epistemology, the Friedman paper: 'if a theory, even if it has been correctly deduced, does not predict according to the facts, loses scientific support'. In other words, in light of what is accepted today mostly in epistemology of management - Friedman Paper - the ideas of Mill and Cairnes illustrate the degree of fundamentalism that existed among the first theorists of management, who went so far as to suppose that divergences between theory and practice were due to 'disturbing causes that obscure the theory' and not to the theory being wrong, on top of theories based on dubious premises, emanating from introspection (that is, the occurrence of some) as the principle of rationality, or emanated from observation, as for example the Law of Malthus, which fortunately has never been verified true.

It is interesting to note that the last three authors mentioned (Senior, Stuart Mill and Cairnes) agree that the search for maximum wealth with the least possible effort is one of the driving principles of man, which is not necessarily consistent with the modern neuroscience findings. But obviously, the coincidence between the three economists is not accidental, but responds to the influence in the England of s. XIX exercised utilitarianism as a philosophical current, which provided key concepts to finish closing the neoclassical paradigm, still current in our science. In particular, English School of Utilitarianism, headed by Bentham, proposes the hedonistic idea that happiness for man is found in well-being, understood as the difference between pleasure and pain. This approach allows us to obtain the relationship between individual values and social values, since utilitarianism postulates, in terms of social aggregates, the principle of maximum happiness for the greatest possible number of people. The next step was to qualify as useful everything that confers welfare to the human being. And from these simple postulates, utilitarianism exerted a strong influence on the economic theory elaborated later, facilitating that the assumption of rationality, understood as maximizing pleasure and minimizing pain, was introduced gradually into management until being described in detail by Stuart Mill (classic) in his characterization of homo economicus, as was recently said. Later, it would reach a more formalized approach thanks to the

theory of the marginal utility, of the hand of Jevons, Edgeworth, Sidgwick, Wicksteed and Marshall, which is well-known in management history.

At this point in the historical narrative, we will highlight - once again in this book, based on what neurosciences today, through neuroimaging, 'transcranial magnetic stimulation' and other scientifically rigorous techniques, demonstrate that Bentham, Mill, Cairnes, Senior, etc. We're not totally true, clearly far from reality. The human being maximizes at times, only sporadically, and does so more in the sense of Simon (limited rationality - already discussed in previous papers) than in the sense of the utilitarians and the neoclassicals. Even some experts, like Argentinean Nestor Braidot, often talk about man as 'unconsciously rational', the metaconscious may act more strongly than the conscious when defining an economic decision. We repeat, then, some force ideas of neurosciences applied to the decision making:

- 'According to scientists, the brain areas of rationality cannot function isolated from the areas of biological-emotional regulation. The two systems communicate and affect the behavior jointly, and consequently, the behavior of the people'.
- 'Moreover, the emotional system (the oldest area of the brain) is the first force that acts on mental processes, therefore determining the direction of decisions.'
- 'The latest advances in neuroscience have shown that consumer decision-making is not a rational process. That is, customers do not consciously examine the attributes of a product or service to acquire it.'
- 'In most cases, the selection process is relatively automatic and derives from habits and other metaconscious forces, among which history, personality, neurophysiological characteristics and the physical and social context that surrounds us all gravitate'.
- 'The fragrance of a perfume, for example, can evoke different sensations. If the client associates it with painful experiences or with a person with whom he does not sympathize, it is very likely that he will not buy it, even when the price-quality-brand ratio is reasonable'.

It is interesting to highlight the criticism of utilitarianism by the brilliant historian of economic analysis J. Schumpeter: 'the psychology really used [...] Was always individual psychology, introspective, and the most primitive type, rarely endowed - if it was ever - of more than a few

simple hypotheses about the reactions of the individual psyche. This procedure was called empirical [...]. There was nothing 'experimental' or inductive, and in reality it was not very realistic, despite the programmatic statements, the war cries and the invocations of Francis Bacon. 'this criticism of Schumpeter does nothing but enhance all that we have been holding in this book, especially our 'bold suggestion' that it is already untenable to continue in management with such rudimentary psychological assumptions, especially after all that has been contributed by neuromanagement.

It is interesting to analyze the contribution of this English author (father of the famous J M Keynes, although with different thoughts), whose main work is the scope and method of political economy (1891), where he summarizes the previous tradition - fundamentally represented by Senior, Mill and Cairnes - in the following points:

- A distinction can be made between positive and normative management, and it is convenient that this difference appears clear to economists, since the attempt to merge research into what is and what should be is likely to prevent a clear and unbiased response to the two questions.
- The correct methodological procedure of management consists of starting from some fundamental facts about human nature. Neville Keynes argues that the point of departure for theories must be fundamentally observation, but he asserts that introspection (as Stuart Mill and Cairnes pointed out) can be useful in this sense, since he considers introspection as a source of obtaining ideas that, in his opinion, can be described as empirical.
- With respect to the concept of homo economicus, Neville Keynes affirms, in an extremely utilitarian way, that the economic behavior that seeks self-interest dominates in reality the motives of altruism and benevolence, that is, for this author the economist works knowing that man is selfish, unlike others who held that one had to work as if man were selfish.
- The appropriate method for management must end with the empirical observation relative to the fulfillment of the theory. However, the contrasts of the theories allow to determine their limits of application but not invalidate them: if a test, apparently, contradicts a theory, the researcher must be aware that this result only shows that the test of the theory has been applied incorrectly.

We have made this detail of the ideas of JN Keynes because we believe they synthesize almost perfectly the methodological position that prevailed among most of the economists in the 19th Century, that is to say, a good part of the classic and almost all of the neoclassicals, who we remember are the founders of the microeconomic theory, even today almost without fissures. This position is further consolidated with Lionel Robbins, who is usually considered the last verifications, that is, the last influential economist who wrote before Popper revolutionized epistemology with his concept of 'falsifications', and that gave rise to the epistemological applications of Milton Friedman to management, without doubts the most influential economist in the matter of economic methodology during Century.

Among the most relevant contributions in terms of economic methodology, we find the work of Lionel Robbins, influential English Economist, who in 1932 published essay on the nature and significance of economic science. In it, the aforementioned author strongly criticizes both inductivism and methodological monism (the same method for both natural and social sciences), showing himself more in favor of the ideas of senior and Cairnes, that is, in favor of employing a deductive procedure in management and dualism in the methodology used by the natural and social sciences. For Robbins, the inductive procedure is not successful in management because there is nothing to indicate that history will be repeated, and thus, historical induction, without the help of analytical judgment, is not a good form of economic methodology.

By criticizing induction so severely in economic theory, it gave way to the justification of the hypothetic-deductive method; for him, the propositions of economic theory are deductions from a series of postulates; and the main postulates are assumptions about simple and indisputable facts of experience in relation to the way in which the scarcity of goods, the object of our science, appears in the world of reality. And from these arguments, he insists again with the highly criticizable foundation of Senior, Cairnes, J.M.Keynes, etc., on the fact that the validity of a theory proceeds, therefore, from the logical derivation of the premises from which it departs. In other words, for Robbins, it does not matter too much to start from correct premises or not, arising from the introspection of some 'enlightened ones' or in a more scientific way; while the model is logically well derived, the procedure is valid.

Robbins criticizes the methodological monism posited by the Vienna Circle, arguing that management is a branch of knowledge where the uniformity that exists in the natural sciences does not occur, since:

- The subjectivity of the individual has an important role, and
- The complexity of reality prevents the initial conditions remain unchanged in different situations; and this lack of uniformity reduces the effectiveness of purely empirical procedures.

Regarding the interrelation between management and psychology -one of the most relevant aspects for this work on neuromanagement-, this influential English author argues that our science must use the rationality of the utilitarian homo economicus as a premise from which to build models and to make the deductions, in a context where a few economists argued that behaviorism could provide too a valid starting point for management in terms of the assumption of rationality. The behaviorists denied the role of introspection - used profusely by neoclassical economists - since, according to them, psychology had to deal only with the external behavior of man; eliminating pernicious metaphysical concepts in the explanation of human behavior. Robbins argues that in management concepts that are not observable, such as those of indifference, preference, choice or expectations, adopting behavioral psychology as the foundation of economic theory would leave the latter incomplete. At this point in our book it is almost redundant to say that, unfortunately for Robbins, today with the help of neuromanagement, variables such as indifference, preference, choice or expectations can be measured with enough degree of success, which for economic theory constitutes a true revolution, not yet assimilated by many, by the recent issue.

And although Robbins says that management cannot be totally separated from psychology, since the economist must start from psychological concepts to elaborate theories; defends - following the tradition of Senior, Cairnes, etc. - that economic theory maintains a certain independence and autonomy from the psychological principles that ensure the validity of the conclusions of the first, even in cases in which the psychology on which it relies is wrong. In fact, Robbins, exemplifies this point with the case of the theory of value, which was constructed - by Jevons, Edgeworth and Gossen, fundamentally - on the basis of hedonistic principles, which were not however vital for the theory since other economists, like Menger, they could reach the same

conclusions based on different assumptions. 'the hedonistic borders of the work of Jevons and his followers were incidental to the main structure of a theory which - as its parallel development in Vienna showed - can be presented and defended in non-hedonistic terms at all. That is, Robbins uses an argument very similar to the famous paper of the irrelevance of assumptions, which Friedman develops more deeply and forcefully years later, where it is stated that starting from unrealistic assumptions does not take away legitimacy to the conclusions of the economic models.

Until the appearance of the philosopher Karl Popper, both in economic science and in many others, the hypothetical deductive method (known as logical positivism or Vienna circle) was enormously influential. We have already seen the contributions of J.S.Mill, senior, Cairnes, J n Keynes and to a greater extent Lionel Robbins, all of them founding fathers of our economic theory, among others, and all of them inspired by this epistemological paradigm of deductivism, which we can basically call verificationism, different from the more evolved paradigm that will come from the hand of the remarkable philosopher of the 20th Century, Karl Popper, called falsificationism. But what was the difference between the two? And in what way did falsificationism impact management?

First, let's start with the two main criticisms towards the hypothetical-deductive postulates: -

- This scientific logic, in the end, means explaining without understanding. The underlying problem that arises at this point is that the hypothetic-deductive method, as it has been described, is implicitly based on Hume's concept of causality, according to which causality is the conjunction of two events a and b that they are contiguous in time and space, so that the previous episode, a, is called cause and the later, b, effect; however, there is no necessary connection between the two episodes, so that causality can simply be a spurious correlation between two phenomena that occur over time. Opponents to the Vienna Circle criticize this human notion of causality and postulate instead that scientific explanation and prediction must include a mechanism that connects cause and effect, so as to ensure that the correlation between two events it is necessary and not merely accidental. Returning to the previous example, it would be necessary to know why b happens when a happens, and not only that both events occur in time with a certain sequence.

- Secondly, logical positivism - and in particular the verification principle - incurs the well-known problem of induction, which has concerned philosophers since David Hume: no arbitrarily large number of verifications can prove that a theory is true because the inferences from the particular to the general lack a logical justification. In other words, a general statement cannot logically be derived from singular claims, even if the number of these singular claims is high; on the contrary, any universal affirmation can logically be contradicted by a singular affirmation. Using the classic example (which has its origin in Stuart Mill), from the observation of a million white swans the inference 'the swan is white' cannot be obtained, but it is enough to see a black swan so that the statement 'the swan is white be refuted'.

Karl Popper - whose main work is the logic of scientific research - defends the logical rationality and the importance of the hypothetical deductive method in the elaboration of theories. However, Popper knew clearly this asymmetry between induction and deduction, verification and falsification and insisted on the idea that by using the contrast of the theories can show that something is false but cannot be demonstrate that something is true. That is, the change in the validation pattern of the theories is important: The Vienna circle advocated empirical observation as a verification mechanism, while Popper shows that empirical observation has no verification capacity but falsification. In this way, science, for Popper, is only a set of knowledge susceptible of being empirically falsified, badly despite the devotees of the Vienna circle.

In short, for Popper, the scientific collection is no more than a set of conjectures that have withstood the refutation attempts so far, and in this way the progress of science consists in the progressive substitution of some theories by others, through a process of trial and error. For example, using the Popperian language, today neurosciences would be falsifying the hypopaper of rationality in force in management; we will come back to this topic later in this paper. Now, how do we obtain the hypotheses from which the theories start, which are then subjected not to verification mechanisms, but to falsification? For Popper, induction is not the appropriate method, since to be able to elaborate generalizations through induction, it is necessary to select some observations from the total of the existing ones (which is an infinite set), that is, an arbitrary mechanism is included. Therefore, for Popper, agreeing with logical positivism and the

Vienna circle, the theories are generated in the understanding of man, anticipate the experience and are not the result of empirical data. In short, for the great Austrian Philosopher, when theorizing, one must start from assumptions, not from facts of reality; but these postulates are only conjectures, which can be falsified at any time, contrary to the statement of the verificationist, much more closed in accepting possible failures of their theories.

But of course, not any falsificationism is appropriate, and at this point we must differentiate between naive and sophisticated falsificationism. The naive falsificationism is the one that maintains that a theory can be falsified only by the fact of finding a simple error in a theory, that's why it is naive, since theories are not invalidated in such a simple way. Popper proposes the sophisticated falsificationism as appropriate, where a theory must specify, a priori, the conditions of observation that would falsify it; moreover, the more exact the specification of those conditions in which the theory would be refuted and the more probable its occurrence, the more risks the theory runs but at the same time there will be more guarantees that, provisionally, it is highly confirmed. In synpaper, although for Popper the theories are 'simple conjectures', to falsify them is not so simple either, but a series of requirements like those mentioned must be fulfilled, requirements that we believe today fulfills the assumption of neuroeconomic rationality to falsify the neoclassical one.

Criterion in Management

Economic science could not remain isolated from the influence of Popper's message. Around the decade of the '30 appear strong critics to the 'state of the arts' of the time, denoting the influence of falsificationism (the ideas of Popper) in the field of economic methodology. The pioneer in these struggles is Hutchison, a graduate of Cambridge, a teacher from 1935 to 1938 at the University of Bonn, a university where he had the opportunity to become familiar with the ideas of the Vienna circle, which he would then criticize strongly. We had already commented, in an earlier paper, that his work the significance and basic postulates of economic theory (published in 1938) is one of the first, during the 20th Century, that rose up against the deficient ad-hoc premise on human (utilitarian) rationality built on management; in fact, his work is a criticism of Robbins' 1932 essay, both because of the aforementioned theme of rationality, and because he advocates a greater degree of utilization of empirical procedures in management. In this way, his thought is considered as a change of tendency with respect to the

previous verificationist methodology and as the explicit introduction of certain Popper contributions in the economic methodology. Hutchison directly attacks the ideas of Robbins and his predecessors (Cairnes, Senior, Keynes, etc.) By stating that the fundamental difference between science and non-science is that the propositions of the former must 'be conceivably capable of being submitted in empirical contrast or being susceptible to being reduced - by logical or mathematical deduction - to testable propositions, recommending economists to stick to these empirical propositions, which are potentially falsifiable.

It is interesting for us, as apologists of neuromanagement, to analyze some of the criticisms that Hutchison's contribution received, for example that of Knight, which insists - with arguments similar to those of Robbins - on the impossibility of the starting point of management are assumptions susceptible to contrast because in human behavior there are important unobservable facets. Knight defends, the convenience of maintaining a position of methodological dualism between social and natural sciences, a fact in which we agree, regardless on the fact that now with neuromanagement there will surely be a big approach between the methods of hard sciences and soft. We will not go into detail again that the criticisms of Kaldor (and similar Machlup) greatly influence Hutchison, even achieving that, years later, the latter retract, opting for methodological dualism, compared to his monist position externalized in 1938; and that in addition it arrives to affirm that their demands of empirical testing refer primarily to the final propositions, not to the premises. In short, although it is not clear why Hutchison retracted his statements in 1938, he is cited here as well because this work is one of the first important that stand against the 'diktat Robbins', the orthodoxy of that time, and it is the forerunner of one of the most important methodological contributions of the 20th Century, Friedman's essay, which managed to attract the attention of professionals of management from the decade of the fifty.

In 1953, the renowned Milton Friedman, intellectual heir of Alfred Marshall and trained in famous American Universities such as Chicago and Columbia, published his article the methodology of positive management, undoubtedly the most influential work in the field of economic methodology of the twentieth Century, which would achieve over the years, monopolize the support of the majority of professional economists, and obviously meant the 'strong entry' of Popperian falsificationism into economic science.

Although we discussed in a previous paper -as closely related to the concept of rationality- the main postulates of this work, some will be mentioned here again, and analyzed in greater depth, to have a clear vision of Friedman's influence on the history (and in the present) of the methodology in management. But in general, we can anticipate that Friedman's contribution has influenced to epistemologically consolidate the current neoclassical paradigm in the future, since through his paper of irrelevance of the assumptions he has put a 'protective shield' to this form of theorizing, that can hardly be changed.

In the field of philosophy, Friedman receives a considerable influx of American pragmatism, which include names such as John Dewey or William James. This stream of thought, in short, held the following:

- The objective of science is to dominate and control nature.
- Experience must be the valid way to achieve the previous objective. The starting point of the hypotheses should be the empirical evidence; the point of arrival is the reality because it is necessary to contrast the implications of the theory. Moreover, the validity of the theory depends on the results it provides. In particular, a theory will be correct if it predicts adequately.
- The validity of a theory, ultimately, derives from the consensus among researchers as to its usefulness, and not from the theory being true or false (these are categories that are no longer considered relevant). In particular, a theory should not be rejected because its assumptions are not realistic.

Following his philosophical influences, we already said that Friedman was a falsificationist, that is to say that he receives from Popper the idea that proceeding to the testing of a hypothesis allows its falsification, and not its verification, as it was defended in the past. Finally, Friedman has some influences from the Vienna circle, especially with regard to methodological monism. Below, some reflections on his main ideas in the field of economic epistemology.

Instrumentalism

The instrumentalists (Friedman is) consider that the status of truth of theories, hypotheses or assumptions is irrelevant from the practical point of view as long as the conclusions logically follow from them are successful.' and if we take into account that for the pragmatism - current that exerts a considerable influence on Friedman - the starting point of the hypotheses is

the empirical evidence, the problem of the induction is latent in the approach of Friedman; let us remember what was said in previous pages when explaining Popper's thought, that induction is not the appropriate method to elaborate the premises, since to be able to elaborate generalizations through induction it is necessary to select some observations from the total of the existing ones (which is an infinite set), and therefore, agreeing with logical positivism and the Vienna circle, the theories are generated in the understanding of man, anticipate the experience and are not the result of empirical data. In this way, the instrumentalist position offers an outlet for the problem of induction that came with the philosophy of pragmatics, a problem of which Friedman is fully aware (induction is not a procedure that argues and establishes the veracity of the conclusions in logical terms, unlike the deduction). And since induction does not provide that guarantee, it is necessary to look for an alternative that allows establishing that a theory is valid. This alternative way, for Friedman, is the success in predictions: the ultimate criterion for judging the validity of a theory is the conformity of its predictions with experience.

This argument of Friedman can be interpreted, in a first approximation, in the light of the pragmatic vision that he has of science: science is a theoretical instrument oriented to solve real problems (in particular the economic theory should be oriented to the economic policy). How to know if the theory will serve in practice in the solution of problems or, in other words, that has reached a sufficient understanding of the phenomenon that allows to manage and control it? For Friedman, the most convincing way to make sure that theory understands phenomena is to prove that it is capable of predicting. In short, Friedman tells us: it does not matter in what way the model is generated, if inductively, deductively, or if with realistic or not so realistic premises about the human being and his rationality; but what matters is that predict according to reality. Perhaps more than one economist, especially those who are not too familiar with the topics of epistemology, share Koopmans' vision (to mention only one of Friedman's critics), that observation allows for (by induction) premises that are true and then logic applies the argument by which the truth of the premises generates true conclusions. That is, for Koopmans it is possible to establish the validity of a theory independently of its applications.

Synthesizing the previous paragraph, any layman would say: yes, it should be like that, i take the assumptions out of reality, and put together a

model, surely the same (later) will predict reality well. Friedman, in one of his most controversial contributions, argued in a completely opposite way: the realism of the assumptions does not matter, it matters the goodness of the predictions that emanate from the economic models. The historical context of the question is as follows: in the years 1946-48 some articles had been published in the American Economic Review arguing that the assumptions of maximization by companies were unrealistic, since the firms do not know the exact position of the companies, its marginal revenue and marginal cost curves. These were followed by other works that aspired to refute this approach, so that generated a debate about what can be considered one of the foundations of neoclassical economic theory: the premise on hyper-optimizing economic agents. Friedman responds to the controversy by affirming, words more, words less, that it is irrelevant that the assumptions of the theory are realistic or not; what is important, as noted before, is that the theory is capable of predicting. Friedman gets into pure logic to explain this topic, particularly with the ideas of modus ponens and modus tollens. What do we mean by this? These are two argumentative mechanisms used in logic. We have to:

- The argument modus ponens implies that if the assumptions are true, the conclusion is true. In other words, 'the truth passes forward', from assumptions to conclusions.
- The argument modus tollens implies that if the conclusion is false, some of the assumptions will be false, that is, 'passes the falsehood backwards' of the conclusions to one or more of the assumptions.
- Employing both procedures in the opposite direction gives rise to different fallacies. Thus, the fallacy of affirming the consequent consists in arguing that if the conclusion is true the assumption is true, which is not necessarily true because 'the truth cannot be passed back'.
- Similarly, the fallacy of denying the antecedent is incurred when it is argued that if the assumptions are false, the conclusion is false, which is not always the case because 'falsehood cannot be passed forward'.

The main argument of Friedman is that if the truth cannot be passed back (fallacy of affirming the consequent), the true conclusions do not require true assumptions. And if an assumption is false, is this a sufficient condition for the falsity of the conclusion? The answer again is negative, because to answer affirmatively would be to incur the fallacy of denying the antecedent. And hence Friedman's main argument, based on the laws of

pure logic: the use of inadequate assumptions does not necessarily generate inadequate conclusions. And this is the main protective shield that Friedman gave to neoclassical modeling: according to him, the use of the modus ponens in the right direction (more or less what neuromanagement raises when demanding the use of true human rationality in models) it would not be a fruitful procedure for science. In fact, says Friedman, the modus ponens is not applicable because scientists proceed by searching, not from correct assumptions, but from predictions that succeed. In addition, in another aspect to discuss 'long and hard', maintains that the lack of realism is not only an obstacle to science but can be an advantage. For Friedman, the most accurate theory is the one that explains and predicts more with less: that less refers to the assumptions, which must capture the essential economic relationships but be simple, so that they are not lost in the tangle of accessory details.

In particular, Friedman is against making one to one representations of reality when modeling. The key to his argument lies in the trade-off between realism and simplicity: the crucial attribute that a theory must possess is to grasp the essential. In this sense, falsity of assumptions does not mean logical or epistemological falsity for Friedman, but a departure from exhaustive descriptivism. And, in the context of the body of knowledge originally raised by the polemic, the neoclassical theory of the firm - as indicated above - Friedman's ideas fit in the following way: 'it is true that employers do not calculate the cut-off point between marginal cost and marginal revenue to determine the optimal amount offered so that profits are maximized, but even if the agents do not really act like that, supposing that they do it is useful and produces results that they are observed in practice.'

At this point of the argument on Friedman thought, we wonder why confuse unrealistic assumptions with lack of simplicity, the first would be for example build models assuming that human beings are hyper-calculating and always optimize their decision (as has been come modeling traditionally); instead the second would involve building models, with realistic neuropsychological premises, but so full of details that they did not contribute much when it comes to improving the predictive model. The reality is that they are two very different issues, of course you have to be simple to model, and try to predict more with less, but not from unrealistic assumptions, less now that neuromanagement tells us what human rationality is like, and not through introspection, as has been a

tradition in our science. Moreover, following Friedman, what if we assume that all human beings are insane, maybe the models would predict better than with the current hyper-rational. According to Milton Friedman, it would seem that anything goes, a kind of casino, where one just have to be lucky with the assumptions (the bets), to get predictive success (the winnings bets).

Deep down, Friedman understands that, although the assumptions are not true hundred percent, they are not so far from reality. That is why the models thus formulated, with a hyper-optimizing logic (for example, the maximization of the net profit of the entrepreneur), predict well. We, the neuroeconomists, perhaps do not agree so much with the 'hyper-optimizing mentality' -in fact the current empirical evidence shows it-, but our models are forced to predict better than the neoclassical ones, otherwise, to the light of Friedman's instrumentalism, will not be an important contribution to science.

The Provisional Carácter

We said earlier that Friedman is notably influenced by Popperian falsificationism: empirical evidence can refute a hypopaper but not prove it, so that a given paradigm is always provisional, a conjecture, that at some point some other theoretical body will come to refute, partially or totally. Then, in his 1953 work, the American Economist tells us his golden rule in epistemological matters, in what is the result of the influences of Popper, the pragmatists and their own added value to management: 'the hypopaper is rejected if its predictions are frequently contradicted (or more often than the predictions of an alternative hypopaper); he is given great confidence if he has survived many opportunities to be contradicted. The evidence of the facts can never prove a hypopaper, it can only stop disapproving it, not rejecting it, which is what we generally mean when we affirm, somewhat inaccurately, that the hypopaper has been confirmed by experience. Finally, Friedman 'puts pressure' on those schools that intend to challenge the neoclassical paradigm -as in the case of neuromanagement-, stating that there are two additional criteria -with which we fully agree- for the choice between alternative theories, once all of them have shown their consistency with the empirical evidence:

- **Simplicity:** a theory is simple when less is the initial knowledge necessary to make a prediction within a given field.
- **Fertility:** a theory is all the more fruitful the more accurate the predictions that result, the

greater the area within which the theory offers predictions and the more lines of future research it suggests.

That is to say, in the end, the neuroeconomic models must come out of the trade-off 'simplicity versus fertility' that is going to arise; since they will surely be less simple than the neoclassical ones but perhaps more fertile, finer in their contributions, otherwise they will not last in time.

Metodológicas Monismo

Milton Friedman is favorable to methodological monism, at least in regard to positive management. And while he admits that objectivity is more difficult to achieve in management than in other sciences (because of the issue of working with human beings and their free will), it is surprising to state, unlike Robbins, that this fact does not entail a fundamental distinction of management with other disciplines of a more experimental nature. And in this way, for Friedman the starting point in the construction of hypotheses must be the empirical evidence - just as it happens in the experimental sciences - and not the introspection, far surpassing Robbins and the neoclassical in this aspect.

To conclude with Friedman's contribution, and after having mentioned the most salient aspects of the correct research method for this influential North American Economist, we believe we have made our position as neuroeconomists quite clear: although we agree with several of its postulates, in our opinion its main fault is in confusing simplicity of assumptions (which is desirable) with assumptions unrealism, subject to the model predicting well; this is the great criticism we have for Friedman's thinking. Obviously, we recognize, it is much easier to defend our criticism now than before (for example in the time of Hutchison), when there was no instrument that today provide neurosciences to refine the assumption of rationality, so it is understandable that the Friedmanian instrumentalism has had so much life, but maybe it's time to stake out again. We will return to this point at the end of the paper.

Epistemological Triumph

The great victory of Friedman was to change the axis of the discussion. It is no longer established in terms of true or false theories but of useful or useless ones. The premises have already become secondary, if the model predicts well, it must be because this and its initial hypotheses are not so far-fetched. That is why it is already becoming clear, too, that in the light of instrumentalism, the only way for future neuroeconomic models to be accepted as a progress

of science is that they surpass those of traditional theory in terms of predictions; in the case that, for example, they only equaled it -and with greater complexity to theorize- traditional modeling would remain fully valid. In this we must recognize an epistemological triumph for Friedman: theories today are useful or useless, beyond their assumptions; and to be more useful than another, a theory must predict better, otherwise there is no progress. Will it be the triumph of what Paul Samuelsson sarcastically calls 'juggling f'? For Samuelsson, the unrealism of assumptions should not be considered a merit of a theory but, rather, a demerit; since theories must, in his opinion, describe reality.

However, when Friedman's argument is coldly analyzed, and although it really sounds like 'juggling' at first, we think it is difficult to refute, since he is saying something very clear: 'I do not care how you made your model, but if predicts better than all others, is the most valid; and obviously, it is implicit that if predicts well, should not be so preposterous his starting assumptions'. And neuromanagement should openly accept the Friedmanian challenge, in the sense that if the neuroeconomic models fail to predict better than the current ones, for better assumptions they have, they will hardly imply a progress for economic theory. In this, we believe Friedman has imposed a test to validate the models: either they are useful or they are useless; methodologically, we repeat again, it is a triumph that must be recognized.

But what would happen if, in fact, the neuroeconomic models do not predict better, but only the same as the current ones, but with a greater degree of complexity in the assembly of the models? Should we neuroeconomists Ross our arms, and not apply any of the latest findings on the true rationality of the human being making economic decisions? We think not, and while we do not discuss the legitimacy of instrumentalism as a methodology, we cannot be satisfied with the current state of the arts. If neurosciences allow us to properly model human rationality when making economic decisions, we have the scientific obligation to use said knowledge; moreover, for neuromanagement, it should be a question of 'tranquility of scientific conscience' to proceed with more realistic assumptions, whether predictions improve or not.

Dominante Paradigma

After all the analysis carried out throughout this paper, we believe we are in a position to summarize some of the ideas in which today, at the beginning of the 21st Century, most mainstream economists coincide, always with

respect to the methodological issue. And obviously, in the constitution of this dominant paradigm, Friedman's contribution has exerted a remarkable influence.

- There is a single science, management, with a double slope, positive and normative; most of the positive propositions are testable, so that, ultimately, their degree of validity can be determined by reference to empirical evidence; on the other hand, normative propositions are not susceptible of empirical testing.
- The demands of logical positivism of the early twentieth Century, excessively empiricist, have softened to some extent. Today it is admitted that within management there can be positive propositions that cannot be verified, but the elaborated conclusions must be testable.
- A branch of knowledge enjoys the status of science if it can submit its conclusions to the contrast offered by the facts of the real world, here we can clearly see the influence of Popper's contribution; but without falling into naive falsificationism (admit that a single test can refute a theory). What is accepted today, on the contrary, is sophisticated falsificationism, according to which refuting a theory is a little more complicated.
- The discrimination between rival theories is carried out mainly in terms of empirical evidence, since, due to sophisticated falsificationism, it is difficult to prove and / or refute the theories in a definitive way.
- There is an interaction between empirical evidence and deductive reasoning in the elaboration of economic theory. Theories rest on assumptions, and in regard to the realism of the assumptions, in general, the contributions of Friedman already described are accepted. In the development of the theory careful attention is given to its logical structure and deductive rigor, so that in the contributions of economists a degree of mathematical complexity can be found increasingly. In recent years the controversy has grown over the desirability that the mathematical content of economic theory was so high.
- It is commonly accepted that human behavior lacks the determinism found in the behavior of the inanimate world, therefore a perfect methodological monism is impossible.

Change of Paradigm

We had started the paper arguing that it was only at the end of the paper that we were going to try to 'throw some light' on the intricate theme

of the paradigm shift, since the issue is not simple and we have to extrapolate too much into the future; remember that neuromanagement as a field is still very embryonic. In order to analyze this topic, throughout this paper we stopped first in the concept of paradigm (Kuhn's, one of the most accepted at present), then we made a brief detail of the main epistemological schools that have influenced management in the last 200 years, to finally take the time to review the historical evolution of the method in management, that is, how the main theorists were working to build our influential science.

That is, along the way traveled to here (we believe) has been relatively clear who were the most influential over the past two centuries, so that the paradigm currently in force (the original neoclassical, although more refined) has triumphed the way he did it. Without fear of being wrong, methodologically there are two names that stand out: Lionel Robbins and Milton Friedman. Can we say the same thing, about thirty years, of Glimcher, Kahneman, Thaler, Camerer, or Paul Zak, to name just a few of the most reputable neuroeconomists today? That is, can neuromanagement revolutionize the current method in our science, the 'Friedman paper' for example? Will this novel field change the prevailing neoclassical paradigm?

And if the answer is affirmative, will it be a strong change in the Kuhn's sense? Or more limited in Lakatos' sense? In what follows, we will try to sketch some ideas on this issue, starting with a brief summary of the ideas of Kuhn and Lakatos, two influential epistemologists of the late twentieth Century, with specific writings on the subject of paradigm's change.

The main writing of Thomas Kuhn is the structure of scientific revolutions, whose first edition is published in 1962. About this work, which is based on an analysis of the historical evolution of several sciences, there is quite a coincidence that:

- For Kuhn, the historical evolution of science is characterized by abrupt changes from one paradigm to another, as opposed to soft changes;
- However, accepted the current paradigm is, there will always be certain inexplicable anomalies in the context of said paradigm; but when the number or magnitude of the anomalies are excessive and the restlessness emerges among the researchers, a scientific revolution or change towards a superior paradigm to the preceding one takes place.

Scientific progress, for Kuhn, would occur in the following steps:

- Be part of a situation of agreement among scientists, of any science, on the problems to be solved and the general forms of the solution (for example, the neoclassical paradigm in management).
- At a given moment, certain theories that introduce the controversy appear, especially highlighting certain 'theoretical anomalies', so that the prevailing consensus is breaking down (in management, all criticisms of the concept of rationality listed in paper 3 could be), among them Hutchison, J.M.Keynes, Simon, Kahneman, and finally neuromanagement).
- A new theoretical approach is consolidated, offering a solution to the problems that until then had been neglected (for example, if neuromanagement were consolidated, with its new hypopaper on human rationality).
- Finally, there is a conversion in the scientists to the new framework (there would be a paradigm shift in management), which becomes the normal science of the next generation, until the process starts again.

However, the main critics of Kuhn argue that revolutions in science have been much slower and less dramatic than he maintains, where it can be said that, at any moment, science consists of paradigms that overlap and they influence each other, and that the new ones do not replace the previous ones in a sudden but gradual way, which leads Schwartz to describe this vision of science (Kuhn's) as 'funny, but false'. However, his paradigm concept is quite accepted:

- 'Universally recognized scientific relationships that, for a certain time, provide models and solutions to a scientific community'
- 'The complete constellation of beliefs, values, techniques, etc., shared by the members of a given community'

Lakatos, contrary to Kuhn's ideas, focuses its contribution around the concept of the research program:

- In science, more than paradigms, there are research programs, with a central core and a protective belt.
- The central core is composed of assumptions, premises or beliefs that are considered irrefutable, largely due to their metaphysical nature (there would enter the concept of optimizing rationality that we have criticized so much in management).
- Positive heuristic or partially articulated set of suggestions or indications on how to change, the refutable variables of the research program.

- Negative heuristics or methodological rules that tell us which research paths we should avoid.
- The protective belt is formed by auxiliary hypotheses that are modified as they are falsified, as a result of the suggestions of the positive heuristic.
- For Lakatos: the hard core remains relatively stable;
- The advance of science, meanwhile, occurs when one research program is considered better than another because it is able to explain all the facts of the second and, in addition, make other predictions, some of which are confirmed empirically. The history of science, therefore, can be conceived, in part, as the abandonment of degenerate research programs and their progressive substitution by other progressive ones;
- Lakatos is widely accepted today among management theorists.

Neuromanagement: Hard Core or Protective Belt?

If we take the Keynesian research program (or paradigm), in the middle of the last Century it seemed that it was going to modify part of the neoclassical hard core (the Keynesians argued that management was full of rigidities, that they contradicted the neoclassical optimizing premises, and their forecasts of fully flexible variables), plus other changes in the protective belt. However, there is some agreement among economists that Keynes' ideas have been discredited a bit in recent decades, at least among the theoreticians, due to the large number of intrinsic contradictions present in their models, and the need of micro fundamentals for macro models. Perhaps Keynes, if he lived now, with the advances in applied neuroscience, would be remarkably more influential than he could be in theoretical matters; since in the end Keynes, like the majority of the theoreticians until not long ago, resorted to the so criticized 'introspection' to construct the premises of their theoretical models, the hard core in terms of Lakatos.

Currently, behavioral management and neuromanagement are strongly attacking the dominant research paradigm / program, although not as broadly as Keynes (due to the variety of topics), but with a lot of hardness and strong scientific arguments against the most basic metaphysical premise of neoclassical thought, the hypopaper of ultra-optimizing rationality, which is no small thing, because from there all models start, the micro and today the macro. That is, today the hard core of our science is the one that could be

modified, with neuromanagement and behavioral management. And from this possibility emerges the following question: would we be closer to a paradigm shift to the Kuhn than to the Lakatos, for the radical of the eventual change? The answer is not easy, however at the end of the paper we propose one; but first let's review the current epistemological critiques for neuromanagement.

Apologists and Detractoras

The birth of neuromanagement has created one of those rare historical moments in which economists stop to reflect on the fundamental questions of our science, that is, the epistemological, as for example the interdisciplinary approach to the economic problem, the borders of management, the objectives of our science, questions of validity or refutation of theories, among others. The debate is hot today, and is on the agenda of many important economic congresses around the world, and the waters are quite divided, although generally with a nod in favor of this novel branch.

On the side of the detractors, we have the already famous Gul and Pesendorfer, for whom neuromanagement is and will be irrelevant to management, both in empirical evidence and in explanatory power, since management and neuromanagement ask different questions and as a result, they use different abstractions. For both authors, economic models should not make assumptions about the physiology or psychology of the brain; giving neuromanagement only the modest role of inspiration for economists, as new neuro studies unravel new issues related to decision-making, and as long as the models include variables about what an economic agent chooses and not about how an economic agent chooses. But the maximizing modeling, and the hyper-rational homo economicus will never be dethroned, in the particular vision of these respected and at the same time highly criticized economists, who join some (very few) who believe that neuromanagement is a branch with a lot of marketing and very little scientific rigor. Luckily, today practically no one in the profession thinks that neuromanagement is irrelevant to economic theory, in fact there are those who think that neuromanagement will allow management, traditional social science, to approach the methods of natural sciences, which use inductive behavior much more than the deductive one (tradition in management), and they tend to be more rigorous from the epistemological point of view than social ones.

However, neuromanagement will certainly not be what camerer, loewestein and prelec said in

2004-05, when they thought this new branch would serve to expose all the theoretical anomalies of traditional management and its hyper-rational models, and would help the profession in a forceful way to refute or accept models and explanations in management, that is, to facilitate the adequate Popperian falsificationism in economic science. Today we know that the epistemological triumph of neuromanagement is not guaranteed at all, and that still remain many years of battle to see certain achievements, but perhaps not as optimistic as those that were believed ten years ago.

In fact, at that time, Camerer, Loewenstein and Prelec came to affirm that 'neuromanagement would allow direct measurement of thoughts and feelings', replacing the weak theories of marginal utility and revealed preferences, which now reign in micromanagement and that date more than 100 years. And they affirmed that the new branch 'would not only increase the realism of the current models, but eventually replace the traditional constructs, with new models,' the neuro models'. In short, an excess of optimism that was wasted at that time by these three founding fathers of neuromanagement. In this way, in recent years Camerer, Loewenstein, Prelec and several other enthusiasts of the first period have been reducing the volume of their claims, since they have found several difficulties with neuro field research, especially with econometric deductions from functional magnetic resonances (fMRI), among some of the methodological limitations found in neuromanagement. However, the latter does not invalidate at all the future of this novel branch of management, since it is truly helping to improve the traditional economic theory, and not only as a source of inspiration, according to Gul and Pessendorfer. Neuromanagement will undoubtedly improve particular explanations of economic phenomena, and thus of aggregate markets in which these phenomena occur; but it is not so sure that the paradigm of traditional hyper-rational modeling changes strongly, because of the Friedman paper, already widely commented.

Who can have doubts that neuromanagement, in a short period of time, will help us understand economic issues as psychologically intricate as the phenomena of monetary illusion, Keynesian sticky prices and strategic interaction in game theory, among other issues; all issues that today maximizing mathematics does not leave well-founded at all. However, probably micromanagement, macromanagement or theory of games, as complete bodies of theory, do not change much thanks to neuromanagement. In any case, today it is difficult

to find theoreticians who strongly criticize the neuroeconomic project, and most still see it as 'a hope for the future', rather than as a 'passing fad'. And undoubtedly, behind the acceptance of neuromanagement as a promise, is that of all its branches derived in business and administration: neuromanagement, neuromarketing, and neuroleadership, among others.

For example, among moderate critics, we find Kuorikoski and Ylikoski, that argue that the idea of a direct connection between management and neuroscience is wrong, and that both fields can only be integrated via psychological theories of decision making. The central paper of them is that the neuro findings only provide primary evidence for psychological research, which should link the neuro with the economic, to be able to explain the economic behavior of agents with greater wealth than traditional modeling.

In line with the above, we find moderate criticism from Alessandro Antonietti, emphasizing that several of the correlations neuro-mind reported by neuroeconomists do not serve much as empirical evidence, given the technical problems still present to identify the exact role played by the different brain structures behind the decision process. And Antonietti ends up agreeing with Kuorikoski and Ylikoski on the important role that psychological theories must play in the connection between neuro findings and economic theory. Particularizing a little more, today the authors already speak of two different neuromanagement, or rather, of two different research programs within the same field:

- Behavioral management in the scanner (BES)
- Neurocellular management (NE)

With regard to BES, it is the branch of neuromanagement that tests via neuroimaging (among other techniques) the main postulates of behavioral management; while the other branch (NE), follows the opposite way: apply economic models to understand the functioning of the brain, and that at the hand of Paul Glimcher, is collecting more and more scientific respect. In fact, one of the most promising research of Glimcher focuses on its so-called 'subjective value' (the 'utility' of economists), based on the dopaminergic signals detected in the ventral striatum and the middle prefrontal cortex of our brain, constituting a 'common zone' of reward or satisfaction (valuation) in the individual decision-maker. Undoubtedly a very interesting research program of the new branch, and with less criticism than the bes, although the latter is the most profusely developed in this last decade (and the most analyzed in this work).

In particular, Harrison and Ross criticize that the bes branch consists of repeating protocols that permanently demonstrate human irrationality under neuroimaging, but in studies carried out with few people, who apply valuations to different alternatives, but always isolated from the ecological-environmental context that surrounds. Subsequently, according to both authors, these irrationalities will try to be shown as anomalies within the theory of 'rational choice', which does not take into account that the architecture of the brain determines that our decisions are a variable mixture of rationality and emotionality. And finish Harrison and Ross, arguing that this way of approaching studies in the bes branch ignores the ecological nature of economic rationality, that is, ignore the way in which people approach that hyper-rationality that illustrates the theory, given by the external structures that exist in the environment, which include especially the cultural issues that limit and condition our decision making.

In short, we see that both authors (Harrison and Ross), in order to criticize the bes branch of neuromanagement, resort to the postulates of the American institutionalist school, an old school, but in a renewed boom within the current theory; but they do not do so using the macro institutions (federal reserve, concrete legal structures, etc.) As is usually done, but the cultural institutions (habits, traditions, customs, etc.) That have generally been exempt in traditional economic models, such as in the neuro. And finally, they recommend for the new models neuro BES, the modern concept of ecological rationality of the Nobel Prize Vernon Smith (School of Experimental Management), which has been growing more and more in scientific acceptance at present. What does this concept of ecological rationality say? That economic regularities, understood as ecological properties of the context surrounding economic agents, can dominate the normal neuropsychological processing that occurs in controlled laboratory experiments, invalidating them.

And later, Harrison and Ross criticize the econometrics derived from brain neuroimaging, in the first place due to the lack of precision of their estimates, which is a topic that technological progress will probably help to improve, but which today is a problem. Recall that the unit of analysis of neuromanagement is the brain, emitting signals per unit of time, from precise and specific points, that are captured by different techniques, especially neuroimaging. Secondly, they mention the problem of 'reverse inference', that is, when activations in certain regions of the brain make it possible to

presume the identification of specific neuro - processes. So, these authors argue that high correlations between well-identified psychological patterns and their neural counterparts are the exception and not the norm in most experiments, and that most of the time the interpretation of neuroimaging is captious. For example, the amygdala and insula, which have been mentioned in several BES studies as playing a central role in making economic decisions, from the emotional point of view, have been identified, in other studies outside management, as playing other roles, not exactly emotional roles. And thirdly, they criticize the problem of blood flow in anticipation of some event, which finally does not end up happening and that disregards the expected relationship between blood flow and neural activity, which helps to confuse the identification of neuropsychological processes applied to decision economic making.

And while we believe that the criticisms of Harrison and Ross towards the BES branch are important, they do not invalidate it at all, since on the one hand technological progress will improve neuroimaging (neuromanagement is a project for the future, not something for the very short term), and on the other hand, these authors fall into a problem that Michiru Nagatsu identifies very well: the neuroeconomists of the BES branch pose questions conceptually different from those of the traditional economists; the first ones consider how the individual economic agents (considered as physical and biological beings) take decisions, while the latter consider the same thing but with ultra-rational beings, that is, the traditional homo-economicus. And Nagatsu concludes that the two approaches can lead to different models of decision making, and that they are not necessarily incompatible with each other. What is not clear is which of the two types of models will end up adopting economic theory, that is, the epistemological theme will be in full debate for the next years.

And this must necessarily be the case, since it will be the epistemology of management that finally decides the scientific validity of neuromanagement, in any of its two branches. In the opinion of Bernheimer, a scholar and critic of neuromanagement at the same time, only when this novel and hybrid discipline provides us with a model derived from research in this field, that improves our measurement of the causal relationships studied by traditional models, the necessary proof will have been passed for its full acceptance. This is what has been called the Bernheimer challenge, which is exactly the same thing that we are proposing with our paper: the

chance that the current and future neuroeconomic models are accepted as a progress of science is that they surpass in terms of predictions to those of the traditional theory; otherwise, traditional modeling would remain fully valid, because it is simpler than the neuroeconomic model.

This is the result of the epistemological triumph of Milton Friedman, back in the mid-twentieth Century, to establish the armor that still today scientifically supports the traditional economic theory, hyper-rational and hyper-unrealistic, and that gave entry into management from theories by Karl Popper, the remarkable twentieth-Century philosopher of science:

- Between the years 1946-48 some articles had been published in the American Economic Review that argued that the assumptions of maximization by companies were unrealistic, since firms do not know the exact position of their marginal income and cost curves, generating a debate about what can be considered one of the foundations of neoclassical economic theory. Friedman responds to the controversy by affirming it is irrelevant that the assumptions of the theory are realistic or not; the important thing is that the theory is capable of predicting accurately.
- According to Friedman, it is verifiable that truly important and significant hypotheses have premises / assumptions that are clearly inadequate representations of reality and, in general, the more significant the theory is, the less realistic these assumptions will be. For him, the reason is simple, since a hypothesis is important if he explains a lot with little, that is, if he abstracts the important elements of the accessory. The penetration of this paper in the standard methodology of management is so great, that the only way that the current and future neuroeconomic models are accepted as a progress of science is that they surpass in predictions those of the traditional theory; since in case they only equaled it, the traditional modeling would remain fully valid, because it would surely be simpler than the neuroeconomic model. In this we must recognize an epistemological triumph for Friedman: he changed the axis of the debate and the theories today are useful or useless, beyond their assumptions; and for a new research program (such as neuromanagement) to be more useful than the dominant program, it must predict better, otherwise there will be no scientific progress.

But suppose that the neuroeconomic models do not manage to overcome the traditional

ones in predictive capacity and only equals them, the neuroeconomists cannot be satisfied with the triumph of 'juggle f' with the assumptions, with a 'anything goes while predicting the model well'. If neurosciences allow us to correctly model human rationality when making economic decisions, we neuroeconomists have the scientific obligation to use such knowledge, although there is a possibility that the models become more complex. In this sense, we share with Antonio Rangel that neuromanagement can very well become a field of specialization within management, regardless of whether or not it meets the Bernheimer challenge.

CONCLUSION

Synthesizing Friedman's thought, with his famous paper of 'irrelevance of assumptions', we believe that he confuses something desirable (the simplicity of the model, explaining much with little) with something undesirable (unreal premises, to predict reality almost by chance - for a success of the lottery type with the chosen assumptions-). In fact, we have already mentioned before that we fully share Samuelsson's qualification to this paper: juggling f. But the penetration of this paper in the standard methodology of management is so great, that the possibility that the current and future neuroeconomic models are accepted as a progress of the science is that they surpass in predictions those of the traditional theory; since in case they only equaled it, the traditional modeling would continue in full force, for its simplicity superior to the neuroeconomic one.

In fact Bernheimer, a scholar and critic of neuromanagement already mentioned in this paper, agrees with our central paper for this paper, when he argues that only when this new and hybrid discipline provides us with a model derived from research in this field, to improve our measurement of the causal relationships studied by traditional models, we will have passed the necessary test for their full acceptance. In short, we recognize that there is a possibility that the current paradigm in management does not change at all with this neuroeconomic boom; that will depend, in the light of 'Friedman Thinking', on the new models predicting better. But one thing we are sure: all those scientists of management who are familiar with the neuro are going to develop over the next few years many models based on postulates about true human rationality (not the one used now), and although they may not predict better than the traditional ones -we still do not know- they will enrich the debate significantly, helping to explain many of the current neoclassical anomalies.

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