Athens Journal of Social Sciences- Volume 12, Issue 3, July 2025 – Pages 217-230

What Constitutes a "Good" Prevision? The Indispensable Link between Futures Studies and Social Science Methodology

*By Carolina Facioni**

Aim of this work is to analyse the link between Futures Studies and the Methodology of Social Sciences discipline. This can be a useful tool for better understanding what defines the quality of a research work aimed to possible futures. The historical debate on scientific explanation can be considered the starting point of the reflection about this very complex topic. The debate began to develop soon after the publication of the 1948 article by Hempel and Oppenheim. Indeed, it was a very heated epistemological discussion, although it never had a real conclusion. Nevertheless, a maybe still interesting topic directly involving the Futures Studies discipline arose directly from it: it was a sort of spin-off born from the main theme of the debate. That is, whether an explanation can also be considered a prevision, and vice versa. Starting from this, given the centrality of the topic of scientificity linked to this aspect, the many elements that emerged at the time can nevertheless stimulate still today a reflection on the criteria of scientificity in terms of research on possible futures.

Keywords: *scientific explanation, relationship between explanation and prevision, futures studies, methodology of social sciences*

Introduction

This work starts from a puzzling question: which criteria can make a prevision (or, in other words, a prediction, a forecast, or a foresight, etc.)[1] something of scientific kind. Indeed, a prevision is an assertion the object of which will take place (or could take place) in a future time, a time more or less distant from the person who supports it. A prevision necessarily aims to the future, and no one can provide any evidence about something that has not yet occurred. A very clear difficulty for scientists, for

*Sociologist, ISTAT, IIF, Italy. The present work is the exclusive intellectual development of its author and does not necessarily reflect the position of the institution to which the author belongs.

[1]It needs to clarify some choices regarding the terms used in this paper. The author uses different concept-words related to the "work on futures": i.e. "prevision", "prediction", "forecast", "foresight", "prophecy", and "anticipation". It is necessary to specify that in the present times these terms are not (or not completely) synonyms. This is because there was an evolution of the concept of prevision in times. The terms "forecast", "foresight" and "anticipation" are indicating very different and specific approaches to methods (and, consequently, to the research work) related to modern Futures Studies (Poli 2019). Other terms are not currently in use in Futures Studies. For example, the possible presence of the term "prediction" will be due solely to the need for literal citation of some authors' works, although the term "prediction" is not currently in use in Futures Studies. In this paper, the author mostly uses the term "prevision" because it is a generic but effective term, which covers many nuances of a very complex concept, without going into the specific merits of the techniques currently used in the Futures Studies discipline. Lastly, the concept-word "prophecy" is present only because R.K.Merton used it – and Merton's theorization is very important in this work's context.

https://doi.org/10.30958/ajss.12-3-4 doi=10.30958/ajss.12-3-4

whom both the topic of prevision and the distinction between what is scientific and what is not are fundamental, from the very beginning of the methodological debate (Campelli 1994). The difficulty in establishing the scientific nature of a prevision arises first from the impossibility of conducting experiments that prove hypotheses – except, of course, mental experiments[2]. An acceptable objection to this thesis is that, in the presence of a scientific law that explains a certain phenomenon, what that law explained for the past is still valid today - and it is reasonable to assume that the explanation can be valid forever. Thus, in this case, an explanation can be, at the same time, a prevision. This assertion is a core theoretical issue in the context of the debate on scientific explanation we will discuss shortly.

Starting from this premise, we could argue that, at least as far as traditional "hard" sciences are concerned, the overlap between explanation and prevision can probably work. However, if we focus on evolutions of hard sciences such as quantum mechanics, the determinism of the traditional model of explanation (and, therefore, of prevision) is no longer – or, at least, hardly - applicable. Furthermore, the prevision in social sciences deserves a separate discussion (Fasanella 1993, Campelli 2004). At most, social sciences can rely on (more or less) well-founded theories, on (more or less) solid models. In social sciences, there are no cognitive tools comparable to laws. Focusing on Futures Studies, and in particular on one of Futures Studies' core topic, the human and social prevision (Barbieri Masini 1986), the problem of the scientific nature of a forward-looking statement is evident (Barbieri Masini 1993).

The type of science to which the explanation/prediction model applies could also be a false problem, taking up David Hume's reflection, in particular his work on human understanding (Hume 1748), that also inspired the work of Bruno de Finetti (de Finetti 1931, 2006). According to Hume, starting from anyone's personal experience, the sun will rise tomorrow. Nevertheless, this statement does not express a certain fact. Indeed, the opposite assertion "tomorrow the sun will not rise" has, in terms of epistemic status, the same weight as that which we intuitively assume as correct ("tomorrow the sun will rise"). Therefore, only empirical experience, the proof of facts, will be able to confirm, or disconfirm, one of the two. In the specific context of prevision, of the reflection on possible futures, Hume's theorisation is evidently problematic, regardless of the type of science and/or discipline to which the scientific community refers. How to get out of this impasse, giving previsions a scientific value?

The Debate on Scientific Explanation, and the Topic of Prevision

A first attempt at systematizing scientific explanation probably precedes Aristotle, but it is to him, in the IV century B.C. that we owe the very first works on the subject. In particular, Aristotle deals with the topic in the First and Second Analytics of the so-called Ὄργανον (Organon)[3]. There we find a first analysis of the structure of the

[2]Mental experiments are at the basis of the modern concept of science: best part of Galileo Galilei's experiments were purely mental (Rossi 1973).

[3]In Ancient Greek, the word organon means "tool". Alexander of Aphrodisias, the Aristotle's commentator, gave this name to the collection of the six works Aristotle wrote on logic.

explanation, in a rigorously deductive key "a reasoning in which, given certain things, some other necessarily follows, for the very reason that they are". The concept of scientific explanation, its evolution, should not be confused with the historical evolution of science - or, better, of sciences - and its methods. The explanation is the specific object of a philosophical reflection: it is within the philosophy of science, and attempts to answer the question "What does a correct scientific explanation consist of?" given the possibility (in the ontological sense) of such an explanation. Some authors reflected about a scientific explanation's correct structure. It is undoubtedly interesting to note how a real debate on explanation begins to assert itself only when the individual scientific disciplines acquire their own specificity. Thus, from the "Aristotle version" (which belongs to the period in which all sciences were subsumed by philosophy), centuries passed before the scientific context produced a new model of explanation. It was only in the XIX century that J.S.Mill, a positivist philosopher, created a new model of scientific explanation. In his work "A System of Logic" of 1848, Mill did not take into account Hume's position regarding inductivism. As also Francis Bacon did in his Novum Organum (Bacon 1620), Mill identified some practical rules of induction, in analogy with the syllogism in deductive logic, with the aim of eliminating the problem that the induction by simple enumeration (but also some aspects of Francis Bacon's theorization) had left unresolved: *e.g*., the impossibility of reaching a certain conclusion.

However, the problematic nature of the link between prevision and explanation only emerged in the context of the debate on scientific explanation, starting from the 1940s. The debate involved a large part of the community of philosophers of science: it started from the publication of a paper (Hempel & Oppenheim 1948)[4] which described the scientific explanation structure. In particular, the paper described the structure of the Nomological-Deductive model[5]: it was the so-called "received view" (this definition seems to be attributable to the philosopher Arthur Pap) of the scientific explanation[6]. During his life, Hempel dedicated many other works to the topic. In effect, Hempel created a sort of "theory of explanation" in the following years. He subjected it to a constant verification and revision, welcoming with open-mindedness and critical sense all the notes and objections addressed to it by the scientific community[7]. In 1962, he published the essay "Deductive-Nomological

[4]Although some themes were already present in a previous article by Hempel on scientific laws (Hempel 1942), the real debate only began following the 1948 paper.

[5]This is the N-D explanation model:

C1, C2, … Ck explanans

L1, L2, … Lr

_____

E explanandum

It means that the event/phenomenon E arises from certain particular circumstances, specified in C1, C2…Ck, in accordance with the laws L1, L2 … Lr. Therefore, given the particular circumstances and laws in question, (C and L together form the *explanans* part), the phenomenon E (*explanandum*) has its scientific explanation – and a sort of prevision: we can aspect, given C circumstances, in accordance with L laws, that an event E may occur.

[6]For further clarification, there were discussions relating to the attribution of the model, but this aspect is not interesting for the topic here discussed. For a focus on the topic, see Fasanella (1993).

[7]In this regard, Hempel gave the scientific community a rare example of both intellectual vitality and correctness.

versus Statistical Explanation" (Hempel 1962), in which he returned to the critical aspects the 1948 article had left open. In the subsequent "Aspects of Scientific Explanation" (Hempel 1965), the model was completed, due to a formulation of the statistical explanation based on the Inductive-Statistical model[8].

Hempel's last work on scientific explaination, "Nachwort 1976: Neuere Ideen zu den Statistischen Erklarung" (Hempel 1977) was added as a Postscript to the 1977 edition of his 1965 work (and so it was for the subsequent editions). It represented the highest point of the debate, personally involving its leading exponents, in particular R.C. Jeffrey, W.C. Salmon and W. Stegmuller. In effect, the debate on the scientific explanation did not have a real conclusion: the discussion continued intermittently until the early 1980s, and many questions remained open (Galavotti 1984). However, entering the specific topic of explanation models is not the purpose of this work, nor is the debate on scientific explanation itself. In this regard, there are the fundamental works of the authors involved. Furthermore, an important work of Salmon (Salmon 1989) gives an undoubtedly exhaustive description of the overall debate, so as of the various authors' contributions. It is indeed a very important reference - not only on a philosophical/epistemological, but also on a historical level.

What interests us here, because it touches on the topic of the scientific nature of previsions, is instead a secondary debate that developed in the context of the larger debate on explanation: we could call it a spin-off of the main debate. In effect, some authors focused on the symmetry between explanation and prevision (they called "prediction"), as theorized by Hempel. During the years, Hempel revised some of his positions over time. Among these, he revised also his theorization of the relationship between explanation and prevision. In Hempel's early works, there was a perfect identification, a sort of structural identity between explanation and prevision. Some of Hempel's critics considered this symmetry one of the weak points of his theory. In particular, Hempel revised some of his assumptions based on the criticisms of Scheffler (1956) and Scriven (1959), and reconsidered the symmetry as two distinct sub-theses:

a) every adequate explanation is potentially a prediction;
b) every adequate prediction is potentially an explanation.

For better understanding, it is here necessary to remember one of the basic elements of Hempel's theorization: the requirement of adequacy (= the premises must

[8]Here is the structure of the I-S model:
p (G,F) = r
Fi explanans
═══ [r]
Gi explanandum
The double line between explanans and explanandum indicates the non-deductive implication between premises and conclusion; the Gi explanandum is a non-modal sentence (it does not contain expressions such as "probably", "almost certainly", etc.). The value r in square brackets expresses the value of the inductive support, given the probability r that F occurs in G. Classic example, G is recovery, F is an illness, i is Mr. Rossi and r is the probability that he recovers. In effect, there was also a third intermediate model between N-D and S-I: the Deductive-Statistical one. Anyway, it did not represent the focus of the debate.

be true). This requirement has the function of reinforcing the assertions of the structure. However, already in the analysis of sub-thesis a) Hempel noted that the adequacy, although indispensable, was insufficient. In fact, if an explanation indirectly also answered the corresponding epistemic question, this was not valid for prediction. Consequently, there could be laws predicting events that punctually occurred, despite not being able to explain them.

Regarding thesis a), Scriven gave three examples to prove its non-sustainability:

> First example: considering a form of paresis caused by syphilis, and considering the low percentage incidence of individuals who contract syphilis and become ill with paresis, the correct explanation and prediction will be these:
>
> 1) if a person is affected by paresis, it is because he has contracted syphilis;
> 2) if a person has contracted syphilis, he will probably not be affected by paresis.
>
> Second example (inspired by evolutionary biology):
>
> 1) an evolved form can be explained on the basis of the laws of evolution;
> 2) on the same basis, no predictions can be made about how it will evolve.
>
> Third example:
>
> 1) someone commits uxoricide due to jealousy;
> 2) the event could have been foreseen only after it had occurred.

Despite the strength of Scriven's arguments, Hempel maintained the point and considered the first sub-thesis valid. He argued that, in many cases, the fact of not knowing critical explanatory factors could depend on contingent aspects, perhaps due to strictly technical deficiencies, however transitory in the short and long term. He welcomed with interest the methodological aspects related to Scriven's arguments – and considered them when he subsequently reformulated the requirement of maximal specificity. Regarding the b) thesis, Hempel agreed with the criticisms of Sheffler and Scriven - who cited examples based on which it was evident that arguments used for a prediction were not suitable for supporting an explanation. Anyway, the question remained open.

Just as the question of the scientific nature of a prevision remains, if one wishes to harness it in a logical structure that justifies its contents and assumptions. The fascinating story of this wonderful intellectual debate can only teach us that tackling prevision with an approach of this type, albeit very profound and intellectually rich, risks making us lose sight of our objective[9].

[9]The Author preferred not to delve into another aspect discussed in the same years alongside the debate on the explanation. That is, the teleological, or functional, explanation. This approach explains an event through its purpose, therefore placing the explanation in a future event with respect to the time in which the Explanandum is located. Among the theorists who dealt with it, R.B. Braithwaite, who (Braithwaite 1946) drew attention to the character of plasticity and variance of actions (thus explaining the changes

Prevision as an Attribution of Subjective Probability: The Contribution of Bruno de Finetti to Futures Studies

At this point, it is time to introduce Futures Studies into our discussion. Futures Studies are a relatively recent discipline. They were born at the end of World War II, during the Cold War. They were born as a response to the fear of a possible Third World War (Bell 2003, 2004, Facioni 2022). The awareness of the possible extinction of the human race due to the atomic bomb was well present in the mind - and conscience - of the scientific community. The Russell-Einstein manifesto, published on 9 July 1955, is probably the most touching and important document this reflection produced. At the dawn of the discipline, new foresighting techniques were developed in the United States (Dalkey 1969), while the philosophical reflection was developed in the European context (De Jouvenel 1962, Barbieri Masini 1993). However, the natural transdisciplinary vocation of Futures Studies - and the tendency to share experiences by creating an international network – helped overcome this initial "division of tasks" over the years. The credit for the creation of a solid international network of "futurists"[10] goes in particular to two Italian protagonists of the discipline. In particular, to the manager Aurelio Peccei, who was co-founder of the Club of Rome (Facioni & Paura 2022), and, above all, to the sociologist Eleonora Barbieri Masini, soul of the World Futures Studies Federation and theoretician of the discipline (Facioni 2011).

However, Peccei and Barbieri Masini were not the only Italians who gave an international dimension to Futures Studies. A fundamental contribution - and perhaps not yet fully understood in its possible theoretical implications - is that of the mathematician - a very reductive definition, given the vastness of his research interests - Bruno de Finetti. In particular, in this context it is necessary to underline the concept of subjective probability de Finetti developed since the 1930s (de Finetti 1931, 1937). Bruno de Finetti's theorization of subjective probability is very important for Futures Studies, as it touches on the topic of *consensus*, which is the epistemic foundation, the theoretical pivot underlying almost all methods and techniques developed in the Futures Studies context.

A paradoxical aspect is that de Finetti himself considered the focus of his contribution to Futures Studies not so much his theorization as a mathematician (de Finetti 1968), but rather his commitment (including his political commitment) in the energy resources field. In particular, in his tireless and passionate fight against the senseless waste of natural resources (de Finetti 1975, 1976). However, an analysis of his contribution as a mathematician also allows us a critical rereading explanation/

in strategies), and previously Rosenblueth et al. (1943) had expounded a cybernetic theory of behaviour. Last but not least, Nagel (1961). The paradox is that this part of the debate, which more than many others focuses on the future, arises in the context of the discussion on *historical* explanation.

[10]In recent years, the international community has agreed to call "futurists" those (scholars, researchers, theorists, etc.) who deal with Futures Studies. There is no connection with the well-known Italian artistic movement born in the first decades of the twentieth century.

prediction models based on statistical induction[11]. Moreover, it helps to understand how it has no sense to attribute any *a priori* probability to any event/phenomenon.

We must consider the cultural climate in which de Finetti developed his theoretical hypothesis. As Marco Mondadori underlines in his preface to "La logica dell'incerto" (Mondadori in de Finetti 1989, vii-xxiv), in the years de Finetti's theorization of probabilism took the form of a proper *research program,* as meant by Lakatos (1978); as well as it became a new paradigm, taking up Kuhn's (1962) theorization. Incidentally, if we consider what de Finetti wrote in "L'invenzione della verità" – an essay he wrote in 1934, but only published posthumously in 2006 - we could say that Kuhn himself is among the authors anticipated by de Finetti. The years in which de Finetti developed the ideas that would lead him to Probabilism are the same in which Heisenberg published the work where he formulated his uncertainty principle. Gödel published his theorem on incompleteness in 1931. Slightly earlier (1927) Bridgman published his work The Logic of the Modern Physics, a text that marks the birth of scientific operationism, which also found its convinced advocate in de Finetti.

In de Finetti's conception, reasoning in terms of probability is, in fact, equivalent to make prevision. Consequently, the topic of prevision acquires a central role in his theorization, in his paradigm of the probabilistic approach. It is maybe more correct to call de Finetti's theorization a paradigm, rather than a (usually one-way) research program. In fact, a paradigm indicates a basic epistemic conception, which can however guide a wide range of choices, and it is consequently more adequate to the pluralist and subjectivist approach of de Finetti. He always meant probability as subjective. Probability is not a metaphysical entity, an *a priori* essence that pertains to the object of prevision itself. Probability is a hypothesis that individuals make, based on their own personal experience and judgment parameters. Moreover, it can be correct to speak in term of "probabilities", using the plural form, as de Finetti himself did in his treatise (de Finetti 1970). Probabilities are therefore the opinions that individuals form on the possibility of the occurrence (or otherwise) of an event (or of a phenomenon). The more well founded the more flexible, being the formulation of probabilities strictly linked to the state of knowledge acquired on the topic.

Exchangeability (another important concept in de Finetti) is the situation in which the measurement of the actual probability attributed by the subject is given. Here it is necessary to recall some terminological elements of which he himself underlined the importance from the beginning: the distinction between the concepts "event", "fact", and "phenomenon". If by "fact" de Finetti simply meant to indicate something that happens without being the subject of a conjecture (de Jouvenel 1964), bet, or probability calculation, the term "event", in his conception, has meaning only if used in a specific sense, never in a generic sense. In fact, by "event" he meant an assertion such that, by stipulating a bet on it, it then emerges in an indisputable way whether the event is true or false (has occurred or has not occurred) - and therefore whether the bet is won or lost. Events are aspects of a "phenomenon":

[11]Due to a precise choice of the author, in this part no mathematical formulas will be presented, in order to give greater prominence to the more typically theoretical-philosophical aspects of de Finetti's contribution

the two concepts, in de Finetti's theorization, are therefore in a hierarchical relationship. The use of the concept-term phenomenon, in the specific subjectivist meaning, therefore aims to eliminate the ambiguities typical of the collective designations, and underlines the problems linked to the topic of classification in research, so strongly felt in the sociological context (Marradi 1980, 2007, Agodi 2001, Bezzi et al. 2010). There is, according to de Finetti, ambiguity in speaking of "evidence of the same event" as it is implied that such evidence must automatically be considered equally probable, or even independent (in a stochastic sense) from each other.

However, the concept that most of all is important in this work's context is that of *exchangeability*. It is a fundamental notion, which has immediate repercussions on the very concept of forecasting - as well as interesting implications on the level of empirical research – also, perhaps above all, on Futures Studies research on the field. By exchangeability de Finetti meant the equilibrium point on the basis of which an individual is no longer willing to modify the probability he/she attributes to the occurrence (or otherwise) of an event (or of a phenomenon, according to de Finetti's theorization), regardless of the alternatives he/she are facing. In this way, even if the probability is subjective, the formulated data acquires its own scientific solidity, based on consensus. It is not a probability of metaphysical kind. It is instead the result of a collective negotiation, at the end of which the single individuals involved reach their exchangeability, i.e., a probability on which all those who made their personal "bet" agree.

The implication with the practice of field research is evident. Above all, it is clear the implication of de Finetti's theorization in the Futures Studies practice of research[12]. In fact, most of the methods and techniques used in the discipline (Glenn & Gordon 2009) are based on reaching consensus among the group called to respond on a given topic. In the Delphi method of interview this aspect is transparent. The interviewees, who do not know each other, are asked to propose their subjective idea of probability in relation to the research topics of interest. Each interviewee has the opportunity to know the answer (not the identity) of the others - and everyone is invited to revise his/her own evaluation based on the response of all the others. Thus, everyone change his/her initial position during various (almost three) rounds of interviews, and gradually the percentages change, until the community reach a common value, which is understood as the shared value of the prevision. The US Rand Corporation experts (Dalkey & Elmer 1963, Dalkey 1969) developed the Delphi method in the context of Cold War. The method was aimed at avoiding the typical criticalities of group interviews – e.g., the leader effect (Bezzi 2013). However, it is clear, that the result of a Delphi is nothing other than the achievement of the state of exchangeability, i.e. the prevision expressed as a percentage value agreed upon by the community of experts interviewed, exactly as theorized by de Finetti. Delphi has never stopped evolving from a technical point of view (Di Zio & Pacinelli 2011, 2019), still positioning itself as the main method among those dedicated to foresighting[13].

[12]A comprehensive overview of the methods and techniques used in Futures Studies is present on the website of the Millenium Project, an international think tank organized in nodes present almost all over the world (Glenn & Gordon 2009). On: https://www.millennium-project.org/ publications-2/.

[13]It is important to make a clear distinction between forecasting (the traditional way statisticians use for making previsions and scenarios: e.g., the demographic scenarios are forecasting).

Thus, De Finetti's lesson is still not something futurists consider a chapter from the past. It is a contribution still valid and essential today, even in its philosophical implications.

R.K. Merton's Contribution

As we have already argued at length, the reliability of a prevision – or, better, its scientific nature – does not pertain to any model of explanation. However, we could also consider that, thanks to de Finetti, it is possible change the perspective. We can maybe find the prevision's scientificity passing first through a sort of *pars destruens*. First, abandoning the rigid models suggested by the epistemologists of the previously in-depth debate. Then, as de Finetti suggested, abandoning the idea of a metaphysical probability, external to experience. That is, reasoning in terms of possible futures – and consequently attributing different probabilities to the occurrence of an event, creating different scenarios – or reaching a common evaluation, as possible in de Finetti's exchangeability. Thus, those who deal with futures must therefore always remember that probability does not have an absolute value, but is the result of a negotiation: there is no absolute formula, nor an a priori model.

Possible futures are the main research object in Futures Studies, which is therefore the discipline most interested in the scientific nature of previsions. In this thematic junction, however, it is necessary to clear away a possible element of confusion. Futures Studies do not only deal with human and social prevision. However, the field of human and social prevision is the one in which the difficulty of not having an immediate confirmation of one's statements is most felt. However, it is necessary to underline an aspect that may seem paradoxical: guessing a prevision does not constitute the "scientific" essence of the prediction itself. This, whether we are talking about short, long or medium range forecasts. This is due to an aspect that Robert K. Merton highlighted in a historic work of his (Merton 1948): a prevision (he used the term "prophecy") can influence the result of the forecast itself. Therefore, it is possible to have both *self-fulfilling prophecies* (events that would probably never have happened without the prophecy itself) and *suicide-prophecies* (events that probably did not happen because of the prophecy: thus, the prophecy self-disproved). In this case, it is still important to underline terms such as "probably", "possible", etc. Indeed, no one can anyway be sure the result depended from the prophecy. No one can be sure of the chain of causes, and contributory causes, that lead (or not) to the occurrence of any event. Moreover, this is even truer the further in time the foreseen event is.

Foresighting use both quantitative and qualitative approaches aimed to the creation of scenarios. Focusing on methods used in Futures Studies is not the aim of this paper. However, it should be underlined that the quality of the data (both used and produced) is essential for methodological correctness - and therefore for the scientific nature of a prevision. This aspect becomes increasingly important as technological evolution grows. In this regard, it is interesting to read the position of the Italian Society of Statistics about the use of Big Data (SIS 2018).

A prevision is formulated at a given moment, in a given context of knowledge. We can call this state of knowledge the "epistemic sphere". There may be different cases of success or failure of a prediction[14], but we focus on two:

a) the event hypothesized in the forecast occurs, but the causes of the event are different from those formulated by the forecast at the time
b) the event hypothesized in the forecast does not occur, and the failure of the prediction depends on the forecast itself

In neither case can we speak of success. In the case a) the success of the prediction is a coincidence. In case b) we are faced with the verification of Merton's theory, but we are still not able to actually know what caused the failure. However, no one is able - in the two cases considered and in all other possible cases - to establish a certain cause-effect connection. This may depend on an almost infinite variety of possible causes. Certainly, an important aspect is that the moment of a prevision's formulation is necessarily different from the moment in which the event is supposed to occur. Just as the temporal context between the moment of formulation and that of verification/disconfirmation of the prevision is different. We face different epistemic spheres, different levels of knowledge, different development of technologies, etc. Thus, no one can predict what will happen with certainty. The future cannot be predicted - possible developments can be guessed at, and this is the best that can be done, especially in the field of human and social prevision. As with the rising of the sun in Hume's theory, human beings have a thousand-year experience of it in the past, and this experience tells them that it will very likely be like this tomorrow too. Nevertheless, there is no certainty in the future. Thus, the actual success of a prevision cannot be the right criterion to determine the scientific nature of the prevision itself. So, where can we search the scientificity of Futures Studies, and in particular the scientificity of human and social prevision?

Conclusion – How a Very Ancient Word Can Help: Μέθοδος

It is necessary to shift the center of attention from the "what" to the "how", if futurists want to have an acceptable scientific criterion. In their looking for a valid criterion that defines fieldwork as scientific, futurists, inevitably, find themselves facing exactly the same kind of difficulties as their colleagues engaged in the more traditional social sciences. The goodness, reliability and scientific nature of a social prevision have to face exactly the same problems of the more traditional Social Sciences, from an epistemological point of view. There are not scientific laws of indisputable validity to rely on. The social research can influence the results, and so is for previsions. There are not solid models of explanation; at most medium-range explanations, based on reasonably well founded knowledge (Campelli 2004).

In the absence of other elements, we need to look at a concept that comes from far away in time. That is: method, how a research is built step by step. The Greek work μέθοδος is composed by two elements: μετά (=through) and ὁδός (=path).

[14]The use of the term "prediction" respects, as the use of "prophecy", the terms used by the cited Author.

Thus, the word indicates the path one crosses to get somewhere. In the scientific context, it indicates every single step in the research work: from the initial conceptualization to the reflection on the results. Having to face the same problems as the human sciences at work, the scientific credibility of Futures Studies may probably consist in the methodological strictness. Indeed, setting the discussion on the correctness of all the steps followed during the research work probably puts futures studies on the same level not only as the social sciences, but as all the sciences as a whole. At the basis of this assert, which may seem reckless, there are some usually overlooked elements. As for any other researchers, the futurists must have a solid wealth of knowledge on the topics they want to address. As for any other researchers, the futurists must conduct their surveys in a correct way at every single step. It means not being superficial in analysing data. It means that scrupulousness, rigor in carrying out procedures, must be a guiding criterion for both the (also social) scientists and the futurists. The evolution of new technologies is not simplifying, but complicating the overall picture. This is a 360-degree challenge, which affects both the level strictly linked to the development of new methods and techniques (i.e., data collection, data control), but also in the broader ethical horizon that AI is opening up (Paura 2022). Dealing with the range of existing methodological techniques of controlling the research quality, in a single paper, is an impossible-to-reach aim. However, not even the in-depth analysis of a single evaluation technique was within the objectives of this paper. This work presented itself as a purely theoretical reflection on a topic that is not very well known, but still important. For Future Studies, the only hope of scientificity is in *how* their research is made step by step. The scientific nature of a prevision lies in *how* the prevision is developed. This, with a further degree of difficulty for the futurists. In effect, social scientists are aware they can influence their research's own results – for example, changing in some way the society they are studying. Anyway, for social scientists this is an only eventual side effect of their work. For futurists, changing the society *is* the work. I am referring, in particular, to that part of Futures Studies called anticipation. Consensus on decisions taken collectively - one of the objectives of anticipatory work – can be useful to avoid (not eliminate) possible problems in the future due to incorrect decisions made in the present. The weight of this responsibility, which the philosopher Heller defines as enormous (Heller 1988, Poli 2011), cannot fail to play a role in the futurists' fieldwork. Scientificity, in Futures Studies, is characterized by an ethical weight (Poli 2019) that other sciences certainly do not know.

References

Agodi MC (2001) Pensare/classificare: appunti sugli obiettivi della classificazione nelle scienze sociali. (Thinking/Classifying: Notes on the Goals of Classification in the Social Sciences). In *Sociologia e ricerca sociale* no. 64. Milano: Franco Angeli.

Aristotle. Ὄργανον (IV Century B.C) consulted translation 2000. Einaudi Milano.

Bacon F (1620) *Novum Organum, sive indicia vera de interpretatione naturae*. (Novum Organum, is a true indication of natural interpretation). Available at: https://www.thelatinlibrary.com/bacon/.

Barbieri Masini E (1986) *La previsione umana e sociale*. (Human and social prediction).

Roma: Editrice Pontificia Università Gregoriana.
Barbieri Masini E (1993) *Why futures studies?* London: Grey Seal Books.
Bell W (2003) *Foundations of futures studies. History, purposes, and knowledge*. New Brunswick (USA) and London (UK): Transaction Publishers.
Bell W (2004) *Foundations of futures studies. Values, objectivity, and the good society*. New Brunswick (USA) and London (UK): Transaction Publishers.
Bezzi C (2013) *Fare ricerca con i gruppi. Guida all'utilizzo di focus group, brainstorming, Delphi e altre tecniche*. (Researching with Groups: A Guide to Using Focus Groups, Brainstorming, Delphi, and Other Techniques). Milano: Franco Angeli.
Bezzi C, Cannavò L, Palumbo M (2010) *Costruire e usare indicatori nella ricerca sociale e nella valutazione*. (Building and using indicators in social research and evaluation). Milano: Franco Angeli.
Braithwaite RB (1946) Teleological Explanation: The Presidential Address. In *Proceedings of the Aristotelian Society New Series* 47(1946-1947): i–xx. Oxford University Press.
Bridgman P (1927) *The Logic of Modern Physics*. New York: The Macmillan Company.
Campelli E (1994) *Il metodo e il suo contrario. Sul recupero della problematica del metodo in sociologia*. (The method and its opposite. On the recovery of the problematic of the method in sociology). Milano: Franco Angeli.
Campelli E (2004) *Per una spiegazione di medio raggio. Ancora sul problema della spiegazione*. (For a medium-range explanation. Still on the problem of explanation). Sociologia e Ricerca Sociale, XXVII, 75. Milano: Franco Angeli.
Dalkey NC (1969) *The Delphi method: an experimental study of group opinion*. Santa Monica (California): Rand Corporation.
Dalkey NC, Helmer O (1963) An Experimental Application of the Delphi Method to the Use of Experts. *Management Science* 9: 458–467.
de Finetti B. *Probabilismo. Saggio critico sulla teoria delle probabilità e il valore della scienza*. (Probabilism: A Critical Essay on the Theory of Probability and the Value of Science). Napoli, Perrella, 1931; in De Finetti (a cura di M. Mondadori), *La logica dell'incerto*, Milano, Il Saggiatore, 1989.
de Finetti B. *La prévision: ses lois logiques, ses sources subjectives*. (The prediction: its logical logic, its subjective sources). Paris, Annales de l'Institut Poincaré (vol. 7, Fascicolo I), 1937; in De Finetti (a cura di M. Mondadori), *La logica dell'incerto*. Milano, Il Saggiatore, 1989.
de Finetti B (1968) *Il previsiometro : presentazione e spiegazione in forma elementare*. (The forecaster: presentation and explanation in basic form). Studi di Mercato n.4, October 1968.
de Finetti B (1970) *Teoria delle probabilità. Sintesi introduttiva con appendice critica*. (Probability Theory. Introductory Summary with Critical Appendix). Torino: Einaudi.
de Finetti B (Ed.) (1975) *Crisi dell'energia e crisi di miopia.* (Energy crisis and shortsightedness crisis). Milan: Franco Angeli.
de Finetti B (Ed.) (1976) *Dall'utopia all'alternativa* (1971-1976). (From Utopia to Alternative (1971-1976)). Milan: Franco Angeli.
de Finetti B (1989) *La logica dell'incerto*. (The logic of uncertainty). Edited by M Mondadori. Milano: Il Saggiatore.
de Finetti B (2006) *L'invenzione della verità*. (The invention of truth). Milano: Raffaello Cortina Editore.
De Jouvenel B (1964) *L'art de la conjecture*. (The Art of Guessing). Futuribles, Monaco: Éditions du Rocher.
Di Zio S, Pacinelli A (2011) Opinion convergence in location: A spatial version of the Delphi method. *Technological Forecasting and Social Change* 78(9): 1565–1578.
Di Zio S, Pacinelli A (2019) *Contributi alla costruzione di scenari su criminalità e terrorismo*.

(Contributions to the construction of scenarios on crime and terrorism). Milan: FrancoAngeli.
Facioni C (2011) *Il contributo italiano ai Futures Studies*. (The Italian contribution to Futures Studies). PhD Thesis. Available at: https://iris.Uniroma1.it/retrieve/handle/11 573/918 519/324213/tesi_dottorato_Carolina_Facioni.pdf.
Facioni C (2022) Why the World Needs Futures Studies: A Social and Methodological Challenge. *Athens Journal of Mediterranean Studies* 8(4): 233–246.
Facioni C, Paura R (2022) Re-discovering Aurelio Peccei's contribution to futures studies. *European Journal of Futures Research* 10(Apr): 9.
Fasanella A (1993) *Concettualizzazione e spiegazione sociologica – Il modello nomologico-inferenziale e la sua applicabilità alle scienze sociali.* (Conceptualization and sociological explanation – The nomological-inferential model and its applicability to the social sciences). Milano: Franco Angeli.
Galavotti MC (1984) *Spiegazioni probabilistiche: un dibattito aperto*. (Probabilistic explanations: an open debate). Bologna: Cooperativa Libraria Universitaria Editrice.
Glenn J, Gordon T (Ed.) (2009) *Futures Research Methodology – 3.0 version*. Available at: https://www.millennium-project.org/publications-2/futures-research-methodology-version-3-0/.
Heller A (1988) *General ethics*. London: Blackwell Pub.
Hempel CG (1942) The function of general laws in history. *Journal of Philosophy* 39(2): 35–48.
Hempel CG (1962) *Deductive-Nomological versus Statistical Explanation*. Minneapolis: University of Minnesota Press.
Hempel CG (1965) *Aspects of the scientific explanation*. New York: The Free Press.
Hempel CG (1977) *Nachwort 1976: Neuere Ideen zu den Problemen der statistischen.* (Afterword 1976: Newer ideas on the problems of statistical). *Erklärung* in Aspekte wissenschaftlicher Erklärung, 1977. Berlin: Walter de Gruyter.
Hempel CG, Oppenheim P (1948) Studies in the logic of explanation. *Philosophy of Science* 15(2): 135–175.
Hume D (1748) *An Enquiry Concerning Human Understanding*. Edited by Stephen Buckle (2007). Cambridge University Press.
Italian Society of Statistics – SIS (2018) *Statement della SIS su Statistica, scienza dei dati e Big Data*. Available at: https://sis-statistica.it/ita/9943/.
Kuhn TS (1962) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
Lakatos I (1978) *The Methodology of Scientific Research Programmes*. Cambridge: Cambridge University Press.
Marradi A (1980) *Concetti e metodi per la ricerca sociale*. (Concepts and methods for social research). Firenze: La Giuntina.
Marradi A (2007) *Metodologia delle scienze sociali*. (Social science methodology). Bologna: Il Mulino.
Merton RK (1948) The Self-Fulfilling Prophecy. *The Antioch Review* 8(2): 193–210.
Mill JS (1848) *A System of Logic: Ratiocinative and Inductive*, ed.2012. Cambridge University Press.
Nagel E (1961) *The Structure of Science: Problems in the Logic of Scientific Explanation.* New York: Harcour, Brace & World.
Paura R (2022), *Occupare il futuro. Prevedere, anticipare e trasformare il mondo di domani*, Torino: Codice Edizioni
Poli R (2011) Ethics and futures studies. *International Journal of Management Concepts and Philosophy* 5(4).
Poli R (2019) *Lavorare con il futuro. Idee e strumenti per dominare l'incertezza*. (Working with the future. Ideas and tools to overcome uncertainty). Milano: Egea.
Rosenblueth A, Wiener N, Bigelow J (1943) Behavior, purpose and teleology. *Philosophy*

of Science 10: 18–24.
Rossi P (Ed.) (1973) *La rivoluzione scientifica da Copernico a Newton*. (The Scientific Revolution from Copernicus to Newton). Torino: Loescher.
Russell B, Einstein A (1955) *The Russell-Einstein manifesto*. Available at: https://websites. umich. edu/~pugwash/Manifesto.html.
Salmon WC (1989) *Four decades of scientific explanation*. Regents of University of Minnesota.
Scheffler I (1956) Explanation, Prediction, and Abstraction. *British Journal for the Philosophy of Science British Journal for the Philosophy of Science* 7(28): 293–309.
Scriven M (1959) Explanation and prediction in evolutionary theory: Satisfactory explanation of the past is possible even when prediction of the future is impossible. *Science* 130(3374): 477–482.