PROSPECTIVE FRUITFULNESS AS A CRITERION FOR THEORY-CHANGE AND RESEARCH-STRATEGY OPTION

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Abstract

This paper aims at emphasizing that «prospective fruitfulness» (i.e., the general ability of a theoretical approach to disclose future promising perspectives for the understanding of nature) could serve as one of the epistemological criteria for evaluating a theory and for orienting scientists' choice among theoretical alternatives. I will first try to show how some twentieth-century developments in philosophy of science may (at least implicitly) hint at such a notion. Then, I will argue that a non-naïve realistic understanding of science centred on the interaction with nature and on the tendency toward truth in the long run, confers further epistemological relevance to prospective fruitfulness. A subsequent section will be dedicated to a current development in cognitive neurosciences (the so-called «neural re-use»), which may seemingly exemplify prospective fruitfulness and its roles in scientific research. Then, I will summarize some key aspects of prospective fruitfulness. Finally, the connections of this notion with the theme of philosophy of nature will be briefly addressed.

Keywords. Prospective fruitfulness, theory change, research strategy, neural re-use.

It is named heuristic because it anticipates insights of that type and,
while prescinding from their as yet unknown contents,
works out their general properties to give methodical guidance to investigations.

A heuristic notion, then, is the notion of an unknown content and it is determined by
anticipating the type of act through which the unknown would become known.
(B. Lonergan, 1957, pp. 45 and 392)

I hope I have shown that instant rationality –and instant learning– fail.

Mature science –unlike pedestrian trial-and-error– has «heuristic power»
(I. Lakatos, 1970, pp. 87 and 88)

1. Introduction

In the course of the twentieth century, philosophy of science has seen several different, sometimes opposing proposals, all aimed, in a way or another, to understand what science is, which are its relations with reality, how it proceeds and is actually practiced. A noteworthy happening in the philosophy of science of the last century has been the so-called «historical turn».[1] With this, it is usually meant that historical considerations became central to the philosophy of science, and that philosophers have begun to look at the history of science as a fundamental «source of data» for their attempts at understanding science. I think that, in general, such a turn has been beneficial: no serious epistemological inquiry on the natural sciences can overlook their historical, diachronic dynamics, and how they have been furthered in the recent or remote past.

Three philosophers are usually considered as the protagonists of the historical turn: Thomas S. Kuhn, Imre Lakatos, and Paul K. Feyerabend. They surely offered key contributions in this direction, especially in that they *explicitly* underscored and put at work the historical dimension in epistemology. However, their work is barely understandable without Karl R. Popper's path-breaking falsificationism. Although not directly concerned with the historical dimension, Popper's philosophy of science clearly suggests that science is characterized by error-correction dynamics that are unconceivable in a synchronic, a-historical stance. Not by chance, the conceptions championed by Kuhn, Lakatos and Feyerabend have all been developed in some «tension» with falsificationism.

Furthermore, I am convinced that other, previous authors offer notable insights in the relevance of diachronic thinking in philosophy of science. Both Pierre Duhem's conception of scientific theories tending to become «natural classifications»,[2] and Charles S. Peirce's view that the «human opinion tends in the long run to a definite form, which is the truth»,[3] reveal that science should be understood in its historical progression.

The historical turn in philosophy of science has imposed a fact to the attention of scholars: along the history, theories and hypothesis have been formulated and rejected, new theories have been built upon older ones or in overt opposition to previous systems, some theories have converged on more unified views and others have progressively diverged. In a word, the history of science has brought to central stage the problem of «theory-change».[4]

[1] A. Bird, «The historical turn in the philosophy of science», in Psillos S., Curd M. (eds.), *The Routledge Companion to the Philosophy of Science*, London and New York: Routledge 2008: pp. 67-77.

[2] P. Duhem, *The Aim and Structure of Physical Science*, translated by P. H. Wiener, Princeton University Press: Princeton (NJ) 1991 (French original edition published in 1908).

[3] C.S. Peirce, *The Essential Peirce*, N. Houser, C.J.W. Kloesel, (eds.), Peirce Project Edition: Indiana 1998: vol. 1, p. 89.

[4] J. Worrall, «Theory-Change in Science», in Psillos S., Curd M. (eds.), *The Routledge Companion to the Philosophy of Science*, *op. cit.*: pp. 281-291.

Now, theory-change implies theory-choice –i.e. how scientists, research groups, and academic communities come to focus on a theory as the best way of dealing with a more or less well-defined cluster of empirical facts, phenomena, processes, and entities. Choosing a theory among possible alternatives requires *criteria* –as any kind of choice does.

This article aims at suggesting that possible criteria are to be neither necessarily present-oriented nor necessarily past-oriented; they may also be future-oriented. Present-oriented criteria concern what a theory accomplishes (or is believed to accomplish) at a certain time. Generally, they are not bound to the particular epistemological view of scientific theories that one may have. For the anti-realist or the instrumentalist, empirical adequacy or the capacity to «save the phenomena» may function as a criterion, as well as coincidence with reality may serve the purpose for the realists, or utility for the pragmatists. Past-oriented criteria may be conceived as considering the gains of a theory with respect to a previous one. Abstractly, they may be comparative diachronic versions of the present-oriented ones. Thus, a theory may be considered as closer to reality, more empirically adequate, or more practically useful than a previous one. However, Kuhn and Lakatos, in my opinion, provide finer insights in what such past-oriented criteria could be. I am here thinking to Kuhn's notion that a paradigm should be able to solve or overcome the anomalies affecting the previous one,[5] or to Lakatos' conception that a progressive research program may replace a regressive one.[6] In the following, I will argue that also future-oriented criteria actually play a role in theory-change and theory-choice; my claim in this paper is that theories (as well as specific hypotheses, or wide theoretical approaches) are not only assessed on the basis of their current success, or of their merits over previous ones, but also because of their *promise*, their «prospective fruitfulness» for the research to come and in view of future achievements.

To be clear, I am not claiming that future-oriented criteria in general, or prospective fruitfulness in particular, should be the only criteria that count. My aim is just of proposing that they might be worth of greater attention than it has been done in the past.

[5] T.S. Kuhn, *The Structure of Scientific Revolutions*, The University of Chicago Press: Chicago and London 1996 (3rd edition; First edition published in 1962), p. 97. See also G.E. Jones, «Kuhn, Popper and theory comparison». *Dialectica* 35, 1981, pp. 389-397; here especially p. 392.

[6] I. Lakatos, «Falsification and the methodology of scientific research programmes», in I. Lakatos and A. Musgrave, (eds.), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge (UK), 1970. Reprinetd in I. Lakatos, *Philosphical Papers Volume 1*, J. Worral and G. Curries (eds.), Cambridge University Press, Cambridge (UK), 1978, pp. 8-101. I. Lakatos, «History of science and its rational reconstructions» in R.C. Buck and R.S. Cohen (eds.), *P. S. A. 1970 Boston Studies in the Philosophy of Science* 8, 1971, pp. 91-135. Reprinted in I. Lakatos, *Philosphical Papers Volume 1*, *op. cit.*, pp. 102-138.

2. Crucial experiments

I would start up with considering one of the oldest tenets concerning possible procedures for choosing among alternative competing theories: F. Bacon's *experimentum crucis*.[7] The notion of «crucial experiment» applies to cases in which there are two (or more) contemporary alternative theories or hypotheses aimed at accounting for the same phenomenon (or, more generally, for two at least partially overlapping domains of natural phenomena). More precisely, a crucial experiment may be designed and executed when two contemporary theories provide different predictions about the same phenomenon (namely, the same experimentally realizable situation concerning a particular phenomenon). In this case, it is possible to run the experiment and ascertain whether the results agree with the prediction of one or the other theory or hypothesis.

As clear as it is, the notion of crucial experiment has ignited several debates among philosophers of science and has undergone a number of criticisms. Perhaps the most famous one is that expounded by Pierre Duhem. Referring to the well-known experiment by J. B. Foucault with which he measured the speed of light in different media demonstrating that it decreases with the density of the medium, thus disproving one of the predictions of Newton's emissive optics in favour of the wave theory of light, Duhem asked: «Does it follows that we can find in the «crucial experiment» an irrefutable procedure for transforming one of the two hypotheses before us into a demonstrated truth?».[8] His answer was loudly negative. His argument proceeds more or less as it follows. For a crucial experiment to establish the absolute truth of a hypothesis by disproving its alternative, it is needed that such an alternative be the *only possible* one. In other words, the empirical confutation of a theory would unequivocally prove the truth of its rival theory only under the condition that the latter is the unique possible alternative. Subsequent history showed that it was not the case of emissive and wave theory of light: Maxwell's electromagnetism and quantum optics readily provide other two possible alternatives. From a logical point of view, that two scientific theories or hypothesis are the only possible alternatives is never the case, and this undermines the feasibility of crucial experiments in Duhem's view.

Duhem's argument is linked with his famous thesis, the «Duhem-thesis».[9] The thesis maintains that it is never possible to test by experiments a single hypothesis (let us call it the «core hypothesis») because no isolated hypothesis, by its own, is able to imply empirical predictions. For deriving a prediction that may be experimentally trialled, several other assumptions are usually needed, like: initial and boundary conditions,

[7] F. Bacon, *Novum Organum*, in J, Spedding, R. Ellis, and D. Heath, (eds.), *The Works of Francis Bacon*, Frommann-Holzboog: Stuttgart 1961-63.

[8] P. Duhem, *The Aim and Structure of Physical Science*, *op. cit.*, p. 189.

[9] Ibíd., pp. 183-188.

values of constants, auxiliary hypotheses, theories of the apparatuses employed in the experiment, approximation methods, etc. In this situation, an experimental confutation of an empirical prediction may at best tell us that there is at least one error in the set of assumptions, but would never be able to point out where the error lies. From this, it follows that the logical possibility of «saving» the hypothesis by modifying some other elements of the cluster of assumptions is always left open.[10] Under this light, I would note, the «core hypothesis» as such has no special position within the entire cluster of propositions that are required to derive an empirically testable prediction, so that we may accommodate the problematic empirical results also by modifying the core hypothesis. This procedure will generate further alternative hypotheses, thus supporting Duhem's criticism to crucial experiments.

All this opens up two issues that are relevant for my argument. First, the fact that it is always *logically possible* to save a hypothesis from an empirical confutation does not imply that doing so is always *epistemologically suitable and convenient*. This point will be addressed in the next section.

Secondly, in spite of Duhem's criticisms, which are logically well grounded, crucial experiments in science have been executed (before and after Duhem), and they have indeed played «crucial» roles in the furthering of many scientific disciplines. Does this imply the science is somehow «*irrational*» or illogical? I do not think so, and part of the answer may lie in the ascertainment of what crucial experiments are (or should be) meant for. Duhem asked whether a crucial experiment might transform one of two competing theories in a *demonstrated truth*. He is right in maintaining that it cannot. But perhaps, this is not the goal of crucial experiments.

To take again the competition between emissive and wave optics in the nineteenth century, before the abovementioned experiment by Foucault, another clear-cut experiment had already been performed.[11] The optical phenomenon that more than others troubled Newton's theory was the diffraction. Diffraction occurs when light passes through narrow slits or around thin obstacles. When it passes around thin obstacles, for example, the shadow produced on a screen is not neat but surrounded by alternating darkened and lightened fringes. According to Newton's optics, such phenomenon should have been explained by the «inflexions» that the light corpuscles were supposed to undergo when travelling in the vicinity of the obstacle's edges (and they were conceived as depending on the gravitational attraction between the light particles and the

[10] «Naïve falsificationists solved this problem by relegating –in crucial contexts– the auxiliary hypotheses to the realm of the unproblematic background knowledge, eliminating them from the deductive model of the test-situation and thereby *forcing* the chosen theory into logical isolation, in which it becomes a sitting target for the attacks of test-experiments»: I. Lakatos, «Falsification and the methodology of scientific research programmes», *op. cit.*, p. 32.

[11] I. Colagè, *Interazione e inferenza. Epistemologia scientifica ispirata al pensiero di C.S. Peirce*, Gregorian & Biblical Press: Rome 2010, pp. 70-78.

matter of the obstacle). A. Fresnel, building on previous insights by T. Young, proposed instead that the fringes were due to the *interference* of the light waves passing at the two sides of the obstacle (thus assuming that light is a wave and not a beam of particles). Now, if the explanation in terms of interference is correct, this implies that if the light passing at *one* side of the obstacle is blocked, then the diffraction fringes should disappear on *both* sides of the diffraction figure (as no interference could be produced anymore). In contrast, emissive theory of light would have predicted the disappearance of the fringes only on the occluded side. The results agree with Fresnel predictions. This may be considered as a crucial experiment, and as such would be subject to Duhem criticism. However, it played a major role in the affirmation of the wave theory of light over the emissive one: Fresnel's *memoire* (submitted in 1817) was awarded by the *Académie des Sciences* in 1819, and captured the attention of one of the most influential physicists of the time, namely Arago. The payoff of Fresnel's work was not that of «transforming the wave theory of light in a demonstrated truth» –it could not have been this, as subsequent history shows. It was, rather, that of beginning to incline the physicists' community to believe that the wave theory could have been a more promising path towards a better understanding of optics.

Foucault's measures of the speed of light in different media came more than thirty years later Fresnel's *memoire*. Now, none of these two experiments has been able of decreeing the «absolute truth» of the wave theory, but each one contributed its part in convincing physicists to «bet on it» in view of its future promise.

As I have said in the introduction, I do not claim that future-oriented criteria should be taken as exclusive. As a matter of fact, in the affirmation of the wave theory over the emissive one, both past-oriented criteria (e.g. the fact that Newton's theory was never fully able to come to terms with diffraction over more than a century) and present-oriented criteria (i.e. the greater empirical adequacy that the wave theory soon displayed, and its capacity to predict correctly a number of phenomena that were not yet been observed in 1817) likely played a role. My point is, more modestly, that the *promise* that a theory may have for future advances should not be entirely overlooked as a criterion often influential (although sometimes only implicitly) in choosing a theory over alternatives. And under this light, crucial experiments assume relevance in particular scientific vicissitudes precisely to the extent to which they provide indications as to such a promise, or prospective fruitfulness.

3. Saving theories and *ad hoc* hypotheses

Popper's falsificationism condemns any «conservative» attitude in science. It insisted on the asymmetry between verification and falsification: whereas a theory can never be irrefutably confirmed, no matter how large is the set of evidence agreeing with its predictions, a single counter-evidence to a theory would require its immediate abandonment.

Thus, in Popper's view a scientific discipline should proceed by eliminating theories as quickly as possible, as «we test for truth, by eliminating falsehood».[12] This is a simplistic sketch of Popper's positions, but helps in understanding the strength with which they (re-)emphasized the *fallibilism* of science.[13] The doctrine of falsificationism has long been at the centre of the debates in philosophy of science and it still possesses considerable authority: falsifiability (i.e., the *possibility* of a theory to be falsified) provides a clear demarcation between scientific systems and ideological, absolutistic, or empty positions.

From a logical point of view, falsificationism (if taken in its most radical, naïve version) may be subject to Duhemian criticisms, much along the lines considered in the previous section. It may also meet with internal difficulties. For example, according to falsificationism, a theory is falsified when at least one of its potential falsifiers occurs. A potential falsifier is a possible state of affair that, if actual, would contradict a prediction of the theory. At this point, Popper says that for effectively falsifying a theory a single isolated counter-instance does not suffice; on the contrary, we need to accept a «low-level empirical hypothesis» that cannot be deduced from the theory.[14] However, accepting a hypothesis, even if a low-level one, cannot mean within Popper's philosophy that it is verified beyond any doubt. This puts in some troubles the attempts at explaining the abandonment of a theory as irremediably false in terms of the *sole* falsificationism.[15]

Moreover, a closer look at the history and actual practice of science reveals that scientists are not Popperian:[16] namely, scientists usually do not give up working with a theory as soon as it encounters some difficulties in accounting for empirical data. This is one of the central tenets of the proposals developed by Kuhn and Lakatos. According to Kuhn, in the periods of «normal science», the scientists working within a paradigm consider many of the inconsistencies between the predictions of the paradigm and the available data as «puzzles» to be solved with further work, and not as confutations; in such periods, many other such inconsistencies do not worry the scientists at all (they are often entirely ignored). Kuhn's thought will be addressed more extensively in the next section.

Imre Lakatos' «methodology of scientific research programmes» may also be understood as an attempt at overcoming some of the difficulties of Popper's approach (and at further refining Kuhn's proposal). In the views of Lakatos,[17] the history of a scien-

[12] K.R. Popper, *Objective Knowledge. An Evolutionary Approach*, Claredon Press: Oxford (UK) 1979, p. 30.

[13] G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 36-43.

[14] K.R. Popper, *The Logic of Scientific Discovery.* Hutchinson: London 1959 [1980], pp. 86-87, (§ 22).

[15] I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 28-29.

[16] Ibíd., pp. 31-32.

[17] I. Lakatos, «History of science and its rational reconstructions», *op. cit.*

tific discipline may be «rationally reconstructed» in terms of a (not-strictly-linear) sequence of research programmes. A research programme is defined by a «hard-core» and a «protective belt». The hard-core gathers all the elements that characterize a specific scientific approach in a definite research domain; such elements are considered irrefutable within the research programme by virtue of a methodological decision. The protective belt consists of a set of partially expressed indications on where and how modifying the particular models and theories developed within the programme with the aim of increasing the agreement with the empirical data without changing or abandoning the elements of the hard-core.[18] In this way, a research programme is constituted by a series of *refutable variants*, all sharing the hard-core.[19] Within a single scientific discipline, different research programmes may follow one another.[20] Lakatos establishes criteria for determining when a research programme replaces another. A research programme is eliminated and replaced when two conditions hold: (i) it has to be in regressive phase and (ii) there must exist another research programme in the concerned discipline that is in progressive phase.[21] A programme is in regressive phase when its theoretical developments merely follow the empirical ones or, in other words, when it can only provide *post hoc* explanations for the empirical findings that it encounters. A programme is instead progressive when its theoretical developments anticipate empirical findings or, in other words, when it is capable of correctly predicting facts or phenomena that have not yet been discovered.[22] Thus, formulating *post hoc* hypothesis is a sure mark, in Lakatos' view, that a programme is in regressive phase.

However (and here we reach the most relevant point for my argument), Lakatos also maintains that a programme enters a regressive phase even when it formulates *ad hoc* hypothesis; and he distinguishes three kinds of such hypotheses.[23] The first kind is constituted of hypotheses that have no additional content with respect to previous ones; the second kind gathers hypotheses that do have additional content, but none of this is

[18] I. Lakatos, «Falsification and the methodology of scientific research programmes», *op. cit.*, pp. 47-52.

[19] Ptolemaic astronomy may be interpreted as a research programme. The hard-core is here constituted by the prescription of reproducing and predicting the movements of the planets, with all their irregularities, by using only combinations of circular and uniform motions: this would be the hard-core of the Ptolemaic research programme. Along almost fifteen centuries, several systems have been developed in order to account for the planets' motions with increasing precision. Each of such systems may be regarded as a refutable variant of the research programme characterized by the mentioned hard-core.

[20] For example, Ptolemaic astronomy was replaced by gravitational astronomy after the Copernican revolution.

[21] I. Lakatos, «History of science and its rational reconstructions», *op. cit.*, pp. 110-113.

[22] Ptolemaic astronomy was in an enduring regressive phase (as it was unable to solve the problems posed by some «stubborn» irregularities in the plants' motions) and it was replaced by the Copernican heliocentric system, which experienced a marked progressive phase thanks to the work of Copernicus, Kepler, Galilei, and others.

[23] I. Lakatos, «History of science and its rational reconstructions», *op. cit.*, pp. 112, footnote n. 2. See also I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 49-53.

corroborated by empirical evidence; the third kind includes hypotheses that are not an integral part of the research programme, that is, that are incoherent with the hard-core of the research programme although are not *ad hoc* in any of the previous senses. The identification of these kinds of *ad hoc* hypotheses not only is of use in indicating that a programme is in regressive phase; it also clarifies what cannot be done in the refutable variants within a research programme. In other words, any research programme should avoid availing of *ad hoc* hypotheses for saving the hard-core. The question is: why?

Lakatos' methodology is motivated by *historical* considerations to a large extent. The analysis of a number of concrete historical cases shows that several research programmes (or what could be interpreted as research programmes) that have been eliminated at a certain time were indeed formulating *post hoc* or *ad hoc* hypothesis for a certain period. From an *epistemological* viewpoint, one could think that a programme formulating *ad hoc* hypothesis is to be considered regressive because *ad hoc* hypothesis are «bad in themselves». However, I think that an additional reason for avoiding *ad hoc* hypothesis is that they wear out the programme's potential capability of accounting for *future* findings in a suitable and coherent manner; in a word, the adoption of *ad hoc* hypotheses wears out the prospective fruitfulness of the research programme that is adopting them. Employing *ad hoc* hypothesis of the first kind (those with no additional content) actually dilutes the tenets of the research programme. Resorting to *ad hoc* hypothesis of the second kind (those with uncorroborated additional content) unsticks the programme from the empirical domain to which it is (or should be) addressed. *Ad hoc* hypotheses of the third kind threaten the internal coherence and compactness of the programme. It seems to me that all these drawbacks of recurring to *ad hoc* hypotheses acquire particular relevance if considered in view of what a research programme is intended or expected to do in the future, and not only in relation to what it has been doing in the past or is currently doing. In my view, it is exactly this consideration that (as mentioned in the previous section) renders the possibility of saving a theory from counter-instances, although always logically open, not always epistemologically suitable and convenient.

4. Reliance on paradigms and «faith» in novelties

Thomas Kuhn's *The Structure of Scientific Revolutions* is another major event in the philosophy of science of the last century. Several elements of Kuhn's philosophy have brought freshness and vivid debates in «post-Popperian» epistemology. A complete account of them is not feasible here; however, there are elements that are relevant for pushing forward my argument, and I will focus on them.

One of the core tenets of what have been often called Kuhn's «historical epistemology» has been the proposal of regarding the history of science and of particular disciplines as constituted of cycles of two alternating, radically different periods: periods of «normal science» and periods of «extraordinary science» follow one another. Normal science is characterized by the steady reliance that the majority of scientists working in a discipline put in the current paradigm. Kuhnian notion of «paradigm» is a complex one and underwent some shift in meaning even during Kuhn's own production.[24] In elementary terms, a paradigm may be considered as the «intellectual environment» in which the scientists of a certain epoch are educated and perform their professional endeavours. A paradigm identifies a number of empirical facts or phenomena and of theoretical models or explanations that are considered of particular relevance: they are paradigmatic in the sense that they specify a set of relevant scientific *problems and solutions*. The main task of the scientists working within a paradigm is that of refining more and more the understanding of those facts, phenomena, models, and explanations, and of extending the applicability of the paradigmatic solutions to a larger and larger domain of natural reality. In this sense, the paradigm guides the activity of the scientists involved in a certain discipline at a certain time.

A paradigm is never completely exempt from difficulties and conundrums, neither when it is already a well-established and solid one. As mentioned in the previous section, the scientists working within a paradigm face a number of inconsistencies between the kinds of theoretical models and explanations defined by the paradigm, on the one hand, and certain facts and phenomena that resist those kinds of models and explanations, on the other. Kuhn names such facts and phenomena as «puzzles», i.e. problems to be resolved with patient and smart work thanks to the progressive development and articulation of the resources offered by the paradigm. This means that the scientists are convinced that the paradigm implicitly has all the resources to solve, soon or later, such puzzles. In spite of the debates surrounding Kuhn's epistemology, I think that this point captures a real aspect of the scientist's work.

This point is relevant not only in its capacity to mitigate the mentioned radical implication of falsificationism, according to which the scientists should not try to maintain any hypothesis or theory in face of counter-evidences. It is also relevant for my argument, in that it shows that scientists rely on the paradigm not only because it has proven able to overcome problems affecting previous approaches (this point will be reconsidered in a moment), or because of its current ability to deal with a number of empirical situations, but also because it is considered in its promise for future developments. Therefore, I propose that scientists put their reliance in a paradigm also because it is considered prospectively fruitful for further advances.

During normal science, as we have seen, the scientists work for expanding the applicability of the paradigm: they try to apply the paradigm's resources to new facts and do-

[24] T.S. Kuhn, «Postscript-1969», in *The Structure of Scientific Revolutions*, *cit.*, pp. 174-210. T.S. Kuhn, «Second thoughts on paradigms» in F. Suppe, (ed.), *The Structure of Scientific Theories*, University of Illinois Press: Urbana, Chicago and London 1974, pp. 459-482.

mains of natural reality. Usually, this brings to an increase in the number of puzzles to be solved. Sometimes, this process may also reveal facts that are largely unexpected within the paradigm. When dealing with such novelties by means of the resources of the paradigm becomes increasingly difficult, and several efforts in this sense fail, the scientists begin to acknowledge that they are faced with an «anomaly», and not merely with a puzzle.[25] An anomaly represents a violation of some key aspect of the kinds of solutions defined by the paradigm. The acknowledgment of anomalies determines a «crisis» of the paradigm, and triggers a «scientific revolution». A scientific revolution, according to Kuhn, is a period of extraordinary science in which the community of scientists begins to explore new theories and solutions that no longer obey the prescriptions of the paradigm. A revolution ends up in the establishment of a new paradigm. Even this sketchy account of Kuhn's scientific revolutions could raise several considerations that fall outside the scope of this work; but there is one point that is particularly relevant for my argument.

When a paradigm has just been established after a revolution, it is extremely underdeveloped. Very few of its potential solutions have been made explicit at the theoretical level and exploited at the empirical level. Moreover, a large portion of facts and phenomena managed efficaciously by the previous paradigm are yet to be tackled by the new one. Speaking precisely about the *decisions* that scientists must make when a new paradigm is being established, Kuhn says: «A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future *promise*. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on *faith*.».[26]

It seems, therefore, that several lines in the philosophy of science of the last century suggest, more or less explicitly, that what I am labelling as «prospective fruitfulness» plays a role in theory change and in the choices that scientists do about the theories that should guide their work. In the following I will explore some more reasons for maintaining the relevance of prospective fruitfulness in science.

5. History, realism, and scientific truth

The history of science has often been taken by philosophers as providing evidence in favour of an anti-realistic understanding of science itself. In a nutshell, the argument might be summarized as it follows. The history of science reveals that all past theories, at least for

[25] T.S. Kuhn, *The Structure of Scientific Revolutions*, *op. cit.*, pp. 52-53.

[26] Ibíd., pp. 157-158 (italics added).

a certain period, have provided good solutions to a number of scientific problems, have given raise to several successful experiments, and have produced useful technological applications. Notwithstanding, the history also shows that all past theories have been soon or later acknowledged as false or no longer adequate and satisfying, and thus abandoned. This seems to suggest that truth (especially in its more diffused meaning of «correspondence to reality») is not something that science is able to reach. This consideration often brings to the conviction that truth should not even be the goal of science, thus promoting anti-realistic understandings of the scientific enterprise according to which (a) science is not conceived as something capable of unveiling reality in its actual characters and constitution, and (b) scientific theories cannot be considered as descriptions of domains of reality. (Note that these two claims are by no means equivalent, as we will see.)

Moreover, the developments in philosophy of science stemming from the proposals of Kuhn and Lakatos have, in my view, prompted a conception of scientific progression and historical vicissitudes not centred on the notion of truth. A new paradigm replacing an old one is not conceived of as a true paradigm overcoming a false one, but as a paradigm able to solve the anomalies affecting the previous one. Similarly, a research programme eliminating a regressive one does this in virtue of its being more internally coherent and less prone to availing of *post hoc* or *ad hoc* hypotheses, and not because it is in some sense «truer» then the rival. Truth, or the tendency towards it, does not play a key role in these epistemological approaches. Incidentally, this may also be one of the reasons why, in spite of the hints at the relevance of prospective fruitfulness that we have extracted in the previous sections, Kuhn's and Lakatos' approaches are mainly past-oriented in character. New paradigms or research programmes are judged fundamentally in relation to their past counterparts.

Those philosophers of science embracing realism, on the contrary, do not renounce the idea that scientific achievements do tell us something relevant about the natural reality –claim (a) above. One relevant argument that they have developed, but by no means the only one, is known as the «no-miracle argument». From this perspective, the undeniable successes that scientific research has achieved in tackling with several portions of the natural reality would be fully incomprehensible if science really had nothing to do with reality: it would be, indeed, a «miracle». Anyway, the reasons of the anti-realists have their strength and cannot be simply ignored.

This conveys the impression that the realistic claim should be weakened to some extent. A way to do this is to maintain that scientific theories should not be considered as *punctual descriptions* of (a portion of) the natural reality –claim (b) above– and this also means that the realistic stance need not imply that scientific theories must be true according to the already-mentioned correspondence notion of truth.[27]

[27] I. Colagè, «Between Realism and Instrumentalism: Description Interaction and Truth», in G. Auletta, (ed.), *The Controversial Relationships between Science and Philosophy: A Critical Assessment*, LEV: Vatican City 2006, pp. 303-

I think that for defending a non-naïve realistic attitude toward the scientific enterprise from the anti-realistic arguments two moves are required. The first one is that of taking the correspondence notion of truth as a «regulative idea»[28] or, following Peirce's insights, as something that defines the *indefinitely future* goal of science. The second move, again inspired by Peirce's thoughts, is that of considering scientific theories capable of informing us about actual aspects of reality not because they are accurate descriptions of nature, but because they are effective guides for our (cognitive) interactions with the latter.

Peirce tightly links the notion of reality with the correspondence notion of truth, and maintains that such a truth is what human investigations tend to: «The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real. That is the way I would explain reality».[29] According to Peirce, moreover, the scientific method is «a method which, steadily insisted in, must lead to true knowledge in the long run of its application, whether to the existing world or to any imaginable word whatsoever».[30] Such a trust in the scientific method and its prospective capability to guide us toward the truth is due to the fact that Peirce lucidly understood, anticipating a central tenet of Popper's falsificationism, that the scientific method is self-corrective in character. In extreme and schematic synthesis, in Peirce's view, science proceeds through cycles of abductions, deductions and inductions, where abduction is the inferential process bringing to the adoption of a hypothesis, deduction serves the task of making explicit the consequences and predictions following from the hypothesis, and induction is the procedure by means of which the predictions drawn from the hypothesis are empirically tested.[31] The inductive stage reveals, soon or later, cases in which the hypothesis fails to account for the empirical data, and this will re-ignite a new cycle, beginning with a new abduction aimed at finding out a new hypothesis able to explain the new, unexpected facts.[32] In this way, the scientific method assumes its self-corrective character, and thus the ability to take us closer and closer to the truth, in the long run.

314. I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 238-244. I. Colagè, «Le scienze naturali e la filosofia della natura: alcune riflessioni epistemologiche», in G. Auletta and S. Pons, (eds.), *Si può parlare oggi di una finalità nell'evoluzione?, Riflessioni filosofiche e teologiche alla luce della scienza contemporanea*, Gregorian & Biblical Press: Roma, 2013, pp. 59-79.

[28] K.R. Popper, *Objective Knowledge*, *op. cit.,* p. 30.

[29] C.S. Peirce, «How to make our ideas clear». *Popular Science Monthly*, 12,1878, pp. 286-302.

[30] C.S. Peirce, *The Collected Papers*, Vols I-VI, in C. Hartshorne and P. Weiss, (eds.), 1931-1935, Vols VII-VIII, A. W. Burks, (ed.), 1956, Cambridge (MA), § 7.207.

[31] C.S. Peirce, «Deduction, induction and hypothesis». *Popular Science Monthly* 13, 1878, pp. 470-482. C.S. Peirce, *The Collected Papers*, *op. cit.*, § 7.206. I. Colagè, *Interazione e inferenza*, pp. 189-230. G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 36-39.

[32] C.S. Peirce, *The Collected Papers*, *op. cit.*, § 2.755.

The idea of a truth asymptotically reachable by means of the scientific method supports the view that prospective fruitfulness actually plays a role in theory change and theory choice: any theory, at any historical time, in any scientific field, should be intended as a step in the process of getting close to the truth. For what we have said so far, this idea, however, is unable to clarify the specific relevance of prospective fruitfulness; further points in this direction will be addressed in the next section. By now, I would just like to note that such a conception of truth may complement the scarce reference to such a notion in the proposals of Kuhn and Lakatos, and precisely in a way consonant with prospective fruitfulness. We have seen that the sequence of paradigms or research programmes is understood, in the works of those authors, mainly in past-oriented terms (i.e. according to the ability of a subsequent approach to overcome the difficulties affecting the previous one). My claim in this respect is that theories are abandoned in favour of alternatives as a way of *spurring the process toward truth*. Additional remarks on this point will be developed in the following section as well.

In the remaining of this section I will tackle with the second move, mentioned above, to keep maintaining a fundamentally but non-naïve *realistic* attitude toward science. This move is centred on Peirce's conception of pragmatism. The «maxim» of Peirce's pragmatism is that «a *conception,* that is, the rational purport of a word or other expression, lies exclusively in its conceivable bearing upon the conduct of life; so that, […] if one can define accurately all the conceivable experimental phenomena which the affirmation or denial of a concept could imply, one will have therein a complete definition of the concept».[33] This allows for regarding scientific hypothesis and theories in their role of guides to actions and interactions with nature: we behave as if the theory were true, that is, as if reality were actually done as the theory suggests. In science, designing and performing experiments is the eminent way in which we act upon portions of natural reality being guided by our theories and hypothesis.[34] Now, this means, at first, that we may establish a contact with reality that does not necessarily rely upon our ability to describe it, but rather on our specific ways of *interacting* with it.[35] Secondly, in this way we can establish *the extent to which* our theories and hypothesis grasp fundamental aspects of reality. As a Peirce's commentator interestingly says: «Reality is that which makes it the case that any particular answer is as good as it is. Or to put it more precisely, in terms of the course of enquiry: when we evaluate any particular hypothesis, reality is that which determines the level of empirical success

[33] C.S. Peirce, «What pragmatism is». *The Monist* 15, 1905, pp. 161-181.

[34] I. Colagè, «Experiments and Causality», in G. Auletta, (ed.), *The Relationships between Science and Philosophy: New Opportunities for a Fruitful Dialogue*, LEV & Notre Dame University Press: Vatican City, 2008, pp. 157-179. I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 161-188.

[35] I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 238-244. G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 44-46.

which the hypothesis will enjoy. On this view, the fact that answer C is worse than answer A is just as much a feature of reality as the fact that answer B is the best one possible».[36] It could therefore be said that our theoretical constructions do grasp actual features of reality *to the extent to which* they function well in guiding our interactions with nature (and note that I am here primarily referring to that specific and cognitive interaction with nature that constitutes the experimental activity). In other words, valuable information about the actual constitution of reality may be gained precisely by the ways in which the latter reacts and behaves in response to our experimental trials. I think that this approach is able to ground a fundamentally realistic attitude toward science, although not a naïve one that would claim our theories to be punctual descriptions of reality. Much more strength would be agreed to this approach thanks to an analysis of the relationships between the theoretical and the experimental components of the scientific enterprise: this is what I am about to do.

6. Theory and experiment

Specialized disciplines, as well as science as a whole, develop through close interactions among theories and experiments. Experiments constitute a crucial step in the furthering of science in that, as we have mentioned just above, they allow for establishing a concrete contact with reality, and because they may be regarded as the attempts at realizing and effectuating our theoretical hypotheses and suppositions about how nature, or the portion of it specifically addressed, might be constituted. Therefore, the experimental activity acquires its peculiar cognitive value because of the relationships it entertains with the theoretical dimension. One may think that experiments just follow a theory, for testing it. Alternatively, one may claim that a theory comes after experiments, to explain the results. To an extent, both views are agreeable; to another, they are not fully adequate. I propose that the theoretical dimension surrounds and envelops the experimental one.[37] It follows experiments to the aim of providing conceptual frameworks capable of arranging and accommodating the array of available experimental results in a comprehensive and coherent view. Several examples may be provided. To take an important one from the history of physics, quantum mechanics was first developed as a way of accounting for, and framing harmoniously, a number of empirical data that resisted previous modes of explanation; they were: the behaviour of the so-called «black body», the photoelectric effect, the Compton effect, the stability of the hydrogen atom, the diffraction of electrons, the discovery of the

[36] I. Farber, «Peirce on reality, truth and convergence of inquiry in the limit». *Transactions of the Charles S. Peirce Society* 41, 2005, pp. 541-566.

[37] I. Colagè, «Le scienze naturali e la filosofia della natura: alcune riflessioni epistemologiche», *op. cit.*

intrinsic magnetic momentum (or «spin»), as well as problems related to the values of the specific heat of diluted gases.[38] This holds true also in other domains of the natural sciences. Although in molecular biology and in cognitive neurosciences the theoretical dimension is somewhat less clearly defined than in physics (and the great majority of publications in these fields consist of reports of experimental inquiries), also in these disciplines conceptual frameworks as well as theoretical hypothesis and models are developed for accommodating large amounts of empirical findings and experimental results. This is often done in that specific kind of publications called «Reviews».[39] The point is that the Reviews and, in general, any theoretical attempt at accommodating the available data, is always pursued in view of possible future developments. In other words, such theoretical efforts accomplish their task not when they say the «last words» about our knowledge of a certain portion of reality, but specifically when they unveil possible new challenges and research directions, when they single out aspects that deserve additional inquiry, or when they suggest further experimental investigations. In this way, the theoretical dimension acquires at once its relevance both in its following and in its preceding the experimental one. Indeed, as already mentioned, experiments must be accurately designed, and must be designed according to a theoretical view that suggests how the portion of reality with which we want to experimentally interact might be done. Only on this condition, an experiment may acquire a clear epistemological and cognitive value, thus resulting in a key step for furthering the research.

What I would like to suggest is that, even from this standpoint, theory-change and theory-choice are affected by the prospective capability of a theoretical framework (or a specific hypothesis) to open up new experimental perspectives, new possibilities for cognitive experimental interactions with nature, and new «roads to reality». Of course, this can be understood only in the light of the diachronic dimension of the scientific enterprise, according to which any particular theory (a) comes from previous theories, empirical findings, and the relations among them, (b) is faced with a number of accepted data, some easily explainable, other less, and (c) *tends towards future developments*. Such future developments may concern both theoretical refinements and the possibility to design novel experimental inquiries that, in their turn, may prompt further theoretical refinements. In this way, «virtuous cycles» or «short-circuits» between theory and experiment are triggered. In the course of such cycles, each moment (be it theoretical or experimental) assumes full relevance not only in retrospect (e.g. for testing, corroborating, or debunking a theoretical element, or else for connecting, framing, or explain-

[38] G. Auletta, M. Fortunato, G. Parisi, *Quantum Mechanics*, Cambridge University Press, Cambridge (UK), 2009, pp. 31-40. G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 98-102.

[39] I. Colagè, «Le scienze naturali e la filosofia della natura: alcune riflessioni epistemologiche», *op. cit.*

ing known empirical data) but also, in prospect (e.g., for manifesting further problems deserving further theoretical or empirical investigation). I would like to propose an argument in favour of this perspective. It concerns the vicissitudes happened around Galileo Galilei and his endorsement of the Copernican system. This argument is inspired by the concluding chapters of Pierre Duhem's *Σωζειν τα φαινομενα* (To save the phenomena).[40]

The short and dense «Essay on the notion of physical theory from Plato to Galileo» is a smart and stimulating attempt at pleading a specific view about the physical theory going through the history of astronomy from Ancient Greece to Galileo. As it is well known, this book upholds a conception of the physical theory in terms of instrumentalism, i.e., that epistemological position according to which a theory should be considered as a mere instrument for deriving well-confirmed predictions without attributing to it any realistic import. I will not enter in a focussed discussion of Duhem's arguments here; however, given the goal of the book, it is not surprising that Duhem does not appreciate the strong realistic convictions that Galilei defended as to the Copernican heliocentric system in astronomy. He is clear, indeed, in criticising Galilei's stance on logical and epistemological grounds. Duhem does not avoid stating that logic was not on Galilei's side when he argued for his conviction during the trials and confrontations that he undertook in the first half of the 17th century. Moreover, it is exactly analysing Galilei's arguments that Duhem develops one version of his criticism to crucial experiments[41]. Notwithstanding, in the very last pages of his work, Duhem cannot resist spending some words of praise for Galilei (and Kepler): «In spite of Kepler and Galileo we nowadays believe, along with Osiander and Bellarmin, that the hypotheses of Physics are nothing but mathematical artifices aimed at *saving the phenomena*; but thanks to Kepler and Galileo we require them *to save at once all the phenomena* of the inanimate Universe»[42]. The question here is: why did Duhem reveal such an admiration?

In the course of his essay, Duhem stresses that instrumentalism in astronomical matters was favoured, during the long history of astronomy, by the radical distinction, rooted in Aristotle's cosmology, between the celestial, super-lunar world and the terrestrial, sub-lunar one. Under this distinction, the claim of knowing the real characters of the «celestial essences», so far away from men and their limitations, has often been considered extremely ambitious and undue, thus justifying a non-realistic understand-

[40] P. Duhem, *Σωζειν τα φαινομενα. Essai sur la Notion de Théorie Physique de Platon a Galilée*, Hermann: Paris 1908.

[41] Ibíd., p. 132-33.

[42] Ibíd., p. 132-33. My translation. Original text: «En dépit de Képler et de Galilée, nous croyons aujourd'hui, avec Osiander et Bellarimn, que les hypothèses de la Physique ne son que des artifices mathématiques destinés a *sauver les phénomènes*; mais grâces à Képler et à Galilée, nous leur demandons de *sauver à la fois tous les phénomènes* de l'Univers inanimé».

ing of astronomical systems. The main merit of Galilei, even to Duhem's eyes, has been precisely that of breaking down this distinction: he opened up the path to the so-called «unification of celestial and terrestrial physics». This was a crucial turn toward Newtonian dynamics (and thus, in the opinion of many, towards modern science as such). Now, my point is that this stresses the relevance of the prospective fruitfulness of Galilei's historical contribution. The Copernican system clearly overcame some of the technical problems affecting Ptolemaic astronomy (thus matching past-oriented criteria). Galilei's positions soon proved effective in accounting for new data, namely, his very observations of the «moon's mountains and valleys» and of Jupiter's satellites (thus also matching present-oriented criteria). But I think that overlooking the future-oriented prospective fruitfulness of Galilei's contributions would mislead our philosophical attempts at understanding theory-change and theory-choice.

There is a further point that I would like to stress, which has to do also with the issue of realism addressed in the previous section. Perhaps not by chance Duhem chose the history of astronomy for supporting his instrumentalism. One reason is surely that astronomy has a longer history, as a technical discipline, than other branches of physics and of natural sciences in general. Another reason, I suspect, is that it is much easier to uphold instrumentalism as to astronomical matters: it is practically impossible to perform experiments with astronomical systems, especially if experiments are taken in the specific sense of controlled interactions with an object-system involving the manipulation of the *state* of the latter.[43] On the contrary, as we have seen, the concrete experimental interaction with some portion of the natural reality may allow for maintaining a fundamentally (although not-naïve) realistic stance. Now, the unification of celestial and terrestrial physics (a process crucial to the arguments, based on the notion of inertia, that Galilei developed for rendering acceptable the Earth's motions implicated by Copernican astronomy) opens the path for extending the physical laws to the entire universe. Indeed, the fundamental physical laws used nowadays in astrophysics are laws that have been extensively experimentally tested… on Earth. Therefore, I think that another element of the prospective fruitfulness of Galilei's work for the furthering of physics has been that of providing the ground for the abovementioned «virtuous cycle» between theory and experiment.

To be clear, I am not claiming that Galilei had clearly in mind all this, and thus opted for the Copernican system. I am just claiming (1) that perhaps Duhem had in mind something similar to prospective fruitfulness in praising Galilei, and (2) that it is quite likely that during the long and troubled process that brought to completion the Copernican Revolution (ended up in Newton's gravitational astronomy) prospective fruitfulness may well have played actual roles (more or less explicitly).

[43] I. Colagè, *Interazione e inferenza*, *op. cit.*, pp. 161-168.

7. The «Neural Re-Use» approach in cognitive neurosciences

In this section I am going to present a rather novel approach to some issues in cognitive neuroscience: the «neural re-use» approach.[44] I will take it as a good example to show the relevance of prospective fruitfulness in the progression of a specific scientific discipline.

This approach is characterized by the notion that neural circuits or limited brain areas having specific, low-level information-processing capabilities can be re-used for various, higher-level cognitive functions involving different *networks* of such circuits. Thus, it implies that the emergence of novel cognitive functions –both at the phylogenetic level (like tool-making or language) and at the ontogenetic level (like written language, as we will see, or arithmetic)– does not necessarily require the formation of new neural tissues specifically dedicated to such a function. Rather, the neural re-use approach requires the instantiation of new networks that recruit already existing neural circuits having basic information processing capabilities useful for the function to be implemented. Such a notion, differently declined in relation to a variety of cognitive functions, has begun to circulate in the neuro-scientific literature just in the last decade.[45] For reasons of space, I cannot address here all the specific features of neural re-use, nor of the different theories that, in a way or another, resort to such a notion. However, a few considerations would help my argument in favour of the notion of prospective fruitfulness.

The first point to be noted is that, at the moment, neural re-use is a significantly speculative notion, and the theories and hypotheses making use of it are highly hypothetical. Specifically, its main weakness at present is that the brain molecular and cellular *mechanisms* possibly underlying the re-use of neural circuits have not yet been singled out. However, I think that this notion is capturing more and more the atten-

[44] I would call this an «approach» as I intend it to gather a class of theories or hypotheses that share a common conceptual core but have also some relevant differences. See I. Colagè, «Human specificity: recent neuro-scientific advances and new perspectives». *ESSSAT News and Reviews*, 23.2, 2013, pp. 5-19.

[45] Here are key references on the main declinations of the neural re-use approach. M.L. Anderson, «Evolution of cognitive function via redeployment of brain areas». *The Neuroscientist*, 13, 2007, pp. 13-21. M.L. Anderson, «The massive redeployment hypothesis and the functional topography of the brain». *Philosophical Psychology*, 21, 2007, pp. 143-74. M.L. Anderson, «Massive redeployment, exaptation, and the functional integration of cognitive operations». *Syntese*, 159(3), 2007, pp. 329-345. M.L. Anderson, «Evolution, embodiment and the nature of the mind», in B. Hardy-Vallee, N. Payette, (eds.), *Beyond the brain: Embodied, situated and distributed cognition*, Cambridge Scholar's Press: Cambridge, 2008, pp. 15-28. M.L. Anderson, «Neural re-use: a fundamental organizational principle of the brain». *Behavioral and Brain Sciences*, 33, 2010, pp. 245-313. S. Dehaene, «Evolution of human cortical circuits for reading and arithmetic: The "neuronal recycling" hypothesis», in S. Dehaene, J.R. Duhamel, M. Hauser, G. Rizzolatti, (eds.), *From Monkey Brain to Human Brain*, MIT Press: Cambridge (MA) 2005, pp. 133-157. S. Dehaene, L. Cohen, «Cultural recycling of cortical maps». *Neuron*, 56, 2007, pp. 384-398. L. Fogassi, P.F. Ferrari, «Mirror neurons and the evolution of embodied language». *Current Directions in Psychological Sciences*, 16, 2007, pp. 136-141. V. Gallese, «Mirror neurons and the social nature of language: The neural exploitation hypothesis». *Social Neuroscience* 3(3-4), 2008, pp. 317-333.

tion of the scholars in the field precisely because of its promise for future development, and here I will point out some of them.

To begin with, it is nowadays acknowledged that the Broca's area (in the left inferior frontal lobe of the human brain), an area long considered crucial for spoken-language production, is indeed involved in a significant array of cognitive functions, and not only in all the fundamental language-related processes, namely phonology, syntax and semantics[46]. It is also involved in tool using[47] and in working memory.[48] The Broca's area is also a core component of the human mirror neuron system,[49] and by virtue of this, it is involved in several additional cognitive functions: action understanding and recognition (i.e. the «classical» role attributed to the mirror neurons since its discovery in the monkey's brain), gestural communication,[50] intention understanding,[51] and imitation.[52] The involvement of a brain area in a so large number of functions is an interesting fact that needs explanation. The most likely one is that Broca's area is apt to process sequential and hierarchically structured information independently of its source or cognitive context, thus being useful in all these functions. However, it is known that such cognitive functions do not appeared contemporaneously in evolution, some of them (e.g. action understanding) being significantly older then others (e.g. language). This points to the relevance of some sort of re-use of Broca's area for the emergence, from time to time, of new cognitive capabilities. But the basic mechanisms by which this could have happened are unknown; therefore, neural re-use should be considered as a prospectively fruitful notion in view of explaining this interesting fact, and not has something that has already proven its effectiveness: it is just promising.

Another interesting fact possibly related to neural re-use is the ascertainment that the brain networks subserving language and tool use in the human being are largely overlapping, although differentiated by some peripheral elements. In particular, both cognitive functions (language and the use of tools) recruit a large network encompassing the Bro-

[46] M. Vigneau, V. Beaucousin, P.Y. Herve, H. Duffau, *et al.*, «Meta-analyzing left hemisphere language areas: phonology, semantics, and sentente processing». *Neuroimage*, 30, 2006, pp. 1414-1432.

[47] J.W. Lewis, «Cortical networks related to human use of tools». *The Neuroscientist*, 12, 2006, pp. 211-231.

[48] M. Makuuchi, J. Bahlmann, A. Anwander, A. D. Friederici, «Segregating the core computational faculty of human language from working memory», *Proceedings of the National Academy of Sciences of the U.S.A.,* 106, 2009, pp. 8362-8367.

[49] G. Rizzolatti, L. Fadiga; M. Matelli, V. Bettinardi, *et al.*, «Localization of grasp representations in humans by PET: 1. Observation versus execution». *Experimental Brain Research*, 111, 1996, pp. 246-252.

[50] F. Lui, G. Buccino, D. Duzzi, F. Benuzzi, *et al.*, «Neural substrates for observing and imagining non-object-directed actions». *Social Neuroscience*, 3, 2008, pp. 261-275.

[51] M. Iacoboni, I. Molnar-Szakacs, V. Gallese, G. Buccino, *et al.*, «Grasping the intentions of others with one's own mirror neuron system». *PLoS Biology*, 3, 2005, no. e79.

[52] M. Heiser, M. Iacoboni, F. Maeda, J. Marcus, J. Mazziotta, «The essential role of Broca's area in imitation». *European Journal of Neuroscience*, 17, 2003, pp. 1123-28.

ca's area (namely both *pars opercularis* and *pars triangularis* of the inferior frontal gyrus), the ventral portion of the pre-motor cortex, the inferior parietal lobe, and the posterior temporal cortices. The two networks are distinguished in that language additionally recruits the primary auditory cortex and the motor cortex controlling the vocal tract, whereas the tool-related network involves the primary visual cortex, the somatosensory cortex and the part of the motor cortex controlling hand movements.[53] Now, the neural re-use approach predicts that cognitive functions that emerged late in phylogenetic evolution (such as tool use and language) would recruit widely scattered regions of the cerebral cortex;[54] a prediction based on the reasonable assumption that the recruited brain areas, having not evolved in specific relation to that functions, are unlikely to be clustered together. The just-mentioned example is in line with such a view, thus making the neural re-use approach arguably promising. However, again, the mechanisms underlying the formation of such kinds of networks in the course of evolution are still largely hidden, thus making neural re-use promising but not yet fully proven.

In my opinion, there are two more points that make the neural re-use approach prospectively promising. The first has to do with the general brain organization principles and with the structure-function relationship (namely, the relations between the brain structural organization and the fulfilment of cognitive functions). Specifically, the neural re-use approach proposes itself as a middle course between localist and holistic views of the brain organization. A localist view implies a strict coupling between a single brain area and a single cognitive function. A holistic one presupposes that any cognitive function involves a great number of brain areas or neural circuits, and that the basic processes implemented in each of such areas or circuits change according to the functional context at hand. Neural re-use is not localist, as it entails that cognitive functions are subserved by large and diffused networks of brain areas. It is neither holistic, as it maintains that the basic information processing carried out by a certain neural circuit remains constant across the functional contexts and networks in which it may, from time to time, be involved.

Finally, a specific declination of the neural re-use approach provides insights in the possible influences that genuinely cultural processes may have on the brain functional architecture. This deserves a somewhat more detailed presentation.

At the onset of this century, L. Cohen, S. Dehaene and colleagues[55] isolated a specific brain area involved in the early stage of the reading process, which they termed the «visual word form area» (VWFA). The VWFA is located within the left ventral visual

[53] D. Stout, T. Chaminade, «Stone tools, language and the brain in human evolution». *Philosophical Transactions of the Royal Society of Landon B*, 367, 2012, pp. 75-87.

[54] M.L. Anderson, «The massive redeployment hypothesis and the functional topography of the brain» *op. cit.*, M.L. Anderson, «Massive redeployment, exaptation, and the functional integration of cognitive operations», *op. cit.*,.

[55] L. Cohen, S. Dehaene, L. Naccache, S. Lehéricy, *et al.*, «The visual word form area. Spatial and temporal characterization of an initial stage of reading in normal subjects and posterior split-brain patients». *Brain*, 123, 2000, pp. 291-307.

cortex, approximately at the lateral occipito-temporal sulcus, and responds selectively to orthographic stimuli (thus constituting a specialized interface between vision and language). It is strictly left-lateralized, mainly visual (i.e., usually processing information), and strictly pre-lexical (responding also to strings of graphemes not corresponding to any real and meaningful word). The VWFA forms in the human brain during the process of reading acquisition, and its location in the left cortex is remarkably constant across individuals and across cultures or writing systems.[56] Additionally, before learning to read, or if an individual does not learn to read at all, this brain region subserves other processes: it is activated by faces, objects and checkerboard patterns. Only during reading acquisition does it begin to specialize for visual orthographic stimuli, progressively becoming insensitive to other kinds of visual inputs. It also displays a posterior-to-anterior gradient of increasing sensitivity for letter-strings closer to real and common words in the known language.[57]

The first important point is that written language has been developed less than 6,000 years ago, and until recent times only a tiny minority of humanity was literate. This means that we cannot assume that the functional specification of such a brain area is directly due to evolutionary processes and to species-specific hard-wired genetic or developmental programs. Moreover, its specification as a visual area for reading does not show critical periods and occurs regardless of the age of reading acquisition.

It seems reasonable, therefore, to hypothesize that the VWFA represents a case of neural re-use. Such a hypothesis receives further strength by the facts that (i) the basic information-processing capabilities of this brain area prior to reading acquisition (i.e. its sensitivity to high-resolution shapes and line configurations presented in the fovea) make it well suited to such a novel purpose, and (ii) its location in the brain is ideal for establishing connections with other temporal language areas.

I think there are two points that render this hypothesis particularly promising. First, the process of specification and specialization of the VWFA may be regarded as a process «in-between» phylogenetic evolution (which should be excluded, as we have seen) and common learning processes such as Hebbian learning. The idea that the formation of the VWFA may not be due to simple neuronal learning is supported by the extremely low variability in the inter-individual and inter-cultural location of the VWFA –which is much lower than expected according to neuronal learning.[58] This makes the neural re-use approach, at least in this specific version, quite promising for the ascertainment of possible new mechanisms underlying neural plasticity. The second point,

[56] D.J. Bolger, C.A Perfetti, W. Schneider, «Cross-cultural effect on the brain revisited: Universal structures plus writing system variation», *Human Brain Mapping*, 25, 2005, pp. 92-104.

[57] F. Vinckier, S. Dehaene, A. Jobert, J.P. Dubus, *et al.*, «Hierarchical coding of letter strings in the ventral stream: dissecting the inner organization of the visual word-form system». *Neuron*, 55, 2007, pp. 143-156.

[58] S. Dehaene, «Evolution of human cortical circuits for reading and arithmetic: The "neuronal recycling" hypothesis», *op. cit.*

perhaps even more relevant, is that the formation of the VWFA is a direct consequence of a *cultural innovation* of our species,[59] and happens at an *ontogenetic* time-scale. In other words, the invention of written language by humans of less than 6,000 years ago affects our brain configuration when we learn to read today. This may potentially open up entirely new ways for looking at the relations between brain and culture, ways that may not necessarily pass through genetic or epigenetic evolution (although this should not be taken to mean that the ontogenetic re-use of neural resources violates or is incoherent with truly evolutionary processes).

Summing up, resorting to a current development in cognitive neuroscience, I have tried to show in which sense I propose that an approach, a theory, or a hypothesis may be considered in its prospective fruitfulness. I think that the interest that the neural re-use approach has, and is raising, is mainly due to its promise, and only secondarily to its actual ability to overcome past problems or to its current empirical adequacy. In a sense, its relevance lies in its potential to open new research paths, and not in closing or extinguishing old ones.

8. Prospective fruitfulness: some consideration on the notion

In the light of what discussed so far, I would now try to summarize some key aspects of the notion of prospective fruitfulness. I am afraid I am unable to propose an exhaustive and unqualified definition of this notion; my aim in what precedes has just been of suggesting that such a notion, or more generally some kind of future-oriented criterion for theory-change and theory-choice, might result in further insightful reflections in philosophy of science, and favour a better understanding of how science is carried on.

Prospective fruitfulness conveys the idea that, among the numerous features that a theory should possess for scholars to choose it and for it to be established in a discipline at the expenses of other alternatives, such a theory should have a significant promise for future developments. This is related to Kuhn's idea that an act of faith is required for embracing a paradigm when it is just emerging after a crisis (Section 4). However, there is also something more, as prospective fruitfulness not only refers to the capability of a theoretical approach to tackle with «puzzles» somehow already envisaged by a paradigm; it mainly refers to the ability to open entirely new opportunities for further advancements –opportunities, that is, for developing novel experimental inquiries and for addressing *new* «puzzles».[60] Incidentally, it is mostly in this sense that I consider the neural re-use approach (Section 7) as prospectively fruitful.

[59] S. Dehaene, L. Cohen, «Cultural recycling of cortical maps», *op. cit.*

[60] Such *new* puzzles may have more or less diffused consequences, even up to the point of requiring a «paradigm change».

Prospective fruitfulness is better framed in an understanding of science as a virtuous cycle of theories and experiments (Section 6) and as an open path toward truth (Section 5). From this perspective, any theory (hypothesis or approach) may be considered as just one step in this intrinsically future-oriented process. Sections 5 and 6 have argued that this does not necessarily imply anti-realism. Moreover, considering a theory as «just one step» in the process should not imply giving up imposing strict requirements to such a theory. From this perspective, Lakatos' methodological suggestion of avoiding *ad hoc* hypotheses (Section 3), as well as the indications that crucial experiments may provide in particular periods of the development of a discipline (Section 2), reveal the need for striving to formulate sound and «healthy» theories. My point here is just that such soundness and health should be also referred to prospective fruitfulness, in a future-oriented perspective.

On the other hand, the perceived need for «sound and healthy» theories is a crucial point at the basis of the extensive reflections, developed in different epochs by philosophers of science as well as by some leading scientist, on the very idea of having criteria for assessing a theory. I suspect, but cannot argue here, that a good portion of the criteria that have been proposed in the past for assessing a theory (e.g., truth or approximation to it, empirical adequacy, predictive power and accuracy, internal and external coherence, simplicity, pragmatic utility, etc.) might prove their relevance not only if considered in past- or present-oriented terms, but also from a future-oriented standpoint. A key point, for example, has to do with the «rigidity» or «strictness» of future-oriented criteria with respect to past- or present-oriented ones. Likely, future-oriented criteria are somehow looser, or less rigid then their past- or present-oriented counterparts, as the assessments based on the latter may resort to precise data as to how a theory has been capable, or is capable, of dealing with the concerned portion of natural reality. Future-oriented criteria cannot resort to such data. From this point of view, an issue deserving further reflection is the possibility that criteria like simplicity, internal and/or external coherence, or broadness of scope had a greater import in the future-oriented perspective than, e.g., empirical adequacy or *actual* pragmatic utility. These are, in my opinion, issues to be tackled before long.

9. Prospective fruitfulness and philosophy of nature

In this concluding section, I will note a possible connection of the issue of prospective fruitfulness with another theme that in recent times is regaining interest and relevance (as the volume hosting this paper indeed manifests): the theme of a philosophy of nature adequate to our times, times in which the natural sciences are so influent both for human knowledge as such and for several aspects our societies. This is a huge theme, which I cannot deal here in all its facets. However, I would like to note that according to a recent proposal,[61] there is a sense in which philosophy of nature may reveal as an active partner of scientific research; this sense, it seems to me, is closely related to prospective fruitfulness as understood in the previous pages.

As mentioned above (Section 6), one of the tasks of the theoretical dimension of the scientific endeavour is that of formulating hypothetical conceptual frameworks aimed at accommodating the multitudes of data available at a certain time. Obviously, each scientific discipline does this as to the portion of reality it primarily addresses. What about a more comprehensive view? A view, that is, with a widely inter-disciplinary (or, perhaps better, cross-disciplinary[62]) character, and interested in distilling those most fundamental aspects of natural reality that the scientific inquiry as a whole seems to suggest, imply or require? According to the mentioned proposal, elaborating such a view might be the task of a renewed philosophy of nature. From this point of view, such effort could be conceived as a further stage of the virtuous cycle characterizing the scientific attitude toward nature.[63] This means, among other things, that such kinds of philosophical elaborations should play an active role in the overall progression of human knowledge (at least for what relates to natural reality). Thus, also philosophy of nature should be prospectively fruitful.

I think there are at least three closely interrelated ways in which it could indeed be so. First, by attempting at formulating general and fundamental conceptual frameworks that cut across several specialized disciplines, philosophy of nature might be in the position of unveiling either potential connections or sharp contrasts among disparate empirical findings and/or theoretical elements. In this way it might help in opening new research perspectives and in formulating new problems (Section 8). Secondly, philosophy of nature may be concerned with the formulation of fundamental principles that, on the one hand, as just seen, would aim at grasping the most fundamental characters of nature, and, on the other, might function as *heuristic* principles –principles, that is, capable of igniting further research.[64] Finally, a specific worry of philosophy of nature should be that of formulating (or helping to formulate) basic *research strategies*. I take the notion of «research strategy» to refer to a set of basic methodologies and conceptual orientations that guide the research in several fields of investigation.[65] For example, a research strategy may set the priority on one (or some) among several possible basic knowing methods (e.g., pure abstract speculation, learning from ances-

[61] G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 11, 23-25, 52-55, and 66-67.

[62] Ibíd., pp. 55 and 57-60.

[63] I. Colagè, «Le scienze naturali e la filosofia della natura: alcune riflessioni epistemologiche» *op. cit.*

[64] An attempt in this direction may be found in the third chapter of G. Auletta, in collaboration with I. Colagè, P. D'Ambrosio, L. Torcal, *Integrated Cognitive Strategies in a Changing World*, *op. cit.*, pp. 95-169.

[65] Ibíd., pp. 19.

tors, sole observation, experimentation, etc); or it may attribute main relevance to one (or some) among several possible fundamental aspects of nature (e.g., the «essences», the material constitution, the formal configuration, the dynamical mechanisms, the final causes, etc.). In doing this, a research strategy stays as a reference point in formulating those specific problems and details to be addressed in specialized discipline, thus possibly assuming, once again, a prospective role.

I have the feeling that this paper does not succeed in solving any of the long-standing problems in philosophy of science; instead, I think it would rather open up new ones. But this, if it were actually the case, would be perfectly in line with prospective fruitfulness.

Acknowledgements

Many thanks to Miguel Ramon, who invited me to write this paper. I warmly thank also Paolo D'Ambrosio, Andrej Pukhaev and Gennaro Auletta for their comments and suggestions, which improved this work. I am grateful to my students at the Gregorian University and at the Theological Faculty «Seraphicum» during the second semester of the academic year 2012-2013: their questions helped me in clarifying my views as to prospective fruitfulness. This work was in part supported by a grant from the John Templeton Foundation (especially concerning the issues treated in Section 7); the views expressed here are those of the author and do not necessarily reflect those of the Foundation.

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Article rebut: 20 de juliol de 2013. Article acceptat: 27 de gener de 2014.