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# Remarks on the scientific exploration of "anomalous" psychiatric phenomena

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## Abstract

**Background:** Scientific research on controversial subjects, such as spirituality-and-health, raises several issues about scientific activity that should be properly clarified for an adequate conduction of the investigations. **Objectives:** To highlight some topics of philosophy of science that can be useful in the exploration of unknown, or poorly known, aspects of reality. **Methods:** By reviewing briefly the concepts of paradigm, normal science and scientific revolution, introduced by Thomas Kuhn, we discuss a set of criteria for evaluating scientific hypotheses, and present some general epistemological guidelines for the scientific exploration of new fields. **Results:** Scientific activity should be based on theories exhibiting empirical adequacy, falseability, predictive accuracy, broadness of scope, simplicity, theoretical integration, theoretical ordering, and capacity to predict new kinds of phenomena. The proposed guidelines are: to take experimental findings seriously, even when they do not fit into the current paradigm; to search for a theory capable of guiding investigation; to avoid both the dogmatic rejection and the hasty acceptance of new hypotheses; and, in theory evaluation, to take care in not attributing undue value to the context in which the theory was first conceived, or to the authority of the persons who profess or reject it. **Conclusion:** The scientific exploration of new areas is rendered more fruitful by a thorough understanding of the nature of scientific activity, specially of what Kuhn has called "extraordinary science" (in contrast with "normal science").

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## Introduction

Contemporary studies in history and philosophy of science, and particularly those undertaken by Thomas Kuhn, have provided ample evidence of the fact that most of the scientists of the so-called "mature" sciences spend most of their time inquiring into phenomena that are already "known", in the sense that they already find a place, at least in their essentials, in an accepted theoretical framework, or "paradigm". Furthermore, these studies have shown that ordinary scientific training is not specifically designed to prepare scientists for the exploration of fundamentally new kinds of phenomena and theoretical hypotheses. This should not be taken to imply that "normal" science is in any way trivial or unimportant, and that the usual scientific education is seriously flawed. But these points should be borne in mind by researchers interested in such a kind of exploration, as certainly are the authors who contributed to this issue of the *Revista de Psiquiatria Clínica*. The relationship between spirituality and mental health is a largely unexplored territory, in which reigns ample divergence regarding the reality of the phenomena, their theoretical interpretation and their clinical and philosophical import. The general aim of the present article is to contribute to the discussions by way of philosophical and methodological elucidations. Although Kuhn's theory of science will be explicitly assumed as its background, the article's main messages are to a large extent independent of the more esoteric and controversial theses about science proposed by Kuhn.

## Elements of Kuhn's theory of science

Kuhn's theory of science, as put forward originally in the classic *The Structure of Scientific Revolutions* (Kuhn,

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1970), hinges on the thesis that the typical development of a scientific discipline occurs according to the following open structure:

> pre-paradigmatic phase → normal science → crisis → revolution → new normal science → new crisis → new revolution → ...

The pre-paradigmatic phase represents the "pre-history" of a science, the period in which there is deep disagreement amongst the researchers about what are the fundamental phenomena of their field, how they should be theoretically explained, the relations of the theoretical principles one with another and with theories of neighbouring domains, the methods and values guiding research, etc. A discipline becomes truly scientific only when reasonable consensus is reached on these points, through the adhesion to a scientific *paradigm*.

In its original, pre-Kuhnian signification, as used for instance in grammar, the term 'paradigm' means 'example', or 'model'. By employing the term in philosophy of science, Kuhn suggests that the transition of a discipline to the scientific phase requires the acknowledgement, from the part of the community of researchers, of an exemplary scientific achievement. Aristotle's mechanics, Newton's optics, Boyle's chemistry, Franklin's electricity theory are some of the examples given by Kuhn of paradigms that promoted the respective fields of research to the category of sciences.

Among the topics forming a paradigm, one could mention: 1) an ontology, indicating the kind of fundamental elements that constitutes the world; 2) basic theoretical principles, specifying the laws that regulate the behaviour of these elements; 3) auxiliary theoretical principles, establishing the connection of the basic principles to the phenomena, as well as to theories of related domains; 4) methodological rules, standards and values, guiding the further development of the paradigm; 5) concrete examples of application of the theory, etc. Kuhn argues, however, that the explication of a paradigm in terms of its elements can never be complete, because knowledge of a paradigm is partially *tacit*, acquired by direct acquaintance with the way of doing science determined by the paradigm. Thus, it is only by *doing* optics in the way Newton did, or electromagnetism in the way Maxwell did that one can thoroughly know, respectively, the paradigms of Newtonian optics and of electromagnetism, for instance.

A paradigm represents a kind of "map" to be used by the scientists in the exploration of nature. Research firmly grounded on the theories, methods and examples of a paradigm is called *normal science* by Kuhn. Normal science aims to extend the knowledge of the facts that the paradigm identifies as relevant, by further elaboration of the theory and by a more accurate observation of the facts themselves.

Normal science is a highly directed, selective activity. This is essential for the development of science, as Kuhn convincingly argues. It is only by focusing their attention on a selected range of phenomena and explanatory theoretical principles that the scientists succeed in going deep in the study of nature. No truly scientific research is possible without the guidance of a body of theoretical and methodological principles, allowing the selection, explanation and evaluation of what is observed. Working under the direction of a firmly established paradigm, the scientist needs not constantly reconstruct the foundations of his field, explaining the meaning of every concept he uses, defending the relevance of every fact he chooses to observe, and justifying every theoretical hypothesis he adopts.

Kuhn describes normal science as a "puzzle-solving" activity, since it deals with a set of given elements, and presupposes well-defined rules, as in ordinary puzzles. This helps to make clear the sense in which we said in the Introduction that the "normal" scientist does not specifically aim to investigate fundamentally new phenomena and theoretical hypotheses. Except for details, the phenomena he examines and the theories he uses are already "given". A typical example is the determination of the genome of the biological species: it is just a matter of establishing the order and number of the nitrogen bases that compose the genetic code of the species. This does not mean, of course, that the normal scientific activity does not require effort and talent, but only that it generally does not involve fundamentally new theoretical insights, nor the observation of fundamentally new kinds of phenomena, whose occurrence is not already predicted in outline by the received paradigm.

As Kuhn observes, scientific education is entirely determined by this specific goal of normal science. Textbooks, for instance, present a worldview strongly moulded by the accepted paradigm, suppressing in their (usually cursory) reference to the history of the field all the elements indicative of the more complex and messy research undertaken by the great geniuses responsible for the creation of the paradigm. Facts and hypotheses that do not fit the paradigm are classified as "mistakes", "failures" or even "errors", corrected in due time by the advance of normal science. Such a sanitized view of the history of science does not impair the ability of the scientist, *qua normal scientist*, to do his job properly. *But it leaves him without adequate resources to evaluate the real nature of science, as a general intellectual undertaking, and to conduct research on fundamentally new subjects.*

This limitation is crucially important when one recognizes that the necessity to deal with new phenomena is as important for the long-run progress of science as is normal science. In the course of the development of a paradigm, some of the puzzles posed by nature prove to be particularly difficult to solve. When this happens, scientists typically do not hastily violate the rules and basic principles of the paradigm in a effort to (dis)solve the puzzle. In the same way as in a jigsaw puzzle, for example, to cut off a non-fitting edge of a piece is not a valid move, in normal science the fundamental laws and standards are not, and should not be lightly abandoned or mutilated when a hard problem is encountered. As long as the paradigm exhibits no serious, persistent and generalised failures, the scientists hold fast to the paradigm, and they are right in doing so, as Kuhn remarks. After all, paradigms, especially at their beginnings, invariably face difficulties, and a certain amount of conservatism is necessary to allow them the opportunity to show up their full potentiality.

But theoretical tolerance should have a limit, of course. When unsolved puzzles involving the *central* areas of the paradigm – called *anomalies* by Kuhn – defy the best efforts of the best scientists for a long time, *crisis* installs, and the time is ripe for considering the replacement of the paradigm. In this situation, the most daring and creative members of the scientific community come up with alternative theories. Once confidence on the dominant paradigm is lost, such theoretical alternatives become appealing to a growing number of scientists. When one of them, accompanied with a whole set of standards, methods, values, finally succeeds in gaining the adhesion of the majority of the scientists, research becomes directed by a new paradigm. Kuhn calls the process of paradigm substitution *scientific revolution*. Most of the philosophical controversies triggered by his work derive from what he says about scientific revolutions. Fortunately, these esoteric controversies need not concern us here. The analyses to be developed in the rest of this article will draw only upon the general schema of the nature of science that we have just summarized, and could, with some adaptation, be imbedded in the framework of other contemporary theories of science, such as those of Imre Lakatos and Larry Laudan, for instance (Lakatos, 1970; Laudan, 1977, 1996).

## Exploring the unknown: the case of psychiatry

Kuhn's ideas about science have resonated in scientific circles, and scientists belonging to different areas of science have often appealed to them to discuss the methodological and foundational issues in which they are involved. Psychiatrists are no exception. Kenneth Kendler, for instance, commenting on the prospects of developing a "philosophical structure" for psychiatry, maintained that the discipline as a whole could, in its actual state, be characterized as still being in a "prescientific 'battle of paradigms'" (2005, p. 433). Focusing on a more specific set of issues, related to the so-called "mediumship", Almeida and Lotuffo Neto suggested that although research on this topic has been undertaken by some of the pioneers of psychiatry and psychology, their investigative efforts have been discontinued, in a phase that could be classified "pre-paradigmatic" (2004, pp. 139-140).

Although the evaluation of these, or similar claims in the literature lies beyond the scope of the present article, it seems fair to say that, like other areas of study of human beings in their mental, social, political dimensions, psychiatry has not yet exhibited a degree of theoretical and methodological consensus comparable to that existing in more traditional areas of science, such as physics, chemistry and biology. This being the case, psychiatry could, indeed, be thought as being in a pre-paradigmatic, and therefore prescientific phase. But such a claim should be tempered by Kuhn's own admission, in later texts, that perhaps one could not expect that the major examples of paradigm discussed in his pioneering book will, or can, be reproduced in all areas of human knowledge (Kuhn, 2000).

Closer attention to research done in several areas of science seems, indeed, to indicate that reasonable amount of agreement can be reached in local, specific sub-domains, although no general consensus exists. Apparently, this "fragmented" paradigmatic pattern is more common in areas dealing with complex subjects, such as are human beings, considered in their intellectual, sentimental and social dimensions. In psychiatry, for instance, one should remark that there are domains of phenomena in which research proceeds with reasonable theoretical unity, as is the case of the pharmacological studies of schizophrenia and mood disorders. A large number of researchers are involved in these studies, sharing methods (e.g. double-blind randomised controlled clinical trials) and theoretical principles, as well as working on a commonly accepted set of empirical results. [1]

There are, however, other domains in psychiatry in which serious disagreement is the rule. A typical example is that of the relationships between mental health and spirituality, and, in particular, of the studies on the so-called "anomalous experiences" and the "altered states of consciousness". [2] It is clear that at least in these domains there is no paradigm, good or bad, guiding research. But although this is a scientifically unsatisfactory situation, we should *not* conclude that scientific research on such topics is impossible. On the positive side, they represent, rather, a stimulating challenge to scientific creativity and ingenuity, capable of leading, in the long run, to substantial progress. In facing the challenge of dealing with new subjects, however, scientists often become aware of the above-mentioned limitation of their training, which aims to prepare them for doing normal, not "extraordinary" science. Several shortcomings resulting from this situation will be commented in Explorning the unknow: some general suggestions, below. But we will first make some general remarks on science

1 It is work remarking, however, that the so-called "pharmacological revolution" occurred mainly as a result of casual findings leading to the first antidepressant and antipsychotic drugs. The heuristic limitation of the "pharmacological paradigm" becomes apparent when due attention is paid to the misgivings on its theoretical assumptions raised by some authors (Moncrieff, 2006), and to the difficulties in obtaining really effective drugs, which do not simply result from minor modifications in the molecular structure of drugs already known (Freedman, 2005).

2 An excellent review of the studies on these topics has been published by the American Psychological Association, under the title Varieties of Anomalous Experience (Cardeña et al., 2000). This book examines, among other things, near-death and out-of-body experiences, cases of perceptual anomalies, and reports of anomalous healing experiences and of past-life experiences.

that help to identify the most common problems arising in the scientific exploration of new areas. We begin, in the next section, by drawing an important distinction between two types of scientific theories.

## Two kinds of scientific theories

In science, there are laws formulating properties of entities and processes that are, in a certain sense, directly observable. Laws of this kind are called *experimental* or *phenomenological laws*, since the word 'phenomenon' originally meant that which is directly given in experience. A typical example is Boyle's law, according to which the pressure of a mass of gas kept at constant temperature varies as the inverse of its volume. Another simple example is the law of heredity stating that blue-eyed parents can have only blue-eyed children. A theory containing only phenomenological laws is said to be a *phenomenological theory*. The most important examples of phenomenological theories in physics are classical thermodynamics and Einstein's theory of special relativity. In biology, one could mention Darwin's theory of evolution by natural selection.

Phenomenological theories describe systematically the phenomena of their domain, allowing us to *predict* the occurrence of a phenomenon from the occurrence of others. The explanatory power of this type of theory is, however, rather limited, and this often leads scientists to search for theories of another type, which one may call *explanatory theories*. In contrast with phenomenological theories, explanatory theories go beyond the observational level. They postulate unobservable entities and processes, purportedly indicating the causal mechanisms responsible for the occurrence of the phenomena, which would, by this way, become explained. Most theories of contemporary physics, chemistry and biology are of this kind.[3]

From a scientific point of view, both kinds of theories are legitimate, playing specific, complementary roles in science. Consider, for instance, the case of thermodynamics. It has been complemented by a powerful explanatory theory, statistical mechanics. Whereas the former systematises the description of thermal phenomena with the help of experimental notions such as temperature, energy and specific heat, the latter postulates a microscopic reality of atoms and molecules, whose mechanical behaviour would account for the phenomena described by thermodynamics, providing a better understanding of their occurrence and interdependence. In biology, Mendel's genetics, which is constituted of phenomenological laws such as the aforementioned law about eye colour, forms a pair with the contemporary explanatory theory of molecular genetics.

From a philosophical point of view, however, phenomenological and constructive theories raise quite distinct problems. Given the nature of their laws, phenomenological theories can be justified through the systematic, controlled observation of phenomena. Although the process of generalization needed to pass from the observation of particular cases to the general law is open to philosophical doubts,[4] these doubts are in a sense much less serious than those about the justification of the fundamental laws of explanatory theories. The latter are explicitly introduced as *hypotheses* and, as a matter of principle, there is no way of deriving then directly from observations. At such a juncture, what the scientist and the philosopher can do is to try to find criteria that may help to evaluate hypotheses, as putative descriptions of the unobservable reality underlying the phenomena.[5] This is one of the main topics of discussion among contemporary philosophers of science, and cannot be pursued here. We shall limit ourselves to present, in the following section, a simplified list of such criteria, accompanied by summary explanations of each of them.

## Evaluating scientific hypotheses

### Empirical adequacy, falsifiability

Given hypothesis H and experimental evidence E, there is no logical path going from E to H, i.e. there is no way of logically inferring H from E. If there were, H would not count as a hypothesis, being simply a logical consequence of observation. The logical relationship between H and E that can, in certain cases, be established is just the inverse, i.e., $H \rightarrow E$. Now *the fundamental criterion* for a scientific hypothesis is that it should permit such inferences. In other words, *scientific hypotheses must have empirical consequences*; otherwise, they would be entirely unconnected to empirical evidence, being therefore cognitively empty. When this basic requirement is fulfilled, two possibilities should be considered:

i) Empirical implication E is *true*, that is, the fact E predicted by H is effectively obtained in the world. In this case H is said to have been *empirically confirmed* by the observation of E. This notion of empirical confirmation should *not*, however, be mistaken for the stronger notion of *proof* – something established with apodictic certainty. Even a false hypothesis can logically have some true empirical consequences. But despite being something less than proof, empirical confirmation is

3 One of the first to draw explicitly the distinction between phenomenological and explanatory theories (in different terminology) appears to have been Einstein, in a paper on the theory of relativity published in 1919 (Einstein, 1954, p. 228). For a more extensive philosophical discussion, see the classic Nagel, 1979, chap. 5.

4 Even when a regularity is observed in a large number of phenomena, it is conceivable that it may fail under circumstances yet to be found. In philosophical terminology, the process of generalization from particular instances is called induction. The point we are making here can, then, be re-expressed by saying that inductive inferences are irredeemably fallible. As a consequence, the popular view that scientific knowledge is synonym of "proved" knowledge is untenable, even in the most favorable case, that of phenomenological theories. Several classic articles on the problem of justification of induction can be found in Swinburne, 1974.

5 Notice, by the way, that we have here a second, more serious reason for rejecting the common opinion that science is formed exclusively of absolutely certain, "proven" propositions. (Cf. the preceding note.)

essential, and is, indeed, all that a hypothesis can have in terms of empirical support. A hypothesis whose empirical consequences are all true is said to be *empirically adequate*. Although the empirical adequacy of a hypothesis can never be strictly established, since the empirical consequences of a typical scientific hypothesis form an open, potentially infinite set, we should check whether, at least, all *known* facts are logically compatible with the hypothesis we are evaluating. When this does *not* occur, we pass to the second possibility:

ii) Empirical implication E is *false*, that is, the facts about the world are not as predicted by the hypothesis. In such a case H is said to have been *refuted,* or *falsified* experimentally. Although researchers often are disheartened by the realization that the hypothesis they are inquiring into is refuted, one should keep in mind that even the refutation of a hypothesis is vitally important in science, since it informs us, at least, that the world is not as the hypothesis says it is. We can, then, set about to devise better hypotheses, capable, hopefully, of getting us closer to reality. Thus, the *falsifiability* of a hypothesis – the possibility of its being exposed to negative experimental evidence – is an essential requisite in science.[6]

### Theoretical integration

Scientists working on the so-called "mature" sciences typically do not propose or discuss isolated hypotheses; they are, instead, concerned with *theories*. And theories should not be conceived as simple aggregates of hypotheses. They are better characterized as complex sets of hypotheses tied by logical links and other relations of mutual support. Theoretical integration is essential not only for conferring coherence to the theory, but also for increasing its capacity of leading to interesting, testable empirical consequences. Several contemporary philosophers of science have even suggested that the basic unit of science is something broader than theory. Thus it was that Kuhn, Lakatos and Laudan have arrived at the notions of paradigm, scientific research programme, and scientific research tradition, respectively.

### Theoretical ordering

A further trait of science, related to theoretical integration, is what we may call *theoretical ordering*. The hypotheses forming the theory of a good paradigm or scientific research programme are typically ordered according to their importance, so that the more important hypotheses form, in Lakatos's phrase, a theoretical *hard core*. There is tacit agreement among the scientists that this core should be modified or abandoned only at last resort. In normal conditions, problems of empirical adequacy arising in the course of development of the programme should be tackled though modification of less central hypotheses, forming what Lakatos call the *protective belt* of the core. Of course, when the programme experiments generalized persistent problems of empirical adequacy, its replacement by a new one should be seriously considered (Lakatos, 1970; Chalmers, 1982). Such a strategy of balancing carefully conservativeness and openness to new evidence and theoretical innovation is similar to the strategy suggested by Kuhn with respect to paradigms, as we mentioned in Section 2, above.

### Prediction of new kinds of phenomena

Among the individual theoretical virtues of a theory, recommending it for acceptation, perhaps the most important is the capacity of *anticipating the occurrence of new kinds of phenomena*. Philosophers and scientists of different areas and historical periods have argued that if a theory predicts formerly unobserved phenomena it cannot fail to correspond to reality, at least partly and approximately. Notice that this theoretical trait is exclusive of explanatory theories. Phenomenological theories, by their own nature, never predict phenomena of entirely new kinds; they come in the wake of facts, and that is the price they pay for their greater stability. As in the stock market, in science greater gains – greater explanatory power, improved predictive power – usually require greater risks: making hypotheses about the deeper, unobservable layers of reality.

### Broadness of scope

On the face of it, the broadening of the scope of theoretical inquiry through the inclusion of more phenomena of different kinds represents a complication, for it would then become more difficult to find a theory capable of fitting all the empirical evidence. This is, in a sense, true; but it is also true that broadness of scope brings an important advantage to scientific inquiry: it is a great help in filtering away false theories. Take, for instance, the kind of research discussed in the present issue of *Revista de Psiquiatria Clínica*. There seems to be no doubt that the richer the stock of empirical facts taken into account, the greater its capacity of refuting bad hypotheses and of suggesting better alternatives. Thus, in this area, attention should be focused not only on the so-called "anomalous" phenomena, but also, if possible, on many other psychiatric phenomena, and even on non-pathological mental phenomena (cognitive, perceptual, emotional, etc.). A truly scientific picture of human being – the ideal toward which our efforts should aim

6 Thus, one of the most influential contemporary philosophers of science, Karl Popper, has explicitly proposed that falsifiability is the basic criterion for demarcating science from non-science, or pseudo-science. Only falsifiable hypotheses allow science progress through an open-ended process of conjectures and refutations (Popper, 1968, 1972a, 1972b).

– should be in principle capable of integrating all aspects of human nature. When one of our theoretical attempts at approaching this ideal leaves out of account some important class of facts, it should, at least, be regarded as provisional. And if it conflicts with known facts, whatever they are, it should, of course, be rejected.

### Precision, accuracy

The more precise and accurate its empirical predictions, the greater the possibility of experimental control of a theory. Vague or inaccurate theories risk of becoming unfalsifiable, which, as we have seen, would be fatal to their scientific credentials. On the positive side, to offer a precise, accurate theoretical description of reality it is usually regarded as one of the most important desiderata of science.

### Simplicity

When two or more theoretical alternatives are available to account for a given domain of facts, the scientists always chose, *ceteris paribus,* the simpler of them. The reason offered often is philosophical: the belief that reality itself is, in some sense, simple, and that therefore theories that are much too complex cannot, *ipso facto*, be true. But philosophers with positivistic leaning regard this association of truth to simplicity as "metaphysical", and therefore of no value to science. In any event, it is acknowledged by all sides that simpler theories are preferable for, at least, pragmatic reasons: it is of course easier, and often more fruitful, to deal with simpler theories than with complex ones.[7]

## Exploring the unknown: some general suggestions

### Stance toward new phenomena

Sometimes fundamentally new phenomena are observed after being predicted by a scientific theory. As we saw in the preceding section, this is often interpreted as particularly strong evidence that the theory is at least approximately true. It may also happen, however, that new empirical facts are discovered accidentally, or by suggestion of non-scientific factors. In this case, the scientists' task is to search for a scientific theory capable of accommodating the new phenomena. But if the phenomena are of such a nature as to diverge entirely of the usual ideas, i.e., if they look too much "strange" in face of the received paradigms, a common reaction from the part of many scientists is simply to reject the very reality of the phenomena, claiming that they "must" be illusory or even fraudulent. Although a certain amount of scepticism concerning new phenomena is always salutary, the outright rejection of the reported facts as "impossible", *without further examination*, is a dogmatic stance highly detrimental to science.

But an equally regrettable fault results from the diametrically opposite stance: that of the blind, uncritical acceptance of reports of new phenomena, often in name of "open-mindedness". In fact, here, as in the preceding case, prejudice is the real motive. And prejudice – either for or against an idea – is never to be tolerated in science. History of science abounds of cases in which it led either to blockage of research for long periods of time, or to false starts and waste of scientific energy.

In psychiatry, for instance, the modern studies on the relationships between religiousness and mental health, begun in the 1980's, suffered from strong resistance partly determined, it seems, by dogmatic attachment to received doctrines.[8] And the opposite stance appears to have somewhat muddled the research on the effects of intercessory prayer. The first reports of positive effects in controlled clinical trials led several researchers enthusiastically to conclude that the received "paradigms" had been refuted, whereas, in fact, subsequent studies on the alleged phenomenon have not been able to reproduce the initial findings (Masters et al., 2006). Thus, further inquiry, both experimental and theoretical, is needed before sound conclusions can be drawn.

### Framing new theories

Once agreement is reached on the reality of the phenomena and on the incapacity of the extant theories to satisfactorily account for them, it is time to devise new theories. Science cannot be limited to fact-gathering. In framing the new theoretical framework, a strategy that often works is to try first to develop a phenomenological theory, capable of establishing correlations between the main phenomena.[9] But as a rule science does not stop at this point. Scientists typically ask for explanations, and in their search they enjoy almost unlimited liberty. Explanatory theories are, as Einstein remarked in a lecture on the method of theoretical physics, "free inventions of the human mind" (Einstein, 1954, p. 272).

But although free in their genesis, theories must, once proposed, be subjected to stringent experimental and theoretical control. It is here that evaluative criteria such as those mentioned in the preceding section become crucially important. Their neglect often leads to a series of methodological flaws, capable of impairing the advance of research. One could mention, for instance, the formulation of hypotheses lacking clear empirical consequences; hypotheses conflicting with known phenomena or with well-grounded theoretical principles of other domains of science; isolated hypotheses, or

7 An accessible philosophical analysis of the value of simplicity in scientific research, as well as of several other topics discussed in this section, can be found in the classic Hempel, 1966.
8 See Swanson, 2003. Particular examples are discussed in Moreira-Almeida et al., 2005.
9 For an example of a phenomenological study of the association between religiousness and mental health, see Moreira-Almeida et al., 2006.

hypotheses forming an incoherent theory; hypotheses with a narrow scope of application; hypotheses that are much too complex; *ad hoc* hypotheses, i.e., hypotheses specifically devised to account for phenomena already known, but without any power of leading to the prediction of new ones; etc.

A different kind of problem may arise when someone, tired of examining a series of failed attempts to develop a satisfactory theory, concludes that, *ipso facto*, the phenomena themselves are not worth investigating, or even that they are not real.[10] But the rejection of a phenomenon in virtue of our present incapacity of explaining it is not a valid scientific move, of course. The inexistence, at a certain moment, of a good explanation for a phenomenon does not imply that such an explanation will *never* be found. And even if, as by absurd, we assume that the phenomenon is indeed completely unexplain*able*, this still would not mean that it is illusory, or should be neglected. Phenomena, whatever their nature, should always have epistemic priority over theories; they come first in the order of knowing.

### Dogmatic rejection of new theoretical possibilities

Dogmatism may lead not only to the rejection or neglect of phenomena, but also to potentially important theoretical insights. Blind attachment to the received theories often induce scientists to forget that no scientific explanatory theory is immune to falsification by further experimental or theoretical evidence, and that therefore theories, whatever they are, do not constitute an absolute, infallible standpoint from which to judge other theoretical proposals. Provided a hypothesis or theory is logically consistent and empirically adequate, it has credentials for, at least, being tentatively explored. By drawing attention to the episodes of theoretical rupture – "scientific revolutions" –, Kuhn has helped to keep scientists alert against the dogmatic rejection of new theoretical possibilities.

Special care should be taken to avoid the potentially blocking effects of the uncritical adhesion to background philosophical assumptions, such are the classical metaphysical positions on the nature of human beings: dualism, idealism and materialism. Although it is now fashionable to take dualism as a focus of heavy criticism, with the accompanying promotion of materialism to the condition of ultimate truth,[11] little or no notice is taken of the fact that philosophers have convincingly established long ago that all the three positions are on the same boat, as regard their strictly hypothetical character.[12]

### Precipitancy in accepting new hypotheses

An opposite problem is the haste and carelessness with which hypotheses are sometimes accepted by their originators or sympathisers. This stance is detrimental not only to their reputation, but also to the hypotheses themselves. Instead of being rationally examined, the hypotheses become object of passionate, scientifically sterile discussions. Based on experimental reports of positive association of religious involvement and health, for instance, several researchers have concluded that the association was due to causes beyond the known natural laws. Although this theoretical possibility deserves further inquiry, one should not lightly exclude the more conservative alternatives for explaining the data: social support, optimism, healthier behaviour associated with religious involvement (Levin, 1996, Levin et al., 2005, Moreira-Almeida et al., 2006.), notwithstanding the fact that a recent review article has not found empirical evidence capable of offering adequate support to such classical bio-psycho-social explanations (George et al., 2002). Thus, the only thing that is clear here is that more research is needed, not only to improve the empirical basis and refine the existing hypotheses, but also, hopefully, to provide new theoretical insights.

### Genetic biases in theory evaluation

The evaluation of new scientific hypotheses or theories is often biased by facts concerning its provenance. In particular, the authority of the originator of a theoretical proposal tend to exert undue positive influence in its adoption by the rest of the scientific community. The opposite case is also possible: the very fact that a hypothesis has not won the approbation of certain scientists may lead to the unwarranted conclusion that it is not worth further inquiry. In making these points we are not, of course, belittling the scientific credentials of the leading figures in science, who have won the respect of the community by their perspicacity and accumulated experience. We are just calling attention to the fact that hypotheses should stand or fall by their intrinsic scientific value, such as appraised in the light of epistemic criteria such as those enumerated in Section 5.

More generally, the context in which a hypothesis was invented may also influence its evaluation, either positively or negatively. Since, as we have already remarked, the framing of hypothesis involve creativity, it is natural that they are first suggested by factors that are not strictly rational. Dreams, metaphysical or religious beliefs, social pressure, lucky accidents, for instance,

10 This has been a typical stance taken toward the controversial homeopathic phenomena, for instance. In a paper recently published in The Lancet, the author says: "We question the results of a randomised trial of homoeopathy because we know that pharmacological action of infinite dilutions is highly implausible." (Vandenbroucke, 2005, p. 691.) And in an earlier review article in the British Medical Journal Kleijnen et al. write: "The amount of positive evidence even among the best studies came as a surprise to us. Based on this evidence we would be ready to accept that homeopathy can be efficacious, if only the mechanism of action were more plausible." (Kleijnen et al., 1991, p. 321.)

11 Kenneth Kendler, for instance, states without further ado that "Cartesian substance dualism is false", and that "in accord with an overwhelming degree of clinical and scientific evidence, we should conclude that the human first-person world of subjective experience emerges from and is entirely dependent upon brain functioning" (Kendler, 2005, pp. 433-434). For a critical assessment of the usual derisive claims on Cartesianism in the medical literature, see Brown, 1989 and Duncan, 2000.

12 For the classical analysis of this point made by John Locke (1632-1704), see Chibeni, 2007. For a recent, detailed critical discussion of monistic materialism, see Kelly et al., 2007.

are often instrumental in giving scientists the needed insights for the development of theoretical ideas. None of these factors should, however, count in the evaluation of the final product of scientific inventiveness.[13]

Attention to a particular form of this problem, occurring in the field of religion-and-health, has recently been drawn by McKay (2004). As a simple example of what he calls "the genetic fallacy", he considers the unwarranted inference that God does not exist from the fact that, according to some psychodynamic theories, belief in God derives from certain forms of psychological wish-fulfilment. McKay cunningly remarks that the same fallacious patter of reasoning could be used to *defend* the existence of God, since one could argue, according to a certain opinion, that atheism derives from sentiments of revolt or disappointment of some people toward their believing parents.

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Although the above list includes some of the methodological guidelines that we regard as among the most important in exploring new territories, it could, of course, be extended and refined philosophically, taking into account, for instance, the nuances characterizing each specific area or topic of research. Our main goal in offering these suggestions is to stimulate further reflection, modestly contributing, we hope, to promote the research and discussions on such difficult and controversial issues to a higher scientific standard.

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13 A philosophical proviso should be made here. The context of discovery may be relevant to theory evaluation in the particular circumstance in which some of the experimental predictions of the theory are observed after the genesis of the theory. In this case, the evidence the observation offers to the theory is usually regarded as stronger than it would be if the temporal order had been the reverse, i.e., if the theory had been formulated in full knowledge of that fact. Thus, in certain cases historical details may count for the appraisal of the theory (Chibeni, 1996, 2006).