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Paolo Parrini
University of Florence

The links between science and philosophy are quite close, but they are neither simple nor ''peaceable''. On the contrary, they are complex and very often conflicting. Among the diverse points of view from which it is possible to discuss these, (ethical and bio-ethical, ontological, metaphysical, pedagogic, anthropological, sociological, psychological, epistemological), my intention is to choose the perspective which most fits my area of knowledge, that is, the epistemological viewpoint. I will address the question by distinguishing between its historical and theoretical dimensions.

The historical dimension

Many scientific disciplines began initially as branches of philosophy before gradually separating themselves from it by a more or less long and complex process. For example, the major work of Newton, which together with the great contribution made by Galileo marks the origin of physics as it is practised today, bears the title of *Philosophiae naturalis principia mathematica.* Since the appearance of that work, however, physics has followed a pathway which has thoroughly freed it from speculative thought. Something analogous has happened in other domains of what once constituted philosophical knowledge. This has happened for example for biology, for the science of society which would lead to sociology, for the studies of the mind which would become psychology (and in some cases, a scientific psychology with mathematised quantitative aspects).

Logic constitutes an interesting case in itself. If on the one hand, it has known since around the middle of the 19th century a process of symbolisation which has led to the transformation into ''mathematical logic'', on the other hand it has maintained a solid link with problems of ontology, gnoseology and deontic logic. Logic, therefore, has in some senses become one of the multiple sectors of mathematics, but in others has retained a philosophical character.

The historical process just described has been sometimes illustrated through a quite famous metaphor. In the beginning – so it is said – philosophy was a great empire which, with its provinces, embraced the large part of all known territories; progressively, however, it began losing those territories which became completely independent of the central state, taking on thereby an autonomous status. This metaphor nevertheless conveys only the most obvious outward aspect of a process that is in reality much more complex and interesting. The metaphor fails to express (and neither gives a hint of) the fact that, even after the process of separation of the peripheral

Corresponding author:
Paolo Parrini, Department of Philosophy, University of Florence, via Bolognese 52, 50139 Florence, Italy
Email: Parrini@unifi.it

disciplines, links between the two modes of investigation have persisted and have been effected in both directions: both from science to philosophy and from philosophy to science.

From science to philosophy

This first direction manifests itself with particular clarity in relation to what might be called the most advanced case of liberation from philosophy, that of physics. From the very beginning, Galileo undertook research which covered not only problems of an experimental and mathematical nature (judiciously conceived experiments and necessary demonstrations), but also questions that were in a broad sense philosophical, for example, what should one study, how to study this something and how to speak of it. In his mind, how one should describe something in words, what methodological principles and theoretical and ontological standpoints to adopt on the nature of the object under investigation, were intermeshed in a strict sense with the scientific questions. And Newton, in his *Principia mathematica*, addressed, alongside the mathematical and physical questions, problems of methodology and ''metaphysics'' such as, for example, whether space and time have an absolute character or else one that is purely relative. Over the course of the following centuries this ''philosophical'' component of the discipline would become even more marked, such that, at a certain point, it would be recognised that physics, besides having taken over for itself many of the topics typical of philosophy, had also obliged this latter to modify some of its fundamental presuppositions.

Let us consider, for example, what has occurred around relativity and quantum mechanics. Both these theories have brought about a profound alteration to the previous framework of philosophical thought, and this from a dual point of view. Firstly, they have quite independently addressed certain fundamental speculative questions such as the nature of space and time, the causal structure of things or hypotheses on the origins and future of the universe. In the second place, contesting the absolute *a priori* validity of whole sectors of traditional knowledge (Euclidean geometry, causal determinism or some principles of classical logic), they have provoked a crisis for certain philosophical theories of major import such as the Kantian conception of synthetic *a priori* judgement. To say nothing of the results drawn from ethology, which have led Konrad Lorenz to elaborate a biological theory of the *a priori* which is different from that of Kant.

It was for such reasons in the first decades of the 20th century that, to those who were lamenting the absence in our generation of great philosophers, Adolf Harnack replied that such regret had no foundation. In fact, he said, there were still great philosophers about, but they were not to be found in the Departments of Philosophy but in those of Physics and answered to the names of Einstein, Planck, Heisenberg etc. (Schilpp 1951: 99). Which equated to saying that, in more recent times, philosophy not only had lost some of its most important provinces but in many senses had been completely ''replaced'' by those of its former provinces which had gone on to greater development. Progress in the natural sciences had been of such significance that any serious speculative research could not in any way leave their achievements out of account.

The logical empiricists – always particularly sensitive to their relationship with science – ended up by advocating and then attempting to found a philosophy ''interwoven'' with the findings of specific sciences. Taking up and further extending a thesis of Helmholtz, Moritz Schlick would go so far as to say, in the *Allgemeine Erkenntnislehre* (1918, 1925[2]), that philosophy is born when scientific discourse is taken to its extreme consequences, and that such radicality can be put into effect by the scientist himself whenever he finds himself in the situation of bringing about a change to the base principles of his discipline (Schlick 1925: 3–4). This is what happened with Einstein, for example, when he developed the theory of relativity. If Einstein can be spoken of as a ‘‘philosopher-scientist’’, it is not because, or not only because, he liked to ‘‘philosophise’’ about science, but because he philosophised while he was doing science and precisely in order to do it! It was not for nothing that another (future) logical empiricist, Hans Reichenbach, discussing in 1921 the philosophical significance of the theory of relativity, would say that his own goal, and that of thinkers close to him like Schlick, was not that of ‘‘integrating the theory into some philosophical system’’ but rather of ‘‘formulating its philosophical consequences independent of any particular point of view and assimilating them as a permanent part of philosophical knowledge’’ (Reichenbach 1921/1959).

From philosophy to science

Nevertheless, on the historical level the relations between the two disciplinary areas did not systematically develop to the sole and distinct advantage of science. Even in recent periods, philosophical speculation had continued to exercise a profound influence on the development of the sciences. In the first place, the critical stance that typifies philosophy has been a fundamental ingredient of some of the most significant moments of transformation and growth of the scientific enterprise. From this point of view there are particularly numerous examples that may be quoted and which deserve to be discussed at certain length.

The most interesting case is found once again in the genesis and development of the theory of relativity. Einstein indeed declared several times that he would not have been able to arrive at his ‘‘revolutionary’’ ideas about space and time (with everything that followed from that on a philosophical level) had he not previously accomplished an empirical-operational analysis of the notion of simultaneity at a distance, ‘‘modelling it’’, so to say, both on Hume’s critique of causality and induction and on Ernst Mach’s critique of absolute space, time and motion. On to this analysis was then grafted the influence of another philosophical-epistemological component central to his discourse which was the conventionalist option of Poincaré (and to a certain extent of Pierre Duhem) which served him as a guide, not only in the formulation of the special theory of relativity, but also in the discussion of the relations between geometry and experience which is at the heart of the theory of general relativity (Schilpp 1951: 12–13, 20–21, 678 ff.). It should be noted besides that both this critical-operative stance along with conventionalism – notions which, furthermore, as I shall shortly go into in greater detail, do not exhaust the philosophical dimension of Einstein’s scientific work – would also provide dynamic impulse for the first steps towards quantum mechanics. They would in fact have a decisive influence – together with the idea that it is theory that determines what can finally be observed – on the direction followed by Heisenberg in eventually arriving at his uncertainty principle.

Another relevant example arises from 20th century Italy. There, from the 1930s, the mathematician Bruno de Finetti developed a concept of probability of a subjectivist type. Initially considered lacking in plausibility, with the passage of years or rather, of decades, this concept has become one of the most important theories of reference in present-day international debate, so much so that de Finetti’s papers have been acquired by the University of Pittsburgh and placed in the Archives of Scientific Philosophy alongside those of many other exponents of the so-called ‘‘scientific philosophy’’ of the past century (Ramsey, Carnap, Reichenbach, Hempel, Feigl, Salmon).

Now underlying de Finetti’s theory are two types of newly elaborated concepts: some are of a mathematical nature and are linked with important theorems such as that of Bayes, but some are philosophical in nature. To arrive at his notion of probability as a degree of the subjective reliability of certain events taking place, de Finetti makes use of what he had learned from the pragmatist philosophers Giovanni Vailati and Mario Calderoni, namely that ''a valid definition of a quantity having a sense (from a methodological, pragmatic and rigorous point of view) [...] may not be constructed on more or less empty or highly refined turns of phrase, but must be *operative*, that is, based on the indications of what experiments – be they purely conceptual experiments – should be followed to determine its measure (de Finetti 1989: 172 ff.). The degree of subjective reliability by which de Finetti characterises the probability is precisely numerically quantifiable by reference to the risk that one is prepared to run by entering into, or not entering into, a wager (or a series of compound wagers) effected with respect to the logical condition of coherence (indispensable for avoiding wagers of the ''Dutch'' kind, being wagers whose result would be a certain loss for the person who incautiously accepted it).

But philosophy has influenced scientific thought also in relation to aspects of content. Here too the examples that could be cited are relatively numerous. The most remarkable is perhaps that associated with the birth of non-Euclidean geometry. As is well known, the elaboration of geometric systems of this type has been one of the principal reasons for the crisis that has smitten various traditional philosophical concepts based on the intuitive evidence of some logical and mathematical truth, and hence on their universal and necessary validity (we may think, for example, of the Kantian concept of mathematics as a collection of synthetic *a priori* judgements). Less well known however is that of one of those who have most contributed to the construction and rigorous systematisation of the mathematics of non-Euclidean geometry, Bernhard Riemann, who developed his work under the influence of the theory of curved surfaces of his master Gauss – the ''prince of mathematicians'' – but also of the philosopher Herbart.

If one reads the key work of Riemann, *On the hypotheses which lie at the foundation of geometry*, one may well observe that this work opens with the distinction between discrete and continuous manifolds. To the continuous manifolds belong space and time, but this space and this time are no longer conceived, in Kantian fashion, as intuitions, and more specifically as pure formal *a priori* intuitions, but rather as concepts according to the model defended by Herbart against Kant. In particular, space becomes a concept which includes within itself a multiplicity of possible spaces (both Euclidean and non-Euclidean). And it is precisely on the basis of such a changed philosophical perspective that Riemann succeeded in dealing with the problem of continuous manifolds in all its generality, and to conceive the possibility of an infinite multiplicity of spaces, of which the Euclidean is but a particular case (even if the most significant).

The development of these mathematical doctrines has been an essential component in the passage from Newtonian physics to the theory of general relativity. And today, around a century since the emergence of the relativist idea, the debate (both in science and philosophy) is still vigorous and tight, whether around the problem of *a priori* knowledge, or on the nature of space-time – and in this debate there meet head-on a broad variety of theoretical options passing from absolutism to relationism, from realism to relativism, conventionalism to objectivism, apriorism to empiricism.

An analogous commentary could be made for the debate around the interpretation of quantum mechanics, where, among other things, the recourse to non-classical logic and the controversies on the very nature of logic itself, in particular on the semantics of possible worlds, play an important role. Then recently a new front opened: that of the debate on the objective value of those theories in which use is made of notions such as symmetry and invariance which are grounded either in aspects of the most recent theories of physics or in the philosophical concepts elaborated by Ernst Cassirer in the first half of the 20th century (and in other similar ones). As can be seen, in all these cases in the work of analysis and in-depth investigation one notes the participation, though granted applying different modalities and with different end-points in view, of researchers from both the disciplinary areas.

Another relevant example of the active role played by philosophical research in relation to science has been offered precisely in these last decades by the problem of reductionism. When declarations are made, say, that mental states are reducible to cerebral states, or else that the expressions of biology are reducible to those of chemistry, and these latter, in their turn, to those of physics, all of these theses not only refer back to empirical-factual knowledge associated with the disciplines from or to which the reduction occurs (psychology, the neurosciences, biology, chemistry, physics) but also pose the problem of the general notion itself of what is reduction. What might be understood by reduction from one discipline to another, from one domain of knowledge to another? Should we speak of epistemological reductionism, ontological reductionism, or both? And what instruments enter into play in a work of reduction? These questions we have just mentioned are not of an exclusively scientific nature; they are also of interest to and within the competence of philosophy (see for example Lanfredini 2003). Questions, besides, which in many cases philosophy in its turn may well be capable of addressing with the sole help of logic. And in fact it is in logic, as recent developments in it have shown, that can be found formally refined instruments that are effective for analysing the relations between theories, including those useful for elaborating a solid reduction theory.

There is more. Logic can also serve to establish the nature of the relationships that link chronologically successive theories and which relate to what seems to be the same area of phenomena. For a long time it has been accepted in relatively non-controversial fashion that the succession of theories like Aristotelian physics followed by Galilean-Newtonian physics then the physics of relativity could be considered as a passage from conceptual structures that were more specific to those of a greater generality. Under this view, the earlier theories would have been, as it were, retained or incorporated into those which followed as particular cases of these latter, retaining their validity but under more restrictive conditions, hence over more limited fields of phenomena. From such a perspective, for example, the special theory of relativity extends beyond classical physics, but at the same time sanctions the validity of the latter for relatively small velocities relative to the speed of light.

Some epistemologists and historians of science have nevertheless qualified as simplistic this way of looking at scientific change. Instead, they have advanced the famous thesis of incommensurability, according to which theories of broad span like those cited above have to be considered no longer as concepts that may be incorporated within each other, but as intellectual constructions implying a different way of constituting reality. When a scientific revolution comes about which causes the base paradigms to change, as Thomas Kuhn (1970: 111) went so far as to declare, there is a need to say that it is the world itself that is changing with them. Well, a way of seeking to avoid conclusions as drastic and extreme (and with all the problems that they pose for the objectivity and validity of scientific statements!) consists precisely in perfecting models of formal logic that can clarify the possible relations between the various theories. It seems to me almost futile to add that all the discussions that I have mentioned to this point, including the debates in the philosophy of mind and in the neurosciences on the validity of the reductionist and eliminativist theses, can only with difficulty admit responses that leave out of account answers that we give to the philosophical problem of the nature of knowledge and of how and how much we can know.

The theoretical dimension

With this I have now got to the heart of the other perspective from which all relations between science and philosophy should be considered, the theoretical perspective. In this regard we cannot but begin from the famous comment of Einstein to the effect that ‘‘the reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is – insofar as it is thinkable at all – primitive and muddled'' (Schillp 1951: 683–684).

Einstein's words admirably express the following fact: the historical aspects of the science-philosophy relationship – those which I have been discussing up to this point – are but the expression of what might be called ''structural'' characteristics of this relationship, at least in the form that this has taken up till now. It clearly seems, then, that there is an inherent or intrinsic philosophical value embedded in the nature of many scientific advances, and it thus comes about that such advances are in turn important stimulus factors for philosophical investigation. On the other hand, to the very extent that the various sciences assert their independence and directly invade territories once the exclusive domain of philosophy, they continue to draw sustenance in various ways from the discipline that they have ''dispossessed''.

I find it quite significant, for example, that the two Italian researchers to whom we owe the discovery of the mirror neurons (which permit us to better understand the problem of inter-subjectivity) have repeatedly declared that they had been ''carried off'' despite themselves into the camp of philosophy. Or else from the study of the structure of the more radical changes in scientific thinking (changes that many might call ''revolutionary'') it has emerged that these changes involve in themselves the re-examination, overtaking and finally the substitution of the theoretico-philosophical presuppositions – called by some the ''metaphysical assumptions'' – of the superseded paradigm. It might be worth recalling that even a philosopher like Heidegger, for whom ''science does not think'', has proved to be conscious of this fact (Parrini 2009). In summary, contrary to what is sometimes suggested, philosophy is not to be found merely in the interstices of scientific discourse; it can also be located within the constituent elements themselves of scientific theories and in their implications.

The tensions between science and philosophy

It is at this exact point, however, that the sharpest tensions between the two disciplines come to light, tensions that are particularly acutely felt in countries like mine, Italy, which cannot claim a robust epistemological tradition on their own account. Among the not-infrequent obstacles which render mutual understanding and interaction difficult, I will limit myself to indicating here just a couple of the more significant ones.

To start with, there are difficulties of what we might call a ''socio-cultural'' origin, to which I will return in the concluding section. Many scientists, it may be said, pass judgement on philosophy without any sufficient knowledge of it, while, from the other point of view, many philosophers have the bad habit of making pronouncements on science (very often felt to be their enemy) of which they possess an almost always generic, if not to say imprecise and sometimes even caricatural, image. Especially in Italy, too many of those who work in the field of philosophy take scarce account of the lines of Giacomo Noventa: ''A poet can make night from day, a philosopher no ...'' even when they trespass on to the territory of logic, physics and the neurosciences. And this produces outcomes that are in many senses deplorable, whereas in the past, it has been precisely philosophers who have sometimes contributed to removing obstacles that, if not completely impeding, were at least retarding the development of a scientific theory. One may think, for example, of the influence brought to bear in the 1920s by the young philosopher of physics, Hans Reichenbach, in promoting the cause of the theory of relativity by his brilliant contributions to the debate around it, which now appear collected in the book entitled *Defending Einstein* (2006).

In second place – and this is what one might call a ''substantial'' problem and certainly much more important than the previous one – there is not always a sufficient awareness of a fact which is in reality quite simple: that scientists and philosophers, while they can indeed have something or even much in common, still proceed according to their own particular methodologies and interests, which are relatively different one from another. For a philosopher, what counts most is the analysis of concepts and the search for perspectives which incline towards ''totalising'' statements. Let us take for example a doctrine in logic such as Bertrand Russell's theory of definite descriptions. It would be impossible to understand the genesis and development of this theory without taking account of the philosophical problems which assailed its author in the years when he was working on his *Principles of Mathematics*; and it would be difficult to understand Russell's decisive orientation towards a certain solution rather than to others (that of giving a truth value to statements containing nominal phrases that do not denote anything, rather than the prospected solutions of Frege and Hilbert) were one to leave out of account his exquisitely philosophical desire to eliminate, or at least to limit, the Platonist compromises of mathematical and common discourse.

In reverse, for a scientist it is the ''growth'' of his own discipline which is of most account. Thus, for a mathematician, his or her fundamental preoccupation will be with the development of mathematics itself, independent of any potential abstract ontological overlay, and of any logical contradictions that could derive from an excess of Platonism (at least to the extent to which these contradictions do not interfere with the work in his field). If we then take a practitioner of this or that empirical science, for that scientist above all will count the data derived from experiment, logically and mathematically sound proofs and the careful examination of well delimited or circumscribable conjectures. Furthermore, his or her strongest hope will always be to find solutions which might avoid the recourse to conjectures and hypotheses which go beyond the factual evidence currently at hand. *I don't craft hypotheses*, Newton used to declare (in Latin), even if even he did in fact make some, and some quite tightly binding ones at that, as would emerge later from the epistemological analyses of Mach, Poincaré, Duhem and, in some ways, even of Einstein.

Another significant example of such (relative) heterogeneity of interests arises from the dualism between the *scientific image* and the *manifest image* of the world, a dualism emphasised in the last half-century particularly by Wilfrid Sellars. The astronomer Arthur Eddington, at the beginning of one of his famous works, had declared that, to write his essay, he had sat down before his two work-desks: the desk whose image Sellars would label the manifest image, made up of pieces of wood having certain visible properties and interconnected in an established pattern, and the desk presenting the scientific image according to which, as sub-atomic physics tells us, is a swarm of electrons governed by certain laws. Contrary to how much this might matter for scientists (or for the majority of them) who have no particular motive to concern themselves with such dualism, for a philosopher it is quite pertinent to understand, we assert, whether we must resign ourselves to accepting the existence of the dualism or whether there is a sense in which we can continue to say that, despite the distinct difference between the two types of images, the desk in front of which we are sitting is one and the same desk.

To take this argument a little further we can turn again to Einstein, and in particular to the famous passage in which he sets out his own approach of ''methodological opportunism'', saying that it is typical of those who pursue scientific research in comparison to those who practise philosophy. In the same text in which appears the statement quoted earlier that a science without epistemology is primitive and shapeless and that an epistemology not fertilised by contact with science is empty, Einstein went on immediately to say:

> However, no sooner has the epistemologist, who is seeking a clear system, fought his way through to such a system, than he is inclined to interpret the thought-content of science in the sense of his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. He accepts gratefully the epistemological conceptual analysis; but the external conditions, which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist: he appears as a *realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as the free inventions of the human spirit (not logically derivable from what is empirically given); as *positivist* insofar as he considers his concepts and theories justified *only* to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research. (Schilpp 1951: 684)

From my way of thinking there is a lot of truth in these words. It is precisely therefore when one ventures into problems which are of interest as much for science as for philosophy that it is necessary to take great care not to start off on the wrong foot. First of all, philosophers should take care not to evaluate the work of scientists on the basis of their own demands for systematicity and exhaustivity, however legitimate these may be in their field. Not to abstain from making such evaluations renders a poor service to both disciplines. Philosophy in such circumstances will be constantly put at risk of resounding refutation on the part of science, which would be seeing itself judged on the basis of demands of a normative type which, if taken seriously, could not avoid being converted into the imposition of absurd limitations to the directions which scientific research might take.

On the other hand, it is equally as important that scientists avoid the practice of giving legitimacy to philosophy's reflections on science only to the extent that these reflections bring philosophy to address specific scientific problems with the intention of contributing to their solution. Scientists, in other words, should learn to accept that philosophy has legitimate clarification and reconstruction goals of its own. It is thoroughly obvious, for example, that a scientist, to accomplish his work, does not need to undertake a preliminary reflection on the possibility conditions of knowledge and even less to construct a carefully argued response to potential scepticism. But it would be hardly sensible to infer from that (as do some scientists and even – alas! – some philosophers) that the problem of scepticism is devoid of any conceptual interest and addressing it cannot bring any useful contribution to the development of knowledge. In proposing a reply to this problem or in constructing a better formulation of the sceptical position, philosophers are able to activate ideas that even the scientist would find interest in. This is not an issue that can be decided *a priori*. Besides, was it not Einstein himself, as we recalled a little earlier, who recognised the influence upon him of Hume's analyses of causality and of the ''incorruptible scepticism'' from which point of view Mach had conducted his own historic-critical analysis of mechanics?

One might even add that the diffidence of the scientist towards the demand for systematicity that characterises a good part of philosophical investigation can also sometimes arise from the desire to be allowed to keep ''hands free'' in grappling with the more thorny questions of science. In developing a theory for his own methodological opportunism, Einstein was thinking especially of the controversies in which philosophers were embroiled; but it could be observed – with a pinch of malice – that that opportunism was quite useful for him in the battle that he was at the time conducting in favour of realism and causal determinism (''God does not play dice'') against the interpretation of quantum mechanics of the Copenhagen School. Proponents of that school like Bohr and Heisenberg had not hesitated, in order to give support to their own antirealism and indeterminism, to themselves appeal to the operationalist component of the position that Einstein had adopted in formulating the theory of special relativity. And this even though they had then tempered their operationalism with a healthy dose of rationalism, having given out (and so continuing to fall in line with other Einsteinian precepts) that it is up to theory to determine what may be observed. Thus, specifically with the purpose of challenging the positions of the Copenhagen School, Einstein had declared – ''opportunistically'', in effect – that a good game cannot be repeated, i.e. ''played'' more than once; which meant, stripping away the metaphor, that he considered it improper that there should be any attempt to ''nail him down'' for reasons of systematicity and coherence to a methodological position (operationalism) adopted once and for all. Seeds of the methodological anarchism of Paul Feyerabend were already clearly present in Einstein's opportunism.

Areas of intersection between science and philosophy

Therefore, taking it as read that the positions and fundamental interests of scientists and philosophers are also in all legitimacy in some respects quite diverse, I would like now to make some observations on the areas of intersection of the two disciplines, or more exactly between philosophy and the various sciences separately. To limit myself to the most general aspects, I would say that there are above all two ''zones'' in which the work of analytical elucidation and (more or less) systematic reconstruction of epistemology and of philosophy in general comes together (again more or less) with the work that is proper to scientific investigation.

Epistemological clarification and notions of scientific discourse – A first area of encounter is presented by all those thematic aspects which can be included in the field of the philosophy of science. Such thematics, naturally, are not of *immediate* interest for the scientist. For example, while both the scientist and the philosopher of science will be interested in scientific explanations, in laws of nature and in theories, the primary interest of the scientist will almost certainly be to arrive at a formulation of these and to discover nomological connections between the phenomena; that of the philosopher of science, on the other hand, is to clarify what in fact should be understood by the notions of ''scientific explanation'', ''laws of nature'' and ''theory'', if anything with one eye on the (thoroughly philosophical) enterprise of pointing out possible differences between a theoretical construct of a scientific nature and a concept of a metaphysical nature (see for example Boniolo 2001, Casamonti 2006, Peruzzi 2009).

Another area of convergence – and one which is potentially quite thorny – is represented by the epistemological study and possible formulation of criteria concerning the inductive confirmation and probative value of experiments. I say it is potentially thorny because the analysis undertaken by the philosopher of science can even flow on to questioning the scientific credentials of a theory (one may think of the thorny case of psycho-analysis) or the modalities under which a scientific experiment has been conducted. Of particular interest, for example, is the discussion on the probative value of the experiment of Benjamin Libet. Studying the relationship between conscious experience and the activation of certain cerebral zones, Libet came up with results that for some might imply at least a limitation in the traditional function accorded to free will.

Philosophy can furthermore deal with notions that occur both in philosophical as well as in scientific discourse. At the present moment for example, both the philosophy of mind and science are making their own contributions to the understanding of complex phenomena such as the nature of consciousness and of mental states, and hence to the attempt to answer problems as old as the contrast between materialism and spiritualism, between dualism and monism (that is, between the existence of two autonomous substantive entities: the mind and the body, or the existence of only one of these), between determinism and free will, between reductionism and anti-reductionism (reducibility of the states of consciousness to cerebral states or the impossibility of eliminating those subjective states referred to as *qualia*) (see for example Lanfredini 2003).

All these investigations involve not simply the reviewing of facts, discoveries, hypotheses and theories derived by science, but also the clarification, construction and reconstruction of certain key notions of philosophy. Take for example the thesis of the identity between subjective states and cerebral states. To sustain this with the greatest weight, one would need to have on hand empirical factual evidence that demonstrated that the verified observation of a particular state of consciousness (e.g. pain, or the perception of redness) is accompanied by the activation, according to certain modalities, of very precise nerve endings or cerebral regions. One would also need to know how should be understood the notion of ''identity'' in the general (and in certain senses generic) statement that certain states of consciousness are ''identical'' to cerebral states. Is it a question of identity of occurrence (*token identity*) or identity of *type*? Of a necessary identity or of a mere co-extensiveness that is empirically verifiable? In other terms, in asserting that identity, do we intend saying that subjective states are *essentially* cerebral states, whatever that may really mean, or only that to a certain state of consciousness there corresponds, as an item of empirical fact, a cerebral state? And in the latter case, is the co-extensiveness that we assert a co-extensiveness that is ''condensable'' into an empirical generalisation or a co-extensiveness that can be extracted from a complex theory of mental and cerebral life? It may be noted in passing, in reference to this latter possibility, that an interesting precedent can be found in physics regarding the empirical equivalence between inertial and gravitational mass. This equivalence in fact, with the advent of the theory of general relativity, can be drawn deductively from the principles that this theory is based on.

Another noteworthy example is represented in the notion of simultaneity at a distance in the special theory of relativity. I have already mentioned that Einstein in his 1905 paper, starting out from an operational analysis, established the necessity of introducing some new definitions and conventions. One of the principal arguments that this led to between scientists and philosophers (let's take the names of Eddington and Reichenbach as representative examples) is the following: in both special and general relativity, alongside the empirical element is there present or not an element of conventional type? In the case of special relativity, the discussion centred around how far it should go in recognising Einstein's assumption about the simultaneity of spatially separated events, that is, the assumption that under appropriate conditions the speed of light is the same on the trip from A to B as on the return trip, or, using the adopted symbolic representation, that ε is equal to ½. What they did try to clarify through this debate was whether, under this particular theory, there was or was not room for assumptions different from the standard assumption adopted by Einstein just mentioned (i.e. $\varepsilon = ½$). In 1977 the philosopher of physics David Malament, the former pupil of another important epistemologist Howard Stein, demonstrated that under the theory there would be space only for a single relation of simultaneity, to wit precisely the standard assumption. In 1999 however Malament's conclusion was contested by Sahotra Sarkar and John Stachel. Expanding on a suggested way of thinking of Einstein himself, they demonstrated that Malament's theorem ''carries a non-guaranteed physical hypothesis'', and that once this is removed, different relations of simultaneity would be possible even *within* special relativity such as to assure – according to their argument – a range of conventional choices (see Malament 1977, Sarkar & Stachel 1999, Calosi *in press*, Parrini *in press*).

Philosophical analyses and scientific research. The problem of specialisation

The debate around Malament's theorem and on the broader problems that this raises is still in progress, but it seems to me that given the state of play up till now it is possible to draw three important conclusions about the relations between philosophy and science.

The first is this: starting from a typically philosophical concern like that of the degree of conventionality present or not present in a scientific theory, it is possible to render much clearer the internal structuring of that theory and identify the possible more general and abstract presuppositions on which it is based. Such presuppositions are often presented as being obvious and do not come problematised. In return, bringing them to prominence can be of significant interest for scientific development. Sometimes indeed, the very explicitation of the ''tacit'' presuppositions of a theory giving a consolidated point of reference together with a critical reflection on them leads – with or without the emergence of new experimental data – to the construction of new theories endowed with superior adequacy and empirical reach.

The second conclusion concerns instead the potential advantages for philosophy. The question of the greater or lesser conventionality of certain scientific principles is closely linked to problems such as that of the cognitive value of the scientific theories and then of what should be the relationship between our theoretical representations and reality itself (the problem of realism). And all these thematic elements in their turn are connected to the so-called empirical underdetermination of theories, that is, to the idea that the acceptance of a scientific theory is not determined univocally by experimental data, but depends also on conventional options. The debate that, from the earliest discussions on relativity, has led on to and followed on from Malament's theorem clearly shows that the traditional controversy on the objective value of knowledge can emerge from the generalisation and approximation by which it was characterised for a long time, and find a solid anchorage in the concrete and specific problems of the philosophy of physics. Problems, that is, that can be treated with the same logical, mathematical and factual rigour with which scientific issues are treated.

I believe that scientists in general, and in this regard physicists in particular, should take care not to regard with too much disdain philosophical issues of this kind. From these there can derive consequences of significant importance for the position and status that science should be granted within the framework of human culture. It should not be forgotten that to ''reduce'' scientific concepts to pseudo-concepts, Benedetto Croce specifically made use of a particular reading of the theses on the objective value of science that had been advanced by scientist-epistemologists like Mach and Poincaré – a reading which in large part was a misreading of them (see Parrini 2004, ch. 2). And one might also think of the relevance of the same questions around the present-day debate (motivated essentially by religious concerns) on the validity of Darwinian evolution or around the epistemological sense to be attributed to the ''Galileo case'' (a sense which seems to me to have been disregarded in some recent, and sometimes quite solemn, reconsiderations of the matter).

The third conclusion regards the currently crucial theme of specialisation. This phenomenon has the power to constitute for the relations between philosophy and science a further factor of disturbance of the type I above called ''socio-cultural''. In certain regards specialisation can be considered one of the ills of our time, even one of the more redoubtable ones because it is a necessary and inevitable ill, closely connected with the advancement, and the very possibility of further advancement, of our knowledge. Furthermore, if it bears a danger for every type of discipline, in that it makes it ever more difficult to locate that discipline within a single all-encompassing idea of the cultural moment in which we are living, specialisation constitutes a real and particular threat for philosophy which, through its originating vocations, tends towards engaging views as broad and general as possible. And yet, discussions like this one on the place of conventions within a scientific theory show, it seems to me, that it is precisely the development of specialisation that can attenuate the ills brought about by that specialisation itself. I will briefly try to explain how.

More than once, when I have given a lecture on the topics presented here, I have felt myself objecting: yes of course, in principle it would be rather useful to develop closer relations between philosophy and science, but for this to happen in non-dilettante fashion there would have to be serious competence in both camps: and this is difficult to bring about because of the every greater specialisation of the disciplines, be they scientific or philosophical. True. But I think, as I have mentioned, that a remedy can come precisely out of that fact. The important thing would be to manage to create, one might say, a virtuous circularity, such as that which arose exactly out of the discussions on the philosophical implications of the theory of relativity. Certainly, a physicist interested in studying that theory can legitimately choose to disinterest himself in all the problems with which Malament's theorem is concerned; and just as legitimately a philosopher who in general devotes himself to the traditional characterisation of knowledge as justified true belief can also take no interest in them. The essential thing however is that then, if and when the physicist-physicist or the philosopher-philosopher start to talk, even if only on the level of journalistic popularisation, of the cognitive value of scientific theories or the contrast between realism and instrumentalism, they will do so knowing that conclusions like those of Malament do exist. The extent of correctness of a theorem like his can be sifted either by the physicist-physicist who for some reason had decided to make pronouncements on questions of a philosophical nature, or by those physicists who might regularly maintain epistemological interests, or, finally, by philosophers of physics like Malament; and to all the above, but especially to these latter ‘‘specialist’’ philosophers of physics, the ‘‘generalist’’ philosopher, our ‘‘philosopher-philosopher’’, will be able usefully to refer when he has felt the need to extend his interests to take in this problem.

It is from a similar intersection of serious specialist competences that can emerge something of real value for the development of relations between science and philosophy. Today perhaps, there are even in Italy the first few tentative steps being taken in that direction as regards a culturally (and mediatically) ‘‘hot’’ topic like the theory of evolution. And that allows a few glimpses of hope for a country like ours which up till now has been little open to the approaches indicated above and traditionally has been drawn instead to ‘‘rhetorical totalology’’ which seems on the contrary to live off an ‘‘intersection of incompetence’’.

Translated from the Italian by Colin Anderson

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