

# Having Science in View: General Philosophy of Science and its Significance\*

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*Quid tandem harmonia?  
Similitudo in varietate, seu diversitas identitate compensate.  
'The Confession of a Philosopher', G W Leibniz 1672-73*

## 1. Preamble

General philosophy of science (GPOS) is the part of conceptual space where philosophy and science meet and interact. More specifically, it is the space in which the scientific image of the world is synthesised and in which the general and abstract structure of science becomes the object of theoretical investigation.

Yet, there is some scepticism in the profession concerning the prospects of GPOS. In a seminal piece, Philip Kitcher (2013) noted that the task of GPOS, as conceived by Carl Hempel and many who followed him, was to offer explications of major meta-scientific concepts such as *confirmation, theory, explanation, simplicity* etc. These explications were supposed “to provide general accounts of them by specifying the necessary conditions for their application across the entire range of possible cases” (2013, 187). Yet, Kitcher notes, “Sixty years on, it should be clear that the program has failed. We have no general accounts of confirmation, theory, explanation, law, reduction, or causation that will apply across the diversity of scientific fields or across different periods of time” (2013, 188). The chief reasons for this alleged failure are two. The first relates to the diversity of scientific practice: the methods employed by the various fields of natural science are very diverse and field-specific. As Kitcher notes “Perhaps there is a ‘thin’ general conception that picks out what is common to the diversity of fields, but that turns out to be too attenuated to be of any great use”. The second reason relates to the historical record of the sciences: the ‘mechanics’ of major scientific changes in different fields of inquiry is diverse and involves factors that cannot be readily accommodated by a general explication of the major metascientific concepts (cf. 2013, 189).

Though Kitcher does not make this suggestion explicitly, the trend seems to be to move *from* GPOS *to* the philosophies of the individual sciences and to relocate whatever content GPOS is supposed to have to the philosophies of the sciences.

I think scepticism or pessimism about the prospects of GPOS is unwarranted. And I also think that there can be no philosophies of the various sciences without GPOS. Defending these two claims will be the main target of this chapter. Still, I do not want to contrast GPOS to the philosophies of the individual sciences. As I will show, there is osmosis between them and this osmosis is grounded on what I will call

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\* This essay is dedicated to the *Usual Suspects*: the mighty members of my research group in Athens, who have helped me for years with their ideas and criticism and who motivated me with this essay when I felt I was blocked. Many thanks to Paul Humphreys for useful comments.

‘Science in general’ and the two important functions GPOS plays vis-à-vis Science-in-general: an explicative function and a critical function.

## 2. What is Science?

There have been various public debates about the nature of science, the most prominent being in the early Victorian period with William Whewell and John Stuart Mill as the main protagonists (see Yeo 1993) and in the early decades of the Third Republic in France, with Henri Poincaré among others playing a key part in it (see Paul 1985). But it was in the first half of the twentieth century that the issue of a sharp separation of science from non-science or pseudoscience became a major philosophical-analytical endeavour.

### 2.1 Searching for criteria

The Logical Positivists aimed to reform philosophy by making it scientific; hence the issue of what counts as ‘scientific’ was taken to be urgent. Moritz Schlick equated ‘scientific’ with a certain way to be cognitively significant: “A proposition has a storable meaning only if it makes a verifiable difference whether it is true or false” (1932, 88). This came to be known as the verificationist criterion of meaningfulness. It rendered science distinct from metaphysics (*aka* speculative philosophy): metaphysics was meaningless, because unverifiable. But it also *reshaped* philosophy by taking philosophical assertions to be significant insofar as they are analytic: true in virtue of the meanings of their constituent words.

But are scientific assertions, *qua* scientific, verifiable? The answer depends on how exactly we should understand the (modal) concept of verifiability: a statement is strongly verifiable if its truth can be conclusively established in experience, whereas it is weakly verifiable if its truth can be rendered probable by experience. The result is that, depending on what exegesis we may accept, we will get different versions of the criterion of meaningfulness—hence of what counts as ‘scientific’. In the thought of Logical Positivists, ‘verifiability’ moved from a strict sense of provability on the basis of experience to the much more liberal sense of confirmability. This was a significant shift. Given a claim of provability, many ordinary scientific assertions, for instance, those expressing universal laws of nature, would end up being unverifiable; hence non-scientific. Not so given a claim of confirmability. But, by the same token, and given the typically holistic nature of confirmation, there was no longer a sharp distinction between science and metaphysics (see Quine 1951; Hempel 1951).<sup>1</sup>

Karl Popper (1963) denied that the evidence can have any bearing on the probability of a theory or a hypothesis, but he nonetheless argued that scientific theories can be falsified by the evidence. Popper took (the modal notion of) falsifiability to be the criterion of *demarcation* between science and non-science or pseudoscience. Scientific theories are supposed to be falsifiable in that they entail observational predictions which can then be tested in order either to corroborate or to falsify the theories that entail them. Non-scientific claims are not supposed to have potential falsifiers: they cannot be refuted. Unlike the Logical Positivists,

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<sup>1</sup> The first to make an effort to distinguish science and metaphysics was Pierre Duhem (1906). He defended the autonomy of science, by taking metaphysics to aim at explanation and science at description and classification.

Popper did not want to separate science from metaphysics. For him, scientific theories emerge as attempts to concretise, articulate and render testable metaphysical programmes about the structure of the physical world (cf. 1994). Still, there was supposed to be a sharp demarcation between science and pseudo-science.

However, given that in all serious cases of scientific testing, the predictions follow from the conjunction of the theory under test with other auxiliary assumptions and initial and boundary conditions, when the prediction is *not* borne out, it is the whole cluster of premises that gets refuted. Hence, it is not the theory *per se* that is falsified. It might be that the theory is wrong, or some of the auxiliaries were inappropriate (or both). As a result, any theory can be saved from refutation by making suitable adjustments to auxiliary assumptions. The point here is not that these adjustments are always preferable. They may be ad hoc and without any independent motivation. Hence the theory might be condemned, as Henri Poincaré (1902, 178) put it, without being strictly speaking contradicted by the evidence. The point, rather, is that *qua* a criterion of marking the bounds of the scientific, Popper's falsifiability criterion fails (see my 2012 for details).

Can we find solace in projects such as those associated with Thomas Kuhn (1970) and Imre Lakatos (1970)? Neither of them, to be sure, offered explicit criteria of demarcation. Yet they offered templates as to how science is structured and how it develops over time, which suggested that there are *structural* ways to capture the bounds of science. In (1977, 277) Kuhn suggested that "the surest reason" for claiming that some activity (e.g., astrology) is pseudoscientific is precisely that it lacks the right structure; that is, it is not governed by a paradigm-led puzzle-solving normal activity. But it follows that, from the point of view of an existing and established paradigm, a new rival and emerging theory is bound to count as pseudoscientific before it acquires the structure of a paradigm-led puzzle-solving normal activity; which is clearly something we do not want to accept.

Lakatos (1970) too developed a structural model of science based on the claim that the unit of appraisal is not a single theory but a sequence of theories known as a *Scientific Research Programme*. In this way, he aimed to improve on Popper's criterion, which was rightly taken to implausibly imply that theories are falsified and abandoned as soon as they encounter recalcitrant evidence. Lakatos emphasised the role of novel predictions in his own structural model. A Research Programme is progressive as long as it issues in novel predictions, some of which are corroborated. It becomes degenerating when it offers only *post hoc* accommodations of facts, either discovered by chance or predicted by a rival research programme. The price of this way to circumscribe the bounds of science is that there is no way to tell when a research programme has reached a terminal stage of degeneration. Even if a research programme seems to have entered a degenerating stage, it seems entirely possible (and it did happen with the kinetic theory of gases towards the end of the nineteenth century) that it stages an impressive comeback in the future.

Expressing the sentiment of despair that was capturing philosophers of science after the so-called 'demarcation debacle', Larry Laudan noted in 1996:

The failure to be able to explicate a difference between science and nonscience came as both a surprise and a bit of an embarrassment to a generation of philosophers who held, with Quine, that 'philosophy of science is philosophy enough.' Absent a workable demarcation criterion, it

was not even clear what the subject matter of the philosophy of science was. More importantly, the failure of the positivist demarcation project provided an important intellectual rationale for the efforts of relativists in the 1960s and 1970s to argue for the assimilation of science to other forms of belief since the relativists could cite the authority of the positivists themselves as lending plausibility to their denial of any difference that made a difference (1996, 23).

The problem, indeed, was and *still* is whether science loses any of its intellectual authority if it is not clearly and sharply demarcated from pseudoscience or nonscience. Is it the case that failing to circumscribe the boundaries of science has to lead to an undermining of the objectivity and epistemic reliability of science?

This, note, is a serious and challenging question *within* GPoS. But the answer is negative. As Laudan himself noted (1996, 24) the *epistemic* problem is what makes scientific knowledge reliable and not what makes it scientific. Addressing this problem does not require, for instance, that creationism is proved to be pseudoscientific based on some general and sharp criteria. But it does require engaging with the *epistemic status* of the creationist theories: their relation to evidence, their integrability with other theories we have independent reasons to accept etc. And this presupposes the development of a rather general account of empirical support and confirmation which will make possible, among other things, to compare evolutionary theory and creationism and to show how and why the latter is epistemically defective. The point I want to stress here is that the very issue of how scientific theories are related to evidence and how theory-appraisal and theory-choice should work is a central concern of GPoS, *even* in the absence of definite and rigorous ways to characterise creationism, or other endeavours, as pseudoscience.

## **2.2 From essentialism to family resemblance**

Science does not have an essence waiting to be discovered by conceptual analysis and/or empirical investigation. An elegant thought, advanced by Massimo Pigliucci (2013), is that ‘science’ is a cluster or family resemblance concept, whose basic content is captured by a two-dimensional graph, where one dimension is theoretical understanding and the other is empirical knowledge. In this graph, the pseudosciences are supposed to occupy the space near the origin: “they all occupy an area (...) that is extremely low both in terms of empirical content and when it comes to theoretical sophistication” (2013, 24).

However, unless there are objective measures of empirical content and theoretical understanding, it can always be questioned whether paradigmatic cases of pseudoscience are low on both. Pigliucci considers a related objection and argues that faced with a dilemma of abandoning astrology and creationism or claiming that “established sciences” are not genuinely different from pseudosciences “the choice is obvious” (2013, 204). Surely it is! But it is so, I claim, because there are plenty of *epistemic* reasons to trust, say, evolutionary biology and astronomy and virtually no reason to trust creationism and astrology. If there is something that genuine scientific theories have in excess over so-called pseudo scientific theories it is that they are supported—objectively speaking—by evidence; they have explanatory content; they cohere better with the rest of the theories in the scientific image of the world. In other words, they score a lot higher in the kind of evidential and non-

evidential ‘metric’ that scientists have always employed to appraise theories.<sup>2</sup> Still, Pigliucci’s overall approach is commendable because it stresses resemblances and common grounds between the various sciences as being enough for a general characterisation of science. James Ladyman (2013, 51) makes this point when he says that “(a) family resemblance certainly exists between the sciences, and the success of fields such as thermodynamics and biophysics shows that science has a great deal of continuity and unity”. This “theoretical simplicity and utility of the concept of science” is enough to make it useful even if science is so heterogeneous.

Family resemblance is the key Paul Hoyningen-Huene (2013) attempt to answer the question ‘What is science?’ by stressing that what makes scientific knowledge distinctive (over other forms of knowledge) is that it is “systematic”, where systematicity is analysed along nine dimensions, including descriptions, explanations, predictions, completeness, systematic defence of knowledge claims, epistemic connectedness and others (2013, 27). Hoyningen-Huene advances his account as a descriptive theory, “describing what exists in science” (2013, 199). But this may well be its chief weakness, since as Hoyningen-Huene admits, he ends up with a “tenuous sort of unity among all of the sciences” (2013, 209), the reason being that the various ways to understand ‘systematicity’ (by means of the concretisations of *each* of the nine dimensions in *each* of the sciences and/or theories) lead to divergent meanings. It is of little consolation that all these concretisations of ‘systematicity’ in different disciplines, sub-disciplines and areas of research “are connected by multidimensional family resemblance relations” (2013, 209). For, what makes the relations of family resemblance possible is that all these disciplines are deemed to be members of the same family, viz., *science*, and what makes all these disciplines be members of the same family, viz., *science*, is that they are connected by family resemblances.

In Hoyningen-Huene’s descriptive approach, the issue of whether scientific theories are supported by evidence is passed over (almost) in silence. One of the dimensions of systematicity—the systematic defence of knowledge claims—is obliquely related to looking for evidence for theories. But he insists that his approach is not evaluative; hence, it is not meant to say whether a theory is supported (or supportable) by evidence more than another; nor to compare, in terms of the success of representing reality, science with and other forms of knowledge (including pseudoscience) (2013, 173). This, however, will leave little or no room for a substantial *criticism*—within science and within GPoS—of various theories.

The family resemblance approach has the advantage of avoiding the pitfalls of essentialism but it has the disadvantage of not adequately explaining where the (family) resemblance lies. In thinking about the question ‘What is science?’ it is important that we rely on our best *exemplars* of sciences and scientific theories and aim to mould a conception of *science* that characterises them. But if we stay at the level of family resemblances we might end up being *too* descriptive: science is whatever is being taught in science departments in universities. In my view, it’s best to proceed the other way around: *first a conception of science, then the resemblance*.

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<sup>2</sup> All this does not imply that the application of scientific methods is algorithmic. In most typical cases, evidence is balanced with considerations concerning theoretical (explanatory) virtues and this is done by exercising the judgement of scientists. I have elaborated on this claim in my (2002) and (2012).

### 3. Science-in-General and General Philosophy of Science

I want to argue that the object of study of GPoS is *Science-in-general*. And though GPoS is not here to legislate what science ought to be—independently of what is going on (and has been going on) in the various *sciences*—it is also the case that GPoS is not here to merely describe (or to provide a synopsis of) what the various sciences do. If a purely normative-evaluative perspective is spinning in the void, a purely descriptive perspective does not make room for grounded judgements about what is the best way to view science and how we characterise the unity there is among the various sciences, despite their differences. In particular, a purely descriptive perspective does not make room for a *critique* of science, that is of a critical engagement with science. In engaging with GPoS, we start with some theoretical conception of science and aim for a reflective equilibrium between this conception and actual and historical features appropriate to science and various knowledge-generating practices which are described or characterised as science.

#### 3.1 The two functions of GPoS

GPoS fulfils two different but related functions and it requires both of them in its operation.

The first function is *explicative*. It aims to explicate (that is, to render more precise and more definite) the various concepts that are employed by the various sciences—and hence, to specify their common content as well as their differences and relations. For instance, it is hard to think of science without thinking of it as offering explanations of the phenomena under study, even if there is no overarching account of explanation that covers all paradigmatic sciences or sub-disciplines within them. It is equally hard to think of science without thinking of it as offering representations of the phenomena, as relying on models and as advancing theories. And, of course, it is even harder to think of science without some notion of experiment and experimental practice. Even if it turns out that, to give an example among the many, the concept of *explanation*, (or *representation*, *theory* and others) has different explications in the various sciences, all of these explications belong to the same genus and it is because of this that they are all concepts of *explanation* and not of something else.<sup>3</sup> The very idea of conceptual pluralism is an idea *within* GPoS and its proper development requires GPoS—for admitting that there is no single concept of, say, explanation requires a standpoint from within which all competing concepts of explanation are examined and compared. And this is the standpoint of GPoS.

The second function of GPoS is *critical*. It aims to criticise (in the Kantian sense of passing judgements on) the various conceptions of science as well as the various ways to present science, its methods and its aims. A key object, for instance, of the critical function of GPoS is to disentangle the part of scientific theories that is up to us and the part which is up to the world; or in other words, the contribution of the mind and the contribution of the world in our scientific image of the world. Another key object is to discuss the scope and limits of scientific knowledge and the epistemic credentials of the various factors (evidence, theoretical virtues etc) that

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<sup>3</sup> For a development of this line of thought, especially in connection to the role of explanation in Inference to the Best Explanation (*aka* abduction) see my (2007).

are involved in the acceptance of theories; or the relation between philosophy of science and history of science.<sup>4</sup>

I will discuss these two functions later on. In section 4, I will present four dimensions along which these two functions operate. But before I do this, I want to explicate this idea of *Science-in-general* as the proper object of GPoS.

### 3.2 A mode of knowledge as well as a discipline

In thinking of *Science-in-general* as the proper object of GPoS, I follow two thinkers. The first is Aristotle. In the opening lines of Physics Book 1, Aristotle noted that *episteme* is both a kind of knowledge *and* a discipline. In fact, this kind of knowledge is what is shared by the various disciplines which are called sciences. Here is how he put it:

When the objects of an inquiry, in any department, have principles, causes, or elements, it is through acquaintance with these that knowledge and understanding is attained. For we do not think that we know a thing until we are acquainted with its primary causes or first principles, and have carried our analysis as far as its elements. Plainly, therefore, in the science of nature too our first task will be to try to determine what relates to its principles (184a10-184a16).<sup>5</sup>

The science of nature (*ἡ περὶ φύσεως ἐπιστήμη* or *scientiae naturalis*, as Thomas Aquinas rendered it in his *Commentary*) is a kind of *episteme*, that is, it is a special way of knowing its subject matter. So from Aristotle, I take the idea that science is a special mode of knowing the world (or an intended domain of phenomena).

The second thinker is Karl Marx, from whom I form the notion of Science-in-General in analogy with his idea of ‘production in general’, which he put forward in the *Grundrisse*:

*Production in general* is an abstraction, but a rational abstraction in so far as it really brings out and fixes the common element and thus saves us repetition. Still, this *general* category, this common element sifted out by comparison, is itself segmented many times over and splits into different determinations. Some determinations belong to all epochs, others only to a few. [Some] determinations will be shared by the most modern epoch and the most ancient. No production will be thinkable without them; however even though the most developed languages have laws and characteristics in common with the least developed, nevertheless, just those things which determine their development, i.e. the elements which are not general and common, must be separated out from the determinations valid for production as such, so that in their unity – which arises already from the identity of the subject, humanity, and of the object, nature – their essential difference is not forgotten (1857-8, 85).

From this rich passage, I take the idea that Science-in-general is a *rational abstraction*. In actual point of fact, there are concrete theories and disciplines with rich and complex histories and structures. And yet, they all fall under a *general* category—*science*—aiming to capture a mode of knowledge of the world, which is subject to different determinations (both conceptually and historically), some of which are general and common to all sciences, while others are different. Neither

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<sup>4</sup> For an account of how these issues were discussed and developed by Henri Poincaré, see my (2014). When it comes to the connection between philosophy of science and history of science, I think that the prototype is still Duhem (1906).

<sup>5</sup> In Aristotle (1984, 315).

their unity, nor their differences should be neglected. To paraphrase Marx, their unity arises from the identity of the subject, viz., that science is a special mode of knowledge of nature, and from the identity of the object, viz., nature itself. Going above the various sciences to *Science-in-general* makes it possible to acquire a (reviseable and historically-conditioned) bird's eye point of view which is necessary for viewing the various sciences as being parts of a common endeavour to understand the world and to acquire a coherent scientific image of it. As I put it elsewhere (2012a, 101): "Science *as such* is a theoretical abstraction and general philosophy of science is the laboratory of this theoretical abstraction".

The Aristotelian idea that I have used is that the common core of science—what characterises Science-in-general *qua* an abstraction—is that it is a form of knowledge, and a concomitant set of practices and methods, which aim to achieve this form of knowledge. This form of knowledge—known as *scientific*—is characterised by a rather rigorous demand for justification as well as for external grounding to (at least some aspects of) reality. The demand for justification renders science an inter-subjective enterprise; the demand for external grounding renders it an objective enterprise. The specific determinations of the form of scientific knowledge change over time. And they change partly because of the critical function of GPoS, (until fairly recently performed by practising scientists themselves as well as by philosophers of science), which unravels problems in the dominant conception of scientific knowledge and its in principle achievability; and partly because of changes in the scientific worldview itself.

Here is a quick illustration of the interplay between the abstraction that constitutes Science-in-general and its concrete determinations. The Aristotelian conception of *episteme*—qua certain knowledge of universal and necessary truths founded, ultimately, on induction on the basis of experience and demonstration from first principles—prevailed (though not uncritically and without challenges) for a number of centuries and was criticised and doubly transformed in the seventeenth century by the competing conceptions of Francis Bacon and Rene Descartes.<sup>6</sup> The difference between Descartes and Bacon concerned both the origins of knowledge and the canons of theory appraisal and choice. But neither of them denied that scientific knowledge required strengthened security—though they disagreed on the degree of strength. Briefly put, Bacon was a new inductivist, whereas Descartes was a new 'demonstrationist'. On Bacon's *new* induction—which was contrasted to Aristotelian induction as this was deployed by the Italian neo-Aristotelians<sup>7</sup>—knowledge starts with experience and the compilation of a detailed natural-experimental history of the phenomena under investigation. It is then acquired by what is in effect an elimination of alternative hypotheses, via the careful construction of tables of presences, absences and concomitant variations. Descartes, on the other hand, had no room for induction in his philosophy of nature. But though he appealed to a demonstrative ideal of scientific knowledge, his conception of demonstration was not Aristotelian syllogistic proof, but more akin to what we nowadays call explanation. "There is a big difference between proving and explaining", as he put (1991, 106) it, meaning that demonstration in science can proceed either from causes to effects or from effects to causes.

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<sup>6</sup> For an account of induction in Aristotle and its reception in the middle ages, see my (2015).

<sup>7</sup> For an important recent study of this, see Sgarbi (2013).



Isaac Newton, as is well known, protested against the use of hypotheses in science, which he took it to be emblematic of the Cartesian approach, and set forth the famous methodological rules for doing science. In a letter to Roger Cotes in March 1713, Newton (2004, 120-1) noted:

Experimental Philosophy reduces Phenomena to general Rules & looks upon the Rules to be general when they hold generally in Phenomena. It is not enough to object that a contrary phenomenon may happen but to make a legitimate objection, a contrary phenomenon must be actually produced. Hypothetical Philosophy consists in imaginary explications of things & imaginary arguments for or against such explications, or against the arguments of Experimental Philosophers founded upon Induction. The first sort of Philosophy is followed by me, the latter too much by Cartes, Leibniz & some others.

Experimental natural philosophy was a new way of acquiring scientific knowledge. It stressed, among other things, the need to use the phenomena (that is, empirical laws) as premises, together with the laws of motion, for the derivation of force laws, and conversely. But as Newton explained in the *Scholium* of Proposition 69 of Book I of the *Principia*, the aim of all this was to argue “more safely” about the physical causes of the phenomena.<sup>8</sup>

Writing about a century later, Marquis Laplace, one of the greatest Newtonians ever, took it that he was enlarging Newton’s method of experimental natural philosophy by applying to it the then newly developed mathematical theory of probability. At the very same time, however, he was exposing it to criticism that was supposed to unravel its weaknesses: “Yet induction, in leading to the discovery of the general principles of the sciences, does not suffice to establish them absolutely. It is always necessary to confirm them by demonstrations or by decisive experiences; for the history of the sciences shows us that induction has sometimes led to inexact results” (1951, 177). His objective was to correct this method by strengthening it, that is by enhancing the degree of certainty it can attain: “then science acquires the highest degree of certainty and of perfection that it is able to attain” (1951, 182-3).

There is a clear sense in which all those thinkers (and many who followed them) were engaged in the same kind of enterprise and doubly so: they were engaged in *science*, in the sense of the special mode of knowledge noted above *and* they were engaged in GPOS too, by developing accounts of the methods appropriate for this kind of knowledge and by criticising competing accounts. They all had the same starting point, viz., scientific knowledge of the world, and yet the specific determinations of the form of scientific knowledge they favoured were different and developed, at least partly, by criticising competing determinations. The specific determinations of scientific knowledge change over time and the various relevant conceptions compete with each other, both synchronically and diachronically. Yet, something remains invariant and this is that science is a special mode of knowledge which is characterised—in its most abstract form—by the demand of increased (but as it turns out never absolute) security and reliability and by the search for methods that make this possible.

### 3.3 The conceptual toolbox of Science-in-General

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<sup>8</sup> For an excellent recent account of Newton’s approach to method, see Harper (2011).

Science-in-general employs a network of concepts—a toolbox, as it were—in order to characterise the special form of knowledge it is as well as its relation to the world. The knowledge achieved by science offers *explanations* of various phenomena; it unravels *causal connections*; it is supported by the *evidence*. Science employs *theories, hypotheses* and *principles*; it issues in *predictions*; it relies on *experiments*. Theories *represent* the phenomena and rely on *theoretical concepts* and *models*. The reality science investigates is governed by *laws of nature*. The *entities* there are in the world have *properties* and *powers* and they stand in various *relations* to each other. This is just a sample of the network of concepts employed by Science-in-general. And though it might not be the case that all of them play a role—or function—in each and every individual science, it is hard to think of any individual science—both in history and as it is practiced today—without some (typically most) of these concepts.

As noted in section 3.1, one important function of GPoS is to offer explications of these concepts. At one point in the history of GPoS, it was thought that the task of GPoS was to offer formal-syntactic explications. This was the time—associated with some Logical Positivists like Carnap—of the ‘logic of science’. The idea was—and it was a noble idea—that as it is formally specified when a proposition A entails a proposition B (given a formal language), so it should be formally specified when a proposition A *explains* a proposition B; a proposition A *confirms* a proposition B etc. Part of the reason why this idea was noble was that it was meant to secure some objective content of a concept when it comes to its applications and to leave issues of interpretation open to dispute. This programme failed mostly because the content of the concepts under explication was too rich to be formalised. For instance, *explanation* cannot be explicated by being formalised as a species of deduction (see the Deductive-Nomological model); not because there are no deductive-nomological explanations, but because not all explanation is deductive-nomological. But this does not suggest that the task of explication is hopeless. It suggests that, in all probability, the rich conceptual structure of the basic concepts of Science-in-general is not formalisable and that, in all probability, there will be more than one *explicata*. It may even turn out, as Carnap famously argued for the case of probability, that there are two *explicanda* (in his case: two concepts of *probability*, one referring to a measure of confirmation, and another to a measure of relative frequency). We should not, I think, equate the failure of formalisation with the failure of explication. Most scientific concepts are explicable, without being formalisable.

More importantly, explicating a concept is bound to be entangled with the explication of a number of other concepts that fix part of its content or ground its role. For instance, when we try to explicate the concept of *mechanism*, concepts such as *power, explanation, causation, laws of nature* and others are involved. GPoS in its explicative function works with clusters of concepts and elucidates all of them as networks. The concept of *mechanism*, for instance, has had a diachronic occurrence in science, (and in various particular sciences) and there have been (and still are) important controversies about its content and its connections with other concepts such as *causation*. As I have shown in some detail in my (2011a), the prevalent accounts of mechanism, though motivated by considerations in various particular sciences, are being offered—and justly so—as general *explications* of a central concept that occurs (or can occur) in any theory or science. This case—the

explication of mechanism—is a good example of the explicative function of GPoS because explication, though not offering a formalisation of a concept, does offer a theoretical account of it; it unravels its inner structure (and hence what kinds of ontic commitments follow from adopting a certain explication as opposed to another); and it makes apparent the connections of this concept with others. In fact, further explicative work suggests that there are two *explicanda* when it comes to *mechanism*: *Mechanical Mechanism* and *Non-mechanical mechanism* (see my 2011a for the details).

#### **4. The four dimension of General Philosophy of Science**

Given an understanding of Science-in-general, and the two functions of GPoS, we can become more specific about the role and significance of GPoS. There are four major dimensions along which the philosophical study of Science-in-general takes place: an epistemic, a metaphysical, a conceptual and a practical.

The *epistemic dimension* deals with a family of issues that have to do with the epistemic credentials of science and in particular with the status of scientific knowledge. Science is not merely a theoretical and experimental practice—it is also (and has always been taken to be) a *mode of knowledge*. It purports to describe and explain the world and employs special methods which offer a systematic understanding of the natural (and the social) phenomena. It relies on theories, hypotheses and principles which, typically but not invariably, go beyond the observable aspects of the world and describe the world as possessing a hidden-to-the-senses causal-explanatory structure. These theories are not entailed by the available evidence but are supported or licensed by the evidence. The methods employed in science are, typically, ampliative: the output of the application of the method exceeds in content the input of the method; hence there is supposed to be a problem not just of describing the way these methods work, but also of grounding or justifying their use. And there is the perennial question of how seriously we should take the scientific image of the world as being a true or true-like image and (relatedly) whether we should think of science as aiming to offer a true image of the world in order to have a just view of science.

The *metaphysical* dimension deals with a network of issues that have to do with the implications of the scientific image of the world about the basic ontological categories of the natural (and social) world as well as its connection with the manifest image of the world and its own ontic structure. Scientific theories describe the world as being subject to laws and causal connections between various entities and processes; they postulate various entities, properties and structures; they deal with natural kinds and species and genera; they purport to unravel the mechanisms that generate or support various functions and behaviours. Two big traditions have fought over the ontic structure of the scientific image of the world: one inflates ontology in order to explain and ground the regularity there is in the world; the other takes regularity as a brute fact; the first posits natural necessities and powers in nature; the second does away with regularity-enforcers and advances a metaphysically thin conception of laws of nature.<sup>9</sup> When it comes to the relation between the scientific image of the world and the manifest image, the lines of

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<sup>9</sup> For a recent attempt to develop this view see my (2014a).

controversy concern not only the issue of ontic priority (which is the *real* image of the world?) but also the issue of their connection.

The *conceptual dimension* deals with a cluster of issues that have to do with the ways scientific theories represent the world as well as with the conditions of representational success. Science represents the world via theories and theories employ a number of representational media, from language, to models, to diagrams etc. Scientific theories employ, almost invariably, idealisations and abstractions in representing natural phenomena. Scientific concepts acquire their content, to a large extent at least, via the theories in which they occur. But experiments too play a significant role in fixing the content of scientific concepts. In fact, theories have to have empirical content and their abstract (typically but not invariably mathematical) structure has to make contact with the world as this is given to humans in experience. The theory-ladenness of the conceptual structure of scientific theories generates a number of problems that have to do with how best to understand conceptual connections between theories; how best to evaluate the representational content of theories; and how best to understand their relation to experience and experiment.

The *practical dimension* deals with a number of issues that have to do with ethical, social and other practical—that is, relating to scientific *praxis*—issues. Science is far from being value-free, and the investigation of the place, role and function of values in science has been an important element of our thinking about science. Values do not function as methods do; yet, they are constitutively involved in scientific judgements and in theory-choice and evaluation in science. They are epistemic values as well as social ones and understanding their interconnections, as well as their role in securing the objectivity of science, is indispensable for having a view about science and its role in society. Feminist approaches to science have played a key role in uncovering various cognitive and social biases and have promoted the image of a socially responsible science. Issues about the ethics of science, the structure of scientific research, risk-analysis and the role of science (and of the scientists and the scientific institutions) in policy making have acquired prominence.

Though it is methodologically useful to separate these four dimensions, in practice, they are intertwined. More importantly, they are all indispensable for having *science* in view. The issues that form each dimension (a sample of which was offered above) have a long history and have been subject to important theoretical debates and controversies. Hence a *proper* engagement with general philosophy of science should be both conceptual *and* historical. The rich history of general philosophy of science suggests that the core of the issues that characterise philosophical engagement with science remain the same in *form* throughout history, though their context, content and the resources available for addressing them have changed over time.

#### **4.1 Unobservables: from medicine to atomism**

As an example, let me mention the issue of the scientific method(s). Though it is true that there is no account of *the* method which can invariably and accurately characterise all individual sciences and all historical epochs, it is also true that science has been taken to be a *special way of getting to know the world*, which is

characterised by two basic features: it is ampliative *and* it is epistemically warranted. Hence, science is different from mere opinion and it purports to extend and enlarge our conception of the world as this is given to us by our senses and our memory. Employing methods which succeed in satisfying these two features has always been a key philosophical and scientific endeavour. This endeavour invites two lines of theoretical investigation, as noted above. The first is to *devise* and *describe* models of methods that purport to satisfy these desiderata; the second is to show that the thus described models manage to meet these two (*prima facie* conflicting) desiderata—that is, to *justify* them. This kind of double philosophical endeavour has preoccupied most scientists and philosophers of science ever since Aristotle identified it as an issue in his *Posterior Analytics*. And no matter how far back we go in time and how far apart in disciplines, we encounter the same *form* of the problem.

Here is a selective, but representative, historical illustration of the idea that the *form* of the problem of the method has a considerable degree of invariance. The starting point of this illustration has, I think, the advantage that it is not widely discussed among philosophers of science. It concerns the debates about method in ancient Greek medicine. For about three centuries (roughly from 300 BCE), there were two competing schools not just for practicing medicine, but for doing science too. The ancient empirics were a group of doctors in the latter part of the third century BC who took it that in practicing the art of medicine, doctors should rely on experience (*empeiria*) alone.<sup>10</sup> Empiricism was developed, at least partly, as a reaction to the proliferation of theories in medical practice. Empirics attacked what they took it to be the dominant school in medicine—the so-called rationalists or dogmatists (*λογικοί/δογματικοί*), who, taking cues from the Stoic theory of signs, argued that there is a special kind of rational inference—called *indicative inference*—which enables the transition from an effect to its *invisible* cause. In particular, the rationalists thought that medicine should be based on understanding and finding the causes of a disease and that this required development of theories about the nature of things and the powers of the causes and of the remedies (cf. Galen, 1985). Indicative inferences were supposed to be grounded on relations of “rational consequence” among distinct existences—relations which were discoverable by means of reason only.

Against all this, empiricists were putting forward the *sola experientia* account of medical knowledge and practice. Medicine, empiricists said, is the accumulation of empirical generalisations—called *theorems*. This accumulation is based on *autopsy* (that is, on one’s own observations) and *history* (that is, reports on other practitioners’ observations). How then can there be theoretical novelty in medicine? The thought that prevailed was that this was achieved by *transition to the similar* (*την του ομοίου μετάβασις/de similis transitione*). This was taken to be a method (*way*) of invention (*οδός ευρέσεως*) (cf. Galen 1985, 5). Yet, there was some active debate about the status of this principle, whose justification is not obvious. The dogmatists were quick to point out that this kind of transition could be justified only

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<sup>10</sup> The founder of the sect (*αίρεσις*), as it came to be known, was Philinus of Cos (around 260BC), but the school spread well into the first few centuries AD. The main proponents of empiricism were Serapion (fl 225 BC) and later on Menodotus, Theodas and Heraclides. Our knowledge of their writings comes mostly from Galen, Sextus Empiricus (who was a sceptic philosopher and an empiricist doctor) and Celsus.

if it was accepted that the nature of things was such that they resembled each other (Galen 1985, 70). In response to this, the empiricists insisted that any justification of this principle—that is of the principle ‘similar cause, similar effect’—should be based on experience (Galen 1985, 70). They therefore wanted to make it clear that there is no experience-independent justification of the principle that underwrites the transition to the similar.

At stake was the issue of the status and justification of methods that enlarge our image of the world by taking us beyond experience. This issue came into sharp focus in the debate about the status of indicative inference. The key problem was the knowledge of non-apparent things (ἀδηλα), which the dogmatists thought was necessary for medicine, but was going beyond experience. The empiricist doctors joined forces with the sceptic philosophers (in fact, some were both) in order to curtail the rashness of reason, as Sextus (2000, Book I §20) put it.

According to Sextus (2000, Book II, §§97-98), the Dogmatists divided entities into two epistemological categories: (a) pre-evident things (πρόδηλα) and (b) non-evident things (ἀδηλα). The former are immediately evident in experience without recourse to inference; they come of themselves to our knowledge, as Sextus put it, e.g. that it is day. The non-evident things are divided into three sub-categories. (b1) those which are non-evident once and for all (καθάπαξ ἀδηλα), e.g., that the stars are even in number; (b2) those which are non-evident for the moment (προς καιρόν ἀδηλα); (b3) those that are non-evident by nature (φύσει ἀδηλα). The real issue was between b2 and b3, Sextus thought. Temporarily non-evident things have an evident nature but are made non-evident for the moment by certain external circumstances (e.g., for me now, the city of the Athenians). Naturally non-evident things are those whose nature is such that they cannot be grasped in experience, e.g. Sextus says, imperceptible pores – for these are never apparent of themselves but would be deemed to be apprehended, if at all, by way of something else, e.g. by sweating or something similar (2000, Book II §§97-98).

Things which are temporarily non-evident and things which are naturally non-evident are apprehended through signs. There are two kinds of sign. Recollective or commemorative (υπομνηστικά) signs, and indicative (ενδεικτικά) signs; and correspondingly, two kinds of inference. The difference between the two concerns the types of things involved in the two inferential procedures. Temporarily non-evident things are known by recollective signs, whereas naturally non-evident things are apprehended via indicative signs (2000, Book II §100)

The standard ancient example of a recollective sign is smoke, as in the case: if there is smoke, there is fire. The fire is temporarily non-evident, but knowing that there’s no smoke without fire, we can infer that there’s fire. So recollective sign-inferences take us from an evident entity to another entity which is temporarily non-evident, but which can be made evident. Indicative-sign inferences take us from an evident thing to another thing which is naturally non-evident. The standard ancient example was the case of sweating as indicative of its non-evident cause (pores in the skin). But there was also another type of dispute: the conclusion of an indicative inference was supposed to be licensed by the fact that the effect (the sign) was *necessarily connected* with the cause (the signified) and flew out of its “proper nature and constitution”, (as bodily movements are signs of the soul), as Sextus put it. The commemorative sign-inference, on the other hand, was based on the

recollection of the past co-occurrence of the evident effect and the temporarily non-evident cause. Upon the observation of the evident effect, we are led to recall “the thing which has been observed together with it and is not now making an evident impression on us (as in the case of smoke and fire)”. So the commemorative inference was supposed to be grounded on past experience and to implicate the memory.

The issue of the distinction between indicative inferences and commemorative ones was centrally disputed among the ancient physicians *and* philosophers. The empiricists employed special *technical* terminology to map this distinction. They called indicative sign-inference ‘analogism’; and commemorative sign-inference ‘epilogism’. Like in the case of Sextus, the key argument of empiricists in favour of epilogism was that the kind of inference involved in it, being directed towards visible things, is “an inference common and universally used by the whole of mankind” (Galen 1985, 133; 135). Analogism, on the other hand, was not universally accepted, because of the invisibility of the things involved in it (Galen 1985, 139; see also Sextus 2000 Book II §102).

This kind of debate came to a halt (temporarily) when Galen provided a synthesis of the views of the two sects—a *via media*—by claiming that empiricism should be open to theory but theory should be open to empirical testing. Reporting his own views, Galen suggested a need for a “reasoned account” to be added to what is known from experience. This reasoned account (a *theory*) should be tested in experience either by finding confirming instances or by disconfirming it “by what is known in perception” (1985, 89).

And yet, the *form* (and at least part of the content) of this debate resurfaced in the seventeenth century, in the context of the mechanical conception of nature. In his *Syntagma Philosophicum* (1658/1972), Pierre Gassendi borrowed the distinction between indicative inference and commemorative one from the ancient doctors and philosophers, and argued for principles that can act as a bridge between the macroscopic world and the corpuscularian world of the new mechanical philosophy. For Gassendi, there are circumstances under which the conclusion of indicative inference can be legitimate. This happens, he said, when the sign can exist in one circumstance; that is, when there is only one explanation of the presence of the sign (hence, when there no competing explanations). Interestingly, this cuts through the visible/invisible distinction. Though Gassendi agreed with the ancient empirics that indicative inferences differ from commemorative ones in the type of entities implicated in them—entities invisible by nature (*occultae as nature*) vs entities temporarily invisible (*res ad tempus occultae*)—he argued that probable knowledge of invisible by nature things (such as the atoms and void) is thereby possible. Note that at the very time that Gassendi was allowing inferential knowledge of unobservables, he was transforming the conception of knowledge appropriate for ‘*la verite des sciences*’ allowing lesser degrees of certainty than his contemporary Descartes.

The terminology that Gassendi borrowed from Sextus to describe the method that was taken to generate and justify belief in unobservable atoms was likely lost in subsequent discussions, but the *form* of the issue resurfaced again and again in different contexts—most notably in the debates over atomism and the kinetic theory of gases towards the end of the nineteenth century. There the issue, as the physical

chemist Jean Perrin (1916, xii) put it, was the explanation of “the visible in terms of the invisible” and the grounds for its legitimacy. The fault-line was among those who took it that enlarging the image of the world by positing unobservables was, strictly speaking, beyond the bounds of science and in the vicinity of metaphysics and those who took it that this enlargement was indispensable for understanding the world *and* licensed by ordinary scientific methods. None of the sides of this debate was monolithic in its approach and arguments. In fact, new arguments turned out to be taken to be relevant and prominent—one of them being what Ludwig Boltzmann (1900) called ‘the historical principle’, viz., that the history of science has shown that theoretical attempts to expand our scientific image of the world by positing unobservable entities have ended up in failure. Another argument, addressed by Pierre Duhem (1906, 88-89) against Gassendi, was that atoms and other unobservables are so unlike ordinary objects that any inference to them should be a matter of the imagination and not of reason. Still, the key issues of the debate were—essentially—invariant.

After his famous work on the Brownian motion and its explanation on the basis of the kinetic theory of gases, Jean Perrin—who like Pierre Duhem was a scientist and not a ‘professional’ philosopher—summarised his overall approach as follows (1916, vii): “To divine in this way the existence and properties of objects that still lie outside our ken, *to explain the complications of the visible in terms of invisible simplicity* is the function of the intuitive intelligence which, thanks to men such as Dalton and Boltzmann, has given us the doctrine of Atoms”. Perrin’s point, as he described in a piece on the method in physical chemistry he published in 1919—in a volume titled ‘the methods of the sciences’—was not to denigrate inductive methods (purportedly staying at the level of sensible entities), nor to promote exclusively deductive methods (aiming at explanation in terms of invisible entities). Rather, as he put it, “the indefinite wealth of nature does not lock itself in a single formula”; hence cooperation various methods is required. Still, trying to “eliminate completely invisible elements”, like many of his contemporary advocates of energetic wanted to do, would amount to leaving the image of the world unsupported—very much like removing the pillars that support a cathedral (1919, 87).

A number of significant details of this episodic account have been glossed over.<sup>11</sup> But I take it the intended message is clear enough. This is only an example of how one central problem of GPoS—whether and how science should expand and extend the image of the world by positing unobservable entities—has kept both its *form* and its *significance* throughout the centuries and the disciplines. To use current jargon, this might be taken to be the problem of scientific realism, though this is a label *we* and not most of the protagonists in the various incarnations of this debate are prone to use. It became a standard problem within GPoS in the twentieth century partly because this expansion of the image of the world by positing invisible elements has become the norm in most—if not all—of the sciences. It is noteworthy—and it is occasionally forgotten—that a great deal of the revival of interest in scientific realism in the early 1950s was related to issues in psychology and the status of the so-called ‘intervening variables’ and ‘hypothetical constructs’ in moving away from behaviourist approached to mentality (cf. MacCorquodale & Meehl 1948). But the

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<sup>11</sup> I have told most of the details in 2011b.



problem of scientific realism became a standard problem within GPoS for another reason. Precisely because of its generality and resilience it can provide a conceptual umbrella under which a number of other problems of the scientific image of the world can be subsumed and discussed; for instance, the role of explanation in science; the relation between evidence and theory; the status of scientific truth; rationality of theory-change and others.<sup>12</sup>

## 5. GPoS vs Philosophy of X?

There is no doubt that the philosophies of the various sciences have flourished over the last forty or so years. There is also no doubt that GPoS was physics-centred until fairly recently. But this trend has been reversed and GPoS has been more pluralistic in its relations with the individual sciences. But though, as noted already, the subject matter of GPoS is Science-in-general and *not* the various individual sciences, GPoS and the philosophies of the various sciences form a seamless web.

What we normally call science X (physics, biology, chemistry, economics, psychology etc) is itself a kind of rational *abstraction*. It's hard to come up with an interesting definition of, say, *physics*; the subject-matter of physics—as well as the name—has changed over time. The expression 'natural philosophy' was used to refer to physics until roughly the middle of the nineteenth century. In 1798, the *Academie Royale des Sciences* of Paris set the following topic as a prize competition in *physics*: "the nature, form and used of the liver in the various classes of animals". In 1918, when Max Planck was awarded the Nobel Prize in physics, the citation of the *Swedish Academy of Sciences* noted that the award was given "in recognition of [Planck's] epoch-making investigations into the quantum theory". In practice, physics comprises a number of fields and disciplines, from mechanics to high-energy physics, to solid-state physics, to physical chemistry and a good many others. Physics, if anything, is a cluster of sciences and disciplines, as these have evolved over time. Philosophy of Physics is the philosophy-of-physics-in general! And, as we all know, there are various philosophies of sub-X within the philosophy of X: philosophy of spacetime; philosophy of quantum mechanics; philosophy of statistical mechanics, philosophy of string theory and so on. Philosophy of Physics, then, stands to the philosophies of various physical sub-disciplines as GPoS stands to philosophies of the various sciences. The same holds for the Philosophy of Biology, for example. Biology, strictly speaking, is a cluster of sciences or disciplines: ecology, paleontology, synthetic biology and others. Actually, a new cluster of biological sciences has been formed: 'life sciences'. The philosophy of biology-in-general stands to the philosophies of various biological sub-disciplines as GPoS stands to philosophies of the various sciences. And as there are no impermeable boundaries among the philosophy of X and the philosophies of sub-X, so there are no impermeable barriers between GPoS and the Philosophies of X, where X ranges over individual sciences. This permeability is strengthened by the fact that new *hybrid* disciplines are being formed, e.g., molecular biology or biophysics. And with them, there is transfer of methods, theories and techniques as well as conceptual problems.

The Philosophy of X-in-general deals with some issues that are not *proper* tasks of GPoS. One such issue is the interpretation of the various theories of X; for any theory

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<sup>12</sup> For the issue of scientific realism in the twentieth century see my 2011.

T in X, there is the question what the world is like according to T. If there are competing interpretations—or even theories—there is the further issue of how they are related. And there is always the issue of the basic conceptual structure of a theory T in X—that is, of the content of the key explanatory concepts of T. And of course, there is the issue of how the sub-disciplines of X are related to each other. Hence, there are questions and issues that belong—more or less squarely—to the philosophies of X, where X ranges over individual sciences. In the philosophy of biology, for instance, there are issues concerning the concept of reproductive fitness; or the ontological status of the probabilities used in population biology; or the understanding of gene in Mendelian and molecular genetics. In the philosophy of chemistry, there are issues concerning the status of explanations in terms of electron orbitals; whether there are some irreducibly chemical laws, e.g., the periodic law of the elements; the issue of ‘chemical substances’, e.g., whether the world consists of one kind of matter or of a great variety of materials; the relation between chemistry and physics. In philosophy of physics, there are the famous issues of the interpretations of quantum mechanics; the nature of space and time; the status of probabilities in statistical mechanics and many others.

Dealing with philosophical issues that arise within the Philosophy of X requires, among other things, an engagement with the sciences or the disciplines themselves that are investigated. The Philosophy of X is in many ways the abstract and theoretical end of X itself. But the Philosophy of X (or the Philosophy of X *in general*, as I would like to put it), *qua* philosophy of science, employs and explores the conceptual resources of GPoS. It performs the two functions of GPoS, the *explicative* and the *critical*, at the level of X. In many ways, it deals with problems of GPoS, as they are concretised in individual sciences. The idea of explanation in evolutionary theory is a case in point. Elliott Sober (1984), for instance, relied on the composition of forces in dynamics in order to account for the change in gene frequencies over time as the result of different ‘forces’, such as selection, drift, and mutation. And so is the issue of laws of nature: are there laws in biology or chemistry? And the issue of the structure of theories in biology. For instance, evolutionary theory has been taken to support the ‘semantic view of theories’.

In fact, there are areas in which the philosophical investigation of issues in the philosophy of X have exerted serious influence on how the relevant issues are treated within GPoS. One important such issue has been the nature of biological species. These do not conform to standard philosophical approaches to natural kinds, especially when the latter are viewed as possessing essences. A fruitful discussion has opened up about how best to reconceptualise natural kinds or even whether the traditional reliance on natural kinds should be rejected.

But we should not lose sight of the fact that there are philosophical questions that belong—more or less squarely—to GPoS. I have already offered an outline of the issues dealt with in the four dimensions of GPoS. But I want to add two issues that I think they are really *peculiar* to GPoS.

The first, unsurprisingly, is the scientific realism debate. This debate does not concern any science in particular. And this is so, because the heart of the problem is about Science-in-general: can and should we trust the scientific image of the world? Think of the challenge of the pessimistic induction, for example. This concerns the very idea that science is in a position to offer knowledge of the world. The challenge,

as is well known, utilises the history of science to undercut confidence in the current scientific image of the world. It does that by noting that there is a pattern of theoretical-explanatory failure in the history of science which warrants the claim that, in all probability, current scientific theories will be abandoned and replaced in the years to come, despite their impressive empirical success. The details of this debate can be found in my (1999) and Stanford (2006). The point I want to stress is that this kind of argument is a global argument about science and not about the particular sciences.

In fact, if we were to break it down to sub-arguments concerning the various individual sciences, e.g., biology, physics, chemistry etc., we might be able to undermine it. For, even if there is radical theory-change in physics, there is not, it seems, a pattern of radical theory-change in biology or in chemistry. This kind of dividing strategy would possibly warrant the claim that we can be more confident about our current theoretical knowledge in biology than we are in physics. But this kind of strategy would not remove the need to address the argument from the pessimistic induction globally. For unless there are relevant differences in the ways we acquire scientific knowledge in, say, physics and biology, the alleged failures in physics would still undermine the alleged successes in biology or chemistry. The strength of the argument from the pessimistic induction lies precisely in its undermining the relations between explanatory and empirical success and truth. Hence, the attempts to neutralise the pessimistic induction should aim to restore a connection between empirical success and truth, even if this connection is subtler and more sensitive to the history of science than, perhaps, was accepted before.

As I have argued elsewhere (2009, 75-77), when it comes to the realism debate, a key task of GPoS is to address the issue of balancing two types of evidence for or against a scientific theory. The first kind is whatever evidence there is in favour (or against) a specific scientific theory. This evidence has to do with the degree of confirmation of the theory at hand. It is *first-order evidence* and is typically associated with whatever scientists take into account when they form an attitude towards a theory. It can be broadly understood to include some of the theoretical virtues of the theory at hand—of the kind that typically go into plausibility judgements associated with assignment of prior probability to theories. The second kind of evidence (*second-order evidence*) comes from the past record of scientific theories and/or from meta-theoretical (philosophical) considerations that have to do with the reliability of scientific methodology. It concerns not particular scientific theories, but science as a whole. This second-order evidence feeds claims such as those that motivate the Pessimistic Induction. Actually, this second-order evidence is multi-faceted—it is negative (showing limitations and shortcomings) as well as positive (showing how learning from experience can be improved).

This balancing cannot be settled in the abstract—that is without looking into the details of the various cases. But the need for balancing shows how GPoS and the Philosophies of X can work in harmony. For, in most typical cases, settling issues about the first-order evidence there is for a theory of X will be a matter of the Philosophy of X (subject, as we have noted in section 2.1 of a general account of the relation between evidence and theory), whereas settling issues about the second-order evidence there is from the history of science or the conceptual structure of Science-in-general will be a matter of GPoS.

The second issue that predominantly belongs to GPoS concerns the *very idea of a scientific image of the world*. GPoS offers the space in which the various images of the world provided by the individual sciences are fused together into a stereoscopic view of reality. Strictly speaking, the various sciences offer us perspectives on reality. They employ different kinds of taxonomic categories and conceptualise the world by means of different structures of concepts. The category of ‘chemical bond’, for instance, belongs to a different conceptual structure than the category ‘gene’ or ‘quark’. Still, there is a presumption—to say the least—that the various perspectives offered by the various sciences or theories are perspectives *of the same world*. Hence there is the need to put together the scientific image of the world; to look at the various interconnections among the ‘partial’ images generated by the individual sciences; and to clear up tensions and conflicts. This is precisely the kind of job that GPoS—and only GPoS—can do. It offers a more global (but not absolute) perspective on reality—for seeing the whole picture. Even if there is no way to put together a coherent and unified image of the world, even if, that is, the scientific image is characteristically disunified and disconnected, this can be ‘seen’ only within GPoS.

GPoS offers the toolkit of concepts that are needed for establishing the relations among various partial images. Employing the expression ‘special sciences’ to refer to sciences other than fundamental physics did play a useful role in promoting anti-reductivist approaches to the relations between the various sciences, but it was a misnomer! There are no special sciences; or, all sciences are special in that they offer perspectives on reality. The matter of ‘putting together’ these perspectives is a central task of GPoS, but here again GPoS should be engaged in both of its functions. It should aim to explicate (and examine the conceptual networks) of key concepts such as *reduction, supervenience, fundamentality, emergence, dependence* and the like. It should aim to criticise the various theoretical accounts concerning the relations among the various sciences—as these were determined both conceptually and historically.

## 6. Concluding thoughts

I have argued that GPoS can work in harmony with the philosophies of the various sciences and that, strictly speaking, the philosophies of the various individual sciences require the framework and functions of GPoS. The particular sciences, and the various theories in them, are in many ways dissimilar to each other: both in their historical development and their current conceptual and methodological structures. Still, science need not have an essence for it to be the object of philosophical study. Nor should the philosophical study of science be merely descriptive of whatever is taught in Science departments. GPoS has two important functions vis-à-vis Science-in-general: one explicative and another critical. Science-in-general is itself a ‘rational abstraction’, the unity of which arises from the identity of the subject of the various sciences, viz., that science is special *mode of knowledge of nature*, and the identity of the object of the various sciences, viz., *nature itself*. As Leibniz states in the epigram of this chapter: “Similarity in variety, that is, diversity compensated by identity”.

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