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Does philosophy have a value for science only (or particularly) in periods of Kuhnian revolutions, and not in phases of "normal science", as argued by De Haro?

Index

1 Introduction

[De Haro 2019] argues that the importance of philosophy towards science appears evident mostly in those periods of science that Kuhn would define as revolutionary [Kuhn 1962]. After [Kuhn 1962], in scientific revolutions a replacement of scientific paradigms takes place, i.e. the replacement of the entire set of theoretical, metaphysical, methodological, epistemic, practical presuppositions which are at the basis of the "normal" (meaning non-revolutionary) scientific practice for a certain scientific community, at and for a certain time. The old paradigm, which entered a crisis because of some anomalies in it, is replaced by another one which is accepted by scientists as new basis (and essence itself) of the scientific practice.

During the revolutionary phases of science (only), critical discussion among scientists is allowed. The exploration for alternative paradigms and the initial articulation of the newly accepted scientific paradigm always require a dynamic dialectic of asking and answering so-called *why-questions* with the aim of establishing a proper and solid epistemic, methodological and axiological groundwork for a new phase of normal science. These *why-questions* are questions for example about which ontological entities should be regarded as constitutive of the world (clearly with reference only to that portion of the world studied by the particular science at issue) and which properties should be attributed to them, about the meaning of scientific terms and concepts (which eventually depends on the first ontological questions), or still, about the formal criteria for the identification of good (and, correspondingly, bad) scientific claims and explanations, about the scientific methods to be adopted for the production of good scientific knowledge and, last but not least, about all the epistemic, ethical, aesthetical, political, social and economic values and factors considered to be of some importance for the scientific practice.

Such *why-questions* dialectic cannot be however entirely managed by science alone, which in fact needs at this stage the intellectual and argumentative tools of *philosophy* especially as aid in the formulation of the new paradigmatic scientific theories and in the explicitation of concepts and presuppositions implicit in them. In this, as [De Haro 2019] points out, does the double function – heuristic and analytic – of philosophy towards science particularly show itself. Once all the *why-questions* have then been answered and are no further questioned – i.e. once the foundations of a new scientific paradigm have become fixed and stable enough – the normal scientific practice can begin, where instead (only) *how-questions* are asked and answered.

Now, if philosophy is relevant for science with regard only to *why-questions* and if *why-questions* are no further questioned during periods of normal science, it can be logically deduced from this Kuhn-based analysis of the relationship between science and philosophy by [De Haro 2019] that philosophy is basically irrelevant for science during periods of normal science and has instead some importance only within scientific revolutions.

Yet this conclusion seems to be *intrinsically* arguable just by looking at the history of science. There are in fact many episodes in the history of science that clearly show how philosophy has been *both heuristically and analytically* relevant for science even outside revolutionary contexts [Section 2.1.1]. Furthermore, De Haro's conclusion is controversial also because philosophy's important role of connecting science with non-scientific knowledge, society, etc. is unconceivable if science is not taken to be something already stable and well-determined, therefore capable of exerting some kind of influence outwardly to a significant and philosophically investigable extent. This means, philosophy's "connection function" is mostly impossible outside periods of normal science [Section 2.1.2]. Finally, De Haro's conclusion can be also *extrinsically* criticized inasmuch it derives from a perspectivist epistemological account of the relation between philosophy and science, that of Kuhn, whose preferability towards other epistemological accounts is not justified at all by the author and could indeed be rejected in bulk [Section 2.2].

2 Critique of De Haro's Conclusion

2.1 Intrinsic Arguments

2.1.1 First Intrinsic Argument: The Reference to History of Science

The intrinsic incorrectness of De Haro's generalized conclusions about the scarce relevance of philosophy as heuristic and analytic tool outside the revolutionary periods of science is firstly shown, as mentioned, by referring to history of science. In the following, we shall consider Helmholtz's acoustic theory and the eighteenth-century debate about the foundations of calculus in mathematics as historical events displaying a clear importance of philosophy in serving respectively a heuristic and analytic function towards science within *already established* scientific paradigms, i.e. periods of normal science.

2.1.1.1 Helmholtz's Theory of Sound

In 1863 the German scientist and physician Hermann von Helmholtz published one of his most impressive works, *Die Lehre von den Tonempfindungen als Physiologische*

Grundlage für die Theorie der Musik [Helmholtz 1863], with which he laid the foundations of the current theory of acoustic physics.

In this work he scientifically accounts for important acoustic and musical phenomena, like consonance, sympathetic resonance, sound quality, sound propagation, partial and combination tones, etc., some of which – that of consonance in particular – had been being discussed even since classical Antiquity. After describing these phenomena from the *physical* point of view, he moves on to the *psychophysiological* part of his theory of sound, in which the mechanism behind auditory perceptions and sensations is thoroughly explained.

Briefly summarizing, in Helmholtz's (and basically still today's) acoustic theory a tone is regarded as a complex mixture of partial and combination tones (tones of different frequencies generated by the fundamental one); depending on quantity, loudness, different possible combinations and frequencies of its partial and combination tones, a certain quality of the tone (timbre) is determined; physically, a tone is describable as a complex sinusoidal wave deriving from the undisturbed superposition of simpler ones – those of the respective partial and combination tones – which propagates through an elastic medium (like air or water); mathematically, a tone can be represented by means of Fourier's trigonometric series; the ear perceives a tone by phenomena of sympathetic resonance; in particular, in the ear, Corti's organ works as a Fourier-analyzer decoding complex sounds waves and resolving them into simple pendular oscillations; in this way, sensations of tone quality, of consonance between two or more tones, etc. can be explained in a mechanistic fashion.

Helmholtz's theory of sound represents the synthesis of about two centuries of research about acoustic phenomena and ear physiology carried out basically within the new empirical and mechanistic scientific paradigm launched for science of music too already during the Scientific Revolution, whereas previously – i.e. from Antiquity until about the 16th century – the causes of acoustic and auditory phenomena were considered to be of purely mathematical and metaphysical nature (see [Cohen 1984], [Borzacchini 2007]). Thus as such, this theory does not represent *per se* a proper case of scientific revolution or paradigm shift in Kuhnian terms, being ultimately, from all points of views, in a relationship of continuity with the previous research in the field. In fact, empirico-quantitative researches about vibrating bodies, sound waves, partial tones, combination tones, beats, etc. were carried out already since the end of the 17th century (see [Wardhaug 2017], [Wardhaug 2008], [Dostrovsky 1975], [Cohen 1984]); mathematical tools (like differential equations, Fourier analysis, etc.) for representing quantitatively and mechanistically sound phenomena as wave phenomena had been being already developed since the beginning of the 18th century; appropriate scientific instruments (resonators, sirens, tuning forks, etc.) for the systematic and accurate empirical observation of acoustic phenomena – thus not just simply musical instruments, as in the past – were in use already since the beginning of the 18th century too; the modern empirical and mechanistic approach in studying human anatomy was already common in the 17th century. Thus, Helmholtz's acoustic theory can be considered entirely in the wake of this particular empirical paradigm defining the scientific study of acoustic phenomena after the Scientific Revolution – maybe as its apotheosis. It shares with the previous research about acoustic and auditory phenomena the same physical concepts, the same mathematical tools, the same scientific instruments and, above all, the same theoretical and methodological presuppositions of mechanistic and empirical kind.

If not revolutionary in itself in the sense of Kuhn, Helmholtz's theory however represented a great solution of an important, old *puzzle* within the normal research about acoustic and auditory phenomena: explaining the physical nature of sound and the mechanism behind human auditory perceptions in the framework of the same, unified mechanic theory. Now, beyond the study of the history of music theory, the analysis of the recent developments and discoveries in acoustics and physiology of hearing, some relevant aesthetic issues concerning the temperaments and the construction of scales and chords, *philosophy* too played for Helmholtz an important *heuristic role* in the formulation of this generalized theory of sound, in particular the positivism and the formalistic aesthetics of music.

The positivist and mechanist philosophy indeed constituted the general philosophical background of Helmholtz's whole scientific work. The optimistic view that science – physics in particular – would soon manage to explain everything by means of the same general mechanic theory was already well settled among nineteen-century scientist. The success of Newton's rational mechanics since the end of the 18th century with great scientists like Laplace and Lagrange, important discoveries in physics like that of the principle of energy conservation by Helmholtz himself, Mayer and Joule in 1847 made scientists think that the most important laws of the Universe were already known; it was only a matter of time and of several calculations to get a definitive mechanical theory explaining and unifying all kinds of phenomena. Positivist philosophy thus certainly represented for Helmholtz a heuristic catalyst in formulating a mechanic theory able to explain at the same time both the physical and the psychophysiological side of acoustic phenomena. Furthermore, some years before *Die Lehre*, in 1857, he published the first part of his *Handbuch der physiologischen Optik* (the second part was published in 1867) [Helmholtz 1867], in which he formulated a mechanic theory of human vision. It is then clear that the launch of the research into the *sensations of tones* in 1955 (see [Meulders 2010], [Cahan 2018]) was aimed at gaining a possibly unified mechanic explanation of all aspects of human perceptions, in the general philosophical context of a positivistic understanding of science.

In this period, positivism influenced however not only science and scientific practice, but also other fields of culture and art, just like music, for example. In fact, at the beginning of the 19th century an entire set of new disciplines arose taking music as a mere scientific subject to study systematically from different perspectives: musicology, aesthetics of music, music criticism, history of music, music paleography, philology of music, music pedagogy and, as we know, acoustics and psychophysiology of music. This practically meant the downfall of musical Romanticism, which conceived music as the only great art capable of representing human sentiments or even some kind of unquantifiable "absolute truths" (see [Fubini 1964, V]). As a result, philosophers of music and music aestheticians too became more positivistically oriented in reflecting about music in itself as an art: the romanticist conception of music was refused and replaced by a formalistic and intellectualistic one; music was now mostly regarded as no longer capable of expressing meanings of any sort, but practically as an autoreferential and asemantic form of art. One of the main proponents of this new "formalistic" aesthetics of music was the Austrian music critic and philosopher Eduard Hanslick in his *Vom Musikalisch-Schönen* [Hanslick 1854], whose ideas have been explicitly shared by Helmholtz in *Die Lehre*, as Hanslick's work is even quoted in the preface to the work [Helmholtz 1863, 2]. For Helmholtz too, in fact, music is pure form without meaning; tones express or represent nothing, they have any purpose but themselves: «Music stands in a much closer connection with pure sensation than any of the other arts. [...] In music, the sensations of tone are the material of the art. [...] music has a more immediate connection with pure sensation than any other of the fine arts, [...] tones and the sensations of tone exist for themselves alone and produce their effects independently of anything behind them» [Helmholtz 1863, 2-3]. It is then because music is conceived as *ontologically* constituted by nothing but tones and sensations of tones which represent nothing else but themselves that, for Helmholtz, a unified mechanic explanation of the physical nature of tones and of the psychophysiological mechanism by which they are perceived and elaborated will be sufficient and appropriate to serve as a basis for a new *theory of music*, even of some aesthetic usefulness for musicians (an analysis of the aesthetical consequences of his mechanic theory of sound will be in fact carried out by Helmholtz in the last part of the work). On that note, it emerges thus clearly that this new particular philosophical conception of music as autoreferential and asemantic endeavor has worked as a further heuristic catalyst in Helmholtz's research for a generalized mechanic theory of sound.

2.1.1.2 The Eighteenth-century debate about the Foundations of Calculus

By the previous historical instance, we showed that philosophy can be relevant to normal science serving an important *heuristic function*, for example as fosterer of the solution of certain puzzles given in certain paradigms. By a further historical instance on the eighteenth-century debate about the foundations of calculus, we will show now how philosophy can be relevant to normal science also serving an important *analytic function*, for example as highlighter of certain anomalies in a certain paradigm.

Whether mathematics is a science at all and, if so, a science which can even know some kind of scientific revolutions – possibly conceptualizable through Kuhn's epistemological categories – is still a question with no definitive answers (see [Gillies 1992]). It remains however generally acknowledged that the invention of the calculus in the late 17th century, traditionally attributed to the joined but independent work of Isaac Newton and Gottfried Wilhelm von Leibniz, represented in mathematics an event of clear revolutionary character (see [Guicciardini 2003], [Jahnke 2003], [Dupont, Roero 1991], [Bottazzini 1990], [Bos 1980], [Grattan-Guinness 1980], [Castelnuovo 1962], [Kline 1953], [Geymonat 1947]). The algebraization and the formalization of old geometrical problems by means of analytic geometry, the important discovery of the reciprocity, in geometry, between the problems of the quadrature of a curve and the tangent to a curve (which will be, in analytic terms, the reciprocity of integration and differentiation), the replacements of the old, complicated methods of infinitesimal geometry with a new, handy mathematical formalism in order to deal with sums and differences involving infinitesimal quantities, calculate areas subtended to curves, etc. brought about a radical change in mathematics, which transformed the entire discipline very deeply. This change did however not imply the refutation of the entire past mathematics, which in fact continued (and continues) to be epistemically valid. Nevertheless, it undoubtedly meant a remarkable decrease of its importance within the whole discipline – indeed, at least for [Dauben 1984], exactly these two conditions (radical transformation of current mathematics, loss of importance of past mathematics) define the characterization of a mathematical revolution (see also [Dieudonné 1976]).

The new "paradigm-launching" mathematical ideas introduced by Newton and Leibniz rapidly spread among European mathematicians and natural philosophers, who, during the first decades of the 18th century, undertook a thorough investigation into their intrinsic potential, as well as into the possibility of their application to problems in physics, mechanics, etc. – Newton himself being one of the first ones in this respect. Now, as [Giorello 1992, 140] points properly out, if in such a phase of initial articulation, «while a new paradigm is still struggling for its life, the rapid progress of events and the need for it to achieve its end prohibit the grounds of its legitimacy to be dwelt upon or questioned[,] once the paradigm has achieved stability through its success, and threatens to take over other realms of ideas, more and more people will be inclined to look into the foundations of the paradigm and examine its soundness», that is, searching for some possible anomalies in it. Indeed, the case of calculus fits this pattern too: already in the 1730s and 1740s several mathematicians and philosophers started to discuss about the foundations of calculus, especially questioning one of its most fundamental cornerstones, namely the use of infinitely small and infinitely great quantities in calculations and demonstrations. This marked the beginning of a long debate in mathematics which will find some solutions for example only with the introduction, in the 19^{th} century, of the *epsilon-delta* approach to mathematical analysis by Cauchy, Weiestrass and others – which will practically exclude infinite and infinitesimal quantities from mathematics, then with Cantor's set theory and transfinite numbers – which will represent a rigorous mathematical justification for the use of actual infinite quantities in mathematics, though not of infinitesimals, and finally with Abraham Robinson's nonstandard model of analysis – whose theoretical basis will be a logically consistent and rigorous construction of the field of the hyperreal numbers containing all real numbers *plus* infinite and infinitesimal numbers, thus legitimately "readmitted" to mathematics (see [Jahnke 2003], [Bottazzini 1990], [Dauben 1988], [Grattan-Guinness 1980], [Geymonat 1947]).

One of the earliest, strongest and most representative criticisms of the calculus was that made by the Irish philosopher and Anglican bishop Georg Berkeley in his *The Analyst; Or a Discourse Addressed to an Infidel Mathematician* [Berkeley 1734]. Berkeley's critique of the calculus can be divided into two interrelated parts (see [Sherry 1987]): the logical and the metaphysico-gnoseological criticism.

The first criticism is addressed just to the logically controversial use of infinitesimals in mathematical calculations and demonstrations. Berkeley says that this use suffers of a specific logical fallacy, the *fallacia suppositionis.* A *fallacia suppositionis* corresponds to gain, within the same argument, certain points from certain initial suppositions, and then to infer the final conclusion combining those points with the negation of the initial suppositions, which clearly amounts to a fallacious argument – in predicate logic:

$$(p \longrightarrow q) \wedge \neg p) \overset{\lightning}{\longrightarrow} s \, . \tag{a}$$

In fact, as the Irish bishop highlights, in calculus, infinitesimal quantities – like Leibniz's infinitesimal differences *dx*, *dy* or Newton's moments *o* – are firstly treated as non-zero quantities, i.e. within the *performance* of demonstrations and calculations, but then made equal to zero in the *conclusion* of the given demonstration or calculation.

For example, in Leibnizian terms, this would correspond to take at the same time, i.e. within *the same* demonstration or calculation, an *infinitesimal* quantity dx with

$$dx \neq 0 \text{ and } dx = 0 \ , \tag{b}$$

and, taking x to be a *finite* quantity and defining the predicates in Formula (a) as follows:

$$\begin{aligned} p &: dx \neq 0; \\ q &: x + dx \\ \neg\, p &: dx = 0, \\ s &: x \ , \end{aligned} \tag{c}$$

to operate consequently with it in a mathematical argument which would however be fallacious:

$$\underbrace{(\ dx \neq 0 \longrightarrow x + dx\ )}_{\substack{\text{Performance of the} \\ \text{demonstration or calculation}}} \wedge \underbrace{dx = 0\ ) \overset{\lightning}{\longrightarrow} x}_{\substack{\text{Conclusion of the} \\ \text{demonstration or calculation}}} \ . \tag{d}$$

In other words, this argument would conclude that

$$x + dx = x \ , \tag{e}$$

whereby the non-zero infinitesimal quantity dx used for the entire performance of a given demonstration or calculation is in the conclusion simply and unfoundedly *discarded*, made equal to zero.

Berkeley thus criticized as logically detrimental using infinitesimals – defined as «ghosts of departed quantities» [Berkeley 1734, XXXV] – in mathematics and thinking even to ground calculus on them. Indeed, despite many recent historical studies about the foundations of Leibniz's calculus deny the presence of proper logical contradictions in it [1], Berkeley logical criticism did not fail to reach effectively its purpose: as we saw above, entire generations of mathematicians tried, in the following centuries, to develop well-founded versions of calculus avoiding the *anomalies* highlighted by Berkeley, mostly banishing the controversial notions of infinite and infinitesimal from the realm of mathematics.

But it was not only for the violation, in some parts, of basic logico-philosophical principles like the law of noncontradiction that calculus was for Berkeley to be regarded as presenting relevant anomalies. For the Irish bishop, in fact, the concept itself of infinitely small and infinitely great quantity was intrinsically problematic, and this from a truly philosophical point of view. Berkeley was namely one of the most important and rigorous representatives of modern nominalism and empiricism. He exasperated empiricist philosophy up to a form of proto-idealism in which the existence of the matter itself as something external and independent from us is denied and made to depend exclusively on our sensations [Rossi 2005], as synthetically expressed in the famous motto «*esse est percipi*» [Berkeley 1710, 38]. The gnoseological counterpart of this ontological position is then obviously imaginable: general or universal concepts are meaningless, since their external references – in form of perceptions – are in any way given to us, and all what we can know, i.e. all what can be in our mind in form of idea, is instead only the content of our perceptions – which, from the ontological point of view, are the things themselves, as we know. Now, if all ideas derive from perceptions and are even the content of these perceptions, and perceptions can be nothing but finite (as finite are the real objects with which they ontologically coincide), it is clear that the ideas of infinite and infinitesimal can be nothing but empty ideas, since no infinite perceptions are possible (and no infinite or infinitesimal objects are experienceable). Like universals, they are thus meaningless concepts. In this consisted Berkeley's further criticism of calculus, namely the metaphysico-gnoseological one.

If, on the one end, nominalistically- and empiristically-minded philosophers and mathematicians almost blindly agreed with Berkeley on these logical and metaphysico-gnoseological critical conclusions about infinite and infinitesimal quantities in mathematics, on the other hand, supporters of Leibnizian and Newtonian calculus – like De l'Hôpital, Maclaurin, the Bernoullis, Euler, etc. – tried to found rigorously their use in calculus, doing this however from diametrically different philosophical standpoints, namely the formalism and the rationalism. Leibniz himself (and partly Newton), indeed, resolutely maintained a formalistic conception of infinite and infinitesimal quantities in mathematics. He conceived them as nothing but mere fictions *without any external reference* or *intrinsic meaning* [Leibniz 1702], [Leibniz 1706], fictions useful to shorten and simplify mathematical reasonings or facilitate mathematical proofs, just like for example imaginary numbers. This conception indeed represented a direct mathematical instantiations of Leibniz's general philosophical presuppositions, opposed to Berkeley's ones. Leading representative of modern rationalism, Leibniz, in fact, conceived calculus within the more general philosophical project of developing a universal formal language – the *characteristica universalis* – through which all sort of philosophical, scientific and mathematical arguments could automatically be carried out and led to correct conclusions. This language was expected to contain symbols and formulas whose combinations were ruled by specific algorithms ensuring the correctness of arguments and conclusions. From this derived the great attention paid by Leibniz in the development of calculus to the formalism and to the symbols, whose *function* (rather than meanings) had to be well defined and, above all, easy to manipulate: after all, as mentioned, he was primary looking for a *calculus*, i.e. for a handy, "automatic" method to deal with old complicated geometrical problems.

Now, what the historical case of the debate about the foundations of calculus can show us is how different philosophical (and especially ontological) standpoints have led, already during the initial phases of this debate, to different ways of conceiving, defining, analyzing and even criticizing the mathematical concepts involved and used in it – just as that of "infinitesimal". In other words, philosophy served as an *analytic tool* both for reformulating in an ever sharper, logically more rigorous way the most fundamental "paradigmatic" concepts of calculus, and for discovering, by analyzing, possible anomalies and shortcomings of them, in a sort of dialectic continuous process (i.e. not periodically interrupted by revolutions) of criticisms, analyses and reformulations which has finally brought about the modern mathematical analysis as we know it today.

2.1.2 Second Intrinsic Argument: Impossibility of Outward Influences by Revolutionary Science

Important functions attributed by De Haro to philosophy are «to provide ethical guidance and discover (broad) goals for science, […] to point out and articulate the interrelations between concepts that are found in different domains of the natural sciences as well as the social sciences and the humanities, […] to explain how observations fit in the broader picture of the world, and to create a language where scientific results and broader human experience can complement and mutually enrich each other» [De Haro 2019, 9]. These are indeed "extra-epistemic" functions, functions of mediation between science and other various non-scientific or extra-scientific instances (ethics, non-scientific disciplines, human experience, etc.) De Haro ascribes to philosophy.

De Haro's presupposed – and agreeable – assumption here is that science can have some kind of influence outwardly, i.e. on non-scientific or extra-scientific instances – as well as the other way around. In this respect, philosophy should reflect about the different aspects and possible problems of this reciprocal influence, about the ways in which it takes place, about the benefits and the downsides it can bring to both the involved parties, as well as help to make this influence fruitful for both sides.

It is however very difficult to concede that this particular mediating function of philosophy is “activated” only or mostly during scientific revolutions and then “disactivated” during periods of normal science. On the contrary, this function becomes really important especially once science – i.e. a new scientific paradigm, in Kuhnian terms – has gained some stability and can therefore exert a significant, qualitatively and quantitatively recognizable influence on society, ethics, or non-scientific disciplines.

Namely, no real, significant, observable influence of science on extra-scientific instances can take place when science is just going through a period of revolution. Society, human experience, etc. cannot be influenced by a transitional instance like revolutionary science, by something still indetermined, unstable and even “unknown”: we cannot say, in a nearly predictive fashion, which kind of science will come out from an ongoing revolution, what its epistemic contents and methodologic aspects will be, which epistemic and non-epistemic values will be followed by the scientific community driving it, etc.. This is because, accepting Kuhn’s view of science, we also accept the incommensurability between paradigms: each paradigm is radically different from the previous one, and we cannot obtain from the study of past paradigms some relevant information on the basis of which such a prediction can be possible. From this it however follows also that the quality, the character, the extent of the possible influence of the coming science on extra-epistemic instances is unpredictable too during the preceding period of scientific revolution and thus not philosophically problematizable yet, this, until the new scientific paradigm namely settles at the end of the revolution and begins to develop. In fact, only once a paradigm has settled as something stable and determined, the scientific community driving and representing it has consolidated his epistemic and non-epistemic values, outward influences take place, which become thus also philosophically investigable. Paradigmatic scientific knowledge, for example, finds applications in technology, which enters society and people’s everyday life, changing them sometimes very profoundly; conferences, journals, books, institutions – principal “permeable touch-points” between science and extra-scientific instances – begin to express directly or indirectly the values of the scientific community guiding normal scientific research, etc. Indeed, instances like these – technology, institutions, etc. – represent the concrete and material side of the influence science can have outwardly, and thus an important (maybe the only possible) empirical, tangible starting point for a philosophical reflection about it. Yet they are namely not characteristic of revolutionary science, but of normal science.

If the influence of science on extra-scientific instances is philosophically investigable only during periods of normal science – when, moreover, this influence also has even the *time* to become concrete and evident – the same applies for the inverse perspective, i.e. considering the possible influences of extra-epistemic factors on science. In fact, the exact identification of those social, psychological, moral factors which, in Kuhnian terms, possibly guided scientists in the search for and in the formulation of a new paradigm during a scientific revolution – identification which would be indeed a task of philosophy – is possible, again, only once the paradigm has been articulated and science has gained new stability after the revolution. Tangible and observable effects of the influence of those factors on science, in fact, will show themselves only in the long-term, normal scientific practice, only in and through the methodological and epistemological decisions scientists will take in their everyday work within the new paradigm. Only then, consequently, they will become philosophically investigable.

2.2 Extrinsic Argument: Rejecting De Haro's Presupposed Kuhnian Epistemological Perspective

So far, we have put forward arguments which tried to refute, from an *intrinsic* perspective, De Haro's conclusion that philosophy is relevant to science only or mostly during scientific revolutions – a conclusion we expressed in a nearly syllogistic fashion in [Section 1]. "Intrinsic" means here that we attempted to refute that conclusion accepting the same general epistemological perspective from which the author implicitly formulates it, namely Kuhn's understanding of science [De Haro 2019, 7]. In fact, as it clearly emerges, our previous arguments shared with De Haro's thesis the same Kuhnian conceptual vocabulary and the same Kuhnian epistemological framework.

De Haro's conclusion can however be criticized also from an *extrinsic* standpoint, i.e. refuting the general epistemological perspective within which it has been formulated and which it depends on with a clear lack of generality. This is possible since De Haro's preference for Kuhnian epistemology as philosophical framework of his arguments and his analyses of the relationship between science and philosophy seems indeed to be quite arbitrary. He does not put forward any explicit justification for why Kuhn's epistemology, Kuhn's way of understanding science should be more appropriate and preferable than others – say falsificationism or Lakatos' methodology of scientific research programmes – for analyzing the relevance of philosophy to science. He just takes them on the basis of a kind of authority principle as apodictically given and not in need to be further justified or problematized philosophically, building consequently on them his entire argumentation.

Thus, rejecting in bulk the Kuhnian epistemological presuppositions on which De Haro's conclusion relies, the logical soundness of his argument, as presented in [Section 1], would be completely lost. And indeed, changing this initial epistemological perspective would make the question itself of whether philosophy is relevant more to revolutionary or normal science turn to a mere philosophical pseudo-problem. In fact, if we supported a Popperian epistemology, for example, we should grant by definition a great importance to philosophy in normal science: in contrast to Kuhn's opinions, for Popper, normal science is by definition critical discussion, meaning that those *why-questions* which for Kuhn are characteristic only of revolutionary science are instead asked and answered for Popper in normal science too, where, as a consequence, philosophy can generally have a great relevance (thinking for example to its heuristic role in the context of discovery). Or still, taking Lakatos' methodology of scientific research programmes as epistemological framework of an analysis of the relevance of philosophy to science, we would correspondingly reject *a priori* the distinction between revolutionary and normal science, since for Lakatos – in contrast to Kuhn – science develops within a rational and continuous dynamics of evolving research programmes, which change from progressive to degenerative and vice versa.

3 Conclusion

In [Section 2] we considered essentially three arguments against De Haro's conclusion about the basic irrelevance of philosophy to normal science (see [Section 1]), the first two of intrinsic nature [Section 2.1] and the third one of extrinsic nature [Section 2.2]. By the first intrinsic argument [Section 2.1.1], we have shown how this conclusion is generally unacceptable since the possibility of philosophy to serve a heuristic [Section 2.1.1.1] and an analytic function [Section 2.1.1.2] in phases of normal science is theoretically admissible as revealed by the analysis of some episodes of history of science: philosophy can help normal science in solving puzzles and in analyzing, criticizing, defining and articulating concepts constituting its epistemic foundations, indeed for the entire life of a paradigm. By the second intrinsic argument [Section 2.1.2], we have then shown that philosophy can serve the mediating function between science and extra-scientific instances De Haro himself attributes to it only once science has settled as a stable, epistemically and axiologically well-determined, *normal* enterprise, whereby the reciprocal influences between the two sides can finally take place, become evident and thus philosophically problematizable. Lastly, in the third extrinsic argument [Section 2.2], we have shown how De Haro's argumentation entirely loses its validity once the Kuhnian epistemological framework on which, without justifications, he bases it is rejected in bulk and other different epistemological frameworks possibly are considered.

On the basis of these arguments, we thus claim that De Haro's conclusion is incorrect and that philosophy can indeed have great relevance in periods of normal science too.

Notes

[1] For example [Bair et al. 2017], [Bair et al. 2013], [Katz, Sherry 2013], [Katz, Sherry 2012] highlighted that in particular Leibniz's use of infinitesimal quantities in calculus (Newton's case would be different here) does not certainly fit the current standards of mathematical rigor, but not for this is to be regarded as logically inconsistent. In fact, as it emerges from his manuscripts, letters and unpublished works about mathematics-related foundational concerns – the most relevant one, *De quadratura arithmetica* (1676) [Leibniz 1676], published only in 1993! – Leibniz embedded his calculus into a theoretical framework with such principles regulating the use of infinite and infinitesimal quantities, as well as the different relations between them and finite quantities – e.g. principles like the *lex continuitatis* or the *lex homogeneorum transcendentalis* – that indeed no inherent logical inconsistency at all could be attributed to it as a whole. Thus, the charges of logical inconsistency, like those of Berkeley and of the major part of the mathematicians and philosophers of the last two centuries (including even Robinson!) are simply to be regarded as due to a historiographically and theoretically insufficient knowledge of Leibniz's himself foundational ideas about calculus.

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