PROBLEMATA
International Journal of Philosophy

TOWARDS A HISTORY OF PHILOSOPHY OF SCIENCE

RUMO A UMA HISTÓRIA DA FILOSOFIA DA CIÊNCIA

Marcos Amatucci[1]

Recebido em: 01/2021
Aprovado em: 06/2021

Abstract: In order to be accepted as scientific projects worth funding and/or as research papers worth publishing, scientists must, through sound methodological canons, that that piece of work "is Science". The accepted methodological descriptions are, however, are often dated and committed with philosophical views of what Science is which are already deeply criticized and no longer accepted in the Philosophy of Science community. A critical history of Philosophy of Science is needed for clarifying what problems the adoption of a given method is likely to raise. This modest communication raises this problem and sketches a direction of such history.
Keywords: Philosophy of Science; History of Philosophy of Science; Science Methodology; Received View; Method.

Para serem aceitos como projetos científicos dignos de financiamento e / ou como artigos de pesquisa dignos de publicação, os cientistas devem, por meio de sólidos cânones metodológicos, que aquele trabalho "é ciência". As descrições metodológicas aceitas são, no entanto, muitas vezes datadas e comprometidas com visões filosóficas do que é a Ciência, que já são profundamente criticadas e não mais aceitas na comunidade da Filosofia da Ciência. Uma história crítica da Filosofia da Ciência é necessária para esclarecer quais problemas a adoção de um determinado método pode suscitar. Essa comunicação modesta levanta esse problema e traça uma direção dessa história.
Palavras-chave: Filosofia da Ciência; História da Filosofia da Ciência; Metodologia da ciência; Visão recebida; Método.

Introduction

When a scientist begins a research project, she establishes, broadly speaking, general goals, relevance, object and method. Methodology is a non-alienable part of research projects, and it is often a central piece of both funding and publishing evaluation.

Research evaluating for funding purposes means that society must rely that the resources

[1] Doutor em Filosofia pela UFRJ. UniCeuma. Email: marcosamatucci@espm.br

will be applied in Science – the special kind of knowledge which society considers reliable. On the other hand, editors of scientific journals, whose prestige depends upon the degree of confidence that scientific community has on its rigorous scientific evaluation, must be sure that the results to be published are scientifically accepted results. Again, scientific community and practitioners must rely on publish results to carry on safe professional practices.

Therefore, however relevant the research theme is, its proponents must demonstrate that the matter at hand will be handled "scientifically" at the proposal moment, the same at the publishing moment. That is the purpose of the methodology section or research proposals and publications.

Thus a methodology section of a research text is meant to grant that that text is… Science.

Nevertheless, what Science is or what it is not (hence how to tell if a given work is scientific or not) varied considerably along the line of the history of Philosophy of Science. Particularly after Hume, the demarcation criteria is still an ongoing discussion and the model of what constitute Science evolved from apodictic knowledge to falsifiable knowledge to the Received View, and, after the refutation of the Received View, it appears that no dominant paradigm has been established.

While this is happening in the field of Philosophy of Science, the methodology section of scientific projects and papers adheres to one "phase" of the scientific canons, along its history, e.g. the hypothetic-deductive method, or the inductive method, or the falsifiability criteria, or protocol sentences and correspondence criteria – with no concern to the critics that a given method suffered in history. As (Suppe, 1977) puts, in relation to "operational definitions",

> It seems to be characteristic, but unfortunate, of science to continue holding philosophical positions long after they are discredited. Thus, for example, Skinner's radical behaviourism, which insists in operational definition, came into prominence and dominated behavioural psychology well after most philosophers had abandoned the doctrine of operational and explicit definitions; taxonomists today insist on operational or explicit definitions for taxa…" (p. 19, footnote).

This is a recurrent problem in projects, papers, thesis and dissertations, and all kind of evaluating systems – funding, reviewing, PhD committees, hiring committees and so forth. Nevertheless, when one looks for a comprehensive History of Philosophy of Science it is nowhere to be found. Most texts on this subject show an instrumental piece of history as a mean to contextualize its problem; and there are books and papers – as well as university programs – on "History *and* Philosophy of Science" (e.g. (DeWitt, 2018; Lancaster, 2019; Matthews, 2014; McMullin, 1970)).

A critical history *of* Philosophy of Science should point the way to scientists to have a better epistemic certainty of where their proposed methodology should fit, of which criticisms it is subject, and the limitations of the certainty of the results.

This modest communication sketches my first attempt of such history.

Brief sketch on History of Philosophy of Science: the usually known history

Parmenides did not write on Science itself, but his delimitation of the principle of rational discourse – the principle of identity and non-contradiction – and the subsequent problem of the movement (if any being is equal to itself, then movement is illusory) set the "agenda" for Plato and Aristotle.

It may be said that Plato was the first to distinguish between two kinds of knowledge – logos and doxa. In Theaetetus (Plato, 142a–210d), just after discussing the reasons of the come-to-be – which is the movement – Plato concludes that there is a rational explanation (*logon*) which differs from mere opinion (*doxa*). But acquiring the true knowledge is reaching the world of forms, while on earth the imperfect and ever-moving individuals are not fit to universal knowledge. That means that Plato needed two worlds to explain this one. Nevertheless, the platonic idea of objective non-real beings will be relived by the nineteenth century logicists, e.g. Frege and Russell, in their struggle against the psychologism in Mathematics[2].

Aristotle manage to maintain Parmenides principles and explain movement through his theory of potency and act: the being is itself along transformations. Also, Aristotle lays the basis of ontology: "*the being is said in many ways*" – the categories, which are, with exception for substance, the subject for excellence, the different ways one can predicate a subject. Thus, one may predicate of a subject its quality, causality, relation, number and so forth[3] (Aristotle, I-XV). In his Metaphysics, Aristotle is addressing the world itself, and answering the ontology question, "what there is". Regarding to Science, Aristotle establishes the concept that will endure 2,500 years: Science is universal and necessary knowledge.

Performing an incredible injustice with the scholastic period, I will state only that in it

[2] The number 3, for instance, is an objective non-real entity, and not a psychological one.
[3] Substance, causality, quality, number, relation, place, time, situation, state, action and passion.

Aristotle was taken, side by side with the Revelations, *the* knowledge already laid to men. This means that it was a period of rich development of interpretative methodologies – hermeneutics – but with no support for innovations. Thus, when the scientific revolution – mainly in Astronomy – began, scholars had few or no instrumental to deal with this new reality. New science and modern philosophers appear together.

The very first philosopher to dare question Aristotle was Bacon, in his New Organon (Bacon, 1984). Bacon lay down rudiments of inductive reasoning, later enhanced and detailed by Stuart Mill. But undoubtedly the main epistemic revolution of this period is the one of Descartes. In his Meditations, Descartes launches a doubt whether we really have access to reality: it may not be the case (Descartes, 1641). The epistemic revolution therefore changes the focus of human investigation from ontology – what there is – to epistemology: what can we know. From now on there is no sense in asking the old question, and the modern period ends with Kant stating that we only have access to phenomena, and all empirical Science will be Science of the phenomena.

The next important step Philosophy of Science after Descartes is the scepticism of Hume. We must have in mind that modern philosophers, notwithstanding their criticism on Aristotle, still retain the concept of Science as universal and necessary knowledge. It is not possible to understand none of the moderns without remembering this – in particular, Kant's Critique has no sense without this concept of Science. Even because it is in function of Hume's demolition of the possibility of making Science from empirical data that the very concept of Science had to be abandoned.

The sceptic argument of Hume (Hume, 1739) is as follows. There are two kinds of truth, the one arising from relation between concepts – this one is capable of necessity; and the one arising from questions of facts. Now, questions of facts are observations of particulars, or generalizations on observations on particulars made through induction reasoning. But why are we authorized to make induction? Hume answers that we are not. The extension of knowledge from observed facts to unobserved facts presupposes too much – mainly, that the future will repeat the past, which is not provable. An analogous reasoning is applied to causality: it is not relation between concepts, neither question of fact, once we do not observe causality, but only its antecedent and consequent. With no induction and no causality, Science as we knew it was doomed.

The response of Kant to Hume, in an attempt to save Newtonian mechanics, is, in very coarse grains, that we are authorized to make universal and necessary propositions from experience because we have *a priori* pure intuitions of space and time that necessarily precedes any experience. This was known as Kant's transcendental reasoning: what are the conditions of possibility for something to happen. And the conditions of possibility for human experience to happen are the *a priori* intuitions of time and space. The objects of our experience must be in time and space, and therefore time and space must be given prior to any possible experience, as *forms* of perception. We do not perceive objects "as they are", but through forms of perception, what turns objects into *objects of our perception*. That is the main argument of the Critique of Pure Reason (Kant, 1787). However, that solution carry the heavy load of making space and time ideal, and, according to some interpretations, subjective; what gave birth to German idealism, and further idealist philosophies of different shades. The

Is was a load because German idealism deviated from the course of Philosophy as a companion of Science, relationship where Kant put Philosophy. The result is that 90 years later, Liebmann call for a turn back to Kant. In the words of Porta, professor specialized in this period:

> With the exhortation "Zurück zu Kant!" (Let's go back to Kant!) The Otto Liebmann's book entitled "Kant und die Epigonen" (1865), in which, the mentioned author defends the thesis that German idealism does not it was the consequent development of transcendental philosophy but, on the contrary, a setback, a loss of direction. Hence the famous appeal, traditionally considered the symbolic beginning of neo-Kantism. (Porta, 2005), p. 35.

The history of neo-Kantians is important to understand how Philosophy of Science evolves from the seventeenth to the nineteenth century. This history has not continuity if we consider only the "best sellers" of German idealism (Fichte, Schelling and Hegel) and the irrationalists (here considering Schopenhauer, Nietzsche and Kierkegaard). The less known neo-Kantians are in great part responsible for the transition. The other stream is the positivist tradition.

Brief sketch on History of Philosophy of Science: the less known history

The neo-Kantianism was the main philosophical position in German until WWI. Beiser attributes to this its end: neo-Kantians were enthusiastic about the German participation in this war, and some such as Natorp, Cohen and Riehl indeed wrote propaganda pro-war (Beiser, 2015). Nevertheless the "back to Kant" moto and the criticism to German idealism, mainly the Hegelian attempt to make Philosophy a surrogate to Science, the movement is still idealist

(Heis, 2018), and has cross-influences with the British neo-Hegelians such as Whewell, Bradley, Bosanquet, DeMorgan, McTaggart and others, who influenced the Cambridge student Bertrand Russell.

The neo-Kantian movement has antecedents in Liebmann, Trendelemburg, Fries and Lotze; but it became known by the two main schools of Marburg and Baden. At Marburg the main philosophers were Hermann Cohen (1842–1918), Paul Gerhard Natorp (1854 –1924) and Ernst Cassirer (1874—1945). They main concern were the Philosophy of natural Sciences. In Baden, Wilhelm Windelband (1848-1915) and Heinrich Rickert (1863-1936) were more concerned with the Philosophy of Cultural Sciences.

In common, neo-Kantians return to the relationship between Philosophy and Science establish by Kant: it is Philosophy job to make explicit assumptions and foundations of the scientific method and reasoning (in opposition to Hegel's stand of substituting Science by Philosophy). Thus, Science and Philosophy are mutually autonomous. The idealist character of the ideas was meant to challenge materialistic dogmas concerning the nature of reality. To even know reality, *a priori* ideal elements were necessary. Not only the pure intuitions of Kant, but also all arsenal of Logic, Mathematics and Geometry are necessary *a priori* for a scientist to conduct its experiment. Therefore, science *does not* "prove that reality is material". This idea would be completely imported by British neo-Hegelians, with social and religious motives to defend themselves from the growing materialism (Passmore, 1966).

The fact that the neo-Kantians were idealist must not be confused with any kind of subjectivity. They believed in the objectivity of reality, and fought the subjectivism arising from the new science of Psychology. Also, and in this respect shoulder to shoulder with Logical Positivism, rejecting any form of speculative metaphysics.

If the apparent disconnection of neo-Kantian with some effective impact on Science activity is due to its inside nature of maintaining autonomy on both fields, or it is the very same lack of a History of Philosophy of Science we are trying to gap (once most of the narratives on the theme verse on more broad themes of History of Philosophy), or yet if is due to the incipiency of my studies, I cannot say at the moment. If anything, though, neo-Kantianism is responsible for carrying important traditions from the modern age to contemporary Philosophy of Science.

If we could say that the neo-Kantian movement as a rationalist bridge from the seventeenth century to the nineteenth, then we could equally say that the logical positivism is the empiricist bridge.

Although logical empiricism is seen as a movement from the beginning of the twentieth century (circa 1920), its roots goes back as far as Bacon, Comte and Mill. Of these, a more comprehensive History of Philosophy of Science should have a whole chapter on Mill's System of Logic

Logical positivism is the philosophy of the Vienna Circle and Berlin School. From the first have we mainly Moritz Schlick (1882-1936) and Rudolf Carnap (1891-1970), also, Alfred Ayer (1910-1989); from the second, Hans Reichenbach (1891-1953). Tarski and Gödel were in contact with the Vienna Circle, participating sporadically in their meetings.

The antecedent of Logical positivism was the mechanical materialism that the neo-Kantians were (successfully) trying to challenge. The mechanical view included Pierre Maurice Marie Duhem (1861 –1916), Ernst Mach (1838-1916), Jules Henri Poincaré (1854- 1912), Hans Hahn (1879-1934), Otto Neurath (1885-1945) and Moritz Schlick himself, who joins the Vienna Circle in 1924. Many of these were critical of the "crude" mechanical materialism; in special, Mach was neo-Kantian for a time, for then rejecting it and setting the basis for the logical positivism. All logical positivists after Mach are in his debt.

Vienna Circle was also strongly influenced by Wittgenstein's Tractatus. In particular, the meaning of scientific propositions, which should have logical, theoretical or empirical *cognitive significance*. That would be summarized by the moto "the meaning of a term is its method of verification", what became known as the verification theory of meaning. All significant assertions about anything in the world should be reducible to *protocol sentences* – sentences expressing observable facts.

This development eventually resulted in the Received View, a set of logical requirements for a theory to be considered scientific. There are several versions of the formulation of the received view; the more important ones laid down by Carnap. Suppe has a nice concise expression of the formalization of the Received view: a scientific theory should be able to be expressed as an axiomatic theory formulable in a logical language L such that:

> (i) The theory is formulated in a first-order mathematical logic with equality, L;
>
> (ii) The non-logical terms or constants of L are divided into three disjoint classes called vocabularies:
>
> a. The logical vocabulary consisting of logical constants (including mathematical terms).
>
> b. The observational vocabulary, V_O, containing observation terms.
>
> c. The theoretical vocabulary, V_T containing theoretical terms.
>
> (iii) The terms in Vo are interpreted as referring to directly observable physical objects or directly observable attributes of physical objects.

(iv) There is a set of theoretical postulates T whose only nonlogical terms are from V_T.
(v) The terms in V_T are given explicit definitions in terms of V_O by correspondence rules C – that is, for every term F in V_T, there must be given a definition for it of the following form:
(x) (Fx ≡ Ox), were Ox is an expression of L containing symbols only from V_O and possibly the logical vocabulary. (Suppe, 1977) p. 16.

The Received View suffered many criticisms, and Carnap and Hempel worked to overcome the ones they saw. They included problems with dispositional terms such as "fragile" which are problematics due to limitations of the logical connective for material implication ("⊃") (which is true of false antecedents); so the rule (v) changed from "explicit definitions" to (v') "partial interpretations" and from that to (v'') an "interpretative system" satisfying a set o five conditions. But it seemed that each adjustment to the axiomatic set would bring difficulties of its own.

Also, many recognized scientific fields such as biology, psychology or anthropology do not seem capable of such axiomatization. Therefore, the application of the Received View was not universal. But even to the scientific theories prone to axiomatization, problems arise from the observational-theoretical distinction, which is heir of the analytic-synthetic distinction – which was challenged by Quine[4]; from the observational-theoretical distinction – Carnap himself observed that "observational" differs from a more strict sense in Philosophy to a broader sense in Physics, and that the difference could not be separated by a border, being a matter of degree (Carnap, 1966). These issues have impact on the partial interpretation (or in the interpretation system), in the correspondence rules and all over the walls of the Received View[5]. After the Received View, no other paradigm took place. What we do have is several different paths, the difference between them being what part of the Received View they criticize, what part they maintain.

It is not possible here to map all the paths. What is possible is to follow one of the paths, in order to illustrate the problem raised by this paper. The main critics of the Received View were the abovementioned Quine, Khun (of whom I will not talk here) and Popper. Let us briefly see Popper and some follow through after him.

Popper goes back to Hume, taking his induction problem, to criticize the verification criteria of Carnap. Popper (Popper, 2012) formulates the induction logical problem of Hume (H_L):

[4] In *Three dogmas of empiricism* (Quine, 1951).
[5] The detailed demonstration can be found in Suppe, *op. cit.*

> H_L Are we justified in reasoning from [repeated] instances of which we have experience to other instances [conclusions] of which we have no experience? [p. 4]

Popper proposes a solution to the problem of induction. First, he rephrases H_L in L_1 ("*an objective or logical mode of speech*"):

> L_1 Can the claim that an explanatory universal theory is true be justified by 'empirical reasons'; that is, by assuming the truth of certain test statements or observation statement (which, may be said, are 'based in experience')? [p. 7]

As Hume, Popper answers no, we (H_L) / it (L_1) cannot. Now using the logical inference of addition (that is, $p \supset p \vee q$; in particular, $p \supset p \vee \neg p$), Popper again rephrases L_1 in L_2:

> L_2 Can the claim that an explanatory universal theory is true or that is false be justified by 'empirical reasons'; that is, can the assumption the truth of test statements justify either the claim that a universal theory is true or the claim that it is false? [p. 7]

To the question L_2 Popper answers positively: "*the truth of test statements sometimes allows us to justify the claim that an explanatory theory is false*".
Therefore, we cannot verify a theoretical statement, but only falsify it. And Science is a provisory knowledge that is valid until falsified.

That is the falsifiability theory which Popper used as demarcation criteria. This became the paradigm of hypothesis-testing in the hypothetical-deductive method. The reasoning of it, as the axiomatization of the Received View, is supported by first-order logic, and it is even simpler. The hypothetical-deductive method postulates that the hypothesis must implicate the evidence: ($H \supset e$).

According to the basic syllogisms of predicate calculus, we can deduce the consequent from the antecedent in *modus ponens* (which do not help here because to know the truth of the antecedent – the hypothesis – is the problem) ; or we can falsify the antecedent from the falsity of the consequent – here the evidence – in *modus tollens*. The *modus tollens* then is:

$$\begin{array}{l} H \supset e \\ \underline{\neg e\quad\;} \\ \neg H \end{array}$$

This is the basis of contemporary scientific quantitative methodology, where one tries to falsify a hypothesis, and failing to do so counts as accumulated evidence that the hypothesis is “solid”.

This view, however popular among researchers, is highly criticized in Philosophy of Science. Quine, Otto-Appel, Khun and others criticized different aspects of Popper’s falsificationism. We will here take a particular criticism from Laudan, who discusses the demarcation criteria, for reason that will be clearer ahead.

In *The demise of the demarcation problem* (Laudan, 1983) drives heavy charges against Popper falsificationist criteria, as well as to the Received View “partial” verificationist criteria. At first Laudan observes that by Popper’s criteria in reality almost anything goes:

> Thus flat Earthers, biblical creationists, proponents of laetrile or orgone boxes, Uri Geller devotees, Bermuda Triangulators, circle squarers, Lysenkoists, charioteers of the gods, perpetuum mobile builders, Big Foot searchers (… very long list…) turns out to be scientific on Popper’s criterion – just as long as they are prepared to indicate some observations, however improbable, which (if came to pass) would cause them to change their minds. [p. 121]

But this mocking criticism is only to deny syntactic and semantic criteria altogether – as opposed to epistemic criteria, syntactic-semantic ones does not consider *belief-worthiness*.

> The central concern of the older tradition had been to identify those ideas or theories which were worthy of belief. To judge a statement to be scientific was to make a retrospective judgement about how that statement had stood up to empirical scrutiny. With the positivists and Popper, however, this retrospective element drops out altogether. Scientific status, for them, is not a matter of evidential support of belief-worthiness, for all sorts of ill-founded claims are testable and thus scientific on the new view. [pp. 121-122]

Laudan go on pointing the contradiction of the falsifying criteria by stating that ill-founded “theories” had been tested, failed, and because of that their practices cannot be rejected as scientific. As the title says, Laudan concludes by the demise of the demarcation criteria, and for that he is criticized by Pigliucci (Boudry & Pigliucci, 2018; Pigliucci, 2013), who says that if we cannot tell science from pseudoscience then Philosophy of Science is doomed, and then we need the demarcation criteria.

Notwithstanding Laudan’s demise conclusion, his important paper has a point. From the History of Philosophy of Science perspective, the epistemic view of Science may – even has to – be more important than syntactic or semantic criteria. However much I praise Logic, maybe axiomatization will not solve all our problems, or, at least, it should be subordinated to more substantial evaluation.

References

Aristotle. (I-XV). *Categories*.

Bacon, F. (1984). *Novum organum ou verdadeiras indicações acerca da interpretação da natureza; Nova Atlântida*.

Beiser, F. C. (2015). Neo-Kantianism. In M. N. G. Forster, Kristin (Ed.), *The Oxford Handbook of German Philosophy in the Nineteenth century*. Oxford: OUP.

Boudry, M., & Pigliucci, M. (2018). *Science Unlimited?: The Challenges of Scientism*: University of Chicago Press.

Carnap, R. (1966). Theory and Observation. In T. A.-K. McGrew, M.; Allhoff, F. (Ed.), *Philosophy of Science - an historical anthology* (pp. 329-343). London: Wiley-Blackwell.

Descartes, R. (1641). *René Descartes: Meditations on first philosophy: With selections from the objections and replies*: Cambridge University Press, 2013.

DeWitt, R. (2018). *Worldviews: An introduction to the history and philosophy of science*: John Wiley & Sons.

Heis, J. (2018). Neo-Kantianism. In E. N. Z. (ed.) (Ed.), *The Stanford Encyclopedia of Philosophy*

Hume, D. (1739). A treatise of human nature. In: New York, NY, Barnes & Noble, 2005.

Kant, I. (1787). *critique of pure reason* (N. K. Smith, Trans.): Read Books Ltd, 2011.

Lancaster, J. A. (2019). Daniel McKaughan and Holly VandeWall, eds. The History and Philosophy of Science: A Reader. London: Bloomsbury Academic, 2018. Pp. xxiii+ 1073. $49.95.

Laudan, L. (1983). The demise of the demarcation problem. In *Physics, philosophy and psychoanalysis* (pp. 111-127): Springer.

Matthews, M. R. (2014). *Science teaching: The contribution of history and philosophy of science*: Routledge.

McMullin, E. (1970). The history and philosophy of science: A taxonomy.

Passmore, J. (1966). *A hundred years of philosophy*. London: Duckworth & Co Ltd

Pigliucci, M. (2013). The demarcation problem. A (belated) response to Laudan. *Philosophy of pseudoscience: Reconsidering the demarcation problem, 9*.

Plato. (142a–210d). *Theaetetus* (Vol. 142a–210d).

Popper, K. (2012). *Objective knowledge: an evolutionary approach*: Oxford: Clarendon Press.–1979.–395 p.

Porta, M. A. G. (2005). Zurück zu Kant–Adolf Trendelenburg, la superación del idealismo y los orígenes de la filosofía contemporánea. *Doispontos, Curitiba, São Carlos, 2*(2), 35-59.

Quine, W. V. (1951). Main trends in recent philosophy: Two dogmas of empiricism. *The philosophical review*, 20-43.

Suppe, F. (1977). *The search for philosophic understanding of scientific theories* (Vol. 2).