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HÉLÈNE METZGER ON PRECURSORS: A HISTORIAN AND PHILOSOPHER OF
SCIENCE CONFRONTS HER EVIL DEMON

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The historian and philosopher of science H el ene Metzger, n ee Bruhl (1889-1944) delivered “Le r ole des pr ecurseurs dans l’ evolution de la science” in 1939 as a lecture of the Institut d’Histoire des Sciences et Techniques of the University of Paris, and it was published in their journal *Thal es* (Metzger 1937-39, repr. in Metzger 1987). In this talk, Metzger not only attacks the notion of precursor and a history of science focused on ‘great men’ and their discoveries, but also makes a strong case for the philosophical value of the history of science. We here offer a translation of this witty and subtle essay in the historiography of science, followed by our commentary.

H el ene Metzger: The role of precursors in the evolution of science

Today I would like to ask you to consider the question brutally posed to me one day by a diabolical jester who, unknown to me, had managed to conceal himself inside me. He did so just when I was doing my best to fit into some elegant scheme the evolution of a scientific problem that, before receiving a solution that satisfies us today, had exercised the sagacity of very many investigators. “To what extent”, this unexpected interrogator then asked me, “do I have the right to assert that the numerous precursors of great men, with whom historians like you like to entertain your readers at length, are created by those historians, not indeed *ex nihilo*, but rather thanks to the discoveries for which humanity is indebted to those great men?”. What was I going to reply to this evil demon, who, evidently delighted to have troubled my professional conscience by surprise, suddenly withdrew, refusing all discussion in order to better enjoy my confusion? I can’t tell you. But since this experience I have tried to render the work of the historian safe from the grave objections amusingly concealed beneath this evident paradox.

It goes without saying that I did not grasp at the time the significance of what a number of you would perhaps consider an amusing witticism... Well then! I wanted in the first place to set my mind at rest. We haven't actually invented the texts of the bygone authors that we have read closely before commenting on them to make them comprehensible to our contemporaries. The passages quoted in our writings are truly to be found in the works once bought in second-hand bookshops or kept in libraries; the ideas that we attribute to our distant predecessors were no doubt the very same that they expressed in such and such a chapter, on such and such a page and at such and such a line in their most learned transactions. Doubtless there are possible challenges. But a critical study allows us on occasion to correct the place and date of appearance of a work. A critical study permits us also on occasion to rectify the name of the author of the work; for example, source criticism claims to have established that the majority of the alchemical writings that flooded Europe from the 16th to the 18th century, and which had been attributed to the Arabs or to the monks, are nothing but fakes brought out by book dealers happy to make great profits. But let us not dwell on mystifications, possible errors, reckless attributions, systematically far-fetched interpretations. Historians would readily unmask the authors of fictionalized biographies as liars and forgers. If one of them writes fiction, they reproach him, they denounce him as a criminal. On the other hand, they know that they themselves can be mistaken; they are very tough on their own writings. They think, given that error is human, it is simply diabolical, as common sense declares, to persist in error.

Now it is certain that the evil demon who came one day to mock the history of the sciences as its most respected practitioners understand and write it, did not insult us by considering us as maleficent devils. He did not for a moment dream that for unknown motives we nourish the dark purpose of systematically deceiving our far too credulous readers. He has not accused us of deceptive invention and he does not suspect us of invention *ex nihilo*. He did

not want to drag us before some court of assizes charged as public malefactors. In inviting us, on the one hand, to specify our methods, on the other hand, to examine what is the significance of our endeavor, he addressed himself solely to our clear judgement. He has taken on the work of the most conscientious among us. He has likewise chosen evident historical facts granted by all; he has not disputed them. Then he has said to us, for example: “By what right do you claim that Jean Rey, who a century before Lavoisier asserted that air enters as an ingredient into calcinated metals, is a precursor of Lavoisier, who proved experimentally that oxides are due to the combination of oxygen and a metal?¹ By what right do you claim that Van Helmont and Robert Boyle, who attached great importance to the weight of the reagent before and after a chemical reaction, are likewise Lavoisier’s precursors?² By what right do you claim that all the persons who, before Lavoisier, had said

¹ Even in the popular *Chemistry*, Metzger emphasises that, although Jean Rey’s hypothesis of the reason why tin and lead increase in weight after burning resembles Lavoisier’s, there is no connection between the two, or, in her terminology, there is no “filiation of doctrines” (see our commentary). Rey’s interpretation was not taken up by his contemporaries, or by later scholars (Metzger 1991/1930, 28-29). See below in this text for her account of the lack of connections between Rey’s and Lavoisier’s ideas.

² Metzger severs the links among works traditionally seen as stages of a unitary scientific development by arguing that they were answers to different questions. She points out that, unlike Rey, Boyle did not aim to answer the question of why the weight of lead and tin increase after combustion. She further discusses this point in (Metzger 2019/1935).

that the element or simple body is so named because it withstands all experimental attempts at decomposition are precursors of the said Lavoisier?"

"Likewise, tell me a little about why the bulk of the dreamers, of the experimenters, of the philosophers and of the astronomers who have accepted either an attraction, or a repulsion, or a sympathy, or an antipathy between similar things or contrary ones, as well as all the partisans of magnetism, whoever they may be, are often and indiscriminately considered as precursors of Newton?"

"By what right do many among you consider Leucippus, Democritus, Epicurus, Lucretius and all the atomists of antiquity as precursors of Dalton, who at the beginning of the 19th century revived the atomic theory in chemistry?"

"By what right do you claim as precursors of Pasteur and microbiology certain Ottoman Turkish doctors, as well as Father Kircher, Hartsoecker, Filhaud de Chimbaud and many others who have at one time attributed contagious disease to the invasion of our bodies by troops of bugs proliferating most rapidly and by their smallness escaping all our means of investigation? By what right do you claim that Redi, who to combat the theory of spontaneous generation demonstrated experimentally that the larvae of flies never appear on rotting meat if one has taken care to prevent pregnant flies from laying their eggs there, is a precursor of the said Pasteur?"

We won't pile up examples. I think I've said enough for the doubt voiced by my unexpected visitor to draw your attention as it did mine. After all, he could have added, ["]you relate the admirable thoughts and lost discoveries of precursors of the great men; you show how they approached the truth of today (we set aside that of tomorrow) that they nevertheless did not attain; you concede to them a portion, a small portion, of the glory of those who later reached the goal ... But really, did the precursors have some place in the work of their more fortunate successors, or did they not? Did they inspire them directly or even

indirectly? Can you establish a line of descent [*filiation*] between the ideas of the ones and the others?³ Did the theories of Jean Rey, of Van Helmont, of Robert Boyle, in some way give rise to those of Lavoisier? Did the old theories that admitted sympathies, antipathies, attractions and repulsions between similar and contrary things, through the mere process of their development give rise to Newtonian physics?⁴ The ancient atomistic metaphysics, did they contain potentially the remarkable discoveries of Dalton? The numerous speculations, including the experimental studies of Father Kircher, of Hartsoecker and many others on the etiology of contagious diseases, did they, in inserting themselves, if one dare express oneself thus, into scientific practice, bring about the hatching of the works of Pasteur?[""]

We could discuss at length each of the cases cited, for they are very different from one another. But we don't want to dwell on this. If we wanted to reply to all the questions of detail, we probably would never finish, not because we lack documentation, but because all our effort would be taken up by the search for that documentation. This is a matter of philosophy, of the philosophy of history, not of history. Let's concede to the evil demon that if he could establish that just one of the above-mentioned predecessors was not a source of

³ On Metzger's views on "filiation" of ideas, see our commentary.

⁴ Metzger regards Newton's idea that masses attract one another without physical contact as a return of the idea, dominant in the Renaissance, and present in many historical periods, that similar things are in a relationship of sympathy. She calls this relationship 'active analogy', which is the action of similar on similar (Metzger 1938). However, she maintains that Newton did not borrow this idea from specific scientific texts; for her it was at his disposal because it is always present in the human mind, and thus may resurface in the study of nature at different times.

inspiration, if he could show that some of them have been plucked from the dust of libraries by scholars having fun who want at all costs to find precursors of great men, then we would have to respond to his doubt. Now, what would be the glory of Jean Rey, unknown in his time (except by Father Mersenne, who knew everybody) and unknown in the 18th century, if the resounding discoveries of Lavoisier had not drawn attention to his works?⁵ We shall return in due course to the discussion of this case. Well then, let us ask ourselves, what do we mean by “precursor”? Etymologically this doesn’t pose any difficulty: “precursor” signifies literally a forerunner. If, before looking into explanatory commentaries, you consult the high authority of bulky and respectable dictionaries, you find that a precursor is “one who comes before another to announce their arrival” or, if you prefer it, “man who precedes another and announces him”.⁶ The precursor would be, so to speak, a prophet who, while bringing to humanity an embryo or, if you like, a most obscure draft of the message, would say “that a day will come” (one day soon, let’s hope) when a more fortunate successor, a genuinely great man, will indeed read with full clarity the announced truth that remained unknown to men.

Let us ask ourselves, then, how could one know that a particular man who wrote a particular thing was in fact a precursor of a particular great scientist [*savant*], who in clarifying it, rendering it more precise and demonstrating it, added a little more harmony to our vision of the world and enabled us to penetrate some of the secrets that nature seems to have made such efforts to conceal from us? Before tackling such a problem we should

⁵ Note that here Metzger draws a distinction between factual historical links based on evidence of reception, and links established with hindsight based only on comparison. Scientists were not aware of Rey’s work, so they could not have borrowed his ideas.

⁶ Cf. Littré 1874, vol 3, 1273.

distinguish it carefully from irritating questions concerning searches for priorities of discovery in the sciences, which in raising so many passions give rise to so many sterile quarrels ... Here are some examples. –Niepce and Daguerre disputed each other's stakes and glory in the invention of photography. The French government granted each a pension large enough to allow them to have a living while renouncing any patent and handing over to the public domain the benefit of their long researches. This did not prevent the partisans of one or the other from writing works crammed with documents interpreted in favor of one or the other hero. Here is a dispute about established historical facts that one could find in certain respects interesting, but which evidently has no philosophical significance. –Scheele, Bayen, Lavoisier, Priestley and others isolated the gas oxygen almost simultaneously. Who deserves, one can ask, priority in the discovery? [It's] a question of fact that historians could try to resolve if they so wished, but which does not in any way pose the problem of precursors that we are examining together. Doubtless one could have said that the discoveries made at one and the same time by several scientists were in the air, that they were in some way prepared by the state of the sciences in the epoch in question. Of course, on this topic there have been written the most suggestive pages, whose reading I recommend to you and which are of the greatest interest. However they do not interest us at the moment; we move on. – The partisans of Higgins have contended with the partisans of Dalton in showing that their unrecognized great man had some years before the officially recognized great man written excellent works containing similar hypotheses on the atomic theory in chemistry. –We cite also the famous quarrel that brought into conflict the partisans of Newton and the partisans of Leibniz, each wanting to attribute exclusively to their master the creation of infinitesimal calculus, which as we know had such immediate fruitfulness. In the last two cases cited the question of precursors evidently does not come into play; we could utter, or could have uttered, the words “plagiarism”, “copying”, “larceny”, “theft”. So it is well established that the problem of

precursors is a matter quite other than the incessantly resurgent dispute that rouses time and again passionate inquiry into priorities of discovery, inquiry that does not go beyond the domain of miscellaneous facts.

In order to clear the ground, we entirely separate the problem of precursors that we are examining today from the quite similar problem of the historical lines of descent of theories, which is not theoretically related to it and which we shall set aside... When, for example, I read that long before Michel Servet or Harvey certain monks or certain doctors expressed in passing some hypothesis having some resemblance to our knowledge of the circulation of the blood, I am not in any way being told that Michel Servet or Harvey knew the work of their more or less distant predecessors. Quite the contrary: most of the authors who note with astonishment the similarity of opinions bitterly regret that the work of the predecessors has remained for so long a dead letter. If, they say, one had listened to these long-unknown masters, science would today be much more advanced than it is... Or again, the authors in question seek intellectual, moral, technical or sociological reasons that prevented the said anticipations from developing and which condemned the precursors, admirable though we suppose them, to remain nothing but precursors. The idea produced by a thinker or scientist, one adds sadly, like the seed thrown on the earth by a sower, cannot germinate and develop unless it falls in a setting favorable to its blossoming.

Let us set aside these well-known issues to ask, lastly, by what unequivocal mark can we recognize that a certain theory, a certain experiment, a certain book or a certain article is the work of a genuine precursor? To view the matter in full light, let us abandon for a moment our texts and our library to consider the present. Would we see many people proclaiming themselves as precursors? The scientists confident of their theory will certainly not do so, and if they are very proud of their discoveries, they will consider themselves as incomparable masters. Doubtless some passionate inquirers who have intuitions that they believe to be

masterly, but that they cannot clearly spell out, will nevertheless wish to make their voice heard. They will advise their colleagues to work on their ideas and will entreat them to accomplish the work that they themselves, despite their inspiration, have not been able to bring to a successful conclusion. Doubtless, however, the majority of the inquirers will remain sadly unappreciated by their contemporaries and will make appeal to the equitable judgement of a distant and more enlightened posterity... Now what might arise in consequence? If posterity orients its work quite otherwise than the inquirer who counted on its favorable verdict, his works will remain unproductive, unknown... But if posterity suddenly follows the route that he had wanted it to take and on which he had stumbled, if at that moment the curious scholar exhumes from the hotchpotch of the libraries his so far unnoticed papers, then our inquirer will become a precursor, an unrecognized genius, a great man whose work the culpable indifference of mediocre men has prevented from developing for the greater good of humanity... We do not here have to shed tears over his unhappy case. We conclude from this account, which so as to be more amusing and clear has taken the form of a symbolic story, that the precursor, whether or not conscious of the value of his message, cannot be recognized as such until after the arrival, expected or unexpected (it matters little), of the scientist who will have provided an improved new edition of the message in a form that can be grasped and used by the intelligence of other men. The contribution of the precursor to scientific knowledge, which always represents the common treasure of humanity (the riches of which are accumulated and elaborated by the successive generations of scientists), can be appreciated, enhanced and gain its historical significance only after the event and, if one dare express oneself thus, "when the time is ripe". The prescience of the precursor will not be known as such except by those who come later and who possess the knowledge of which it was a sketch. The prescience of the precursor will not become accessible until it is illuminated by the blinding light that the work of the authentic scientist

will emit, thanks to which it can be interpreted.⁷ The forerunner will have the right of belated recognition by men only when the runner they announce has reached the finishing post. Then, but only then, will the historians be able to despoil the archives to find in the hotch potch of innumerable documents the writings of those who will henceforth fulfil the mission of precursor and whom they will, of course, zealously highlight, glorify and admire. We cannot refuse to concede to the evil demon who has come to mock us that the precursors are extracted from a mine full of diverse samples, and that this mine is so rich that we cannot fail to find them, if we search carefully. But the evil demon requires us to admit that the precursors are created by the historians. Not for sure, he said, *ex nihilo*, for the matter from which the precursors are fabricated is provided by the documents, but by shaping this matter with the help of the works by the great men whom the precursors heralded, which therefore will therefore play the role of a tool.

In other words the evil demon accuses us of having unknowingly modernized the ideas provided by the precursor. He asks us to examine the works that seem to sketch those that have later merited glory, to see whether we have sought them out and interpreted them in a sense favorable to our claim. Having reached this point in the debate we cannot pursue our investigation in good faith without providing precise examples. Here is one that I have already mentioned.

Why was Jean Rey so often considered as a precursor of Lavoisier, as a Lavoisier come too soon for his frightfully backward century to have been able to listen and profit from his

⁷ Metzger here mocks the historians who use the concept of precursor. By contrast, she vows to avoid “with the utmost care” interpretation of past theories in the ‘blinding light of our current theories’ (Metzger 1974/1930, 6).

hypothesis? Didn't Jean Rey assert in 1630 that if the weight of tin and lead increased when calcinated, it was because the atmospheric air, thickened air of course, had become incorporated into their substance?

The plausible but unverified hypothesis that he proposed had at the time no success. Why? It is simply that it was not verifiable, as no one, no more Jean Rey than any other, took the steps to show experimentally that no metal can oxidize in the absence of air, that at the time of the reduction of the calcinated metals to metal, the air absorbed by the calcination leaves the metal to form a new compound with the reducing agent (to give an idea, let's say carbon). The chemistry of gases was still non-existent, no one having thought that one could collect them in a vessel or any other receptacle before studying their properties. We note also that Jean Rey's experiment was not at all new, that he did not seek any means of improving it, and that he himself cites the hypotheses formed by others to account for a provocative and amusing enigma offered by nature to the sagacity of chemists. In accord with his method of working, his hypothesis, like those of his rivals, remains in the list of endlessly debatable possibilities.

Here are some of the reasons that one may readily notice; but there are other more important ones that one does not understand unless one knows well the chemical theories of the middle of the 17th century:

1. The author still accepts the then entirely out-of-date theory of the four elements of Empedocles or Aristotle. Moreover, to establish his system he believes himself obliged to mock the world of Copernicus, something that could only displease.
2. Since Van Helmont, chemists had considered the air as a single body occupying space, but incapable of combining with other elements and almost totally devoid of weight. This air, a simple body that had some resemblance to the ether of modern physicists, did not form the entirety of the atmospheric air, full of foreign bodies and a

veritable repository of impurities. It is these accidental impurities that give some weight to the atmospheric air. This is precisely why, when in 1677 Duclos observed the increase in weight of metal calcinated by a burning mirror, to explain this fact he conjectured that the air that circulates the blazing materials under examination allows sulphur particles, previously swept along in its mass, to penetrate them. To further justify our view, we note that when Van Helmont invented the concept of gas, he designated by this word the class of air-like fluids that take the form of air without being air. It was not until the epoch of Lavoisier that air was considered as a mixture of gases, a thing that Rey could never have suspected and which no logic dictates.

3. It is unwarranted to ascribe to Jean Rey, as do certain historians, the honour of having been the first to recognize the conservation of mass; for this conservation is a truth of common sense, recognized generally and without reflection from the remotest antiquity. It is true that some people have on occasion doubted its universal application: the vitalists who believed that the soul is the principle of lightness, given that dead bodies fall to the ground; and astronomers speculating on the causes of weight. It is true that these doubts have sometimes cropped up in writings devoted to chemistry; but we should add that they never had genuine influence on the theory or practice of this science. Besides, neither Van Helmont, nor Boyle, nor Lavoisier himself, all of whom had the opportunity to affirm and make use of the constancy of weight of matter, would in their lifetime have drawn any glory from an assertion that their contemporaries, like them, saw as obvious... We add that Lavoisier made use of what today is considered as the first law of chemistry in weighing solid, liquid or gaseous reagents before and after the reaction, to make sure that in reality no substance had been removed or added in the course of the laboratory operation.

4. Here, lastly, is the most important [reason]. In Jean Rey's times, the concept of combustion was not in any way defined. No one compared the combustion of wood, of charcoal, or of sulfur, to the calcination of metal. Consequently, his hypothesis, that one could not have dreamed of generalizing, explained only a particular case, without opening any new way for the theory or practice of the science.

It is true that when in 1777 Lavoisier published the result of his work on the metallic oxides, a scholar named Gobet amused himself by providing a new edition of Jean Rey's essay, unknown to everyone including, obviously, Lavoisier.⁸ It is true that, with the admiring approval of Lavoisier, Jean Rey was then consecrated as precursor genius... But the evil demon will say: ["In the light of the examination that you have just carried out, can you see in the work of Jean Rey a preformation of the work of Lavoisier? Does Rey truly deliver the same message as Lavoisier? You must grant that Rey's hypothesis derives its prophetic and scientific value from the interpretation of it that the work of Lavoisier provides, that the glory of Rey is simply usurped and is in fact but a reflection of that of Lavoisier himself.["]

The example that we have just examined was cunningly chosen by the evil demon to guarantee himself victory. We recognize this victory without a reaction of hurt or vexed pride and without a reaction of idle and, so to speak, shamed humility. Are we going to conclude with our enemy that the historian of the sciences has suffered a defeat, an irremediable defeat? Are we going to say with Descartes and Malebranche that the scholarship that we have endeavored to acquire is incapable of helping us ever to reconstitute that past, that it merely satisfies the harmless and sterile curiosity of an elderly scholar, occupying his leisure

⁸ Rey 1777.

as best he can?⁹ In abandoning ourselves to despair are we going to capitulate entirely by abandoning history and, like Candide disillusioned with his earlier ambitions, are we going to devote ourselves to the cultivation of our gardens?¹⁰ Before resigning ourselves to making such a gloomy resolution, we recall first and foremost that wisdom consists above all in knowing how to benefit from our setbacks, even to transform them into victory. So how shall we manage to make the most of the lesson that we have been taught so severely?

Of course, we readily recognize that research into the picturesque, the anecdote, the similarity between a perfectly constituted theory accepted by everyone today and a theory of the past expressed by an isolated scientist, can be amusing and curious, but that it is of no value either for the study of the development of science or for epistemology.¹¹ If the historian

⁹ In his commentary on Rule 4 of his *Regulae ad directionem ingenii* (written 1626-8, first published the original Latin, Amsterdam, Artur Buchenau, 1701), Descartes denounces scholars prey to “blind curiosity” (Descartes 1984, 15). In *De la recherche de la vérité*, Malebranche denounces the “foolish curiosity” of scholars fascinated by rare, ancient and obscure things (Malebranche 1886/1674-75, Ch. 3).

¹⁰ In Voltaire’s satirical *Candide* (1759), Candide, after he and his companions endure horrific sufferings, concludes that the secret of happiness is to forgo their former ambitions and to “take care of [their] garden” (Voltaire 2006, 276).

¹¹ Metzger packs into this sentence several of her historiographical and epistemological views. Among these is her conviction that the study of “perfectly constituted” theories is of little use to the historian or the epistemologist. By contrast, for her it is the study of nascent science and of the tortuous processes of inquiry and discovery that reveals the historical links among ideas and practitioners. Moreover, this study will provide the material that the

does not have a sound knowledge of the mentality of the periods that he studies, and if he is content to note cursorily some striking resemblances of whose significance he is unaware, he condemns himself to be unaware of the historical lines of descent of theories, which we cannot consider today.

Shall we conclude then that the historian has wasted his time when he has put together a methodical inventory of recurrent inspirations constantly similar through the most diverse vicissitudes of history and humanity? You have doubtless observed that the evil demon was obliged to recognize that the hypothesis provided by Jean Rey was perfectly plausible, and he could even have insinuated that if one looked closely one could find precursors to this precursor... What would he have said if one had enumerated to him the list of doctors and hygienists who attributed contagious diseases to the development of parasites living inside our bodies, giving us good advice to protect us from contagion? Doubtless to enhance his victory he would be pleased to observe that such a supposition, which springs up spontaneously in us and recurs sporadically, is, so to speak, outside of the history that sees it arise from time to time.... Moreover, as I recognize, in speaking thus he would be entirely reasonable... But this time the historian will not be at all defeated. For without his research, who would have suspected that a supposition made by Pasteur and verified by the said Pasteur in the middle of the 19th century results from a certain way of considering reality?

epistemologist needs to understand the mechanisms of the formation of knowledge. She does on occasion consider isolated scientists in her work, but mainly as exemplifying ways of thinking that belong either to humankind or to a certain epoch. However, for her it is crucial to establish the connections between scholars. An isolated scholar could not have contributed to the development of science.

Likewise, with the historian of the sciences seeing atomic hypotheses constantly recurring, will he suspect, with Hannequin, that these hypotheses result inevitably from the structure of the human mind?¹² Is it not thanks to this historian that we know that there are fundamental inspirations that are free as such from the history of the descent of theories and that one sees appear anew in certain epochs?

After this little success the historian, sure at least of being listened to, will do his utmost in good faith to show his detractor that without his research one would be unaware that certain very general schemes recur as a guiding thread of scientific theories, [schemes] of most unequal significance and, besides, with some applying to the whole world considered in its entirety, others to a part of the world artificially defined... Duhem has already insisted on the similarity of inspiration of the ancient theory of natural place and the cosmology that certain physicists have wanted to derive from thermodynamics¹³... For ancient science the elements, earth, water, air and fire, are each assigned to a place in the sublunar world. They may be moved from this place by a violent motion, but their natural motion tends to return to it. The world, as the symbolic experiment of the flask of the four elements shows, has a tendency towards equilibrium.¹⁴ The philosophy that forms itself by analogy with thermodynamics and,

¹² See Hannequin 1899, 2-13, where it is argued that “The hypothesis of atoms is a necessary hypothesis, which derives from the very constitution of our knowledge”.

¹³ See, for example, Duhem, “Physique de croyant”, an essay first published in 1905, and appended to the second edition of Duhem 1981/1914, 462-472, Engl. tr. Duhem 1991/1914, 305-311.

¹⁴ In this experiment, described in the entry “Ovide” in Voltaire’s *Philosophical Dictionary*, particles of metal are mixed up with three liquids of different densities by shaking in a flask,

above all, with the principle of Carnot, affirms that the universe constantly approaches a state of stable equilibrium; and that it is the state of being out of balance that is the efficient cause of the approach towards equilibrium that can be considered as the final cause.¹⁵ We could show that the alchemists who maintained that imperfect metals tend to the ultimate perfection of gold present an analogous scheme for our consideration; moreover, they acknowledge the origin of their theory, declaring “that the imperfect metal is to the perfect metal as the child is to the man”. We find in schemes that appear the furthest from the living a biological inspiration from which science has escaped but with which it preserves certain links. I am well aware that the justification of the historian that I have attempted is too brief, that to be effective it would require me to open a weighty dossier, something that I cannot talk about today.

But I cannot end the account of the meditation inspired in me by the evil demon come to mock the historians without showing you that, while it obliges us to renounce facile methods, it does not force us to renounce work altogether. What I want to establish is, first and foremost, that the search for the predecessor should concern itself more with the general explanatory scheme than with the things explained, things that can be absolutely anything with respect to that scheme. Anyone who has delved into the history of chemistry knows that the scientist who devotes himself to this science and studies qualities of different substances without interesting himself in any sort of object offered to the science by common sense (for

then with the flask at rest seen to return to their original stacking in order of density; this being offered as a model for what would happen to the four elements following a shattering of the entire universe into least particles (Voltaire 1789, tome VII, 49-50).

¹⁵ Cf. Duhem 1991/1914, 287-8.

example, an animal, a cat, let's say Kiki or Pompon) can carve up the world as he pleases. That is why the concept of sulfur – a body that is yellow, solid, combustible, etc., not being tied to any shape, for one speaks of sulfur in general or just of sulfur, but one cannot speak of a sulfur, but rather of a piece of sulfur – is transformed into the principle of combustibility, and why the ancient chemists lamented on occasion that they did not know what they had in mind when thinking of sulfur.¹⁶ For the imagination to recover, as is pleasing to it, an individualistic image, it has to ascend to the inaccessible atom of sulfur. That is why in judging chemistry from experience one can say with Chevreul that for it the individual is identical with the species.¹⁷

I don't want to dwell on this. I know that certain philosophers devoted to metaphysics have discovered by dialectic that which manifold reflections on the evolution of science can reveal to the historian. I know that those metaphysicians who seek to fathom what Monsieur Lalande calls *constituent reason* [*la raison constituante*] can say, on the one hand, that they do not need the historian in order to take cognizance of the truths of which the historian's precise conclusions are only illustrations, and that they can, on the other hand, reproach the historian for deceiving himself about the significance of his method, if he seeks to explore a

¹⁶ Here Metzger is referring to the historical concept of 'principle' in chemistry. Principles were conceived as active bearers of properties that can be impressed on more passive substances. For her discussion of the principle of combustibility in particular, see for instance Metzger 1926, 54ff; and Metzger 1969/1923, 110ff, in the context of Stahl and his school's 'ambiguous' attitude vis-à-vis the three Paracelsian principles of salt, sulfur and mercury.

¹⁷ See Chevreul 1824, 34-5, on atoms and their combinations as the individuals that represent chemical species; and Chevreul 1866, 84-86.

terrain to which he has no means of gaining access. I recognize at once that the historian does not have the right to go beyond *constituted reason* [*la raison constituée*]¹⁸. But I add that the general culture offered by the study of the different aspects taken by science across the ages is for this more modest task extremely instructive. With regard to this task, the examination of schemes common to certain past theories that remained practically sterile and to certain more modern theories whose fertility is recognized offers us an incomparable tool of investigation. Past science can sometimes help us in understanding the science of today... But I cannot carry the discussion with the evil demon onto this terrain nor convey to you today the result of my inquiry. I will ask you then, above all, to cast new light on the problem examined at the start of the talk. At the same time, I shall take pleasure in what the metaphysician, even if he was an evil demon, recognizes: that the manner of philosophizing of the historian of the sciences, even if it does not allow one to reach the primary questions, is nonetheless most estimable.

Commentary

Metzger's talk in context

When Metzger delivered “The role of precursors in the evolution of science” at the Institut d’histoire des sciences, she had already been lecturing there for seven years, since its

¹⁸ For Lalande’s view of two types of reason, see Lalande 1925; Lalande 1967, 39-76; cf. Lalande 1999/1926, vol 2, 882ff.

foundation in 1932.¹⁹ The previous academic year she had also stood in for Alexandre Koyré at École Pratique des Hautes Études, and had promptly turned her cycle of lectures into *Attraction universelle et religion naturelle chez quelques commentateurs anglais de Newton* (Metzger 1938). These teaching engagements, however, never afforded her a much-desired stable academic position. There is no doubt that her gender was a major obstacle. She did not comment on the extreme rarity of female professors in French universities, but she was well aware that the educational pathway that her father, a jewel merchant, had chosen for her as a daughter (very different choices were made for her half-brother) did not lead to an academic position.²⁰ She attended a *collège* rather than a *lycée*, followed by a *diplôme d'étude supérieures* in crystallography. Her interest in history was independent of her formal education: she wrote *La genèse de la science des cristaux* after her degree, during the First World War. This manuscript, however, gained her a *doctorat d'Université*, which, unlike the *doctorat d'État*, did not normally lead to an academic post, and was published at the end of the war (Metzger 1918). Her monographs in the history of chemistry followed: *Les doctrines chimiques en France du début du XVII^e à la fin du XVIII^e siècle* (winner of the Binoux Prize of the Académie des sciences morales et politiques) (Metzger, 1969/1923), and *Newton, Stahl, Boerhaave et la doctrine chimique* (Metzger, 1974/1930), separated by the

¹⁹ Some of her lectures were published as *La philosophie de la matière chez Lavoisier* (Metzger 1935)

²⁰ Metzger analyses her father's plans for her and her sister as expression of widely-held attitudes in their social milieu in a letter to George Sarton, published in Freudenthal (ed) 1990: 254-55

philosophical book *Les concepts scientifiques* (winner of the Bordin prize the Académie des sciences morales et politiques) (Metzger 1926).

Like “The role of precursors in the evolution of science”, her other published talks are on the historiography of the sciences. They are informal in presentation, lacking references and bibliography.²¹ In these talks, the tone is particularly lively and polemical. Indeed, her mentee and friend Suzanne Delorme remarked that at historians of science’ meetings, Metzger “enlivened the discussions with her subtle and often ironic remarks, which were always pertinent and erudite, if somewhat disconcerting in their impulsiveness” (Delorme 1947-48, 342).

Her tone could have been partly due to her awareness that her audience was generally in disagreement with her views of the methods and aims of the history of the sciences. The Institut d’histoire des sciences was the creation of Abel Rey (1873-1940), who had a positivist approach to history. For him, the historical method is the same as that of the positive sciences, and philosophy is a general reflection on the sciences. Metzger delivered other talks at the Centre de synthèse, an independent research institution founded and directed by Henri Berr (1863-1954), where she was very active as secretary of the Unit for the history of science. Berr, like the other historians at the Centre, also conceived of history as a positive science, focused on gathering of facts and sources, and on precise dating of events. Metzger’s “philosophical method in the history of the sciences”, as the title of one of her talks at the Centre translates, was received with suspicion, whereas the ‘*a priori*’ that the historians bring to their work, which she discussed in another talk (Metzger 2019 [1936]), just mystified

²¹ See the talks in the section “De la méthode en histoire des sciences” in Metzger, 1987, and Chimisso’s translations of four of them appended to Chimisso 2019.

her audience. Her complex concept of ‘*a priori*’, discussed later in this commentary, when applied to the historian mainly indicates the historian’s philosophical assumptions, worldview and conceptual universe.²² Her interest in exploring the mind of the historian and above all the minds of the scientists whom she studied, had solid bases in the informal philosophical education that she gained by attending Léon Brunschvicg’s lectures at the Sorbonne and discussing her work with many eminent philosophers, including her uncle Lucien Lévy-Bruhl (1857-1939), André Lalande (1867-1963) and Émile Meyerson (1859-1933). She shared their goal of studying the mind through examining the history of science (or the cultures that ethnologists were documenting at the time, in the case of Lévy-Bruhl). Yet, she could not fully accept their use of history of science. Whereas the positivist historians regarded primary sources as transparent, and access to them as unproblematic, the philosophers for her were often guilty of bending sources to their own theories. Metzger’s original approach to the history of science placed her in a rather uncomfortable place between the historians and the philosophers. Unlike either group, her approach to her sources was interactive: she read them with conscious philosophical goals and assumptions, which in turn were susceptible to modification by her sources.²³

²² For her concept of mental *a priori* see Chimisso 2019: 50ff and *passim*.

²³ Gad Freudenthal has described her method in terms of philosophical hermeneutics (Freudenthal 1990a). For further biographical information, see Freudenthal 1990b, and Chimisso 2019, “Introduction and biographical notes”.

History of the great men versus filiation of doctrines

In the present talk, others' modes of engagement in the history of the sciences are gently mocked as are the grand claims of metaphysicians. Her audience would have recognized polemical targets beneath the playful structure of her "meditation" (as she calls it toward the end), with its clear reference to Descartes' *Meditations* and his evil demon. As for her overall strategy, it is one of repeated concessions to her evil demon, followed in turn by self-defensive remarks, both concessions and defenses being phrased with ironic modesty.²⁴ The content of Metzger's debate with her demon is complex. She invokes a wide range of motivations for the seeking out of similarities between earlier and later theories, rejecting some and setting aside others. And she concludes with a brief advertisement of the scientific and philosophical value of her own style of history of science, which detects differences as well as commonalities among sciences from different times at the level of concepts and inclinations belonging to the human mind, as opposed to the superficial level favored in the type of history of science aimed at the search for precursors of modern theories.

At the outset her demon questions her right "to assert that the numerous precursors of great men, with whom historians like you like to entertain [their] readers at length, are created by those historians, not indeed *ex nihilo*, but rather thanks to the discoveries for which humanity is indebted to those great men". She and the demon agree that historians of the sciences are not in the habit of committing fraud by inventing sources, and that the charge is rather that of "modernizing" the ideas of supposed precursors in the light of works of "the great men whom the precursors heralded". Here Metzger targets triumphal histories of scientific progress at the hands of "great men". Further dismissal of such histories is implicit in her

²⁴ On Metzger's rhetorical strategies, see Christie 1987.

concession of victory to her demon if he could show that some cases of precursion are “plucked from the dust of libraries by scholars having fun who want at all cost to find precursors of great men”; and, more assertively, when she declares that “we readily recognize that research into the picturesque, the anecdote, the similarity between a perfectly constituted theory accepted by everyone today and a theory of the past expressed by an isolated scientist, can be amusing and curious, but that it is of no value either for the study of the development of science or for epistemology”. For a more explicit account of her objections to triumphal tales of scientific progress, we may turn to the introduction to her *Newton, Stahl, Boerhaave et la doctrine chimique* of 1930:

We have avoided with the utmost care judging the value of a lapsed science in the blinding light of our current theories considered as definite attainments, durable for all eternity. Such an enterprise – though it has often been undertaken – cannot but be illusory and runs the risk, besides, of impairing our view of the development of our knowledge in two ways. First, need it be said, it resurrects that kind of triumphal epic that is not yet completely dead and that, by relying on superficially observed facts, proudly concludes that scientists only need to reject absurd and unintelligible old prejudices and to experiment without any pre-conceived idea in order to come up with good and solid chemistry...

Second, such a method, which would inevitably imprison the fundamental concepts of chemistry in immutable definitions, would form an opaque screen between the old text and present-day readers, and would prevent them from grasping well a thought that constantly eludes their insight. And it does so precisely because the significance of the commonest words, the

range of meaning, connotation and value of scientific expressions, are all susceptible to variation and do, indeed, vary little by little when the scientist, pursuing his researches on an as yet unexplored terrain, grappling with numerous difficulties, poses in a new way important questions that he is currently doing his best to resolve (Metzger 1974/1930, 6-7).

Metzger is at first similarly dismissive of “irritating questions concerning searches for priorities of discovery in the sciences, which in raising so many passions give rise to so many sterile quarrels”. She then concedes the interest of “facts” concerning priority, setting aside the issue only for its irrelevance to her dispute with her demon and its evident lack of historical significance. It should be noted that Metzger worked in milieus, notably the Centre de Synthèse, in which the task of establishing the priority of discoveries was taken very seriously indeed. The Unit for the history of science of the Centre had even a group dedicated to chronological tables, of which Metzger was a member.²⁵ She had a rather negative view of such exercises (Metzger, 1969/1923, 9). In fact, she refused to write a history of discoveries altogether, as commentators have noted in her time (Sarton 1924, 58), as well as ours (Bensaude-Vincent 1987).

By contrast with tales of progress through glorious discoveries by great men, Metzger emphasizes throughout her works her concern with opinions typical in the periods studied. For example, in *Les doctrines chimiques en France*, she states her commitment to “reconstruct the average opinion of professionals and amateurs of chemistry” (Metzger 1969/1923, 9). In a letter of 1923 to George Sarton she declares

²⁵ About the Centre de synthèse and its Unit for the History of science, see Chimisso 2008, Chapter 4.

... so as not to have emphasized the work of Great Men, seen as extremely rigorous, I would say that I have wanted first of all to reconstruct the social milieu or rather the average opinion from which the great works have broken away ...” (in Freudenthal (ed) 1990, 252).

And in the present piece we find:

If the historian does not have a sound knowledge of the mentality of the periods that he studies, and if he is content to note cursorily some striking resemblances of whose significance he is unaware, he condemns himself to be unaware of the historical lines of descent [*filiations*] of theories ...

Here, as elsewhere in her talk, Metzger sets aside the issue of “filiation” of doctrines, whilst making it clear by implication that the establishment of lines of descent is a proper task for the historian. Such “filiation” cannot be established by simple comparison, without evidence that the more recent scientist was acquainted with the previous ideas in question and made use of them. As shown in her works on the history of chemistry, use of an idea may take many forms: simple appropriation, application of it in a different domain, elaboration, emendation, critical or polemical reaction. And “filiations” can be more subtle: scientists may preserve old ideas with which they are acquainted implicitly in their own work, even if they are in fact at odds with their own explicit ideas. One such case that Metzger discusses is that of the legacy of old doctrines that for her can be found in Stahl’s work, despite his attack on those previous systems of knowledge, notably iatrochemistry (Metzger 1974/1930, 115-116; 1991/1930, 39-41). Metzger’s studies of “filiation” avoid undue focus on iconic figures, tracing rather the sources and reception of works across whole ranges of “communities” of chemists, pharmacists, medics, metallurgists and others. Hence her en passant remark in the

present piece that knowledge of “the mentality of the periods he studies” is needed for the historian to trace lines of “filiation”.

In the course of her talk Metzger endorses the establishment of historical facts through study of the provenance and significance of sources. She indicates that only in this way can “fictionalized biography” be unmasked, issues of priority in discovery settled, and the “filiation” of doctrines established. However, as already noted, she is dismissive of mere accumulation of raw facts about resemblance between doctrines, unaccompanied by study of their provenance and significance

After her demonically inspired analysis of the weaknesses of the claim that Jean Rey anticipated Lavoisier’s account of the oxidation of metals,²⁶ Metzger refuses to admit that the historian of science has suffered “an irremediable defeat”. Despite all her earlier concessions, she insists that systematic comparison of suppositions from different times can reveal that “there are fundamental inspirations that are free as such from the history of the descent of theories and that one sees appear anew in certain epochs”. Note that she mentions “inspirations” rather than theories or even ideas. Human beings for her have at their disposal an arsenal of concepts and indeed emotions that they deploy in different ways. Similar “inspirations” can guide the most diverse theories; for instance, elsewhere she argues that “active analogy”²⁷ guided alchemists as well as Newton, but there was no historical link between alchemy and the theory of universal gravitation (Metzger 1938). This means that for her the history of the sciences can simultaneously isolate many common “inspirations”, as

²⁶ See footnotes 1 and 5 for further details about Metzger’s criticism of the widespread view that Rey was a “precursor” of Lavoisier.

²⁷ See below for the types of analogy.

she did in *Les concepts scientifiques*, and provide accounts of how a particular “tendency” produces very different theories and practices.

Science in the making and the study of mentality

For Metzger, the analysis of the finished products, established disciplines and theories is of no use if one wants to capture the metaphysics, concepts and emotions behind theories.

Rather, the historian and the philosopher should focus on the nascent phase of science, or science in the making, that is the processes of emergence of ideas and formation of disciplines.²⁸ Metzger distinguishes two components in the formation of scientific theories: the “expansive form” in which thought “forges ahead spontaneously in all directions” and the “reflective form”, in which thought harshly criticizes itself and denies all that appears incomprehensible or illogical” (Metzger 2019/1937, 210). It is resemblances and differences across time and space in the nascent phase, rather than in the finished products, that Metzger takes to be of philosophical significance. These comparisons of concepts behind theories, however, on their own do not tell us whether different theories stand in a relation of historical continuity or discontinuity, a question that requires the historical analysis of “filiations”. In this she stands very much at odds with the French historiography of science dominant in her period, in which the narratives are based on comparison of past theories, without overmuch concern with details of their genesis or with lines of “filiation”. Historians and philosophers

²⁸ On Metzger on science in the making, see Chimisso, 2019, Chapter 2. She investigated the formation of the disciplines of crystallography in Metzger 1918, and chemistry in Metzger 1969/1923 and 1974/1930. Throughout her books, she studied science in the making as the phase of invention and creation, including the process of formation of concepts (Metzger 1926, 8) and ‘thought in its nascent phase’ (Metzger 2019/1937, 203-5; 1938, 12).

who support historiographical models that include sharp discontinuities or revolutions, such as Gaston Bachelard (1884-1962) and Alexandre Koyré (1892-1964), and those who deny the presence of such discontinuities, including Pierre Duhem (1861-1916) and Émile Meyerson, base their views on comparisons that for her neglect both historical filiations and properly historical interpretations of past theories. They compare ways of thinking and logics of ancients and moderns, and judge whether they conform to the same pattern, or a different one. Metzger's review of Gaston Bachelard's *La formation de l'esprit scientifique* (Metzger 1987, 189-196) is interesting in this respect. She argues that Bachelard dismisses early-modern natural philosophy because he compares it with modern science. The "light" of modern science blinds him to the value of old theories, which would be apparent if considered in their historical setting, and alongside the judgment and use that their contemporaries and later scholars made of them (Metzger 1987, 189-196).

Crucial for Metzger in the study of nascent science, as generally in historical inquiry, is grasp of the "mentalities" of the periods studied. And, as noted above, what is required here is access not merely to the minds of great discoverers, but to the patterns of thought of whole communities. Through such study the historian can apprehend what she calls the "*a priori*". In a talk delivered in 1935 Metzger distinguishes two types of *a priori*, "in actuality", which represents "all the concepts in place prior to experience and on which experience relies" and "in potentiality", which represents the "fundamental tendencies" that produce those concepts. The former is the result of the interaction of the *a priori* in potentiality with the "experience of life" (Metzger 2019/1936). The *a priori* "in actuality" can take many forms and is never

monolithic, hence her preference for the plural form *a priori*, and more often “mental *a priori*”.²⁹

In *Les concepts scientifiques* (Metzger 1926), Metzger provides a typology of components of the *a priori* (there designated “concepts”). First there are those based on resemblance: “virtual analogies”, by which we infer from observed similarities between objects the sharing of further attributes; “formal analogies”, whereby the forms and structures of things are matched; and “active analogies”, the postulations of actions of things on things that resemble them” (Metzger 1926, 15-40). There follow those “based on permanence of substances” and those involving “different forms of evolution” (Metzger 1926, 51-77, 79-143). The “general schemes” and “recurrent inspirations” sketched in the closing remarks of her talk fall into these categories. Thus, as Metzger spells out in detail elsewhere, the idea common to Jean Rey and Lavoisier in their accounts of the calcination of metals is conservation of mass, an idea that falls into the category “permanence of substance” (Metzger 1926, 385-6). The attribution of contagious diseases to the development of parasites presumably instantiates virtual analogy, drawn between effects of visible parasites and those of postulated invisible ones. As for the cryptically presented case concerning cosmology and thermodynamics, citing Duhem and Carnot, this is spelled out in *Les concepts* in the section on the concept of parallel evolution (Metzger 1926, 107-116).

At the very end of her talk Metzger ironically defers to metaphysicians who have access to what André Lalande (her former philosophy mentor) calls constituent reason [*raison constituante*], and thus “take cognizance of the truths of which the historian’s precise conclusions are only illustrations”. By contrast, the historian is limited to constituted reason

²⁹ For Metzger’s complex concept of mental *a priori*, see Chimisso 2019, 50-56, 68-70.

[*raison constituée*]. Nevertheless, she assures us, examination of “schemes common to certain past theories ... can help us in understanding the science of today” and that the “manner of philosophizing” that they provide is “most estimable”.

In a talk of 1933 Metzger distinguishes Lalande’s two types of reason as follows: “*constituent reason*, which in its permanent and essential tendency is human reason in its entirety, and *constituted reason*, which is the aspect that this reason presents at a particular moment of human development” (Metzger, 2019 [1933], 178). Constituent reason, conceded by Metzger to metaphysicians, is human reason in its entirety. By contrast, constituted reason is the aspect that reason presents at a particular moment and setting of human development. Constituent and constituted reason correspond to the *a priori* in potentiality and the *a priori* in actuality seen above, expressions that Metzger favored in her work. Where in the 1933 talk, as in the present one, metaphysicians’ access to constituent reason is treated with gentle irony, elsewhere they are openly mocked. For example, in the above-mentioned 1935 talk, she declares:

All in all, it seems clear that the study of the history of science would eventually heal the philosopher (if the disease could be cured) of the strange habit of wanting to present definitive *a priori* or *a posteriori* concepts, on which the mind could quench its quest for certainty, and which one could call concepts of divine right (Metzger 2019/1935, 190).

And this is not an isolated outburst. In many of her works Metzger shows herself strongly opposed to all who make dogmatic philosophical claims concerning the working of the human mind. Such opposition challenges not only “armchair philosophers”, but also, by implication, such associates as Émile Meyerson, whom she gently chides in reviewing his

work for self-serving choice of historical examples to illustrate general claims about the epistemology of the sciences.³⁰

As for her passing remark on the contribution of her approach to “understanding of science today”, this is clarified in the 1935 talk. There, following her jest on how the history of science might heal the dogmatic philosopher, she spells out the way in which her study of schemes of nascent thought in past sciences can be of great benefit to scientists:

If then the history of science (or a reflection on certain episodes in the history of scientific thought, if you prefer) teaches us quickly to gather together all the possible experiments that a hypothesis could inspire; if it teaches us quickly to discover all the possible hypotheses stemming from the same experiment; if by this it succeeds in giving our mind, constantly kept alert and clear of all dogmatism as well as of useless and sterile skepticism, a little of that active plasticity that is a condition of all fruitful research, then it would render a service both to science in the making, and to the soul of the researcher, liberated from an inconspicuous and tiresome routine (Metzger 2019/1935, 190).

Throughout her works Metzger’s philosophical history is indeed, we believe, exemplary in clearing the mind of both dogmatism and sterile skepticism.

³⁰ See for instance the remarks in her reviews of Meyerson’s *Essais* (Metzger 1987, 112).

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