

Science as an Other

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IN A RECENT ESSAY ON the pedagogy of sciences in elementary and high school, Ernesto García Posada insightfully captures one of the deepest and most firmly rooted conceptions of our contemporary culture:

Taking on the risks inherent to every schematization, we shall say that the image of science that reigns without restriction in the pedagogy of sciences features at least the next problematic characteristics: *a)* dogmatism, expressed in the idea that only those hypotheses that have been proved by “reality” are part of the body of science... *b)* empiricism, which means that the theories and concepts of the sciences are taken directly from their respective objects of study, of which they are but specular reflections more or less complete according to the state of research of the respective discipline. *c)* pragmatism, in as much as it is assumed that the theories and concepts “discovered” by science may and ought be directly “applied” to reality in order to control and reproduce it at will. *d)* mechanicism, which assumes that science is a homogeneous system of certain raw materials (information or “data”) to be processed through certain fixed procedures (designated under the omnipotent denomination of “scientific method”) in order to produce scientific results sequentially and cumulatively.¹

In fact, this is the image that pervades our everyday life discourse about science. Common expressions like ‘scientific approach,’ common questions like ‘should musicology be a science?’ have the four characteristics as implicit definitions. The little prudence in the use of the word ‘science’ as a name for that definition (sometimes, at most, substituting the most specific appellative of ‘hard science’) only proves the solidity of a general consent and the confidence it conveys.

¹Ernesto García Posada, “Una Teoría Crítica de la Ciencia como Fundamento de la Didáctica de la Ciencia” (“A Critical Theory of Science as a Foundation for the Pedagogy of Science”), paper presented at the inaugural session of *PAIDEIA*, Instituto Superior de Pedagogía, Medellín (Colombia), 1997, p. 3. My translation.

Somewhat ironically, however, the solidity of the consent contrasts with the weakness of the contents. The fact that the four characteristics are “problematic” (to say the least) as a definition of ‘science’ is usually overlooked, if not simply unnoticed. Surely this is not the fault of musicologists (or any other kind of ‘humanists’): the image is not only generated from a negative ‘lack of knowledge,’ but above all from an entire, positive, cultural construction: textbooks, popular science books and videos, the basic premises of good scholarship (‘objectivity’ and the like) that give ‘science’ both its great prestige and its terrifying power to daunt ‘less rigid’ spirits—all boost this image. The figure of the scientist as “the dry, dull, diligent, pedantic, uninspired, scholarly book-worm or laboratory worker,” as Arthur Koestler put it,² is another side of the construction, also deeply rooted in popular (and not so popular) imagination.

But then it becomes all the more interesting and intriguing: a discipline that, like ours, has undergone such immense waves of skepticism about so many cultural constructions—much of which skepticism is directed precisely to objectivity and other ‘scientific’ claims (the popular image of the scientist that I just mentioned is very much what the so-called ‘New Musicology’ criticizes and satirizes about the ‘old’ musicology)—such an eclectic discipline buys the “image of science” wholesale.

THE ISSUE BECAME RELEVANT and interesting for me when trying to put in a presentable form my conjectures and speculations about some tantalizing similarities between the ‘Copernican Revolution’ and what could be called ‘Schönbergian Revolution.’ Part of the task, of course, involved an exploration of the relationship between sciences and arts. My initial idea was easily to dismiss the “image of science” and the essential gap it creates between the nature of sciences and arts, as illusory and plainly wrong. I soon realized, however, that this would have not been correct, for the impact and import of this image is much more powerful and determinant than I thought. It is from the realization that I had to tackle the problem that the following reflections were born.

The text developed originally as a response to Leonard Meyer’s “Concerning the Sciences, the Arts—AND the Humanities.”³ This article is the most systematic writing by a musicologist on

²Arthur Koestler, *The Act of Creation* (New York: MacMillan, 1964), p. 256.

³Leonard B. Meyer, “Concerning the Sciences, the Arts—AND the Humanities,” *Critical Inquiry* 1 (1974), re-

the relationship of sciences and arts that I know of. As the title shows, Meyer's purpose is to demonstrate the essential distinction not only of sciences from arts, but also of 'humanities' from both. Needless to say, I do not agree with either separation: the distinction of science and art depends on, and makes no sense without, the mentioned "image of science;" once this distinction is rejected, the separation of humanities loses of course both relevance and tenability. But the fact is that Meyer's article is an explicitation of the usually implicit "image of science," and, as such, evidences all the problems that remain hidden when the image is just assumed.

I shall therefore focus on Meyer's explicit expositions of the "image of science" and some of the consequences that it has for him, and for musicology in general. The issue of teleology and the common uses of the word 'positivism' among musicologists lead me to propose that recent trends of musicology (including, but not limited to, 'New Musicology') that claim to depart from old traditions have constructed an Other out of an exoticization, essentialization, and 'monolithization' together of 'old musicology,' 'science,' 'positivism,' and 'dogmatic naturalism' (which is a more accurate name for what the common, simplistic definition of 'sticking to the facts' holds 'positivism' to be).



WHAT I CALL MEYER'S 'explicit expositions of the 'image of science"' amounts actually to just a handful of sentences from his substantially long essay. There are many other discussions and ideas that would deserve close attention, but which would take much longer than it is sensible here. In general, it reigns in Meyer's text a confusion between the product, the producers, and the production of the activities 'art' and 'science,' that makes his arguments weak but very hard to disentangle.⁴ There is also little clarity as to what Meyer defines as 'art:' some of his arguments

published with minor modifications (essentially more footnotes) as the first chapter of Leonard B. Meyer, *The Spheres of Music: A Gathering of Essays* (Chicago and London: The University of Chicago Press, 2000). References to page numbers in this text are to the original version.

⁴For example, he differentiates the scientists (scientific *producers*), who "very rarely... read an original work by an earlier scientist," from the artists, who "insist upon seeing the original work" (pp. 168–9); but the difference loses all its relevance because of what he himself rightly points out: the scientific theory (scientific *product*) is "*propositional*,"

apply to crafts (ceramics, gardening, cooking, etc.) as well as to the Seven Arts, while other completely collapse if folk and popular music (for example) are included; it sometimes seems that the best way to describe what he considers art is ‘everything that responds to his arguments,’ which then makes the logic highly circular, and therefore poorly convincing for whom is not convinced beforehand.⁵

But, as I said, I will rather focus on what Meyer defines as ‘science.’ This is represented mainly by three following excerpts, which I present in an order slightly different from the original; emphases, however, are his:

The relationships which scientific theories explain *exist* in nature. (p. 165)

[T]he truth of a theory or hypothesis is never more than provisional. . . because it is impossible, in principle, to be certain that at some future time facts will not be found which will prove the theory to be partly or wholly mistaken. . . . And as mistaken theories are discarded, the validity

not “*presentational*” (pp. 166–7), so that the “original work” of a scientist is *not* the particular book written by the very hand of the scientist, but the ideas contained in it, and therefore it is hardly arguable that scientists do not appeal to the original works of the past. On the other hand, it is not all that strong to say that artists “insist upon seeing the original work:” very rarely, musicians use Beethoven’s manuscripts. Rather, there are the figures of the editor and, more importantly, the performer (or the translator in literature), which could be seen as corresponding to the authors of scientific “‘textbook’ summaries in which the results of past scientific discoveries are digested” (p. 168). In addition, textbooks on history of art could be said to ‘digest’ for the reader the works of the past, and if they present the latter, it is arguably for the less essential tasks of exemplification and justification of their interpretations than for the sake of “presentational” art. The matter is thus all but settled.

⁵His second “contrast in behavior” between scientist and arts, which addresses the fact that “scientific theories supersede one another, while works of art do not” (p. 169) is a good example of many of these faults: if we take art before the time—around the Renaissance—when the present ‘cult to personality’ took its form in Western culture, we see that the Gothic *did* supersede, for all intents and purposes, the Romanesque (more often than not, if one part of the cathedral collapsed, it would be built again following the *current* style, not in the original one). If we take popular music, we see that the *Back Street Boys* are indeed obsolete, and we can expect that Britney Spears will be ‘superseded’ (from which it does not immediately follow, let us remember, that their music is ‘bad’ or that they are not ‘artists’). Again, there is a confusion between the product, the producer, and in this case also the consumer: not because today we attend concerts with Mozart’s music can we say that *composing* like Mozart today is not obsolete. This would entail the same fallacy as to say that Latin was not superseded because texts in Latin survive and there are many people that read them and study them.

of those that survive becomes more probable. Thus through a kind of cultural-historical trial and error, science moves toward truth. (p. 164)

Although they can be confirmed only provisionally, theories and hypothesis can be *disconfirmed* unequivocally. (p. 164)

A word of warning is good before starting: the fact that these opinions seem so obviously true might be misleading: as can be easily seen, they nicely embody the “image of science” that García Posada describes, with all four characteristics. It is this image, so widespread and deeply rooted among all of us, which makes Meyer’s opinions so obvious—but also so suspect. Meyer embraces and develops these views uncritically, and with the confident, almost paternalistic tone of the one who goes, step by step and through a diaphanous and illuminating logic, from the most obvious and clear to the most sublime and complex. But in doing so he ignores completely what has been going on in the discipline called ‘philosophy of science.’ From the outside, the philosophy of science can be said to stem precisely from the untenability of those ‘obvious’ statements such as “the relationships which scientific theories explain *exist* in nature.” For a start, I propose briefly to visit that fascinating and esoteric branch of knowledge.

THE FIRST OF MEYER’S statements is an instance of what has been sometimes called ‘scientific realism,’ other times ‘metaphysical realism,’ and, more colloquially, ‘naive realism.’ The following is a description by Alfred Tauber: “A simplified definition [of ‘Scientific Realism’] is that the picture science gives us of the world is true and faithful in all details, and that the entities postulated *really* exist.”⁶ As in García Posada’s ‘dogmatism’ and ‘empiricism,’ scientific theories are defined as such because they reflect nature, and nothing more, as it is.

Today, such views can only be held only by one shares the “image of science”—which is to say the vast majority of the people, including most scientist, but importantly *not* philosophers of science. The problems of involving ‘truth,’ ‘reality,’ ‘nature,’ and the like, in a discourse about science are immense, ranging from the fact that—as Meyer himself grants in the second excerpt quoted above—reality is not directly accessible (and thus theories cannot be certainly proven), to

⁶Alfred I. Tauber, “Introduction,” in Alfred I. Tauber (ed.), *Science and the Quest for Reality* (London: MacMillan Press Ltd., 1997), p. 11.

the historical problem that so many theories that would doubtlessly count as ‘scientific,’ such as Newton’s gravitation law, were after all false and did *not* explain what “exists in nature.”

For Meyer, “[t]hese complexities, together with the historical vicissitudes of once well-established beliefs, have made scholars reluctant to assert that theories are true.” Then he explains, in a footnote to which I shall come back later on, that “this seems to be the case with Thomas Kuhn” (p. 183). The quotation from Kuhn’s *The Structure of Scientific Revolutions* that he makes immediately—a quotation hardly relevant to decide whether Kuhn believes or not in truth—let us deduce that Meyer is looking at the paragraph that Kuhn starts by saying: “It is now time to notice that until the last very few pages the term ‘truth’ had entered this essay only in a quotation from Francis Bacon.”⁷ It does not “seem” that Kuhn is “reluctant to assert the truth of theories:” he clearly has written the first twelve of the thirteen chapters of one of the most influential books ever written on philosophy of science without even invoking the concept! Why is Meyer’s comment on Kuhn, the only major name from philosophy of science that is mentioned at all in the essay, so unsure and prudent? The prejudice of the “image of science” is so powerful in Meyer’s thought, that he quite overtly cannot conceive how somebody seriously questions it. It “seems,” not because Kuhn is cryptic about his opinions, but because Meyer’s mind is way too closed about these matters to accept them.

However, the debate in philosophy of science is not, as the previous paragraphs might make appear, one confronting some scholars who believe that theories are true against some others who do not. Kuhn (who is *not* mainstream) represents one side of the debate, but the opponent is not ‘scientific realism.’ The other side is as contrary to the latter, i.e. it is as “reluctant to assert that theories are true,” as Kuhn himself is. In fact, a good description of this side, again by Tauber, is:

A dominant opposing [to “Scientific Realism”] view is more modest, claiming that the aims of science can be well served without encumbering it with truth criteria that cannot be met. In other words, instead of proclaiming a theory to be true, this ‘modest realist’ would simply display it, and enumerate its virtues, ... empirical adequacy, simplicity, comprehensiveness, coherence, predictability, etc.... This view, which some label ‘anti-realist,’ is what Bas van Fraassen calls

⁷Thomas S. Kuhn, *The structure of scientific revolutions*, 3rd. edition (Chicago and London: University of Chicago Press, 1996 [1962]), 170.

“constructive empiricism,” Hillary Putnam “inner realism,” and Arthur Fine, “the natural ontological attitude,” each of course offering his own twists to what is essentially a neo-pragmatist position.”⁸

Note that it is *these* neo-pragmatists (and not Kuhn and the rest) who are called ‘*anti*-realists’ at all. In other words, it is not, as Meyer pretends, that some scholars (Kuhn, it “seems”) have been somehow fooled by the “complexities and historical vicissitudes” and have failed to see that, obviously, theories do explain the truth. The “historical vicissitudes”—by no means contingent, exceptional, or otherwise negligible—are precisely what challenges *all* scholars, all models of scientific change.

What separates Kuhn from other, more orthodox approaches, is not disagreement about the *existence* of truth, but about its *relevance*: for him, whether or not truth (or ‘reality,’ etc.) exists is of no consequence to the nature of science and of the scientific endeavor. The neo-pragmatists, on the contrary, think that the concept of truth, whatever the difficulties it poses, is still relevant; this makes them face the problems of un-provability, uncertainty, etc., and they are thus forced very heavily to qualify their conception of ‘truth,’ in order to ‘rescue’ it so to speak, ‘reducing’ it to a kind of illusory ideal. Sometimes, Meyer’s intuitions come close to resemble the seeds of these neo-pragmatic approaches, for example in the second excerpt above. In fact, were it not for his naive assertion that what science explains “exists in nature,” his thought would be a good introduction to this side of the debate (an introduction for the very beginner, of course).

Again, matters are far more complicated than suggested. In other contexts within the discussions in philosophy of sciences, all those who consider truth ‘relevant,’ regardless of how ideal or illusory it might be, are grouped into the category of ‘realism,’ and opposed to that of ‘relativism.’ Of course, ‘realism’ in these contexts means not the strong realism of Meyer’s first excerpt, but what I have been referring (after Tauber) as ‘neo-pragmatism.’ This is the sense in which Larry Laudan uses the word in his essay “Explaining the Success of Science: Beyond Epistemic Realism and Relativism.”⁹ Since Laudan includes (with some reason) the name of Sir Karl Popper as one of

⁸Tauber, *Op. Cit.*, pp. 11–2.

⁹Larry Laudan, “Explaining the Success of Science: Beyond Epistemic Realism and Relativism,” in Alfred I. Tauber (ed.), *Science and the Quest for Reality* (London: MacMillan Press Ltd., 1997).

the main figures of ‘realism,’ a quotation from his writings is not out of place—also incidentally considering that Popper was educated in musical composition and attended Schönberg’s private concerts in Vienna:¹⁰

Our theories, beginning with primitive myths and evolving into the theories of science, are indeed man-made, as Kant said. We do try to impose them on the world, we can always stick to them dogmatically if we so wish, even if they are false (as are not only most religious myths, it seems, but also Newton’s theory, which is the one Kant had in mind). But although at first we have to stick to our theories—*without theories we cannot even begin*, for we have nothing else to go by—we can, in the course of time, adopt a more critical attitude towards them. We can try to replace them by something better if we have learned, with their help, where they let us down. Thus there may arise a scientific or critical phase of thinking, *which is necessarily preceded by an uncritical phase*.¹¹

The skeptical tone of this view (the emphasis on the falseness of even Newton’s theory, the blurring of the separation between myth and science) is noteworthy. Science comes into its real shape, for Popper, when “once-established beliefs” are criticized, not when they are “stuck to dogmatically.” This ties into one of the central elements of Popper’s thought—the theory of ‘falsification’—but the point is that Meyer’s triumphant image of science as accurately and truthfully describing what really “exists in nature” is almost opposed to Popper’s conception. So, while Meyer’s view is the quintessential definition of (strong) ‘scientific realism,’ philosophers that would be ‘on his side’ (against Kuhn) have moved far, very far, away from it. “Indeed—says Laudan—on the best-articulated sense of truthlikeness (namely Popper’s theory of verisimilitude), it can be shown that a theory may have a high ‘truth content’ yet have all its observable consequences false.”¹²

I have been making the impression that the opposition between what Laudan calls “Epistemic Realism and Relativism” is impersonated in the famous and fascinating controversy between Popper and Kuhn. The warning needs to be made, again, that this is too simple a way to put it. But

¹⁰Jamie C. Kassler, “Apollo or Dionysus? Music and the Growth of Knowledge,” in *Music, Science, Philosophy: Models in the Universe of Thought* (Aldershot, Burlington, Singapore and Sydney: Ashgate, 2001).

¹¹Sir Karl R. Popper, *Unended Quest: An Intellectual Autobiography* (Glasgow, 1976), pp. 59–60 (original emphasis).

¹²Laudan, *Explaining the Success*, p. 145.

there is some truth in identifying the opposition with the Kuhn-Popper controversy, at least enough truth to render the following quotation relevant for the present purposes. It comes from Kuhn speaking of Popper (of whom he was a pupil and remained an admiring contender) in his article “Logic of Discovery or Psychology of Research?,” one of the most important documents of that controversy:

On almost all the occasions when we turn explicitly to the same problems, Sir Karl’s view of science and my own are very nearly identical. We are both concerned with the dynamic process by which scientific knowledge is acquired rather than with the logical structure of the products of scientific research. Given that concern, both of us emphasize, as legitimate data, the facts and also the spirit of actual scientific life, and both of us turn often to history to find them. From this pool of shared data, we draw many of the same conclusions. Both of us reject the view that science progresses by accretion; both emphasize instead the revolutionary process by which an older theory is rejected and replaced by an incompatible one; and both deeply underscore the role played in this process by the occasional failure of the older theory to meet challenges posed by logic, experiment, or observation. Finally, Sir Karl and I are united in opposition to a number of the most characteristic theses of classical positivism. We both emphasize, for example, the intimate and inevitable entanglement of scientific observation with scientific theory; we are correspondingly skeptical of efforts to produce any neutral observation language; and we both insist scientists may properly aim to invent theories that *explain* observed phenomena and that do so in terms of *real* objects, whatever the latter phrase may mean.¹³

This almost amounts to say that, in as much as one is not familiar with the esoteric details and specific elements of their theories, both scholars—representing opposing approaches to nature of science—think very much alike.¹⁴ The nature of the disagreements is hidden behind and

¹³Thomas S. Kuhn, “Logic of Discovery or Psychology of Research?,” in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago and London: University of Chicago Press, 1977), p. 267.

¹⁴The similarity is in fact so striking that I might be suspect of not having chosen the representatives of truly opposed views. But the case is not unique. This is how Laudan describes the opposition between Kuhn and Imre Lakatos: “[I]argely in response to Kuhn’s assault on some of the cherished assumptions of traditional philosophy of science, Imre Lakatos has developed an alternative theory. . . [P]rogress, for Lakatos even more than for Kuhn, is a function exclusively of the *empirical* growth of a tradition” (Larry Laudan, *Progress and its Problems* (Berkeley, Los Angeles, London: University of California Press, 1977), 76). But one of the main differences between the two is that Lakatos allows for what in Kuhn’s terms would be different, competing ‘paradigms.’ This is a further rejection of realist assumptions like Meyer’s statement that theories are accepted and “unequivocally *disconfirmed*” (164) on the basis of

underneath their general aims and ideologies. This is clear and enough proof that the philosophy of science has gone well beyond the superficial reach of common sense and obvious immediacy, from which Meyer so confidently departs. The way in which it seems so easy for him to refute and disregard those assertions by Thomas Kuhn that he does not quite understand resembles the long quotations from Augustine that Galileo included in his *Letter to the Grand Duchess*, just at a time when Jesuits and Dominicans were at the height of controversy about predestination: “the Dominican’s (and later the Jansenists’), arguments were mainly based on Augustine’s, so that the African saint’s opinions had become a very controversial subject. Galileo’s innocent reliance on Augustine’s authority shows how unwise it was for a layman to venture out into the rarified but highly charged air of theology;”¹⁵ it all reveals just too plainly and sadly Meyer’s little familiarity with and respect for the complexities of the nature of science, and for those who have devoted their lives to them.



BUT, JUST WHAT ARE THE problems that make the concept of truth the red-hot core of the debate in philosophy of science, rather than the safe starting point it is for Meyer? I have spoken of the ‘un-provability’ of theories, for example. I mean that it is not possible to prove that ‘the Sun will rise tomorrow,’ or that ‘this stone will fall down when I drop it.’ Gravitation theories *explain* why and how it will fall—but do not *prove* it. Meyer dismisses this difficulty lightly, for “it seems to me, however, that some theories are so securely supported by the weight of evidence and by the larger pattern of hypotheses that their truth cannot be denied. Even to qualify them as provisional would be to cavil—like arguing that the statement, ‘all men are mortal,’ is only probable because the evidence is not yet complete” (183). All of us (including philosophers of science) will agree

experimental evidence. Again, a thinker with whom Meyer would side (if only he knew him) is closer to Kuhn than to Meyer. See also Kuhn’s “Reflections on my Critics: Philosophical Essays, 1970–1993, with an Autobiographical Interview,” in James Conant and John Haugeland (eds.), *The Road since Structure* (Chicago and London: University of Chicago Press, 2000).

¹⁵ Arthur Koestler, *The Sleepwalkers: A history of man’s changing view of the universe* (London: Hutchinson, 1959), p. 450.

that the truth of those statements “cannot be denied,” but this is not to say that they can be proven. The very assumption that a certain amount of ‘evidence’ will be ‘enough’ and will ‘proof’ a theory poses immediate questions for whose answer there are no grounds whatsoever: ‘just how much evidence?’

Surely, scientists themselves have been content with certain amount of evidence, so that they can go on with their inquiries; science does not need to proof its statements beyond any doubt (which would be impossible, as Meyer himself grants in his second excerpt above). But this does not allow *philosophy of science*, or any discourse about science, to equate un-deniability to proof. (Incidentally, note that un-deniability is very relative: that the three statements *can* be denied with claims of truth is shown by the Bible.)

It is interesting that this particular error, mistaking overwhelming evidence for proof, was committed also (and more explicitly) by Gary Tomlinson: “Cultural History, like cultural anthropology, searches for meaning, not proof. And meaning, once again, arises as a function of context, deepened as the context is made richer, fuller, more complete.”¹⁶ Overwhelming evidence is, as far as I can tell, just one kind of ‘context,’ and, like ‘meaning,’ ‘proof’ (in the empirical sciences) is *also* ultimately a function of context. In fact, what is it, if not precisely context (the context provided by virtually the whole recorded history), what allows us to expect that the stone will fall down? If by ‘proof’ we want to mean something decidedly autonomous from context, the only activities that are left with are mathematics and logic, and these offer a rather irrelevant comparison with cultural history, and surely not the one Tomlinson intends.

The problems of un-provability are closely related to the problems of Meyer’s first excerpt: that scientific theories explain what “exists in nature.” Meyer uses this ‘fact’ to make his first distinction between science and art, for the works of art do not “exist in nature” before their creation: “[t]he structure of the DNA molecule was what it was before Watson and Crick formulated a theory of its structure... This is not, however, the case with works of art: *Hamlet*, *Guernica*, and the C-sharp Minor Quartet quite simply *did not exist* prior to Shakespeare’s writing, Picasso’s painting, and Beethoven’s composing” (p. 165). Consequently, “we do not say that Beethoven ‘discov-

¹⁶Gary Tomlinson, “The Web of Culture,” *Nineteenth-Century Music* 7 (1984), p. 355.

ered' the C-sharp Minor Quartet but that he *created* or *composed* it. Conversely, one would not say that Watson and Crick 'created' the double helix but, rather, that they *discovered* the structure of DNA or that they formulated a theory of its structure."

To begin with, here we see one of the mentioned confusions: the object of science (namely the DNA molecule itself) is not compared with the object, but with the *product* of art. The scientific product, in this case, is Watson and Crick's theory itself, and of that it not so easy to maintain that it "existed in nature"—in fact it is not easy to say that it exists in nature even now, after its formulation. Needless to say, there are many things that are 'created' but are no art: almost all sentences, for example, are "new linguistic forms—often new in one's experience or even in the history of the language."¹⁷

But, of course, refuting the example does not refute the thesis. The thesis, again, is that "the relationships which scientific theories explain *exist* in nature." One can but wonder: where in the universe did phlogiston exist? Or ether, or the desire of rocks to reach home by falling down?¹⁸ Does 'dogmatism' ("only the hypothesis that have been proven by 'reality' are part of the body of science") apply retrospectively? In other words, should we opine that, since the mentioned theories (and so many other, including Newton's gravitation) have been discredited and their elements and relationships proven *not* to exist in nature, they are not 'really' scientific theories?

The second and third excerpts quoted above from Meyer allow for scientific progress, so there we can look for illumination about the latter questions. Theories are provisional; they can be "*disconfirmed unequivocally*" (presumably through empirical testing); and with this disconfirmations there is a 'sorting-out' process by which wrong theories are discarded, and science advances

¹⁷Noam Chomsky, *Language and Problems of Knowledge: The Managua Lectures* (Cambridge, Mass.: M.I.T. Press, 1988), p. 5.

¹⁸'Phlogiston' was the name given to the material stuff that burning things 'gave away' to the atmosphere, as burning things were empirically observed to lose weight. The theory was ultimately proven false by Lavoisier and the 'discovery' of oxygen—triggering off the birth of modern chemistry. The better-known 'ether' was the material medium through which light waves were presumed to propagate (since it had been empirically observed that every wave needs a material medium to propagate; today we no longer require that the medium be material). And the 'desire to reach home' was the Aristotelian explanation to the falling down of heavy objects (whose home was the earth) and the rise of light ones (whose home was heaven).

“toward truth.” Thus, about phlogiston we can deduce that, when it was a scientific theory, it had not been “*disconfirmed unequivocally*,” but rather was “confirmed only provisionally.” Eventually, in one more of the happy cultural-historical steps toward truth, it was “discarded:” a new theory explained the non-existence of phlogiston. It is surely undeniable that the non-existence of phlogiston did in fact “exist in nature”! This does not yet include the theory in the body of sciences (for it never explained things that “existed in nature”), but at least it gives it a honorary place: thanks to it, there was a new fact (phlogiston’s non-existence) to be ‘discovered’—not ‘created,’ for, as the DNA molecule, it “was what it was” before Lavoisier “formulated” it.

However, when this latter move “toward truth” took place, the theory of phlogiston was all but “unequivocally *disconfirmed*.” “By 1777, probably with the assistance of a second hint from Priestley [whose experiments predated any work of Lavoisier on the matter,] Lavoisier had concluded that the gas was a distinct species, one of the two main constituents of the atmosphere, [i.e. oxygen,] *a conclusion that Priestley was never able to accept.*”¹⁹ Along the same lines, consider also the following quotations from Michael Polányi:

The destruction of belief in witchcraft during the sixteenth and seventeenth centuries was achieved in the face of an overwhelming, and still rapidly growing body of evidence for its reality. Those who denied that witches existed did not attempt to explain this evidence at all, but successfully urged that it be disregarded. Glanvill, who was one of the founders of the Royal Society, not unreasonably denounced this proposal as unscientific, on the ground of the professed empiricism of contemporary science. Some of the unexplained evidence for witchcraft was indeed buried for good, and only struggled painfully to light two centuries later when it was eventually recognized as the manifestation of hypnotic powers.²⁰

¹⁹Kuhn, *Structure*, p. 54 (my emphasis). Kuhn’s next paragraph starts an important discussion about the nature of discovery, which perfectly complements Koestler’s section mentioned in note 20. In page 52, Kuhn has opined that the “distinction between discovery and invention or between fact and theory will... immediately prove to be exceedingly artificial.” Cf. Meyer’s distinction between Watson and Crick, on the one hand, and Shakespeare, Picasso, and Beethoven, on the other.

²⁰Michael Polányi, *Personal Knowledge* (London: Routledge & K. Paul, 1958), p. 168, quoted in Koestler, *The Act of Creation*, p. 243. This whole section of Koestler’s book, ‘*Limits of Confirmation*’ (pp. 240–6) is a wonderful account of many instances of one and the same ‘crucial experiment’ both verifying and disconfirming different, often opposed, theories.

The laymen, taught to revere scientist for their absolute respect for the observed facts, and for the judiciously detached and purely provisional manner in which they hold scientific theories (always ready to abandon a theory at the sight of any contradictory evidence) might well have thought that, at Miller's announcement of this overwhelming evidence of a 'positive effect' in his presidential address to the American Physical Society on December 29th, 1925, his audience would have instantly abandoned the theory of relativity. . . . But no: by that time they had so well closed their minds to any suggestion which threatened the new rationality achieved by Einstein's world-picture, that it was almost impossible for them to think again in different terms. Little attention was paid to the experiments. . . .²¹

Note particularly the second quote: the first part is another excellent description of the kind of ideas about science and scientists that Meyer holds. That of the layman, educated by popular science textbooks and videos (sadly including the fascinating James Burke of *Connections*) that teach us, against Einstein's own word, that he *derived* Relativity *from* Miller's experiment, when in fact the latter proved it 'false'! Again, Polanyi's examples are by no means unrepresentative. Those, and the history of phlogiston, ether, Newton's law and his derivation (from the Bible) of the age of the universe, Kepler's harmony, Aristotelian dynamics, . . . , those are not "historical vicissitudes" of scientific theories that caught Kuhn as Meyer thinks: those are the kind of things that any philosophy of science has to deal with.

Another consequence of the mechanist progress of science, one readily accounted for by Meyer, is that sciences has advanced farther in some areas than in others: "Current hypotheses in particle physics, for instance, are quite provisional, while theories of ordinary matter are evidently both rigorous and reliable" (p. 164). Although the sentence is not quite accurate (even for the time Meyer first wrote it), what he is trying to say comes through. He then quotes Gerald Feinberg: "I cannot see any. . . unknown regions on our present map of nature. Accordingly it seems to me that we now have a model of the structure of bulk matter that is fairly complete and unlikely to change in its essential parts."

Feinberg wrote this in 1967. But it is curious that his statement would fit the thoughts, if only not the style, of the average medieval theologian, whose confidence in *The Great Chain of Being* provided him with a *truly* complete "map of nature"—indeed one of the whole Creation—, and a

²¹Polanyi, *Op. Cit.*, p. 12–3, quoted in Koestler, *The Act of Creation*, p. 244.

“structure of bulk matter,” both of which were for him *truly* unlikely to change in its essential parts. For, as Kuhn says (my emphasis): “though the bulk of scientific knowledge clearly increases with time, what are we to say about ignorance? The problems solved during the last thirty years did not exist as open questions a century ago. *In any age, the scientific knowledge already at hand virtually exhausts what there is to know, leaving visible puzzles only at the horizon of existing knowledge.*”²² This, for example, accounts for the unquestionable ‘scientificity’ of Newton’s universe: roughly two and a half centuries were needed for scientists to perceive the anomalies that would finally lead to its destruction and replacement. There can be little surprise that Newton himself was so blatantly fooled by nature, or that the medieval theologian did not see the falsity of his ideas about the world.

Conversely, little surprise, but also little consequence, can be extracted from Feinberg’s confidence about his knowledge of the bulk of matter. The testimony of a contemporary witness, who is naturally bound to know only what he knows, and to ignore what he does not, is obviously the weakest measure of the validity and status of that knowledge, and it is indeed very hard to explain how a serious thinker like Meyer can give it any weight at all. Does he not realize that astronomers before the era of the big telescopes would maintain without hesitation that the non-existence of Uranus, Neptune and Pluto “existed in nature”?²³ Again, would he really expect the holders of phlogiston theory (and the rest of false theories) to claim that the elements ascertained by their respective models did not really exist in nature, that they were making up a whole set of spurious concepts only to provide someone in the future with the chance to “discover” their non-existence?



THIS ALL SURELY SOUNDS familiar to a musicologist, as there is a word that can promptly summon up all remembrances: ‘teleology.’ Concerns and questions about it have been raised, in my opinion most successfully, by Leo Treitler. In *What kind of Story is History?* he characterizes

²²Kuhn, *Logic*, pp. 289–90.

²³For example, the question that set Kepler’s work in motion, the one that nobody before had thought about, was ‘why are there 6 planets, and not 8, or 2?’ He not only did not know that the other three plants existed, in effect he *positively* knew that they did not.

the “Whig interpretation which narrated English history as a gradual and inevitable development toward liberty, [and which] was but a version of a historiographical paradigm that had begun to inform historical writing on every subject in the late eighteenth century and has continued to do so until very recently. It was a way of reconciling continuity and change in stories of transformation that were *immanent, gradual, unilinear, progressive, and teleological*, and that were marked by steady *improvement* in the sense of the fulfillment of a purpose, the realization of a goal, or the *approach to perfection*.”²⁴ This of course is also a good description of Meyer’s approach to the history of science and scientific change (the “cultural-historical trial and error” that drives science “toward truth”). Since “in the nineteenth century [the Whig interpretation] assimilated the metaphor of history as organic growth, especially in the history of the arts,”²⁵ this ties into Treitler’s general criticism of the ‘narrative’ way of doing history of music, most direct and explicit in *The Historiography of Music*: “The implied dynamic of continuity and change is that of causality of a kind associated with organic growth[, that has] an aura of inevitability”²⁶ and the sense of an “unspecified force that is driving the narrative.”²⁷

But there is another important aspect of Treitler’s criticism, directly concerning teleology itself (not the closely related inevitability and organic growth): “It has been difficult for scholars in this field to write from the point of view of the present without treating the outcome as a goal or norm inherent in the process of change... It is the general problem that Walter Ong focused when he spoke of the concept of ‘oral literature’ as akin to referring to a horse as a wheel-less automobile.”²⁸ As has been seen, the difficulty for scholars in this field is all the more dangerous when they are trying to write about science, above all when they let common-sense guide them. The

²⁴Leo Treitler, “What kind of Story is History?,” in *Music and the Historical Imagination* (Cambridge: Harvard University Press, 1989), p. 160 (my emphasis).

²⁵*Idem*, p. 160.

²⁶Leo Treitler, “The Historiography of Music,” in Nicholas Cook and Mark Everist (eds.), *Rethinking Music* (Oxford and New York: Oxford University Press, 1999), p. 362. This and the following pages are devoted to the criticism of the ‘narrative’ history of music.

²⁷*Idem*, p. 363n.

²⁸Treitler, *What kind*, p. 164–5.

acceptance of a truth, or more precisely of the *relevance* of the existence of a truth, immediately implies a teleological view of the history of science: whatever the actual process of scientific evolution, whatever its round-about and mistakes, there is from the beginning a ‘goal,’ an entity that pulls the whole process *from ahead*. Under those conditions, it is natural that the phlogiston theory, if acceptable as ‘scientific’ at all, is in any case a primitive sort of science: ‘the function to which phlogiston eventually evolved—its falsification—was inherent in the process of evolution as its goal, and accordingly progress toward that goal depends on the recognition of it by the actors in the story.’ This latter sentence (after the colon) is an almost literal paraphrase from Treitler, after his quotation of the teleological approach of Peter Wagner’s to the notation of a manuscript from the tenth century.²⁹

And this brings me, for a number of reasons, back to the footnote to Meyer’s page 183 that I have already mentioned (page 6). This is the rest of it (Meyer is quoting and commenting upon Kuhn’s *The Structure of Scientific Revolutions*):

However, to achieve “an increasingly detailed and refined understanding of nature” is to do more than merely move “from primitive beginnings.” It is to move toward and, at times, to arrive at truths. It is not necessary to adopt a teleological view of scientific progress—one which reifies *truth* as a kind of goal that is objectively “there” to be discovered—in order to believe that science moves toward truth. Kuhn’s analogy between the history of science and biological evolution, which seems to play a part in his position, is misleading. Not only do scientists, like other human beings, have goals, while natural processes do not, but the relationships that science explains exist as phenomena in the world and provide data for the evaluation of theories, while the phenomena themselves (the facts of evolution) have no reference in terms of which either goals or truths could possibly be evaluated.

This deserves close examination. First of all, from his own definition of ‘teleological view,’ we can safely conclude that Meyer is indeed adopting a teleological view from what has been just

²⁹*Idem*, p. 165. Excerpts from the quotation, that comes from Wagner’s *Einführung in die Gregorianischen Melodien*, are: “This diastemata is of a primitive sort;” “what is striking is that one did not proceed further in this direction once the way was opened;” “no one drew from that the conclusions that . . . lay so near at hand;” “[circumstances] removed the goal of neumatic writing from before their eyes.” Treitler’s original comment reads: “In other words, the function to which neumes eventually evolved—the designation of pitch patterns—was inherent in the process of evolution,” etc.

said. Therefore, the mysterious comment that “it is not necessary to adopt” such a view should be understood as ‘even if you are so blind not to adopt a teleological position, you have to grant that science moves toward truth.’ Kuhn, in fact, does believe in some kind of ‘progress,’ although he does not (or at least claims not to) adopt a teleological view. But Meyer would find it interesting to learn that one of the most intense critics of Kuhn, his disciple Paul Feyerabend, bases his criticism *precisely* on this point, Kuhn’s belief in progress, which he regards—with very good reasons—as inconsistent with his theory. As has been seen, the concept of a ‘truth there,’ which Meyer takes for granted, Popper tries to rescue, and Kuhn rejects, is closely related to the concept of ‘progress,’ and more specifically to a *teleological* view of progress. The rejection of truth renders deeply questionable any idea of ‘progress’ in science, and—again—this questioning has been much more explored, defended, and important than Meyer can possibly conceive.

We will address Kuhn’s analogy of progress of science to biological evolution—it will prove beautifully relevant—but first a word on Meyer’s criticism of it. Here, quite simply, logic fails to Meyer, in what is perhaps the most offensive passage of the essay. “Scientists have goals while natural processes do not.” Of course “natural processes do not have a goal,” but giraffes surely do. Is not ‘eating that tall tree’ a goal? “The facts of evolution have no reference in terms of which goals could be evaluated.” Is not the height of the edible trees a perfectly reliable reference to evaluate the success or failure of the length of one’s neck? If there are subtle confusions of categories in the rest of the essay, the one here is but too obvious: Meyer is comparing scientists, the individuals of one of the evolutive processes, to the other process *as a whole*. Following a similar ‘logic,’ I could say that ‘dogs, like other animals, have goals, while cultural processes do not’—but I think I would better refrain from such a step.

Of course, given this, Meyer misses the whole point of Kuhn’s analogy. The evolutionary model Kuhn has in mind for a “conceptual transposition” into his alternate view of history of science is Darwinian evolution *as opposed to* “the well-known pre-Darwinian evolutionary theories—those of Lamarck, Chambers, Spencer and the German *Naturphilosophen*.” For the latter, “[e]ach new stage of evolutionary development was a more perfect realization of a plan that had been present from the start,” while “[t]he *Origin of Species* recognized no goal set either by God or

nature.”³⁰ Kuhn thus makes the right kind of comparison: *just as* natural processes do not have a goal, even if individual animals do, *the same* can perhaps be said of science: even if individual scientists do have goals, science as a process does not. Darwinian evolution provides an antecedent, a model to follow, in order to achieve the shift that Kuhn advocates for, namely the substitution of “evolution-from-what-we-know for evolution-toward-what-we-wish-to-know.”³¹

Kuhn’s exhortation, evidently, is identical to Treitler’s repeated longing: “it is strange that [Darwin’s evolutionary scheme] has had so little influence on thinking about evolution in humanistic disciplines (where there is often a mistaken belief that Darwinian ideas have been assimilated),”³² and “conceptions about the evolution of the arts have never been very much influenced by Darwinian theory, which posits a very different [from teleology] process of change.”³³

One of the best and most illuminating examples of ‘wrong teleological views, and how to correct them’ is doubtlessly the study of neumatic notation. Just as it happens with scientific theories of the past, and differently from, for example, old languages or the history of musical style, the most immediate impression that one can have when looking at an instance of neumatic notation is that it is somehow in an earlier, primitive, undeveloped phase. In other words, when looking at it, or when reviewing old scientific ideas, it is very hard to avoid seeing not only *it*, but also a pale image (whether positive or negative) of what we now know. It is a truly remarkable act of imagination—thoroughly documented in Treitler’s “Reading and Singing”—to realize that neumatic notation is not the beginning of the solution of our present needs, but the solution to *other* needs, stemming from different ends and priorities than ours. “The historical question is not ‘What is the origin of the neumes?’ but ‘What sort of thing was music-writing, and how and for what purposes did it come to be done?’ It is a question of semiotics, not alone of paleography.”³⁴

³⁰Kuhn, *Structure*, p. 171–2.

³¹*Idem*, p. 171.

³²Leo Treitler, “Reading and Singing: On the Genesis of Occidental Music-Writing,” *Early Music History* 4 (1984), p. 196.

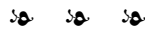
³³Treitler, *What kind*, p. 160.

³⁴Treitler, *Reading*, p. 208.

Again, compare Kuhn:

Like [most earlier historians of science], I asked of my texts [Aristotle's texts on mechanics] the questions: How much about mechanics was known within the Aristotelian tradition...? Even at the apparently descriptive level, the Aristotelians had known little of mechanics: much of what they had had to say about it was simply wrong...

[Now] I offer a maxim: When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning.³⁵



I SUPPOSE IT IS NOW MY turn to 'ask myself how a sensible person could have written the absurdities' found in Meyer's essay, and how it is possible that so many sensible people tend to accept them. Many of the absurdities can (and ought) be easily explained in the terms I have been using or suggesting so far: ignorance, misunderstanding, and a strong suspicion that Meyer did not really read what he claims. But there are more important reasons to discover, and above all the problem deserves attention of how it is possible that these absurdities have passed un-criticized from generation to generation.

One first thing to notice is the strong relationship between 'scientific realism' (both strong and modest) and positivism. To be most accurate, the relationship has to be made in negative terms: non-realistic views of science are entirely incompatible with positivism, since non-realists reject the external truth that, through recognition and reification, positivism departs from. In logically strict terms, this implies no more than the compatibility of positivism and scientific realism—not their identity, in any case. However, it is safe to assume that scientific realism is in a sense the development of positivism in the particular sphere of ideas about science. The neo-pragmatic attempts at rescuing truth from the shaky position in which the "historical vicissitudes" of scientific theories leave it reveals but the desperate need to keep arbitrariness and freedom safely outside the realms of natural knowledge. Facing the problem that knowledge is an active construction, instead

³⁵Thomas S. Kuhn, *The Essential Tension: Selected Studies on Scientific Tradition and Change* (Chicago and London: University of Chicago Press, 1977), pp. xi–xii.

of the passive, self-denying and centered ‘reception’ of truth that ‘happens to one,’ as it were, positivist-oriented conceptions of science were forced to replace the ‘external truth’ (which would never be completely ‘external’) by suitable *ad hoc* concepts (such as ‘verisimilitude’), in the hope of keeping science attached to the natural world. For, as long as too much autonomy from nature was granted to the development of science, the positivist would not be able to satisfactorily spare science of remaining “infantile, in the stage of theological or metaphysical thinking; it would be in reality not science but fetishism.”³⁶ So, ‘scientific realism’ is, in its most basic ideology, positivist. Let alone the “image of science” for whose acceptance I am seeking an explanation: that is positivist in its very baseness.

The explanation begins to take place only when, in addition to the relationship between scientific realism and positivism, the peculiar deformation of the very conceptions of both ‘positivism’ and ‘science’ is taken into account. In musicology, Treitler has already pointed to the inaccuracies and mistakes of the usual references to the former: “since the publication of Joseph Kerman’s *Contemplating Music*, a watered-down notion of positivism has gone into the label ‘positivistic musicology’, applied to that branch of musicological activity that entails ‘the presentation of the texts of early music and of facts and figures about it’.”³⁷ Similar, but more direct, concerns have been issued by Richard Taruskin: “[Kerman’s] insufficiently discriminating demonization of ‘positivism’ has provided a banner under which many disciplinary faith-healers have congregated.”³⁸ As a result (at least in part), today the word ‘positivism’ is almost synonymous with ‘archival work’ in everyday-life musicological thought.

But in more historiographically oriented discourses, ‘positivism’ is used in a different way: “In reviews of New Musicological writings,... it has become almost ritually binding to explain that such writing has been made possible by the release of music study from two formerly controlling

³⁶Ernst Cassirer, *The Problem of Knowledge: Philosophy, Science, & History since Hegel*, translated by William H. Woglom, M.D., and Charles W. Hendel, with a preface by Charles W. Hendel edition (New Haven and London: Yale University Press, 1950), p. 244.

³⁷Treitler, *Historiography*, p. 376.

³⁸Richard Taruskin, “Others: A Mythology and a Demurrer (By Way of Preface),” in *Defining Russia Musically* (Princeton: Princeton University Press, 1997), p. xxxn.

strategies of thought, formalism and positivism. . . It is only with the demise of positivism, so goes this line of diagnostic thought, that musicologists have been freed for the work of interpretation that was precluded under its dominion.”³⁹

The word ‘positivist’ has been converted into an automatic instrument, almost a button, of de-qualification. Dianna Fuss, speaking of ‘essentialism,’ has said that few words in the critical vocabulary have been “so little interrogated, and so predictably summoned as a term of infallible critique.”⁴⁰ ‘Positivism,’ doubtlessly, is another such word. It is “ritually” used to stigmatize those ‘old’ traditions that, supposedly, stick rigidly to the confirmable, the correctible, the certain, that accept no other explanations than those of causation and determinism. I will allow myself to mention here one ‘real life case’ (from among so many), because, interestingly, the article from which it comes is an exceedingly valuable piece of scholarship thanks *precisely* to the most healthy positivist spirit with which it dismantles an old myth based on spurious assumptions with no factual support whatsoever. Its author, however, explains: “Conforming to the progressive, ‘positivist’ trend in humanistic disciplines at the time, late-nineteenth-century ideology advocated the search for the recovery of the composer’s intentions. Within this system, the early critics based their story on what they thought was in Mahler’s mind while he wrote the [ninth] symphony, and on the assumption that this was formulated directly into the music.”⁴¹ One would say that if something can be regretted in the early critics, it is not that they were ‘positivists,’ but in any case that they were so bad at it that they seemed not to be positivists at all. . .

Of course, all this is just a matter of names (and Micznic wisely encloses the word within quotation marks), but that is exactly the point. The word was turned into a *tabula* (almost) *rasa*, with some loose ties to ‘the facts,’ but capable of containing anything that the proponents of a ‘new’ way to do musicology wanted to reject—in other words, anything that stood as the ‘Other’ in their self-construction. The conviction that “History is not a thing that can be deduced”—a

³⁹Treitler, *Historiography*, p. 376.

⁴⁰Diana Fuss, *Essentially speaking: Feminism, Nature, and Difference* (New York, 1989), p. xi, quoted by Ruth Solie, “Introduction: On ‘Difference’,” in Ruth Solie (ed.), *Musicology and Difference: Gender and Sexuality in Music Scholarship* (Berkeley, Los Angeles and London: University of California Press, 1993), p. 4.

⁴¹Vera Micznic, “The Farewell Story of Mahler’s Ninth Symphony,” *Nineteenth-Century Music* 20 (1996), p. 148.

sentence that could be in the cote of arms of the so-called New Musicology, but that has in its ranks also other defenders that would not accept that label (including the present writer)—is the battle-cry behind the varied reactions, worth and not-so-worth, against ‘old’ musicology. With few exceptions, those who bear it point at those who did not, and call them ‘positivists.’ Most of them, in any case, ignore or forget that the beautiful sentence comes from a book written in 1854, by no other than August Comte, the father of positivism himself.⁴²

Not casually, it may be assumed, the semantic distortion of the word ‘positivist’ coincided with its association to ‘old’ musicology. It goes without saying that the association needed a deformation of the latter as well: ‘old’ musicology is not ‘positivist’ even in the distorted sense. As Margaret Bent recalls with transparent logic, “[o]ne ghost that needs to be laid to rest straight away is the caricature of the obsessive fact-grabbing positivist. Many findings by major scholars such as Bukofzer and Lowinsky have since been entirely superseded, though their work none the less remains valuable and influential. If they were merely amassing facts and ‘unveiling truth’ this would not be so.”⁴³ In the identity-construction process I am describing, the ‘old’ was not inherently ‘positivist;’ what they were was, rather, the incarnation of the Other, perceived variously as the neglecter of criticism, the Germano-, Euro-, or otherwise ethno-centrist, the arrogant presentist, the rationalist male, . . . The ‘resisters,’ armed with the most current (and controversial) philosophical theories—the label of ‘post-modern,’ taking on the risks of its ambiguity, is not completely inaccurate to describe them—and with a revolutionary spirit, were prompt to identify and target

⁴²August Comte, *Système de Politique Positive* (Paris: Chez Carilian-Goeury et Vor Dalmont, 1854), ‘Appendice général’, quoted by Cassirer, *Op. Cit.*, p. 246. Things are again infinitely more complicated, in view of the fact that the original positivist spirit of Comte’s philosophy was rapidly transformed by the positivists themselves. Cassirer goes on: “In the development of the positivistic method and historiography in France and England this basic idea was not only relegated to the background but was at times either obliterated or else turned into the exact opposite.” The identification of ‘positivism’ with dogmatic naturalism, closer to (but not quite the same as) what the word has come to refer to in musicological writings, was partly based on these modifications, and is what Kuhn has in mind when he speaks of ‘logical positivism’ (*Structure*, pp. 98ff.). In any case, Comte’s philosophy of history resembles much more the thought of ‘anti-positivist’ musicologists than what they mean by ‘positivism.’

⁴³Margaret Bent, “Response,” in David Greer et al. (ed.), *Musicology and its Sister Disciplines* (New York: Oxford University Press, 2000), p. 192.

‘positivism’ as the weakest point on Modernity’s fort.

SCIENCE FITS ON ALL THIS also by way of faulty association. The “image of science,” with its uncritical emphasis on realism and its naive assumption that natural science is the example *par excellence* of a ‘dogmatic naturalism,’ with objectivity, detachment, and respect for the ‘facts’ at their best manifestations, provided the perfect crowning piece of the construction of an Other for ‘post-modern’ musicology. That is probably why the problematization of a ‘positivistic’ approach to history and culture that populates their historiographical writings goes very frequently in hand with the explicit differentiation between ‘humanistic’ and ‘natural’ knowledge. How this is a misconception, how its acceptance requires little less than self-imposed blindness, has occupied the first part of this essay. The fact itself that such an article as Meyer’s was written at all, and that a correction (or at least questioning) is needed of its basic, un-skeptical assumptions, is enough to realize that ‘science’ has been mysteriously given a meaning and many references that do not essentially belong to it.

I have also quoted Tomlinson’s slip about ‘proof’ and ‘context,’ which is but a concise manifestation of the need to differentiate essentially between the study of culture (and history) and that of nature—a need which in Tomlinson’s writings is perhaps most clear. His program for the study of ‘humanities’ is beautiful and compelling. But on the background, more or less implicit, there is always a sense that it is some sort of ‘liberation’ of the ‘humanities’ from the natural sciences—that ‘humanists’ should by now have realized that the methods of natural science do not apply to them. The flaw in all this is not that the methods of natural science should apply to ‘humanities,’ but the fact that they do not apply, as has been shown, to natural science either, in the first place. The fallacies of ‘dogmatic naturalism,’ teleology, and objectivity, are to be denounced not only in ‘humanities,’ but also (and perhaps with more difficulty and effort) in the empirical sciences. Tomlinson does the first, but he depends for that on not having done the second, in fact, on accepting those very fallacies as regards science.

This can be seen, for example, in some of his interpretations of Treitler. One of the latter’s main arguments, developed in a number of his writings, is the impossibility of an objective knowledge, that kind of knowledge that ‘happens to one,’ “the only kind of knowledge worth having from the

positivist point of view.”⁴⁴ Tomlinson takes it rightly to back his ‘historiographical’ program, but what he does not realize is that Treitler’s points affect issues of scientific knowledge as directly or more than they affect historiographical ones.

In one of Treitler’s essays referred to by Tomlinson one finds a quotation from Michel Polanyi (from whom I have also borrowed to discredit the empirical, “unequivocal *disconfirmation*” in natural sciences, see page above). This is what Treitler quotes, about ‘detachment’ (or the pursue of ‘objective knowledge’):

I start by rejecting the ideal of scientific detachment. In the exact sciences, this false ideal is perhaps harmless, for it is in fact disregarded there by scientists. But we shall see that it exercises a destructive influence in biology, psychology, and sociology, and falsifies our whole outlook far beyond the domain of science.⁴⁵

The harm that the “false ideal” inflicts on non-exact scientific activities, the one Polanyi is referring to, is what happened when those activities, including musicology, *aimed* at it, in what Treitler himself called “the scientism to which the humanities and social sciences aspired since the 19th century.”⁴⁶ The ideal has been revised, but only partially, ‘in the wrong way,’ so to speak. Most of us—with Meyer—still believe that detachment is a marker of natural science. Many—with Tomlinson—argue from that belief. In some cases, the consequences of this partial revision are extreme.⁴⁷

⁴⁴Treitler, *Historiography*, p. 376. Section II (pp. 353–6) of Tomlinson’s *Web of Culture* is a critique of objective knowledge, with quotations from Treitler: “Knowing is an active process of assimilation that incorporates an act of appraisal,” “Theory—seen as interpretive patterns of structures—is in effect a screen between the knower and the things known.” The quotations lack a precise citation, so I have not been able to pinpoint them within Treitler’s dense and extensive output. Cf. also Leo Treitler, “Homer and Gregory: The Transmission of Epic Poetry and Chant,” *JAMS* 40 (1970), which addresses the same kind of issue about memorization, described after Bartlett as process of construction, not reproduction” (p. 345). Interestingly, Arthur Koestler makes exactly the same point, also drawing on Bartlett, in Chapter X, “Perception and Memory,” of his *The Act of Creation*.

⁴⁵Polanyi, *Op. Cit.*, p. vii, quoted by Leo Treitler, “On Historical Criticism,” *Musical Quarterly* 53 (1969), footnote to page 190.

⁴⁶*Idem.*

⁴⁷For example, Lawrence Kramer’s much-maligned writings, and, more to the point, that strange book of Tomlinson

The belief is in fact so pervasive that even in Treitler himself one finds incipient and subtle manifestations of an un-skeptical use of the word ‘science:’ right after one of his descriptions of teleological views of history already quoted above (page 16), he goes on to say “[h]istory that laid claim to causal explanation as the motor of its narrative progression could claim to be scientific. . . [It] is a paradigm for music history that I shall call ‘scientific,’ for the reasons just given.”⁴⁸ Treitler here runs the risk of implying, consciously or not, a relationship closer to identity than it can be tenable between positivism, as seen in teleology and causation, and science. (That the danger is perfectly kept under control and thus results completely harmless for his arguments, of course, I willfully grant.)



THUS WE HAVE SEEN HOW an association is ‘easily (and usually)’ made between ‘positivism,’ ‘old musicology,’ and ‘science,’ and how this association is possible only through the misconception and distortion of all three terms. The issues implied in this way of putting things, the (mis)construction of identity and difference, are one of the most fruitful fields of study of ‘post-modern musicology.’ In fact, this is yet another piece of essentialization, arbitrary association (‘monolithization’), exoticization, and the rest of those fashionable words. The interesting thing is that in the particular case of musicology itself, the whole point has passed unnoticed. And this is not a unique case of something of the sort (cf. Castelvechi’s brilliant note that “apparently the stigma [against evolutionary approaches] does not apply to the history of musicology, which can still be viewed in evolutionary terms [as in the label ‘New Musicology’]”).⁴⁹

At any rate, this points to an initially satisfactory explanation for the uncritical acceptance of the “image of science” by many of our best musicological minds. Only in other constructions of

on magic (*Music in Renaissance Magic: Toward a Historiography of Others* (Chicago and London: University of Chicago Press, 1993)). See Taruskin’s excellent review to the latter (*Op. Cit.*).

⁴⁸Treitler, *Historiography*, p. 362.

⁴⁹Stefano Castelvechi, “Statement,” in David Greer et al. (ed.), *Musicology and its Sister Disciplines: Past, Present, Future: Proceedings of the 16th International Congress of the International Musicological Society, London 1997* (New York: Oxford University Press, 2000), p. 186.

difference have so absurd and untenable misconceptions been embraced with so general a consensus. ‘Old Musicology,’ ‘Science’ and ‘Positivism,’ as the Other to-be-constructed, had to be perceived, whatever the reality was, as rigid, stubborn, dogmatic, empiric, detached,. . . (note that, for a change, in this case it is the Other, and not the Self, what is conceived of in those, ‘masculine’ if you want, terms). These negotiations were necessary for the reaction—the construction of the Self—to take place. (Whether the reaction itself was necessary is not all clear; with Castelvechi, in Taruskin and Treitler I read *good*, not ‘new’ or ‘old,’ musicology.)

On the same light, it now makes sense the seemingly striking fact that, from philosophy of science, only the name of Kuhn (with his ‘anti-positivist’ neglect of truth) has made it to ‘humanities.’ Not only the name is sometimes mentioned directly, but the expressions ‘paradigm’ and ‘paradigm shift’ are of course part of the generic vocabulary of today’s ‘humanist’ writing. Other people, other theories of philosophy of science—well, those are scientific, positivistic, so that ‘post-modern’ cultural studies should better not mix with them.

To conclude, I want to come back to the case of Meyer, where things are more interesting and complicated because of his further separation of ‘humanities’ from the bicephalous creature of science vs. art. Presumably, Meyer’s studies of music from very positivistic (which is not to say wrong, or old) psychological and cognitive grounds required ‘re-validation,’ against the wave of ‘post-modern’ skepticism and stigmatization. He needed not only a science to represent an Other, but also a non-excluded third that somehow validates his positivistic views. “Concerning the Sciences, the Arts—AND the Humanities,” then, is understandably not the product of a critical and self-controlled examination (‘objective,’ ‘detached’), but the justification and rationalization, sometimes not very rational, of deep premises of thought. Whatever the reality had been about sciences, arts, and humanities, he would presumably have still arrived at the same conclusion: essential and consequential distinction.

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