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Getting "Wrong" Right

COURTNEY ROBY
Cornell University

Daryn Lehoux begins *What did the Romans Know? An Inquiry into Science and Worldmaking* (2012) with the broadest of questions: "What's in a world?" In the chapters that follow he examines the spectrum of answers constructed by a selection of Roman authors, analyzing how the differences between their answers (and between theirs and ours) might be predicated on different ways of seeing the world in front of us and different systems of "facts" constructed from what we see. He devotes particular attention to Roman accounts of the natural world which have typically been ignored or quickly dismissed as "wrong" (including astrology, divination, and the devastating effects of garlic on magnets), making these "errors" yield up broader epistemological truths.

The reader is immediately warned against taking for granted too much uniformity between "scientific" cultures, and particularly against assuming a one-to-one correspondence between an observer's truth and things that are actually present in the world. This search for difference, for Lehoux, lies at the heart of the enterprise of history of science: "we could have done it differently. Indeed, on one way of looking at it, the history of the sciences is virtually a catalogue of different ways of doing it…" (Lehoux 2012, 1) Estrangement of Roman thought, and of ancient science more generally, is a crucial part of Lehoux's strategy for allowing his reader to see Roman scientific thought through new eyes.

Each chapter is structured around a particular problem and an exemplary text or two: Seneca's *Natural Questions* informs a discussion of the extent to which a judicial system which relied on rhetoric influenced the exploration of nature in the early Empire; Ptolemy's *Tetrabiblos* and Manilius's *Astronomica* yield a spectrum of possible models underlying Roman beliefs in astrology; and so on. The final two chapters are completely dedicated to the problems a "realist" standpoint creates for the history of science, and to alternative theories of truth and their explanatory power for both modern problems in scientific epistemology and the now-familiar Roman problems explored elsewhere in the book. Where possible, Lehoux links the discourse in the ancient texts to persistent puzzles in epistemology and philosophy of science: the importance of witnessing, the validity of analogical models, the requirements for "laws" in nature, and so forth.

What Did the Romans Know? is not exactly a book about the history of Roman science, not even if this history is defined broadly in sociological and material terms as well as those of more traditional intellectual history. It is rather about the philosophical, linguistic, and rhetorical terms of the discourse of epistemology as it evolved around a series of particular problems. The book is grounded not so much in Roman social *realia* or scientific or textual practices, as in a set of philosophical "worldviews" which differ more or less radically from our own, though they operate on familiar objects (which Lehoux exemplifies at the book's end as "walls and eyeballs,

stars, nerves, paintings, and perfume” (Lehoux 2012, 232)). In this sense it is a very different work from, for example, Serafina Cuomo’s *Technology and Culture in Greek and Roman Antiquity* (2007), though the two books bear a superficial resemblance in their organization into chapters treating a series of disparate scientific or technological topics from the ancient world using a series of disparate analytical frameworks.

Multiple-parameter variation of this kind allows more ground to be covered than could ever be managed through a more methodical approach. The danger, of course, is that the reader will fill in the blanks incorrectly, left without guidance as to how applicable the historical or philosophical conclusions of one chapter are to the different social or intellectual contexts of the others. Lehoux’s treatment sidesteps this danger for the most part through cross-references in the chapters that establish continuity between their various problems and methodologies. For example, he returns several times to examine why the curious belief that garlic inhibits the power of magnets turned out to have such longevity. The later chapters also devote progressively more space to methodological and broad-based epistemological considerations, picking up threads from earlier chapters to illustrate the breadth of conclusions where possible.

Particularly eloquent on these matters is the fifth chapter, on “The Embeddedness of Seeing.” In this chapter Lehoux addresses the familiar and deceptively tricky mirror-reversal problem: why does a mirror reverse left and right, but not up and down? The chapter’s aim is not to provide an explanation for this problem (though Lehoux does this as well), but to address something more interesting: the apparent behavior of objects in a mirror simply does not comprise a problem for Ptolemy in the same way as it does for us. Lehoux accounts for this by saying that the mirror-reversal “paradox” can be viewed as a product of a modern division of the study of sight into its physical-optical, physiological, and psychological components. Ptolemy’s optics was a different kind of science, in which these components were more unified, and as a result “Ptolemy does not have the same blind spots as I do” (Lehoux 2012, 108).

Discussion of Ptolemy’s account of seeing (and later of Galen’s) gives way to a broader problem on different “epistemologies of seeing” (Lehoux 2012, 125). Lehoux observes claims by Skeptical philosophers, especially Pyrrhonians, on the dubious nature of many seen things, particularly given the idea that different people (of different constitutions, etc.) may see things differently, just as humans and dogs may. In the course of this discussion the elegant efficiency of the book’s argument becomes particularly clear. This problem mirrors the challenge of the book as a whole: we and the Romans may see things differently, so we ought not to read our own experiences too much into these debates, striving instead to understand these debates on their own terms. While each of the problems starring in the individual chapters drive this point home to some extent, here the value of the exemplary ancient problems in motivating a coherent, generalized epistemological investigation becomes especially plain.

In particular, Lehoux argues that the view that “people believed things – nay, even *saw* things – because they were true, because they were really out there in the world to be seen” (Lehoux 2012, 14) is itself problematic. Taxonomy is very important to Lehoux’s answer to what turns out to be the book’s central question (which he draws from Bas van Fraassen): “Is there any

rational way I could come to entertain, seriously, the belief that things are some way that I now classify as absurd?" (in Lehoux 2012, 152; van Fraassen 2002, 73). The shift in plausibility of the garlic-magnet hypothesis, for example, is fundamentally predicated on a shift in categorization: "magnets used to be the kind of thing that was sympathetic, as was garlic. Now magnets are the kind of thing that are magnetic, and garlic in our experience is not" (Lehoux 2012, 151). These categories are not simply labeled drawers for the things the world is made of; taxonomy brings with it a whole theoretical infrastructure explaining how the things in a given category are related to each other.

This infrastructure, in turn, is generally the product of induction, and this has typically been enough reason for modern philosophers of science committed to the "realism" of laws and theories to reject these approaches to understanding the world as quaintly wrong-headed. In turn, this frequently results in the disqualification of ancient investigations into nature from membership in the class of "scientific" practices in the proper sense of the word (on this, see the excellent discussion of "laws of nature" in the third chapter). The alternative has traditionally been a cultural relativism which makes room for such beliefs at the cost of intelligibility: scrupulously avoiding interpreting Roman "scientific" thought according to the rules of modern scientific practice is good as far as it goes, but what means does it give us for understanding the practices of a culture so remote from our own?

Lehoux takes a third path, which he labels "coherence": he asks not how well Roman science conforms to science as it happens to be practiced generally today, but how well Roman descriptions of objects in the world and the relationships between them cohere with one another. The criterion of truth that goes along with this approach is that "we count as true any propositions that have passed the best tests we can think of to throw at them" (Lehoux 2012, 243); Lehoux identifies this as a "pragmatic" theory of truth. Pragmatic it certainly is: the tests of truth can (and do) change with time and practitioners, and the focus on what "we believe" is true allows for a greater breadth of discourse than the realist "what is true." Is the pragmatic theory satisfying, as a structure that might be generally applied to ancient scientific practices other than those described here? Lehoux makes a very good account of it, and of the need to "read" theories into the world (Lehoux 2012, 239) rather than reading them "back" to axiomatic foundational claims. At the same time, the scope of the book quite reasonably does not allow for a full discussion of the justifications for such a theory, which is always at risk of slipping back toward relativism if the criteria for testing and believing are not moored quite tightly.

In rejecting the received wisdom of a 20th-century philosophy of science which strives at all costs to fit scientific theories into a deductive model, Lehoux's approach can be constructively juxtaposed with that of Rom Harré's *The Principles of Scientific Thinking* (1970). This work critiques the "deductivist" view of philosophy of science, in which logical order is taken to match up with the natural order of phenomena being studied, and mathematics to be an approachable ideal for scientific knowledge of all kinds. Harré identifies several problems with this approach, beginning with the assertion that modern scientific theories truly have the degree of deductive rigor attributed to them by those who subscribe to the deductivist view. He begins

his critique with a question strikingly similar to Lehoux’s motivating query: “Of what nature is the world scientists attempt to describe and understand?” (Harré 1970, 10). While Harré would later be associated with a kind of scientific realism, and might therefore appear at first glance to be at odds with Lehoux, he went on to distinguish a variety of types of realism, arguing in an essay in Derksen’s *The Scientific Realism of Rom Harré* (1994) for a realism that depends on inductive arguments working over type-hierarchies in order to create models. The seeds of this version of realism are already present in *The Principles of Scientific Thinking* (1970), and it is those aspects which can be most usefully compared with Lehoux’s approach.

Theories tell us why *patterns* of phenomena are the way they are. In order to do so, a theory often has to provide us knowledge we do not previously have; it does so by providing a model for the unknown. This model is patterned after objects and processes that are already known, and at a rough approximation is hypothesized to work as an analogue for the real, unknown, mechanism. The model is absolutely central to Harré’s epistemology, while a system of deductive laws is what he refers to as “a desirable heuristic device” (Harré 1970, 2). This is an inversion of the “deductivist” view, in which models are at best secondary to a system of formal logic which is taken to reflect the rules governing the phenomena themselves. In practice, Harré argues, constructing such a model requires tools other than the logic of statements: “If this logic is used as the exclusive tool of analysis, it imposes an unnatural structure upon theory” (Harré 1970, 42). The sentential explanans cannot stand on its own, as the deductivist view suggests: it requires additional information in order to refer to new objects (or processes) rather than familiar givens, and so analogy is required to make the statement meaningful.

Harré’s rich descriptions of these analogical processes provides useful background to Lehoux’s appeal to building categories based on perceived likenesses as a tool for generating theories about the natural world, and in particular the questions Lehoux raises about the degree of difference between ancient and modern scientific practices and standards. Taxonomy is extremely important to the criteria of truth Harré develops, particularly in his articulation of the “transforms” that link the expression of a model with that of the phenomena it represents. For example, Harré describes the discovery that salt crystals are lattices of sodium and chlorine ions as a “modal transform” between the shape of the crystal and its internal lattice structure, a description likely to prompt classification of salt crystals with other crystals of a similar lattice structure. He contrasts this description of salt, labeled as “sodium chloride,” with one that might arise in a domestic context, where it might be classified alongside peppercorns, bay leaves, and parsley. Depending on the disciplinary context, Harré argues, "the taxonomic weight cannot so easily or so plausibly be placed upon the technically most advanced concepts" (Harré 1970, 54). Even in the practice of modern science, then, taxonomy is not a simple matter. Harré opposes the “positivist myth of infinite arbitrariness of classification” to the “realist” stance (further specified in Harré 1994 as “entity realism”) in which things and substances have real differences in their constitutions which are manifested in external characteristics, and works to find ground between these two extreme stances which seems to correspond to the realities of scientific practice and discourse.

Ultimately Harré subsumes the criteria of truth that fall under the categories of correspondence, coherence, and pragmatic success under a single larger criterion which he argues is met by all of them. This overarching criterion requires that the sentence used to make a true statement "is constructed in such a way and of such elements as are conventionally used to locate and individuate something where the subject of the statement actually is, and to ascribe to it a property which it actually has" (Harré 1970, 189). It is not my purpose here to address the validity of this claim with regard to Lehoux's definition of "coherence" and "pragmatic truth," but rather to note that the interested reader might use Harré as a guide in order to put them in a more detailed context. Harré later "type-based inductive argument for scientific realism" (Harré 1994, 8) invokes a form of "realism" which explicitly recognizes the model-building value of observations made within a certain hierarchy of types.

Harré also challenges the view that the "vehicles of thought" must necessarily be sentential statements, or indeed any kind of linguistic structures, and the concomitant belief that statements expressed this way have a particular logical validity in explaining phenomena in the world. He proposes instead that scientists in fact work with a "complex" of vehicles of thought which include linguistic formulations as well as visualizations, in all cases focusing on these vehicles as thought processes rather than reflections of outward phenomena, and on their outward manifestations, such as pictures or speech (uttered or written), as manifestations of internal thought processes and not of the phenomena themselves. The model, again, is central here: the model stands in for the natural mechanism (known or unknown); it may be expressed in logically linked sentences, but those sentences are themselves neither the model nor a direct reflection of the rules underlying the phenomena the model describes. The flexibility Harré's system allows in defining the tools of scientific thought harmonizes productively with Lehoux's call for a broader range of patterns of thought and speech to be classified as tools seen *by their practitioners* as appropriate for investigation of the natural world.

The complementarity of these two approaches is made particularly clear in their treatments of "laws of nature." Lehoux's approach rejects the stipulation that using a word that can be translated into English as "law" to refer to the mechanism behind some natural phenomenon is either necessary or sufficient for "laws of nature" in a particular worldview to be acknowledged. Instead, the criterion ought to be that "lawlike statements" (Lehoux 2012, 74) are made about a given natural phenomenon. Lehoux quite rightly calls attention to the importance and difficulty of defining "lawlike" in this context. Ultimately, the examples that he cites as successful in this vein are "intelligible, measurable, and predictable" (Lehoux 2012, 70), thus far matching the requirements stipulated by Jane Ruby (Ruby 1986, 350).

Lehoux suggest that a "lawlike" statement may further be portrayed as operating on mathematically-framed objects (e.g. "circular epicyclic and circular deferential" (Lehoux 2012, 72)) and generating mathematical relationships (e.g. the apparent planetary station that occurs at a given point), without incurring any objection from Lehoux that it interferes with its explanatory power for real objects in the natural world. However, his conception of the "lawlike" also allows the mechanism to be divine, thus permitting discussion of the "laws" perceived to govern

practices like astrology and divination. Lehoux makes a strong argument for linking discourse about these practices with that about practices which correspond more closely to those currently classified as scientific. This argument is predicated at least in part on the acknowledgement that even among the disciplines now uncontroversially counted as sciences there are significant differences in what it means to say phenomena obey “laws of nature.” Lehoux uses biology and physics to stake out this spectrum, motivating this problematization with the famous dismissal (possibly Rutherford’s) of sciences other than physics as “stamp collecting” (Lehoux 2012, 75).

These disagreements suggest more than anything else that there is work to be done on the modern side as well in defining what “laws of nature” mean for different scientific practices, and here too Harré’s analysis may prove useful. He emphasizes the importance of understanding the mechanism behind a process in valorizing induction-based “laws of nature” behind it. Laws, in this view, are causal laws, defined by their “universality,” “necessity” (which emerges from the generative mechanism), and “assent” (that is, generative power must be justified by reference to a generative mechanism). We recognize patterns in nature (like cubical salt crystals) and explain them in terms of what Harré calls the “powers” of the objects involved (like those of the sodium and chloride ions which make them bond in certain shapes). “Laws of nature” are then the statements that describe these patterns; at some level they involve the “generative mechanisms” (Harré 1970, 125) which result in phenomena in a particular state. Such a law (which may or may not be strictly verbal) must provide an account of a causal mechanism, or be demoted to a “protolaw.” These criteria, like those Lehoux proposes, are more inclusive than restrictive, and hence allow for discourse both about the exact sciences and other possibly scientific activities (e.g., evolutionary biology, psychology, astrology). Harré makes no requirement of mathematical expression, or even of exclusively symbolic representation; the focus is on accounting for some causal mechanism, whose nature is not precisely stipulated.

Like Harré, Lehoux focuses on continuities of content rather than terminological coincidence. His approach to the question of whether “laws of nature” predate the early modern period, for example, rejects as invalid the grounds for Ruby’s assertion that nothing equivalent to a “law of nature” existed for medieval Islamic science. Ruby bases this claim on the fact that “Arabists tell me there is no word corresponding to ‘law’ in Arabic” (Ruby 1986, 344 n. 19). Lehoux eloquently refutes this by arguing that “If the history of the *Frisbee* cannot be pushed any farther back than the changing of its name from *Pluto Platter*, then we are talking about the history of a word, not of a thing” (Lehoux 2012, 68 n. 57). Neither Lehoux nor Harré denies the importance of linguistic structures for formalizing and propagating scientific knowledge, but both are insistent on the need to drill beneath the surface of such structures in search of the conceptual frameworks which underlie them. Both authors find problematic the requirement that in order to qualify as “science,” investigations into phenomena must be expressible as sentences which conform to deductive logic. Lehoux objects because this view disqualifies a great deal of ancient investigation into nature which seems otherwise to map fairly well onto activities we usually think of as “scientific,” while Harré argues that if one is scrupulously attentive to scientific practice, it disqualifies a fair amount of what is labeled “science” today as well.

Both views likewise call into question what is meant by a scientific explanation. Lehoux's approach is modeled on Philip Kitcher's definition of an explanation as a member of a set of sentences which "collectively provide the best systematization of our beliefs" (Lehoux 2012, 180; Kitcher and Salmon 1989, 430). Harré (1994) attaches a comparable measure of "ontological plausibility" to the success of a process of induction over theories or models, only then judging a theory's degree of empirical success. Harré (1970) claims in a more detailed argument that an explanation is just part of the way a theory explains phenomena; the theory is based on a model and consists of sentences linked to the model by "transforms," as in the case of sodium chloride above. He emphasizes in particular that the deductivist view claims a symmetry between explanation and prediction which simply is not there. In neither case is a successful explanation defined as the end product of a chain of deductive reasoning performed on sentences, which happens to mirror a causal system present in the phenomena under investigation. Indeed, both authors problematize the equation of predictive success and explanatory power – Harré on the grounds that there are cases in modern or recent science in which one but not both is found, Lehoux on the grounds that any theory succeeds at prediction until it doesn't, and that the predictive successes of modern science are measured on the order of decades at best, which looks rather paltry by the standards of ancient science.

The eclectic approach of Lehoux's project is justified, at least in part, by his assertion that "the Romans were of course not looking for a comprehensive theory of explanation itself – that question is very much a product of twentieth-century philosophical concerns" (Lehoux 2012, 180). Lehoux does not insist that the diverse scientific problems and intellectual contexts he invokes throughout the book add up to a comprehensive and uniform entity called "Roman science," whose players all follow the same rules – seeking to discover and describe those rules would be a quixotic enterprise, to say the least. Instead, he provides a series of well-chosen examples – the very stuff of which philosophy is made, as he observes (Lehoux 2012, 87) – and uses them to carve out a space dedicated to the consideration of complex questions about the complex enterprise of scientific thinking in the Roman world.

The resulting work is not (and does not claim to be) the last word on the questions it raises; it is rather an incitement to further work on them and the new questions they raise in turn. This epistemologically sophisticated interrogation of Roman "scientific" activities represents an exciting opportunity for a new beginning in the dialogue between philosophy of science and the history of scientific practices in the ancient world. Works like Harré's, which foreground the role of taxonomy in scientific model-building practices, provide some useful auxiliary tools for investigating the fascinating question of what "wrong" can tell us.

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