

*FOCUS: NATURAL PHILOSOPHY  
AND INSTRUMENTALITY*

What Is the History of Science  
the History *Of*?

Early Modern Roots of the Ideology  
of Modern Science

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ABSTRACT

The mismatch between common representations of “science” and the miscellany of materials typically studied by the historian of science is traced to a systematic ambiguity that may itself be traced to early modern Europe. In that cultural setting, natural philosophy came to be rearticulated (most famously by Francis Bacon) as involving both contemplative and practical knowledge. The resulting tension and ambiguity are illustrated by the eighteenth-century views of Buffon. In the nineteenth century, a new enterprise called “science” represents the establishment of an unstable ideology of natural knowledge that was heavily indebted to those early modern developments. The two complementary and competing elements of the ideology of modern science are accordingly described as “natural philosophy” (a discourse of contemplative knowledge) and “instrumentality” (a discourse of practical or useful knowledge; know-how). The history of science in large part concerns the story of their shifting, often mutually denying, interrelations.

THE HISTORICAL CONTINGENCY OF “SCIENCE”

THE QUESTION IN MY TITLE ARISES from an anxiety that the history of science as a scholarly specialty is less obviously self-defining than it once was. This essay

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therefore concerns itself with various aspects of the field in order to clarify what does or does not qualify the history of science as a legitimate area of scholarly specialty.

The overriding reason for asking such a question is that, especially for those who work on the premodern period, it has become increasingly apparent over the last couple of decades that identifying some theme or topic as a part of the history of *science* is less straightforward than might once have seemed the case. For example, Isaac Newton's activities included work in various mathematical sciences, in theology and biblical chronology, in alchemy, in parliamentary politics, and in running the Royal Mint. As arguments for the imperviousness of the boundaries between those areas of activity have become increasingly less convincing to many historians, a corresponding tendency has arisen to incorporate them into broader and more complete accounts of Newton and the meaning of what he did. That tendency, and similar ones in other topical areas, has made the history of science, especially for early modern Europe, resemble other kinds of sociocultural history. As our history has in many respects become better, it has also become less identifiable as being specifically the "history of science."

The problem has been compounded by other areas of work in science studies that call into question the whole idea of there even being something—a natural kind in the world—that corresponds to the label "science" at all. Historians have responded to these doubts by embracing the notion, together with its research agenda, of studying "naturalistically" ideas, practices, and institutions that have, through contingent historical processes, come to be established as what people usually mean when they speak of "science" and "scientific knowledge."<sup>1</sup> Rather than studying the history of something that we always knew in advance how to identify, historians of science have turned more and more to studying how that "something" itself has been formed as a historical object, with no permanent, transcendental identity. Everything thus becomes historicized and contingent, and the days of the history of science as an apologetic or celebratory enterprise serving present-day science have become, so to speak, a thing of the past.

So the very category "science" has become historicized—and hence very slippery. The argument for having an academic specialty called "the history of science" becomes increasingly a pragmatic, locally situated one, rather than the expression of a particular scholarly enterprise that takes its special character from the peculiar properties of its subject matter. Long gone are the days when George Sarton could say that "scientific activity is the only one which is obviously and undoubtedly cumulative and progressive," and his characterization of science as being marked by a series of discrete "discoveries" is liable to sound quaint to our ears.<sup>2</sup> What, then, has happened to the scholarly specialty that Sarton attempted so tirelessly to promote, a history of science that stood at the center of what he called "the new humanism"? Should we, in fact, throw in the towel and admit that there is no specifiable kind of activity called science for which a continuous and identifiable history can be investigated?

There seem to be several resources that can be used to defend the integrity of what historians of science, broadly speaking, are engaged in. First of all, the history of science is not at all unique in facing the kinds of conceptual problems I have noted. The field of

<sup>1</sup> The term "naturalistic" has been used by philosophers of science for the past quarter-century or more to describe a concern with studying science as it is actually practiced rather than as an epistemological ideal. For a survey of such approaches see, e.g., Werner Callebaut, *Taking the Naturalistic Turn; or, How Real Philosophy of Science Is Done* (Chicago: Univ. Chicago Press, 1993).

<sup>2</sup> George Sarton, *The History of Science and the New Humanism* (New York: Braziller, 1956), p. 10.

comparative religion has to deal with the embarrassing fact that its specialists have come increasingly to recognize that there is no helpful demarcation criterion to indicate what should count as a religion and what should not, any more than there is one to distinguish clearly between science and nonscience.<sup>3</sup> Art historians have long since ceased trying to define what “art” is; they simply do what they do—although they also turn more and more into social and cultural historians as they do it.<sup>4</sup>

One historical specialty that seems to be immune to such problems is political history. Political historians, no matter what part of the world or what period they study, seldom have relativistic nightmares about their subject matter. All they really seem to need is some kind of central control of a region’s people, and then they can look at how that control is realized, mediated, or offset by other competing or subsidiary structures. Perhaps their lack of worry about the nature of “politics” stems from a conviction that they know a state when they see one.<sup>5</sup> Be that as it may, we historians of science are by contrast very self-conscious about our analogous conceptual difficulties. The former close alliance between the history and the philosophy of science may help to account for this, but to the extent that the history of science has always engaged in creating knowledge about knowledge creation, it probably would have invited self-reflection anyway.

This is a particularly acute issue for historians of early modern science—as well as of non-Western science. Early modernists have learned to be very careful about the terms they use to describe the early modern enterprises they talk about. The first step, which seems to have been widely adopted, was to start speaking about “natural philosophy” instead of “science.” That was a useful stopgap, but it most often amounted to people saying “natural philosophy” but thinking “science” and then proceeding exactly as before. Several of the enterprises that we now call “science” were not in fact seen in early modern Europe as “natural philosophy” at all: some were “mathematics,” some were “natural history,” and so on.<sup>6</sup>

In practice, early modernists in the history of science do, chiefly, one of two things (or else a mixture of the two). The first is the study of the history of early modern cultural enterprises that have their own integrity, independent of the modern category called “science”; this involves engaging in a form of historical anthropology, of the kind practiced by many cultural historians of early modern Europe. The second enterprise pursues the history of possible antecedents of modern science—a much trickier approach and one that historians of science often try to avoid acknowledging, since we were all brought up to regard with horror something rather imprecisely called “whig history.”<sup>7</sup>

<sup>3</sup> Most studies of religion in the history of science have focused on Christianity alone. For an attempt at a slightly broader canvas see John Hedley Brooke, Margaret J. Osler, and Jitse M. van de Meer, eds., *Science in Theistic Contexts: Cognitive Dimensions* (Osiris, N.S., 2001, 16).

<sup>4</sup> Svetlana Alpers and Michael Baxandall, trailblazers in this area, have both been notably important for historians of science.

<sup>5</sup> A recent collection of essays that attempts to develop new approaches to political theory with the aid of ideas from science studies is Sheila Jasanoff, ed., *States of Knowledge: The Co-Production of Science and Social Order* (London: Routledge, 2004). Important precedents include Yaron Ezrahi, *The Descent of Icarus: Science and the Transformation of Contemporary Democracy* (Cambridge, Mass.: Harvard Univ. Press, 1990); and James C. Scott, *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed* (New Haven, Conn.: Yale Univ. Press, 1998).

<sup>6</sup> For a discussion of such issues see Margaret J. Osler, “The Canonical Imperative: Rethinking the Scientific Revolution,” in *Rethinking the Scientific Revolution*, ed. Osler (Cambridge: Cambridge Univ. Press, 2000), pp. 3–22.

<sup>7</sup> The term owes its origin to Herbert Butterfield, *The Whig Interpretation of History* (London: Bell, 1931). One of the most thoughtful discussions of the themes that it spawned is Adrian Wilson and T. G. Ashplant,

Nonetheless, the integrity of the history of science as a distinct field of scholarship might indeed lie in this enterprise of understanding the antecedents to, as well as the ongoing development of, modern science. In order to do that without falling into anachronism or teleological explanation, however, we must be mindful of some necessary tasks. There must be an unambiguous idea of what precisely it is about modern science that we wish to understand historically—especially for periods before the nineteenth century, when there really was no such thing as modern science according to the usual accepted historicist criteria. There must also be a recognition of the nature of those antecedents themselves, so as to ensure that superficial similarities to later developments are not taken as necessarily genetically related to them when they easily might not be.

A particular set of themes that lends itself quite well to such an enterprise develops out of my own interest in early modern materials. These themes, in part, concern natural philosophy as a presumed antecedent to modern science.

#### THEORY AND PRACTICE IN EARLY MODERN EUROPE

A notable feature of early modern taxonomies of knowledge is the pairing, both medieval and early modern, of the terms *theorica* and *practica*. This pairing, as a unit, applied to various areas of knowledge, but especially to medicine, astronomy, and music together with the other designated mathematical sciences (including fencing); it was also sometimes used for alchemy and for various other areas falling under the heading of “natural magic.” But the pairing seems not to have been applied to natural philosophy, even though philosophy as a whole contained a so-called practical part that encompassed ethics and politics. According to the standard usage, *theorica* concerned the technical apparatus of the science or art in question, such as how to compute planetary orbits in astronomy, or compound ratios in music theory, or talk about the humors in medicine; whereas *practica* was the part of the discipline that used that technical apparatus to achieve certain purposeful goals, such as casting horoscopes or calculating calendrical questions in astronomy, composing polyphony in music, or applying a cure in medicine.<sup>8</sup>

The fact that the pairing usually found no application in natural philosophy thus underlines a crucial point: traditionally, up to the early seventeenth century, the early modern category of “natural philosophy,” inherited from the medieval university legacy of predominantly Aristotelian philosophy, had referred to a specifically contemplative endeavor.

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“Whig History and Present-Centred History,” *Historical Journal*, 1988, 31:1–16; paired with Ashplant and Wilson, “Present-Centred History and the Problem of Historical Knowledge,” *ibid.*, pp. 253–274.

<sup>8</sup> The clearest way to get a sense of this overall point is to look at the use of the two words *theorica* and *practica* in medieval and Renaissance book titles. Robert S. Westman’s ongoing work considers this theme in astronomy, where the use of *theorica* is much better known by scholars than that of *practica*: see Westman, “The Literature of the Heavens and the Science of the Stars: Roots of an Early Modern Classification,” paper presented at the conference “Wrestling with Nature: From Omens to Science,” Madison, Wisconsin, Apr. 2001, esp. sect. 10, which discusses the dual categories as a feature of numerous disciplines. For an overview of the theme in music see Thomas Christensen, “Introduction,” in *The Cambridge History of Western Music Theory*, ed. Christensen (Cambridge: Cambridge Univ. Press, 2002), pp. 1–23. In alchemy, the notable example of Paul of Tarento’s work *Theorica et practica* is discussed in William R. Newman, *Promethean Ambitions: Alchemy and the Quest to Perfect Nature* (Chicago: Univ. Chicago Press, 2004), pp. 69–72; and Newman, “Technology and Alchemical Debate in the Late Middle Ages,” *Isis*, 1989, 80:423–445. In medicine, the *theorica/practica* division lasted well into the eighteenth century, and Thomas H. Broman argues that its echoes played an important role in shaping the emergence of a modern medical profession: see Broman, “Rethinking Professionalization: Theory, Practice, and Professional Ideology in Eighteenth-Century German Medicine,” *Journal of Modern History*, 1995, 67:835–872; and Broman, “Conclusion,” in *The Transformation of German Academic Medicine, 1750–1820* (Cambridge: Cambridge Univ. Press, 1996), pp. 193–202.

The natural philosophy that Francis Bacon and others complained about in the seventeenth century had been effectively defined as a discipline aimed only at understanding the natural world. It was not supposed to be about craft production or the deliberate creation of physical effects—the kinds of practices that counted for an Aristotelian as art, Aristotle's *technē*. Natural philosophy was not, then, generally seen as having a practical part at all; by the same token, neither did it have, strictly speaking, a technical, “theoretical” part, since the two categories were complementary. The content of natural philosophy was essentially and solely speculative because it was about understanding things, not doing things. On the basis of usage, it appears that just about any other branch of knowledge that related to the natural world could in principle be described in the terms of *theorica* and *practica*. Natural philosophy stood out as different simply because it was not conceived as knowledge to be used for practical purposes: as Rudolph Goclenius, the German author of a 1613 philosophical lexicon, puts it, for the Peripatetics philosophy is concerned with the behaviors and properties (*disciplinis et habitibus*) of things, excluding tools (*instrumenta*).<sup>9</sup> Thus Goclenius explicitly excludes practical techniques and their uses; those are not what (natural) philosophy is about, and to think otherwise would be a category mistake.

An associated feature of natural philosophy during the period of the Scientific Revolution is that it carried a certain intellectual prestige that tended to set it above most other areas concerned with natural knowledge. As is nowadays well known from the work of Robert Westman and others, formal disciplinary hierarchies in the universities and colleges put natural philosophy above mathematical sciences such as astronomy.<sup>10</sup> However, it is also the case that medicine stood apart from both natural philosophy and mathematics, since it ranked as one of the lofty higher faculties in the university, even though, like the mathematical disciplines, it was a subject routinely conceptualized in the terms of *theorica/practica*. Clearly, then, a discipline with avowedly practical dimensions did not necessarily hold lower, artisanal status—although academic physicians, as Vesalius famously claimed, still often disliked getting their hands dirty. The office of the physician evidently had enough prestige in its own right to offset the allure of the pure, intellectual, speculative character of natural philosophy.<sup>11</sup>

These observations set in relief one of the most notable developments of the Scientific Revolution: a restructuring of natural philosophy that turned it, in the learned European world, into a very different kind of enterprise—one where works, as Bacon put it, could act as testimony to philosophical truth and where the production of works was advertised as a major moral justification for natural philosophy. This change came to be expressed in the form of so-called experimental philosophy, and that notion of experimentalism, over time, did some very strange things to natural philosophy.

Bacon's *Advancement of Learning* (1605) was the earliest published expression of his programmatic ideas, and it displays a particularly interesting strategy for handling the issue of natural philosophy's relationship to questions of utility. In defining natural philosophy as a formal branch of learning, Bacon wrote: “These be the two parts of natural philosophy, the inquisition of causes, and the production of effects; speculative, and operative; natural

<sup>9</sup> Rodolfus Goclenius, *Lexicon philosophicum* (Frankfurt, 1613), p. 828, col. 1.

<sup>10</sup> The classic study is Robert S. Westman, “The Astronomer's Role in the Sixteenth Century: A Preliminary Study,” *History of Science*, 1980, 18:105–147.

<sup>11</sup> See the useful reading of Vesalius's preface to *De fabrica* in Andrew Cunningham, *The Anatomical Renaissance: The Resurrection of the Anatomical Project of the Ancients* (Aldershot: Scolar, 1997), pp. 121–124.

science, and natural prudence. For as in civil matters there is a wisdom of discourse, and a wisdom of direction; so is it in natural.”<sup>12</sup>

Bacon thus attempted to represent natural philosophy, quite against its usual academic Aristotelian grain, as necessarily having a practical or utilitarian dimension. As he tried to justify this picture, he proceeded, artfully, to conflate two different things. He began by taking the scholastic notion of analysis and synthesis (also known as resolution and composition, or demonstration *a posteriori* and *a priori*) and explicated it in relation to natural philosophy in a fashion perfectly consistent with the standard late sixteenth-century treatment by Jacopo Zabarella. However, he did so in such a way as to imply that “effects” or phenomena were tantamount to practical *uses* of natural philosophy’s explanatory principles. Thus, shortly after the passage just quoted, he continued:

Now although it be true, and I know it well, that there is an intercourse between causes and effects, so as both these knowledges, speculative and operative, have a great connexion between themselves; yet because all true and fruitful natural philosophy hath a double scale or ladder, ascendent and descendent, ascending from experiments to the invention of causes, and descending from causes to the invention of new experiments; therefore I judge it most requisite that these two parts be severally considered and handled.<sup>13</sup>

The standard view on which Bacon drew was solely concerned with the inferential motion back and forth between causes and effects. It had nothing whatever to do with putting natural phenomena to work; it was concerned only with developing causal explanations for phenomena. Bacon’s attempt at subtly shifting the apparent implications of this *regressus* theory was evidently a part of his general strategy to make his new program for natural knowledge appear to conform as closely as possible to received ideas and ways of doing things: “wheresoever my conception and notion may differ from the ancient, yet I am studious to keep the ancient terms.”<sup>14</sup>

Bacon’s artful conflation of *phenomena* with *uses* served potentially to open up new, different ways of promoting and developing a particular kind of natural philosophy—one that would be judged not on whether it successfully explained aspects of the world but on whether it could produce desired effects on command. Bacon provided labels and categories to create room for such an innovation a little later in the *Advancement of Learning*: “For as we divided natural philosophy in general into the inquiry of causes, and productions of effects: so that part which concerneth the inquiry of causes we do subdivide according to the received and sound division of causes. The one part, which is phisic, inquireth and handleth the material and efficient causes; and the other, which is metaphisic, handleth the formal and final causes.”<sup>15</sup>

<sup>12</sup> Francis Bacon, *The Advancement of Learning*, in Bacon, *The Advancement of Learning and The New Atlantis*, ed. Arthur Johnston (Oxford: Clarendon, 1974), pp. 3–212, on p. 88. On differences between *Advancement* and the later Latin version, *De augmentis scientiarum* (1623), see Sachiko Kusukawa, “Bacon’s Classification of Knowledge,” in *The Cambridge Companion to Bacon*, ed. Markku Peltonen (Cambridge: Cambridge Univ. Press, 1996), pp. 47–74.

<sup>13</sup> Bacon, *Advancement of Learning*, p. 88. On this passage see also Lisa Jardine, *Francis Bacon: Discovery and the Art of Discourse* (Cambridge: Cambridge Univ. Press, 1974), pp. 99–100. A good overview is Nicholas Jardine, “Epistemology of the Sciences,” in *The Cambridge History of Renaissance Philosophy*, ed. Quentin Skinner, Charles Schmitt, Eckhard Kessler, and Jill Kraye (Cambridge: Cambridge Univ. Press, 1988), pp. 685–711.

<sup>14</sup> Bacon, *Advancement of Learning*, p. 88.

<sup>15</sup> *Ibid.*, p. 90.

Bacon went on to reject the appropriateness of final causes for this kind of “metaphysic,” now understood as a subdivision of natural philosophy. Furthermore, formal causes were to be understood solely in terms of Bacon’s own definition and understanding of “forms,” which are, just as in his later work, inverted operational rules for producing those forms.<sup>16</sup> Thus, taking formal causes together with the material and efficient, Bacon’s reformulation of natural philosophy aimed at providing detailed recipes suited for operational use, all without appearing to rock the boat of established understandings of natural philosophy.

Bacon’s attempt to provide a respectable intellectual pedigree for operative knowledge required a passage through novel conceptualizations of the field of knowledge called natural philosophy. His convolutions in trying to invest practical, operational knowledge with the status and legitimacy of natural philosophy, however, indicate how far his was from natural philosophy’s usual profile. Bacon envisaged the superinducing of desired properties onto matter by mechanical means—“mechanical” describing here the sort of operations performed by a “mechanic,” or manual laborer; such superinduced properties were in effect driving out purely natural-philosophical entities—Aristotelian substantial forms were, after all, posited as ways of explaining things, not as ways of doing anything related to mechanical operation.<sup>17</sup> Nonetheless, the subsequent growth of experimentalism in seventeenth-century natural philosophy demonstrated a reluctance, similar to Bacon’s, wholly to abandon the Aristotelian project. That the term “natural philosophy” itself continued to be used indicates that the goal of providing contemplative understanding of the natural world remained a crucial concern. The period’s “mechanical philosophy” was not, after all, actually very useful, despite its being couched in terms of particles of matter being moved around; it was chiefly a means of explaining phenomena. Another well-known theme, physicotheology, provided from the seventeenth century onward a particular sort of understanding rather than a means of manipulating nature. Utility, instrumentality, had become attached to natural philosophy but had not usurped it.

A typical example from the second half of the seventeenth century may be had from Robert Hooke. In the famous “Preface” to *Micrographia* Hooke extols the virtues of instrumental aids to the senses, detailing inventions and various other sorts of practical utility. There are also, however, moments at which he explicitly makes room for philosophical, contemplative knowledge, not least in the description of his very Baconian program as “experimental philosophy”: by eschewing other labels, such as “natural magic” or even (conceivably) “natural mechanics,” Hooke retains the speculative, contemplative, and above all intellectual trappings of a high-status branch of liberal learning, philosophy. At the same time, he describes the program that he lays out as “the Design which the ROYAL SOCIETY has propos’d to it self.” Hooke explains how the society’s great stress on the importance of the senses in the study of nature amounts to a melding of practical purposes with the contemplative goals of natural philosophy:

By this means they find some reason to suspect that those effects of Bodies, which have been commonly attributed to *Qualities*, and those confess’d to be *occult*, are perform’d by the small

<sup>16</sup> *Ibid.*, pp. 91–94; cf. Francis Bacon, *New Organon* (1620), Bk. 2, aphs. 2–4.

<sup>17</sup> On these aspects of Bacon’s thought see Paolo Rossi, *Francis Bacon: From Magic to Science*, trans. Sacha Rabinovitch (Chicago: Univ. Chicago Press, 1968); and Rossi, *Philosophy, Technology, and the Arts in the Early Modern Era*, trans. Salvatore Attanasio (New York: Harper & Row, 1970), esp. App. 2. See also Stephen Gaukroger, *Francis Bacon and the Transformation of Early-Modern Philosophy* (Cambridge: Cambridge Univ. Press, 2001), esp. Ch. 1.

*Machines* of Nature, which are not to be discern'd *without* these helps, seeming the meer products of *Motion, Figure, and Magnitude*. . . . And the ends of all these Inquiries they intend to be the *Pleasure* of Contemplative Minds, but above all, the *ease and dispatch* of the labours of men's hands.<sup>18</sup>

The logical incommensurability between natural philosophy and utility is short-circuited by having natural philosophy speak only in the terms of mechanical tools: the invisible causes of various qualities and effects are taken to be tiny machines, scaled-down versions of those artifacts that facilitate operational, or mechanical, effects on the everyday human scale.

The passage elaborates further on this theme: "They [the Royal Society] do not wholly reject Experiments of meer *light* and *theory*; but they principally aim at such, whose Applications will *improve and facilitate* the present way of *Manual Arts*."<sup>19</sup> Hooke, like Bacon, is trying to construct a way of having natural philosophy and utility as parts of the same enterprise.

#### THE TROUBLE WITH MATHEMATICAL SCIENCES: BUFFON AND THE "SCIENTIFIC ARTS"

European sciences of nature in the seventeenth and eighteenth centuries experienced the development of two mutually supportive, but analytically distinct, enterprises or "discourses." One of them was "natural philosophical," in the sense of its being contemplative and aimed at understanding the natural world; the other was instrumental and was geared toward the production of practical effects, whether to do with moving weights or improving agriculture. In effect, this period saw the establishment of a new enterprise, one that took the old "natural philosophy" and rearticulated it in the new terms of instrumentality: the engagement with the world that, in the nineteenth century, produced modern science was thus born of a discursive hybrid of these analytically unrelated endeavors.<sup>20</sup>

One prominent aspect of the dichotomy in early modern Europe, when it was still visible as such, took the form of the conventional scholarly distinction between mathematics and natural philosophy. The difference between the two was usually expressed in terms of causation: natural philosophy identified the causes of some occurrence or property, prototypically in terms of the four Aristotelian causes; while mathematics—meaning the mathematical sciences in general, including astronomy, optics, mechanics, and many others—referred strictly to quantities, regardless of the kinds of things whose quantities were at issue. Thus, if an object six feet high were called for, whether the object was a six-foot man or a six-foot sunflower was irrelevant; dealing with behaviors or characteristics that were determined by quantities and their interrelations ignored the *natures* of things and was not, therefore, natural philosophy. People such as Johannes Kepler took a different view, of course, and even the dominant conceptualization became confused by the advent in the seventeenth century of such hybrids as "physicomathematics" and by the appearance, late in the century, of Newton's "mathematical principles of natural philoso-

<sup>18</sup> Robert Hooke, *Micrographia; or, Some Physiological Descriptions of Minute Bodies Made by Magnifying Glasses* (London, 1665), "Preface," sig. g1r.

<sup>19</sup> *Ibid.*

<sup>20</sup> An earlier attempt at sketching this argument is Peter Dear, "The Ideology of Modern Science" [essay review of Pamela Long, *Openness, Secrecy, Authorship*], *Studies in History and Philosophy of Science*, 2003, 34A:821–828; the argument also plays a significant role in Dear, *The Intelligibility of Nature* (Chicago: Univ. Chicago Press, in press).



phy.”<sup>21</sup> Nonetheless, the idea that there was a dichotomy, a basic difference in kind, between mathematical sciences and true natural philosophy—where the latter, unlike the former, provided real physical understanding of the world—remained for many people a basic presumption about how natural knowledge should be addressed. For a well-known example, consider Christiaan Huygens’s complaints concerning Newton’s 1687 *Mathematical Principles of Natural Philosophy*: Huygens regarded the book as containing mathematics *rather than* natural philosophy, insofar as it described mathematically the motion of bodies in the solar system on the basis of gravitational behavior but failed to consider, let alone explain, the causes of gravity.<sup>22</sup> This famous response by Huygens, who was certainly no Aristotelian natural philosopher, was founded on a categorical distinction between mathematics and natural philosophy that was still a vital legacy of Aristotelianism itself.

Mathematical disciplines, moreover, as much in the eighteenth as the seventeenth century, were closely allied to practical use, as their traditional representation in terms of *theorica* and *practica* showed. Astronomy served for navigation, among other things; mechanics was closely related to engineering; geometry itself was foundational for surveying practices; fortification and architecture counted as mathematical sciences, and so, too, by the eighteenth century, did gunnery. The distinction between natural philosophy and instrumentality that Bacon and others attempted to erase often appeared, therefore, in the guise of the distinction between natural philosophy and mathematics—a second distinction that was itself often debated and contested.

The sensibilities formed by these dichotomies remained alive well into the eighteenth century and can be illustrated by examining the work of the naturalist and zoologist Georges-Louis Leclerc, later comte de Buffon. In the 1740s, at the beginning of his career, Buffon had just switched to a focus on natural history from an early interest in mathematics. Buffon often represented himself as a staunch follower of Newton; aspects of what he took to be the role of mathematics in Newton’s achievement appeared in his publications in 1749. One, in the *Memoirs* of the Royal Academy of Sciences (inevitably backdated for 1745), involved a defense of Newton against the attempts of the mathematician Alexis Clairaut to solve the long-standing Newtonian problem of accounting precisely for the moon’s orbit. Clairaut had hypothesized that the force of attraction between gravitating bodies might not vary exactly as the inverse square of their distances. Buffon tried to argue in response that the inverse-square law was so conceptually fundamental that it made no sense to try to modify it; one of his basic arguments related specifically to what it meant to apply mathematical reasoning to nature.<sup>23</sup>

Clairaut suggested the addition of an extra term to the mathematical expression for gravitational attraction, one that would itself decrease with distance more rapidly than the basic inverse-square relationship expressed in the first term:

<sup>21</sup> See Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: Univ. Chicago Press, 1995), Chs. 6, 8.

<sup>22</sup> Accounts include Alexandre Koyré, “Huygens and Leibniz on Universal Attraction,” in Koyré, *Newtonian Studies* (Chicago: Univ. Chicago Press, 1965), pp. 115–138, esp. pp. 115–124; and Roberto de A. Martins, “Huygens’s Reaction to Newton’s Gravitational Theory,” in *Renaissance and Revolution: Humanists, Scholars, Craftsmen, and Natural Philosophers in Early Modern Europe*, ed. J. V. Field and Frank A. J. L. James (Cambridge: Cambridge Univ. Press, 1993), pp. 203–213.

<sup>23</sup> See the discussion in Jacques Roger, *Buffon: A Life in Natural History*, trans. Sarah Lucille Bonnefoi (Ithaca, N.Y.: Cornell Univ. Press, 1997), pp. 53–58.

$$F \propto \frac{1}{x^2} + \frac{1}{mx^4}$$

Buffon argued against the admissibility of such a modification to an expression meant to convey a physical law: “Every time the expression of a law is not represented by a single term, the simplicity and unity of the expression . . . no longer remains, and in consequence there is no longer any physical law.”<sup>24</sup> This rather abstract claim, Buffon recognized, was in need of elucidation; he accordingly focused on the concrete issue of *measurement*. A term in a mathematical expression governing some basic kind of physical phenomenon, he explained, must correspond to some specific thing in the world that can be measured: the value of the term will vary as the corresponding measurement varies.

In whatever way we could therefore suppose that a physical quality could vary, since this quality is singular, its variation will be simple and always expressible by a single term, which will be its measure; and as soon as one wants to use two terms, one destroys the unity of the physical quality . . . two terms are indeed two measures, both of them variable, and independently variable . . . if two terms are admitted to represent the effect of a star’s central force, it’s necessary to allow that in place of a single force there are two of them.<sup>25</sup>

Newton himself, Buffon noted, had explicitly acknowledged the possibility that other forces besides inverse-square-law gravity might be involved in determining the moon’s motion.

Buffon’s perspective on mathematics was explicitly constructivist and was quite similar to the arguments put forth a century earlier by Thomas Hobbes, among others. Such views were echoed to a surprising degree by Newton himself: mathematical objects should be understood as things that human beings make; they cannot, as Buffon said, substitute for talking about real things themselves. Lines are generated through the motion of a point, surfaces through the motion of a line, and so on; the creation of mathematical objects in general is explicitly artificial, the work of human artifice.<sup>26</sup>

This view of mathematical objects as categorically separate from the self-sustaining furniture of nature was further clarified in Buffon’s great statement of 1749, the “Premier discours” to Volume 1 of his *Histoire naturelle*. A few years before, in 1744, Buffon had unleashed before the academy his famous attack on Linnaean taxonomy, an attack that implicated everyone involved in the taxonomic enterprise. Buffon argued that the various

<sup>24</sup> Georges-Louis Leclerc, comte de Buffon, “Réflexions sur la loi de l’attraction,” in *Histoire de l’Académie royale des sciences avec les mémoires de mathématique et de physique, année 1745* (Paris, 1749), pp. 493–500, on pp. 497–498: “toutes les fois que l’échelle d’une loi ne sera pas représentée par un seul terme, cette simplicité & cette unité d’échelle . . . ne subsiste plus, & par conséquent il n’y a plus aucune loi physique.” (Here and elsewhere, translations are my own unless otherwise indicated.) Cf. the translation of Buffon’s paper by Phillip R. Sloan in John Lyon and Sloan, eds., *From Natural History to the History of Nature: Readings from Buffon and His Critics* (Notre Dame, Ind.: Univ. Notre Dame Press, 1981), pp. 79–85.

<sup>25</sup> *Ibid.*, p. 498: “De quelque façon que nous puissions donc supposer qu’une qualité physique puisse varier, comme cette qualité est une, sa variation sera simple & toujours exprimable par un seul terme qui en sera la mesure; & dès qu’on voudra employer deux termes, on détruira l’unité de la qualité physique . . . deux termes sont en effet deux mesures, toutes deux variables & inégalement variables . . . si on admet deux termes pour représenter l’effet de la force centrale d’un astre, il est nécessaire d’avouer qu’au lieu d’une force il y en a deux.”

<sup>26</sup> On Hobbes’s mathematics see Douglas M. Jesseph, *Squaring the Circle: The War between Hobbes and Wallis* (Chicago: Univ. Chicago Press, 1999); on Newton’s constructivist mathematics see Dear, *Discipline and Experience* (cit. n. 21), Ch. 8. Buffon sneers at Clairaut that “comme je l’ai dit, on peut tout représenter [i.e., one can represent anything] avec un calcul, & on ne réalise rien”: Buffon, “Réflexions sur la loi de l’attraction” (cit. n. 24), p. 500.

hierarchically organized categories of species, genus, family, and so forth currently lacked any legitimate foundation in observation and clear thinking and that any classificatory system relying on their use was therefore not acceptably philosophical. His aim was to justify natural history as a department of natural philosophy.<sup>27</sup>

In effect, Buffon held natural-historical classification to be meaningless as a form of philosophical understanding; furthermore, he aligned this point with what he saw as contemporary misrepresentations of mathematics, because he held that mathematics too was devoid of true philosophical content:

In this century itself, where the Sciences seem to be carefully cultivated, I believe that it is easy to perceive that Philosophy is neglected, and perhaps more so than in any other century. The arts that people are pleased to call scientific have taken its place; the methods of calculus and geometry, those of botany and natural history, in a word formulas and dictionaries preoccupy almost everyone. People imagine that they know more because of having increased the number of symbolic expressions and learned phrases, and pay no attention to the fact that all these arts are nothing but scaffolding for achieving science, and not science itself.<sup>28</sup>

The “scientific arts,” according to Buffon, were those bodies of technique that were good for calculating and classifying but that did not offer insight into the proper concerns of true natural philosophy—namely, causes and the natures of things. It might, therefore, seem paradoxical that Buffon’s own practical natural history chiefly took the form of detailed descriptions of the animals discussed in the gigantic *Histoire naturelle*. The organization of that work ignored taxonomic categories in favor of descriptions of morphology, accounts of characteristic behaviors and habitats, and, famously, even details about the animals’ use to human beings. This focus on usefulness was a perfectly self-conscious expression of Buffon’s belief in the importance of the senses and empiricism in learning about nature: human uses of animals and animal products represented the most intimate empirical knowledge of those animals that human acquaintance with organic nature had provided. In this respect, then, Buffon’s approach tended to conceive of natural history as a kind of dominion over nature—a particular form of instrumental knowledge that he nonetheless yoked together with true natural philosophy. Although he criticized the “scientific arts” for failing to yield proper natural philosophy, he recognized that they were nonetheless valuable in their own right: instrumentality, practical usefulness for accomplishing particular tasks, was here closely allied to natural philosophy without actually supplanting it. This was an alliance, entirely typical of the seventeenth and eighteenth centuries, that expressed itself in various ways: natural philosophers would talk about their work in the various sciences sometimes in the terms of natural philosophy and sometimes in the terms of instrumentality. No settled relationship existed between those two alternative representations of their work.

<sup>27</sup> Phillip R. Sloan, “The Buffon–Linnaeus Controversy,” *Isis*, 1976, 67:356–375.

<sup>28</sup> Georges-Louis Leclerc, comte de Buffon, *Histoire naturelle, générale et particulière*, Vol. 1 (Paris, 1749), “Premier discours,” p. 52: “Dans ce siècle même où les Sciences paroissent être cultivées avec soin, je crois qu’il est aisé de s’apercevoir que la Philosophie est négligée, & peut-être plus que dans aucun autre siècle; les arts qu’on veut appeller scientifiques, ont pris sa place; les méthodes de Calcul & de Géométrie, celles de Botanique & d’Histoire Naturelle, les formules, en un mot, & les dictionnaires occupent presque tout le monde; on s’imagine sçavoir davantage, parce qu’on a augmenté le nombre des expressions symboliques & des phrases sçavantes, & on ne fait point attention que tous ces arts ne sont que des échafaudages pour arriver à la science, & non pas la science elle-même.” Cf. Lyon’s translation in Lyon and Sloan, eds., *From Natural History to the History of Nature* (cit. n. 24), p. 122.

## THE IDEOLOGY OF MODERN SCIENCE

One of the characteristic features of modern science since its appearance in the nineteenth century is the perception of it as being, fundamentally, natural philosophy (in the strict sense discussed above). That perception regards any practical techniques associated with scientific knowledge as little better than fortunate by-products. A quite recent statement of such a belief, taking the particular form of a purification argument, appeared in an op-ed piece in the *Washington Post*. The essay, by a senior biomedical scientist at the University of Michigan, presents complaints about the recent efflorescence of life-science institutes, which allegedly depart from the proper mission of science:

In place of the collective search for ever-more powerful explanations, the new view of the life sciences would change the focus to proprietary craft knowledge—knowledge that can be owned and held confidential. This kind of knowledge has no proper role within the academy, an institution centered on the permanent curriculum of arts and sciences and traditionally unconcerned with secrecy—or with capital gains.

The mission of life science institutes emphasizes manipulation and control. Academic sciences, by contrast, are about understanding.<sup>29</sup>

That position takes for granted a sort of dichotomous division similar to the one established in the nineteenth century between the categories of “pure” and “applied” science.<sup>30</sup> Furthermore, the same hierarchy between the two is clearly suggested: pure science is the unadulterated, real thing; applied science is simply derivative of it. T. H. Huxley, in an address from 1880, expressed this idea in a characteristically uncompromising, albeit unfamiliar, way:

I often wish that this phrase, “applied science,” had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge, which is of no practical utility, and which is termed “pure science.” But there is no more complete fallacy than this. What people call applied science is nothing but the application of pure science to particular classes of problems. It consists of deductions from those general principles, established by reasoning and observation, which constitute pure science. No one can safely make these deductions until he has a firm grasp of the principles; and he can obtain that grasp only by personal experience of the operations of observation and of reasoning on which they are founded.<sup>31</sup>

For Huxley, pure science is the only kind of science; he sees no intellectual integrity or self-sufficiency in mere practical know-how.

Huxley’s idea of the unpolluted life of the mind, of “pure” science, seems to have been central to science’s attainment of a high cultural standing in the modern period. Such a view seems to have been behind Sarton’s heralding of science as the foundation of what he called the “new humanism.” In the 1956 edition of his lectures on this subject from the

<sup>29</sup> Fred L. Bookstein, “Biotech and the Watchdog Role of Universities,” *Washington Post*, 30 July 2001, p. A15. My thanks to Adrian Johns for bringing this article to my attention.

<sup>30</sup> See, e.g., Ronald Kline, “Construing ‘Technology’ as ‘Applied Science’: Public Rhetoric of Scientists and Engineers in the United States, 1880–1945,” *Isis*, 1995, 86:194–221.

<sup>31</sup> Thomas H. Huxley, “Science and Culture” (1880), in *Science and Education* (New York: Collier, 1902), pp. 120–140, on p. 137. John Tyndall made similar arguments in the same period; see the discussion in Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (Chicago: Univ. Chicago Press, 1999), Ch. 1, esp. pp. 53–55.

1930s, he wrote that “science is the conscience of mankind” and that, for the “true” scientist, “the infinite treasures which science has yielded and is constantly yielding are incidental; the main purpose of science, and its main reward is the discovery of truth.” And once more: “The chief aim of scientific research is not to help mankind in the ordinary sense, but to make the contemplation of truth more easy and more complete.”<sup>32</sup> Thus was the moral principle of the scientist’s disinterestedness justified.

The routine identification of modern science with natural philosophy has also been reinforced by the way that most philosophers of science have tended to treat questions about the nature of science as fundamentally epistemological. Besides such groups as logical empiricists and scientific realists, this generalization also applies to many of those recent philosophers of science who have responded positively to sociologically informed approaches to science: even philosophers such as Arthur Fine and Ian Hacking have still tended to ask about the status of scientific knowledge claims; even they treat science as if it were fundamentally natural philosophy.<sup>33</sup> But to the historian of science, it is surely evident that it is not.

While the Scientific Revolution was a period that produced a curious and uneasy alliance between natural philosophy and instrumentality, that alliance was subsequently both maintained and theorized so as to seem perfectly straightforward—even natural. Nowadays, to say that science (seen in its guise of natural philosophy) lends itself to instrumentality raises no eyebrows at all. It is, after all, common sense to hold that belief in a particular scientific theory is justified by the fact that the theory *works*; some philosophers speak of the “success” of science in this sense as something to be explained in terms of the truth, or probability, or verisimilitude—or else the reference—of its theories.<sup>34</sup> This Baconian belief in the practical efficacy of scientific ideas uses that efficacy as evidence for the validity of the natural philosophy that is assumed to underlie and explain it.

However, the historian of science knows that such inferences are in practice flawed. Countless instrumentally effective techniques have been developed in concert with theoretical accounts of bits of the natural world, only for those accounts subsequently to be rejected or radically revised. Thus radio waves were produced by Heinrich Hertz on the basis of a version of James Clerk Maxwell’s electromagnetic theory, leading Hertz to agree with Maxwell that there existed an all-pervading aether—an assumption that had informed Maxwell throughout the development of his theory.<sup>35</sup> Nonetheless, there is little belief

<sup>32</sup> Sarton, *History of Science and the New Humanism* (cit. n. 2), pp. xii, 14, 188.

<sup>33</sup> Arthur Fine, “The Natural Ontological Attitude,” in *The Shaky Game: Einstein, Realism, and the Quantum Theory* (Chicago: Univ. Chicago Press, 1986), pp. 112–135; and Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge Univ. Press, 1983). Apparent exceptions to this generalization are various forms of positivism, most glaringly logical positivism, which seem to dispense with “science as natural philosophy” in favor of seeing it in terms of pure instrumentalism restricted (in the ideal case) to codified sense experience. That positivist sensibilities shaped actual scientific work in various fields at various periods is of course the case and serves to show that the dominant “ideology of modern science” with which this article is concerned has not been unchallenged. See, e.g., John L. Heilbron, “Fin-de-Siècle Physics,” in *Science, Technology, and Society in the Time of Alfred Nobel*, ed. Carl-Gustav Bernhard, Elisabeth Crawford, and Per Sèrböm (Oxford: Pergamon, 1982), pp. 51–71.

<sup>34</sup> Richard N. Boyd, “The Current Status of Scientific Realism,” in *Scientific Realism*, ed. Jarrett Leplin (Berkeley: Univ. California Press, 1984), pp. 41–82; and Boyd, “Scientific Realism,” in *The Stanford Encyclopedia of Philosophy (Summer 2002 Edition)*, ed. Edward N. Zalta (<http://plato.stanford.edu/archives/sum2002/entries/scientific-realism/>).

<sup>35</sup> An accessible recent treatment of Maxwell’s work is P. M. Harman, *The Natural Philosophy of James Clerk Maxwell* (Cambridge: Cambridge Univ. Press, 1998); on Hertz’s work in this area see Olivier Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford Univ. Press, 2000), Ch. 6, esp. pp. 234–258. Needless to say, the detailed story is more complicated than the sketch given here.

today in the existence of a Maxwellian aether, even though people still make routine use of Hertzian electromagnetic radiation. Instead, different theories of electromagnetism, which posit different constituents of the universe, are now attributed the explanatory efficacy that Maxwell's aether ideas were once supposed to possess. To some extent or another, similar stories could be told about an endless variety of instrumental techniques usually associated with science. Nonetheless, practical efficacy is routinely taken to be a reason for believing in the natural-philosophical content of scientific theories—including by scientists themselves.<sup>36</sup>

Similarly, just as instrumentality is routinely assumed to provide support for natural-philosophical assertions, so too arguments of a natural-philosophical kind are often used to *explain* the instrumental success of particular techniques. The wave properties of electrons, a part of the natural philosophy presented by quantum mechanics, are used to explain how and why electron microscopes work; DNA typing is an effective technique because people believe that the natural philosophy of modern genetics and molecular biology explains and justifies it. If the natural philosophy were not believed to be in some sense true of the world, the technique would not be seen as effective.

The amorphous category called “science” thus acts, in a variety of often inconsistent ways, as an accounting resource often associated with effective instrumental techniques. One of the implications of attributing instrumental effectiveness to science's natural-philosophical content, in the manner exemplified by Huxley, is that it clearly does a grave disservice to the work and intellectual content of all kinds of engineering—whether mechanical, genetic, computational, or any other sort of practical intervention in the world. Such achievements are in fact the outcomes of complex endeavors that involve a huge array of mutually dependent theoretical and empirical techniques and skills, with only a tenuous and highly mediated path back to any natural-philosophy component in amongst the tangle. When specific instances of the apparently direct “application” of “basic research” or “pure science” are examined closely, the results tend to show that not just the practical but also the theoretical work necessary to get complicated things to function properly is of a much higher order than the “pure science”/“applied science” relationship would imply. The sociologist Michael Mulkay made such an argument in a 1979 article, while the historian of technology Edwin Layton has stressed this view in order to redeem technology and its history from an implied subservience to science; but it is borne out in countless empirical studies in the history of science and technology. Furthermore, Andy Pickering's notion of the “mangle of practice” points toward an analogous understanding of the day-to-day work of experimental science itself—categorical distinctions between physical and intellectual labor in the sciences are increasingly less persuasive as accounts of how things really happen.<sup>37</sup>

<sup>36</sup> At the same time, many techniques corresponding to the *practica* side of the old *theorica/practica* sciences are still employed for instrumental purposes, even though the content of their conceptualizations (that is, of their *theorica* parts) is no longer believed to be literally true. So engineers still use Newtonian-style mechanics, and earth-centered astronomy is still used for navigation. Westman points to some modern counterparts to the *theorica/practica* distinction in “Literature of the Heavens” (cit. n. 8).

<sup>37</sup> Michael J. Mulkay, “Knowledge and Utility: Implications for the Sociology of Knowledge,” *Social Studies of Science*, 1979, 9:63–80; Edwin T. Layton, Jr., “Conditions of Technological Development,” in *Science, Technology, and Society: A Cross-Disciplinary Perspective*, ed. Ina Spiegel-Rösing and Derek de Solla Price (London/Beverly Hills, Calif.: Sage, 1977), pp. 197–222; Layton, “Through the Looking Glass; or, News from Lake Mirror Image,” *Technology and Culture*, 1987, 28:594–607; and Andrew Pickering, *The Mangle of Practice: Time, Agency, and Science* (Chicago: Univ. Chicago Press, 1995).

Such distinctions are still, however, routinely invoked: the widespread and long-standing assumption that the instrumentality part of science is simply a matter of “applying” the knowledge provided by science’s natural philosophy has had, and continues to wield, an enormous cultural impact. The authority of science in the modern world rests to a considerable extent on the idea that it is powerful, that it can do things. Artificial satellites or nuclear explosions can act as icons of science because of the assumption that they legitimately represent what science really is: in such cases, the instrumentality of science stands for the *whole* of science. Conversely, when appeal is made to science as the authority for an account of how some phenomenon or object really is in nature—when science is understood as being natural philosophy—it then receives back from its presumed instrumental effectiveness an image of truthfulness that this instrumentality has already been accepted as confirming.

The overall totalizing effect of this double-faced conception of science has been to make the legitimacy of science in practice unassailable. How are science’s instrumental capabilities achieved? By virtue of the truth of science’s natural philosophy. How is science’s natural philosophy shown to be true (or likely) in the first place? By virtue of science’s instrumental capabilities, including those folded into experimental work. “Science” can be represented in the modern period in its guise as natural philosophy, from which instrumentality flows, or in its guise as instrumentality, on the basis of which an acceptable natural philosophy is founded. It can be represented as one or the other, but not both simultaneously—the decision to speak of science in the terms of one representation necessarily subordinates to it the other, complementary, representation. These two logically distinct ways of representing what “science” is provide one another a sort of bootstrapping, or alternating, mutual support. But if they are interrogated side by side, the total picture ceases fully to make sense. This odd situation, of which we are the direct inheritors, precisely represents the basic ideology of modern science—a systematic misrepresentation of what science and scientists actually do.<sup>38</sup>

#### CONCLUSION

Looking at modern science from the perspective of the early modern period shows the ways in which this accommodation between natural philosophy, as a contemplative branch of general philosophy, and instrumentality, in the form of the Baconian goal of practical utility, took on its modern shape. Contemplative natural philosophy had not generally used claims to being productive of works as grounds for belief in the truth of its assertions; it had originally been independent of science’s instrumentality. The early modern accommodation between the two seems to have been facilitated by the elevation of an explicit, theorized kind of experimental practice that linked claims about the nature of the world to instrumental techniques for exploiting it.<sup>39</sup> This accommodation, however, never worked

<sup>38</sup> “Ideology” is here meant in the Marxist sense of systematic misrepresentation.

<sup>39</sup> On this topic see esp. Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, N.J.: Princeton Univ. Press, 1985), the classic investigation of meanings of experiment in the seventeenth century; see also Dear, *Discipline and Experience* (cit. n. 21), Ch. 8. Another systemic historiographical legacy, the odd partial segregation of the history of science and the history of technology, is also illuminated by this overall perspective. The division is often described nowadays as artificial; in consequence, talk of “technoscience” has become increasingly popular, following Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard Univ. Press, 1987). But—artificial or not—the division is one the ambiguity of which seems to be deeply rooted in the very

of itself; much rhetorical work was required to represent it as being unproblematic, as being *natural*.

This understanding of the elaboration of modern science allows the instrumental side to be seen as a distinct, culturally contingent element of science rather than simply a natural and inevitable support for its natural-philosophical aspect. Conversely, the instrumentality in modern science need not be seen as necessarily reliant on science's natural philosophy. Such a view also renders less remarkable the fact that interpretive accounts of the natural world in cultural contexts other than that of Western societies in the past three or four centuries, such as classical and medieval Chinese natural philosophy, have not tended to use instrumentality as an argument for their own truth—natural philosophy in its strict sense has never had a necessary connection to instrumentality.<sup>40</sup>

Classic Marxist views of science, such as those put forward by Friedrich Engels, Boris Hessen, J. D. Bernal, or Benjamin Farrington, have opposed the idealist philosopher's one-sided "natural philosophy" view of science with what amounts to an equally one-sided "instrumentality" conception. The oddness of that situation is indicated by the productions of Joseph Needham and his collaborators in the volumes of the monumental *Science and Civilisation in China*. The work comprises some material on natural philosophy, such as cosmological ideas, but largely focuses on completely distinct issues of technical work and innovations, such as papermaking, gunpowder, navigation, printing, and chemical processes. The nature of science as a knowledge enterprise never arises as a problem within Needham's project, since it rests so firmly on the assumption that, at root, science is a Marxist kind of technical industrial enterprise. Consequently, the natural-philosophical dimension is treated as largely epiphenomenal but is not entirely ignored—presumably because it resembles the natural-philosophical aspects of Western science.<sup>41</sup>

However, if "science" is treated explicitly as a particular set of cultural endeavors deriving from a Western, or European, tradition, it can usefully be examined as a kind of dialectical interaction between natural philosophy *and* instrumentality. What this means in historiographical practice is that the stories we trace and the "big pictures" that we sometimes like to confect will tend to display, as a recurrent and often dominant feature, the use of these two different idioms and the ways in which they have been characterized and situationally related by historical actors: the ways in which people have constructed this set of material, instrumental, social, linguistic, cultural, and conceptual techniques that have made, sustained, and subverted their tales of natural philosophy and instrumentality. Modern science, in order to have a premodern as well as a modern history, has no need of being an essentially timeless subject, always and everywhere the same kind of thing. Instead, it should be seen as a subject constituted by its temporal story. Since the dominant ideology of modern science is inherently unstable, what counts as science constantly requires reestablishing and remaking; such processes appear most clearly in the especially perspicuous cases of colonial science.<sup>42</sup> The discipline of the history of science now often

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intellectual foundations of science itself. Natural philosophy and utility, pure and applied science, science and technology—are all pairings that have functioned at various times by purifying what has always been a deeply uneasy relationship.

<sup>40</sup> On Chinese natural philosophy see, e.g., the essays in Nathan Sivin, *Science in Ancient China: Researches and Reflections* (Aldershot: Variorum, 1995).

<sup>41</sup> Joseph Needham *et al.*, *Science and Civilisation in China* (Cambridge: Cambridge Univ. Press, 1954–). A useful recent overview of Marxist approaches is Harold Dorn, "Science, Marx, and History: Are There Still Research Frontiers?" *Perspectives on Science*, 2000, 8:223–254.

<sup>42</sup> See, for relevant perspectives, Gyan Prakash, *Another Reason: Science and the Imagination of Modern India*



tries to tell, in place of a Sartonian story of inevitable progress, stories about constant struggles to make and unmake particular configurations of the cultural sorcery called “science.”<sup>43</sup>

Acknowledging that science is not one thing, a natural kind, while at the same time recognizing that the symbol “science” is culturally very real indeed, may liberate our discipline from the twin dangers of hyperhistoricization and essentialist universalism. The former danger has been frequently skirted in recent years by historians concerned to understand in its contextual, local specificity some cultural practice that used casually to be called “science” by earlier historians; the latter danger was that into which those earlier historians themselves fell. But each danger has its virtues too. My erstwhile Cornell colleague, the Oxford Sanskritist Christopher Minkowski, suggests that there ought to be some way of speaking coherently about those knowledge enterprises, such as the mathematical sciences, that had a career that spread across the Eurasian continent, where Ptolemaic planetary models show up in seventeenth-century Jaipur as well as ninth-century Cologne.<sup>44</sup> In such cases, techniques spread through adoption, rather like the apparently organic growth and decline of language groups, even though, as in the case of historical linguistics, we know that the spread is effected by countless episodes of human social interaction. Perhaps something might be gained from using the plural term “sciences,” in the manner common in other languages, for collections of individual knowledge enterprises (whether qualitative or mathematical), and using the blanket term “science” only to designate the kind of ideological construct that this essay has been concerned to sketch out.

Not only would this clarification restore intellectual integrity to studies of local cultural production by the simple expedient of labeling them parts of the “history of the sciences”; it would also integrate sciences from many parts of the world into that same history without of necessity implicating them in the ideology of modern science. That ideology involves not the mere summing of “instrumental” and “natural-philosophical” aspects of the sciences but requires that the two be intimately related in historically contingent, and logically tense, ways. Therein lies the particularly “Western” contribution.

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(Princeton, N.J.: Princeton Univ. Press, 1999); and the essays in Roy MacLeod, ed., *Nature and Empire: Science and the Colonial Enterprise* (*Osiris*, N.S., 2000, 15), esp. Kapil Raj, “Colonial Encounters and the Forging of New Knowledge and National Identities: Great Britain and India, 1760–1850,” pp. 119–134.

<sup>43</sup> For just one example see Gieryn, *Cultural Boundaries of Science* (cit. n. 31), Ch. 5; very many historical accounts can be recast in this way, of course.

<sup>44</sup> Christopher Minkowski’s work is ongoing as part of a cooperative project called “Sanskrit Knowledge Systems on the Eve of Colonialism,” with support from the National Endowment for the Humanities and the National Science Foundation.