Do physicists need myths?

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In textbooks and popular writings physicists repeat familiar stories about the history of physics that historians have long identified as myths. Examples of remarks about Galileo and Aristotle are used to illustrate this practice. These examples lead to a discussion of several reasons why these myths might play a positive role for physicists and whether physicists can dispense with myths about the history of their field. © *2006 American Association of Physics Teachers.*
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In a recent book Lederman and Hill repeat the familiar view that Galileo was a paragon of a true scientist, as illustrated by his grasp of the principle of inertia, and Aristotle and Plato were exponents of "antiscientific mumbo-jumbo, leading to dogma."[1] Students of the history of science know that this view is a myth, but it is a myth that physicists perpetuate in textbooks and popular writings. Moreover, it is part of a wider myth that depicts genuine physicists as those who arrive at correct results, and those who make serious errors as confused or worse. Given the continued prevalence of such stories, I consider whether they play a positive role in promoting the advance of physics. I will begin by telling a fuller story of Galileo's achievements and failings, then briefly discuss Aristotle. I pick on Lederman and Hill's book because of its general excellence, the eminence of its authors, and its recent vintage.

I preface my remarks about Galileo by emphasizing that my aim is not to challenge Galileo's accomplishments or importance. Rather, I want to challenge the view that historical figures should be evaluated on the basis of whether we now recognize their results as correct. I note that Galileo did arrive at several results that have stood the test of time. These include his telescopic discoveries whose difficulty should not be underestimated: lenses were often of poor quality and would fog up and there were no good mounts. It took care and patience to observe the new phenomena so that after sending a telescope to the Medici court, Galileo suggested that they should not attempt to observe Jupiter's moons until he could be present to guide them.[2] Nevertheless, every one of Galileo's telescopic discoveries was correct. The same holds for his conclusion that an object's rate of fall (in the absence of air resistance) is independent of its weight. Galileo claimed to have done the experiment,[3] although he also gave an *a priori* proof of the result.[4] Given this conclusion, Galileo proceeded to determine the rate of fall experimentally. He carefully prepared a groove in an inclined plane to minimize friction and rolled a bronze ball down this groove and measured the time it took to move different distances; he then repeated the measurements at different inclinations of the plane.[5] Having determined the acceleration of falling objects, Galileo generalized the result, including doing a calculation of how long it would take for a cannon ball to fall from the orbit of the moon to the center of the earth.[6]

Lederman and Hill place special emphasis on Galileo's discovery of the law of inertia. At the head of Chapter 6 they quote a passage from *Dialogue*[7] in which Galileo concluded that a "moveable body placed upon a surface with no slope upward or downward"[8] would move perpetually as long as the space is unbounded (and ignoring friction). But the ideas that Galileo associated with this language are quite different from ours.[9] Salviati, Galileo's spokesman, tells us that "in order for a surface to be neither downward nor upward, all its parts must be equally distant from the center."[10] Thus the unbounded space Galileo mentioned is a circle, and the perpetual motion he is discussing is motion on a circle. His key example is a ship moving on a calm sea. If we eliminate friction, Galileo maintains, the ship will continue to move in a circle around the center of the earth.[11] In his later book, *Two New Sciences*, Galileo treated continued motion as linear when he argued that projectiles move on parabolas (another enduring contribution), but he is clear that this linear motion is an approximation to an arc of a large circle. He justified this approximation by appealing to the authority of Archimedes who "takes it as a true principle that the arm of a balance or steelyard lies in a straight line equidistant at all points from the common center of heavy things…."[12] In addition, Archimedes treated the cords used to hang weights from a balance as parallel, while architects treat plumb lines as parallel when erecting tall towers. These approximations are justified because "the distances we employ are so small in comparison with the great distance to the center of our terrestrial globe…."[13]

Galileo was generally dubious about the role of straight-line motion in dynamics. He denied that straight-line motion ever occurred in an ordered universe[14] and suggested that the only role for straight-line motion is to restore order that has been disrupted.[15] He even suggested that straight-line motion might not exist at all: when we see such motion, we are actually seeing circular motion from a limited point of view. Thus he considered the possibility that the actual path of a stone falling from the top of a tower is an arc of a semicircle with one end at the top of the tower and the other end at the center of the earth.[16] He drew three conclusions from this account: only circular motion occurs in this case; the distance the stone moves in falling to the earth is the same as it would traverse if it stayed at the top of the tower; and the actual motion is never accelerated.[17] The last conclusion follows because Galileo viewed circular motion as the only nonaccelerated motion.[18] Galileo described this account of fall as "very probable"[19] and repeated it later in the book.[20]

Three other items from Galileo's work are worth mentioning. In Ref. 3 Galileo attempted to give a mathematical account of the strength of structural members. His approach to this task was constrained by the limited mathematics available to him. He understood the law of the lever, and sought to redescribe beams as systems of levers. I contend that this approach was an important historical step, even though the approach and Galileo's conclusions are not valid. Galileo's book, *The Assayer*,[21] is often cited because of a passage in which he maintained, contrary to the prevailing view, that mathematics is the appropriate tool for studying the physical world. However, this book was part of an exchange with a Jesuit astronomer about the comets. Two views were in dispute: Aristotle's view that comets are atmospheric phenomena and the new view that they are in the heavens. In this exchange, Galileo defended the Aristotelian view and maintained that comets are not in the heavens but are purely terrestrial phenomena. In his book on floating bodies[22] Galileo defended the Archimedean view that whether a body floats depends on its density and opposed the Aristotelian view that floating depends only on a body's shape. An important example was presented by thin slivers of dense material, such as gold, silver, iron, and ebony, floating on water. This case poses a serious challenge to Galileo's view, and we note that the participants on both sides of this discussion were focused on the empirical evidence. At the time in question no one could reach the correct result because no one was aware of surface tension. Galileo's defense of the density view was based on a very sharp observation. He noted that in such cases the floating sliver is depressed slightly below the surface of the water and argued that the relevant density is the combined density of the sliver plus the bit of air between the top of the sliver and surface of the water.[23]

We see that Galileo got it right some of the time, but often defended results that we now recognize as wrong. I contend that Galileo's importance as a scientist should not be judged by how many of his results we now accept. Nor should his errors be thought of as resulting from lapses of scientific method. Galileo was an exceptional thinker, but he was still a man of his era who had no access to most of the data, concepts, and techniques that are now available. With this in mind, I now discuss Aristotle's physics and astronomy.

Aristotle gave several reasons for holding that the earth is spherical. After giving a number of theoretical arguments he added:

> "The evidence of the senses further corroborates this. How else would eclipses of the moon show segments shaped as we see them? As it is, the shapes which the moon itself each month shows are of every kind—straight, gibbous, and concave—but in eclipses the outline is always curved; and, since it is the interposition of the earth that makes the eclipse, the form of this line will be caused by the form of the earth's surface, which is therefore spherical. Again, our observations of the stars make it evident, not only that the earth is circular, but also that it is a circle of no great size. For quite a small change of position on our part to south or north causes a manifest alteration of the horizon. There is much change, I mean, in the stars which are overhead, and the stars seen are different, as one moves northward or southward … All of which goes to show not only that the earth is circular in shape, but also that it is a sphere of no great size; for otherwise the effect of so slight a change of place would not be so quickly apparent … Also, those mathematicians who try to calculate the size of the earth's circumference arrive at the figure 400,000 stades. This indicates not only that the earth's mass is spherical in shape, but also that as compared with the stars it is not of great size."[24]

This thinking is hardly antiscientific or mumbo-jumbo.

Aristotle's explanation of why the earth is spherical would not be taken seriously today. He held that the terrestrial portion of the world is made up of four elements: earth, water, air, and fire. Each element is characterized by its natural motion, that is, the way it moves in the absence of any forces. Earth, in particular, has a natural motion toward the center of the universe. Suppose that the pieces of the earth were generated at various places in the universe. They would move naturally toward the center of the universe and this motion would necessarily result in a spherical earth.[25] This theory explains an empirical result. Although the theory embodies concepts that have been completely rejected, the approach is hardly unscientific. Aristotle's account of terrestrial motion generated a problem about projectiles: projectile motion is not natural motion and any such motion requires a force to sustain it. We note that Aristotle recognized that this motion is a problem for his account and proposed a solution, albeit one that fails utterly.[26] Stephen Hawking tells us that in the Aristotelian tradition, "one could work out all the laws that govern the universe by pure thought; it was not necessary to check by observation."[27] Yet Aristotle saw that projectile motion occurs, recognized it as a problem for his theory, and attempted to solve this problem.

For these reasons I contend that the contrast Lederman and Hill drew between Galileo and Aristotle as paradigmatic of scientific and antiscientific thinking should be rejected. Both engaged in scientific thinking within the constraints of the eras in which they worked. This contrast is part of a common practice in which failures of those early figures deemed to be genuine scientists are ignored, as are the accomplishments of those who are not currently viewed as having this status.[28] This results is a picture of history that is riddled with myths.

Does the perpetuation of these myths play a role in fostering physics? Although I do not claim to have a definitive answer, I want to consider two possible reasons why they might.

One possibility is that these myths make research a more attractive career goal. By pursuing the scientific method each researcher can be assured of adding—or in these days of large collaborations, helping to add—a permanent brick to the growing edifice of knowledge. It might be significantly more difficult to motivate people to do research if we held out the prospect that regardless of how well you do your work, there is a significant probability that you will pursue dead ends. You might make a contribution by finding evidence that you are pursuing a dead end, although this conclusion might not be recognized until after you have left the scene. For example, the failures of the unknown Ptolemaic astronomers who tried to improve the fit between theory and available data by adjusting circles contributed to growing doubts about their approach, which contributed to Copernicus' reasons for trying a radical alternative. Sometimes first-class scientists spend their professional lives pursuing dead ends. A recent example is the long nineteenth-century pursuit of the atomic weights that were believed to characterize each chemical element. Frederick Soddy offered this description:

> "There is something, surely, akin to if not transcending tragedy in the fate that has overtaken the life work of that distinguished galaxy of nineteenth-century chemists, rightly revered by their contemporaries as representing the crown and perfection of accurate scientific measurement. Their hard-won results, for the moment at least, appears as of as little interest and significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty."[29]

These persistent failures did lead one important chemist, Crookes in 1886, to express hesitant doubts about the existence of such unique weights.[30] But it was the completely unexpected discovery of radioactivity that led to an understanding of isotopes and to the abandonment of the guiding principle behind this earlier research.

Although the image of genuine scientists as making permanent contributions to the body of knowledge may have a motivational role, I want to contrast a different image. Suppose you are attempting to find the equation of a very complicated curve. At a given time you have only a limited body of data from measurements of a small part of the curve, and you do not yet recognize its full complexity. This research might lead to interesting results and motivate you to generalize well beyond the available data. Galileo's extrapolation of his law of fall and the widespread belief in the nineteenth-century that Newton had said the last word on the fundamental nature of the physical world serve as examples. By now we have a substantial history of cases in which new data from newly probed portions of the curve undermined such generalizations. Still, these generalizations may have contributed to progress by providing the framework for research that led to their overthrow.

This alternative image suggests a more modest goal: finding generalizations that cover portions of the curve within a limited range. The current move to effective field theories in high-energy physics is an example of a step in this direction. But we need to recognize that even these more modest results depend on the precision of our instruments and can be undermined by new evidence. Galileo's law of fall is not correct in any case. It is useful in some situations, but these situations depend on our particular goals and on the accuracy of our measurements. The view that the earth is flat is adequate when building a patio, but it is not true even in this limited range. These examples also point out a danger of using myths for motivational purposes, because the motivation might vanish when the myths are revealed as such.

A second reason for maintaining these myths is forensic. In these days in which much science has become very expensive and the value of science is being attacked from a number of directions, it is considered to be important to present a public image of science that will promote its support. But I suggest that the presentation of myths is a misconceived way to pursue this goal. Many specific instances of these myths are easily discovered to be myths with just modest research effort or a single course in the history of science, as is the overall myth that real science inevitably yields permanent results. One source of skepticism about science arises from scientists making public declarations about what they have achieved or will achieve that are then publicly found to be wrong. A more viable approach for assuring public support for science would dispense with myths.

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[1] L. M. Lederman and C. T. Hill, *Symmetry and the Beautiful Universe* (Prometheus Books, Amherst, NY, 2004), p. 121.

[2] Letter to Belisario Vinta, March 13, 1610, in *Le Opere di Galileo Galilei*, edited by A. Favaro (Barbara, Florence, 1890–1909), Vol. X, p. 289.

[3] Galileo, *Two New Sciences*, translated by S. Drake (University of Wisconsin Press, Madison, WI, 1974), p. 66.

[4] See Ref. 3, pp. 66–68.

[5] See Ref. 3, pp. 169 and 170.

[6] Galileo, *Dialogue Concerning the Two Chief World Systems*, translated by S. Drake (University of California Press, Berkeley, CA, 1967), p. 224.

[7] See Ref. 6, p. 148.

[8] See Ref. 1, p. 117.

[9] The account of Galileo's views on inertial motion that I present is not universally accepted. Many eminent Galileo scholars attribute the modern law of inertia to Galileo. A detailed development and defense of my reading is in my book, *Conceptual Systems* (Routledge, London, in press).

[10] See Ref. 6, p. 148; cf. Ref. 3, p. 172.

[11] See Ref. 6, pp. 147 and 148. Writers who attribute linear inertial motion to Galileo include R. P. Feynman, R. B. Leighton, and M. Sands, *The Feynman Lectures on Physics* (Addison-Wesley, Reading, MA, 1963), Vol. 1, pp. 7-2 and 9-1; P. G. Hewitt, *Conceptual Physics*, 8th ed. (Addison-Wesley Longman, Reading, MA, 1998), pp. 22 and 23; F. Rohrlich, *From Paradox to Reality* (Cambridge U. P., Cambridge, 1990), p. 36; and F. K. Richtmyer, E. H. Kennard, and J. N. Cooper, *Introduction to Modern Physics*, 6th ed. (McGraw-Hill, New York, 1974), p. 13.

[12] See Ref. 3, p. 223.

[13] See Ref. 3, pp. 223 and 224.

[14] See Ref. 6, pp. 19 and 31.

[15] See Ref. 6, pp. 242 and 243.

[16] See Ref. 6, pp. 162 and 167.

[17] See Ref. 6, pp. 166 and 167.

[18] See Ref. 6, pp. 31 and 32.

[19] See Ref. 6, p. 165.

[20] See Ref. 6, p. 264.

[21] Galileo, "The assayer," translated by S. Drake and C. D. O'Malley, *The Controversy of the Comets of 1618* (University of Pennsylvania Press, Philadelphia, 1960).

[22] Galileo, *Discourse on Bodies in Water*, translated by T. Salusbury (University of Illinois Press, Urbana, IL, 1960).

[23] See Ref. 22, pp. 35 and 36.

[24] Aristotle, "On the heavens," translated by J. L. Stocks, in *The Complete Works of Aristotle*, edited by J. Barnes (Princeton U. P., Princeton, NJ, 1995), p. 489.

[25] See Aristotle, Ref. 24, pp. 488 and 489.

[26] See Aristole, “Physics,” translated by R. P. Hardie and R. K. Gaye, in Ref. 24, pp. 445 and 446.

[27] S. Hawking, *A Brief History of Time* (Bantam Books, New York, 1990), p. 15.

[28] See also R. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge U. Press, Cambridge, 1983) for a rich view of Newton’s intellectual life, including his deep involvement in alchemy and theology; and *Science in the Middle Ages*, edited by D. Lindbergh (University of Chicago Press, Chicago, 1978) for examples of accomplishments in a period in which science is often depicted as nonexistent.

[29] F. Soddy, *The Interpretation of the Atom* (John Murray, London, 1932), p. 50.

[30] G. Bruzzaniti and N. Robotti, “The affirmation of the concept of isotopy and the birth of mass spectrography,” Archives Internationales D’Histoire des Sciences **39**, 309–334 (1989).

A WORLD WE’D HARDLY RECOGNIZE

The overarching lesson that has emerged from scientific inquiry over the last century is that human experience is often a misleading guide to the true nature of reality. Lying just beneath the surface of the everyday is a world we’d hardly recognize.

Brian Greene, *The Fabric of the Cosmos: Space, Time, and the Texture of Reality* (Knopf, 2004), p. 5.