Scientific Myth-Conceptions

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ABSTRACT: Using several familiar examples—Gregor Mendel, H. B. D. Kettlewell, Alexander Fleming, Ignaz Semmelweis, and William Harvey—I analyze how educators currently frame historical stories to portray the process of science. They share a rhetorical architecture of myth, which misleads students about how science derives its authority. Narratives of error and recovery from error, alternatively, may importantly illustrate the nature of science, especially its limits. Contrary to recent claims for reform, we do not need more history in science education. Rather, we need different types of history that convey the nature of science more effectively. © 2003 Wiley Periodicals, Inc. *Sci Ed* **87:**329–351, 2003; Published online in Wiley InterScience (www.interscience.wiley.com). DOI 10.1002/sce.10055

INTRODUCTION

Even nonscientists know about Charles Darwin and the voyage of the *Beagle*, Gregor Mendel and inheritance in pea plants, and James Watson and the double helix of DNA. Stories of scientific discovery permeate our culture and science curricula. So one may wonder why recent calls for science education reform (e.g., National Research Council, 1995; Rutherford & Ahlgren, 1990) advocate further history in science teaching. The problem, I contend, is not deficit of history. Rather, the concern should be what *type* of history is used. I refer, in particular, to popular histories of science that romanticize scientists, inflate the drama of their discoveries, and cast scientists and the process of science in monumental proportion. They distort history and foster unwarranted stereotypes about the nature of science—all for the sake of "telling a good story" (Shaffer, 1990). While based on authentic events, these histories are deeply misleading. They subvert the goal of teaching the "history and nature of science" central to those reforms. Here, I focus specifically on the rhetorical architecture: How are the mischaracterizations embodied in rhetorical devices

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and shaped by the narrative format? Many errors result, I claim, from rendering science in a mythic form, in a literary sense. These are not just ordinary misconceptions of science. They are *myth*-conceptions.[1] Science educators especially, I fear, tend to perpetuate such myth-conceptions. But educators are also ideally positioned to remedy them. Here, I profile the problem and some prospective solutions.

I explore this topic by, first, surveying five familiar historical cases: Gregor Mendel and genetics, H. B. D. Kettlewell and the peppered moth, Alexander Fleming and penicillin, Ignaz Semmelweis and handwashing, and William Harvey and circulation of the blood. While the errors in recent accounts may themselves be informative, my chief concern is analyzing the source of the errors. By highlighting the rhetorical strategies and devices in the context of deeper history, I hope to show just *how* such stories "lie" or mislead about the nature of science. Ultimately, the suite of cases exhibits a syndrome, not fully visible in any one case. These historical narratives of science exhibit conventional literary features of myth. (Readers interested in my thematic conclusions here may proceed directly to the section on "The Architecture of Scientific Myths" any time, returning to the data (Cases 1–5) as occasion demands.) Finally, I consider the role of scientific error in teaching nature of science, among other guidelines for educators and teaching strategies.

Critiquing deficits in histories and stories of science is almost commonplace now in our postmodern era. Indeed, such critiques are not difficult once one recognizes that any narrative of science is inherently limited. Every account must be selective in some respect and hence can be portrayed as biased (Turnbull, 1993). Several perspectives significant to the science classroom are now widely documented. Among the familiar potential distortions, one may cite hagiography, Whiggism (or inevitabilism; Butterfield, 1959), gender bias (Rossiter, 1982), and accounts imbued with political ideology or culture (Foucault, 1972; Haraway, 1989); all these may concern the science educator. However, in my analysis the storyteller's perspective and/or "interests" are peripheral. The authors seem to try "honestly" to convey the nature of science. Yet one may still find their accounts significantly misleading for students. The problem is not truncation itself. Selectivity does not inherently yield *myths*. I focus specifically on the *narrative elements* as problematic. How can the very act of telling a story shape content or foster flawed renderings? Once again, I want to interpret the fundamental *myth*-conceptions, not merely (more) historical *mis*conceptions.

Recently, rhetorical analysis has turned to scientific texts and discourse (for example, Gilbert & Mulkay, 1984; Gross, 1990; Halliday & Martin, 1993; Myers, 1990; Selzer,

[1] Here, I may clarify my meaning of the term "*myth*". In the popular mind myth is *false belief*, often opposed to science (as true belief). When applied to science or science education ("the myth of the scientific method" or "the myth of scientific objectivity"), critics use myth to refer especially to *widespread and unquestioned* false beliefs (Bauer, 1992; MacCormac, 1976; McComas, 1998; Savan, 1988; Sismondo, 1996). In these cases, the term "myth" serves primarily as a rhetorical device for casting the author as an authority rescuing readers from credulity. I am *not* using myth in these common senses. Others construe myth in science as *a foundational religious metaphor* (MacCormac, 1976; Midgely, 1992). They equate myth with a cultural perspective or cognitive meta-structure (sometimes critiquing it, sometimes endorsing it). While much can be (and has been) said about scientism, I do *not* mean scientific myth in this sense either.

In Greek, *mythos* means "telling" or "story". In my use, therefore, a myth is always a *narrative*, a literary form, style or genre. (I do *not* adopt the adjunct meanings used theoretically by anthropologists or psychologists.) Like parables, myths *function primarily as explanations and/or justifications* (Bauer, 1992; Milne, 1998). Thus they generally *contain superhuman elements and/or natural phenomena*, whence they, in part, draw their persuasive power. In many cases, myths embody a worldview by *providing formulae or archetypes for appropriate or sanctioned behavior*, hence their narrative format. In construing myth in this way, my analysis does not focus on false belief itself. Rather, I examine how errors symptomatically reveal the structure of the narratives and how they function as myths. The elements noted here are profiled further below ("The Architecture of Scientific Myths") as they apply to stories and histories of science.

1993). Such work has delved into the craft of persuasion, concepts of evidence, the "construction" of knowledge, and the contrasting narratives of science and nature. While this work focuses chiefly on professional communication, it offers tools for considering scientific texts in educational contexts. For example, one may be alert for literary devices, persuasive constructs, or recurring plot patterns. Parallel work has focused on public understanding of science, especially on its role in public decision-making (for example, Martin, 1991; Toumey, 1996). How might the often simplistic conceptions be rooted in historical treatments in science education and elsewhere? Is there any potential remedy? Further resources may be found in analyses of narratives and historical explanation (for example, Munz, 1997; White, 1987). Finally, one may consider how the psychological context of storytelling affects the nature of stories in the science classroom. For example, the relationship between storyteller and hearer/reader may affect how the story is told. Gilovich (1991) identifies at least two major social functions in telling stories: information and entertainment. Content tends to be selected and sometimes altered to meet these two functions. Again, one may be alert to how cognitive factors shape educators' histories of science. Ultimately, how can all these analytical tools inform practicing teachers who approach histories of science as part of an effort to teach nature of science? What simple themes and tools can be distilled for someone with perhaps limited expertise in history, philosophy, sociology, and rhetoric of science?[2]

CASE 1: GREGOR MENDEL

Consider first an icon of science: Gregor Mendel. Few biology textbooks fail to mention Mendel and his work on pea plants. He is, as historian–biologist Jan Sapp (1990) notes, "an ideal type of scientist wrapped in monastic and vocational virtues." That is, he is an exemplary scientist. Stories about him implicitly contain morals about the nature of science. For example, Mendel worked alone in an Austrian monastery. Lesson: Scientists modestly seek the truth, not ambition. Mendel used peas. Moral: Scientists design studies using appropriate materials. He counted his peas: Scientists are quantitative. He counted his peas for many generations over many years: Scientists are patient. He counted thousands and thousands of peas: Scientists are hard-working. After all this, Mendel was neglected by his peers, who failed to appreciate the significance of his work, but he was later and justly "rediscovered": Scientific truth triumphs over social prejudice. Above all, Mendel was right: Scientists do not err. His figures, in fact, were too good to be true, statistically speaking. But he was ultimately correct. Any hint of fraud should only confirm the depth of his theoretical insight. The common story about Mendel is a lesson in the nature of science. And it is mythic in proportion. Although he has not been canonized by the Church, biologists certainly honor and revere him so.

Visual images echo this lesson. Texts sometimes use photos to illustrate scientists from history, and Mendel is no exception. Some texts, however, use illustrations instead. For example, one popular biology text (Campbell, Reece, & Mitchell, 1999, p. 249) abandons the stuffy posed photo in favor of a painting of the scientist "at work." Mendel is portrayed observing: That's how we know he's a scientist. He wears his eyeglasses and a white apron, features naive students often include when asked to "draw-a-scientist." Moreover, a

[2] Readers curious about the author's rhetoric will find the case studies exhibiting the style of Greek tragedy on a small scale: dramatizing the consequences of *historical* hubris (forsaking humility in professing knowledge of history of science). The analyses are rich in irony, where inconsistencies, formed by shifts in context, tend to discredit apparently simple statements. Substantial metaphorical/analogical work follows to link the various ironies. Features of the five cases are treated as a synecdoche of certain narratives of science, characterized as mythic.

soft-edged medium and pastel color palette epitomizes the idealized, romantic tone of the standard narrative. Such illustrations, too—even with no caption—convey a lesson about the nature of science.[3]

On the surface, nothing seems flawed with the conventional story of Mendel. But historians' interpretations differ remarkably (Brannigan, 1981; Corcos & Monaghan, 1993; Hartl & Orel, 1992; Monaghan & Corcos, 1990; Olby, 1985, 1997; Sapp, 1990). First, textbooks typically elucidate "Mendel's" two laws. Paradoxically, though, in his classic 1865 paper, Mendel did not explicitly formulate a "Second Law," the principle of independent assortment. While he did perform dihybrid crosses and reported 9:3:3:1 results, there was no formal recognition of the "independent" behavior of the two character states. In fact, geneticists did not distinguish "Mendel's" 1st and 2nd laws until several years after the revival of his work, when they encountered anomalous ratios in offspring. Bateson, for example, found a 12:1:1:3 ratio in sweet peas for flower color and pollen shape: alleles segregated, but the genes did not assort. Ironically, then, while Mendel's 2nd law bears Mendel's name, he himself did not state it. Accounts credit Mendel with more than he did.

Second, Mendel worked at the level of observable characters. He did not distinguish clearly between traits and material units of heredity—today's phenotype/genotype distinction, at the heart of Mendelian genetics. Nor did Mendel see his "elementes" (today's genes) as occurring in pairs in each organism. Mendel's notation clearly shows, that an $A \times A$ cross yielded $A + 2Aa + a$: the homozygous form was "A" and not a diploid "AA" (Olby, 1985). Mendel, it seems, was not quite "Mendelian." Again, Mendel gets undue credit.

Everyone knows how Mendel examined seven character pairs in peas: tall–dwarf, smooth–wrinkled, green–yellow, etc. Mendel actually investigated 22 (Di Trocchio, 1991). He set aside the ones whose results were too confusing. Yet stories have long paraded the image of a perfect a priori experimental design. Textbooks seemed eager to boast of Mendel's insight, even when no evidence supported it.

Mendel referred to his traits as dominant and recessive. This strikes us as Mendel's discovery. But the notion of prepotency—that one parental trait determines the trait of the offspring—was common among breeders. Mendel followed a few earlier biologists in merely attributing it to the trait, rather than the parent. Texts also present dominance as foundational, its exceptions as "non-Mendelian." Mendel himself, however, seemed aware that dominance was not the norm. Just before introducing dominant traits he noted that (Mendel, 1866, Section 4)

> with some of the more striking characters, those, for instance, which relate to the form and size of the leaves, the pubescence of the several parts, etc., the intermediate, indeed, is nearly always to be seen.

He noted other exceptions: stem length (the hybrids were actually *longer*; Section 4), seed coat color (hybrids were more frequently spotted; Section 4), flowering time and peduncle length (Section 8). For Mendel, his law applied only to "those differentiating characters, which admit of easy and certain recognition" (Section 8). Other characters followed another, different rule or law. Mendel's concept of dominance, initially a linguistic convention for labeling traits, has been universalized. In some cases it is a "law." The notion has been aggrandized and, simultaneously, credited to one person (Allchin, 2000).

Textbooks widely dub Mendel as the founder of modern genetics. Yet Mendel did not study abstract principles of inheritance. The clarity of Mendel's original 1865 paper to

[3] I do not intend to single out this text for criticism. Rather, I hope that comments about a popular text can reflect a widespread problem.

modern readers—even high school students—is deceptive. Rather, he focused narrowly on a problem related to trying to create pure-breeding hybrids and to characterize the identity of species. His paper presents a "law of hybridization" and a mathematical formalism that describes it (Hartl & Orel, 1992). His sequel work (1869) on hawkweed showed that he could not easily generalize his results on peas. Not all things Mendelian are Mendel's. Through the attribution, his achievement, once again, becomes inflated (Brannigan, 1981).

The aura of Mendel and his achievement is further evidenced in how biologists and historians present Mendel as supporting contradictory claims. Despite their disagreement, Sapp (1990) notes, all nevertheless appeal to Mendel's monumental authority to bolster their own claims. Indeed, their goal of securing Mendel's mantle seems to explain their contrary interpretations of his work. His monumentality seems more important than what he wrote. Sapp's observations should alert educators to dissect the architecture of how textbooks, likewise, portray Mendel. The problem is not just telltale elisions. In many instances, Mendel has been recreated historically to fill a monumental, heroic image.

CASE 2: H. B. D. KETTLEWELL

Another favorite textbook icon is the peppered moth, which evolved during Britain's industrial revolution. The popular images of the moths against different backgrounds epitomizes the classic study by Bernard Kettlewell, contrasting survival in polluted versus rural forests. Many biology texts describe—and typically celebrate—the elegant design of these experiments. Recent accounts have sought to update the science (Majerus, 1998; Rudge, 2000). Here, I am concerned primarily with how the story is told.

Again, the history contrasts sharply with the canonical classroom image (Allchin, 2001a). First, Kettlewell's monograph, *The Evolution of Melanism*, plainly shows that in addition to the familiar dark and "peppered" forms, there is a series of intermediates, known as *insularia* (1973, plate 9.1). The range of coloration in *insularia* indicates greater complexity. One easily finds such specimens, Kettlewell noted, in museum collections, and he included them in his own field studies. Having recruited observers from around Britain, Kettlewell catalogued the relative frequency of the three forms in various locations. The incidence of *insularia* was sometimes as high as 40% or more (Kettlewell, 1973, pp. 134–136). *Insularia* was no trivial exception. Still, while Kettlewell documented *insularia* in his research, it became eclipsed in subsequent renditions of his work. For example, his 1973 book cover sported the now canonical images, omitting *insularia*. Kettlewell himself seemed to promote the streamlined story publicly. Pursuing the ideal of conveying the process of science, some textbooks include the original published data of the contrasting Birmingham and Dorset environments. But while Kettlewell tallied survival rates for all three forms (Kettlewell, 1955, 1956), the texts omit *insularia* (e.g., Hagen, Allchin, & Singer, 1996, p. 7). Although the essential conclusions do not differ, the implicit lesson *about science* does. That is, in the simplified image, science sorts things crisply into black and white, true and false, without any "shades of grey," partial conclusions or residual uncertainties. Science is black and white—like the moth images.

Like Mendel's work, Kettlewell's research has become rendered conceptually. The idea of a well conceived controlled experiment yielding solid evidence contrasts with the history, however. Hagen (1993, 1999) observed that at first, Kettlewell presented data only from the polluted Birmingham woods. He made no reference whatsoever to Dorset, now considered a critical complement in the study's design. Nor did he give any hint that his study was incomplete or preliminary or partial. Why? One hidden task of Kettlewell's studies was collecting enough organisms to mark, release, and then recapture. Kettlewell was apparently limited by sheer logistics in managing moth pupae (Rudge, 2001). Yet he also proceeded anyway, publishing only "half" the study. Perhaps Kettlewell did not see the "control" as important initially, at least as texts portray it today (Hagen, 1993). There were numerous other controls, generally overlooked today (Rudge, 1999, pp. 19–20). Ultimately, Kettlewell's first study was heavily criticized. Ornithologists claimed that birds did not prey on the moths at all. In his follow-up study, Kettlewell enlisted ethologist Niko Tinbergen to document the predation on film (Rudge, 2001). He also added data from the unpolluted Dorset woods. Kettlewell, then, patched together several separate studies. The history belies the stereotypical image of great scientists. There was no "Eureka!" Rather, conclusions flowed from a series of less extraordinary modes of thinking and working. Again, the process of science seems not so "black-and-white" as the textbook story implies.

The potential danger in habitual simplification is that teachers can convey a false image of the nature of science. In a sense, they condition students to expect simplicity. When students encounter complexity, they may feel betrayed or "simply" lack the requisite interpretive skills. Consider, for example, Wells' recent criticism of the peppered moth case as a "myth," in the sense of "not an account of objective reality" (Wells, 2000, p. 1) and "no better than alchemy" (p. 155). That is, he denies that it provides evidence for evolution. Why? In his arguments, Wells points disparagingly to every uncertainty and discrepancy in the evidence. For Wells, if the evidence does not match the story as told in the textbook, then the *scientific* conclusions are wrong. Uncertainties, doubts and lack of unambiguous evidence mean, simply, that the evidence is "impeached" (p. 151). Discrepancies count as outright flaws. No qualifications are allowed. No nuances in interpreting the evidence or considering multiple causal factors. This approach seems to take seriously the "discipline" of science (p. 2). There is no room for ambiguity or resolving complex evidence. Science educators may recoil in horror. But in the preface to his creationist tract, Wells admits that even through graduate school he believed almost everything he read in his textbooks as true, plain and simple (p. xi). For Wells, science seems black and white. And the result, in his case, appears to be rejection of evolution, because the real science does not match exactly the textbook ideal. The Kettlewell case may thus alert educators to the potential consequences of casting the process of science as black-and-white—like the peppered moths one sees in the textbooks.

CASE 3: ALEXANDER FLEMING

My third case is perhaps the most celebrated example of chance, or accident, in science: the discovery of penicillin by Alexander Fleming. In the conventional story (e.g., Ho, 1999; WGBH, 1998), a stray mold spore borne through an open window landed on an exposed bacterial culture. Then, as *Time* reports in its *100 Persons of the Century* (Ho, 1999):

> Staphylococcus bacteria grew like a lawn, covering the entire plate—except for the area surrounding the moldy contaminant. Seeing that halo was Fleming's "Eureka" moment, an instant of great personal insight and deductive reasoning . . . It was a discovery that would change the course of history. The active ingredient in that mold, which Fleming named penicillin, turned out to be an infection-fighting agent of enormous potency. When it was finally recognized for what it was—the most efficacious life-saving drug in the world—penicillin would alter forever the treatment of bacterial infections. By the middle of the century, Fleming's discovery had spawned a huge pharmaceutical industry, churning out synthetic penicillins that would conquer some of mankind's most ancient scourges, including syphilis, gangrene, and tuberculosis.

Fleming himself often underscored the role of chance in his work. In receiving numerous honors, he was fond of reminding others, "I did not invent penicillin. Nature did that. I only discovered it by accident." Fleming, as hero, is a role model: Someone who had the insight to capitalize on a chance observation, consequently giving health, even lives, to millions. This story is deeply misleading, even where not demonstrably false. It excludes relevant details, mischaracterizes others and arranges the narrative suggestively (Macfarlane, 1985). "Undoing" the rhetoric, I hope, shows how it creates its lesson about nature of science, especially about the roles of context and contingency.

First, consider the phrase, "when it was finally recognized for what it was." Because originally, in 1928, Fleming hardly envisioned penicillin as the great drug it later became. He did not strongly advocate treating humans with it until 1940. What happened in those 12 years? Initially, Fleming had indeed been searching for antibacterial agents. But he was not impressed with penicillin's therapeutic potential. It was not absorbed if taken orally. Taken by injection instead, it was excreted in a matter of hours. For Fleming penicillin was limited, perhaps to topical antisepsis. Hardly momentous. In the ensuing years Fleming used penicillin, but as a bacteriological tool. It suppressed the growth of certain bacterial species and allowed him to culture certain others. That became valuable for manufacturing vaccines—a major task Fleming managed at St. Mary's Hospital in the 1930s. Meanwhile, Fleming's research had turned to another group of chemicals, the sulphonamides. Without further work, Fleming's discovery would have languished, another relatively mundane scientific finding (Figure 1). Chance is reserved for Fleming's first observation, not its subsequent development. It sparks the plot, but does not let it wander without direction.

The ultimate pursuit of penicillin in treating human infections was due entirely to another lab, led by Howard Florey in Oxford. In 1938 Ernst Chain, Florey's associate, began searching for natural antibacterial agents, endeavoring to elucidate their mechanisms more fully. He chose three to study, penicillin just one among them. Chain used Fleming's 1929 paper, but with his own, quite different, purpose (Figure 1). By early 1939 Chain and Florey began to suspect the medical potential of penicillin. But because of the war effort, Florey had problems securing funds for testing. They also faced several technical challenges. They needed to improve production and purification methods, refine an assay to determine the strength of their extracts, and scale up production. After 5 months of work, all with no guarantee of success, they had enough brown powder to test on a few mice, which yielded promising results. While this work reflects the bulk of the scientific process, the traditional story consolidates it as uninteresting drudgery.

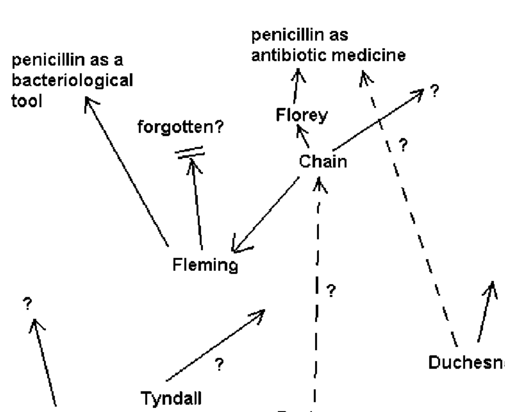

Figure 1. Alternative histories in the discovery of penicillin: history as a web, rather than a timeline.

Now, the popular story sometimes notes, "As the world took notice, they swiftly demonstrated that injections of penicillin caused miraculous recoveries in patients with a variety of infections" (Ho, 1999). But the work was hardly minimal. Or swift. For tests on humans, they needed substantially more penicillin. The Oxford labs culturing the mold scaled up from flasks and biscuit tins to hundreds of bedpan-like vessels stored on bookshelves. Purification turned from the laboratory to dairy equipment. After the first test they had to find ways to remove impurities that caused side effects. The tests eventually went quite well, but it had required two professors, five graduate students, and 10 assistants working almost every day of the week for several months to produce enough penicillin to treat six patients. While a narrative of science might well celebrate hard work, here the "swift" pace linking insight to triumph seems primary. Also, emphasis on the downstream work would reduce the dramatic role of the chance event as central.

Fleming noticed Florey and Chain's striking results. Yet he did not disturb his research agenda. He knew that penicillin's value still lay in economical mass production. Thus, the research—and, in a sense, the discovery—was *still* not complete, and certainly not Fleming's alone. One can now imagine the details of 3 more years of work before the U.S. could produce enough penicillin to treat a quarter-million patients per month. The ultimate achievement was indeed monumental and worth celebrating in the classroom. However, the story exaggerates the scale of Fleming's role, thereby creating a distorted image of genius in science (as true also for Mendel, Case 1). Fleming, Florey, *and* Chain all shared the Nobel Prize in 1945. If Fleming "changed the course of history" (Ho, 1999), it was not without the help of Florey, Chain and dozens, even hundreds, of technicians. An aura surrounds Fleming, like an inspiring tale of a scientist winning the lottery: vicariously, we thrill in his good fortune. But the story inflates the role of one scientist at the expense of representing how science happened.

While this episode exemplifies the role of "chance," popularizers nevertheless credit Fleming, as hero, with *noticing* the antibacterial properties: the "'Eureka' moment" that Ho (1999) described. Others, however, besides Fleming had noticed the antibacterial properties of *Penicillium*, including Joseph Lister, John Tyndall, Ernest Duschene, Louis Pasteur, and Jules Joubert (Figure 1).[4] Fleming was not as uniquely perceptive nor as singularly lucky as the popular story suggests. Moments of mythic insight may involve large doses of opportunity, context, and contingency, not just intellectual prowess. Given other circumstances, the history might not have included Fleming at all. But this history is harder to package into a compelling narrative.

Classroom histories tend to follow only a single linear plot. The narrative connects Fleming directly to the status of penicillin today. Other plot lines and scientists become invisible. The outcome thus seems inevitable. But to understand the process of science as it moves forward, the alternative futures and potential alternative discoveries are essential (Figure 1). Educators must portray "science-in-the-making," advancing blindly, not "science-made" unfolding predictably (Latour, 1987). Contingency does not define just the moment in 1928

[4] In 1871, Joseph Lister (noted for introducing antiseptic practice into surgery) found that a mold in a sample of urine seemed to be inhibiting bacterial growth. In 1875 John Tyndall reported to the Royal Society in London that a species of *Penicillium* had caused some of his bacteria to burst. In 1877 Louis Pasteur and Jules Joubert observed that airborne microorganisms could inhibit the growth of anthrax bacilli in urine that had been previously sterilized. Most dramatically, Ernest Duchesne had completed a doctoral dissertation in 1897 on the evolutionary competition among microorganisms, focusing on the interaction between *E. coli* and *Penicillium glaucum*. Duchesne reported how the mold had eliminated the bacteria in culture. He had also inoculated animals with both the mold and a lethal dose of typhoid bacilli, showing that the mold prevented the animals from contracting typhoid. He urged more research. Following his degree he went into the army and died of tuberculosis before pursuing that research. Chance, here, worked *against* his discovery bearing fruit (Judson, 1981).

when Fleming turned his attention to the discarded, now a famous culture in the tray of lysol. It permeates the whole process. It may not fit a standard plot trajectory conveniently.

In celebrating Fleming, therefore, one might focus instead on his habits: the *context* that fostered the moment so often depicted as critical. Fleming was not known for running a "tidy" lab. Abandoned cultures heaped unattended in a basin would not have been at all unusual—less "chance" than the story suggests. Such "messy" circumstances invite the unexpected. For molecular biologist Max Delbrück, this promotes discovery. He labeled it the "principle of limited sloppiness" (Fisher & Lipson, 1988, p. 184; Judson, 1981, p. 71). In addition, Fleming was accustomed to play and pursue "idle" curiosities. At first, he simply found the halo of inhibited bacterial growth *interesting*. Later in the day he toured the building trying to interest his colleagues—who were largely unimpressed: no promise of miracle cures yet. There was no "instant of great personal insight and deductive reasoning" (Ho, 1999), as dictated by the heroic plot template. Rather, personal amusement. Later, Fleming drew pictures with *Penicillium* on culture plates and watched them "develop" over several days as the bacteria grew in the negative spaces. A teacher might have pined that Fleming was frequently not "on task." A very different image of science emerges when one sees sloppiness and play as contributing significantly to Fleming's "chance" moment. The plot becomes less algorithmic.

In the traditional history, science appears to rely on an exceptional individual and a rare moment of insight to propel it forward. The fuller story reminds us, dramatically, how it might have been otherwise. The conventional story is narratively cozy. It celebrates modesty and good luck. Science appears formulaic and sure, even where the critical event is portrayed as chance. A more authentic history, however, reveals the many contingencies and contextual factors that shape the scientific enterprise through multitude potential pathways (Figure 1).

CASE 4: IGNAZ SEMMELWEIS

My fourth case concerns tragic death from childbed fever in Vienna in the mid-1800s and the discovery of the importance of handwashing by Ignaz Semmelweis (Carter, 1983). Here is how the case appeared recently in the Journal of College Science Teaching (Colyer, 2000) and on a major website of case studies[5]:

> Ignaz Semmelweis, a young Hungarian doctor working in the obstetrical ward of Vienna General Hospital in the late 1840s, was dismayed . . . that nearly 20% of the women under his and his colleagues' care . . . died shortly after childbirth. [Students pause here to hypothesize causes.]
>
> One day, Semmelweis and some of his colleagues were . . . performing autopsies . . . One of Semmelweis' friends . . . punctured his finger with the scalpel. Days later, [he] became quite sick, showing symptoms not unlike those of childbed fever. [Students are again asked what to do next.]
>
> In an effort to curtail the deaths in his ward due to childbed fever, Semmelweis instituted a strict handwashing policy amongst his male medical students and physician colleagues in "Division I" of the ward . . . Mortality rates immediately dropped from 18.3% to 1.3% . . . [Students interpret.]

[5] For economy, I have edited the content heavily, while hoping to preserve the sense of the original. Colyer's approach is certainly not unique. See, for example, Episode 622 from the radio series "Engines of Our Ingenuity" (also on the web). The romanticism of popular historical narratives, especially among medical professionals, is evident in such sensational labels as "conquest," "tribute," "pioneer" and "prophet,"all found in titles of articles on Semmelweis in medical journals from the last two decades.

> Despite the dramatic reduction in the mortality rate in Semmelweis' ward, his colleagues and the greater medical community greeted his findings with hostility or dismissal . . . Semmelweis was not able to secure the teaching post he desired . . . In 1860, [he] finally published his principal work on the subject of puerperal sepsis but this, too, was dismissed . . . the years of controversy and repeated rejection of his work by the medical community caused him to suffer a mental breakdown. Semmelweis died in 1865 in an Austrian mental institution. Some believe that his own death was ironically caused by puerperal sepsis.

Semmelweis is the quintessential scientific hero, defending scientific truth in the face of adversity. The predominant tone is tragic. But the lessons about science are essentially the same, albeit often inverted. The exercise above aims appropriately at reviving science-in-the-making (Case 3) by engaging students in situated decision-making. Still, a sense of drama is crucial. Making the story compelling largely depends on suppressing relevant facts and perspectives. It is worth considering how the history is traded for drama.

First, authors sometimes portray Semmelweis as *the* person who noticed the problem of childbed fever, although the reputation of the Division I ward was notorious, even among patients. This creates a stronger protagonist. But it also inflates his genius.

Second, in this case we hear how Semmelweis noticed his colleague's wound during an autopsy, implying his subsequent insight about his illness was immediate and clear. In fact, he would have noticed the illness *first*, without context, and had to puzzle in reconstructing its cause. Why would he have even suspected the cadaveric material? Perhaps he already had an inkling. Gradual realization, however, does not drive a plot quite as well as an "aha!" insight.

In this episode, a key element is the rejection of Semmelweis's conclusions. Thus, in conventional stories, the critics were wrong. All wrong. Anything less would diminish Semmelweis's stature. In a sharp dichotomy, the evidence favors Semmelweis exclusively, while "unscientific" factors (must?) bias his critics. Authors cast the negative response as permeated with personal prejudice and social ideology. Some commentators note that Semmelweis was Hungarian and portray him as a victim of Austrian xenophobia. They disregard the contemporary intellectual context, however. Vienna was then viewed as "the Mecca of Medicine." Therapeutic caution had emerged, correcting earlier excesses of bloodletting, purgatives, etc. (Johnston, 1972). Our modern practices of diagnostics and loose bandaging of wounds began here. Many thus found Semmelweis's results empty. He did not identify what caused the disease, for example. Without knowing the cause, one could easily err—and be diverted from searching for the real cause. With no concept of germ theory (still decades away), Semmelweis's peers may thus have responded to his conclusions cautiously *for good reasons*. But this does not contribute to either building sympathy for the protagonist or portraying science as methodologically transparent.

Few stories mention Semmelweis's polemical, sometimes offensive, tone in discourse. Instead, his critics receive all the blame. Their attributes are all negative: xenophobia, hubris, pettiness, and hostility. The asymmetry sharpens the sense of conflict. It encourages the reader to sympathize with the main character's struggle.

Now, had Semmelweis been wrong, stories would likely hail the role of the scientific community in catching error. They would extol the social system of checks and balances in science (because it led, in retrospect, to the right answer). When the same system leads to skepticism about ideas we now consider right, one tends to find only conservatism and condemn it. What seems to matter is not profiling the process of peer review—for better or worse—but matching the outcome with a method that justifies it. Right answer?: right method. Wrong answer?: wrong method. It fits an easy narrative formula.

In this episode, the rejection of Semmelweis is typically overstated. In fact, hospitals across Europe widely (although not universally) instituted handwashing. Semmelweis was not as neglected as the typical story suggests. But suppressing this heightens the drama of vindication. The story can trumpet the triumph of science and truth, contrasted against social prejudice (inscribed also in the Mendel case).

Now, why did Semmelweis lose his position at the hospital? It may seem natural to extend the pattern of his science being rejected and it gives a sharper edge to his heroism. But documents suggest Semmelweis was caught in larger institutional power struggles. Furthermore, he was obsessed with childbed fever. Neglect of his other duties likely served as adequate excuse for dismissal. Here, the narrative tends to interpret his whole life narrowly through science, even where it may not be irrelevant.

Finally, the irony that Semmelweis succumbed to the very disease he sought to cure seems too poignant not to mention. Some narratives say he was driven to suicide, amplifying the sense of tragedy. Our best evidence indicates that guards beat him while he was trying to escape an insane asylum, leaving lethal injuries and infected wounds. But this ending is not very powerful rhetorically. Ideally, a story ends in uplifting triumph or cathartic tragedy.

Thus, many elements of popular narratives heighten the drama, even if not well informed historically. When the history misleads, of course, so too does the portrayal of science.

CASE 5: WILLIAM HARVEY

Finally, consider the case of William Harvey and the circulation of the blood (Pagel, 1967, 1976). Harvey, physician to royalty, claimed that the blood did not move on its own to its "natural place," but was propelled by the action of the heart. Moreover, blood is not merely used up in the extremities. Rather, it continues to flow as in a natural cycle. Harvey's conceptual achievement was certainly recognized by his peers, although, one might note, not without some particular disputes. He also epitomized the emergence of experimental investigation in the early 1600s.

Here I focus on an account by Lawson (2000), which uses history explicitly to promote a particular view of the process of science. Harvey is presented as an example of the centrality of hypothetico–deductive reasoning in science. But why history? Here, history is a critical persuasive tactic. By inscribing the philosophical perspective into the work of a renowned scientist, an author gives it the semblance of "naturalness" or authority (recall Sapp's comments on Mendel, Case 1). An imaginary example simply does not carry the same cachet. In this case, Harvey is first established as the desired authority by dramatizing his discovery (Lawson, 2000, p. 482):

> Galen's theory of blood flow was virtually unquestioned for nearly 1500 years until 1628 when the English physician William Harvey . . . published a book."[6]

Here, the monumental time scale (over a millennium!) functions to impress us with the scale and singularity of the achievement. Yet this apparently modest statement collapses contributions from several physicians over a century into just one person: William Harvey. Michael Servetus in 1553, Realdus Columbus 6 years later, and Andreas Cesalpius in 1603 each claimed that blood follows the "pulmonary transit," although each for a different

[6] Harvey's discoveries were actually mostly complete by 1616, when he started lecturing about them. Publication followed years later. Misdating seems like a minor quibble, but it confuses the publication with the research itself, a point echoed below.

reason.[7] All questioned Galen's authority. And each introduced new ideas about circulating blood flow. Moreover, with wider scope, one finds that Ibn al-Nafis discussed pulmonary blood flow in the 1200s, during the Golden Age of Arabic science. Cultural slight and historical details aside, the magnitude of Harvey's achievement has been grossly exaggerated. Here, a reader can see more clearly how the inflated genius (Cases 1, 3, 4) is part of a persuasive strategy. The implicit lesson for the reader?: Harvey possessed some special form of reasoning, which his peers did not, that is critical to success in science.

Lawson's account emphasizes especially Harvey's reasoning against Galen. Galen believed that blood must flow from the heart to the lungs, and that some blood flowed back to the other side of the heart, but he also reasoned that blood might permeate the septum of the heart directly. This is treated as somewhat astonishing, even outlandish. Harvey, we are told, put the mistake right with hypothetico–deductive reasoning. (Never mind that Vesalius had criticized Galen on this very point based on his observations decades earlier.) The uninformed reader never learns that Galen was a pioneer in dissection. He hardly would have advanced such a claim foolishly, in absence of any observation whatsoever. Here, Galen, as straw man, fills the narrative role of adversary, or villain. We never learn how *Galen* might have reasoned, nor why *his* ideas were respected for so long. Indeed, the question does not even arise, although this presentation purports to illustrate scientific reasoning historically.

Later, the reader learns of what was supposedly Harvey's greatest triumph: the prediction of capillaries (e.g., Asimov, 1964, pp. 24, 29; Baumel & Berber, 1973, pp. 12–13; Lewis, 1988). Though no one could observe them at the time, Harvey apparently saw the bold implications of his theory:

> *If* . . . the blood flows away from the heart in the arteries, and
> *If* . . . the bloods flows towards the heart in the veins,
> *Then* . . . the arteries and the veins *must* be connected.

Harvey's impressive "if–then" reasoning, we are told, was vindicated in 1661, 14 years after his death. Here, the drama is framed to demonstrate the power of deductive—that is, "scientific" —reasoning (Lawson, 2000, pp. 483, 484). Well, this is how one might reconstruct the reasoning knowing that capillaries exist. When I began teaching, I encountered this story about Harvey's prediction and I believed it. I had not yet read Harvey's original work. In his classic *De motu cordis*, Harvey describes how blood *percolates* in the lungs and is collected as though from a sponge (Ch. 7). Blood *permeates the pores* of the flesh, he said (Ch. 10, 14). It is *absorbed* and *imbibed from every part* by the veins, he echoed in a later publication (*A Second Disquisition to John Riolan*). Harvey did not reason blindly. He had dissected many "lesser" animals that have hearts but no blood vessels ("open circulatory systems," in our terminology). He had observed directly that connections were not needed. Harvey did not predict capillaries. That misattribution eclipses the 17th-century perspective in which he reasoned. Yet it fits the narrative goal of framing Harvey as a scientific hero who champions a certain style of reasoning.

Harvey supposedly further exercised if–then reasoning to frame numerous tests, as described in his landmark book. But this means reading Harvey as plainly describing the investigative process, rather than trying to persuade his readers. Indeed, it is not too difficult to discern Harvey's own rhetorical strategy. He offers numerous *demonstrations*, such as

[7] Servetus gave new importance to the air in vitalizing the blood and hence to the blood's passage through the lungs. Columbus inferred the direction of blood flow from the structure of the blood vessels. Cesalpius linked the pulmonary circuit to thinking about cycles in nature and chemical distillation (resonating with Fludd's interpretation, see later).

the one portrayed in the renowned figure of ligatured arms. These are not "tests" in the sense of inquiry or epistemic probes. They take the form:

> *If* . . . you don't believe me,
> *then* . . . do X to prove it to yourself by direct observation.

This underlies much of Harvey's if–then language. Here, the publication is mistaken for the science itself, with misleading results. Narratively, however, a method is only justified by showing how it leads to discovery.

Consider, finally, the treatment of one of Harvey's central ideas:

> Harvey's guiding analogy was . . . circular planetary orbits and the belief that large-scale planetary patterns should be echoed in smaller-scale physiological systems (p. 482).

This is the microcosm–macrocosm concept of the chemical philosophy, shared by Robert Fludd, a close friend of Harvey's. The analogy also extended to chemical reactions, strengthening the analogical resonance. Harvey used this image throughout his book, sometimes explicitly as an argument. He described the heart as the sun of the microcosm, giving warmth and life to the body. That seems very strange to us today–and decidedly "unscientific." Yet historians document that this analogy was *integral* to Harvey's very *reasoning*. Some may want to discount that this microcosm worldview could lead Harvey to "discover" something we now regard as true. No doubt because the analogy is false by today's standards. Attributing it to Harvey appears to lessen his status as a scientist. But it is coupled with Harvey's achievement. It is essential if we want to understand scientific reasoning, and portray it faithfully to students. But in Lawson's article, the analogy is only curtly acknowledged, then abandoned as peripheral. The historical facts, even about reasoning, seems secondary to the persuasive aims of the "historical" narrative.

Errors pervade Lawson's brief two-page treatment of history. But the errors themselves are not as important as the source of the errors. Harvey is repeatedly shoehorned into a particular method of scientific reasoning for rhetorical purposes. When one delves into history to prove a point, rather than to "listen" to what it has to say, one can easily err. Historians talk about *respecting* history—that is, regarding history (ethically) as an end, not an instrumental means towards some other goal. Historians ideally endeavor (intellectually) to find and decipher the details and complexities of historical context, and not just remap their own views onto the past. In Lawson's account one finds the persuasive elements of the monumentalized Harvey and the historical errors intimately coupled. Mythic grandeur and misconception arise together. That is what constitutes a *myth-conception*.

THE ARCHITECTURE OF SCIENTIFIC MYTHS

Ultimately, historical reconstructions of scientific discovery—whether of Mendel, Fleming, Kettlewell, Semmelweis, or Harvey—designed to fit certain narrative patterns are the source of myth-conceptions in science. While the cases I've recounted are all biological, I trust that they are so familiar that even nonbiologists can recognize them and appreciate the analysis. One can equally find these trends in stories of Antoine Lavoisier and oxygen, Alfred Wegener and continental drift, Galileo and his advocacy of Copernicanism, or Isaac Newton and his optics. All narratives of science and of scientists risk drifting into myth. Teachers need to be mindful to respect history. In the spirit of recent reforms in science education, educators need to jettison mythic history if they want to portray the nature of science faithfully.[8]

In profiling the historical misperceptions in the five case-myths above, I do not mean to imply that every science teacher should be a professional historian. My primary concern *here* is not historical accuracy per se, but the mythic style.[9] The lesson is not in the details of each history, but in their common narrative elements. Namely, what transforms plain stories into myths? Particulars aside, what features characterize this type of history? Can one generalize its internal structure? Teachers who understand the rhetorical dimension of stories, I hope, will be better able to regulate how their own storytelling affects students. Here, then, my emphasis shifts from historical to literary, or rhetorical, analysis. What is the architecture of scientific myths? (See Footnote 1.)

Monumentality

First, all the scientists, as literary characters, are larger than life. They are heroic (Milne, 1998). Their personality exudes virtue. They exhibit no character flaws. For example, as scientists, they do not err. Also, as I have noted repeatedly, their achievements are inflated. Discoveries that, historically, were gradual and distributed over several persons are concentrated in one person, and often in one momentous insight. Historians have long criticized hagiographies, idolizing biographies that willfully omit any trait deemed negative. But the cases here go beyond merely "sanitizing" history. They introduce historical error and transform human scientists into superhuman characters. The scientists thus share with their wholly fictional literary counterparts the traits of heroes, legends, and sometimes even gods. Their monumental features serve a major function: to engage the reader.

For some, these mythic, superhuman characters function as role models that inspire students. Paradoxically, this seems to subvert the current goal of portraying science "as a *human* endeavor" (National Research Council, 1995, pp. 200–201, emphasis added; see also Rutherford & Ahlgren, 1990, pp. 9–13). The situation is certainly more complex than when Brush (1974) famously suggested that history of science be rated "X" in the classroom. Brush was concerned that students exposed to real, non-mythic scientists might not want to become scientists themselves. More recently, Brush (2002) has partly "recanted" and now considers *some* scientists, at least, to be good role models. But Brush has focused primarily on recruitment, and he has not addressed directly the topic of misrepresenting scientists or exaggerating their achievements. In any event, educational goals now address science for *all* students. Moreover, they incorporate history of science in roles other than as a vehicle for recruitment (National Research Council, 1995, pp. 2, 200–204; see also Rutherford & Ahlgren, 1990, pp. v–xi, 9–13, 135–153). Still, the role of history—or *mythic* history—in providing role models may need to be addressed.

First, the assumption that role models must be universally positive has yet to be fully studied. Certainly, *some* anecdotal evidence suggests that *some* scientists have been inspired by such myths. But we do not know whether the mythic image alone was causally significant

[8] The phrase "nature of science" is ambiguous. It may be either descriptive or normative. That is, the educational objective may be to teach science *as an idealized process* or *as it is practiced*. There is a tension because, historically, science has not always realized the ideal. For my part, professional ethics seems to mandate that teachers portray (without endorsing) science as it is practiced in a real, human—even if imperfect—world. Teachers should also articulate the ideal "nature of science" and discuss how and why it may vary from the real.

[9] Of course, I certainly hope that teachers endeavor to respect historical facts as much as scientific facts. For example, many favorite classroom anecdotes of scientists are apocryphal. Here, I focus on what might cue the inexperienced teacher when to question such stories.

(such individuals may already have been oriented towards science for other reasons). We do not know if great, but less hyperbolic figures may serve the same role. Nor do we know whether the myths are significant across the entire population of scientists. Many, or even most, scientists may well be inspired by other factors. For example, other historical evidence (also anecdotal) indicates the importance of simple encouragement, even where no role model existed (for example, in the cases of some women and minorities). At the very least, educators need substantially better research on the causal relationship between mythic characters as role models and recruitment in science before asserting its importance.

Further, we do not know how many potential scientists are alienated by such myths. In what ways (that may now go unnoticed) do these mythic figures *discourage* students from pursuing science? They may be *negative* role models. That is, a student with a keen interest in science (but perhaps unproven ability) may infer that she or he cannot make a meaningful contribution, so why try? I have certainly encountered students who complain that "science is only for geniuses." Research results on attitudes towards science and recruitment should certainly be interpreted in terms of different types of history (e.g., Martin & Brouwer, 1991). Further, the assumption that heroes must be perfect to be role models may well be questioned. Indeed, they are much more human and real—and more accessible to students, I contend—if educators acknowledge their flaws and limits as well as their triumphs (Milne, 1988, p. 184).

In addition, focusing exclusively on recruitment upstages questions of establishing "role models" in other contexts. That is, these mythic figures also generate expectations for how scientists should perform in society. They are role models of a very different kind. How much is public sentiment towards science shaped by failure of scientists to meet the standards of the myth? How do the superhuman images of scientists shape public discourse on issues that are informed by science? Again, more research would be helpful. In any event, teachers ought not to confuse mere engagement with larger-than-life characters as personal inspiration. In summary, the importance of mythic scientists as role models in education is not clear.

One may, nonetheless, interpret their role within the mythic narratives themselves. Because it is not just the characters that are monumental. Everything is grand in scale. For example, Fleming did not just discover the antibacterial properties of some fungus. No, he "*conquered some of mankind's most ancient scourges*" (Ho, 1999). Harvey did not discover circulation merely. Rather, he rescued us from a mistake that had persisted *for 1500 years*. Kettlewell, by his own popular account (1959), discovered "Darwin's missing evidence." He thus apparently secured evolutionary theory from a century-long vulnerability. A single scientific study seems to have immense significance. All this has strong implications for the relationship between the storyteller and the hearer/reader. The storyteller feels more valued (and more powerful) by telling an important story. Likewise, the reader is impressed by its significance. The mutually favorable emotions seem to validate the story—and foster a similar story the next time.

Moreover, when cast in a grand scale, a story is more easily interpreted as representing all science. For Lawson, at least, this is explicit: Harvey's reasoning should illustrate an important principle about science generally. These narratives of science, then, are "mythic" in proportion, both cognitively and affectively. Ultimately, this monumentalism amplifies the importance of the story—and whatever "moral" it contains.

Idealization

A second architectural feature of myths in science is idealization. The case-histories exemplify phenomena about storytelling familiar to psychologists: *sharpening* and *leveling* (Gilovich, 1991, pp. 90–94):

> What the speaker construes to be the gist of the message is emphasized or "sharpened," whereas details thought to be less essential are de-emphasized or "leveled."

Qualifications are lost. Extremes emerge. What remains is a black-and-white image of science, rendered quite literally in the case of the peppered moths. The intermediate *insularia* moths are leveled. So, too, are the logistics of field tests. Meanwhile, the contrast between the Birmingham and Dorset woods is sharpened. So, too, is the clarity of the experimental control. In Semmelweis's case, the intellectual and social contexts directly relevant to assessing his claims are not just leveled, but *razed*. Mendel's and Harvey's precursors are leveled, while the scientific heroes are sharpened. Sharpening and leveling *for the sake of telling a "good story"* easily leads to misleading oversimplification.

One particular consequence of simplified narratives is a streamlined plotline. The multiple lineages of thought and action that characterize a *web* of history (Figure 1) are reduced to a single *timeline*. Stories, like those about penicillin, are reduced to the "essentials" linking the past to the present. Fleming's role becomes sharpened. Duchesne's and others' become leveled. Although many will acknowledge that science involves trial and error, stories of science rarely include blind alleys (except for dramatic effect). The resulting sequence of events, leading item by item to the discovery and then to its meaning to us today, is all too easily interpreted as inevitable. Historians have long criticized Whiggism: interpreting (or rewriting) history as leading to—and justifying—current states of affairs (Butterfield, 1959; Kuhn, 1970, pp. 136–143). Whiggism is conventionally construed as a political bias. Here I am suggesting, instead, that it may also be due to *rhetorical* bias. The storytelling tendency to sharpen and level leads to minimizing plotlines and excluding the alternatives. Hence, the narratives understate the uncertainty and mislead about the process as it moves forward. Teaching about the nature of science would suffer accordingly.

As stories are shaped to heighten their apparent informativeness, certain types of details will tend to be lost. The particulars of the discovery—details of time, place and culture, contingencies of personality, biographical background, coincident meetings, etc.—become secondary. For example, the myths highlight "positive" contributions. Errors or "failures" are eclipsed. Mendel's 15 "confusing" pea traits are forgotten. Harvey's microcosm analogy is discounted. The story about Semmelweis frequently omits all the possibilities that he first considered, then (critically) ruled out. All these details seem to drag down the plot. In a good story, the pace is exhilarating. As a result, stories tend to preserve just the elements needed to justify the outcome *narratively*. Indeed, this might seem appropriate if one intends the history as a lesson in the nature of science.

The stories of science thereby become idealized and universalized, in accord with their monumental status (above). Although the achievement of any given mythic scientist is singular, their methods are cast as transcending their particular occurrences (Milne, 1998, pp. 178–179). They illustrate a method of science, writ large. Here, the details or contingencies cannot be too important, lest they subvert the general lesson. Consequently, the idealized *narratives* foster the conventional *school philosophy* of a scientific method, in the sense of an algorithm guaranteed to find the truth. Bauer (1992) profiled well many deficits in the standard account of "the scientific method." He even labeled it a "myth." Yet Bauer did not consider the role of narratives. In my view, the persistence of perceptions of an algorithmic scientific method is strongly linked to the attractiveness of the myths that sustain it. By myths, of course, I mean the narratives of science in which it is inscribed. Telling these stories surely perpetuates the mythic method by embodying it. But I suggest further that the storytelling tendencies may themselves be a source of the problem. In my view, educators should reconsider the power and impact of these narratives.

The power of the idealization in narrative is especially evident in the various errors it generates. While the stories are all about history—events that happened—they sometimes drift into stories of what "should" have happened. Witness Harvey's imagined prediction of capillaries. Or Mendel's Second Law. Or Fleming's posture on penicillin's use for humans. Sometimes, the desire for a cozy story may overtake the historical facts. Simplification may seem inevitable in education. Simple concepts, even if flawed by overgeneralization, seem essential foundations. However, this leaves educators with the additional responsibility of articulating how such simple concepts can "lie" (Allchin, 2001a).

While method is unquestionably important in science, the mythic structure oversimplifies the process. It seems flawless. When coupled with the monumental scale, it overstates the guarantee on the claims it generates. With no place for error, except as pathology, the process appears more efficient than it actually is. In failing to understand the work of scientists, people sometimes expect too much of science. Like Wells (Case 2) perhaps, they can feel betrayed when science does not perform according to the idealized account. Recently, several individuals have sued scientists for making mistakes (Steinbach, 1998). What fostered such a stark frame of mind that expects science never to err? Did textbook accounts of famous discoveries help shape their thinking? Virtually all the recent calls to promote "scientific literacy" appeal to the role of science in social decision-making. Most such issues are quite complex. They often deal with scientific uncertainty and incomplete and/or conflicting results (Anand, 2002). Yet partisans of a particular view appeal to science in simplistic black-and-white terms (nicely profiled in Martin's account of the fluoridation controversy [Martin, 1991] and in Toumey's analyses of creationism, cold fusion and HIV-testing [Toumey, 1996]). Their expectations seem to echo the idealized classroom-histories. Again, there is opportunity for more research on how public sentiment about science is related to the implicit promise of scientific myths. In seeking to remedy misimpressions about science, as recommended in recent reforms, applications of history of science should be a solution, not a source of the problem.

Affective Drama

A third element of the mythic architecture is literary techniques whose purpose is entertainment and persuasion. One may enhance a story's power to engage and persuade with many *rhetorical devices*—that is, literary constructs or familiar plot patterns. They intensify images, heighten drama, and deepen the aesthetic response. They make a story more compelling, possibly even more persuasive or believable. Through their emotional effect, the stories become more *memorable*. I suspect, this is one reason why the culture perpetuates the myths even though they are false or misleading: simply because we remember them and enjoy telling and re-telling them (Milne, 1998, p. 177). But it is all in the literary craft: the style, the plot construction, relationships among characters, word choice, etc. Among these rhetorical devices—and I hope that this phrase enters the lexicon of science educators—one may list

- The thrill of the moment of discovery (the stereotypical light-bulb cartoon)
- Vindication
- The surprise of chance
- The reward of integrity (loyalty to evidence, resistance to social prejudice)
- Shame (for example, challenging an ultimately correct idea)
- Tragic irony

Truth always triumphs, but typically only after dramatic conflict. The "aha!" phenomenon deserves special note. Nothing drives a discovery plot quite like a well framed "eureka!"

For added effect, it comes in the wake of despair. Another very strong rhetorical device, epitomized in melodrama, is amplifying the good by contrasting it with the bad: hero versus adversary, scientist versus suppressor of the truth, Harvey versus Galen, Semmelweis versus his Austrian critics, Darwin versus Lamarck, Lavoisier versus the phlogistonists, Galileo versus the Church, and so on. I hope the concept of rhetorical devices becomes a standard and familiar element in educating teachers. Good teachers, I think, understand what elements make stories persuasive—and manage them responsibly in their own storytelling.

Explanatory and Justificatory Narrative

The final element that makes these histories mythic is their explanatory role. They are not "just" stories of science. They are "just-so" stories of science. Like Kipling's fables—"How the Leopard Got His Spots," etc. (Kipling, 1902)—they explain a certain outcome through narrative. Every history—every story—has an implicit "lesson," or moral. Historical tales of science implicitly model the scientific process by showing how a series of events *leads to* a certain result: in our cases, a renowned scientific finding. The narrative inherently couples process and product. As idealized accounts, most are rational reconstructions and serve to justify the authority of the scientific conclusion. Right method, right ideas. Wrong method, wrong ideas. The story of a discovery *explains*, narratively, the methods of science and, hence, the authority of science.

The architecture of scientific myths, then, ultimately serves a function of explaining and justifying the authority of science. The elements conspire together to collapse the nature of science into an all too familiar just-so story of "How Science Finds the Truth":

- Science unfolds by a special method, independent of contingencies, context, or values.
- All experiments are well designed and forestall any mistakes.
- Interpreting evidence is unproblematic, and yields yes-or-no answers.
- Achievement relies on privileged intellect. (Scientists are special, extraordinary people, whose authority is beyond question.)

Thus:

- Science leads surely and inevitably (and uniquely) to the truth, without uncertainty or error. (Anything less abandons objectivity and reduces to relativism.)

No one need critique these features yet again. They have also already been labeled as "myth" (Bauer, 1992). I wish to highlight, however, how they tend to derive legitimacy from the explanatory power of the narratives. Because of the *monumental scale*, the resulting authority is monolithic. Because of the *idealization*, simple method seems sufficient to account for all scientific achievement. Because of the *rhetorical devices of affective drama*, the features, however misleading, are immensely persuasive and emotionally commanding.

Myths of science also exhibit another important, related feature of classical mythology. Traditional myths often explain natural phenomena—e.g. the movement of the sun, the seasons, the rainbow, etc.—through the actions of human-like gods. While appearing to interpret nature, the myths also, conversely, inscribe human behavior in nature. Thus, the myths function implicitly to legitimize certain actions or norms of human conduct by framing them as "natural." In a similar way (especially clear in Harvey's case), particular views of science benefit by being inscribed in history. The historical tale is not just an illustration. It is a persuasive tactic. The author's view of scientific norms seems to emerge naturally from the history. Here, the myth's lessons derive status from the recognized value of historical scientific achievements. But the act of persuasion is not betrayed by any "arguments." The story conceals the rhetorical work. The reader focused on the story sees the history and the norms as real causes. Unless trained, they rarely see the framework for composing the story. This is why histories are more potent cognitively than mere descriptions of sciences and its methods. The architecture is invisible. Sometimes even to the storytellers themselves.

Myths of science are myths, not just idle stories, because their architectures—the syndrome of elements including monumentalism, idealization, rhetorically crafted drama—are all designed to explain and buttress the unqualified authority of science.

STRATEGIES

In the Introduction section, I suggested that educators need to replace the *types* of history that students learn. It is the mythic "classroom histories" that mislead students. They distort the nature of science, even as they purport to show how science works (see Footnote 8). They are *pseudohistory* of science (Allchin, 2001c). Like pseudoscience, they foster false beliefs about science—especially about the nature and limits of scientific authority. One might imagine that the only solution would be to purge science textbooks—and the culture at large—of all historical error. However, such a utopian goal is hardly necessary. Nor does every teacher need expert credentials in history at the outset—although educators might surely seek guidance from professional historians. Educators might begin with two simple strategies, profiled below. First is reflexivity. With just a few powerful analytic tools and a few good examples, one may recognize the rhetoric of myth for what it is. Second, teachers may profit from a small repertoire of discrepant myths: stories that break the conventions of mythic storytelling in science and may (like any anomaly) provoke rethinking and lead to deeper understanding.

First, then, we should equip teachers (and students) with the tools to recognize and deal with the misleading myths they will inevitably encounter. The cases above are examples. In addition, teachers new to history may rely on a few brief maxims to help evaluate any history. For example (based on the analysis above): Suspect simplicity. Beware vignettes. Embrace complexity and controversy. Discard romanticized images. Do not inflate genius. Mix celebration with critique. Scrutinize retrospective science-made. Revive science-in-the-making. Explain error without excusing it. And above all respect historical context (see Case 5). (Note that these maxims focus on the rhetoric of science stories rather than on the "nature of science" directly.) For those who like mnemonic devices, one may express the "SOURCE" of the problem and the "SOURCE" of the solution as summarized in Figure 2. Thus, while we may not ever eradicate mythic narratives about science, one might nevertheless be able to neutralize them. Analytical tools empower teachers to recognize myths and regulate their effect.

Mythic Narratives	***Nature-of-Science History***
Science-made	**S**cience-in-the-making
Overinflated genius	**O**pportunities
Unqualified Universality	**U**ncertainties
Retrospect, Romanticism	**R**espect for historical context
Caricatures	**C**ontingency, Complexity, Controversy
Expected results & Excuses	**E**rror Explained

Figure 2. Comparison of sample features characterizing mythic history and history that portrays nature of science more informatively, using the mnemonic "SOURCE."

Second, teachers may benefit from alternative stories of science that break the norms of the mythic structure and hence begin to expose the conventions at work. One core of the mythic architecture is a simple narrative formula for science

$$\text{right methods} \Rightarrow \text{right conclusion}$$

Viewing scientific methods as foolproof is a caricature, of course. Science proceeds by trial and error, we often hear. Everyone seems willing to acknowledge that science is fallible. This means, of course, acknowledging that scientists can err. It means acknowledging *honestly* that *good* scientists can err—even Nobel Prize winners (Darden, 1998). We cannot gently excuse the errors or explain them away, as in the myths. Nor can we cast all mistakes in science as fraud or as aberrant pathology (e.g., Dolby, 1996; Langmuir, 1989; Rousseau, 1992; Youngson, 1998). It is not just that scientists lapse from some ideal method. The "right" methods do not always yield right ideas. Sometimes

$$\text{right methods} \Rightarrow \text{wrong conclusion}$$

Hence, even a single case of fallibility, well articulated, may serve as a corrective to the mythic caricature. Ideally, educators should introduce *some* histories that chronicle how evidence at one time led reasonably to conclusions that were only later regarded as incorrect (Hagen, Allchin, & Singer, 1996, pp. 116–127). This is how teachers can *explain* the limits of science without vague handwaving about skepticism or tentativeness. They must help undo the myth-conception and show how doing science can, on occasions, lead to error. We must explain the error, not excuse it (Allchin, 2001b). Ideally, educators will also show what allowed scientists later to recognize a mistake and remedy it. Narratives of error and recovery from error, I claim, convey *both* what justifies *and* what limits scientific conclusions.

Myths of science are unquestionably seductive. They tempt the teacher eager to engage students. They entertain. On the surface, they seem to inform. These are reasons why the *mythic* forms of the history of science already haunt the classroom and our culture at large. But they are misleading. They do not promote understanding of the process of science or nature of science. Contrary to reform claims (see Introduction), having *more* of them solves nothing.

We need to promote less mythic narrative frameworks instead. While less monumental, they may still be equally dramatic and humanly inspiring. While less idealized, they may still serve as modest exemplars (in concert with others) for understanding the process of science. Other rhetorical devices may evoke responses: the excitement of opportunities, the suspense of persistent uncertainty, the reward of hard work, the surprising significance of "trivial" events, the tension of even-handed debates, the tragic consequences of human limitations, and the aesthetic of resolving error. The new stories will celebrate insight achieved through perseverance, creative interpretation of evidence, and shrewd insights enabled by depth of experience. They will reflect how scientific conclusions are assembled, how they are challenged, how error can occur and how knowledge is sometimes revised. Alternative narratives of science need not reduce the greatness of scientific achievement. But, ideally, they will also portray equally both the foundations and limits of scientific authority and foster deep understanding of the nature of science. Effective histories of science will avoid engendering myth-conceptions.

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