Correcting the "self-correcting" mythos of science

Douglas Allchin *

Abstract: In standard characterizations, science is self-correcting. Scientists examine each other's work skeptically, try to replicate important discoveries, and thereby expose latent errors. Thus, while science is tentative, it also seems to have a system for correcting whatever mistakes arise. It powerfully explains and justifies the authority of science. Self-correction thus often serves emblematically in promoting science as a superior form of knowledge. But errors can and do occur. Some errors remain uncorrected for long periods. I present five sets of historical observations that indicate a need to rethink the widespread mythos of self-correction. First, some errors persist for decades, wholly undetected. Second, many errors seem corrected by independent happenstance, not by any methodical appraisal. Third, some errors have been "corrected" in a cascade of successive errors that did not effectively remedy the ultimate source of the error. Fourth, some errors have fostered further serious errors without the first error being noticed. Finally, some corrections to erroneous theories have themselves been rejected when initially presented. In all these cases, scientists failed to identify and correct the errors in a timely manner, or according to any uniform self-correcting mechanism. These historical perspectives underscore that error correction in science requires epistemic work. We need deeper understanding of errors, through the emerging field of error analytics.
Keywords: scientific error; self-correction; error cascade; compounded error; error analytics

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* The Minnesota Center for the Philosophy of Science and STEM Education Center. University of Minnesota, Minneapolis, MN, U.S.A, ZIP 55455. E-mail: allch001@umn.edu

Corrigindo mitos "autocorrigíveis" da ciência

Resumo: Em caracterizações padrão, a ciência é autocorrigível. Os cientistas examinam o trabalho uns dos outros ceticamente, tentam repetir descobertas importantes e por isso expõem erros latentes. Assim, embora a ciência seja tentativa, parece também haver um sistema para corrigir quaisquer erros que apareçam. Isso explica poderosamente e justifica a autoridade da ciência. Autocorreção portanto serve em geral emblematicamente para promover a ciência como uma forma superior de conhecimento. Mas erros podem ocorrer e ocorrem. Alguns erros permanecem sem correção por longos períodos. Eu apresento aqui cinco casos de observações históricas que indicam uma necessidade de repensar o mito largamente difundido da autocorreção. Primeiro, alguns erros persistem por décadas, completamente despercebidos. Segundo, muitos erros parecem corrigidos por casualidade independente, não por alguma avaliação metodológica. Terceiro, alguns erros têm sido "corrigidos" em uma cascata de erros sucessivos que não remediam efetivamente a fonte final do erro. Quarto, alguns erros promoveram erros posteriores, mais graves, sem que o primeiro tenha sido notado. Finalmente, algumas correções a teorias erradas foram elas próprias rejeitadas quando inicialmente apresentadas. Em todos esses casos, os cientistas falharam em identificar e corrigir os erros em tempo hábil ou de acordo com algum mecanismo uniforme de autocorreção. Essas perspectivas históricas ressaltam que a correção do erro na ciência requer um trabalho epistêmico. Nós precisamos de uma compreensão mais aprofundada do erros, por meio do campo emergente da analítica do erro.

Palavras-chave: erros científicos; autocorreção; erro em cascata; erro composto; analítica do erro

1 INTRODUCTION

In standard characterizations, science is self-correcting. Scientists examine each other's work skeptically, try to replicate important discoveries, and thereby expose latent errors. Thus, while science is tentative, it also seems to have a system for correcting whatever mistakes arise. In addition, because scientists police themselves, fraud is rare. It powerfully explains and justifies the authority of science. Self-correction thus often serves as an emblem in promoting science as a superior form of knowledge.

But this potent element of the scientific mythos is ill informed. While correction can and does occur, science has no inherent mechanism for self-correction. Errors can persist, sometimes famously for decades. And sometimes with adverse cultural consequences. The "self-correcting" image itself needs correcting.

My concern here stems from a larger project on understanding error in science. How do errors arise? How are they discovered? – Or not. How are they ultimately remedied, or resolved? When should the public view public scientific cautiously? And how can we diagnose or assess the scope of potential errors? I call this project error analytics. Many people tell me that it is irrelevant – precisely because science is already self-correcting. So this is where reflection begins.

2 A COMEDY OF ERRORS

Let me open today with a historical case that shows rather plainly, I think, that something is amiss in the imagined ideal of self-correction. The example involves a series of errors, ironically compounded on one another. Like Shakespeare's play, it is "a comedy of errors". In Shakespeare's version, twins (with twin servants), each separated at birth, converge unbeknownst to each other in the same town. Mistaken identity leads to miscommunication. More mistaken identity follows, with more misdelivered messages and yet more misinterpretations. Hilarious consequences ensue. It is a stock comedic formula in modern entertainment. A character first makes an unintentional error. Then ironically, in trying to correct it, things only get laughably worse. Such was the case with Joseph Priestley's 18th-century discovery of the restoration of air by plants (Nash, 1957; Schofield, 2004; Johnson, 2008).

The story begins in the early 1770s, in Leeds, England. Priestley – minister, avid experimentalist, and self-taught chemist – had been investigating various kinds of air. At this time, he was examining various ways of making air noxious: with dead mice or rotting cabbage, by burning charcoal or candles, by mice breathing (all processes that exhaust the oxygen, in today's terms). Such "air" would not support animal respiration. What was the nature of this "air" and how might its goodness be restored? Priestley, who liked to play with variations of his experiments, investigated the possible airs emitted by plants, as well. In a now famous passage, he later recalled:

> On the 17th of August 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the

> 27th of the same month, another candle burned perfectly well in it. This experiment I repeated, with the least variation in the event, not less than eight or ten times in the remainder of the summer. (Priestley, 1781, pp. 52-53)

Then he tested just oil of mint, to see if the effect was caused merely by the plant's aromatic "effluvia". It was not. Subsequently, he tried the experiment with balm, groundsel, and spinach. All modified the air to support sustained burning. Animals, too, could breathe longer in the treated air. Plants, Priestley had found, could restore the "goodness" of the air depleted by respiration or combustion.

Others were eager to build on Priestley's discovery about plants and the restoration of air. But they could not always get the same results. Today, we might say that they failed to replicate his experiment. That created confusion. Priestley returned to his own experiments a half-decade later. By then he had moved to a new city. Like others, he could not consistently obtain his earlier results. Indeed, in some cases, the plants now seemed to *worsen* the quality of the air! His original claims seemed in question. Should he "retract" them? Priestley had already received the prestigious Copley Medal for his work. His findings had been praised by the President of the Royal Society. And the original conclusions fit comfortably with his religious belief that nature was designed for human life. He thus discounted the significance of the negative results:

> […] one clear instance of the melioration of air in these circumstances should weigh against a hundred cases in which the air is made worse by it. (Priestley quoted in Nash, 1957, p. 360)

Once the "discovery" had been made, Priestley seemed reluctant to acknowledge any error.

Priestley persisted. Eventually he noticed the effect of sunlight. His original workspace had a window, the new one did not. Priestley now had a new relevant variable. Was this the triumphant "discovery" of the role of light in photosynthesis? No. Here, the story becomes a comedy of errors. Priestley wondered if light alone – *not* plant life – was key. He tried simple samples of well water exposed to sunlight, without plants in them. They, too, yielded the "purer", more respirable air. Priestley now felt confident that he had identified the source of error in his original work. The process of restoring the air, he concluded, was related to light, not plants! Error resolved. Or so it seemed to Priestley.

Ironically, the newly revised conclusion was the error. Here, we shift focus to Jan Ingenhousz who noticed that the well water left in sunlight also generates a green scum. He and others connected the green scum to green plants. With further microscopic analysis they concluded that the scum was living algae. So, microscopic plants had transformed the air. But only in light, they now realized. Ingenhousz demonstrated the connection more fully through an extensive series of controlled tests. Both green plants and light together were needed to restore the air, not one or the other. Ingenhousz, and then others, also saw that plants producing good air in light was opposite to burning plants, which used up good air and released light. The plants were absorbing the light somehow to make fuel. That coincided with restoring the air. It was the reverse of combustion. Here, correcting Priestley's successive errors led to the modern discovery of photosynthesis.

Priestley had noticed the green scum, too. But he had considered it secondary. No light, no bubbles; no bubbles, no scum. In retrospect, Priestley's experimental results were ripe for mistaken identity. Correlation could resemble causation, in two ways. First, the light seemed directly responsible for the restored air. Priestley saw, but did not appreciate the significance of the correlated green matter. Second, he thought the enriched air caused the green scum, not the other way around. We can laugh, of course, because we know how easily we, too, could have been fooled. To his credit, Priestley acknowledged his error, once the new explanation had been clearly demonstrated.

Error resolved. Scene fades. Humor lingers.

The case of Priestley and Ingenhousz on light, plants and air conveys a complex image of science. Failure to replicate does not necessarily indicate error. And self-correction cannot be taken for granted. Correcting Priestley's errors took further investigation and evidence. It involved worldview, personal motivation, observational technology, contrasting theoretical perspectives, and continued experiment with varying conditions. Correcting error required epistemic work.

3 THE PUZZLE OF LONG-STANDING ERRORS

Let me now take a broader, more systematic view. Using history as a guide, one can see the standard view of self-correction as problematic in at least five ways. I will describe and provide a notable example of each.

First, some errors persist for decades. They are *not* corrected immediately. Errors in science do not merely erode or disappear as time passes. Scientists and others readily and confidently declare that "the truth will out" (Gilbert & Mulkay, 1984, pp. 91-111). Yet such claims rarely mention a time scale. They are typically vague and elusive. The promise is "sooner or later". Or "eventually". Or "in time". Or "ultimately". When, precisely, is "ultimately"? We should expect a "self-correcting" science that earns its name to correct itself within a reasonable amount of time. What duration of error might one reasonably expect?

Consider the case of the miscount of the number of human chromosomes (Kottler, 1974; Martin, 2004). In the 1920s prominent cytologists "determined" that the number was 48, or 24 pairs. Later that was revised to 46 (23 pairs). "Later", in this case, was 33 years. As historian Aryn Martin has put it, it is hard to keep one's dismay in check: "Can't anyone count?" Based on this case, one might justifiably "think twice" about whether "science is self-correcting".

Another example is the viceroy butterfly and the monarch as a case of Batesian mimicry, reported for over a century (Walsh & Riley, 1869; Brower, 1958; Petersen, 1964). The close resemblance of the two butterflies is easy to appreciate. The similarity is surely not due to chance alone. Mimicry through natural selection is certainly the reasonable inference. But there are two kinds of mimicry. As proposed by William Bates, an edible species may mimic a distasteful one. Alternatively, two unpalatable species may converge in appearance, as a signal to predators to avoid *both* species (Mullerian mimicry). In 1869 Benjamin Walsh and Charles V. Riley presented the viceroy as an example of Batesian mimicry, the first case from North America. They drew on their informal field observations of predation and the population sizes of viceroys compared to their closest relative. They remarked on the strong resemblance, of course. One author had even misclassified the viceroy in the same genus as the monarch! Later, biologists identified the milkweed plant as the source of the monarch's toxins, apparently confirming the story. No source of toxins seemed obvious for viceroys. The pairing became a classic case of Batesian mimicry, appearing widely in textbooks and popular culture. The obvious tests were not done until the early 1990s. Then, both species proved distasteful. They exhibit Mullerian, not Batesian, mimicry (Ritland & Brower, 1991). Yes, the error was ultimately corrected. *Ultimately*. But should a one-century delay be taken as an acceptable measure of self-correction?

Some errors seem to hide in full view. Or so it seems in retrospect. But if we cannot – or do not – detect the errors, how can science correct itself? Perhaps the longest-lasting error in the history of science is the humoral theory of bodily function, which guided medicine from ancient Greece (4th century BCE) into the middle of the 19th century. Health was attributed to the balance of four body fluids, or humors: blood, phlegm, yellow bile, and black bile. The doctor's task was to diagnose any imbalance and adjust the fluid levels accordingly. This was the origin of bloodletting. It was still a commonly accepted procedure in 1799 when U. S. President George Washington died from excessive bloodletting. Of course, the ancient Greeks considered letting out only a few fluid ounces of blood. Washington was relieved of more than 112 ounces: roughly 4 litres, or nearly half of his total blood volume. A different error, perhaps? Of course, doctors take refuge in the fact that humoral theory was "eventually" corrected. But after how many ineffective treatments and deaths?

From a modern biomedical perspective, it is hard to imagine the original power of humoral ideas. But it was accepted by no less a scholar than Leonardo da Vinci. Da Vinci dissected corpses. He studied their anatomy. And he had the fine draftsmanship skills to render just what he observed. In one drawing, however, he drew a narrow duct which, according to Galen's physiology, channeled black bile from the spleen to the liver. Of course, no such vessel exists. Da Vinci could not have seen it. Yet there it is in his drawing, where it remains still, a tribute to the power of erroneous humoral ideas. One must imagine that da Vinci, not finding the expected vessel, concluded that he had destroyed it while dissecting and drew it anyway. Da Vinci "corrected" his own apparent mistake, but not the errors of humoral theory (Mathé, 1978, pp. 79-80; Vavadan, 2005).

The recurrence of long-standing errors indicates that science may not have any systematic method for finding and fixing errors. We need to distinguish an imaginary, abstract, idealized science from authentic science as documented historically. The enduring errors underscore the need for studying more fully just how errors are remedied. Without knowing precisely how each error was found and fixed, one should not conclude that they are fixed by some deliberate or planned process. We ought not assume, without evidence, that scientific reliability somehow takes care of itself. Ultimately, we should at least be clear how science corrects itself, if indeed it does.

4 "SELF-CORRECTION" AND HAPPENSTANCE

In some cases, error correction seems lucky – based on chance, accident, or discoveries in other contexts. These cases pose a second, even deeper challenge to "self-correction" as a foundational principle. They indicate further that, while long-standing errors may be corrected, the correction may be neither intentional nor deliberate.

Consider, for example, the case of the central dogma of molecular biology. Howard Temin had been studying the Rous sarcoma virus. His question was: how does it causes tumors in chickens? The virus was only composed of RNA and a protein coat. What could the RNA do? Temin felt that action through DNA, a more stable molecule, was more likely. He showed that inhibiting DNA synthesis disabled the virus. Meanwhile, David Baltimore was studying how viruses replicate. Having first discovered an enzyme that synthesizes RNA from other RNA, he turned to tumor viruses (specifically, the Rauscher leukemia virus in mice). But these viruses, he found, could not produce RNA from RNA. Yet they could produce DNA. Both investigators had encountered evidence of an enzyme that produced DNA from RNA. The unexpected discovery of reverse transcriptase was a significant exception to the central dogma of molecular biology, presented by Francis Crick in 1958. But neither researcher had set out to test molecular biology's central tenet. They were trying to understand cancer and viruses. The error correction, sometimes celebrated as one of the most significant achievements of 20th-century biology, had occurred unintentionally, during research in another context.

Other cases exhibit the same accidental discovery of error. In the mid-1600s Marcello Malpighi was using the new microscope to view the fine structure of the lungs (Malpighi [1661], 1929; Adelmann, 1966, I, pp. 171-198). His mentor, Alfonso Borelli, had recommended careful observation over theorizing. Malpighi wrote over several weeks to his mentor about his observations. First, he was surprised to find that at end of all the branchings in the lungs, there were small closed sacs. Then he examined the blood vessels. With unaided observation, he saw the blood divide into smaller vessels, lose its red color, then apparently pour out into empty space, whence it was "collected again by a gaping vessel". But with his microscope Malpighi observed "that the blood flows away through the tortuous vessels, that it is not poured into spaces but always works through tubules […]" (Malpighi [1661], 1929, p. 8). Malpighi had discovered capillaries, the tiny vessels that connect arteries and veins. Earlier, William Harvey had claimed that blood circulated without such connections. Malpighi corrected that. But in his publication, Malpighi did not refer to Harvey at all. He did not notice the error correction himself. The observation of capillaries was unexpected. So, too, was the correction of Harvey's earlier error. The accident in error correction is especially important because this case has been portrayed as a discovery arising from hypothesis, prediction, and planned tests. Popular impressions of science often neatly reconstruct events based on a simplistic and misleading template, widely presented as "the scientific method" (see Allchin, 2003). Yet science is far less orderly – and more interesting.

Sometimes, errors get remedied by accident, not by any systematic "self-correcting" process.

5 "SELF-CORRECTION" AND ERROR CASCADES

A third problem with accepting the "self-correcting" thesis appears in series of unsuccessful efforts to correct certain errors. That is, a "correction" may be made but – as in Priestley's "correction" about the role of light – may merely lead to another, different error. The core problem, ironically, remains unfixed.

For example, consider the history of scientific interpretations of what makes humans unique (see Allchin, 2012). A major feature of human behavior, first proposed as distinctive even without evolution, was making and using tools. Our hands – especially with their opposable thumbs – seem well adapted to grasp tools, to shape them, to modify the environment and so ensure survival. Tool-use also fit with the distinctive trait of walking upright. Apparently our hands were free to do their important work. By the early 1960s, at least, the uniqueness of humans as tool-users was well accepted. Thus, when Louis and Mary Leakey identified the first fossil associated with tools in 1960, they gave it the landmark status as the first of our genus, naming it *Homo habilis*, or "handy man". Yet the claims proved overstated and misleading. It is the successive history of that error linking human uniqueness to tools that is instructive, here.

With new discoveries in the mid-1900s, humans could not maintain their unique status as tool-users. Egyptian vultures use rocks to crack open thick ostrich eggs. California sea otters use rocks to break mussel shells. The Galápagos "woodpecker" finch uses cactus spines to probe holes and collect ants. *Polyrachis* ants secrete thread to fasten leaves together. Tailorbirds "sew" their nests with grasses. And so on. Human tool-use was not so unique after all.

Still, humans, like the early *Homo habilis*, seemed the only animals to *make* tools. The Duke of Argyll claimed that "the fashioning of an implement for a special purpose is absolutely peculiar to man". He further contended "that this forms an immeasurable gulf between him and the brutes" (Darwin, 1871, I, p. 52). Renowned evolutionist Theodius Dobzhansky, too, noted that tool-*using* may be instinctual, but "tool-making is a performance on a psychologically higher level"

(Dobzhansky, 1962, p. 194). And so human uniqueness was redefined: from tool-use to tool-making. Error corrected, based on the new evidence.

Yet (we have discovered since) other animals, notably our primate cousins, do indeed make tools. Chimps crush leaves to make sponges to collect water from hollow logs. They strip leaves from branches to use as probes for insects. They sharpen branches with their teeth for hunting and spearing bush babies. They arrange two stones as "hammer and anvil" to open very tough panda nuts. Sometimes, they even use a third wedge stone to level the pounding surface. Various chimp groups leave behind complete "tool kits", generally of about 20 tool types, distinctive of each group's culture. Primatologists now comfortably discuss "chimpanzee technology". So, the erroneous claim of the uniqueness of *making* tools was corrected. Again, based on the evidence.

Then the unique trait retreated to *teaching* tool use. One can now guess what happened next. Yes, adult chimps were observed to help younger chimps learn how to use the hammer-anvil technique. The chimps not only conspicuously demonstrated the method, but also sometimes corrected the orientation of the learner's stone hammer.

So, an occasion for error correction – again. But the pattern of correction may be clear. The claim of human uniqueness based on tools was never abandoned. As George Schaller observed, "there still appears to be a wide mental gap between preparing a simple twig for immediate use and shaping a stone for a particular purpose a day or two hence" (Mason, 1972, p. 388). Only humans *plan* tool-use. Or so it seemed at the time. Bonobos and orangutans have now demonstrated in tests that they can select appropriate tools, save them, and retrieve them for later use. So much for tools as distinctive.

Not surprisingly, the very same pattern of successive errors has been exhibited in claims about humans and language (Allchin 2012).

The history of errors about tools and language is telling. Evidence led to "correcting" each claim. But each time, more fundamental error remained uncorrected. Researchers persistently sought to characterize humans as *unique* – biased by a human perspective. The assumption guiding the scientific question was itself in error. The core mistake was (is?) the cultural assumption that humans are distinctively unique and qualitatively better. One minor error leads to another minor error because the root error, which embraces the whole series, is not yet identified or remedied. I call this an *error cascade*.

Error cascades are another reason for regarding the widespread acceptance of the "self-correcting" thesis as misinformed. One maxim states, "Man proposes, nature disposes". Evidence supposedly exposes scientific errors. While evidence may show that things are "wrong", error cascades indicate that negative results do not necessarily show what or where precisely the error is. Thus, an error in science is not always promptly and fully "self-corrected".

6 "SELF-CORRECTION" AND COMPOUNDED ERRORS

A fourth type of anomaly to the self-correction thesis is errors that lead to other, further errors before they are corrected. Rather, the error is *compounded*. That is, one initial error leads to another, and then to another, and another, without the successive errors ever being "caught" or the need for a correction exposed.

Here, my case is dramatic, although not biological. The series of compounded errors came to light in the 1920s. E. Bächlin was a graduate student assigned to measure the wavelength of X-rays. That involved what was, by then, a standard procedure based on X-ray crystallography. Crystals bend X-rays as they "reflect" off its internal molecular surfaces. A standard formula (Bragg's equation) related the wavelength of the radiation, the crystal structure, and the angle of bending. One can thereby calculate one unknown value knowing the others. In this case, Bächlin was using a familiar crystal, sodium chloride, or table salt. But Bächlin could not obtain an X-ray wavelength value consistent with other experimental approaches. Something was amiss. The discordance between experimental values, here, signaled an error somewhere. But where? Was his value for the distance between atoms in the salt crystal correct? That depended on whether one had correctly calculated the density of the atoms. That, in turn, depended on a correct value for Avogadro's number, the number of molecules in a standard mass of substance. But this number is not easily determined. Contemporary values depended, in turn, on a correct value for the charge of the electron. The value of *e* had been determined by Robert Millikan in his now renowned oil-drop experiment. But Millikan himself had relied on other values to calculate *e*. In particular, he had used Stokes equation in interpreting the mass and velocity of his falling oil drops. That required a value for the viscosity of air. Millikan (or his student-assistant, Harvey Fletcher) had re-examined that value experimentally. But that value, Bächlin found, was incorrect. So the error had been compounded from an incorrect value for the viscosity of air via Stokes equation to the charge of the electron, to Avogadro's number, to the density of salt, to the distance between its atoms and, finally via Bragg's equation, to X-ray wavelength. The original error had flowed from one value to the next, completely undetected for more than a decade. It involved the work of 5 Nobel Prize winners. A veritable thicket of error (Bassow, 1991).

Yes, the error was corrected. "Ultimately". But with the multiple compounded errors, one is hard-pressed to say it was remedied through any meaningful system of finding and correcting errors. Here, that was left to an enterprising graduate student, having to interpret an unexpected mistake in an otherwise routine activity.

7 SELF-CORRECTION AND THE REJECTION OF CORRECTED THEORIES

Finally, we may consider a fifth type of anomaly to self-correction: when new theories that correct earlier errors are rejected. The error correction is, at least initially, not accepted by the community.

One well known example is the cause of ulcers (Thagard, 2000). For much of the 20th century, physicians believed that ulcers were due to stress or spicy foods. They continued to prescribe antacids into the 1990s. No one seemed to notice their limited effectiveness in preventing relapses. When Barry Marshall and Robin Warren introduced the notion that a bacterial infection might cause ulcers, it was widely dismissed by practicing physicians as professionally ill informed. Correcting that error was not easy, although one can always contend that "eventually" it was. Marshall and Warren ultimately received the 2005 Nobel Prize in Medicine for their discovery. It was in large part the correction of an error that had persisted for over six decades, which had been dismissed at first.

Other cases of theory change are familiar to historians of biology. Often the new theories involve revising misperceptions of earlier thinkers – a form of error correction. For example, the discovery of reverse transcriptase was not immediately endorsed, as it contradicted the deeply held central dogma. Temin found resistance in publishing his results. He was also asked to redo experiments and conduct additional demonstrations because his conclusion was, at the time, quite impossible, and he had obviously misinterpreted his data. In the 1990s Stanley Prusiner encountered a similar rejection to his characterization of prions, another correction to the central dogma. Likewise, chemisosmotic theory was introduced by Peter Mitchell in 1961, but was accepted only some 16 later, also an occasion for a Nobel Prize. In all these cases, correcting errors earned Nobel recognition. They certainly seemed more worthy than some everyday practice of "self-correction" in science.

8 REFLECTING ON THE "SELF-CORRECTION" MYTHOS

In summary, I have presented five types of cases that argue against self-correction in science: long-standing errors; errors corrected only by happenstance; error cascades; compounded errors; and rejection of corrected theories. Together, I think they constitute a powerful indictment of the self-correction thesis.

My examples have not been esoteric. They are commonly known. Why, then, does the image and rhetoric of self-correction persist? Certainly, one important factor is that the concept functions to help justify the value of science. "Self-correction", if true, seems to privilege science as a way of knowing. Yes, it is an idealized norm. But the ideal seems to have been transformed into an unassailable fact. By inscribing the idealization into history, one can then appeal to the history as support. The flawed history hides the very assumptions that went into assembling it.

"Self-correcting" has been inappropriately naturalized into history (Allchin, 2008). In this way, the claims of self correction adopt the conventional structure of myths. Culturally, the notion of "self-correction" is important in justifying the grandeur or the power of science. It does not merely explain the history of scientific errors. For this reason, one may refer to it as *The "Self-Correcting" Mythos*. Correcting that mythos involves exposing its persuasive architecture and analyzing the evidence use to support it.

A cultural myth may or may not be true. Recently, several critics have challenged the "self-correcting" mythos. Awareness of retractions has grown. Concern about delayed discoveries of errors in medical studies has increased. Many people now wonder whether science truly is "self-correcting". Can we trust tests on the efficacy and safety of drugs and other medical treatments, or of the environmental impact of some industrial process? Here, the concerns about error-correction are generally quite narrow. The foremost issues are conflicts of interest, limited sample size, and statistical inference. The cultural context, too, is narrow: concerning the politics of policy and regulatory actions (Ioannidis, 2005; Lehrer, 2010; Marcus & Oransky, 2011; Sarewitz, 2012).

However, this discourse is important. While the perspective is critical, the tone is often positive. That is, if science is failing to meet its mythic ideal, the critics want a remedy. Accordingly, they diagnose the process. They look for apparent weaknesses in the system. For example, should the process for reviewing and publishing drug studies be modified? How can the influence of conflicts of interest be minimized? Should there be more a rigorous system for reviewing drugs after initial studies are published? Is there any effective way to track retractions? The ideal of self-correction remains. The aim is to make science more "self-correcting", closer to its ideal.

Thus, while articulating the errors in the "self-correcting" mythos may seem to threaten the ideal of self-correction, when viewed appropriately, they can help inform the challenge of bolstering or improving error-correcting strategies. But this involves acknowledging first that error correction involves work – scientific work – a topic for another occasion.

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