humans. Such infections across species boundaries can lead to a dead end, with the virus not being readily transmissible from human to human. (This might explain, for example, the scarcity of the HIV N group.) On the other hand, successful human-to-human transmission of H5N1 avian influenza would create a global crisis. Not knowing which particular genetic variant will sustain human-to-human transmission compromises our ability to formulate a vaccine in advance—and this is just one of the many practical and immunological challenges for developing a vaccine. Nonetheless, contrary to Davis's assertions about natural selection, the leap between species does not necessarily "favor increased virulence" in the new host. Although many people infected with H5N1 have died, the number of asymptomatic cases is unknown.

In the 1980s, the Institute of Medicine warned that "the United States was ill-prepared to face the threat of emergent diseases." The breakdown in the public health infrastructure, Davis argues, was colliding with "radical changes in disease ecology being wrought by globalization." Few pharmaceutical companies still manufacture vaccines, and the ones that remain have been plagued by production difficulties. Without surge capacity, our ability to stockpile oseltamivir to prevent and possibly treat infection is also impeded. (Then again, an H5N1 influenza isolated from pigs in Java was resistant to the drug, a likely consequence of its inappropriate use.) Davis claims that the government misspends its money on biodefense initiatives to protect us from biological threats that are unlikely to occur. In his view, governments and industry—each for their own selfish reasons—have formed a confederacy of dunces. Only lone scientists, with their tireless work, have captured the author's admiration and escaped his condemnation. To Davis, the mantra of epidemiologists and basic scientists alike is loud and clear: we are not prepared for the next pandemic.

No doubt that avian flu is a threat to guard against. But the deeper question—which remains unanswered in *The Monster at the Door*—is how do we accurately gauge the risk. For chickens the risk is substantial, and, granted free will and free range, it would make sense for them to flee the routes followed by migratory birds infected with H5N1 influenza. For us, the challenge is to achieve a proper balance between the dire warnings of Chicken Little and the folly of playing ostrich.

References

1. Subtypes of the influenza A virus are named for the forms of two surface glycoproteins, hemagglutinin (H) and neuraminidase (N).
2. M. Davis, *Ecology of Fear: Los Angeles and the Imagination of Disaster* (Metropolitan, New York, 1998).
3. Y. Guan *et al.*, *Proc. Natl. Acad. Sci. U.S.A.* **101**, 8156 (2004).

10.1126/science.1123435

PHILOSOPHY OF SCIENCE

Wallowing in the Wastebin

Douglas Allchin

In science (so the saying goes) "man proposes, nature disposes." Many theories thus end up in the wastebin. We no longer talk of phlogiston, caloric, electrical fluid, pangenes, bodily humors, or immobile continents. Such historical errors puncture easy interpretations of cumulative scientific progress. For John Losee, in his provocatively titled *Theories on the Scrap Heap*, they are also prime occasions to consider how scientists evaluate theories. In a clever turnabout, he asks not how investigators establish evidential support for theories but why they find certain theories inadequate, even if once widely accepted.

Theories on the Scrap Heap
Scientists and Philosophers on the Falsification, Rejection, and Replacement of Theories

by John Losee

University of Pittsburgh Press, Pittsburgh, PA, 2005. 216 pp. $24.95. ISBN 0-8229-5873-2.

Losee, an emeritus professor of philosophy at Lafayette College in Pennsylvania, feels that any account should be responsible to history. Prescriptive ideals should, as he has argued in earlier works, give way to a descriptive philosophy of science. Any proposed standard for evaluating theories should have proven effective in the past.

Respect for historical evidence can yield surprising results. For example, how important are confirmations of novel predictions? How do they fare relative to post hoc accommodations? One can easily cite a handful of dramatically vindicated predictions: Halley's comet, Mendeleev's new elements, Adams's and Leverrier's eighth planet. Some folkloric histories, however, are misinformed: neither Einstein's gravitational bending of light nor Poisson's bright spot as proof of the wave theory of light had the historical significance they are often granted in retrospect. Contrary to widespread stories, William Harvey did not even predict the capillaries later "confirmed" by Marcello Malpighi's observations. In other cases, such as Bethe's theory of solar energy production and Agassiz's glacial explanation of erratics, accommodation of available evidence alone seemed sufficient.

Moreover, successful prediction does not guarantee correctness. Belief in phlogiston led Priestley to predict that the substance produced when metals are dissolved in acid would act like charcoal (in today's terms, hydrogen is a reducing agent). Other phlogistonists predicted that electricity should reduce calxes to their metals. Dalton and Gay-Lussac each used the concept of caloric (heat as a fluid) to predict that all gases should share the same rate of expansion as temperature rises. Ptolemy's Earth-centered cosmology still predicts eclipses and the positions of the planets. Yet all three theories now lie abandoned in disrepute. Using past experience as a benchmark, "it would seem that the predictivist thesis is false."

Such cases may seem to warrant another common belief about science: one can never prove a theory, but one can disprove it (typically with a single, well-framed study). Here, Losee tackles Popper's notion of falsification, which is crudely expressed in the proposal-disposal aphorism. To gauge current views

CREDIT: CHIP CLARK/NATIONAL MUSEUM OF NATURAL HISTORY, SMITHSONIAN INSTITUTION

Tales of Irish antlers. Supporters of orthogenesis presented the increase in antler size and subsequent extinction of the "Irish elk" *Megaloceras* as prima facie disconfirming evidence for the theory of natural selection. Other evolutionary biologists interpreted the antlers as adapted for display in open environments but nonadaptive in thick forests that arose as glaciers retreated.

The reviewer is at the Minnesota Center for the Philosophy of Science and in the Program in the History of Science and Technology, University of Minnesota, Minneapolis, MN 55455, USA. E-mail: allch001@umn.edu

about falsification, I surveyed the use of the term in *Science* over the past 10 years (*1*). Three-fifths of the cases referred to misconduct: falsified data or research reports. Of the remainder (46), three-fifths appealed to falsifiability as a hallmark of science or of proper rigor in science (from archaeology and chemical bonding to climate change and paleontology). For example: "Science is based on the falsification of hypotheses." Scientists "work late into the night in order to destroy or falsify another scientist's hypothesis." Researchers who fail to present falsifiable theories are "not playing the game." A theory that cannot predict falsifiable hypotheses is not "sophisticated enough."

In 16 cases, single findings were interpreted explicitly as falsifying some claim. A news item noted that critics of teaching evolution frequently apply such stark falsificationist views. In far fewer (three) cases, authors deemed such judgments too simplistic. One cautioned against rejecting a theory prematurely. Losee agrees, echoing a decades-old consensus among philosophers of science (*2*, *3*). He details through historic cases how one set of negative results is rarely decisive, except for quite low-level hypotheses. Rather, researchers typically finesse the evidence by redefining terms, modifying theories, restricting their scope, or even tolerating unresolved anomalies. Effective reasoning seems to integrate both counterevidence and evidence, and weaker theories wane.

Falsification may also be construed as a methodological guide: guard against error through rigorous self-criticism. Indeed, Popper profiled his "severe tests" as self-referential. Ironically, appeals to falsification in this journal invariably seem to target critics instead. Losee thus opens his book appropriately by characterizing falsification as foremost a "rhetorical strategy," not a touchstone of science. The basic lessons about reliability may be better, if less dramatically, expressed as the significance of empirical import (testability), systematic review of possible sources of error, and thoroughly ruling out alternative explanations (*4*).

Losee's discussion, although offering students a foundational introduction, may strike well-informed readers as dated and conspicuously incomplete. The author leads us to the brink with some tantalizing puzzles. Unanticipated regularities may be predicted by strictly false theories, but how? Theories based on nonreal entities may be empirically successful. Lavoisier could help develop calorimetry, even though the caloric it purported to measure seems illusory. To interpret these paradoxical achievements, one may reconceptualize theories more modestly, as not universally applicable. Philosophers have crafted an alternative based on local, delimited models, which may overlap and possibly even conflict (*5*). They can thereby articulate how to deal with exceptions without sacrificing the ideal of invariant causal generalizations (*6*). One need not jettison phlogiston as an inviable theory. Alternatively, one may accept it as a truthful model, provided one frames its context appropriately. As Losee acknowledges, we still accept Newtonian mechanics, knowing full well we cannot apply it to very light, very fast bodies. Rather than judge theories wholesale, a reflective investigator will seek a more nuanced framework for focusing on and managing errors (*7*, *8*).

Another puzzle is how eminent scientists can disagree—what separated Newton and Leibniz or Bohr and Einstein. Losee describes how individuals may differ due to principled themes or biographical "idiosyncracies." If so and if the scientific enterprise is collective, how do divergent interpretations interact (and possibly become reconciled)? How do researchers accommodate one another's blind spots? How might diversity among practitioners foster more fruitful checks and balances? Readers will want to complement Losee's account with recent epistemic analyses of the social structure of science (*9*–*12*).

Theories on the Scrap Heap provides a lucid, nicely consolidated introduction to the appraisal of scientific theories. Nonetheless, many perspectives in the past several decades—cognitive, rhetorical, gendered, and cultural—extend far more deeply our appreciation of the roots and remedies of error.

References and Notes

1. A keyword search of wildcard <falsif*> in *Science* online index covering October 1995 to November 2005 revealed 124 cases.
2. A. Donovan, L. Laudan, R. Laudan, *Scrutinizing Science: Empirical Studies of Scientific Change* (Johns Hopkins Univ. Press, Baltimore, 1988).
3. P. Kosso, *Reading the Book of Nature: An Introduction to the Philosophy of Science* (Cambridge Univ. Press, New York, 1992).
4. F. Suppe, *Phil. Sci.* **65**, 381 (1998).
5. R. Giere, *Science Without Laws* (Univ. Chicago Press, Chicago, 1999).
6. J. Woodward, *Making Things Happen: A Theory of Causal Explanation* (Oxford Univ. Press, Oxford, 2003).
7. L. Darden, *Theory Change in Science: Strategies from Mendelian Genetics* (Oxford Univ. Press, Oxford, 1991).
8. D. Allchin, *Perspect. Sci.* **9**, 38 (2001).
9. D. Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Univ. Chicago Press, Chicago, 1988).
10. H. Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Princeton Univ. Press, Princeton, 1990).
11. A. Goodman, *Knowledge in a Social World* (Oxford Univ. Press, Oxford, 1999).
12. M. Solomon, *Social Empiricism* (MIT Press, Cambridge, MA, 2001).

10.1126/science.1122678

BROWSING

Vanishing Act. Art Wolfe. Text by Barbara Sleeper. Bulfinch, New York, 2005. 144 pp. $50, C$67. ISBN 0-8212-5750-1.

In their efforts to capture wildlife on film, most photographers strive to make the animals stand out from their surroundings. In this series of 101 photographs, Wolfe instead documents how his subjects disappear into their environments. Some are camouflaged by their coloration or patterning, others rely on their shapes and behaviors, and many combine several approaches to deceive predators or prey. Because photographs cannot capture all of the distractions of the landscape, Wolfe uses depth of field, the placement of his subjects within the frame, and inclusion of bigger or brighter distractions to make his images visually challenging. The book is filled with examples of animals—mammals, birds, reptiles, amphibians, fish, insects, and crabs—that seem to vanish in plain sight, such as the Elliot's chameleon, *Chamaeleo ellioti* (above), photographed in Rwanda's Parc National des Volcans. Concise descriptions by Sleeper provide details about the organisms and their ecology.

CREDIT: ART WOLFE/WWW.ARTWOLFE.COM