Biologists abandon Popper at their peril

Sir,

Dr. Holliday boldly articulates(1) a view which I believe is prevalent among biologists, though seldom expressed—that Popper’s philosophy of science is inapplicable to biology. This view is not only wrong, it is catastrophically wrong. Its present popularity is, I feel, among the most insidious obstacles to progress in the biological sciences.

Holliday asserts that biological laws remain true even when exceptions to them are found. If we define “laws” as he uses the word, this is a truism. Exceptions to biological laws are likely consequences of the opportunism that natural selection promotes, so biologists positively expect them.

Crucially, moreover, an identical distinction exists between different areas of physics. For example, the discovery of “creep” in liquid helium did not overturn the law that liquids find their own level, because that law was understood as an emergent property of complex systems, whose newfound non-universality was no threat to the deeper laws describing such systems’ elementary constituents. This is easily generalised: A physical system’s complexity strongly influences scientists’ confidence that a hypothesis about it will survive its next test. If ten diverse objects all fall at the same rate in a vacuum and ten diverse organisms all use the same genetic code, we may well infer that these properties are universal. We may call both hypotheses “laws,” but we will nonetheless be much surer that the eleventh object will fall at that same rate than that the eleventh organism will use that same genetic code. This purely comparative distinction is all that separates the meanings of “law” in different sciences.

This brings us to the dangers of Holliday’s thesis. Because they investigate such complex systems, biologists are in particular danger of placing undue faith in their laws—of viewing them as more “proved” than is merited. That failing is undoubtedly encouraged by the suggestion that biological laws are unfalsifiable. Prominent cases, such as the Central Dogmatists’ resistance to Temin’s provirus theory, often end with Nobel prizes for the innovators; but no amount of belated adulation restores the intervening wasted years. How many valuable hypotheses are abandoned without publica-

tion due to the un-Popperian opposition of entrenched reviewers? We can never know.

Conservatism towards new hypotheses is bad enough, but perhaps worse yet is the effect on experimentalists’ evaluations of their findings. My field, biogerontology, has recently seen two extreme cases: homozygous ablation of neither telomerase(2,3) nor non-mitochondrial superoxide dismutases(4,5) reduces mouse lifespan. These results flatly contradict some very prominent hypotheses for how mammals age, making them among the foremost discoveries of modern biogerontology. Yet, the articles announcing them displayed oblivion of their importance, instead highlighting the comparatively trivial findings that such mice are sensitive to artificial challenges. Consequently, gerontologists have been decidedly slow to assimilate this vital new information. These are prime examples of expert experimentalists (not to mention reviewers and editors) losing the Popperian plot—seeing more significance in tangential confirmation than in cast-iron falsification.

Popper was as right, and as wrong, about biology as about physics. Without induction, hypotheses cannot become authoritative enough to be much worth falsifying; but thereafter, only falsification matters.

Induction is not a myth, but scientific proof certainly is. Biologists and physicists alike—but perhaps not philosophers!—must strive for one thing above all: to be wrong.

References

Aubrey D.N.J. de Grey
Department of Genetics
University of Cambridge
Downing Street
Cambridge CB2 3EH
United Kingdom
Email: ag24@gen.cam.ac.uk