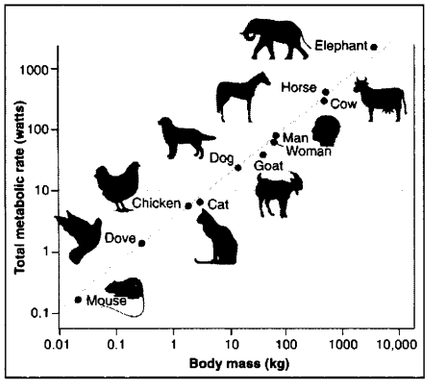
It is pointed out that relationships among size, metabolic rate, and the longevity of species are more complex than scaling laws would predict. And the essay "Being an absolute skeptic" was found to be thought-provoking, but its premise was questioned by many readers: "Does [the author] detect no tingling sense of warning that the mountaintop on which he stands is slippery with oil and beginning to shake?" asks one reader about the statement "Skepticism is correct." Another congratulated *Science* on publishing a spoof. The author answers the readers' criticisms.

Large Animals in the Fast Lane

In her commentary (News Focus, 4 June, p. 1607) on an analysis of fractal geometry and allometric scaling (G. B. West *et al.*, Reports, 4 June, p. 1677), Dana Mackenzie describes how new perspectives in mathematics and biophysics have at last begun to explain "why smaller animals spend life in the fast lane and die young, while larger ones burn energy more slowly and live longer." While we agree that it would be pleasant if nature followed the rules laid out by biophysicists, we want to point out that the relationships among size, metabolic rate, and specific longevity are considerably more complex and variable than one would guess from scaling laws. A wide range of well-documented observations is at odds with the oversimplified idea that little animals burn up quick and die young.



Correlation between body size and total metabolic rate

CREDIT: K. SCHMIDT-NIELSEN, *SCALING: WHY IS ANIMAL SIZE SO IMPORTANT?*

1) Within species, superior longevity is associated with body size in dogs (*1*), mice (*2*), flies (*3*), and probably humans (*4*).

2) Mutant dwarf mice live much longer than standard size mice (*5*).

3) Among mammals, some species live more than seven times longer and some less than half as long as predicted by scaling calculations (*6*), with longer life span typically associated with relatively risk-free ecological niches.

4) There is no relationship between body size and longevity among mammals below a body weight of approximately 1 kilogram (*6*), despite the enthusiastic deployment of fractal capillary networks by these smaller creatures.

The yearning to find an elegant theoretical explanation for why (some) big species live longer may need to yield to a suppler and more flexible theory based on a detailed understanding of genetic modulations to selective pressures.

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Skepticism and Relativism

Kudos to *Science* and David Miller ("Being an absolute skeptic," Essays on Science and Society, *Science*'s Compass, 4 June, p. 1625) for airing the claims of postmodernists and relativists that science deserves "no special claim to attention." Miller, following in the tradition of Pyrrho of Elis and David Hume, concurs that "absolute skepticism is correct" and that "science has no authority." But he relies heavily on Hume's claims that scientific theories based on observation of nature (induction) offer no predictive security because no argument can establish that nature is uniform. Although Hume's arguments are seductive, absolute skeptics are in error. Admittedly, nature is in flux. But it displays repetitive patterns, many cyclic. Were this not so, life could not have evolved as it has over billions of years owing to the inevitable time lag between environmental change and adaptive genetic responses. Our organs of sensation and data processing evolved to generate roughly accurate adaptive representations of nature. Therefore, careful inductive inferences about the future are rational and promote not only individual and group adaptation but also scientific progress.

Few scientists dogmatically claim true and certain knowledge. Most practice a mitigated skepticism and will allow a favored theory to be falsified by robust experimental evidence. Long ago, absolute skepticism was devised to neutralize disturbing truth claims so that the philosopher might not fret unduly about them. Absolute skepticism is an entirely appropriate attitude to take if confronted by truth claims that lack empirical justification, for example, the supernatural claims of dogmatic faiths. But it is not an appropriate or an adaptive one when confronted by microbial or molecular explanations of disease, or scientific analyses of evolutionary processes, or predictions of biospheric degradation. If philosophers, postmodernists, and relativists continue to urge absolute skepticism of science, the community of interacting scholars and citizens will become as balkanized as the political arena and international scene, where many *are* governed by absolute faith in their own views and absolute skepticism of all others.

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Miller says, "Skepticism is correct." Does he detect no tingling sense of warning that the mountaintop on which he stands is slippery with oil and beginning to shake? As he ably states it, skepticism denies that any opinion, even when applied only to the level of common sense, is more likely than another, on the basis of experience or evidence. I agree only insofar as Miller's position is unsupported by evidence. Actually it is a self-nullifying paradox. His calling Hume's opinion a "discovery" is another. Philosophers are struggling to hold on to science as simply one subdomain of their province of knowledge through reason. Quite to the contrary, I have felt for some time now that science has left philosophy behind, having found and held tightly the simple idea that one can ask certain questions in ways that increase our ability to predict nature's behavior. It is no more or less than that, and so it has escaped from philosophy's grasp. Science's core principle of falsifiability is a harsh master—one, in

my opinion, that philosophers prefer to evade. Kudos for Sokal and Bricmont. The postmodernist-deconstructionist emperor had no clothes, only arcane verbosity signifying nothing.

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As a practicing biochemist and cell biologist for close to 60 years, I cannot identify with the image of the "scientist" offered in Miller's essay. I have never claimed—and I know no scientist who has —that "science has conclusive or unimpeachable authority." On the contrary, whatever research I have been engaged in was always seen as a step, continually subject to the verdict of experiment, in the direction of what eventually could be considered established. It may happen that this goal is reached with a considerable degree of conviction (note that I avoid the word "certainty," which does not belong to the scientist's vocabulary) far beyond the status of a "lucky guess." To substantiate his view of scientific hypotheses being no more than lucky guesses, the author of the essay typically mentions a case—the existence of extraterrestrial life—on which there is as yet no firm evidence, only more or less reasonably supported conjectures. I wonder how he would have reacted to a statement such as "all known living organisms are descendants from a single ancestral form of life," which, appropriately qualified by the term "known," rests today on overwhelming evidence.

I also question the author's affirmation that scientists, "faced with such detraction, retort ... that science, unlike witchcraft, works." This is not the point. It is true that science works. But so do, under certain circumstances, homeopathy, acupuncture, and other alternative medicines, even witchcraft. This does not make the underlying theories or principles valid. Science derives its authority from the fact that it is continually self-questioning and self-correcting and cannot venture beyond the limits of verifiability (or falsifiability).

That there is a subjective element to many scientific debates cannot be denied. This happens most often when objective data are insufficient, judgment needs to be exercised, or the issue carries an emotional, ideological, or ethical component. To cite a recent example from my own country: how much dioxin should be allowed in food? It is also true that science addresses only the intelligible aspect of reality. Philosophy, the arts, poetry and literature, and theology and religion offer access to other important aspects. Finally, it must be acknowledged that not all scientists conform to, or even approach, the idealized picture they draw of themselves. Science has its share of rotten or not-so-fresh apples. But so has philosophy, not to mention the worlds of politics or business, or, for that matter, art. Despite all these limitations, the scientific endeavor has been enormously successful and for the most part trustworthy, thanks to its unique methodology.

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Miller appears to confuse logic and intellectual authority and how Karl Popper resolved the problem of scientific knowledge with the critical search for errors. The assumption of philosophy is that, because the authority created by scientific arguments depends on deduction, the authority created in turn by such arguments must arise also from a parallel kind of deduction. Philosophers call this justification. But Popper showed that this is a false parallelism because the authority of scientific arguments is created by logic in regard to our pursuit of truth as a goal. Miller ignores this and so asserts rather than explains Popper's key insight that the rational authority of science comes from its search for errors.

David Miller

This point about rational authority coming from goals rather than justifications may seem obtuse, but consider the case of aircraft safety. Here an intense process of error detection occurs based on logical and empirical argument, yet the authority created by such deductions about airworthiness does not link to any ultimate justification, as it comes entirely from the pursuit of safety as a goal. The same process in regard to truth backs the authority of science.

Popper once observed, "Here I am being showered with honours as no professional philosopher before me; yet three generations of professional philosophers know nothing about my work" (*1*, p. 272). This statement is still true of those working in 1999.

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I found Miller's essay informative and thought-provoking. However, I do not feel better knowing that science can be distinguished from pseudoscience by its process, when it is still maintained that both have equal claims to credulity.

A common belief from the scientific culture is that the predictive ability of models and theories implies a high probability of their absolute truth. Arguing from Hume, relativists and skeptics reply "that scientific theories and scientific predictions cannot be justified by experience, even in part...." What is probably not clear to many scientists is why, in this contest of assertions, philosophers take the position that it is Hume who must be right. It is the more curious because, if Hume is right, there are then no grounds to justify taking Hume's own assertions more seriously than those of science. In fact, don't relativism and skepticism lead to a circularity where nothing has meaning? And if this is the situation, then ignoring such a sterile philosophy in favor of the more optimistic assertions of science would seem not hubris, but philosophically justified, and the most pragmatic and productive course for humanity.

Miller's list of the reasons why philosophy is valuable to science and scientists is most helpful. In the same spirit, could not philosophy benefit from valuing more highly the assertions of science? Of course, it is not quite the same thing to say, as scientists wish to, that because experiment enables predictivity, then predictivity justifies experiment. However, developing this unifying line of thought and showing how Hume's arguments (developed in the mid-1700s, long before most of the achievements of science) might be fallacious, or at least dispensable, would seem as productive as the divisiveness of relativism or skepticism. Why should philosophers be more hesitant, or less clever, than scientists at modifying old theories when they no longer meet the case?

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Once I recovered from the sciences being dressed down so in Miller's essay, "Being an absolute skeptic," I realized that I agree with his thesis and have since graduate school. In particular,

1) Scientists do often overstate the quality and value of their work. This partially explains how the Baltimore case became so intractable.

2) Scientists often equate skepticism with hindrance, and treat it accordingly.

3) Scientists arrive at logically dubious results embarrasingly often. I have collected examples from *Science* itself over the years to prove the point.

4) Every scientist should emulate Popper's method. I attempted this in my Ph.D.

CREDIT: ALLAN BURCH

dissertation. It caused the sort of bickering that people refer to as "character building."

Thus, while being an absolute skeptic promises some rewards, it isn't likely to advance anyone's career or impress colleagues.

I was surprised to read in an essay that admonishes scientists for unwarranted certainty, that "Absolute skepticism is correct." How do we know?

Or, "[i]f scientists would stop overlauding scientific rationality in the face of reason, then there might be fewer disgruntled defections to irrationalism." What argument leads from this premise to its conclusion? I realize that my experience provides no reason for certainty, but people embrace irrationalism apparently for two reasons. (i) It fits in better with their world view (religion, ideology, politics, and so forth); and (ii) no one suffers negative consequences for it, except the fundamentalist who refuses effective medical attention. In the university humanities department, in fact, irrationalism and tweaking the noses of scientists increases a faculty member's stock.

Certainly science in any form cannot prevent self-interested defections.

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Response

The purpose of my essay was to distinguish skepticism from relativism, to applaud the former and to deplore the latter. I tried to discriminate between truth and justified truth (where "justified" may signify less than "certain"), to acknowledge that science often achieves truth, and to deny that it sometimes achieves justified truth. The absolute skepticism that I advocate is absolute not only because it is undiluted but because it presupposes the ideal of absolute truth.

Postmodernists, I granted, are right about skepticism, but very wrong about relativism. Most other philosophers, in contrast, and many scientists, are right about relativism but very wrong about skepticism.

Yet for Roberts "philosophers, postmodernists, and relativists" are a single group, and for Barnes "relativists and skeptics" are a single group. A similar marriage of contrary sentiments is apparent in Odgren's letter, especially in its three final sentences. Let me repeat that I am a philosopher and a skeptic, but I am not a postmodernist and I am not a relativist. My skepticism is not nihilistic, and is miles away from any policy of suspending judgement on "truth claims that lack empirical justification" (Roberts). To my mind all scientific hypotheses lack empirical justification, but that need not stop us from judging them. Our judgement is conjectural, but it is no more subjective than is any other human activity (de Duve), since it can be submitted in its turn to empirical criticism.

It was an ancient objection to skepticism that if skepticism is true then it is not justified. The best skeptics accepted this conclusion. I accept it too, and happily (Kilty need have no fears on this point). But it is not a valid objection to skepticism, still less "a self-nullifying paradox" (Odgren). Only those who embrace what I called "the ruinous doctrine that all rational opinion is justified opinion" could think that. Skepticism is unjustified and unsupported, but it is not untrue. Applied to relativism, however, at least in some of its cruder versions, the same line of argument is devastating; relativism indeed spirals into something like "a circularity where nothing has meaning" (Barnes).

In explaining my position I appealed to Hume's famous skeptical arguments, and bemoaned the way in which his conclusions are sometimes airily, even smugly, disregarded. To refute Hume it is not

enough to point to science's success! Scientists may think that "the predictive ability of models and theories implies a high probability of their absolute truth" (Barnes), but logic, I am afraid, says otherwise. The case of celestial mechanics at the time of Kant and Laplace should sound a warning note.

I for one have no trouble in agreeing that "science...can ask certain questions in ways that increase our ability to predict nature's behavior" (Odgren); and that "nature ... displays repetitive patterns" (Roberts). Who denies it? But the principal task of science is to separate the apparent patterns from the genuine ones; and what Roberts calls "careful inductive inferences about the future" can be distinguished from carefree ones only when we already know how to perform this separation. Induction remains a piece of fraudulent nonsense. I have no quarrel with Barnes's suggestion that "Hume's arguments...might be fallacious"—but it is an empty dream unless supplemented with specific criticism. In any case, it is scarcely fair to accuse philosophers of not doing their best to fault Hume's arguments. In my essay I mentioned my own misgivings over the way in which he proceeded, the main difficulty, as I see it, being the lack of operational meaning of such phrases as "probable argument" (Hume), "reasonably supported" (de Duve), and "inconclusive evidence" (*1*). But I reported that Hume's arguments can be satisfactorily repaired, and that his skeptical conclusions are unimpugned. These problems are discussed with some intensity in my *Critical Rationalism* (*2*).

It was in defense of the claim that guesses can, even in the absence of evidence either way, properly be described as true and false, that I made an example of the conjecture that there is life elsewhere in the galaxy. It would have been inept to have chosen for this job a conjecture that many think "rests today on overwhelming evidence" (de Duve). Like de Duve, I am not much bothered by the idol of certainty; like him, I explicitly repudiated the idea that "scientific hypotheses...[are] no more than lucky guesses"; I maintained, as he does, that it is "its unique methodology" of conjectures and refutations that sets science apart; and I too implied that the empirical success of an enterprise "does not make the underlying theories or principles valid"; on all these points de Duve and I are in agreement. The only real difference between us, it seems, is that he thinks that science obtains authority, but not conclusive authority, from its method, whereas I contest this (and in addition do not understand why inconclusive authority is of value).

Skoyles too holds that "the rational authority of science comes from its search for errors"; and he hints that on this issue I misunderstand and misrepresent what Karl Popper had to say. Yet Popper also wrote, "Science has no authority" (*3*, p. 259), words that I would not have echoed so plonkingly had I remembered the passage. The theme that science is wonderful, but has no authority, is to be found in the inspiring final section of Popper's first book (*4*), and throughout his later writings, for example in his sketch of a "new professional ethics" (*5*, p. 201 and following). Of course, people are free to use the word "authority" in such a way that a well-tested theory may be said to have authority. You can even say that the "certified airworthiness" of an aircraft is a kind of authority. All this is fairly harmless, as long as it is appreciated that there is no logical connection between airworthiness and safety. Authority has no authority.

How can relativism, postmodernism, extreme social constructivism, and other insufficiently endangered species of irrationalism, have taken a hold on thoughtful people? Doubtless there are many extrinsic causes, as Kilty suggests, ranging from piety to envy. My essay proposed, not at all originally, an intellectual explanation: relativism is the offspring of skepticism, which is true, and the equation *rationality = justification*, which is false. More interesting than why relativism persists is why justificationism (antiskepticism) prevails. All the arguments indicate that there is no positive role that evidence can play, yet it is almost universally thought that evidence has a positive role to play. Part of the explanation may lie in the near-imperceptibility of the linguistic convention that reads into any assertion a silent claim by the assertor to have some evidence for what is being asserted—a convention whose existence I cannot gainsay but do very much lament. The convention may be seen at work in Kilty's assumption that when I wrote "Absolute skepticism is correct" I claimed justification (or even certainty) for what I said. But here, as elsewhere, I claimed nothing more for what I said than what I said: absolute skepticism is correct.

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