toxicity. In principle, the 800×10^8 tons of coal burned annually in the US could be replaced by mining 40 000 tons of uranium, eventually saving 230 000 lives by removing the uranium from the ground, in addition to the 8600 lives that would be saved by not burning coal.

The conclusion is very clear: If one considers the very-long-term effects of radiotoxicity, coal burning is a major killer, and nuclear power is a major lifesaver.

Reference

1. B. L. Cohen, Health Physics **40**, 19 (1981).

BERNARD L. COHEN
University of Pittsburgh
Pittsburgh, Pennsylvania

GABBARD REPLIES: My letter, under the headline "Can Coal Combustion Breed Pu in the Sky?" (May, page 88), has generated considerable discussion, which has prompted a reevaluation of the data I utilized from the 1990 reference document,[1] a standard at this facility. Calculations of specific data relevant to my letter have shown the reference data to be in error to such an extent that the points I raised are no longer at issue. When the 1960 source document[2] was consulted, the erroneous reference data could not be duplicated and now stand as an inexplicable error that apparently was repeated in the 1990 document used in my study. Consequently, the recalculated radiotoxicity data are in line with international standards, and discussions of natural-source plutonium are of no significance.

I trust that this resolves the issue.

References

1. *ORNL Health Physics Manual*, RP-2.16 (1990).
2. W. S. Snyder, *A Single Criterion for Determining an Equivalent Amount of Various Radionuclides on the Basis of a Relative Hazard*, ORNL Central Files no. 60-3-116 (1960).

ALEX GABBARD
Oak Ridge National Laboratory
Oak Ridge, Tennessee

Universities Use Their Faculties Well

In his broadside (August, page 13) against the participants, such as the undersigned, in PHYSICS TODAY's roundtable on research universities (March, page 42), Martin E. Ross poses a series of questions, such as "How many of the introductory lectures and laboratory courses at their respective universities are taught by graduate students?" As for Cornell University's physics department, I can report that all such lectures are given by faculty, and largely by senior faculty, as are a substantial portion of discussion and laboratory sections. At any time, several of these courses are undergoing major renovations led by senior faculty and requiring sizable commitments in university and departmental resources. Cornell is not a singular case; many other research universities have departments with very strong research and graduate programs and are led by faculties deeply committed to undergraduate education.

KURT GOTTFRIED
Cornell University
Ithaca, New York

Cosmological Claims Prompt Model Discussion

Many scientists have begun to worry about lapses of scientific rigor in cosmology, and two cases in point have recently appeared in print. An article in *Nature*[1] by Peter Jakobsen and colleagues on the possible detection and quantification of cosmological helium has been widely hailed as a major discovery for the field. It may well be. However, the article's abstract ends with the statement, "The detection also *confirms* that substantial amounts of helium existed in the early universe, *as predicted* by Big Bang nucleosynthesis theory" (emphasis added). This choice of words is unfortunate. In science, confirmation is not achieved with a single experiment, especially a very difficult one. Moreover, when there are serious alternative interpretations of the data, it is not proper to presuppose that one alternative must be correct, no matter how desired that result might be.

It is possible that the observations of Jakobsen and colleagues are due to an interloping source rather than the distant intergalactic medium. Gary Steigman has demonstrated that this explanation may be more likely in a parallel case involving the proposed detection of cosmological deuterium.[2] Jakobsen and colleagues presumably knew that the interloper interpretation was a strong challenger to the cosmological helium interpretation, and so the choice of the word "confirms" is inappropriate. In itself this mistake is not a very serious matter, but when multiplied a thousandfold—most discussions of cosmology include several such examples—the problem does become a serious concern. Errors in scientific logic, exaggerated claims and confusion between what is known and what is assumed have become alarmingly common in cosmology.

The second case in point involves the respected cosmologist P. J. E. Peebles, who stated in a recent paper: "A semiempirical cosmogony uses a general framework within which *one adjusts parameters to fit observations* [emphasis added]. If this cannot be done it shows the framework is wrong. If it can, the parameter adjustment may force us to a useful approximation to reality; the evidence would be that the result accounts for more observations than there are free parameters."[3]

But hold! I hear shades from Ptolemy's era explaining the rationale for using epicycles to model retrograde motions of planets. This is not the scientific method—a system that has worked beautifully for centuries. The standard version of the scientific method is as follows. (1) Observe nature objectively (not with mathematical or philosophical prejudices in mind). (2) Hopefully an idea, theory or model, based on a principle or pattern, will emerge. (3) If the hypothesis has real merit, it will lead to definitive predictions.[4] (4) Empirically test the predictions and humbly accept the results. If a prediction fails, do not tweak the model or fudge the data; instead go back to (1).

Perhaps our concerns have been inflated, but a growing number of us see cosmology as a loose cannon. The scientific method is a priceless guide to knowledge; we should not accept dubious substitutes.

References

1. P. Jakobsen, A. Boksenberg, J. M. Deharveng, P. Greenfield, R. Jedrzejewski, F. Paresce, Nature **370**, 35 (1994).
2. G. Steigman, Mon. Not. R. Astron. Soc. **269**, L53 (1994).
3. P. J. E. Peebles, Astrophys. J. **432**, L1 (1994).
4. R. Oldershaw, Am. J. Phys. **56**, 1075 (1988).

ROBERT L. OLDERSHAW
Amherst, Massachusetts

JAKOBSEN REPLIES: While we agree with Oldershaw that research in cosmology is often somewhat wanting in terms of scientific rigor and that, as he implies, one should choose one's words with great care when publishing in this field, my coauthors and I beg to differ in the case at hand and plead not guilty to the accusations made.

In keeping with Oldershaw's explanation of the scientific method, the presence of helium in intergalactic gas is indeed a long-standing definitive prediction of conventional Big Bang cosmology that has long been searched for and has now been confirmed by observation. This can be a violation of the scientific method only insofar as Oldershaw appears to be unwilling to accept our detection as valid.

As for "serious alternative interpretations," Oldershaw is correct in stating that we "presumably knew that the interloper interpretation was a strong challenger." This possibility is explicitly discussed at some length in our paper, where we cite the *a priori* probability of such a chance occurrence as being on the order of 2 percent and discuss whether other astronomical observations show any evidence to suggest such an occurrence (they do not).

In any event, there is now little reason to question our intergalactic helium detection since it has since been confirmed in two further cases toward two other quasars at different redshifts—one using the Hubble Space Telescope and the other using the Hopkins Ultraviolet Telescope flown on the Astro-2 mission. [See the story on page 19.]

The outstanding open question is not whether intergalactic helium has been detected or not, but whether the detected gas resides in the so-called Lyman-alpha forest clouds or in the diffuse intergalactic medium.

Peter Jakobsen
European Space Agency
Noordwijk, The Netherlands

PEEBLES REPLIES: Since deductions from extragalactic astronomy are indirect, one has to treat them with caution, as Oldershaw points out. One deals with this by checking for consistency among different lines of evidence. I am impressed by the detection of helium (by Jakobsen *et al.* and the Hopkins Ultraviolet Telescope group) at redshifts of roughly $z = 3$ in material with generally very low abundances of heavy elements. This result alone certainly does not substantiate any particular cosmology, but it does count as a significant piece of evidence to be added to the wealth of results from the older observations and new detectors and telescopes. My conclusion (detailed in my 1993 book *Principles of Physical Cosmology*) is that the evidence ties together in quite a strong case for the standard relativistic cosmological model of an expanding and cooling universe.

Since the world is a complicated place, it is reasonable and healthy that the standard cosmological model allows a lot of free parameters to adjust by the trial-and-error method alluded to in the passage quoted by Oldershaw. The standard model cannot be adjusted to fit any observation, however, and it could have been ruled out by the observations on how structure formed and evolved. The constraints on structure formation are going to get even tighter in the next several years as a result of ongoing observational programs. I see no reason to expect the new data to contradict the standard cosmology, but that is a possibility we have to bear in mind. If there are no contradictions or other surprises, we should soon have a pretty clear picture of what happened as our universe expanded and cooled and the galaxies formed and evolved and clustered.

I am not predicting the end of research in cosmology, for there are many open puzzles left for future generations and I expect more to show up. But I do assert that cosmology is making real and substantial contributions to the advance of knowledge in physical science.

P. J. E. PEEBLES
Princeton University
Princeton, New Jersey

More on Sagdeev

Roald Sagdeev recently underwent a fierce attack by Yakov Alpert in PHYSICS TODAY (July, page 15) for his alleged mistreatment of Jewish refuseniks. Having been the first emigrant (1973) from the Institute of Space Research headed by Sagdeev, I would like to present a different viewpoint.

Soviet authorities used to hold the institutes of the Soviet Academy of Sciences and their directors responsible (in fact, holding them hostage) for their employees emigrating. In this delicate situation Sagdeev's attitude was irreproachable. He rejected attempts by the local Party organization to fire me from the institute, so that I was able to continue my work until my departure for Israel. No doubt his firm and noble position helped me and my family to emigrate quickly, rather than our having to join a row of refuseniks.

ALEXANDER ERSHKOVICH
Tel Aviv University
Tel Aviv, Israel

Use of Classical Laws Isn't Hard and Fast

In his article "The Physics of Cerebral Aneurysms" (February, page 24), George J. Hademenos applies the classical laws of Hooke and Poiseuille to arterial walls and blood flow. However, Jean L. M. Poiseuille developed his law from experiments on the flow of liquids through glass capillaries with uniform cross sections. Arterial walls are more rubberlike than glasslike, because they consist of intertwined long-chain molecules as found in elastomers, and they usually have nonuniform cross sections, because of variation in pressure along the walls.

Nearly 50 years ago Poiseuille's law was generalized so that it could be applied to elastomeric tubes such as blood vessels. Any study like Hademenos's not only should be aware of the great difference between the elastic properties of soft body tissues and of more rigid structures like glass but also should note that the classical laws of Hooke and Poiseuille do not apply to systems constructed of soft body tissues. Review articles on this subject may be found in *Medical Physics*, volume II (Otto Glasser, ed., Year Book, Chicago, 1950, pages 188 and 303).

ALLEN L. KING
Dartmouth College
Hanover, New Hampshire

HADEMENOS REPLIES: In my article I made every attempt to stress that the use of Hooke's law and Poiseuille's law served as a simplified basis for understanding the development of aneurysms. I would like to think that the readership of PHYSICS TODAY is capable not only of distinguishing between rubberlike arteries and glasslike tubes but of understanding the implications of applying those classical laws to arteries.

I fully agree with Allen L. King that Poiseuille's law and Hooke's law do not absolutely characterize the statics and dynamics of aneurysm development. As I wrote in my article, under the heading "Prognosis for the future": "The immense complexity of blood flow and its role in the development of cerebral aneurysms cannot be overemphasized. From a theoretical standpoint the 'simple' case of blood flowing through a normal artery involves the pulsatile flow of a non-Newtonian fluid through a tapered, viscoelastic tube and requires the solution of a time-dependent nonlinear differential equation with variable boundaries. The presence of an aneurysm introduces another level of complexity into the problem. Still, application of elementary physical principles gives us a qualitative (and to a lesser extent a quantitative) understanding of the hemodynamic processes involved." I made similar points in numerous other places in