sap would rise less than 0.29 m, rather than the 3 m calculated by Vogel. Assuming the effective water-filled radius of capillaries in the trunk, branches and leaves to be $10^{-4}$ mm, then the maximum height of a tree that can sustain sap flow would be 146 m, rather than the 1500 m mentioned by Vogel. The world's tallest tree, according to the *Guinness Book of Records, 1998*, was a eucalyptus found at Watts River in Australia and estimated to have been over 500 ft (152 m) tall. The Bond number analysis suggests that the leaf pores and supporting capillary tissue in such a tree would have to have had radii of less than $10^{-4}$ mm. Water flow would be slow in such small pores. Flow resistance may be the reason for the ultimate limit on the height of trees.

**GLENDON GEE**
*(glendon.gee@pnl.gov)*
*Pacific Northwest National Laboratory*
*Richland, Washington*

VOGEL REPLIES: I agree with both points made by Franklin Felber; I believe that his argument about heights of jumps was first made by Galileo. In reality, air resistance should reduce the flea's range by 80%, and only with a cunning trigger mechanism can the beast meet our crude expectations. As for the trot-to-gallop transition, I meant merely to imply that my reasoning was speculative, since we lack the experimental base that R. McNeill Alexander, C. Richard Taylor, Rodger Kram and others have given us for the onset of trotting.

Dietrich Bechert's uncertainties have largely been put to rest by the plant physiologists, whose textbooks give the details. A remarkable body of evidence supports the notion that water, under substantial tensile stress, is drawn up tree trunks by evaporation from the leaves. That water has sufficient cohesion was shown by Lawrence Briggs,[1] who (with admittedly heroic precautions against bubble nucleation) got tensions as high as 290 atmospheres! And we have good (if indirect) evidence for tensions of up to 120 atm in living plants. Bechert correctly notes that evaporation could not provide the positive pressures needed for the initial sap rise in the spring and observed at other times as well. Those pressures mainly come from pressure in the roots, generated osmotically and paid for metabolically. Incidentally, Bechert's proposed "simple check" wouldn't be so easy to do, in that a water column breaks from imperfect adhesion between the water and the tube's walls, or from bubble nucleation at that interface.

Glendon Gee points up an embarrassing error; in fact, I made two mutually offsetting errors. As he notes, capillary rise should be about 0.3 m, not 3.0 m. But the pore radius in the walls of the cells within the leaves is about 5 nm, not 100 nm.[2] So, drawing in air would require a sap column nearly 3000 m long, which doesn't constrain tree height. Since water leaves by evaporation rather than bulk flow, the small size of the pores shouldn't matter. In any case, the aggregate area of pores lining the internal air spaces of leaves exceeds their huge external surface area by more than tenfold, so no rapid movement need be assumed.

In closing, let me note that, based on what I've heard since my article was published, physicists may be assuming excessive originality on my part. In general, I have learned that they are surprised at how much decent physics can be found in the biological literature. As an ecologist, Joel Cohen, once remarked, "Physics envy is the curse of biology."

## References

1. L. Briggs, J. Appl. Phys. **21**, 721 (1950).
2. See P. Nobel, *Physicochemical and Environmental Plant Physiology*, Academic, New York (1991), pp. 96–97.

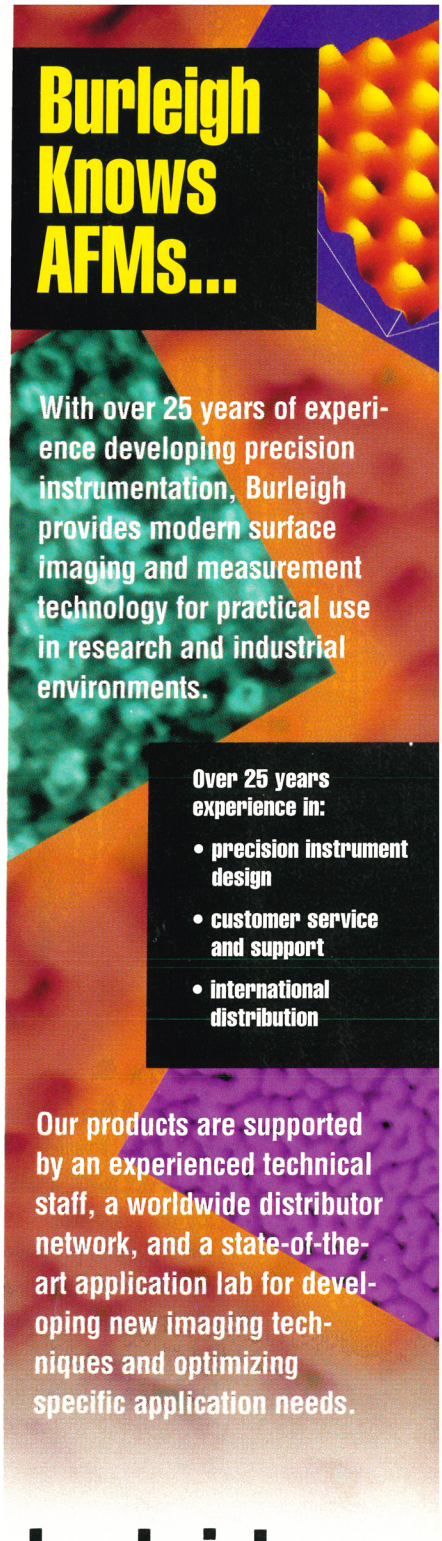
**STEVEN VOGEL**
*(svogel@duke.edu)*
*Duke University*
*Durham, North Carolina*

# 'First World' Journals Should Publish More 'Third World' Science

As both a physics professor at the University of Havana and a member of the American Physical Society, I write to express my concern that researchers in developing countries have too limited opportunities to have their work published in the scientific journals of the developed countries.

It is unquestionable that researchers in developing countries face more difficulties than those who work in developed countries, and that their results may not always contribute to the leading edge of research in the particular discipline. Nevertheless, the majority of researchers in developing countries take their work seriously, and when they obtain some noteworthy results, they attempt to share them with the scientific community through publication in journals that are read all over the world. In many cases, though, their manuscripts are summarily rejected by the





journals of the developed countries.

Statistical studies, including some published in the US (such as Wayt Gibbs's "Lost Science in the Third World"[1]), show that journals in developed countries appear to be reluctant to publish papers by researchers in developing countries. Unfortunately, the rejection of manuscripts is too frequently based on the opinions of editors or referees who fail to provide any meaningful technical criticism or commentary. Instead, they express subjective opinions, such as "The topic is not of interest to the scientific community," "We have too many papers to publish in our journal, so we recommend you send yours to another journal," "The topic of your paper does not coincide with the interests of our journal" and "This topic has not generated publications in the past few years, so we do not recommend the publication of this paper." I have personally encountered such forms of rejection, as have colleagues of mine, and regrettably they are not uncommon.

I do not want to start any kind of intellectual war concerning this issue. Rather, I just want to appeal to the conscience of our peers in the developed countries, who have the power to influence publishing decisions when reviewing manuscripts, and to ask them to evaluate submissions strictly on technical merit and to arrive at recommendations based on factors other than weak subjective justifications.

A more reasoned and impartial review process will help us in the "third world" to make our modest contributions to science, and to feel that we are part of a truly global scientific community and on an equal footing with our colleagues elsewhere.

## Reference

1. W. W. Gibbs, Sci. Am., August 1995, p. 76.

**José Marín Antuña**
*(marin@ffuh.fmq.uh.edu.cu)*
*University of Havana*
*Havana, Cuba*

# More on Topic of Faculty Retirement and Full Faculties

In his "Nibbling the Bullet" (PHYSICS TODAY, June 1998, page 11), Daniel Kleppner argued that faculty members over the age of 70 should retire in order to create openings for younger scientists and to help balance department budgets, increase department morale and develop new physics. Subsequent letters to the editor (October 1998, page 11) have commented mainly on the role of the individual in this matter.

What is still needed, though, is a complementary approach at the group level that takes a holistic perspective, explores options and is likely to facilitate individual decisions. Here, I offer such an approach, which calls for making changes at the department and university levels.

First, to maintain its high standards of teaching, research and service, a physics department must be given flexibility in its employment and budgetary practices, including the right to extend the working lives of faculty members on the basis of their abilities and productivity. Accordingly, there should be no mention of age.

Second, the university should step in to help if the physics department is having trouble setting its maximum number of tenured professors, as can happen when department income is determined almost solely by student enrollments. In such cases, I propose, a stable minimum size should be set and guaranteed by the university on the basis of its endowments.

Third, physics departments and their universities should increase their willingness to accept nongovernment and industrial funding of professorial appointments, together with the implied obligations associated with corporate research and training. A loss of some academic freedom for such positions would be an acceptable price to pay for the advantages accruing from increased expertise and flexibility. Furthermore, increased entrepreneurial activity is likely to help improve the department's morale and vitality.

Acceptance of such an approach by both the department and the university, coupled with goodwill all around, would enable the department to create a more stable and flexible working environment for all of its faculty members.

**J. F. (Jim) Williams**
*(jfw@physics.uwa.edu.au)*
*University of Western Australia*
*Nedlands, Perth, Australia*

Some of the letters to the editor commenting on Daniel Kleppner's essay question whether there is a problem, and others suppose it must be deadwood clogging the system. I would argue that if there is a problem, it is quite the opposite: active researchers not retiring when they could.

Although mandatory retirement may be gone, university policies predicated on it are often still in place. Those policies reflected what was best for the one in the driver's seat—

*continued on page 113*