

science can coalesce. (Let us trust our intelligence and worry about how to divide the pie later.) Third, knowing the world is competitive, the assumption that the flexible term "per capita rate" will take off on an upward trajectory among the global economic powers is not an unreasonable one. Thus this approach will save us much repetitive work.

As is always the case for an unpolished idea, this alternative may also have its simplistic shortcomings and undoubtedly suffers from a lack of sophistication. However, as in most human enterprises, the first small step is often the most important one, despite the usual groping, staggering and occasional self-doubts.

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An Issue of Neglect for Astronomy?

It was with great anticipation that I opened my April 1991 copy of *PHYSICS TODAY*, but my heart was filled with great sadness as I scanned the table of contents. Why, with many distinguished women astronomers and astrophysicists out there, were women not represented among the authors in this special issue on astronomy and astrophysics?

I do not know how articles are chosen for such an issue. Surely some of them must be solicited from the authors, and perhaps some women were asked to contribute to the issue and did not. If the latter is true, then I must fault those women, but I strongly suspect that none were asked.

Time and again I have seen statements from The American Physical Society and the American Astronomical Society, member societies of the American Institute of Physics, that more women must be encouraged to consider careers in physics or astronomy. But where are their role models in *PHYSICS TODAY*?

The very fine PBS program "The Astronomers" prominently features some of our brightest and best women working on the cutting edge of science. What a fine example PBS is setting for young women in our secondary schools. But *PHYSICS TODAY* continues to fail in this respect.

The *New York Times* has a "critics' choice" once a week featuring young successful people in the arts. Why doesn't AIP follow this example? Praise need not be limited to young female physicists, but they are certainly out there among the young

men. We need to encourage all talented people to pursue careers in the physical sciences.

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5/91

A glance at the April 1991 issue confirms the feeling of most solar physicists that we are the Kurds of astronomy. It is a pity you could not devote a little space to the rich scientific problems of our very own star.

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5/91

THE GUEST EDITOR OF THE APRIL 1991 ISSUE REPLIES: Astronomy and astrophysics have profited enormously from both recent and historical research contributions of women. Those of us who helped to organize the astronomy and astrophysics decade survey, from whose committee members the authors of the April 1991 issue were drawn, tried to include women at the highest levels. We were moderately successful. Eight percent of the members over 40 of the American Astronomical Society are women, while women constitute 11% of the total membership of the society. The percentage of women among the panelists of the decade survey was almost exactly halfway between these two numbers, namely, 9.5%. There were 2 women among the 30 panel chairs and members of the executive committee. As Shirley W. Harrison might have guessed, some very distinguished women astronomers declined to serve either as panel chairs or on the executive committee because of other commitments. When we presented the results of our study to the media, two of the five speakers were women, and when we described the recommendations in a symposium at the National Academy of Sciences, one of the five speakers was a woman.

Astronomy of the Sun is mentioned or discussed on five of the seven pages of my summary of the astronomy decade survey in the April 1991 issue (page 24). Two of the 14 outstanding achievements of the previous decade listed in the summary refer to solar astronomy, helioseismology (carried out with superb precision at Harold Zirin's Big Bear Solar Observatory) and solar-neutrino experiments.

The editor of *PHYSICS TODAY*—a widely respected female physicist—and I jointly selected the topics and authors for the April 1991 issue, based on what we believed to be of

most interest to the magazine's readers.

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8/91

Theory Lack Shouldn't Prevent Publication

Philip W. Anderson has read my inmost thoughts! Although he refers to solid-state physics in his Reference Frame column "Solid-State Experimentalists: Theory Should Be on Tap, Not on Top" (September 1990, page 9), one thing he writes applies in general to most fields of physics, and most certainly to my field of experimental plasma physics: "Much more serious is the distortion of priorities, of communication and of the refereeing process that occurs when excessive weight is given to theoretical interpretation. We don't want to lose sight of the fundamental fact that the most important experimental results are precisely those that do *not* have a theoretical interpretation."

In spite of being an experimentalist, until recently I was (for 17 years) a member of an institute for theoretical physics. I often tried in vain to inculcate in my colleagues there the understanding that physics is not only (mathematical) theory but that there are such things as phenomenological models, which an experimentalist has to develop at first, simply as a strategy.

Let us assume you find during experimentation that a phenomenon that has been known for a long time suddenly shows some new features that disagree with the hitherto applied and generally accepted theory. This theory may have rested on a number of simplifying, even incorrect, insufficient or inapplicable assumptions that are at odds with the experimental arrangement. Of course this fact was also known to other scientists who had investigated the phenomenon, but some of them preferred to ignore it, since a number of experimental data agreed quite well with the theory.

In view of your new results you are forced to reconsider the old theory, and you develop a new, phenomenological model capable of explaining them. Of course you try to find a theorist who has time and interest enough to develop a new theoretical model. However, in view of the complexity of the system this will keep your theoretical colleague busy for several years. But in the meantime you want to publish proudly the results of your experiments together

with your own phenomenological model.

Then just wait for the comments of the referee! Inevitably he or she will criticize the *lack of a theory*, even though you have clearly stated that there is no theory as yet and that the old one is not applicable. You may even get an accompanying "comforting" letter from the editor advising you to put "some theory" into your paper. It appears that in the eyes of some referees and editors a physics article is not publishable without mathematical theory, while just which theory you use does not seem to be all that important.

I don't understand this strange point of view. Are we really so mesmerized by mathematics? Shouldn't mathematics be an *auxiliary* science of physics? Where are our intuition and imagination that help us to "see" electrons and ions gyrate around magnetic field lines and space potentials oscillate up and down? Do we really only believe in phenomena when they are clad in mathematics and wrapped inside formulas?

No referee would dare to reject a paper presenting a self-consistent theory that had nothing whatsoever to do with reality, so long as the theory was mathematically correct. Why then are referees often so quick to criticize and reject purely experimental papers because they lack a theory? I see this attitude as just another remnant of the ancient Western-Christian philosophy that the "spirit" (the mathematical theory) is something much more worthy than the "matter" (the experiment), a philosophy that has caused so many problems and taboos from which we all still suffer. Of course it would be ideal if an experimentalist could simultaneously present the experimental data *and* a theory that explained them in every detail. Since, however, most experimental physicists are normal mortals and not universal geniuses (otherwise most theorists would be unemployed), you have only two possibilities: Either try to strain the old and generally accepted theory to suit your experimental data, just to have some equations in your paper, even though *you know* that the theory is inapplicable, or withhold your publication until your theoretical colleague presents a new theory, even if this will take several years. But the first option is unethical, and the second is neither wise nor fair to yourself and your colleagues, who should get an opportunity to verify the new features of the phenomenon.

Just recently I heard a nearly incredible story that shows the con-

tempt of certain theoretical physicists for experimental physics:

A young American colleague and friend of mine was working in the plasma physics department of a high-ranking European university. One day he had an idea for a new experiment and suggested it to the head of the department, a distinguished theoretical plasma physicist. The professor asked him whether there was a theory to explain the expected experimental results.

"Yes," my friend answered, under the impression that he had succeeded in attracting the interest of his superior. "There is a theory, and perhaps we could verify it for the first time with our experiment."

"In that case," the professor replied, "we do not need to carry out the experiment! It is enough that there is a theory."

If scientists of earlier generations had held this weird "Aristotelian" attitude, not even the neutron would ever have been discovered!

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10/91

Stereopsis and Science

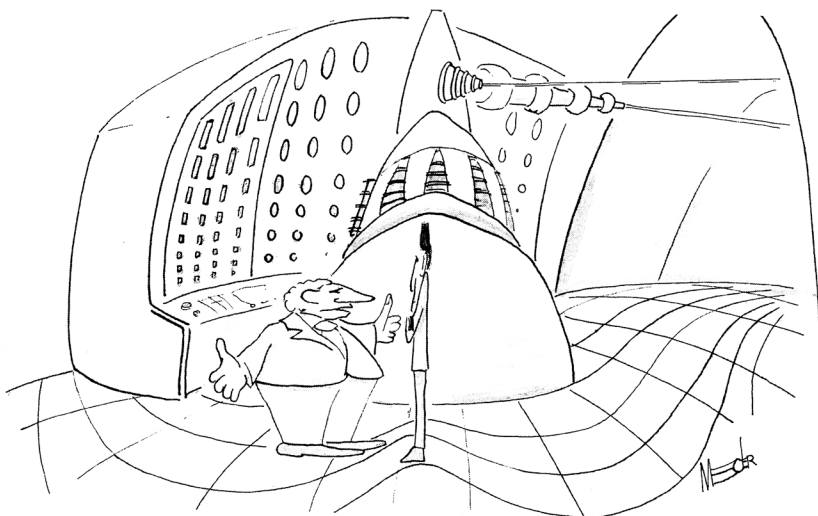
Arthur Sleight's perspective drawings of $\text{YBa}_2\text{Cu}_3\text{O}_6$ (June 1991, page 28) demonstrate an "obvious" phenomenon that goes virtually unobserved. Because a pair of views of adjacent unit cells is equivalent to the stereoscopic views of a single cell by adjacent eyes, we can see these diagrams in striking stereoscopic 3-D simply by crossing our eyes to fuse adjacent cells. This extension of perception can be used on any photo-

graph of a repeating object that has a translation vector of symmetry normal to the line of sight.

Then we can turn the page 90°, cross our eyes to fuse the images of the two different structures, and instantly perceive the differences between them. The technique is a copy editor's delight—and it's virtually unknown. I have similarly detected planets, comets and asteroids stereoscopically.

Stereopsis is more often than not thought to be identical with depth perception. One physicist, when asked what characteristics we might expect extraterrestrial visitors to have, replied that two eyes with overlapping vision was one of the most certain. He reasoned that space travel implies good spatial reasoning, which implies depth perception, which implies stereopsis, which implies overlapping vision by two eyes. However, some birds have excellent depth perception but no overlapping vision and no stereopsis, and cetaceans probably have sound-wave imaging capabilities. Human "depth" is a coordinated system of many elements: relative motions of visual images as we move, converging lines, haze, stereopsis, touch as we reach out, plus many others.

Likewise, human concepts are coordinated systems of many elements. Missed elements commonly cause misconceptions. Misunderstandings of science have many roots here. The capacity to do work is a complex interaction of many properties of inputs and byproducts of a thermodynamic system but is often oversimplified into a single parameter, such as that expressed by the logical fallacy "Energy is the capacity to do work." An ill-defined, complex colloquial "en-



"Surely, Dr. Lowe, if there were gravity waves, we would have detected them by now."