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Information for Contributors appears on pages 37–39 of the 7 January 1994 issue. Editorial correspondence, including requests for permission to reprint and reprint orders, should be sent to 1333 H Street, NW, Washington, DC 20005.
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LETTERS

AAS Position on the Space Station

In the News & Comment article "Societies drop opposition to station" (15 July, p. 311), Christopher Anderson reports that the American Astronomical Society (AAS) has changed its position and now supports the space station. This interpretation is incorrect. First, the AAS never officially took a previous position with respect to the station. Second, the AAS council recently debated the pros and cons of the station, but voted to adopt no official stance until its Committee on Astronomy and Public Policy had thoroughly examined all the issues and made its recommendations. Our letter to Vice-President Al Gore thus addressed the perceived threat of the possible cancellation by the Senate Appropriations subcommittee of the Advanced X-ray Astrophysics Facility (AXAF) or Cassini missions—missions which "have been widely studied and assigned the highest priority in the science community and are well into their hardware development stage." Our letter went on to say

> We need your assistance to resolve the [s]ubcommittee's funding shortfall and to avoid a confrontation between the space station and space science programs. The construction of the space station should not come at the expense of the AXAF and Cassini missions. Each of these programs provides a different path for exploring the universe. We urge the Administration to take the long view on these noble and worthy investments for the future.

To the extent that this appeal had any influence on the final favorable action taken by Senator Barbara Mikulski's subcommittee with respect to the AXAF and Cassini missions, the AAS is pleased and grateful. In a similar context, the statements made by Peter Boyce, the Executive Officer of the AAS, in his interview with Anderson constitute not an endorsement of the station, but rather a recognition that the justifications for its construction are complex and involve geopolitical and economic issues that transcend purely scientific considerations.

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Superconductivity: The Real Dichotomy

The News article "New clues to superconductivity" by Daniel Clery (12 Aug., p. 860) reflects a sincere attempt to clarify a complex situation made more difficult by the climate of controversy that surrounds the field. However, the scientific points are somewhat subtle and, without prejudice as to the work of others, I would like to make my own position clearer.

Clery understandably presents a dichotomy, when in reality there is a wide diversity of views; but in doing so he gives the impression that I belong in what is in some sense a "traditionalist" camp, as opposed to the spin-fluctuation camp of David Pines, Douglas Scalapino, and Toru Moriya. In fact, from the start I have been a strong advocate of the idea that the mechanism cannot be the traditional one of pairing by means of phonon polarization, and the record shows that I actually introduced the model that is now being studied by Pines, Scalapino, and others into the field (Reports, 6 Mar. 1987, p. 1196) and pointed out the importance of antiferromagnetic interactions between spins. In my opinion, the experimental evidence, of a wide variety of types, excluding traditional phonon mechanisms of pairing has long been overwhelming. The main point relevant to Clery's article on which I differ from many of the physicists in the field is in thinking that the symmetry of the gap is of little importance and was not necessary to prove that the mechanism is basically electronic. (A somewhat irrelevant point is that some of my collaborators were the first to suggest d-wave pairing).

The real dichotomy is whether one may or may not use traditional physics, that is, diagrammatic perturbation theory, in these materials. Pines and Scalapino are "traditional" in this sense. Experimental data of many kinds [some, but not all, of which I detailed in a *Science* article in 1992 (12 June, p. 1526)] proves that such theories are at best rough guides and on some crucial details they are qualitatively incorrect. In particular, they do not explain several striking features of charge transport in the normal state.

I have proposed a mechanism by which a nontraditional theory can lead to high T_c which agrees with many types of data. This proposal predates the work of Sudip Chakravarty *et al.* (Reports, 16 July 1993, p.

337), in which we supposed for the sake of some detailed calculations that the symmetry of the gap was determined by phonons, and is independent of that symmetry. In suggesting, if they do, that in some sense d-wave symmetry—which I was correctly quoted as saying is favored by the weight of the evidence—is evidence for the spin-fluctuation theory, Pines and Scalapino seem to be misrepresenting the situation. The d-wave is necessary but far from sufficient evidence for their theories, and it says nothing about the issues that divide their theories from my school, which are fundamental (and have implications for a wide spectrum of phenomena in solid state physics).

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Recession of Tropical Glaciers

In his report "Quantifying global warming from the retreat of glaciers" (8 Apr., p. 243), Johannes Oerlemans calculates a global warming rate over the last century from glacier terminus variations, having assumed that these were solely a result of rising air temperature. He properly notes that factors other than temperature affect glaciers and that the monitoring of glacier mass balance is a better way to infer climatic change. He also depicts the terminus record of Lewis Glacier (figure 1 in the report), which stems from the University of Wisconsin's long-term glaciological program on Mount Kenya (*1*, *2*). I believe some rectification is necessary concerning the climatic interpretation of this record.

The year 1893 marked not only the earliest terminus observation, but was also close to the onset of glacier recession in East Africa. This contrasts with the beginning of ice retreat around the middle of the 19th century in the Ecuadorian Andes, as documented from the evaluation of historical sources (*3*), and in New Guinea, as inferred from numerical modeling (*4*). The modeling of Lewis Glacier (*1*, *5*) indicates that the onset of recession resulted from a drastic decrease in precipitation and concomitant cloudiness and radiation effects during the last two decades of the 19th century, which was also reflected in a drastic drop of East African lake levels. Sensitivity analyses of the glaciers of Mount Kenya (*6*) indicate that a warming trend with enhanced sensible heat supply favored further ice shrinkage in the first half of the 20th century, while in the most recent decades increased atmospheric humidity contributed to the accelerated ice wastage. The much later onset of ice recession in East Africa appears to be related to a reduction in precipitation during the boreal autumn rainy season, which resulted from an acceleration of westerlies in the equatorial zone of the Indian Ocean during the last two decades of the 19th century (*7*).

Processes other than global warming merit attention for the understanding of glacier variations in the low latitudes. The Lewis Glacier on Mount Kenya is the only ice body in the tropical half of the Earth for which measurements are regularly reported (*8*). The continuation of this long-term glaciological program is uncertain.

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References and Notes

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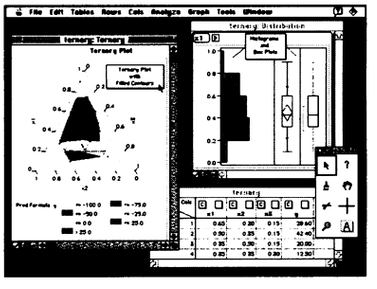

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