Letters

NASA and University Astronomers

The Research News article "Why won't NASA talk to scientists?" by M. Mitchell Waldrop (18 Aug., p. 699) is incomplete, both in its report on community reaction to NASA's plans for the Advanced X-ray Astrophysics Facility (AXAF) and in its description of the plans themselves.

The draft document referred to did not leave all astronomers who saw it "aghast." In fact, it is consistent with the mode of operation employed in *all* of the most successful astrophysics missions over the past 10 years and in the highly productive and widely praised Astrophysics Data Program. NASA makes the formal selection of winning proposals upon recommendations of a peer-review panel and then provides funds to support this research from headquarters. The Space Telescope Science Institute is the only exception to this rule—the director makes final decisions on observing time and funding. It is certainly too early in the HST program to conclude that the latter method is vastly superior.

The management structure NASA has proposed for the AXAF center is modeled after the Infrared Processing and Analysis Center at Caltech, the science center which received the highest grades for service to the scientific community from a senior peer-review panel 18 months ago. The plan specifies a "close teaming arrangement" between a manager who "provides day-to-day management" and a chief scientist who "establishes scientific goals," "represents the scientific interests of the [community of] users," "acts as the primary interface to the AXAF project office," and so forth. That is not really such a horrifying division of responsibilities. Finally, with a mandate that includes "defining an optimum observing strategy for AXAF," "organizing and conducting the review processes for the selection and allocation of observing time," "providing continuing scientific guidance, advice, and analysis in support of the AXAF project," and "performing scientific research," the center NASA envisions hardly seems like a passive "library."

Many astronomers feel that large institutes such as the Space Telescope Institute are not the only way to run a successful space science program. The Space Science Board Committee on Space Astronomy and Astrophysics of the National Academy of Sciences issued a report 2 years ago which explicitly endorsed a number of alternative strategies for the operations of the "great observatories." It is interesting to note that this advisory structure has not been consulted on the current issue.

It is, perhaps, an indication of the openness of the "astronomers contacted by *Science*" to a constructive dialog that, while speaking "not for attribution" they felt free to impugn NASA civil servants as exhibiting "intellectual quality [that] is mediocre at best." The scientific productivity of the magnificent AXAF mission will be optimized only through an early and continuing alliance that includes NASA headquarters, NASA center scientists, and a broad cross-section of the scientific user community. Let's get on with it.

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It is certainly not a motivating, inspirational thing to see one's institution characterized as a place where the the "intellectual quality is mediocre at best." M. Mitchell Waldrop received this assessment of civil service staff members of the NASA field centers as he was exploring the reasons for conflict concerning the proposed AXAF science center.

What leads to such an assessment of NASA in-house scientists? We suggest that all space scientists have been thrown into an increasingly competitive and restrictive environment in which too many good ideas are chasing too few fiscal resources. This has led to bashing of NASA scientists by the university community (and vice versa in some cases). The AXAF controversy may simply be another example of the sort of acrimonious relationship that has grown over the last decade due to the perception that some "other" group is getting too many of the limited resources.

There has been an unfortunate erosion of support of space science in this country since the 1960s. NASA centers have lost much of the flexibility that led to the remarkably high-risk, high-payoff feats at the dawn of the space age. Universities have also lost key engineering and technical personnel to such an extent that most university groups can no longer deliver space-qualified hardware. Thus, more and more hardware capability is being concentrated in the NASA centers and at other large federal laboratories. Since most of the NASA space science money flows to hardware activities, it is natural that resentment has grown in the university community.

The energy being wasted in these turf battles is not going into productive scientific

discovery. With the advent of major new NASA research missions and increased research and analysis dollars, there may be an infusion of funds in fiscal year 1990 above the "keep alive" level. Without this the university, commercial, and NASA center scientists are going to continue scrapping over the bits of money left after the hardware bills are paid. Then, tragically, the full scientific potential of the NASA space projects will not even be approached.

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Superconductivity Applications

Robert Pool's Research News article "Superconductivity: Is the party over?" (26 May, p. 914) summarized some unique properties of the high-temperature oxide superconductors. The article focused on two newly discovered effects, giant flux creep and flux lattice melting, that may present major obstacles to the achievement of high critical current capability in these materials. Both these phenomena lead to energy dissipation during high-current transport, especially in large magnetic fields and high temperatures. The article briefly mentioned the hopes, but current confusion, over a rigid flux line glass state and the commonly observed but unidentified flux pinning.

Recently, researchers at Oak Ridge National Laboratory (ORNL) have determined experimentally that dissipation due to current flow in a large magnetic field was strongly reduced when the field was applied parallel to the Cu-O planes of epitaxially oriented, single-crystal films of $Y_1Ba_2Cu_3O_{7-\delta}$ (*1*). The observed critical current density, J_c, exceeds 10^5 amperes per square centimeter at 77 K in fields up to 8 teslas. Rotating the field to the crystalline c-direction, perpendicular to the Cu-O planes, led to a rapidly decreasing J_c above 4 teslas that vanished near 6 teslas. The latter results are similar to those observed in flux melting experiments and were confirmed on the thin film samples by ac magnetic loss measurements at ORNL. At present it is unclear whether the onset of high-field dissipation is due to flux lattice melting (*2*), field-enhanced flux creep (*3*), or a breakdown of three-dimensional superconductivity (*4*). The experiments are being extended to ascertain whether the high critical current observed for fields parallel to the copper-oxygen planes arises from the newly predicted phenomenon of intrinsic flux pinning that occurs due to the short coherence length and the layered crystal structure (*5*), or from a simpler effect originating from the planar, quasi-two-dimensional film geometry. Preliminary measurements on a series of films having different cross-sectional areas point to the confirmation of a bulk flux-pinning phenomemon. This result provides a demonstration of the existence of high critical current densities at liquid nitrogen temperatures and in substantial magnetic fields, an important step in establishing feasibility for applications in high-field, high-current systems.

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The "Misplaced" Fossils

Heinrich K. Erben (Letters, 15 Sept., p. 1165) seems to have misconstrued the fundamental thrust of our monograph (*1*) and its shortened version (*2*)—that scores of reports coauthored by Vishwa Jit Gupta happen to be based on items that can be purchased for nominal sums from curio counters and fossil dealers all over the globe and are often found in collections of amateur paleontologists, universities, and research laboratories. Such items include the springboard for Erben's critique, the Late Devonian ammonoids "said to have come from the vicinity of Erfoud, Morocco" [(*1*), explanation of figure 6; omitted in (*2*), figure 3] bought at a rock shop in Paris. The words "said to have come from" should be noted, as Erben asserts that I stated "without qualifications" that the ammonoids were from "the vicinity of Erfoud." He then uses this as basis for throwing a mud pie: that I "appear to have trusted" information from a shopkeeper and have therefore been delinquent as regards my primary facts.

Erben overlooks the element of farce in the original monograph and the occasional tongue-in-cheek style. To emphasize the ease of obtaining such materials (from highly specific localities) and the ease with which we believe an unscrupulous individual could have emulated Gupta's activities, we quoted prices from recent fossil-dealer catalogs for many of the items we flagged as spurious or dubious. In the case of the Late Devonian ammonoids, we gave the addresses and telephone numbers of the fossil dealers from whom the particular specimens were purchased. That such items could be readily purchased from Alain Carion in Paris or from Stella's Rocks and Minerals, a stall in the weekend Paddington Market in Sydney, underlines not only the ease with which such material might be obtained but the near impossibility of determining the origin of Gupta's "Khimokul La" specimens.

The apparent 25-year pattern of Gupta's activities has been described (*3*) as a "cumulative joke": a joke that, although it backfired, has injured scores of innocent scientists, among whom Erben would be one of the most eminent. Few scientists have the inclination to "blow the whistle" on fellow scientists' obviously spurious data, or do they have the necessary expertise with which to indulge in psychiatric explanations of research behavior. They choose to ignore such problems, as my colleagues and I did for 16 years after discovering in 1971 that some of the published reports by Gupta and his coauthors did not match up with what we found in the field. Our reluctance to go public earlier, regrettably, allowed the edifice of disinformation to reach monstrous proportions before being confronted.

Gupta's coauthors surely were victims, but the "unwitting" of our phrase "unwitting participants' implies innocence even where obvious incongruities in Gupta's contributions to joint papers passed unnoticed. That Erben and so many other "unwitting participants" were misled is a commendable demonstration of the trust that most scientists have in one another.

Erben insists that whistle-blowers should "refrain from overzealous exaggerations," implying that we have erred in this regard. No one would disagree with this general principle, but in our case we (*1, 2*) deliberately *underplayed* the situation, giving only a sample of the disinformation known to us at that time. Recycling—using the same specimens as underpinning for reports from widely separated areas—is, however, more pervasive than we did realized (*4*). That we (*1, 2*) were examining only the tip of an iceberg of vastly greater dimensions and having perhaps more serious implications than we imagined is also brought out clearly by evidence from four of Gupta's coauthors (*4*) and materials in an avalanche of correspondence we have received since publication of our monograph (*1*).

Erben plays down our position by asserting that "really cogent evidence is indeed lacking" and that our "circumstantial evidence. . .*seems to be* rather convincing" (empha-