Letters

Ground-Based Astronomy: The NAS 10-Year Program

There is considerable discussion in the NAS report on ground-based astronomy (*1*) of a proposed large cross antenna (pp. 50–52). The program for this instrument is to be primarily the study of bright extragalactic radio sources and cosmological studies. I do not think that statistical studies of vast numbers of radio sources will reveal anything important about the size, structure, and history of the universe. Herschel expended a lot of the same kind of effort on star gauges, attempting to learn the nature and shape of the Milky Way. His map (*2*) gives a cruder idea than may be secured on a clear, dark night by the naked eye, especially in the Southern Hemisphere.

Real progress in optical astronomy followed the introduction of instruments that could be used to study stellar spectra. The same will happen in radio astronomy. The proposed cross has a very limited usable range of wavelengths. Its resolution is inadequate. Probably a realization of these matters has caused the construction of the Benelux cross (mentioned on page 18) to be cancelled.

Despite its recommendations for "smaller special-purpose instruments" (pp. 54–56), the report seems to encourage the single-basket effect in radio astronomy by placing its emphasis on huge instruments. Instead of statistical generalities about vast numbers of radio sources, much more solid, detailed information is needed about a moderate number of specific radio sources. Diameters, shapes, components, surface brightness, position angles, intensities, and how these parameters *change with wavelength* are all very important. Such information may be secured from a program of lunar occultations and very-long-baseline interferometry. A vertical-incidence instrument having progressively variable resolution similar to my horizon instrument (*3*) atop Haleakala would be very useful. This work may be spread over many institutions, and many small groups may participate simultaneously. The funds required would be nominal and the results would be of great fundamental importance. The radio astronomy spectrum covers from less than 0.1 meter to more than 100 meters. The most useful devices will continue to be special-purpose instruments.

The panel's comment that "there is already danger of an imbalance between the strong federal support given to the national center for radio astronomy . . . and . . . the support given to the varied activities in the same fields in the universities" seems to me to understate the case. Table C (pp. 102 and 103) shows that 94 percent of the NSF funds and 60 percent of the total federal expenditures for major radio-astronomical facilities have gone to the National Radio Astronomy Observatory. In my opinion 20 to 25 percent of the total would be a suitable amount for a national facility.

A design study for a large steerable parabaloid is discussed on page 56. Twenty-nine years ago I introduced this device to radio astronomy. It has been pushed far into the region of diminishing returns. A current feasibility study of a dish 400 to 600 feet in diameter under a radome displays an acute lack of imagination (*4*).

The future of radio astronomy lies in the study of nonthermal phenomena. These become increasingly important at wavelengths longer than 1 meter and dominate the scene at 10 meters. The most suitable technique comprises arrays of wires. The major emphasis of the future should be upon the design, construction, and use not only of adjustable but of steerable and tracking multiple-beam arrays. In comparison with dishes, their cost is trivial and their rewards are immense.

The panel notes that "the specifications of many of the instruments to be built cannot be predicted today. The past history of radio astronomy has shown that we are still in a stage of development in which many of the more important celestial phenomena and observing techniques are yet to be discovered." I should like to lay additional stress on this point. The astronomers of the next generation will not be content to merely carry along the programs of their predecessors. The new men are going to have their own ideas of what constitutes a worthy problem.

They will want to build their own equipment and perform and interpret their own experiments. Some new ideas and technology have already come above the horizon (*5*). Able, energetic young people will seek situations where they have opportunities to spread their wings. I believe my young compatriots have the wit and imagination to invent new schemes of observation, the ability and skills to design and build equipment, the intelligence to analyze the data, and the acumen to interpret the results. Moreover, provision must be made for the art in science. I quote Oppenheimer (*6*):

> It is probable that no laboratory should ever be so dedicated to its practical mission or missions that it cannot afford a reasonable proportion—perhaps a sixth or a fifth—of work that is on the face of it unrelated to its purposes (*7*).

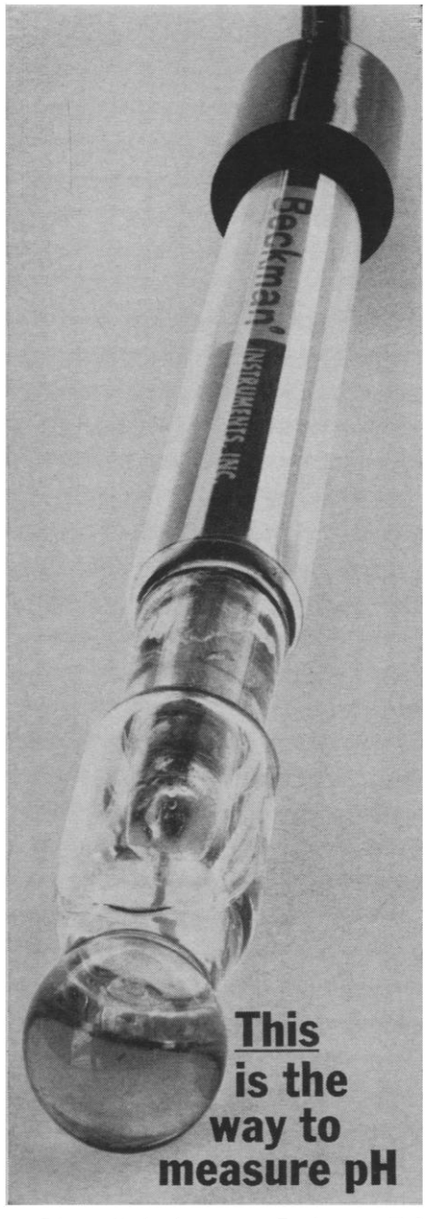
Finally, I should like to note some omissions from the report:

In common with solar optical astronomy (*8*), solar radio astronomy has been barely mentioned.

My old work is acknowledged on pages 4 and 104, but none of my current activities are mentioned. This oversight is difficult to understand, because during 1962 I gave a major address (*9*) at which several members of the panel were present. Also one member visited here the following year and spent a day and evening inspecting the installation, taking pictures, and examining my data. I also provided him with samples. Table A (p. 90) should have an added line:

> Bothwell, Tasmania, Australia. Filled-in array with circular periphery 3520 ft (1.1 km) diameter. Frequency range 1.8 to 2.4 megacycles. Beam 8° diameter adjustable in north-zenith-south plane. 1962. Supported by Research Corporation.

GROTE REBER
Dennistoun, Bothwell, Tasmania



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References and Notes

1. *Ground-Based Astronomy, A Ten-Year Program* (National Acad. of Sciences, Washington, D.C., 1964).
2. See M. Hoskin, *Endeavour* **23**, 18 (1964).
3. G. Reber, *J. Geophys. Res.* **64**, 287 (1959).
4. H. Hoover, *The Memoirs of Herbert Hoover: Years of Adventure, 1874–1920* (Macmillan, New York, 1951), p. 132.
5. G. Reber, *J. Franklin Inst.* **281** (1966).
6. J. R. Oppenheimer, in *Symposium on Basic Research*, AAAS Publ. No. 56, D. Wolfle, Ed. (AAAS, Washington, D.C., 1959), p. 11.
7. G. Reber, *Castanea* **25**, 122 (1960).
8. R. Leighton, R. Howard, H. Zirin, *Science* **147**, 1087 (1965).
9. G. Reber, *IEEE Trans. Military Electron.* **8**, 257 (1964).

Conservation of What?

I heartily agree with P. T. Flawn ("Geology and the new conservation movement," 28 Jan., p. 409) that the absence of geologists from today's conservation groups is unfortunate. It is also unfortunate that the training of geologists, foresters, wildlife biologists, and others who can contribute to conservation is usually deficient in the humanities and the social sciences. . . . The conservation movement is severely handicapped by a shortage of men of broad vision. . . .

Flawn criticizes "preservationists" as being unrealistically "opposed to change." But preservation of noneconomic values has its place along with sensible exploitation of natural resources. It is shortsighted to say, as Flawn does, "The preservation of an old building simply as an architectural and historical monument in the midst of a growing city where there is great demand for space can hardly be justified unless the building can be converted to serve a useful purpose as well as being a monument. This is multiple use." In this sweeping statement, the University of Texas professor says, in effect, that the Alamo in San Antonio is useless, that it should either be destroyed or converted into—for example—a shopping center. Can a dollar value be placed on the Alamo? It is a priceless shrine to patriots who died for the sake of Texas liberty. Texans unborn deserve the opportunity to visit the Alamo. As a citizen of Illinois, I would gladly pay taxes to preserve the Alamo. Illinois has some old buildings, too. How much is the Lincoln home in Springfield worth? The house is near the state capitol, and the site would be desirable for an office or an apartment building. Would its destruction be progress? Would the destruction of the Acropolis in Athens and the construction of a hotel on its site be progress? How about Mount Vernon as the site of a sewage treatment plant, and Independence Hall as an office building? Wouldn't historians and architects be better qualified than geologists or economists to judge the importance of such buildings and sites?

Flawn continues, "Likewise, preservation of a potential rock-quarry site as a woodland glade constitutes elimination of a valuable mineral resource and costs society a substantial amount of lost tax revenues and lost payroll." But doesn't the value of the glade depend also on its botanical and ecological significance? Who is better qualified to judge the importance of a particular woodland glade, petroleum geologists or a team of plant ecologists, plant taxonomists, landscape architects, and park planners? Gravel pits are needed, but so are woodland glades, especially near centers of population. Certainly the redwoods of California could be eliminated to someone's profit. Grand Canyon can be converted to Grand Lake and enhance the real estate market in Central Arizona. . . .

The starving and impoverished, to be sure, can have little interest in esthetics. In conservation, as in other large problems, there are no short cuts to wisdom. We need master planning for resource use on the international as well as a local scale. But man's future does not rest upon economic expediency alone. We need to define and practice what the late Aldo Leopold referred to as the land ethic. Both tangible and intangible values must be considered. Why shouldn't we be willing to pay a price for the preservation of beauty, of flora, fauna, and geological wonders, and of reminders of history, all of which enrich the quality of man's existence?

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. . . Flawn picks a questionable example to illustrate what he refers to as "the multiple-use concept." Discussing the choice between preservation of a woodland glade and operation of a rock quarry, he writes: "In line with the multiple-use concept, the rock could be quarried over the economic life of the deposit and thereafter the area could be landscaped and restored for other uses." What he proposes is not multiple use; it is one kind of use followed by another kind of use, and