Comments and Communications

Radio Noise of Ionospheric Origin

Numerous investigators (Dellinger, J. H. *J. appl. Phys.*, 1937, **8**, 736; Heightman, D. W. *Wireless World*, 1938, 356; Stetson, Harlan T. *Science*, 1948, **108**, 354) have reported observation of a special type of radio noise, frequently associated with ionospheric disturbances. In most of these cases no source of the noise was indicated, although a few investigators suggested that it originated in the ionosphere (Arakawa, D. *Rep. rad. Res. Japan*, 1936, **6**, 31; Nakaganic, M. and Miya, K. *Electrotech. J. Japan*, 1939, 216; Watts, J. M. *Terr. Mag. atm. Elec.*, 1946, **51**, 122). For the past two years, measurements of cosmic noise have been carried out at the Central Radio Propagation Laboratory at frequencies of 25, 50, 75, and 110 megacycles. The antennas for these measurements consist of half-wave dipoles one-quarter wavelength aboveground. These have broad patterns so that radiation from nearly the entire sky is received. On numerous occasions, large amounts of noise were observed for sustained periods of several hours, which exceeded the recording limits of the instruments. Because of the broad patterns of the antennas, it seemed unlikely that this noise could originate in the sun, and an effort was made to determine whether the noise was coming from the entire sky or from the sun alone.

An opportunity to check this effect was afforded on November 23, 1949, when the recorders went to the scale limits. A solar radiometer was available at the time, adjusted to a frequency of 50 megacycles. It consists of a 25-foot Wurzburg type parabola excited with a folded-dipole antenna and reflector elements each approximately 9 feet in length. During the period of high noise level, the solar radiometer was directed at different parts of the sky, with the result that no appreciable decrease in the intensity of the radiation was observed. The radio noise field intensity was approximately sixfold that normally received from a quiet sun. Only when the antenna was directed toward the ground did the radiation fall off appreciably. A solar radiometer operated at 480 megacycles and pointed directly at the sun showed no unusual disturbances. Although the pattern of the Wurzburg antenna is very broad, this seems to be sufficient evidence that the radiations observed were coming from the entire sky and not from the sun. Presumably, this radio noise was of terrestrial origin, generated in the outer atmosphere of the earth.

HERMAN V. COTTONY
National Bureau of Standards, Washington, D. C.

Errata

In our recent article ("Production of Mesons by X-Rays," by Edwin M. McMillan, Jack M. Peterson, and R. Stephen White," *Science*, December 2), the second sentence in the last paragraph on page 581 contains a typographical error. When a negative π-meson is made by a photon striking a neutron, the products are π^- and p^+, not π^- and π^+. Also, near the center of page 580, in the third paragraph "π = mesons" was incorrectly printed for "π-mesons."

EDWIN M. MCMILLAN
Radiation Laboratory
University of California, Berkeley

Scientists' Responsibility for Preventing War

The article "Scientists, Scientific Societies, and the Armed Forces," by Herman S. Wigodsky, in the August 5 issue of *Science* is clearly reasoned; it definitely needed to be written. Scientists ought to be grateful to Dr. Wigodsky for having opened this question.

In two respects, however, I believe the article is, shall I say, too brief. That is a soft way of saying something that could be said with hardness, but would probably be misunderstood. Of the two, the minor one is that he says: "A mechanism can be established whereby scientific societies and academic institutions may assist the armed forces in obtaining necessary scientific personnel of sufficiently high caliber to meet the needs." This sounds very much like assigning (drafting) people. Possibly it only means publicity that would encourage voluntary enlistment.

My other point is a major one. Dr. Wigodsky refers almost casually to a coming conflict, and to the obvious great importance of scientists when that "national emergency" comes. But what about encouraging academic institutions, scientific societies, and individual scientists to take up the job of preventing the war? In that service is where true patriotism and civic duty lie. There is the Number One job; war tools are necessary, probably, but second.

Of course one could argue that by turning our great scientific ability to the help of the armed forces we might make such a big noise that temporarily we could provide America with a peace-by-force-and-bluster. There is something to be said for that. But should not Dr. Wigodsky, or someone equally as eloquent, write an article for *Science* on the responsibility of scientists and scientific societies to work with triple effort on the job of construction rather than destruction, on the problem of survival rather than questionable domination, on the job of preventing the "national emergency" to which he refers?

There are a few people of my acquaintance (very few, but possibly many more of Dr. Wigodsky's) who believe we could have a jolly good war, with our scientists being exceedingly ingenious and murderous, and still save civilization for the further advance of science and human welfare. But most of us who think hard about it, and who look at the continuing tragedy of the last conflict, have grave doubts about the outcome of World War III.

I still believe that a good job has been done in opening this question. I hope the scientific societies may get more socially conscious, that they may work for the constructions of peace as hard as they work for the destructions that are probably inevitable if we build further our already overwhelming military might.

HARLOW SHAPLEY

Harvard College Observatory,
Cambridge, Massachusetts

It is not my purpose either to enter into a debate with Dr. Shapley concerning several of the points raised in his letter or to defend the statements I made in my article, but rather I desire to supplement these statements in order to clarify any misunderstanding.

In regard to the first of the two points raised, I certainly did not mean to imply a draft of scientists. I stated, "The medical profession, having failed to take similar action in the face of a parallel and long-standing problem, is now faced with a draft. Will a similar crisis be required to stimulate scientists?" I believe that a draft of scientists would be an admission of failure on the part of science as a whole and on the part of the military—failure to establish conditions conducive to research in the armed services. My feeling is that science has an obligation to see that the armed services are made attractive to scientists. Having done this, science has a further obligation to assist the services in staffing military research organizations by encouraging scientists to enter the armed forces and by assuring them that they will have the weight of organized science behind them. I have opposed (somewhat vociferously) a draft of medical personnel, and I would oppose equally a draft of scientists. However, I do believe that unless physicians in the field of medicine, and scientists in their field, take vigorous action soon, the physicians certainly and the scientists probably will be faced with a draft in the future.

Dr. Shapley's second point is an exception to my tacit assumption that there will be another world conflict. I have made this assumption on the basis of world events since the end of World War II which indicate that man has not changed his attitude toward war, and on the historical basis of man's previous approach to the solution of his problems in international relations. I will not say that World War III is enevitable, but based on these two observations, I do believe that it is likely. In view of this, it appears to me that scientists have a double obligation—to work for peace, but at the same time to prepare for national defense. I do not believe that these two obligations are in conflict.

I am in complete agreement with Dr. Shapley regarding the desirability and necessity of scientists' working toward a permanent peace not because they are scientists, but because they have been trained in a way of thinking that views war as the absurdity and illogicalness it is. It has been my experience, however, that scientists all too frequently shed their fine scientific way of thinking with their laboratory coats. I share Dr. Shapley's hope that "the scientific societies may get more socially conscious." My article is objective evidence of this.

I support wholeheartedly any logical unemotional efforts by scientists or anyone else to establish a permanent peace. This is consistent with a scientific training and particularly with a scientific way of thinking. At the same time, I am convinced that, for the preservation of our way of life, it is imperative that we provide ourselves with the strongest possible national defense until such time as a permanent peace is established. Such a strong national defense can come about only as the result of the cooperation of scientists and the armed forces.

I appreciate greatly Dr. Shapley's good letter and the motives which prompted him to write it.

HERMAN S. WIGODSKY

Silver Spring, Maryland

The Probit Method

In "Application of Probits to Sweet Corn Earliness Data" (*Science*, May 27), Gordon Haskell proposes the use of the probit method to estimate 50 percent silking time of corn from data showing tassel date for each plant separately. This is a sufficiently common misapplication of the probit method to justify a correction.

In his opening remarks on the probit method D. J. Finney says on this point, (*Probit method*, Cambridge: University Press, 1947. P. 14) "If the tolerance [*read* tassel date, J. C.] of each subject has been separately and independently determined, the set of values obtained may be subjected to the same analytical processes as measurements of length or weight; the estimation of means and standard errors, the comparison of distributions, and the making of tests of significance present no new features." In short, when tassel dates are available separately for each plant, the 50 percent tassel date may be computed directly as the median of the recorded tassel dates. If the sample data have been drawn from a normally distributed population, as is assumed by Haskell, a more efficient estimate would be the mean tassel date. The mean tassel date can of course be computed by converting each observed tassel date to days from some convenient reference date, such as July 1 or July 15. Similarly, the slope of the probit line can be computed as the reciprocal of the standard deviation of the individual tassel times.

The basic reason for the inapplicability of the probit method in this and similar problems in which individual measurements are available for each experimental subject is that the assumptions on which it is based are not satisfied. The probit method assumes that the sample data showing percent having silked on or before a given date are statistically independent. That this assumption is not satisfied in the present example is apparent from the fact that if the sample showed that 10 percent of the plants had tasseled by July 20, it would of necessity show that 10 percent or more had tasseled by July 21. In the pharmacological and other applications in which the assumptions of the probit method are satisfied, no such necessity exists. It is possible, and frequently happens, that a larger proportion of animals in an experiment are killed by a low than by a high dose of a toxic substance. A linear relation between probits and stimulus is a necessary, but not, as is sometimes assumed, a sufficient condition for the applicability of the probit method.