Letters

Congress and Space Projects: Imbalance in Hearings

McDonald's letter (29 May 1964) calling attention to the unscientific approach to certain aspects of the lunar exploration program is most timely. I suspect that a large part of the difficulty stems from congressional procedures in allocating funds for space research. . . .

During the hearings on 10 and 11 June 1963 before the Senate Committee on Aeronautical and Space Sciences, testimony was taken from 12 witnesses. Of these 3 opposed and 9 were in favor of the space program. Furthermore, 8 of the 9 proponents either were receiving substantial grants from NASA or represented companies and universities that were.

The hearings before the House Committee on Science and Astronautics on the 1964 NASA authorization resulted in 3540 pages of testimony printed in five volumes. There were more than 100 witnesses, all associated in some fashion with NASA, and no witnesses who were critical of the fundamentals of the space program. In the hearings before the same committee on the 1965 authorization, 2840 pages of testimony were printed. Again, all the witnesses were associated with NASA, and there were no critical witnesses.

The danger in this unbalanced testimony seems clear. Proponents of the moon race can make all sorts of statements and claims which are not subject to rebuttal, since the congressmen are not scientists (although some appear to have sound engineering backgrounds). Two examples are of interest. On page 204 of the hearings on the 1965 NASA authorization, a NASA official, commenting on the search for life in space as related to the fundamental nature of life, said:

> At this stage in the development of bioscience, the bioscientific community finds the pursuit of these basic discoveries and the development of an encompassing biological theory the most important single task of the day.

On page 503 of the same hearings, another NASA official, replying to a question about newspaper criticism of the Apollo program, said:

> I think you will find there is a growing body of scientific opinion which has now begun to examine in some detail what, actually, one can do in space. . . . That growing body of scientific opinion is that it is, in fact, not only valuable but essential to have a man to make the selection to do the geological survey work. . . .

One wonders how these NASA officials know what the bioscientific community is thinking, or how general scientific opinion views the lunar manned-spaceflight project. I am not aware of any polls or questionnaires seeking our opinions. On pages 1662 and 1663 of the hearings on the 1964 NASA authorization, the chairman of the Subcommittee on Space Sciences says:

> As I have said before, I think none of us really are qualified to interrogate scientists, because we ourselves are not scientists. I would say in the field of science we are probably less informed than other members of other committees are informed on the subjects before their committees. Another real problem which I see which makes it difficult for this committee to function properly is that we never get two sides of the argument. . . . We don't have people appearing before this committee in opposition to the manned lunar landing program, or the Orbiting Geophysical Observatory, or the Surveyor program, or whatever it is.

I would like to suggest a solution to this problem. Let the congressional committees invite scientists who are critical of certain aspects of the space program to testify before them.

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One-Tailed Test and Other Statistics

The excoriation of the one-tailed test of significance by W. Dixon Ward (29 May, p. 1089) underscores the fact that authors and teachers of statistics have somehow failed to make clear just when this procedure is appropriate. It is not "a ploy in which the researcher claims partial precognition . . ."; it is the appropriate statistical procedure when the research worker is interested only in a unidirectional effect.

When a treatment is being investigated in which there is an equal interest in an increase or a decrease resulting from its application, a symmetrical (in the probability sense) two-tailed test is required. If more interest is associated with, say, an increase, an asymmetrical two-tailed test would be appropriate. When all the interest is focused on, say, an increase, the extreme of an asymmetrical test, a one-tailed test of significance, is the procedure of choice. Thus when the experimenter cares only whether treatment X produces an increase in Y or no effect, and *does not care* about a decrease in Y, a one-tailed test is appropriate, correct, and optimum.

The only "abomination" is the a posteriori selection of a one-tailed test; and, if the reader feels so inclined, he can transform this into a two-tailed test by the simple expedient of doubling the significance level quoted by the experimenter.

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In response to W. D. Ward's letter I happily stand up to be counted as one of "those who go farther and say that the gathering of data must be preceded by a specific experimental hypothesis." Physicians are generally among those who believe that once a phenomenon has been observed it is fact and truth forever after. Consequently a sample size of one is sufficient. Such abominations as probability, inference, tests of significance, distribution theory, are all worthless window dressing, and who needs statistics (or statisticians) anyway?

While it is obvious that statistics is no panacea for poorly organized "research data," it is nevertheless a useful tool which most logical-minded, objective researchers embrace. When a scientist and a statistician talk over a problem (discuss an experimental design), aspects of the problem are very often uncovered which might have gone undetected had not the statistician needed to ask questions in order better to understand what it is the scientist is trying to learn. Often a statistician will be able to point out