Letters

Biological Research Center

I hope I will not do violence to the logic of John Platt's article on national laboratories for biology [*Science* **136**, 859 (1962)] by putting it in the form of a syllogism, as follows:

I. ". . . biologists will simply not be able to solve these problems [developing a direct read-out microscope, and so on] successfully unless they begin to form new organizational arrangements" (p. 860).

II. "At present we have no comparable arrangements or organizations [comparable to those in the physical sciences] for systematically exploring and developing new devices and methods for basic biological research" (p. 859).

Conclusion. ". . . it may be that the only way to achieve [rapid development of new tools for biology] will be to take a leaf from the physical scientists' book and establish a permanent national biological research and development center, a kind of small-scale Los Alamos for biology" (p. 860).

Apart from the fact that Los Alamos will seem to many scientists a poor choice for a model of the laboratory of the future, I think that when the argument is stated in this form it is immediately obvious that the conclusion does not necessarily follow from the two premises. With equal logic (or lack thereof) one might state that we need to take a leaf from European experience with CERN (the European Organization for Nuclear Research) and establish a supranational biological research center. Or to move in another direction, perhaps such a laboratory should have an urban-regional basis. For instance, the scientific and engineering firms of the San Francisco Bay area, in cooperation with other industries and with educational institutions, might pool their resources to establish and support such a biological research center. The federal government might aid such developments through tax relief to cooperating corporations.

From a strictly logical viewpoint Platt's conclusion is qualified properly by the phrase, "it may be that the only way"; however, nowhere in his article does he explore the possibility that establishment of a national research center may *not* be the only way. I would merely like to suggest that anyone seriously considering the problem situation so excellently stated by Platt should examine several alternative methods of dealing with that problem.

JOHN MARTINSON

2214 Russell Street,
Berkeley, California

On the Moon Illusion

I read with great interest the articles by Rock and Kaufman on the moon illusion [*Science* **136**, 953, 1023 (1962)]. Their proof that the visible terrain between the observer and the horizon contributes to the moon illusion is an important finding and is based on ingenious experiments. I cannot, however, agree with the authors when they imply that the presence of terrain provides the sole cause of the illusion. I do not think that their method of measuring the illusion with the help of two artificial moons at optical infinity yields the full illusion. They obtained average illusion ratios below 1.5, whereas an earlier investigator, Pozdena, who had his subjects match an artificial moon at a distance of 4 meters to the zenith and the horizon moon, obtained the much larger average illusion ratio of 2.5. I strongly suspect that additional factors enter into the ordinary moon illusion.

One of these is the angle-of-regard illusion, which was most thoroughly investigated by Holway and Boring. It consists in a strong reduction in apparent size when the moon is viewed with eyes raised or lowered relative to the head. Holway and Boring measured this illusion by matching the apparent moon size with a luminous disk at a distance of 3.5 meters. Rock and Kaufman, however, found no effect of eye elevation on apparent moon size and therefore suspect Holway and Boring's results. But there is no necessary contradiction: Rock and Kaufman used a different technique, employing an artificial moon at optical infinity as comparison object instead of a luminous disk at near distance. In this context, they claim that only their technique is adequate and has a bearing on the ordinary moon illusion.

To me the difference in the outcome of the two experiments makes sense. As Rock and Kaufman explain, perceived size is a function of registered distance; with the size of the retinal image constant, the larger the distance the larger the perceived size. Registered distance depends on two kinds of cues, convergence of the eyes and configurational cues when patterned surfaces extending toward the object are visible. Being caused by eye position, the angle-of-regard illusion clearly must be a matter of convergence and thus should occur only when the distance of at least one of the two objects to be compared is within the effective range of convergence. Since this is not the case in Rock and Kaufman's experiments, it is not surprising that they did not obtain the angle-of-regard illusion.

I turn now to their claim that Holway and Boring's way of testing the moon illusion, by matching an object at close distance to the apparent moon size, is inadequate. They point out that the apparent size of an object at infinite distance is to a high degree undetermined because no adequate cues for distance are available, and that therefore the comparison object ought to be at infinity also. I do not agree. I prefer the procedure used by Holway and Boring and by Pozdena. If a comparison is to tell me something about the perceived size of an object, I prefer to have as comparison object one whose perceived size is accurately determined by distance cues and therefore definite and not spontaneously variable.

Rock and Kaufman also claim that their method of comparing two moons at optical infinity is more in keeping with the ordinary moon illusion. They overlook here, I think, the work of Schur, who showed that the moon illusion by no means pertains only to objects at infinity. Experimenting in dark rooms of various sizes, Schur demonstrated a strong size-elevation illusion which ties the moon illusion to size perception at medium distances.