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.

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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.

Working with two projected disks of light, one in horizontal direction and the other above the observer and at the same distance from him, Schur found a size difference in favor of the horizontal disk which increased steadily with increasing distance of the two disks from the observer. The distances were varied in six steps from 3 to 33 meters. For the distance of 3 meters the mean illusion ratio amounted to 1.16, and at distances of 22 and 33 meters, to 1.71 and 1.87. Today these findings are supported by the result of Hermans' whose illusion ratio of 1.06 for distances of 4 feet fits well with Schur's data (and does not refute them, as Rock and Kaufman claim), and by the results of Leibowitz and Hartman; only Rock and Kaufman's planetarium experiment is in disagreement.

I see Schur's size-elevation illusion as a thing apart from the terrain effect. Rock and Kaufman surmise that there was enough stray light in the rooms used by Schur to illuminate the floors. I don't think that this explanation is tenable. Schur's report shows that a great deal of attention was paid to the task of keeping walls and floors invisible; the projectors were completely enclosed, and the subjects were not allowed to become dark-adapted. Besides, if Schur's results were really due to a vestige of a terrain effect, one would expect her illusion ratios to be smaller than those obtained by Rock and Kaufman with good terrain visibility. The opposite is the case; for the 33-meter distance Schur's illusion ratios show an effect almost twice as large, and with much less variability. Since Schur also found her effects to be partly independent of the angle-of-regard illusion—she did obtain an angle-of-regard effect with disks of light at distances of 5.2 meters which, when set against the ordinary size-elevation effect, would diminish but not overcome it—it looks as if we have here a third condition which can produce a moon illusion.

I want only to set the record straight, not to advocate more discussion or research on the ordinary moon illusion. As is the case with many phenomena of our daily lives, its causes are probably too complex to warrant detailed unraveling. But I don't like to see the angle-of-regard illusion and the size-elevation illusion neglected as unimportant. These facts are of genuine scientific interest, having relevance to perception independent of the moon illusion.

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Kaufman and Rock's thorough investigation of the dependence of the moon illusion on the presence or absence of a visible, uninverted, intervening terrain makes important reading and seems to me, the surviving member of the Holway and Boring team and Kaufman and Rock's most clearly defined target, to lend strong support to Ptolemy's theory and the effectiveness of Emmert's law in this situation. It can be said, of course, that Ptolemy's theory *is* Emmert's law, and that everyone knows that Emmert's law works. Emmert's law is the principle that the size of a perceived object increases with its perceived distance in those unusual cases, such as a visual afterimage, when the size of the retinal image remains

constant as actual distance varies. If visually filled distance looks greater than empty distance, then the horizon moon, separated from the observer by a filling of terrain, should look further off and, subtending always the same visual angle, should appear larger. The difficulty here is that the preponderance of evidence, against which Kaufman and Rock cannot do better than set up their fiat, is that the horizon moon looks nearer than the moon in elevation. Of course, say the subjects, it looks nearer because it looks bigger, and that leaves us with a paradox.

Neither Holway and I nor Kaufman and Rock made any well-designed study of the perceived distances of the moon, although Holway and I did ask questions about perceived distance of a good many observers. If one is studying the effect of apparent distance on apparent size, one would do well to measure the relative apparent distances as well as the relative apparent sizes, would one not?

Kaufman and Rock say that the size of the elevated moon is indeterminate. Now surely that is nonsense. All size is relativistic, and, when not under comparison, size is necessarily indeterminate. You spend an hour watching a puppet show on a lighted stage surrounded by the dark, the curtain falls, the general lights go on, and the actors themselves who make the little figures work appear to take their bow. A gasp of astonishment goes through the audience when these apparent giants appear. Actually, the puppets lost apparent size when they lost their standard of comparison. No perceived object has determinate size except as its size is fixed in a comparison.

Then Kaufman and Rock object to the fact that Holway and I used heterotelic comparisons of size (*heterotelic* is from *tele*, afar, as in *telescope*, not from *telos*, end, as in *teleology*). Abstractive comparisons, such as the heterochromatic equating of visual brightnesses, are more difficult to make than judgments of identity, but they are not invalid. The measure of difficulty lies in the large size of the interval of uncertainty about the point of subjective equality and in the longer decision times for the more uncertain judgments. Given a difference somewhat larger than the interval of uncertainty, be that interval large or small, the judgment is immediate. Holway and I found, for our extreme heterotelic comparisons, that the judgments were certain and instant. Conversely, even identity judgments near the critical point are slow

and unsure. There is nothing wrong with the heterotelic method except that it is less precise than homotelic comparisons of size, or, one might say, the heterotelic method is as accurate as the homotelic but in respect of larger units.

Now let us raise the question as to how much we must stretch credulity to believe that Emmert's law, set up by the filled distance of the terrain, can account for the moon illusion. I have read Wallach's letter about these researches with assent. He notes that there is probably no single correct theory of the moon illusion, and even Kaufman and Rock admit in a note that more than one principle may be operating synchronously. Emmert's law is consistent with the principle of size constancy: that the receding object maintains the same apparent size as its retinal image shrinks, and thus that a receding object whose retinal image does not shrink (an after image) grows in apparent size. Another law, incompatible with this one, might be called Euclid's: a receding object appears to shrink in size as its retinal image shrinks; and conversely, *mutatis mutandis*. The observers in the Holway and Boring studies were appealing to Euclid's principle when they said that the horizon moon looked near because it looked so large. Certainly this principle often works, and then, of course, size constancy fails.

Now, Kaufman and Rock are arguing that these two principles operate simultaneously, each of them effectively, though in opposite directions. They adopt Woodworth and Schlosberg's word *register*, which means that a perceptual cue operates below the level of consciousness. A distance may be great as registered while being small as perceived. Let me extend this argument and suggest its treachery by being explicit. The visible terrain is effective as a cue. It operates below the level of consciousness to register the moon as far away. Under Emmert's law the moon is thus, because of its remote registry, perceived as large. Looking large, it seems, under Euclid's principle, near. And might one not then add that, under Emmert's law appearing near, it looks small? That would be the whole circle of logic of these two principles: the horizon moon, being far, is big; being big, is near; being near, is small. Obviously, still more research and more thinking are needed on this matter.

Kaufman and Rock could have aided their readers by being more precise in

their terminology. Again and again they speak of "the greater apparent distance" of the horizon moon. Only occasionally do they use the word *register*, introducing it in connection with a half-hearted admission that the apparent distance to the horizon moon may be small although the registered distance is great. They speak of "phenomenal size" and seem to mean by that term the consciously apparent distance. They speak occasionally of "report" as if it could be expected to contradict the character of the phenomenon reported upon. The situation described and the theory based upon it need careful explication, and it would help were these five words defined and used with rigor: *apparent, perceived, registered, phenomenal,* and *reported.* Then the paradox outlined earlier would become clear —or so I think.

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Wallach and Boring make a number of interesting points concerning our work on the moon illusion. On the whole they raise different arguments, except for one on which they agree. They both defend the method used by Boring and his colleagues (and Pozdena) to measure the moon illusion (in fact, Wallach believes it preferable to the method we have developed). We still disagree. The disk projected on the nearby screen in Boring's method appears to be of a definite linear size. But the moon does not; we believe it is correct to say that the moon is indeterminate as far as an impression of linear size is concerned. How, therefore, can we rely on a comparison object which appears to be of a definite linear size to tell us about the apparent size of the moon? Wallach implies that such a comparison is desirable because at least the size of the comparison object is stable and definite. How, then, does he explain the fact reported by Boring that, if the observer backs away from the screen, the disk he has just selected no longer appears to match the moon? Obviously the moon's size would not be affected by these few additional feet and the disk's size would not change, because size constancy would obtain. Apparently the observer is, at least in part, comparing the moon and the disk in terms of their visual angles. Where is the stability of the comparison object Wallach hints at? (It is surprising

to find Wallach arguing for Boring's method when he has recently published a paper, with McKenna, the main conclusion of which is that comparison of an indeterminate object at an indeterminate distance with a determinate object at a determinate distance is essentially not possible!)

Boring's statement that all size is necessarily relativistic obscures the important difference between determinate and indeterminate linear size. An apple at some definite distance has determinate size, whether or not one is comparing it to anything. Of course, by "determinate size" we mean a size that has meaning in the whole scale of sizes in our world (for example, smaller than the hand but bigger than a grape), and this is no doubt what Boring means by "relative." But the size of the moon, particularly when it is at the zenith, is none of these. One cannot rank it anywhere on the scale of linear sizes we deal with. It is not even necessarily very large, linearly speaking. Unlike the apple, it is indeterminate, and for a very good reason. Its distance is more or less indeterminate. That it is indeterminate is an empirical fact. Subjects cannot decide or agree on its linear size. Of course, the moon does have a size relative to other astronomical objects—for example, to the distance between two stars, or to itself in other positions. But this is not a matter of relative linear size. Our method is based on such a nonlinear comparison.

Wallach seems to forget that our focus was the moon illusion as it exists in daily life. Are we to believe that that illusion requires comparison of the moon with some nearby object and that it is the cross-comparison with that object that mediates the moon illusion? That is, are we to believe that the illusion is based on the observer's first comparing, say, the zenith moon with some nearby object and, on some other occasion, comparing the horizon moon with that object? When we say the horizon moon seems large to us, don't we mean large in comparison to the size we remember the zenith moon to be? That is why we strove to use a method wherein the observer could compare one moon with the other, as he does in daily life. Even if such a comparison is not reliable for the reason Wallach suggests (although we don't agree that this is the case), the plain fact is that that comparison *is* the moon illusion. Boring himself said in 1943 that the illusion "is a comparison of the moon with itself seen previously."

Wallach's defense of the eye-elevation hypothesis is that eye elevation affects convergence and convergence could not influence the illusion unless "the distance of at least one of the two objects to be compared is within the effective range of convergence." Hence, a method such as Boring's is required to produce the effect. Our question, again, is: Does the illusion in daily life require mediation by way of some nearby comparison object? If so, no one seems to be aware of it. Hence, if Wallach's reasoning is correct, eye elevation could not possibly explain the moon illusion, although it could explain Boring's findings. However, a point we made in our paper is worth repeating—namely, that in spite of the quantitative results based on eye elevation, the observers in the Holway and Boring studies were impressed with the large size of the (geographical) horizon moon and the smaller size of the (geographical) zenith moon.

The supine observer says the zenith moon "does not appear large to him, yet he equated it to an artificial moon to which, when erect, he had already equated the horizon moon." As Patrick Rizzo recently pointed out in the bulletin of the Amateur Astronomer's Association, the moon illusion is a "seeming," an impression of size. Boring's subjects reported this impression while giving quantitative data of a different kind. In such a case of contradiction it is the method employed that must be questioned. One final point about Wallach's argument: we do not understand why convergence with respect to the nearby *comparison* object is required before eye elevation in viewing the zenith moon can emerge as a cause. The convergence is the same for the comparison object whether the standard is the horizon or the zenith moon. Does Wallach mean to suggest that what is crucial is the *transition* from eyes level to convergence on a nearby object or from eyes elevated to convergence on a nearby object? What evidence is there for such an effect, and what is its rationale? Suppose the observer rests his eyes a moment after looking at the moon before focusing on the comparison object. Would Wallach now predict no moon illusion?

There is another curious fact. Both Wallach and Boring seem to admit that our evidence for the role of the terrain is convincing (although this hypothesis had been discarded ever since Boring's research was published 20 years ago). If apparent or registered distance does affect the illusion as strongly as our evidence shows, and if Boring's method is indeed a good one, why is it that his data show no influence of the terrain? For example, Holway and Boring obtained an illusion ratio of 1.0 when observers viewed a horizon moon normally and a zenith moon with head elevated and eyes level.

We turn now to a second point. Boring still seems reluctant to subscribe to the apparent-distance hypothesis (in spite of his opening sentence) because of what he calls "the preponderance of evidence . . . that the horizon moon looks *nearer* than the moon in elevation." The preponderance of evidence to which he refers is presumably the reports of observers that, of the two, the horizon moon is *nearer*. We will not repeat here all our reasons for not being greatly concerned with this report. We thought we had done more "than set up a fiat" when we suggested that such reports were probably based on judgmental reactions to the difference in *phenomenal* sizes of the two moons (which difference *is* the moon illusion) and when we backed up this suggestion by two experiments. One showed that subjects do use the apparent relative size of the moon as a basis for answering the question, Which moon seems nearer? The other showed that, with moons absent, observers do report the horizon sky to be farther away than the zenith sky.

Boring tries to reduce our argument to an absurdity by the following deduction: if the horizon moon is indeed judged nearer because it seems larger, it now ought to seem smaller because

it is judged nearer. We would answer that a judgment of this kind (which in this case is perhaps only elicited by a question) does not influence perceived size. A judgment or inference is not to be equated with a sensory cue to distance. Perception is rarely if ever influenced by knowledge about the situation—if it were there would be no moon illusion.

Wallach believes we have not obtained the full moon illusion, citing earlier work by Pozdena in which an illusion ratio of 2.5 was obtained. This argument has little force when the method used by Pozdena (being like that of Boring) is itself in question. We wonder, however, what Wallach's explanation is for the fact that Boring and his colleagues did not obtain an illusion of that magnitude with the same method. Their illusion was closer to the values we obtained.

Finally we come to Wallach's reference to the earlier experiments of Schur in dark rooms of various sizes, and to the recent work of Hermans and of Liebowitz and Hartman. Common to all these experiments is the finding of an illusion when equidistant horizon and zenith objects are compared in a dark field at a finite distance. First, we would point out that this method is different from that of Boring's. It is in fact similar to ours in that both objects are now at the *same* distance. To the extent that distance perception is diminished in the dark, the method is even closer to ours. Hence, Wallach has shifted his position insofar as preferred methodology is concerned (for example, convergence could have little effect if both objects are at 33 meters). Second, the question arises as to why the effect increases with distance (it is quite small at 3 meters, for example) if convergence is what is crucial. Boring answered this question long ago by saying the moon illusion is greatest when distance is indeterminate. Third, if the previous point is correct, is not the ideal experiment one in which distance increases to a maximum? (Extrapolation from Schur's curves leads to this prediction, and Hermans made the same prediction.) In that case, our indoor experiment in a dark field with the two disks at optical infinity is ideal. Yet we obtained only a negligible effect. Our planetarium dark-field experiment yielded a similar result.

This leaves us with the problem of explaining Schur's results and those of Liebowitz and Hartman. Perhaps Wallach is right in stating that our specu-

lation that they are based on stray light is not tenable. A more promising lead is a recent finding of Gruber, King, and Link to the effect that an illusion indoors depends on the observer's first gaining some impression of the distances involved in the room prior to the darkening of the room—on a kind of memory effect which may itself be a function of (remembered) apparent distance.

We agree with Boring that there is a tendency toward imprecision in our use of certain terms. We believe this is largely due to the fact that we sought to derive the moon illusion from certain already known facts and principles in the area of size perception. Unfortunately, that area is itself still beset with theoretical difficulties.

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Battle Not Won

The item entitled, "Congress shrugs at proposals on laboratory animal welfare" [*Science* **126**, 863 (1962)], could easily give the impression that all is well and that those interested in animal research have little cause for concern.

It is true that Congress will not have time in the few remaining weeks of this session to consider proposals to regulate research and teaching involving the use of animals. It probably is true also that most members of Congress do not at this time favor such legislation. Further, it is true that there are only about 6 million antivivisectionists in the United States—a small minority of the population.

But this is where the good news ends. Members of Congress report that mail on the Moulder, Griffiths, and Clark bills is running approximately 20 to 1 in favor of regulation. Experience in legislative bodies around the nation shows that politicians will, in the end, do what they believe the voters want, regardless of their own convictions. Experience further shows that a tiny minority of antivivisectionists can, by writing letters day in and day out, create an illusion of public sentiment that is very difficult to ignore. Thus, the antivivisectionists won in the states of Illi-