LETTERS TO THE EDITOR

Letters express personal opinions and may critically examine any aspect of physics or physics instruction. They need not conform to our regular editorial policy and ordinarily are not reviewed. From the large number submitted, published letters are selected for their expected interest for our readers. They must be brief and are subject to editing, with the author's approval of significant changes. Comments on regular articles and notes are reviewed according to a special procedure and appear in the Notes and Discussions section (see the "Statement of Editorial Policy" in the January issue). Running controversies among letter writers will not be published.

THE OPTICS OF THE CRESCENT MOON

Lars Falk's reiteration of a well-known problem about the appearance of the crescent moon[1] states that "...many people I have tested [have] failed to solve the problem correctly." Mr. Falk's impression may be due to the fact that his own "solution" is incorrect. In contrast to his claim, it is simply false that the Sun's distance *behind* the moon will lead to any deviation from perpendicularity between the line linking the horns of the moon's crescent and the line linking the centers of the Sun and moon, as has been pointed out in Minnaert's classic book.[2] The correct explanation for the *apparent* lack of perpendicularity involves the observer's projection of this latter line onto the perceived dome of the sky, as has been fully explained by Jearl Walker.[3]

David G. Stork
Clark University
Department of Physics
Worcester, Massachusetts 01610
15 December 1987

[1]L. Falk, Am. J. Phys. **55**, 1095 (1987).
[2]M. Minnaert, *The Nature of Light and Color in the Open Air* (Dover, New York, 1954), p. 152.
[3]J. Walker, *The Flying Circus of Physics with Answers* (Wiley, New York, 1977), pp. 149, 285, and references therein.

"RESPONSE TO 'THE OPTICS OF THE CRESCENT MOON' BY DAVID STORK"

First of all, I want to apologize for presenting a problem without knowing the answer. My best excuse is perhaps Strindberg's book itself. *A Blue Book* is his last major work and it is outrageous as well as outrageously funny. In two volumes Strindberg presents his scientific and religious views but often with his tongue in his cheek.

His suggestion that the corncrake migrates to the south by first walking round the Baltic can hardly be surpassed as a parody on scientific arguments. In many other cases, however, the reader will doubt both the sincerity and the sanity of the author. Strindberg was, for instance, convinced that he had made gold during his chemical experiments.

Unfortunately I judged all of Strindberg's text in that spirit although the first part of his reasoning is entirely correct. The crescent must be directed toward the Sun since the illuminated surface is symmetric with respect to a plane defined by the Sun, the moon, and the observer. This is also what several readers from lower latitudes claim to see. On the other hand, at Swedish latitudes (Strindberg made the reported observation in Stockholm at 59° N and he was never south of France), a crescent near the horizon will always appear completely vertical to an unsuspicious observer!

The correct explanation is not obvious judging from the many suggestions I have received. The effect is an illusion caused by our inability to extend lines correctly over the sky as pointed out by some readers. An example of this difficulty can be found in Strindberg's text: He very much overestimates the elevation of the moon over the horizon when he says it was 35°. A calculation shows that it was, in fact, 22°.

The descriptions of the illusion provided in the literature are brief and do not really explain it in detail. It would be quite interesting to determine the magnitude of the effect experimentally. This might be done, for instance, by using the artificial moon projector described by Kaufman and Rock[1] and applied by them to explain the better-known illusion of the size of the moon.

If my error has generated new interest in Strindberg's work I can hardly regret it. I still feel, however, that Strindberg should have consulted some scientist—but possibly not me!

Lars Falk
SGAB
P. O. Box 1424
S-751 44 Uppsala, Sweden
16 March 1988

[1]L. Kaufman and I. Rock, Sci. Am. **207** (7), 120 (1962).

EQUILIBRIUM DISTRIBUTIONS OF REPULSIVE PARTICLES IN A SPHERE

How do N identical repulsive particles arrange themselves when they are free to move in a spherical volume or, stated more precisely, which configuration of N particles in a sphere minimizes the sum $\Sigma\, r_{ij}^{-p}$, where the summation runs over all pairs of particles i and j, separated by a distance r_{ij}, and with $1 \leqslant p < \infty$? This problem has received much attention in the past decades,[1] and interest was revived recently by A. A. Berezin,[2] who raised the question of whether it would be profitable not to have all particles on the surface (as was believed to be a prerequisite for the correct solution), but one particle *inside* the sphere and N on the surface. While in the electrostatic case ($p = 1$) a well-known theorem precludes such a situation,[3] it could be possible if the interaction potential r^{-p} were nonharmonic: $p > 1$. Indeed, with $p \geqslant 6.3$ it was found that for $N + 1 = 13$ the situation with all particles on the sphere is less favorable than that with 12 on the sphere and 1 in the center.[3]

We now ask: Can a particle be held in stable equilibrium in (or near) the center of the sphere by the remaining

N particles on the surface for $N < 12$? That is, we search for local rather than for global minima. Why the existence of local minima may be expected can be illustrated with the simple $N = 4$ case. Four particles are placed in a tetrahedral arrangement on the sphere and a test particle is placed in the center. In the electrostatic case ($p = 1$), a small departure of the test charge from the center in a direction opposite to one of the surface charges will result in a force driving the particle to the surface, even if the surface charges are kept fixed in their positions (if not, the surface-charge distribution will adjust and a symmetrical 1:3:1 configuration results). If $p > 1$, however, the test charge will be pushed back to the center (if the displacement was not too large), *provided the surface charges are kept fixed.*[4] If the surface charges are allowed to move, the barrier preventing the test particle to move to the surface vanishes. It is likely, however, that, with large N and p, the mobility of the surface particles becomes insufficient to remove the barrier, so that the test particle becomes trapped in (or near) the origin.

We have examined the cases $4 \leqslant N \leqslant 8$ with $1 < p < 30$. We started with the known equilibrium configurations[1]: tetrahedron ($2:\tilde{2}$), octahedron ($3:\tilde{3}$), and compressed square antiprism ($4:\tilde{4}$) (*not* the cube 4:4), for $N = 4$, 6, and 8, respectively, trigonal bipyramid (1:3:1) for $p < p_0 = 15.048\,08$ or square bipyramid (1:4) for $p > p_0$ for $N = 5$, and the configurations 1:5:1, $1:2:\tilde{2}:2$, 1:4:2, $1:3:\tilde{3}$ with transition points $p \sim 2$, $p \sim 5$, $p = 5.597\,91$ for $N = 7$. We found trapping to occur in the following cases:

N	$p_{\min} \leqslant$	$p \leqslant p_{\max}$
6	2.4	9.9
7	3	17
8	3	23

The equilibrium position of the test particle was found in the origin, except in the case of $N = 7$, where it was found shifted *ca.* 0.01 from the center of the unit sphere, in a symmetry direction. The position of the barrier was also close to the origin. Pushing the test particle beyond the barrier usually resulted in a change of configuration.

Finally, we note that the physically important value $p = 12$ is in the range of interest for $N = 7$ and 8, and probably beyond.

B. W. Van de Waal
Department of Physics
University of Twente
P. O. Box 217
7500 AE Enschede
The Netherlands
25 January 1988

[1]T. W. Melnyk, D. Knop, and W. R. Smith, Can. J. Chem. **55**, 1745 (1977); H. A. Munera, Nature **320**, 597 (1986); N. Ashby and W. E. Brittin, Am. J. Phys. **54**, 777 (1986).
[2]A. A. Berezin, Nature **315**, 104 (1985).
[3]M. G. Calkin, D. Kiang, and D. A. Tindall, Am. J. Phys. **55**, 157 (1987).
[4]A. A. Berezin, Am. J. Phys. **55**, 199 (1987).

EXTENDING SPACE OR SEPARATING GALAXIES?

I find it difficult to ignore the challenge posed by Lewis Epstein in his letter on the above topic.[1] I suggest that the experiment that can prove that space is not expanding is one that establishes that h and c are invariant with cosmic time.

Presumably, readers will concede that if "space" is capable of extending, meaning that it has some kind of structure that can define a changing unit of distance, then that structure will (1) play a role in determining the fundamental physical constants that relate to space, such as the speed of light c, and (2) be likely to have a particular geometrical significance that relates in some way to the determination of the dimensionless constants that we associate with radiation through space. The latter would involve either G, if one has gravitational wave radiation in mind, or Planck's constant h, if one thinks of photon radiation. Unless one accepts this proposition, the idea that space is extending is merely one option for interpreting the Hubble redshift.

There are theories for the fine-structure constant α, which depend upon the analysis of the space-time metric,[2,3] but these would seem to demand that α is a true constant not varying over the cosmic time scale. Petley has reviewed the evidence showing that hc and c are both constant to part in 10^{12} precision per year,[4] the latter being based on the fact that light from near stars and distant galaxies shows the same aberration. Both theoretical and experimental results would therefore indicate that an expanding space metric has to be ruled out.

However, this does not force us to accept that the redshift arises from Doppler effects caused by galaxies separating. There is the third option known as the "tired light hypothesis." This is a viable alternative in spite of the earlier argument to the contrary, owing to the fact that the space-time metric is the seat of certain background activity involving energy. The Hubble constant has been calculated from the same theory[5] that gave an evaluation of the fine-structure constant,[3] but such theory has yet to find acceptance. Nevertheless, it is clear that if we are now seriously thinking that the expansion of the universe, meaning separation of the matter forming the galaxies, could be a wrong interpretation of the cosmological redshift, then we have no choice but to look into the properties of space or "ether" to search for the truth.

H. Aspden
Department of Electrical Engineering
University of Southampton
Southampton SO9 5NH, England
10 February 1988

[1]L. Epstein, Am. J. Phys. **55**, 970 (1987).
[2]A. Wyler, C. R. Acad. Sci. Ser. A **269**, 743 (1969) and **271**, 186 (1971).
[3]H. Aspden and D. M. Eagles, Phys. Lett. A **41**, 423 (1972).
[4]B. W. Petley, *The Fundamental Physical Constants and the Frontier of Measurement* (Hilger, Boston, 1985), pp. 42–47.
[5]H. Aspden, Lett. Nuovo Cimento **41**, 252 (1984).