reasonable 10- to 15-degree inclination of the equal-pressure surface, was from 110 to 170 grams in this case, in which the object weighed only 650 grams. Although his results were admittedly crude (the scatter in many of the experimental runs exceeded the magnitude of the expected "Hayes effect" itself) and were certainly not conclusive, Scholander in effect invited Hayes to abandon hydrodynamics and to compete with him in the experimental verification of the balance-of-force principle of mechanics. Hayes's explanation is based on such fundamental principles that to deny it is to deny Newtonian mechanics; thus, I cannot agree that the question is "unresolved."

It is curious that whereas Woodcock's original analysis of the forces required for wave riding was in error, his conclusion, that dolphins experience less friction than an equivalent solid body, seems to be true. Quite apart from the evidence of low friction inferred from the unusually high swimming speed of dolphins, mechanical models of dolphin skin made of rubber have been shown to exhibit only about 40 percent of the surface drag coefficient of otherwise equivalent rigid skins (*6*). The present question seems to be, not whether dolphins have an anomalously low friction drag, but rather how low this drag is. Our report of the sustained swimming speeds of dolphins was intended to provide some of the data needed to answer that question.

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Comets

In Thornton Page's report on the Fourth Berkeley Symposium on Mathematical Statistics and Probability [*Science* **132**, 1870 (1960)] there are several points on which I would like to comment.

First, in the legend of Fig. 4, Page states that the semimajor axis of an orbit is one-half the maximum distance from the sun. Instead, the semimajor axis is one-half the *sum* of the maximum and the minimum distances from the sun. Only in the case of the "sungrazing" comets can the closest approach distance be ignored, because it is small in comparison with the maximum distance. In fact, in most parabolic or near-parabolic orbits it is quite impossible to give a good value for the semimajor axis.

Secondly, Page says (p. 1875), "Because the space about the sun is so nearly empty, comets move with frictionless ease. . . ." If this were true, one would have to neglect the effect of solar radiation pressure on the comet—an effect which is probably responsible for the anomalous acceleration of some periodic comets after all classical perturbations have been taken into account. This effect seems to be difficult to evaluate for the large mass of data required for a statistical study such as Page described in the report.

Although I have not yet had an opportunity to examine the papers discussed in the article, I feel that the conclusions drawn concerning Lyttleton's theory are unwarranted at this time for the following reason. Most of the comets that can be observed at present or that have been observed in the past have been those that come in relatively close to the sun and to the major planets, and they are probably not a fair sample of the comet population. The question cannot be decided with certainty until observations are made of comets that remain invisible from the earth. Those that do not become visible are objects that miss the sun by a great distance. If one finds that these are much more numerous than the ones that come close to the sun, one might have to adopt the Lyttleton theory. If they are less numerous, the Oort theory would fit the data better. Of course, it is possible that *both* views are correct in a restricted sense. Further research is required on the orbital mechanics and physical nature of comets before any definite conclusions can be reached.

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Meisel is of course correct in his comments concerning the definition of semimajor axis (a) and the effect of radiation pressure on the orbits of comets, but I believe that both are of little consequence. Values of a are so large, and the eccentricity is so near unity for "new" comets, that the difference between $2a$ and aphelion distance is less than one part in several thousand. Moreover, in these orbits of high eccentricity, "new" comets spend most of their time so far from the sun that the effect of solar radiation pressure is limited to two small (and nearly opposite) impulses near each perihelion passage. These amount to a very small bias in the random planetary perturbations considered by Kendall and Hammersley.

In connection with these comments it should be emphasized that Kendall based his study on a carefully selected set of 23 "*new*" comets, excluding of course the periodic comets which, on Oort's theory, have suffered large or many perturbations from their original orbits. Kendall's analysis takes account of the residual observational selection (due to fewer approaches of comets of longer period) but does not concern itself with comets of large perihelion distance simply because neither Oort nor Lyttleton predict large angular momentum of newly formed cometary material about the sun.

The statistical studies I reported cannot be said to disprove Lyttleton's theory or to prove Oort's, as yet, but the limited observational data certainly indicate that "new" comets fall toward the sun from considerably greater distances than Lyttleton's theory would predict. Moreover, Lyttleton's own analysis of the directions of major axes of comet orbits failed to show the expected preference for directions associated with the solar motion relative to nearby stars and interstellar clouds.

The greatest weakness of these statistical studies, as Meisel possibly implies, is the selection of "new" comets for comparison with either theory. Such selection is essential, since random planetary perturbations soon smear out any record of the original direction or distance of fall toward the sun. Even if we could observe all comets within 10 or 20 astronomical units of the sun, the key to their origin would lie in recognizing the new ones that preserve some record of the initial conditions.

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Exposures in Lunar Photography

If the errors in *Outer Space Photography for the Amateur*, reviewed by Charles H. Smiley in a recent issue of *Science* [**133**, 271 (1961)], are typified by the example given in the review, they must be "few and . . . unimportant" indeed.

It is well known among astronomers that the full moon is about nine times as bright as the first and last quarters. But since the quarter moon is only half illuminated, the surface brightness of the full moon is only about four and a half times that of the quarters. Thus, the book's suggestion that the exposure for the quarter moon be four times that for the full moon is substantially correct, and the reviewer's "correction," giving the factor of nine, is wrong.

However, to paraphrase the reviewer, if a professional overexposes his first moon photograph, he can make corrections on his second try.

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I shall leave my statement as it is, with the factor nine. Young's arithmetic is satisfactory, as far as it goes, but some judgment is needed in addition. The full moon, flat-lighted, is low in contrast; most astronomers expose and develop to increase the contrast. If one is to develop to a high gamma and yet have a reasonable maximum density, one will choose an exposure on the low side, down one or two stops from that indicated by Young's arithmetical solution.

For the moon at either quarter, the situation is different. Then the interesting lunar area is that near the terminator, where the natural contrast is high. One may reasonably choose to expose for the partly illuminated areas and develop for less than full contrast. One might also take into account the fact that the surface brightness of the moon at first quarter is about 20 percent greater than at third quarter.

Modern black-and-white negative materials provide such a generous latitude that the amateur does not need