tural proteins seem to agree reasonably well with those calculated for random permutations of bases, except for excesses of glycine and alanine (*19*).

These observations are most easily explained if we suppose that the genetic code has evolved in such a way that it provides larger numbers of codons for the more common amino acids, and consequently fewer for the rarer, rather than that the proportions of amino acids are passive consequences of random codon assignments. The degree of order in the code, the extent to which chemically related amino acids are coded by mutationally related codons, lends strength to a belief that the code has evolved in relation to the requirements of protein synthesis (*11, 20*).

Contemporary evidence of the selective importance of amino acid substitutions. If the majority of amino acid substitutions that have occurred in the course of evolution have been neutral or near-neutral in selective value we should expect to find evidence of neutrality (or little evidence of selection) in contemporary protein polymorphisms. However, in all the polymorphisms that have so far been studied in detail, the action of natural selection can be inferred.

The only known hemoglobin polymorphisms in man (hemoglobins A, S, and C) have been shown to have a strong selective element (*21*). Some protein polymorphisms show morph-ratio clines that can be related to climatic factors, others show a constancy of morph frequencies over large areas and indicate strong forces maintaining a balance despite great changes in local environments (*22*). Yet others show significant excesses of heterozygotes (*23*) or seasonal changes large enough to exclude random genetic drift (*24*). None seem to show the patterns expected for neutral substitutions, the fixation or near-fixation of different alleles in different isolated populations without relation to their environmental circumstances.

It might be argued that the selective effects mentioned above are the results, not of the protein polymorphisms themselves, but of other closely linked polymorphic loci in linkage disequilibrium with them. If this is so, then the linkage disequilibriums themselves require explanation. In the absence of selection they would not persist for long evolutionary periods, yet for the only two protein polymorphisms that have been studied from this point of view (α-amylase and larval protein 10 in *Drosophila pseudoobscura*) it has been shown that particular alleles have been associated with particular chromosome inversions for a period longer than the history of the species itself (*25*).

The hypothesis of neutrality can be crucially tested by observing natural and artificial populations, but these observations are not considered by King and Jukes.

Conclusion. Few would dispute that both random genetic drift and natural selection have a part to play in the evolution of proteins, as in the evolution of other aspects of the phenotype. It is nevertheless desirable to estimate their relative importance. King and Jukes argue that random genetic drift has been primarily responsible for the majority of amino acid substitutions, but the weight of evidence does not support them. Protein sequences, like other characters, seem to have evolved under the dominating influence of natural selection.

BRYAN CLARKE

Department of Zoology, University of Edinburgh, Edinburgh, Scotland

References and Notes

1. J. L. King and T. H. Jukes, *Science* **164**, 788 (1969).
2. M. O. Dayhoff, *Atlas of Protein Sequence and Structure 1969* (National Biomedical Research Foundation, Silver Spring, Md., 1969).
3. W. M. Fitch and E. Margoliash, *Biochem. Genet.* **1**, 6S (1967).
4. M. Lamotte, *Bull. Biol. Suppl.* **35**, 1 (1951).
5. A. J. Cain and P. M. Sheppard, *Genetics* **39**, 89 (1954).
6. O. Smithies, *Brookhaven Symp. Biol.* **21**, 243 (1969).
7. G. M. Edelman and J. A. Gally, *ibid.*, p. 328.
8. H. B. D. Kettlewell, *Proc. Roy. Soc. London Ser. B* **145**, 297 (1956).
9. H. G. Zachau, D. Dütting, H. Feldmann, F. Melchers, W. Karau, *Cold Spring Harbor Symp. Quant. Biol.* **31**, 417 (1966).
10. B. L. Strehler, D. D. Hendley, G. P. Hirsch, *Proc. Nat. Acad. Sci. U.S.* **57**, 1751 (1967).
11. T. M. Sonneborn, in *Evolving Genes and Proteins*, V. Bryson and H. J. Vogel, Eds. (Academic Press, New York, 1965), p. 377.
12. I am grateful to Professor P. M. B. Walker for drawing my attention to this possibility.
13. E. Margoliash and E. L. Smith, in *Evolving Genes and Proteins*, V. Bryson and H. J. Vogel, Eds. (Academic Press, New York, 1965), p. 221.
14. E. Margoliash, W. M. Fitch, R. E. Dickerson, *Brookhaven Symp. Biol.* **21**, 259 (1969).
15. E. Mayr, *Animal Species and Evolution* (Harvard Univ. Press, Cambridge, Mass., 1963).
16. B. Wallace, *Topics in Population Genetics* (Norton, New York, 1968).
17. B. Clarke, in *Evolution and Environment*, E. T. Drake, Ed. (Yale Univ. Press, New Haven, Conn., 1968), p. 351.
18. S. Seifter and P. M. Gallop, in *The Proteins*, H. Neurath, Ed. (Academic Press, New York, 1966), vol. 4, p. 153.
19. T. H. Day, personal communication.
20. C. J. Epstein, *Nature* **210**, 25 (1966).
21. A. C. Allison, *Cold Spring Harbor Symp. Quant. Biol.* **20**, 239 (1955).
22. S. Prakash, *Proc. Nat. Acad. Sci. U.S.* **62**, 778 (1969).
23. M. C. Kelsey, thesis, University of Edinburgh (1969).
24. R. Semeonoff and F. W. Robertson, *Biochem. Genet.* **1**, 205 (1967).
25. S. Prakash and R. C. Lewontin, *Proc. Nat. Acad. Sci. U.S.* **59**, 398 (1968).
26. I am greatly indebted to the following for fruitful discussions, and for criticizing the manuscript; Mr. T. H. Day, Dr. A. H. Maddy, Prof. J. Maynard Smith, Dr. D. T. Parkin, Prof. A. Robertson, Dr. E. M. Southern, and Prof. P. M. B. Walker. The work was supported by the Science Research Council.

15 December 1969

Lightning and the New-Generation Aircraft

In the article by Finger and McInturff (*1*) several meteorological problems associated with flying supersonic aircraft are discussed. Here I would like to bring to the attention of those authors, and of the public as well, a meteorological problem that was not discussed in their article and that should be closely examined by persons involved in operating the SST's and also the "jumbo" jets. This problem is lightning strikes to aircraft.

It has been estimated that there are approximately 500 lightning strikes per year to commercial jet airplanes operating in the United States alone. Most, if not all, of the lightning strikes are triggered by the aircraft, as was very probably true of the lightning flashes that occurred during the launch of Apollo 12. Because of the larger size of the new-generation aircraft (SST's and jumbos), this lightning hazard will increase. If the new aircraft are permitted to fly under the same meteorological conditions that are considered allowable for present aircraft, the probability of the aircraft's being hit by lightning will be considerably increased.

Apollo 12 was launched through a cloud system that was electrically active, as was indicated by potential gradient meters on the ground, although no lightning activity had been observed in the vicinity. The Apollo 12 lightning incident provides a documented example of a large group of electrically active clouds that may not produce natural lightning and may not be considered thunderclouds by the meteorologist but, nonetheless, have the potential for producing triggered lightning and should be avoided by aircraft.

In most cases (there have been

tragic exceptions), these lightning discharges do very little damage to conventional aircraft. It is doubtful that the new aircraft will be as "safe" with respect to this hazard, because of their extensive use of more sophisticated hardware, which is more susceptible to damage from lightning.

The consequences of lightning strikes to aircraft, the increased danger presented by the new, larger aircraft, and the meteorological situations in which lightning might be triggered are all matters which should receive thorough investigation.

A. A. FEW
Department of Space Science, Rice University, Houston, Texas 77001

Reference

1. F. G. Finger and R. M. McInturff, *Science* **167**, 16 (1970).

19 January 1970

One-Way Radar Range to the Moon

It is perhaps rash to comment on a communication (*1*) signed by no less than 14 authors with names ranging from Alley to Wampler. However, I do so with regard to the conversion of radar travel times to distances in meters. The point in question is found in the paragraph beginning "The basic uncertainty in measuring the approximately 2.5 second round-trip travel time. . . ." The paragraph ends with the remark that an error of 0.5 nsec in travel time would lead to the conclusion that "an overall uncertainty of ± 15 cm in one-way range seems achievable." In spite of a second remark made by Alley *et al.* regarding the use of the light-second as unit of distance, the paragraph in question can be read to mean that the distance to the moon could be known to an accuracy of ± 15 cm because of the fact that the radar travel times were measurable to an accuracy better than one part in 10^9. For the benefit of those who are not experts in radar, it is perhaps useful to point out why such an interpretation is untenable. The demonstration is this. Suppose, for the sake of argument, that the one-way travel time is assumed to have the value $(1.25 \pm 0.5 \times 10^{-9})$ second, and that there are no errors, other than the one shown, in this time interval. The speed of light adopted by Cohen and DuMond (*2*) is

$$c = (299792.5 \pm 0.4) \text{ km sec}^{-1}$$

and they also quote one value of higher accuracy due to Froome (*3*), namely,

$$c = (299792.5 \pm 0.1) \text{ km sec}^{-1}$$

If the Cohen-DuMond value is employed, the one-way range to the moon is

$$(299792.5 \pm 0.4) \times 10^3 \times (1.25 \pm 0.5 \times 10^{-9}) \text{ m}$$

The error in this range is therefore

$$\pm (400 \times 1.25) \pm (0.5 \times 299792 \times 10^{-6}) \text{ m} = \pm 500 \text{ m} \pm 15 \text{ cm}$$

whereas, if the Froome value is used, it becomes

$$\pm 125 \text{ m} \pm 15 \text{ cm}$$

In both cases, the error in the range depends on that inherent in the value of c, the ± 15 cm due to the error in the travel time being entirely negligible in comparison. In my example, the one-way range would be known to ± 15 cm only if a value of c were available correct to 1 part in 10^{10}, which is at least three orders of magnitude better than has so far been achieved.

G. C. MCVITTIE
University of Illinois Observatory, Urbana 61801

References

1. C. O. Alley *et al.*, *Science* **167**, 458 (1970).
2. E. R. Cohen and J. W. M. DuMond, *Rev. Mod. Phys.* **37**, 537 (1965).
3. K. D. Froome, *Proc. Roy. Soc. London Ser. A* **247**, 109 (1958).

5 February 1970

A value of 299,792.5 km/sec for the speed of light has been adopted for use in astronomical and geophysical work by the International Astronomical Union, International Union of Geodesy and Geophysics, and the International Scientific Radio Union. Essentially all current measurements of astronomical distance within the solar system are based on this value. Most geodetic measurements of the highest accuracy over long base lines are also made in terms of the speed of light. Thus we have, in effect, two distance scales at present. One is based on an adopted value for the wavelength of the orange line of krypton and is used mainly in laboratory measurements. The other is based on the adopted value for the speed of light and is widely used in astronomical and geophysical measurements. While this situation is not ideal, it is also not unusual in metrology. There are no important scientific experiments which we are prevented from doing because of our not yet knowing the conversion factor between the two scales with sufficient accuracy.

McVittie is correct in saying that an accuracy of 0.5 nsec in the one-way travel time of light does not permit one to deduce the range to high accuracy in terms of the meter as defined by the General Conference on Weights and Measures. However, as stated in our article: "The present uncertainty of three parts of 10^7 in the knowledge of the velocity of light will not affect the scientific aims of the experiment, since it is the practice to measure astronomical distances in light travel time." Which distance scale we use for finding the scale factor for the lunar orbit is not important, since we are mainly interested in whether the form of the motion can be reproduced by the theory. The only other distances that we expect to measure with accuracies greater than that of the present value of the speed of light are the coordinates of the ground stations with respect to each other and to the axis of rotation and center of mass of the earth. Here it is the changes in the coordinates which are of major interest, and in any case more accurate measurements of the speed of light in the near future are likely to make the question academic.

P. L. BENDER
Joint Institute for Laboratory Astrophysics, National Bureau of Standards, and *University of Colorado, Boulder 80302*

1 April 1970