after, at increments of about 100 msec, and with negative deflections evenly spaced at intermediate points. Relative to this curve, the 12 higher-frequency trials yielded an average EP whose deflections are not only opposite in sign but are also attenuated. The average for all 24 trials yielded an attenuated copy of the average EP obtained from the 12 lower-frequency trials, as might be expected. The first three positive and negative peaks of the 12 lower-frequency trials differ significantly at the .01 level from the six time-associated points of the 12 higher-frequency trials.

These results indicate that the process of averaging over all trials may be misleading if it is assumed that the trials are drawn from a homogeneous population. Walter has noted regular serial changes in the EP in a given run, (*12*), Brazier has demonstrated systematic changes between successive averages of 60-trial blocks (*13*), and my study shows reliable nonserial changes within a single 24-trial block. These results suggest that, where averaging is employed, variance should also be considered.

In Fig. 1 each point in the curve of the 12 higher-frequency trials is the average of 12 values. In general, the variance of these 12 values is greater than the variance of the same time-associated values comprising the lower-frequency curve, a result which is consistent with the greater dispersion of points in the higher-frequency curve of Fig. 1. A similar result is obtained when the 24 trials are divided into three groups of 8 trials each, or four groups of 6 trials each, again according to subsequent alpha frequency. In each case, the variance increases with an increase in the alpha frequency of the trials.

In addition to Fig. 1, further evidence of the relation between EP and subsequent alpha frequency is revealed by the fact that, in general, the variance at each time point tends to decrease with a decrease in the alpha frequency range of each group. Of course, associated with this decrease in range is an increase in the number of groups and a decrease in the number of trials in each group. Despite this decrease in the number of trials, the standard error of the mean at each time point often shows a decrease, especially for the lowest-frequency group.

Inasmuch as the subject used in this study was not selected on the basis of his evoked potential and alpha frequency characteristics, characteristics which analysis revealed as normal, it is assumed that the relation between evoked potential and alpha frequency occurs in the general population. Nevertheless, the validity of this assumption has yet to be confirmed.

Finally, I have assumed that the relation between the evoked potential and subsequent alpha frequency is based on trial-to-trial variation in attention. However, since attention was not measured independently in this study, alternative explanations, such as that based on the phase of alpha at the time of stimulus presentation, cannot be discounted.

EDWARD LEVONIAN
Institute of Transportation and Traffic Engineering, University of California, Los Angeles 90024

References and Notes

1. W. A. Cobb and G. D. Dawson, *J. Physiol. London* **152**, 108 (1960); K. A. Kooi and B. K. Bagchi, *Ann. N.Y. Acad. Sci.* **112**, 254 (1964).
2. C. Shagass and M. Schwartz, *J. Neuropsychiat.* **2**, 262 (1961).
3. R. E. Dustman and E. C. Beck, *Science* **142**, 1480 (1963).
4. E. G. Walsh, *J. Physiol. London* **118**, 500 (1952).
5. M. H. Goldstein, Jr., *Electroencephalog. Clin. Neurophysiol.* Suppl. 20, 59 (1961).
6. G. Horn, *Brain* **83**, 57 (1960); G. P. Frommer and R. B. Livingston, *Science* **139**, 502 (1963); E. Garcia-Austt, J. Bogacz, A. Vanzulli, *Electorencephalog. Clin. Neurophysiol.* **17**, 136 (1964); R. M. Chapman and H. R. Bragdon, *Nature* **203**, 1155 (1964).
7. M. Haider, P. Spong, D. B. Lindsley, *Science* **145**, 180 (1964); H. Davis, *ibid.*, p. 182; P. Spong, M. Haider, D. B. Lindsley, *ibid.* **148**, 395 (1965).
8. H. H. Jasper, R. M. Cruikshank, H. Howard, *Psychol. Bull.* **32**, 565 (1935).
9. R. M. Cruikshank, *J. Exp. Psychol.* **21**, 625 (1937); S. H. Bartley, *ibid.* **27**, 624 (1940); G. H. Bishop and M. H. Clare, *Electroencephalog. Clin. Neurophysiol.* **4**, 321 (1952).
10. I thank Michael Kaufman, who not only cooperated as the subject but also helped with the instrumentation.
11. E. Levonian, "Measurement and Analysis of Physiological Response to Film," *UCLA, Dept. Engrg. Rept. No. 62–66* (1962). This transfer was facilitated by the cooperation of the UCLA Computing Facility.
12. W. G. Walter, *Ann. N.Y. Acad. Sci.* **112**, 320 (1964).
13. M. A. B. Brazier, *ibid.*, p. 33.

28 March 1966

Statistical Analysis in Toxicity Experiments

H. W. Norton [*Science* **152**, 671 (1966)] makes three criticisms of Lindsay and Kullman's "Pentobarbital sodium: variation in toxicity" [*ibid.* **151**, 576 (1966)].

To the first paragraph of his letter I would reply as follows. Survival rate varied as much as ninefold between consecutive time periods on a given day, or, at the other extreme, did not vary at all between as many as four consecutive periods. Nevertheless, the question of whether dependence of toxicity on time varies from experiment to experiment is not of primary importance. The major purpose of the paper is to indicate that a simple quadratic may not be sufficient to explain the dependence of toxicity on time.

The comment that the comprehensive test of interaction reaches only the probability of 0.092 may be irrelevant. I believe it is possible for interaction not to be significant in the comprehensive test and yet be real in individual tests.

Although the arcsin transformation does, as Norton notes, alter the form of the toxicity curve slightly as sensitivity changes, I do not believe that this contributes significantly to interaction. The effect of this transformation would be to flatten the curves lying near the extremes of the ordinate more than others. However, near the lower extreme (the only extreme approached) the curves are already rather flat; in the experiment in which survival was lowest, for example, one animal survived in each of four successive periods. The straight-line portion of this curve is not distorted by the transformation, yet it contributes strongly to interaction. It should be noted that flattening would tend to hide fluctuations in the curve and thus lessen the chance of their detection in tests of significance. We think, however, that a variance-stabilizing transformation is needed and that the arcsin is the best available for the data.

Concerning the parenthetical remark in Norton's second paragraph, I would say that having begun this line of reasoning Norton should have extended it to include calculation of the probability of both the quadratic and septic effect occurring by chance in the same sample; the probability would be <0.04 at the 5-percent level, and should the actual levels be used (0.005 and 0.032, respectively), much less. Furthermore, in testing whether the curve is more complex than a quadratic (as indicated by the septic effect significant at the 0.032 level), only tests of the higher-order effects need be considered. A comprehensive test of cubic through

septic effects yields a level of significance of 0.08.

Concerning the "serious deficiency in the design of these experiments," the point should be made that the animals were randomized into cages. If their survival rate were dependent upon the cage it would have to be due to different treatments ("environment") of the cages. I have no reason to suspect that the treatment of cages was different. In any case, I have redesigned the experiment as follows: experimental animal, female mice of the same strain but obtained from Charles River Laboratories rather than Hilltop Caviary as were the previous animals; animal quarters, a sound-damped room in which the animals were kept in isolation without any daylight; light schedule, the same periods of light and darkness from a single light source under which the cages were grouped in a circle to receive equal illumination; elevation of cages, all cages on the floor to avoid any possible temperature gradient; order of cage selection, random. The results to date support the previous conclusions.

Since publication of this work another paper has been brought to my attention by one of its authors which might assist the reader in reaching a point of view concerning this work. I refer to R. E. McCafferty and H. T. Mack, *Quart. J. Exptl. Physiol.* **49**, 394 (1964). The data in Fig. 2 seem to support the concept of short-term fluctuations in metabolic activity in this same species.

HUGH A. LINDSAY
West Virginia University, Medical Center, Morgantown

15 March 1966

Innovation in an Eskimo Culture

In "The Anaktuvuk mask and cultural innovation" [*Science* **151**, 1337 (1966)], Atamian makes an interesting contribution to the theory of innovation in isolated cultures. His conclusion, however, appears incomplete if not actually misleading. It proposes the following hypothesis for further testing: "In nonacquisitive or noncompetitive societies, the innovations most readily adopted are those which are not symbolically related to the innovator." The circumstances he narrates in support of this proposition are as follows. The making of ceremonial masks is not part of the immediate cultural heritage of this isolated group of Eskimos (the Nunamiut), but two young hunters received much community acclaim for innovating two such masks and wearing them during a holiday festivity. On their behalf, the two masks were later sold to an interested white visitor by the chief *umialik*, the highly respected village elder. The small community thus became aware that masks could be made and sold for money, but no further masks were made or sold for 4½ years. Finally, the chief *umialik* himself made and sold some masks, and at once almost the entire community took up this activity and developed it into a profitable and substantial source of income. Why the lag in adopting this innovation? Atamian's explanation is that in Eskimo society the ordinary individual's activities are considered identical with his person, that competitive acquisition is contra-ethical in this society, and that others could not make or sell masks without violating a prohibition against taking a property (mask-making) that was part of the identities of the two young hunters. However, when it occurred to the chief *umialik* to make and sell a mask, since he symbolized the entire community rather than a single private person, the innovation could then be generally adopted as a collective, community effort.

Assuming Atamian's observations and inferences are correct, I suggest the following reformulation of the hypothesis: In nonacquisitive or noncompetitive societies, the innovations most readily adopted are those that are sanctioned or engaged in by community leaders who symbolize the entire community rather than the persons of private individuals. In this form, the hypothesis gives greater weight to the role of the chief in bringing about innovation. The issue does not appear to be met by hypothesizing that adopted innovations are those which are "not symbolically related to the innovator." Rather it appears to be related to who does the innovating. Apparently the opinion-maker prevails in the icy tundras of Alaska as well as in the concrete jungle of Madison Avenue.

WALTER S. NEFF
Department of Psychology, New York University, New York 10003

23 March 1966

Actinomycin D and Growth Response of Chick Comb to Androgens

In the report titled "DNA-dependent synthesis of RNA is not implicated in growth response of chick comb to androgens" [*Science* **150**, 1315 (1965)], Talwar, Modi, and Rao give no indication of whether actinomycin D presented by either inunction or subcutaneous injection of the comb is an effective inhibitor of either RNA synthesis or growth in general in this organ. In all the experiments they report, actinomycin D was tested only in the presence of administered (Tables 1 and 3) or endogenous (Table 2) androgens. Without information about the effect of actinomycin D alone when presented by the same methods, no valid conclusion can be drawn about the autonomy of the androgen effect.

WELLS E. FARNSWORTH
Veterans Administration Hospital, Buffalo 15, New York

3 March 1966

Quasar Red Shifts

With reference to Hoffman's recent letter on quasar red shifts [*Science* **152**, 671 (1966)], the model I had in mind was one in which chunks of material fell into a mass center from time to time, lighting up and giving emission lines as they fell.

The Burbidges [*Astrophys. J.* **143**, 271 (1966)] report three components in the MG II (λ2798) emission line in the quasi-stellar object 3C345. The wavelengths of these components exhibited large shifts over a period of months.

An example of an extreme case of light variability is the quasi-stellar source 3C2, which increased its blue magnitude around 1.5 magnitudes between 1954 and 1963 [Sandage, Véron, and Wyndham, *Astrophys. J.* **142**, 1309 (1965)].

To put some of the unlikely explanations of quasar red shifts into the category "disproved" would be valuable, as Hoffman says. But perhaps, at this stage, we are left without even a clean-cut "unknown cause."

HALTON ARP
Mount Wilson and Palomar Observatories, Pasadena, California

21 April 1966