sults: the absence of a rise in blood pressure following carotid occlusion or sectioning of the buffer nerves in the decerebrate cat. Since we obtained equally active vasomotor reflexes in the intact and in the mid-collicular decerebrate cat, the conclusion is inescapable that the basic control mechanism for baroreceptor reflexes must reside in the brainstem and that the influence exerted by suprapontine structures is not essential.

R. L. KATZ, C. Y. CHAI, N. KAHN
S. H. NGAI, N. N. SHARE, S. C. WANG
Departments of Pharmacology and
Anesthesiology, College of Physicians and Surgeons, Columbia University, New York

References

1. D. J. Reis and M. Cuénod, *Science* **145**, 64 (1964).
2. S. C. Wang and C. Y. Chai, *Am. J. Physiol.* **202**, 31 (1962); C. Y. Chai, N. N. Share, S. C. Wang, *ibid.* **205**, 749 (1963).

5 August 1964

Our conclusions were based on four observations: the two well-established facts that (i) decerebration by itself does not result in a fall of blood pressure, and (ii) section of the buffer nerves results in a sustained elevation of blood pressure; and (iii) our positive result that decerebration in animals with three or four severed buffer nerves results in an immediate and sustained fall of blood pressure (which Katz *et al.* appear also to have observed in their vagotomized animals, as their records in the cited references indicate), and (iv) our "negative result" that section of the buffer nerves in decerebrated animals fails to result in a sustained rise of blood pressure, although a transient rise immediately following nerve section has been observed. It is not clear from the correspondents' comments whether the blood pressure rise which they observed after buffer nerve section persists after the minimal 30 minutes interval which we used as our criteria. Without this essential information, a true difference between our results and theirs cannot be established.

The "negative result" used in support of our conclusions, and published elsewhere, was that the pressor response to occlusion of one carotid artery proximal to the only innervated carotid sinus was inhibited. Since the pressor responses which Wang and his associates clearly found unchanged before and after mid-collicular decerebration were elicited by bilateral carotid occlusion, the experiments are not comparable.

Finally, we do not claim that the mechanism of baroreceptor reflexes does not reside in the lower brainstem, that is, in the pons and medulla. Hence, we are not in disagreement with Wang and his colleagues on this point. It is our contention, supported by our facts, that the excitability of these reflexes may be modified by suprapontine structures and that this reflex excitability may be changed without changing the resting mean blood pressure. It is through a modulation of this reflex mechanism that we propose that rostral brain structures exert some tonic control of blood pressure. We have not addressed ourselves to the essentiality of this control. We have merely pointed out its presence.

DONALD J. REIS
MICHAEL CUÉNOD
Department of Neurology, New York Hospital–Cornell Medical Center, New York

19 August 1964

"Cytoplasmic" Sterility

The report on "cytoplasmic" sterility by Ehrman (10 July, p. 159) has some fascinating implications for the field of gene-cytoplasm interactions in general. One possible explanation of his results would be that the Mesitas and Santa Marta cytoplasms have some common structures which interact with genes affecting male fertility; further, that these structures occur in different proportions in the two cytoplasms, the particular ratio in either one being a response to natural selection for effective interaction with the genome. Cytoplasmic structures do not seem to replicate by the same system as the nuclear genes, and it is not necessary to assume that only two kinds of cytoplasmic "alleles" can be present for any one genetic locus, that equal distribution must occur at mitosis or meiosis, or that all of them necessarily multiply at the same rate under all conditions.

VESTA G. MEYER
Delta Branch Experiment Station, Stoneville, Mississippi

3 August 1964

Meyer's explanation is very close to a working hypothesis which I am planning to test. The Mesitas strain consistently carries a heavy infection of microsporidia, while the Santa Marta strain is free of them. It is, of course, possible that other symbionts or parasites of various kinds (protozoans, bacteria, viruses) may also be discovered in these flies. Suppose, then, that each of the six morphologically indistinguishable races or incipient species of the *Drosophila paulistorum* complex carries a symbiont to which it is adapted, and that this hereditary "infection" is transmitted via the egg cytoplasm. The nonhybrid genome keeps the infection under control so that it does not interfere with male fertility. The genotype of the hybrid disrupts this control, and the male hybrids are sterile. The symbionts are controlled by the genotype of the race in which they occur, but they may get out of control in individuals of hybrid genotypes. This may, then, be a causative factor which brings about the reproductive isolation between these incipient species.

LEE EHRMAN
Rockefeller Institute, New York City

2 September 1964

Wild and Domestic Animals as Subjects in Behavior Experiments

In a recent report, Kavanau (*1*) sets forth several generalizations which he says "have important bearings on the rationale and design of experiments on learning and reinforcement." Two of these generalizations seem especially likely to mislead those readers who are not actively engaged in behavioral research. They imply that a new era has arrived in which wild animals must wholly replace domestic animals as subjects in learning experiments.

I would agree that there certainly are differences between wild and domestic animals—differences in rearing and living conditions, in structure, in physiology, and in underlying genetic factors—and that, as a consequence, there are behavioral differences as well (*2*). Granting these does not concede Kavanau's position.

Consider first his statement concerning evolutionary processes:

> Using such atypical species representatives as domestic rats and mice for laboratory studies of behavior narrows the animal response spectrum to a point where its significance for adaptation, survival, and evolution becomes highly questionable. . . . only wild animals provide the full range and vigor of responses upon which solutions to the central problems of behavior must be based.

"Must" is much too strong a word for use in this context, as the following examples indicate. Narrowing of the response spectrum under intensive selection has been used as a tool, in behavior genetic studies, to evaluate the role of the evolutionary process in behavior (*3*). In such work, two groups are often selectively bred, one high and the other low in some trait such as maze-learning ability, emotionality, susceptibility to audiogenic seizure, and so on. The foundation stock for such work may be either wild or domestic. After many generations, the progeny may be thought of as hyperdomesticated, that is, highly inbred and more highly adapted or ill-adapted to the experimental environment used in the selection process. Very often, however, the two selected groups, when taken together, cover virtually the same response range as the foundation stock, and the full response spectrum is maintained. Existing stocks of highly inbred mice may also be useful in considering problems of behavioral evolution. For example, using such stocks, Thiessen has investigated the interaction of genotype and population density as they affect activity level. In another report he considers the developmental effects of a single gene substitution on several behavioral measures (*4*). Hall has noted the potential importance of single gene effects for behavioral evolution, and work bearing on this question with stocks selectively maintained on nonbehavioral criteria is beginning to appear (*5*). Such work requires the use of highly inbred animals. The significance of behavioral experimentation with domestic rats and mice, in an evolutionary context, is thus not in doubt a priori.

The second questionable generalization concerns learning phenomena. Kavanau notes that mice of the genus *Peromyscus* learn mazes of "unprecedented complexity" and goes on to say:

> There is no reason to believe that these remarkable feats even approach the limits of the learning capacity of the wild animal, although they far exceed the performances of domestic rodents.

He cites no evidence for the statement concerning the domestic animals, and meaningful evidence of this sort might be difficult to obtain. Species differences have been demonstrated in laboratory-bred *Peromyscus* on habitat-selection behavior (*2*), and strain differences are found in *Mus musculus* on nearly any behavior test (*6*). Further, there is evidence (*5*) for a different mode of inheritance of susceptibility to audiogenic seizure in these two kinds of mice. This being the case, even a direct demonstration of performance differences between domestic and wild animals of different genera would seem trivial, since behavioral differences of a higher-order genetic basis are known. Note also:

> For the analysis of the basic nature of hereditary differences in learning ability, complex tasks such as maze-running are unsuitable, since success depends upon so many factors. Perhaps we can fairly state that there is, as yet, no good evidence in animals for a general factor of intelligence which operates in all learning situations. In so far as some animals are superior to others in trainability on a number of tasks, an explanation can ordinarily be given in terms of adaptation to the testing situation and the animal handler (*7*).

In the last sentence, Fuller is making the same point that Kavanau makes in the early part of his report.

This widely accepted generalization finds support in experiments on the effects of early environment and early experience (*8*). I have cited evidence above that domestic animals are required in some experimental designs because the genetic basis of their behavior may then be studied. This point alone refutes Kavanau's position. But further, the very fact of being laboratory-bred may make the domestic animal a superior subject for some experiments. We assume that wild animals, through evolution and experience, are maximally adapted to the environment in which they are found (*2*). As Kavanau points out, domestic animals are often "hundreds of generations removed from the wild." The evolutionary process is not suspended in the breeding colony; so by the same assumptions we expect that line-bred animals, through evolution and experience, are maximally adapted to the environment in which they are found—that is, the laboratory. In highly inbred rats, living in the same physical environment, strain differences have been shown to be of direct importance in adaptation to experimental equipment (*7*). It has also been found that through changes in environmental and rearing conditions strain differences can be masked (*8*).

With these points in mind, it is understandable that the learning theorist prefers, for many experimental situations, uniform though not highly inbred subjects which are adapted to the laboratory environment. With such animals he has maximum viability, minimum problems in acquainting subjects with apparatus, and stability of group performance unlikely to be found in wild animals of more variable experiential background. This is not to say that wild animals may never be used in the laboratory. Rash generalizations should not be made from their performance, however. Today as in the past, one must choose animals to fit the experimental problem at hand.

JAMES D. HAWKINS
Psychology Department, Claremont Graduate School, Claremont, California

References

1. J. L. Kavanau, *Science* **143**, 490 (1964).
2. E. B. Hale, in *The Behavior of Domestic Animals*, E. S. E. Hafez, Ed. (Williams and Wilkins, Baltimore, 1962), pp. 48–50.
3. J. L. Fuller and W. R. Thompson, *Behavior Genetics* (Wiley, New York, 1960), pp. 394–395.
4. D. D. Thiessen, *J. Comp. Physiol. Psychol.* **57**, 412 (1964); ———, in preparation.
5. C. S. Hall, in *Handbook of Experimental Psychology*, S. S. Stevens, Ed. (Wiley, New York, 1961), p. 321; V. H. Denenberg, S. Ross, M. Blumenfield, *J. Comp. Physiol. Psychol.* **56**, 290 (1963); J. D. Hawkins, in preparation.
6. J. L. Fuller, in *Annual Review of Psychology*, P. R. Farnsworth and Q. McNemar, Eds. (Annual Reviews, Palo Alto, 1960), p. 53.
7. ———, in *The Behavior of Domestic Animals*, E. S. E. Hafez, Ed. (Williams and Wilkins, Baltimore, 1962), pp. 67–68.
8. V. H. Denenberg, *ibid.*, pp. 109–134.
9. This paper was prepared during my tenure under research fellowship MH 16,374 from the National Institute of Mental Health.

27 May 1964

The studies of behavior genetics and of the effects of early environment and early experience that Hawkins emphasizes fail to come to grips with the central issue of the relevance of the behavior of domestic animals to that of wild ones. Moreover, those justifications for studying the behavior of domestic animals which are based upon convenience—genetic uniformity and stability of group performance, minimum problems for the experimenter, maximum viability, and so forth—no longer carry the weight they did in the past when facilities, objectives, and knowledge were more limited and less emphasis was given to the comparative approach. Certainly the behavior of domestic rodents merits detailed study. But the investigator should not lose sight of the fact that these rodents are specialized offshoots produced by laboratory rearing and artificial selection, and that their activities give but weak and often distorted reflections of those of wild animals. Choosing the animal to fit the experiment has, indeed, proved its value in many fields of study, but this approach contains pitfalls for the behaviorist.

My statement that the remarkable feats of mice of the genus *Peromyscus* far exceed the performances of domestic rodents referred to the known performances of domestic animals. I do not doubt that domestic rodents could be coaxed into performing some of the feats that wild animals learn with comparative facility. In this connection, however, the importance of offering extrinsic rewards to deprived animals in order to elicit responses has been overemphasized in theories of motivation because of the lethargic, comparatively sedentary behavior of the animals that customarily are employed. Thus punishment and severe deprivation often must be used to elicit activities of domestic rodents which the wild rodent engages in spontaneously.

I do not, of course, subscribe to the view that wild animals are the only ones that can be used meaningfully in learning experiments. However, if general principles of behavior are defined as those that apply to the representatives of many taxonomic groups, in order to establish such principles it is imperative to concentrate our studies upon a wide representation of wild animals rather than upon the artificially produced variants of a few species. Unfortunately, in the past the emphasis has been reversed.

A new era in the rationale and design of psychologically oriented animal experimentation may indeed be in its inception. I suggest this because I received numerous highly favorable communications from psychologists concerning my report but no adverse ones. The comments imply a radical change in the climate of thought regarding the design, value, and limitations of behavioral studies of domestic animals.

Many investigators appear to have the impression that all wild mice are fierce and unmanageable. Therefore it should be noted that even freshly captured mice may be very "tame" and tractable. After a few weeks in the laboratory, captive and captive-born animals often are as gentle and easy to handle and work with as domestic mice and rats. In addition many species of *Peromyscus* breed readily in confinement and most are as easy to care for as domestic rodents. Accordingly, the psychologist can use his familiar techniques on these forms as readily as upon domestic rodents and make meaningful comparisons between them.

J. LEE KAVANAU
Department of Zoology, University of California, Los Angeles

9 July 1964

ASSOCIATION AFFAIRS

Election of AAAS Officers

The following nominees for AAAS offices were selected by the AAAS Committee on Nominations and Elections at a meeting that was held on 9 June. All have agreed to serve if elected.

President-elect (one to be elected)
John W. Gardner
Alfred S. Romer

Members of the Board of Directors (two to be elected)
James D. Ebert
Mina S. Rees
William C. Steere
John A. Wheeler

Members of the Committee on Council Affairs (three to be elected)
Stanley S. Ballard
Barry Commoner
Hugh H. Hussey
Trevor Lloyd
Charles F. Savage
Oscar Touster

On petition signed by no fewer than 30 members of the Council and submitted to the Executive Officer no later than 1 November, the names of additional nominees may be included on the election ballot that will be mailed to Council members shortly after 1 November. Results of the election will be announced at the Council meeting on 27 December in Montreal.

Brief biographies of the nominees follow.

For President-Elect

John W. Gardner was appointed to the Board of Directors in 1963 to complete the remaining two and a half years of a term vacated by the resignation of William W. Rubey. Alfred S. Romer was a member of the Board from 1960 through 1963.

John W. Gardner

John W. Gardner, 51 (psychology), instructor, Connecticut College, 1938–40; assistant professor, Mount Holyoke College, 1940–42; head, Latin-American Section, Foreign Broadcast Intelligence Service, Federal Communications Commission, 1942–43; staff member to president, Carnegie Corporation of New York, 1946–; president, Carnegie Foundation for the Advancement of Teaching, 1955–; American Psychological Association: chairman, Committee