Letters

Animal-Care Legislation

In recent years measures have been introduced regularly before Congress to regulate animal research. In the past, bills have, fortunately, failed to make progress in the legislatures. A few new twists have been added in the current session and merit comment. Senator Joseph Clark, one of the most determined of the supporters of such legislation, this year has been joined in his sponsorship of S. 1071 by Senators Bartlett, Muskie, Young (Ohio), and Byrd (Virginia). Thus those Senate liberals who usually sponsor these animal-care bills are now supported by a mighty vote of conservatism.

In his introduction of the measure, Senator Clark sounded a new note. Referring to the frustration of his proposed legislation in earlier sessions of the Senate, Clark said:

> And yet opposition to this bill, conducted under the pretense of protecting legitimate research from redtape and government bureaucracy, has delayed action on the legislation. This opposition comes in large measure from the organizations which prey upon helpless animals for profit made by selling the animals to laboratories for research experiments.

There we have it. Biological and medical research workers are not really the group opposed to bills like that introduced by Senator Clark. It is really a cabal instituted by companies who reap profit from the sale of experimental animals. I think it incumbent upon investigators who experiment on animals to prove the falsity of this view by registering their opposition to his measure through letters sent to Senator Lister Hill, chairman of the Senate Labor and Public Welfare Committee. In the House of Representatives, Congressman James Cleveland has proposed a bill (H.R. 5647) which is the companion piece to S. 1071.

Senator Maurine Neuberger, who formerly supported the Clark bill, has changed her thinking on the subject and now sponsors a bill, S. 1087, which represents a distinct improvement over her earlier views. There is, however, a portion of this measure which must find great opposition among biomedical scientists. Section 7 of S. 1087 proposes the establishment of a reference library, within the National Institutes of Health, which will accumulate a record of all research involving the use of animals and supported by government funds. Section 7 further directs that each agency and department must check the files of the reference library before supporting animal research in order to determine whether similar or identical research has been previously conducted. In her introduction of S. 1087 Senator Neuberger noted that "this library would have no censorship functions. It would merely provide the granting or contracting authority with the background information as an aid to determining the efficacy of a proposal."

The fact is that NIH already has in the National Library of Medicine this very information. Furthermore, the ultimate decision on whether proposed research will be supported is made by study sections composed of authorities in particular fields of investigation, experts who more than anyone else are familiar with the pertinent literature. A measure of this sort can readily delay grants for months before someone has collected and digested masses of information, mostly irrelevant but provided by the computer of the reference library. (And who will this someone be?) It should also be obvious that this criterion of "similar or identical research" if applied at all would doom nearly all research to nonsupport by the government. The examples of one investigator's success where others have failed or of one worker's new insight and technical advancement in similar or identical research are too numerous to cite.

Finally, there is a measure before Congress which warrants support. Representative Edward Roybal has introduced a bill (H.R. 5191) whose purpose and wording should be acceptable to a great majority of biomedical investigators. The bill clearly states the need for continuing use of experimental animals, not only for applied purposes but also for the acquisition of fundamental knowledge and for teaching purposes. The bill authorizes governmental research into better animal care, training and education in the best technics, and dissemination of information on the subject. The Surgeon General is authorized to set standards for the operation of animal facilities and receive assurances as he deems necessary that these standards are being met. The basic difference between the Roybal bill and the other measures is that Roybal has not started out with the assumption that a serious crime—cruelty to animals—is escaping notice. His measure is not punitive but constructive, providing for an improvement in animal care. The other measures provide for the regulation of biomedical scientists and can only hamstring a creative and successful community.

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Information Race Again

Because of the lag in most scientific journals between acceptance of a paper and its publication, experimenters often find that their work duplicates studies that have been completed and are in press. Knowledge of these studies as they are accepted rather than when they are published would be of inestimable value. The American Psychological Association's Project on Scientific Information Exchange in Psychology [see W. D. Garvey and B. C. Griffith, *Science* **146**, 1655 (1964)] is testing methods for improving the immediate flow of completed research. One improvement is the publication of the titles and authors of manuscripts as they are accepted. This is of great help to the researcher in maintaining contact with ongoing work in his specialty.

We suggest that a central reference pool could be created to provide a similar service for all scientific journals. Perhaps the National Science Foundation or a similar body (AAAS?) could support the establishment of this service. Scientists could pay an annual fee for each area of interest about which they would like to be kept up to date. A mailing list for each such topic could be maintained, and subscribers could be provided with monthly reports of articles accepted for publication and the names and addresses of the authors. This would prevent

overlapping experimentation and allow researchers to diversify more rapidly within a given area. Further, an investigator would not have to peruse each individual journal as it appears. On a subscription basis the service might become relatively self-supporting.

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Endocrines, Behavior, and Population

J. J. Christian and D. E. Davis devote the latter half of their article on "Endocrines, behavior, and population" (*1*) to discussing data from other investigators that are contrary to their theory that "the behavioral-endocrine feedback" is of primary importance in the regulation of mammalian populations. Concerning our work (*2*), for example, they make the following statement:

> In some situations no correlation has been shown between adrenocortical function and changes in population, but so far the cases fall into two categories. The first is that where the sample is too small to demonstrate any correlation. For instance, Negus studied only 98 animals over a 2-year period, of all ages and both sexes.

While we would not debate that a larger sample would have been desirable, our concern here is the considerable confidence these authors express in much weaker data of their own in support of their hypothesis. For example, in the paragraph preceding the one quoted above, the authors apparently place complete confidence in a paper published on sika deer by Christian, Flyger, and Davis (*3*), in which a total of 17 adrenal weights, collected over a 6-year period, is reported. These weights were recorded from animals of various ages and both sexes, and in 4 of the 6 years of study the sample consisted of either one or two adrenal weights. Histological evidence for adrenal changes is presented, but the critical period during the decline of the population is represented by adrenals from only three adult deer and two immature deer. In the summary of this study (*3*) the authors write:

> It was concluded that physiological disturbances, induced by factors associated with high population density, probably hierarchial-behavioral, were responsible for the deterioration and death of these deer as well as for the manifestations of glomerulonephritis and hepatitis.

Yet no attempt to study the deer's behavior is reported. The authors give two reasons for believing that excessive browsing on pine bark was unrelated to the decline in population: (i) that the deer appeared well fed and (ii) that the degree of browsing on pine was the same before and after the decline. The critical period 1958–1959 is represented by only three adults. Of these, two in 1958 were 8 and 9 kilograms lighter than the average of five adults during a favorable year. No data on browsing of the sika deer were collected. In view of the absence of behavioral data and the presence of numerous environmental circumstances relating to the decline, the conclusions reached in a previous paper by Flyger and Warren (*4*) seem more tenable. With reference to the identical declining deer population, they summarize the circumstances leading to the catastrophe as follows:

> (1) A large herd of animals had built up which severely overbrowsed their range.
> (2) A substantial amount of food was lost in a fire.
> (3) Severe weather conditions required greater food consumption.
> (4) The feeding area was restricted by an ice barrier around the island.
> (5) The deer were forced to eat unpalatable materials including loblolly pine bark containing pine oils.
> (6) The condition of very little food, severe weather and consumption of poisonous substances resulted in mass mortality.

We have here criticized the data relating to deer, but some of the other data offered in Christian and Davis's article are equally controversial (*2*). The self-contained nature of the Christian hypothesis has probably accounted for its widespread acceptance in textbooks. The validity of their theory is not at issue here, but it needs to be pointed out that the data on population dynamics are complex and variable, and the resolution of the problems will not be furthered by overzealousness in the defense of a particular hypothesis.

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References

1. J. J. Christian and D. E. Davis, *Science* **146**, 1550 (1964).
2. N. C. Negus, E. Gould, R. K. Chipman, *Tulane Studies Zool.* **8**, 95 (1961).
3. J. J. Christian, V. Flyger, D. E. Davis, *Chesapeake Sci.* **1**, 79 (1960).
4. V. Flyger and J. Warren, *Proc. Ann. Conf. Southeastern Assoc. Game Fish Comm.* **12**, 209 (1958).

Any reference to the validity of numbers implies a consideration of variability, comparability of pooled samples, adequate numbers of samples, and other conditions imposed by statistical considerations. Negus and Gould's discussion (*2*) of the relationship of adrenal weight to population status in *Oryzomys* is based on five samples containing both sexes and one of males only, taken irregularly in the approximately 3-year period of study. Moreover, they have pooled adrenal weights of male and female, young and adult rats in unknown proportions, a procedure that their own data indicate is invalid, as there are clear differences in adrenal weight, in the directions one would expect, between males and females in the two samples taken during times of reproductive activity. The variability of adrenal weights in most of their samples is such that much larger numbers would be required to demonstrate significant differences between samples. However, this variability could no doubt be reduced greatly by putting weights from mature females, mature males, and immature males and females in separate groups for each sample, if their first sample consisting entirely of males can be used as a criterion. The data as published do not permit a conclusion in favor of either their hypothesis or ours.

Aside from the fact that large numbers of deer are difficult or impossible to obtain, we submit that the data from our study of sika (*3*) are strong despite the small numbers, for the following reasons:

1) The numbers required are determined by the magnitude of the differences and the variances of the samples. In our samples there was no overlap in the adrenal and body weights by sex and age between samples taken before and after die-off.

2) Complete autopsies were performed and were supported by histological studies.

3) All samples were collected at comparable times of the year (late winter to early spring).

Regarding the decline in weight of the sika, the appropriate "favorable