During the stated period, 366 individuals served on the 11 study sections; only 7 of these served twice. They were affiliated with 113 institutions that were distributed geographically as follows.

*Group 1.* New England, plus Delaware, New Jersey, New York, Ohio, and Pennsylvania. Of the total number of reviewers, 38 percent, or 140 individuals, came from 48 institutions. Thirteen of the institutions contributed five or more individuals; of these, only the University of Pennsylvania contributed nine or more.

*Group 2.* Alabama, Florida, Georgia, Kentucky, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and West Virginia. Of the total, 17 percent, or 61 individuals, came from 25 institutions. Duke, Johns Hopkins, National Institutes of Health, and Vanderbilt contributed five or more; none contributed nine or more.

*Group 3.* Colorado, Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, and Wisconsin. Of the total, 22 percent, or 81 individuals, came from 20 institutions. Eight of the institutions contributed five or more and, of these, the University of Illinois and Washington University, St. Louis, contributed nine or more.

*Group 4.* Arizona, California, New Mexico, Oregon, and Washington. Of the total, 23 percent, or 84 individuals, came from 20 institutions, six of which contributed five or more individuals. Of these, Stanford, University of California at Los Angeles, and the University of Washington contributed nine or more.

In order to get some rough idea of the pool from which these reviewers were drawn, we counted the number of current grants reviewed by these same study sections that have been continuously funded for six or more years. This number should approximately reflect the number of projects that have successfully competed for funding more than once and may give some indication of the number of scientists who are qualified by experience alone to sit on review panels. There are 1167 such grants currently funded. If this number approximates even within a factor of 2 the number of scientifically mature individuals in these areas of science, it appears that a substantial fraction of such scientists serve on a peer review panel at some time in their career.

These data do not prove that the study sections are free from bias; for example, we did not look at the replacement sequence in particular study sections to see if "scientific lineage" is a major determinant in the composition of the study sections. Nevertheless, the data do show a wide representation, they show that no small group of scientists has served repeatedly on any of these review groups, and they show that certain prestigious departments or schools have not had a disproportionate voice in the funding process. We feel that the data are consistent with the confidence that the scientific community has generally had in the NIH peer review system.


EDWARD W. WESTHEAD

*Department of Biochemistry,*
*University of Massachusetts,*
*Amherst 01002*


**Notes**


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## Insect Control

In discussing future means of controlling insect populations, Djerassi, Shih-Coleman, and Diekman (15 Nov. 1974, p. 596) fail to emphasize a method of biological control that may deserve more attention than it has received since its discovery (*1*), namely, introduction of alleles causing sex ratio distortions. In 1969, Hamilton (*2*) proposed control by locating "driving" sex chromosomes, isolating the relevant alleles from epistatic modifiers, and introducing those alleles into (outbreeding) populations lacking the modifiers. He noted that completely driving alleles, in the absence of modifiers, should cause extinction. Incompletely driving alleles, on the other hand, would cause an immediate, rapid decline but would not extinguish the population completely (*3*). Apparently no further attention has been paid to Hamilton's idea, despite his intriguing speculation that the general inertness of the "sex determining" chromosome suggests a prevalence of such loci (they are more effective on sex chromosomes, and more effective on the Y than on the X in insects and in man). If genes for such effects could somehow be manipulated to thwart the spread of modifiers, perhaps by successive introductions of different sex chromosomes bearing genes at different loci producing similar effects, populations might be collapsed to levels at which supplemental means of maintaining control or effecting local exterminations might be feasible. In their review of genetic means of controlling insect populations, Smith and von Borstel (*4*) mention meiotic drive, but mainly in connection with the introduction of dominant lethal mutations. Perhaps the paradox of natural selection favoring the spread of an allele whose net effect is to reduce a population has hindered research on this apparently important phenomenon.

PAUL W. SHERMAN
RICHARD D. ALEXANDER
*Department of Zoology* and *Museum of Zoology, University of Michigan, Ann Arbor 48104*

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In our article we did not attempt to list all, or even many, methods of biological insect control. We cited some general leading references, but our main purpose was to illustrate the crucial policy questions associated with future insect control methods by selecting a few specific examples. Therefore, our omission of specific mention of sex ratio distortion should not at all be interpreted as denying its potential; we share Sherman and Alexander's view that this as well as many other biological methods merit more intensive work. However, as we pointed out in our article, the economic realities of research funding and of the market place as well as current government policy offer more lip service than real incentives for fundamentally novel approaches to insect control.

CARL DJERASSI
*Department of Chemistry, Stanford University, Stanford, California 94305*
CHRISTINA SHIH-COLEMAN
*School of Medicine, Stanford University*
JOHN DIEKMAN
*Zoecon Corporation, Palo Alto, California 94304*

## Weather Modification: Possible Effects

Kellogg and Schneider (27 Dec. 1974, p. 1163) discuss the future and problems of climate stabilization and propose a type of "climate disaster insurance." I wish to point out an extremely important and quite neglected possible consequence of both climate stabilization and weather modification.

Engineering works for the storage and transmission of water within and between river basins are designed on the basis of streamflow records that can be statistically evaluated for reasonable design criteria. For example, flood control works are generally built to contain the "100-year flood" or that flood with a 1 percent probability of occurrence in a given year. If 100 years of streamflow records are available, the water stage and discharge statistics are highly reliable, but if only 50, 20 fewer years of records are available, then the statistics are less reliable. In such cases, flood control structures must be overdesigned, not because of the possibility of large floods, but because of the uncertainty of the design criteria.

Weather modification and climate stabilization have the potential of changing rainfall and runoff patterns to a largely unknown degree. If such programs are successful, the result could be the invalidation of previous streamflow records as a statistical basis for the design of new water engineering works. New structures would have to be overdesigned, and older engineering works might well need to be changed or supplemented with new works, all at a tremendous cost.

This potential effect of climate stabilization and weather modification should be considered in the planning and before implementation of any local, regional, or worldwide weather and climate modification program.

CHARLES J. ROBINOVE
*EROS Program, U.S. Geological Survey, Reston, Virginia 22090*

The article by Kellogg and Schneider poses questions, not of man's scientific ingenuity, which the authors demonstrate so well, but of man's political intelligence, which still lurks in the shadows of uncertainty.

However, what we call political "science" has wrestled with the solution of the types of problems to which the article refers. In regard to water rights, there are numerous precedents. In the