Letters

Favored Organisms

An informative article by Joseph Palca about a project to sequence the genome of the miniweed *Arabidopsis* (Research News, 14 July, p. 131) contains a remarkable aside that raises an important question about biological research: how many different organisms should be used to investigate problems in molecular biology and related fields? Palca quotes biologist Ron Davis as saying that "progress in plant molecular biology has been slowed by the multitude of different plants being studied." "It was Max Delbrück who started the concept that you can't do that," says Davis. "You can't work on a whole bunch of different organisms. You have to work on one, and only one." This quote exemplifies the extreme of a current emphasis on a short list of model systems in molecular biological research, as also emphasized, for example, in an issue of *Science* last year (10 June 1988) that was unabashedly devoted to a few of what Daniel E. Koshland, Jr., called in his editorial (p. 1385) "preferred models for biological systems" such as "the bacterium" (that is, *Escherichia coli*). These days one often hears discussions of whether work on a given organism that is not on the short list should be supported or whether an individual that works on such an organism should be on the short list for a position.

Emphasizing a few model systems has great utility—certainly we do not want to extend the current genome mania to sequencing all the DNA of all the beasts—but when the focus is carried to an extreme it is scary. Where would biology be if the advice of Max Delbrück had been followed consistently even just for his favorite bacteriophage? All work would have been on the T-even phage, T2, and perhaps T4, with no discovery even, much less study, of such exotic beasts as—just to mention some of those found in "the bacterium"—temperate phage such as λ, single-stranded phage such as øX174, male-specific phages such as f1 and M13, or the RNA phages f2 and Qβ. In a similar sense, where would our understanding be if all "botanists" always studied a single plant? If that plant were *Arabidopsis*, Barbara McClintock never would have discovered transposable genetic elements (in maize). Armin Braun and others never would have studied and understood the remarkable crown gall tumors (in tobacco and other plants) that led to Ti plasmids and the genetic transformation system that helps make *Arabidopsis* attractive. Going back further, Gregor Mendel, who so wisely chose peas but now perhaps could only get funded to work on *Arabidopsis*, never would have achieved the remarkable understanding of inheritance we call mendelism. These are just a few examples, and they only include bacteriophage and angiosperms. Other examples could be given of recent exciting discoveries that involve organisms not on the short list. One such example is Tom Cech's discovery of self-splicing RNA, which he made while studying the processing of preribosomal RNA in the ciliated protozoan *Tetrahymena*.

For certain goals it is wise, even essential in the case of megaprojects such as the genome games, to focus on certain research subjects. Yet to structure the overall support system to restrict biologists to a chosen few organisms, or even to excessively focus on them, would create a world where understanding is locked on yeast, fruit flies, and mice, and now, perhaps, a roundworm and a miniweed. Such focus would also miss the marvelous opportunities for fundamental discoveries still offered by the evolutionary diversity of organisms. A proper balance between emphasis on a few organisms in depth and a broader use of other organisms that are favorable for particular problems is crucial. Biologists studying fundamental problems should work on a suitable organism or organisms for good reasons, but they should not necessarily work only on an organism that is in vogue this week. Let us continue to creatively pursue interesting biological problems and choose organisms suitable to these pursuits, not just suitable genomes to sequence.

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Congratulations to Thomas J. Gill, III, and his coauthors on the publication of "The rat as an experimental animal" (Articles, 21 July, p. 269). In an era seemingly dominated by cellular approaches to experimental problems, a reminder of the contributions of the use of a particular species of animal in research is laudatory. A rapidly expanding technology combined with the intrigue of the unknown has led more researchers toward cellular and molecular pursuits. Monumental strides already have been made toward the institution of appropriate therapies for previously incurable diseases with this approach. However, the organism is more than just a collection of DNA or cells. Without experimentation at all structural levels (molecular, cellular, systemic, and organismic), there would be no appropriate application of the results. Gill *et al.* describe how these various approaches have been used interdependently to solve the complex problems in medical science today. Most important, this article reminds researchers, educators, and health care workers of the important role that animals play in the scheme of the scientific pursuit of the multifactorial analysis of disease.

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Gill *et al.* are to be congratulated on their extensive overview of behavioral research in "The rat as an experimental animal." However, they assert that, in rats, "the effects of aging and of various pharmacological agents, including alcohol . . . on behavior have been explored." Actually, the most frequently used animal in basic alcohol research is the halibut—hence the expression "to drink like a fish." It is also noteworthy that a strain of rat studied in three of the four pharmacology papers cited by Gill *et al.* is the "Fischer" strain, named on account of its derivation in Germany from the halibut (*Heilbutt*). Despite these minor inaccuracies, we found it remarkable that four pathologists not only take an interest in behavior but can come up with such startling insights.

The review by Gill *et al.*, together with the fact that rodents have survived on this planet for millions of years, should provide convincing evidence that the rat is a full-fledged animal and should no longer be considered "experimental."

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Feasibility of the "Flying Wing"

The article by Wayne Biddle reporting on a 40-year-old exchange concerning flying-wing aircraft (News & Comment, 12 May, p. 650) appears to misrepresent the modern significance of a petty dispute. The 1945 report by William R. Sears, Irving L. Ashkenas, and others was an extensive engineering study of possible future aircraft configurations. A minor appendix to that report attempted to use a simplified aerodynamic analysis to show the trends resulting from varying the ratio of the total-airplane vol-

ume, including the fuselage, to the wing volume. It was intended to indicate the *feasibility* of proving analytically the aerodynamic desirability of the flying wing. Joseph Foa in 1947 uncovered a calculational error in the appendix, and Sears immediately, 42 years ago, acknowledged the error. The error appeared in only a technical aside and did not bear on the conclusions of the study.

The 1945 Sears-Ashkenas analysis was so simplistic that it would not have been very significant even if the arithmetic had been correct. In 1945 little was known about optimal jet flight paths, and the cruise altitude was assumed to be constant. We now know that an optimal solution depends critically on the appropriate altitude being chosen for each configuration studied. Furthermore, a meaningful airplane design study must include the effects of structural weight and engine size requirements.

The fundamental advantage of a flying wing is that the lift-to-drag ratio of an airplane, a major measure of aerodynamic efficiency, is much improved by omitting the drag of the fuselage and the tail. Obviously the weight empty is also reduced. If the wing area required to carry the weight efficiently is so large that all the fuel and payload will fit within it, a flying wing is clearly a winner from a performance standpoint. If, on the other hand, the wing area must be greatly increased beyond the aerodynamically desirable area in order to provide the necessary wing volume, then the increases in the wing weight, surface-area drag, and drag due to flying at an inefficient angle-of-attack–altitude combination more than negate the gains due to omitting the fuselage and tail. Even aircraft as heavy as an 800,000-pound Boeing 747 have neither the volume nor the wing thickness to accommodate its passenger load. When aircraft become so large that the flying weight justifies a wing area and associated aerodynamically permissible thickness that allows passengers to stand up in the aisles in the wing, we may see flying wing passenger aircraft. With bombers, which carry concentrated loads requiring small volumes, a flying wing may be the desirable choice, especially when radar reflection is a significant factor.

Studies of flying wings have been conducted dozens of times during the last 40 years. Certainly many different configurations were studied before the design of the B-2 was chosen. A minor appendix in 1945 could not have had anything to do with the B-2 design.

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Erratum: In her Research News article "NCI team remodels key AIDS virus enzyme" (11 Aug., p. 598), Jean L. Marx wrote that Tom Blundell and his colleagues at Birkbeck College in London determined the three-dimensional structure of a recombinant AIDS virus protease. She neglected to mention that researchers from Pfizer Central Research in Groton, Connecticut, and Sandwich, England, made the recombinant enzyme and collaborated in the structural analysis.

Erratum: On page 1362 of the report "Phylogenetic stains: Ribosomal RNA–based probes for the identification of single cells" (10 Mar., p. 1360) by Edward F. DeLong, Gene S. Wickham, and Norman R. Pace, note 4 contained an error in the sequence given on lines 6 and 7. The sequence should have read, "5′-TTGYAGCC/ICGCGTGYM/IGCCCSGSM/ISA/ITTCGGGGC-3′." (The additional base T—indicated in bold-faced type—was omitted).

Erratum: In Joseph Palca's News & Comment article "New round in *Dingell* v. *NIH*" (28 July, p. 349), the Baylor College of Medicine in Houston, Texas, was incorrectly referred to as "Baylor University." Baylor University is in Waco, Texas.

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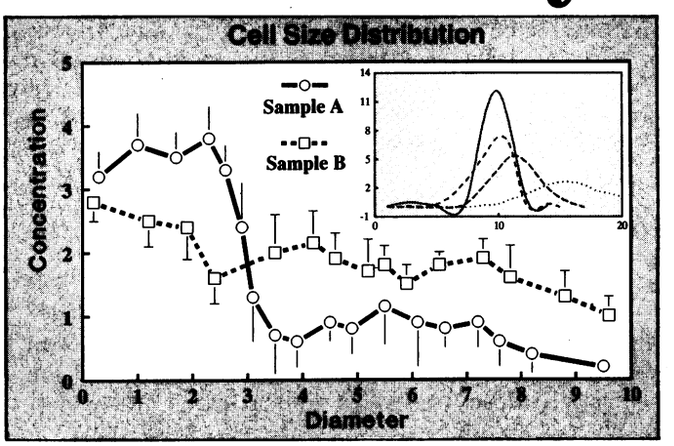

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