Fundamental and Applied Research in Agriculture

A dichotomy in biology harms both theory and practice.

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The issues raised by Nicholas Wade's report (*1*) have to be set into a broader context than simply the policies of the U.S. Department of Agriculture. Agriculture research in the United States, whether federally supported or carried out by the agricultural chemicals industry, seed companies, or by the faculties of agricultural colleges, has suffered from a narrow pragmatism that is reinforced by the general indifference of researchers in "basic science" to problems of agriculture. The result is two cultures in biology with very little productive interchange.

Within agricultural science, America's traditional anti-intellectualism survives as an antitheoretical bias. This is seen in the reluctance to take intellectual detours, and a preference for the accumulation of direct experience with a crop as a guide to practice. It is reinforced by the search for marketable products (mostly chemical) as the central strategy for improvement of agriculture, by the practice of research administrators to transfer staff members to new problems before a deep understanding of why something works or doesn't work has been achieved, by the way in which the success of agricultural research is evaluated, and by a narrow acceptance of the present structure of agriculture as a given condition which restricts options. For example, the consideration of mixed plantings is inhibited by the present design of farm machinery. Therefore research into the ecology of a mixed sowing only makes sense as part of a broader program that must include an engineering effort to redesign the machines.

Within the centers of fundamental research in the large urban universities, there is a general bias against "applied" work. Further, there is a tendency to separate organisms and communities into fundamental and applied: *Drosophila, Escherichia coli*, the bird of paradise, and liver homogenate are fundamental organisms while the corn borer, wheat rust, and tomato are merely applied. (Corn is in an ambivalent position since it played a fundamental role in cytogenetics in spite of being edible.) The tropical rain forest, the coral reef, and the tundra are fundamental communities, whereas the cultivated field, the midwestern lake, or the urban lot are applied communities. (The temperate forest, entitled to the rank of fundamental insofar as it is uncut, is apparently demoted to applied status by virtue of falling under the jurisdiction of the National Park Service.)

The long-term improvement of agricultural research requires first of all an intellectual rapprochement between agricultural research and fundamental research in the border domain of the theoretical underpinnings of applied research. It could develop along the following lines.

"Fundamental" researchers must be convinced that there is a rich intellectual content in the study of agriculturally relevant systems. (i) Economically important insects are kept under long-term surveillance over a wide area by U.S. Department of Agriculture personnel. Therefore they are more suitable for many ecological and distributional studies than organisms which have been watched only during the career of an individual investigator. (ii) The cultivated field and its associated fauna is an historically new system. Many of the pest species have undergone recent drastic shifts of their range, a few have even arisen in historical times, such as the apple maggot fly. Therefore these systems are ideal for the study of dynamic aspects of evolutionary ecology. As the major interest in population biology shifts from equilibrium to changing systems, the environments, communities, and organisms of agricultural significance will acquire increasing fundamental significance. (iii) The dynamic complexity of the cultivated area is compensated for by a relatively simple fauna and flora, while different fields sown to the same crop are closer to being replicate experiments than are different canyons in the same mountain range. (iv) The unifying principle of a management goal tends to break down disciplinary boundaries and should create an integrated population biology in which community structure, physiological responses, migration-extinction patterns, bioclimatology, and natural selection can be studied together. The inner logic of population biology requires increasingly that we study complex systems as wholes. A practical long-range goal may get us to do so.

At the same time that people working in basic science must be convinced that agriculturally relevant research is also of fundamental significance, agricultural scientists must be convinced that theory is practical.

(The National Academy of Sciences report as quoted was remiss in this area. It failed to indicate which if any major advances in agricultural productivity or pest control were derived from recent fundamental research, and ignored the more difficult question of where the absence of fundamental knowledge is a brake on practical research.)

As our research horizons increase, we have to deal with systems that are more complex. On the one hand, our commonsense intuition derived from the single step behavior of components in isolation becomes less reliable. The frequently contraproductive results of insecticide use is a paradigm for the importance of indirect effects in ecology. But on the other hand, as more variables are considered the numbers of possible combinations go up more rapidly. Thus it is possible to test 20 varieties of a crop for performance. If we consider mixed plantings of two varieties, we have to test 190 combinations. We soon reach the point where a basic understanding of plant inter-

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actions in mixed stands is the most practical approach.

If a domain of applied science does not stimulate the fundamental research it requires, it must borrow the tools from other fields. The attempts to apply mathematical techniques to biology have usually been most disappointing when the methods created for physics have been transferred passively, and most successful when biology has stimulated the creation of the most suitable mathematics. Thus the relatively weak contribution of fundamental research to agriculture is not a critique of theory per se, but rather reflects the absence of the appropriate theory stimulated by the needs of agricultural practice. Where fundamental research was stimulated by agriculture, as in the development of biometrics at the Rothamstead Experiment Station or in Vavilov's gathering of the world seed collection and study of origins of cultivated plants, the practical significance was great.

Among the areas of agriculturally relevant fundamental research which are relatively lagging and which suggest themselves to a population biologist are:

1) Natural selection within communities. There is a suggestion that feedbacks within the community may result in a species selecting itself to local extinction. Manipulation of community parameters can harness selection to our ends.

2) The genetics of expanding species.

3) The study of the distribution of pests and their predators over a region within the framework of the extinction-migration theory of quantitative biogeography.

4) Host location in a heterogeneous environment.

5) The process of niche shift in the origin of pest species and the artificial selection of control organisms.

6) The community ecology of fine-grained and coarse-grained mixed plantings, both interspecific and at the level of polymorphisms.

7) The development of selection theory for coevolution.

8) The modeling of the cultivated field as an ecosystem in which the microbiological and cryptozoan communities are included.

9) Acclimation and adaptation in the widespread and narrowly restricted insect species.

10) Local race formation in low-mobility groups such as soil nematodes.

It is unlikely that any large-scale federal support will be forthcoming for work of this kind. (My own attempts to obtain small-scale support to cross over into this domain of fundamental applied research have been rebuffed both by agricultural and "basic" funding agencies.) Nor can we expect to alter the orientation of federal or state agricultural research programs. Rather the initiative will have to come from working scientists who go out deliberately to break the barriers between the "applied" and "fundamental" communities, between "applied" and "fundamental" research, between the state agricultural schools and agriculture research stations on the one hand and the urban "fundamental" biology departments on the other, building up a network of informal channels of discussion and cooperation that at least initially can be independent of administrators' approval.

Perhaps an informal committee for fundamental research in agriculture could meet as an adjunct of the meetings of one or more professional societies for this purpose.

References

1. N. Wade, *Science* **180**, 390 (1973).

NEWS AND COMMENT

Strip-Mining: House, Senate Gird for Renewed Debate

Close in the wake of a bitter contest over the future of the trans-Alaska pipeline, Congress is warming up for this year's second major debate in which the nation's appetite for fuel and the interests of the energy industry are matched against conservationists determined to preserve the American landscape. The subject is strip-mining, and pressures on Congress for some form of federal regulation are growing.

One reason is that nationwide coal production is accelerating, and so is a shift from underground mining to more economical, but damaging, surface operations, which, for the first time last year, produced more than half the nation's coal. Superimposed on these trends is a shift in stripping operations from Eastern states to the West, where more than 70 percent of the nation's low-sulfur coal lies relatively close to the surface of stupendous reaches of plains and desert, from the Navajo lands of northern New Mexico to the Canadian border.

Demands for low-sulfur fuel have been increasing in Eastern and Midwestern cites, and long "unit trains" of 100 coal cars and more are already plying to Chicago from newly opened coalfields in Montana. Higher levels of production have been encouraged by the Nixon Administration to offset the nation's reliance on foreign oil, but as stripping increases, so does the amount of ravaged land. The President's Council on Environmental Quality (CEQ) estimated in March that strip-mining, which began in earnest only after World War II, has dug up 4 million acres, mostly in Appalachia, only half of which have been reclaimed. Stripping operators have left behind some 200,000 acres of unrestored "orphan lands," for which owners cannot be found; several thousand miles of streams polluted by strip mine wastes; and 20,000 miles of exposed "highwalls." The Environmental Protection Agency (EPA) has estimated that stripping disturbs between 4000 and 5000 additional acres a week; environmentalists' opposition has grown apace.

In spite of the lateness of the season, congressional observers see some chance that the House and Senate will agree this session on a bill creating a permit system to regulate strip mine operators. The same law would probably establish a set of minimum environmental "performance" standards operators would have to meet, both in mining and in rehabilitating the land. Still to be settled, however, are working details of the permit system; the precise role of the federal government