Goldfarb's Thanks

After my arrival in New York (News & Comment, 31 Oct., p. 539), it was a pleasure to hold an issue of *Science* in which portions of the Briefings section were not removed by a censor, as they are in Moscow. It is from these blank spots as well as from visits and phone calls by my Western colleagues that I learned about the efforts on my behalf in the United States and Western Europe. But it was not until I was presented with a thick file of materials and correspondence that I realized the scale of this campaign.

I want to thank my colleagues for their help and support. Visits to my home by Benno Muller-Hill, Ekke Bautz, and Wolfram Zillig; letters written by André Lwoff, Bill Hayes, and Elie Wollman (*1*); and regular phone calls from Simon Silver were important for me not only as demonstrations of personal friendship but also as very persuasive arguments in my dialogue with Soviet authorities. On several occasions during the past 7 years, I had a chance to witness how the authorities and the bureaucracy of the U.S.S.R. Academy of Sciences softened their attitude to me as the result of this pressure. It was due to the strong stand of George Melchers that I was not removed from the editorial board of *Molecular and General Genetics* after applying for an exit visa in 1980. Telegrams and letters from my colleagues convinced the Soviet Academy to reemploy me as a consultant in my old institute from 1983 to 1984. The flow of inquiries about me on the eve of the Federation of European Biochemical Societies meeting in Moscow resulted in the Soviet Academy's attempt to resolve my case over the objection of the KGB in 1983. And most of all, the protests by my colleagues and the moratorium on sending bacterial strains to the U.S.S.R. organized by the Committee of Concerned Scientists in 1984 saved me from criminal prosecution for an "attempt to smuggle out of the country" a collection of *Escherichia coli* auxotrophs.

It is impossible for me to personally thank everybody involved in this campaign. Yet I hope I will be permitted to list the authors of the moratorium—Max Gottesman, Charles Yanofsky, Simon Baumberg, and Michael Yudkin—and the 16 signatories of the cable to the Soviet Academy (*2*) as individuals primarily responsible for the fact that officer Gusev of the Moscow KGB one day officially informed me that the charges against me were dropped.

Let me finish by saying that there are others left behind in Moscow. Among them are our fellow biologists Iosif Irlin and Valery Soifer. I hope that they will follow me soon.

DAVID GOLDFARB
Columbia Presbyterian Hospital,
622 West 168 Street,
New York, NY 10032

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Alternative Crops

Noel Vietmeyer's article "Lesser-known plants of potential use in agriculture and forestry" (13 June, p. 1379) is a valuable survey of little-known plants with potential for agricultural development, particularly in the tropical countries. His suggestion that tapping these existing plant genetic resources can make as great a contribution as genetic engineering in solving world resource problems is sound. Unfortunately, however, Vietmeyer presents a view of tropical agriculture as a tabula rasa awaiting development by outside experts. The scientist's role is to improve the yield characteristics of individual crop species, which are then extended to the farmers for their adoption. This approach ignores the systemic character of tropical subsistence agriculture in which the acceptability of new crop species, however productive they may be on the experiment station, is determined by a complex set of social and ecological factors.

Benjavan and Kanok Rerkasem of Chiang Mai University in Thailand have shown (*1*) that each species, and indeed each variety within a species, must fit into an existing agroecological niche for acceptance by small farmers. In their study of the persistence of traditional rice varieties in the Chiang Mai Valley, the Rerkasems discovered that, despite government efforts to replace traditional varieties with genetically improved high yielding varieties (HYV's), yield was only one of many important niche parameters. In those areas where farmers grew garlic after rice, a traditional variety yielding large quantities of straw was retained, despite its low grain yield, because the high demand for mulch for the garlic beds made the straw nearly as valuable as the rice itself. In other areas, where farm plots were small and the farmers depended upon income derived from harvesting the fields of larger landlords, late maturation was the key niche parameter. This allowed time for them to complete harvesting the fields of others for wages before cutting their own grain. The HYV's could not satisfactorily fill such specialized niches and were therefore rejected in favor of the traditional varieties.

Another example is a study of traditional Javanese "homegardens" by Otto Soemarwoto and his colleagues of the Padjadjaran University Institute of Ecology in Indonesia. The homegarden is a complex agroecosystem with as many as 60 species densely packed into a small area, reflecting land shortages in overpopulated Java. An important strategy for maximizing production per unit area is to plant species that form a multistoried canopy with coconut palms (*Cocos nucifera*) as the emergent level. Without considering the niche assigned to coconut palms, development experts suggested distributing dwarf Samoan palms to the farmers. These palms bear at an earlier age, give higher yields, and are easier to pick than the tall variety. In the homegarden system, however, these advantages did not outweigh the disadvantage of the dwarf palm directly competing for space and light with other species.

One of us (P.E.S.) with colleagues at the University of Philippines at Los Baños found that even farmers' perceptions of legal codes for landownership affect species acceptance. Seeking to stabilize upland farming systems, the Ministry of Natural Resources distributed tree seedlings of *Acacia mangium, Leucaena leucocephala*, and *Mangifera indica* to Palawano farmers. Although these tree species were well adapted locally and offered free of charge, no Palawano farmer accepted them, believing that planting "government seedlings" gives the government title to their ancestral lands.

The explosive expansion of cassava (*Manihot* spp.) in northeastern Thailand in the 1960's, without government encouragement, offers an example of farmer adoption of a new species because it fits into an otherwise unoccupied niche. As has been shown by Terd Charoenwatana and his associates at Khon Kaen University, the introduction of cassava allowed farmers to grow a valuable cash crop on previously unused uplands without competing for the limited labor available to plant the subsistence rice crop in the lowlands during the brief rainy season.

These examples, multiplied many times from our colleagues' work in the Southeast Asian Universities Agroecosystem Network (SUAN), cause us to question the fundamental approach to agricultural research underlying Vietmeyer's article. Identifying new plant species having potential value and then having agronomists "improve" them will not, in our view, lead to solution of the problems faced by tropical subsistence farmers. Instead, one must first understand existing agroecosystems and identify potential niches within them, then seek new plants that appear suitable for filling these niches. This calls for a different research strategy from the essentially reductionist approach conventionally used by agriculturalists. Interdisciplinary work is needed by natural and social scientists who share a human ecological perspective on agricultural systems.

Unlike the search for new species, either in the wild or genetic engineering laboratories, agroecosystem research does not promise instant solutions for the problems of agricultural development in the tropics. It is labor-intensive and site-specific and cannot be centralized at a few elite international research centers. It is best pursued by scientists working in local institutions having deep knowledge of their own rural areas (*2*). Developing agroecosystem research capability at local institutions in the tropical countries is a slow and expensive process. With rare exceptions, such as the Ford Foundation's support over the past decade for development of SUAN, funding for these activities has been difficult to obtain. Unless much more support is provided for research on local agroecosystems, however, we fear that many of the plant species being identified by Vietmeyer and his colleagues will remain as little more than scientific curiosities rather than realizing their real potential for improving the lives of tropical subsistence farmers.

A. TERRY RAMBO
PERCY E. SAJISE
East-West Center,
1777 East-West Road,
Honolulu, HI 96848

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Response: I certainly do not look on agriculture as being an "empty slate." Like Rambo and Sajise, I believe that farmers should, and will, be the final judge of what they produce. But farmers—who constantly deal with changing markets, outbreaks of diseases, and hundreds of other variables—deserve to have many options available to them. This is why devoting scientific attention to alternative crops is important.

I do not agree that turning development over to the local institutions in tropical countries to develop "agroecosystem research capability" is the only (or even the best) way to improve the lives of tropical subsistence farmers. Doing that is likely to merely help the researchers and the publishers of scientific papers.

I do not agree, either, that farmers automatically know what is best for themselves. There are innumerable examples where farmers were so conservative that they refused to adopt valuable crops that later proved vital to their well-being. For instance, early this century American farmers vehemently rejected the soybean, and in the 1700's German farmers so opposed the potato that the ruler had to force them to plant the new tuber on pain of death.

Nor are all farmers contented with their existing crops. There is probably not a country in the world right now that doesn't have many farmers who are anxious—even desperate—to try new, and often exotic crops. Collectively, Filipino farmers are among the most entrepreneurial in this regard. Former generations of such pioneers gave the Philippine economy and diet such now beloved mainstays as sinkamas (*Pachyrhizus erosus*; from Mexico), chico (*Manilkara zapota*; from Central America), and sweet potatoes (*Ipomoea batatas*; from the Caribbean).

Actually, Rambo and Sajise and I are pointing up related areas of science that are underappreciated in mainstream agricultural research. I believe our views are more complementary than confrontational.

Finally, I would like to note that my article was written with far more areas than lowland tropical villages in mind. There were sections on crops for arid lands, for highlands, and for temperate zones. Those are regions where "agroecosystem research" of the Rambo-Sajise type is less important because crops there are not all jumbled up together, as they are in the backyard of a Southeast Asian village.

NOEL VIETMEYER
National Research Council,
2101 Constitution Avenue, NW,
Washington, DC 20418

Insect Resistance

I wholeheartedly endorse the view of L. B. Brattsten *et al.* (14 Mar., p. 1255) that the most promising approach for delaying insecticide resistance is integrated pest management, the judicious use of chemicals in combination with biological and cultural control techniques. However, their statement that "exposing a population with incipient resistance to a low insecticide dose leads to rapid fixation of resistance" is misleading and could have serious negative consequences. Simulations and analytical models showing that high doses can retard resistance (*1–3*) are based on highly restrictive assumptions that include: (i) presence of permanently susceptible pools of individuals in refugia from insecticide treatment, (ii) successful interbreeding between susceptibles from refugia and resistant individuals from treated areas, (iii) complete recessiveness of resistance, and (iv) low initial resistance gene frequency. Although such conditions may apply in certain special cases, they are generally not applicable (*3*). As noted in the simulation study cited by Brattsten *et al.*, "Even a very small deviation from complete recessiveness of resistance has a disastrous effect. . ." and is likely to arise if dose is not rigorously controlled (*1*).

Even if conditions are suitable for using high doses to delay resistance in one pest species, this strategy may greatly accelerate resistance development in other pests present and disrupt biological control by natural enemies, thus causing serious secondary pest outbreaks (*3*). Attempts to suppress resistance with high doses would also exacerbate the public health hazards and pollution problems associated with insecticides. Our best bet for slowing resistance lies in the most obvious strategy—reducing the use of insecticides.

BRUCE E. TABASHNIK
Department of Entomology,
University of Hawaii,
Honolulu, HI 96822

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The article about resistance to chemical control by Brattsten *et al.* does not account for the unique response of the two-spotted spider mite to Pentac or the development of fitness by a resistant strain.

In greenhouses during the 1950's, this mite rapidly escaped chemical control. The introduction of Pentac in the early 1960's resulted in "100%" control, which continues to the present time (*1*). This response was not due to lack of a resistance mechanism.

Selection during the formation of a multiple hybrid swarm, for rapid response to selection (*2*), resulted in a 1000-fold increase of Pentac resistance (*3*). The limited gene pool, the absence of a single resistance factor, and the lack of gene flow maintained susceptible mites in the greenhouses.

Organophosphate selection of a hybrid mite strain rapidly resulted in a high level of stable resistance, whereas as strain devel-