Science, Biopolitics and Risk: Margins of Uncertainty

Sheldon Krimsky

Department of Urban and Environmental Policy
Tufts University
Medford, Massachusetts 02155

Kawar and Sherlock review the science and policy literature on the release of genetically engineered organisms (GEOs) in the environment and conclude that (1) knowledge has not advanced enough to be able to make competent assessments of the risks; (2) the complexity of ecosystems is a formidable obstacle to developing a predictive ecology in the future; (3) the basic assumption behind releasing GEOs—namely that humans have a right to rearrange biotic life on the planet—is bankrupt.

Their essay contains a number of loosely framed arguments that include explanations, political analyses, and interpretations of scientific literature. I shall direct my remarks to points (1) and (2) of their analysis. My goal is to deconstruct some of their arguments in order to provide a finer-grained discussion of their conclusions. Overall, I find that their discussion glosses over some relevant complexities of the problems associated with deliberate release. Their discussion of the scientific literature is selective and they offer no solutions to the problem of divergence among experts on the risks of introducing GEOs in the environment. Although the authors draw upon comparisons and distinctions between chemical and biological entities, their arguments that the uncertainties or hazards are greater for the latter are weak and poorly substantiated. I shall begin my analysis by examining some of the key themes in their essay.

Risk assessment methodology for deliberate releases of GEOs does not currently exist.

Among the reasons given for the above are:

a. No general principles for a predictive ecology are available.

b. GEOs are not analagous to chemical toxins, where advances have been made in risk assessment.

c. Specific knowledge of GEOs required for risk assessment (infectivity, pathogenicity, host range, horizontal gene transfer) are absent or unreliable.

d. Laboratory experiments cannot be used as a basis for describing the activity of GEOs in an actual ecosystem.

e. Knowledge of *specific ecosystems* cannot be used to predict the effects of introducing GEOs into general ecosystems.

Proposition (a) is widely accepted by ecologists. However, it is not generally accepted that *no satisfactory risk assessments* can be made for *some subclass* of GEOs. As an example, in the case of ice minus, where the GEO was constructed by deleting a gene from the ubiquitous organism *Pseudomonas syringae,* once the data were made available there was wide agreement among ecologists that a field test would not introduce any significant risk to the environment. That there are always uncertainties in risk assessment (even when the weight of expert opinion supports the no-risk outcome) is not an argument that there do not exist some classes of GEOs that can be satisfactorily assessed for environmental risks.

Regarding point (b), it is true that advances have been made in risk assessment for chemical substances. The authors are also correct in citing fundamental differences between new synthetic chemicals (NSCs) and GEOs. Nevertheless, there are environmental scientists who have argued persuasively that some, but not all, of the existing methods for evaluating the risks of NSCs are also applicable to GEOs (Suter, 1985).

A report issued by the National Science Foundation (Covello and Fiskel, 1985:47) makes even a stronger case for the similarity of risk assessments.

> Risk assessment methods developed for microbiological applications in medicine and agriculture prior to recombinant DNA are equally appropriate for modern biotechnology, since the technical risk assessment issues are essentially the same.

Much hinges on what are considered to be the methods of risk assessment and how the self-reproducibility of GEOs factors into such ans assessment.

Items (c) through (e) build on the distinction between chemicals and biologicals. The authors fail to substantiate a number of presuppositions to their central argument, namely, that risk assessment applied to chemicals (under laboratory conditions) has been successful (or more successful than GEOs) in predicting the behavior of chemicals in an ecosystem; that the uncertainties of releasing a GEO are substantially greater than that of releasing NSCs; and that there are intractable limitations in assessing risks of GEOs that do not exist for NSCs. Since NSCs have been around longer than GEOs, one cannot compare the existing state of risk assessment as an indicator of what the future holds. The authors have not provided supporting evidence that ecosystems with GEOs exhibit more uncertainty, greater vulnerability to perturbations, or more radical ecological disruptions than ecosystems with NSCs. The introduction of chemical agents in the environment has been associated with ecological impacts on a global scale as in the case of chlorofluorocarbons (CFCs).

It should not be surprising that chemists are more concerned about the adverse environmental consequences of GEOs than they are of NSCs, while some molecular biologists view chemical introductions as far less predictable than that of genetically modified life forms. Brill (1985) asserts that the risks of genetic engineering should be viewed in the context of traditional genetic and chemical practices. Moreover, minor changes in chemical structure are far more problematic than minor genetic modifications.

> Experience has shown that it is important to test the degree of toxicity of each newly synthesized chemical before it is used internally or added to large areas of land. Even if a new chemical is only a slightly modified analog of a known safe chemical, the degree of safety cannot be extrapolated from the safe chemical. In fact, analogs of normal metabolites can be most dangerous. By comparison, minor modifications obtained by breeding safe plants or mutating safe microbes do not yield progeny that become serious problems. (Brill, 1985:383)

We may find similar divergences in the assessment of chemical hazards between fields like toxicology and epidemiology. The former place much greater confidence in laboratory screening tests and animal models than the latter. Epidemiologists are more likely to argue that real environments are far too complex and unpredictable to model a chemical introduction. These alternative perspectives on risk are grist for a social constructionist view of science (Bartels, 1985). The authors fail to take account of the disciplinary fault lines that are responsible for opposing perspectives on GEOs.

It is a fair hypothesis that, pertaining to risk assessment, our current knowledge of GEOs lags behind that of NSCs. Can we reduce the uncertainties for GEOs in general or for a particular organism to make the potential risks minimal, acceptable, or worth the expected benefits? The ecological community—a discipline that is not yet tied to the commercialization of applied genetics—is not of one mind on the possibilities of risk assessment. The authors provide no evidence of the weight of opinion among environmental scientists on this issue. Their selection of scientific support for their position rides on generalities but not on specific examples of GEOs. Even without a universal law of motion, we may be able to describe the precise trajectory of a falling body.

Regarding points (a), (c), (d), and (e), the authors state that no general agreement has been reached among ecologists as to what parameters should be measured to assess adverse ecological effects. They ignore the widely used schema for risk assessment of GEOs first introduced by Alexander (1985:57-68):

> Will a GEO survive?
>
> Will it multiply?
>
> Will it spread beyond its original area of application?
>
> Can it transfer its genetic material to other organisms?
>
> Will the original organism or any of those that might pick up its genes prove harmful?

These framing questions are part of a conventional wisdom that says that GEOs are applied in highly specific circumstances and risk assessment has to take these into consideration, and that no generalized method can be used to assess the risks. The authors are correct in reporting that the above questions cannot be answered by a set of canonical tests. But can they be answered in specific cases with sufficient degree of confidence? If not, why? The essay has not provided a satisfactory response. In my concluding remarks I shall argue against the reduction of the deliberate release issue to one of "scientism."

Ultimately, decisions about GEOs fall into categories: scientific, philosophical, and political. Kawar and Sherlock build their primary case that deliberate releases should not be carried out on the basis of the insufficiency of science. I have argued that their evidence is selective. Even skeptical ecologists can conceive of "safe" releases when a sufficient set of conditions are met. The authors refer briefly to the philosophical objection to genetic engineering characteristic of a "deep ecology" perspective wherein "Nature knows best."

There is a set of political objections to deliberate release that I find more persuasive than the technical or philosophical objections. They have been overshadowed in the public policy debates by the scientific discussions, particularly between ecologists and molecular biologists.

The following set of questions highlights these concerns.

> How much science will be deployed to answer the questions about the risks of GEOs?
>
> Will each proposal for a GEO receive the attention that ice minus received?
>
> Will approval for the release of a GEO be given

when the social benefits are suspect?

How can we be reasonably certain that the assessment of GEOs will not be controlled by special interest groups whose preeminent concern is the promotion of biotechnology?

A regulatory sector is not in place to give adequate attention to these questions. We already have behind us the dreadful failures in the regulation of NSCs. These failures were not in large part the result of bad or inadequate science. They were the result of an imbalance between the innovative sector (industry) and the social guidance sector (public interest groups and government). As I see it, the best science will not be consistently and uniformly brought to the problems of risk assessment. The academic field of molecular genetics has extensive ties to the development sector (Krimsky, 1985:45-55). The intense pressure for commercialization and international competition is very likely going to weaken the resolve of the regulatory sector regardless of the science. This has been the experience in regulating chemicals. It has been turned somewhat by chemical catastrophes and the mobilization of public outrage. The greatest obstacle to insuring the safety of GEOs resides in the policy sphere. Failure of the public will to adequately regulate and set high standards of scientific review and acceptable risk makes the philosophical objections to GEOs increasingly more attractive.

Politics and the Life Sciences

Volume 7
Number 2

The journal of the Association for Politics and the Life Sciences

Politics and the Life Sciences is published semi-annually in February and August by the Association for Politics and the Life Sciences, which is located at Northern Illinois University, DeKalb, Illinois 60115.

Politics and the Life Sciences is indexed in ABC POL SCI., *Biology Digest, Current Biotechnology Abstracts, Current Contents: Social and Behavioral Sciences, International Political Science Abstracts, Sociological Abstracts, and Social Sciences Citation Index.*

Membership in the Association, subscription to the journal and other mailings about the activities of the Association are included in the annual fee of $8 for students, $20 for regular memberships, $30 for sustaining memberships, and $35 for library subscriptions.

ISSN 0730-9384