

he will accept, can the biology teacher be truly intellectually honest.

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Walker, Mertens and Hendrix Comment

Mr. Bedford points out quite correctly that the manner of the origin of life, having already occurred, is currently beyond empirical verification. According to the creationist position, the origin of each major life form was a single supernatural event. Processes were employed in creation that are not to be found currently in nature. Since *this* account of creation involved processes that are supernatural and not repeatable, these processes cannot be subject to empirical verification. This important distinction in verification between the evolutionist position and the creationist position exemplifies the metaphysical distinction between the two accounts of the origin and development of life forms. As Mr. Bedford asserts, there may well be evidence which supports the creationist position; however, such a correlation should not be interpreted as scientific evidence for cause and effect.

Unfortunately, Mr. Bedford fails to note any distinction between the *process* of evolution (natural selection) and the *products* of that process. Natural selection has been verified empirically and is a biological process which can be subjected to the test of predictability. Modification in notions about the products (i.e., phylogenetic sequence) that arise through differing interpretations of existing data or the development of new data does *not* negate the process. Science has quite properly inquired into the question of *how* the various life forms originated—a question metaphysically excluded from religious inquiry. Religion, on the other hand, may answer the

question of *who*—a question metaphysically excluded from scientific inquiry.

Research on Recombinant DNA

Although the following comments are focused on Marc Lappe's article (ABT 39(4):200) the points I wish to raise apply rather generally to other ethical and safety considerations concerning recombinant DNA and, indeed, several similar subjects.

First, the method of risk estimation used here and elsewhere, which consists essentially of multiplying together the individual, estimated probabilities along any one path to disaster and, in some cases, adding the joint probabilities for all paths, is likely to be irrelevant because disasters often occur through previously unanticipated paths.

Second, overcontrolling "unprecedented but potentially risk-generating procedures" is a delicate business: slightly too much control and the work won't be done at all with the attendant risk that valuable information and technology will be lost.

Third, "scientific accountability for uncertain consequences of research" presents at least four, partially synergistic, difficulties unless the level of the research is so close to the point of application as to place the research more in the area of technology than of fundamental science:

(a) How likely is it that a worker in fundamental science can anticipate each possible use, misuse or unintentional effect of a finding he might make?

(b) How likely is it that a scientist can make a reasonable risk estimate for all possible uses that might be made of the result of research he has not yet even performed, at some indefinite future time and in some unknown future set of circumstances? It is of no use to make a set of totally fail-safe estimates since such a set would probably interdict practically any socially worthwhile research.

(c) How far back does the chain of responsibility go? Do we hold Alfred Nobel responsible when a terrorist group blows up a building with dynamite, or do we go back to John Dalton perhaps?

(d) To what extent can the research worker rely on the political process and the legal machinery to prevent undesirable uses of his results? Not to do so would be to throw out the time-honored principle of *abusus non tollit usum*, abuse does not bar use. It seems to me that *this* is the level where "the people" can have a useful input into the use of science since, on the one hand, the political mechanism is more or less in place and, on the other, the issues are usually clearer, more easily understood, and more direct than at the level of decision for or against the performance of basic research. Let the wrongful user of a discovery be punished and the rightful user be exalted.

Fourth, the issue of informed consent by the public for experimental work that poses a danger to it revolves initially around the perception of that danger, and finally, and much problematically, around the word "informed." When, as in the present instance, the "experts" are in disagreement, when the amount of information reaching the public from various groups with differing views depends greatly on the relative financial support of those views, as in the nuclear power controversy, and when, as is generally the case, the process of public education must work into a tremendous load of both irrational prejudice and indifference, it becomes questionable whether the public can really give or withhold its *informed* consent. And since we are not a nation of philosopher-kings, the appeal to the supposed consent or lack of consent of "the public" can easily, I fear, become, like an appeal to patriotism, the last resort of scoundrels.

Lastly, is altruism on our part hardly more than a means of deceiving ourselves about safety? Even if we stopped all recombinant DNA

work in the United States completely, would that induce the "President-for-Life" of Graustarck not to put a few scientific hacks to work to deliberately introduce "genetic material to a recombinant that codes for toxins?" I would rather rely on worldwide individual consciences, weak as such a reliance must unfortunately be, than on an ostrich-like unilateral ban.

It would seem that a great deal more thought needs to be devoted to some of these issues.

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Television Tapes in the Classroom

James A. Brenneman's article, "Television Videotapes in the Biology Classroom," (*ABT* 39(4): 237) contains some excellent ideas. However, one important problem is not discussed: these programs are covered by copyrights. How does Brenneman handle this?

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Brenneman Comments

I was not previously aware that using videotapes strictly for educational purposes was in possible violation of copyright laws. I got the idea of using videotapes in the classroom several years ago at an NSF-sponsored Chautauqua-type course, in which the speaker stated emphatically that the air waves are free. As I recall, no mention was made of copyrights.

Classroom use of videotapes not only greatly facilitates the educational process, but it also serves to encourage the viewing of such programs. However, if the practice is in violation of copyright laws, it should of course not be done.

AIBS Membership

I believe that John A. Behnke's "Letter to the Editor" (*ABT* 39(1):53) deserves a reply. I once had the pleasure of working with John Behnke on a curriculum development project and have the very highest respect for his opinion regarding biological education. But, I cannot agree with his implied assertion that membership in AIBS be considered a requirement for officers and directors of NABT.

In a subtle (however unintentional) manner, Mr. Behnke implies that the NABT members are not qualified to elect their own officers and directors. This hardly seems fair to the hundreds of NABT officers and directors who have worked so hard over the years in helping NABT become the fine organization that it is today. It is even more unfair to imply that fifteen of the officers and directors elected for 1977 lack proper credentials just because they are not members of AIBS.

One could just as well argue that candidates for NABT elective office be urged to subscribe to *Scientific American*, or required to join NSTA, or The American Museum of Natural History, or, for that matter, be involved in some way with the Biological Sciences Curriculum Study (BSCS). The only criterion should be membership in NABT.

The officers and directors of NABT are elected by the NABT members at large. Each member is sent a ballot along with literature about each candidate including his or her professional affiliations. Members of NABT have the knowledge and ability to elect their own officers and directors.

I wish to end my negative reply to Mr. Behnke's letter by urging that he do everything in his power to help increase membership in both NABT and AIBS. If more biology teachers became involved with AIBS there could develop an exchange of ideas between members of NABT and AIBS that could be of great benefit to biology teachers everywhere.

However, the pros and cons of research on recombinant DNA, or a discussion of the international food crisis are not sufficient reasons to join AIBS. I am sure that most conscientious biology teachers are well aware of these topics. I suggest that Mr. Behnke be invited, along with other members of AIBS, to come forth with some really strong reasons why biology teachers should join AIBS.

What concerns me even more, is the great number of science teachers who do not belong to NABT or any other science teachers organization. Let us all get behind an effort to reach these teachers!

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Behnke Comments

I regret that Mr. Abraham interpreted my letter as a slur on NABT officers. I have worked with many of them in the past and have the highest respect for them.

AIBS needs them and all biology teachers in their important efforts in the field of public responsibility in science. The range of material in *BioScience* is far greater than the examples selected.

Conserving Frogs

The article, "Redleg: Cause and Treatment in Laboratory Frogs" by Kevin F. Fitzgerald (*ABT* 39(2): 112), relates a well known (but difficult) prophylactic/curative procedure for dealing with "redleg." In small numbers, frogs can be successfully treated in this manner, and the procedure and antibiotic regimen is well known to those of us in the frog supply industry.

As a more practical procedure and one more attuned to the treatment of large numbers of frogs, I would like to recommend a highly successful means of "redleg" prevention with a high rate of cure.