Letters

Costs and Benefits of Recombinant DNA Research

The discussions in *Science* of the recombinant DNA problem, beginning with Singer and Soll (Letters, 21 Sept. 1973, p. 1114) and Berg *et al.* (Letters, 26 July 1974, p. 303) and continuing with Chargaff (Letters, 4 June, p. 938) and Simring (Letters, 4 June, p. 940), present the issue as if it were a balance between the costs to society of possible disastrous infections and the benefits to a few biologists of pursuing their professional careers. Chargaff asks, "Have we the right to counteract, irreversibly, the evolutionary wisdom of millions of years, in order to satisfy the ambition and the curiosity of a few scientists?" If this were really the question at issue, the inevitable answer would be negative. But in fact the technology of recombinant DNA offers potential public benefits which are at least as significant as the dangers. The public costs of saying no to further development may in the end be far greater than the costs of saying yes. Unfortunately, our legal and political institutions were designed to count the costs of saying yes to unsound technological ventures, and have no established procedures for counting the costs of saying no.

Biologists who defend their work on the ground that it may be of benefit to humanity come under suspicion of serving their own interests. Biologists feel comfortable saying that they do their work for fun or for a living; they feel uncomfortable posing as saviors of humanity. Therefore I find it appropriate, as a physicist having no personal stake in recombinant DNA, to make certain claims which the biologists are inhibited about making for themselves. I claim that the exploitation of recombinant DNA techniques may lead to an understanding, and conceivably to a cure, of cancer. It may lead to the creation of improved food plants which could save hundreds of millions of people from imminent starvation. It may lead to the creation of energy crops which offer benign alternatives to nuclear fission and fossil fuels. These claims are of course impossible to substantiate. There is no way to estimate numerically the probability that these things will happen. I can only say that in my nonexpert opinion, and in spite of Chargaff's eloquent derision, these possible benefits of recombinant DNA research are more likely to materialize than any of the most extreme dangers. I do not deny or belittle the dangers. I say only, let us not leave the starving millions of humanity out of account when we balance the dangers against the benefits. It is perhaps not irresponsible, but rather an act of enlightened courage, to expose ourselves to an unknown risk of disastrous epidemics in order to give ourselves a change of lifting some hundreds of millions of our fellow humans out of the degradation of poverty.

Finally there is the warning of DeWitt Stetten, Jr., quoted by Simring, "the real hazard is the one no one around this table has dreamed of yet, and this you cannot specify against." This is true. But it is equally true that the real benefit to humanity from recombinant DNA will probably be the one no one has dreamed of. Our ignorance lies equally on both arms of the balance. All that we can say with certainty is that prodigious changes in the conditions of human life must come within the next century if civilization is to survive. The exploitation of recombinant DNA is only one of these changes, and perhaps not the most dangerous nor the least hopeful.

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I want to thank Erwin Chargaff for having written his profoundly important letter to *Science*. He has laid bare, it seems to me, the very mechanism which generates the illusion of technological inevitability that is so much part of the current zeitgeist. First there is a new idea or a new technique. Its announcement is followed by intensive speculation about what could conceivably be accomplished were the idea or technique extrapolated to, or even beyond, its uttermost limits of feasibility. Usually such extrapolations assume that vast domains of science in which our ignorance is virtually total have been or are about to be conquered. Speculatively foreseen "results" are reified by being seriously discussed, for example, as they appear with increasing frequency on agendas of "technology assessment" groups. Once having been given the appearance of very nearly concrete achievements, these "results" are then put forward as justification for societal support—governmental funding, and so on. Among other things, the groundwork for the position that "If we don't do it, someone else will" is thus laid. By then the "it" which actually has no antecedent has been made to seem a natural product of scientific progress, a product that can no longer be warded off. But, apart from the fact that they concern themselves mainly with fairy tales, expert assessors usually see what are fundamentally ethical questions as merely technical problems on which they, the experts themselves, claim to be most competent to have opinions and which they claim they alone can solve. They generally do not ask whether a thing *ought* to be done now or at all, but only how the work, which as a foregone conclusion *must* be done, can be done with reasonable safety. That is how the self-fulfilling nightmare of technological inevitability is born and nurtured.

I speak from experience. In my own field (computer science) there are papers intended to be taken seriously on computer-administered psychotherapy (*1*), on coupling computers directly to living brains (*2*), and on intelligent machines whose "range of [problem-solving ability] will be—in the visible future— coextensive with the range to which the human mind has been applied" (*3*). These wonders, and many more like them, are seen to be as inevitable as the alternation of the seasons. The only question appears to be whether "we" or some others will accomplish them first.

Those of us who speak up against such proposals are often accused of preferring ignorance to knowledge—thus of being anti-intellectual and anti-scientific. What our super-rational critics fail to consider, however, is that all knowledge is purchased at some price and that some prices may be too great to pay no matter what they may buy. I hope no one would argue, for example, that *any* medical knowledge that might be gained would

be worth the price of institutionalizing settings in which experimental brain surgery would be performed on healthy human beings.

Chargaff's letter movingly teaches us once more that all of us have an obligation to consider the price we and future generations may have to pay for whatever knowledge is to be gained by "playing games with 'recombinant DNA.' " Even if one's obligations are not derivatives of some explicit ethical or religious philosophy, one could surely agree that mere prudence legitimates the raising of such questions and that placing them on the agenda is in no way anti-scientific or anti-intellectual. It is a sad commentary on our time that letters like Chargaff's require courage to write and to publish.

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Circadian Rhythms

Crocodilian activity patterns may well be circadian, but the use of the term, based on the data J. W. Lang presents (Reports, 13 Feb., p. 575), is inappropriate. Circadian rhythms are by definition endogenous rhythms with a period of about 24 hours that persist even with the loss of external synchronizers, that is, under constant conditions (*1*). In the absence of such experimental evidence, characterizing the locomotor activity pattern of juvenile *Alligator mississippiensis* as circadian is premature.

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I utilized the term "circadian" as it was originally defined by Halberg (*1*): "Thus, 'circadian' might be applied to all '24-hour' rhythms, whether or not their periods, individually or on the average, are different from 24 hours, longer or shorter, by a few minutes or hours. 'Circadian' thus would apply to rhythms under several conditions. It would describe 1. rhythms that are frequency synchronized with 'acceptable' environmental schedules (24-hour periodic or other) as well as 2. rhythms that are 'free-running' from the local time scale, with periods slightly yet consistently different from 24 hours (e.g., in relatively constant environments)." The usage of "circadian" to characterize various 24-hour rhythms was the subject of considerable debate over a decade ago (*2–4*). Some workers (*2, 4–5*) prefer to restrict the term only to those clock-controlled rhythms that persist under constant conditions.

The periodicity exhibited by certain organisms may damp out within several periods when the organisms are transferred to constant conditions. As Aschoff (*6*) points out, such a result is neither convincing proof against "endogenous" nor for "exogenous" factors. In the experiments under natural conditions described in my report, I demonstrated that movements between land and water shifted gradually into phase with altered light cycles. Phase-shifting under these conditions, I believe, is a clear demonstration of the role of a clock-controlled rhythm in the modulation of the amphibious behavior of juvenile alligators.

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Clinical Lab Standards

Barbara Culliton's article (News and Comment, 7 May, p. 531) on clinical laboratory problems does not reflect *Science*'s reputation for factual reporting and a balanced analysis of issues. By emphasizing only one side of the issue, the article appears to support only massive and expensive federal nonsolutions to the problems. Concerning the need for more federal control, both the Assistant Secretary for Health Theodore Cooper and Administration officials state that there are already adequate laws and authority and indicated to the congressional committees that they have established procedures to handle the problems.

Culliton's article dismisses all of that as "euphemistic" and plunges ahead on its predetermined bias. It claims there are not enough controls and regulations over laboratories. Please note that every laboratory that takes Medicare patients (and they all do) is under federal regulatory control. The 25 states with licensing are those having 75 percent of the population of the United States. All hospital laboratories are already under multiple layers of governmental and professional surveillance.

Culliton's article fails to delineate the two key but separate issues of the whole flap. One is financial fraud; the other is the charge of serious substandard laboratory test results across all of America. They need to be analyzed and treated separately. Stealing and fraud are crimes and are so defined in Medicare, Medicaid, and state laws and regulations. It is curious that it has taken so long to discover these crimes. Culliton's article does not raise one accusing finger to ask about possible criminal negligence in law enforcement and government auditing. Why are more laws needed when the existing ones are not enforced?

To "combat" substandard work, the Clinical Laboratory Improvement Act of 1976 is proposed that will federalize scores of thousands of professionals in these laboratories. This will be very complicated, very expensive in dollars and bureaucratic red tape, and worst of all, it deals with problems already being solved. Culliton's article does not discuss any of the many serious costs and counter-efficiency effects that further government controls will have in this field. The least expensive and only way to really solve the problems is to have the federal and state governments work with the responsible professional societies to strengthen their monitoring and enforcement systems, with each using its already adequate authority and respective expertise.

The reader of *Science*, having heard only one side, can draw only one conclusion. Such a presentation of a single option is below *Science*'s standards.

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