Letters

Research Practices

The article by William J. Broad on fraud and the structure of science (News and Comment, 10 Apr., p. 137) and the subsequent letter from Edith D. Neimark (22 May, p. 873) raise moral questions. I would like to mention other, more common practices.

1) Collaborators or supervisors put their names on manuscripts (and thus assume intellectual responsibility for them) reporting research work which they themselves have not done and which they have discussed inadequately or not at all with the workers who carried it out.

2) Research workers do not submit for publication single experiments or series of experiments which do not fit in with their hypotheses.

3) Scientists fail to do relevant crucial experiments which they themselves have identified, or to which their attention has been drawn.

4) Authors deliberately fail to cite other authors whose work predates or contradicts their own.

5) Referees fail to read sufficiently carefully manuscripts of papers, book, or theses, thus missing findings or desiderata which are crucial to the validity of potential publications.

These widespread practices have a considerably greater impact on knowledge than the relatively rare acts of fraud.

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Human Life

Rosenberg is quoted (News and Comment, 22 May, p. 907) as saying, in his testimony at the Senate hearing, that he knows of "no scientific evidence which bears on the question of when actual human life exists." But, leaving aside the question of what the word *actual* means with its theological overtones, Rosenberg would surely agree that the new *biological* human life begins with the activation of the age at fertilization. The fertilized egg is certainly human, since it belongs to no other species than *Homo sapiens*; it is certainly alive, since it can die (as good a definition of life as most!); and it certainly constitutes a uniquely separate human organism, no longer forming any part of its mother's body and already genetically as distinct from both of its parents as it will ever be, right from the start. It is no less a separate organism because at this stage it may not represent one single individual, being still capable of developing into monozygotic twins: if there are problems here, they are theological rather than biological ones, however.

Presumably, what Rosenberg means is that there is no scientific evidence bearing on the question of the existence of the human *person*, as distinct from biological life. Since only a human can have the status of a person, this is not a problem which arises with the development of other animal species. The biological life of a chimpanzee, for instance, starts with the fertilization of the egg, as it does with a human, and it then regularly develops to maturity and death. It is only with humans that there is this further problem as to whether and when the developing organism begins to exist as a person.

In law, a "person" is a being possessed of human rights and, sometimes, duties; and it is for society, influenced by moral and practical considerations, to define a person in this sense in any way it chooses. The simplest solution, and to many the most logical and ethically satisfying one, is to equate the existence of the "person" with the whole of biological life, starting at fertilization: but that isn't the only possibility—which is what the argument over the rights and wrongs of abortion is all about. One could equally well define the status of a human person (though not human biological life, which is a question upon which the scientific evidence *does* have a bearing) in any other way one fancies, dating it from the implantation of the blastocyst perhaps, or from the "quickening," or viability, or birth, or from the acceptance of the infant by its parents or by the rest of the community; or even from the appearance of self-consciousness and rational thought. Nor is there any reason to stop there, since humans who were slaves, or belonging to particular races or religions, have at times been denied their rights as human persons.

However, if defining a living human organism as a person with human rights is to be delayed until some arbitrarily selected time after its biological life has started, this will involve accepting the existence of a class of humans that are *not* to be recognized as human persons, any more than chimpanzees, for example. There may be pragmatic reasons for doing this—to justify procuring abortions, or the nonvoluntary euthanasia of unwanted children, or of the aged and defective—and there certainly are historical precedents for defining and treating particular classes of humans as "unpersons"—rather unhappy precedents, some of them, at that!

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Rosenberg's statement that there is "no scientific evidence which bears on the question of when actual human life exists" is remarkable. It is the sort of claim one would expect to find in a 19th-century journal of theosophy rather than in a publication devoted to studies in the empirical sciences.

It should not be necessary to point out that human life has a physical dimension to it and that to be human is to be (among other things) a physical being. Granted this is true, then the testimony of the physical sciences is certainly relevant to the question of when human life begins. Just how relevant it is should be apparent to anyone familiar with the claims made so often in the past by proponents of abortion that the fetus is "merely a blob of tissue" or "simply part of the mother's body." The facts presented at the Senate hearings were intended to refute those contentions. This use of physical evidence to aid in judging when a human life begins is consistent also with the widely accepted practice of using physical evidence in judging when a human life has ended. If medical testimony is relevant in the latter case, surely it is applicable in the former.

Implicit in the reliance on physical criteria for determining when a human life exists is a recognition of a human being as someone who (i) shows signs of

physical life, and (ii) has human parents.

The problem with any other set of criteria (for example, social or intellectual awareness), aside from those which are wholly arbitrary, is that they would exclude not only unborn infants but also many children and, quite possibly, a number of handicapped adolescents and adults as well. By some stringent standards only a handful of people could legally qualify as persons. Neither Rosenberg nor any of the authorities he quotes offers a definition of a human being—the only thing they say for certain is that a fetus is not one. . . .

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I found Rosenberg's testimony before Senator East's committee very interesting. His point of view is a novel one for a geneticist and certainly thought-provoking. It would have been a more interesting commentary if the testimony of geneticist LeJeune (University of René Decartes), Gordon (Mayo Clinic), and Matthews-Roth (Harvard University) had also been presented. . . .

The implication for our society in the matters discussed are not trivial. May we scientists have all the evidence.

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Information Management

The News and Comment article by Constance Holden about the dispute between the National Library of Medicine (NLM) and Excerpta Medica (5 June, p. 1125) raises the more general issues of the appropriate role of government agencies in the dissemination of scientific information and the prices that should be charged for information produced by or distributed by the government. Holden indicates that, despite regulations from the Office of Management and Budget (OMB) (*1*), and a judicial decision exempting MEDLARS (Medical Literature Analysis and Retrieval System) information from the Freedom of Information Act (*2*), the policies under which NLM operates its information system are neither clear nor beyond controversy.

Unfortunately, these problems are pervasive. Recent studies by the General Accounting Office (*3*), the Federal Coordinating Committee on Science, Engineering, and Technology (*4*), and others (*5*) have documented the inconsistent application of those policy statements that are extant. This lack of rational and consistent policy and practice affects users of the Educational Resources Information Center, the National Cartographic Information Center, the Landsat program, and the National Technical Information Service—to name but a few of the government organizations that provide information to the scientific community.

If we, as a nation, are to be able to utilize scientific information efficiently, we may well need a complete overhaul of Title 44 (which establishes the Government Printing Office and its relationship to the Joint Committee on Printing), as well as the inconsistent and inappropriate OMB guidelines.

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References

1. *User Charges* (Circular A-25, Office of Management and Budget, Washington, D.C., 1974); *Policies for Acquiring Commercial or Industrial Products and Services for Government Use* (Circular A-76, Office of Management and Budget, Washington, D.C., 1978); 31 *U.S. Code* Section 483a.
2. S.D.C. Development Corp. *v.* Matthews, 542 Fed. Rep. 2nd Suppl. 1116 (9th Cir. Ct.) (1976).
3. *Better Information Management Policies Needed: A Study of Scientific and Technical Bibliographic Services* (Report PSAD-79-62, General Accounting Office, Washington, D.C., 1979).
4. Federal Coordinating Committee on Science, Engineering, and Technology, *Report of the Working Group on Private Sector/Governmental Relationships for Scientific and Technical Information* (National Science Foundation, Washington, D.C., 1980).
5. D. King and N. Roderer, *Report on Pricing of NCIC Information Products and Services* (King Research, Rockville, Md., report for U.S. Geological Survey, Washington, D.C., 1978).

Wine Ranking

Wyatt (Letters, 12 June, p. 1212) suggests that differential light-scattering (DLS) patterns are predictive of wine quality. It would have been more convincing had an a priori prediction of quality been compared to the test panel's judgment. I therefore ranked the wines according to my own interpretation of the DLS patterns (Wyatt's figures 1 and 2) and compared these rankings to those of Wyatt's test panel. In addition, I compared the panel's ranking to a ranking based on cost.

Table 1. Wine rankings.*

Identification†	DLS				Cost rank
	Smoothness	Slope	Total	Rank	
G	1	1	2	1	1
F	3	3	6	2	2
C	7	2	9	4.5	4
A	4	6	10	6	5.5
B	2	5	7	3	5.5
E	5	4	9	4.5	3
D	6	7	13	7	7

*Data taken from or reinterpreted from Wyatt. †In order of preference by panel.

My wine-ranking scheme considered both slope and smoothness of the DLS curve for each wine. I ranked the wines separately for each parameter, summed the two scores, and ranked the result (Table 1). The Spearman rank correlation coefficient, r_s (*2*), was significant ($r_s = .78$, $P < .05$) for the DLS-panel comparison, indicating that DLS patterns were good predictors of wine quality. However, the cost-panel relationship was equally good ($r_s = .78$, $P < .05$). Thus, the wine consumer would be as likely to make a satisfactory choice based on cost as one based on DLS pattern.

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References

1. S. Siegel, *Nonparametric Statistics for the Behavioral Sciences* (McGraw-Hill, New York, 1956), pp. 202–213.

Wouldn't it be wonderful if wineries followed Wyatt's suggestion and recorded a wine's differential light-scattering (DLS) pattern, rating, or "parameterization thereof" as an aid to the enophilic consumer. Then we could all select wines which are gloriously indistinguishable. How wines are "polished" before release certainly affects their clarity, but aging, nature's time-honored way to remove the "noisy" particulates, also imparts to the wine the subtle nuances we have come to associate with the gallo- and ellagitannins and other sensory-associated highlights. This is a slow process, and if one notes that even the smoothest DLS pattern (wine G, $12) was associated with a wine judged by the taste panel as "young," it would appear that a single DLS rating may be of as little value to the consumer as Wyatt's attempt to try and find Pinot Noir worth loving before its time.

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Erratum: In an article titled "How safe is Bendectin?" (News and Comment, 31 Oct. 1980, p. 518), it was reported that Richard W. Smithells conducted a Bendectin study of 2000 women and 11,000 controls. The correct number of women in the control population is 1100.