LETTERS

The Science Court Experiment

Earl Callen's letter (10 Sept., p. 951) expresses his fears about the potential for the science court to become an authoritarian instrument, stifling the ability of scientists to speak out on public policy matters. His views may be shared by many scientists. It is important that wide public debate be held on the science court concept so that this and other possible arguments against the court can be fully aired. The public session on the science court at the April meeting of the American Physical Society was a start in this direction. The Colloquium on the Science Court held from 19 to 21 September 1976 at Leesburg, Virginia, was another step.

As a member of the task force that has been developing the science court idea, I have, as Callen says, taken the position that the court should be regarded more as a set of procedures to be used as needed than as a continuing institution with a life of its own. It is my impression that this view is not uniquely mine among the members of the task force and, indeed, it is quite consistent with the discussion of the science court presented by the task force in the article "The Science Court experiment: An interim report" (20 Aug., p. 653).

It is incorrect to suggest, as Callen does, that my views are the basis of a different plan for a science court that is being considered by the Consumer Product Safety Commission. First of all, the only plan that I am aware of is the one being developed by the task force. Second, the Commission has not formally discussed the science court concept, nor has it considered any specific plan. I believe the science court has merit and that it could, if properly developed, be useful to the Consumer Product Safety Commission as well as other government regulatory bodies. However, I am only one of five Commissioners, and the question of use of a science court has yet to be addressed by the full Commission.

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Answers to the points made in Callen's letter are to be found in the Interim Report published in the 20 August issue of *Science*.

The public discussion that Callen calls for we hope has been stimulated by the Colloquium on the Science Court held at Leesburg, Virginia.

Callen talks about the science court issuing statements of "scientific Truth." The first page of the Interim Report says, "We have no illusions that this procedure will arrive at the truth, which is elusive and tends to change from year to year."

Callen asks "which facts" will be dealt with by the court. The procedure for selecting the facts to be dealt with is specifically discussed in the Interim Report, and a procedure has been suggested in which the case managers for either side propose the factual statements which will be considered by the science court. Thus, both sides will have full opportunity to bring forth those relevant facts they consider important.

Callen makes the broad statement that "In social policy questions it is impossible to separate facts from values." This is, of course, a question which has been debated by philosophers for centuries. We don't propose to enter into that debate, but simply to avoid issues where the distinction cannot be made.

Finally, Callen announces that "The science court will stifle public debate." The Interim Report points out that the process will be conducted entirely in public, and the only authority that will attach to its results will arise out of the credibility the public assigns to its procedures.

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Wald and the Cambridge City Council

Barbara Culliton's article in the 23 July issue of *Science* (News and Comment, p. 300) on the Cambridge City Council's involvement with recombinant DNA contains a small factual error in saying, "Wald went to see Mayor Vellucci, whom he persuaded that the potential threat of P3 recombinant experiments to the public health is a very real one." Mayor Vellucci needed no persuading. He had several days earlier put this matter on the docket of the next City Council meeting, on the strength of an article on genetic recombination in the Boston *Phoenix* of 7 June.

This is not an apology; but I do not want to be given unjustified credit for an event I did not bring about.

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Heritability of IQ

Feldman and Lewontin (19 Dec. 1975, p. 1163) make numerous references to my writings on the heritability of intelligence, often in ways that are so incomplete as to be inaccurate or misleading. Serious readers may find this out for themselves, since I have written in some detail on the various criticisms of the heritability concept as related to mental measurements mentioned by Feldman and Lewontin: the meaning of heritability in the behavioral sciences and the question of genotype-environment interaction (*1*), genotype-environment covariance (*2*), the heritability of differences between groups (*1, 3*), and the broader educational and social implications of the substantial heritability of IQ (*4*).

On one fundamental theoretical point on which I have not previously written in any detail, however, Feldman and Lewontin draw an unwarranted conclusion. They state that ". . . as selection progresses, the additive genetic variance is 'used up' so that the h^2_N [the narrow heritability or proportion of additive genetic variance] is decreased finally to zero, or nearly so. A consequence of these theorems is that, if natural selection has long been in operation on a character, the additive genetic variance for the character should be small, and the only genetic variance left should be nonadditive (dominance and epistatic variance). Thus we may be able to judge, from the ratio of h^2_N, which goes to zero during evolution, to h^2_B [the broad heritability or the proportion of the total phenotypic variance due to all genetic factors, additive and nonadditive], which does not, how much selection has gone on." They then argue that the difference between the empirical estimates of 0.75 and 0.40 for the h^2_B and h^2_N, respectively, of IQ, forces the conclusions that ". . . whatever it is that IQ measures, it has not been under intense selection for very long. Conversely, if there is a great deal of nonadditive genetic variance, but very little additive, we may guess at a long and consistent history of selection." These are weak inferences in the absence of knowledge about selection intensities, as Feldman and Lewontin rightly point out.

The one reasonable inference that can be drawn from the present evidence is that the intelligence measured by IQ is a fitness character—*the genes involved in IQ variance have undergone selection in the course of human evolution*.

But theoretically we are not justified in concluding, as do Feldman and Lewontin, that whatever it is that IQ measures