Correspondence

SCIENCE AND TRANS-SCIENCE

28 February, 1972

Sir,—I find Dr. Weinberg's distinction between science and trans-science a useful one for discussing political controversies having scientific or technical aspects.[1] However, I think the concept of trans-science would be clearer if a further distinction were made between questions which are inherently unanswerable by the institutional mechanisms of science and those which are currently unanswerable because of limitations in the contemporary state of scientific knowledge. In his examples Dr. Weinberg mixes together these two distinct kinds of trans-science.

For example, he discusses the problem of determining the effect of 150 millirems of radiation on the mutation rate of mice. The question is unanswerable only because of inadequate scientific understanding of the physico-chemical mechanisms of radiation damage to genetic material. If the theory of such damage were as well developed as, for example, the theory of γ-ray or electron damage to pure monocrystalline silicon used in solid state electronic devices, there would be no need for large experiments on billions of genetically identical individuals. The dependence of mutation rates on intensity of radiation could be predicted as a function of environmental conditions on the basis of a thoroughly understood physical mechanism, and could thus be extrapolated to indefinitely low dose rates.

On the other hand, the probability of extremely unlikely events such as nuclear reactor accidents is almost certainly a trans-scientific issue in principle. It depends upon small random events and human errors which are inherently unpredictable, and there is a definite limit to the amount by which these uncertainties can be reduced by further scientific understanding of the component elements of the problem.

Dr. Weinberg's example of the "axiology of science" is probably also to a large extent inherently "trans-scientific", although some aspects of what Dr. Weinberg calls "scientific value" could be subjected to empirical test, and are therefore scientific rather than trans-scientific. Thus detailed retrospective studies of scientific discoveries and the circumstances under which they were made throw light on the relative importance and effectiveness of various strategies for the pursuit of science. Case histories of the origins of significant applications of science can similarly throw light on the effectiveness of "basic" and "applied" decision trees in the planning of research aimed at application. While there will always be a considerable element of subjective interpretation in any conclusions drawn from such case material, I think it is a serious exaggeration to say that questions of scientific value are purely relative and matters of taste or aesthetics. Such judgements or interpretations are codifications of several hundreds of years of experience in scientific research, and are subject in many cases to systematic empirical test. The sociology of science is a relatively undeveloped field, and so reliable generalisations, or theory based on systematic empirical observation, are rare. But the obstacles to better theory can be removed by intellectual effort which is in the scientific, not the trans-scientific, way of proceeding.

[1] Weinberg, Alvin M., "Science and Trans-Science", *Minerva*, X, 2 (April, 1972), pp. 209–222.

In the example of the debate on the supersonic transport (SST), my distinction between what is inherently trans-scientific and what is only temporarily so because of the current state of science can be brought out clearly. The effect of nitrogen oxide exhaust discharges in the lower stratosphere on the thickness and configuration of the ozone layer is a question which is in principle clearly answerable by the traditional procedures of science, though not within the time required by the particular political circumstances which surrounded the debate in the United States Senate. If more basic research on upper atmospheric chemistry and stratospheric circulation had been carried out during the preceding decade, the question might have been answerable even at the time of the debate with sufficient confidence and accuracy to make possible a more definitive conclusion about the health hazard raised by the opponents of the SST. On the other hand, I would certainly agree with Dr. Weinberg that the decision whether then to proceed with the development of two prototypes involved issues which were inherently trans-scientific. There had to be a balancing of conflicting social values which could not be resolved by scientific procedures. The quantitative issue of which values would be affected and to what extent they would be affected by various alternative courses of action could not be resolved by the available scientific evidence and economic analysis. This, of course, made for confusion in the debate, but in the last analysis, the debate dealt with trans-scientific questions.

With respect to the social sciences and the problem of predictions which can be made only on a statistical basis for populations or aggregates, it is important to recognise that there are situations in the physical sciences which are similar to those discerned by Dr. Weinberg in the social sciences. The phenomena which involve turbulence in fluid mechanics are of this nature. Even though the laws of mechanics are deterministic in principle, the boundary or initial conditions in turbulent flow cannot be specified so as to make possible other than statistical predictions. Numerical studies of weather forecasting, for example, have shown that after about 15 days numerical models lead to predictions in which the variance approaches that of the random fluctuations about the climatic average. The basic reason for this is that fluctuations on a scale smaller than that included in the model eventually produce effects on a larger scale which prevent predictability. Since turbulent fluctuations of various scales are coupled through the non-linearity of the problem down to an indefinitely small scale, the smallest scale fluctuations will eventually destroy the determinateness of any numerical model. As a particular application we note that it is possible to forecast the occurrence and probable severity of thunderstorms or tornadoes over a large region, but the exact paths of such storms, their points of origin and the courses of their subsequent development and decay cannot be forecast, even in principle. Again the problem is that these phenomena result from events on an indefinitely small scale.

In each of the examples cited, as in social phenomena, we have a situation where indefinitely small-scale causes produce large-scale effects. Although the problem might be regarded as microscopically determinate, in practice the initial conditions are more complex than the effects to be forecast, so that any properties of macroscopic interest can only be forecast with a certain probability, which eventually becomes random after the lapse of sufficient time. Such phenomena are for practical purposes not subject to causal interpretation, and they are as common in the physical sciences as in the social sciences.

Dr. Weinberg discusses at some length the value and limitations of adversary procedures in resolving scientific or trans-scientific issues. He concludes that the institutional mechanisms of science—criticism and consensus—work best for scientific questions, while adversary procedures may be useful in dealing with trans-scientific issues, at least under some circumstances. There seems little to add to his thorough and perceptive discussion. Adversary procedures may be especially valuable in bringing out unanalysed evaluative assumptions or premises which underlie the testimony of experts when they deal with trans-scientific issues. Adversary procedures are also important in circumstances when greater and greater economic, political or professional commitments have been made to a particular line of action, and when such commitments are to be either greatly scaled up or down as a result of a policy decision based on scientific and trans-scientific testimony. In such situations, of which the SST is a very good example, the social costs of a major change in the scale of commitment may become as severe as the alleged environmental side-effects of the project itself. Thus at some point the effects on employment and on other features of the economy of terminating the SST, or of an economically unsuccessful SST, would exceed the environmental damage which might be produced were the project eventually to reach full-scale commercial application. As technological undertakings increase in scale, the scale of commitment will become an increasingly frequent issue in public debate. This argues for keeping alternatives of choice open as long as possible and for delaying decisions which involve large expenditures until the last possible moment in order to accumulate as much understanding and consensus as possible on scientific questions which might bear on the final decision. It also suggests that, if we are to continue to undertake technological innovations on a large scale, it is important that the proportion of expenditures on basic and applied research be increased in relation to expenditures on development, in order to make our decisions as informed as possible, to increase the scientific bases of decisions and to reduce those which are trans-scientific.

Yours faithfully,

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THE DILUTION OF ACADEMIC POWER IN CANADA

4 March, 1972

Sir,—As a graduate (B.A. and M.A.) of the University of Toronto of mid-1940s vintage, and a teacher there since on three widely separated occasions, most recently in 1967 when the agitation over the government of the university was in its first full flush, I found the account by Professor Murray Ross [1] of the historical evolution of the new act and its main provisions fascinating, and his grounds for alarm at the possible and even probable results broadly sympathetic. I too would be greatly worried by the dangers posed by increased government and student participation in university management, decentralisation and the parity principle to the maintenance of academic standards. I would, however, argue two points on the other side.

First, as explained at length by Professor Buchanan and Dr. Devletoglou

[1] Ross, Murray G., "The Dilution of Academic Power in Canada: The University of Toronto Act", *Minerva*, X, 2 (April, 1972), pp. 242–258.