always by coincidence—if the biases of the questioner agree with those of the scientist.

But the careful scientist needn't fall for this. How many scientists, on the spot before Congress or on television, will come out and say they are not competent to speak on a point, or will draw a line between their own expert and their own lay opinion? Sometimes yes, but rarely enough to make the questioner suspect a desire not to answer the question rather than a desire to be simon-pure. The ego of the scientist is here apparent, and he will repeatedly speak with equal conviction on points on which he is expert and on points on which he is not even reasonably well informed.

It seems to me that it is not to be expected that a simple answer to these communications problems will be forthcoming. Maybe, in fact, both the Government and the press will have to face up to the fact that scientists are people. I guess the best known method on earth for getting at real "communicative accuracy," as Weaver speaks of it, is by taking advantage of the always existing interplay and conflict of personalities—that is to say, biases. Lawyers cross-examine. In the [Congressional] hearings last year on "The nature of radioactive fallout and its effects on man," the discussion seminar, with panels of scientists of not too coincident views, was used to advantage. What was brought out by these seminars perhaps was of no importance to science, but it was important from the standpoint of what scientists had to say about fallout. Scientists, facing each other across the table, literally or figuratively, are going to be more careful about "communicative accuracy." Reputations are at stake.

Similarly, the Government and the press are going to have to go much farther toward obtaining sufficient basic technical competence to avoid being "snowed" by overzealous, or just plain erroneous, scientific statement. I myself do not feel that obtaining this competence is impossible or even necessarily very difficult, once the need is recognized. Clear thinking and understanding of basic scientific facts, principles, and philosophy will go a long way toward giving this competence.

Not to go in this direction means continued reliance, without understanding, on the statements and judgments of scientists. For both the press and the Government, such reliance is often placed on a very few individuals of high reputation. (Government secrecy in excess has, as one of its more insidious effects, the furthering of this tendency to lean in one direction; either the facts can't be brought out publicly or else only one "side" knows them.) It seems to me that for either press or Government to lean heavily on one, or a few, scientists can be exceedingly dangerous.

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Fluorescence of Ethylenediamine Derivatives of Epinephrine and Norepinephrine

A. de T. Valk, Jr., and H. L. Price [*J. Clin. Invest.* **35**, 837 (1956)] and more recently G. F. Mangan, Jr., and J. W. Mason [*Science* **126**, 562 (1957)] reported the observation that a greater fluorescence is obtained when the condensation of ethylenediamine with epinephrine [H. Weil-Malherbe and A. D. Bone, *Biochem. J.*, **51**, 311 (1952)] is performed in a solution containing acetic acid which had been passed over a column of alumina than when it is performed in water, or in acetic acid not thus treated. According to Valk and Price this difference amounts to 100 to 150 percent; Mangan and Mason found a difference of only 30 percent.

This phenomenon, of course, would not have escaped our notice if it had occurred under our conditions. Checks on the adsorbant have been and still are regularly carried out in our laboratory; the recoveries of epinephrine, after adsorption on a column of alumina, vary between 80 and 100 percent, but they never exceed 100 percent. We repeatedly and unsuccessfully attempted to observe the effect by adding epinephrine to dilute acetic acid which had been filtered through a column of alumina. We also added aluminium ions to epinephrine solutions; these did not affect the fluorescence of the ethylenediamine-condensation product up to concentrations of $10^{-4}M$. At higher concentrations they reduced the intensity of fluorescence.

Both Valk and Price and Mangan and Mason used the Farrand fluorimeter, which differs from our fluorimeter in that test tubes are used instead of rectangular cuvettes and the fluorescent solution is exposed to focused exciting light instead of parallel exciting light. We therefore measured the fluorescence of the condensation product, prepared in the presence and absence of alumina-treated acetic acid, under conditions approaching those obtaining in the Farrand fluorimeter. Again, no difference was found.

Neither Valk and Price nor Mangan and Mason made any attempt to offer a rational explanation for their observation. In view of our failure to confirm it, the burden of finding its cause rests with them.

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