Letters

Initial Modesty

Authors "who believe that the use of *I* or *we* is immodest" were gently chided for their false modesty in the lead editorial, "Passive voice," in *Science* for 22 March 1957 [**125**, 529 (1957)]. But authors who prefer not to write in the first person are much more honest than those, like the author of the editorial in question, who write in the first person without signing their names.

An editorial, report, or article whose author is identified only by initials has an anomalous status. Does it represent the view or opinion of the editorial board, as competently expressed by one member? Or does the use of initials imply a disclaimer by the board as a whole, so that only the owner of the initials is to be held responsible?

If cryptic initials are used only for internal identification, they should be much less obtrusive—perhaps in 6-point type, or in the form of a code number or of initials run together without periods, even in reverse order. As used, full size, in *Science*, initials usually can be matched up with one of the names in the masthead, so they offer little anonymity. The editorial in question was signed "R.V.O." —presumably Robert V. Ormes, a member of the editorial staff. Was Ormes so ashamed of his editorial that he did not wish his full name attached to it?

(Perhaps he should be ashamed. For an essay on grammatical purity, its own purity leaves something to be desired. I was particularly set on edge by the sentence: "In the editorial office we still see gerunds and participles used in this manner, and it is discouraging." Is the manner discouraging?)

In addition to making an unclaimed orphan out of an editorial or note, the use of initials, rather than an honest name, imposes an unwarranted burden on the poor bibliographer. Forever more, this editorial must be listed as: "R(obert?) V. O(rmes?): Passive Voice," or "R.V.O. (Robert V. Ormes?): Passive Voice." Bibliographers should not be forced to pay thus for an unsure author's false modesty.

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Radiation Hazards

In the article entitled, "Mice, men, and fallout" [*Science* **128**, 637 (1958)], M. P. Finkel presents some interesting results regarding the effects of low doses of Sr^{90} on mammalian life expectancy and incidence of certain tumors. However, in discussing these results, the author draws far-reaching conclusions relating to the danger to man (or rather lack of it) from present Sr^{90} fallout. The concluding sentence states, "the present contamination with strontium-90 from fallout is . . . extremely unlikely to induce even one bone tumor or one case of leukemia." I would like to raise the following points with regard to this conclusion.

1) In the data presented, the uncertainty was such that a 7-percent shortening of life span in an experimental group did not represent a statistically significant deviation from the control, nor did a threefold increase in the incidence of osteogenic sarcomas. Yet the above statement refers to effects on the world's population which would amount to a small fraction of 1 percent.

2) No statistically justifiable extrapolation for determination of "threshold doses," or even demonstration that there is a threshold different from zero, seems possible from the data as presented. In fact, these data appear to indicate that the experimental design used is inadequate for this purpose.

3) At the present time, sober and accurate evaluations of the effects of chronic low-level irradiation of human populations, from internal and external radioisotopes, are essential for the formulation of safe and wise national and international policies regarding the testing of nuclear weapons and the development of nuclear power. It is unfortunate that at this time a statement such as that quoted above is published with the implication that it is based on experimental evidence, when actually it appears to be without objective, logical support. Unfounded statements *minimizing* radiation hazards can be especially harmful if they turn out later to have been false.

A more appropriate conclusion from the data would seem to be that drawn by Austin M. Brues from a discussion of other data relating to carcinogenesis [*Science* **128**, 693 (1958)]—namely, that a linear dose-effect relation is less probable than a nonlinear relation, and that a threshold *might* occur.

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I should like to comment on the article by Miriam P. Finkel. First of all, it is difficult to tell whether this article should be considered as an editorial or as a strictly scientific paper. If the latter is the case, I should like to strenuously object to the opening paragraph, which in a back-handed kind of way casts disrepute on some of the most eminent scientists of our time who have been concerned with the effects of fallout on human beings.

Aside from this, I particularly wish to criticize some of the scientific conclusions. The type of effect that one is looking for with respect to the action of fallout on man is such that it has been predicted that several tens of thousands