COMMENTS
by Readers

One wishes that every scientist and engineer might read the memorandum which accompanied President Truman's veto of the recent National Science Bill, S.526, and the more recent pamphlet, *Science and public policy*, by John R. Steelman, chairman of the President's Scientific Research Board.

From the first comes an appreciation of the complications that necessarily inhere in attempting, as in S.1850 and again in S.526, to make a single organization responsible at once for the allocation of funds to support pure science and for the direction of military research.

The latter makes it abundantly clear that in the rapid growth of science the politician sees ample justification for giving it his increasingly close attention so that it may be properly assimilated into the national life. Repeatedly the report stresses the concept of planning and guiding research effort so as to coordinate it more closely with the scientific programs of the Federal bureaus and with military research as well as with the industrial needs of the Nation (see, e.g., passages on pp. 5, 6, 9, 26, 31, 34, and 61).

Thus, the combined effect of the veto memorandum and the Steelman report is most revealing.

Doubtless, the President stands on firm ground when he demands executive control of any government agency engaged in military research and development. This alone would have justified his veto, but he implies very pointedly that he vetoed S.526 because it failed to give him the broad control over scientific effort which he believes he should have. This is the minimum political price which must be paid if the foundation is created to discharge the dual purpose.

So why strive longer to domicile an antelope and a bear in the same pen? Why create the national fiction that the interests of pure science require intimate association with military research, or vice versa? I wish to imply nothing for or against the hypothecated need of a central organization for military research and development after the pattern of OSRD. This may be very desirable; but in proportion as it is desirable, it is certainly undesirable that it be yoked to the most international of all undertakings. To do this seems as confusing an approach to world relationships, scientific and otherwise, as it is an unpromising approach to the problem of supporting science here at home.

Given a clean-cut separation of these two functions that envisages a science foundation whose sole purpose is to allot funds for pure science, this foundation could receive Federal aid though it were quite unattached to the executive arm of the Government. Congress can and does continually appropriate money to organizations over which the President has no control.

Adopting such an arrangement for science, the resulting foundation might have a wholly private status, or it might be quasi-governmental after the pattern of the National Academy of Sciences. Some will recall that the Willis Bill before the 79th Congress proposed this latter arrangement. Its sponsors recognized the basic undesirability of associating military research with the allocation of considerable sums for the support of pure science, and feared also the additional threats to scientific freedom which must lie in any foundation subservient to the executive arm of the Government. But what in the minds of some of us is equally important, an extra-governmental agency closes no door upon whatever personal and corporation giving may again be stimulated, either by modification of income tax credits or otherwise.

We do not, however, regard the road leading to such an agency as strewn with violets. American science is to date inexperienced in the centralized partitioning and distribution of its lifeblood. The veto memorandum, in effect, declares that the problem cannot be solved without the intervention of political personnle and political authority; and though this is, perhaps, its one unhappy note, all must admit that the President's apprehension offers a constructive warning. He points out that members of the foundation, as envisaged in S.526, would be employees of institutions or organizations eligible for grants, and that out of the resulting conflict of interests and responsibilities, and regardless of their complete integrity, the members might be suspected of favoritism. But the course he proposes of taking control away from the scientific members and giving it to political appointees will be regarded by many as epitomizing the dangers most to be feared. Fortunately, alternatives are at hand.

There is, for example, the representative type of institution, operated within the compass of science itself. After all, broad representation is the traditional American way and protects to a degree which is probably unattainable otherwise.

This solution might be approached through a sort of constitutional convention, whose deliberations so far as American science is concerned would promise to rank in significance with those epochal sessions held in Philadelphia a century and a half ago. The aim of this science convention would be to formulate and agree upon a representative central fiscal agency. This would be embodied in a constitution and a system of elections for officers and members such as to merit the approval of both the scientific fraternity and leaders of public opinion. As to the desired safeguards and flexibility of such an agency, these have been emphasized so often in the course of the debates on science legislation that they need only be summarized in a word or two here. Chiefly, they would insure that the allocations of research money would be widespread geographically, they would insure the recognition of scientific merit and promise wherever these might arise, and they would shoulder few, if any, encumbrances upon the recipient. (ROBERT W. KING, *463 West Street, New York City.*)

The present communication deals with almost forgotten material on the dental aspects of prenatal injury. Preparation of a bibliography on the effects of German measles during pregnancy recalled to the writers a concept, commonly taught in dental schools in this country, to the effect that acute infectious diseases in the mother during pregnancy will cause congenital anomalies of

the deciduous dentition in the infant, if these diseases occur during the time when the teeth are forming. It is also understood that these same diseases in infants and young children, occurring during the period when the permanent teeth are forming, will similarly affect these teeth.

A partial study of the available dental literature on this subject has revealed an interesting discussion which includes the presentation of a case similar to those found by Evans (*Med. J. Aust.*, 1944, **2**, 225). This was reported in 1893 by J. J. R. Patrick as follows (*Dent. Rev.*, 1893, **7**, 439):

"I have entered thus far into a description of the origin and development of the teeth, in order to illustrate the detrimental influence of eruptive fever on the teeth during the periods of their formation; and while eruptive fevers are not the only factors to be recognized in producing abnormal conditions in tooth development, I consider such fevers a prolific source of faulty nutrition in these organs, as the following cases observed in my practice will fully exemplify.

"Mrs. S., during the fifth month of gestation was afflicted with scarlet fever—recovering, in the course of time gave birth to a child. This child's teeth appeared unusually early, the enamel discolored, pitted and granular and absent at the incisive margins, and the teeth barely extended beyond the gum. He is now fifteen years old with all his permanent teeth except the third molars. These second teeth, with the exception of a little decay, are in good condition, the enamel smooth and white and all the teeth large and strong.

"Three years later another child was born, and soon afterward the first child contracted the measles and the second child also contracted the disease. In the course of time the deciduous teeth of the second child appeared, all in excellent condition, but were lost unusually early, and the second set appeared in the following order: The sixth year molars at four years. The central incisor at five years. The premolars between the sixth and seventh years; and the cuspids between the eighth and ninth years. All these teeth are discolored, pitted and granular, and the enamel is worthless as a protection to the dentine."

Later in the same year Dr. Patrick attended the World's Columbian Dental Congress at Chicago, where a paper on the pathology of congenital defects of the deciduous dentition was presented by Otto Zsigmondy. During the discussion of this paper Dr. Patrick made the following remark (*Trans. World's Columbian dent. Congr.*, 1894, **1**, 65):

"All eruptive diseases during gestation must necessarily affect the enamel of the deciduous teeth if these eruptive diseases take place during the formative process of the enamel. How could it be otherwise?"

The quotations given above not only show that Dr. Patrick had developed a clear concept of the role of acute disease of the mother in the causation of congenital defects of the infant in the field of dental development, but also point out two factors which are of importance in the current efforts to clarify the medical aspects of this problem. The teeth are relatively permanent structures, and their embryology has been worked out in considerable detail. It is therefore apparent that the presence or absence of defects in the deciduous teeth will be of great importance in the evaluation of the effects of various disturbances of pregnancy on the fetus during the period of tooth formation. A study of the simultaneous occurrence of dental defects and malformations in other organs will also be of considerable interest.

Dr. Patrick's second case points out that the problem of developmental anomalies is not limited to the period of gestation, but also extends for some time beyond the birth of the child. This postnatal aspect of the problem deserves more study from a medical point of view. The importance of the presence or absence of dental defects, especially in conjunction with defects in other parts of the body, is again apparent. (R. J. M. HORTON, *Harvard School of Public Health*, and J. M. DUNNING, *Harvard School of Dental Medicine.*)

To express in a mathematical formula a complicated physiological response such as that involved in body-temperature reactions of cattle to climate, the fundamental requirement is that it be based on as complete an accumulation of facts as possible. To modify a formula that satisfies this condition by introducing a factor based on an assumption unsupported by experimental evidence is to confuse rather than to advance the knowledge of the subject.

In a recent paper R. F. Gaalaas (*J. dairy Sci.*, 1947, **30**, 79–85) has modified the formula

$$HT = 100 \div [10(BT - 101.0)]$$

originated by Rhoad (*J. animal Sci.*, 1942, **1**, 85) to

$$HT = 100 - [14(BT - 101.0)].$$

In the Rhoad formula the factor 10 is used merely to remove the decimal from the result within the parentheses. In this manner a whole-number coefficient in a scale of 100 is obtained, with 100 equal to perfect efficiency in maintaining a normal body temperature of 101.0°F., under prescribed rules of a test procedure (A. O. Rhoad. *Trop. Agric.*, 1944, **21**, 162–164).

The factor 10 was changed by Gaalaas (p. 80) "based on the assumption that a body temperature of 108°+ at 90°F. air temperature and under otherwise normal conditions would indicate a complete loss of control in the regulation of body temperature, or zero per cent efficiency in eliminating surplus body heat. Conversely, a body temperature of 101°F. is considered normal and a cow that would maintain that body temperature at 90°F. air temperature would be considered 100 per cent efficient. . . ."

The reader is led to believe that the factor 14 was derived by dividing the 7° range between 101 and 108 into 100 equal parts, thereby placing a value of 0.14285716 . . . ∞ on each tenth of a degree on the clinical thermometer.

In correspondence, Gaalaas states that the factor 14 is a correction factor necessary because his cows had "access to shade instead of requiring them to be held in the sun. . . . The factor 14 is enough larger to correct for the observed average difference in body temperatures." A check on the results published earlier by Gaalaas (*J. dairy Sci.*, 1945, **28**, 555–563) gives credence to this interpretation of the origin of the factor 14 and is acceptable as a correction factor, as it is properly supported by satisfactory data.

One wonders, therefore, why an unsupported assumption is given as the base for changing the factor 10 to 14 in the original formula! (ALBERT O. RHOAD, *Inter-American Institute of Agricultural Sciences, Turrialba, Costa Rica.*)