more illuminating and valuable had it been prefaced by the following statement of facts.

About 20 years ago a heating engineer told me of his winning $2 by betting that a pint of boiling water would freeze sooner than a pint of ordinary drinking water if both pints in similar metal vessels were placed outdoors in zero weather. He had successfully tried this experiment several times before and so was guilty of betting on a certainty. Since that phenomenon seemed to me mysterious I tested it at our physics laboratory and got the same result. The mystery disappeared when I weighed the ice and water in the vessel originally hot and found how much less it weighed than the water in the other vessel. The difference was amazing. This will answer the question asked in the last sentence of Professor Sanford's letter. And in the first sentence of his letter he strangely attributes to me a statement entirely different from the one I made.

The experiment which Professor Lyon performed when a schoolboy disgusted his elders, he says, but they erred in concluding that their experience gained through many years was rendered valueless by a single solitary experiment performed by a youngster. His elders knew that when hot-water and cold-water pipes were near each other the hot-water pipes were generally the first to freeze, but they were unaware of the influence of the air contained in the cold water. In experiments like the one I performed Professor Lyon rightly assigns to evaporation the dominant role. In the experiment which disappointed him he used hot water of unstated temperature. My water was 100° hot.

Professor Wakeham and I apparently disagree only in that while he thinks that both the ancients and Roger Bacon were guilty of generalizing I have too much respect for the intelligence of the ancients to believe them capable of teaching that, although boiling water freezes sooner than an equal weight of lukewarm water, the same phenomenon would be observed in case the colder water was only a few degrees above the freezing point. Professor Wakeham's experiments are of great interest and value, and would be of still greater value had he been able to specify not only the minutes but also the approximate number of seconds in each of his observations. His experiment in which the boiling water at 93.3° and the 20-degree water froze in equal times harmonizes entirely with my experiment in which the 100-degree water froze first. For if Professor Wakeham had been able to start with water boiling at 93.3° and an equal weight of 100-degree water, he would have found that when both masses had come to the same temperature, near the freezing point there would have been considerably less of the 100-degree water than of the other, for in that vessel many more calories had been spent in causing vaporization. Of two unequal masses at the same temperature the smaller freezes first, and consequently Professor Wakeham would have observed the 100-degree water freezing before the 20-degree water just as I did. The 93.3-degree water wasn't quite hot enough to do the trick.

The 2-minute margin of victory of the 10-degree water over the 93.3-degree would have been considerably narrowed, perhaps obliterated, had 100-degree water been at Professor Wakeham's disposal; but a victory should surprise no one, as will appear from the following considerations. As long ago as 1889 Professor Tyndall wrote, "This halt in contraction of the approaching molecules at the temperature of 39° F. (about 4° C.) is but a preparation for the subsequent act of crystalization." Further, it should be kept in mind that water is highly polymerized, a compound of H_2O, $2H_2O$ and $3H_2O$ and that the ratio of these polymers changes with the changes in temperature. According to Rao (1933) 59 per cent. of ice at 0° is composed of $3H_2O$. Also many ice crystals exist in water from 0° to 10° and beyond. With all these advantages it is not surprising that 10-degree water freezes sooner than water boiling at 93.3°.

From the above facts it is clear that not only was the writer of the book from which I quoted ill-advised in denouncing as "drivel" the findings of the ancients, but also that, even ignoring considerations of courtesy in saying "the ancient author was a liar," Roger Bacon was mistaken.

Joseph O. Thompson

Amherst College

A COUNTER-STATEMENT

The statement that follows has been approved by those whose names appear as signers.

In Science for May 3, 1940, there appears an appeal for signatures to a peace manifesto sponsored by the American Association of Scientific Workers. It seems desirable to put on record the fact that this statement does not represent the unanimous opinion of American scientific workers.

One may question whether the manifesto will represent the considered opinion of all its signers, for a casual reading of it might easily fail to disclose its real implications.

It is a platitude that all right-thinking people are, in general, in favor of peace—and the fact is not worth troubling to specify for scientific workers in particular. To lay emphasis on the point now can only be interpreted as implying that in the opinion of the signers our only concern is the re-establishment of peace, regardless of the terms on which it is based.

The manifesto lays emphasis on the importance of the United States keeping out of the war in order to insure the continuation of intellectual progress. Intellectual matters know no national boundaries, and a purely national culture must be a poor thing indeed. The primary concern of any intelligent person must be the establishment and preservation of intellectual freedom and intellectual activity in the world as a whole. In a large part of the world these things have already been suppressed and in another part they are now in serious danger. If this country announces that under no circumstances will it take an active part in the struggle the sole effect will be to encourage the forces opposed to democracy and freedom of thought.

It might have been supposed that proponents of ''peace at any price'' would have been silenced by the proof that peace alone is not enough to insure intellectual freedom (as in Russia and Germany), and by what has happened to such peace-loving countries as Czecho-Slovakia, Finland, Denmark and Norway.

I. W. Bailey, Harvard University
James Bonner, California Institute of Technology
Robert Chambers, New York University
Alfred E. Cohn, Rockefeller Institute
W. J. Crozier, Harvard University
Hallowell Davis, Harvard University
Th. Dobzhansky, California Institute of Technology
Sterling Emerson, California Institute of Technology
Alexander Forbes, Harvard University
Ernst A. Hauser, Massachusetts Institute Technology
Hope Hibbard, Oberlin College
Leigh Hoadley, Harvard University
Hudson Hoagland, Clark University
T. H. Morgan, California Institute of Technology
Linus Pauling, California Institute of Technology
Peyton Rous, Rockefeller Institute
Karl Sax, Harvard University
A. H. Sturtevant, California Institute of Technology
Albert Tyler, California Institute of Technology
R. H. Wetmore, Harvard University

From the responses obtained it is clear that, had more time been available, a much longer list of signatures could have been secured.

A. H. STURTEVANT

SCIENTIFIC BOOKS

EMBRYOLOGY

The Rise of Embryology. By ARTHUR WILLIAM MEYER. Stanford University, Stanford University Press. 1939. xv + 367 pp., 97 figs. $6.00.

IN his new work, "The Rise of Embryology," Professor Meyer has chosen wisely to present "the history of the basic ideas in embryology," and wisely too, in the reviewer's opinion, has quoted liberally from the original sources, often in his own translations, "to avoid misinterpretation and to indicate something of the intellectual atmosphere of the time." The author has sought to efface his personal views, "for they are of the day," and "to reveal facts, not to utter dicta." The treatment throughout is sympathetic, for Dr. Meyer has a commendable understanding of the difficulties under which the early workers strove, and it reveals an unusually wide acquaintance with the sources, a fact to which the excellent bibliography of 19 pages abundantly testifies. In most cases the author brings his account down to the first quarter or half of the nineteenth century.

The first chapter deals with "Aboriginal Ideas of Reproduction," the beliefs of primitive peoples. Chapter II, "Early Historic Ideas of Reproduction," presents in the briefest possible way some of the more important views of the civilized peoples of antiquity, and especially those of Greece. There follows an interesting chapter on the tenacious doctrine of spontaneous generation which reached the height of absurdity, and, perhaps charlatanry, in Paracelsus. Chapter IV traces the history of the doctrine of epigenesis to von Baer's day. The author quotes the "New English Dictionary," which ascribes the first use of the word "epigenesis" to the year 1807. However, the reviewer finds the term used as an English word in the 1653 translation of Harvey's "De generatione animalium" (*e.g.*, Ex. XLV, p. 224). The treatment of the preformation theory in Chapter V is excellent, and this is followed by brief but adequate discussions of "Pangenesis," and "Panspermism or Panspermatism" in Chapters VI and VII. Chapter VIII presents the absorbing story of "The Search for the Mammalian Ovum." On p. 100 Fabricius is said to have recognized "three parts in the uterus of the hen: (1) the ovary, and (2) the superior and (3) the inferior portions of the oviduct, which he included in the uterus." More correctly, the "superior uterus" of Fabricius is the ovary, the "inferior uterus," the entire oviduct; the latter Fabricius divides into three portions. On the same page Adelmann is incorrectly stated to have said that Coiter "noticed the openings in the ruptured ovarian vesicles," etc. That statement was made about De Graaf (see *Annals of Med. Hist.*, N. S., 5: 338–339). Coiter does not mention the rupture of the Graafian vesicles, but on p. 140 Dr. Meyer says he does. The statement that "to both Harvey and Fabricius the ovum was the beginning of the development of any animal" (p. 101) is incorrect as applied to Fabricius, nor is it true that "both Fabricius and Fallopius expressed the idea that viviparous animals may arise from egg-like primordia" (p. 128). Certainly Fabricius never speaks of the "conception" of the vivipara as an egg, or even as "egg-like." Dr. Meyer has apparently been misled by a