always recognized more or less explicitly that science deals with means, not ends; that the ends, not the means, are the finally decisive factors; and that the ends must be conceded to depend on a system of values which cannot be determined by scientific method but which rest on the nature, and immediately on the emotional nature, of man. Among these values, these emotions, these "instincts," that of self-preservation has usually been put at the head of the list. Suddenly, after several centuries of apparent freedom from this impeding or limiting consideration, even the physicist—the most disinterested of scientists—has found that self-preservation is as intimate and inescapable a factor in his cosmogony as it is in that of the humble rodent.

Science is only a means to an end. Ah, but to what end? Is that not the first question?

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### Remarks on "Science and Man's Dilemma"

There is a point in Bruce L. Melvin's impressive article (*Science*, 1946, **103**, 241–245) on which I venture to take issue with the author. He mentions *domination* as a type of social behavior, and in effect proceeds to say that war, the desire for huge fortunes, the urge to surpass others are all basic in our economic, political, and social *systems*.

It seems to me that much turns on this idea, and that the idea is erroneous. It seems to me that war, the desire for huge fortunes, the urge to surpass others are not basic in our economic system. I think that they are basic in the *practices* that prevail in the system, that these practices are essential to much of the personnel that now dominates the system and are deemed so to more of it.

I think that this personnel is fearful of its position, that it senses unworthiness in itself, and that it turns against the system antagonism that it knows should be directed against itself and its practices. Its fear is increased by knowledge that its practices are, in fact, not essential to the system, but rather are detrimental. It tries to change the public idea of our system and to change our system itself, to its own advantage.

Its fear is further increased by knowledge that its practices are not essential to the general welfare and happiness or to the development of the best that is in the world and in men. It tries to make its practices essential to the welfare of individual persons, in order to provide for the continuance of these practices. It fears practices other than its own, knowing well that these might be to the good of the system as well as to the general good. Its great ally is material want, and it seeks to maintain this or to substitute, as may be possible. It desires continuance or discontinuance of any system according to its own chances of continuing in these practices, in which it has found itself to excel.

It talks freedom, competition, patriotism, fearing them all, and it talks teamwork—hoping to continue to drive the team. It looks toward an end of these practices as toward its own doomsday, as something to be put off as far as possible, come what come may. It wants freedom from fear—for itself only—and a beggar in our country would be more sure to have it.

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### Reply to Dr. Asmous on Russia

It is surprising that in 1946 a letter similar to that of Vladimir C. Asmous could appear in a scientific periodical (*Science*, 1946, **103**, 281). It seems it does not contain facts but only feelings. In keeping with the tendency of the latter I should like to express the opinion, shared by many Americans, that the people whose language Dr. Asmous "does not understand" have saved his life, as well as those of 130,000,000 other citizens of this country. At least they have saved us great troubles. They did it with an unprecedented amount of sacrifice and an unparalleled development of science. And everyone, wherever he is in the world, who enjoys freedom and an opportunity to work in science, is indebted to the 20,000,000 Russians who lost their lives in defeating consciously the Nazi tyranny. Individual mistakes, failures, and tragedies are occurring everywhere and in all times. The leading article, "Science and Man's Dilemma," in the same number of *Science* contains many amiable answers to questions raised by Dr. Asmous. It is well known that the Soviet scientists and people have shown on many occasions sincere and friendly relations toward the American scientists and people. A letter like that of Dr. Asmous certainly does not contribute to the friendship of two nations.

In conclusion I should like to change the sentence of Dr. Asmous, "We are probably talking again different languages although we both were born in the same country," as follows: All three of us were born in the *same* country, talked the *same* language, and breathed the *same* air but we have *different* feelings (at least two of us). And is it not the commonest fact (often an evil) in this incoherent yet attractive promising world of the human race?

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### Rediscovery of Vitamin A

Dr. Smith's letter (*Science*, 1946, **103**, 281) raises an important point, namely, the difficulty of scanning the immense literature of biochemistry well enough to know whether an observation is a new discovery. The difficulty is intensified when the early work is in Russian or Japanese, for instance, with a long interval before revival.

My own "discovery" in 1936 of the ester condition of natural vitamin A was due to unfamiliarity with the field, since I had recently emerged as a biochemist after a training in photography and high vacuum. In a later paper (*Chem. Rev.*, 1944, **34**, 95, sec. II, line 1) I mention my mistake and give credit to the earlier discoverers of the ester story.

The moral surely is that we must all—authors, editors, and printers—be very careful, making first reports as

accurately as possible and correcting errors as soon as discovered. With this in mind, *Science* may care to trade Messrs. Koscher and Barter for Kascher and Baxter, who were the actual authors of the article which raised Dr. Smith's ire. This will prevent a fourth (and imaginary) set of discoverers of the vitamin-A acid earth-blue color creeping into the literature.

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[*Science* acknowledges the responsibility of introducing two fictitious investigators.]

## Inactive (Non-oxygen-combining) Hemoglobin in the Blood of Ophidia and Dogs

The observations of E. Ammundsen (*Science*, 1939, 90, 372; *J. biol. Chem.*, 1941, 138, 563) revealed the presence of non-oxygen-combining (inactive) hemoglobin in 40 per cent of human bloods and in amounts varying from 2 to 12 per cent of total hemoglobin. Similar results were obtained by W. N. M. Ramsay (*Biochem. J.*, 1944, 38, 470) in 17 horses, with a frequency of 82 per cent and a mean value of 8.4 per cent for inactive hemoglobin (ferrihemoglobin). In agreement with Ammundsen, Ramsay identifies the inactive hemoglobin as methemoglobin—an assertion not shared by W. S. Cox and W. B. Wendel (*J. biol. Chem.*, 1942, 143, 331), who deny the presence of methemoglobin in the blood of various species, including man.

Since the subject is still a point of issue, it seemed worth while to extend these observations to other animal species, such as Ophidia and dogs.

In the experiments to be reported below, active Hb was determined by the oxygen capacity method of D. D. Van Slyke and W. C. Stadie (see Hawk and Bergeim's *Practical physiological chemistry.* (11th ed.) 1937); total Hb, by the iron method of E. Ponder (*J. biol. Chem.*, 1942, 144, 333). The observed difference between active and total Hb was attributed to inactive Hb.

The following table gives the values of inactive Hb found in four species:

| Author | Species | Number of animals | Number of blood samples | Inactive hemoglobin: Frequency % | Inactive hemoglobin: Limits (g%) | Inactive hemoglobin: Average (g%) |
|---|---|---|---|---|---|---|
| Ammundsen (1941) | Man | 53 | 82 | 40 | 2–12 | ... |
| Ramsay (1944) | Man | 38 | 38 | 55 | 1.5–7 | 3.3 |
| Ramsay (1944) | Horse | 17 | 17 | 82 | 3.0–25.5 | 8.4 |
| Prado (1944) | Jararaca | 23 | 23 | 100 | 6.0–28 | 17.0 |
| Prado (1945) | Dog | 22 | 31 | 82 | 3.5–20.5 | 10.9 |

The table shows the impressing fact that in *Bothrops jararaca,* frequency (100 per cent) and mean quantity (17 per cent) of inactive hemoglobin are much greater than in the other species.

This is, to our knowledge, the first observation of inactive hemoglobin in the blood of poikilothermic animals and might be of some value in the study of the physiological significance of this unusual form of hemoglobin.

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# Book Reviews

*Animal breeding plans.* (3rd ed.) Jay L. Lush. Ames, Ia.: Collegiate Press, 1945. Pp. viii + 443. (Illustrated.) $3.50.

This book continues to serve as a blueprint for improvement of animals through breeding. It is conveniently organized for a logical presentation of the subject, beginning with the background of animal breeding and the genetic principles which form a basis for scientific animal breeding. Breeding plans based on selection, relationship, and somatic likeness occupy a majority of the pages. Other pertinent topics concerning breeding plans and relating to reproduction are included.

Dr. Lush has been successful in presenting the genetic bases for animal improvement in a thorough and realistic manner. Necessarily complicated genetic explanations have not been avoided or oversimplified, but have been carefully clarified. Many of the principles developed by Wright, Fisher, and others are organized and discussed in order to make them more readily available to the student, research worker, and practical animal breeder. While this book serves as a text for undergraduate courses in animal breeding, it is well adapted to the use of graduate students and research workers. For the latter groups a more complete bibliography would increase its usefulness. The references are adequate for supplemental reading and, for some chapters, are arranged under subject headings so that the reader may readily choose those which fit his particular needs.

Commercial animal breeders who have an elementary understanding of genetics will find many aids to guide them in designing a breeding program which is most efficient for their conditions. They should welcome the realistic discussion of the rate of improvement which can be expected from various breeding plans and the clear statements of what each breeding method will and will not do in changing the genetic makeup of their herds.

Only minor changes have been made from the second edition. In general these involve the addition of recent references and are adequate in bringing the book up to date in most details. No mention is made of the