At the present time, the only general means available for establishing presumptive identity or nonidentity of an antibiotic with reported agents, disregarding the special cases where some unique property of an antibiotic can be utilized, is the so-called bacterial spectrum. We believe that the use of the bacterial spectrum for this purpose has a number of disadvantages: (1) It requires a considerable number of different types of organisms; (2) unless the organisms are standard, easily obtained strains, a comparison of literature values of the known antibiotics with the values for the unknown agent is subject to much uncertainty; and (3) as a result, a satisfactory comparison of the unknown antibiotic with known agents requires the test to be carried out simultaneously with the known antibiotics. The latter, however, are frequently unavailable. With these considerations in mind, the following approach is suggested as a possible solution to the problem presented.

In describing a new antibiotic, the investigator should determine, under defined conditions, the relative inhibitory effect of his antibiotic on two strains of each of two species of bacteria. One strain of each species should be the parent, susceptible to the antibiotic. The other should be a strain "made" highly resistant to the agent. The two species should preferably originate from standard bacterial strains. If possible, one species should be a representative gram-negative organism, such as *E. coli* (A.T.C.C. 9637), and the other a gram-positive organism, such as *Staph. aureus* (A.T.C.C. 6538). The four strains should then be filed with a central agency such as the American Type Culture Collection so that they would be readily available.

An investigator confronted with an antibiotic whose identity with certain reported agents is in question could then compare the relative effect of his antibiotic on the parent and the various indicated resistant strains obtained from the central agency. For example, an antibiotic is discovered which resembles subtilin in certain respects. If the unknown antibiotic is as active against a strain of *Staph. aureus* (A.T.C.C. 6538) highly resistant to subtilin as against a normal strain of *Staph. aureus* (A.T.C.C. 6538), the conclusion is justified that the two agents are not identical. In many instances, however, it would be necessary to compare the inhibition ratio of the unknown antibiotic with the published value for the known antibiotic in order to interpret the results. If necessary, the test could be extended to eliminate other reported antibiotics. Armed with this knowledge, the investigator would be in a more favorable position to report on the possible nature of his antibiotic while the investigation is still in a preliminary stage.

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Regional Research Stations in Basic Science

The proposals for federal support of scientific research embodied in S. 1850 should have the hearty support of all scientists. Senators Kilgore and Magnuson, and their colleagues, the sponsors of these proposals for the expansion of scientific research in the United States, are to be congratulated on the able way they have developed the program. I venture to propose, however, a slight addition, which I believe will help to achieve the objectives desired.

A station for research in basic science should, in my opinion, be organized in each state and territory. These basic science research stations should be supported both by federal and by state funds. They should receive the monies apportioned to the states under the present provisions of S. 1850. In addition, each state research station should receive from the state concerned an annual amount at least equal to that received from the Federal Government.

In each state where there is a state university the state basic science research station should be associated with the state university in the same way that the state agricultural research stations are now associated with the state agricultural colleges. Each basic science research station should be under the control of the same body that administers the state university. In a state where there is no state university the basic science research station could be administered by a state agency established for the purpose or assigned that duty.

The scope of the basic science research stations should embrace the whole field of science. Research in the social sciences would therefore form a proper part of the program of each station.

Substations for basic research could be maintained, if desired, at research centers situated at places within a state other than at the state university. Research grants also could be made to research workers at other institutions.

These proposed basic science research stations would serve to coordinate those researches sponsored by the National Science Foundation with the researches in progress at the universities. Possible duplication of effort would thus be avoided. Such stations also would tend to prevent the possibility or suspicion that the Foundation might come to dominate too greatly the scientific research of the nation. At the same time the Foundation would serve as a coordinating agency and would act in an advisory capacity to all the state research stations.

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Science—A Means to What End?

Suddenly—the "instinct" of self-preservation has invaded the field of physics. A physicist nowadays is like a small boy who has been playing baseball in the back yard of a friend and who has knocked a home run through the big window of the living room in the house of his friend's father. His triumph at baseball is suddenly confronted with another and quite different system of values, in terms of which his triumph is no more than a misdemeanor.

In the social, unlike the physical, sciences we have

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