cember, a score of social scientists spoke of similar difficulties. I suspect that the same experience will occur with respect to the President's war-on-poverty program. Millions will be spent for action, but very little either to plot the course of poverty over time or to evaluate the action programs undertaken. There is no dearth of social scientists willing and eager to work on the massive social changes which are under way in our time, but there is little matching enthusiasm on the part of those institutions that should be supporting basic or fundamental work on these problems.

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One week after publishing the latest in the unsatisfactory series of exchanges on the race issue between Garrett and George and the AAAS Committee on Science in the Promotion of Human Welfare (28 Feb., p. 913), *Science* asks, "Why do social scientists not take better advantage of major and foreseeable social changes to study the processes and effects . . .?" *Science* has part of the answer to its question in its own pages. The kind of "thinking" and name-calling and intimidation and appeal to faith that accompany studies in race relations explain why many social scientists stay aloof from such practical research. For what if the "good guys" (most of our colleagues) were proved wrong? We are timid men and, as they say in our jungle-cities, "Who needs an enemy?"

GWYNN NETTLER
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. . . Here in Los Alamos there is a beautiful opportunity for studying major social changes as they occur. Here is a community, built from nothing, so to speak, which has evolved in a decade from a secret army post, to an expanding, totally government-owned town, to the present community-in-transition, in which the citizens are being asked to buy their homes, utilities, and so on. . . . In a mountain setting and 20 miles from the nearest village, it is completely isolated geographically. . . . Its social and cultural development, in a vacuum, as it were, is fascinating, and someone should do a thorough study now, while the original patterns are still evident. . . .

The attitudes of primitive peoples toward orbiting satellites, the new fears, new folklore, or new curiosity resulting from "new stars moving in the sky," should merit some study by social scientists. . . .

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Coauthorship: Too Much Laissez Faire

Page's editorial, "Some perils of authorship" (10 Apr., p. 139), should move all scientists to re-examine their practices as authors and editors. Authorship is probably still the primary form of social "currency" in the research community despite continuing competition from such old rivals as officership, professorship, lectureship, and committee membership, and the appearance of many new challengers —granteeship, contractorship, consultantship, study-section membership, paper givership, inviteeship, and international commutership. Like monetary currency, the coins of authorship come in different sizes (books, articles, and technical reports) and vary in value with the standing of the issuing agency (publisher). The wise or suspicious sometimes will not accept the coin until they have tested its metal themselves. But the analogy soon breaks down. Authorship coinage is regulated by vague, unwritten conventions assumed to be universally observed and to be passed on unchanged from generation to generation in the same manner as legends. Page has pointed out that this assumption is unjustified since, in reality, practices vary widely with regard to whose names appear on a paper as authors and how these names are ordered. This lack of common practices leads to dissension among collaborators and to debasement of authorship as the currency of science.

Though perhaps not as serious, additional types of damage follow actions and decisions based on the false premise that the first author named on a paper is always equivalent to the "senior" author and that the order of authors' names has, at present, any universal significance. Page touched upon some of these consequences, and others can be found. For example, the value of author indexes is reduced when, on the assumption that the first few names on a paper are the most important, all authors after some arbitrary cut-off point are omitted. As mechanization and automation of the production of bibliographic tools increases, the economies to be realized by truncating the full list of authors will become more tempting.

Page's suggestions provide an excellent basis for developing the standardization required to correct the damage resulting from our present *laissez faire* with regard to authorship. He rightly indicates that achievement of common practices ultimately depends on authors. But editors can speed this development greatly *if they can agree* on explicit, operational guidelines for authors; individualistic editorial policies will only aggravate the problem. The American Standards Association is working toward national and international consensus on other conventions in scientific publication. It represents an existing mechanism that could be used to develop practical, generally accepted guidelines in cooperation with scientific societies, publishers, and organizations specifically concerned with scientific publication—Section T of the AAAS, the American Medical Writers' Association, the Conference of Biological Editors, the Society for Technical Writers and Publishers, and so forth. The importance of the problem justifies the effort.

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We can turn to the wisdom of the ancients for advice on the problem of multiple authorship discussed by Page.

In the *Ars Poetica* Horace says, "And in one scene no more than three should speak."

CARL LAMANNA
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Polonium-210 and Bladder Cancer

Radford and Hunt report (*Science*, 17 Jan., p. 247) that the Po^{210} contained in cigarette smoke may act as a cocarcinogen in lung cancer. This observation seems even more interesting in the light of the finding that the urine of heavy smokers contains nearly

six times as much polonium as the urine of nonsmokers. In the report of the Advisory Committee to the Surgeon General on "Smoking and Health" (unfortunately available to me only as reports from Swedish newspapers) it was mentioned as an as yet unexplained observation that heavy cigarette smoking is correlated with an increase not only in lung cancer but also in the bladder-cancer death rate. I want here to draw attention to the possible connections between increased polonium content in the urine and increased death rate from bladder cancer in heavy smokers. Since the tar carcinogens of cigarette smoke seem not to find their way to the urine, the role played by polonium may be a major one for smokers' bladder cancer and perhaps also greater than supposed for lung cancer.

HANNES EISLER
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Safeguarding the Biologic Record

L. F. Yntema's proposal (Letters, 3 Apr., p. 12) that field biologists and biology departments should assume a greater responsibility for the recognition and acquisition of natural areas of scientific value deserves widespread and prompt support. Our department has taken steps in this direction, and its experience might prove useful to others. A departmental committee on natural areas, composed of biologists with a field orientation, locates, visits, and evaluates representative areas of local streams, lakes, forests, and fields. Particular sites are selected for acquisition, priorities are assigned, and departmental approval is sought. The actual work of locating owners, searching titles, and purchasing is done by administrative officers.

Ideally, acquisitions should not be limited to unique biological entities such as bogs, virgin forests, or prairies, but should include large tracts, such as abandoned farmland or cut-over forests, if such are available locally. These tracts are typical of our man-dominated environment and are necessary for the study of contemporary ecological problems. Further, we are acting for future generations as well as ourselves, and such tracts, by natural processes, will ultimately develop climax characteristics.

Yntema has rightly emphasized the need for natural areas in a program of balanced biology teaching. An even stronger argument for their acquisition can be mustered if the future development of biology departments is considered. The population explosion with its environmental implications foreshadows a much greater effort in the area of environmental biology. Extensive, diverse, and well-planned holdings of natural areas will be basic to this effort. Perhaps even more important is the fact that these areas will provide a set of natural biological standards against which biologists can measure the success or failure of man in the manipulation of his environment.

F. H. BORMANN
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I would like to add the following comments to the dialogue stimulated by P. H. Abelson's excellent article "Trends in Scientific Research" (17 Jan., p. 222). I would first like to concur with the general tenor of commentary made by R. H. Painter (Letters, 20 March, p. 1276) against the assumption that all is known about the qualitative composition of the living world. The more support there is for scientific research, the more fields, once unattackable, many previously undeveloped, can be opened up to scientific analysis. But this does not mean that work in the more classical fields "has been largely completed." . . .

There are few geographic areas, save those near the larger universities in Europe and the United States, from which there are even partially complete analyses of the fauna and flora, even in their qualitative aspect, to say nothing of the quantitative picture. The lack of such information is unfortunate because of the great revolutionary changes in environments ushered in by wide use of pesticides, by stream pollution from expanding outputs of industrial wastes, and by air pollution from a variety of causes. It is difficult to perceive how these new factors are affecting segments of the living world when we do not know what that living world is. . . . If we do not know these things, how can we develop ideas on the effects of the new attempts—pragmatic efforts, not scientific endeavors—to modify what has evolved on this planet over countless millions of years? There are many taxonomic categories about which we know nothing, especially for marine environments. There are known taxa about which we know little, and there are very few segments of the total living world of which we can say we know all about the gross morphology of that phylum, that order, that family, or even that genus . . . vast areas of this fascinating planet are still unknown to descriptive biology.

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Cuba

An author (or editor) often feels like arguing with the reviewer of his book, but knows that this is not in the rules of the game. If I hope for publication of this letter, it is because Adolf A. Berle, in his review of the book *The Atomic Age*, (*Science*, 24 Jan.) ascribed to Leo Szilard a statement taken from my article in the *Bulletin of the Atomic Scientists* (Sept. 1960)—that "having mistaken Chinese communists for agrarian reformers, Americans may now be mistaking Cuban agrarian reformers for communists." Szilard has made enough controversial statements of his own; he should not be held responsible for mine!

On this occasion, I would like to say that, in my opinion, this statement has not been revealed as false by subsequent events. Anyone acquainted with the history of the Chinese Communist movement and with the writings of its leaders had no reason to expect from them anything but Communist dictatorship. Castro's revolution, on the other hand, was not predestined to make Cuba a Communist dependency. Several options were open to it, and I believe—but obviously cannot prove!—that the American tendency to treat the Cuban revolution almost from the beginning as a Communist conspiracy contributed to its throwing its lot in with Moscow.

Climbing out still farther on the limb, I suggest that even now Cuba is not irreversibly committed to the status of Moscow satellite. At present, flirtation with Peking is the only way in which Castro can show his independence and extract from Russia a burdensome tribute in support of Cuba's faltering economy. His position, how-