the nervous system. This is not the first attack on the NATO Science Programme, and it displays the same reluctance as others to consider the relevant and quite readily available facts. The authors of the letter raise three questions and then make a number of points of debatable relevance. Perhaps direct answers to these questions would be helpful to your readers.

What are the motivations and purposes of the sponsors? The NATO Science Programme was established in 1958 to strengthen the competence and capabilities of the scientific communities of the member countries. This was (and is) believed to contribute to the security of these countries as do strong and viable economies, orderly political institutions, cultural ties and even military capabilities. The programme is designed and guided by a Science Committee composed of senior scientific advisers from each country; the consensus of their individual viewpoints decides the programme. Their goal is clearly the healthy progress of science through provision of opportunities for education, research and communication. Since 1958 approximately 10,000 NATO Fellows have spent up to a year or more in the laboratories of foreign colleagues. Over 500 research grants have brought together techniques and samples, theoreticians and experimentalists, measurements and their interpreters, in fields ranging from aeronomy to zoology. Some 600 Advanced Study Institutes (ASIs) have been sponsored, permitting about 40,000 advanced students to take advantage of coherently structured presentations by distinguished groups of international experts.

Is free scientific communication a permitted consequence of the meeting? All of the above activities are carried out with the complete intellectual freedom and communication which is the hallmark of the good scientist. A majority of the ASIs result in published proceedings, many of them considered benchmarks in authoritative treatment of their subjects. Anyone who has taken part in an ASI will confirm that informal, uninhibited communication, during and after the meeting, is strongly encouraged and is a key element in the high reputation of this series (for example, compare Giorgi, *Nature*, **238**, 57; 1972).

Those interested in directing an ASI choose a subject in their own fields of interest and make application to the NATO Secretariat. An advisory panel, composed of distinguished scientists, serving as individual experts rather than governmental representatives, selects the successful applications on the basis of scientific importance and timeliness of subject, and quality of lecturers. The director then selects the participants. It is thus the individual ASI director who has complete control over the choice of

subject, selection of participants, conduct of the ASI and the ultimate publication of its proceedings. Over the years a consistent 10%–20% of the participants have come from non-NATO countries; nationals of almost seventy countries, including Eastern Europe and the USSR have taken part in ASIs. The records show no participation from the Chinese People's Republic or North Vietnam, but this has also been noticeably lacking in other scientific meetings held in Western Europe and North America.

Are the deliberations of the meeting of use for military purposes? Space does not permit a listing of all the ASIs sponsored by NATO; such a list is available upon request. The first six titles in the published 1972 programme may be given as examples: "Magnetic Resonance in Inorganic Chemistry"; "Feldspars"; "Data Bases: Theory and Implementation"; "High Energy Astrophysics"; "Molecular Dynamics in Simple Liquids"; "Modular Functions".

To the extent that the results of these meetings, or any others, or any published research, anywhere, are of use for military purposes, they will quite possibly be so used by anyone who follows such matters for such purposes. But the implication that the fifteen countries in NATO have conspired to spend some £20 million over the last fourteen years for the purpose of reaping direct military benefits from a few deliberately designed conferences is really far-fetched.

Yours faithfully,

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Scientific Community

SIR,—The proposal presented below represents a possible way of remedying three major problems (in my view) of contemporary science—a disintegrating environment within science itself, the misuse of the results of scientific studies and overwhelming problems of communication both within science and between scientists and the general society. The proposal deals specifically mainly with the first problem, but might help with the other two as well.

Most people who become scientists are attracted to the field for a varying mixture of two rather idealistic motivations: the creative joy of it and a feeling of potential contribution to the welfare of humanity. Instead, the contemporary atmosphere is often far different—that of a collection of entrepreneurs struggling for personal power and self-glorification with great determination but little joy, creative or otherwise. The drive to be first to publish is, of course, the major symptom of this atmosphere.

Secrecy, jealousy, exploitation and despair are the costs.

To try to improve the situation, I suggest that scientists with a common research interest form a true intellectual community around that interest. Progress from such a research group would be reported about twice a year in a book which would be derived from a meeting of the group. The book would present data, conclusions and discussion for each sub-area of current research in the form of a collection of papers, but individual contributions would not be identified in the published work. Everyone who contributed would be listed as an author (in alphabetical order) of the book. Each sub-area would have an editor (or editors) who would differ from year to year and be chosen by election. The role of the editor would be to introduce and summarize an area of research—not to decide what is or is not published. People unable to attend could send in their contributions. The book could be published rapidly (perhaps by a photographic printing process) since editorial work should be minimal. The meetings would be advertised well in advance and would be open to anyone; however, each group meeting should be typically rather small because it would deal with an area of specialization (the Temperate Bacteriophage Meeting at Cold Spring Harbor is one example of such a possible group meeting). Alternative proposals are discussed below.

I feel that the procedure outlined above would encourage communication, cooperation and the exchange of ideas among members of the group, since emphasis would be placed on advancing the field as a whole and not on individual contributions. We might hope to regard the communal nature of scientific progress which has already existed as a desirable and important feature rather than something which should be minimized in an effort to emphasize personal contribution. Furthermore, communication with other fields would be greatly improved, since progress in one area of research would be presented, summarized and discussed in a single place, rather than scattered through many journals at many different times. (Of course, communication of results and ideas would not be limited to the annual meetings—only formal publication.) Finally, an association of scientists truly dedicated to the value of science as a creative undertaking and benefit to humanity would be more effective and more highly motivated in pointing out potential values and misuses of their research to the general society.

The major potential disadvantage I can see is that granting agencies might complain that past contributions were not extensively documented. However, since the grant is presumably awarded

mainly for the proposal and not the past, this is not a severe problem. Competence in the area could be accomplished by reference to the applicant's work in the group publication (for example, *RNA Polymerase*, 5, 65). Perhaps a less competitive environment would also encourage scientists to worry more about the tendency to a concentration of an increasing fraction of the financial resources for science under the direct control of a few people. University promotion committees would also have to be familiar with the system.

Three alternatives follow, which would in my view be less effective. One of the possibilities is for a group meeting as above, but with individual papers signed as in a typical current symposium.

The race into print would be eliminated, but personal contributions would be documented. My third suggestion is that certain issues (perhaps two per year) of standard journals should be devoted to a particular research interest. This fact would be advertised well in advance, papers would be collected and published together with an introduction and summary (signed) by members of the editorial board of the journal. This should improve communication with other fields and might lessen competition without the technical problems of an organized meeting and book publication. However, the research group would retain less control over its summarizers and the general publishing policy. Yet another possibility is an informal agreement that people would actively arrange with others working along similar lines to publish simultaneously or jointly and to try to minimize duplication. (This is, of course, sometimes done at present.)

Yours faithfully,

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Science Libraries

SIR,—Sandison (*Nature*, 234, 368; 1971) has cast doubt on the accuracy of Brookes' (*Nature*, 232, 458; 1971) model for optimizing a library of scientific periodicals on the grounds that the members of the Bradford set are not constant and that rank within the Bradford set changes with time.

base year are taken into account in the optimization process.

With 100% target percentage all error in the model is due to papers in newly contributing periodicals. The percentage error due to this cause is approximately the same all through the target range of interest. This allows the calculation of an expected percentage of papers in the library in 1940 allowing for periodicals

Table 1 Percentage of Relevant Papers in the Library in 1940 for Various Target Percentages Set by the Bradford Cut in Various Base Years

Base year	Percentage papers in library in 1940 for target % x			Expected % papers in library in 1940 taking into account new contributing periodicals for target % x		No. of contributing periodicals in base year
	$x=50$	$x=75$	$x=100$	$x=50$	$x=75$	
1925	15	25	38	19	29	39
1930	29	44	61	30	46	96
1935	40	57	77	38	58	190
1925*	26	44	59	30	44	166
1930*	33	54	77	39	58	284
1935*	40	72	91	46	68	471

* Optimization process based on cumulated experience to base year by using all papers published up to and including base year for ranking periodicals.

An estimate of these errors can be obtained from Needhams' bibliography (*Biochemistry and Morphogenesis*, Cambridge University Press, 1942) which can be considered an approximation to a complete set of papers relating to the biochemistry of morphogenesis up to 1940. This bibliography is appended to a comprehensive review of the subject and contains 6,668 references to papers in periodicals. It conforms with Bradford's law and the negative exponential distribution of papers with time.

If a librarian has optimized his library by Brookes' Bradford cut in 1925, 1930 and 1935 with the object of providing 50%, 75% or 100% of papers, the actual percentage that would have been in the library in 1940 is shown in Table 1. It also shows the percentage in the library when all papers up to and including the

that begin contributing after optimization (Table 1).

The difference between the expected percentage and the actual percentage represents error due to change in the ranking of periodicals. If the ten most productive periodicals for the years 1925, 1930, 1935 and 1940 are listed each title appears on average 1.6 times. Despite this change in ranking, the error due to this change is small by comparison with error due to new contributing periodicals. For an expanding literature such as the one studied, review of the Bradford set must be more frequent than every five years if Brookes' model is to yield acceptable predictions.

Yours faithfully,

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Obituary

Dr P. H. Leslie

THE death, in June of this year at the age of 72, of Patrick Holt Leslie has deprived the world of ecological science of one of its most brilliant bi-mathematicians, one to be ranked with Raymond Pearl and Alfred Lotka. Educated at Westminster School and Christ Church, Oxford, he obtained an honours degree in physiology in 1921, but was later prevented by a very serious illness of the lung from completing a medical course. After several

years of research in bacteriology at the School of Pathology in Oxford, he began an association with the Bureau of Animal Population (under Charles Elton as Director) that continued from 1935 to his retirement in 1967. He became a Senior Research Officer there and his activities fell into two closely-related spheres: advisory work on statistical matters, and his own research upon demographic methods and problems. Leslie (known to his family and a large circle of friends as "George") was compelled by his earlier

ill-health to live a quiet and somewhat secluded life. His habit was to visit the laboratory in the morning to advise and read, but to spend the rest of his time working at home. Thus, like Darwin and unlike many in the academic hurly-burly of today, he had uninterrupted opportunities to think, analyse and calculate. Through these years he was greatly helped by the devoted care of his wife. It is to be noted with some astonishment that Leslie had no formal training in advanced mathematics, and his extra-