CORRESPONDENCE

Fungus Research

SIR,—It is becoming increasingly obvious that diseases of fungal origin are of major importance. The use of cytotoxic and immuno-suppressive drugs, corticosteroids and broad spectrum antibiotics is leading to an increasing number of local and generalized mycotic infections. The need for control of non-bacterial infections in extensive—for example, open heart—surgery is a major problem. Even dermatophyte infections are responsible for serious morbidity and wastage of man-hours. In industry, for instance, there were more than 5,000 spells of incapacity attributable to dermatophytosis from June 1967 to June 1968. The average length of absence from work was 13 days and a total of more than 111,000 man-days was lost (Department of Health and Social Security, 1969). In the veterinary field, mycotic abortion and *Dermatophilus* infection are also of considerable economic importance.

In order to gain some information concerning the amount of work being done in the United Kingdom in the field of medical and veterinary mycopathology, the Medical Mycology Subcommittee (disbanded 1969) of the Medical Research Council sent a questionnaire and a letter explaining the aims to a number of centres and individuals. The distribution list was compiled on the basis of persons known to be associated with the subject, either because of their membership of the British Society for Mycopathology or because of their personal contact with members of the Medical Mycology Committee. In fact, almost all of the medical teaching centres in the country and the majority of veterinary teaching establishments were included. The results of this were communicated to the MRC which has since given mycology a high priority. The main points elucidated are set out as follows.

Of 87 replies received out of 92 questionnaires sent out, 10 were from departments or sections employing at least one trained mycologist. All were engaged in research, a diagnostic service was provided by 8 and teaching was carried out in 6.

Replies from 34 medical centres (dermatological, bacteriological and pathologic departments) indicated that although teaching was carried out in the majority this was mainly clinical and in dermatology departments. Of the 34 replies from veterinary establishments (largely ARC sponsored units) 33 offered a diagnostic service but only 8 and 7 centres also contributed to teaching and research respectively. It was clear that medical mycology was taught piecemeal to medical and veterinary students (one or two lectures) and hardly ever to science students.

The survey also emphasized that there is a remarkably small number of persons employed in medical and veterinary mycology. This was confirmed by reference to membership list of the British Society for Mycopathology. Of the 83 British members only 31 had received formal training in mycology and of these 9 were young individuals still under training.

One of the elements involved in this unsatisfactory situation is the lack of a firm career structure for medical mycologists. Implementation of the Zuckermann report in the NHS might alleviate this but nevertheless the training of more medical mycologists is essential.

A place should be found in undergraduate curricula in science and medicine for more extensive teaching in medical mycology, and the institution of an MSc course would also be of the utmost value. In this way, a supply of adequately trained workers for university and hospital departments would become available and thus provide a better mycological service throughout. Fundamental research would also be stimulated and would have an influence beyond mycology itself. The fungi provide excellent systems for the study of the general problems of host–parasite relations and there is no lack of subjects which urgently require direct investigation.

Yours faithfully,

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Transferrin

SIR,—In a recent *Nature* article[1], Chernelch and Brown concluded that their *in vivo* experiments designed to test the Fletcher–Huehns hypothesis of functional differences of iron atoms bound to transferrin[2] failed to support the predictions of this theory. These authors apparently have overlooked or misinterpreted two earlier publications which indicate that an exchange of iron atoms occurs among molecules of transferrin and apotransferrin. Morgan *et al.*[3] have shown that *in vivo* experiments in humans "provided evidence for the return to the plasma of appreciable amounts of radioiron attached to a second transferrin". Aisen and Leibman[4] demonstrated that at physiological concentration of citrate and at *p*H 7.4, there was complete and rapid exchange of iron atoms between transferrins and apotransferrins. In light of this evidence it is difficult to understand how Chernelch and Brown could be able to follow the course of transferrins selectively labelled predominantly on sites A or B of transferrin molecules in an *in vivo* study wherein exchange would occur. The failure of their experiments to behave in the predicted manner does not invalidate the Fletcher–Huehns theory as they have suggested.

As a criterion for their hypothesis, Fletcher and Huehns postulated that there be no redistribution of iron from one binding site to another. This is not a mandatory dictate for their theory. If iron exchange among transferrin molecules is mediated by a low molecular weight chelating agent such as citrate, possibly by the formation of a ternary complex[5] or if the exchange is due to feedback from a second reflux compartment of the iron pool[3], then even in the event of complete equilibrium of iron transferrin binding site exchange, there still will be equal numbers of molecules with iron bound to either site and metabolism would be regulated by the number of receptors and their rates of reactivity.

Yours faithfully,

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1 Chernelch, M., and Brown, E. B., *Nature*, **226**, 356 (1970).
2 Fletcher, J., and Huehns, E. R., *Nature*, **218**, 1211 (1968).
3 Morgan, E. H., Marsaglia, G., Giblett, E. R., and Finch, C. A., *J. Lab. Clin. Med.*, **69**, 370 (1967).
4 Aisen, P., and Leibman, A., *Biochem. Biophys. Res. Commun.*, **32**, 220 (1968).
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Meat Factories

SIR,—For several reasons, the practice of killing animals for their meat may well become more and more impractical in the future. There is a moral standpoint, for instance, from which one perhaps ought not to eat meat if one is not at least prepared to kill the animal personally; however, vegetarianism seems a dubious (not to say unsatisfactory)

proposition to most people, who will successfully ignore this moral point and continue to insist on meat as long as it is available. It may not be available indefinitely, however; as the world's population increases, there must come a point where industry will cast an unromantic eye at cow pastures, sheep paddocks and other such inefficient institutions, and will insist on building something useful there.

My own research field is not a biological one, and the following may not be very practical but, I believe, ought to be given some thought: have tissue-culture specialists thought of applying their techniques to the culturing of edible animal tissues? A number of advantages over conventional animal culture spring to mind, the most important one perhaps the fact that, theoretically at least, one should be able to produce more "meat" in a given volume or area than by sending cows out to graze (even if the American cattle industry continues to rationalize its business). The technical problems and costs involved may well be tremendous, but this will become less and less relevant as the population increases. The anti-killing moralists would concede, I think, that a mass of cultured animal tissue is (except nutritionally) little different from cultured plant tissue (the hidden moral point—that, given an increasing human population density, we will eventually not be able to allow other animals much space—may be conveniently ignored here).

If the technical problems are soluble, there would be gastronomical advantages in this—there seems to be no reason why one should not culture a vast variety of "meats"; there will be no such thing as a rare and costly tissue—although it is doubtful that this "meat" would turn out like the fibrous stuff we eat now.

I envisage meat factories for the year 2000.

Yours faithfully,

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Multiple Authorship

SIR,—In this day of "publish or perish" one increasingly encounters papers authored by several persons. A high percentage of team work, and thus a high percentage of multiple authorship, can, in fact, be considered a reflexion of the state of advancement of a particular science[1]. The January 30, 1970, issue of *Science*[2] reporting on the scientific results of the Apollo 11 moon expedition is an excellent though exceptional case in point. In this issue there were 144 papers authored by a total of 619 persons, an average of 4.3 authors per paper. One paper was by 18 authors (is this a record?), two other papers by 14 authors each, one by 12, and two by 11 each; at the other extreme, ten papers were authored by only one person. Merely the names and addresses on the paper with 18 authors required five column inches of space.

The very number of publications listed in a bibliography of a scientist often gives an inflated estimation of the scientific contribution of that person. Obviously more entries are possible if a person participates in a great deal of team work. What is needed is some method to rate the equivalent value of a scientific paper authored by several persons. Each paper, no matter by how many authors, should count as unity (one equivalent paper). That is, the paper with 18 authors, if listed in bibliographies by each of the 18 authors, should count as one paper total, and not 18. The following table presents sample equivalent values for papers with up to six authors:

Paper authored by:	Values of equivalent papers per author Author: A	B	C	D	E	F
A	1.00					
AB	0.67	0.33				
ABC	0.50	0.33	0.17			
ABCD	0.40	0.30	0.20	0.10		
ABCDE	0.33	0.27	0.20	0.13	0.07	
ABCDEF	0.29	0.24	0.19	0.14	0.09	0.05

For example, three papers individually authored by "X" (total of 3 equivalent papers) are "worth" slightly more than six papers authored by "Y" as follows: Y, YB, YB, AY, ABY, ABCY (total of 2.94 equivalent papers), even though "Y" has twice as many publications as "X".

There are two possibilities for situations with six or more authors per paper since the contribution of the sixth and additional authors ranges from 1/21 to $1/\infty$ (euphemism for essentially zero: for example, the contribution of author number 18 is 1/171 or 0.006 equivalent paper): (1) the contributors in excess of five might well (preferably!) be relegated to acknowledgment status in a footnote, or (2) they might be listed alphabetically (as is currently done with the more notable movie stars in epics).

A final plea: in personal bibliographies of scientists, entries for papers by several authors should include a list of the authors in the sequence they appear on the paper. Thus, in a bibliography for author "Y" a paper by "ABY" should be cited as "by ABY" and not, as is so commonly done, as "with AB", since the latter gives no indication of the ranking of the authors (and who did all the work).

I leave it to other workers to develop more exact and complex relationships taking into consideration other significant variables (length, type of paper (for example, taxonomic monograph, review paper), etc.).

Yours faithfully,

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[1] Manten, A. A., *Earth Sci. Rev.*, **6**, 181 (1970).
[2] *Science*, **167**, 417 (1970).

Cycles in Behaviour

SIR,—The whole approach of analogizing between the natural sciences and the behaviour of human society could well distinguish itself only by its naivety. Nevertheless, history is strewn with examples of the fertility of cross-disciplinary activities, and at least one Great Man has urged us to "only connect".

Young and Ziman (*Nature*, **229**, 91; 1971) concern themselves with establishing a nomenclature to facilitate discussion of cycles in social behaviour, by borrowing terms from physics. This they do very convincingly except that they do not make the important distinction between an oscillating function of time and a periodic function of time. An oscillating function is normally understood to be one which exhibits a sequence of turning points: thus one speaks of super-critically and sub-critically damped harmonic motion, where the former exhibits a monotonic trend toward some asymptote and is non-oscillatory, and the latter is oscillatory but not periodic. A periodic function would exhibit a waveform that is exactly repeated over intervals of the period.

The distinction between oscillations with regularly spaced turning points and periodic motion vanishes when the ordinate is non-numerable in the sense that an event can only be said to occur or not occur, for then only the intervals between events matter. However, there clearly exist cases where more quantification of a social variable is possible. For instance, as the authors point out, historical events sometimes display temporal influences that decay in a manner suggestive of a relaxation time. The "modulation" of a periodic function such as the yearly religious festivals by a decaying historical influence could clearly result in an oscillating social variable that is aperiodic.

Perhaps it is sometimes appropriate in discussing the behaviour of human society to use a logarithmic rather than a linear scale of time. The significance to us of a fixed interval of time seems to depend on average roughly how long ago that interval is placed. This follows if events have relaxation times. The "larger" the event and/or the longer its relaxation time the longer its significance: the memory we now have of some interval in history depends on the sum of its remnant influences, and the farther back the interval the less cause, on average, we now have to remember it. History, archaeology, geological eras, scientific papers, personal experience and future forecasting all seem to imply a roughly