gain: they regard their labour as a general service to the scholarly community, to maintain high literary and scientific standards, for the benefit of their colleagues as authors and as readers.

Our complaint is at the selfish and frivolous waste of our professional time and effort in the name of "free competition". Not nearly enough trouble is taken by most scientists to improve the quality of the books they must read: it is scandalous that the little we do should be spent thus fruitlessly. It is poor consolation, incidentally, to observe afterwards that the author has not hesitated to benefit (without acknowledgment) by the improvements we have proposed.

We also believe that the credibility of scientific publications depends upon an agreed and orderly framework, in which well qualified editors and reputable publishers respect themselves and one another. The margins of technical book publishing are not so elastic as to allow wide variations in the real terms that can be offered to authors. The ultimate financial return on a book depends on much more imponderable factors than the apparent royalty percentage or the quality of the burgundy at a tax deductible lunch. A good book can easily be published and an author does much better by bargaining hard and honestly with almost any single good publishing house than by "shopping around".

Some people do not seem to have thought this out, and fall for the fast buck. Let us spell it out to them. They know well enough that they must not submit the same research paper simultaneously to two different journals. We suggest that the behaviour here reported must be considered a comparable breach of the unwritten ethical code of the scientific community.

Yours faithfully,

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Viral DNA Integration

SIR,—In a review article "Vintage Year for Tumour Virology" (*Nature*, **233**, 28; 1971) John Tooze states: "and the recent experiments of Wall and Darnell (*Nature New Biology*, **232**, 73; 1971) dispel any doubts about the integration of polyoma (actually SV 40) virus DNA into host cell DNA". Recent experimental evidence throws new light on this situation (Gelb *et al.*, *J. Mol. Biol.*, **57**, 219; 1971). Because of it, there is to my knowledge no published evidence which conclusively demonstrates the integration of SV 40 DNA into the DNA of the transformed host cell. Gelb *et al.* have recently shown that host specific SV 40-like sequences exist in non-transformed cell green monkey and mouse DNAs. About one-half copy of host specific SV 40-like sequence is present in each cell. Published experiments designed to demonstrate the integration of SV 40 into transformed cell DNA have relied on the reaction of SV 40 C-RNA (RNA made *in vitro* from SV 40 DNA) with transformed cell DNA which had a molecular weight much higher than SV 40 DNA (Sambrook *et al.*, *Proc. US Nat. Acad. Sci.*, **60**, 1288; 1968). It was assumed that any reaction of C-RNA was with virus specific SV 40 sequences, which had been integrated into the host cell DNA. However, the existence of host specific SV 40-like sequences in DNA from transformed cells complicates the interpretation of these data. It is not known whether the C-RNA reacted with host or virus specific SV 40-like sequences. The interpretation of Sambrook *et al.*, that SV 40 DNA is integrated, rests entirely on the greater degree of reaction of SV 40 C-RNA with high molecular weight SV 3T3 cell DNA than with PY 3T3 high molecular weight DNA. This result could be the consequence of: (*a*) the integration of virus specific SV 40 sequences in the host DNA, or (*b*) the differential replication in SV 3T3 cells (and not in PY 3T3 cells) of the chromosomes which contain the host specific SV 40-like sequences.

The data of Lindberg and Darnell (*Proc. US Nat. Acad. Sci.*, **65**, 1089; 1971) and Wall and Darnell are also difficult to interpret for the same reason. It is not known whether the large RNAs they detect, which contain both SV 40-like and host specific sequences, arose from virus specific SV 40 sequences or from the host specific SV 40-like sequences which are present in non-transformed cells.

In summary, I do not believe that the virus DNA integration into transformed cell DNA has been proven.

Yours faithfully,

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Library Optimum

SIR,—In his recent article[1] B. C. Brookes propounds an ingenious mathematical framework to determine which periodical volumes a library should hold. He is careful to point out that the selection will need regular review and revision, in case the value of the ageing factor *a* or the contents of the Bradford set change from year to year. There is as yet very little experimental evidence on the consistency of either. Such limited evidence as there is suggests that the ageing factor is reasonably constant. But the position of the Bradford set is less satisfactory. The Nature Conservancy librarians (J. M. Weingott and S. M. Penny, unpublished) have lent me a list of titles cited in the *Journal of Ecology* three or more times in 1955–56, and a similar list for 1965–66. There are 150 periodical titles in the two lists, but only forty-two (28%) appear in both. Of the thirty-three titles cited nine or more times in either year, only eight (25%) attained that level in both, and twelve were cited less than three times in the other year. The Kendall rank correlation coefficient between the two years is 0.18 and not significant.

There is another major practical problem. The article assumes that the data analysed to obtain ageing or utility factors and Bradford sets are valid parameters of the relative value of the literature to the readers. There is no mention of the type of data to use. The reader who sought guidance from the earlier literature cited would find practical techniques described in which analyses of citation frequencies are used to calculate *utilities* discussed in terms of library use. Krauze and Hillinger[2] have discussed the difference between citations in one article and future citations to that article. Their work implies a more complex relation between *a* and *u* than Brookes suggests. In any case, the validity of citations for forecasting library consultations remains unproven, and there are *prima facie* reasons why the relationship is not necessarily close. For example, one item in a list of references is often intended to lead to a chain of earlier papers. Again, each citation represents an author's selection from a wider group most of which he has consulted in a library. In neither case is there any inherent reason for similarity of age distribution or of pool of titles between the list of citations and the items read by the author or his readers.

Most of the practical studies of citations or library use have so far been based on the relation between frequencies and age or title, without considering the number of items available for reference. But, to be useful as parameters of the relative value to scientists of groups of volumes, the data must be presented as the number of references per available item, and not as the numbers from groups of differing size. The need to correct "obsolescence rates" for the fact that there is much less of the older literature to cite or read is becoming generally recognized. When the appropriate corrections are made, it has been shown[3] that in some library contexts the older literature can be more heavily used than the younger. In all calculations based on Brookes's utility concept it is therefore essential that the utility factor *u* be derived from an ageing factor *a* repre-