Correspondence

Biased touching of the editorial tiller

Sir,

I was saddened to find that your redrafted instructions to authors¹ confirm that you continue to allow authors of 'negative trials' a mere 500 words to introduce and state their hypotheses, give an adequate description of their methods and results, and state their conclusions. Although you do not define the term negative trial, presumably you use it to refer to studies in which no statistically significant differences between treatments have been shown.

This biased touching of the editorial tiller has two serious implications. Firstly, it discourages submission of statistically powerful treatment comparisons in which no statistically significant differences are observed. Secondly, because statistically significant differences observed in small trials tend to overestimate true treatment differences, it actively fosters such overestimates in the material you make available to your readers.

It has been suggested² that the onus of ensuring appropriate design of clinical trials should be on the editors of scientific journals. Over recent years, there have been several publications detailing criteria for designing and reporting clinical trials, some of them directed specifically at journal editors.^{3,4} I hope you may consider adopting an editorial policy which places more emphasis on sound research methods than on statistical significance.

Meanwhile if there are investigators among your readers who at any time over the past 30 years have completed perinatal trials which have never reached publication (either because the results were 'negative' or for any other reason), they are invited to send brief details for incorporation in the National Perinatal Epidemiology Unit Register of Controlled Trials in Perinatal Medicine. In collaboration with colleagues in North America, we wish to use this information in an attempt to learn more about the 'selective publication bias'.

References

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Dr Chalmers is undoubtedly correct in pointing out the selective bias that exists as a result of international medical

journals being more willing to publish papers that show a new positive result than those reporting trials yielding a negative result. It was because of this bias that we decided, as a matter of policy, to try and include more 'negative trials' providing that they were based on a reasonable hypothesis. Since space is at a premium, however, and we receive many more good original articles than we are able to publish we ask that the length of these papers be reasonably brief.

In relation to our journal the position is not quite as bleak as Dr Chalmers suggests. Much will depend upon the nature of the trial and the hypothesis on which it is based. For instance, if we received a long paper reporting a well conducted trial which showed that the vitamin status of mothers at the time of conception was completely irrelevant to the occurrence of neural tube defects in their babies we would wish to publish that in full—because it is an important, negative result contrary to many present beliefs. If, however, we received a paper showing that the mother's smoking habits during pregnancy were unrelated to the prevalence of bed wetting in her child at school age we would be unlikely to wish to publish that in full, however large or carefully done the study. Thus the interest and strength of the hypothesis on which the study is based is all-important. Between the two extremes I have cited come a number of negative results trials based on reasonable, if somewhat tenuous hypotheses reporting careful work. At present these rarely achieve publication anywhere (which is not surprising because most of them are very boring), yet it is these that we would wish to report in some way. Therefore we offer the opportunity for such studies to be reported briefly as short reports in our journal, if only to prevent other people from embarking along that particular path. In our notice to contributors we suggest that 'a short report of not more than 500 words is more likely to gain acceptance than a longer report'. Those who are interested in the study may communicate with the authors for more detailed information. Ed.

Alcohol and the fetus

Sir,

I read with interest the annotation by Smithells and Smith¹. It seems to be generally implied that the alcoholic mother is the sole cause of the fetal alcohol syndrome, and so should be the one that abstains from alcohol during pregnancy. In some children whose clinical picture matches perfectly that of fetal alcohol syndrome, I have been unable to obtain a definite history of alcohol abuse by the mother; though in other cases, such as a mother who is a barmaid with cirrhosis, it is obvious.

I recently reviewed a child who had all the features of the syndrome. She was small for gestation, and at 11 months weighed 6.1 kg. She was an active, sociable child with a small central cleft and a systolic heart murmur. She