reviewer claims to have read material in the book that in fact does not exist in it. The probability of accepting a null hypothesis is never mentioned. The acceptance of a null hypothesis is a common fallacy (p 63) arising from the misinterpretation of the result of a significance test. In addition, tests in the book are not described as "best," whether related to the t test or χ^2 test.

(4) The reviewer states that a description of what to do with small two by two tables is called for. If by "small" he refers to the fact that the table has only two rows and two columns, then a full description of what to do is provided (pp 91-92). If on the other hand "small" refers to small expected frequencies in the two by two table, then it is stated (p 91) that Fisher's exact test is warranted and a reference is provided for a full discussion of the test. This is generally adequate for the non-specialist, as Fisher's exact test is cumbersome and is rarely performed without the aid of a computer, so that it is not deemed necessary to include details of the method in an elementary text.

(5) Finally, the reviewer states that the comparison between three or more groups is never mentioned. Clearly he failed to read p 118, where precisely this situation is discussed.

Unfortunately the reviewer saw fit to consider only these five points in relation to my book. The remainder of his review, although topical, is not specific to the book.

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*We sent a copy of this letter to the reviewer, whose reply is printed below.—ED, *BMJ*.

SIR,—The basic difference between the author and myself is akin to the difference between physiology and pathology. As a consultant statistician a high proportion of the "cases" I see concern the abuse of statistics. My fundamental criticism of the book is that it is unsuitable for its audience. To answer the author's points serially:

(1) The vital link is omitted. The "excellent table" does not mention the connection between type of variable and type of test, nor do the chapters on estimation and hypothesis testing state anywhere that nominal and ordinal data should not be pumped into calculations whose basic assumption is that the variable is at least continuous without great care. This is a very common mistake and one to which the medical profession, with its use of classifications and rating scales, is especially prone. A book on *medical statistics* should warn the reader, particularly any book written after the publication of one of the standard works on the application of non-parametric test; I agree that pp 87 and 132 give the appropriate uses for these tests, but my point is the other way round.

(2) In my typescript and the proof there was a comma between "briefly" and "once," the omission of which does change the meaning and I sympathise with the author. I did not in fact say that the example was wrong. My point was that saying that small is "less than 20" (p 73) and using sample sizes of 12 and 13 in an example (p 83) "can only confuse the non-specialist." I believe it does. Incidentally, pretesting for equality of variances is still

controversial and is not necessarily to be recommended as routine in a book at this level.

(3) The words "the probability of accepting a false null hypothesis" do not appear in the book, but p 57 contains a too brief discussion of type II error, which is the same thing. I do apologise for substituting "best" for "good," which is the word in the book, but surely my point still stands. The χ^2 test is not a "good" test in the sense used and no warning of its dangers is given. Since the power of the tests to be used is a very important consideration in medical research²⁻⁴ one would have hoped that a book aimed at medical research workers (p ix) would contain more on the subject.

(4) With the advent of pocket calculators with memories Fisher's exact test is very easy to perform⁵ and in a book published in 1978 I would argue that a description of it is at least desirable, especially for the non-specialist; χ^2 for two by two tables with small frequencies is too conservative with Yates's correction and too unconservative without it.

(5) I overlooked the statement on p 118 because it is in the wrong chapter. It appears under "Linear Regression and Correlation," but surely its proper place is in the chapter dealing with t tests. Again, my point is that the repetitious use of t tests without the proper precautions is a too frequent error by medical writers.

In conclusion, may I add that I did not like writing as critical a review as I thought (and still think) the book deserves. I wrote three separate drafts before committing myself. But the book is still an inappropriate text for its target market.

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¹ Siegel, S, *Nonparametric Statistics for the Behavioral Sciences*. New York, MacGraw-Hill, 1956.

² *British Medical Journal*, 1978, **2**, 1318.

³ Freiman, J A, et al, *New England Journal of Medicine*, 1978, **299**, 690.

⁴ Clarke, I, *British Medical Journal*, 1978, **2**, 1497.

⁵ Swinscow, T D V, *Statistics at Square One*. London, British Medical Association, 1976.

Postanaesthetic oxygen

SIR,—Your leading article (25 November, p 1452) seems to make heavy weather of a situation which appears relatively straightforward. It is first necessary to distinguish the overt causes of postoperative hypoxaemia, which include underventilation and pulmonary shunting. These are largely avoidable by careful attention to anaesthetic technique. However, even with impeccable anaesthetic technique a considerable proportion of patients without cardiorespiratory disease have arterial hypoxaemia during the first few hours after even trivial surgery. Most cases cannot be explained by underventilation¹ and the most likely explanation is the reduction of functional residual capacity shown by Alexander *et al.*²

Although hypoxaemia is more pronounced in older patients, its incidence is unpredictable in the individual patient. In a survey of 169 patients arterial P_{O_2} levels were in the range 5-7 kPa (37-6-52-6 mm Hg) in 14 patients aged 39-70.³ This is not a degree of hypoxaemia which should go untreated when it is so safe and simple to raise the inspired oxygen concentration above about 35%, which gives satisfactory results.⁴ Selection of patients at risk is not really feasible and routine

administration of oxygen in the early post-operative period seems sensible. Simple disposable masks can be used as the precise concentration of oxygen is not important.

In a very small number of patients who have lost their carbon dioxide sensitivity there is a danger of hypoventilation when the hypoxic drive to respiration is withdrawn. Such patients must be identified and carefully monitored in the postoperative period. Venturi masks should be used since precise control of oxygen concentration is mandatory in these patients.

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¹ Nunn, J F, and Payne, J P, *Lancet*, 1962, **1**, 631.

² Alexander, J I, et al, *British Journal of Anaesthesia*, 1973, **45**, 34.

³ Nunn, J F, *Lancet*, 1965, **2**, 466.

⁴ Drummond, G B, and Wright, D J, *British Journal of Anaesthesia*, 1977, **49**, 789.

Diagnosis of breast tumours

SIR,—With reference to the letter from Mr Peter R Simpson and others (18 November, p 1430) we have calculated the cost of screening a woman for breast cancer in our London clinic so far this year at £13.80. This includes, in just over half, x-ray mammography, and the cost takes into account staff, x-ray and other materials, rent, rates, heating, lighting, and a depreciation charge on capital equipment and a contribution towards staff training, appointments, medical secretariat, and administration. This appears cheaper than the University College Hospital clinic, which is surprising, particularly as in our accounting we include overheads which the NHS often does not.

The difference in cost highlights two important points. Firstly, we rely heavily on paramedical staff who are specially trained in specific techniques, and a considerable saving in cost is thereby achieved. Secondly, a screening unit needs to see several thousand patients a year so that the unit cost is reduced (in our central unit we have already screened 5000 patients this year). The unit cost is higher in our mobile units, where the number of women screened is much lower.

A paper on the cost of detecting breast cancer is in preparation.

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Use of the telephone in consultant practice

SIR,—I would like to comment on your leading article (21 October, p 1106) from the standpoint of the consultant. Consultant practice in Hamilton is closer in its habits to the British than the usual North American in that patients are seen only by referral. Initially I was diffident about the advice I received to use the telephone freely for two reasons. Firstly, I thought it would be time-wasting and impractical because of the difficulty of getting hold of the referring doctor and the annoyance my interruption of his work would cause. Secondly, I thought that use of the telephone weakened the letter as a record of the consultation. I soon found that both fears were groundless and it is now rare for me not to