made monthly during the trial but in general they were badly recorded. Complete tablet counts were obtained for all 31 Glasgow patients but for only 6 of the 61 patients attending other centres, and the influence of the regularity of taking medication could not therefore be assessed. Data from Glasgow patients indicate that relapse was more likely in those patients who did not take their medication regularly, but the number of cases from this one centre was too small for any firm conclusion to be drawn. However, it would not be too surprising if patients took medication more regularly when regularly asked to return unused medication at each visit to the clinic.

Dr. Leff bolsters his case by referring to the clinical arrangements made in Glasgow, and this requires some clarification. One of the difficulties in conducting this trial was that a great many patients referred to the psychiatric clinic had already received some antidepressant medication. In order to improve the accuracy of selection and of the initial ratings Glasgow doctors who regularly referred their cases to the clinic were asked to refer patients before prescribing any medication, arrangements having been made to see such referrals at short notice. Dr. Leff suggests that "patients referred in response to this plea would be less seriously depressed" but to my knowledge only four such referrals were made; three were unsuitable for the trial and the remaining patient did not respond to antidepressant medication. Dr. Leff also by suggesting that "nearly all the Glasgow patients were contributed by one psychiatrist" perhaps implies that his criteria for selection must have been liberal. The patients were, in fact, contributed by four psychiatric teams but for administrative convenience and in the interest of consistent rating these patients were transferred to the care of one consultant and his assistant. The patients were therefore always seen by the same doctor whenever they attended the clinic and it is possible that this increased the psychological support they received. Such "non-drug" factors are known to have considerable influence in psychopharmacology and this is difficult to control or assess in large multicentre trials. However, the variation in the ease with which tablet counts were obtained in the several clinics is probably some indication of the differences in the general clinical setting and management of the trial between Glasgow and elsewhere and perhaps tells something about the relationship between the patients and their doctors.

It is my view that the low relapse rates in Glasgow, in patients taking placebo or active drugs, are less likely to be a reflection of case selection than of the differences in residual symptoms, drug defaulting, and perhaps especially the "non-drug" factors.—I am, etc.,

R. N. HERRINGTON

University Department of Psychological Medicine,
Southern General Hospital,
Glasgow

¹ Mindham, R. H. S., Howland, C., and Shepherd, M., *Lancet*, 1972, 2, 854.

² Mindham, R. H. S., Howland, C., and Shepherd, M., *Psychological Medicine*, 1973, 3, 5.

SIR,—Dr. J. P. Leff's diligence in peeping behind the apparently unequivocal results of three recent clinical trials (20 October, p. 156) has led to some important findings.

After reading his paper it is easy to see how two very similar investigations can yield opposite conclusions because of differences in selection procedure. It is because of this that what may appear to be relatively trivial details should be included in published reports.

Trials are many and journal space is limited, but we are all too often in the dark about such items as source of patient referral, reasons for inclusion and rejection, previous treatment, and the therapeutic expectations of the physician. Dr. Leff has shown us that these are at least as important as descriptions of the experimental design and use of double-blind procedure; they should be given equal prominence if the value of a clinical trial is to be fully assessed.—I am, etc.,

PETER TYRER

Department of Psychiatry,
Southampton General Hospital,
Southampton

Influenza Vaccination

SIR,—Once again this winter there will no doubt be panic demand for the latest influenza vaccine and perhaps this is a good time to remind everyone, especially those with jet vaccinators, that Tauraso *et al.*¹ examined the effects of giving influenza vaccine intradermally in a much smaller dose than that usually recommended and reported that the antibody response to the first inoculation given intradermally was higher than that to subcutaneous injection of the standard dose.

It would appear that the current influenza vaccines could be given intradermally in a dosage of 0.1 ml and thus five to 10 times more people could receive protection with the limited amounts of vaccine that are going to be available. Saving of cost to the country could be enormous, and especially in industry, where large amounts of vaccine are used. In the winter of 1971 some £2m worth of vaccine was used and had this been given intradermally, using a vaccinator gun, the saving in the United Kingdom alone could have been £1.8m. Multiply this over the world and there is ample money for vaccination campaigns in underdeveloped countries to be undertaken.—I am, etc.,

JOHN AVISON

London S.W.15

¹ Tauraso, N. M., *et al.*, *Bulletin of the World Health Organization*, 1969, 41, 507.

Diabetic Pregnancy

SIR,—The comprehensive survey of diabetic pregnancy by Dr. N. L. Essex and her colleagues from King's College Hospital (13 October, p. 89) raises some questions. Apparently the number of "gestational" diabetics seen annually is six. As the number of deliveries in the obstetrical unit is not quoted it is not possible to make direct comparisons, but it would appear that the number of latent diabetics detected at the antenatal clinics at K.C.H. is substantially less than elsewhere—for example, in Boston,^{1,2} Belfast,³ and Dublin.^{4,5} If this is in fact so, it would be of interest to know what programme of case detection is applied at the antenatal clinics in K.C.H.

In detailing their method of management the authors discuss their attitude to family planning and to sterilization but do not refer to the use of therapeutic abortion. How many abortions on diabetic patients were performed in the 22-year period under review and what were the indications? This question is not posed in any pejorative sense, but in the conviction that it is not possible to evaluate any system of management without having all the data. It is only then that we can judge whether "we are seeing a real improvement."—I am, etc.,

M. I. DRURY

National Maternity Hospital,
Dublin

¹ Wilkerson, H. L. C., and Remein, Q. R., *Diabetes*, 1957, 6, 324.

² O'Sullivan, J. B., and Mahan, C. M., *Diabetes*, 1964, 13, 278.

³ Hadden, D. R., Harley, J. M. G., Kajter, T. J., and Montgomery, D. A. D., *Diabetologia*, 1971, 7, 87.

⁴ Drury, M. I., and Timoney, F. J., *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1970, 77, 24.

⁵ Drury, M. I., and Timoney, F. J., *Journal of the Irish Medical Association*, 1970, 63, 281.

Crohn's Disease and Intestinal Cancer

SIR,—With reference to your leading article "Does Crohn's Disease Predispose to Intestinal Cancer?" (6 October, p. 3), a statistical assessment of malignancy in Crohn's disease has already been carried out on a relatively large series of patients.^{1,2} The results, from both morbidity and mortality data, demonstrated a significant excess of tumours within the digestive tract which could be attributed specifically to tumours of the small bowel and pancreas. No corresponding excess of tumours in the colon or rectum could be shown. The analytical approach was to compute an expected number of tumours as a basis for comparison with the number of tumours observed in the patients. The cancer incidence rates which were used in the analysis were directly applicable to the series because they related to the region of the country from which the patients were drawn.

The practice, by no means uncommon in the literature, of expressing cancer morbidity rates in terms of a crude percentage of total cases is unacceptable in the context of the quoted analysis.³ The resultant rate will depend on (1) the initial age distribution of the series, (2) the length of time for which the patients were observed, and (3) the pattern of mortality experienced by the series. For these reasons it is invalid to combine or compare individual series in terms of crude percentages. Further, cancer incidence rates based on population figures are readily available,⁴ so that there seems little point in attempting estimations from two sets of data which refer to different populations in different continents.

The final analysis, as quoted in your article, compares annual incidence rates combined with a prevalence rate for Crohn's disease (whether point or period prevalence is not stated) with the crude incidence of the association. Assuming that the prevalence figure does refer to the number of established cases existing during one calendar year, the resultant multiplicative probability would then relate to one calendar year. The crude percentage incidence of the association, on the other hand, is based almost certainly on the number of tumours arising in several