to be emphasized that topical application of steroid preparations to the skin lesions of herpes zoster is not only useless, but could be quite harmful.

Dr. Pearce stated that he knew of no double-blind controlled trials on the effects of corticosteroids in acute zoster, but in fact such a trial was conducted by Eaglstein *et al.*³ in the U.S.A. Unfortunately they only included 34 patients, of whom 19 were controls and 15 treated, and only 24 out of the 34 were over the age of 59. I should think most people will agree that there is no case for giving steroids to patients under the age of 60 years. Nevertheless, Eaglstein and his colleagues found that in the over-sixties 73% of the control group had post-herpetic neuralgia (which they define as pain which has persisted for more than two months from the onset), while in the treated group only 30% developed postherpetic neuralgia.

I think, therefore, that there is a good *prima facie* case for a large-scale double-blind controlled trial as suggested by Dr. Pearce.—I am, etc.,

H. DAVIES

Salford

¹ Elliott, F. A., *Lancet*, 1964, 2, 610.

² Gold, S. C., *Practitioner*, 1972, 209, 204.

³ Eaglstein, W. H., Katz, R., and Brown, J. A., *Journal of the American Medical Association*, 1970, 211, 1681.

Sedation in Pregnancy

SIR,—Your leading article "Neonatal Behaviour and Maternal Barbiturates" (14 October, p. 63) and Professor Philip Rhodes's recent address on the obstetric prevention of mental retardation (17 February, p. 399) make it plain that in the present state of knowledge central nervous system depressants should not be administered to women at any stage of pregnancy except when absolutely indicated. In current obstetric practice the most frequent and prolonged use of sedation is in the treatment of pregnancy hypertension, where it is believed to augment the effect of bed rest in hospital in providing the "absolute mental and physical rest" these patients are thought to require. If this usage of sedation could be minimized, many fetuses would be spared the potential loss of cerebral capacity that they are at present being exposed to.

That it can be minimized was strongly suggested by the results of the Sheppey experiment,¹ in which admissions for toxæmia were reduced by some 70% and sedatives were eschewed even in severe cases. Bringing these results up to date, the 286 cases of pregnancy hypertension seen up to the end of 1972 were treated with one maternal loss from accidental haemorrhage, caesarean section, and chronic cardiomyopathy; two instances of eclampsia (one antenatal before referral to the hospital, the other 10 hours postnatal after a normotensive pregnancy); and eight perinatal deaths, including three from obstetric accidents during labour and one from congenital malformations incompatible with life. Such overall results could hardly have been obtained if bed rest and sedation were as important in the management of toxæmia as has so often been claimed.

In addition to the potential dangers of sedation for the baby and the doubts that

must now exist regarding its necessity in the management of toxæmia, the positive advantages of a policy of non-sedation should also be considered. Only the un-sedated mother can give the obstetrician a useful account of her own symptoms and the activity of the baby. Modern writers seldom even mention symptoms in their communications on allegedly severe toxæmia but their prognostic value was fully appreciated by established authorities such as Theobald,² who stated unequivocally that they are a better index of the likelihood of eclampsia supervening than the degree of hypertension or albuminuria. Certainly, the only common factor in the two cases of eclampsia mentioned above was that both were preceded by intense and increasing headache.

Has not the time come for the toxæmias of pregnancy to be rescued, like the temples of Philae, from the sea of sedation that has buried them for so long.—I am, etc.,

D. D. MATHEWS

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Sheerness, Kent

¹ Matthews, D. D., Patel, I. R., and Sengupta, S. M., *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1971, 78, 610.

² Theobald, G. W., *Pregnancy Toxæmias*, p. 236. London, Kimpton, 1955.

The Placenta—An Environmental Problem

SIR,—Dr. D. I. Rushton states in his recent review (10 February, p. 344) that primary placental insufficiency is probably not a pathological entity and that correlation between placental function and pathology is frequently very limited. While I agree wholeheartedly with the first half of this statement, I find it difficult to accept that the latter is true.

In a recent publication Miss J. M. Jordan and I¹ have shown an 88% correlation between morphological study of the placenta and paediatric assessment of the baby, the placentas being examined blind without foreknowledge of the mother's or baby's condition apart from the expected date of delivery. We were also able to show that the degree of placental insufficiency (measured by a simple scoring system) could be correlated with blood sugar levels in the baby except in some cases of postmaturity.

Up till now the main drawback in pathological examination of the placenta has been the tendency to lay too much stress on the individual lesions without consideration of the placenta as a whole and the extent to which pathology interferes with function. Examination of the cord and membranes has also been largely ignored. Such factors have all been included in the new scoring system, which is graded as follows: normal, less than 10; borderline 10-14; and placental insufficiency 15 or more. Many cases giving a high score on naked-eye examination can be diagnosed in the labour room and need not await pathological examination.

This is an obvious advantage to the paediatrician, but reviewing the problem purely from the angle of the obstetrician Dr. Rushton states that morphological studies are mainly of academic interest and contribute in no way to the obstetric management of individual pregnancies. While this is true, we feel that by delineating the problem more clearly and assessing to what extent failure of placental growth, patho-

logical change in the placenta, or both play a part one may eventually be able to recognize types of placental insufficiency which are liable to recur.—I am, etc.,

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¹ Scott, J. M., and Jordan, J. M., *American Journal of Obstetrics and Gynecology*, 1972, 113, 823.

Non-pharmacological Influences on Therapeutic Efficacy

SIR,—I cannot help thinking that the letter by my old friend Dr. A. Káldor and his colleagues (10 February, p. 352) has been written with their tongues in their cheeks. They tell us that the sphygmomanometer when applied to the arms of hypertensive patients who are taking hypotensive drugs is not affected by "fibs" that the doctor tells his patients. Great news, indeed, but telling fibs bears no relationship, as claimed, to "interactions between doctor and patient" or "the therapeutic efficacy of the doctor's attitude."

What is meant by the doctor's attitude to his patient (which indeed does affect and influence patients both emotionally and somatically; references on request) has been explained at great length by Dr. Káldor's former fellowcountryman, the late Dr. Michael Balint, in his book *The Doctor, His Patient and the Illness*. If Dr. Káldor and his colleagues really want to assess the influence of the doctor and his attitude on the effects of hypotensive drugs, I would recommend that they form two groups of patients. One group should be treated like the patients mentioned in Dr. Káldor's letter, it being immaterial whether or not they are being told fibs about the variation of their blood pressure. A second group should be treated by a doctor evidently interested in his patients as human beings, in that each patient would be permitted to talk freely about his personal and emotional problems (in private) and be "listened" to (in the sense in which this word has been used by Balint) and then the state of their physical and emotional health determined by something more than just the rise or fall of the sphygmomanometer.

Then, and only then, will clinical pharmacologists be able to determine whether the attitude of the doctor affects drug action. Incidentally, the experiment would also throw light on the meaning, genesis, and appropriate treatment of raised blood pressure and its concomitant symptoms and signs.—I am, etc.,

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¹ Balint, M., *The Doctor, His Patient and the Illness*. New York, International Universities Press, 1964.

Results of Renal Transplantation

SIR,—We read with great interest the article by Mr. P. R. F. Bell and others (18 November, p. 408) reporting their experience of renal transplantation in 33 patients in Glasgow and were impressed by the survival figures, which represented a dramatic improvement on most published results. We are concerned that their graft survival figures of 85% at one year and 82% at two years

have been widely quoted. We should like to make several comments on these figures and to emphasize the caution needed before drawing conclusions based on small samples.

The Glasgow authors kindly allowed us access to the dates of transplantation of each of the 33 patients and this has enabled us to check their basic data. Several small transposition errors were noted relating to graft survival of patients 23-28. There was one further error in the graph showing "actuarial survival of 33 renal transplants (calculations by method of Barnes)" in that the dip at month 5 corresponding to the rejection of patient 31 was omitted. Re-calculating transplant survival using the adjusted data gives:

	All 33 Transplants	29 Cadaver Transplants
1 year	79%	75%
2 years	79%	75%
3 years	79%	75%

In each case survival remains constant in the second and third years since no patient rejected a kidney after the first 12 months. In fact, the three-year figures were not emphasized in the summary but did appear in the graph mentioned above; presumably they were not quoted because they were based on two patients. We suggest that the two-year figure should be excluded on similar grounds as it is based on eight patients. Our own studies have led us to discount figures at two and three years as they become quite inaccurate with small samples of the Glasgow size. We prefer to plot the monthly survival, calculated by dividing the number of grafts surviving for the given number of months by the number of grafts that could have survived that number of months. This calculation gives results that differ from those given by the actuarial method, but when the two methods are compared using very large series the differences disappear.

Our calculations of monthly survivals can be further refined by using computer calculations to construct a negative exponential curve to fit the data; a weighting system is incorporated into the programme which attaches more importance to those results where the sample size is larger. The monthly survival calculations and the computer-fitted survival curve have been applied to the Glasgow results, giving the following figures:

	All 33 Transplants		29 Cadaver Transplants	
	Calculated Value	Computer-fitted	Calculated Value	Computer-fitted
1 year	78%	79%	71%	74%
2 years	67%	69%	56%	59%

The results which we feel should be more widely quoted are the survivals at one and two years of the 29 cadaver transplants. We would suggest that it is these figures which are more meaningful than those in the Glasgow paper, which makes no allowance for the extremely small sample size.—We are, etc.,

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Integrated Child Health Service

SIR,—May we please reply to some of the comments on our recent article (*Supplement*, 20 January, p. 20). In suggesting that there should be 24-hour paediatric dispensaries it was not our intention to cause more fragmentation of services but to face the regrettable reality in some urban areas. Such a scheme could, however, be used to promote integration, since staffing would have to be jointly by family doctors and paediatricians and would offer the former the "foot through the hospital door" that they have justifiably wanted for so long.

We are grateful for the comments of Dr. Helenor F. Pratt (24 February, p. 489) and of Dr. K. S. Holt (10 February, p. 352). It seems to us to be unnecessary to create a new journal of child health and development since the present excellent journal *Development Medicine and Child Neurology* already provides a focal point for multidisciplinary activity in this field.

We share the view expressed so well by Dr. C. S. Smith (17 February, p. 415) that the service should be staffed by established specialists with the area medical officer acting as a general co-ordinator. In our view the hierarchical administration within which the social and preventive child health services have operated in the past has been very damaging.—We are, etc.,

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Unconventional Publication

SIR,—It was gratifying for those involved in planning the medical sciences section of the International Research Communication System to see that you feel the project important enough to warrant comment in a leading article (17 March, p. 631). You asked a number of questions. As chairman of the executive editorial board, I hope I may be permitted to reply.

The ultimate, admittedly ambitious, aim of the I.R.C.S. is to set up a central information bank which will contain author-prepared 500-word reports of all biomedical research performed throughout the world. Reprints of these reports will always be available. Most of the research will also be published in conventional journals; some items and in particular negative reports which often now go unpublished, will appear in the I.R.C.S. and nowhere else. Few would deny that the aim is worthwhile, and the names of the outstanding medical scientists who are acting as consultant editors indicate the level of academic support already obtained. There is room for considerable discussion as to the methods by which the aim may be achieved, and the I.R.C.S. editors and management stress that they are fully prepared to modify the system in the light of informed comment by scientists and editors.

Apart from the concept of a single central information bank, the most important aspect of the I.R.C.S. is its speed of operation, with a maximum of six weeks' delay between receipt of a report and publication. Thus within about two months of material being written up medical research workers anywhere in the world will be able to receive from a single source information about all research going on. Those associated with the four leading English language medical weeklies may be unaware of just how slow many journals are, with delays of 6-18 months between receipt and publication, and further delays of 3-18 months before the titles appear

in the *Index Medicus*. Those who have ready access to major libraries may be unaware of the advantages of obtaining information from a central source. But the majority of scientists who neither publish most of their work in the weeklies nor have daily access to a major library will have little difficulty in seeing the potential advantage of the I.R.C.S.

If an author submits a report to the I.R.C.S. at the same time as a full paper to a journal, the report will usually be reproduced in the I.R.C.S. before it has even been refereed by the journal. Such speed raises the question of quality control. In this respect we emphasize purely objective criteria such as the numbers of cases or experiments described and the presence or absence of statistical treatment of the results. Unlike almost all other journals, we do not before incorporating a report into the system attempt to assess the "importance" or "triviality" of a piece of work. We believe that it is impossible for one or two individual referees to give any valid assessment of the "significance" of a piece of work which other individuals have felt important enough to carry out. One wonders, for example, what most referees would have made of the importance of Mendel's tall and short plants! We feel that all pieces of work which meet basic objective criteria should be made available for assessment to the scientific community as a whole.

Journal editors may also be unhappy about the appearance of work in the I.R.C.S. before conventional publication. It is our informed belief that material in most conventional papers has, long before publication, been made available, often by means of meetings, to an "in-group" of research workers in the field. The idea that conventional publication is still a form of exclusive transmission of material never previously revealed to anyone is largely a myth. The slowness of most journals has forced scientists to disseminate their results in other ways. Such informal "publication" is often in media not routinely purchased by even large libraries and it leads to unpleasant priority disputes. We think that by formalizing a very rapid method of publication which is backed by the highest academic authorities we can deal satisfactorily with the problems. The information will be available not only to the "in-groups" but to any interested person anywhere. The detailed classification will ensure that each individual looks at reports only in areas in which he is interested.

We hope that journal editors will not refuse to consider full papers describing work which has previously appeared in the I.R.C.S. Journals now publish papers on work which has already been briefly reported either in correspondence columns of weeklies such as the *B.M.J.* or in conference proceedings. We can see no difference in principle between this and the publication of material which has been reported in the I.R.C.S. The I.R.C.S. simply makes the procedure more systematic and universally available. Brief report and full papers perform two quite distinct functions. It is impossible for any scientist to read in full all the papers relating even to a very narrow field. All read the summaries of papers whose titles seem of interest, but only in a few cases is the paper then read in detail. Advances in printing techniques now enable author-prepared reports to be circulated almost immediately without waiting for the publication of the full paper. It would be unfortunate if the much slower techniques required for conventional publication were to delay the circulation of the brief reports which in most cases are all that most readers require. We have informed about 100,000 biomedical research workers about the I.R.C.S. and the overwhelmingly favourable response indicates that this is a desirable and much-needed scheme. Contrary to what the editorial implied, librarians especially seem to welcome the formalization of what is now a chaotic rapid publication situation. They see no problems in citing in bibliographies classified and numbered reports which are on microfilm or microfiche. The