was there any clinical difference in the basic varicose condition of both groups.—We are, etc.,

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Measles Vaccination and Dental Dysplasia

SIR,—Whereas it appears to be general knowledge among dentists that acute infections, including measles, can cause dental dysplasia, this does not seem to be known by many doctors. It was with some concern that I became aware of this, in view of the present schedule of measles vaccination with live vaccine which we are advised to give in the second year when the permanent dentition is being laid down. As many of us have seen, this particular method of vaccination can produce a sharp reaction, and it would follow from this that there may be risk of deformities occurring in the teeth, and specially in the enamel of children so affected.

Could those with experience on this subject give us their considered opinion, as it would be a minor tragedy if in five or six years' time cases of dental dysplasia were found as a result of vaccination? If there is any appreciable risk it would seem sensible to postpone measles vaccination until about 4½ years of age—that is, before school but when the enamel of the incisors is nearly completed—as this would have little effect on the ultimate eradication of this disease.—I am, etc.,

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Thrombolytic Therapy

SIR,—We have read with some satisfaction the timely letter of Dr. G. P. McNicol and Professor A. S. Douglas (18 January, p. 180), and wish to support their serious concern with regard to the views expressed in your leading article on thrombolytic therapy (21 December, p. 721). There is little doubt in our minds that this article completely blurred the impression of cautious optimism expressed by J. Hirsh and his colleagues in their excellent paper (21 December, p. 729). We are of the opinion that the primary fact to emerge from this study was not that all physicians and surgeons should include this form of therapy in their therapeutic armamentarium, but that the time has now come for several large-scale controlled trials to be instituted. The object of this exercise must be to ascertain whether thrombolytic therapy is not only efficacious and safe but in what circumstances, if any, it has clear advantages over adequate systemic heparinization—a much less expensive therapeutic agent.

The difficulties of setting up such a trial are formidable, not least is the decision to use streptokinase or urokinase. Although urokinase is non-antigenic and relatively non-toxic it is considerably more expensive than streptokinase, and the vast quantity of urine required for one course of treatment is such that its future availability for the care of patients outside the confines of controlled trials must be questioned. It is of interest to note that Professor Sol Sherry, chairman of the United States Multicentre Urokinase-Pulmonary Embolus Trial, has recently concluded: "Despite the limitations imposed by the antigenicity of streptokinase, the fact that this agent can effectively mediate thrombolysis in vivo, and that its production, availability, and cost pose no serious problems to the pharmaceutical industry, leads this reviewer to conclude that full-scale trials in well-defined disorders (similar to those contemplated for urokinase) should be instituted in the United States."[1]—We are, etc.,

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REFERENCE

[1] Sherry, S., *Ann. Rev. Med.*, 1968, **19**, 247.

How Much Documentation?

SIR,—The reviewer of *Record Linkage in Medicine* (7 December, p. 632) is clearly against any unnecessary increase in medical documentation. Thus, one might expect him to be sympathetic with the view that better use should be made of existing documents to obtain required information on influences contributing to ill-health, the effectiveness of various methods of treatment, the prevalence of particular diseases, and so forth, and to avoid asking for the same information more than once (as is now commonly done because of the relative inaccessibility of back records).

Since more efficient use of existing documents was in fact the main theme of the International Symposium on Medical Record Linkage, it is unfortunate that your review of the *Proceedings* gives quite the opposite impression.—I am, etc.,

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Nursing and Nurses

SIR,—The Salmon Report[1] has given nursing an authoritative structure and indicated the lines on which a career in nursing administration could be made available. What is equally important and carefully emphasized throughout is the clear exposition that this career is for nursing and nursing alone. The Report makes the point that delegation, secondment, and the demonstration of sapiential authority are concerned only with the judicious and selective distribution of nursing duties. In accepting the dual purpose of Salmon it would not be out of place to explore still further the present nursing duties required of qualified and unqualified staff. If we accept that the practical non-administrative duties in the care of a patient in hospital can or should be summed up as "nursing the patient," then why are nurses in the operating-theatre? Some, both nursing and medical, will immediately say we couldn't manage without them. My reply is: of course we couldn't manage without them, but are we managing with the right people?

My contention is that nurses in theatre are not by any stretch of the imagination nursing patients. In fact, it might be true to say that the medical profession depends in the theatre on nursing personnel who no longer care for nursing. To take my argument an essential step further, the present nursing complement of any theatre dealing with any branch of surgery could be replaced entirely by operating-theatre technicians. Such technicians would become (as nurses are now) thoroughly familiar with instrument knowledge, techniques, sterilization, cleaning, and setting up. There is no reason why, without any knowledge of medicine or surgery, they should not become efficient both as assistants and in "taking the case." It would, of course, be of personal benefit to them to have explanatory lectures and demonstrations in surgical pathology and techniques, but this knowledge should not be a criterion of initial competence. Operating theatre technicians could release nurses to nurse patients. This is not to say that the State-registered nurse has no value in the theatre situation, merely that it is unnecessary as a criterion for either entry or competence.

The syllabus of the Register or the Roll is ostensibly aimed at practical and administrative nursing care in the home, the factory, or the hospital bed. Need it form the only available entry to the side of the anaesthetized patient, who has probably never met this expert nurse and who will probably never meet her again, at least not while conscious? Indeed, how can she claim that she has exhibited nursing care to this patient to whom she has probably not spoken, whose face she may not see, whose voice she may not hear, and with whose condition she is not in a position to sympathize. How can she call herself a nurse in this situation? She has become a technician—but a long way round.

Turning to another essential department in hospital—the outpatient clinics—are we really manning these departments with the right people when we delegate trainee nurses, S.E.N.s, and S.R.N.s to this work? Over 95% of patients attending clinics are not acutely ill. True, there are mechanically disabled patients, but most of these require trolleys, wheelchairs, and strong arms to help them. Ambulant patients to other specialties require no more than a kindly, understanding hostess. For the "hostess" to be S.R.N. qualified with nursing knowledge gained over three or more years is of no real benefit to such an outpatient clinic attender without the essential sympathy and understanding. After all, what do most consultants want in a clinic? The right patient at the right time in the right chair—someone to bring them in, to take them to an examination couch, and finally to escort them to the exit door. Even if our hostess was S.R.N. she could not and would not dream of explaining any proposed investigation or the meaning of any medical phrase picked up, just in case she was wrong or gave the wrong impression. The important point in dealing with any patient at the end of the interview is often missed and is the cause of so much frustration. This is asking the patient whether they would like to have another word with the doctor before they go. I know doctors will say, "Heavens, more delay!" but of all

aspects of the interview this is often the most important and often the most neglected.

How many doctors use nurses in clinics as "nurses"? How many can find time to teach nurses in the clinic situation? How many in reality use nurses as chaperons? I suspect most. As Salmon indicates, wise delegation on sapiential lines could see that clinics could have access to either an expert path. technician or to a qualified S.E.N. or S.R.N. as required. Both these experts being unhampered by the straightforward activities, neither should have to act as a clinic hostess. In this way much more efficient use could be made of nursing expertise in this area of the hospital.—I am, etc.,

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REFERENCE

1 *Report of the Committee on Senior Nursing Staff Structure*, 1966. London, H.M.S.O.

Phenylketonuria: Therapeutic Problems

SIR,—The assertion in your leading article "Screening Tests for Phenylketonuria" (5 October, 1968) that successful dietary therapy exists for patients with phenylketonuria and other inborn errors of metabolism cannot be accepted. With regard to phenylketonuria, there seems to be little consensus that dietary therapy constitutes "satisfactory treatment."[1-3] Leaving aside considerations of experimental design, one may alternately conclude that dietary therapy is of minimal value[4] or may even be deleterious,[5] prevents most but not all of the mental retardation,[6] permits completely normal mental development,[7] or may even produce individuals with higher I.Q.s than their unaffected siblings.[8] Moreover, the following serious defects of experimental design exist in all studies of the dietary therapy of phenylketonuria:

(1) Each study involves a small number of patients with exceedingly heterogeneous characteristics. It is impossible to compare in sufficient numbers the intellectual performance of patients who are matched in all but one variable—for example, length of therapy.

(2) Marked differences among studies frequently prevent valid comparisons of data.

(3) The ages of the population for whom dietary therapy seems the most beneficial (neonates), and those of the "control" groups differ markedly. The latter include phenylketonuric siblings or unrelated, affected infants, both treated at a later age,[6 8] and an institutionalized adult population.[9] These groups do not control the environmental factors attending the interactions of the medical team, parents, and phenylketonuric infants. Moreover, as emphasized by others, the results of tests of mental development of infants (D.Q. or I.Q.) cannot be validly compared with those of tests in older children (I.Q.).[2 7 10]

(4) No effort has been made to minimize environmental influences which might perpetuate a patient's trend of development. Since the phenylketonuric infant under treatment is highly dependent on the physician (and the parents), his response to therapy may be influenced by the attitude of these adults (perhaps the result of their ability to achieve good dietary control, etc.). None of the studies are sufficiently controlled to separate the effects on mental development of such reinforcement from those of reduced serum phenylalanine concentrations.

(5) The criteria employed routinely for the diagnosis of "true" phenylketonuria (that is, absence of phenylalanine hydroxylase activity) may not exclude other hyperphenylalaninaemic syndromes, such as those caused by variant, and less efficient, forms of phenylalanine hydroxylase.[11] The ability of these patients to hydroxylate phenylalanine, albeit decreased, may facilitate their biochemical control by diet. Since a significant proportion of such hyperphenylalaninaemic individuals are not mentally retarded,[11] their inclusion in these studies may skew the results of well-controlled dietary therapy in favour of normal mental development and function.

(6) Lack of knowledge regarding the natural history of phenylketonuria, and particularly the interaction of environment with genotype, preclude valid conclusions from the available studies of dietary therapy.

Your desire to adopt the best screening tests for phenylketonuria and other inborn errors of metabolism is certainly justified. It is hoped, however, that the Guthrie test will be used, with other more definitive tests, to select a group of patients with true phenylketonuria, on whom a controlled trial of dietary therapy may be conducted. Only by this means will we be truly able to determine the efficacy of this therapy.—I am, etc.,

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Library Sale in Edinburgh

SIR,—As a former Librarian of the Royal Medical Society I welcome the interest and sympathy expressed in your leading article (1 February, p. 269) on the sale at Sotheby's of a large part of our collection.

It is, however, I think necessary to draw attention to some of the more positive aspects of this transaction. The Society will retain its unique and priceless collection of *Dissertations*, and in addition to this has selected over 150 volumes of particular domestic interest to represent in nuclear form the library which it formerly possessed. The Society has also taken steps to ensure that it will not sell any books of which there are not already copies in one or other of Edinburgh's medical libraries.

The decisions which have led to this sale have not been easy to make and they have not been taken lightly. As you rightly imply, the burden of caring properly for such an enormous asset has become too much for the resources of an undergraduate society. Nevertheless, we will not be entirely bereft of our tangible links with the past, and what we do retain should increase in value by being better preserved and more readily accessible.

I firmly believe that this radical step, sad in some ways as it is, will ultimately add to the strength and prosperity of a flourishing and important institution.—I am, etc.,

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Utilization of Folate in Man

SIR,—About 75% of derivatives of folic acid (pteroyl-L-monoglutamic acid) in a normal diet are claimed to be heptaglutamates,[1 2] but this may not be so.[3] These polyglutamates could be used by degradation of the heptaglutamate to the monoglutamate followed by absorption of the monoglutamate. The action of phenytoin in producing subnormal serum folate levels has been attributed to the inhibition of an intestinal conjugase converting heptaglutamate to the monoglutamate.[4 5] Since the *p*H optimum of this enzyme is 4·6 and that of the intestinal lumen is from 6·5 to 7·0, unless this enzyme has an unusually wide range of activity it should be ineffective.

Dr. I. Chanarin and Miss Janet Perry (30 November, p. 546) claim that the polyglutamate forms of folate are absorbed and utilized to about one-third of the extent of the monoglutamate forms. Examination of the data suggests that this is not so, and that in these experiments the polyglutamate folates in yeast were not absorbed. Dr. Chanarin and Miss Perry describe the heptaglutamate form present in yeast as the pteroyl-L-heptaglutamate. Analysis of the folates present in yeast shows little of this form present. The forms present are the N^{10}-formyl derivative of either pteroylheptaglutamate or dihydropteroylheptaglutamate or both (possibly derived during the analysis from N^{10}-formyl-5,6,7,8,-tetrahydropteroylheptaglutamate) 63%; the N^5-methyl derivative of dihydropteroylheptaglutamate or tetrahydropteroylglutamate or both 20%; and the N^5-formyl derivative of tetrahydropteroylheptaglutamate 14%.[6] A similar complexity is found in the 3–5% of yeast folate present as the monoglutamate. Since the proportions of those forms of the mono and heptaglutamate with substituted reduced pteridine rings may vary with the foodstuff and diet it seems impossible to extrapolate Dr. Chanarin and Miss Perry's results with yeast into a general statement about dietary folates.

Dr. Chanarin and Miss Perry use microbiological assay with *Lactobacillus casei* and *Str. faecalis* to assess the amount of folates present and in some cases to identify them. Using standard solutions of pteroyl-L-monoglutamic acid we found microbiological assay with *Str. faecalis* gave results in good agreement with the chemical standard, while *L. casei* gave results which varied as much as 50% from the standard.[7] Any scientific deductions drawn from small variations in the response of *L. casei* or in small differential responses between *L. casei* and *Str. faecalis* are therefore probably invalid.

The major part of the paper by Dr. Chanarin and Miss Perry determines the utilization of polyglutamate by short-term studies using urinary excretion of folates, and