Forensic Pathology

SIR,—I refer to your report in the *Journal* (April 27, p. 1151) giving an account of Professor C. J. Polson's paper at the recent international conference which I attended in London. His suggestion that there should be some form of national forensic pathology service is not entirely new, and I think most pathologists would agree that the cover for this work in the country as a whole is rather uneven.

Very considerable advances have been made in the quality and availability of pathological services since 1948, and there are now many more consultant pathologists in the hospital service. These changes have resulted in a better over-all standard of coroners' necropsies in most areas, and the suggestion made in Professor Polson's plan that ultimately this work should be taken away from hospital pathologists should be resisted as undesirable and impracticable. The large majority of these necropsies (perhaps 95%) are matters of pure morbid anatomy and intelligent interpretation and fully within the competence of the hospital pathologist. They are in fact best performed by doctors who are also dealing with hospital necropsies, and in my opinion it would be a retrograde step to set up a new special group of pathologists dealing only with coroners' necropsies and entirely divorced from hospital and clinical work.

I suggested in my presidential address to the Association of Clinical Pathologists in 1961¹ that outside London and a few of the larger cities which require special consideration the whole coroner autopsy service could be based on hospitals. There are now many joint user arrangements of mortuaries between the hospital service and the local authority, and these could well be extended with the saving of the pathologists' time and the avoidance of unnecessary duplication of buildings.

A two-tier structure should be considered: (a) routine coroners' necropsies to be done by the hospital pathologists, and (b) the investigation of murder, manslaughter, and other cases involving serious criminal proceedings to be done by special forensic pathologists, one or more of which would be attached to each forensic science laboratory. The hospital pathologist could at any stage call in the specialist if he found a case to be outside his experience or beyond his proper competence.

Many pathologists will support me in thinking that these matters are getting a little out of balance, and that widening the gulf between pathology and forensic pathology is to be deplored. There is certainly a place for the specialist, and there is need for more training of the hospital pathologist in forensic pathology, but I see no justification for a new service on the lines suggested.—I am, etc.,

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Exeter, Devon.

G. STEWART SMITH.

REFERENCE

- ¹ Smith, G. S., *Med. Sci. Law*, 1962, p. 244.

Rickets in Immigrant Children in London

SIR,—I found the article by Drs. P. F. Benson, C. E. Stroud, N. J. Mitchell, and A. Nicolaides (April 20, p. 1054) of particular interest, as Dr. J. D. Gray and I had investigated the problem of early diagnosis of rickets at Belgrave Hospital for Children over a period of two and a half years from 1946 to 1948. Our patients

must have been drawn from an area of London which overlaps that described in the present paper, but in our series there were no West Indian children, as the investigation was carried out before immigration had come into vogue. In five years the paediatric departments of Guy's and King's College Hospitals discovered 16 children under 3½ years with florid rickets. In our series¹ three cases of severe rickets were found in approximately half that time, the serum alkaline phosphatases being between 80 and 220 King-Armstrong units. We were particularly interested in confirming the diagnosis in suspected cases of rickets before bone deformities and radiological changes left no room for doubt about the clinical entity—for example, in infants and children showing non-specific symptoms and signs such as irritability, poor muscle tone, an unexpectedly large anterior fontanelle, etc. Forty-six children under 3½ years were found with a serum alkaline phosphatase raised above our maximum normal level of 20 K.-A. units. (Fifty-six controls gave an average of 17 K.-A. units with a spread of 11-29 units.) A serum alkaline phosphatase of 30-40 K.-A. units was commonly found in children who presented with symptoms but no conclusive physical signs.

In the paper by Dr. Benson and his colleagues each child was considered to have florid rickets, and the serum alkaline phosphatase was more than 45 units in all except two instances. At the same time they found that the calcium and phosphorus levels were frequently within normal limits, once again confirming the observations made by Barnes *et al.*² that the serum alkaline phosphatase is a much more reliable indication of the activity of the disease.

With specific therapy in the form of large doses of vitamin D it is usual for the serum alkaline phosphatase level to fall steadily, but transient increases have been noted by several observers,^{3,4} and occurred in four out of 20 of our cases in whom serial estimations were made. I should be interested to know whether this phenomenon was seen among any of the children in this series recorded by Guy's and King's College Hospitals, and also if the authors can give any information regarding the numbers of children they have seen with doubtful or suspected rickets where the serum alkaline phosphatase has been in the order of 25-40 K.-A. units.

Finally, Dr. Benson and his colleagues have given a timely reminder that there are children in the United Kingdom still who are in need of, but are not receiving, the benefits that the State has provided for them, owing largely to the ignorance and apathy of their parents.—I am, etc.,

Barcombe, Sussex.

F. STEPHEN CARTER.

REFERENCES

- ¹ Gray, J. D., and Carter, F. S., *Arch. Dis. Childh.*, 1949, **24**, 189.
² Barnes, D. J., Kaucher, M., and Munks, B., *Amer. J. Dis. Child.*, 1946, **71**, 622.
³ Josefsson, E., *Ann. Paediat. (Basel)*, 1941, **157**, 169.
⁴ Yieh and Wissler, H., *ibid.*, 1939, **152**, 348.

Obliterative Pulmonary Hypertension

SIR,—During recent months there has been a marked increase in the volume of papers and correspondence in the literature on this and related subjects. The amount of interest which is now being directed to this disease would suggest that either it is becoming more common or that its existence as an entity is more widely appreciated. I believe the latter, at least, to be true, but am also

convinced that awareness of the condition has not yet reached its warranted level among the medical community as a whole.

Thrombo-embolic pulmonary hypertension may not be a common disease, but its early diagnosis is literally vital. As with other malignant conditions, it should be actively sought in those patients known to be at risk, rather than be left to present at a late and irreversible stage. It is in the hope of bringing this tragic condition into greater prominence, particularly in the minds of those responsible for post-natal care, that I am permitted to submit the following case summary:

In November, 1960, a healthy young woman, aged 24, was delivered of her first child after a prolonged and difficult labour. Her pregnancy had been normal apart from some discomfort in the leg veins during the last six weeks and three fainting attacks during the two weeks prior to confinement. In the puerperium she was treated for a mild degree of anaemia, which was considered to be responsible for the ease with which she became tired. During the fifth month post-partum she "fainted" on two separate occasions while pushing her pram up a steep hill. Thereafter she appeared to be fairly well, although she did not quite regain the vitality for which she was noted before the pregnancy.

Her second pregnancy was in its 24th week when, in February, 1962, she again fainted after walking briskly up a steep hill. Subsequently she became progressively more fatigued and subject to mild dyspnoea on exertion, these symptoms being attributed to the combined stresses of her pregnancy, the rigours of caring for her first child, and a moderate anaemia which was refractory to treatment due to an intolerance for iron. From the 30th week she also had considerable trouble with varicose veins of both legs, which further reduced her vigour.

Two months after the uneventful birth of her second child the patient suffered three severe syncopal attacks in the space of a few days, each following only mild exertion. The diagnosis of obliterative thrombo-embolic pulmonary hypertension was then made, and it was concluded, following thorough investigation, that the condition dated from about the time of her first delivery. Despite intensive therapy, including the use of anticoagulants, her condition rapidly deteriorated and she died in December, 1962, leaving two children aged 7 months and 2 years respectively.

This particular case heavily underlines the observation of Dr. J. F. Goodwin, Professor C. V. Harrison, and Dr. D. E. L. Wilcken (March 23, p. 777) that "the clinical signs of pulmonary hypertension may be unimpressive . . .," for the patient was a midwife, and her husband a doctor who, for more than a year after the birth of their first child, was registrar to a cardiovascular unit.—I am, etc.,

Paignton, Devon.

D. M. G. MURPHY.

** Leading articles on this subject have appeared in the *Journal* this year in the issues of January 5 (p. 3) and March 23 (p. 764).—ED., *B.M.J.*

Hypersensitivity and Hypnosis

SIR,—In two important contributions (April 6, p. 925, and April 13, p. 990) Dr. Stephen Black has demonstrated that even so localized and fairly constant an allergic phenomenon as the Prausnitz-Küstner reaction is often diminished by hypnosis. The methods used were temperature-readings, colour photography, and measurement of size of weal and of skin density at the site of the reaction. In a large proportion of individuals concordant results were obtained showing a diminution of the reaction under the influence of hypnosis. Nevertheless, especially in the weal-size experiments, occasional exceptions occurred, which the author attributes to faulty techniques. I would, however, submit

that one variable requires further consideration. In our experiments Küstner and I administered both the reagin-serum and the antigen by intradermal injection of 0.1 ml. This is a very exact method, since by tangential injection into the skin surface a measured quantity of reactant is deposited within the epithelial layer. No such accuracy is obtained by the "prick-through-drop" administration of antigen, as used by Dr. Black. Here the needle is pricked vertically through the skin, but one does not know how much of the reactant is picked up by the surface of the needle as it passes through the drop, nor how much of the reactant is deposited in the various areas of the needle track. Some may well end in the dermis and be washed away by the circulation. However, in view of the other concordant results this point is of minor importance.

The chief problem is how to explain this phenomenon. Firstly, it would be well to explore the effect of hypnosis on the intradermal injection of histamine: this may well provide valuable clues. Secondly, one might consider the influence of the abolition of pain by hypnosis. Every injection produces localized pain, causes hyperaemia and transudation. If at the same time histamine is produced in such an area by the interaction of reagin and allergen, the histamine might be spread over a wider area than if pain had previously been abolished by hypnosis. Thirdly, conflicting with this explanation, there is the interesting fact that sometimes such diminished reactivity was seen to persist for many days after the hypnosis. This would point to a far more complex psychic phenomenon. Whichever explanation proves to be correct, Dr. Black's observations have opened up a wide new field of research.—I am, etc.,

Ventnor, I.O.W.

CARL PRAUSNITZ GILES.

** Dr. Prausnitz Giles died on April 21.—ED., *B.M.J.*

SIR,—While I am sure Dr. Stephen Black would not want it so, his paper (April 6, p. 925) could be misinterpreted as describing a characteristic of "hypnosis." He states what I believe to be a correct inference from his data when he attributes the results to "... an emotional or psychic experience of sufficient significance. . . ." To infer from his study that the hypnotic state is anything more than one such experience, or that it is uniquely efficacious in this respect, is unjustified.

Lest I be accused of tilting at windmills let me say that the literature on hypnosis is full of just such undemonstrated assumptions.—I am, etc.,

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JOHN B. STURROCK.

Research in Mental Deficiency

SIR,—I read Lord Taylor's Mental Health Research Fund Annual Lecture (April 27, p. 1107) with the greatest enthusiasm and interest. As a worker in mental deficiency I was also pleased to see how much space had, very rightly, been given to the results of research in this subject. We in mental deficiency practice might feel prouder about this if it were not for one or two brutal truths.

First, those advances in research have been made by workers whose primary interests are in medical, not mental subnormality, problems. Secondly, those who have contributed to any advances in our knowledge of the physical basis of mental illness rarely, if ever, think of mental subnormality as a field for psychiatric research. Thirdly, we full-time practitioners in mental subnormality, a small and somewhat ignoble band, do