Medicine in Ancient India

SIR,—Two years ago there was a symposium under the auspices of the National Institute of Sciences of India on the history of sciences, including medicine, of ancient India.

The following quotations from my paper with S. N. Pandeya¹ regarding the importance of theoretical and of practical training in medical education in ancient India will, I am sure, be of interest to your readers:

1. "The student who obtains his knowledge only from books is like an ass with a burden of sandalwood, for he knows the weight but not the value thereof."²

2. "He who is versed only in books, but not skilled in practice, gets alarmed and confused, like a coward in the battlefield when confronting active disease."³

3. "He who is skilled in practice but is audacious and ignorant of the theory of science, does not receive approval of the learned men and receives capital punishment from the king."⁴

4. "Both these groups of persons (items 2 and 3) are lacking in dexterity and inept in the discharge of their duties, for they know only half the science and are like birds with one wing only."⁵—I am, etc.,

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- ² *Susruta, Sutrasthanam*, chapter 1, verses 3-4.
- ³ *Ibid.*, chapter 3, verse 48.
- ⁴ *Ibid.*, chapter 3, verse 49.
- ⁵ *Ibid.*, chapter 3, verse 50.

Desensitization for Dapsone Dermatitis

SIR,—I have read with much interest Dr. S. G. Browne's comprehensive and well-documented article on this subject (September 14, p. 664). I have used this method, oral administration of dilute solutions of solapson ("sulphetrone"), in many cases of lepra reaction precipitated by sulphone treatment, as well as the less common allergic drug dermatitis. It was originally suggested to me by Dr. E. J. Currant about 1955. I used a different dose schedule from Dr. Browne's, which I think may be worth reporting.

As solapson is unstable in dilute solution I prepared the dilutions once a week from 50% stock solution. The dilutions were 10%, 1%, and 0.1%. Starting with 0.1% solution I gave daily doses by mouth of 1 ml., increasing by 1 ml. each day up to 9 ml. Then 1 ml. of 1% solution was given and similarly increased, leading in turn to 10% solution in similar doses. When 5 ml. is reached, containing 0.5 g. of solapson, tablets may be substituted and gradually increased up to therapeutic doses. By this very gradual increase a slight reaction can be detected, in which event the treatment is stopped until the

reaction has subsided and started again at half the last dose.

This method proved very effective both in lepra reactions and in dapsone dermatitis. Patients have different degrees of allergic sensitivity. I think this method is more flexible and can be more readily adjusted to the individual. Some patients can be rapidly desensitized; others may take a very long time; but some cases of progressive lepra reaction, which appears to be intolerant of any treatment, get symptomatic relief on sub-therapeutic doses.—I am, etc.,

H. JOCELYN SMYLY.

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Tranlycypromine and Convulsions

SIR,—The incidence of convulsions associated with treatment by tranlycypromine is, I understand, not high. Within the last two years, however, out of a total of about 20 patients treated with normal doses of this drug I have had two who developed temporary epilepsy whilst under treatment. Both have been free from attacks since the treatment was withdrawn, and one of them has not had any anticonvulsant drugs for four months.

The importance of this observation is that one should bear this drug in mind as a possible cause of new cases of epilepsy. In this way some patients may be spared the unnecessary investigations, anticonvulsive drugs, loss of driving licence, and other inconveniences associated with this condition, when a simple withdrawal of the tablets may solve the problem.

It may be worthy of note also that one of these patients had only one major convulsion, and volunteered the information that after this event she began to feel better and her depression lifted—a new type of convulsive therapy!—I am, etc.,

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The Title of "Dr."

SIR,—I read with interest Mr. David H. Patey's letter (August 17, p. 447) on the subject of the title of "Dr." His suggestion that the Association approach the Press Council in an effort to get the press to refrain from using the title "Dr." when referring to laymen who are practising medicine in any of its branches is an excellent one. However, even if successful, it could only have a temporary and interim effect. Surely what is actually required is a private Bill before Parliament to exclude all people in this country who are entitled to use a foreign doctorate of any description from doing so if they are in any way connected with the practice of medicine, so that members of the public may know clearly who holds a registered British medical qualification. Such a Bill would, of course, require very careful construction, and it might be that the B.M.A. or the General Medical Council would have to maintain a register of non-British medical

qualifications that are acceptable for use in this country as distinct from those foreign qualifications which enable a practitioner to get on to the *Medical Register*. Otherwise many fully qualified medical practitioners with foreign degrees might be unnecessarily hampered in their work and status, a situation one would wish to avoid at all costs.

Another and perhaps a simpler and better method and one which I would personally commend is that all registered medical practitioners should use the title of "M.D.," as is the case in America, where the system works most satisfactorily. It only needs a lead from the Association.—I am, etc.,

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Chronic Pneumonitis and Asbestosis

SIR,—In a letter (January 12, p. 123) Professor J. G. Thomson discussed the "new pulmonary condition, basal asbestosis," which he had described in a paper¹ then in press.

Professor Thomson's diagnosis of basal asbestosis seems to depend upon the recognition of a limited slight chronic interstitial pneumonitis in conjunction with finding one or several isolated asbestos bodies in the lung juice. However, various degrees of chronic interstitial pneumonitis which are not necessarily limited to the basal portions of the lungs, of unknown aetiology, and not associated with asbestos bodies, are common findings in unselected necropsied hospital deaths,² particularly if the silver impregnation technique for reticulin is routinely employed in the study of the lung sections.

There can be no disagreement with Professor Thomson's statement that the presence of asbestos bodies in the lung merely indicates exposure to asbestos fibres and not the existence of asbestosis. When numerous asbestos bodies are regularly found in conjunction with a diffuse chronic interstitial pneumonitis, the probability that the two findings are related would appear to be overwhelming. When, however, only one or two asbestos bodies can be found and the association with chronic pneumonitis is infrequent, there would appear to be a *possibility* that the pneumonitis is non-specific and the asbestos body a fortuitous finding. This possibility could become a strong probability if it can be ascertained that the person in whom this lesion is found had no significant exposure to asbestos dust. In view of the commonness of chronic interstitial pneumonitis of some degree in the lungs of adults, particularly in the older age groups, it may be wise to adopt a conservative attitude regarding the significance of isolated rare asbestos bodies in the lungs.

Professor Thomson's assumption that the very small amount of asbestos dust which may be found in urban air is sufficient to produce "basal asbestosis" raises the question, "How much asbestos dust

must be inhaled in order to produce asbestosis?" Although we have no answer directly applicable to man, we do have some indirect evidence that fairly heavy concentrations of asbestos dust are required to produce alveolar fibrosis in animals.

Thus, in one experimental investigation¹ exposure to inadequate concentrations of asbestos dust has been held responsible for the dictum that short-fibred asbestos dust was non-pathogenic,⁴⁻⁶ a concept now believed erroneous.⁴⁻⁶ We have found 10 mg. of short-fibred chrysotile dust injected intratracheally in rats to produce only transient focal fibrosis⁴ and exposure of guinea-pigs and rats to extremely fine chrysotile dust (19 mg./c.mm.) for three and a half months resulted in barely perceptible fibrosis in some animals and none in others as long as 10 months later.⁷

The implications in Professor Thomson's communications point up the need for more direct data than are now available on the minimal amount of asbestos dust required for the production of recognizable asbestotic alveolar fibrosis. Our present information, although indirect, suggests that asbestos dust is less pathogenic than is quartz dust on a weight for weight basis.—I am, etc.,

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Transient Focal Cerebral Ischaemia

SIR,—May we reply to the comments of Professor D. Denny-Brown (October 12, p. 930)?

He states that presyncopal symptoms commonly accompany a local cerebral haemodynamic crisis occurring as a result of blood loss, cardiac arrhythmia, or shock. This is not to be denied. But transient ischaemic attacks (T.I.A.) occurring in this setting constitute only about 5% of cases. The arresting feature about the overwhelming majority of T.I.A.s is that they occur without evidence of disturbance in other parts of the central nervous system or in other systems. He further states that the symptoms described may not indicate cerebral ischaemia but may be due to carotid sinus stimulation; but surely the ultimate effect of this stimulation is to produce cerebral ischaemia? The fact is that if the common type of T.I.A. was due to hypo-

tension one would expect to be able to reproduce it before the onset of signs of general ischaemia.

The transient disorders of extreme hypertension are not irrelevant to the task of alerting clinicians, for the case quoted is not the only example we have met of hypotensive therapy being stopped on the assumption that it was causing hypotensive episodes, when in fact the crises were hypertensive in nature.

We did not, of course, produce generalized cerebral ischaemia at pressures of 95, 90, and 70–80 mm. of mercury. These pressures were taken from the digital arteries, as stated in the paper; the cerebral blood-pressure is much lower than this. The question as to whether hypotension is well tolerated in hypertensive states can only be settled by clinical trial. We have other evidence on this point which we hope will be published shortly.—We are, etc.,

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Bacteriological Examination of Urine

SIR,—The problem¹⁻³ is essentially one of obtaining an approximate quantitative measure of the numbers and types of bacteria in urine, with particular reference to counts over 10^6 /ml. I suggest that the drop method^{4,5} using a platinum-glass pipette^{6,7} might be a very convenient method for this purpose.

The pipettes are held in tubes containing 70% alcohol and pre-poured plates of a suitable medium (nutrient agar, blood agar, MacConkey agar, or any desired differential medium) made available as required. Plastic Petri dishes are very convenient. Immediately before use the pipette is "burnt off" and one drop of urine added to 10 ml. Ringer solution using screw-capped bottles or tubes with Astell seals. After mixing, and using a fresh pipette, one drop of the dilution is allowed to form very slowly and fall from a height of about 1 in. (2.5 cm.) on to the centre of the plate. The greater the height the larger the spread. A drop of Ringer is approximately 0.02 ml., so that this procedure adds 1/25,000 ml. of urine to a plate. The size of the drop is given by the

formula $V = \frac{2\pi r \sigma}{d g \phi}$ where V = volume, r = radius of tip, σ = surface tension, d = density, g = acceleration due to gravity, and ϕ is a correction factor. For the greatest accuracy the pipette used for the urine should be calibrated for urine, as this will have slightly different values for σ and d , but this is probably unnecessary for the level of accuracy involved in this type of work. If the count of the urine is 10^6 about 40 colonies are obtained per drop. Three drops may be utilized and the average taken if desired. If a high count is anticipated one drop may be spread over the whole plate using a platinum or iron wire, bent in a gentle curve.

This spreading technique is especially useful because *Proteus* is often present in urine and swarming colonies may spoil the plate. For this reason the well-known

devices should be employed to reduce this spreading—e.g., using a high concentration of agar, a thin layer of medium, a selective medium, counting the colonies at the earliest possible time, etc.

A second dilution can of course be made for very high counts, but this is usually not necessary for a preliminary screening test.

In addition to the convenience of this method (the outfit is very light and portable), surface culture has certain advantages over pour-plate methods.⁷ The pipettes may of course be autoclaved if desired.—I am, etc.,

London W.1.

J. G. DAVIS.

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Ingrowing Toenails

SIR,—Ingrown toenails, also ingrown fingernails, are very common among the masses in China. The question of how to treat them has always been with us; articles in the medical press have been relatively common for the past 40 or 50 years, to my knowledge.

For people who in general walk barefooted or with straw sandals or cotton shoes the ordinary methods of treatment invariably fail, as I found to my cost after about ten years of trial in Chinese out-patient departments. It thus came about that eventually I was forced to attack the nail directly, in spite of all the warnings to the contrary. For the past 20 years or more I have found it entirely satisfactory, and I have even cured myself by the same method. There need be no blood.

The method is to anaesthetize the digit, and with fine sharp-pointed scissors, any granulations having first been removed, cut down the side of the nail so that a sliver of about $\frac{1}{4}$ in (3.2 mm.) or less is free down to the nail-bed. This piece is then avulsed.

With a little practice this can be done without anaesthetic (and one got plenty of practice in China). The point of the scissors must point outward—that is, against the under-surface of the nail and not against the very sensitive "quick." Further, the plane of the blades of the scissors must be in the radius of the digit. Only the fact that this works gives me the courage to write in this way.—I am, etc.,

D. VAUGHAN REES.

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SIR,—In your annotation on "ingrowing" toenails (October 12, p. 884) you rightly commend the efforts of Lloyd-Davies and Brill¹ in showing that conservative treatment can be effective in the management of embedded toenails. It must be appreciated, however, that their