deepest section of the paraffin-embedded block (> 4 mm from the cortex).

Although our definition of excess fibrosis is "imprecise in a histoquantitative setting" we have at least attempted to differentiate osteitis fibrosa from other forms of bone resorption which may not be related to high circulating parathyroid hormone levels. The failure of the bone resorption "rate" to fall after parathyroidectomy in dialysed patients described by Mr. Carroll and his colleagues² could indicate that such patients had another form of resorptive bone disease³ such as acute osteoporosis.

However sophisticated the scanning techniques, the sole use of surface criteria may be unreliable, particularly when osteoid is considered. Measurement of the surface coverage by osteoid is related to the volume of bone, the magnification, and the number of trabeculae in the field, which will be limited in the 3 mm diameter specimens used in Williams's method. It is hazardous to define normality on the basis of a single criterion. We would agree wholeheartedly that techniques should be standardized in such a confused field, but it is important that valid criteria should be chosen, particularly as bone biopsy is the only effective tool for diagnosing osteomalacia.

With regard to Mr. Carroll's comments on the effect of nephrectomy, we were fully aware that production of the key vitamin D metabolite 1,25-dihydroxycholecalciferol is likely to be decreased whether both kidneys have been removed or remain virtually functionless *in situ*. However, we doubt that there is the simple clinical relationship between renal glomerular function, vitamin D metabolism, and bone disease which Mr. Carroll infers from his study of transplant patients. Our sequential studies show that osteomalacia can heal spontaneously without change in renal function. In one nephrectomized patient, and in several others with intact but poorly functioning kidneys, this improvement was associated with deteriorating hyperparathyroid bone disease. Furthermore, others have described nephrectomized patients maintained on haemodialysis without vitamin D supplements for several years with no sign of osteomalacia in their bone histology⁴.—We are, etc.,

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¹ Garner, A., and Ball, J., *Journal of Pathology and Bacteriology*, 1966, **91**, 545.

² Carroll, R. N. P., Tha Aung, Williams, E. D., and Shackman, R., *Proceedings of the European Dialysis and Transplant Association*, 1969, **6**, 276.

³ Woods, C. G., Bishop, M. C., and Nicholson, G. D., *Journal of Pathology*, 1972, **107**, 137.

⁴ Bordier, P. J., and Tun Chot, S., *Clinics in Endocrinology and Metabolism*, 1972, **1**, 197.

How Infectious is Gonorrhoea?

SIR,—Dr. W. K. E. Bernfeld, (21 October, p. 173) is right to ask "How infectious is gonorrhoea?" and Dr. W. F. Felton (18 November, p. 431) is right to stress the need to elucidate this question. On a priori grounds one would expect, in the event of coitus being unguarded by mechanical means, that the infected male would be more infectious to the uninfected female than vice versa. His gonococci may be considered to be thrust upon the appropriate culture medium (cervical epithelium), whereas her gonococci would only reach his

urethral epithelium by seepage in the reverse direction. In many instances urination or washing by the male soon after coitus would dispose of the invading organisms. Some figures for 1970 from Newcastle upon Tyne are relevant to the correspondence concerning the infectivity of gonorrhoea.¹ Of 97 female "subsequent" contacts—i.e., women at risk from known male cases—72 (74%) were found to be infected. This figure would be greater were it not for our practice of treating such "at risk" patients epidemiologically when their initial tests are negative. Untreated male "source" contacts of known female cases are harder to come by owing to the overt nature of male gonorrhoea, which results in the majority of such contacts having already been treated. "Subsequent" male contacts are even fewer in numbers owing to the propensity of promiscuous female patients to "cross-name" those who have named them as the source of their infections. Of 28 male "source" contacts, 16 (67.8%) were found to have gonorrhoea. Of 11 "subsequent" male contacts, 6 (54.5%) had gonorrhoea. The figures are small but suggest the validity of the thesis enunciated above.

If the infectivity of females for males were low the high venereal disease rate and climbing graphs would suggest a vast source of untapped female gonorrhoea to be sought out by contact tracing and screening techniques. With 87.7% of available female "source" contacts and 97.1% of available "subsequent" contacts being traced in Newcastle; with an overall 80.4% venereal disease involvement being found in these contacts; and with a 1.6:1 male:female gonorrhoea ratio amongst all our cases, one would expect Newcastle's venereal disease problem to be all but solved. However, contact tracing merely keeps the rising graph at a lower level than would otherwise be the case (80% of the national average). Moreover, routine screening of obstetric and gynaecological cases is productive of an infinitesimal amount of gonorrhoea, as figures yet to be published will show. This may be related to the successful contact tracing efforts in the region. What emerges from these findings is that the reservoir of untapped gonorrhoea is not so large as has hitherto been surmised and that the presumption therefore that gonorrhoea is highly infectious seems fully justified.—I am, etc.,

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¹ Wigfield, A. S., *British Journal of Venereal Diseases*, 1972, **48**, 37.

Referring Patients for Electrolysis

SIR,—In his letter (2 December, p. 551) Dr. I. W. Caldwell, Chairman of the Dermatologists Group Committee, draws attention to the decision of the Central Ethical Committee that it is now proper for doctors to refer patients requiring electrolysis to members of the two main professional groups which ensure adequate standards of training in this form of treatment.

He criticizes the harsh advice sometimes given to such patients and ends darkly with the remark that he and his committee regret and dissociate themselves from the relevant paragraphs in the B.M.A. Family Doctor booklet *So Now You Know about Your Skin*. As its author, I should be glad to learn which paragraphs are offensive. On

pp. 18-19 I state that most women with superfluous hair do not have glandular abnormality. This is meant to reassure such women that they are not unfeminine. On p. 20 I state that electrolysis is not undertaken by many hospitals and has to be carried out in commercial clinics. I warn against the use of "home kits" for self-treatment and say that chemical depilatories are likely to irritate facial skin. I mention that "if not too bad," superfluous hair may be removed by plucking or the use of wax; or in the worst cases by shaving. It is presumably the mention of the razor to which Dr. Caldwell objects. Surely he and his committee are aware that there are women who shave their legs or armpits, not with pleasure but without disgust? I do not state or imply that shaving is the simple answer to facial hirsuties in women. I affirm that there are cases when the growth of hair is so heavy that electrolysis is impracticable and "shaving provides the best available, though admittedly inadequate, solution."—I am, etc.,

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¹ Textbook of Dermatology, ed. A. Rook, D. S. Wilkinson, and F. J. G. Ebling, p. 1373, Oxford and Edinburgh, Blackwell, 1968.

Tragic Dilemma

SIR,—I think that all who read the reports in the press concerning the child in Hull suffering from spina bifida will welcome your thoughtful leader "Tragic Dilemma" (9 December, p. 567). The treatment of cases of spina bifida poses moral problems that are even more difficult than the medical ones. All concerned with the treatment and care of these patients will therefore welcome the ray of hope for the prevention of this condition that is raised by the preliminary report incriminating "blighted potatoes" as a possible cause of this malformation.¹—I am, etc.,

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¹ Poswillo, D. E., Sopher, D., and Mitchell, S., *Nature*, 1972, **239**, 462.

Idiopathic Scrotal Gangrene

SIR,—I was most interested to read the report by Messrs. B. M. Frier and A. D. Howie (7 October, p. 26) of a case of scrotal gangrene in asymptomatic myeloma as I have recently published a study of eight cases of Fournier's (scrotal) gangrene.¹ In the case described left orchidectomy was performed to achieve a primary closure of the remaining skin. In my experience the testes come to no harm from exposure after removal of sloughs and, once granulations have formed, regeneration of the scrotum allows closure of the scrotal defect by secondary suture without orchidectomy. Many years ago castration was advocated as a part of the surgical management of Fournier's gangrene.² This was abandoned when the rapidity of granulation and scrotal regeneration was recognized, and Randall,³ with his experience of 16 cases, reported that a new scrotum is formed with normal elasticity and mobility on the underlying testes after treatment with dressings alone.

Gangrene of the scrotum has constantly defied attempts to find a unified and

generally accepted theory of aetiology. Most of my cases appeared to be infective in origin, and two of them followed instrumentation. It does appear that the disease is associated with poor hygiene. Fournier's gangrene was fairly common in Europe and North America at the turn of the century but is now rarely reported from these areas. More recent reports of more than one case have come from areas with poor living standards.⁴⁻⁶

It was suggested that the patient described may have had an increased tendency to vascular insufficiency because of a high titre of cold haemagglutinins in his serum. It is possible that this and the trauma he suffered were enough to precipitate gangrene of the scrotum by vascular thrombosis. Several authors have proposed that the cause of Fournier's gangrene is an arterial thrombosis precipitated by an infection, analogous to cavernous sinus thrombosis.⁶⁻⁸ This case could be compatible with the thrombosis theory of aetiology.—I am, etc.,

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- 1 Buchanan, D. J., *Medical Journal of Zambia*, 1972, 6, 141.
- 2 Allen, J., *Journal of Cutaneous and Genito-Urinary Diseases*, 1894, 12, 55.
- 3 Randall, A., *Journal of Urology*, 1920, 4, 219.
- 4 Thomas, H. O., *British Journal of Urology*, 1953, 25, 60.
- 5 Dennehy, P. J., *South African Medical Journal*, 1951, 25, 222.
- 6 Sachdeva, Y. V., and Khanna, B. C., *Indian Journal of Surgery*, 1949, 11, 178.
- 7 Carver, J., *British Journal of Urology*, 1939, 11, 68.
- 8 Mansfield, O. T., *British Journal of Surgery*, 1946, 33, 275.

Asthma in the Elderly

SIR,—May I briefly comment on two points in the paper by Drs. H. Y. Lee and T. B. Stretton (14 October, p. 93)?

Reversible obstruction of the airways occurs very often in both young and elderly patients suffering from chronic non-specific lung disease. The attacks, especially when repeated, are mostly accompanied by signs of alveolar hypoventilation. I feel that the 15 elderly patients studied by the authors fulfilled only partly the criteria for the diagnosis of asthma—no hypercapnia and no reversibility of obstruction were found in most of them. Diminished vital capacity may indicate a restrictive ventilatory disturbance. I wonder if measurement of the FEV₁ alone is suitable for the diagnosis of obstruction; if the residual volume is increased, a low FEV₁ may simply result from high intrathoracic pressure during expiration. Now that body plethysmography is available, assessment of the residual volume and/or intrathoracic gas volume has become very simple, and the resistance of the smaller airways during normal expiration may also be calculated. I suggest therefore that the term "asthma" should be reserved for those cases in which reversible airways obstruction is quite clearly demonstrable and that it should not be used simply as a symptomatic label.

I also disagree with another statement of the authors concerning sputum eosinophilia. Our findings in more than 15,000 samples of bronchial secretion prepared from sputum by Mulder's technique during the past eight years have shown that eosinophilia is a very common consequence of bronchial hyperreactivity. Moreover, a long-term follow-up study in Europe's first model

village for the treatment of chronic non-specific lung disease in Zákányzsek, South Hungary, showed bronchial eosinophilia to be fairly constant in the greater part of a total of 384 patients, only a few of whom ever had real obstructive attacks. Eosinophilia correlates closely with bacterial infection of the bronchi and may serve as a basic indicator in the assessment of airway infections,^{1,2} as active bronchial infection causes the eosinophils to disappear. Since the main physiological role attributed to eosinophils is the local release of histamine and since immune reactions of type I (reaginic type) often cause the production of eosinophil chemotactic factor,³ bronchial eosinophilia seems to be a very complex phenomenon. It is surely oversimplifying the position to say that "sputum eosinophilia is of considerable value in assessing asthmatic patients." It may be of value and it may not.—I am, etc.,

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- 1 Lányi, M., *Lancet*, 1967, 1, 1165.
- 2 Lányi, M., *Deutsche Medizinische Wochenschrift*, 1968, 93, 2390.
- 3 Kay, A. B., Stechschulte, D. J., and Austen, K. F., *Journal of Allergy*, 1971, 47, 118.

SIR—I read with interest the article on this subject by Drs. H. Y. Lee and T. B. Stretton (14 October, p. 93). I am rather concerned, however, about the suggested treatment. While systemic corticosteroids are essential and life-saving at times their constant use has created over the past 10 years the problem of corticosteroid-dependent asthma. In my unit we have been trying over the past 18 months to treat these patients with initial doses of systemic corticosteroid followed by aerosol inhalants of beclomethasone. This is a synthetic, poorly-absorbing corticosteroid, and we have used it successfully in 82 out of 95 cases of late-onset asthma. These have been followed up for 12 months, and we hope to publish the results.—I am, etc.,

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Bicarbonate Solutions for Infusion

SIR,—It has come to our attention that 1.4% sodium bicarbonate infusion solutions are no longer available commercially in the U.K. and that the lowest concentrations being produced regularly are 2.74% (Polyfusor—Boots) and 4.2% (Travenol Laboratories—Baxter). These firms may produce 1.4% sodium bicarbonate as a special order but this often takes several months. It is therefore important that all hospitals which may at some time or another admit acutely poisoned patients should review their procedures for forced alkaline diuresis, as many of these regimens include 1.4% sodium bicarbonate solution.

Sodium bicarbonate solutions are used in forced alkaline diuresis regimens to promote the alkalization of the urine and therefore it might appear that any procedure would be adequate which adjusts the proportion of sodium bicarbonate to the total infusion volume to give a urinary pH of 8. (The pH indicator portion of Bililabstix is not suitable, in our experience, for this purpose and,

if a pH meter is not available, narrow-range indicator paper should be used.) However, owing to the length of time required for the renal compensation of acid/base disturbances, the patient may become more alkalaemic than hitherto before producing a significantly alkaline urine if these more concentrated solutions are used. In our experience there may be a transient rise in the blood levels of phenobarbitone when the phenobarbitone-poisoned patient first develops a base excess, and we are concerned that this rise may be considerably higher if the rate of infusion of alkali in a forced alkaline diuresis regimen is increased at any time. It would appear to us that one method which would overcome this difficulty is to give the sodium bicarbonate simultaneously with other infusion fluids connected with a Y-piece, but we would be interested to hear what other centres are planning to do.—We are, etc.,

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Normal Range of "Effective Thyroxine Ratio"

SIR,—Standardized reagent kits for measurement of the "effective thyroxine ratio" (E.T.R.) have become available recently (Mallinckrodt U.K. Ltd.). This test has been shown^{1,2} to have a diagnostic accuracy comparable to that of a free thyroxine index in categorizing patients according to their thyroid status. Since the tentative normal range (0.86-1.13) published by the manufacturers was based upon a population of wide geographical distribution, users were recommended to establish their own normal values. Having used this test routinely in our biochemistry department for some months, we have now analysed our first 1,000 results with this object in view, and present our findings here for the interest of other current or potential users.

We used the reagent kits according to the manufacturers' instructions, except that a pool of at least 50 human sera taken from routine samples was used as a standard instead of the lyophilized material supplied; this modification was found to improve the precision of the assay (from S.D.=0.032 to S.D.=0.019 within batch).

The results, plotted on probability paper according to Hoffmann³ are shown in the Figure, where it can be seen that the cumulative percentage incidence of the values found is linear between approximately 10 to 85%. It seems reasonable to assume that this range of intermediate values, containing some 75% of all the results, is derived from euthyroid subjects and that, since the linearity of the graph in this region indicates a Gaussian distribution, the range of E.T.R. values for 95% of euthyroid subjects may be obtained by extrapolation of this straight line as suggested by Hoffmann.³ The range thus found is 0.88-1.09. This is similar to that found by Thorson *et al.*¹ in 198 normal subjects (0.89-1.09), but broader than that found by Murray *et al.*² in 80 such subjects (0.89-1.05). The latter authors suggested the existence of "borderline" zones between 0.86 and 0.89 and between 1.06 and 1.09; this is consistent with our own findings, as shown in the Figure, where deviation from