## Cholera

SIR,—I read with great interest your leading article (June 10, p. 1664) on cholera. The mortality figures referred to by you indicate that cholera has been receding steadily during the last decade, both from eastern India and from East Pakistan. This trend of recession had been distinctly noticeable in Calcutta till the last year. The current epidemic of cholera in Calcutta has, however, shot up the mortality to a level considerably higher than that of the last year. Perhaps the welcome decline in the incidence of cholera throughout India is not quite irreversible. Complacency arising out of apparent recession of cholera in India may lead to slackening of efforts, and thus may spell disaster in certain big cities of India where population tends to go up and sanitation tends to go down.

I should also be inclined to believe that you did not intend to convey the impression that, given adequate resources and enthusiastic public co-operation, mass immunization could eradicate cholera from the permanently endemic areas in spite of the "combination of dense population and abysmal sanitation" which, as you have truly pointed out, still prevails in these areas.

It is true that cholera vaccine has contributed to a certain extent towards prevention of cholera. Nevertheless, it would be unwise to ignore the scepticism which still persists among some scientists regarding the magnitude of prophylactic efficacy of cholera vaccine. Wilson and Miles[1] have stated that "nothing, we believe, but a properly controlled trial will tell us what the real value of vaccination is." The Study Group of the World Health Organization[2] have recommended "that a field trial was absolutely necessary in order to demonstrate that the protective efficacy of cholera vaccine is correlated with its performance in a laboratory potency test."

During the recent conference on cholera which was sponsored jointly by the S.E.A.T.O. and the National Institutes of Health, U.S. Public Health Service, and was held in Dacca, East Pakistan, December 5–8, 1960, Smadel[3] said that "he was impressed with the unanimity of opinions that cholera vaccine was of some value even though every one stated that conclusive scientific data on its efficacy were lacking. It was apparent that an adequate field evaluation of cholera vaccine was necessary."

These considered remarks would indicate that perhaps it would be far more discreet to subject the cholera vaccine to the test of well-controlled field trials before involving an underdeveloped country into the colossal expense and effort essential for mass immunization campaign. If the areas for conducting the field trials are carefully selected, even a small fraction of the expense and the effort required for effective mass immunization may fill up the existing lacuna in our knowledge on immunization against cholera.—I am, etc.,

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## Premalignant Conditions of the Vulva

SIR,—The paper on premalignant conditions of the vulva by Professor T. N. A. Jeffcoate and Dr. A. S. Woodcock (July 15, p. 127) will be of interest to dermatologists chiefly on account of its admission that vulvectomy is not a reliable method of relieving intractable pruritus vulvae. This is at least an advance on the gynaecological aphorism, "If it's white and it itches, I cut it out," heard from other lips some ten years ago at a special meeting in a provincial teaching hospital. The authors go on to state that prophylactic vulvectomy for leucoplakia is usually unjustifiable. They may be right, in which case the gynaecologists will have more time for other things and the dermatologists will be left to struggle on by themselves.

However, the evidence they present is far from convincing, and, as a serious contribution to the advancement of knowledge on the subject, their paper is profoundly disappointing. To the dermatological mind it contains so many discrepancies and inconsistencies that space permits comment on only the most important points. After comparing selected extracts from the literature over the past 55 years and showing that, not unnaturally, there have been various conflicting opinions during that period, they presume to dismiss them all contemptuously and try to lead us back to the jungle. The same could as easily be done in relation to any other group of diseases. But this does not justify a repudiation of the careful work, much of it by dermatologists, on which are founded the modern criteria for the differential diagnosis of the intractable itching dermatoses affecting the vulva.

Lichen sclerosus et atrophicus occurs frequently in other parts of the body. It is seen rarely in men, not so rarely in young girls. It can scarcely be attributed, therefore, to an ageing process in the tissues and it is absurd to deny its very existence. Leucoplakia is seen by dermatologists as frequently on the lip as on the vulva, and the essential histological features are the same in both sites. Few would question its precancerous nature on the lip, and those who believe the same to be true as regards the vulva are unlikely to change their opinion without really convincing evidence. As in all fields, some atypical cases are perplexing, but there is fairly general agreement nowadays, among dermatologists at any rate, not only that leucoplakia, lichen sclerosus et atrophicus, and neurodermatitis are three distinct diseases, wherever they may be found, but also on the combined clinical and histological criteria by which they are differentiated. The current views on their prognosis in the vulva may be wrong, but any evidence suggesting their modification is unlikely to be accepted unless these diagnostic distinctions are kept.

Such advances can only be made, as the authors rightly maintain, by further careful observation and even more frequent recourse to biopsy. Yet here is an even greater obstacle to enlightenment. It would seem from the authors' photomicrographs and the words beneath them that gynaecologists have ideas very different from dermatologists on the histological interpretation of vulval sections. For instance, Fig. 1, labelled histologically as "hypertrophic leucoplakia," depicts, to the dermatological eye, simple lichenification, i.e., the result of prolonged rubbing, whether a neurodermatitis or due to an external irritant such as moniliasis. Similarly Fig. 2, labelled "atrophic leucoplakia," is clearly lichen sclerosus et atrophicus; Figs. 3 and 4

suggest Bowen's intra-epidermal cancer; Fig. 5 is non-specific, and Fig. 6, the only section showing undoubted characteristics of leucoplakia, is described by the authors as neurodermatitis of 20 years' duration! Is it possible that rubbing and scratching may be one of the factors in the aetiology of leucoplakia?

It would seem that any author wishing to find general acceptance for his opinions on this subject would be well advised, be he gynaecologist or dermatologist, to seek the co-operation of his colleagues in the other specialty in all stages of his work.—I am, etc.,

Exeter. J. R. SIMPSON.

SIR,—For a name to have any value there must be general agreement about what it denotes, and I cannot help having some sympathy with Professor T. N. A. Jeffcoate and Dr. A. S. Woodcock (July 15, p. 127) in their desire to abandon practically all those names attached to various well- and ill-defined conditions of the vulva which may or may not subsequently undergo malignant change. Abandoning several names, however, does not immediately make several different conditions any less different, and there is a danger thereby of abandoning at the same time useful knowledge tediously acquired by authors whose only fault was to choose an unsatisfactory name. Any constructive review of a difficult subject must include the opinions of previous reviewers, but surely those who take on such a task must exercise a little discrimination or the result is likely to be purely destructive, and once again there is the danger of abandoning useful knowledge tediously acquired—this time by authors whose only fault was to correct someone who was talking rubbish.

Dermatologists may perhaps err in keeping an old name like leucoplakia to denote no longer white plaques as such but areas of unstable dyskeratotic epithelium, but at least they know and endeavour to teach others what they mean—and they do not mean lichen sclerosus et atrophicus. Yet brown and ginger hair can both end up as grey. It would be a tremendous help to those puzzled by the condition known as lichen sclerosus et atrophicus if they would study it as it occurs on other parts of the body and then apply the knowledge gained to their exercises in more unusual sites such as the glans penis and the vulva. The histology is well shown in Fig. 2, a biopsy section from a patient who apparently also had a fungus infection of the feet.

If one thing is clear from all the wrangles of terminology in the past, it is that there are several rather similar conditions of the vulva, some easily reversible like the simple lichenification shown in Fig. 1, some less easily so, and some not reversible at all. Some, perhaps any, of these may become malignant, some sooner, some later. Careful study of the natural history of these conditions leads to the grouping of similar cases, and this should not be wasted or discouraged by lumping them all together as just chronic epithelial dystrophies, either because of ignorance of the work of others or because of difficulties of nomenclature.—I am, etc.,

Plymouth. R. D. SWEET.

## Chloroquine and the Eye

SIR,—Although it seems fairly certain that there is a true and definite relationship between prolonged chloroquine medication and the ocular changes described, this may not be the whole truth. I have observed somewhat similar changes occurring in patients receiving different forms of treatment for rheumatoid arthritis—for example, gold and phenylbutazone. These cases were being followed up at the Birmingham and Midland Eye Hospital, as they presented the features of Sjögren's syndrome. Since chloroquine is mainly used in precisely that type of case in which Sjögren's syndrome occurs—namely, lupus erythematosus and rheumatoid arthritis—might the ocular changes really be manifestations of an exacerbation of this syndrome? It is possible that the metabolism of epithelial cells is running at a low level in Sjögren's syndrome and drugs such as chloroquine, by inhibiting adenosine triphosphatase, depress metabolism to such an extent that symptoms arise.

I grant that the corneal picture as described by Mr. A. H. Osmond (July 15, p. 177) is not very characteristic of kerato-conjunctivitis sicca, but I have seen one or two cases where radiating lines have appeared before the onset of a filamentary keratitis. Similar changes have also been described by Cristiansson,[1] who describes a "spoke-like pattern in the deeper corneal epithelium" in a case of kerato-conjunctivitis sicca complicating jaundice.

I was also interested to note the occasional complaint of night-blindness in patients on chloroquine. Here again, this symptom has been noticed in Sjögren's syndrome and an impairment of the visual threshold and in the rate of adaptation was noticed in some 15 cases.

The issue, therefore, appears to be whether chloroquine by itself causes these eye changes or whether they occur only when chloroquine is administered to patients with an undetected Sjögren's syndrome. Schirmer's test may be helpful in deciding the issue.—I am, etc.,

Birch Hill Hospital, Rochdale. JOHN MCLENACHAN.

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## Pancreatic Lithiasis

SIR,—May I refer to Dr. P. Rabindran's letter (May 6, p. 1322) about my article on pancreatic lithiasis (March 4, p. 626)? Though investigations regarding pancreatic functions were done pre-operatively in many cases and post-operatively in some, the investigations have not been complete enough to draw any conclusion. But the general condition of the patients has improved considerably and they are back at their respective occupations. As regards sphincterotomy, a perusal of the literature shows that on the whole the results of this operation are unsatisfactory. Sphincterotomy was not done except in Case 3, where cholecystojejunostomy also was done at the same time. There was relief for only one year, when nerve resection was done (left side in October, 1954, right side in July, 1955), and he has been free from pain thereafter. In Case 6 sphincterotomy was attempted, but the sphincter could not be located even when attempts were made by passing a Desjardins's forceps and catheters from the duct opened over the head of the pancreas.

I don't think cholecystojejunostomy was responsible for the disappearance of the calculi. In the two cases of calcareous pancreatitis where the calcified shadows disappeared (Cases 3 and 4), cholecystojejunostomy did not bring about any relief of pain. The pain disappeared