

## FOCAL SEPSIS.

*A Correction.*

SIR,—May I correct a serious misstatement in the abridged report of my share in the discussion of this subject, which appears in the current number of the JOURNAL? In describing what might be considered as the pathological test of a "septic tonsil," the report states—

"Septic tonsils were associated with excess of polymorphonuclear leucocytes on the surface or in the substance of the gland, the presence of bacteria or debris in the gland not being of such great importance."

What I did state—and I quote from the typed copy of my remarks—was to the following effect:

"A tonsil is of pathological significance when one or more of its crypts contain an excess of polymorphonuclear cells, or when these are found making their way between the epithelial cells which line the crypts. The mere presence of bacteria in these recesses is no evidence of disease, but if and when such organisms pass through the epithelium into the lymphoid follicles of the glands then morbid conditions may arise, productive of local or systemic symptoms."

And again:

"... the small plugs of epithelial debris which can generally be squeezed from a tonsillar crypt have no pathological significance, although they may be of importance in that they provide a suitable medium for bacterial growth. Only when this assumes such a proportion as to destroy the normal epithelium and induces excessive polymorphonuclear migration will the portal be thrown open to invasion of the lymphoid tissue by the organisms and their toxins."

I think you will agree that these statements give an entirely different view of the matter from the inaccurate and distorted one which appears on page 324 of the JOURNAL, wherein no mention is made of the essential site and method by which infection enters the gland tissue of the tonsil.—I am, etc.,

London, W.1, Feb. 22nd.

HERBERT TILLEY.

\*\* Peccavimus! It is sometimes possible to give the gist of sixteen lines in four, but here the attempt failed, and Mr. Tilley has not let us off.

## COCCIDIA OF FISH IN HUMAN FAECES.

SIR,—I fear that in my letter which appeared in your last issue (February 26th, p. 347) my meaning may be subject to misinterpretation. When I wrote that I had pointed out to Dr. Thomson that I had suggested, in the manuscript of my forthcoming book, that the coccidia in question might be merely coccidia of fish I did not intend to imply that his work had been undertaken in consequence of this. As a matter of fact it was only after his work had been commenced and the significance of it realized by him that I told him my views on the subject. It is clear that the view as to the possible origin of these coccidia had occurred to both Drs. Thomson and Robertson and myself independently.—I am, etc.,

London, W.C.1, Feb. 19th.

C. M. WENYON,  
Wellcome Bureau of Scientific Research.

SIR,—My attention has been called to the article by Drs. J. G. Thomson and A. Robertson regarding coccidia in fish, and, while their findings are very interesting, I protest strongly against their statement that "systematic zoologists" adopt "an attitude of superiority" towards medical men.

Anyone acquainted with the works of the zoologists Schaudinn, Grassi, Minchin, Doflein, etc., would not accuse such of neglecting or unduly criticizing the findings of medical men, and such statements are liable to have a bad effect on the workers in both medicine and pure science.

The one case which Drs. Thomson and Robertson refer to rather cryptically appears, illogically enough, to concern a worker who is engaged, not in systematic zoology, but in medical research, and in any case they might reflect that one does not condemn the whole race of birds for one butcher-bird.—I am, etc.,

February 22nd.

J. S. DUNKERLY,  
Lecturer in Protozoology and Helminthology,  
University of Glasgow.

## CYESOEDEMA.

SIR,—The condition of cyesoe'dema described by Dr. A. Lendon in the BRITISH MEDICAL JOURNAL of December 19th, 1925 (p. 1179), is not of such rare occurrence as would appear from reading his article.

This general thickening of the cutis of the pregnant female, as already pointed out by me some time ago,<sup>1</sup> is, as a matter of fact, a common accompaniment in all pregnancies. But while it is well marked in some cases, especially in primiparae, it is in others of so slight a nature as almost to pass unnoticed.

According to Tandler and Grosz this "coarsening of the facial features and the thickening of the skin is due to a proliferation of the subcutaneous tissues." This is in keeping with the general proliferation and overgrowth in other tissues of the female during pregnancy—for example,<sup>1</sup> thickening of the mucous membrane of the respiratory tract, which is probably responsible for the huskiness of voice and intractable coughs which are often met with in pregnancy. These proliferative changes are brought about by the functional increase in the physiological activity of the "hormone of growth" (anterior pituitary), obviously necessary at that period. It is therefore not surprising to find that, when the increased functional activity of the anterior lobe of the pituitary becomes excessive, as evidenced by a marked cyesoe'dema, the foetus should in such cases also be of greater weight than usual. This coincides with Dr. Lendon's observations on the cases he reported.

Dr. Lendon also states that cyesoe'dema did not recur in subsequent pregnancies which came under his notice. My experience is, however, that it does recur, though but rarely, and in such cases<sup>1</sup> we get the condition known as progressive enlargement of the foetus.—I am, etc.,

Capetown, Jan. 26th.

S. E. KARK.

## INDIVIDUAL OVERDOSE OF ULTRA-VIOLET RAYS.

SIR,—With reference to Dr. Paige Arnold's interesting letter in the JOURNAL of February 13th (p. 304), I may state that I have noticed the symptoms he describes in many patients treated during the last eighteen months with the quartz mercury vapour lamp. Such untoward symptoms—lassitude, depression, headache, etc.—I have always found to be associated with low blood pressure readings. Given an unduly low blood pressure one may expect such a patient to be intolerant to ordinary doses of ultra-violet rays.

By giving subminimal doses, possibly exposing only the face and forearms to the rays, with several days' interval between the doses, these patients will often respond as the ordinary individual does, and show increased energy, appetite, and a feeling of *bien-être*. There is a very definite "critical point" in the dosage of such cases, and this must not be passed.

Babies and toddlers who are thus sensitive often become irritable, sleepy during the day but restless at night, and begin to lose weight.

Dr. Arnold wonders whether ultra-violet radiation is as innocuous as is generally supposed, and suggests the possibility of untoward results such as followed on the pioneer use of x rays. We have it on the authority of Drs. Sequeira, Axel Reyn, and others with over twenty years' experience of the rays that no such dangers exist. I would, however, suggest one possible danger—that of cataract, such as is met with in glass workers, tin-plate workers, chain-makers, and iron-smelters. Here cataract probably arises from insufficient protection against ultra-violet rays emitted from these molten substances affecting the workers improperly protected, and is not due to heat waves, as formerly supposed. It is known that coagulation of the crystalline lens occurs with exposure to ultra-violet rays even with wave-lengths of 2,800 to 3,220 A.U. This coagulation is only made evident in the presence of calcium chloride solution. These facts are significant, and at any rate strongly suggest that operators of ultra-violet lamps

<sup>1</sup> The Toxaemias of Pregnancy, *South African Medical Record*, April 8th, 1922.