and I know of no certain proof that the pregnant uterus even at full term ever does press on the ureters, to a sufficient degree to interfere materially with the flow of urine. This is one of those statements which is repeated by one writer after another, but which is probably incorrect and anatomically impossible. In most of the cases of dilated ureters complicating pregnancy which have been described the site of the dilatation was in reality below the level of the pelvic brim. The striking frequency of right-sided infections is possibly to be explained by the ureteric canal being rather better developed, as a general rule, on the right side than on the left, but the truth of this is for the anatomists to determine, and impressions gained from operation on patients with carcinoma of the cervix may well be misleading, but this is the impression I have formed when performing such operations entailing as they do the dissection and complete exposure of the lower ends of both ureters.

The presence of the ureteric canal is, I believe, then, the explanation of the greater frequency of *Bacillus coli* infection in the female as contrasted with the male, and is probably also the explanation of its greater frequency on the right side than on the left, and affords the only feasible explanation of the frequent occurrence of bacilluria in the early months of pregnancy at a time when any pressure on the ureters by the pregnant uterus is quite out of the question.

I am, Sir, yours faithfully,

Wimpole-street, W., June 26th, 1922. G. BLACKER.

THE CAUSATION OF "CASCADE STOMACH."

To the Editor of THE LANCET.

SIR,—It is a matter of considerable surprise to me to find that your reporter of the meetings of the recent Congress of Radiology and Physiotherapy in his note of my contribution there on "cascade stomach," instead of giving a synopsis of what I said (as he does in the case of all the other papers), comments adversely as follows:—

"Dr. J. H. D. Webster described the causation of cascade stomach; some of the examples given by him in support of his theory would hardly have been accepted by other workers as characteristic 'cup and spill' stomachs." THE LANCET, June 17th, p. 1210.

Comment of this nature would have been welcomed at the meeting, and have added to the interest of the discussion (which in so far as it referred to my paper was only favourable), but as many months may elapse before my paper can appear in the *Archives of Radiology*, or elsewhere, I should like to point out now that in my view this comment of your reporter contains errors in fact and in principle. Its errors in fact are, first, that I have not (and no one could have) one theory which could account satisfactorily for the great variety of cases included under the term "cascade stomach," and secondly, I gave only two examples of my own, one of which was typical, while the second corresponded to other already published examples. More seriously, its error in principle, in my view, is that what purports to be an unbiased record of proceedings is surely not the place for anonymous criticism. Am I not right, Sir, in saying that criticism in a report of this nature should only take the negative form of omission of matter which (in the reporter's view) is of slight moment and not worthy of record?

Criticism not made at the Congress itself would be more fair and suitable if deferred till my paper appears in print, when your readers will have the paper before them, or accessible, and can judge for themselves the facts and views to which your criticism refers. At present comment is almost like a review of an unpublished book. In general, criticism or approval of new, or obscure, or rapidly changing medical questions cannot be justly valued if the writer remains anonymous: but appearing in your journal it always carries weight. Hence this protest.

I am, Sir, yours faithfully,

J. H. DOUGLAS WEBSTER.

Chandos-street, Cavendish-square, W., June 17th, 1922.

THE INFLUENCE OF THE SUN'S RAYS ON THE SKIN.

To the Editor of THE LANCET.

SIR,—The leading article on this subject in your issue of June 24th is highly interesting, and will assist in a better appreciation of the action of the sun as a cause for disease. I am, however, at a loss to understand the implied pathology in normal persons. The commencement and the end of the article suggest a purely local action like that of a chemical irritant or destructant. Then in the second paragraph you refer to the direct action of the rays in normal people determining the localisation of certain eruptions of internal causation. Is it not possible that the actinic rays cause some nervous activity in the nature of a defence?—this defence being erythema in fair-skinned, and increased pigmentation in dark-skinned, persons. In both these instances the suggestion is defence, inasmuch as the actinic rays do not easily penetrate the blood or dark pigment. Where papules, vesicles, and bullæ form, they would seem to be analogous to the formation of a blister in scalding or burning, again defensive.

The first integumentary defence against excessive actinic energy is the sebaceous secretion, the second perspiration, the third the dermis. If these fail, erythema and pigmentation are called into play. If all these defensive conditions are insufficient the increased blood-supply will result in some hypertrophy which may be irregular or regular. An irregular hypertrophy is a skin disease. If the hypertrophy has a low resisting power it will form a suitable camping ground for bacteria. That actinic rays have a causal influence in some skin diseases is shown by the result of excluding them from the skin by means of an actinic ray-proof covering in the form of a greasy or metallic ointment or some other impervious material. The case of the lenticular eruption reported by Dr. Rasch is evidently rare. The most interesting part of the question is, What happens to a person whose defences are imperfect or, at the time of exposure, more or less inactive? Actinic rays are said to have an inhibitory and a destructive action upon the tubercle bacillus and the spirochæte of syphilis. In disease these are securely hidden away in places where the actinic rays cannot penetrate. When benefit accrues from exposure to those infected the benefit is probably due not to the direct action of the rays upon the germs, but rather to the training of the defences for the admission of the healthful amount of actinic rays to the body, and so an improvement of the internal defences, antibodies, and the like.

I am, Sir, yours faithfully,

R. CRAWSHAW HOLT, M.D. Durh., M.R.C.P. Lond.
Manchester, June 24th, 1922.

PROTEIN-BALANCE IN INFANT FEEDING.

To the Editor of THE LANCET.

SIR,—In his letter in your issue of June 17th on this subject Dr. Ronald Carter expresses the opinion that I do not sufficiently differentiate between the protein of human and cow's milk in the modification of cow's milk for the purposes of infant feeding, and in support of his criticism he quotes Dr. Emmet Holt as saying that "it has become increasingly clear that the majority of infants will not thrive normally on cow's milk unless they are given two or three times as much protein as is contained in woman's milk." I cannot say that this has been my experience, nay, rather with increasing experience I find that beyond a certain comparatively low percentage the larger the quantity of protein the worse the nutrition; the optimum amount is not a fixed quantity, it differs with the individual and it differs with the circumstances, but it never greatly exceeds the amount present in breast milk. This experience is supported on a priori grounds from a study of the known facts of the amino-acid make-up of the proteins of the two milks.