

## Correspondence.

"Audi alteram partem."

## THE HIGHER FUNGI IN RELATION TO HUMAN PATHOLOGY.

To the Editor of THE LANCET.

SIR,—In studying the series of Milroy lectures, by Dr. A. Castellani, in recent numbers of THE LANCET, I have been struck by the classification of the fungi which is therein adopted. The recognition of the sugar reactions as the sole means of separating or identifying such organisms as the yeasts, which are otherwise morphologically identical, appears to me unjustifiable at the present moment. The fermentation of the commoner carbohydrates employed in laboratories, such as glucose, mannite, maltose, saccharose, and lactose, forms an extremely unstable foundation upon which to base any classification of bacilli, or yeast-like organisms; they are regarded by most bacteriologists in the nature solely of confirmatory tests.

It is a matter of common knowledge that such a well-known organism as *B. paratyphosus* A varies considerably in its powers of fermentation of glucose, some strains producing acid only, while others produce acid as well as gas. The confusion which has arisen out of attempts to classify organisms of the Flexner group in this manner has been a source of considerable anxiety to those who are attempting to simplify the problem of bacillary dysentery. But confusion becomes worse confounded when dealing with gas-forming organisms of the *B. coli* group, which are extremely unreliable in this respect and whose reactions do not remain constant after long subculture, call them *asiaticus*, *pseudo-asiaticus*, *pseudocoli*, or what you like. The same rule holds good, only more forcibly so, when one attempts to classify yeasts belonging to the genus *Monilia* in this manner. I can claim with some justification that this cannot be done, and should one conscientiously apply every test and not endeavour to create new species one will find that the carbohydrate reactions of these organisms vary from day to day, depending upon the length of incubation, length of subculture, and so on.

Considering these facts it must be conceded that Dr. Castellani has been particularly fortunate in being able to identify and name such a number of species upon such slender evidence, for one must remember that no two authors are yet agreed upon the proper reactions which such a common yeast as the thrush fungus, *Monilia albicans*, should give. In my work upon sprue in Ceylon in 1912 and 1913<sup>1</sup> I isolated 106 strains of yeasts, mostly from cases of sprue, and had I then had the scientific temerity, which I fortunately had not, or been able to employ a big enough series of sugars, I would no doubt have been able to name 106 new species.

It seems to me, Sir, that what is wanted at present is an attempt to simplify, not to complicate or confuse, the nomenclature of tropical medicine; the essence of science is simplicity, not complexity. The needless multiplication of names and species is most galling and perplexing to the sincere student. By these means we are descending into a veritable terminological morass, and are departing as far from Koch's postulates as it is possible to get. The cry of the student of tropical medicine is for facts, not names, and the practical application of these facts to the cure of disease.

After a due consideration of the views set forth above if we attempt to apply the reactions of these yeasts to the identification of different sugars in the urine, it appears that the inferences drawn are unwarrantable. There is only one sugar which it is important to detect in the urine, and that is glucose; and, further, the usual tests suffice; but even were the yeast test as described so accurate as to be specific, has it any practical application, and, if so, are we to understand

that the tropical practitioner of the future is to carry round with him cultures of *Monilia balcanica*, *M. krusei*, and *M. tropicalis* with which to inoculate catheter specimens of urine in sterilised tubes in his improvised laboratory; and what is he to do now that the former strain has been destroyed by fire?

Finally, on the supposition that the classification and recognition of these so-called pathogenic fungi is in such an indefinite state, is it not a little premature to describe so many new diseases of the intestine, respiratory tract, cerebrum, genito-urinary system, and skin as being due to their agency? One would imagine that much investigation is required on this subject before finally accepting their existence as definite clinical entities in man.

I am, Sir, yours faithfully,

PHILIP MANSON-BAHR.

Weymouth-street, W., May 5th, 1920.

## THE SURGICAL TREATMENT OF UTERINE AND VAGINAL PROLAPSE.

To the Editor of THE LANCET.

SIR,—Dr. W. Blair Bell, in his lecture on the above subject, which appears in your issue for May 8th, pays no attention to the musculature occluding the pelvic outlet; and in his surgical treatment limits himself for the most part to visceral structures, as though this musculature did not exist and is not concerned with the genesis of prolapse. This idea is supported by his reference to the prophylaxis of prolapse—a condition "almost invariably a sequel of parturition"—for the only predisposing condition he mentions is a puerperal retroflexion of the womb. Thus, we may suppose (1) that the babe passes by the musculature referred to, without affecting it, or that the muscular injury is immaterial (perineorrhaphy sufficing to deal with it); (2) that prolapse is not a hernia; and (3) that if Gilliam's operation were performed as a routine in the puerperal state, prolapse would (almost invariably) be prevented. The untenableness of this position is, I think, shown by the interval of time which usually elapses between the last parturition and the occurrence of prolapse. Cases do occur soon after labour (within a few weeks), but in the majority the interval is usually many years. Dr. Blair Bell does not indicate the percentage of these early or acute cases in his series, and I wish he had; nor does he say whether any particular procedure is necessary for their cure. This is unfortunate, for they are important from the point of view of testing operative procedures. Moreover, the occurrence of "congenital prolapse" shows that the influence of pregnancy and labour is unnecessary. Dr. Blair Bell objects to this term being applied to that prolapse which occurs within the first few days of life, and thinks it should be limited to the appearance of the cervix at the vulva (or beyond) in the young virgin adolescent, cases of elongation of the portio being excluded. Against this view we have "congenital prolapse" occurring, not only in the young adolescent, but in older virgins. Further, he does not show reason why this condition should be considered congenital, merely stating it "is no doubt due to inherent defects of the pelvic floor." But this pelvic floor is for him apparently a visceral structure, and in line with this the only way he has been able to treat these cases—the despair of surgeons—with success is by his extensive visceral procedure.

Unfortunately, Dr. Blair Bell, in his lecture, was unable to deal with the genesis of prolapse—the whole of prolapse cannot be treated in one lecture. But unless we have clear ideas of the genesis of a disease, its treatment is bound to be empirical. That confusion obtains and the surgical treatment of prolapse is empirical is shown by the lecturer's references to the opinion and practice of other surgeons; and largely explains his statement that "many cases can be cured by almost any method." Nor do I think Dr. Blair Bell's position impregnable. For if retroflexion or retroversion predisposes to prolapse, how is it these conditions are so often found without prolapse, and without the signs and symptoms of impending prolapse? How can prolapse of the vagina occur without depres-

<sup>1</sup> Report on Researches on Sprue in Ceylon, pp. 143-145.