still remember with pleasure their visits with him to his perennial borders and rose-hedges, as well as his intimate acquaintance with the characters and habits of every plant. But, indeed, there was no limit to the number of his interests. A spirit of inquiry amounting to restlessness and his true liberalism would carry him off on occasion from the remote north to Edinburgh, London, or the Continent. These were no ordinary visits. They had an object, professional or public; and they meant meeting with men of thought and action like himself, privately or officially, where he discussed in his shrewd way some problem of health, education, agriculture or politics. It was in these circumstances that he was recognised to be at his best. In 1898 his Alma Mater conferred on him the degree of LL.D. in recognition of his professional standing.

Dr. Bruce is survived by his wife and the whole of their family of six children. His second daughter is wife of Professor McKerron of Aberdeen; his eldest son is Mr. A. B. Bruce, late of the Indian Civil Service, who now fills an important appointment in the Board of Agriculture; and his second son is Dr. Ironside Bruce. Dr. Mitchell Bruce is his youngest brother.

Correspondence.

"Audi alteram partem."

A UNIFIED TERMINOLOGY.

To the Editor of THE LANCET.

SIR,—I beg the attention of your readers to some proposals for unifying the terms used in the nomenclature of disease which the delegates of Peru presented at the recent International Conference held in Paris to revise the classification of the causes of death. This memorandum indicates that practical medicine might derive advantages similar to those which have long been obtained in chemistry by attaching definite significance to particular terminations.

The proposals made for the purpose are simple and logical. The termination "*itis*" should in all cases be applied to inflammatory diseases; and the termination "*algia*" to painful conditions. All diseases caused by a well-determined bacillary agent should have the termination "*osis*"—e.g., tuberculosis, typhosis, syphilosis, leprosis, &c.—a group which would be added to as knowledge advances. Similarly the termination "*iasis*" should be restricted to diseases caused by parasites, as is already the case with many diseases such as trichiniasis, trypanosomiasis, elephantiasis, &c., this should be extended to paludiasis, acariasis, &c.

It is not proposed to ask for the disuse of terms like small-pox, cancer, diabetes, &c., which belong to the language, but it is suggested that if, for scientific purposes and particularly for purposes of international comparison, an official nomenclature is constructed, it can with great advantage incorporate such simple rules as those which are suggested.

The Conference adopted the terminology suggested as alternatives to the terms at present in use.

I am, Sir, your faithfully,

J. GUILLERMO ARBULÚ.

55, Rue de la Victoire, Paris (IX^e), Nov. 21st, 1920.

"HIDDEN SEPSIS."

To the Editor of THE LANCET.

SIR,—Colonel White Robertson's address to the Hunterian Society, reported in your issue of Nov. 20th, and doubtless perused with interest by your readers, calls for a few comments, for which I venture to ask a little of your valuable space.

It is refreshing to find Colonel White Robertson pleading for thorough and combined clinical and laboratory investigation of every individual case of infection as it presents itself, thus emphasising the need for unity in diagnostic methods. The author's insistence upon the importance of tissue susceptibility in chronic infections,

and the factors leading to this susceptibility, is also timely. Too often an undue prominence is given to the microbic factor in chronic infective processes, and the practitioner is led to expect fabulous results from therapeutic measures that are directed solely against this factor; and this especially at a time when curative vaccine therapy has a popular vogue somewhat in advance of its scientific achievements.

These general comments of Colonel White Robertson's are therefore excellent reading. But when the author of "Hidden Sepsis" gets down to detail, and when we hopefully await the help of his mature experience, expecting to glean some practical hints that will assist us in our daily efforts to solve the problems of obscure infection, some of us must admit disappointment. We do not feel sure that he is a trustworthy guide. He seems to expect very thin ice to be capable of bearing people whose weight demands thicker stuff, and there are times when it seems doubtful if the water is even frozen over. As an instance of ice that is too thin let me only mention the diazo-reaction, which the author holds to be of positive value in the diagnosis of an "entero-coli" infection, whereas it is probable that, apart from its being of *negative* value in the diagnosis of typhoid fever, it has no other value at all. It is certainly present in many cases of phthisis, meningitis, pneumonia, and scarlet fever. As an instance of water which it is not certain is frozen over let me only mention the Arneth index, to which Colonel White Robertson attaches so much importance in the diagnosis of actual and "latent" tuberculosis. If Colonel White Robertson would, once and for all, demonstrate convincingly to us that the Arneth index bears any more definite relation to infection by the tubercle bacillus than it does to other infections, this contribution to practical medicine would be invaluable.

Did space allow, I might refer to Colonel White Robertson's use of high and low blood pressures, of temperature ranges, of the patient's facies, of the percentage of large mononuclears in the blood picture, and of the appearance presented by a Gram-stained smear of the fæces as guides to differential diagnosis. To most of us the trouble is that these things lack the element of *specificity*, and do but, at most, spell infection or subinfection of some part of the body. (The last-named observation does not necessarily take us even so far as this.) But infection or subinfection by *what*? It is the *specific* test, be it clinical or be it clinico-pathological, that we need for guidance, not the mere sign, nor any number of signs, that interaction is taking place between the tissues and an infecting agent. Non-specific signs we have in comparative abundance, and their number increases yearly; specific tests come but rarely, one in a decade or so, for they are the slow fruit of prolonged and painful endeavour.

I am, Sir, yours faithfully,

Harley-street, W., Nov. 22nd, 1920.

THOMAS HORDER.

PSYCHICAL REPRESSION.

To the Editor of THE LANCET.

SIR,—May I reply to Dr. H. Yellowlees's note on our Conception of Psychological Repression, appearing in your issue of Nov. 6th, in which he points out that some confusion has arisen from the way in which psychoanalysts have attempted to explain this mental process? The confusion is due to the fact that "conscious" and "unconscious" are terms which, on the one hand, were in common use years before psychoanalysis was thought of, and have a popular meaning; while, on the other hand, they have a very precise psychoanalytical meaning. Nevertheless, for the sake of brevity and simplicity psychoanalysts are liable to use the words in a non-technical sense, especially when addressing an uninstructed audience. They (I should say "we" because I know that I am one of the offenders) try to avoid complicating elementary expositions by reference to a third department of the mind—viz., the "preconscious" sometimes called the foreconscious or subconscious, which is really responsible for the function of repressing intolerable thoughts, memories, and desires into the unconscious.