

terised by that circumspection which Dr. Burdon Sanderson so wisely commends.

Take this tuberculosis question, for instance, and let us see what curious oscillation medical opinion has undergone in regard to its pathology. Laennec, in his "Auscultation Médiate," 1819, impressed with the Hippocratic doctrine of the individualisation of disease, affirmed: "One cannot, without torturing the results of observation, and committing a strange abuse of all reasoning, regard tubercles as the product of inflammation of any of the constituent parts of the lung. On the other hand, a multitude of facts prove that the development of tubercles is the result of a general disposition, that it occurs without previous inflammation, and that when the latter coincides with the tuberculous affection it is most frequently posterior to it in date." This was the doctrine our fathers learnt, but not so were we instructed; for had not Felix von Niemeyer thundered forth that "Laennec's dogma, that every form of pulmonary phthisis is caused by a specific new growth, was simply a pathological hypothesis which by the more recent researches in the field of pathological anatomy had been entirely refuted"; for, said he, "tuberculosis is in most cases a secondary disease, originating, in some way unknown to us, in the action of cheesy morbid products on the organism." The view current only thirteen years ago was thus stated by Green in his much-read text-book: "The development of the general tuberculosis is not due to anything specific in the substances inoculated, but the products of various inflammatory processes (for the most part inflammations of slight intensity) may constitute the infective agents."

In 1874 Sir John Simon reported to my Lords of the Council that "he would mention it as among the most hopeful advances of modern preventive medicine that some specific diseases seem now beginning to confess in detail a birthplace exterior to man, a birthplace amid the controllable conditions in the physical nature which is around us, a birthplace amid the 'common' putrefactive changes of dead organic matter. The pathological studies of late years, including eminently certain very instructive researches which Professor Sanderson has conducted under my Lords of the Council, have clearly shown that in the 'common' septic ferment, or in some ferment or ferments not hitherto to be separated from it, there reside powers of disease production as positive, though not hitherto as exactly defined, as those which reside in the variolous and syphilitic contagia. And a further still more instructive teaching of the artificial infections is this—that the 'common' ferment, which in its stronger actions quickly destroys life by septicæmia, can in slighter actions start in the infected body chronic processes which will eventuate in general tubercular disease." All this reads very well, but what are we to make of it in the light of Professor Sanderson's most recent and final acknowledgment that the tubercle bacillus is the *materies morbi* of tuberculosis? I am not aware that any reconciliation of these views has been attempted, and if such be possible I would submit it is likely to be found rather in a more liberal recognition of the variability both of form and function of micro-organisms in obedience to varied soil and environment than by the too fashionable adhesion to a rigid specificity. Or, are these researches, so "hopeful" and "instructive" in their day, to be relegated to the same category as that to which the microphyte of variola ovina, which came out about the same period, has been consigned? Its recognition was heralded (Privy Council Reports) as having been made with "a completeness not yet attained in regard of any other such case"; but a year or two later, it having been found that "a very important error of interpretation" had been introduced, the medical officer thought "it would clearly be convenient that the author should at the earliest opportunity rewrite under the present correction his account of the morbid anatomy of the two diseases, and in preparing to do this should re-examine, so far as any doubt could be raised, the whole ground of the former investigations." That such "errors of interpretation" still happen an important discussion in Section 3 of the Congress tended to show.

The most recent research and its results, like the child's last toy, is invariably the most popular, and enthusiastic welcome is more appreciated than cool criticism. It was curious to note that a speaker was ruled out of order in the discussion on tuberculosis for venturing, in seconding its adjournment for three years, to express a doubt as to the rôle of the bacillus tuberculosis. It is an interesting speculation whether State-aided investigators are more or less trustworthy than others; they are certainly more apt to secure

the ear of State departments, and I think the results required of them are apt to prejudice their researches. No doubt definite results are more acceptable to my Lords of the Council than barren records of patient pointless labour. The deficiencies inherent in medical science, and its consequent inadaptability to legislative mechanism, are apt to be forgotten. It is thus that a tendency to pathological positivism is engendered, and the lesson needs to be enforced that dogma is equally objectionable in the realm of the infinitely little as in that of the infinitely great.

I am, Sirs, yours truly,

W. J. COLLINS, M.S., M.D., B.Sc. Lond.

Eastbourne, Sept. 2nd, 1891.

CARBOLIC ACID POISONING.

To the Editors of THE LANCET.

SIRS,—After reading Dr. Greenway's interesting case of carbolic poisoning, I thought the following case, unique I think in one feature, might prove interesting and strange enough for publication. Some seven months ago I was hastily summoned by a gentleman living near to come at once to his servant who was "in a fit." On my arrival I found the girl, aged seventeen years, in a state similar to that described by Dr. Greenway. She was perfectly insensible, eyes shut, pupils very slightly contracted; her breathing faint and quiet; her pulse almost imperceptible; face deadly pale. The mistress gave me the following account. About half an hour previously the girl, after her ordinary supper of bread and cold meat, went to bed. About ten minutes afterwards, her mistress, wishing to speak to her, knocked at her door; not receiving an answer she opened it, and found the girl on the floor in the state I have described above. I injected ether at intervals, and gave milk and brandy per rectum in order to keep life in her. In about three hours she began to make a little improvement; the breathing became stronger and the pulse more perceptible at the wrist. One hour afterwards she opened her eyes, was able to say she "felt better," and then seemed to settle into an ordinary sleep. When I was leaving I was naturally asked as to what kind of "fit" I thought it was, whether she would be subject to them, and whether I could account for the present one. I was bound to own myself puzzled, but said "her state greatly reminded me of a case of poisoning with carbolic acid which I had had a fortnight previously." In this present case, however, we had not the slightest reason for thinking she had taken anything. There was no smell, and nothing whatever was found in her room, and she had the same supper as her master and mistress. The next morning I was somewhat startled by the mistress bringing me a small scent-bottle which had contained carbolic acid, which she had found in the garden under the girl's bedroom window. On my round I called at the girl's home, where she had gone after her recovery, and questioned her on the subject of the bottle. She denied all knowledge of it, and that she had taken anything at all. As she was a respectable girl from a respectable family, I felt bound to believe her. On questioning her mother I found that she had never had a fit of any kind before, but added that the girl for the last twelve months had often complained of "pains in her stomach," and "feeling queer" at times. These symptoms she very naturally put down to her menstrual periods. One afternoon, six months after, a message came for me to go as soon as possible to see the same girl. The messenger stated, "she had had another fit; not so bad as she had before in her other situation, and that they had taken her home in a cab." I found she had been left in the house alone, and when her mistress returned she found her insensible. Her mother was sent for, and as she had then somewhat recovered she was taken home. She was very pale, but sensible, and able to answer questions. I was told she was very sick in the cab over her dress. When that was brought to me it smelt strongly of carbolic acid. She again stoutly denied having taken anything. This time, however, of course I did not believe her; "smelling" was certainly in this case "believing." In the evening her master called and told me they had found a small bottle of carbolic acid in the kitchen, together with lumps of sugar, some of them more or less soaked with the acid. We discovered afterwards the chemist who had supplied the acid, and the girl eventually confessed that she had been in the habit of taking "drops" on "sugar," "because she liked it," for over a twelvemonth.

I am, Sirs, yours truly,

JOHN B. HARRIS, M.D.

Aug. 29th, 1891.