7,000 convicts from the mainland of India are maintained. The natives at first were very shy, and it was some years before amicable relations were established with them, but now, owing to the judicious and considerate treatment of successive English governors, they have laid aside their former reserve, not to say hostility, and much information has been collected about them which places their moral character and social condition in a much more favourable light than the earlier accounts led us to believe. Much light has been thrown upon their arts and customs by a large collection of native manufactures, lately sent to England by Mr. Man. Regular habitations they have none, nor clothing, in the proper sense of the word; but a thick coating of mud, or coloured earth, mixed with pig's grease, is a very good substitute for the latter, being especially efficacious in guarding their skins from the attacks and stings of insects so abundant in the islands. They are perfect swimmers and divers, and expert in managing canoes. Their principal weapon is the bow and arrow, but they also use spears, harpoons, etc. They do not cultivate the ground, or breed any domestic animals, but they have a rude kind of hand-made pottery, and make vessels for holding water out of hollow stems of bamboo. They also make fishing nets and baskets. They live chiefly on wild pigs, birds, turtles, fish, wild fruits, and seeds and honey. No trustworthy evidence has been obtained that they are, or ever have been, cannibals. As with almost all people in a primitive condition of civilisation, they are divided into numerous small tribes, speaking different dialects, as many as nine of the latter having already been recognised in the islands. The name "Mincopie", often applied to the Andaman Islanders in European books, does not appear to be known to themselves.

Although the statements of some travellers would lead to a contrary impression, the most reliable evidence, that of skeletons and photographs, shows a remarkable uniformity in physical type amongst them. This kind of evidence has, however, at present only reached us from one district, the neighbourhood of Port Blair; and it is to these people that the description to be given in these lectures will apply. Hitherto, their osteological characters have only been known from one skeleton briefly described by Professor Owen, two crania by Mr. Busk, and two by Professor Quatrefages. During the last half year, the College Museum has received a valuable series of skeletons, collected, at the request of Sir Joseph Fayrer, by the late Dr. J. Dougall, senior medical officer at Port Blair; others have been lent for the purpose of illustrating this course by Professors Rolleston and Allen Thomson, amounting altogether to nineteen skeletons, and about thirty crania: sufficient to enable us to form a very fairly accurate idea of their characters and proportions.

One of the most distinctive physical characters of the Andamanese is their diminutive size. Except, perhaps, the bushmen of South Africa, they are the smallest known race. Those who have had opportunities of observing them living, have estimated the average height as something under five feet. Although, of the nineteen skeletons mentioned above, only one has been articulated, they supply some valuable evidence as to the height of the race. The common estimate among Europeans, only approximately of course with individuals, though fairly correct for averages, is that the femur is to the height of the living person as 275 is to 1000. As the one mounted skeleton of the Andamanese race shows exactly the same proportion, it may, at all events until better evidence is obtained, be applied to the others. Calculated on this basis, the average height of the skeletons of males would be 4 feet 9 inches, the tallest being 5 feet 3 inches, and the shortest 4 feet 6 inches. The average height of the ten skeletons of females would be 4 feet 6 inches, the tallest being 4 feet 10 inches, the shortest 4 feet 3 inches.

NEWTON ABBOT RURAL, WOLBOROUGH, AND DAWLISH. — This is a report of more than usual interest, as it contains some observations of value tending to show that fevers do not always "breed true", as is usually supposed. The death-rates of the three districts under Mr. Armstrong's charge, of course, vary considerably. In the Rural district, the rate was 18.65 last year, as against 20.65 in Wolborough and 22.49 in Dawlish. There seem to be no conspicuous sanitary defects in any of the districts, though the water-supply is susceptible of improvement in many places. Dawlish, after a firm struggle, got last year Parliamentary powers to provide a public supply, which will doubtless be a great improvement. Zymotic diseases were not more than ordinarily fatal, diphtheria being noticeable by its absence. A curious behaviour on the part of scarlatina is recorded in the report on the Wolborough district. Though that disease was severely prevalent in Wolborough parish, and some scores of children suffered from it, not a single child died of it in the parish, whilst on the other side of the brook, in another parish, where scarlatina was also epidemic, eight children succumbed to its effects.

CASES OF ENLARGEMENT OR INFLAMMATION OF THE MEDIASTINAL GLANDS.*

By JAMES F. GOODHART, M.D.,

Assistant-Physician to Guy's Hospital and the Evelina Hospital for Children.

(Concluded from page 543 of last number.)

HERE, then, are five cases of mediastinal disease, with certain groups of symptoms. The remarks that I have to make upon them must be somewhat fragmentary and discursive in their embrace; but I want to say a word, and it will be only a word, upon the following points: 1. The causes of some cases of glandular enlargement, more especially in children, with which it will have been noted the cases mostly deal; 2. The adequacy of such enlargements to produce the symptoms I have attributed to it; 3. The mechanism of such production; 4. The treatment; 5. The prognosis and diagnosis.

1. With reference to the first—the cause of some of these cases; it will be, of course, understood, that I am not giving a summary of the causes of glandular enlargement in general, but merely picking out points which these cases illustrate, and which are not unworthy of being brought into prominence. You will observe that three of these cases were instances of glandular enlargement in, so far as one could see, healthy and even robust children, and with no ascertainable tendencies to phthisis or its allies. One of them had nothing the matter whatever, save a swollen thymus. Now, with regard to the latter case, the swelling was not pathological; the gland was simply over-full of its own secretion. But I infer that this evidence of over-action is evidence of over-supply; in other words, that the gland had been over-fed. I want to apply the same explanation to the other two cases also. Everyone is familiar with the maxim that over-feeding of infants does more harm than anything else; but, so far as I understand it, this does not convey quite the idea of danger in the direction at which I am aiming now. It produces gastritis, enteritis, diarrhoea, wasting, etc., from the mere irritation to the intestinal canal. But this is not quite the point. Over-feeding does harm in another way; by over-stuffing the lymph-glands, giving them more than they can do; choking them with their own materials, and leading directly to tabes mesenterica, bronchial glandular enlargement, and even, I believe, to a general disease of the glandular element, in many parts—a process which, at its onset, is more aptly compared to stuffing turkeys or geese, than to any strictly pathological condition. I want to insist upon this, because these cases are looked upon by many, and, it seems to me, in the face of facts which tell directly the other way, as the result of a constitutional tendency; and I feel sure from watching many of them, that they are nothing of the kind. I say this, from the absence of any history of hereditary taint in many cases, from the healthy appearance of the patients themselves in many instances, and from the results which follow upon treatment which keeps a local origin in view. I, of course, do not wish to depreciate the constitutional element in many cases; but I do think that the constitutional tendencies have been exalted by utterly ignoring the local element; while, if treatment is to be of any avail, the latter factor is surely the more important one.

2. Is such an enlargement of the mediastinal glands adequate to produce the symptoms which these cases exhibited? It is, of course, no new notion that enlargement of the mediastinal glands or thymus may lead to spasmodic affections of the respiratory passages. Fifty years ago, Leblond in France,* and Becker of Berlin,† described the condition; and, even still earlier, an observation of Wrisberg's is recorded of the same kind; and, in 1834, some lectures were published in the *London Medical Gazette*, on Hoarse Respiration in Infants, and its connection with a Morbid State of the Thoracic Ganglia, not to mention Clark,‡ Rilliet and Barthez,§ West, and others, Kopp has specially called attention to the thymus as a cause of the symptoms under the term thymic asthma. But of late there has, it would seem, been a tendency to disregard former observations on the subject, and to make of small moment the morbid changes in the mediastinal tissues, as causes of laryngeal and pulmonary asthma. It is well to approach the less evident from the ground of already acquired knowledge; and I may therefore remind you that mediastinal tumours of all kinds—glandular, aneurismal, syphilitic, or what not—are recognised causes of paroxysmal dyspnoea. I have also seen a case in which pericarditis of a peculiar kind was associated with the same symptom. Again, it has more than once fallen to my lot to make a necropsy, and find a small localised mediastinal abscess as the only probable cause of the same. If we allow the connection between the symptoms and such pronounced

* Sur un espèce de Phthisie particulière aux Enfants.

† De Glandulis Thoracis Lymphaticis atque Thymo, etc.

‡ A Treatise on Pulmonary Consumption.

§ Maladies des Enfants, 2nd edition, vol. iii, p. 600.

causes as these, what reason is there for refusing to acknowledge that less evident forms of bronchial enlargement, or enlargement of the thymus, will very likely lead to similar symptoms, more or less evident according to the amount of disturbance produced in this or that individual case? If I have gone a step further, and attribute even such a condition as coma to similar causes, apart from the evidence which seems to justify this, I want pointedly to push home this conclusion by so doing; that if, as a result of mediastinal enlargement, we admit one form of nervous explosion in the shape of peripheral disturbance, such as I have narrated, there is no reason for refusing to admit the occurrence of more profound central explosions in occasional cases. And some corroborative evidence I would advance in favour of this is derived from that group of cases where a comatose condition ensues upon surgical interference with the pleural cavity. There are many explanations of the sudden death or coma attending upon interference with the pleura in cases of empyema. They cannot be discussed here; but the one which appears to my mind the most satisfactory is, that a central encephalic disturbance is produced through the influence of the peripheral branches of the pneumogastric.

One great obstacle to the acceptance of the view that I have been supporting here has been that its advocates have let it run away with them, and have offered bronchial enlargement as an explanation of all forms of paroxysmal dyspnoea. Whooping-cough, laryngismus stridulus, crowing respiration, spasmodic asthma, have all been explained in this way. I may disclaim any such notions for myself; and, indeed, I think that a careful consideration of the symptoms of individual cases proves conclusively that no one of these symptoms is due to any constant cause. Thus some cases of laryngismus are part of a central epileptic tendency; others are due to peripheral irritation in all sorts of places; and others, perhaps, to some local muscular fault in the larynx, as Riegel thinks. Then, too, it is said that so many cases of bronchial enlargement occur without the manifestation of any paroxysmal dyspnoea, that something besides the mere glandular swelling is wanting to lead to the production of the symptom. But here, again, no two cases are quite alike, either in the exact seat of the disease, in the direct implication of the nerves, or in the reacting circulatory troubles; and any one of these may just settle the question between paroxysmal dyspnoea or not. I am, however, inclined to think that a tendency to hasty nervous discharge *pre-disposes* to its occurrence, as might be expected, under slighter peripheral abnormalities. I am referring now more particularly to the frequency with which crowing respiration occurs in children who have actual convulsions.

3. What is the mechanism of the production of this paroxysmal dyspnoea? If I say that peripheral excitement leads to central stimulus and to nervous discharge, I only state what is every day before us as we live and move and have our being, and is illustrated sufficiently often in practice, where fits and dentition, or intestinal disturbance, are found associated. But I want to go further than this; because, in the first place, these cases of paroxysmal dyspnoea are not always entirely paroxysmal; they are always paroxysmally exaggerated; but there is, I think, in many, though all writers seem to deny this, a *persistent*—slight, it may be—inspiratory snuffle for weeks or months. It seems to me that Brown-Séquard's explanation of certain genito-urinary cases helps us here; and that we have to do with a reflex paralysis or impairment of function about the medulla oblongata and pons Varolii, greater or less according to circumstances; greater in the case of the comatose state before alluded to; less in the simple inspiratory dyspnoea; but both the one and the other merely extremes in a chain of which we see the many links in a long series of cases of varying severity.

I suppose that, as the result of the abnormal stimulus, some erratic nerve-discharge is effected, or some circulatory disturbance is thereby engendered, leading to arterial spasm, which, if complete, as in some cases, leads to sudden death; if less complete, may cause coma, or convulsion, or slight enervation and paresis of peripheral muscles.

Then, as to the seat of the local muscular fault; it is generally described as being in the glottis; no doubt correctly in many cases; but if it were so, in the desperate cases of which I have given instances, tracheotomy would get below a mere glottic spasm, and would relieve them, at any rate temporarily. But it does not; and then, moreover, in these cases, not unfrequently—and I think Killiet and Barthez corroborate this—there is a considerable want of free expansion of the lung, even in the slighter cases, where there is not much evidence of glottic spasm; and I am therefore inclined to think that the condition is one of spasm of the whole lung, closely allied to what we suppose to be the case in spasmodic asthma.

4. The two points that I want to bring before your notice with regard to treatment are these.—*a.* As has just been said, tracheotomy is of no use; not for the reasons generally assigned, but because the real obstruction is quite as much in the whole lung as in the glottis itself. If this be the case, large doses of belladonna—and, in children, by large

doses I mean thirty drops of the tincture, repeated frequently, if necessary—are most likely to relieve such cases, or, perhaps, nitrite of amyl or other muscular relaxors. *b.* I have expressed my belief that some of these cases are cases of over-feeding; and yet, in nearly all cases, we fly to cod-liver oil and good living, and, in doing so, may make things worse. I beg to offer for your consideration and discussion that some of these cases may advantageously be *starved* rather than fed; and I am quite sure, with reference to tabes mesenterica, an allied condition, of course, that the most spare diet possible is the only treatment which in many cases suffices to stop the diarrhoea and keep the child in comparative health. The lymph-streams or chyle-streams are choked, and the lacteals cannot absorb; hence the continual passage of undigested food, and the constant *diarrhoea*. Reduce the supplies till the block is relieved, and, I believe, a cure may sometimes be effected.

5. Of prognosis nothing need be said. The cases themselves afford the indications, when coupled with the remarks already made on the treatment. But with regard to diagnosis, a good deal has been said by some as to the possibility of diagnosing the disease by careful percussion over the sternum or in the vertebral grooves. Some have even gone so far as to say that, in a large number of cases of whooping-cough, they find by this means that the bronchial glands are enlarged. I have gone into this question somewhat carefully amongst my out-patients, and, as a result, I believe it to be quite impossible, in the greater number of cases even of *considerable* or great enlargement of the mediastinal glands, to make out any increase of size by percussion. I have made it out once or twice, and found myself wrong on the *post mortem* table; so that probably, in many other cases also in which I had supposed it present, it was not so. To say this reduces the number of cases actually diagnosed by percussion to very narrow limits. Percussion will help us occasionally, but not often; and practically we shall have to form an opinion from the character of the dyspnoea, the aspect of the patient, and other points which arise in any particular case. Lastly, cases have been described in which all the symptoms of implication of the thoracic branches of the pneumogastric have been present, in which the symptoms speedily disappeared under stimulant and antispasmodic treatment; and it has, therefore, been supposed by some that a functional disturbance, with symptoms similar to those of organic disease, may exist. Such a case is given by Dr. Habershon in his Lumleian Lectures on the Pneumogastric Nerve, delivered last year at the Royal College of Physicians.

THE ETIOLOGY OF ENTERIC FEVER.

By SHIRLEY F. MURPHY, M.R.C.S.Eng.,
Medical Officer of Health, St. Pancras.

WHILE Dr. Collie's able paper on the Etiology of Enteric Fever is still fresh in the memories of the readers of the BRITISH MEDICAL JOURNAL, I would ask them for a short time to consider the subject from the view-point of one who does not altogether agree with the theory entertained by the author concerning the manner in which enteric fever is communicated from the sick to the healthy.

I do not wish here to discuss the question whether enteric fever can arise from any condition of ordinary filth, but will assume, with Dr. Collie, that a specific poison is necessary for its production, and that each case of the disease requires for its existence an antecedent case.

Dr. Collie has stated as his opinion that certain cases which arose at the Homerton Fever Hospital among the attendants resulted from exposure to fresh enteric stools; he, however, adds, "We do not mean to say that cases do not arise from decomposing or decomposed stools, or from emanations from the lung and skin".

The question, so far as the attacks of attendants on the sick are concerned, is consequently limited to an inquiry into the possibility of these attacks arising from: 1. Conditions relating to the patient only, such as infection from breath, skin, and fresh stools; and, 2. Conditions relating to the hospital *plus* the patient. Now it may undoubtedly be assumed that, at Homerton, reasonable care is taken to dispose of the evacuations of a patient when they are passed, and that the stools are not kept in the ward for a longer time in this hospital than in another. If, therefore, the same precautions in this respect be taken in different hospitals, the number of attacks among the attendants, due to the first class of causes, must have some relation to the number of susceptible persons exposed to infection, for it must be remembered that the first class of causes is constantly operative. That is, there are always skins, breath, and fresh stools for the nurses to be exposed to; and, on the other hand, the second class, due to the hospital as well as to the enteric stools, will only be in force under certain conditions; such conditions, perhaps, as lead to the escape of air from the enteric stool-infected drain into the