Amongst the local causes, it is singular to notice how frequently the first symptom of the disease is assigned as its cause. Meat sticking in the throat is given as a cause of Cancer of the œsophagus; a blow, which calls attention to a tumour already of some size, as a cause of Cancer of the breast; choking with dust, as the occasion of Cancer of the bronchial glands; miscarriage, labour, an injury by the medical attendant during labour, or flooding subsequently to a severe fall, as giving origin to Cancer of the uterus. A case which perhaps needed most discrimination was one of Cancer of the uterus in a patient of Mr. Shaw's, whose husband died of Cancer of the penis. As nine years, however, had elapsed between the time of his death and her ailment, it may be asserted that the two diseases arose independently of one another. On the other hand, in as many as five cases there had been an abscess in the breast many years before the appearance of Cancer in it; and piles and constipation in a patient of Mr. T. H. Smith, many years before the occurrence of fatal Cancer of the rectum. Irritation in the eye had preceded for some years the outbreak of melanosis of the eyeball; an open fistula for forty years, and a blow with a cricket-ball, were severally the occasion of Cancer of the head of the tibia. These may have been real local causes of the Cancer afterwards developed; and, in the same manner, Cancer of the cheek is fairly attributed by the reporter of the case to an old wart in which it first sprang; a scratch by a horse's tooth may have set up Cancer of the skin of the hand; and Cancers of the lip may be traceable to the tobacco-pipe.

Original Communications.

BOILS AND CARBUNCLES.

By Tilbury Fox, M.D.Lond., Physician to St. John's Hospital for Skin Diseases.

The points involved in Mr. Startin's letter are of so much pathological interest, that I hope I may be allowed space for a few comments. Mr. Startin's therapeutical experience is entitled to the profoundest respect; but the explanation which he has given of the cause of boils and carbuncles is scarcely that which *modern* pathological observations would seem to indicate. Unfortunately, empiricism of the rankest and most tyrannical kind has held its sway for many a long day over cutaneous medicine; and no one (since Carswell's day) specially conversant with the facts of general pathology has thought it worth while to study the subject; yet, unquestionably, the philosophical study of skin-diseases is pregnant with results of great general significance, and the case of carbuncle is fully illustrative of this fact.

Mr. Startin views boils and carbuncles as having "frequently or constantly a parasitic origin"; and he bases his belief upon the facts, (1), that they are sometimes contagious; and (2), the success and efficiency of the practice in the cure of these ailments, rather than on microscopic verification: in other words, on the occurrence of *occasional contagion*, and the *beneficial action of acid nitrate of mercury*. One word will suffice in reference to the second argument. Acid nitrate of mercury, in virtue of its caustic properties, removes a host of ills—lupus, acne, warts, cancerous masses, and other diseased structures the most dissimilar. Are they then parasitic? Mr. Startin's chief ground for his belief in the parasitic nature of boils and carbuncles is the occurrence of contagion. This, however, is only occasional; and, considering the absence of all relative proportion between the amount and kind of the local diseased action and that observed in parasitic maladies; the absence of parasitic growth in the vast majority of cases; the fact that fungi will but very scantily develope in purulent fluids; the absence of any aperture by which the fungus-germs could enter from without into the cellular tissue; the non-access of air; and the want of relation between the amount of tissue-change and that of the fungus when present,—the unlikeliness of its parasitic nature is evident. And, if we seek amongst the parasitic diseases of animals, of human beings, or of plants, we shall not find any analogical grounds (nay, just the contrary) upon which to rest such a belief. Again, the constitutional conditions anteceding, accompanying, and following the local changes, in relative proportion to the extent and character of the latter, are not seen in any parasitic disease. The occasional presence of vegetable parasites is common to all diseases. Parasites are essentially ubiquitous, and they may be found in almost all skin-diseases; it is only when they *luxuriate*, that they give rise to *special* mischief. It is, unfortunately, fashionable to ascribe too many diseases to the influence of parasites.

How, then, explain the contagion of boils? for they seem to be occasionally contagious. What mean we by contagion? The labours of all pathologists seem to show that it is essentially connected with the growth of living particles of matter, detached from living bodies, and carried to others—of course, under favouring circumstances. Occurrences of the kind are universal in the vegetable kingdom; and there seems no reason why animal cells should not be transplanted and grow as well when isolated as in masses; and they do so. The cells in the secretion from a leprous sore, from Egyptian ophthalmia or the mucous surface of a rinderpest cow, cancer-cells, the pus-cells of syphilis and of small-pox, and, from recent observation it seems likely, tuberculous cell-matter, all possess this faculty; and, to take another example, in the case of molluscum the cells found in the little "varioliform" tumours are the means by which, being transplanted from person to person, the occasional contagion of molluscum is to be explained. One feature that is necessary in all these cases is the presence of free proliferation on the part of the cell-growth, and an adapted state of nutritive fluid (blood). In the active and early stage of boils, the cells of the enlargement may, no doubt, be removed from one body to another, and, growing under favourable circumstances, reproduce the original disease. Why not? What law would this contradict? Dr. Laycock's cases of contagious furunculoid are explicable upon the same ground.

Contagion is scarcely a distinctive feature of any one disease: the *degree* of contagion no doubt is. If it were possible to transplant an alphos scale, and it were to grow and produce alphos on a second subject, there would be no great mystery in it; it would harmonise (though an unusual occurrence) with true pathological facts, and be contagion in one sense of the word. There is nothing improbable, but probable, to say the least, in the supposition that the cell-growth in a boil may be the means by which the disease is rendered "contagious". In carbuncle there is a good deal of superadded inflammation, and a tendency to gangrenous change, which, implying a tendency to the death of the cell-tissues, is accompanied by a very much less likelihood of contagion.

But what is the pathology of boils and carbuncles? We may assume that in kind it is the same; the difference between the two diseases is dependent upon (1) variations in the vigour of constitution, (2) the state of the nutritive fluid (the blood), and (3) the activity of the local tissues. In the central part of boils and carbuncles are one or more pieces of dead tissue, sloughs, or cores. How is the tissue killed?—by arrest of the circulation, or failure of nutrition? What has been noted about the blood? Three very important sets of facts: 1, bacteridia oftentimes in great amount; 2, excess of urea in the urine, and uric acid in the blood; 3, diabetes. Bacteridia, however, seem to be developed only secondarily, and to be unable *per se* to produce furuncle. The excess of urea and uric acid can scarcely be said to be the cause of carbuncle and boils; and we come to the third condition, noticed by Cheselden, Prout, Latham, Landouzy, Marchal de Calvi, and others—viz., a tendency to, or actual, diabetes. Dr. Wagner has given details of fifty-two cases of gangrenous inflammation, including carbuncles and furuncles, in which a diabetic condition existed; and M. de Calvi has confirmed Wagner's observations. My own observations on this point are small; but I am convinced that, if we would clearly understand the true pathology of carbuncle, we must carefully investigate the matter in connexion with the production of sugar in the system. The existence of a diabetic habit explains satisfactorily the fatality of carbuncular disease, and the serious constitutional disturbance. Nothing is more common than for carbuncles to arise in the course of diabetes; and it will be remembered, that Cardinal Wiseman suffered for no less than four years before his death with carbuncles. More recently, Dr. Fonseca, of Pernambuco, has investigated the subject; and he tells us that in Pernambuco anthrax is very common, and that one of its forms is regarded as diagnostic of diabetes. Küchenmeister, Menestrel, and Jordão of Lisbon, have also given similar evidence.

And at this point Mr. Startin's therapeutical experience comes in to confirm the theory I have briefly sketched. He finds successful treatment in the use of *aperients, animal* diet, *tonics*, and free stimulation without malt liquors. The avoidance of all saccharine and amylaceous matter is an essential point; but I venture to affirm that, of all drugs, opium, judiciously used, is the most important. Clinically, I know that it has cured, and does help to cure, carbuncular inflammation, when other things fail; and therefore, if we add to Mr. Startin's recommendations the use of opium, we shall be in possession of a plan of treatment which is not only empirically dictated by the largest experience, but consonant with the most recent truths which pathology has taught us. The acid nitrate of mercury acts well, of course, as a *caustic*.

There are many other points—the origin of the local mischief especially—that I would like to notice; but I have only attempted to indicate that there is a much truer explanation than the "parasitic" hypothesis as to the cause of carbuncle, involving very wide pathological considerations. Skin-diseases have been so long handled from a *surgical*, that it is a novelty indeed for any one to investigate them from a purely *medical* point of view, and to trace connexion between them and such a profoundly subtle disease as diabetes; but I again reiterate the remark I have elsewhere made, that "the physician must be possessed of all that general medicine can teach before he can become the successful dermatologist."

43, Sackville Street, Piccadilly, W., November 1866.

NOTES AND OBSERVATIONS ON DISEASES OF THE HEART AND LUNGS.

By T. Shapter, M.D., F.R.C.P., Senior Physician to the Devon and Exeter Hospital, etc.

[*Continued from page* 10.]

In the preceding paper it was mentioned that, in the present day, there might perhaps be no very great difficulty in setting forth the quality and general characteristics of the normal sounds of the heart, or in describing what may be the peculiar sounds proper to the several lesions of this organ, but that, nevertheless, in practice, their due appreciation and perfect recognition were not infrequently found to be embarrassed by many sources of error. Some few of the occasionally recurring difficulties towards forming a correct diagnosis will, by way of illustration, be here briefly referred to. In doing this, it must not be inferred, though the sounds and murmurs belonging exclusively to the valves in their relations to the circulation of the blood are here solely referred to, that other signs no less important, and independent of these, are ignored. On the contrary, the value in diagnosis of some of these latter will be in due course not only considered, but perhaps seen to be of paramount importance, so that, without them, prognosis in disorders of the heart will be essentially at fault. The object at present, however, is mainly to illustrate the positions advanced as to the immediate cause of the sounds, and then to show what may be the importance of these sounds by themselves, towards estimating the condition of the heart itself.

In investigating, by auscultation, any of the disorders of the heart, the first and the chief point to be arrived at, in reference to sound, is the accurate ascertainment of the presence or the absence of either of the two normal sounds.

If there be an absence of either of these sounds, it may then be inferred, as a general rule, that some other sound has taken its place, and that this other sound is not a normal one—that it is, in fact, a new sound, and manufactured, as it were, by diseased structure or by disordered action.

If the above position be true, it will be at once seen how very important it is to ascertain the existence, or the contrary, of both normal sounds; and that here confusion in diagnosis must be sought to be carefully avoided, lest the inferences thence deduced be erroneous.

A careful observer, and one of our best authorities upon diseases of the heart, says that absolute deficiency of either sound, or of a murmur taking its place, has never fallen under his observation; that, in fact, neither systole nor diastole has ever been, in his experience, absolutely noiseless over the entire cardiac region. The above strong and pointed statement is made by Dr. Walshe (p. 78). After noting that, in cases of extreme weakness, the first sound may be *quasi*-deficient at the left apex, he says: "But it will then be found at the right apex and at the base. So, again, the second sound may be *quasi*-deficient at the base from excessive feebleness, or from being covered by a prolonged systolic sound or systolic murmur; but, in the first case, excitement of the heart, increasing the energy of its contractions, will invigorate the sound, and in the second case the sound will be heard at the right apex."

Though cases have occasionally presented themselves to my observation in which I could not satisfy myself of the absolute conclusiveness of Dr. Walshe's statement, that both the sounds, if not superseded