journey, a marked improvement in her health would take place. Shortly afterwards, in accordance with my advice, she took up her residence on the Cimies hill, about three hundred feet above the sea. The case progressed most satisfactorily, the cavity remaining dry, and showing a tendency to contract. I saw this lady in summer 1873, at Geneva, in the enjoyment of fair health. Thus, as a rule, cases with softening or a cavity do well on the Mediterranean coast on the sea-level, so long as they are free from febrile symptoms; but, should a high temperature and quick pulse set in, together with a rapid loss of weight, then a change of residence must be insisted upon without any delay. This alteration for the worse is much more likely to occur at the seaside than at an elevation of a few hundred feet above the sea; and the probability is, that it will be less marked if the patient be residing at the time in some high locality. But, should the experiment be made of moving down to the seaside while in this febrile condition, with the object, perhaps, of seeking a more cheerful neighbourhood, it will be attended with very great risk, and even perhaps with danger, to the patient.

I do not mean to state that phthisis is not fatal in winter in elevated regions near the Mediterranean coast; but it is certainly less so than in England or on the Mediterranean seaside. In the following case, life was certainly prolonged from a residence on the hills. This lady, aged 40, had been ill for six years. She was in a very low state of health when I first saw her at Nice, on December 4th, 1872; her lungs exhibiting a state of consolidation, with cavities on both sides. I advised her to move up to Cimies. There the pure light air of the hills revived the patient; she improved to some extent, and recovered her spirits, naturally buoyant and cheerful. The winter proved much less trying to her than might have been anticipated; she went out for daily walks in the garden of the hotel without suffering any pain, and hardly conscious that her life could not last much longer. After a time, a Bath chair had to be sent for; then she took to her bed, and in April sank peacefully. No doubt, in this case a residence on the hills preserved life throughout the winter, and succeeded in mitigating pain and mental distress to a considerable extent.

Thus it is that a pure air, possessed of a genial warmth, with a comparatively low barometrical pressure, are beneficial, as a rule, to all cases of phthisis with a high temperature and a quick pulse. Occasionally, these symptoms are arrested; in most instances, they are relieved; and, where a fatal termination cannot be averted, life is either prolonged, or, at all events, carried on under the most favourable circumstances.

Some consumptive patients, however, do well at the seaside. The main character of these cases is the absence of febrile symptoms; they enjoy a fair state of health, although with deficient breathing power. All the other functions are equally weakened, although normal in other respects, thereby re-establishing the balance of energy which should exist between them. I am inclined to believe that the most suitable resort for this kind of cases is some warm sheltered spot on the Mediterranean coast. These invalids live a comfortable painless existence, taking a moderate amount of exercise on the level ground, but unequal to any real exertion.

To the question, Are consumptive people, as a rule, better or worse on the Mediterranean coast at the end of the winter season than at the beginning? I would answer: They are sometimes better, and often much the same, but would probably have been worse had they wintered at home. In general, especially if a residence be carefully selected, consumptive patients undergo at first a marked improvement. The following case may be cited, amongst many others, as an illustration of the favourable change which occurs in health on arrival in the South. A young lady with consolidation and softening in progress in one of her lungs gained over one stone of weight in two months and a half after her arrival at Cannes. She weighed on November 9th, 1874, 7 st. 1½ lb., and on January 23rd, 1875, 8 st. 4½ lbs. It is true that she was in a tolerably fair state of health on leaving home; still this increase in weight is truly remarkable. She now (January 30th) looks the picture of health; but softening has returned, after having been completely arrested for some weeks. An early improvement under proper management often meets with a check from some unaccountable cause in the middle of the season, or about the middle or end of January, when a mild damp weather succeeds to a fine, clear, and bracing atmosphere. Consumptive patients often begin falling off in health rapidly, and it is very difficult to arrest this downward progress. The only plan I know is to insist on an immediate change of residence, moving uphill to an altitude of a few hundred feet above the sea, if possible.

Here is the greatest difficulty we have to contend with. Not only is there a want of accommodation for invalids on the hills near the Mediterranean coast, but an objection, on the ground of such a resi-

dence being lonely and dull, has very often to be overcome. My usual answer is, that the pleasure derived from the consciousness that health is improving will greatly outweigh any drawback consequent on a removal from a centre of social attraction.

With respect to the influence of a winter season near the Mediterranean upon the progress of laryngeal phthisis, I have not met in the South with any instance of its acute form. Several of my consumptive patients, however, had their larynx affected. This extension of the disease was usually attended with little or no pain, although accompanied by more or less discomfort and aphonia. When the aphonia is complete, I question whether the voice can be recovered; when only partial, the voice may improve under treatment. The climate which suits best in cases of laryngeal phthisis is also that most likely to benefit the pulmonary affection. The laryngeal disease, as I have seen it in the South, had nothing of the virulence I often found it to exhibit in London amongst hospital patients. But then it must be remembered that those who can afford to resort in autumn to the mild climate of the Mediterranean coast would also be likely to suffer least at home from laryngeal phthisis.

In my next communication, I shall allude to the subject of climate, and explain, by having recourse to physiological considerations, how it is that stations at some elevation above the sea are those which are most likely to benefit consumptive invalids.

(To be continued.)

IMPERFECT TEETH AND ZONULAR CATARACT:

BEING THE SUBSTANCE OF OBSERVATIONS AT THE
PATHOLOGICAL SOCIETY, MARCH 2, 1875.

By JONATHAN HUTCHINSON, F.R.C.S.,
Surgeon to the London and Royal Ophthalmic Hospitals.

FOR some years past, it has been a matter of general knowledge amongst ophthalmic surgeons that, when children are the subjects of cataract, they usually show also badly developed teeth. I do not know with whom this observation originated, but it is acknowledged in most of our standard works. By some, the malformations have been supposed to be connected with inherited syphilis; and by others they have been associated with rickets and with general defects of development. My object in the present paper is to endeavour to give a little more precision to our knowledge in respect to the coincidence of these conditions. In attempting to do so, it is necessary first to state that I believe that imperfect teeth are, as a rule, met with in connection with one form only of the cataract of childhood. The form to which I allude is known as the "lamellar" or "zonular" form, and is very peculiar. In it, neither the nucleus nor the peripheral part of the lens is opaque; a thin layer of fibres, at a greater or less distance from the nucleus, and completely surrounding it, being alone involved. The defect is compatible with very fair vision, and is often not detected until the child attempts to learn to read, sometimes not even till adult life. It is, I believe, almost invariably symmetrical, and is in a large number of cases quite stationary. It is not associated with any special diathesis, and it is very exceptional to meet with it in more than one member of a family. All these facts would suggest that it is probably the record of some temporary disturbance in the nutrition of the lens, rather than the result of any permanent peculiarity in the patient's state of health. It may be added, in support of this view, that lamellar cataract has not, I believe, ever yet been recognised at the time of birth, but is generally discovered at earliest during the first few years of the child's life. The congenital cataract belongs probably to a wholly different category, and is not lamellar. Whilst the congenital forms are often attended by other defects in the development of the eye, and with very imperfect vision, the lamellar opacity is, I believe, as a rule, the only defect to be found in the organ.

I must next describe the kind of defect in the development of the teeth which it is usual to meet with in connection with these curious cataracts. It is wholly different from that met with in congenital syphilis, and consists not so much in alteration of the form of the teeth as in defective development of the enamel. It is very often met with in association with the malformations which characterise hereditary syphilis, and hence probably some of the confusion which has resulted. The incisors, the canines, and the first molars, are the teeth which suffer most; and as a rule, with but very few exceptions indeed, the bicuspsids escape entirely. The contrast between the clean, white, smooth enamel of the latter, and the rugged discoloured spinous surface of the first molar, is often very striking. The first molars may, indeed, be counted as the test teeth as regards this condition; just as the upper

central incisors are in that which is due to syphilis. In these teeth, it occurs equally in both jaws. They are sometimes affected when all the other teeth escape, and I believe they never escape when the others suffer. I have been speaking throughout of the *permanent* set of teeth; for here, as in syphilis, although the temporary teeth often show unsoundness, they do not, I believe, exhibit any changes upon which it is safe to rely for purposes of diagnosis. The drawings which I hand round will convey a good idea of the kind of defect which is meant. They show the incisors and canines in various degrees pitted, dirty, and broken, often presenting very sharp edges, and sometimes almost spinous. In some cases, a horizontal line crosses the crown of the incisors and canines at one level; the part of the tooth below the line being narrower from before backwards, sharp, and broken. Non-development of enamel and erosion of the exposed dentine appear to be the essential features.

In the first molar, it is usually the surface alone which is affected, the sides of its crown being often covered with good sound enamel, whilst its surface is denuded, brown, and rugged.

I must next state that, although lamellar cataracts are generally attended by defect of teeth, yet the coincidence is not invariable. I have before me the notes of three cases, characteristic examples of lamellar cataract, in which the permanent teeth are stated to have been quite sound. The converse statement, that these peculiarities of teeth are often met with in patients who have not lamellar cataract, is a fact with which all will be familiar. In the face of these facts, it becomes difficult to entertain the hypothesis that there is any direct correlation between the nutrition of the lens and that of the permanent teeth, by which the coincidence adverted to might be explained.

Some years ago, Professor Arlt of Vienna made the important clinical observation, that those who suffered from lamellar cataracts usually had the history of attacks of convulsions during early periods of infancy; and my belief is, that it is in connection with this fact that the dental defects are to be explained. As the result of a considerable amount of inquiry amongst those whose teeth presented the peculiarities described, my conclusion is, that the defects generally result from attacks of inflammation of the gums occurring in early infancy; and that, amongst the causes of such stomatitis, mercury holds by far the chief place. There seems reason to believe that, in a large number of cases in which infants suffer from fits, mercury is given, and not unfrequently in large and repeated doses. I believe that it also enters into the composition of some of the most popular teething-powders.

My suspicion is, then, that when malformed teeth are met with in connection with cataract, they prove only that the patient has taken mercury in infancy, at a period when the enamel of the teeth was undergoing calcification. On this supposition, we have a ready explanation of the order in which the teeth suffer, since it is precisely that of their priority of development. A very considerable collection of facts justifies me in the inference which has just been stated. Of late years, in cases of lamellar cataract, I have always made it a rule to examine the teeth, and to inquire as to the history of fits, and as to the measures of treatment to which the patient was subjected in infancy. The connection between fits and cataract seems almost universal; that between cataract and malformed teeth general, but with marked exceptions; whilst, when the cataracts and malformed teeth are found together, it is very exceptional, provided the mother of the patient can be seen, not to obtain testimony as to the treatment of the fits by means of mercury. Of course, in some cases, the evidence on the latter point is either not forthcoming, or imperfect, but in many it is most strong.

I wish it to be distinctly understood that nothing which I have stated above has any claim to novelty, excepting, perhaps, the attempt to explain the connection between the different conditions mentioned. It is more than ten years since Arlt published his observations respecting the connection between fits and lamellar cataract; and, exactly ten years ago, Dr. Davidsen, then a student at Zürich, embodied the views of Professor Horner of that university in an inaugural thesis, in which most of the questions which I have discussed are entertained. I was not aware of the existence of this thesis (which was never published) until a few months ago, when it was obtained for me by the kindness of my friend Dr. S. L. Frank. Dr. Davidsen arrives at conclusions very similar to my own on most subjects, excepting the possible influence of mercury in producing the deformities of the teeth. This he does not even discuss, but speaks throughout of the dental defects as characteristically those of rickets. His table of cases does not place the coincidence between convulsions and lamellar cataract in such a strong light as do the facts collected by Professor Arlt and my own. It is obvious, however, that a considerable margin must be left here for cases in which no trustworthy history of the patient's infancy was ob-

tainable. Of these, Dr. Davidsen makes no mention, but he appears to count all cases in which no history of fits was given him as if their absence had been proved; and, as most of his patients were adults, a serious source of fallacy in the calculation of percentages is here introduced.

Professor Horner has favoured me, through Dr. Frank, with the statistics of seventy-eight cases observed by himself. In these, he found a history of convulsions in 76 per cent., deformities of the teeth in 85 per cent., asymmetry of the head in 35 per cent., imbecility in 2 per cent., and rachitic malformations of the extremities in 4 per cent. The remark which I have just made as to the difficulty of obtaining accurately the history of the patient's infancy applies also to these percentages; and the 76 per cent. with history of convulsions must be distinctly understood to mean that, in this proportion, convulsions were proved, whilst probably they were by no means disproved in all of the remainder. Professor Horner, who has given great attention to the subject, still believes in rickets as the cause of the dental malformation, and does not think that the hypothesis of mercurial treatment as the cause would hold good in Switzerland.

Although it seems to me probable, as already stated, that the convulsions stand in the relation of cause to the cataract, and the mercury given for the convulsions in that of cause to the dental malformation, yet I by no means wish to imply a belief that these associations are invariable. Certain apparent exceptions occur which require further investigation before we are justified in entertaining confident opinions on these points. Thus I have several times seen sets of teeth which I should have considered characteristically mercurial, in cases in which all history of drug treatment in infancy was denied; and it must be admitted to be quite possible that other forms of stomatitis will produce similar results. There are also certain rare cases in which zonular cataracts are met with in several members of the same family in the same or in different generations, and in some at least of these I believe that there is no history of convulsions, and that the teeth are not malformed. Upon the peculiarities which attend this class of cases, however, more detailed information is required.

In reference to the suggestion of the Zürich investigators, that the peculiarities in the teeth, in the skull-bones, and in the general development, are all due to rickets, I must be allowed to say, that it is as yet wholly unproved. I am not aware that any author has described what are so freely spoken of as "rachitic teeth", if that term be applied, as it is by Dr. Davidsen, to the permanent set. Professor Vogel of Dorpat, in his work on *Diseases of Children*, states, in reference to the effects of rickets on the teeth, that, "as the disease disappears before the second dentition commences, these phenomena are not observed in the permanent teeth". Yet it is upon the state of the permanent teeth almost solely that the diagnosis of rickets in the Zürich *clinique* is based; for in only 4 per cent. did Professor Horner find evidence of rachitic malformation in the extremities. The irregular formation of the skull, defects in symmetry of the face, and mental peculiarities when present, are perhaps quite as easily explained by reference to the preceding attacks of convulsions as by the hypothesis of rickets.

By way of summary, I think it may be stated—

1. That it is exceptional to meet with lamellar cataracts, excepting in association with an imperfect development of the enamel of the teeth; but that definite exceptions, in which the teeth are quite perfect, do occur.
2. That the kind of defect observed in the teeth consists in absence of the enamel, and is shown in the incisors, canines, and first molars of the *permanent* set, to the almost invariable exemption of the *pre-molars*. That, for purposes of diagnosis, the first molars are by far the most important, and may rank as the test teeth, since they not unfrequently show the defect when the others escape.
3. That it is highly probable that the defects in the development of the teeth are usually due to the influence of mercury exhibited in infancy; although it is quite possible that other influences, attended perhaps by inflammation of the gums, may occasionally produce similar results.
4. That teeth of the kind alluded to are met with very often in persons who are not the subjects of zonular cataract.
5. That the very important observation made by Arlt, that the subjects of lamellar cataract have usually suffered from convulsions in infancy, is fully borne out by further examination; and that it is very unusual to find lamellar cataract without such history.
6. That it is probable that there is a direct connection between the occurrence of convulsions in infancy and the development of lamellar cataract.
7. That, whilst there is every reason to believe that the defective teeth which are met with in connection with lamellar cataract are the

results of mercury, the evidence seems opposed to the belief that the lenticular opacity is also due to the influence of the drug. The great frequency of mercurial teeth without lamellar cataract, and the not very infrequent occurrence of lamellar cataract without mercurial teeth, are opposed to this view.

8. That the very frequent coincident occurrence of lamellar cataract with defective teeth, is to be explained by reference to the frequency with which mercury is given for the treatment of convulsions in infancy.

9. That there is no reason whatever for supposing that lamellar cataracts have any connection with hereditary syphilis.

10. That, whilst it is certainly true that lamellar cataracts are commonly met with in young persons who show general defects of development, short stature, ill-shaped heads, defective intellect, dwarfed lower jaws, or other physiognomical peculiarities, yet there is seldom any proof of the existence of rickets; whilst it is quite possible that the peculiarities mentioned may be due to the disturbance of the nervous system in infancy in connection with the convulsions.

11. It is very important to distinguish between mercurial teeth and syphilitic teeth, and the peculiarities presented by each usually render this easy; the two are, however, as might have been expected, not uncommonly met with together.

THE CLINICAL THERMOPILE.

By T. CLIFFORD ALLBUTT, M.A., M.D.,

Physician to the Leeds Infirmary.

LEISURE, as Mr. Seymour Haden says, presents itself to the busy man in an ever diminishing quantity; and hence it is that I have long deferred the publication of my experience with a thermo-electric apparatus closely resembling that described by Dr. Lombard in a recent number of this JOURNAL. In the years 1866 and 1867, I was engaged, with the assistance of Messrs. Harvey and Reynolds of Leeds, in perfecting a clinical thermometer, which, being portable, should find its way into general practice. I was also wishful to obtain an instrument to record surface-temperatures; and to this inquiry I gave a good deal of time. Mercurial thermometers for this purpose gave very imperfect results. Instruments were made with button bulbs, with spiral bulbs, with bulbs guarded above by non-conductors, and so forth; but little came of them. I, therefore, turned my attention to a thermo-electric apparatus; and Messrs. Harvey and Reynolds constructed for me in 1868 an instrument far less perfect and elegant, but substantially the same as that of Dr. Lombard.

It consisted of piles attached to handles, exactly identical with those figured by Dr. Lombard, of a rheostat, and of a galvanometer. The piles were of bismuth and antimony, but they resembled those figured so closely as almost to startle me when I saw them represented. The rheostat was far inferior to Dr. Lombard's, and the galvanometer less perfect in detail. I am told by physicists that my instrument was as good as could be had at that time; and so I must think, for I constructed it with the help and advice of the first physicists of the day. I am also told that much of the excellent detail in Dr. Lombard's calculations was scarcely possible before the date of some publications by Professor Tait, of which, I regret to say, I know little. My galvanometer was provided with stops to prevent deflections of the needle beyond the points which give true indications. But, although I found it easy to calculate relative temperatures with great accuracy, I found a difficulty in expressing these accurately as absolute temperatures in the centigrade or any other scale.

I have had my machine, such as it is, in work since 1868, and have gained a great deal of experience with it. For some time, I used it in the wards of the infirmary; but the connecting wires, etc., made the instrument too cumbersome for ward-work. Still, I made many very valuable observations in the wards, which I have supplemented in my chambers. The general outcome of this is, that even a very imperfect instrument is thoroughly trustworthy as a means of recording surface-temperature, and is very easy to work and to keep in order. As to results, I fear I cannot say much that is of high clinical value. Perhaps the most valuable results I have obtained are the relations between superficial and internal heat. I have found that there is little, if any, constant ratio between the two. I have found superficial heat to ascend with the increase of internal heat; I have found it stationary during such ascent; and, on the other hand, I have found it to descend, making the contrast between the two greater and greater. Again, I have found that the ratio between internal and superficial heat varies with the parts examined. While internal heat is fairly constant, superficial heat is very variable. I have often found superficial heat rising on the abdo-

men while it was falling in the extremities, facts which admit a tolerably probable explanation. I have also made some curious observations upon the localities of sweating in hectic and similar states. Some patients sweat on the limbs, others on the trunk, fore or back, others about the head or neck; and I hope to be able to correlate these observations with other facts. The chief point, however, which I have before me at present is, to say that some outbreaks of sweat are preceded by a rise of surface-temperature, while others are preceded by a fall. During the sweat, a fall of surface-temperature is nearly constant. But, not to occupy too much space, I may go on to say that I have not found that inflammation as such raises local temperature, but that local afflux of blood does so raise it, whether this be a congestion or a blush. Finally, in nervous diseases I have used the piles more, perhaps, than in any others, with very various but no definite results. I have found that in neuralgic districts the surface-temperatures sometimes rise and sometimes fall, that in other cases a rise may precede a fall, while in a fourth class a fall precedes a rise. Upon all this, I must dwell more fully another time. In palsies, the surface-temperatures are often very interesting, not clinically so much as physiologically, but I have not yet obtained any constant results. As to railway cases, the more I see of them, the more I find them unfit for scientific investigation; and I have found that common clinical sense is a far more useful organ for us in them than minute instrumental research.

CASE OF COMPOUND DEPRESSED FRACTURE OF THE CRANIUM, WITH LACERATION OF BRAIN-SUBSTANCE, CAUSED BY A BLOW FROM A QUOIT.

By R. W. FORSAYETH, Surgeon 95th Regiment, Fleetwood.

ON the morning of September 12th, 1874, as I had concluded my visit and was about to leave the hospital, I met in the passage a man bleeding from a wound in the head. He was brought in by two comrades, but was quite able to walk without their assistance, and seemed so little affected by the injury he had received, that at first sight I was inclined to take a trivial view of the case. However, I ordered him to be placed in bed; and, being provided with sponges and tepid water, proceeded to examine the wound: a deep laceration, triangular in plan, severing the scalp for a couple of inches, and situated at the anterior superior border of the left parietal bone. Considerable oozing of blood was going on, which obscured the view; so, introducing my index finger as a probe, I distinctly felt a deep oblong depression, bordered by sharp edges, and, at the same time, was dismayed to see small particles of brain-substance escaping with the blood from the wound. At this time, no other symptoms existed to denote the serious nature of the injury. Countenance, pupils, pulse, respiration, and consciousness were alike unaffected. The patient himself also seemed to think it of little consequence.

Notwithstanding this apparently quiescent state of affairs, on consideration, I determined to operate as soon as possible for the removal or elevation of the depressed bone, and not to await the supervention of symptoms of compression, which, from the serious nature of the injury, I felt certain would sooner or later set in. As I had no military medical colleague on the spot, I sent for Dr. Orr of Fleetwood to assist and advise me; and, while waiting his arrival, learned the history of the case. The man was playing a match at quoits. Two pins, or marks, being placed in the ground at a distance of twenty-one yards, he and his opponent threw from one end of the space thus marked out, his partner and opponent from the other, alternately; using six-pound quoits. On one occasion, thinking that the quoits had all been delivered from the opposite end, he ran in and stooped to raise them from the ground. Simultaneously with his movement, another quoit was thrown by one of the players at the far end; and, as he bent towards the ground, it struck him on the head. Hurlled to the earth, he remained unconscious for a few seconds; then, being raised by his comrades, behaved as if drunk, resisting their endeavours to lead him to the hospital; but, in a few seconds more, he became quite himself again, and walked thither with little or no assistance.

About 3 P.M., Dr. Orr arrived. The patient's state was then indicative of commencing mischief. He was inclined to sleep, breathing was more laboured, and the pulse slower and weaker. Dr. Orr consequently, after examining the injury, agreed with me that immediate action was desirable. As the man was unable to bear the pain caused by handling the wound, Dr. Orr kindly administered chloroform; and,