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,