charge said to have occurred before admission was melæna of a purely passive character. Had the man then been under medical care, this symptom might have thrown much light on the case, and rendered an accurate diagnosis easy.

Original Communications.

THE METEOROLOGY OF 1856 AND 1857.

By J. A. HINGESTON, Esq., Brighton.

[Continued from page 87.]

In each year in which the cholera has broken out in this country, the barometer has invariably stood at 30 inches, a little more or less, throughout the whole period of its continuance. This was the case in 1832. My attention was drawn to the circumstance from observing a high barometer with a cloudy sky, although I was not aware of the eventual importance of the observation I was then making. In 1853 this was remarkably the case: the disease was at its worst when the barometer was at 30 inches, and it declined in exact proportion as the mercury sank to 29.500 inches, or thereabouts. I published an account of it in the *ASSOCIATION MEDICAL JOURNAL*, No. XLIII, October 21st, 1853. It was again the very same in 1854, according to a series of very careful observations that I made in that year, as well as from all that I could gather from the observations of others published on this interesting point. At p. 33 of the *Registrar-General's Returns* for the quarter ending September 30th, 1854, it will be seen that, at forty-seven stations in England, Wales, Jersey, and the Isle of Man, where authentic observations were taken, the barometer stood at 30 inches, a little above or below, but chiefly at 30 inches, during September, when the disease was at its worst; and that subsequently, when the disease was declining, the returns from the same localities for the quarter ending December 31st, 1854, showed the barometer at a mean elevation of 29.740 inches, and occasionally as low as 29.600 inches. But last year, 1857, which was so remarkable for its salubrity, the barometer oscillated between 28.500 inches and 30.578 inches—a difference of two inches or more, as may be seen by referring to the *Registrar-General's Returns* for the three first quarters of that year, giving the observations taken at the same places as those already quoted for 1854. My own observations agree entirely with these returns. In the *ASSOCIATION MEDICAL JOURNAL*, No. CXLVIII, October 18th, 1856, I published an account of the barometer for that year, when cholera was absent, in comparison with that for 1854, when cholera was present; and the contrast was as striking as it was complete and conclusive. It showed the barometer obstinately at 30 inches in the year of the cholera, and oscillating considerably above or below it when the disease was absent or declining.

As to the electricity of healthy and unhealthy years, the results are less satisfactory, because the subject, in a medical point of view, is a recent, if not a new one; and yet my observations, such as they are, supply data by no means insignificant, and which, when coupled with those of the Astronomer-Royal at Greenwich, published in the *Registrar-General's Weekly Returns*, furnish matter of no small value and interest. Thus, during the cholera of 1853, the terrestrial and atmospheric electricity was generally considered negative; while, during the cholera of 1854, it was reported positive, but weak. In 1832, I had not thought of connecting electricity with pathology, nor am I aware of any published records on this subject of that date; but, judging from the kind of sky which prevailed, the stillness of the air, and the debility that characterised disease at that time, I should, on comparing it with what I now know of the question, have no doubt in regarding it as negative, weak, or *nil*. My opinion is problematical, and open to correction by those fortunate enough to possess any authentic remains on this point, and which would be most acceptable were they recovered and brought to light. In 1854, my private observations led me to conclude that the electricity was negative, certainly weak, and frequently inert. In 1853, when the cholera was so severe at Newcastle, in the month of September, the prevailing electricity was negative.

We do not yet know the precise meaning of *positive* and *negative*, as applied to electricity. They are conventional terms used to express opposite scientific relations, but not significant of any acknowledged quality or state. Neither is it possible to designate either the one or the other with the

same exactness as we describe an algebraic quantity, a mathematical axiom, or a medical fact. We rather imply conditions, the extent and nature of which are indefinite. This ambiguity is unavoidable, but not irremediable. The subject is still *sub judice*; and an approximation to the truth is preferable to no knowledge of it. To me it appears all but incontestable, that what is called negative electricity goes with diseases called asthenic, while the positive belongs to such as are sthenic or inflammatory. Negative electricity is coincident with mild and moist weather; the positive with the cold and frosty, or the hot and dry. In the summer, the electricity is considerably less energetic than in the winter; but then, in the former season, its energy is restored by lightning or gales of wind. In 1857, when the temperature was high and dry, the electricity was for the same space of time (270 days) positive and generally active, with strong tension; and the negative was limited to a very small number of days. (See the *Astronomer-Royal's* observations in the *Registrar-General's Weekly Returns* for 1857.) Now, that year was a singularly healthy one. More extended inquiries may show many of these notions to be erroneous or nugatory; and, on some future day, electricity itself may stand on a new and firmer basis. My mind is open to conviction, and I am ready to consider the question in all its bearings.

Be this as it may, the kind of electricity is certainly connected with the amount of daylight. There is less light on those days on which the negative electricity prevails, than on the bright, when the electricity is, with few exceptions, positive. In cholera periods, the light is diminished. I have already described this kind of atmosphere in the *ASSOCIATION MEDICAL JOURNAL*, No. CXLXXVII, August 2nd, 1856. Of the two years 1856 and 1857, the latter, though much fairer than the former, was darker by one-eighth. This diminished daylight was owing to the greater amount of clear blue sky in 1857; for blue sky gives back no reflected light. Large white-headed clouds were numerous, and there was a good deal of snow, in the winter of 1856, both of which increased the reflected light greatly. But reflected light contains polarised rays, which are reasonably supposed to be less favourable to health and life than the direct solar beam; and the insalubrity of the moon's ray is thus accounted for. The year 1857, although somewhat darker than 1856, was illumined by the direct solar beam, which may help in accounting for its greater salubrity.

The returns of health are seldom favourable when the sky is grey, the air moist, the daylight diminished, and the electricity negative. The cholera atmosphere is a case in point. On the contrary, the public health is good when the season is open, the clouds distributed in masses, the moisture condensed into showers, and the electricity positive. A large amount of solar beam is as essential to the human as it is to the vegetable creation: hence the insalubrity of cities enveloped in smoke, the northern aspect of dwelling houses, barracks, and hospitals; and the short lives of miners, clerks, and artisans, shut out from the daylight at noon.

By a small contrivance, an account of which was published in the *ASSOCIATION MEDICAL JOURNAL*, No. CII, November 23rd, 1855, I have been able to measure and record the amount of daylight for the last two years and upwards. I regret that I had not found out the means of doing so twenty-five years ago. I find that February is the darkest month in the year, and August the brightest; the last six months are brighter by one-fourth than the first six, the atmosphere seeming to have acquired a luminosity of its own, which it does not lose till the end of January. If carefully prosecuted, it is an inquiry intimately associated with health and longevity; and it would be most interesting, were it applied to town and country, high and low situations, sea and land, northern and southern latitudes.

The years following an outbreak of cholera have been hitherto marked by an increase of human life, shown by the returns of a diminished rate of mortality. This phenomenon has been prominently exhibited in the last three years, succeeding to 1854. It has been variously accounted for. The epidemic was supposed to have carried off the sickly, and left only the strong; but this is contrary to the fact, for it was the mature and strong that were the most obnoxious to the disease. Others have surmised that cholera occupied the place of other diseases; and that, when it had attacked and carried off its particular victims, it left a healthy population behind. This is so far true, as that, in 1853, the rate of mortality from the ordinary causes was one-fifth below the average. Again, it has been conjectured that the better habits of hygiene adopted by the public, impressed with a dread of infection, tended to im-

prove the general health; but can this improved moral and hygienic discipline be shown and vouched for? In 1855 and 1856, the natural increase of the population was ascribed to the return of the army and navy from the seats of war; but this infusion of fresh life does not serve to account for the singular absence of fatal disease throughout the rest of the population. It is evident that the phenomenon remains to be explained upon principles of which we are not yet cognisant. During the past year, the number of persons who have died at a very advanced age is some proof, as far as it goes, of the old being able to survive the deleterious influences of a prevailing epidemic and a malarious atmosphere conjoined. Of more than 200 nonagenarians recorded in the *Registrar-General's Weekly Returns* for 1857, a large proportion is stated to have been living not only in unhealthy localities, but among the lower classes of society. Of 123 women, the greater number were widows. Thirty-five only were men, of whom a scanty few are designated as *gentlemen*. Seven had reached the verge of 100 and upwards. This return of extreme old age is only for one year, but it is sufficient to show that the old escape the pest as well as many of the young. How, then, shall we account for the extraordinary phenomenon of the increase of life, if not of longevity, after an outbreak of cholera? The answer to the question involves the consideration of a wide range of circumstances, pathological, social, and atmospheric; and the results of experience extended over a large portion of the globe, and continued uninterruptedly for a great length of time, are requisite for arriving at a definite conclusion.

ON DISEASES OF JOINTS.

By HOLMES COOTE, Esq., F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital, and to the Royal Orthopædic Hospital, etc.

IV. LATERAL CURVATURE OF THE SPINE.

[Continued from page 115.]

THE body of a newly born infant was brought to me by Mr. Dunn for examination, as presenting a singular combination of abnormalities. There were noticed:—

1. Deficiency of the cranial bones posteriorly, and the presence of a large bag containing fluid communicating with the interior of the skull.
2. Hare-lip.
3. Fissured palate.
4. Congenital deficiency of the abdominal parietes, so that they burst during birth.
5. Talipes varus of both feet.
6. Double lateral curvature of the spine.

From numbers 1 to 4, we have illustrations of arrest of development. The fetal layers, which curve forward to complete the cavities of the body by uniting in the mesial line, had here performed their work ineffectually; development had been arrested while there were two separated halves of the body. Hence the open fontanelles; the hare-lip; the fissured palate; the thinned abdominal walls. We see the same in the fissured sternum; in epi- and hypo-spadias; and in the permanent and normal openings of the nares, mouth, vagina, urethra, or anus.

In 5 and 6, we have instances of intrauterine deformities, caused by long continued pressure of the uterine parietes upon the growing fœtus. The inward-turned feet cannot execute those movements, by which the first approach to the normal direction is attained. The muscles, ready formed to contract, are impeded; and the club-foot becomes permanent. The elongating spine has no space for its increasing growth. It becomes compressed, and curves laterally; *i.e.*, in that direction where it meets with least resistance. If the compression be slight, the curve is single, and extends in one sweep from the upper dorsal region to the pelvis. If it be severe, the curve is double, convex in one direction, and concave in another.

Do these deformities, depending upon compression, ensue from accidental deficiency of the liquor amnii, or from natural defect in the uterus, by which its power of expansion is limited? I should be inclined to doubt whether accident ever produced intrauterine errors. But yet others of great experience have stated that congenital deformities may occur in one child only in a family; the others being perfectly well formed. I confess that at present I have not data sufficient to answer the question.

In this communication, I wish to confine my remark to "lateral curvature". The fœtus was carefully dissected, in order that we might see why it was that the spine was twisted.

First, the vertebrae and the muscles, which acted upon them, presented no abnormality. The *erectores spinæ*, those assemblages of muscles which lie on each side of the spinous processes, were complete in every part, and equally developed, whether on the convexity or the concavity of the vertebral column. They were formed ready to execute their allotted function; but it was obvious that the deviations of the spine from the straight line had so displaced their insertions, as entirely to alter their sphere of action. The action of the sacro-lumbalis and the *longissimus dorsi* on the side of the concavity would have been, if any, to increase the curve. The same muscles spread over the convexity would twist or rotate some vertebrae, but in greater part would wither or become atrophied. I particularly desired to witness the effect upon the curve of the consecutive detachment of each muscular slip from its insertion, and accordingly cut through the tendons of both the principal muscles (namely, sacro-lumbalis and *longissimus dorsi*) first on the right and then on the left side, pressing and examining the curve at each instant, in order that I might detect the moment when resistance of any sort or kind was overcome. No effect ensued. The curve was as resistant as ever.

It may be as well to remark that owing to a stupid error, long since corrected by Professor Owen and others who have followed in the course of homological anatomy, but hitherto continued in the schools, the anatomy of the muscles of the back is inaccurate, and rendered so incomprehensible, that it is quite the fashion to profess ignorance of the subject. These muscles, however, are simple to a degree, and extend from homologous points of bone from the pelvis to the head. In the same manner as the *interspinales* reach from spine to spine, so do muscular and tendinous slips composing the *longissimus dorsi* pass from diapophysis to diapophysis (*i.e.*, the tubercle of the lumbar region; the transverse process of the dorsal; and a rough spot near the articulating surfaces in the cervical region) and those of the sacro-lumbalis from one rib to another.

We may conclude, therefore, that these muscles, the great extensors of the spine, have nothing to do with the production of lateral curvature; when the spine bends, they lose their proper relations, and cannot execute their functions. In old standing cases, which I have examined in the dissecting rooms, I have found the muscles on the convexity elongated, attenuated, and wasted; those on the concavity wasted also, but tense from secondary or *adaptive* contractions. But cut them through in every way, and the scoliosis, or lateral curvature, remains unaltered. No wonder that surgeons, who have been taught to believe in the "cure of a crooked back by the healthy exercise of its muscles," should be doomed to disappointment: that the practice of putting weights on the head of one, whose spine is already too weak to support the natural burden, should aggravate instead of alleviate the evil. "When I first became engaged," observed Sir Benjamin Brodie, in a lecture delivered at St. George's Hospital, 1846, "in a considerable private practice, and cases of curvature of the spine were presented to my observation, I was in doubt as to their nature and treatment; in this respect my education had been imperfect, and I had learnt nothing of them from my teachers."

What is it, then, which holds the spinal column thus curved, if the bones are primarily unaltered in shape, and the muscles are not concerned in the deformity? Unquestionably the spine yields to the superincumbent weight through weakness of the ligaments; these structures yield and contract, as we know ligamentous structures can contract, along the concavity. It is not one but every ligament which contributes to this end; those between the transverse processes; the fibrous capsules of the articulating processes; the corresponding part of the anterior and posterior common ligaments; moreover, there exists a general adaptation of surrounding parts. The yielding of these ligaments, easy and comparatively painless in a growing girl, is attended with extreme suffering in the adult, as was shown by the case of the late Gideon Mantell, the *post mortem* examination of whose body has been most accurately given in the thirty-seventh volume of the *Transactions* of the Royal Medical and Chirurgical Society, by my colleague, Mr. Adams.

To effect a cure, the spine must be slowly pressed into its proper shape, and there held by means of instruments until the same adaptive process of contraction, which ensued in the deformity, re-establishes the figure.

These remarks naturally point to some important conclusions in the matter of treatment.

First, any system based upon the development of muscular