and districts specified, and I added a column showing the range or difference between the greatest and least annual averages.

The table will be found in an essay of mine on the Relative Value of Averages from Different Numbers of Observations, published in the *Journal of the Statistical Society*, vol. xiii, p. 30. It results from this table that, as a general rule, subject, as are all general rules, to exceptions, the range or difference between the greatest and least number of male births increases as the number of facts decreases. Thus (I exclude exceptions) the range for all England, with upwards of half a million of facts, is 3,028; for the north-west counties, with more than 76,000 facts, it is 5,428; for the metropolis, with more than 59,000 facts, 4,321; for the eastern counties, with upwards of 32,000 facts, 4,855; for the northern counties, with nearly 28,000 facts, 6,405; for Staffordshire, with nearly 16,000 facts, 8,672; for Derbyshire, with nearly 8,000 facts, 9,582; for Cambridgeshire, with close upon 6,000 facts, 15,502; for Huntingdonshire, with about 2,000 facts, 34,794; for St. George's in the East, with 1,500 facts, 37,848; for Rutlandshire, with about 700 facts, 60,304; for Canterbury, with 368 facts, 73,680; for Salisbury, with 271 facts, 107,016.

Having now established by an appeal to facts the first of the main positions which I undertook to prove—having shown that when our facts are numerous our average results are trustworthy, and *vice versa*, and further, that for the same order of facts, we approximate to a true average as we gradually substitute larger for smaller groups of facts, I now proceed to inquire what number of facts in any given inquiry will suffice to yield a true average; and what use are we allowed to make of averages derived from comparatively small numbers of facts.

It is not easy to answer this question. It is probable, and even certain, that the number will vary with the nature of the facts themselves, and that, as a general rule, more facts will be necessary in the case of units of variable magnitude than in the case of simple units; more facts in the case of events brought about by the combined action of many forces than in the case of events brought to pass by a small number of forces acting together; more facts when, in such a question as the duration of human life, we admit into our tables several classes than when we restrict our inquiry to one class. Take, as cases in point, the attendance of male patients at an hospital, and the duration of life of the aristocracy and of the upper and middle classes of English society respectively. The causes which determine the attendance of a patient at a given hospital on a given day are both few in number, and for the most part trivial in kind; but the causes which determine the death of a man at a certain age are very numerous, and have been in continuous operation for a long period of time. Now the parallel columns of figures which represent the range or difference between the highest and lowest averages in these two classes of facts certainly approximate more rapidly in the case of the attendances at hospital than in the case of the ages at death of the upper and middle classes of society, though the total number of facts and groups of facts is the same in both instances. Again, if we compare the single class of the aristocracy with the mixed upper and middle class, the first displays the most rapid rate of approximation of the parallel columns of averages. For the aristocracy the ranges, or differences between the highest and lowest averages, for 50, 100, 200, 400, and 800 facts, were as the numbers 11, 7, 5, 3, 1; but for the mixed upper and middle class, for the same numbers of facts, 28, 18, 12, 6, and 4. Though the averages are more numerous in the case of the mixed class, being 64 as against 16, and the comparison therefore wanting in exactness, the difference in the rate of approximation is too considerable, I think, to be accounted for otherwise than by the different character of the facts themselves. Be this as it may, I think that the true average age at death of the English aristocracy would be ascertained—certainly for every practical purpose—by less than 1,000 facts; and the average age of the upper and middle classes by considerably less than 3,000 facts.

This question is, from the very nature of the case, insusceptible of mathematical treatment; but you are probably aware that there are mathematical formulæ for calculating the limits of possible error attaching to any given number of facts, large or small, irrespective altogether of the nature of the facts. Of these mathematical formulæ, Gavarret has made much use in questioning the sufficiency in point of number of some collections of facts made by Louis. These calculations may, of course, be applied in cases of unusual difficulty, where the validity of an argument based upon average results is called in question. But such applications of the pure mathe-

matics must be very rare; and they are certainly not free from objections.

But, whatever the proper answer to this question may be, it must be quite obvious that, if we are obliged to collect one thousand, two thousand, or three thousand facts, in order to arrive at a true average, and to content ourselves with nothing short of these high numbers; and if we must then submit our averages to the ordeal of a mathematical formula, we shall be driven, if not for state purposes where we can command almost unlimited supplies of facts, at least for scientific purposes, to forego the use of the numerical method altogether. For it does not often happen that, for ordinary scientific purposes (I am thinking especially of medical science), we can bring together any great body of facts. This being the case, the very important question arises, Are we debarred altogether from making use of small numbers of facts? Is there not, in the absence of certainty, at least a fair probability, that the average results of even a small number of facts may be entitled to confidence? Admitting the possibility of such averages being remote from the truth (and this admission is forced upon us by the facts brought forward in this lecture), is there not some escape from the very disagreeable dilemma of being obliged to reject all average results except those derived from one thousand, two thousand, or three thousand facts? I reserve the answer to this question for my next lecture.

A Course of Lectures ON THE NATURE AND TREATMENT OF DEFORMITIES.

DELIVERED AT THE ROYAL ORTHOPÆDIC HOSPITAL,
MAY 1860.

BY
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LECTURE I (concluded from p. 451).

Causes of Non-Congenital Deformity. In the non-congenital deformities, the causes are numerous. First, we have contraction of the toes from mechanical causes—tight boots, for example; rheumatism—a common cause—when all the toes suffer; as also in contraction of the heel, when the toes also suffer, as will be explained hereafter.

Disease in the ankle-joint or its neighbourhood, rheumatism, general debility, curvature of the bones, irritation or disease of the spinal marrow or its membrane from numerous causes, inducing an irregular nervous supply or complete paralysis of one or more muscles,—produce deformity of the ankle-joint, in the shape of talipes equinus or equino-varus, which is, in fact, precisely analogous to talipes varus; talipes valgus, talipes calcaneus, in which the gastrocnemius is paralysed; and lastly, talipes calcaneo-valgus.

In the knee-joint, the same causes will be found to have existed, with the addition of idiopathic inflammation, producing, however, more displacement than is ever met with in deformities of the ankle-joint, inasmuch as the knee is perhaps of all joints most dependent upon its passive attachments and the integrity of the muscles, from the nature of the articulation, and from its being situated midway between the trunk and the sole of the foot. Hence, when once a deviation occurs, the mechanical effect of the weight of the body acts as a constant secondary cause in increasing the deformity. We then meet with the following varieties: contraction in the flexed or extended position, as in the congenital deformity; in the inverted or everted, producing outward or inward inclination of the knee-joint—the latter in combination with curvature of the bones. There is also combined with contraction in the flexed position more or less displacement of the tibia (to which I have previously alluded), when the contraction arises from inflammation of the joint, either primarily or secondarily. This last condition is met with in cases where disease of the bone has existed in the immediate neighbourhood of the joint. Paralysis of the extensors of the leg is also a common cause of contraction of the knee-joint.

In the hip-joint, there are the same causes of deformity, with the exception of paralysis producing contraction in the

flexed and adducted position, very rarely in the extended. I have seen but one instance in the adult, a gentleman upwards of fifty years old, who had loss of power in the glutei muscles. Oftentimes, when the hip suffers from rheumatism, complete disorganisation ensues, resulting occasionally in complete ankylosis, more commonly in dislocation on the dorsum of the ilium.

A case presented itself some time since—a man forty-five years of age, in whom both thigh-bones were dislocated upon the dorsum of the ilium; and, as far as an examination permitted, the ligaments appeared to be completely destroyed, as in every instance arising from this cause. The importance of the fact that complete paralysis of the muscles of the hip-joint is not found to coexist with paralysis of the extensors of the knees and flexors of the feet—apparently the most hopeless cases—will be shown to you when we come to that subject.

In the spinal column, irregularity of the motions of the lower extremities from deformity or other cause, general debility, disease of the bones producing what is called Pott's disease, are the most frequent causes. Rheumatic inflammation terminates at times in complete ankylosis of the whole of the vertebræ, three cases of which have passed under my notice—one, a physician, fifty years old; another, a gentleman forty-two years of age, in whom there was also partial ankylosis of the hip, knees, and elbows, so that the patient was utterly helpless, and remained immovable in whatever posture he might be placed; the third, a patient twenty-one years of age, in whom the same condition was taking place.

Idiopathic inflammation is a cause assigned by some writers on curvature of the spine. I have endeavoured, by close and careful examination, to satisfy myself on this point; but have not as yet traced inflammation of the ligaments. Curvature of the spine in the adult is generally attended with pain, which is at times most severe. This I attribute to the resistance offered by the ligaments, inasmuch as I have always found it to be relieved by the application of efficient support. Many cases have come under my notice, who had been treated for supposed inflammation of the ligaments, and subjected to cupping and every variety of counterirritant, preserving the horizontal position for months, and some for years; yet, in the course of two or three months, they have been relieved from pain, and enabled to take exercise with comfort and freedom, and gradually been restored to health by efficient mechanical support. A patient now under my care, forty-five years old, had suffered for more than five years from what she termed most frightful spinal pains, and loss of power in the lower extremities, for which a perpetual blister had during five years been applied to the lower part of the spine, affording temporary relief. Some of the highest authorities both in England and Ireland had been consulted, all agreeing in the opinion that her complaint was "creeping paralysis"; and the only consolation held out to her was, that it was unusually slow in its progress. I found, upon examination, a slight curvature to the right side of the whole of the dorsal vertebræ. I stated that I thought it more than probable that that was the cause of her suffering and the loss of power in the lower extremities. I applied an instrument. In two months, the pains ceased altogether, have not returned, and she is now (twenty months since my first visit) recovering the use of the legs. She was also losing power in the right hand, but that she has long since perfectly recovered. I shall mention other similar cases when we come to speak of lateral curvature of the spine.

Pain is at all times complained of in those cases of non-congenital deformity in the adult, in which the ligaments are subjected, as in lateral curvature of the spine, to an undue stretching. We invariably meet with it in talipes varus, or flat-foot, when the ligaments on the inner side of the ankle, and those of the sole, are subjected to undue pressure; most commonly in knock-knee, on the inner side of the knee-joint, from the stretching of the internal lateral ligament.

In the spine, as soon as the curvature becomes permanent and the ligaments subjected to this stretching by the weight of the head and shoulders, pain is the consequence, which, I believe, has been too often attributed to inflammation. I do not mean to say that inflammation never occurs; but I have never been able to detect it among the numerous cases which I have had opportunity of examining.

Lateral curvature of the spine, as you are aware, presents itself in a single curve, in its commencement; then in a double, in the form of the italic S, or in a treble curve; and it varies greatly in its severity. It is occasionally combined with rachitis, in which there is not only simple deviation of the spine from the straight position, but an actual change of the

relative proportions of the bones themselves. These, as I shall hereafter show you, present the most frightful examples of deformity.

I have in no instance met with paralysis of the muscles of the spine, which so far correspond with the muscles of the hip. In three cases only a loss of power was apparent; and in those, every voluntary muscle was affected.

In the neck, we have disease of the cervical vertebræ, rheumatic inflammation, complete or partial loss of power of one or other of the sterno-mastoid muscles, producing wry-neck.

Contraction of the lower jaw is seldom met with. In one out of four cases that have passed under my observation, rheumatic inflammation was the cause. The remaining three were produced by ulceration from the use of mercury.

In the shoulder-joint, we find the fixed condition produced by rheumatic inflammation, or by inflammation following a blow or other injury.

Complete paralysis of the deltoid muscle is another condition; the muscles of the arm, forearm, and hand, retaining their functions. Upon examining these cases, no trace of the deltoid can be detected. The head of the humerus appears perfectly exposed, with the exception of the skin and cellular tissue, and with little force can be dislocated in any direction.

In the arm, we find contraction both in the flexed and extended positions, from inflammation in, or in the neighbourhood of, the joint; from scrofulous disease; from injury to the joint itself, or to the olecranon process of the ulna.

In the forearm, you will find contraction in the prone position, from injury, disease, or interruption to the functions of the nerves, producing spasmodic affection of the muscles, combined with that general spasmodic state of all the muscles of which I have spoken.

Paralysis of the extensors of the wrist and fingers, injuries, rheumatic inflammation, contraction of the fascia of the palm of the hand, are causes producing contraction of the fingers in the flexed position. The last mentioned condition comes on without any assignable cause, and appears to arise from an unhealthy state of constitution. I have been consulted upon incipient cases which presented numerous little nodulated enlargements in the palm (evidently connected with the fascia, as in all similar cases), the fingers slightly contracted, which have totally disappeared upon the application of mild medicines, together with an alteration of diet. This condition appears to be the result of very slow chronic inflammation of the fascia of the palm of the hand, and, being unattended with pain, is very generally neglected until one or more of the fingers have become permanently contracted.

Curvature of the bones is a very common cause of deformity in delicate children, and arises from any of the numerous ailments incidental to early life. The same condition, combined with rachitis, is not unfrequently seen; but, in the latter, every bone appears to suffer not only from simple curvature, but from an actual alteration of their normal size and proportions.

I have now, gentlemen, hastily and very imperfectly alluded to the principal deformities for which you will be consulted, with their causes. I have mentioned paralysis as a common cause in each of the non-congenital, with the exception of the hip and spine. Thus far, there is an analogy with the congenital. In the foot, inflammation of the joint is rarely met with; whilst in the knee and elbow it is perhaps most common. Disease in or in the neighbourhood of the joint is an occasional cause in the ankle, wrist, and fingers; very common in the knee, hip, spine, and elbow. Rheumatic inflammation is a cause in almost all articulations, more frequently in the knee, hip, elbow, toes, and fingers.

There is another cause, which is entirely unknown to us. Contraction comes on gradually and insidiously, without paralysis or any evidence of disease, without pain, or, as far as can be discovered, any interruption to the general health—the motor nerves only being affected. Some time since, I was called to see a young lady, fourteen years old, who had been suddenly seized with contraction of both elbow- and wrist-joints, and of the knees and feet, with spasmodic action of the diaphragm, and, of course, difficulty of breathing. This condition had existed during the whole of the day, but increased in severity towards the evening. She was perfectly sensible, complained of slight pain in the head; the tongue was clean; the skin moist; the pulse at 90, full, but weak. I ordered six leeches to the temples, a mustard poultice along the whole course of the spine, calomel and colocynth, and an aperient mixture combined with solution of tartar emetic. In four hours, the contraction was considerably relieved, and during the night subsided. On the following day, there was a slight return, which

was relieved on the reapplication of the mustard poultice. Ten days afterwards, whilst visiting this patient, I was requested by her father to see her sister, aged seven years, who was attacked with contraction of both knees and feet, and adduction of both thighs. On the previous day she had been, to all appearance, well and in good spirits. In the evening of that day, she was noticed to walk lame, and on the following morning was scarcely able to move. She did not complain of the least pain in the head or elsewhere, was perfectly cheerful, had a good appetite, the tongue being clean and the bowels regular. Upon inquiry, I ascertained that she had just recovered from the whooping-cough. The same treatment was here followed, excepting the leeches, and with the addition of a hot bath. This was applied at twelve in the day. At half-past nine the same evening, the contraction was relieved, and on the following morning had disappeared. I ordered that she should be confined to bed, and the treatment continued in a less active form. On the following day, to my surprise, and in opposition to the advice given, I found her walking about, and apparently as well as usual. In the evening, a relapse followed, precisely as in the former instance. She was immediately placed in a hot bath, and was relieved during its application. I kept her under the influence of tartar emetic in solution for some days, when, with the administration of tonics and nourishing diet, she quickly and permanently recovered. Thus in both instances was there this great disturbance of the functions of the motor nerves, which would, in all probability, have resulted in contraction and deformity, if unattended to, without any interruption to the feelings or the general health of the patients, perfectly coinciding with the history of numerous cases of deformity which present themselves for relief at this hospital.

In the operations for deformities, the least part of a necessary whole, you will find that they will require at times all your self-possession, as well as a thorough knowledge of anatomy and surgery; for, although the division of the tendo Achillis, for example, is a very easy matter, yet even this, the most simple, is not free from casualties. In one instance, false aneurism was the result of division of that tendon, from puncture of an artery, most likely the posterior tibial. In another instance, the surgeon was compelled to cut down and tie an artery, which was found to be bordering the tendon in the form of an irregular distribution.

In division of the adductor longus, again, no man can venture to perform that operation, unless he is prepared to cut down and tie one or other of the large vessels. In the neck, alarming hæmorrhage has followed division of the sternomastoid muscle; and I am informed that a patient died on the table from the admission of air into the vein during that operation.

I mention these cases to show you the necessity for the exercise of great care and caution, as the operations, subcutaneously performed, appear to the casual observer most simple; and although, with the ordinary precautions, you will seldom meet with any difficulty, yet, by the neglect of these precautions, serious results have and may follow.

The after treatment, upon which the cure or useful relief depends, will demand all your patience, and at times long continued perseverance. In young subjects, the division of the tendons alone will afford an immediate relief in most of the deformities; and, with proper care, a short time will suffice to restore the joints to their proper position. But in the older cases, whose deformities are either congenital or the result of accident or disease, nothing but a cautious, careful, and determined course of treatment can possibly effect a cure.

THE IRISH MEDICAL ASSOCIATION AND MEDICAL EDUCATION. At the Annual Meeting of the Irish Medical Association, on June 4th, the following resolution, proposed by Dr. Rawdon Macnamara, seconded by Dr. Walshe of Clara, and supported by Dr. Browne of Belfast, was adopted:—"That the Medical Act passed in August 1858, has proved a seriously defective measure, sufficient provision not having been made in it for the sound preliminary education and subsequent practical experience of those persons entitled to register as qualified practitioners; and that we, therefore, suggest to the Medical Council of Ireland that such changes should be sought for by amendments of this Act as will secure these and such other improvements as may be deemed necessary."

Addresses and Papers

READ AT THE

TWENTY-SEVENTH ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

[Held in LIVERPOOL, JULY 27th, 28th, and 29th, 1859.]

RETROVERSION OF THE GRAVID UTERUS.

By THOMAS SKINNER, M.D., Liverpool.

[Read in abstract 29th July, 1859.*]

"MEDICAL facts, when distinctly discovered, become extremely valuable. If many of a similar kind are brought collectively into one view, the conclusions from them by induction afford a high degree of certainty." (*Haygarth*.)

For all practical purposes, we may look upon retroversion and retroflexion of the gravid uterus as one and the same displacement; but, so far as either of them is concerned, I think it advisable to make two distinct classes; namely, those which occur prior to, or happen coincident with, conception, and which I would designate *Retroversio Uteri ante Conceptionem*; and those which occur subsequently to impregnation, from accident or overdistension of the bladder, or both, and which I would call *Retroversio Uteri post Conceptionem*.

I.—RETROVERSIO UTERI ANTE CONCEPTIONEM.

From my own observation, and from careful study of the observations of others, I am inclined to believe that displacement of the uterus backwards is, in itself, no great hindrance to conception. I am aware that this is an unpopular view of the subject, but I am convinced it is founded in fact; and, as it is necessary, so far as the sequel of my remarks is concerned, to establish the truth of this proposition, I offer the following observations.

Professor Churchill of Dublin, in his excellent manual of obstetric literature, says:—"The most important consequence of retroflexion is sterility; it is very unlikely, not to say impossible, that impregnation should take place when retroflexion exists, because of the mechanical difficulties." (*Diseases of Women*, 3rd edit., p. 287.) In 1849-50, Dr. Rigby published a series of nine cases of retroversion of the uterus, to prove the same point. His various papers, which appeared in the pages of the *Medical Times*, are headed "Retroversion of the Uterus as a Cause of Sterility." At one time, I must confess I was led astray by this imposing title; it was only a few years ago that I discovered it to be the only part of the articles which had any weight. On analysing the cases, I found that one of the patients was a *widow of nine years standing*, while another was a *single lady*. How these two cases could be brought forward to prove the etiological relation of retroversion of the uterus to sterility, it is difficult to comprehend. So far as the other cases are concerned, the evidence of the displacement being the cause of the sterility is only negative, and at the best doubtful. In two of the cases only is there an appearance of positive evidence—namely, cases Nos. 5 and 9; in neither of them can it be said that the uterus was not retroverted at the time of conception. From the narrative of case No. 9, I conclude that the uterus was retroverted; and so far it supports my proposition, that simple uncomplicated displacement of the uterus backwards is no great impediment to conception.

In the early experience of Professor Simpson, it would appear that conception was not an uncommon occurrence with females having the uterus in a state of retroversion; for he says, "When a patient with a retroverted uterus becomes pregnant, abortion is apt to take place." (*Obstetric Memoirs*, vol. i, p. 205.) This statement implies the observation of several cases where conception must have occurred during the existence of retroversion uteri. He further adds, "But I have seen various instances in which the uterus became spontaneously rectified in position as it became larger, and utero-gestation went on to the full time." (*Op. cit.* p. 205.) Does not this statement also imply still further observation of cases where conception had taken place and the uterus in a state of retroversion?

Since 1853, I have personally conducted four cases of *retroversio uteri ante conceptionem*, where the patients have gone to

* We have only lately received the manuscript of this paper from the author.—EDITOR.