ESSAYS IN SURGERY.

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No. III.

DISLOCATION OF THE RADIUS FORWARDS.

BEFORE the publication of Sir A. P. Cooper's great work on dislocations and fractures, the displacement of the upper extremity of the radius over the front of the external condyle of the humerus, was so seldom discovered, that it was considered impossible by some of the most eminent surgeons, notwithstanding a case had been communicated by the French Academy of Surgery. Desault, one of the surgeons to the Hôtel Dieu, disbelieved that it could ever occur, and Mr. Samuel Cooper, the compiler of British and foreign surgery, also doubted whether such an accident could ever happen:—"Hence," observes the latter, "accidental dislocations of the radius, suddenly produced by an external cause, must, if they ever happen, be exceedingly uncommon at its upper end."†

For a description of the accident, and an account of the appearances found on dissection, the reader is referred to Sir A. P. Cooper's work before alluded to, p. 482.

The two first cases, which came under the notice of this celebrated surgeon, after every attempt then known to effect replacement of the dislocation, were left unreduced; but his experience in two subsequent cases led him to adopt a proceeding, which proved successful. The mode of reduction recommended by him consists in extending the radius alone by applying the extending force to the hand, previously rendered supine. The following case illustrates the advantage of combining with this mode of extension the assistance of the radius as a lever, which, I feel assured, is the most effectual and speedy mode of accomplishing our wishes.

CASE.—Aug. 9, 1833. Master Jenkin, of Alscotte, ætat. 10, was thrown from a horse. The elbow came, with great force, in contact with a stone on the road, which fractured the internal condyle of the humerus, and forced the upper head of the radius forwards over the outer condyle. The forearm was semi-flexed and the hand prone. The radius admitted of rotation, and its dislocated head could be seen in motion at the same time in its new situation; the forearm could neither be bent nor rendered straight beyond a certain point. There was a puckering of the integuments just above the dislocated head of the radius, and the fractured condyle was felt projecting below; so that, at first sight, the displacement appeared to be lateral.

The accident having occurred two days before I saw the patient, the parts adjoining the injury were swollen and inflamed. Extension, according to Sir Astley Cooper's direction, was tried without success. I then placed the arm over the back of a chair with the external condyle uppermost, and confining it in this situation by an assistant, I bent the forearm downwards, gently extending the hand at the same time, and thus readily replaced the head of the radius.

† Dictionary of Practical Surgery. Second edition, p. 324.

EFFECTS OF RESPIRING CARBONIC ACID.

To the Editor of THE LANCET.

SIR:—Having endeavoured to call the attention of the Medical Section of the "British Association for the Advancement of Science," to the subject of "the Comparative effects of Human Respiration, and of the Combustion of Charcoal upon the Atmosphere," &c., by the accompanying essay (which was intended to have been read, with some slight modifications, at the meeting of the Association at Newcastle, but was interdicted, by reason of its having been printed, although it was stated to have been printed solely for the purpose of gratuitous distribution amongst such of the members as might think it worth their time to peruse it at their leisure), I send you some of the results of an experiment made by myself relating to the subject in question.

On the night of the 24th inst., at twelve o'clock, I retired to my library, a room 16 feet 6 inches long, 13 feet 10 inches wide, and 9 feet 4 inches high; its capacity was 2129.82 cubic feet. The chimney was closely built up with bricks and mortar. The window was very large, but was perfectly tight as to the admission of air. In this window were two casements, each 3 feet 1 inch high, and 1 foot 2 inches wide; each of them fitted well. The door was 6 feet 5 inches high, and 2 feet 10 inches wide; it fitted well against the stopping strips, but it left a vacancy of about a twelfth of an inch from the floor; and having a key-hole of the ordinary size, it may be said to have afforded a space for the ingress or egress of air, equal to an aperture 3 inches long and 1 inch wide. The temperature of the room was 54 deg. Fah.

The stove employed on this occasion was on Harper and Joyce's principle, and was about 7 inches in diameter and 16 inches deep. The inverted cone, through which the air entered at the bottom of the stove, was perforated with 12 holes, each about $\frac{1}{4}$ inch in diameter. The ventilator on the top was about 4 inches in diameter, and of the "wheel" construction; it was left perfectly open.

The charcoal was of the common kind, and not recently prepared; the stove could contain 4lbs. avoirdupoise of this charcoal, which always required about two hours combustion before the aqueous vapour was entirely dissipated. On several occasions I had weighed the stove, with its contents, immediately after the complete dissipation of its aqueous vapour, and again after an interval of from 3 to 4 hours. The loss usually averaged after the rate of 1 ounce per 18 minutes. When fully charged with common charcoal, 4lbs. of fuel would always maintain combustion during from 20 to 21 hours, the ventilation being quite open all the time. This stove was charged with fuel, and lighted at 11 P.M. on the night of the 24th inst.; it was placed out of doors for one hour, and then taken into my room, and placed at a distance of 7 feet from the front of the sofa upon which I lay, and at right angles to my head. The sofa was 3 feet wide and 2 feet high, consequently my head may be supposed to have been about 8 feet 6 inches distant from the stove, and perhaps about 2 feet 3 inches above the floor. Having fastened the door inside, I lay down in a thick, woollen dressing-gown at 12. At 4, A.M. I felt a slight degree of giddiness, which was scarcely perceptible, unless any attempt was made to turn upon the pillow. This increased until 5½ A.M., when my sensations resembled, in every respect, those felt by many persons when at sea, viz., intense vertigo, aggravated by the slightest motion; a desire to be sick, without the power; great prostration of strength, and apparent inanition, or want of capability to move any muscle of the body. To these were added a very full, throbbing, quick pulse, producing an impression upon the brain as though the arteries were rapidly distended to their very utmost capacity; the maximum impulse being accompanied by a peculiar thrill, resembling that produced by a light touch upon a piece of catgut, which had been strained until it was about to break. The cephalalgia was of unwonted and agonising violence, and particularly affected the occipital regions. I felt no symptoms of suffocation, although I could easily have fancied myself poisoned. It was evidently time to move, so I quickly slid over the edge of the sofa, and tried hastily to open the nearest window. My strength failed; I fell immediately upon the cushion of the sofa, and in less than a minute was literally streaming with perspiration. In a few minutes I made another effort to open the other window, and with great difficulty succeeded. I crawled upon my hands and knees to the sofa again, and for a short time remained insensible. The fresh air, however, soon revived me, and I recovered sufficiently to get to the door, and opened it, and dragged the stove into the adjoining passage, and got again to the sofa, where I remained for 13 hours, enduring the utmost distress. About 7 A.M. my wife came into the room; she saw me very ill, but I could not give her any information as to the cause. She invited me to take some pulv. rhei., which was the only medicine she could command. I nodded assent, and it was with the greatest difficulty that I could sustain myself in any position to swallow it.

In the course of the day (about 2 P.M.) a medical friend, Mr. T. Davis, of Nailsea, accidentally called upon me. He saw me in my predicament, and prepared a few effervescing draughts, which soon set the rhubarb in action. At 7 P.M. I dressed myself, and while everything was fresh in my memory I tried to write; but the headach returning with the effort, I was obliged to discontinue. At 12 last night I retired to the same sofa, all things remaining as during the preceding night, with the exception of the stove, slept well, and this morning am almost recovered.

If one ounce of charcoal was consumed in 18 minutes, 18.33 ounces would be consumed in five hours and a half. If 12.94 grains of carbon be equivalent to 100 cubic inches of carbonic acid gas (Dr. Thomson), 18.33 ounces, or 8019.375 grs., of carbon will be equivalent to 61973 cubic inches, or 35.85 cubic feet, of carbonic acid gas. If the respiration of an adult human being consume the oxygen of 151.1 cubic feet of atmospheric air (vide Essay, p. 13) in 24 hours, it will produce 30.22 cubic feet of carbonic acid in that time, or 6.92 cubic feet in five hours and a half. Hence the total quantity of carbonic acid gas, eliminated by both stove and respiration, amounted to $35.85 + 6.92 = 42.77$ cubic feet.

Now, the total capacity of the room was 2129.82 cubic feet; and, if we could suppose the chamber to have been perfectly air-tight, the total quantity of carbonic acid that could possibly have existed in its atmosphere at the expiration of five hours and a half, would have been but 2 per cent.

	Cub. feet.
The nitrogen would remain as before	1703.86
The oxygen would have been reduced to	383.19
And the carbonic acid, instead of being about 1 cub. foot, would be increased by	42.77
	2129.82

I regretted, exceedingly, that I could not avail myself of a mercurial trough which was standing upon a table between myself and the stove, all in readiness, with receivers filled with mercury, &c., to have retained some of the air of the apartment before the window was opened; but it was absolutely impossible for me to have done so at the proper time.

And now comes the point:—Is carbonic acid a poisonous gas (Dr. Christison)?—or does it kill by suffocation (Dr. Thomson)? I conceive Dr. Thomson's (?) opinion to be correct. In my own case, all the symptoms of poisoning were apparent, and none of those of suffocation, and I attribute them entirely to the noxious effluvia which escaped with the carbonic acid gas; for unless the carbonic acid did really gravitate, which is not at all probable, the quantity proportioned to the atmosphere of the apartment was not sufficient, even at its maximum, *to have produced suffocation*; and I am certain that I have breathed a larger proportion of pure carbonic acid gas, *without being poisoned.*

In the instance of death related by Dr. Christison, of a boy who had been teazed by a party of smiths, who held a recently-extinguished candle under his nose, the effect never could have resulted from the carbonic acid gas, but must have been occasioned by some such noxious principle as so completely enervated yours, most truly,

CHARLES T. COATHUPE.

Wraxall, near Bristol, Oct. 26, 1838.

THERAPEUTIC ACTION OF THE ERGOT OF RYE.

To the Editor of THE LANCET.

SIR:—I trust that the few remarks I am about to make upon the action of the secale cornutum, will not be unacceptable to the generality of your readers. In the cases which I published in the 24th and 1st numbers for Sept. 16th and Oct. 7th, 1837, of the "Lon. Med. and Surg. Journ.," I attempted to show the specific properties of this powerful remedy, and my more extended experience, within the last few months, still further confirms my opinion of its utility in promoting uterine action. It has been stated by many practitioners of eminence, that the use of the ergot not only produces most alarming consequences to the mother, but likewise, in some instances, proves fatal to the child, and that it occasions retention of the placenta. Such effects I have not witnessed, as I always endeavour to prevent the sudden expulsion of the body of the fœtus resulting from the full action of the ergot, and thus to allow the uterus to contract gradually on the secundines, which I invariably find lying within the vagina.

Such being the grounds upon which the opinions respecting the inutility of the ergot are based, I am led to observe, in differing from these opinions, that the drug, when productive of unpleasant effects, must have been either deteriorated in quality or administered to persons of a leucophlegmatic temperament. The only symptoms which I have noticed when given to such individuals have been a sensation of sinking at the pit of the stomach, together with irregularity in the heart's action. In such cases, I generally administer it in a small quantity of warm ale or spirit, which prevents such an occurrence. Ingleby, in his "Treatise on Uterine Hæmorrhage," remarks that, "To denounce the ergot, and deny its efficacy, without an experimental acquaintance with its action, is not only most unphilosophical, but altogether inconsistent with patient research;" and I feel confident that if the spirit of the above quotation had been properly observed, the number of failures would have been comparatively few, since, in my own practice, the action of the ergot has been as manifest as that of opium or mercury.

My experience leads me to consider the following appearances as indicative of the genuineness of the ergot:—If the powder be put into a small vessel (a cup for instance), and about ℥j. of boiling water poured thereon, then immediately covered, and allowed to stand for a few seconds, on removing the cover, the ergot should remain totally insoluble, and the infusion should assume a deep pink colour; on the contrary, if small portions of the ergot be seen floating on the surface of the water, and the infusion present a milky appearance, then the action of the remedy cannot be relied upon, for I have never known it, when given under such circumstances, to produce the least increase of uterine contraction, but invariably to be followed by more or less of a feeling of sinking at the pit of the stomach, attended with irregularity of the heart's action, and total cessation of pain. The above fact has not, as far as I am aware, been noticed by any writer, either on materia medica or midwifery. Should the test I have named, as connected with the administration of the ergot, prove to hold true in other hands, as it has done in mine, I shall be fully rewarded for my labour in endeavouring to find out some way of judging of its purity prior to its administration. Dr. Waller appears to think that if the pains are feeble, though regular, the ergot may produce increased action of the uterus; but that if the pains have ceased, its action is less to be depended upon, and it cannot be trusted when it is found necessary to produce premature labour. Dr. Ramsbotham, in one of his lectures, published in the "Med. Gazette," has enumerated several cases wherein he produced premature labour by means of the ergot alone, and the same result has occurred, in more than one instance, in my own practice.

CASE I.—I was called upon at 11, A.M., Sept. 26th, 1838, to attend Mrs. R., a woman of stout and robust habit, whilst in labour of her fourth child. I ascertained, on my arrival, from her own account, that she had been in labour for more than two weeks