belong to every artery, and probably to the arteries of the cord. It is quite true that the foetal tufts dip into the placental sinuses, and, as it were, float freely in the tiny wells of maternal blood. But, nevertheless, there may exist some nerve-communication in the structures which hold these foetal tufts down in the placental sinuses. The possibility of such nerve-communication is suggested by the transmission of impressions from the mother's brain to the foetal tissues. We are all aware that such mental impressions have produced very curious results under different circumstances; for instance, the well known case where a mother saw her son's fingers crushed by a falling window, and in half an hour's time her own finger showed changes similar to those in her boy's hand—changes which could only be the result of nervous influence. It would further appear that, whatever be the means of communication, the transmission is much more easily effected in some systems than in others. We know that, in numerous cases, intense anxiety on the part of the mother, a persistent fear that some change in her child will correspond with something that she has seen that has shocked her, has not resulted in any effect. On the other hand, these transmitted effects are occasionally recurrent in the same individual, as stated in the second case I related and in Mr. Thompson's case, where such change occurred a second time in the same woman. In my case, the first was a rat, the second a dog; in Mr. Thompson's, the first was a toad, and the second a tortoise, which left their impress upon the embryo. The systems of these women must have been inordinately susceptible to such impressions. This arrest of development, producing a resemblance to some natural object, some lower form of the vertebrata, is rendered comprehensible by the speculations and observations of Von Baer. Long before the hypothesis of Mr. Darwin had stirred the still waters of our ideas as to progressive development, Von Baer had observed that the human embryo corresponded, at different periods of its existence, to the foetal forms of the vertebrata. At first, the human ovum is undistinguishable from that of a fish, as in the well known ovum of the late Professor Agassiz. It was a vertebrate ovum, and no more could be said. Then it is homologous to that of a reptile, then to that of a bird, and ultimately to that of a mammal.

Such shocks, then, might arrest the development of the embryo, when they occur (as they did in the cases I have related) before quickening. In fact, in all the cases I am acquainted with, the impression was received in the early months of pregnancy. This being the case, it produces such a perversion of tissue-growth as induces a retrograde movement in the foetal lineaments, already assuming the characteristics of humanity, and a reversion to a lower type. Certain it is that these impressions occur some time before quickening, and that a certain amount of development secures the foetus from the effects of any of these modifying causes. Neither are such impressions made upon the nerve-centres of the mother, and communicated to the foetal form, confined to the human mother only. Certainly we have no record of a shock affecting any lower animal when frightened during pregnancy; but a very interesting case is given by Danniell in his *Field Sports*, which bears upon this matter. Danniell had a pointer bitch of pure breed, which he was conveying to a pure sire residing at some distance. We all know how carefully the true sportsman guards against the possibility of any *faux pas* which might militate against the purity of the strain. Consequently, when Danniell, in passing through a village, found the susceptible dogs pressing their attention upon his convoy, and the flirtation with one objectionable cur threatening to become specially serious, he felt no hesitation in shooting the intrusive canine cavalier. No physical intercourse had ever taken place, and the object of his journey was carefully secured. But, to his dismay, all the pups bore the impress of the cur he had sacrificed, and the nerve-centres of the pointer-bitch transmitted to her progeny the image which was indelibly stamped on them and fixed in her memory.

The transmission of these impressions is a matter which deserves more attention than it attracts, and a larger collection of cases and further investigation of the subject may perhaps elucidate the question, and clear up some of its difficulties. But it is now time to leave this section of the subject, and turn our attention to the practically interesting side of the question. This is the relation of such malformations to the diagnosis of the presentation. I can assure you, from my own experience, that the diagnosis of such a malformation, when it forms the presenting portion of the foetus, is a very puzzling matter. No writer on obstetrics with whom I am acquainted has ever described such malformations as we are now considering in relation to their presentation, and the difficulties arising therefrom. Even that brilliantly suggestive man, my obstetrical teacher Sir J. Simpson, did not give us any indications for the recognition of such modified forms; neither, so far as I am aware, have any statistics been collected to show that unusual presentations are more than ordinarily frequent in foetuses so altered, or to demonstrate that such changes in the configuration of the foetal

head, altering its relation to the pubic cavity, modify the position of the foetus, or cause unnatural presentations to be more frequent. Such modified and altered foetuses, when the change is pronounced, never survive. It is fortunate that they do not, as it renders it quite unnecessary to consider the question from the moral side, of the propriety or necessity of inducing their destruction by acts either of commission or omission. Still it is somewhat suggestive that babies so altered, anencephalous or other monsters, always seem conscious of the impropriety of survival in their case, and atone for their shortcomings and indiscretions by early demise. Probably they are not equal to maintaining an individual and separate existence.

CASE OF FRACTURE THROUGH BOTH RAMI OF THE LOWER JAW, TREATED BY EXTERNAL INCISION AND THE WIRE SUTURE.

By THOMAS ANNANDALE, F.R.S.E.,

Surgeon to the Edinburgh Infirmary, and Lecturer on Clinical Surgery.

IN September last, I was asked to meet Dr. Moodie of Stirling in consultation on the case of J. C., aged 18, who had received a severe injury to his face from a railway waggon. A few days previously to my visit, the patient, when following his employment of a clerk on the railway, was knocked down, and the wheel of a waggon passed over the lower part of his face. Great swelling of the whole face followed the injury; and Dr. Moodie, who was called to see him, at once recognised a fracture through both rami of the jaw. Careful means were used to keep the fragments in position, but without success, and I was accordingly asked to see the patient.

An examination determined a fracture on the right side of the lower jaw at the junction of the angle and ramus, and one on the left side through the ramus, immediately above the angle. The body of the bone was uninjured. By a little manipulation and elevation of the body of the bone, the fragments could be brought nearly together; but, when the force was removed, the fragments at once separated, and the body of the jaw fell downwards so as to leave the mouth open and fixed in this position, as the patient had no control over the movements of the bone. There was a small wound on the right side of the cheek anterior to the seat of fracture, which was discharging pus freely. The experience of the treatment which had already been adopted having shown that some special means would be required to keep the fragments together, it was decided that the patient should be brought to town and placed under my care in the infirmary. Accordingly, he was admitted into my wards on September 28th; and, on the following day, I placed him under the influence of chloroform, and proceeded to secure the fragments together by means of the wire suture. Owing to the position of the fractures, it was found impossible to drill the jaw from the inside of the mouth, and I therefore made an external incision about two inches in length over the seat of fracture on the right side, exposed the fragments, and having drilled them, brought them together with strong silver wire. A similar incision was made over the fracture on the left side, and the fragments were drilled and treated as on the right side. The fragments on this side could not be brought accurately together, but they were secured as nearly so as possible. After the operation, there was still a slight tendency for the body of the jaw to become depressed, and in order to counteract this, a piece of soft gutta-percha was introduced between the molars of the upper and lower jaws on the left side, and held in this situation until it took a mould of the parts and became hard. It was then allowed to remain in this position, and a chin-bandage was applied to keep the jaws steady. The suppuration, which had existed before the operation, continued for several weeks, and from time to time small portions of necrosed bone separated, and were removed by the mouth. On November 13th, the wire on the left side being quite loose, was removed; and, on December 5th, that on the right side also became loose, and was removed. Both wires had been twisted on the inner aspect of the jaw at the time of the operation, and consequently both were removed by the mouth. After the removal of the second wire, the suppuration almost ceased, and the patient was dismissed on Dec. 9th. On January 6th, he returned to show himself, and there was then only a small superficial wound on the right side. The movements of the jaw were excellent, and the patient could masticate with ease. When the jaws were closed together, there was a slight lateral displacement of the lower one; but with this exception, and a slight swelling of one cheek, the parts were natural in appearance.

REMARKS.—Fractures of the lower jaw have been occasionally treated by means of the wire suture (see Hamilton on *Fractures and Dislocations*, fourth edition, p. 119), and I have myself used this method

in several cases of comminuted fracture of the body of the bone, drilling the bone through the mouth or through a wound which already existed. Mr. Bickersteth of Liverpool (*Medico-Chirurgical Transactions*, vol. xlvii) has suggested and practised a method of securing the fractured ends of this and other bones by means of a drill, and has given a most interesting account of several cases in which it was successfully adopted. The employment of special external incisions, as in the case related, has rarely if ever been practised in connection with recent fractures of the lower jaw. The objection to them is the resulting external cicatrix or cicatrices which, although not of so much consequence in the male, as the whiskers will usually completely cover them, must cause some deformity in the female. For this reason, I would only advocate an external incision or incisions in cases which cannot be successfully treated by other means. In the removal of the tongue, or portions of it, and in some cases of tumour of the jaw, I always now bring the ends of the divided jaw together by means of the wire suture; and my experience of this method is, that it is safe, efficient, and easy of application. Since writing this, Mr. Mason of St. Thomas's Hospital has been kind enough to direct my attention to the ingenious method of employing the wire suture in bringing the end of bones together, practised by him (*Medico-Chirurgical Transactions*, vol. liv). His method allows the wire to be easily removed at any time, and is, therefore, to be preferred to that which has usually been employed, should the case admit of the needle being passed in an oblique direction through both ends of the bone.

ANOTHER CASE OF POISONING BY HOMŒOPATHIC SOLUTION OF CAMPHOR.

By GEORGE JOHNSON, M.D., F.R.S.,
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In a paper of mine published in the last volume of the Clinical Society's *Transactions*, there will be found notes of five cases in which symptoms more or less distressing and alarming were caused by homœopathic concentrated solution of camphor. I am indebted for the particulars of the following case to a former pupil, who gives me permission to publish them, but begs me to withhold his name, for the reason that it is a rule of the public service in which he is engaged that no medical officer shall publish his cases without special permission, which is not always granted when asked for. I give the case as reported in a letter addressed to me by the medical officer who saw the patient.

"I was called in to see a lady about thirty-five years of age, and found her with a very pale face and weak pulse. She said she had been suffering from a bad cold and feverishness; that she had got up late, and had her dinner at about two o'clock. About an hour after dinner, her sister, who is an experienced nurse, dropped seven drops of homœopathic solution of camphor on a lump of sugar, and gave it to her. Immediately after taking it, she was attacked with a very faint feeling, which compelled her to lie down flat on the hearth-rug; and she nearly lost consciousness. This lasted about five minutes. When I arrived, her face was very pale, and her pulse was weak. I ordered her to bed, and gave her some aromatic spirit of ammonia and lavender. She was very drowsy after she got to bed, but had no more vertigo or faintness; and in a day or two she got rid of her cold. I am led to believe that this was a case of camphor-poisoning similar to those which you have published, for the following reasons: the attack occurred immediately after taking a dose of camphor; the patient says that she had never before had a similar feeling of faintness; and the vertigo or faint was followed by drowsiness."

There can, I think, be no doubt that the symptoms were a direct result of the camphor. The dose was smaller than in any other case which I have met with in which poisoning has resulted from the incautious use of this dangerous compound. The homœopathic concentrated solution of camphor is a saturated solution of camphor in alcohol, the proportion being an ounce of camphor to an ounce and a quarter of spirit. Seven drops would be an uncertain quantity; but seven measured minims would contain rather more than five and a half grains of camphor. In one of the cases which I have recorded in the *Transactions* of the Clinical Society (vol. vii), a dose of twenty-five drops, taken by a young lady for a cold, caused violent convulsions and nervous sequelæ which continued for several months. In another case, eight doses of three drops each, taken by a clergyman within forty minutes, caused violent headache, sickness, and inability to work for two months. In a case communicated to the *BRITISH MEDICAL JOURNAL* by Dr. Clifford Allbutt (Dec. 6th, 1873), two doses of at the most ten drops each rendered a robust mountain-guide so giddy that he was unable to proceed on his journey, and he had to be deposited in a

cave for safety. Lastly, in two other cases, a teaspoonful dose caused convulsions and coma.

It is time that the public should be warned of the danger which they incur by the use of this poisonous homœopathic concentrated solution of camphor, which is more than seven times the strength of ordinary spirit of camphor. It is notorious that the modern disciples of Hahnemann have gone from the ludicrous extreme of infinitesimal dilution to the dangerous extreme of the greatest possible concentration of active and poisonous drugs.

ON THE RELATIONS BETWEEN DIPHTHERIA AND SCARLET FEVER.*

By ARTHUR RANSOME, M.A., M.D. Cantab.,
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THE following case shows the close connection between these two diseases.

On February 23rd last year, a boy at school had scarlet fever. He was isolated until March 25th, when his brother saw him and took the complaint. Other cases occurred at the school on the 29th, 30th, and 31st. On April 2nd, one of the masters had a rigor and sore-throat; he left at once, and was seen by me next day. On the third day, diphtheritic patches covered the tonsils; but there was no rash. Slight albuminuria accompanied the pyrexia; both disappeared by the seventh day. On April 9th, six days after he arrived at home, his mother was attacked, membranous exudation appearing in both tonsils; but there was no albuminuria, and no rash.

The origin of these cases must surely be traced to the epidemic of scarlet fever at the school; for there were no cases of diphtheria either in the neighbourhood of the school or near their home. The close relationship of these diseases has often been pointed out. In the *Report of the Medical Officer to the Privy Council* for 1859, Dr. Burdon Sanderson gives cases of diphtheria and scarlet fever coexisting; there also I find three cases of diphtheria following three of scarlet fever, and the former spreading to a neighbouring cottage. These diseases have much in common. They are alike in their zymotic or epidemic character; both are characterised by high pyrexia; their chief point of attack is the throat and the glands of the neck; and a rash is found in both diseases. Their sequelæ also are similar: suppuration in the lymphatic glands, ulceration of the ears, arthritic affections with or without cardiac complication, kidney-disease and dropsy, general or localised paralysis of the nervous system. I have seen each and all of these consequences after both diseases.

Some physicians have been led by these points of resemblance to regard them as one disease, the same poison producing different manifestations; but, in spite of their similarity, I venture to think the differences are too important and too numerous to permit so simple a solution of the problem. In the first place, diphtheria is seen to arise directly from scarlet fever. I have never known the converse action. There are other etiological differences. Though both are contagious, diphtheria is less so, and more frequently occurs in single cases. Where several cases occur together, some general cause is probably concerned. Scarlet fever is more frequently conveyed by clothes or other fomites, by attendants on the sick, and it may be conveyed by means of milk. I am not aware that diphtheria has ever spread by any of these means. The two diseases differ in their geographical distribution. Diphtheria is common in India, where scarlet fever is unknown. Colonies and all new settlements seem peculiarly liable to diphtheria; it has appeared in the Australian colonies and in South America, attacking the otherwise healthy town of Buenos Ayres. In Lima, it was noticed that the black races withstand its contagion. It selects high and well drained districts, when low-lying lands in the vicinity escape. Dr. Blake, in the *Transactions of the Medical Society of California*, mentions a most fatal epidemic at an elevation of 4,000 feet. Many places in England subject to diphtheria are either high or well drained. Scarlet fever also haunts certain places; but these scarlet-fever fields are not at high levels, nor are they those in which diphtheria is especially prevalent. Diphtheria is often connected with defects in the house-drainage, not always due to imperfection in the house-connection with a general sewage-system, but to leakage from private cesspools; so that a question of spontaneous origin may arise in this disease, which is hardly admissible with respect to scarlet fever. Season has a different influence on the two diseases; scarlet fever being most prevalent in the autumn, low barometric pressure and greater humidity in the air favouring its diffusion; while diphtheria may arise at any season, and, in my own experience, its most virulent epidemics have occurred in the summer months.

* Abstract of a paper read before the Epidemiological Society of London, January 13th, 1875.