the effects of cold packing, and, as soon as preparations could be made, sheets wrung out of iced water were wrapped round the body, and renewed as soon as they became warm. At 1.15 P.M., after one wet pack, the temperature in the rectum was 106.2 degs.; at 1.20 P.M., 105.4 degs.; at 1.30 P.M., 105 degs. The circulation had, however, become very much more feeble, and the respiration somewhat gasping; he was, therefore, removed from the pack, and placed between blankets. 2.30 P.M. Temperature 102 degs. in axilla. Respirations 57. 2.40 P.M. Respirations 48. The right conjunctiva was decidedly more sensitive to touch than at any time previously. The patient contracted the forehead, and turned away the head when it was touched; the left conjunctiva was scarcely at all sensitive. 5.10 P.M. Temperature 103.9 degs. Respirations 56. Pulse 180 at least. The pupils acted very vigorously; both conjunctivæ were sensitive, especially the right. He was fed again with beef-tea, milk and gin. 10.15 P.M. Temperature 105.5 degs. Respirations 58. Pulse 180. The pupils acted very sluggishly; the conjunctivæ were quite insensible to touch. Six ounces of urine were withdrawn. 10.45 P.M. He was fed by means of the stomach-pump.

April 29th, 3.50 A.M. Respirations 52. Temperature 106.7 degs. in the axilla. Pulse imperceptible. The pupils were dilated and insensible. He died at 4.30 A.M., having lived sixty-eight hours after he was found.

Necropsy, thirty-five hours after death. On removing the calvarium and dura mater, the vessels of the surface of the brain were observed much more full of blood than usual. On slicing the organ, the surfaces of the sections were of a darker colour than normal, the puncta cruenta more numerous, and studded over all the surfaces were small clots of extravasated blood, the size of large pins' heads. These clots were observed throughout the sections of the cerebrum, but not in the cerebellum or medulla oblongata. The spinal cord was normal. The spinal dura mater was dark and congested. The laryngeal surface of the epiglottis was mottled with pink spots. Five drachms of bloody serous fluid were found in the pericardium. There was a white patch of old lymph on its anterior surface. The right auricle was very full of dark blood, fluid and coagulated. There was a good deal of dark soft clot in the right ventricle, and a little in the left ventricle. The valves were competent. There was a patch of extreme congestion in the inner surface of the left ventricle between the aortic and mitral valves, involving both. The surfaces of the lungs were of a dark claret colour, mottled with spots of a bright pink hue. The lungs were very full of dark blood; the sections had a similarly mottled appearance to that of the surface. There was no pleural extravasation. The bronchial tubes were free; their inner surface was of a dark purple hue. The digestive organs were normal. Both kidneys were very much congested; their capsules were not adherent; their outer surfaces were smooth, mottled; a few bright pink spots were seen on section of the left kidney.

REMARKS.—Those of the profession who have inquired into the subject of poisoning by the inhalation of coal-gas are principally indebted to M. Tourdes (formerly of Strasbourg, now of Nancy) for an account of the symptoms during life, and *post mortem* appearances in cases of poisoning by this agent. I have not myself had access to the original work of M. Tourdes, and owe the slight knowledge I possess of it to a paper in the *Edinburgh Medical Journal* for July 1874, by Dr. William Taylor, in which he gives a very complete account of the symptoms and treatment of a case under his care, which terminated fatally, but of which there was no *post mortem* examination. In some respects, the present case resembled Dr. Taylor's. In both there was a certain resemblance to apoplexy. In both were observed the rotation of the eyeballs, than which, Dr. Taylor remarks, "no individual symptom was more remarkable". But, in Dr. Taylor's case, there is no record of high temperatures; on the contrary, the surface is reported to have been cold. The same is said of one of M. Tourdes's cases; in another, however, which recovered, the patient is reported to have been feverish for one night. As the result of his observations, M. Tourdes "considers the following to be the peculiar *post mortem* characteristics which we may expect to meet with in cases of asphyxia by coal-gas. 1. Coagulation of the blood; 2. Its deepened tinge; 3. Extreme brilliancy of coloration of pulmonary tissue; 4. The abundance and nature of the froth in the air-passages; 5. Intensity of cerebral congestion; 6. Engorgement of vertebral nervous system, and extravasation of coagulated blood in spinal column; 7. Vivacity of injection of the mucous surfaces at base of tongue; 8. Occasional rose-coloured patches on thighs." No mention is made of the kidneys.

In the present case, the most remarkable *post mortem* appearances were, the congestion of the brain, the minute clots of extravasated blood which were found in large number throughout the brain substance, the colour of the lungs (dark claret, mottled with bright pink

spots), the distended condition of the right heart, and the congestion of the kidneys.

As regards the treatment, the cold affusion was the only agent that seemed to have the slightest beneficial influence, and that was temporary only. The administration of oxygen certainly had no good effect whatever; on the contrary, it seemed temporarily to increase the distress. In any future case, I should feel disposed to feed the patient from the commencement by means of the stomach-pump; to employ the cold affusion and ice to the head; and, in case of a high temperature supervening, to have recourse to the cold pack.

This case supports the view already proved by the cases recorded by M. Tourdes, that a proportion of coal-gas to atmospheric air less than that at which the mixture becomes explosive, may be sufficient to cause death, if respired for a sufficiently long time. As recorded by this gentleman, a family of six persons were poisoned by an atmosphere in which a stove had been a long time the seat of active combustion, and in which a candle had burned to its end.

According to M. Tourdes, it requires a minimum of one part of gas to eleven of air to constitute an explosive mixture; whilst, in his experiments on animals, he found that an atmosphere containing one-fifteenth of gas caused the death of rabbits in nine, twelve, or fourteen minutes, and of pigeons in five minutes; that it proved fatal, though in much longer time, when it formed one-thirtieth of the atmosphere, and that forming one fiftieth, it still ultimately caused death. (Quoted by Dr. Taylor.)

ON A NEW RHINOPLASTIC OPERATION.*

By JAMES HARDIE, M.D., Manchester.

ESTHER MITCHELL, aged 16, came first under notice on February 5th, 1874. She had lost her nose in early childhood, from disease, of which, however, she could give no account. She was well nourished, of good colour, and evidently in the enjoyment of robust health. With the exception of a portion of the columna, and of both alæ, the entire nose, including the cartilages, nasal bones, septum, and portions of the turbinated bones had disappeared. In place of the nose, there was thus a large cavern, through which the upper part of the throat was plainly visible. The edges were surrounded by thin, white, scarred skin, which was adherent to the subjacent bones. This condition was especially marked at the root of the nose. Here there was a considerable depression instead of a ridge, and from it a white scar ran up on the forehead, from which it appeared probable that this part of the frontal bone had also been implicated by the disease. The portion of each ala which had been left was about half an inch in length and a quarter of an inch in width. These parts were turned upwards, and had united with the cheek, forming the boundary of the cavity at the lower extremity of each side. About a third of an inch of the columna remained, and this projected forward, with its end free. The hard palate was perforated by a long narrow aperture.

During the summer of last year, I made an attempt to form an artificial nose from the upper arm, by the Taliacotian operation, preferring this operation to that from the forehead, partly from an unwillingness to cause further deformity, and partly because I feared that, from the thin, cicatricial condition of the parts which would form the pedicle, the vascular supply might, in this case, be insufficient for the nutrition of the flap. This same cause, however, rendered the operation from the arm completely abortive, not the smallest union taking place. I, therefore, replaced the flap on the arm, and allowed the face to heal. This failure was scarcely to be regretted, for, on reflection, it seemed quite improbable, however successful the operation might have been, that the result could have been better than a mere covering to the cavity. Owing to the complete loss of the nasal bones, it would have been a difficult matter to have given any projection forwards to the transplanted flap, or in any way to have fashioned it into the similitude of a nose.

The problem which, therefore, now presented itself was, how to provide a substitute for the nasal bones and cartilages, so as to give the required prominence. This is, perhaps, the most serious difficulty which has to be overcome in rhinoplastic operations. Attempts have been made to meet it by reflecting a piece of the periosteum from the frontal bone; but it is scarcely to be expected that very successful results would follow such a method; and, when the frontal bone has also been involved in disease, it is obviously not to be thought of. It occurred to me that it might be possible, with a sharp chisel, to detach forwards and inwards the nasal process of each superior maxillary bone,

* Read before the Surgical Section at the Annual Meeting of the British Medical Association in Edinburgh, August 1875.

which should then be united in the middle line, so as to form a basis of support for a subsequent superstructure of soft parts. Such a method might, I believe, be usefully had recourse to in certain cases of partial loss of the organ; but, on further consideration, I was doubtful whether it would be very serviceable in the present case, in which both bones and cartilages had to be provided for.

The idea of using a portion of one of the patient's fingers then presented itself. The object to be secured being the provision of a support which should have prominence and resistance, and at the same time be capable of assisting in the nutrition of a future covering of skin with which it would be invested, my thoughts were at first directed to the possibility of transplanting a toe, as in such respects suitable. At the suggestion, however, of Mr. Peter Tytler, the house-surgeon, a finger seemed much more feasible; and, as a portion only would probably be required, this appeared to be a small loss, if a nose could thereby be gained. I, therefore, explained the matter to the patient, and, with her sanction, determined to try Mr. Tytler's proposal.

Accordingly, on December 10th, I made an incision along the middle of the palmar aspect of the distal phalanx of the left forefinger, with a transverse incision at its base, and reflected two lateral flaps. The margin of the nasal cavity was then denuded, and the skin slightly raised from the bone, so as to allow of its being somewhat everted. In this way, a tolerably broad fresh surface was secured to oppose the flaps from the finger. The parts were then carefully stitched together, the arm being supported by long strips of plaster passed round the neck, over the arm, and under the elbow. A strip of plaster was also passed round the head and over the face; but it was found in a day or two that this was of little service, and it was afterwards discarded. The patient was put to bed, and enjoined to be as careful as possible against disturbing the parts, and a nurse was appointed to watch her during sleep, for several nights, with the same end in view.

During the first three days, the constrained position caused a certain amount of restlessness, and it was observed that the finger had slightly shifted its position from the perpendicular. After this, however, no further complaint was made, and the parts were left undisturbed for ten days. It was then found that adhesion had taken place on the whole of the left side, but that on the right side there was some discharge under the finger. In the course of another week, when it was considered safe slightly to raise the finger, it appeared that but little union had occurred on the right side; but, as it was hoped that the process of granulation might effect a good deal, the course of matters was not interfered with. It soon appeared that this conjecture proved correct. In time, union took place on this side nearly as completely as on the left.

Being most anxious that the vascular supply between the face and the finger should be complete before I divided the latter, having before me the risk of losing a portion of the patient's finger, without obtaining the end for which the operation was undertaken, I kept the arm bound up to the face for the long period of three months. And it is worthy of remark that, after the first few days, the patient never appeared to suffer from the irksomeness of the restraint.

On February 17th, 1875, an elastic ligature was tied round half the circumference of the finger at the middle of the second phalanx. On March 10th, the ligature was passed round the other half; and on the 24th, the bone was divided with the forceps, thus severing the connection of the distal phalanx with the hand. Both ends of the bone bled, showing a satisfactory vascular supply from the face. This method of gradual separation was adopted as a precautionary measure, in order to assist the newly formed vessels in accommodating themselves to their increased area of supply.

The portion of the middle phalanx which remained on either extremity was then dissected out, and the ends covered with the soft parts, the separated phalanx being protected with cotton wool.

For some time after the arm was released, there was a little stiffness of the elbow-joint; but, in the course of two or three weeks, it had completely passed off.

A few days were sufficient to show that vitality was perfect in the whole of the removed phalanx.

On April 9th, the alæ were separated from the cheeks and attached to the lower extremity of the transplanted finger. In a fortnight afterwards, it was observed that the nail, which hitherto had remained of its original length, had made a considerable amount of new growth, owing to the increased quantity of blood which was now supplied to the part. In the beginning of May, the nail, with its matrix, was dissected off, and in the end of the month the aperture which remained on each side, between the alæ and the original line of union of the phalanx with the margin of the cavity, was closed by sliding a small triangular flap from the cheek over it. The columna was also at the same time attached to the tip of the phalanx.

It had been my intention, as will have been gathered from previous remarks, to use this transplanted portion of the finger simply as a basis of support for a flap of skin which should be afterwards adapted in one of the usual ways. I was, however, so well satisfied with the appearance of the part, as in itself a substitute for the nose, that I carried out these latter operative details, which otherwise had better been left till the covering of the skin was applied.

For some time, the appearance remained remarkably good; but lately, and since I announced this paper to the section, the part has receded into the nasal cavity to a considerable extent, so that it by no means fulfils the ornamental purposes it at one time gave promise of, and now I propose to carry out the intention I originally had, of covering it with skin, either from the cheek or arm.

So far as I know, the operation I have described has not been previously had recourse to; and, although I do not now regard my case as quite complete, it is so far complete as to show the feasibility of its distinguishing feature. Hunter's experiments of transplanting the spurs of cocks to their combs, and the testicles of the same animals to the abdomen of the hen, are sufficient to show that there is no *a priori* reason why a portion of the body may not be transferred to a new situation. Indeed, Hunter found that, in the first mentioned experiment, the spurs grew larger than in their normal situation. Still, I was not without some doubt whether I should be quite successful in retaining the phalanx in its entirety, being somewhat suspicious that the bone at least might exfoliate. No untoward result of this kind, however, has occurred; and I believe that the case justifies me in introducing the operation as one which may usefully be had recourse to in certain cases such as I have described. With greater precautions, too, I am led to think that the finger itself, without further covering, might be fashioned into a very fair nose; and, among the principal of these precautions, I would mention—1, leaving a longer portion than one phalanx; 2, adopting means to narrow the gap as much as possible, so as to prevent its receding; and 3, taking care that it does not become twisted from its straight line on the face.

REPLY TO DR. MACLEAN'S OBSERVATIONS IN HIS CRITICAL EXAMINATION OF DR. MUNRO'S VIEWS ON MALARIAL FEVERS, ETC.

By C. F. OLDHAM, Surgeon-Major, Indian Army.

IN the BRITISH MEDICAL JOURNAL of 19th December last, I notice a critical examination by Surgeon-General Maclean of Deputy Surgeon-General Munro's remarks upon malarious fevers, etc.

Dr. Munro, whose views agree with mine in the essential point that so-called "malarious" fevers are of purely climatic origin, is, I know, well able to "hold his own", without any help from me. Dr. Maclean, however, has, in the course of his paper, challenged me to explain the origin of the severe forms of "malarious" fever, which occurred in the force on the West Coast of Africa.

My friend Dr. Inman has entered the lists in my behalf, and has replied to all the objections to my views brought forward. But, lest silence should seem to arise from a knowledge of the weakness of my cause, I hasten to reply myself to Dr. Maclean's observations.

The nature and origin of climatic, or, as they are usually called, "malarious", fevers are fully and distinctly explained in my work on *Malaria*, published in 1871. And the intermittents and remittents of the Gold Coast differ in no respect from those of other tropical regions, save that, climatic influences being there intensified, the type of disease caused by them is more severe. If the views I have advocated be mistaken, let them be refuted. As yet, Dr. Maclean and those who think with him have pointed out no flaw in my facts or in my reasoning. They have contented themselves with raising objections, which I have already met, but my answers to which they utterly ignore.

In his observations upon Dr. Munro's paper, after remarking upon the formidable paroxysmal fever which prevailed amongst the troops in Africa, Dr. Maclean asks: "Will the certain electrical conditions of Dr. Munro, or the chill-theory of Dr. Oldham, account for it?"

To this, I reply that the chill-theory will account for it, to all who will study that theory, who will reflect that loss of heat, beyond very moderate limits, is inimical to both animal and vegetable life; and that continuous exposure to a very high temperature renders the body sensitive to, and liable to be injuriously affected by, a degree of chill which would otherwise be harmless.

Dr. Maclean quotes the saying of Dr. Parkes, "that when we speak of an unhealthy climate, we mean a malarious one".

The correctness of this observation, no one with any tropical experi-