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-

ence will dispute. What, then, are the characteristics of such a climate? Lind, an old but very good authority, thus vividly describes them: "The first proof of an unhealthy country is a sudden and great change in the air at sunset, from intolerable heat to chilling cold. This is perceived as soon as the sun is set, and for the most part is accompanied with a very heavy dew. It shows an unhealthy swampy soil, the nature of which is such, that no sooner the sunbeams are withdrawn than the vapours emitted from it render the air *raw*, *damp*, and *chilling* in the most sultry climates; so that, even under the equator, in some unhealthy places, the night air is cold to an European constitution."

Dr. Maclean will, no doubt, admit that this is an accurate description of the climate of the Gold Coast. Here, then, we have the predisposing cause of climatic fevers, viz., intense heat by day, followed by the damp chill atmosphere at night, exposure to which is generally the exciting cause of these diseases. An officer, who held a very important position in the late expedition, says: "What kills people on the Gold Coast is cold"; and adds: "We always find that some distinct act of imprudence producing chilliness has preceded fever in this country." This is not the testimony of a person whose views are biased in favour of the "chill-theory". On the contrary, this officer, who was himself attacked by fever on taking a cold bath after a walk, thinks that the nauseous penetrating smell, which proceeded from the decay of the marshy vegetation, and by which his clothes, etc., were pervaded, must have been associated with some malarial poison. So difficult is it to escape from the influence of impressions once formed.

If so-called "malarious" fevers occurred only in swamps, and only where the odour of decomposing vegetation existed, there would be good reason for connecting the foul smell with the fevers, as cause and effect. There is, however, no disputing the fact, that these diseases prevail not only where there is no smell, but also where vegetable decomposition is impossible. One condition only is always present, and that is chill!

I need scarcely point out, that no poison or other agent, which does not exist *whenever* and *wherever* these diseases prevail, can possibly be the exciting cause of "malarious" fevers; unless, indeed, the causes be many.

In my work on this subject, many instances are quoted, and I could mention many more, in which these fevers have occurred in the absence of vegetation, living or dead; where the soil is utterly sterile, and the ground undisturbed; where the drinking-water is faultless; and even on board ships far out at sea. Moreover, I have demonstrated, that the disorders referred to do not prevail in the presence of any of the conditions, hitherto supposed to produce them, unless these are associated with some influence capable of abstracting heat from the body. Still further, I have shown that the severity of these fevers is in proportion to the previous exposure to a continued high temperature; and that, owing to this, in cool climates they are mild, and in hot climates severe. I think, therefore, it cannot with justice be said that I have failed to explain the nature and origin of the fevers which prevailed in the force on the Gold Coast.

In conclusion, I fully agree with Dr. Maclean as to the very severe form which climatic fevers often assume, and as to the great mortality caused by them. The gravity of the subject, however, should serve to stimulate inquiry. How else is the truth to be established?

I fail to see that anything but advantage can ensue from the exposure of the untenable nature of a theory which utterly fails to account for the origin of these diseases; and which, by diverting attention from the real cause (as at Hong-Kong), tends to prevent the adoption of rational measures for their prevention.

ON A RARE FORM OF INFANTILE PARALYSIS (?).

By J. MEREDITH, M.D. Edin., Wellington, Somerset.

TOWARDS the end of the year 1873, when I was civil medical officer of the town and district of Pooree, Bengal, a case which I have designated as above, was brought to me at the public dispensary of the town as an out-patient. The history was briefly as follows.

A boy named Muria, aged about five years, son of a washerman residing in the town of Pooree, was stated to have been well and healthy until he was nearly four years of age. He could talk, play, sleep, use his hands, etc., like any other child of his age. At this period, he had attacks of the endemic fever of the country, enlargement of the spleen followed, then convulsions, and he was for some days unconscious; during these days, his upper extremities became paralysed, and had remained so. His lower limbs were not affected. When the fever and unconsciousness passed away, the boy was left a

mute, having lost all power of articulate speech; although able to cry and to make various noises.

The boy when brought to me appeared healthy and strong, with well formed limbs and in good condition. His head might be taken as being a little larger than usual in children of the same age belonging to his race, particularly in the transverse occipito-parietal diameter. On each temple he had a large scar where he had been cauterised, according to the native fashion, by a cow's tooth heated in the fire; these marks, however, surmounted a merry laughing face. The boy could put out his tongue straight, and without any tremor or apparent want of control. His sight and hearing were perfect, as was also the sense of touch in every part of his body. The curious thing about him was, that he could not endure the sound of coughing. Whenever any one coughed near him, he would generally fall down and become unconscious for some seconds of time; I say generally, because after undergoing an attack, he could endure the sound for a certain time without again being paralysed by it.

While I was looking at the boy, some one coughed near him, and at once he fell in a sitting posture on the floor as if shot, with his head bent forward, his eyes glazed and fixed, his mouth a little open, and saliva trickling out of it. Both arms were rigidly thrown out before him; the hands and fingers a little bent; all, however, quite rigid. There were no convulsive jerkings in any part. He was evidently completely unconscious, and remained so for some seconds; then he recovered himself, looked round at the bystanders, smiled, got up and began playing about the ward as he had done before. The boy was not, however, affected by the sound of his own cough, and he coughed strong and hard at times. Some one stated one day that other noises besides coughing would throw him into a fit, but could not say what kind of noise. To test this, I had him taken through the native bazaar where, I fancy, well nigh every variety of sound is heard. The piping shrieks of elephants, barking of dogs, beating of drums, shouts, and cries; but none of them affected the little patient further than amusing him for the time. In short, after trying him with every form of sound I could discover, I found that nothing affected him except a cough. A make-belief cough was hardly successful either, while a real catarrhal one seldom failed to paralyse him.

He came to me in the beginning of November 1873, and was given five grains of bromide of potassium thrice daily. On the 29th of the month his condition was as follows. "He cannot now be made to fall by a cough; he only stops suddenly if he be moving, and puts out his arms straight from the shoulders, the palms turned upwards. Yesterday and the day before, his friends say that he did not fall once when coughed near, either intentionally or accidentally; he only stopped with a start, and put out his arms as if to ward off danger. His face became grave and serious for a second or two; after that, all was right again."

He had never used his hands to feed himself since his illness, and various means were tried to induce him to do so, but to no purpose; he seized his food directly by his lips or teeth. He could put his hands up to his face as he rubbed his eyes when he cried, but he was incapable of deliberate co-ordinate action in this respect.

The next note taken of his case was on December 4th, when he appeared as lively and cheerful as ever. He had had no falls since last entry; was only staggered as above mentioned. While examining him on this day, the hospital assistant happened to cough just behind him. At once the little patient's attitude became fixed, his eyes assumed a glazed appearance, his face looked grave, his left arm was jerked out to full extension, with the palm turned upwards; the right flexed at the elbow with the fist resting near the axilla. He stood thus for some seconds; then recovered, looked round rather confused, not knowing whether to cry or laugh; but the latter inclination prevailed, and he was soon running about the ward in his usual manner.

December 28th. The boy on this date was said to be worse again. He had not been regular in his attendance, nor taken his medicine of late. He had had several falls, besides being staggered frequently. On December 31st, I had an opportunity of showing the case to Deputy Surgeon-General Buckle, C.B., of Calcutta, and some non-professional friends. Three attacks were induced in the usual way by coughing in his presence; and that within a very short space of time. The boy happened to sneeze at the time; and by this he was staggered, and reeled nearly to falling. I lost sight of the case early in January 1874, as he ceased to attend at the hospital.

The boy's father was a blind old man, and had a troublesome cough, due to chronic bronchitis, and as he and his boy were much together, the little one's peculiar sensitiveness was often put to sore test. Even at night during sleep he was thrown into attacks nearly as bad as when awake and running about in the daytime. His attacks during sleep were nearly always due to his poor old father's chronic cough.