sutures. The external meatus and tympanic membrane were found on examination to be quite normal, and the hearing was ascertained to be unaffected.

Mr. D. R. Cameron, L.D.S., has been successful in making a very satisfactory artificial ear, which the boy has been using for the last two years. A plaster cast was first made of the left, or normal, ear, and also of the remains of the right auricle. An artificial right ear was then modelled in wax, conforming to the left as far as possible, and finally made of Yela rubber vulcanised. The artificial auricle is kept in position mainly by the insertion of the process (photograph, Fig. 2), into the external meatus of which it is a cast. For the admission of air it is perforated right through. The application of seccotine has been found quite sufficient for the necessary adhesion of the internal surface of the artificial auricle to the corresponding parts remaining. Fig. 3 shows the artificial auricle *in situ* from the side.

The boy's father has devoted much attention to the pigmentation of the auricle, and has succeeded very well in copying the colouring of the normal ear.

THEORIES OF INHERITANCE.

By CHARLES MERCIER, M.B., F.R.C.S.,

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It is questionable whether the ignorance of medical practitioners upon this subject is so great as Dr. Archdall Reid supposes. Certainly if they are unacquainted with his views, they have wilfully neglected the very ample opportunities with which he has furnished them.

Such names as Hunter, Virchow, Huxley, Devay, Hodgkins, Paget, and Bastian remind us that after all there may be medical men who are not wholly ignorant of the subject, and although they have written for the most part with hesitation and diffidence, it is not certain that cocksureness is the sole criterion of mastery of a subject. There are, in fact, statements in Dr. Reid's own paper which, if he were not so sure of them, I should think were open to doubt. Dr. Reid repeats in his paper, as he has repeated many times before, that acquired characters are not transmissible, and as to their transmissibility says, "It is unbelievable that this can be true." That such transmission is unbelievable to Dr. Reid we have copious evidence to show; but the statement cannot be accepted as universally true unless Dr. Reid can show that believers are as non-existent as, according to his account, are instances of transmission of acquired character. But this is certainly not the case. Plenty of people have believed in the transmission of acquired characters, and plenty of people believe in it still. And, moreover, a thing is not necessarily untrue because some people, or even most people, find it unbelievable. Were it so it would be untrue that the earth goes round the sun, that there are antipodes, and that a railway journey of thirty miles an hour could be performed without the death of the passengers. "What is the machinery," says Dr. Reid, "by which this magical process is carried out? We know of none. On the face of it, therefore, the transmission of acquirements appears wildly incredible." By a parity of reasoning the fall of a stone is wildly incredible, for we are equally ignorant of the machinery by which this magical fall is effected. We know of none.

"The human body," says Dr. Reid, "is compounded of billions of cells, but a future individual springs from only one of them, a germ. The other cells—the somatic cells—afford shelter and nutrition to the germs, but there is not a tittle of evidence that they do more. They are specialised for their several functions, just as the germ is specialised for reproduction." This is indeed a new doctrine. It has usually been considered that the power of the germ to develop into a new individual lies in the total absence of specialisation. It is because it is general in the widest sense, because its direct descendants are capable of specialisation in every normal direction, that it is a germ and can develop into an individual in the likeness of its parent. If a cell is specialised, some have hitherto believed, it can produce only specialised offspring like itself.

"Were acquired characters transmissible," says Dr. Reid,

"the child of an old man ought to be clearly distinguishable from the child of a man 40 or 50 years younger. In fact, the children of the aged should be born aged. But no medical man is able to distinguish the child of an aged couple from the offspring of a boy or girl who have (*sic*) barely reached puberty." In this declaration there seem to me to be several fallacies. If acquired characters are transmissible, a child begotten in the old age of the father, which is, I think, what Dr. Reid means by the child of an old man, ought to be different in some respects from the child begotten in the boyhood of the father. This I think we should all grant; but it does not at all follow that the difference should be clearly distinguishable. As a matter of fact, there are differences between offspring begotten in the old age and offspring begotten in the extreme youth of the parents, and although these differences are not always so pronounced as to be "clearly distinguishable," yet in extreme cases the difference is clear enough. Children begotten in the extreme youth of the parents are commonly physically puny and shortlived; children begotten in the extreme old age of the parents are commonly deficient in intelligence. This is a considerable difference, but it is not a clearly distinguishable difference.

What Dr. Reid means by the child of the aged being born aged I do not clearly understand. Does he mean that the aged child should, when it is born, have already lived as many years as its father? Or does he mean that it should be born with grey hair, arcus senilis, presbyopia, and other signs of age? If the former is his meaning, I think that he misapprehends the problem that he is dealing with. If the latter, he misapprehends the position of his opponents. I am not concerned to champion the inheritance of acquired characters, a task for which my knowledge of the subject is not adequate, but when dogmatic assertions are somewhat importunately thrust upon us, we may be allowed to examine them, and the grounds upon which they rest. Is Dr. Reid not acquainted with the doctrine of inheritance at corresponding ages, and with the acceleration and retardation of the appearance of a quality in successive generations, and is he prepared to say that the tissue changes characteristic of old age do not appear earlier in offspring begotten late in the life of the father than in offspring begotten early in the life of the father? The difference, if any, produced in a single generation would probably be so small as to be inappreciable, especially having regard to the normal variation in the time of life at which these changes appear, and to their gradual onset, which makes exact estimate of their time of occurrence impracticable; but all these considerations go to show what an extremely complex problem we have here to deal with, and how very unsafe it is to dogmatise upon it.

The doctrine of the non-transmission of acquired characters is, in my opinion, wrongly entitled. What the upholders of this doctrine really advocate is not the non-transmission of acquired characters, but the non-acquirement of transmitted characters, which is as different from the former doctrine as a horse-chestnut is from a chestnut horse. Even Dr. Reid admits the influence of use and disuse in increasing and diminishing, respectively, the size and development of a part. These effects, in a single individual, he calls acquirements, and says that they are not transmissible. But if we point to the increase in size of, for instance, the great toe, and the diminution in size of the little toe, as examples of the accumulation by inheritance of the effects of use and disuse, then we are pitied for our ignorance, and told that individuals whose organisations varied spontaneously in the direction of a larger hallux and a smaller fifth toe have been naturally selected, while those who varied in the opposite direction, or did not advance as their predecessors, have been eliminated. By the adoption of this mode of reasoning it is easy to "break down" the "hundreds, if not thousands, of instances" in which the transmission of acquired characters has been alleged.

Dr. Reid's attitude of tolerant but contemptuous superiority towards Darwin's doctrine of pangenesis is a very familiar one. I have passed through it myself, and have witnessed it in scores of others; but I am old enough to have witnessed also the effect that ten, fifteen, or twenty years of experience and meditation have upon this attitude; and among my own contemporaries I know few who do not now, as I do, blush at the remembrance of the crude condemnation that they for-

merly passed upon the doctrine of the greatest biologist that the world has ever seen.

With Dr. Reid's dictum that all theories of heredity are in essence theories of evolution, I should not be disposed to quarrel; but if we are to accept it *in toto*, then the transmission of acquired peculiarities, that is to say, the modification of the germ by the influence of the soma stands upon the same footing as the modification of the individual by outside influences, such as climate. Now we know that individuals are directly modified by changes of environment. Dogs in Guinea become fox-like, and cease to bark; differences of food and climate seriously affect the fleece of sheep, as every woolgrower knows; seed wheat grown in one country and sown in another often changes its character, and many similar occurrences are known. So that upon this ground there is nothing improbable in the modification of the germ by the soma.

As I have said before, I do not pretend to speak as an expert upon this subject, but it does not need an expert knowledge of facts to detect fallacies in an argument, and I submit the non-transmissibility of acquired character is by no means such a dead certainty as Dr. Reid would have us believe. If it is to be accepted it must be in evidence different from any that has hitherto been submitted.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, Etc.

RETROGRESSION IN MAMMARY CARCINOMA.

THE communication from Dr. McNicol in the *BRITISH MEDICAL JOURNAL* of November 9th, 1901, on the subject of carcinoma of the mamma and oöphorectomy prompts me to record a case at present under my care.

This patient, now aged 44, was operated upon for scirrhus of the right breast in August, 1898. In October, 1900, she consulted me. I then found recurrence in the shape of a row of angry nodules below the inner end of the right clavicle; the patient had also an uncontrollable paroxysmal cough, apparently dependent upon involvement of the mediastinal glands; cachexia was marked. The question of oöphorectomy was entertained, but the general state of health was considered to negative it. I considered the patient's expectation of life at that time to be about three months, but it has already extended to more than a year.

The early nodules have increased in size and at one time seemed on the point of ulcerating; numerous nodules of varying size have appeared under the skin of the abdomen and chest; there are enlarged glands in the axilla and neck; there is evident mediastinal involvement, shown by dilatation of the pupil and aphonia; the cough is severe, and there is now "currant-jelly" expectoration.

During the last few weeks, while the asthenia has not abated there has been a marked change in other manifestations of the disease. The first nodules, which had commenced to excoriate, have completely disappeared, others have disappeared or have diminished much in size; the enlargement of glands is less marked; the aphonia, which was almost complete, has ameliorated.

Evidently in this and similar cases some "influence" is at work tending to hold the disease in abeyance. If we could only understand the nature and genesis of this cancer might be robbed of its horrors. Dr. Beatson considers the "influence" associated with the function of the ovaries. In my case menstruation, previously regular, has ceased since May; but it would be hazardous to say that the abatement of the disease is to be associated with this fact.

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JAMES PEARSE, M.D.

CARCINOMA AND MALARIA.

In connection with the paragraph on Carcinoma and Malaria in the *BRITISH MEDICAL JOURNAL* of November 2nd, 1901 (p. 1367), the following observations made by me during a sojourn of eight years in Dutch Guiana (Surinam), where I was attached to the Military and Colonial Hospital, which contained about 500 beds, may be of interest:

Many of the patients were inoculated with malaria, and malaria in all its varieties was the disease that presented itself principally for treatment. But various malignant forms of disease were not at all uncommon.

I remember excising the whole tongue of an escaped Cabyle convict for carcinoma. Another case that comes vividly before my mind is that of a Jew, about 50 years of age, born and bred in the colony, who suffered from a very rapidly-growing carcinoma of the glands of one side of the neck. These were excised, but the growth rapidly reappeared, and the patient succumbed soon afterwards, the new growth having attained the size of a large orange and bleeding profusely.

A case of melanotic sarcoma of the external auditory meatus in a coloured man from Nickerie (close to the confines of Dutch and British Guiana) came to hospital for treatment, but was deemed inoperable; the patient died soon afterwards.

A case of carcinoma of the tongue occurred in a Chinaman, with fatal result. The patient had been living in the colony for many years, and was a storekeeper's assistant.

I operated on several cases of carcinoma of the breast in Creole women who had lived all their lives in the colony. Another case that I well remember was a case of malignant disease of the bladder in a coloured man who had been born in Paramaribo, the principal town of the colony. He was a prison warder about 40 years of age.

I operated on several cases of epithelioma of the lip and penis in coloured people who had been resident all their lives in Surinam, and had been inoculated over and over again with malaria. It was also reported to me from trustworthy sources that a former principal medical officer of the hospital, who had spent the greater part of his life in Surinam, went to Europe to be operated upon for carcinoma ventriculi. He died in Europe.

I think that, taking the cases cited above, of which I remember the details very distinctly, there can be very little antagonism between malaria and carcinoma.

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VESICAL CALCULUS (259 GRAINS) IN A BOY AGED TWO YEARS: SUPRAPUBIC OPERATION: RECOVERY.

DR. MANSEL SYMPSON'S case¹ made me think mine might be of interest.

In March, 1900, N. D., an anæmic boy, 2 years old, was brought to me for "fits." He had had them since he was 10 months old, several in a day, short and accompanied by screaming; his bowels caused pain when acting, often bled, and sometimes "came down." Treatment during that time led to no improvement, though he had been "operated on" (circumcision). This is briefly the mother's story.

He had a "fit" in my presence. It was a paroxysm of intense pain, followed by complete relief. On examining the child the size of the penis struck me. It was large enough for a child of five. The abdomen was very tender. In reply to questions, the mother said that there was no pain on micturition, no blood in the water at any time, no interruption of stream, or any noticeable frequency of micturition. He resented jolting or rocking, walked but little, and cautiously; usually he went on all-fours, slept on his face and hands always, and from the time when he could assume the position always went on his knees and elbows when having a motion, or to pass water.

It was the character of the pain and the enlargement of the penis that determined my questions. Having no sound of proper size to hand I put the child under chloroform. Between a finger in the rectum and a hand above the pubes, a hard substance of some size could be felt. It was quite close to the rectal finger, but in trying to ascertain its size it slipped away as if released from an attachment. There was blood in the next water passed. I gave my diagnosis, and placed the case in the hands of Mr. Littlewood, Leeds Infirmary, who on March 9th removed, by the suprapubic method, a smooth oval uric acid stone, 259 grains in weight. He made a grand recovery, putting on flesh rapidly, and sleeping well. He has so far had no reminder of his illness.

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PATRICK J. SCANNELL, M.B.

¹ *BRITISH MEDICAL JOURNAL*, October 19th, 1901, p. 1160.