ening appear to me to negative this hypothesis, and to force upon us not only the idea of pathological change, but also that of inflammatory connective-tissue multiplication. Take, for instance, the arteriole, a portion of whose wall I have figured in Plate 5 of my monograph. Here the thickened tunica intima, far from being exclusively fibrous, contains a large number of rounded or ovoid cellular elements, which can hardly be regarded as anything else than the products of a pathological multiplication of the connective-tissue cells of the middle layer of the inner coat. The elastic elements of the outer layer of this coat are visible as a number of sharp wavy lines separated from one another by the elements originating in the proliferative process. When viewed in transverse section under a power of about 600 diameters, these lines are seen to run through the thickened intima, presenting an appearance somewhat comparable to that of river-markings on a map. It is difficult, indeed, to resist the conviction that the wavy lines represent the split-up and displaced lamellæ of the elastic or outer layer of the inner coat, while the bulk of the thickening is constituted by an increase in the number of the elements forming the connective tissue or middle layer. The structural peculiarities of this variety of thickening are almost identical with those visible in many syphilitic arteries. The patient whose kidney contained the arteriole figured in Plate 5 was, however, known not to have suffered from syphilis.

I purposely omit all mention of the appearances observed in the vessels of the remarkable kidney, an arteriole from which is represented in Plate 6 of my monograph. In this case the arteries were waxy, and there was a suspicion of syphilitic history which might tend to invalidate any conclusions based exclusively upon a consideration of the changes in the vessels of this specimen. Accordingly, lawyer-like, I prefer not to cross-examine a doubtful witness. I have, however, in my possession, a number of specimens in which the arteries are in a very similar condition. In these there is no warrant whatever for the supposition of a causatory syphilis.

It now remains to explain the reasons for my hesitancy in accepting Dr. Johnson's etiological explanation of the arterial and cardiac hypertrophy of interstitial nephritis as being due to an antagonistic action on the part of the heart and arteries, the one insisting on supplying the poisoned blood to the various tissues, and the other, in obedience to vaso-motor mandates, doing their utmost to exclude it.

My first objection is an *à priori* one, and shares of course to some extent in the weakness of all such arguments. It is this. The tendency of the various factors in all living organisms inclines so obviously towards mutual accommodation, that one is conscious of a *prima facie* reluctance to admit the existence of long continued antagonism on any grounds short of absolute demonstration. But Dr. Johnson replies, In what other manner can we account for the high arterial tension and increased cardiac action in certain cases of acute renal disease with uræmia? In the first place, I might rejoin that in these cases the time necessary for the establishment of a give-and-take toleration is not allowed. A transitory antagonism is more conceivable than a persistent refusal to come to terms. But the real objection to this ingenious argument from analogy appears to me to be the following. Increased cardiac action with high-tension pulse are by no means invariably observable in cases of uræmic toxæmia. In all such cases the blood is presumably poisoned; why then are the consequences as regards the heart and vessels not invariable also, or at least fairly constant? Yet the name is legion of those cases of uræmia where the heart's action is insufficient, and the pulse slow, compressible, irregular, and intermittent—and this not merely in the later stages, but even from the very outset. What then is the explanation of this non-uniformity? Is it not to be sought in the constantly threatening tendency to serous and other inflammations, which constitutes so ever imminent a contingency in these cases? Congestion of the lungs, pneumonia, pleurisy, pericarditis, and peritonitis are each and all of them sufficient to account for a high-tension pulse and cardiac overaction. So too is that inflammation of the alimentary canal which occasions the severe diarrhœa so frequently seen as a harbinger or concomitant of uræmia. We cannot but recognise the influence which incipient inflammatory conditions of this kind must exercise on the circulation. According to my observations of uræmic cases, circulatory inefficiency is quite as frequent as excitement; and I have been accustomed to regard the latter, not as a consequence of antagonism between heart and arteries, but

as a warning to be on the alert for inflammatory complications.

I have been led to the opinion that the hypertrophy of the heart in interstitial nephritis is due to diminution in the calibre of the arterioles, because of the apparent relation subsisting between them in those cases which I have hitherto examined. I do not consider my series sufficiently extensive to warrant as yet the expression of a positive opinion; and I therefore merely wish to indicate, in the meantime, the direction in which the balance of evidence appears to incline. There can, however, be little doubt that a condition of thickening of the tunica intima is very common in cases of interstitial nephritis; that it reduces the calibre of the arterioles generally; that this diminution in the sectional area of the arterial conduit-pipes must tend to produce a rise in the blood pressure; and that this rise in the blood pressure must be compensated by cardiac hypertrophy, either as direct result or concomitant, if the efficiency of the circulation is to be maintained. These considerations, when viewed in relation to the fact that in eighteen cases a very constant relation was maintained between the diminution in the calibre of the arteries and the degree of cardiac hypertrophy, seem to point in the direction of a strong probability, which further observations must either negative or confirm.

I have already indicated my present inability to propound a feasible explanation of the causes which issue in the production of hypertrophy in the tunica media of the arterioles. I trust that a new series of observations, which I am preparing to undertake, may possibly throw some light on the subject. If anyone has explained the *raison d'être* of this unquestionable feature in the morbid process, it is Dr. Johnson; but in the absence of further evidence, I cannot but regard his ingenious theory as provisional and tentative. A few carefully conducted experiments on animals by the administration, say of ergot, for a prolonged period, might do much towards settling the question as to whether continuous contraction of the muscular coat of the arterioles might or might not, in the absence of obliterative changes in the intima, prove sufficient to give rise to hypertrophy of the muscular fibres of the heart, and those of the arteries themselves. At present, however, when the head of every tomcat in the British Islands is held as sacred as that of the bull Apis in ancient Egypt, it is to be feared that any and every theory pertaining to this subject must remain unsettled in the absence of experimental corroboration.

Edinburgh.

ABSCESSSES CONNECTED WITH BARE BONE, TREATED WITH AND WITHOUT LISTER'S METHOD.

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THE object of this communication is to ask a question and to state a case. In a recent number of THE LANCET there was an account by two able and well-known men of a phthisical young man, aged twenty-six, who had an abscess situated over the fifth rib, which was opened "antiseptically" on December 7th, 1879. A few drachms of thick curdy pus were evacuated, and the surface of the rib was found to be bare and rough for nearly two inches. On February 23rd (i.e., eleven weeks after the abscess was opened) the drainage-tube was extruded and the wound healed. The commentary is: "This affords a beautiful illustration of the power of nature to repair, if only protection be afforded, against fresh irritation. The diseased bone was kept free from all irritation, escape being provided for the discharges preceding and accompanying repair, and perfect asepticity being secured by the use of Lister's method. Had the abscess been opened without antiseptic precautions, prolonged fetid suppuration and exfoliation must have resulted."

The questions I would ask are: "Can the infatuation of disciples—*addicti jurare in verbis magistri*—go beyond this?" "Does not any system which has good in it flourish rather than suffer by such tributes to its glory as the above?" "Is it in accordance with the principles which should render to carbolic acid the praises which belong to

it, and to common sense and the ordinary process of repair the tributes which belong to them?" The following case may assist to an answer:—

On Nov. 11th, 1880, Ellen W—, aged twenty-six, a weakly, very anæmic and pale-looking blonde, who all her life has had bad health, was admitted into Regent ward of the Middlesex Hospital with an ovoid swelling the size of a large walnut in the roof of the mouth, projecting downwards towards the tongue. It was situated chiefly on the right of the median line, and fluctuated and discharged daily a small quantity of rather offensive matter into the nose as well as into the mouth. Her jaws were filled with the decayed, ragged, and offensive stumps of molar and bicuspid teeth. The swelling in the mouth appeared three weeks before admission, and was attributed to the irritation caused by the first right upper bicuspid tooth, which had been for a long time carious, and had decayed away, leaving only a stump. This stump was extracted nine days before admission. A fortnight before coming in the swelling on the palate was lanced by the patient's regular medical attendant, and a quantity of blood and matter came away. It filled again in three or four days, and subsequently broke of itself, and discharged in two places—one where lanced, and one just behind the front teeth. A week before admission the discharge into the nose began. The mouth was slightly examined on the day of admission, but owing to her weak state of health and her fainting under examination, nothing was done until the next day (Nov. 12th), when a free antero-posterior incision was made into the abscess, and two or three drachms of mixed blood and pus evacuated. With a probe the whole palate process of the right maxilla was felt bare and rough, and the probe passed through a carious opening from the mouth into the right nostril. A wash for the mouth, consisting of Condy's fluid, was ordered, and the wound was kept open by the daily introduction of a probe. On Nov. 25th there was still some fulness in the seat of the abscess, but the mucous membrane had settled down upon the hard palate again. The mouth was still in a bad state owing to the stumps of teeth.

Dec. 3rd.—Palate looks quite sound, but patient thinks there is a trace of discharge at night into the mouth. She describes an uncomfortable "hollow sort of feeling" in the nose when she goes into the cold air. Is now well enough to bear the extraction of the stumps, all of which are to come out. The right second incisor, which was decayed, and two stumps were removed this morning.—Dec. 11th: There is now nothing wrong to be detected about the palate. Five more stumps have been removed since the 3rd of Dec. She is very much improved in health, and now only remains in hospital to have the other stumps removed.—Dec. 21st: Discharged well.

Remarks.—Cases are numerous enough in which an abscess connected with rough bare bone has been opened without what is now understood by the antiseptic precautions and dressings, and where no exfoliation has followed. But the above case is one in which such an abscess was opened not only without the perfect asepticity secured by the use of Lister's method, but in the presence of unavoidable and most septic conditions, the pent-up matter of the abscess having a communication with the nose, as well as with the mouth and its offensive tooth stumps. Yet neither exfoliation nor prolonged suppuration resulted, and the abscess, thanks to the ordinary processes of repair, was quite well in less than four weeks.

This case is open, no doubt, to the argument that the presence of the decayed and foul stumps was an important and most favourable element in the recovery, serving as potent centres to attract the germs of putrefaction, and thus draw them away from the abscess cavity and the diseased palate bone, like wasps which hover about the cracked and over-ripened fruit of the greengage tree to the neglect of the heliotrope and roses. Further, this deduction—it would not be a logical one—may be made from the case—namely, that it would be well in every instance to have a powerful centre of putrefaction at no great distance from a wound in order to serve as a counter-attraction for the said germs. I am not, however, disposed to agree with so cynical a disputant; any more than I can believe, even when asked to do so by distinguished men, especially with the evidence of such cases as the above before me, that Lister's method is requisite for the cure of these abscesses.

It has for some time past been quite the general practice in describing any successful operation, be it never so simple, or one ever so frequently performed, to state that it was done

under Lister's method, and to imply that the success was chiefly due to the employment of this method. If this habit continues the students of medicine in the immediate future will run the risk of growing up to think that before the use of the "antiseptic system" there could have been no successful surgery at all; and when they get into country practice they will have to make provision for always carrying on horseback, or in their gig, a steam spray and rolls of gauze and protective, so as to avoid the qualms of conscience which must follow if in a weak moment they open an abscess without these precautions, and make the horrible discovery that there is rough bare bone. Every week contributes its quota to this end, and we are almost persuaded to think that without carbolic acid there can be no good from herniotomy, no safe colotomy, no satisfactory excision of a tumour or a joint. Such are the expressions which are daily heard that one cannot help feeling, with one of my old teachers at Guy's, a past President of the College of Surgeons, who said, "If all we now hear is true, I wonder we old fellows ever had a successful operation." It is to be hoped, for the sake of the just appreciation of Lister's system, that many of these "old fellows" will long remain amongst us, to hold the balance between the true and false, and to recall to our minds some of the successful results of pre-Listerian practice.

Mansfield-street, W.

ON THE HERPES IRIS OF BATEMAN.

BY T. COLCOTT FOX, B.A. CANTAB., M.B. LOND.,

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OUR largely-growing knowledge of diseases of the skin, which has resulted from the general advancement of medicine and surgery and the special increase of work in a long and much-neglected section, has borne fruit in the fulfilled desire for a more scientific classification and a simplification of the grouping of these diseases. In the latter process, however, there may arise a danger of being hurried on too fast, and it is well to call a halt occasionally, and make sure of our ground, that we may not introduce confusion by having subsequently to retrace our steps. It is my intention by the following remarks to ask careful consideration of the subject before we yield to the growing tendency amongst dermatologists to swamp, as it were, in the group erythema multiforme the rare but very characteristic disease herpes iris, so well described by Bateman. Unquestionably a very real advance was made by Hebra when he grouped together under the term erythema multiforme the various varieties of inflammation of the skin, hitherto described as erythema annularis, iris, papulatum, tuberculosum, circinatum, and so on, because, although each of these varieties has distinctive features both as regards site, appearance, &c., they are frequently associated together, and are undoubtedly mere varieties due to the amount and site of the fluid effusion, and to the mode of extension of the inflammatory process. Now, it is an all-important point to recognise that the effusion of fluid in sufficient quantity or superficially enough to form vesicles or bullæ is a perfectly natural stage of the erythematous inflammation, although an exceptional termination, and by no means to be regarded as a characteristic feature. Still the term erythema ("inflammatory blush") must for the future be given a wider significance than heretofore, and must denote a type of inflammation which may exceptionally, but naturally, terminate in vesiculation. When the process is very acute or the effusion of fluid superficial enough we see such vesiculation exceptionally in many varieties of erythematous inflammation, as—e.g., in urticaria (*U. bullosa*, which has been described as pemphigus), in the eruptions produced by the ingestion of some balsams, in the erythematous rashes characterising many of the acute specific fevers, in erysipelas, &c. And, further, it is necessary to note that from the peculiar evolution and spread of the inflammatory process the vesicles or bullæ may be arranged singly or grouped in some variation of the annular pattern.¹

In herpes and pemphigus, on the other hand, the effusion

¹ Those interested specially are referred to a posthumous paper by the late Dr. Tilbury Fox, which I edited for the January number of the Archives of Dermatology, 1880, entitled A Clinical Study of Hydroa.