great an extent as if they had breathed air. Note the importance of this observation. It proved that the blood gave off, for a short time at all events, carbonic acid, even in the absence of oxygen. Further, Spallanzani showed that many animals absorb oxygen and give up carbonic acid by the surface of their bodies without the action of either lungs or gills. He also demonstrated that even tissues breathe, a bit of muscle exhaling carbonic acid and consuming oxygen. He proved, by quantitative experiments conducted with much skill and accuracy, that the amount of oxygen absorbed and of carbonic acid exhaled depends on the degree of activity of the body as a whole, or of the particular tissue. By cooling the animals, and thus retarding the vital processes, he could diminish the amount of carbonic acid formed. He was also the first to show that respiratory combustion may be influenced by the nature and quantity of the food. All these researches demonstrated that the union of oxygen with carbon does not occur in the lungs, or even in the blood (except to a small extent), but in the tissues; and they paved the way for the modern generalisation that each element of tissue has, in a sense, a life of its own, selecting from the nutrient and respiratory blood oxygen and suitable pabulum, and giving up carbonic acid and other waste products. The conception of a combustion or burning is very crude. No doubt it is in harmony with the known facts relating to the absorption of oxygen and the production of carbonic acid; but a more careful study of the phenomena occurring in living matter leads irresistibly to the conclusion that respiration of tissue is not so simple as a combustion, but is more akin to the phenomena of fermentation. Here again we have penetrated far beyond the standpoint of the Professor of Pavia.

In reading over the record of Spallanzani's work one cannot but be struck by the absence of two great ideas, which, in our day, dominate and direct scientific thought. The first is that of the permanence of matter and the second is that of the permanence of force. Men had not in Spallanzani's day reached the far-reaching conception that all the phenomena of Nature, as seen on our earth, and including among those all the phenomena of life, depend upon interchanges of the elements of which the earth is formed. Nor had the corresponding thought arisen that, while the various forces may apparently disappear, they reappear under new forms; in other words, that the sum of energy is always the same, and no force, no more than an atom of matter, is destroyed. These ideas, gentlemen, you will find running, like golden threads, through all your studies, or, to vary the metaphor, they are the keys by which you will unlock the storehouses of knowledge. They were almost unknown to the Italian philosopher. They may have arisen like dim lights on the horizon of his thought, but many years had to pass before they became the beacons of science, radiating into all the dark regions of the universe. In no department have they been more potent than in physiology, and the outcome of it all is that the votaries of this science now hold the opinion expressed in the striking language of Donders that no life exists without change of matter, and no life manifests itself without change of force.

Such were the chief researches of Spallanzani. A sketch of his career has perhaps given you some insight into the nature of the studies in which you will be engaged in your medical curriculum, and at the same time it may have shown you how much may be achieved in one lifetime of earnest and hearty labour.

You are already, or you are about to become, students of Nature, and you will be called upon to take your share in the work of investigation. In a true sense, every student is an investigator. Your knowledge is not to be derived only from books. You will be brought face to face with natural phenomena, and you will be asked to interrogate these for yourselves. Your teachers will all strive so to present knowledge as to call forth your powers of making it your own. You should note each fact brought before you, and each experiment demonstrated in your presence, as if you were personally observing the fact and making the experiment. Like Spallanzani, be accurate, methodical, and truthful in all your work. Science is the truth about things. A scientific training teaches us to discriminate between what is real and what is apparent. It brings us face to face with things as they really are. Your education will not consist merely in

acquiring a knowledge of certain facts. It is more correct to say that it will be a discipline. I know no education which has a wider or grander sweep than that of a student of medicine, nor one more fitted to call forth all the powers of a strong and healthy mind. Let me, in conclusion, wish you much success and happiness in your studies and a share in all those influences that build up a manly character—influences which can only be enjoyed, in their fullest measure, in an ancient and famous University like your own.

ERRORS IN DIAGNOSIS.¹

By WILLIAM CADGE, F.R.C.S.,

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I ASK permission to say that I stand here now as a conscript rather than as a volunteer. I have no unique or striking cases that I desire to communicate, and I am not conscious of possessing any new development or theory of disease struggling to find utterance. When, therefore, I consented to read a paper to-day, I thought that, next to endeavouring to advance the general stock of knowledge, the best course to adopt might be to try to add to the security of that which we have by pointing out errors into which I and others have fallen. Having yielded in this matter, chiefly to my friend Mr. James Worthington's pressure, and having mentioned the subject I should probably handle, he replied to me in this pleasant way: "By the way, do you remember a particular case in which you made a great mistake?" This made me pause, and I reflected that, if many of my friends here to-day should, as they probably can, round on me in this prompt fashion, I should have a bad quarter of an hour, and should repent of my rashness. However, I resolved to persist in my intention. I am willing to offer myself as an oblation to the advancement of science, and I even challenge Mr. Worthington, when I have done, to describe this case in which I was so mistaken, because it is clinically interesting, as it led to an error in treatment which he had the sharpness to detect and the skill to rectify.

Confession of error, however, need not imply blame. Mistakes in diagnosis may be due to various causes—to inherent obscurity of signs and symptoms of disease, to misleading statements by patients or malingerers, or to our imperfect physiological knowledge; but in addition to these inherent causes of error, mistakes too frequently arise from the carelessness or the ignorance of the surgeon. We may, perhaps, divide medical men into two classes, those who are over-cautious and those who are over-confident. The over-cautious are less often wrong than the others, but they labour under a great disadvantage, for a patient who sees that a doctor cannot make up his own mind is apt quickly to find somebody who can. There is an exception, however, to this rule: an eminent London surgeon or physician can withhold an opinion and plead for suspense of judgment without forfeiting the confidence of the patient, while the general practitioner would at once lie under the imputation of ignorance. I was calling one day last year on a prominent surgeon in Liverpool; he had a patient at the time in his consulting room with a doubtful tumour of the breast, and he invited me to examine it. I asked my friend to give his opinion, and he replied: "I think I have reached a position here which entitles me to say I don't know what it is."

The over-confident men, men who are cocksure, who in their own opinion never make mistakes and who pronounce their decision boldly, are, in my experience, very often wrong, and when they are wrong they are palpably and hopelessly enveloped and beyond friendly extrication. The cautious man who is in error has probably left himself a loophole of escape through which he can in some measure save himself, but the cocksure man stands convicted by his own bold declaration, and his only alternative is to declare that he has been misunderstood and that after all he really saw the true bearing of the case.

In my student days I was clinical clerk to a distinguished physician of the cocksure type. A man had died in hospital of peritonitis, as proved at the *post-mortem* examination.

¹ Read at a meeting of the East Anglian Branch of the British Medical Association.

The physician examined the general condition and took his leave; he constructed an ingenious theory to explain the cause of the inflammation, but he did not know, what I discovered after he had left, that there was a malignant stricture of the rectum. This was carefully removed, and the following day at the clinical lecture, after the professor had got well beyond the etiology of the disease, the specimen was presented on a plate, with many apologies for being late; there was a momentary halt and confusion, but our over-confident teacher quickly recovered himself, and asserted boldly that he had had a strong feeling that disease of the rectum might be present, and he thought that he had mentioned it.

This same physician made another bad shot; the case is historical, and is worth mentioning. Mr. Liston was suddenly attacked with hemoptysis to an alarming extent, the source of which could not be explained by Sir Thomas Watson and other men of note who examined him; they could find no physical signs indicative of aneurysm until signs of pressure on the windpipe and on the recurrent laryngeal nerve made the diagnosis of tumour or aneurysm evident, and this was only a short time before his death. The over-confident physician I have mentioned, however, at an earlier period of the case declared that there was evidence of tuberculous consolidation at the left apex; he attributed the bleeding to this cause and prescribed accordingly; but when I made the *post-mortem* examination and found no disease whatever of the lung, but an aneurysm of the aorta situated precisely behind the centre of the sternum, the physician declared that he had suspected the existence of aneurysm or tumour, which I, who was present at the consultation, can declare he never mentioned; and I have documentary evidence to prove that, in his opinion, Liston's case was one of pulmonary tubercle.

I am tempted to allude briefly to one more mistake of this self-confident physician. When I was acting as demonstrator of anatomy at University College I was asked to make a *post-mortem* examination of a gentleman who had died very suddenly; he and Dr. Roots were hurriedly summoned and arrived at the house together, but not until after the death of the patient. They heard the history of the seizure, and gave the opinion that apoplexy was the cause of death.

Just at that time Mr. Paget was giving a course of lectures at the College of Surgeons on Pathology, which I was attending. Amongst other subjects, he dealt with fatty degeneration of muscular tissue, and I had in my mind his clear and vivid account of this change. In conducting the *post-mortem* examination I was somewhat taken aback to find that the brain was entirely healthy and that there was no indication of the apoplexy of which it was supposed the patient had died. Pursuing the inquiry, I found the heart large, soft, and greasy, and under the microscope exhibiting in an extreme degree that fatty degeneration which Mr. Paget was then describing. My eyes were opened wide at this discovery; the subject, so well understood now, was new at that time—more than forty years ago. I thought the matter well over and saw its important clinical bearings. Other similar cases soon occurred showing the same pathological appearances, and I felt sure that I was on the path towards valuable results. I soon found, however, that my old friend, Dr. Quain, was working on the same lines, and I retired from the inquiry, giving over to him the cases and notes which I possessed. I only allude to this matter now because it may interest our honoured visitor, Sir J. Paget, to know how quickly his excellent teaching began to bear fruit.

There is, perhaps, another class of medical men who are apt to fall into errors, not from carelessness or ignorance nor from too much caution or over-confidence. Rather they are men of argumentative ability, of subtle thought, and of much learning, prone to search for hidden and remote causes when plain ones lay straight before them, and they are fond of constructing cunning theories when common sense and experience will easily explain all. They lack that faculty of estimating the true value of this or that symptom, that intuitive insight which may be designated as sound judgment. Oliver Wendell Holmes has illustrated this class in a very humorous way when he makes the hero of the breakfast table consult Dr. Benjamin Franklin about a discoloration on his forehead the result of a slight bruise; the learned young doctor examines it in every possible way, goes elabo-

ately into the family history, hints that it may be a case of morbus Addisoni, and explains fully the nature of that disease. The patient is alarmed; but his landlady eases his mind, applies a vinegar cloth, and the disease quickly subsides. The patient moralises on the matter thus: "Science is a first-rate piece of furniture for a man's upper chamber if he has common sense on the ground floor; but if a man hasn't got plenty of good common sense, the more science he has the worse for the patient."

I well recollect some years ago visiting a case for a medical friend, now deceased. The patient was rich, indolent, lived in a fine house, and fared sumptuously every day; he had a swelled and inflamed foot, and, putting these conditions together, my friend informed me that it was a case of gout. Probably the aspect of things had altered somewhat between his visit and mine, but it was obviously not gout at all, but ordinary phlegmonous cellulitis, which spread up the whole limb and ended in suppuration and sloughing of the cellular tissue, which nearly cost the patient his life.

Let me, however, advert briefly to one or two diseases which in my observation have been most frequently overlooked or mistaken: and I would give the first place to diabetes, a disease which manifests itself with so few objective symptoms and in such a variety of ways. Cataract, as we well know, is often the accompaniment of diabetes, and I have heard Sir W. Bowman say that he had in a vast number of cases recognised diabetes—before unsuspected—by the peculiar appearance of the opaque lens. The coexistence, too, of boils and carbuncles and of gangrene of a toe or finger—or even of suppuration of a joint of a toe or finger—with diabetes is common and familiar enough; but I have seen quite a number of cases of so-called prostatic disease in old men in which the frequent micturition depended not on the readily-suspected hypertrophy of the gland, but simply on the fact that, having a large quantity of urine to discharge, the bladder required frequent relief, and it is well to remember that this large quantity of urine and frequent micturition, though commonly due to ordinary diabetes, is occasionally the effect of diabetes insipidus. So, too, the coexistence of diabetes and lithic acid gravel and stone is not uncommon, and should be borne in mind, for the combination is formidable, and may prevent the cure of the otherwise curable stone.

The mention of stone leads to the remark that next to diabetes the presence of stone in the bladder is perhaps most frequently either overlooked or undetected. There is no surgeon living—no matter how great his experience and his manipulative skill may be—who does not occasionally fail to detect a stone that exists in the bladder. I plead guilty to this myself, for I have had it brought home to me, and have felt not a little chagrin and self-reproach at my shortcoming. I am not going to describe the various methods of sounding for stone, but I wish to allude to one source of mistake only, which I have met with over and over again. I can best illustrate it by a case. A London hospital surgeon who was spending his holiday on the coast near Cromer met with a sturdy old fisherman who had all the symptoms of stone. He induced the man to go to his London hospital, but he was surprised that, when there, no stone could be detected; he examined again and again, and called in one or more of his colleagues, and eventually sent the patient home with the opinion—afterwards expressed to me—that malignant disease of the prostate was probably the cause of his symptoms. Yet this man had a stone which was successfully removed by lithotomy in the Norwich Hospital, and the explanation of failure to find it I believe was this: the patient was a tall fat man, aged 72, with an enlarged prostate, and the bladder was very deeply placed and distant; an ordinary sound would go to its usual extent, and could be partially rotated, but I soon found that it required considerable further introduction and even to push back the flexible part of the urethra on the sound in order to reach the bladder; when it was reached, the stone could not be missed, but I have no doubt that my London friend had never passed the sound fully into the bladder, but had only examined the deep prostatic sulcus, and the limited movement of the sound he probably attributed to a contracted bladder.

I am warned by time that I must forego the mention of the mistakes and difficulties which so frequently occur in the diagnosis of tumours; whole chapters—even volumes—could

be usefully written on such a subject: so, too, on the detection of feigned diseases, but I will conclude with the mention of one of the latter kind which has found a place in the standard literature of surgery.

In the third volume of Brodie's miscellaneous works is a description of what he calls "dry or white gangrene of the skin." A portion of skin of the size of a shilling, or larger, dies, and turns white, or dries into an amber-coloured, horny slough, and is separated by ulceration: the wound heals, but a succession of these mortified patches follow one another, and seem interminable. He gives three cases, and showed in lecture a wax model of one of them; all the patients were young women with anemia and irregular menstruation; little is said of the treatment pursued except that it was not very effectual, and the patients left the hospital and were lost sight of.

How many of us have ever seen a case of white gangrene of the skin? One such came under my notice long ago, when I was house-surgeon at University College Hospital. It tallied exactly with Brodie's description: piece after piece of skin about the size of a shilling or half-a-crown turned white, necrosed, and was separated and the wound healed. The patient was a young woman, and she was the object of great attention by the surgeons and the students because of this rare and peculiar disease, and because, too, she was very good-looking. She stayed in the hospital many months, then she left, and was lost sight of for a time. At length the history of the case was completed; she became an in-patient of the Brighton Hospital; there suspicion arose, a watch was set on her, and the cause of the disease—which in London was explained by some ingenious hypothesis in more than one clinical lecture—was found to be a pennyworth of strong sulphuric acid and a glass rod! She was ignominiously expelled from the hospital, and I believe she committed suicide soon after.

I have always thought that Brodie's white gangrene of the skin had no better foundation as to its etiology than, and probably the same as, existed in this case. I may, however, be mistaken, and I give this opinion with diffidence; but if I am right—if so astute, so careful, and so experienced an observer as Sir B. Brodie was mistaken—we may all take comfort in the reflection that neither age, learning, nor experience can prevent occasional error. The older we grow the more wary do we become; we know the pitfalls into which we may fall and probably often have fallen; whereas the young practitioner, who has not yet been much confronted with his own blunders, with the enthusiasm of youth and the complete teaching of the schools, is apt to be more sure of his opinion, more free in expressing it, and therefore more frequently in error. This, however, I may say, quoting the witty words of the late Master of Trinity College, "We are none of us infallible, not even the youngest."

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, ETC.

SLOUGHING OF THE PREPUCE.

ISREE, a native mule driver, was admitted into hospital on August 7th. He stated he had contracted a sore on the penis about two months previously. He had no regular medicine, but had some sort of treatment from a native "hakim." About a month before admission the prepuce had sloughed.

On examination it was found that the prepuce had sloughed extensively, the glans protruding, and the remainder of the prepuce hanging down. From the root of the penis to the margin of the wound was about $1\frac{1}{2}$ inch, and from the opposite margin to the end of the prepuce a good inch. The wound was almost completely healed, and it was evident he had only applied for treatment as he recognised his inability for sexual intercourse.

On August 11th, chloroform having been administered, the whole of the lower portion of the prepuce was cut away, and the margins trimmed as well as could be done round the glans. Several silk sutures were passed through from the mucous surface to the skin. There was a little bleeding,

which was easily stopped by compression. The lower part of the wound was hard and indurated, apparently the remains of the chancre. The wound was dusted over with iodoform, and dressed with perchloride of mercury lotion (1 in 2,000) lint. It did very well, but healed rather slowly. After about ten days black wash was substituted for the perchloride lotion. The wound has now completely healed, and the induration gone. There have been no secondary symptoms.

REMARKS.—This case is, I think, interesting as illustrative of the extreme amount of inconvenience and suffering a native will endure rather than apply for treatment and expose himself for examination. This man evidently had a very acute phimosis, and must have suffered extreme pain. Notwithstanding this, he continued doing his work as best he could, and only came to hospital when he found himself debarred from sexual intercourse.

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POISONING BY THE BERRIES OF ATROPA BELLADONNA.

I WAS called out on September 27th to attend a boy who was said to be "fitty." On seeing him I found him highly delirious, and with a temperature a little above the normal, 99°. He had intense thirst, and appeared to have a choking feeling about the throat. The pupils were widely dilated and insensible to light. Skin very hot and dry. The face, at one time pale, would, after a short interval, become intensely flushed. The urine was scanty and passed in the bed. The bowels were unopened, even after free doses of castor-oil, which, however, almost immediately brought on vomiting. Delirium had come on at 1 A.M., and had continued almost without interruption up to the first visit I paid—12 P.M. There were jerking movements of the arms and legs, and frequent contractions of the muscles of the face. The arms occasionally moved, as if plucking something from a tree, after which the hands were carried to the mouth and the act of swallowing followed.

These symptoms combined led me to inquire as to whether he had eaten anything unusual, and a berry was at once produced, of which he was said to have eaten eight or nine. The berry corresponded exactly to the description of the fruit of the atropa belladonna plant—"globose, juicy, half to three-quarters of an inch in diameter, smooth, very dark purple, and containing numerous small seeds imbedded in the pulp." He had partaken of these the previous afternoon. In the evening of the following day he was still delirious and going through the same movements, but next day he was much better, though the pupils were still widely dilated. After a few days he was quite well, with the exception of slight headache.

The boy's father, who ate three of the berries, complained of intense dryness of the throat and inability to read on account of the letters "all running together." His pupils were dilated. Two other men who had taken one or two of the berries also complained of dryness of the throat and skin afterwards, but I had not an opportunity of seeing them.

Littlehampton.

FRED. HALL, L.R.C.S., L.R.C.P.

DEATH FROM CHLOROFORM.

THE case was that of a boy, aged 11, who had suffered from a decayed tooth in the left inferior maxilla. This tooth set up an alveolar abscess. His mother took him on two separate occasions to a dentist, who tried to remove the roots of the tooth without any anæsthetic, but without success. The mother then took the boy to another dentist (Mr. Brooke, of Nottingham), and he consented to extract the roots if chloroform were administered.

The boy was prepared by diet, etc., for 11 A.M. on October 8th, and I administered chloroform on an ordinary towel. The roots of the tooth in question were successfully extracted and the boy began to struggle, and a little more chloroform was administered to enable the dentist to extract two other decayed teeth. This was done and the boy still seemed all right.

The anæsthetic was given in an ordinary easy arm-chair, and whilst contemplating the removal of the patient to the bed he suddenly turned palid. The breathing continued, but the pulse was lost. The boy was at once lifted into bed and