back, they had slipped four times, the head passing backwards above the promontory during traction. This was Mrs. A.'s fourth confinement. The previous children—two girls and a boy—had been born alive, delivery in each case having been effected by the forceps. The traction had varied from twenty to thirty minutes, and the head of the third child had been considerably bruised and cut.

On examination I found the vagina filled with blood-clot, the os almost fully dilated, and the head presenting above the brim. The posterior fontanelle lay on the right side, and somewhat to the front of the middle line, the position of the anterior fontanelle being just above and posterior to the left side of the promontory. I have said the position of the anterior fontanelle, for there was really no fontanelle, the four sutures actually meeting in a point. The cranial bones were, however, freely movable at their junctions, and could evidently be made to overlap during compression of the head. The diagonal conjugate measured three inches and a half, the interspinous measurement was ten inches, whilst the distance between the iliac crests was ten inches and a quarter. This was, therefore, a case of slightly undersized as well as flat pelvis, with a true conjugate of scarcely two inches and three-quarters, the head lying extended, and with its long diameter in the left oblique diameter of the pelvis, as far as this was possible in such a pelvis. Careful and repeated examinations were made over the whole of the abdomen by Dr. Mehan and myself, but no fetal heart-sounds could be heard. Now, Simpson's forceps had been proved to be unsuitable for the case. Turning, owing to the length of the labour and the almost certain death of the child, few would have considered justifiable, whilst craniotomy was not warrantable if my antero-posterior compression forceps could deliver in a short time and with moderate traction. These instruments were therefore applied, the head having first been carefully placed with its long diameter in the transverse diameter of the pelvis. The introduction was easy, though locking was not so easy as I have always found it to be, apparently on account of some obstruction at the extremity of the anterior blade.

Traction was made by means of a towel passed through the ring and held by the left hand; the right hand, which grasped the handles, being reserved for compression and pendulum movement. After less than one minute of very moderate traction the head passed into the cavity with a thud; and in less than one minute more the head was born, perineal pressure being applied to complete its expulsion. Although some little difficulty, there was no unusual delay with the shoulders. The cord was pulseless, but as a matter of form artificial respiration was tried for a minute or two. The child was a female, mature and large. On examining the head it was found that not only was there no depression caused by the promontory, but, with the exception of some elongation of the head, there was nothing in its shape to indicate that it had been exposed to unusual pressure. There was an indentation about an inch below the occipital protuberance, right in the middle line; and an outline of the extremity of the blade of the forceps was well seen on the middle of the brow, about an inch above the root of the nose. These markings showed that Simpson's forceps had grasped the head exactly in its long diameter. There were no marks on the ears nor on the sides of the neck. The site of the anterior fontanelle was again examined, and found to be exactly as described. The following are the measurements of the head, taken immediately after birth: bi-parietal diameter, $3\frac{1}{2}$ inches; coronal, $3\frac{1}{4}$; bi-mastoid, 3; antero-posterior, $4\frac{1}{2}$. The patient made a good recovery.

PHARMACEUTICAL EXPLOSIONS.—According to the *Farmaceutitschesky Jürnal*, December 4th, 1888, p. 783, a sad accident recently occurred in the shop of a St. Petersburg chemist. A powdered mixture, consisting of an ounce of chlorate of potassium and one drachm of tannic acid, was ordered by a doctor who probably had forgotten his chemistry. The chemist, who also ought to have known better, set to work to dispense the prescription, and was punished for his ignorance by a violent explosion of the incompatible substances. His hands were severely burned, and his beard and eyebrows were singed off. He was only able to resume his occupation several weeks afterwards. A similar accident occurred about two months before at Warsaw, where a chemist tried to prepare a mixture consisting of chlorate of potassium, tannic acid, and oil of peppermint (*loc. cit.*, October 23rd, p. 688).

THE CLINICAL CHARACTERISTICS OF TUMOURS

AND

THE SHOCK CAUSED BY SURGICAL OPERATIONS.

*Being a Portion of the Presidential Address delivered at the
Annual Meeting of the Clinical Society of London on
January 25th, 1889.*

BY CHRISTOPHER HEATH, F.R.C.S.,
President of the Society.

AFTER some preliminary remarks on the past good work of the Society, and some suggestions as to its future labours, Mr. Heath proceeded thus:—

Mention of the Pathological Society (of which I suppose we are most of us members) reminds me of the saying of an old friend who visited that Society one evening after an absence of many years, and reported in these words: "I found them showing the old specimens under new names, and talking a language which I did not understand, but they were more positive about the correctness of their views than ever." Now it is this positiveness of the pathologist that I, as a clinician, venture to enter a protest against, and I think one of the most useful functions of the Clinical Society may be to check the results arrived at in this room by the members of the sister Society. Because a tumour shows microscopically an appearance which brings it into the class of sarcoma, I should be very loth to classify the case as one of "malignant" disease, without knowing fully the history of the patient both before and after the operation. Let me quote an example or two from my own practice. In 1883 I removed the upper extremity, with the scapula and part of the clavicle, from a lad, aged 16. The growth was a spindle-celled sarcoma, undergoing ossification, and upon that evidence alone, when I brought the case before the Royal Medical and Chirurgical Society, I was taken warmly to task by two ardent pathologists for expressing a hope that I had permanently relieved my patient, and was told that the disease would certainly recur and destroy him. It did recur, for seven months and a half later I removed a small nodule from the cicatrix; two years and a half after the amputation I cut a small mass out of the pectoral muscle; and again last November, more than five years after the amputation, he returned with two nodules near the cicatrix, which I removed, leaving him in apparently perfect health. In October, 1883, I removed a sarcoma from the muscles on the inner side of the thigh of a girl, aged 14. Recurrence took place, and five months and a half later I repeated the operation, but was unable to get away all the disease, and was only prevented from amputating at the hip-joint by the fact that even then some of the growth would have been left in the pelvis. Yet the patient is still alive and thriving, though there is a tumour in the thigh which shows little tendency to increase. In 1882 I removed a myxo-sarcoma of the upper jaw from a man of 53. In five months he reappeared with recurrence of the disease, which was again freely removed, a third time after another five months, and a fourth time after a like interval; so that four serious operations were performed in about twenty months, and yet he is now, nearly five years after the last operation, in perfect health and without a vestige of disease. And yet in a well-known work on *Malignant Disease* I am taken to task by name because in my book on *Diseases of the Jaws* I have separated the sarcomata other than the round-celled from the malignant tumours of the jaws, which include, of course, all varieties of carcinoma. The fact is that the class sarcomata includes growths of very varying degrees of malignancy, ranging from melanotic sarcoma at one extreme to myeloid sarcoma at the other, as, indeed, all pathologists allow; but what I maintain is that many cases of spindle-celled sarcoma are infinitely less malignant in their clinical history than the histological appearances would lead one to expect.

I venture also to enter a protest against the idea that the malignancy, or otherwise, of a tumour, so far at least as concerns the necessity for its prompt removal in any given case, can only be settled by microscopic examination. If the surgeon, with the patient before him, and with a full history of the growth of the

tumour, cannot make up his mind whether it ought to be removed or not without previously extracting a small portion for microscopic examination, I venture to think he has mistaken his vocation. Clinical experience alone will guide the surgeon most satisfactorily as to the cases he ought or ought not to interfere with, and it is the well-being of that particular patient, and nothing else, that ought to weigh with him. Of course it is very satisfactory when the microscope confirms the diagnosis of the nature of a growth made before its removal, and yet how often do we find the after-history belie the prognosis of the microscopists, even when they agree!

Now, I believe that incorrect deductions are at present drawn because the number of cases recorded in full detail is comparatively small, and it is impossible for any author, however diligent he may be, to follow up all the cases and learn their final results. Here, then, is a field for clinical work which it seems to me our Society might well engage in. Every London hospital has or ought to have a surgical registrar, and many of these gentlemen are already members of this Society. If we could induce a committee of registrars to undertake the work of looking up all the cases of sarcoma and carcinoma which have passed through the wards of their respective hospitals during a period of ten years, providing for them the funds which would be necessary for following up each case to its termination, we should have a mass of material of the most valuable and reliable description, which would go far to decide many moot questions of pathology. Even as regards carcinoma there are many points on which accurate information, and not mere impression, is greatly needed. I should have supposed that surgeons were at one with regard to removing the entire breast in cases of cancer, but I find that Mr. Butlin does not consider this necessary or advisable. Mr. Banks, of Liverpool, and Mr. Pearce Gould have urged that the removal of all the lymphatic glands of the axilla is advisable, whether they are diseased or not; whilst Mr. Butlin brings forward weighty arguments against the practice. Again, should the lymphatic vessels leading to the glands be removed or let alone?

Whilst ready to give the credit to the antiseptic system, introduced by a former distinguished occupant of this chair, of diminishing the risk and pain of operations by procuring healing by first intention, I am not quite sure whether our recurrences after operations for cancer are not more rapid in these beautifully-healed linear cicatrices than they were in the old days when profuse suppuration probably eliminated any particles of disease which had escaped the knife. My friend Mr. Swain, of Plymouth, a surgeon of large experience, tells me that he has given up using stitches in the removal of a breast, because he noticed the tendency of each little scar to become affected with recurrent disease. Is that a fact, and if so—how is it to be explained? Again, in operations which are, from the nature of the case, incomplete for example, where the surgeon finds it impossible to remove the whole of an extensive epithelioma—does the interference light up greater activity in the portion left, and is the last state of the patient better or worse than the first?

Mr. Butlin, whilst recognising the benefits of Listerism, maintains that its success is leading surgeons into two errors. "The first, and by far the less serious, of the two is the performance of operations which are far more severe than the individual cancer calls for. The second and by far the most important is the extension of operative surgery to the cancers of parts of the body which can only be removed with the gravest danger to the patient." I am afraid, from what I have both seen and heard, that there is some truth in these indictments; and I would venture on my own account to add another, which I mentioned in this room nearly a year ago. It is that patients will and do suffer from shock to their nervous systems after severe operations, notwithstanding all antiseptic precautions, and, indeed, in some cases because of them. Mr. Banks, of Liverpool, in his paper on "Cancer of the Breast," puts it very forcibly thus: "Let any healthy woman get out of a warm bed, strip herself naked to the waist, and then go and lie down on her kitchen table for forty minutes, and I shall be very much surprised if she has not a cold next day. But, in addition to this, let a cold spray play upon her, and furthermore let a great mass of heat-conserving skin and fat be carried away from the chest, so that nothing but ribs and muscles intervene between the lung and the air, and still let the cool spray play upon her. Is not this likely to be very depressing? I am convinced that it is; and if the operator, in place of doing his work rapidly, niggles and fiddles about it, so that the patient is kept for a long time under the combined lowering influences of cold spray, anæsthetic, and

loss of blood, then the result is that a distinct catarrhal pneumonia very speedily appears."

Again, my friend Dr. David Cheever, of Boston, has kindly sent me a reprint of a paper on Shock which he read at the First Triennial Congress of American Physicians and Surgeons, held at Washington last September. In it he says:

"Formerly the time consumed in an operation was short. An amputation was hurried, now it is deliberate; an abscess was incised, now it is aspirated and curetted; a joint injury was cut off, now it is excised; the peritoneum was peeped into, now it is boldly explored; the bladder was cut for stone, now it is a prolonged crushing and washing; a breast was amputated, now the axilla is formally dissected. The old method was a matter of minutes, now it is one of hours. Patients are frequently from one and a half to two hours on the operating table, and three hours in recovering consciousness so that they can swallow. Do we realise what this prolonged cutting, pinching, and dissecting means to the nervous system after anæsthesia is past? Does not the long exposure of the great veins to the air in dissecting tumours increase coagulability and future infarction? Can the peripheral nerves be lacerated *seriatim* without exhausting their constitutional irritability? It is recognised that long-continued and large dissections on the front and sides of the neck are especially fatal." And again: "In feeble subjects the lack of nourishment which precedes an operation, desirable on account of safe anæsthesia, is much aggravated by their inability to retain food after the operation. This has an important influence in bringing about collapse."

"Lowering of the bodily temperature is constant after an operation under anæsthesia. The thermometer frequently falls to 97°, to 96°, and, after severe and prolonged operations, to 95° F. This is a very serious matter, and has a marked influence in delaying reaction from shock. This chilling of the vital heat is induced, first by anæsthesia, which, if prolonged, ends in a dripping sweat; next, by careless exposure during an operation. Then also it is largely due to antiseptic irrigations, to vapour douches of similar agents, to applications of cloths wet in corrosive or carbolic solutions around the site of the operation. The axillæ, the neck, the thorax, and the abdomen are especially prone to deleterious chilling in this way."

Of course I am perfectly aware that many surgeons, myself among them, hardly ever employ the carbolic spray nowadays: but even without this there is, I believe, considerably increased risk to the patient in the exaggerated slowness of modern surgery. For a patient to breathe the vapour of ether or chloroform for over an hour is a serious matter for both heart and lungs; and when we add to that a steady drain of blood going on for an equal time, one cannot be surprised at the production of deadly shock. And is there any gain after all in elaborate dissections of the living as though it were a dead body? I believe not; and, having had a fair amount of experience in removing tumours of all sizes from all parts of the body, I would urge surgeons to use the left hand a little more and the right a little less than now seems to be the fashion. I mean, to make forcible traction with the one hand whilst dividing the tense structures with the other, so as to avoid dissecting-room proceedings with scalpel and forceps as much as may be. I hope I shall not be misunderstood as decrying careful dissection in appropriate cases—for example, the ligature of large arteries; for I fear, from what I see in examining candidates for the higher surgical degrees, that a blunt director must be used by some teachers in what I consider to be a very dangerous manner.

In order to guard against the shock of large operations on weakly patients, I have for some years now availed myself of a suggestion which I owe to an American surgeon, whose name I have unfortunately forgotten—namely, half an hour before the operation to inject into the rectum with a long tube two ounces of brandy with four of hot water. This acts as a reserve of power which can be absorbed at leisure, and has the advantage over the administration of a stimulant by the mouth that it cannot embarrass the breathing. The injection can be repeated during the operation if necessary, and may be combined with the subcutaneous injection of ether. Lastly, I strongly advise the subcutaneous injection of from gr. $\frac{1}{4}$ to $\frac{1}{2}$ of morphia, combined with gr. $\frac{1}{32}$ of atropine before the patient leaves the table, believing that not only is pain thus relieved, but that sickness is materially kept in check by the combination of drugs.

I have ventured to make these remarks, gentlemen, believing

that the word "clinical," as applied to our Society, should be taken in the broadest sense—as relating not merely to the recording of cases, but to all that wide field of therapeutics which may enable us to relieve our patients *cito, tuto, et jucunde*.

THE LETTSOMIAN LECTURES ON SYPHILIS AND THE NERVOUS SYSTEM.

Delivered before the Medical Society of London, January 21st, 1889.

By W. R. GOWERS, M.D., F.R.S.

LECTURE II.

[Concluded from page 171.]

THE inadequacy of the clinical history of a syphilitic lesion, when that history is based only on certain evidence, is conspicuous in the case of the disease of the walls of the arteries. The classical researches of Heubner, who first placed our knowledge of this subject on a secure footing, were based, of necessity, to a large extent on fatal cases. The clinical picture delineated from those facts, faithful as it is so far as regards the effects of extensive and severe disease, is yet imperfect as a representation of the slighter and more common consequences of the morbid process. Passing over the former, because well known by description, although now seldom met with, I would ask your attention to some facts regarding these slight forms. The importance of the early diagnosis of this condition is unequalled even among syphilitic lesions. There is no luetic process which is so likely to cause grave and lasting damage, no process the effects of which, when once produced, are so entirely beyond our power, and yet can be so surely averted by prompt treatment. For this reason I think it is wise to devote more time to this than to the other special questions, and also because it is a disease in which the diagnosis during life can generally be made with sufficient confidence to give a high value to clinical facts.

The chief mechanism by which this process is effective in causing symptoms, as we have seen, is by the sudden closure of an artery. The suddenness of the closure is no doubt due to the fact that the narrowed cavity is finally occluded by clot, and in this respect the syphilitic disease does not differ from the other common form of arterial disease—atheroma. The closure is probably not often that of the main artery whose wall is diseased, but of a branch that comes off at the spot, the orifice of which is narrowed, so that the blood in the part beyond tends to stagnate and to clot. The closure has the usual consequence: sudden anæmia and necrosis of the brain tissue, unless there be sufficient anastomoses to permit a collateral circulation. This may be either complete, restoring function, or so incomplete that the continuity of the tissue is preserved only by a process which results in sclerotic induration.

The characteristic symptoms are those of a sudden focal lesion of the brain. Until the degenerative period of life is reached, such a sudden lesion is excessively rare, except from embolism. Embolism can usually be excluded if there is no valvular heart disease or other source of embolism, and if the patient has not recently suffered from a disease known to cause endocarditis. Hence it follows that the occurrence of such symptoms in an adult under 45, who has had syphilis and has not heart disease, may generally be held to indicate syphilitic disease of a cerebral artery, and this diagnosis may be made with a confidence possible in few other syphilitic lesions. This confidence is confirmed by the occasional opportunities for a verification. When, under such conditions, the diagnosis has been made, and the patient has subsequently died, the arterial disease has been found—invariably, so far as my own experience and reading have gone. It is true that, in rare cases, certain other causes of arterial closure must be excluded. I will mention these presently; they do not really lessen the validity of the statement just made, since the need for considering them is not only rare, but is also generally obvious. This confidence can, however, exist only before the degenerative period. When this period arrives, other causes of arterial closure come into operation

—causes which we cannot exclude with the readiness and confidence with which we can exclude embolism. This introduces an element of doubt into the diagnosis of cases over 45 or 50 years of age. Hence, in endeavouring to ascertain the history of these cases from mere clinical observation, it is desirable to confine our attention to those in which the diagnosis can be made with most confidence. The disease does occur during the later period of life; we have to recognise the fact in our practical diagnosis. It may be that this renders some of our conclusions from the earlier cases imperfect, but we must accept this imperfection in order to obtain as much certainty as possible.

The following conclusions are based on a series of cases that have come under my observation, excluding those over 45 years of age. The cases are 50 in number, but on some points (owing to omissions) the precise number available for comparison is somewhat less. The patients were over 25, with the exception of one, aged 21. Males were to females as three to one, a proportion that may not be far from the incidence of syphilis itself.

Many observers have noted how various are the intervals between the primary disease and the cerebral lesion, and the variation is conspicuous in this series. In one quarter, the lesion occurred during the first two years after infection, and the remainder were pretty evenly distributed during the next twelve years: 1 to 2, nine cases; 3 to 5, seven cases; 6 to 10, nine cases; 11 to 15, eleven cases; 16 to 20, four cases. There was an interval of nineteen years in a man who had well-marked syphilis at 18; and at the age of 37 an attack of right hemiplegia, while a year later paraplegia came on in a subacute manner. Eighteen years intervened in a woman who at 16 married a man of unsteady habits; her first two children were born dead, and hemiplegia came at 35, with an onset characteristic of arterial thrombosis. The shortest interval was apparently only three months, but the case does not seem otherwise open to doubt; a man, at the age of 21, had, for the first time, a chancre—a distinct hard sore—followed by indolent enlargement of the glands. Three months after the appearance of the sore, he had sudden hemiplegia, which slowly passed away, but was followed six months later by a second attack on the same side. In these cases, as indeed in all those compared, careful search revealed no other cause. In the only other case in which the interval was less than a year, the attack occurred six months after infection.

By far the most common effect of the lesion is hemiplegia, indicating disease of the middle cerebral artery, and probably the closure of a branch to the central ganglia. In only one case did convulsions at the onset suggest that the damage was limited to the cortex. (Simultaneous central softening necessarily prevents such special indication of a cortical lesion.) In three cases, hemianopia suggested that the arterial disease was in the posterior cerebral. It is evident, however, that these cases present only some of the effects of vascular disease, and do not present at all its gravest consequences. Lesions may occur in parts of the brain where they cause no focal symptoms; and, on the other hand, disease of the basilar and vertebral arteries, sufficient to cause definite symptoms, is seldom survived. These grave effects, however, are well known, and I therefore pass them over. In probably nineteen cases out of twenty, the Sylvian artery is that by which arterial disease causes symptoms—a fact easily explained—and of such disease this series probably presents a fair example.

The degree of the paralysis and its course present variations as great as, and similar to, those met with in embolism. It may be slight and transient, or severe and then often lasting, with only such recovery as occurs from compensation—a slow return of power in the leg and some in the upper arm, the hand remaining powerless, or almost powerless, with late rigidity. I have a few times known two attacks of hemiplegia to occur, at intervals of months. Very rarely symptoms of some other syphilitic process coincide with the onset. It is only in such a case, in which there is reason to suspect a gumma, that I have seen optic neuritis. In several cases the patients have, at some other time, suffered from paraplegia, such as might be caused by a gumma pressing on the cord.

The onset occurred during sleep in a third of the cases, a proportion nearly corresponding to the time spent in the sleeping state. During the waking state, the onset is seldom attended by loss of consciousness; it was so attended in only one-tenth of the cases—a fact of much importance. Giddiness and vomiting were occasional accompaniments. Of great significance also is the fact that the onset, in more than half the cases, was preceded by headache. The pain was usually great, and was either general or