sound ground to work on; and, if we add to that the reports on the health of towns, published by the commission of which the present Duke of Buccleugh was president, we shall have stated some of the most important foundations of modern sanitary science. The establishment of the General Board of Health, under Mr. Chadwick, was one of the valuable outcomes of this remarkable movement. Although the original Board of Health was brought to an end in 1854, yet its work has been continued and expanded under Mr. Simon, his colleagues and successors.

We have a long roll of names connected with the public services which must ever be remembered with honour; in the Navy we have such names as Lind, Blane, and Burnett; and in the Army Pringle, one of the most philosophical physicians who ever lived, Brocklesby, Fergusson, McGrigor, and a host of others. The labours of the late Sir Alexander Tulloch, Deputy Inspector-General Marshall, and Surgeon-General Balfour, in collecting and arranging the army statistics, were of the highest value; and it is not too much to say that the publication of the first Army Medical Statistical Report marked an epoch in hygiene, especially in that part that deals with climatology. The Army Medical Statistics are continued now yearly, but it is a matter of regret that they have been allowed to be published in so abstract and undetailed a shape, as to deprive them of much of their utility. But perhaps the most remarkable contribution the army has made to sanitation has been by the evidence given to the Royal Commission of 1857, which met after the Crimean War, to investigate the causes of the sickness and mortality of our troops. The paramount influence of foul air in the production of lung-disease was proved to demonstration, and the art of ventilation was placed on a secure foundation. The Barrack and Hospital Committee, of which Dr. Sutherland and Captain Douglas Galton were the active members, laid down a series of regulations for the construction of barracks and hospitals, which have been followed with the utmost benefit both at home and abroad. Following this came the Indian Commission, which did for that vast dependency what the Home Commission had done for the rest of the empire.

To recognise an evil, and its cause, is half way to curing it; and, after the lapse of a quarter of a century, we can point to such an improvement as might at one time have been looked upon as chimerical. The death-rate of the army at home is only two-fifths of what it was before the Crimean War; the death-rate in India is only one-third, and the death-rate in the West Indies barely one-tenth. In civil life, it has recently been shown that the improvements of later times have resulted in a diminution of 2 per 1,000 in the general death-rate; and, with the knowledge we now have of the causes of disease, we may be sure that a general death-rate of not more than 15 per 1,000 may be confidently looked for.

I might extend this lecture by reference to the various theories of propagation of disease, but time would not permit it, even if it were otherwise desirable. I may, however, say that no one theory yet promulgated completely satisfies the requirements of the case, and that there may be some basis of truth, even in the more conflicting views. It is quite clear that it is only by a knowledge of the causes of disease that hygiene can be advanced, and that it can never be in any way perfected without a complete system of etiology. And we are at present in this position, that practical hygiene has, to some extent, outstripped the knowledge of causes of disease. We look, therefore, anxiously towards the pathological investigations of the time, and we deeply deplore the well meaning but misguided zeal which is at present placing such grave obstacles in the way of the only means by which true science can advance, namely, direct experiment.

Although there are many names to whom I might refer as good workers in hygiene, abroad as well as at home, there is one which we cannot omit in a lecture like this, more especially as it is the first delivered in this museum, which has been founded to his memory.

Edmund Alexander Parkes did more than any other one man in this, or any age, to make hygiene a positive fact, a practical science based upon, not only philosophical conceptions, but actual experiment. Starting in life as an army medical officer, he was able to produce during his short service in India and Burmah, works upon Dysentery and Cholera, which will always be of the greatest value. Retiring into civil life, he became eminent as a physician and teacher, and in 1855 he undertook the organisation of the hospital at Rénkioi in the Dardanelles, which was a perfect model of successful hygienic administration. Struggling with distressing and dangerous disease, he continued to lead a life of intellectual activity, not often accomplished by the most robust; and, when in 1860 the Army Medical School was established by Lord Herbert of Lea, Sir James Clark had no hesitation in advising that Dr. Parkes should be

secured, if possible, as the Professor of Hygiene. How excellent the foresight of that eminent physician was, we all know, for Dr. Parkes was not only the first Professor of the science in this country in point of time, but also the first in every sense of the word. The publication of his well known *Manual of Practical Hygiene* gave us for the first time a work on the subject, which was not merely a string of opinions and surmises, but at every point brought opinion to the test of figures and experiment, where it was possible, and thus laid the foundation for a real science in the future. Similarly with his teaching he pressed upon the Government to establish practical laboratories for his pupils, where they could do for themselves as much of the experimental work as time and opportunity allowed; and he impressed upon those who studied under him, the necessity of testing everything by actual investigation, and bringing all statements to the proof of figures before accepting them as true. There was never probably a man of calmer and more judicious mind, a man more rigidly critical of his own work, or more kindly disposed to allow every credit to the work of others. Having known him personally for many years, during thirteen of which I was his assistant and colleague, I can bear confident testimony to the exceeding beauty of his character, in which "sweetness and light" were never more truly displayed, and to the scrupulous accuracy and care with which every investigation of his was carried out. The science of hygiene could have no purer and better founder, and its votaries no brighter and more spotless example.

CLINICAL REMARKS

ON

PRIMARY OR IMMEDIATE LIGATURE OF THE FEMORAL ARTERY IN POPLITEAL ANEURYSM, AND ON THE DIVISION OF THE ARTERY BETWEEN TWO LIGATURES.

By T. HOLMES, M.A., F.R.C.S. Eng.,
Surgeon to St. George's Hospital.

THREE cases of popliteal aneurysm have been treated at St. George's Hospital within the last few months by the Hunterian operation without any previous trial of compression. As this is a somewhat unusual circumstance at the present time, I have thought it might probably interest the readers of the JOURNAL to have the subject of ligature of the femoral artery brought under their notice, especially at the present time, when the old plan of dividing the artery between two ligatures is being reintroduced by Mr. Walsham and other surgeons at St. Bartholomew's Hospital.

After seeing something of popliteal aneurysm, and studying the literature of the subject, and the practice of other surgeons, with a certain amount of industry, I have come to a very confident opinion that the safest plan of treatment for all large, all rapidly growing, and all thin-walled popliteal aneurysms is to tie the femoral artery at once, that is, after a few days' confinement to bed; and that the safest way of tying it is that which interferes the least with the natural connections of the artery. I believe, further, that the safest material for the ligature is either the ox-aorta ligature of Mr. Barwell or the kangaroo-tendon ligature which was introduced at St. George's Hospital by our then assistant-surgeon Mr. Stirling, and which was brought under the notice of the Royal Medical and Chirurgical Society by Mr. T. Smith and Mr. Girdlestone of Melbourne (*Med. Chir. Trans.*, vol. lxv, p. 71). On this latter point, however, our experience is as yet insufficient.

I will illustrate the three points above-mentioned by a short account of one of the above cases (the only one which was under my personal care), and by a very brief reference to the other two, which were treated by Mr. Haward and Mr. Dent.

My patient was thirty-three years of age. He was a married man, and there was, as far as I know, no history of syphilis, though there seems reason to think that he had been given to drinking. He first noticed something wrong about a month before his admission, when he was getting off a horse, and felt "something snap" behind his right knee. It did not seem very clear how soon afterwards he was conscious of the existence of a tumour and pulsation; but it soon became very painful. Still he continued at his work (that of a coachman) up to the day of his admission. When he presented himself at the hospital, on April 23rd, there was a very large

aneurysm, filling up the whole popliteal space, and consisting of several lobes. The main portion of the tumour, opposite the flexure of the joint, felt more hard and tense than the rest, and was separated by a distinct sulcus from another portion, which extended upwards and outwards, where the wall seemed much thinner. There was also another part projecting on the inner side. The bruit was loudest opposite the joint. Pressure on the artery did not much diminish the size of the tumour. The man was grey-haired, and seemed much above his alleged age. He was restless and very nervous. He was kept in bed for a few days, with the effect of at first much diminishing the pain of which he complained in the leg and in the tumour; but the latter was obviously increasing in size, and the pain soon began again to trouble him.

On consultation with my colleagues, it was agreed that the best course would be to tie the femoral artery at once; and the operation was fixed for April 26th; but the patient's resolution failed him, and he begged that it might be postponed till the 28th, on which day he consented to its performance. In the interval, the increase of the tumour was very decided. The artery was tied at the usual place with a stout kangaroo-tendon, tied in a double knot. The wound was washed out with carbolic lotion, completely united, and dressed with carbolic gauze. Immediately on the ligature of the artery, the tumour collapsed to about half its size, and the temperature of the limb fell perceptibly. This diminution of temperature persisted for two or three days; but otherwise there was no symptom to record. The wound united throughout by first intention. Sixteen days after the operation, it was noted that the tumour was nearly, if not entirely, solid, about one-third its former size, and the man was to be allowed to get up for a short time, but not to walk. He has since gone on perfectly well, and will soon leave the hospital.

The other two cases in which the femoral was tied without previous pressure may be thus summarised. One was a man, aged 57, under Mr. Haward's care in July last with disease of the lungs, in whom, after a slight and very trivial trial of genuflexion, the artery was tied, as it was not thought desirable to submit him to any prolonged or irritating treatment. The substance used for the ligature was chromic catgut. In this case, the wound healed almost, if not entirely, by first intention, and the aneurysm was entirely cured. The man died twenty-four days after the ligature, from the rupture of a vessel in the brain. Mr. Haward will, I believe, publish an account of the dissection. It will be enough here to say that everything about the aneurysm and the ligature had been completely successful. The ligature was intact, surrounding the external coat of the artery; the lower coats were cut through. The artery was occluded with clot in the usual manner. The aneurysm was filled with laminated clot. The other was one of the rare cases of popliteal aneurysm in the female. The woman was fifty-nine years old, under Mr. Dent's care. The aneurysm was very large, and extended up beyond the popliteal space. The artery was tied with a kangaroo-tendon on October 5th. The wound united by first intention, and all went well, except that the tumour remained large and without consolidation for an unusually long period. Ultimately, however, this entirely subsided; and when she was last seen, there was hardly any trace of the enormous aneurysm to be felt.

These three cases, then, may all be classed as successful; for the death of Mr. Haward's patient had no connection whatever with the operation, and the latter had completely succeeded in its object of curing the aneurysm.

REMARKS.—Much is said in the present day about "progress" in everything, and not least in surgery; everything is constantly changing, and it seems almost to need an apology if a surgeon treats his patient as Sir A. Cooper, or even as Syme, would have treated him. There is no wonder, then, that the treatment of aneurysm by the ligature has become much restricted. We no longer look on pressure in the treatment of popliteal aneurysm with the disfavour with which Syme regarded it. In fact, in one or other of its many forms, it is indisputably the routine treatment; yet, like all other "routine" treatments, it is liable to do mischief if it be not used with discrimination; and this seems to me specially the case with the two forms of pressure which are the most uncertain in their effects, and the least easy to regulate—I mean genuflexion and the use of Esmarch's bandage. But even the less dangerous methods of pressure, by the finger or by instruments, not uncommonly do considerable harm, even when they are borne perfectly well by the patient. I have never ventured on the application of Esmarch's bandage to a large, rapidly growing, thin-walled tumour; nor do I think that a prudent surgeon would advise the treatment; while, as I have been

successful in the treatment of such cases of small aneurysm as I have had under my care, by methods with which I am more familiar, and in which I have more confidence, I have no personal experience of the method, nor have I heard that any of my colleagues have used it. I published in the *Clinical Transactions*, vol. ix, the account of a case in which I should certainly have used Esmarch's bandage had I then been acquainted with the treatment. And even with those methods of pressure to which I allude, I have, I think, seen more harm than good from a too long persistence in their use. If they do not succeed quickly, I believe it is better, on the whole, to give them up, and resort to ligature; though I am aware that success has often been obtained by the protracted use of pressure, still I have seen many cases in which, after a good deal of suffering, the patient has been left in a worse general condition to bear the ligature, and with no improvement in the local conditions, and even one or two in which the aneurysm has ruptured, and a case which might have been easily dealt with by primary ligature has become complicated and very dangerous. Hence, while fully recognising the value of all the forms of pressure, of which, indeed, my own experience has been sufficiently favourable, I do not think it wise to insist on their employment unless some considerable improvement has been obtained in a week. And in persons who are irritable from alcohol, or are unusually susceptible of pain, the use of pressure requires still more consideration.

That the success of the cutting operation has much increased of late years, I think there can be no doubt (see *System of Surgery*, 3rd ed., vol. iii, p. 139), though there may be some difference of opinion as to the main cause of that success. That "antisepticism" is the chief reason I am firmly convinced, if we use the word in its broadest sense, to include all modern improvements in surgery whereby the rapid union of the wound is favoured; but it seems also reasonable and consistent with experience to hold that another cause is the very slight exposure and separation of the artery which is now the common practice with most surgeons.

And this leads me, in the second place, to notice the proposal lately emanating from Mr. Walsham to revive the old method of dividing the artery between two ligatures. There appears to be an ancestral proclivity to this method at St. Bartholomew's, for it seems to have been constantly adopted in former days by Mr. Ramsden.* But is there really any advantage in it to compensate its obvious drawbacks? The latter are plain enough: that it involves a much freer dissection and exposure of the artery, separating it for the distance of about half to three-quarters of an inch from its connections and from the accompanying vein. The assumed advantage is that, by taking off the tension of the elastic artery, secondary hæmorrhage will be made less probable;† but for my own part, I venture to question the theory. There can be no question that two ligatures are substituted for one. If those ligatures are made of silk, both have to separate, and either, I submit, may bleed. To say that this never happens except from the slipping of a ligature when an artery is tied after a wound, is, in the first place, to make rather a hazardous assertion; and, in the next place, the cases are not strictly analogous, for wounded arteries are presumably more healthy than those which are aneurysmal. Secondary hæmorrhage after ligature for aneurysm is a very rare event, at least, it has proved so in my experience of the practice of St. George's Hospital. I will set aside two cases in which secondary hæmorrhage occurred after the use of the silver ligature, and which obviously depended on the fact that the ligature had been drawn too tight.

The difficulty of regulating the tension of this ligature is a strong objection to it, and has led to its disuse with us, though I had one very successful case of ligature of the femoral with silver for popliteal aneurysm. Excluding, then, these two cases, I can find but one instance of secondary hæmorrhage in our, the *St. George's Hospital Reports*, among more than twenty operations on large arteries in all parts of the body, and with various forms of ligature, and none at all, I think, in which animal ligatures have been employed; for, in the case in question, though the material employed is not stated, I am nearly certain that it was silk. I know, of course, that secondary hæmorrhage has occurred with catgut ligatures, but, if I am not mistaken, the cause has usually, if not always, been the melting,

* Ramsden's *Practical Observations* contains a history of four cases of ligature of main arteries—the subclavian (the original case), the common femoral, and the superficial femoral twice; and in all the last three he divided the artery between two ligatures. All three cases succeeded. It would be interesting, were it possible, to know why his colleagues did not adopt or continue the practice. In tying the common femoral, Ramsden says that he placed his two ligatures an inch asunder. In the superficial femorals, a shorter tract of artery was obliterated.

† See Mr. Walsham's article in the *BRITISH MEDICAL JOURNAL* for April 7th, 1883.

breaking, or untying, of the catgut,* and it is difficult to see how we render this catastrophe less probable by using two ligatures instead of one. As far as our present experience of the simple (*i.e.*, unmanufactured) animal ligatures has yet gone, no secondary hæmorrhage has attended their use. I mean by "unmanufactured," those in which the animal substance, whether it be ox-aorta or tendon, is not previously putrefied. But surely the use of such ligatures as ox-aorta or kangaroo-tendon is safer on an undivided artery than on its two ends after division; nor is it easy to see how so large and slippery a ligature can be safely applied to a divided artery without denuding a large part of the vessel, and thus handling and contusing the artery and the parts around much more than is done if the sheath be only opened to the extent required to admit the aneurysm-needle. I quite agree with all that Mr. Walsham says about the importance of opening the sheath to as small an extent as possible; but it seems to me an odd corollary from this proposition that the most successful plan is likely to be one in which the sheath is "freely opened," and at least half an inch of the artery exposed to view, and separated from all its connections (see *BRITISH MEDICAL JOURNAL*, May 12th, 1883, p. 906). There is no reason that I can see for saying that, in the ordinary method, if only the surgeon take care to make as small an opening in the sheath as is necessary to admit the needle, any part whatever of the artery is permanently separated from its vascular supply. That "its blood-supply has been more or less interfered with," as Mr. Walsham says, is no doubt true; but that interference is very transitory, and there is no reason at all for saying that it is ever the cause of secondary hæmorrhage. If it were, the bleeding ought to come on much earlier than it usually does, and should be of a very different character, since the whole tube of the artery would become necrosed.† If the artery be securely tied with a material which will keep its hold on the vessel until the seat of ligature is buried in a mass of new fibroid material, secondary hæmorrhage, if not impossible, is at least very improbable. We seem on the eve of arriving at something like certainty in the attainment of that end; and I confess it is to me disappointing to find that, at our chief hospital, this advance in the surgery of the arterial system is so little appreciated, that we are advised to return to the ideas of "the elder Bell and the Arabian physicians." My objection, however, to Mr. Walsham's proposal is not the sentimental one that it is reactionary, an objection which he would justly deride; but the practical one, that it inflicts unnecessary damage on the artery, that its efficacy as a safeguard against secondary hæmorrhage is very dubious, whilst it seems more likely to delay the healing of the wound, and expose the patient to greater risks of phlebitis and gangrene.

Of course, these opinions are only put forward as opinions. We have far too little experience as yet of the animal ligatures to dogmatise on the subject. Clearly, the experience of secondary hæmorrhage, and of renewal of pulsation after the use of the catgut ligature, is not in point, and it is the fact that Mr. Walsham uses that experience only which makes his paper to my mind unsatisfactory. Carbolised gut was, in the opinion of most surgeons, a great improvement on the silk ligature, but the natural tissues (such as tendon) are a great improvement on catgut; and I would plead for a full experience of their action in the simple use of the ligature, before going back on the complicated and antiquated method which Mr. Walsham employs, and which I strongly suspect has not been as successful in the hands of the colleagues and successors of Ramsden, at an earlier date in the history of St. Bartholomew's, as it was in the eight cases treated by Mr. Walsham and Mr. T. Smith. At least, it seems odd, if division of the artery is a great security against secondary hæmorrhage, that it should have passed out of use when secondary hæmorrhage was so much more common, in order to be reintroduced when that accident is seldom met with.

Our experience at St. George's of the tendon-ligature has certainly been small as yet, but it has been eminently satisfactory. I know of no cases as yet, but beside the two mentioned in this paper—Mr. Pollock's, reported by Mr. Dent (*Medico-Chirurgical Transactions*, vol. lxxiv), of double distal ligature of the subclavian and carotid; and one under Mr. Pick's care, of ligature of the femoral after the

failure of pressure. In all these cases, the ligature acted well, and the wound healed kindly. It would be absurd to found any general conclusion on so small a number of cases, but our present experience would certainly lead to a continued use of this form of ligature; and, even if we had to confine ourselves to the carbolised or chromicised catgut, I have seen no reason for modifying our method of applying the ligature.

ABSTRACT OF LECTURES ON PHYSIOLOGICAL DISCOVERY.

Delivered at the Royal Institution of Great Britain.

By JOHN G. MCKENDRICK, M.D., LL.D., F.R.S.E., F.R.C.P.E.
Professor of Physiology in the University of Glasgow, and Fullerton
Professor of Physiology, Royal Institution.

TENTH AND LAST LECTURE.—THE NERVOUS SYSTEM: THE HIGHER CENTRES AND THE BRAIN.

THE first portion of the lecture related the investigations by which the paths of sensory and motor impressions were traced in the spinal cord. Sir Charles Bell, in 1811, first conceived the idea that the posterior columns are sensory and the anterior motor. This naturally followed his views regarding the functions of the spinal nerves; but, in a less definite form, a similar view was promulgated by Boerhaave (1668-1738). At a later period, in 1835, Bell modified this opinion, and stated that the lateral columns of the cord must convey sensory impressions, because these may be traced to the cerebrum, and also because they receive the posterior roots. These views were strongly supported by Longuet, about 1840; but they were opposed by various physiologists, and, in particular, by R. B. Todd, who pointed out various anatomical difficulties. The lecturer then described generally the mode of solving the problem by making sections of the cord, wholly or partially, at certain levels, as practised by Magendie, Stilling, Schiff, and others; and, in particular, and with special success, by Brown-Séquard (first in 1849). He showed how the latter physiologist arrived at the conclusion that sensory impressions were not conveyed by the posterior columns, but by nerve-fibres passing through the grey matter, and that further there was a decussation in the cord of these sensory fibres. Similarly, Brown-Séquard was able to trace the course of the motor fibres. Such important results could not have been obtained by any other method, and the whole research is a striking example of the value of the experimental mode of investigation.

He then referred to the histological researches of Jacob Lockhart Clarke, who, in 1851, first described his beautiful method of preparing, for microscopical purposes, thin transparent sections of the spinal cord. Thus, histological inquiry aided experimental investigation, more especially as regards the distribution of the sensory fibres of the posterior roots.

As an example of another method of investigating the distribution of nerve-fibres in nerves, and in nerve-centres, the lecturer again described the method of Augustus Waller. By this method, it was shown that in the anastomosis between the sympathetic and pneumogastric nerves, fibres are given by the sympathetic to the pneumogastric, and not by the pneumogastric to the sympathetic. Again, it was shown that the chorda tympani was really a branch of the facial, coming into communication with the lingual nerve. Waller's method has also been successfully applied to the investigation of pathological phenomena in the human being, by tracing the path of degenerated nerve-fibres, as has been done by Türck and Charcot.

The functions of the brain and cerebral ganglia have been investigated along various lines of inquiry.

1. There are the data supplied by investigating its structure and relations in the human body. The lecturer alluded to several important steps in this department. Galen dissected the brain chiefly with the view of demonstrating the position of the ventricles, to which he attached great importance. The notion of many of the older anatomists, as of Varolius (born 1543), Sylvius (1478-1555), and Willis (1622-1675), was that the brain was an addition to the spinal cord. It was a great step to dissect the brain from below upwards, and thus to trace its relations to the spinal cord and nerves, as was first done by Varolius. The lecturer again referred to the important observations of Willis, who may be regarded as the founder of the comparative anatomy of the brain. Malpighi thought the grey

* Mr. Callender's case, referred to by Mr. Walsham, may, perhaps, be an exception (see his article in the number for April 7th), but I have tried in vain to find any account of it in the *St. Bartholomew's Hospital Reports*, and Mr. Walsham hardly gives sufficient details to enable us to judge; he merely says that the knot was intact; but this does not show that the rest of the ligature was so, and we know that, in some cases, the catgut ligature has softened prematurely, and the force of the circulation has ruptured the weakened vessel. I see no reason why this should not happen after the division of the artery as well as before it.

† See Mr. Bartlett's case, quoted by Mr. Walsham.