The same thing is frequently seen in young infants who have been played with and over excited immediately before being put to bed. The infant is uneasy and restless, starting frequently, and waking up with a fretful cry. This is not found with all infants, but is especially noticeable in those of delicate organisation and great impressibility of the nervous system, and is, therefore, a frequent symptom of commencing rickets, where the irritability of the nervous system is very great.

Sleeplessness in infants is thus produced by many different causes, each of which will require a different method of treatment for its removal. To look upon such a condition as a distinct disease, removable by any so-called specific, is in the highest degree unphilosophical and unpractical. Opiates, and perhaps bromide of potassium, may be occasionally useful in quieting excessive irritability of the nervous system, and may be, therefore, of service in the treatment of sleeplessness arising from the last two causes which have been mentioned; but to employ either as a universal remedy in such cases would be at least useless, even if it were not injurious. The screams of an infant suffering from an accumulation of undigested food—to take the commonest case—may certainly be quieted for the time by a narcotic; but so long as the cause remains, the screams will be renewed as soon as the soporific effect of the drug has had time to pass away. In such a case, bromide of potassium produces no effect whatever. In every case of sleeplessness in infants, the cause may be easily ascertained by careful investigation; and, when it is discovered, there is little difficulty about its removal.

AN ADDRESS

DELIVERED IN

THE SECTION OF PHYSIOLOGY,

*At the Annual Meeting of the British Medical Association,
in Leeds, July 1869.*

By JOHN HUGHES BENNETT, M.D., F.R.S.E.,
President of the Section.

GENTLEMEN,—Three years ago, in the Address in Medicine which I had the honour of delivering at Chester, I reviewed the existing state of medicine as a science and as an art. I pointed out that the former, by which may be understood physiology, was now so far advanced, owing to the progress of collateral sciences, that one could by its aid solve most of those problems which rendered the latter so uncertain. In other words, it was my endeavour to show that certain questions as to whether any remedial agents or plan of treatment could produce a definite influence on the living body, was now within the reach of experiment. I argued, therefore, that what we required was combined labour to solve these problems, and that, if this British Medical Association would advance the necessary funds, exact knowledge might be made to assume the place of vague opinion. As a result of the appeal then made, grants of money were given to a Committee under my direction, which undertook to answer the question whether mercury was or was not a cholagogue—and this question, after two years' laborious inquiry, has been decisively answered in the negative. No pains have been spared by the Committee in making their inquiry exhaustive. Their experiments and observations were so planned, so carefully performed, and so fully considered, that up to this time no adverse criticism has been advanced. The results have been laid before full meetings of the Association in Dublin and in Oxford, and before the Physiological Section of the British Association for the Advancement of Science, at its last meeting in Norwich. They have also been extensively published, and no one has endeavoured to show that they were erroneous. It may, therefore, now be received as an unquestionable fact, contrary to the almost universal opinion which previously existed, that mercury has no special action on the liver, nor possesses any power whatever of increasing the flow of bile from that organ. The Committee has further shown that such action and power have been also erroneously attributed to taraxacum and podophylline. Considering that, up to this moment, every book, and every lecturer on the *Materia Medica* and on Therapeutics, has assumed that these drugs do possess the properties referred to; further, that they have been universally given to stimulate or control biliary formation; and, lastly, that to this end their administration in countries where hepatic diseases are common, has been such as too frequently to injure rather than benefit mankind—the demonstration of such an error must be admitted to be of the utmost importance to medical knowledge and to medical practice. It may be said, as it has been said, that this in no way alters the value of mercury as a purgative. But if this drug, henceforth, is only to rank

in such a category—that is, as an *evacuater* and not a *secreter* of bile, many other aperients, less dangerous to the system, will answer the purpose.

But there are other important problems which physiological experiment has the means of solving with like certainty; and I have now to ask whether the Association is to stop in its attempt to further scientific and practical progress? Can we not appoint another Committee, and entrust to it the solution of another great difficulty in medicine?

Let me refer shortly to one of these difficulties, the practical value of which cannot be too highly estimated, one, which if scientifically investigated, must throw a flood of light on the whole subject of therapeutics, and one which is certainly within the reach of careful experiment.

It has been proved beyond the possibility of doubt, that certain medicinal agents, such as morphia, atropine, conium, digitaline, strychnine, woarara, and others, possess distinct, yet apparently opposing, actions on the nervous system. Some of them act primarily on the brain; others on the spinal cord. Some excite and others paralyse nervous action. Some keep persons awake, such as green tea; others put them asleep, such as morphia. One certainly excites the motor powers of the cord, such as nux vomica; another as certainly depresses or paralyzes it, such as hemlock. These facts necessarily give rise to a question, that, in the present state of science, requires to be authoritatively answered. It is this.—Does there exist an antagonism between these substances, and, if so, to what extent? In other words, what truth is there in the idea that one drug is capable of counteracting the agency of another. Does it follow that, because we can excite the motor powers of the cord with strychnine, that when so excited we can tranquillise them with hemlock? Again, how far does the combination of drugs having similar or different actions assist or impede their influence? Further, to what extent are these substances capable of controlling functional or organic diseases of the nerve centres? Without entering into other inquiries that such an investigation naturally suggests, I think no one will doubt the utility of having the questions I have put *positively answered*; and, for example, the fact ascertained whether, as has been suggested, tobacco really is an antidote to strychnine, or whether coffee, tea, or belladonna, are or are not antagonistic in their action to opium.

The instruments of precision now in the possession of the physiologist, enable him to determine many of these questions with the utmost exactitude. All that is required is time and labour properly directed, and a careful report of the results. But the experience acquired in directing the Edinburgh Mercurial Committee has satisfied me that it will be impossible to carry out a sustained effort in this or any other direction without means to remunerate the investigators. Not only would the actual expense for drugs, instruments, animals and their food and service, be considerable, but the unrequited and unacknowledged work in all Committees must be remunerated. It should be remembered that special skill, knowledge, and reputation, must necessarily be merged in that of the Committee as a whole, and thereby the great stimulus for individual research, that is reputation, cannot exert itself. Thus, while to attain perfect results, Committees composed of men who excel in different departments are absolutely essential, no return whatever for the time, skill, and knowledge expended can be hoped for without money. In every Committee, some men must work more than others. Certain manipulations are best performed by one or two persons. The chemist has to make laborious analyses and calculations. The physiologist has to perform delicate experiments, and, too often, unpleasant operations on living animals. The histologist has to sacrifice much time in microscopical observations, in drawing, and in putting up numerous preparations. The pharmacist has to furnish the purest drugs, which frequently have to be procured or prepared for the purpose. Even keeping the minutes of such a joint investigation, comparing them with each other, presiding over the discussions to which they give rise, and compiling from the whole a careful report, is in itself a great labour. No one, or at least few, can expect that in any given inquiry such varied researches and such trouble could be continuously carried on for years without remuneration, and with the certainty that the most able individual efforts will only throw honour on a general committee, the names of the members of which may be altogether unknown.

It is worthy of consideration, therefore, how far an annual grant out of the funds of this Association, say to the extent of £200, might not be made, for investigations of this nature. If the plan succeeded, and the funds admitted it, not one only, but several Committees, by and bye, might be engaged in chasing away medical difficulties. Our JOURNAL would be the necessary recipient of the results, and the means of diffusing the information arrived at. The annual meetings, at which some great medical truth evolved, or some error refuted, would receive increased interest, and thus all the different departments of our

Association would assist, in the most effectual way, the great objects of its foundation.

I cannot for a moment suppose that the Council of this Association, if urged by you to do so, would resist such an appeal; and it is to me certain, that, in this manner, real advantage to the profession, and a sure advance in our knowledge, might be annually effected without encroaching upon any other useful objects. Should you agree with me, I would urge that a very strong representation to that effect be sent from this Section to the Council. At any rate, I trust you will forgive my bringing this matter before you, feeling, as I do, that, in the present condition of medicine, what we require is not brilliant hypothesis, or ingenious speculation, but that solid genuine work to which we may look for furnishing us with truthful data. Let us resolve, by means of renewed experiments and careful observations with our modern appliances, to overcome the difficulties which surround us, and there can be little doubt we shall render the practice of our art worthy of the advanced state of science which now prevails.

ON AMPUTATION AT THE ANKLE-JOINT.*

By GEORGE H. B. MACLEOD, M.D., F.R.S.E.,

Professor of Surgery in the Andersonian University, Glasgow, and Surgeon to the Royal Infirmary and Lock Hospital; formerly Senior Surgeon to the General Hospital in Camp before Sebastopol.

THE great frequency of disease implicating the tarsus and the tibio-tarsal articulation makes amputation at that joint a very common operation. Various methods were resorted to by Vacca, Brasdor, Textor, Baudens, and others, before our great surgeon, Mr. Syme, perfected it in our own time; and, as experience has declared against amputation low in the leg, and also through the tarsus, it very often falls to the hospital surgeon to perform his operation. I had, during and after the Crimean war, considerable opportunities of judging of the success attending the *sus-malleolaire* amputation of Lenoir, and was strongly impressed with its drawbacks. The only valid argument in its favour is the small mortality which attends it; and, in that respect, circumstances (as during a military campaign) may occur in which it might be desirable to have recourse to it, notwithstanding its disadvantages of a painful ill-covered stump. It is very rarely the case, so far as I have observed, that disease of the ankle-joint implicates the bones of the leg so extensively as to render Syme's amputation inapplicable. Further, in those cases of disease of the tarsus, or in destruction of the foot, as by round shot or a wheel, in which it might be possible to amputate through the tarsus by Chopart's method, or by any of those plans by which the astragalus or os calcis are retained, experience has, I think, proved the greater advantage of disarticulating at the ankle, and retaining, if possible, the thick cushion of the heel as a covering for the ends of the leg-bones.

The opinions expressed in the following paper are founded on a series of fifty amputations at the ankle, performed during the last fifteen years, and with only one death. So low a mortality is quite exceptional, especially when it is considered that the majority of these operations were performed in public institutions, many of them in military service, and for most severe accidents. The only fatal case of the series was the last but one, where death followed an amputation performed for disease on the first of this month; the patient, a young lad, dying of purulent infection on the sixteenth day. No doubt several other patients suffered from severe complications, and were nearly lost; but ultimately all recovered from the operation, except the one referred to. A long run of success such as this is not unfrequently observed by surgeons in connexion with certain operations—when, without apparent cause, the tide turns, and in the end the average mortality becomes equalised to that experienced by others. It was doubtless in the belief of such a fact that, on a celebrated surgeon meeting a professional friend in the street, and boastfully telling him he had cut so many patients for stone without a death, he was met by his friend's remark that, "under the circumstances, he would be very sorry to be his next patient."

The patient who died under my charge was by no means in so unfavourable a condition for operation as many others who recovered without a bad symptom.

The mortality following amputation at the ankle has been variously stated. My friend M. Legouest, in his *Traité de Chirurgie de l'Armée*, puts it at 23.2 per cent. In our army, during the latter and best period of the Crimean war, it was 22.2 per cent. In the American

civil war, it was 13 per cent.; and the most favourable civil experience with which I am acquainted reckons it at 8 to 10 per cent. Weber, one of the most recent authorities, calls it 16 per cent.

Of the entire number of operations, thirty-two were by Syme's method; and it is chiefly on some details connected with this procedure that I wish to dwell in this paper. Eleven were by postero-internal flap, as first proposed by Jules Roux of Toulon, and ably advocated by my late much valued friend, Dr. Richard McKenzie of Edinburgh; three were by Pirogoff's method, or its modification by Sédillot; two were irregular, the soft parts being much destroyed, and the coverings taken from different sides of the joint; and two were by long anterior flap, after Baudens' plan, in cases in which the heel was destroyed.

Only fourteen of the patients were females, and the ages ranged from five to sixty-nine. In most of the cases, the patients varied in age from ten to eighteen; these operations were almost all necessitated by disease, as, with few exceptions, the traumatic cases occurred in adult males.

In one case (twice counted), a Pirogoff had, after an interval of two and a half years, to be converted into a Syme, from the os calcis becoming caries; and, in another patient (also counted as two), both feet were removed at different times for disease. Thus, for fifty amputations, there were but forty-eight patients.

Of the fifty cases, thirteen only were traumatic. Some of these occurred in the East during the war; the rest in civil practice in Glasgow. Ten of the thirteen operations were primary—*i. e.*, performed within twenty-four hours of the accident; the rest were had recourse to after suppuration had set in.

Of the thirty-seven operations undertaken for disease, three were performed on Jewish children during my residence in the Levant. Extensive caries of the tarsal bones, combined or not with disorganisation of the ankle-joint, was, in the great majority of cases, the cause necessitating the operation. In many, the disease was of long standing, and the general health grievously impaired. Two were for fungous disease of the sole of the foot; one was occasioned by the presence of a fibrous tumour, which had burst up the tarsus; one was to get quit of a defective stump resulting from a Chopart, in which the cicatrix was weak and irritable, and the heel retracted; two were for severe burns of the foot; and two for old and intractable ulcers of the dorsum, which had received only temporary benefit from plastic operations and other measures.

Such, then, is a short analysis of the series of cases I wish to submit to this distinguished assemblage, and on which I would further beg their permission to make a few remarks.

Having myself, then, had considerable experience of amputation at the ankle, and having, in the hands of others, seen a large number of cases, I have very decidedly been led to consider Syme's method as that which best fulfils the requirements of the vast majority of those cases in which we wish to remove the foot. It is not my wish at present to discuss the question of excision, either of the articulation, or of special bones in the neighbourhood of the joint; nor do I stop to insist that the cases I operated on were such as could not have been managed by any procedure short of amputation. I will simply say that I did not believe them capable of being *cured* by any other method than amputation, though it is possible that a certain measure of success might have been temporarily obtained from a less serious alternative.

In the series of cases alluded to, then, the great majority were examples of Syme's amputation; and my experience of it, as compared with other modes of operating, is so favourable, that in the future I intend to adhere to it in all cases fitted for its employment. That the proceeding of Roux, by internal flap, gives most admirable results, no one can, I think, deny, who has tried it fairly. In cases in which the outer surface of the heel is destroyed, that operation is capable of supplying a most excellent substitute for Syme's amputation. The covering is ample, the blood-supply good, the proceeding easy, the drainage excellent, and the position of the cicatrix perfectly satisfactory. The drawbacks to it are, that the blood-vessels and nerves are drawn across the end of the bones; and that, if sloughing attack the flaps, they cannot spare much of their extent without becoming deficient. The quadrilateral internal flap of Sédillot is very much the same thing, as is Soupart's amputation by internal flap.

Pirogoff's amputation, though ingenious, and theoretically superior to Syme's, is in reality much inferior to it. It is not at all fitted for cases of disease, as the portion of bone left becomes again in time the seat of disease. It certainly saves the dissection of the heel-flap, which many find so difficult; and the greater length and increased firmness of the stump, together with the non-interference with the blood-vessels of the heel, are made the most of by its partisans. In traumatic cases, it succeeds very well; but it is found that, as regards the fitting of a false foot, the presence of the heel-bone is a great drawback; and the back

* Read in the Surgical Section before the Annual Meeting of the British Medical Association in Leeds, July 1869.