be, would be avoided. Dr. Guérin has repeated an experiment which was performed by Blundell and others long ago—a sort of double transfusion. Two dogs are placed side by side, and the carotid artery of each animal connected by means of a tube with the jugular vein of the other. In this way, they obtain a common circulation, after the fashion of the Siamese twins. Although a most interesting experiment, this can only be regarded as one of the curiosities of transfusion.

It might happen that it would be impossible to get a supply of blood of any kind just when it was wanted. In such a case, it would be an advantage to have a substitute at hand. Some have recommended milk; others a very weak solution of ammonia. Saline solutions of various kinds have been used, also serum, or an imitation of it, particularly in cases of cholera; only, however, with partial success. The solution should be about the same specific gravity as that of the blood; and, according to Dr. Pavy, the salts of potash should never be used (*Guy's Hospital Reports*, 1868, page 6). Injecting plain water into the veins is dangerous, as it causes the blood-corpuscles to burst, and disorganises the blood. Further experiments are required to establish the safety and utility of saline solutions and other substitutes for blood in transfusion.

If the question be now asked, Which is the best and safest form of transfusion? I think it must be admitted that, in the present state of our knowledge, each of the four principal forms I have mentioned may be employed with an almost equal chance of success.*

For a more minute description of the various steps of the operation in the different modes of transfusion than I have given, and for fuller details of cases and descriptions of instruments, I must refer you to the writings of the authors whose names I have mentioned. It is one of the misfortunes of transfusion that, like a great many other therapeutic measures, its *modus operandi* is not quite understood. To place it on a sound scientific basis, we must know more than we do at present about the physiology and pathology of the blood. It will not do, however, to reject transfusion because we cannot explain the exact way in which it does good and saves life. If this rule were followed, we should have to give up nearly all our best remedies. It has generally been supposed that, when blood is injected into a vein of an almost moribund patient, for the purpose of resuscitation, it goes direct to the heart, which is stimulated to increased action by the presence of its natural stimulus. Hüter, however, states, and seems to have proved by experiment, that by venous transfusion the blood does not go direct to the heart, but becomes lost or diffused in the general venous system. He recommends arterial transfusion, but of a different kind to that already referred to—namely, sending blood to the heart through one of the arteries in the neighbourhood of that organ (*Archiv für Klin. Chir.*, vol. xii; *Centralblatt*, 1869, No. 25). There are many obvious objections to this plan, and the trials and experiments that have been made to illustrate it are not encouraging. I mention it here partly for the boldness and novelty of the suggestion, and partly to show that we are still somewhat in the dark about this part of the subject. If Hüter's theory regarding venous transfusion be correct, we get rid of one of the supposed dangers of transfusion—that is, over-distension of the right side of the heart. The dread of this danger has been pretty generally felt, and to avoid it we are cautioned to inject the blood slowly and in moderate quantities. It has been a matter of surprise with many, that the quantity of blood used in transfusion is so small, compared with the quantity lost; but, when a patient is reduced by loss of blood to the lowest ebb, a few ounces would be sufficient to turn the scale either way—by further abstraction, towards death; by addition, towards a restoration of the equilibrium of the circulation. Whether the latter is brought about by the mechanical effect of adding something to the bulk of the blood in the body, or by stimulating the heart and vessels, are points that can only be settled by experiment.

There are several other points connected with the subject surrounded with difficulties. Indeed, transfusion, although somewhat ancient, may still be said to be in its infancy. By many, it is altogether regarded with scepticism. It is said that those who recover after transfusion would recover just the same without it. One writer says that "transfusion is a sort of Will-o'-the-wisp of science, holding out the most brilliant prospects, which never seem to be realised." There is, no doubt, much in the history of transfusion to justify scepticism as to its real value; but there is also no doubt that it has had its triumphs as well as failures. A large number of well authenticated cases of success are on record—quite enough to encourage us to thoroughly investigate the subject, to endeavour to remove all doubt, and thus, if possible, to secure to the profession an effective means of saving life when all other

means fail; for it must always be remembered that transfusion is only to be used as a last resource. In this important work, the Committee on Transfusion invite the co-operation of all those members of the British Medical Association who take an interest in the subject. The points calling for further inquiry appear to the Committee to be as follows.

1. The exact time that human blood takes to coagulate when drawn from a vein. (This is suggested from the different results arrived at by various authors, and from the fact that Mr. Higginson has found but little difficulty in transfusing blood in its natural state.)
2. Does blood, when injected into a vein, go direct to the heart, or does it become lost or diffused in the general venous system?
3. The effects, immediate and remote, of transfusion with blood kept in a state of non-coagulation by means of phosphate of soda, as recommended by Dr. Hicks.
4. The effects, immediate and remote, of transfusion with blood containing ammonia, as recommended by Dr. Richardson.
5. The effects of transfusion with milk.
6. The effects, immediate and remote, of transfusion with defibrinated blood.
7. The microscopic examination of defibrinated blood.
8. The effects of transfusion on one of the lower animals by the immediate method, on Dr. Aveling's plan, from an animal of the same species.
9. Transfusion with blood in its natural condition, as in Mr. Higginson's cases.
10. Further inquiries to show whether the blood of the lamb or of any other animal might be introduced with impunity into the human system.
11. Transfusion with defibrinated blood, with the addition of ammonia or phosphate of soda, or both combined.
12. Transfusion with saline solutions.
13. Further experiments to show how long an animal deprived of food would live on blood transfused daily.

REMOVAL OF A LOOSE CARTILAGE FROM A KNEE-JOINT UNDER THE ANTISEPTIC TREATMENT.

By GEORGE LICHTENBERG, M.D.,
Surgeon to the German Hospital and Tottenham Training Hospital.

THE removal of loose cartilages in joints by operation has been, and is even now, considered by most surgeons as a procedure connected with more or less risk; and one which, according to their opinion, should only be resorted to when a palliative treatment previously tried has utterly failed. And if we examine minutely the statistics of this operation, we must confess that there is truth in this assertion; and no surgeon is to be blamed if he undertake the operation with a certain feeling of diffidence as to the ultimate result. Cases, however, will occur which imperatively demand the removal of loose cartilage by operation, which operation, great as the risk may be, is not comparable to the suffering and even danger in which the patient is placed by allowing it to remain; and here arises the question, What kind of operation should be performed? Is the loose cartilage to be removed by the direct incision or by the subcutaneous one? We all know that, according to present experience by the direct method, the risk to life is greater than by the subcutaneous one; while the latter operation is more difficult to perform, and therefore has resulted in more failures. But, speaking of the failure of the subcutaneous method, it would be neglect on my part not to mention the very favourable results which Mr. Square, Surgeon to the South Devon and East Cornwall Hospital in Plymouth, has achieved by his subcutaneous incision. Mr. Square refers to no fewer than twenty-four cases operated upon either by himself or by others according to his particular subcutaneous method; not one failure among them has taken place, and all cases, without exception, have made a rapid recovery. Having carefully read the different cases, I certainly should not hesitate to follow Mr. Square's advice, and not attempt to employ any other method than the subcutaneous incision for the removal of loose cartilages in joints, had not Professor Lister produced such a thorough revolution in the treatment of wounds by his antiseptic treatment. Although it lies not in the compass of these short remarks to enlarge upon this really grand achievement (I am not entering into the details), I may be allowed here to mention that several years' experience has thoroughly convinced me that by this treatment conservative surgery has gained a triumph which, though at present I regret to say it is not fully appreciated, will in the future bear bountiful fruit, and

* I ought, perhaps, to state that in this paper I am only giving my own opinions. No formal or definite conclusions have as yet been arrived at by the Committee.

prove not only a blessing to the profession, but principally to suffering mankind. My past experience, therefore, emboldened me also to use, in the following case, for the removal of a loose cartilage from the knee-joint, the direct incision instead of the subcutaneous one, the account of which I herewith relate.

Mr. L., of Ilfracombe, the subject of the present paper, aged 21, had four years ago a severe fall on his left knee. An abscess formed outside; this was laid open, and remained so for one month before it entirely healed up. Two years after this accident, the patient felt occasionally severe pain round the joint, particularly in walking downhill, and he fancied he heard now and then a snapping noise in the knee-joint. Last year an attack of synovitis set in, and made it necessary for the patient to lay up for six weeks; after this he sometimes felt suddenly excruciating pain in the joint after over-exertion, and about that time he frequently himself felt a foreign body moving about in the joint, which occasionally he could lay hold of with his own fingers, mostly on the inner, but now and then also on the outer, side of the joint. The symptoms were at times most distressing; and, as every palliative treatment had been of no avail, the patient wished anxiously to submit to a radical cure in order to get rid of this severe suffering, and for this purpose he was admitted on May 2nd into the Sanatorium of the German Hospital. On the following day, the patient, after having himself got hold of the foreign body with great dexterity near the inner condyle, was placed on the bed, and, the limb having been put in semi-flexed position, Dr. Nieden was enabled to hold the foreign body (which, felt through the integument, was seemingly of the size of a plum-stone) firmly between his thumb and index finger). I then made a longitudinal incision about two inches in length over the foreign body, during which time a continual spray of carbolic acid solution, according to Professor Lister's method, was directed on it; and then, gently cutting on and limiting gradually the length of the incision as I cut towards the capsule, I opened this part so far that, by a slight pressure of Dr. Nieden's fingers, the body jumped out of the joint. The wound was immediately united by five carbolised catgut sutures, and dressed likewise according to Professor Lister's method with the carbolised gauze; the whole limb was then placed on a straight splint and rather tightly bandaged, as a little oozing of blood had been going on between the interstice of some sutures, and for this reason also ice was applied to the knee. During the following night, the patient complained of much pain, due, however, merely to the tightness of the bandage, the pain leaving directly the bandage was loosened. Twenty-four hours after the operation I looked at the wound, the carbolic acid spray of course being administered during the inspection; the entire wound was glued together, and only between the two lower sutures a small superficial interstice was discernible, due very likely to the slight oozing of blood which still had continued after the operation, and which had prevented a perfect union through the entire thickness of the skin. The neighbourhood of the wound, however, was not in the least reddened; the whole knee had a natural appearance, and was nowhere painful to the touch; the general health of the patient was perfect. On the second day I did not find it necessary to undo the bandage, the temperature being normal and everything going on most favourably; on the third day I removed three sutures; and on the fourth the last ones were taken out, when the wound itself was found to be perfectly united. One circumstance I wish to mention at this point: the carbolised catgut sutures had decidedly irritated the parts more than I had anticipated; and therefore, as in simply incised wounds we shall seldom require to leave the sutures longer in than at the utmost from three to five days, I think the finest Chinese silk will be by far preferable to the carbolised catgut sutures. I have always used this kind of silk in my rhinoplastic operations, and found it answer exceedingly well; the irritation it produces is very limited, and has never interfered with the ultimate object I had in view, viz., union by first intention. Although now on the fourth day the patient could be pronounced to be perfectly well, I did not venture to allow him to get up before the end of the first week, when I considered it safe to do so. The patient remained in the hospital up to May 17th, when he was discharged. I therefore had ample opportunity of watching the result, which proved satisfactory in every way.

Although I am well aware that the direct incision for the removal of loose cartilages in joints has often been successful without having had recourse to Professor Lister's treatment, yet no surgeon can and will deny that the fact of opening a joint by an incision is of itself always a procedure connected with more or less danger; while, on the other side, I firmly believe that we shall soon consider the simple opening of a joint under Lister's method for the removal of a loose cartilage as an operation connected with no more danger than a simple incision into other less important parts of the body.

In concluding these remarks, let me express the hope that the reader

may not think this case alone has convinced me of this fact; there are other cases on record treated in a similar way also with a favourable result; and Professor Lister's case (*Glasgow Medical Journal*, November 1868) is particularly instructive. I merely wish to say that this case is for me a new and strong proof of the invaluable help we have at our disposal by the employment of the antiseptic treatment.

THE DEPRESSANTS OF THE CIRCULATION AND THEIR USE.*

By J. MILNER FOTHERGILL, M.D., M.R.C.P.

[Continued from p. 9 of last number.]

THE most important matter, however, connected with our subject was the introduction of a vegetable depressant agent into the treatment of pyretic conditions. In 1844, Dr. Fleming published his Inaugural Thesis for graduation at the University of Edinburgh, entitled "An Inquiry into the Medicinal Properties of the *Aconitum Napellus*," and struck the key note of a new treatment. Certainly aconite had been studied before, so early indeed as by Störck, in 1763; but Fleming made the subject his own. His conclusions were, that aconite is a powerful antiphlogistic, and that it is calculated to be of great service in all cases where there is inordinate activity of the circulation. The data on which these conclusions are founded were, the lowering of the pulse, its alteration of character, the effect upon the fever along with the action upon the cutaneous vessels, produced by the administration of aconite. The reduction in the frequency of the pulse was marked, amounting even to thirty-six or forty beats per minute; a reduction which must exercise a marked influence upon the circulation and the chemical interchanges. The cutaneous exhalation was increased with much itching, and not unfrequently an eruption of vesicles indicated its powerful action upon the skin. Thus we see that aconite acts in a manner very similar indeed to antimony. The immediate dangers in the use of both are much the same, but the after effects of aconite are less objectionable. The use of aconite as an antipyretic agent has maintained its position, and indeed is gaining ground. Its empirical use has been the sheet-anchor of homœopathic practice in pyrexia; and though we cannot credit that body of irregular practitioners with a very scientific administration of such an agent, we cannot withhold from them their just claim to have demonstrated on a large scale the practical value of aconite in the treatment of pyretic conditions, especially in inflammatory and simple febrile affections.

In 1872, Ringer, in his well known work on *Therapeutics*, writes in a manner which vindicates the expectations of Fleming. He says, "Aconite is to be most esteemed for its power, little less than marvellous, of controlling inflammation and subduing the accompanying fever. It will sometimes at once cut short an inflammation. It will not remove the products of inflammation, but by controlling the inflammation it prevents their formation, so saving the tissues from further injury. When given in the earliest stages of the commoner and milder pyrexia, the skin, dry, hot, and burning, becomes in a few hours comfortably moist; and in a little time longer is bathed in a profuse perspiration, often to such an extent that drops of sweat run down the face and chest. With the sweating comes speedy relief from many of the distressing sensations, as restlessness, chilliness, heat, and dryness of the skin, aching pains and stiffness; and at the same time the quickness of the pulse becomes much reduced in frequency, and in a period varying from twenty-four to forty-eight hours, both pulse and temperature reach their natural state."

Finally, Ringer sums up, "It has been shown that aconite lessens the rapidity of the circulation. It may, therefore, be used in all cases where it is needful to subdue vascular excitement; in fact it may be given in precisely those cases which were formerly treated by bleeding."

Thus we see that after thirty years, and those years too during a most sceptical era, a time almost of eclipse as regards faith in therapeutics, the predictions of Fleming have been endorsed, and the line of treatment inaugurated by him developed into a scheme which includes many of our new remedial agents, and shadows out the practice of the future.

The depression of the heart, and with it the lowering of the circulation and the chemical changes, on the one hand, and the dilatation of the peripheral vascular area on the other, including the consequences of cutaneous exhalation, are the two grand factors in the modern treatment of pyretic conditions, co-equal with the direct use of cold.

* Read before the Medical Society of London.