possible to use this method in man—and I shall be convinced of the impossibility only when I see it fail—there would be a great increase in the percentage of recoveries after attempted resuscitation in man.

Other Factors requiring Attention in Resuscitation.

In experiments which I performed in the latter part of 1915 I found that there were certain other points which have an important influence on the speed and completeness of resuscitation, and which I may take this opportunity of pointing out.

In the first place, when natural respiration begins (after arrest of the heart, massage, etc.) the respirations to begin with are slow. When the tube through which artificial respiration has been performed is allowed to remain in the trachea they tend to continue slow. I found on looking over the data of experiments that when this tube was removed from the trachea there was invariably a sudden and pronounced increase in the rate of respiration. This, no doubt, was partly due to increase in the available lumen of the trachea consequent upon the removal of the tube. But it seemed greater than could be accounted for by that alone, so that it is possible that the presence of the tube, from reflex irritation of the air passage, tends to slow the natural respiration. Whatever the theoretical explanation the practical conclusion is clear: As soon as natural respiration has become established the tracheal tube should be removed.

In the second place, there is one danger point in the course of resuscitation which I believe is of great importance and which has not to my knowledge been noted before. After considerable experience in resuscitating animals killed by chloroform, I found that I could anticipate with some confidence that I would be able to resuscitate an animal, by the methods described, to the stage that the heart beats and natural respiration were resumed. But here an unexpected difficulty arose, because I found repeatedly, but not invariably, that after the first gasp or two of natural respiration the heart, hitherto beating well, suddenly ceased beating and could not be restored even by massage. I had this experience with three rabbits in succession in one day. This was distinctly disappointing. In casting about for an explanation the only one that suggested itself—and it is not entirely satisfactory—is this: After stoppage of the circulation for several minutes the central nervous system is paralysed. The centres recover when the circulation is resumed and the blood aerated. When the respiratory centre recovers, probably other medullary centres recover somewhere about the same time. It is conceivable that when the respiratory centre commences to function the vagus centre commences at the same time or only just later, and that after its prolonged abeyance of function there is a sudden and excessive discharge of vagal impulse which stops the heart. Acting on this hypothesis, whether it be correct or not, in subsequent experiments I gave an intravenous injection of atropine to prevent vagal inhibition, and after that I did not experience this type of cardiac failure. The result may, as so often happens, have been due to an increase in skill in performing the experiments of which I was not cognisant. But in any case, seeing that atropine can do no harm, I think it would be advisable provisionally to adopt this safeguard in resuscitation in man.

Lastly, in all resuscitation experiments great care must be taken to maintain the body temperature. After stoppage of the circulation and temporary paralysis of the whole central nervous system, the animal does not easily recover its power of heat regulation.

SUMMARY.

To sum up, I would venture to lay down the following directions for resuscitation in cardiac arrest in man or animals, whether from chloroform anaesthesia or from any other condition in which cardiac arrest is "accidental."

If the heart has stopped, artificial respiration should be resorted to immediately by the usual methods. If, at the end of three or four minutes, no pulse can be felt, and especially if no heart beats can be heard on auscultation, adrenalin should be injected into an external jugular vein, by an assistant if available; if no assistant be available, I believe the gain from adrenalin more than compensates for the loss of time, provided the materials are at hand, as they ought to be. In the meantime, or subsequently,

as the case may be, the abdomen should be opened high up, one hand inserted in the opening, passed up over the left lobe of the liver, and the heart felt for above the diaphragm. The heart should then be massaged intermittently. I endorse all that Mr. Fisher says in regard to not relying upon artificial respiration alone and in regard to boldness of procedure if the heart has stopped. When it has stopped there can be no further risk. As soon as spontaneous beats of the heart, however feeble, are felt massage should be interrupted for several beats to allow the contractions to establish themselves; otherwise, continuous massage may abolish them again. The heart should then be massaged again for a short time, when the beats will probably improve. When the heart has begun beating it is safer to use too little than too much massage. The hand should not be removed from the abdomen till the heart is beating well. When the heart continues to beat regularly and strongly an intravenous injection of atropine should be given. Artificial respiration must be kept up continuously, also after the heart begins beating. After five minutes it may be interrupted to see if natural respiration will begin, but not for long. No harm is done by keeping up artificial respiration, even if it inhibits the natural respirations, whereas, on the other hand, it is not safe to interrupt artificial respiration very long, if natural respiration does not commence.

If it be found that positive artificial respiration can be used in man by, for example, a Brodie's pump of larger size than that used in the physiological laboratory, the problem of resuscitation will be greatly facilitated.

Natural respiration may be expected to begin in five to ten minutes after the heart begins to beat again, but may be delayed for thirty minutes.

During the manipulations, and afterwards if these procedures be successful, care must be taken to maintain the body temperature.

All attempts, successful or not, at resuscitation after cardiac arrest in man should in the present state of knowledge be recorded. When it is possible—and it cannot well be possible unless assistance is available—accurate notes should be kept not merely of the sequence of events, but of the actual times of occurrence.

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THE PRACTICAL GAINS OF CLINICAL CARDIOLOGY SINCE 1900.*

BY

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DURING the last twenty years a great deal of interesting and valuable work has been done in medical cardiology. What I propose is briefly to summarize the practical gains which we, as clinicians, have derived from it. In so limiting my field, I am forced to exclude from it much that is interesting, as belonging to physiology rather than to medicine. But I hope that what my paper thus loses in attractiveness it may atone for by usefulness.

CARDIOLOGY IN 1900.

I need hardly remind an audience well read in the literature of twenty years ago that all the factors involved in cardiac failure were then clearly recognized; that no one called in question so obvious a truism as that the cardiac muscle was of predominant importance; or doubted the danger of such mechanical impediments as a leaking aortic orifice or a contracting mitral. No thoughtful person then referred to "back pressure" as a cause of heart failure, but only as a consequence and complication of it. Gaskell's brilliant work had shown that the heart muscle was more independent of nervous control than had previously been imagined, and Kent and His had demonstrated the path along which the normal stimuli of contraction travelled. The effects of vascular disease, in and

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outside of the heart, were then fully recognized, as were also the injurious consequences of anaemia. The influence of the endocrine glands had begun to be investigated. Poisons, organic and inorganic, of bacterial or other origin, were attracting the attention they deserved. Overstress had been discussed. Malformations had been elaborately described. The infections of the endocardium were being enumerated. It was fully recognized that many irregularities of the pulse were of little consequence, and that some murmurs mattered, whilst others did not. Thus murmurs were divided into "functional" and "organic," and amongst these latter the decided differences in seriousness were carefully indicated; the minor significance of irregular pulse in childhood had not been overlooked, intermissions were usually held to need no treatment, and "delirium cordis" was regarded as a special indication for digitalis—although it must be noted that in mitral stenosis the continued administration of the drug was often found badly borne. Our excellent existing classification of cardiac diseases had been already built up and almost all our present remedies were in familiar use.

OUR GAINS SINCE.

I will divide my material into the following sections: Treatment, prognosis, diagnosis, physical signs (detached for convenience), and etiology.

TREATMENT.

Dr. Caton has added an important chapter to the subject of *preventive medicine* by showing how we may prevent many cases of incipient rheumatic endocarditis from passing into permanent valvular disease by prolonged rest in bed, the administration of sodium iodide and the application of blisters.

Dr. Arthur Goulston, by his ingenious use of *cane sugar*, has given us a valuable aid in dealing with chronic heart failure. It is noteworthy that beet sugar is without effect and that the cane sugar must not be cooked.

The intravenous use of *strophanthin* in urgent cases has been found of unmistakable value. *Theocin sodium acetate* has proved its usefulness in dropsy which has not yielded to other diuretics. Sir Lauder Brunton's insistence on the importance of *morphine* in the distressing *dyspnoea* of late heart disease has promoted its general use in that condition, and the discovery of the immediate value of its hypodermic injection in *acute pulmonary oedema* (which occasionally occurs in cardiac patients) has put a valuable weapon in our hands. In "soldier's heart" *graduated exercise* has expedited the generally slow recovery.

Strychnine has been unduly discredited. Surgical shock, in which it has been rightly replaced by *pituirrin*, is a different condition from ordinary heart failure, and although there are those who prefer injections of *camphor* in cardiac breakdown, we shall do wisely if we hesitate to discard from our therapeutics a remedy which the sober judgement of competent clinicians has so long relied on.

The use of *salvarsan* in cardiac syphilis appears to have special risks and, if used at all, it should probably be given in smaller doses than ordinary.

In streptococcal endocarditis (the most frequent variety) we have learnt that whereas *anti-streptococcal* serum occasionally succeeds, streptococcal vaccines almost invariably fail. *Standardized digitalis* is a valuable advance.

Three surgical procedures require medical notice. Brauer's operation of "*cardiolysis*" is of service where a heart is gravely hampered by adhesions of the pericardium to the chest wall. *Direct massage of the heart* through the diaphragm, in cases of heart failure under operation, has been the means of restoring life. *Incision of the pericardium* seems at last to be recognized as a safer proceeding than the older paracentesis.

PROGNOSIS.

The prognosis of *cardiac syphilis* has received a good deal of attention, and would seem to be very unfavourable. Dr. Mitchell Bruce has stated that only 50 per cent. of the cases improve at all, and that 20 per cent. die within a few years. Patients with syphilitic aortic disease, he finds, generally die within a little over five years. Adler and Weber, however, believe that early treatment may cure. All authorities insist on prompt and vigorous measures.

Perhaps now that fuller means of diagnosis are at our disposal, as in the Wassermann reaction, earlier recognition may lead to a better outlook.

In 1898 a distinguished authority, whose experience was chiefly derived from a great industrial area, wrote that cases of *aortic regurgitation* had never, in his recollection, survived for fifteen years. This was so different from Devonshire experience, and from experience in other non-industrial centres, that the discrepancy suggested that possibly great local differences in general expectation of life might be showing themselves incidentally in cardiac patients. Accordingly, in 1909, I examined the proportion of the population which survived to 75 and over in all the registration districts of England. The result was remarkable. In Leeds, where the shorter survivals alluded to were observed, only 6 per cent. of the population reached those ages, in Manchester only 2 per cent., whereas in some Devonshire districts, such as Axminster and Crediton, 25 per cent. did so. Since then a number of observers have repeated the older observation of cases of aortic regurgitation surviving for twenty-five and even thirty years; and a case of combined aortic and mitral regurgitation, which survived for over thirty years, had been for nearly twenty-eight years under my own observation in Exeter. This fact of *local prognosis* is one which, I would suggest, needs further attention.

DIAGNOSIS.

A number of new instruments have been added to our equipment. The *sphygmomanometer*, introduced a little before 1900, has only come into general use since that time, and Dr. Batty Shaw has pointed out its value in distinguishing secondary from primary cardiac disturbance.

The *x rays*, with or without *orthodiagraphy*, are necessarily becoming more and more a help in the examination of our cases. Especially useful have they been in establishing or correcting the diagnosis of aortic aneurysm. As I shall presently point out, they afford valuable assistance in verifying the findings of digital percussion.

Mackenzie's *polygraph* has been an important means of distinguishing—and of giving us other means to distinguish—the different varieties of pulse irregularity. To this I shall return.

Einthoven's *electrocardiograph* promises to assist in the unravelment of certain problems. As yet, however, it is difficult to see that it has proved of much practical use to the clinician.

Leyton's *differential stethoscope* may enable us to compare clinically the relative intensity of a patient's heart sounds at different periods and under different conditions.

Even the older modes of auscultation admit of revision and comparison. Thus I have recently compared the old-fashioned "single" straight wooden stethoscope with its more popular successor, the ordinary "double" or binaural flexible stethoscope, and both of them with the almost discarded method of "immediate auscultation"—that is, of listening with the ear laid directly on the chest wall with only some thin fabric intervening. I have found that whereas a slight mitral regurgitant murmur is more loudly and distinctly heard with the double stethoscope than with the single, a slight aortic regurgitant murmur which may be missed with the double stethoscope can be heard clearly with the single; also that an aortic regurgitant murmur may be occasionally inaudible to both these stethoscopes and yet be made out unmistakably by immediate auscultation. This applies to both the upright and recumbent postures.

Sir William Osler's *spots* in malignant endocarditis were, I believe, discovered within the period we are discussing.

The *oculo-cardiac reflex*—which, by the way, may be lost in locomotor ataxy—may furnish us with a means of checking a heart rate just too rapid for exact examination.

In the absence of mitral stenosis or of recent vigorous exertion it appears to me that the presence of a *distantly heard first heart sound*—by which I mean "heard without a stethoscope at a distance greater than the length of the ordinary straight wooden stethoscope, that is, about 7 in."—is strongly suggestive of hyperthyroidism.

I think there can be no doubt that whilst the importance of symptoms has been rightly insisted on for many years and directions have long been given for examining hearts

after some brief exertion, the routine care, now in general use, of inquiring into evidence of cardiac failure from the degree of exertion needed to produce distress, as also the present routine of re-examining hearts after exertion, is mainly owing to the insistent teaching of Mackenzie and his school.

PHYSICAL SIGNS.

I. On Pulse Abnormalities.

We are chiefly indebted to Sir James Mackenzie and Dr. T. Lewis for the brilliant investigation of pulse abnormalities by graphic methods. The subject is most interesting and has engaged many able workers for several years past. If one cannot admit all the clinical claims which enthusiasts have made for their scientific results, it is not that one does not recognize the general debt we owe to their skill and labour.

Total Arrhythmia.—Dr. Lewis has made the interesting discovery that this well-known accompaniment of cardiac failure is caused by fibrillation of the auricle, a condition in which the auricular fibres contract in such separate and inco-ordinate fashion that the auricle as a whole stands still. As a scientific explanation of the auricular inaction and the coincident total arrhythmia of the ventricle, this is an important piece of work. But clinically it has, so far, had very little effect. As regards diagnosis, the condition underlying it has still to be determined by other and older methods; in prognosis, a condition which may end in a few weeks, or may last for years with little apparent detriment, cannot be regarded as very helpful; whilst in treatment it is difficult to see how its recognition has affected us at all.

Heart-block.—Perhaps no chapter in cardiology is more fascinating than that which, beginning with Gaskell's researches, has included such fine work as has been done by Kent and His, Tawara, Erlanger, Keith, Mackenzie, and Lewis—to mention only a few. In the future we may hope for clinical results, such as will aid us in the prognosis and treatment of the condition evidenced by the Stokes-Adams syndrome, but at present we stand as we did.

Intermissions.—Except that the premature dwarf contractions, which our stethoscopes alone formerly detected, are now called "extra-systoles," we have made no practical progress in our knowledge of these. But it is a gain to have recognized that intermissions may be due to pauses of the whole heart or to occasional heart-block.

Paroxysmal Tachycardia.—We have learnt that one variety, due to "auricular flutter," is amenable to large doses of digitalis, which convert the flutter into auricular fibrillation, which in turn, on discontinuance of the drug, reverts to the normal rhythm. In other varieties we seem to be much where we were.

Pulsus Alternans.—This sign, originally described by Traube, has lately been assigned a most serious significance. Yet so able an observer as Sir William Broadbent wrote in 1897, "I have learnt not to attach serious importance to the regular alternation of a strong and weak beat, so long as there is no great frequency with it." Carver has lately recorded its occurrence in persons with no apparent cardiac lesion, and stated that it may disappear, leaving the patient apparently none the worse. Which view of its significance is correct?

Sinus Arrhythmia.—Here we are clearly indebted to the graphic methods. They have made precise the meaning and diagnosis of this usually harmless form of irregularity. Sinus arrhythmia is a vagal irregularity, its rhythm starting, as it normally should do, from the sino-auricular node and involving the whole heart. Its pulse rate rarely exceeds 100, and more usually is as low as 60 or 50, whilst anything which quickens the pulse, such as exertion or rise of temperature, removes the arrhythmia. Atropine (which has no effect on auricular fibrillation), by its action on the vagus, removes it. With the three exceptions mentioned below it needs no treatment, not even change of habits. It is the common irregularity of childhood up to 10, a period in which premature contractions are unusual, heart-block rare, and auricular fibrillation very rare. It is less common in adults. When, as is the rule, there is an obvious relation to respiration, ordinary or deep, says Dr. Lewis, its nature is evident; a gradual waxing and waning of rate is always highly suggestive, if not conclusive. The radial beats and apex beats correspond; the heart sounds show no trace of extra-systoles. The pulse beats are full and equal in amplitude.

There are, however, three varieties in which there is no association between the pulse irregularity and phases of respiration—namely: (1) Sudden prolonged pause of the heart beat, usually with syncope—a rare condition. (2) Relatively abrupt slowing of the whole heart to 20 or 40 beats a minute with fall of blood pressure, which Lewis believes is the commonest cause of fainting attacks in adults. (3) Variation of pulse rate in phases lasting ten, fifteen, or more seconds—uncommon, sometimes following large doses of digitalis or its congeners, sometimes apart from these. All these are due to vagal influence and are removed by atropine—a valuable therapeutic indication.

It is noteworthy that Dr. Watson-Williams has observed that well marked respiratory undulations in a boy's pulse "implied, if anything, that he would make a good sportsman."

II. Influence of Respiration on Murmurs.

It has long been taught that systolic heart murmurs which are obliterated by a deeply-held breath should be suspected of being exocardial (so-called "*respiratory murmurs*"), that is, negligible so far as the heart is concerned. I have, however, recently found that a deep-held inspiration is capable sometimes of obliterating the soft diastolic murmur of slight aortic regurgitation in young adults. The chief significance of this lies in the doubt it casts on the present interpretation of systolic "*respiratory*" murmurs.

III. The Influence of Posture.

The advice to examine heart patients both erect and recumbent is old, yet it was rarely followed until recently. "Scientific" accounts of cases, twenty years ago, were constantly being published with no note of the posture at the time of observation. No ordinary textbook that I know of prior to 1906 (when I published what I believe were the first observations on the effect of posture on the normal cardiac sounds and on the normal cardiac dullness), and few since, have said a word about posture in percussion—Broadbent's book does not once allude to it! Only Dr. Blackhall-Morison, in his work on *Heart Failure* (1897), gave a figure illustrating the change in the dullness in a case of mitral disease (the first such figure, I believe, ever published), and added this admirable statement: "Whatever method be pursued in determining the dimensions of the heart by percussion, it is of the first importance that the posture of the patient should, so far as possible, be exactly the same on successive occasions when this point is under observation."

For the following additional points I have made myself responsible:

1. That the *real width of the heart* is most closely determinable by percussion when the patient is erect.
2. That in cases of enlarged heart the *difference in the dullness* produced by change of posture from the recumbent to the erect may be relatively great; three examples will suffice:

		Erect.		Recumbent.
Case A.	...	9 in.	...	5 in.
Case B.	...	8½ in.	...	4½ in.
Case C.	...	8 in.	...	5 in.

These show what confusion may arise from examining the patient in different positions on different occasions.

3. That this difference is chiefly due to the change in the *rightward edge of dullness*.

These three facts have recently been made abundantly evident by x-ray photographs, in which the lateral limits of dullness, previously percussed out in the two positions, have been marked by pieces of metal attached to the chest wall.

4. That the *presystolic murmur of mitral stenosis* is sometimes only audible in the erect position.

5. That the *systolic murmur of aortic stenosis* is sometimes only audible in the recumbent position.

6. That an *accentuated and reduplicated second sound* over the pulmonary artery is best heard in recumbency and that sometimes the reduplication is only evident in recumbency.

7. That the *cavity of the chest* is usually appreciably deeper antero-posteriorly in the upright than in the recumbent position. Dr. Blackhall-Morison's luminous remark, that a much-enlarged heart has to be considered as a form of intrathoracic tumour, lends importance to

this fact. Also, from a consideration of the differing influence of posture on the best-known heart murmurs together with this effect on chest depth, it has been possible for the first time (1902) to supply a rational explanation of the influence of posture on heart murmurs—namely, that it is due, in part to the effect of gravity on the intracardiac currents, and in part to the change in the chest depth. It is necessary to recall this explanation, as it has recently been misquoted.

8. That what I have called *distantly-heard heart sounds* are more distantly heard in recumbency. Thus in a case of Graves's disease, the first sound was audible 5 ft. away in the recumbent position, but only about 2 ft. away in the erect.

9. That from all the above considerations it is clear that no important cardiac case is properly examined or described in which posture is left out of account.

10. That in certain cases of *cancer*, especially cancer of the digestive organs and in their later stages, the recumbent cardiac dullness (R. C. D.) is remarkably diminished (to under 2 in., or more typically to about 1 in.), or even altogether lost. This "*cardiac sign in cancer*" I have found of great value in the diagnosis of obscure cases. In one series of fifty cases there was only 6 per cent. of error.

Another, and a very interesting effect of change of posture, is that described by Dr. Blackhall-Morison, which he considers valuable as a warning of grave heart failure. It is that the crepitations of pulmonary congestion, if heard at one lung base, can be transferred at once to the base of the other lung by turning the patient on to the opposite side from that on which they were first audible.

Dr. Batty-Shaw has used change of posture to distinguish the *pulmo-arterial diastolic murmur*, due to high pulmonary pressure. In the erect posture this murmur, being heard alone, might be taken for the murmur of aortic regurgitation. "Recumbency will, however, change the whole picture and reveal the presence of an obstructive or of a double mitral murmur."

ETIOLOGY.

The most important etiological study has been that of the so-called "*soldier's heart*"—the "*irritable heart*" of Da Costa in the American Civil War—for which the late war has furnished a superabundant mass of material. In its most striking form the signs of structural heart disease are absent, but the patient is short of breath on exertion, easily exhausted, and complains of palpitation, precordial pain, giddiness, and more rarely of actual fainting; exertion, excitement, and sometimes change of posture, unduly hurry his pulse, and this hurried pulse is unduly long in returning to its normal.

In other cases there may be slight cardiac enlargement, with soft systolic murmurs, sometimes perhaps due to relative incompetence, or being sometimes "haemic" or "respiratory" in character. Now and then the syndrome may be superposed on actual structural heart disease.

At first the condition was attributed to physical overstrain, and especially the overstrain of men unaccustomed to strenuous exertion suddenly submitted to arduous military training, aggravated perhaps by excessive cigarette smoking. But as the war went on it was noticed to follow on various infective diseases, on mental shock, and on exposure to great heat of climate. Cardiac drugs were found useless, and rest was then thought to be the only remedy. The cases were, as Da Costa had recorded, exceedingly obstinate. Presently the disease appeared amongst the women undergoing anxiety at home, and its psychical aspects came more clearly into view.

Present-day opinion may perhaps be briefly thus stated. It is a condition of neurasthenia in which the heart is especially affected, depending in part on inherited physical and psychical defects, and more immediately on infective toxæmia (as from enteric, malaria, trench fever, amoebic dysentery, tuberculosis, influenza), or on exposure to high temperatures, the cumulative strain of fatigue with insufficient food and sleep, or great nervous shock or stress, associated sometimes with endocrine disturbance (thyroid or adrenal), the psychical factor becoming evidently more and more important as observations accumulate. Rest, though useful, indeed necessary, at first, has later been superseded, after a sufficient interval, by carefully graduated exercises, carried on amidst the curative

psychical atmosphere of a busy and successful treatment centre. For such "vagal" symptoms as fainting, with slow, feeble pulse, atropine has been used with advantage. But the disease remains exceedingly refractory. Many cases are still uncured, and many have only recovered after many months. The majority may, however, be perhaps regarded as recovered.

Speaking with much diffidence from a limited personal experience I cannot but think there are possibly several types still not disentangled. For I have seen cases which seemed obviously associated with hyperthyroidism, and others equally suggestive of toxæmia with no obvious endocrine change—one such I afterwards heard was fatal from syncope.

For the rest, still uncured, we may well continue to hope; but Da Costa's experience that 28 out of 200 afterwards developed "cardiac hypertrophy," and the record of some writers on the Balkan war cases (quoted by Dr. Carey Coombs) that "premature arterio-sclerosis" (probably the same condition as Da Costa's) commonly occurred as a sequel, must be borne in mind. It will be important to determine, if we can, what particular causes have been connected with the cases in which such a sequel shows itself.

In conclusion, gentlemen, I hope that the shortcomings, which you cannot but find in this very condensed summary, will meet with the excuse that my material has been exceptionally difficult to deal with. If in anything I have seemed too frank, I hope I have at least not failed to be fair.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

RADICAL CURE OF FEMORAL HERNIA BY THE INGUINAL ROUTE.

LIKE the majority of surgeons, I have for many years operated upon femoral hernia by the ordinary methods through the saphenous opening. Two or three years ago my attention was called to the method of operating by the inguinal route, and I have no hesitation in strongly recommending it. Mr. Percival Cole's description of the operation in the *BRITISH MEDICAL JOURNAL* of June 21st, 1919, is an excellent one and should be followed.

The advantages of this method are: (1) The greater likelihood of a permanent cure, for the ordinary operation, even in the hands of experts, is attended by frequent recurrence, and I have at present under my care a woman with a femoral hernia upon whom six unsuccessful operations have been performed by hospital surgeons. (2) A thorough examination of the gut can be carried out, and, if necessary, the gut can be resected. Quite recently I drew down and repaired the places which had been damaged in a tightly strangulated hernia. By the old method this could only have been done by weakening the ring, with the probability of a speedy return of the hernia. (3) Abnormal vessels are avoided. This may not be of much importance, but quite recently I tied the obturator as it curved round the neck of the sac.

There are also disadvantages. It is a rather more difficult operation than the ordinary one, and I have found the passage of the needle when suturing Cooper's ligament to the conjoined tendon troublesome, especially in stout patients. It is easier in females than in males, as in the latter the cord obstructs. In a recent textbook the operation is not, in my opinion, correctly described, for whereas the conjoined tendon should be sutured to the posterior border of the ring, the book in question describes the suture of the ring itself and does not mention the conjoined tendon.

Liverpool.

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ECTOPIA VESICAE SUCCESSFULLY TREATED BY TRANSPLANTATION OF THE TRIGONE INTO THE SIGMOID.

MANY operations have been devised to remedy this distressing condition, but few have proved satisfactory. None of the many attempts to restore the bladder by plastic operations has answered the purpose; it is impossible to reconstruct the sphincter, and the operations have added