as a rule, accompanied by urgent symptoms, and one of its most striking features is the absence of severe symptoms, notwithstanding the presence of very extensive physical signs. This may afford considerable help in diagnosis. Symptoms such as cough, dyspnoea, &c., would seem to be more marked in the post-operative variety of the affection seen in civil practice, more especially after abdominal operations, and it may be that these cases are rather of a different type.

Complications.

Complications in the massive collapse following gunshot wounds are, on the whole, rare, but certain inflammatory complications may develop in the collapsed lung, such as pneumonia, purulent bronchitis, and sometimes pleural effusion.

In cases under observation it can be clearly ascertained that these are true complications in that they supervene on an initial condition of pure collapse, but if only seen after the complication has developed it is probable that their true nature would not be recognised. When a pleural effusion develops as a complication in a case of massive collapse very remarkable physical signs are produced in that the pleural effusion may cause a displacement of the heart, so that it tends to return to its normal position, the retraction of the chest still persisting. With the increase in the size of the pleural effusion the retraction of the chest wall diminishes and may disappear, and then the signs become similar to those of an ordinary pleural effusion. It is, however, of interest that for a time in such cases retraction of the chest may be present; room is found for the fluid not by bulging of the chest wall but by displacement of the heart. The occurrence of bronchitis as a complication of massive collapse is also of interest, as it is in marked contrast to the absence of bronchitis in lung collapsed beneath a hæmothorax, a point I have already alluded to in the earlier part of this lecture. I am under the impression that those inflammatory complications in massive collapse are more liable to occur in the cases of post-operative origin, and in those complicating severe gunshot wounds of the buttocks and thighs. I have not seen them in the cases of massive collapse following the slighter non-penetrating wounds of the chest wall, although the massive collapse present in these cases may be of the most extensive variety and involve the whole of one lung. In cases where death has occurred as a result of pneumonia involving a collapsed lung, it is of interest that on post-mortem examination the lung, although solid, is not of the large size usual in pneumonia. This is especially obvious where both the collapse and the subsequent pneumonia have been limited to one lobe.

Nature of Massive Collapse.

There are many difficulties in the way of any simple explanation of this remarkable condition, and more especially of the variety described above as contralateral collapse, the sequel of non-penetrating wounds of the chest. Collapse of the lung is so readily produced by anything obstructing the bronchial passages that it seems natural to invoke some such mechanism in these cases.

Where massive collapse in one or other of its forms occurs as a sequel to a penetrating wound it must necessarily be difficult to exclude the possibility of the presence of blood or other foreign body in the bronchi in any given case. In the case of non-penetrating contour wounds with no symptoms of any urgent character it would seem very unlikely that any such cause is operative, more especially in the contralateral cases. The practical question would seem to be whether the immobility of the thoracic parietes in these cases is the cause, and the pulmonary collapse the result, or whether the chest wall is immobile because the lung is collapsed primarily. In the absence of any evidence pointing to the presence of a foreign body in the bronchial passages, the only other mechanism would seem to be obstruction of the bronchioles by muscular contraction or pathological exudation. It is difficult to accept either of these possibilities to explain massive collapse involving an entire lung in the absence of any symptoms pointing to bronchial obstruction. Lesions of the smaller bronchi causing obstruction to the entry of air produce symptoms and signs of considerable urgency; in these cases these phenomena are absent. In cases where a patient is confined to bed or has to assume some constrained posture, the hampering of the full respiratory movements of the chest may lead to the retention of secretions in the bronchial passages, and so increase or perhaps produce some varieties of collapse; but such an explanation will not meet the cases of massive contralateral collapse. Impairment of the movements of the chest wall and of the diaphragm are, however, most important factors in causing collapse of the lung directly, as has been noted by previous observers, especially J. Pearson Irvine, and W. Pasteur. If the respiratory movements ceased on one

side of the chest the lung on that side might collapse, if not by the "deflation" postulated by Pasteur, at least as a result of the absorption of the air in the lung by the circulating blood. There is perhaps no great difficulty in attributing massive collapse to loss of respiratory movement of the thoracic wall and diaphragm, and some of the facts I have brought to your notice would seem to suggest that the chest wall is not merely passive. Thus, in hæmothorax, where retraction of the chest wall may be well marked, the heart is displaced, as in pleural effusion, away from the affected side. It is difficult to understand how this comes about if the condition of retraction is not an active one. Further, in cases of massive collapse, when pleural effusion occurs as a complication, the heart undergoes the usual displacement. The difficulty is not that immobility of the thoracic parietes is not an adequate mechanism for the production of collapse, but rather that it is so difficult to understand how this condition should supervene on one side of the chest as the result of a more or less trivial injury to the opposite side. The condition of the chest wall presents some analogy to that of the abdominal wall in cases of injury or disease of the viscera, but it is doubtful whether it is more than an analogy and it would seem as if more experimental work is required before an adequate explanation of the phenomena of massive collapse can be given.

The study of these cases of massive collapse of the lung as the result of gunshot wounds suggests that collapse of the lung is a very common, perhaps the most common, result of a gunshot wound of the chest, although in a very large proportion of cases its presence is masked by the development of a hæmothorax. I am of opinion that this is the reason why the physical signs of a simple sterile hæmothorax are so often different from those of a pleural effusion. In the latter the collapse of the lung is produced gradually in association with the pleural exudate; in the former massive collapse of greater or less extent is produced more or less suddenly, and the hæmorrhagic effusion of the hæmothorax is only in part responsible for the collapse of the lung present.

ON THE END-RESULTS OF PERIPHERAL NERVE INJURIES TREATED BY OPERATION.

(Being the substance of an introductory address to the Section of Surgery at the Annual Meeting of the British Medical Association.)

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IN civil life injuries to the peripheral nerves, although by no means rare, do not occur in sufficient numbers to allow of the collective investigation which they receive in war. Hence much of our earlier clinical knowledge was founded upon the work of Weir Mitchell during the American Civil War, and since that time research has been conducted mainly either on experimental or pathological lines or on the limited clinical opportunities of a few observers. The European war has again supplied us with a large amount of material, which has been thoroughly studied, and which has been treated under the most modern surgical conditions.

Great numbers of these cases have been submitted to the operation of secondary suture, and not a few to that of grafting, so that we may hope that it will be possible ultimately to express almost in a statistical form the end-results which have thus been obtained. This day, however, has not yet arrived, and a long period of time and a careful series of observations by neurologists and anatomists will be required for full investigation. We are thus not yet able to give a perfect picture of end-results; all that can be done is to collate some of the more complete records and to formulate what may perhaps be regarded as an interim report.

The Legitimate Limit of Hope.

It may now be agreed that the regeneration of a peripheral nerve is in all cases accomplished by the downgrowth of axons from the proximal end into

channels or sheaths awaiting them at the peripheral end. The proximal axon is connected with a sensory or motor neurone, while the peripheral sheath leads to, or is connected with, a terminal organ, either motor or sensory.

Apart altogether from anatomical continuity, it must be recognised that repair which may lead to a good functional result requires that a sufficient number of axons shall enter into a sufficient number of peripheral sheaths which are capable of leading them in the right direction. Thus, for example, motor axons entering channels connected with sensory terminals are probably lost for ever, and so with afferent axons which become connected with muscle-endings. So far as I know, the union of a purely motor with a purely sensory nerve is not followed by any physiological connexion whatever.

Clearly, motor axons must enter motor channels and afferent axons sensory channels. Not only is this the case, but it is important that as far as possible the down-growing axons should enter the identical channels for which they were originally intended. Thus, if the spinal motor cells for, let us say, the extensor longus pollicis become connected with the peripheral channels for the brachialis anticus, which lie in the same nerve, physiological confusion will result, and the spinal cord will not be able to produce the movements which the brain demands.

Similarly, irregular connexion of afferent nerves with terminal organs in the skin or elsewhere for which they were not intended will necessarily lead to more or less of chaos in the interpretation of afferent impulses. No doubt errors of this type can be to some extent corrected by means of education, but such re-education will be necessary in the great majority of cases of nerve suture, and it is especially necessary in connexion with suture after the excision of large cicatrices from complicated junctions such as the brachial plexus.

Macroscopic and Histological Shunting.

We are thus brought face to face with the question of shunting, or, as the French call it, "aiguillage," of axons into peripheral channels. Shunting may be gross or macroscopic, as when the proximal end of the hypoglossal or spinal accessory nerve is attached to the distal end of the facial, in which case a limited physiological result may be obtained. Motor axons here enter channels for which they were not intended, and the resulting movements are confused and imperfect, but they can and do occur.

The gross shunting of a purely sensory on to a purely motor nerve or vice versa can produce no result at all. The operation which has been adopted in the past of implanting the two ends of a cut nerve into an intact adjacent nerve ignores altogether the essential linking up of original channels, and has in practice entirely failed. In this connexion I would urge that the word anastomosis, which is founded on a false analogy between nerves and hollow channels such as blood-vessels, should be entirely abandoned. Anastomosis in the real sense of the term never occurs, and the use of the word is apt to lead to confusion of thought.

Apart, however, from gross or macroscopic shunting, histological shunting probably occurs in the vast majority of operations. In the case of end-to-end suture every endeavour is made to avoid errors in shunting by preventing rotation of the approximated ends; but even with this precaution some error probably always occurs, owing to the nerve fibres running in a spiral course, so that, however perfect the anatomical adjustment, the minute apposition is not likely completely to restore the axons to their original channels. To a much greater extent this difficulty arises when a graft has been interpolated. In such cases the axons of the proximal end are expected to enter the neutral channels of the graft, to pass through them, and then, so far as possible, to re-enter their proper channels at the other end of the graft. It is clear that under such conditions the prospects of correct shunting will be at least very greatly diminished, and that, even if good anatomical results can be obtained, the physiological results are likely to be most imperfect.

Economic Value of the End-result.

We have one other consideration of importance in judging of an end-result. Such a result can be anatomically good in the sense that nerve continuity is restored; it may be physiologically or neurologically satisfactory to the extent that on stimulating the nerve every muscle with which it is connected may be made to contract and the whole of its cutaneous distribution may present at least some sensation, but we have also to consider what I may call the economic value of the end-result.

A result which is neurologically good may be economically poor, in the sense that the patient is unable to use the affected part as he ought to do. Such defect may be due to psychological conditions, to the secondary changes of muscular atrophy or of arthritis, or possibly to a form of ataxia in which, while all muscles are capable of action, they can only be properly used while their action is under visual control. This local ataxia, which may render economically imperfect an otherwise excellent limb, is probably due to want of restoration of the deep afferent fibres by which are judged and controlled perfect muscular action.

Alternatively, and more fortunately, the economic result may be better than the physiological result. Thus in cases of injury to the ulnar nerve with well-defined symptoms, those who do not require the finer movements of the hand may find little practical inconvenience, and even a complete lesion of this nerve may exist without recognition by the patient. Again, vicarious movements may admirably replace those of lost muscles. How varied such vicarious movements may be and to what a degree patients may conceal paralysis which actually exists is well illustrated in a recent paper by M. F. Forrester-Brown.¹ Vicarious movements and substituted movements of this type have an enormous economic value, but they constitute a veritable pitfall in the estimation of neurological results for those who are not constantly upon their guard or not thoroughly familiar with the possibilities of misreading end-results. The importance of their consideration is illustrated by the fact that I have personal knowledge of cases reported by good observers as "cured" in which there is no neurological evidence whatever of regeneration; and it is never possible to rely upon the statements of patients as evidence that recovery has occurred after operation.

A Summary of Reported Results.

Turning to some of the more extensive series of reported results, they may be thus summarised:—

Of 108 personal cases of suture or grafting analysed by Tinel,² 14 were complete failures, 22, or say 20 per cent., showed practically complete recovery, and the remainder were regarded as improved.

S. W. Daw³ says that 40 per cent. of his cases showed signs of returning motor power after seven months.

J. Sherren⁴ found that of 21 cases of secondary suture some return of motor power took place in all, but that it was never complete.

R. Kennedy,⁵ examining after a long period cases occurring in civil life, claims that 73 per cent. of all sutures were completely successful.

Dumas⁶ investigated 115 personal cases of operation for war wounds of one nerve only, the musculo-spiral; but it must not be forgotten that for several reasons this yields better results than any other nerve. He claims evidence of recovery in 42 per cent. of his cases, and of some regeneration in 65.2 per cent., the remaining 35 per cent. being presumably entire failures.

J. L. Joyce⁷ found in 36 cases of secondary suture 8 recoveries, 6 with considerable improvement, 8 with some improvement, and no result in 14.

H. S. Souttar and Twining⁸ found that of 61 cases of

¹ M. F. Forrester-Brown: Brit. Jour. of Surg., April, 1920, p. 495.

² Tinel: Nerve Wounds, London, 1918.

³ S. W. Daw: Orthopaedic Effects of Gunshot Wounds, London, 1919.

⁴ J. Sherren: Injuries of Nerves, London, 1908.

⁵ R. Kennedy: British Journal of Surgery, October, 1918, p. 317.

⁶ Dumas: Bull. et Mém. de la Soc. de Chir., June 5th, 1917.

⁷ J. L. Joyce: British Journal of Surgery, January, 1919, p. 418.

⁸ Souttar and Twining: Brit. Jour. of Surg., October, 1918, p. 279.

suture 7 recovered, 32 were recovering at the time of report, 7 were doubtful, 2 were failures, and 13 were not traced; but their results, like those of Joyce, would probably be improved by a later report, as in many cases the time elapsed had not been sufficiently long.

Adson⁹ reports the results of operation on 41 cases, four-fifths of which were end-to-end suture and the remainder plastic operations. Of 30 cases traced "the average amount of improvement obtained was 58 per cent. return of the sensory, 62 per cent. return of the motor, and 67 per cent. return of the trophic function"; 73.1 per cent. showed improvement.

These figures, which are by no means easy to compare, leave the impression that secondary nerve suture gives very good results in somewhere between one-third and two-thirds of all cases, but the most careful study of original articles, as well as my personal experience, leads me to believe that a perfect neurological recovery is rarely if ever obtained. Permanent disability may be masked in various ways, but is almost invariably to be detected if carefully searched for. Complete failures are by no means uncommon, and must be looked for in a very definite percentage of even the best operations. Such appear to be the general results in the case of secondary suture.

Grafting as a Last Resort.

In the case of grafting, as we have already seen, much less is to be hoped for. Certain isolated cases are reported in which brilliant results are claimed, and it is, of course, possible that the anatomico-pathological difficulties to which I have referred may occasionally be overcome, but there is certainly as yet no large number of good results obtained from grafting. Personally I have no experience at all of this operation, and it will, I think, be found that the greater the experience of end-to-end suture the less often the need for grafting will be found to arise. So much can be done by bold operating, by extensive exposures, by thorough stretching of nerve trunks, by alteration of their anatomical course into a shorter bed, and by the operation of stretching in two stages that grafting will always be, and should always be, a last resort. I have yet to see personally any cases in which it has been a success. I have seen cases in which it has been reported as successful, but which to my mind did not carry conviction. I hope, however, that there are those present who will be able to establish a claim to have obtained results more fortunate than I have met with.

A number of facts now well established materially influence our prognosis in the case of different nerves and different levels of these nerves. Nerves which supply large muscles, and especially muscles of homogeneous type, such as the musculo-spiral, give much better results than those whose muscular distribution is small and complicated, such as the median and ulnar. Nerves also which are mainly motor or mainly sensory are more amenable to treatment than those whose function is more complicated. In both these cases, no doubt, the ill-effects of bad shunting bear more heavily upon the more complicated trunks. Dual lesions are more serious than single ones, so that a combined injury of the median and ulnar nerves is susceptible of very little improvement indeed.

Factors in Recovery: Time.

The time required for recovery is very great. On the basis of what is known as Tinel's sign the downgrowth of axons occurs at the rate of about 1 mm. per diem, which would require, say, 200 days for a fibre to grow from the wrist to the tip of the index finger, or two years from the middle of the thigh to the foot. The slow return of epicritic sensation, even after a perfect primary suture, is admirably indicated by Sherren's operation upon Head. An ophthalmic surgeon under my care some years ago divided the digital branches of his left middle and ring fingers near the web by a cut from a piece of glass, and did not obtain perfect tactile sense for 18 months or two years.

On the other hand, the rate of recovery is not in such direct proportion to the length of nerve to be traversed

as was formerly believed, and probably a large share of the time actually occupied is required for the linking up of axons with end-organs and for re-education. In this connexion J. S. B. Stopford¹⁰ has some most interesting figures; analysing 58 cases of suture of the musculo-spiral nerve in the lower, middle, and upper thirds of the arm he finds that the supinator longus begins to recover after about four, five, and seven months respectively, other muscles showing similar differences; so that it takes only about three months longer for recovery in a section close to the axilla than in the case of a section quite near to the origin of the branch for the supinator longus.

It would also appear from the work of Professor Stopford, who has most carefully tested 271 cases of nerve suture, that recovery is more perfect the nearer the suture is to the spinal cord, which may be due to its closer connexion with its trophic centre or to the greater ease of surgical operation upon thicker trunks.

Before concluding I should like to refer to the so far unpublished results obtained at the Grangethorpe Military Hospital in Manchester, staffed by old students of my own, with whose work I am thoroughly familiar. Most of the operating work here has been performed by H. Platt, and the results have been checked by Professor Stopford from a purely anatomical and neurological point of view. In a recent communication to the Royal Society of Medicine Professor Stopford states that in no cases of secondary suture can the result be described as perfect. At the best there has been a perceptible difference in power when comparing the two sides, and at least an incomplete recovery of epicritic sensibility, even three and a half years after the date of the suture. The recovery of sensation has been particularly disappointing.

Local Peculiarities.

Of sutures in the upper arm, failures amounted to 12 per cent., and in the forearm to 24 per cent. At both levels the recovery of voluntary power has been much less over muscles supplied by the nerve in the distal than in the proximal part of the limb. On the whole, it may be concluded that a good functional result is to be expected after secondary suture of the musculo-spiral nerve. The extensors of the wrist usually regain full power, the extensor communis digitorum recovers satisfactorily, but the extensors of the index finger and thumb are much less perfect.

As regards the median nerve, in quite a large proportion only incomplete recovery of protopathic sensibility has occurred, and voluntary power in the abductor brevis pollicis has only been found in 45 per cent. The ulnar nerve is particularly disappointing, and the recovery of voluntary power in the small intrinsic muscles of the hand was exceedingly poor. On the other hand, from the economic point of view, it is interesting to note that many of these injuries to the median and ulnar nerves did not prevent the patients from obtaining employment. I do not propose to weary you with details in reference to the nerves of the lower limbs.

These remarks apply to cases which date back for a period of three years; still further lapse of time may improve them, but will also tend to confuse the issue by permitting of improved vicarious function. As they stand they are, I believe, a perfectly fair expression of the best efforts of the surgery of to-day.

As to the dates at which recovery, if any, can be expected after the operation of nerve-grafting we have no reliable statistics, and the comparatively few records of such recovery which will even bear investigation are not numerous enough for generalisation.

Treatment of Nerve Compression.

In the treatment of compression of nerves my own experience has been very considerable, and almost, if not quite, uniformly successful. I have in the past had many examples of operation for compression of nerves, and I do not think that, however grave the symptoms, I have ever failed to obtain recovery which was at least economically complete; I have not the details to

⁹ Adson: *Annals of Surgery*, August, 1919, p. 169.

¹⁰ J. S. B. Stopford: *Brain*, vol. xliii., Part I., 1920.

show how far they were neurologically perfect. My personal experience thus differs sharply from that of Kennedy, who found that neurolysis yielded slightly less good results than nerve section and suture. I have only one reservation to make: after the removal of pressure by cervical ribs it is most rare for atrophy of muscles to be entirely repaired; this I am inclined to attribute to the fact that a chronic neuritis has followed the original pressure lesion, and that such neuritis leaves indelible changes which are rarely found in traumatic cases of pure compression.

I have not painted the picture in rosy colours; the greatest optimism is generally that of the man who records a few remarkable cases—often a single case. After many years I can only say that if a surgeon obtains 50 per cent. of really good economic recoveries after secondary nerve suture he is doing well; if he obtains 75 per cent. of such recoveries, which will bear investigation, his work is both brilliant and fortunate.

PROGNOSIS IN CERTAIN AFFECTIONS OF THE HEART.¹

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To those who are suffering from affections of the heart prognosis is of the utmost importance. The parents of the boy whose heart was strained ask whether he may safely indulge in any form of athletics at school; others wish to know whether the condition of the heart will debar their son from entering one of the Services. The young man wonders whether the valvular heart disease for which he was invalided from the army is any bar to his intended marriage, or a life assurance company may desire to know his expectation of life. The anxious wife's first question is will her husband be spared, and he himself soon begins to inquire whether he can look forward to complete recovery, or whether he must needs relinquish all, or only some of, his former activities. To the physician prognosis is also an important problem because the treatment he recommends is often based not only upon a correct interpretation of the patient's symptoms and physical signs, but also upon a reasonably accurate forecast of the degree of health to which the patient may be restored.

It may be said that prognosis in heart disease is notoriously speculative and uncertain, and it is true that many intercurrent factors may arise and upset our most careful estimate of the patient's future. Nevertheless, there are certain general principles which we recognise as of the utmost value in guiding us to the knowledge that whereas one patient will make a complete recovery and another may look forward to some years of relatively good, though impaired, health, the life of another is fast drawing to a close.

Prognosis is primarily always dependent on accuracy of diagnosis. The precise data furnished by radiograms, the sphygmomanometer, the polygraph, and the electrocardiograph have placed diagnosis on a firm basis, and the co-relation of this knowledge with symptoms and physical signs has now become so well-established that in practice we can usually dispense with the electrocardiograph and other elaborate instrumental methods.

Cardiac Affections.

The prognosis in relation to cardiac affections is practically always more favourable if organic disease of the heart can be excluded. I do not hold with the theory which sought to minimise or to deny the unfavourable effect of organic valvular disease. Now, no one seriously believes that men with organic valvular disease are really first-class lives, or even that they are fit for military service in category A. It is true that some men with organic valvular disease are fit for a hard game of football, or for strenuous physical work, but these cases are exceptional. In the course of the

acute endocarditis from which the patient suffered some years previously there were probably more focal inflammatory lesions in the myocardium than we suspected, and these must have left some permanent damage to the heart muscle. Consequently a chronic valvular lesion, apart from the mechanical disability it imposes upon the heart, thereby lowering its reserve force, is often associated with at least some degree of chronic myocarditis.

The prognosis, however, should never be founded solely on a study of the heart. We have to take into consideration the patient's age, sex, and occupation, the state of the arterial walls, the blood pressure, the efficiency of the kidneys, and the state of the pulmonary and other systems. Moreover, we have to remember that chronic degenerative changes due to spirochætal infection are slowly progressive, this being of particular importance in relation to aortic incompetence; whereas acute rheumatic lesions of the myocardium and endocardium are liable to recur, especially if the primary source of infection, whether it be in the tonsils or elsewhere, cannot be located and removed. Even after all the available data have been elicited and weighed we are reminded of the fact that no two cases are exactly comparable, and that each individual case must be judged on its merits.

I do not propose to enter upon the consideration of the phenomena associated with the patient's response to physical exertion, though this is of the greatest value in determining the functional state of the heart muscle, and consequently in ascertaining, for example, whether there is some degree of cardiac failure in addition to valvular disease. Nor shall I consider the symptoms and signs which collectively indicate pronounced cardiac failure, nor the prognostic significance of deficient renal activity in heart disease; but I shall limit my remarks to certain of the more common cardiac disorders which we encounter in the course of our daily work.

Dilatation of the Heart.

Let us first consider the prognosis in dilatation of the heart. I shall not deal with the dilatation which follows as a late and often terminal result of myocarditis, with or without valvular disease, but wish to speak of the cardiac dilatation which apparently ensues as a direct result of some unusual physical stress, or which is first recognised during, or in the early phase of convalescence from, an acute infective illness.

In regard to the former group of cases—namely, those usually spoken of as cardiac overstrain—it may safely be affirmed that physical exertion never causes a pathological degree of dilatation in a perfectly healthy heart. The recognition of this fact renders it difficult, or impossible, to give a wholly favourable prognosis to a patient suffering from cardiac overstrain, although the immediate prognosis may be good. The patient has usually attained an age which makes it almost certain that arterio-sclerosis has already begun, and we remember that age is a matter of tissue change, not merely of years.

The case of a joiner, aged 52, affords a good illustration. He was employed in a colliery, and enjoyed excellent health until he volunteered for the unaccustomed work of firing boilers during a miners' strike. Ten months later he was still breathless and fit for little work; the heart was preserving its normal rhythm, there was no drowsy, and no murmurs were audible, but the blood-pressure readings were high, the diastolic pressure being 85 and the systolic pressure 170, and after slight exertion the pulse-rate took four minutes to decline to its pre-exertion rate of 90. In such cases of cardiac overstrain the patient must be content to live on a lower plane of activity than formerly. He must avoid calling upon the heart, the reserve force of which is lowered, for more work than it can successfully undertake, and any physical effort which occasions dyspnoea, pain, or præcordial distress is harmful. The best advice to give to most of these patients is that which I learned from Cowan, of Glasgow—namely, that an active or a merry life will mean a short one.

In cases of cardiac dilatation from acute infective disease the chief difficulty centres in the question of

¹ A paper read before the Medico-Chirurgical Society of Edinburgh on July 7th, 1920.