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CLINICAL MEDICINE

"*I do not know, nor can I conceive, any human contrivance that can more effectually and irresistibly oblige the physician to study carefully the case of his patient; to attend to every symptom or change of symptom; to exert himself to the utmost for his patient's relief; and at the same time to be as cautious as possible in the remedies that he employs; than to find himself under the necessity of giving a minute account of everything he has done, in a very public manner, and before a number of competent judges.*"—DR. JAMES GREGORY, "Additional Memorial to the Managers of the Royal Infirmary of Edinburgh."—Page 382.

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CLINICAL MEDICINE

OBSERVATIONS RECORDED AT THE BEDSIDE WITH COMMENTARIES

BY

W. T. GAIRDNER

PHYSICIAN TO THE ROYAL INFIRMARY OF EDINBURGH, AND LECTURER ON THE PRACTICE OF MEDICINE.

EDINBURGH
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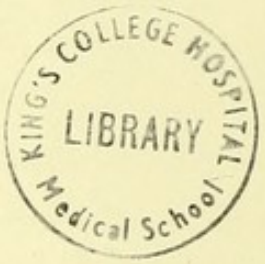
1862



PREFACE.

THE facts and opinions presented to the reader in this volume are in the strictest sense the result of personal experience; yet it is no less true that experience alone might have produced a very different book. The mere accumulation of notes of individual cases of disease would have been extremely easy; and it would have been not much more difficult to have afterwards reduced these to something like order, filling up, at the same time, the gaps of observation by more or less systematic discussions ranging over the whole field of medical practice. The object of the present work is different; it aims neither at being an encyclopædia of separate facts, nor a systematic treatise on the practice of medicine. It is rather an attempt to render into written words the substance of clinical teaching; the very facts observed, the very ideas suggested by the facts, and, as nearly as possible, the very doubts, difficulties, successes, and failures actually encountered by a teacher of some years' experience, in communicating with his pupils at the bedside on cases of more than ordinary interest. I do

not know that this attempt has ever before been made exactly after the manner of the present work, and it is not without some degree of hesitation that I have departed from the usual forms of the clinical lecture, and of the well-known hospital report, so far as is done in some of these pages. The objects I have had in view will be found adverted to in the last article (XX.) of this volume.

The portions of the work which are not framed after this strictly clinical model are nevertheless pervaded by the same spirit. The experience of the bedside has been closely kept in view throughout, and the entire volume may, therefore, be regarded in the light of a series of contributions towards a faithful account of his stewardship, by a hospital physician. The few papers which have not this character will scarcely, I trust, appear out of place where they are introduced; their object being the explanation of the author's principles of treatment in relation to certain controverted questions, which lie at the very foundations of medical practice.

The introduction of diagrammatic representations of physical diagnosis into the records of the cases observed, is to a considerable extent a new feature in works of this kind; and this mode of illustration has not been adopted in the present volume until it has been rendered perfectly familiar to successive clinical classes by daily use at the bed-side. Being fully satisfied of the

advantages to be derived from this method of recording facts, I venture to recommend it to practitioners in connection with the "Outline Figures" mentioned in p. 701, which enable diagrams of this kind to be easily made, even by those who have no eye for drawing. The diagrams employed in the present volume have been very carefully cut on wood after the rough sketches executed by my assistants from ink markings traced out by me in the presence of the students, and transferred to the "Outlines" referred to.* Having had these diagrams submitted to me at every stage of their progress, and being fully satisfied of their accuracy in detail, I trust they will be found useful to the practitioner as faithful representations of facts actually observed.

A twofold index has been added to the volume; the latter index, especially, having been compiled with great care as to details, and with particular reference to the object of the work as an aid to the investigation of disease at the bed-side.

It now only remains for me to acknowledge, with feelings of no ordinary gratitude and satisfaction, the unvarying kindness and support I have received from the managers of the Royal Infirmary of Edinburgh, during the period of my connection with that noble

* Mr. J. M. Corner, 10 Brighton Street, Edinburgh, has in these and the other woodcut illustrations of this work, proved himself both a careful and a skilful artist.

institution. To the many gentlemen who have acted as resident physicians, or as non-resident clinical assistants in the wards assigned to my care, I can only return warm personal thanks for services willingly rendered, and inspired by a very high sense of duty. It is a source of sincere pleasure to me, in editing these memorials of the past, to think that they will fall into the hands of some who will feel sure, as they read, that it was good for them to have passed through the period of unremunerated, but not unprofitable work, of which they will be reminded in these pages.

EDINBURGH, *July* 4, 1862.

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CLINICAL NOTES.

I.

RETROSPECT OF CASES TREATED DURING THE SESSION 1855-6 (NOVEMBER TO MARCH).*

I PROPOSE to occupy two lectures at this period of the session (middle of March) with a review of the ground we have passed over. I think it may be worth while to look collectively at some of those things which have interested us in detail; and I prefer doing this now to doing it later, because it will give you an opportunity of thinking over some matters of great practical importance, to which we may hereafter have occasion to revert, in connection with cases which may yet come under our notice. I propose, *first*, to review the mortality of our wards, with the view of shewing you the elements of which it has been composed; and, *secondly*, to invite your attention to the cases of acute inflammation of the lungs, to their treatment, and to its results. The materials which we have before us are not, indeed, very abundant; not even so abundant as usual, for the season has not been a sickly one; still they are suffi-

* From notes taken by Messrs. S. P. Spasshatt and Thos. Chisholm.

cient, I hope, to afford us both interesting and profitable reflections.

Proportion of Deaths to Cases.—Since the commencement of the session, there have been 48 cases in Ward VI. (general ward for men); among these there were 9 deaths. In Ward XV. (general ward for women), there have been 75 cases and 11 deaths; while in Ward XVIII. (fever ward for women), we have had 43 patients, only 2 of whom have died. In Ward VI. the mortality has been great, between one-fifth and one-sixth of the whole number having died. The mortality has been less in Ward XV., but there it has been large also; it amounted to more than a seventh part of all the cases. Ward XVIII. is remarkable for the small number of deaths that have occurred in it; this is owing, of course, to its having contained only fever cases.* This shews the way in which the general mortality of an hospital is made up. The fatal cases gravitate into certain wards, as they do into certain hospitals; you must take care, therefore, not to compare the mortality of one ward, or of one hospital, with another, without knowing all the facts.

For instance, a great many patients are sent to my wards, because it is supposed their diseases will prove interesting to you; many of these are cases of extreme organic disease; this tends to increase the frequency of death considerably. Thus, you see, this large mortality is not due to the treatment, but to the nature of the cases. This will appear more clearly immediately. The

* Although scarlet fever was prevalent about the beginning of the session, none of the deaths could be attributed to it.

mortality of the Edinburgh infirmary is large. In this respect it resembles some of the London hospitals, whose number of deaths is greatly increased by the character of the cases admitted ; Guy's Hospital, in London, for example, presents a high rate of mortality, and so do many others. The mortality is large here in Edinburgh owing to the gravitation of incurable diseases into this hospital. The number of organic diseases to be seen here is very great. In fact, we seldom see or treat trivial disorders, such as may be found in many of our provincial hospitals.

Let me now say a word upon the *post-mortem* examinations which we have had. The whole number of fatal cases amounts to 22 ; 9 of these occurred in Ward VI., and we had examinations in 8 of them. We had 10 examinations out of the 11 deaths, which took place in Ward XV. The two in the fever ward were not examined. We had thus examinations in all except four cases. This speaks well for the zeal and tact displayed by my friend and yours, our excellent resident physician, Dr. Gilfillan. It has, besides, been a matter of great importance to you.

Details of Mortality.—We should always look back thoughtfully upon our fatal cases, to consider if we could, by any possibility, have cured the disease or prolonged the existence of the patient. This custom should be particularly attended to in private practice ; it is a duty which we owe to the public, considering the great responsibilities that rest upon us. I shall now present you with a synopsis of our fatal cases. You must recol-

lect, in relation to these numbers, how frequently it happened that more than one disease occurred in the same person. Thus, among the 22 fatal cases, there were,—

2 deaths from typhus fever.
8 from tubercular diseases.
5 from organic disease of heart.
5 from Bright's disease of kidney.
1 from softening of the brain.
1 from cancer of liver and stomach.
1 from emphysema and bronchitis, of old standing.
1 from caries of ribs, and protracted unhealthy suppuration.
1 from diabetes mellitus.
1 from splenic leuchæmia.
1 from pyæmia following erysipelas.
1 from foreign body in pharynx, followed by suppuration and pyæmia.

This list shews you the large proportion of organic diseases, and the small number, comparatively, of an acute nature that have been fatal in my wards. Let us make a more close analysis of these cases.

And, first, of the eight tubercular deaths :

Three of these presented few peculiarities, and merely the ordinary appearances of large cavities in the lungs, with ulcers in the intestines. They are, however, not devoid of interest. Thus,

M. had phthisis supervening upon diabetes mellitus. This is a frequent mode of termination in diabetes.

W. presented a good example of tubercular chronic laryngitis ; he was found to have necrosis of the carti-

lages. This corresponded with the affection of his voice, and several other symptoms, presented during life; he had suffered a great deal from cough, and he had experienced pain on pressure over the larynx. He had aphonia nearly complete.

Phthisis. Cod Liver Oil. Contrast of two cases.

Many of you will remember the lad H. I was personally much interested in his case. He came under my charge at the same time with another patient in whom I was equally interested, more than a year ago. These two patients were very similar in every way. They were about the same age; they presented the same complexion; they had the same colour of eyes, and the same kind of hair: they were, in fact, so like, that I sometimes mistook them for each other. Their strength was about equal, and each possessed about the same amount of flesh. If there was any perceptible difference, H. was perhaps the fairer of the two. They were also in much the same state as regards the condition of their health. A little disease could be detected at the apex of the lung in each, as was shewn by the presence of mucous râles, and perhaps by a little dulness. They were treated in the same way, but the results were very different. They both took cod liver oil; the one that improved took it with facility, whereas he in whom the disease was advancing did not do well with it. This is often the case. Whether cod liver oil have any specific curative virtue in phthisis or not, it is certain that the capacity to assimilate it and other fattening substances, is to some extent a test of

curability. Those who cannot assimilate these substances are, generally speaking, in a bad way. H. went out, but the disease continued to advance almost unchecked, and he returned two or three days before his death. The other lad continued to improve, and was soon able to fill a situation in the house. How far the comfort of his position here may have co-operated in securing this happy result, I cannot tell; he has had very fair wages, but he has also had hard work, and has frequently been a good deal exposed to cold in the course of his duty. [He has returned to the wards lately with a bronchitic attack, and he still plainly exhibits the tubercular taint.] The parents of H. were in rather good circumstances, and he had a comfortable home to live in; he was in all respects well-cared for, both as regards medicine and everything else; yet he has had much the more rapid downward march of the tubercular disease. Such are some of the remarkable differences in the vital dynamics, so to speak, of tubercular consumption. It is right to observe them, none the less that they are at present beyond our comprehension.

I will say a word or two now upon each of the remaining five tubercular deaths.

K. came from London, in the last stage of exhaustion from rapidly developed phthisis; he died in a few days. The treatment we employed was the moderate use of stimulants and good diet. This was a case of acute tuberculosis. On *post-mortem* examination, the lungs were seen to be very voluminous, and were found to be extensively infiltrated with miliary tubercles, in small

opaque masses. Cavities could scarcely be said to exist, even of the smallest size.

The physical examination in this case was made in a very cursory way, owing to the very weak condition of the patient on his admission. We found no trace of any cavity, but we heard abundantly in both lungs fine crepitating râles, verging in some places to mucous. There was not much dulness on percussion, so that the disease was correctly inferred rather from the general symptoms than from the physical signs.

A. T. presented on admission the signs of a considerable cavity in the left apex. I was inclined at first to consider that her disease was owing to the irritation of the dust, connected with her trade, viz., that of a flax dresser, and not to any hereditary transmission; she presented the signs of an apparently healthy constitution. Nevertheless, her disease made rapid strides, and I frequently remarked to you that she was sinking more rapidly than any other patient in the ward. She had constant hectic fever, and severe night perspirations. One day, on stepping out of bed, she was suddenly seized with a severe pain in the side, and great dyspnœa. On physical examination, Dr. Gilfillan found all the signs of pneumo-thorax in that side which had previously shewn the fewest marks of disease. The dyspnœa increased, and she died asphyxiated on the following day. On *post-mortem* examination, it was found that fatal pneumothorax had occurred on the least diseased side. The fatal event was due, not to the giving way of any large

cavity, the more considerable excavations being all in the other lung (and being, besides, sealed by adhesions), but to the rupture of a small excavation near the upper part of the lung.

M. R. was a long time in the wards. She was about twenty-seven years old. She suffered repeatedly from severe paroxysms of what was described as dyspnœa or difficulty of breathing, but perhaps had more real resemblance to angina pectoris. From the very peculiar and very intermittent character of her sufferings, we were at first inclined to suppose that they were owing to hysteria, the more so as the paroxysms were alleviated by the usual remedies for this disease. Prior to this illness she had been of irregular habits, having in fact been a common prostitute. While under the care of Dr. Robertson, several years ago, she was being treated as a case of phthisis, when an empyema took place. Thus the left lung was quite disabled, so that respiration was entirely carried on by the right. We afterwards accounted for these paroxysms of dyspnœa, partly by the fact of there being little sound lung left, and partly by the idea that they might be complicated both by hysteria and by pericarditis, as she had the evidences of an old inflammation of the pericardium. From the persistence of the friction murmur up to death, and from the non-development of the signs of adhesion in a well marked form, I inferred roughness on the pericardium, but did not anticipate general adhesion. I was wrong; for *post-mortem* examination shewed that there were tight adhesions all over the

Phthisis. Empyema. Adherent Pericardium.

pericardium.* This case, while it shews the dreadful tenacity with which tubercular disease often pursues its victims to the grave, also shews how much may frequently be done for such patients, when placed in favourable circumstances. When under Dr. Robertson's care, several years ago, she was in such a condition, that she could not have been expected to live many months. Nevertheless, she survived the progress of tubercular deposit, and finally perished from the severity of other organic complications. She sank from an illness extending over a series of years.

The list of tubercular diseases is terminated by two which were complicated with Bright's disease of the kidney, which will be mentioned presently.

We shall now pass to the consideration of the five fatal cardiac cases.

The first of these that I shall mention died very shortly after admission. I saw very little of him ; he was found to have pulmonary hemorrhagic condensation besides the disease of the heart ; and in fact, may be said to have come into the hospital only to die.

You will remember the young man D., who died of disease of the heart ; he was about the age of 21. We all took a great interest in his case. He had suffered for some time from great palpitation. The action of the heart was greatly exaggerated. You could feel the heart bounding, as it were, under the hand.

* For further details of this case, in relation to the diagnosis of pericarditis, see *Edinburgh Medical Journal*, April 1859, p. 911, or the reprint of this paper—"*Clinical and Pathological Notes on Pericarditis.*" Edinburgh, 1860.

There was also considerable lateral bulging over the cardiac region. The sounds of the heart were abnormally weakened. The transverse dulness was increased, and the apex of the heart beat two inches below and to the left of the nipple. We had, in fact, all the signs of well marked dilatation. We had, besides, a murmur both with the first and second sounds, heard loudest at the base, and also one at the apex, indicating mitral regurgitation. We made the diagnosis of this case quite correctly as regards the heart. He perished from true angina pectoris; for two days before his death he had frightful suffering. It could not be called dyspnœa, for he had no difficulty in performing the respiratory acts, and there were no physical signs of respiratory obstruction; it rather might be said to be a feeling of impending dissolution. There was considerable resemblance, in this respect, between his case and that of M. R., alluded to before among the tubercular cases. In the case of D., also, there was an adherent pericardium, but there was not any unequivocal sign by which we could have discovered the affection. As usual, it gave rise to no murmur; and the other signs were those of the enlarged and dilated heart only.

T. and C. had cardiac valvular disease, complicated with disease of the kidney. The combination of these two affections is frequent, and the connection between them is not well understood. It is often difficult to tell which began first and which was the sequela. The cases in question do not add much to our knowledge on this point.

I will now go over the remainder of the list of fatal cases in order.

H. was admitted in extremis from Bright's disease. She died, rapidly, with severe dyspnœa.

There were two cases of retrograde tubercular disease, terminating in Bright's disease; I refer to J. and S. The latter was a case of great importance in relation to treatment. S. came in with a diarrhœa that had lasted several weeks; his urine was found to be albuminous, and of low specific gravity, thus clearly defining the case. The diarrhœa had already reduced the patient's strength considerably. After a trial of the pernitrate of iron, I ordered him an astringent mixture containing opium. I neglected at the moment to observe that the kidney was diseased; if I had kept this fact in view, I would have avoided, if possible, using the opium at all. After having taken this mixture for a short time, the man passed into a comatose condition; but I don't blame the opium for this, as it was discontinued the moment his altered state was observed, and the coma went on for a much longer time than the opium would have taken to be eliminated. The coma deepened, and the man died. The pathology of the case was then evident; it was one of Bright's disease of the kidney. This organ had become disabled, and led to the retention of urea. The diarrhœa was owing to the uræmia, and to some retrograde ulcers, of tubercular origin, in the intestines. We treated of this case at length in the lectures at the time, and deduced from it those cautions, which it is fitted to inspire as regards the

Phthisis, with Bright's disease. Opium.

treatment. How easily might we have had ourselves to blame, apparently, for the death of this man! How anxiously we ought to watch from hour to hour, so as to avoid such a result!

S. W. was brought in, in a state of coma from softening of the brain, and died shortly after admission.

The next case is that of F., upon whom I expatiated at the time. She had cancer of the liver and stomach, and though seen under rather unfavourable circumstances (for she was only two days in the house), a correct diagnosis was formed. The *post-mortem* examination revealed cancer of the liver and stomach.

L. was admitted in a state of great prostration from intense bronchitis complicated with emphysema; she died after being a short time in the house.

S. came in with a chronic abscess in the groin, closely resembling lumbar abscess; I was at first inclined to consider that it was due to disease of the spine, but we found that the only bones affected were the 9th, 10th, and 11th ribs. An abscess had formed in connection with the carious bone, and it finally opened into the lung, when a copious expectoration of pus ensued. He sank at last under a state of system induced by the absorption of pus, and of other morbid materials.

The case of diabetes mellitus and tubercle was alluded to before in the list of tubercular cases. There was nothing particular to remark upon as regards the diabetes.

There was one case of leuchæmia in my wards during the session. It was that of the old woman Harper;

she was long under our observation. On *post-mortem* examination, the spleen was seen to be greatly increased in size, and firmly wedged between the pelvis and ribs. The usual appearances, of large opaque greenish yellow clots, were seen in the heart and great vessels. The white corpuscles were greatly increased in number.

I have now mentioned all the cases ending in death during the period over which my remarks extend, with the exception of four, which belong to the series of cases of acute disease, on which I mean now to make some remarks. You will observe, as regards all of those I have mentioned hitherto, that they were cases of extreme organic disease, and that few of them were of such a kind as to leave any doubt that life could not, under the circumstances, have been prolonged. Such, then, is a great portion of the mortality of an hospital like this.

Deaths from Acute Disease.—In passing from the consideration of the chronic cases, allow me to make one remark, which has a very important bearing on what remains to be said. It is this, that out of the whole deaths, twenty-two in number, noticed in last lecture, there have been *four* only due to what can fairly be called acute disease, or, excluding the *two* deaths from typhus fever, there have not been more than *two* deaths out of *twenty* referrible to acute inflammations. In particular, I beg your attention to the fact that, with the two exceptions presently to be noticed, inflammations of the lungs and pleuræ have not been directly or indirectly

fatal in our wards in a single instance during the present session.

The exceptional instances alluded to were, both of them, cases of no ordinary interest.

Fish-bone swallowed. Purulent Infection.

The first, which occurred very early in the session, was that of a middle aged female servant, who was sent up to the fever ward under suspicion of contagious disease. When I saw her she was in a very prostrate condition; her extremities were cold, and not only cold, but livid, almost like those of a patient in cholera; the pulse at the wrist was very feeble and rapid; the breathing oppressed, not noisy, but like a succession of deep sighs, interrupted frequently by short, dry, harassing cough; the intelligence was perfect; there was a good deal of pain, not distinctly localized; the voice was impaired, and she seemed, by the motion of her hands towards the throat, as well as by something in the character of the cough, to suffer under irritation of the larynx or upper part of the trachea. The history of the case was this: some days before admission she had swallowed a piece of fish-bone, which had, as she believed, stuck in the throat. She applied to a surgeon, who, after some efforts to remove the foreign body, believed he had dislodged it, and told her so.* The acute pain which at first had pointed out the

* In the course of an interesting discussion in the Medico-Chirurgical Society of Edinburgh (January 5th, 1859), which followed the reading of a paper by Mr. J. Jardine Murray, on the successful extraction from the pharynx of a needle which had penetrated the neck, the principles which should guide interference in case of such foreign bodies (when swallowed) were brought under review. The following remarks

situation of the fish-bone had disappeared, and she was quite positive that the cause of it had been removed. The present symptoms had, however, gradually arisen, and she was now evidently in the greatest danger from acute inflammation, accompanied by more than ordinary depression of the vital powers. Physical examination gave evidence, but not conclusive evidence, of double pleuro-pneumonia; that is, impaired percussion in both backs, with enfeebled respiratory murmur, and mucous râles; but nothing absolutely characteristic, unless it were a râle resembling friction, which was heard once, and once only, on the left side of the chest, and which was absent next day. The patient was, moreover, in too feeble a state to admit, either of repeated examination,

by Mr. Spence will probably be regarded as a correct expression of the views of practical surgeons on this point. While there is danger, as the case above mentioned shews, in allowing such bodies as needles, fish-bones, etc., to remain unremoved, there is often equally great danger in too much interference;—"The most judicious plan is to feel for the foreign body with the finger; and if it is not easily felt, there should be no groping in order to reach it. If the body has passed down into the stomach, the precaution should be taken to feed the patient upon some tenaceous aliment—such as prunes or figs, in order that the substance may be coated, and carried away by stool." The discussion here referred to (*Edin. Med. Journal*, February 1859, p. 769), contains particulars of two other cases of death remotely connected with the swallowing of fish-bones; in one of which the foreign body had penetrated the duodenum, and after becoming imbedded in the serous coat of the liver, had apparently become the cause of abscesses in the brain; while in the other case the bone lay among chronic adhesions of the small intestines (but quite external to their canal), and in this position had led to effusion, supposed to be of renal or hepatic origin. Dr. Gillespie has published in the *Edin. Med. Journal* for January 1858, p. 595, a very curious case of abscess opening below Poupart's ligament, evidently communicating with the intestine, from which three ribs of some small animal were discharged, with a successful result.

or of active treatment. She died within forty-eight hours after her admission to the ward.

It was perfectly plain to me, that this case was not one of fever, but of complicated and grave thoracic inflammation. But whether that inflammation was connected with the injury from the fish-bone, or whether it was a separate and distinct disease, was not quite so plain, though the former opinion seemed probable. The *post-mortem* examination shewed that the fish-bone had not been extracted, as the patient supposed. It had, on the contrary, perforated the back wall of the œsophagus, or rather the lowest portion of the pharynx, and was found imbedded in pus in the cellular tissue behind this part. Along nearly the whole length of the œsophagus, there was diffuse suppuration of the neighbouring cellular tissue ; both pleuræ contained pus ; both lungs were œdematous, and contained small abscesses or sloughs, in an early stage, at their back part. Finally, the pericardium was covered by a layer of soft lymph, and contained several ounces of purulent fluid. This last lesion was not discovered during life ; whether from too rapid examination, or from the greatly enfeebled action of the heart concealing its disease, I shall not now venture to determine.

We have, in this instance, an example of several coincident inflammations, of the kind usually determined by external injuries and surgical operations, and supposed to be due to the introduction of a specific poison into the blood, in connection with the pus formed at the seat of the injury, and afterwards absorbed.

Without entering further into the pathology of this matter at present, we shall call it a case of *purulent infection*, or of *pyæmic inflammation*.

Not less distinctly of the same kind was the other fatal case. The cause of the purulent infection here, however, was different.

Erysipelas. Putrid infection.

A girl, about 20 years of age, was affected, from no obvious cause, with erysipelas of the face. In two particulars, however, the disease differed from its ordinary form in this part of the body. It was limited to one side; and it presented over the right malar bone and zygoma a diffused swelling, which rapidly tended to suppuration, and had to be freely opened, giving vent, within a few days after the first appearance of the disease, to a large quantity of very fetid pus. You may lay it down as a general rule, that erysipelas of the face, in its ordinary and manageable form, does not tend, however general, to phlegmon, but to vesication. A little pus, not unfrequently, is found during the decline of the disease to have formed in the lax cellular tissue of the eyelids; but even this is exceptional, and a general suppuration under the erysipelatous surface, as in this case, is of rare occurrence. I do not know whether, in this case, there was anything specific in the nature or cause of the disease, to account for this unwelcome peculiarity. I was not without suspicion of disease of the malar bone, extending along the periosteum, and perhaps to the dura mater, for there was marked delirium, preceded by exceedingly severe pain in the head. Soon after the discharge of pus had

taken place, there occurred a new train of symptoms; shiverings, repeated and severe, sickness, cold sweats, a somewhat sallow hue of the surface, a remarkably cadaverous odour of the whole body, increased delirium and prostration, finally, unconsciousness and death. This succession of events was observed between the Saturday and the Monday visit, during which interval of 48 hours all the more threatening symptoms ran their course. It was impossible not to see, in this case, a marked instance of purulent, or perhaps rather of putrid, infection; and so it was; for besides the gangrenous smell of the whole body and of the blood, we found incipient gangrenous points in both lungs, and a little putrid pus in the pleuræ. I wished to examine the seat of the primary disease, and also the brain; but we were obliged to forego this satisfaction.

Infrequency of Death from Simple Acute Inflammations.—I presume that no one would consider either of the cases now narrated simply as an example of thoracic inflammation; at least of thoracic inflammation amenable to what is called antiphlogistic treatment. They were separated alike in their symptoms, their anatomical appearances, their pathological cause, from cases of simple pleuro-pneumonia, and that by a sufficiently broad line. And so in treatment; it was impossible to think, even for a moment, of bleeding, of antimony, of calomel, of blisters. Internal stimulation, and external warmth, were demanded throughout, to save the sinking powers, and restore, if possible, the failing circulation.

In both cases, however, the disease seemed, from an early period, to baffle all remedies ; and it is further important to observe, that the rapidity of the fatal event was out of all proportion to the extent of inflammatory lesions discovered after death. In short, you have here the picture of a disease in which the effects of a poison on the system were as distinct as they are in a case of scarlatina or small-pox ; and almost as distinct as in the instance of a person bitten by a venomous reptile.

If we make deduction of these two cases of purulent infection, we have not had, during the past session, a single case of death from acute inflammation ; in other words, we have had no death at all from acute disease invading an organ previously healthy, and in a system previously undisturbed by constitutional or local disease of grave character. In particular, we have not seen a single fatal case of what is called idiopathic or simple acute inflammation of the lungs. This is not a little remarkable, considering that, in the Registrar-General's returns of mortality, pneumonia scarcely ever fails to take nearly as high a numerical position as consumption, of which disease we have had, at least, six or seven fatal cases during the session. Let us look for a moment at the probable reasons which may be assigned for this paucity of deaths, from so well-known and so large a cause of mortality in the eye of the world, and even of the medical profession, as pneumonia.

Of course, we are disposed to credit our treatment with something of this favourable result. But, before we ascribe to our treatment any *peculiar* influence, we

shall do well to consider, if either the treatment or the results have been so unique as to require that we should regard them as superior to those of others. Now, you have seen the treatment adopted in these wards in a good many cases of acute disease. As a general rule, you have seen that it has not been guided by any more *special* principle, by any more recondite system of doctrine, than that of other educated physicians. We have appealed to no unvarying law—we have introduced (as it happens) no new remedy, nor re-introduced any old and forgotten one, in any of these cases of acute disease. Nor have we aimed at striking into a new path, by reposing on remote analogies and complex theories of disease or of cure. While we have received with reserve, and applied with such caution as seemed necessary, the dicta of past experience, we have in no instance thought ourselves at liberty to give an arbitrary denial to conclusions founded on the long-continued observation of disease by multitudes of distinguished physicians. Such conclusions we have held sacred thus far, that we regard them as not to be at once overthrown by the rash speculations, crude theories, and indiscriminate experiments, of a few modern physicians ; but, on the contrary, to be again and again submitted to trial, in carefully selected and carefully watched cases, until the revised experience, not of one or two, but of hundreds, has corrected or adopted them. In a word, in dealing with acute and well-marked forms of disease, we have considered ourselves as acting under a grave responsibility to society and to our profession ; wherefore, we

have scarce done anything that has not been done hundreds of times before, or omitted anything that has not been omitted hundreds of times before, on the very same grounds on which these things have been done, or omitted by us. Our treatment has, we trust, been safe, judicious, and successful; but it has not, in the cases in question, been peculiar or novel.

Within the limits, however, which we have prescribed to ourselves, there is ample scope for all kinds of treatment—good, bad, and indifferent. Have we been then, so much more successful, or so much more lucky than our neighbours, as to be able to draw any inferences favourable in any extraordinary degree to ourselves? I shall answer this question by appealing to Dr. Haldane's records, and, to avoid any invidious comparisons, I shall give you the results, not of any one physician's practice, but of the whole medical practice of the house in that "princeps morborum acutorum"—pleuro-pneumonia. Not counting cases in which this disease supervened upon chronic (mostly tubercular) organic chest disease, I find four cases only of pleuro-pneumonia which have been examined after death in the entire medical department of this large hospital, since November. One of these was a case of pleuro-pneumonia, *with*, and probably *from*, chronic disease of the kidney; another was a case, apparently of neglected measles, admitted almost *in extremis;* a third was a case of double pleurisy after scarlatina, with suppuration of the cellular tissue of the neck; and the fourth was an instance of severe pneumonia of the left side, in a

man aged forty, not otherwise much diseased, but having a fatty liver. Of all these, the last alone, and it doubtfully (for I do not know the full circumstances),* has a right to be admitted into the list of cases of idiopathic acute pneumonia, amenable to active treatment.

It results, then, from this inquiry, that in one of the largest hospitals in Britain, into which the sick poor are admitted with scarcely any restriction, and on no other

* *Note by Mr. Spasshatt.*—I think I remember this latter case. It occurred at the very commencement of the session, in Ward No. VII., under Dr. Keiller's care, of whose wards I at that time had charge.

The patient had for some time previously lived in the constant and habitual abuse of alcoholic stimulants. He had been ill some four or six days before admission. The history of the attack was one of pleuro-pneumonia, but, occurring in a constitution much weakened and depraved, the fever assumed a very adynamic type. On admission, he was almost in a state of collapse: countenance pallid, shrunk, and anxious—surface of the body cold—occasional rigors—pulse scarcely perceptible at the wrist, and perceptible at all, only for a short time when it was very rapid—heart sounds almost, if not entirely inaudible—breathing very hurried—exhaustion and prostration almost complete—sputum bloody—percussion over two lower thirds of left back dull—respiratory murmurs in the same situation inaudible, except towards the upper third, when crepitation was faintly perceptible—both light and strong percussion over anterior of left chest, very resonant (most likely from the stomach being distended). Patient was also suffering from diarrhœa. He was placed on a stimulating plan of treatment; but, in the course of about twenty-four hours, he succumbed from pure exhaustion.

Post-mortem examination entirely bore out the diagnosis of pleuro-pneumonia of two lower thirds of left lung, with great preponderance of the pneumonic element. The pleuræ on this side were adherent, and the two lower thirds of the left lung presented a very beautiful specimen of pneumonic condensation just passing into pus. There was also some purulent fluid in the pericardium, with some slight deposit of unhealthy lymph on the visceral portion.

I learned that, before coming into the hospital, he had been treated rigidly antiphlogistically, not by blood-letting, but other agents, viz. ant. tart., in full doses, etc.

plea save poverty and urgent sickness ; an institution in which there is a higher rate of mortality, and, therefore, probably, a greater pressure of severe disease than in most others, we can find during four winter months (during which it must be granted there was, on the whole, mild weather, and little epidemic sickness), only one recorded fatal event from primary, idiopathic, or well-marked sthenic pneumonia, that is, from pneumonia amenable, as such, to the ordinary modes of treatment in this disease. Is not such a fact a convincing proof that pneumonia of the type alluded to, and especially uncomplicated pneumonia, is in Edinburgh and the neighbourhood, at the present time, an exceedingly rare cause of death, even under treatment only moderately good? For, recollect, the results of treatment pursued in an infirmary are, in many cases, unavoidably worse than that of private practice in the better and middle classes of society. We see our cases later in the disease, we receive them under less favourable circumstances, we treat them with less attention to individual comfort, perhaps with less special care, certainly with less of personal sympathy, than patients in our own rank of life or near it. Further, the cases themselves are often selected cases, and selected on account of their severity and urgency. I say, therefore, without hesitation, that making allowance for the differences between hospitals and private practice, and for the large number of cases of organic disease which are daily drafted into our wards from all Scotland, contributing so greatly to raise our mortality, that the very small number of fatal cases

of simple pneumonia shews the eminently curable character of that disease apart from epidemic or other disturbing causes.

But you will remind me, and justly, that the Registrar-General's tables, to which I referred a moment ago, shew a very different result. True; but there are many reasons why these returns should not be accepted as the correct exponents of the state of this question. I will mention only a few. These tables are so framed as to exhibit in each case only one cause of death. Now, a very large proportion of the cases of pneumonia must have been, as indeed they are everywhere, complicated cases, often perhaps less of acute than of chronic disease. Thus, pneumonia is a convenient synonym for cases of phthisis which terminate acutely and with fever; as also for many cases of Bright's disease, of disease of the heart, etc. The affection of the lung is plain and unmistakeable; the chronic disease is often overlooked; besides, the feelings and the interests of the relatives are often consulted in singling out the acute disease for mention, instead of the other. Besides all this, there is a great deal of really faulty diagnosis involved in the matter.

I have no time to go into the wider considerations connected with possible or probable changes of type of pneumonia; but I think these facts justify us in concluding that *the acute diseases of the chest, as we see them at present, are, if uncomplicated and skilfully treated, rarely fatal.* I will go farther, and say, that although a large majority of the cases met with in practice are com-

plicated, I am convinced that pneumonia, pleurisy, or pericarditis, in such cases, will usually end favourably, if the primary disease is such as to afford a reasonable prospect of recovery; and this, I do not say independently of treatment, but independently of any special element in the treatment applicable to the whole or the majority of the cases.

In illustration of this position I shall adduce only a few examples from the list of our successful cases; and they shall all of them be cases where the thoracic inflammation was prominent and severe; so much so as when combined with other disease to mask, and sometimes to destroy, the symptoms of that disease, and leave us in doubt as to its existence. The number of cases of pleuro-pneumonia of this prominent kind has been, as nearly as I can calculate, 7; but this takes no count of those minor forms of disease in which certain of the physical signs or symptoms of pneumonia have existed, while the whole physiognomy and general character of the case was opposed to that view, or indeed to the view of any serious disease. Only the other day I shewed some of you a man lying ill of dysentery, or at least of obstinate diarrhœa, in Ward No. VI., in whom well-marked physical signs—viz., crepitation and dull percussion—existed at the base of the left lung, without a single pulmonary symptom requiring treatment. Much more frequently such signs, or signs not very dissimilar, co-exist with one or two trifling symptoms in fever, in catarrh, in acute diseases generally, and in not a few chronic diseases: the great majority, of course, of such

patients recovering without, or in spite of, special active treatment addressed to the lung. Such cases I never call pneumonia, though sometimes it happens that they are pathologically allied to it, while on other occasions they are very different. I mention one such instance, in which the physical signs alluded to were discovered; but it was mere accident which led to the discovery in this case, and it is probable that there have been a great many more which have been overlooked, and which indeed could only have been discovered and noted in the course of a promiscuous search after stethoscopic curiosities; a search which, I confess, I never think it advisable to make, and which would be very inconsistent with the higher duties of the physician.

Again, there are many cases of disease possessing something in common with pneumonia, but in which the symptoms of that disease, and even its physical signs, are lost in those of more complex disorder of the chest, or of the system. Such is very often the case in tubercular disease, in which, as you know, what is called intercurrent pneumonia often takes place, with characters very different from the genuine acute disease. Such is, also, the case in the pneumonia of purulent infection.

It follows, then, that any attempt to estimate numerically our entire experience of pneumonia, is subject to grave causes of fallacy. I do not, therefore, make that attempt; and I advise you, when it is made by others, to remember that they are subject to the same sources of fallacy as we should be, were we to throw into appa-

rently precise numerical formulæ the necessarily uncertain data which are afforded by the experience of this session. We might, in fact, magnify or diminish the field of acute inflammation to an extent almost unlimited, according as we looked at it through the large or the small end of our theoretical telescope.

Detailed Treatment in Pleuro-Pneumonia.—The first two cases I shall mention occurred in Ward XV., in the early part of the session, in the persons of two women, C. and S., whose cases were dwelt upon at much length at the time. They were cases which might be fairly styled acute pleuro-pneumonia. They both commenced with marked inflammatory fever—the expectoration, scanty at first, became profuse afterwards—in one case bloody on admission, in the other becoming so whilst under treatment; in one there was acute pain in the right side, in the other in the left, and simultaneously with the pain, the ordinary physical signs were present, viz., dulness over the whole lower third in one, and two thirds in the other. We never doubted that there was pleuro-pneumonia in both cases, but their former history led us to suppose that this affection had in both supervened on former, possibly latent, tubercular disease. I told you at the time, that this did not necessarily remove them from the category of acute pleuro-pneumonia, although it modified our prognosis, and, possibly, our treatment. I also told you, that I should not be surprised if one or both of them should make good recoveries, notwithstanding the presumed tubercular com-

plication. They had both advanced some steps in the disease before admission. In one, exudation had taken place, fever had subsided, acute symptoms had diminished. The other was not quite so far advanced, but sufficiently far to preclude bloodletting; which, to be of service, must be performed in a very early stage of the disease. Tartar-emetic, however, was used in both. In one, C., it was continued without any accident, and apparently with good effect, for some time. In the other it determined diarrhœa, and we then immediately refrained from its exhibition. Both were treated on the same principles; but, in both of them, the operation of the remedies was carefully watched, and made, as it were, a test of the character of the disease. Accordingly, we found, that in S.'s case, we could not push the ordinary active treatment; and, in this case, it afterwards proved certain that the tubercular taint was well marked; the cure was very imperfect and lingering; dulness still remained on her leaving the hospital, and there was a strong suspicion, from existence of cracked-pot sound, and something very like cavernous râle at left apex, that the tubercle there was considerably advanced.

In connection with these cases, it is very interesting to look back on that of the man in Ward VI., A. B., who, on admission, presented obvious signs of acute pneumonia of three weeks' standing, of the whole upper lobe of the right lung. He suffered from intense hectic fever. His history pointed to very acute pain and fever at the beginning of the attack. From the implication of the

apex, the character of the disease, the prostration and nervous disturbance, we were disposed to augur badly, and there was an extremely strong probability of the disease being one of acute infiltrated tubercle, and not of pneumonia. On the symptoms alone, however, we adopted active antiphlogistic measures ; not bloodletting, but tartar-emetic in full doses ; watching carefully, however, in order to diminish or omit it if found to produce any bad result, as it did in the last case. We also gave him occasional opiates at night, in pretty large doses. This treatment acted admirably well, the hectic diminished, the patient got better from day to day, and from hour to hour, and after a week prognosis became more favourable, though not entirely satisfactory even up to the last moment. It is still a question whether or not there was a little nucleus of tubercular deposit at the apex.

A case almost similar, but not so severe, was that of Catharine M'K., a young girl of sixteen years of age, suffering from acute pleuro-pneumonia of the left apex, in this case certainly conjoined with tubercular disease. There was no tartar-emetic given in this case, but only simple cough mixtures ; there was a gradual resolution of the pneumonia, but the tubercular taint remained.

There were two cases, one of pleuro-pneumonia, and the other of broncho-pneumonia, which were admitted and cured in a single week under equally simple treatment.

Finally, you will recollect the case of Mary R., a young florid healthy girl, admitted on the third day of

the disease. She had all the symptoms, well marked, of acute pleurisy of the right side, attended with grave inflammatory fever; two lower thirds of right back were dull. The extreme antiphlogistic measures were adopted in this case. Bloodletting was had recourse to from the arm, and tartar-emetic was given, and with the best possible results. There was speedily a great amendment, and the resolution was extraordinarily rapid. We had no reason to regret having used the lancet in this case; on the contrary, all that has been said with regard to the benefit of bloodletting in acute inflammatory fever attacking robust individuals, was fully borne out; the reduction of the fever and the other symptoms within a few hours having been such as was not observed in any other case during the session. She was dismissed in five weeks, perfectly well, but the cure dated much farther back than this, for there having been no great pressure on the wards this winter, we have kept acute cases a long time after convalescence, in order to assure ourselves of their recovery, and to give them every possible chance.

These are the results which have been observed from the system of treatment which we have adopted. That treatment, as stated before, has not been founded on any single principle or method, but has been in accordance with general experience, and, I think, with common sense; at all events, its results have been such as have left us no room to doubt the propriety of continuing to be guided, generally, by the same rules.

In treatment, there are two lessons to be learned.

It is important to know what to do, and when to do it, but it is also important to know when to refrain. Again and again, after giving tartar-emetic, you have had occasion to observe during the session, that it has been stopped just when you might have supposed it was doing most good, viz., when the fever was subsiding rapidly under its use. But it was stopped in these cases because it was no longer needed; we had helped the patient over the critical part of the disease, and this done, we have always, with perfect confidence, left the rest of the cure to nature. These facts, however, should not lead you to inert practice; and I beg you to remark, that we have had no reason to repent of the use of active measures; indeed, we have run through nearly the whole gamut of the orthodox remedies, and found that every one, in its own proper time and place, has been productive of good.

II.

REMARKS ON THE TREATMENT OF PNEUMONIA, AND ESPECIALLY ON THE TREATMENT BY BLOODLETTING.

THE preceding paper, though limited in its object, and not at all controversial in character, forms the first decided expression, in point of time, of views which about a year afterwards I had occasion to bring under the notice of the Medico-Chirurgical Society, in connection with what has since been often called "The Bloodletting Controversy." As the statement of my opinions on that occasion led to a somewhat too warm discussion, from which I thought it right to withdraw as soon as I observed that it had ceased to turn on questions of scientific truth, I gladly embrace the present opportunity of disentangling the leading statements of fact and of opinion which I then submitted to the Society, from the controversial element in which they became imbedded. This is the more necessary, as several papers of the present series will be observed to bear relation, more or less closely, to the important practical questions so warmly entertained by the medical profession of Edinburgh during the discussions re-

ferred to. I shall be able, however, very easily to avoid all unpleasant and unprofitable matter, and also to exhibit very briefly the conclusions to which I am anxious to lead, by referring for many details to a paper published by me in September 1857, in the *Edinburgh Medical Journal.** This paper, together with the reports of the proceedings of the Medico-Chirurgical Society, as given at considerable length in the same Journal during 1856 and 1857, and some memoirs published long before the controversy, form the documents on which the following remarks are almost entirely founded :—

So early as 1850, Dr. Alison had indicated, in a published clinical lecture, the remarkable fact, that acute pneumonia, such as it had been observed and described by Cullen and Gregory; viz., accompanied with full hard pulse, well marked synocha or inflammatory fever, and violent pain or extreme dyspnœa of abrupt invasion, had almost disappeared from the field of hospital experience in Edinburgh. This change, it is well known, Dr. Alison regarded as due to an *altered type of disease* arising from unexplained causes, and bringing with it, on the side of the sick, a marked comparative intolerance of bloodletting and evacuant remedies ; rendering necessary, also, in many cases of acute inflammation, as well as in fevers, the liberal use of stimulants. It would hardly be incorrect to say, indeed, that the doctrines pushed to such an extreme by the late Dr. Todd were

* *Remarks, etc., etc., on Bloodletting and Antiphlogistic Treatment, etc.*

very much those taught by Dr. Alison, with more or less urgency according to what he observed to be the epidemic tendencies of the period, for many years before 1850, as all of his old pupils must well remember; with this important difference, however, that Dr. Alison recognised the disuse of bloodletting, and the increased employment of stimulants, as a *consequence* of the changes he observed in the character of disease; while Dr. Todd entirely disowned the idea of such a change, and evidently came, at last, to look upon the administration of stimulants almost as matter of routine, a practice to be pursued in all acute diseases very much without regard to their special vital manifestations, and in all manner of persons, old and young, strong and weak, temperate and intemperate. Dr. Alison was in this matter emphatically a *vitalist,* and paid much more regard to the *dynamics* of disease, so to speak, than to the mere *statics*, or to the anatomical changes produced. The altered type of disease, the changes observed in its physiological manifestations, and the gradual disappearance of those forms of acute inflammation which had appeared to require, and to bear, bloodletting, were with him the foundation in theory of his therapeutic method; and not only Dr. Alison, but almost all the older practitioners in Edinburgh concur in asserting that, over a period of a quarter of a century or more previous to 1850, the number and prominence of the really acute cases of disease, such as had of old required bloodletting, had been diminishing to a remarkable degree, and the field for the employment of the so-called antiphlogistic

remedies had been becoming correspondingly restricted.* On this point there is, indeed, in Edinburgh, hardly any difference of opinion among men old enough to have been personal observers of the change; although some, and perhaps most of the same observers fully admit also that bloodletting used to be extravagantly employed, and that its diminished employment now-a-days is not entirely the result of the change in disease, but partly also of a change in the minds of men. With this qualification, I have always accepted the statements of my seniors as to the facts within their observation; and without discussing the matter as a general doctrine, have been content to register such additional facts as came under my own personal notice, in hospital practice or otherwise.

Now, of these facts, unquestionably the most important are those relating to pneumonia, and to the epidemic fevers. It is upon these, accordingly, that the attention of the reader will be concentrated in the present series of papers; and I venture to believe he will there find the results of experience accurately and fairly recorded, whatever may be the theoretical conclusions he may incline to build upon them. At the same time, the practical results of inquiries into the use of a remedy so important as bloodletting, have naturally occupied a large share of my attention as a teacher of medicine; and apart altogether from the controversial discussions

* See Dr. Alison's paper in *Edinburgh Medical Journal*, March 1856, p. 782, *et seq.* See especially p. 785, and the reference to Laennec in p. 787.

above alluded to, my opinions on the subject had been carefully and deliberately formed, after full consideration of the evidence, previously to my first course of lectures on Practice of Physic in 1853-4. The conclusions at which I arrived then, and from which I have seen no occasion to vary in any material particular, may be shortly stated as follows:—

1. Bloodletting is a remedy of great power, the use of which, mainly in consequence of the strikingly beneficial effects witnessed from its employment in select cases, has often tended to degenerate into a vicious routine.

2. The circumstances in which experience has shewn bloodletting to be thus strikingly useful are—*a*, in the very early stages of acute inflammations; *b*, in uninjured constitutions; and *c*, when the disease is attended by marked inflammatory fever, or by urgent symptoms, not indicative of exhaustion, and developed with great rapidity. In the opposite class of cases—viz., where *a*, the early stage has passed; or *b*, the constitution is impaired; or *c*, the fever is attended with much debility and exhaustion, or is typhoid in character, bloodletting is always much more dangerous than useful; and, therefore its employment should always be founded on a careful consideration of the vital character of particular cases, not on the mere nomenclature, or pathological character, of the existing disease.

3. The employment of remedies without such a truly

practical selection of cases entirely vitiates the experiment, considered as one of therapeutics, by converting it into the expression of a blind and senseless routine; and, therefore, it is not to be wondered at, but quite in accordance with what was to be expected, that Dietl and others should have found that it was better to leave cases of pneumonia altogether to nature, than to bleed them indiscriminately, and to set the results against other cases otherwise treated, with the view of obtaining statistics of mortality under different methods of treatment. Nevertheless, the testimony of Dietl, and other like sceptics, to the efficacy of bloodletting *in relieving symptoms*, must be accepted as part of the evidence in favour of bloodletting in suitable cases, all the more that it comes somewhat in the form of an unwilling admission, derived from personal experience. There is, in fact, an almost complete unanimity of statement upon this point among observers, whether of the school of "expectation," or of the opposite tendency; and this concurrence as to a main fact in the argument is the more remarkable, considering the extreme diversity of theories under which bloodletting has been practised or rejected.

4. Generally speaking, statistics of the mortality of diseases under bloodletting cannot be obtained in such a form as to admit of comparison with the results of other methods. For, if carefully-selected cases only are bled, the cases so selected cannot be set against unselected cases, or against those selected for treatment on a different principle. And if cases are bled without

selection, the experiment is incurably bad, and the treatment vicious *ab initio*. The utmost that can be obtained by a therapeutic experiment on numbers of cases is to shew how bloodletting modifies the symptoms and the duration of the disease, *in those cases to which it is considered to be truly applicable.*

5. The carefully conducted inquiry of Louis, and the still more conclusive experiments of Grisolle,* have established beyond all question the power of bloodletting when employed near the beginning of the disease, and in fitting cases, to abridge the duration of pneumonia. The experiment of Louis, however, shews with equal clearness the danger of indiscriminate bloodletting, and especially of repeated bloodlettings performed without due regard to symptoms, or at an advanced period of the disease.

6. There is extreme danger in regulating the use of bloodletting, or of active remedies, in pneumonia, by the physical signs alone ; inasmuch as pulmonary condensations revealed by the stethoscope only, are, in a large proportion of cases, quite distinct in their pathology from acute pneumonia, and are indications of debility and exhaustion, rather than of inflammation. It is clearly established that, for some time after the introduction of the stethoscope, the treatment of pneumonia was most un-

* A historical analysis of both these inquiries is contained in the article above referred to, *Edin. Med. Journal*, September 1857, p. 219, *et seq.*

favourably affected by the rash application of old methods of treatment to newly discovered diseases. See on this point the *British and Foreign Medico-Chirurgical Review*, No. XXV., p. 217-219.*

7. Pneumonia as deduced from physical signs, based upon the diagnosis of pathological changes in the lungs, is not at all comparable, statistically or otherwise, with pneumonia as inferred from symptoms alone. (See the article quoted above, p. 211.) It is quite certain that the modern idea of pneumonia includes a great many cases not known as such to the older physicians.

8. Besides this, it is well established that the more intense and acute forms of pneumonia, as marked by symptoms, and as described by Cullen and Gregory, have of late years become exceedingly rare ; the great majority of cases now observed being unattended by urgent symptoms, and yielding readily to mild treatment, or getting well without treatment.

9. Hence the modern comparative disuse of blood-letting, though unquestionably well founded as a general rule of practice, ought to make allowance for exceptional cases in which this remedy may still be required ; and the *criterion* of such cases will be the urgency of the fever, pain, and dyspnœa, and the general strength and condition of the patient ; not the pathological condition of the lung as ascertained by physical diagnosis.

* *On Collapse of the Lung, and its Results, considered in relation to the Diagnosis and Treatment of certain Diseases of the Chest.*

10. The treatment by antimony, by stimulants, and by all other methods in pneumonia, ought in like manner to be regulated by the knowledge of its tendency to a spontaneous favourable termination; and, therefore, is to be addressed rather to the accidental symptoms of urgency, than to the disease in general, as a simple pathological fact. The principles of treatment in detail will be shortly considered in the next article.

III.

FIVE YEARS' HOSPITAL EXPERIENCE OF PNEUMONIA.

In the first paper of this series (originally published in May 1856) I advanced certain opinions in relation to pneumonia, and the acute diseases of the chest generally, which further experience now (March 1860) enables me to confirm. These opinions, if correct, are very important in their bearing on practice; and I shall, therefore, accompany a short statement of the facts which I have now to adduce by such an amount of commentary as may enable their bearings to be clearly understood.

While acting as pathologist to the Royal Infirmary from 1848 to 1853, I became convinced of what I had, indeed, suspected long before—that the tendency to death of many inflammatory diseases, and, in particular, of inflammation of the lungs and pleura, when uncomplicated, was very much overrated by those who had formed their ideas upon the received doctrines of the schools. This I inferred from the rare occurrence of deaths due to pneumonia and pleurisy (and, I may add, to pericarditis, peritonitis, and acute meningitis), apart from those organic diseases or surgical accidents which might be

said naturally to terminate in one or other of these acute affections. I also inferred, from my experience as pathologist, that this tendency of inflammations to a favourable result was on the increase; or rather (to put the precise character of my convictions in a clearer light), that, in proportion as we were getting rid of the severer forms of epidemic disease (fever, dysentery, scurvy, influenza), which had deteriorated the health of the population previously to 1848, we were also getting rid of the more severe and unmanageable types of acute inflammation; especially the inflammations of the serous membranes of the chest and abdomen, which I had seen in fearful activity during a few months of the year 1847, and pneumonia, which, within my own experience as a student, had been a much more fatal disease in hospital practice than it ever became after 1848.

Although it was reasonable to ascribe part of the diminished mortality of inflammations under treatment to improvements in treatment, it did not appear to me possible that the whole, or even the greater part, of the change could have been thus brought about,—1*st*, Because there were various principles of treatment in operation within the Infirmary itself, whereas the change was not in any one set of wards, but over the whole institution. 2*dly*, Because many of the cases admitted to the Infirmary were only admitted at very late stages of their disease, when treatment, however good, could be of little avail, and when active treatment was out of the question. 3*dly*, Because I had myself seen, in connection with the epidemics of 1846-48 (during which time

I had acted as resident physician), a large number of cases of inflammatory disease which I was convinced would have yielded to no treatment, and which, in fact, proved fatal with very little opportunity for treatment beyond stimulation. These cases had made a deep impression on my mind, and had convinced me that inflammatory diseases, like fevers, were to a great extent subject to unknown epidemic causes of increase and diminution, both as regards frequency and severity. This doctrine I see daily more and more reason to believe well founded. I have, in fact, no doubt whatever, that, during the last twelve or thirteen years, a very great diminution has taken place in the intensity, as well as the frequency of inflammations generally—a change corresponding in all respects with that which I have elsewhere noticed as having occurred in regard to typhus fever.*

Into the causes of this favourable change I do not propose at present minutely to inquire, although the inquiry is one of great importance. I cannot, however, refrain from stating my belief (even if it should appear at present unwarranted by precise facts) that the acute inflammations are quite as much, or very nearly as much, within the domain of the sanitary reformer as the more obviously epidemic fevers ; and further, that some even of the chronic organic diseases have already yielded, and may be expected still further to yield, to the improved habits, the better clothing, the greater abundance of food, and the diminished destitution of the population generally. Looking back over thirteen years of almost

* See No. VIII. of the present series of papers.

continuous hospital experience, I feel assured that chronic affections of the kidney and liver, and perhaps also of the heart, have diminished in frequency, as causes of death, to a most material extent. Let us hope that these changes in the right direction will become even more apparent than now, and that they will be rendered permanent as regards future generations.

In the paper alluded to at the beginning of this article, founded upon an analysis of the cases under treatment for four months of the winter session 1855-6, I remarked "that, out of the whole deaths, twenty-two in number, there have been *four* only due to what can be fairly called acute disease ; or, excluding two deaths from fever, *there have been not more than two deaths out of twenty referrible to acute inflammations.*" I further shewed that, with the exception of these two (both well marked cases of pyæmia, one from erysipelas with gangrene, the other from a fish-bone lodged behind the œsophagus), "inflammations of the lungs and pleuræ have not been, directly or indirectly, fatal in our wards, in a single instance during the present session."

I propose, in this paper, to illustrate still further the actual state of the case as regards pneumonia, or rather pleuro-pneumonia, by submitting a brief report of the mortality in my wards, in so far as it bears on this disease, during five complete years ending in December 1859. There is only one way of doing this so as not to mislead ; viz., to give an account of all the deaths, whether directly from pneumonia or not, in which decided pneumonia formed part of the disease existing at

the time of death. All these cases, so far as I can discover them, will be here found. I have to add two remarks in the way of caution. To use these data as statistics of death and cure, to be set against other statistics of death and cure, possibly founded on a different estimate of pneumonia, will be to make an unguarded and, I think, an unscientific use of them. Further, to argue from these data as confirmatory of any special principle of treatment in pneumonia, will be wrong; inasmuch as my treatment, though I may venture to hope it has not been a bad treatment in fact, has not been at any time founded on special or peculiar principles; but simply on the watchful adaptation of means to ends in the individual case. Indeed, in our hospital, freely admitting, as we do, cases in all stages of disease, often utterly neglected before admission, and, when not neglected, treated in every conceivable manner, it is evident that we must have the most ample opportunities of seeing what nature does and can do, either unassisted by art, or, in some instances, worse than unassisted. It should be understood, too, as respects the ordinary physicians of the hospital, that though they have a limited power of selection (which is always exercised in favour of urgent cases by preference), they have practically none of that power of rejection of cases supposed to be unsuitable, which is, by custom, allowed to a large extent in certain other wards. It may fairly be assumed, therefore, that if the Edinburgh Infirmary has admitted many of the most urgent cases of pneumonia from among the poorer classes of the community, not the least

desperate, nor the least neglected, of these cases have passed through my hands as an ordinary physician.

For several years before 1855, while I was occupied as assistant-physician and junior acting physician to the Infirmary, the cases under my personal care were, to a disproportionate extent, instances of chronic disease, and were limited to the male sex. Since that period, owing to arrangements among the ordinary physicians with regard to clinical teaching, into which it is unnecessary to enter here at large, I have never had under my care fewer than fifty beds fairly divided between male and female patients; all the male, and by much the greater proportion of female, beds having been devoted to ordinary miscellaneous cases, most of them severe and urgent; and the succession of these having been, as a rule, fully more rapid and varied than in the wards of the other ordinary physicians, especially during the winter months. I shall, therefore, record my experience only from the year 1855, taking in so much only of the period preceding the changes referred to as is necessary to complete that year, and make a five years' survey from 1855 to the end of 1859. I believe, however, that my experience before 1855 was exactly in accordance with that presently to be related, in respect to the point now under discussion.

Here, then, is my entire personal experience of fatal acute inflammation of the lungs, whether simple or complicated, during these five years.*

* To ensure accuracy and completeness, I have not only searched carefully through the ward-books, but also through Dr. Haldane's regis-

In 1855, the only case distinctly of this kind that occurred to me was one of double pleurisy, with pyæmic abscesses in the lung, from a fish-bone imbedded in the cellular tissue behind the œsophagus. This case is one of great interest, and is recorded with some detail in the former paper on this subject (page 14). It was originally admitted to the female clinical ward, and was afterwards sent up to my ward by the resident physician, under the impression that it was a case of fever, the patient being at the time in a state of great exhaustion. The girl, a respectable domestic servant, had been under treatment outside the hospital, and the fish-bone was supposed by herself, and by the surgeon who attended her, to have been removed.

In 1856, I find three fatal cases of pulmonary inflammation ; all, however, evidently secondary to grave disorders, sufficient of themselves to compromise life, and of older standing than the pneumonic affection. One of these patients, John R., æt. 40, was said to have been an epileptic ; he had fallen (it was said) during a fit, and had injured his head ; he lived for eight days with all the signs of compression of the brain, and was afterwards found (as was suspected before death) to have fractured the skull in the lateral regions and at the base. The pneumonia in this case presented hardly any symp-

ters of the Pathological Department ; and further, I have had a complete list made out by Mr. Welsh (to whom I would express my obligations), of all the fatal cases of pneumonia, pleuro-pneumonia, or broncho-pneumonia, simple and complicated (in all 40 cases), returned in the statistical register of the entire hospital from 1855 to the end of 1859.

toms. In another case, Robert S., æt. 43 (like the preceding, admitted to the ward for noisy patients, of which I had then the charge), there was severe maniacal delirium tremens, which was afterwards discovered to be complicated with fracture of the first rib, diffused abscess around the seat of the fracture, suppuration of the shoulder-joint, and pyæmia. This case is noticed in the *Edinburgh Medical Journal* for August 1856, p. 129. The third case, Laurence C., though nominally under my care, was treated in Ward VI. during my absence from town. It was one of broncho-pneumonia supervening on Bright's disease of the kidney.

In 1857 there were two fatal cases of pneumonia, both secondary to chronic organic disease. The first was only two days in hospital. He was an intemperate man, John S., the subject of Bright's disease, and, as was afterwards found, of fatty liver in a high degree. He was admitted to Ward X, on the 1st of January, far gone in double pneumonia, and with intense albuminuria and renal desquamation. He died on the 3d. The other case was that of Patrick F., æt. 39, subject to aggravated chronic bronchitis and emphysema for nearly a year before his death, which occurred from the complication of these disorders with a rather chronic condensation of the lower lobe of the left lung. He died on 8th April (Ward IV.)

In 1858 there were two cases of pulmonary inflammation under treatment, and ending in death, very much resembling those recorded above. One, Anne S., æt. 39 (died 16th October, Ward XVI.), was complicated with

Bright's disease of the kidney, in an advanced stage. The other, James B., æt. 30 (died 5th November, Ward IV.), was a case of emphysema and bronchitis of old standing, on which pneumonia had supervened about a week before admission. He was five days under treatment. I must also, for the sake of completeness, allude to the case of a child of 15 months, apparently of feeble organization from birth, which was brought to me evidently moribund on the 8th of July. As the mother was greatly distressed, and wished something to be done, I sent her up stairs to a ward to warm the child, and to administer a little wine. It died, however, two hours after admission, and considerable inflammation of the lungs was found by Dr. Haldane on a *post-mortem* examination.

In 1859, I witnessed, within the space of little more than a week, two more fatal cases of pneumonia, one of which only was complicated. Neither of these cases was under treatment more than forty-eight hours. The complicated case was one of severe and long-standing emphysema and bronchitis, with cardiac dilatation to a moderate degree, admitted *in extremis* on the 4th June, and dying on the 5th June (Ward XV., Bridget D.) The other case was that of a man of excessively intemperate habits (Thomas M'C., æt. 55), admitted into Ward IV. on the 27th May, with absolutely complete hepatization of the upper lobe of the right lung, which was so much enlarged by inflammatory effusion as to cross the middle line of the sternum, and was already becoming disintegrated by suppuration, with the characteristic prune-juice expectoration in very large quantity. The

patient had been ill only about a week, but was exceedingly exhausted on admission. The treatment was almost exclusively by stimulants; but he died on the second day after entering the hospital, and the ninth of the disease. There was no organic complication.

The preceding details shew, that out of ten or eleven cases of inflammatory affections of the lungs, being absolutely the whole number that can be discovered after the most diligent search, in which acute pneumonia, or anything like acute pneumonia, occurred among all the cases terminating in death under my care from 1855 to 1859, only *one* can fairly be said to be a death from idiopathic or uncomplicated pneumonia. Further, that in this one case the disease occurred in a man of excessively intemperate habits, and had gained ground so far before admission that the whole upper lobe of the right lung might be said to be in a state of destructive suppuration. I believe I may say further with truth, though from the old date of some of the cases I am not quite sure of their whole history in this respect, that in no one of all these cases had anything approaching a rigidly antiphlogistic treatment (as it is called) been pursued, either before or after admission. Many of them, indeed, had been entirely neglected; but in such as had been seen by medical men before admission, it had apparently never occurred to the medical man to use such a treatment as I have indicated; and, in particular, not one of all these fatal cases was bled. The treatment in the hospital, so far as treatment was possible, consisted of diffusible stimulants and cough mixtures,

with such food as could be taken, mild diuretics, and, in one or two, the extremely cautious use of antimony. I think it therefore perfectly clear that there is no room for the supposition of any death having been brought about by too great activity in the use of heroic remedies in any of the cases here recorded.

During the same period (1855-1859), I find from the hospital books that I have treated, on an average, from 12 to 20 cases of inflammatory disease of the lungs annually; or, in the aggregate, from 60 to 100 cases, including, in both instances, under the larger number a rather vague estimate of all kinds and varieties of disease in which acute or serious symptoms went along with the evidences of pulmonary condensation; and under the smaller, a rather restricted calculation of those cases which I regarded at the time as being genuine pneumonia. Some readers may possibly be surprised at the latitude here allowed to my statistics; but I have long been of opinion that not all cases attended with pulmonary condensation, and marked by acute symptoms, are pneumonia in the proper sense of the term, and I therefore prefer to leave it to the reader to take either the larger or the smaller number, according to his own idea of what should be called pneumonia. I have formerly made some remarks on this subject, which will indicate my own views,* and will shew on what principles I have proceeded in the naming of my cases. I

* See the two preceding papers of this series; and the different memoirs there referred to on the diseases simulating pneumonia; especially *British and Foreign Med. Chir. Review*, No. XXV., p. 221.

have already expressed my conviction, that severe cases (judging by symptoms) were the exception; mild cases the rule. Still, there were not wanting numerous cases having all the more characteristic symptoms and signs of pneumonia as described by Grisolle, and other well-known authorities; and though very serious dyspnœa and fever were exceptional, I have rarely failed to find the characteristic expectoration, which I have always estimated at a far higher value in diagnosis than any of what are commonly called the physical signs taken singly, or even, in some cases, than several of them taken together.

I have said that it is not the object of this paper to vindicate a system of treatment in pneumonia; for the simple truth is, that I have no system to vindicate. Some years ago (in 1851, I think), not very long after my appointment to the charge of a single hospital ward as assistant-physician, I was asked by an Italian physician then in London, of high reputation, and fully versed in continental opinions, what was the system pursued in Edinburgh, and what I followed myself—meaning, he said, "Are you antiphlogistic, or contro-stimulant, or stimulant, or expectant, in your practice?" I replied, "So far as I can observe, we have no system in the matter; for myself, within the last three months, I have treated different cases by all these methods, believing that what is to be treated is not so much *the pneumonia*, as the *individual patient*." I mention this, to shew the state of mind in which I began hospital

practice. Nevertheless, it may tend to some useful purpose, or at all events may afford a point of comparison or contrast with the observations of others, if I state as shortly as possible the rules by which I have been guided in the administration of remedies. Among single drugs, antimony has been first on my list: the greater number of the cases (not excluding some of those which appeared most debilitated), which were attended by marked fever and oppression, have had it in one shape or another; commonly in the ordinary form of tartar emetic, in doses varying from $\frac{1}{20}$ of a grain to 1 grain every hour or two. I have differed from some of my friends, and I believe from some of my colleagues, in giving the antimony *always alone*, *i.e.*, with nothing to mask its physiological effects; and in always simply withdrawing it, or diminishing the dose, so soon as any form of unfavourable effect, such as vomiting, purging, or depression of the system, was continuously manifested. This I believe to be better practice, on the whole, than the current method of giving the antimony with opium. Further, I have always withdrawn the antimony the instant the fever appeared to be decidedly checked, and the patient in the way of convalescence; having rarely found any relapse to follow from this practice, which has the great advantage of allowing the diet of the patient to be carefully adjusted to his capabilities of digestion in convalescence, without the chance of disturbance by a superfluous medicine. Very many mild cases, and some severe cases coming in late in the disease, have been treated by little more than common

cough mixtures. Opium has sometimes been given, but chiefly as a palliative; calomel with opium (indeed mercury in any form) has been very little employed; having been given only in obstinately continuing condensations, and then only as an experiment, with great caution, and with, as I think, little positive result. In one case, indeed, the resident physician had prescribed calomel and opium in the acute stage, and I continued it, experimentally; and I am bound to state that the patient made a good recovery; but his gums were not touched. Blistering has been reserved, for the most part, for severe cases and obstinate condensations threatening to become chronic; poultices, warm fomentations, turpentine, and, more rarely, leeches, have been used as local applications in the stage of acute pain; chloroform and other stimulating liniments, blisters, and iodine, at later stages, when pain was not removed. Bloodletting has been used in two cases by me, and in two or three more before the patients' admission. All that were bled did well, but I seldom see cases early enough, and acute enough, and in sufficiently robust individuals, to justify the use of this remedy; in which, nevertheless, I have by no means lost faith, believing it to be both very useful in fit cases, and very apt to be made a bad use of in incautious hands. Stimulants, and especially ethereal stimulants, have been freely used in cases in which the vital powers seemed in danger of failing; and that, whatever the treatment in other respects may have been. But my practice has differed entirely, if I rightly apprehend the matter, from that of the late Dr. Todd of London, in

respect that I have never given stimulants very largely, or as a matter of routine, or of aliment; almost never in slight cases, or in the early stages of the disease; and very rarely indeed to young persons, or to those not habituated to the use of alcoholic drinks. Food has been given simply according to the patient's powers of digestion; neither withheld, nor pressed: in the febrile period, bread and milk, or beef-tea with arrowroot, or both; during convalescence, a diet more generous and varied, but not over-stimulating. The *diète absolue,* if I may judge from what I was taught and have seen myself, is in no favour in Edinburgh either in pneumonia or in other febrile diseases. And to conclude, in all cases of doubt and difficulty, I have uniformly adopted the principle that nature is to be trusted to a great extent; believing that patients will recover much better and sooner under no active treatment at all, than under a routine treatment blindly enforced; or, in other words, that nature is a better manager than a bungling physician, who has always an inexorable system in hand to control her operations.

IV.

ON THE USE OF ALCOHOLIC STIMULANTS IN HOSPITAL MEDICAL PRACTICE.*

HAVING now for some years acted as one of the ordinary physicians of the Royal Infirmary of Edinburgh, with a full complement of wards for males and females, for fever and for general cases, it occurred to me quite recently, in consequence of the discussions that have taken place as to the treatment of acute disease by stimulants, that it would be of some public advantage were every hospital physician to attempt to arrive at an idea of the general average of stimulants employed by himself in his public practice; keeping in view, at the same time, the character of the cases treated, and the circumstances that have guided him in the employment of these remedial agents. It can hardly be doubted, I think, after what has been already published upon this subject, that the differences between individuals would be very great; so great, indeed, as to reveal real differences of opinion and principle of no small importance. It is probable, therefore, that, from the careful study of these differences, and the

* Read to the Medico-Chirurgical Society of Edinburgh, April 3, 1861.

calm and scientific discussion of them, some considerable benefit might accrue to general medical practice. I was glad to find, accordingly, after inquiry, that the books of the Royal Infirmary,—carefully kept, from week to week, by the Superintendent with a view to regulate the consumption of alcoholic liquors—present unimpeachable data on which to base the present investigation. For a series of years the allowance of wines, spirits, and malt liquors—*i.e.*, ale and porter (exclusive of table beer)—in every ward in the house, and the average population of the ward, have been duly registered in a manner that precludes any large amount of error, as to the quantity of alcoholic liquors actually consumed.

Of course there is a chance of error to a limited extent. The regulations of the house may have been evaded, and spirits or wine may have been surreptitiously brought in by patients and their friends, or by the officers of the house, in some instances ; or, in a very few instances, they may have been openly brought in with the consent of the medical officers. So far as my own wards are concerned, I do not believe that this has taken place in any appreciable degree—certainly not to such an extent as to vitiate the official average—except in one ward, which was only occasionally under my control, and in which a very considerable abuse was detected and remedied about a year ago. I have avoided including within the present statement any of the data so vitiated.

Another source, not indeed of error as to facts, but of possible fallacy as regards conclusions, is to be found in

the circumstance that the administration of alcoholic liquors is to a considerable extent regulated by the resident physician, to whom the making up of the diet rolls is confided, and by whom, in the absence of his principal, or when uncontrolled by the express instructions of the latter, wines and spirits may be ordered and kept on the rolls at discretion. There is little doubt that, in the management of a large number of patients, through the instrumentality of medical officers who have in the first instance often their duty to learn, and whose period of service rarely much exceeds six months, there may be occasionally a certain latitude in the administration of stimulants, not sanctioned by the opinion of the principal; and, in particular, that spirits and wine, once given, are apt to be continued on the rolls too long; that is, after the emergency for which they were ordered has passed away. Still, as errors of this kind are the direct consequence of deficient control, or of the want of a proper understanding between the principal and his subordinate, I see nothing for it but for the former to take the responsibility of what is done in his name by the latter. Of late years, inadvertent errors in this direction have been in some degree checked by the monthly statement submitted on the part of the managers to the physicians and surgeons, of the total expenditure of alcoholic stimulants in each ward; and I believe, in my own case, that the variations from this cause are within moderate limits, though, doubtless, it has often happened that the allowances have been somewhat greater than a careful and critical judgment would have led me to consider ex-

pedient. It is only necessary, therefore, to regard the averages, which will be presently cited, as being in some degree *in excess* of the direct instructions of the physician, and the true bearing of this source of fallacy will be appreciated as nearly as may be.

I have now before me the rather difficult task of attempting to trace, retrospectively, the principles by which the administration of alcoholic stimulants has been guided in my wards, in connection with the details which I shall present, and of which it may very safely be said that they have accumulated without the least thought on my part of the use that was to be made of them. In doing so, I shall have to claim the indulgence of my friends among the "nephalists," or teetotallers, as well as among the stronger heads of the opposite party, for not a few exhibitions of what they may regard as want of clear reasoning up to conclusions, or insufficiently firm hold of principles. But while treating of many important questions as undecided, let me claim the credit of being open to conviction. I do not pretend to put forth these details of practice as models, but rather as landmarks and bases for future investigation. The truth is, that, not having as yet formed, or pretended to form, any ultimate theory of the action of alcoholic stimulants in disease, I have always regarded their administration in practice as a sort of compromise, of physical and moral considerations often opposed to each other. I am quite willing, therefore, in candidly declaring my own motives of action, to consider them as admitting of modification in either direction.

My object will be, in the first instance, to make the figures before you speak for themselves, rather than to make any broad statements of personal opinion, which might be open to the objection of being framed after the view of the facts. The tables now presented to your notice (p. 69) are the condensed result of some much more elaborate ones in my possession, giving the total consumption of wines, spirits, and malt liquors in each of my wards during each month of the last five years, with the average population of the wards, and the daily average consumption per patient carefully calculated from the two preceding data. I have thought it sufficient to exhibit here the annual averages, founded on the total consumption for the year, divided by the number of days and by the mean number of patients in each ward, as ascertained by the regular weekly census of the whole hospital.

The results are in some respects such as I did not anticipate. In two particulars, indeed, they are most unexpectedly opposed to all my previous impressions. I fully believed that the use of alcoholic stimulants had been, in my hands, at least stationary, if not decreasing. It appears, on the contrary, that there has been a nearly uniform increase (taking wines and spirits together) over the whole period under review. The increase is greater, too, in the female ward than among the males. The quantity of wine in both male and female general wards reaches its maximum in 1859 ; but in 1860, with a slight decrease in the average of wine, there is a decided and much more than commensurate increase in the quantity of spirits. So that, on the whole, the consumption of

1860, in the general wards, is decidedly greater than that of any previous year.*

In another point of view the results of the table are rather startling. It appears that, among the women in the general ward, the consumption of wine during the entire period of five years is decidedly greater than in the corresponding male ward; while that of spirits is very little less, on the whole, than in the male ward, and malt liquors are at least as much consumed among the women in sickness as among the men. As the two wards are very much alike as regards the class of cases admitted, and have been during very nearly the whole period subject to exactly the same administration, it is hardly possible that this fact can be accounted for otherwise than by the apparent need for the exhibition of stimulants, or the demand for them, having been actually greater among the women under my care than among the men. I am quite certain, at least, that there is nothing in the personal convictions of myself or my assistants to account for the fact of so liberal a comparative expenditure of wine on the female side, inasmuch as all my own prejudices and those of most other people were assuredly in the opposite direction—viz., to the effect

* It is just possible that one cause of the increase may be a greater urgency and importance of the cases under my care, as a longer period of service naturally brings with it a larger proportion of such cases. I cannot, however, positively assert that this is so to such an extent as to account for any considerable part of the increase; and, on the whole, I incline to view the increase as indicating a really increased confidence in the employment of these agents as remedies within the limits assigned in the sequel.

that women ought to require, and in the better classes of society do require, a less amount of alcoholic stimulation than men when suffering under disease, and especially acute disease.

It is matter for serious consideration, and on which I prefer to avoid dogmatizing too much at present, how far this increase of alcoholic stimulants in my hands has been a genuine and necessary result of experience of their usefulness ; how far, therefore, it is an example to be followed ; or how far, on the contrary, it may have gone at any period in the direction of an excess. It is probable that more accurate comparisons with the results of the hospital practice of others similarly recorded may one day lead to more fixed views on this subject. In the meantime, I can only assure the reader that I am conscious of no prejudice either for or against the administration of stimulants, which could have interfered with the teachings of experience in the matter. Both on economical and on moral grounds, certainly, it has always been my object to keep the supplies as low as appeared to me consistent with the comfort and welfare of the sick. This has been particularly the case with the stronger spirits, which have never been given under my orders without what was considered to be an urgent necessity, founded either on the previous habits of the patient, or the extreme character of the symptoms. I have been particularly careful, in most instances, to avoid giving whisky, or even wine, to the young; and, even in fevers, have never done so without a very positive and urgent reason. By far the greater

number of the persons under 20 or even 30 years of age who have passed through my hands have indeed had no stimulants at all; and, when these have appeared to be required, a few ounces of wine for a short period, or a little table beer or light ale, has usually amply served the purpose. The same rule of caution in the administration of the stronger stimulants has been followed, so far as appeared reasonable or possible, at the more advanced ages; but it has not been considered expedient in all cases to deal rigorously with habits already in existence, and confirmed by years of indulgence previously to admission; nor has it been considered right to deny the reasonable use of the stronger stimulants to the dying, when this indulgence has obviously tended to the relief of suffering, without producing any approach to intoxication.

In accordance with these views, it will be observed that the proportion of the stronger spirits to wines is, on the whole, much lower in these returns than, I believe, will be found usual in hospital practice. I have always, in fact, preferred the more expensive luxury to the cheaper one in cases in which a choice was at all possible, and especially in the case of convalescents and young persons of sober habits. The risk of implanting bad habits of habitual indulgence (which every physician should most religiously guard against by every means in his power) has always appeared to me to be less when the stimulant used is one beyond the ordinary resources of the class frequenting the hospital; when, therefore, it is clearly seen to be a temporary

allowance, granted for special and temporary ends during the period of sickness. On this ground chiefly, or indeed almost exclusively, I have not hesitated to charge the Infirmary with the heavy expense of wine in many cases where spirits would possibly have answered every purpose of mere stimulation; and, on the same ground, I recently felt it to be a duty to decline the suggestion of a committee of the managers as to the ordinary substitution of spirits for wine. A good bitter beer or light ale would probably be far preferable to either, whether in convalescents from acute diseases, or in most chronic affections. But the habits of the Scotch labouring population are not readily changed; and beer is, unfortunately, seldom relished as a substitute for either wine or whisky. Still, it should be observed that table beer is used in considerable quantities, which do not appear in the official returns submitted with this paper.

It would be going beyond the bounds proposed in this review of facts to enter into a general theoretical dissertation on the medical uses of alcohol, or on its physiological action. But I can hardly avoid stating, what indeed it is no less than the duty of every one who so believes to state, that I have been throughout guided in the use of alcoholic stimulants by the conviction that they are really *stimulants* and *tonics*—i.e., *medicines*—and not food, properly so called. The maintaining of the opposite view by so high an authority as the late Dr. Todd, and the practice founded upon it of giving these stimulants, as a general rule, at an early period of all acute diseases, and in very frequently repeated doses

all through the day and night, I cannot but regard as a grave error, leading to the probability, almost the certainty, of an injurious excess in their use. My own habitual practice has been to give stimulants, if at all, only in very moderate quantities along with the food, and, in general, as an aid to the digestion of food,—the only exceptions being in the case of persons largely and habitually dependent upon stimulants from old and formed habits, and in a comparatively small number of acute cases for a very few days, sometimes only a few hours, to help the system over a dangerous crisis, or to co-operate with other needful remedies, such as antimonials in pulmonary inflammation. I feel quite assured that there have been no such facts in my experience of alcoholic stimulants as are mentioned in Dr. Todd's last volume*—*e.g.*, brandy, at the rate of 6 drachms every hour, given to a girl of 17 years of age in rheumatic inflammation (Case of Jane Cook, LXIV.) ; or, in another case (Sarah Butcher, LXXI., age and habits not stated), a pint a day of brandy for a month together in pyæmic inflammation. Such facts are, I cannot help thinking, the indications of a great excess, if not of an entirely wrong direction, in the use of these powerful remedies ; which, to be powerful for good and not for evil, must be maintained strictly within the limits of their medicinal action, and given, not *as being food* in themselves, but rather as *adjuvants to food—i.e.*, as aids to the gastric digestion, and stimulants of the nervous system and circulation. I quite agree, however, with Dr. Todd in

* *Clinical Lectures on Certain Acute Diseases.* London, 1860.

thinking that when stimulants really act beneficially in acute disease, they diminish the frequency of the pulse and restrain the tendency to delirium, while improving the appetite and producing an amendment in all the general symptoms. On the other hand, I cannot but demur to his inference that delirium and other bad symptoms, even if increased under small doses, are to be kept down by giving much larger quantities. To regard flushing of the face and increased feverishness, for example, as not a contra-indication, but a reason for increased administration (Lecture viii., p. 269), is opposed entirely to the practice I have followed; for, under such circumstances, or even when stimulants have not been obviously followed by reasonably good effects after a cautious trial of small quantities, I have nearly always abandoned them at once as being unsuitable remedies, at least for the time; and, accordingly, it has never occurred to me to have to "sluice the head well with cold water," or to use any of the other means recommended in Dr. Todd's fourteenth lecture, in order to distinguish "the coma of alcohol" from "the coma of disease" in cases of accidental over-stimulation.

Let me add, that another opinion prominently put forward in Dr. Todd's book appears to me to demand qualification, viz., that "it is far more dangerous to life to diminish or withdraw alcohol than to give too much." So far from having had constantly before me the fear of sacrificing life by diminishing or withdrawing a habitual allowance of stimulants, I have made it part of my regular practice to do so in most cases of persons accus-

tomed to the use of ardent spirits in excess, and especially in many cases of delirium tremens, or of other acute diseases modified by alcoholic excesses ; and though not committed to the treatment of any disease entirely without stimulants, I can entirely corroborate the remarkable statements made by Dr. Peddie in his very important memoir on delirium tremens,* viz., that the suppression of the habitual allowance is not, *per se*, dangerous in most cases ; but, on the contrary, extremely conducive to the cure. In regard to other diseases, I believe that much more mischief is done by the routine administration of stimulants than could possibly result even from their entire suppression in hospital practice. In the case of young persons affected with fever, indeed, it has been almost made matter of demonstration by statistical data, that the stimulating practice, pursued on Dr. Todd's plan, was the opposite of useful in the saving of life.† Nor can it be doubted that in pneumonia, as well as in fever, Dr. Todd's practice, as recorded by himself, was less successful than that of many practitioners who are less liberal in the administration of alcoholic stimulants. It is quite true that comparisons cannot always, or indeed often, be drawn with accuracy from limited numbers of cases in different spheres of observation ; nevertheless, I feel well assured that the recorded mor-

* *On the Pathology of Delirium Tremens, and its Treatment without Stimulants or Opium.* Edinburgh, 1854.

† See Dr. Murchison's interesting and convincing arguments in the *British and Foreign Medical Review* for October 1860. The subsequent controversy in the *British Medical Journal* and *Lancet* for November, appears to me only to strengthen the original case.

tality from the cases of pneumonia or fever occurring under Dr. Todd's care in King's College Hospital, would be regarded as excessive in the Edinburgh Royal Infirmary. On this subject it is sufficient for the present purpose to refer to the "Five Years' Hospital Experience of Pneumonia" (III.), and to the remarks on typhus fever (VIII.) in the present volume, although neither of these papers were compiled with a view to the present inquiry; nor would it be fair to Dr. Todd to assume that his cases were parallel to those referred to in these papers.

I may, in conclusion, be permitted to refer to the succeeding article (V.) of this series, first published in February 1858, as evidence that the opinions now put forward had been carefully considered long before this paper was thought of. The recent progress in the physiology of the subject, and especially the researches of Lallemand, Perrin, and Duroy in France, and of Dr. Edward Smith of London, appear to me to be in general confirmatory of the ideas expressed in that article; although, no doubt, some of the questions in controversy may still for a long time remain open to discussion on points of detail.

The object of this paper will be served if it shall be the means of procuring more accurate records than hitherto of the actual expenditure of alcoholic stimulants in hospital practice. Considering the vast moral issues involved in this question, and considering also the important economic interest which the governors of our public charities have in keeping within reasonable bounds the administration of stimulants, it is surely not

too much to suggest that in every hospital in this country monthly returns should be made, exhibiting, as in the Edinburgh Royal Infirmary, the aggregate amount of the various alcoholic liquors supplied in each ward, and also a calculated average of the amount supplied daily to each individual patient. By such averages, physicians would be insensibly guided to the truth ; and the results of various practice would, when carefully compared, supply data hitherto wanting for the settlement of a great many scientific questions connected with alcoholic stimulants.

Average Daily Consumption of Alcoholic Stimulants per Patient during Five successive Years, in the Royal Infirmary, Wards 4, 15, and 16.*

	1856.	1857.	1858.	1859.	1860.
General Ward, Males—					
Wines (ounces) . .	0·158	0·465	0·710	0·928	0·739
Spirits (ounces) . .	0·056	0·312	0·287	0·184	0·454
Malt liquors (pints) .	0·039	0·040	0·025	0·053	0·058
General Ward, Females—					
Wines (ounces) . .	0·446	0·534	0·799	1·498	1·200
Spirits (ounces) . .	0·295	0·312	0·223	0·164	0·510
Malt liquors (pints) .	0·064	0·069	0·048	0·061	0·048
Fever Ward, Females—					
Wines (ounces) . .	0·715	1·256	1·734	1·725	1·140
Spirits (ounces) . .	0·069	0·083	0·346	0·052	0·135
Malt liquors (pints) .	0·023	0·029	0·135	0·069	0·027

* In regard to the two latter wards, it is to be observed, that in the middle of 1858 a change took place in the distribution of fever and general cases, which has been carefully kept in view in reducing the monthly to the annual average. The tabular statement, therefore, does not correspond with the results of either ward separately calculated.

V.

THE DUTY OF THE PHYSICIAN WITH RESPECT TO ALCOHOLIC STIMULANTS.

[The following paper is an extract from a review* of Professor Miller's well-known volume on "Alcohol ; its Place and Power." It is inserted here because I have had occasion to refer to it in the preceding article, and because it gives full expression to opinions which have often found their way into practice at the bedside, and also into the lecture-room ; though, of course, in a more subdued and less obvious form. I have excluded the portions of the review which are more strictly critical, and have retained only those dealing with the general subject. I ought, perhaps, to apologize to Professor Miller for retaining his name in this connection at all ; but feeling sure, as I do, that full confidence may be placed in his friendship to this extent, I will give him his revenge by directing the reader's particular attention to a very able reply to this review in the succeeding number of the Journal, p. 833. I have referred to the reply in one place in a note.]

* * * *

It needs no extended pleading to shew, that the question of alcoholic drinks should be warily approached and guardedly discussed by the physician ; for while his advice is naturally sought, and must be given when

* *Edinburgh Medical Journal*, February 1858, p. 736.

sought, there is danger, on the one hand, that his opinion may be made a cover for social abuses of a very lamentable kind ; or, on the other, that he may be committed to reforms, not only impracticable in themselves, but tending by the very zeal of their advocates to evils worse than those they are intended to repair. Fifty or a hundred years ago, when drunkenness was not only not disgraceful but even fashionable, the physician, who had every day to combat the effects of excess in its most glaring form, had (it seems strange to say, but it is none the less true) comparatively an easy duty to perform to society. He was not called on to play, at every turn, the severe moralist ; nay, had he done so—had he frowned upon his age, rebuked its license, and sternly opposed himself to its indulgences, he would have been laughed at and disregarded—in short, considered a fool. Perhaps scandal would have found or made the opportunity of pointing her finger at him, as Horace has told us of the elder Cato—

> " Narratur et prisci Catonis
> Saepe mero caluisse virtus."

But, although the physician of the last age was not expected to perform the part of " censor morum," his duty to the individual man was none the less clearly recognised ; and if he found it necessary to place a professional veto between the dyspeptic or gouty sufferer and his quart of claret, he might then as now, indeed, be thwarted by the caprice or the infirmity of the patient, but he obtained the praise due to a man who had acted

honourably and well in the spirit of his office. Nor was there any very refined casuistry necessary in order to discharge this duty under the circumstances. It was tolerably certain that everything in the direction of temperance which the physician could effect, was a positive gain to the individual concerned, and a partial barrier against the tide of excess which overflowed society. As it was with nearly every man a principle to drink as long and as hard as his constitution would allow him, the physician was never consulted until the doubtful limit had long been overpast, and until the course to be enjoined was but too clear. In the majority of cases, indeed, the physician was consulted chiefly with a view to obtain his aid to dispense with the destructive social usage ; and his advice, though it might be neglected, was received with respect and gratitude.

The physician of the last age was, therefore, a practical missionary of temperance, though necessarily within a limited field. He was not, for the most part, an abstainer or an advocate of abstinence, in the sense in which we now use the term ; to be so would have been to place himself at war with society ; it would have simply neutralized his influence for good, by connecting his name with what was universally regarded as an unreasonable and impracticable formula of conduct. The late Dr. James Gregory used to say, that he never got a patient by water, though he had got hundreds by wine. And though Dr. Gregory was no abstainer, this was probably the nearest approach which could have been made at the time to the doctrine which has since been so widely

diffused through the agency of Total Abstinence Societies and Maine Law agitators.

We now live in a very different social atmosphere ; and the duty of the physician is correspondingly modified.

We find society divided into two factions; the total abstainers on the one hand, the advocates of alcoholic liquors, in what is considered to be their rightful and moderate use, on the other. The old set of habitual topers, who practised, and even justified, what was manifestly an excess, cannot, indeed, be said to have disappeared ; but, like Tories and Jacobites, Protectionists, Slave-traders, and other advocates of persecuted or forgotten faiths, they hide their heads in the dark places of society, and fancy themselves very much ill-used men. The physician is called on for his verdict—he is asked to take his place among the defenders or the assailants of alcoholic liquors ; and he can hardly avoid doing or saying something which shall range him with the one party or the other. We have, for our own part, no hesitation in admitting, that if a clear case could be made out for total abstinence, as a physiological necessity for the human race, or a moral duty for the individual man, the physician would be above all others bound to adopt it, both by precept and example, as a part of his creed. But if otherwise, we hold that he is free ; and more, that he should jealously guard his freedom. But in this case he may view abstinence as a simple question of expediency, and may, without any formal resolution, adopt it in practice, from motives of various kinds ; personal dislike to alcoholic liquors, or a sense of the benefits,

personal, economical, and social, involved in abstaining.

A few of what we must call the ill-advised advocates of alcoholic liquors, in the present day, have attempted to gain for them the physiological position of a necessary sustenance; and, as it is all but certain that stimulants add nothing permanently to the body in the shape of tissue, the theorists to whom we allude maintain the doctrine, that these substances prevent, for the time, or diminish, the destruction of tissue which is the result of vital change. A similar function is claimed for tea, coffee, for all diffusible stimulants of the ethereal class, and probably for some other substances. We may grant, for the moment, that the experiments on which this view is founded may be correct; that during the action of wine, spirits, and even strong beer, upon the frame (especially if taken in excess), vital processes are suspended; and that, with their suspension, destruction of tissue is sometimes diminished. But is this a desirable result, or the contrary? Are our tissues built up for the purpose of being *preserved*, or for the purpose of being *used?* And if the latter, are they in reality economised, is the body in reality benefited, by the employment, from day to day, of substances which, by paralysing the functions, interfere with the atomic changes on which life depends? which curb the healthful play of vital activity, in order to restrain the physiological disintegration of tissue? Not so; security from change is not life. By arresting the disintegration of tissue, we might (by hypothesis) be made as perma-

nent as Egyptian mummies ; but we should cease, at the same time, to be living, thinking, acting beings.

We deprecate, as strongly as the most conscientious abstainer can desire, the use of such an argument as this in favour of alcoholic liquors. The physiological fallacy on which it is based is the least part of the evil ; and, to our mind, Professor Miller has erred on the side of concession, in according to this theory even the smallest degree of force as an argument in favour of alcohol. For consider, that if this argument be good for *any* use, it is good for *all* uses of these stimulants. The physiological experiments, indeed, on which these speculations are based, go to shew that a certain amount of what we would call *excess* is necessary, to produce any tangible result in the way of stopping the disintegration of tissue. Who shall tell us when or where to stop in this perilous attempt to abate the friction of our machinery? If a pint of beer have power to effect so much saving of tear and wear, will not a further saving be effected by a tumbler of toddy, by two tumblers, three tumblers, five, six, ten tumblers ? When the brain is being disintegrated by care and distress, or by over-straining of the mind in study, in business, what remedy so potent to lull the excited emotions, and save the wasting brain, as alcohol ; carried, however, as it ought to be for such a purpose (and as we carry chloroform), right over the stage of excitement into that of forgetfulness ? The advocates of the "saving of tissue" theory, however well-meaning, may find that they have much to answer for.

But, while we deprecate this physiological pleading

for the habitual use of alcoholic liquors, as one that will serve equally to cover the excesses of the drunkard, the occasional social festivities of the sober man, the pint of beer of the labourer, and the glass of sherry of the dyspeptic, we by no means concede to the abstainer the victory in argument. We admit at once that, to the perfect and ideal man, living in the enjoyment of all natural and wholesome vital stimuli, with his senses, his mind, his muscles, his viscera, congenitally perfect, and working in perfect harmony, amid perfect hygienic conditions,—to such a being alcohol is probably, in all its forms, a superfluity, and perhaps worse than a superfluity. We freely admit, moreover, that many men, happily endowed by nature and circumstance, can live in comfort for years without feeling the want of alcoholic stimulants. Add to this, if the abstainer will, that most men who use them at all, use them occasionally or frequently when they are not strictly required, and not unfrequently when they had better have been dispensed with. These admissions we most readily make. But still, amid the tear and wear, the fag and worry, the disjointed and imperfect machinery of human life, we believe that alcoholic drinks are at times a very necessary medicine, at times a very useful help, at times a very enjoyable and harmless luxury; and in none of these respects are we willing to disown them when honestly tested by experience, and kept within bounds by reason and prudence. We are content, in this view of the case, to set aside physiological abstractions and theories for future investigation, and to take our stand on

the great broad series of facts recognised from the time of Noah downwards, that, at certain seasons "wine maketh glad the heart of man;" that at others it quickens the flagging appetite; at others relieves pain of body or mind, revives for a time the weary frame, and supports the sinking spirits under difficulties and in distressing emergencies. We recognise in these well-known facts the true application of alcoholic liquors; we claim the right of reason to guide us as to their use or abuse; and we decline to place ourselves under any pledge, real or implied, save that of moderation, and of temperance in all things—a pledge, however, which every one who rejects the other is, we think, bound to take, and to keep to the best of his power.

And, setting aside the question of disintegration of tissue, we can well understand how the arrest or slackening of a particular function, at a particular moment, may be a matter of the greatest importance to the economy. We arrest altogether the function of the brain by anæsthetics, when we wish to perform a grave surgical operation; we stop the pulse of a limb, to prevent bleeding from a wound; we place a fractured or a bruised limb at perfect rest for weeks, to allow it to heal. Why should we deny to the distressed and dyspeptic stomach (to go no further for an example), in cases where experience clearly guides us, the anæsthetic influence (were it no more) of alcoholic liquors in moderation, during its painful and protracted labours?

It is said by abstainers in general, and by Professor Miller in particular, that alcohol has its true place

among the poisons. True; but so has carbonic acid a place among the poisons, and nitric acid, and ammonia, and iodine; yet all of these form part of the air we breathe. Common salt is a poison, when taken in excess; apples and plums contain one of the most deadly of poisons, in very notable quantity. Nay, is not oxygen gas itself—the very *pabulum vitæ*, the universal solvent and alchemist of organic and inorganic nature—at once the strongest and most diffusible of stimulants, and the subtlest and most corrosive of poisons? There is hardly anything that may not become destructive, if wrongly used; hardly anything that is not restorative, if used aright. And so with alcohol.

Observe, that we do not even touch the moral and social question, how far we are bound to abstain from alcoholic liquors for the sake of an example to others. Beyond all doubt, the man who thinks that he does more good (whether as regards himself or others) by abstaining than by the opposite course, does well to abstain. On this we only say, with the apostle, "Let every man be fully persuaded in his own mind." We are now discussing the question of abstinence in a medical, not in a moral or social point of view; and, in a medical point of view, we believe the doctrine to be unsound. We are well assured that we often do right, medically, in using alcoholic drinks, under certain restrictions as to quantity and quality, which we need not stop to indicate, as they are well known to all sober and temperate, but non-abstaining men.

Let us hear upon this point the testimony of one of

the oldest and most respected of American physicians. Professor James Jackson is one of those men whom the weight of advanced years and untold experience, together with the kindly regard of all men, have not succeeded in making a dogmatist or a bigot. His recent "Letters to a Young Physician," amply demonstrate that he is still willing and able to learn, as well as to teach; that, with a spirit quick and vivid as that of a boy,—with a simplicity as great, and a heart as warm as in the first flush of youth, he has risen above the region of intellectual crotchets and moral agitations, into a serener and purer atmosphere. On this subject of temperance, as on most others, he can speak like a man who knows what he is about, and who is not to be disturbed in his carefully-formed opinions on matters of fact, by ever so much of association or agitation founded on views opposed to his own. After speaking of the use of chocolate, tea, and coffee, wine, malt liquors, and (Oh! horrible!) *brandy* in dyspepsia, he thus proceeds:—

I am fully aware of the terrible evils, which may arise from the excessive indulgence in the articles above mentioned; and I have heard it said that physicians have made drunkards by allowing the use of them. This is a matter to be considered gravely. So I thought before the *days of temperance reform*. I advise you to consider it in every case, where you think of prescribing articles capable of producing intoxication. This should be done especially as to the alcoholic articles, as the temptation is to use them, mixed with water, stronger and stronger. Accordingly, I would never order them to one whom I suspected to be deficient in prudence and self-control. But, keeping these things in mind, I have often directed the use even of brandy. In doing

this, I have been in the habit of saying to the patient, "If I ever hear of your indulging to excess in the use of this, or any similar article, I will call on you and exhort you to stop." In one instance, and only one, in the course of a long life, have I been called upon to redeem my pledge. This was in the case of a worthy lady, some twenty years after I had directed the measured use of brandy. At my request she immediately gave up the use of all spirituous and fermented liquors, and I have reason to believe that she never resumed them. I do not, then, call the risk very great of such prescriptions, when made with proper caution. In regard to the benefit in some cases of dyspepsia, and in various other cases, I have not any doubt. And, that I may tell the whole, let me say, that I have repeatedly seen very great benefit from giving wine to young children. The benefit has been particularly marked in some children struggling feebly through the period of dentition ; and I can name some to whom I had made this prescription more than forty years ago, among whom not one has shewn any peculiar fondness for wine in subsequent years. I exhort all young people in health not to adopt the practice of drinking wine. I deprecate everything which shall tend to intemperance, and I believe that many men suffer from the use of wine and spirits even in a moderate way. But I love to tell the truth, even when it is unfashionable. I believe that very many persons are benefited by the juice of the grape, and I choose to say so. Moreover, I believe that persons disposed to intemperance are not to be restrained from indulging their vicious propensity, by the abstinence of their more prudent neighbours. These are opinions at which I have arrived after much attention to the subject. Others, men of the first respectability, disagree with me entirely. Let it be so ; but I trust that the majority will agree that it is possible for them to be in the wrong, and not insist upon controlling the minority on this subject, any more than as to the question whether animal or vegetable food is the most wholesome. It is not a settled point whether woollen clothing should be worn next the skin. Shall a minority be obliged to submit to the majority in this matter?

This is, in our opinion, the whole matter in a nut-shell; and it could not possibly be better said.

* * * *

Professor Miller admits the use of alcoholics, as freely as we can for a moment be disposed to ask, in faintness or shock, in affections of the heart with enfeebled action, in dropsies, in the advanced stages of inflammation, in cases of hectic with profuse discharge (yet, in another place, he says that alcohol does nothing to sustain the system, when habitually used, but rather to exhaust it!), in "sinking," after operations; in exhaustion affecting the frame from sudden causes, in chronic general debility from positive disease, or from excessive labour, either of body or mind; lastly, "in dyspeptics of a certain class, in whom the stomach is deficient in tone and energy." With all this we agree, both as to the directions given, and the warnings against abuse; indeed, in the latter direction, we are willing to go with Professor Miller, not only to the end, but further than his words, at least, bear on the face of them. No language can be too strong or too impressive on this subject; and we very much doubt the necessity of daily doses of alcoholics, in some of the cases in which they are here apparently sanctioned. Arguing from the "abstinence" point of view, we should certainly have been less liberal than the author, as regards the case of chronic disease.

But here we reach the climax of our difficulty. How is it possible to give to wine, beer, and spirits, so large a function, in so many derangements of the system, from

so many different causes, and yet to say to the individual man, in average, but not uninterrupted health, " I debar you from the use of alcohol, except under a medical prescription ? " Whether Professor Miller goes this length or not, we have no means of discovering from his book,* but we know that this is the avowed object of the " abstinence " party in general. They tell us that alcoholic liquors are *not* food ; that they are *not* even condiments ; that they are not legitimate luxuries, nor yet proper stimulants for exhausted nature, in any state at all approaching to health ; that disease must have been fairly begun, and the physician have been called in, before they become legitimate. If total abstinence does not mean this, we submit that it means nothing at all, but what good men have preached and practised in all ages.

To this " abstinence " doctrine we object on many grounds. *First*, It is founded on a fallacy. It is not true that there is a sharp line of definition between disease and health ; or, that alcoholics have valuable properties on one side of the line, which are denied to them on the other. And we hold, that whatever is founded on error, even unconscious error, can never form a right spring of human action. *Secondly*, The doctrine in question tends to the subversion of man's moral freedom, as we see clearly enough displayed in the attempt at a Maine Law. *Thirdly*, It tends to place the license, withdrawn from the individual man, in the hands of the physician ; to make him, in fact, the conscience-keeper

* He explains, in the reply, that he does not go so far as this.

of humanity in the matter of intoxicating drinks. To this we decidedly object ; having no vocation to such an office, and knowing too well our ignorance and our infirmity to suppose that such a responsibility would be safe in our hands. *Fourthly*, The doctrine of total abstinence tends, in our opinion, to produce reaction—as absolute power, misused, tends to revolution and assassination—as too great pressure of steam tends to explosion—as the repressive Puritanism of the Commonwealth tended to the license and fatal demoralization of the succeeding age.*

We trust it is unnecessary to explain that the preceding remarks are by no means intended to justify the physician in pandering to the bad habits of his patient, or in allowing the sanction of a medical prescription to be given to a mere indulgence. It is one of our chief reasons against total abstinence as a system, that we think such a result would inevitably follow. Indeed, it is even now one of the evils of the extreme opposition to the use of alcoholic drinks under all circumstances, that physicians are much more frequently consulted than heretofore by those who wish to arm themselves against public opinion by medical authority. We fear, indeed, judging from circumstances which have occasionally come to our knowledge, that there are a few—we trust, very few—medical practitioners who make a trade of their complaisance in this respect, and know how to make a capital business out of copious bumpers of champagne and claret, which they prescribe to their patients

* See note at end of article.

in all stages of all fashionable diseases. If anything would drive an honest physician to abstain altogether from prescribing alcoholic liquors, it would be the scandal and disgrace of such a method of practice as this. The subject is one, obviously, of great delicacy ; but we dare not refrain from saying, that we have heard, in many quarters, statements to this effect. In London, especially, it is pretty generally understood that the reaction against depleting modes of practice has gone to the extent that patients are known to live, and, what is worse, to die, under the direction of their medical attendants, in a state approaching to intoxication. The practice is, no doubt, quite exceptional ; and we are happy to say that we have never heard it spoken of, except in terms of strong disapproval. But it is said to be the practice of very eminent men ; and, unless decidedly condemned by public opinion, may result in a very lax state of professional morality among those who follow it under such leaders. Not many weeks since, a patient of great intelligence, firmness, and individuality of character, told us of a prescription which he had received from a celebrated surgeon, under circumstances which illustrate this remark. He is sufferer from various chronic and distressing disorders, among which one is an external disease. It was for this last that he received the prescription in question, which was nothing less than brandy or whisky toddy *ad libitum*, every night, in addition to arsenic internally, and external applications of an ordinary kind. The patient assured us, that he was specially directed to go to bed, *every night, not absolutely drunk,*

but as near it as possible. And what proves that this was not a forced interpretation in accordance with his own desires, is, that he tried the remedy (!) for several weeks, and then spontaneously broke it off, absolutely, on finding that, as might have been expected, it did not agree with him. We are happy to say that we never heard of any case, north of the Tweed, at all approaching to this in enormity; though we have known here, too, what we consider to be great abuses.

As regards the proper course to be observed by the physician, in prescribing alcoholic liquors, we think that Dr. Jackson's remarks pretty nearly exhaust the subject. They are none the worse, that they require the influence of personal character to enforce them; for this is exactly what good advice will always require. Without being himself strictly, even markedly temperate, the physician's mission as a temperance reformer will fail. We are happy to think that the general practice of our profession, and especially of its leaders, in this point of personal conduct, is but little open to objection; but if Professor Miller, or any one else, can win for us more converts from the *abuses* of alcoholic liquors, we have only to wish him again—and we do it most cordially—God speed!

[The reply of Professor Miller to one part of this review (p. 83) deserves special attention. He says, "The apprehended danger from reaction—excess resulting from abstinence—is surely a fallacy; inasmuch as, precisely on the same ground, all energetic movements against physical, social, or moral evil, may be suspected and disowned. Don't attempt to make men healthy,

cleanly, sober, industrious, honest, chaste, religious, all at once, or even speedily ; mending them *thus*, for a time, they may suddenly break loose, and become worse than ever. Don't go fast, for you may fall. Creep by inches, then you are safe."

On this I would merely remark, that so long as "energetic movements" are directed exclusively against what is morally or physically wrong, and so long as they thus remain in harmony with the common-sense of mankind, there is no chance of reaction. But a movement against drunkenness is one thing, and a movement against the use of alcoholic liquors is quite another thing. The former engages the sympathies of all good men, and can hardly go too far, if it be not complicated by the use of improper means. A movement in favour of total abstinence, on the contrary, can only have the result of banishing the habitual use of wine or beer from a certain proportion of dinner tables, and, possibly, of making a kind of social distinction among men, founded on their being "nephalists," or the contrary. But as this distinction never can be, and never ought to be, accepted by the general public as commensurate with the moral distinction between good and bad, honest and dishonest, industrious and idle, temperate and intemperate, its only effect would be to keep up a perpetual spirit of resistance on the part of men of perfect respectability, who will insist on choosing to drink wine or spirits, the more their moral right to do so is rudely challenged. And if by any chance wine could be banished for a time, by the simple tyranny of fashion, from every respectable dinner-table in the country, I apprehend the only effect would be to give a greater impulse to private indulgence, and to remove the infirm will of the drunkard from the restraining influence of society, to an extent hitherto unknown. In short, distinguishing carefully between the use and the abuse, I think it plain that a large proportion of mankind will always employ alcoholic liquors habitually, more or less ; and the proper aim of the reformer ought therefore to be, to establish the rule that *they are to be used in society, and under the eyes of sober men, and not otherwise ;* the best way of accomplishing which object is clearly to give the

sanction of thoroughly good society to their moderate and reasonable use, but only in conjunction with food, and at the tables of the sober and temperate.

The operation of the spirit license has led to an evil of which it is difficult to exaggerate the consequences among the people of this country, viz., that eating and drinking have come to be, to a great extent, practically dissociated, and a trade has been established in alcoholic liquors, separate from the trade in food. Yet all the efforts of abstainers are generally directed to the increase rather than to the removal of those legal restrictions on the supply of alcoholic liquors, which have caused such enormous mischief by creating a kind of vested interest in drunkenness, and thus marking with a degree of suspicion the claim of a large body of apparently respectable men to be allowed to pursue the trade of which they have been assigned a practical monopoly by the law. What can be expected to result from this but a total confusion as between the use and the abuse of alcoholic liquors? The curious anomaly, so often complained of by the parties concerned, that teetotal magistrates are set to regulate the legal supply of an article which they honestly believe ought not to be supplied at all to the public, is evidently only one of a long series of results of the inconsistency springing from that fatal confusion.]

VI.

INFLUENZA.

(*Lecture, Friday, November* 20, 1857.)

I INVITE your attention to-day to a subject of great importance, and very directly suggested, not perhaps by any one case now in the wards, but by a combination of circumstances which you have witnessed during the last fortnight. It is to the prevalence of certain diseases in our hospital wards, which, taken collectively, amount to the proof of an epidemic morbid tendency ; that is to say, which shew, by the extent and manner of their diffusion, the existence of a morbid influence operating temporarily upon the population at large. I cannot, indeed, shew you in the wards a single typical case of this epidemic disorder, as it is seen so frequently outside, unless it be that of the woman just admitted into the fever ward. But, although I cannot place before you the ordinary forms of the epidemic (because these are commonly too mild to be admitted into hospital), I can shew you its accidents and complications in sufficient number to furnish a text for some remarks on its nature and prevalence.

You may recollect that, at the beginning of the month, we had very few acute cases of disease, though

there were many interesting chronic cases, chiefly of abdominal affections, and almost all of organic diseases. The few acute cases that we had were fevers, and these almost all of one kind, viz., enteric fever, about which I may have more to say another time. Now, on the other hand, the wards are crowded with more or less acute cases of disease; and most of these are diseases of the chest. Let me enumerate a few of them.

There is the case of the woman already noticed (Christina H., aet. 22) as having been admitted to the fever ward. She is a healthy-looking young woman, who has been occupied as a domestic servant. She was seized, a few days ago, with shivering, succeeded by headache, pains in the limbs, sickness. Along with these there was a certain amount of catarrh, which has now settled (not very severely, however) upon the chest. The fever is now intense, and very much out of proportion to the severity of the catarrh. Headache persists, the skin is hot, the tongue loaded, the colour dingy, and the general aspect of the patient certainly goes far to justify her being sent to a fever ward. Nevertheless, I believe it will turn out to be a case not of fever, in the ordinary acceptation of the term, but of the current epidemic—which I will take the liberty of calling, if it has not already been called—*Influenza*.

Had this been the first case of influenza presented to my notice, it might have passed for a case of continued fever or typhus. But even then I should have remarked its singularly abrupt invasion, the great amount of prostration in this early stage, the extreme

severity of the headache and articular pains, as being rather out of character (so to speak), in any fever to which we have lately been accustomed. Knowing what I do of other cases, I have no doubt these symptoms are owing to influenza. The only question is, whether this woman may have influenza and fever combined. This question must remain open for the present.

Now, by considering this case of catarrhal fever, or of feverish cold (if you like to call it so), in relation with the other facts to which I shall allude presently, you will draw for yourselves the picture of the epidemic, as we have it.

The first indication we had of anything out of the usual course was, perhaps, that downward tendency of several of our cases of phthisis, which, you will recollect, I remarked to you more than a fortnight ago. It does not always happen that cases of phthisis are the first to shew a tendency to influenza, and, in this instance, it may have been a coincidence; but it is a curious coincidence, that, when we had picked out four cases of phthisis as fit subjects for trying the new remedies—the hypophosphites of lime and soda—and had noted them carefully for that object, three out of the four should have been seized with acute symptoms, within a short period of our commencing the novel treatment. I told you at the time that I had no reason to blame the remedy for this result, and that it was probably a mere coincidence; I am now disposed to believe that it was one of the first manifestations of the morbid influence of which we have since seen so much.

[Two of these patients have since died; one went out relieved; another survives, considerably enfeebled, but without acute symptoms.]

On the 11th November, we saw together a case in the female general ward, of very old-standing chest disease, apparently emphysema of the lungs, in which acute symptoms had supervened, and the patient appeared to be in extreme danger from respiratory oppression, with feverishness and bronchitis. Under a very simple treatment, this woman is now improving; but her case is, no doubt, one of the epidemic in a debilitated subject.

Shortly before this case was admitted, a boy (George M., aet. 11) was brought to the waiting-room screaming with pain, which he referred to his left side. He was also very feverish. He had not much catarrh, but auscultation left us in no doubt that there was a degree of dry pleurisy on the left side, and also a friction sound, not so well marked, over the pericardium. Under moderate leeching and opiates, he was soon convalescent; but the respiratory friction sound continued loud and characteristic, and we have detained him in the ward mainly for your benefit. I had some doubts, at first, whether this boy had not suffered perforation of the lung; but it was not so. I do not say it was a well-marked case of influenza, but I mention it by the way.

The next case was that of a boy (Alex. C., aet. 11) from the Industrial School, who had gone through a distinct attack of feverish catarrh before we saw him. The traces remained in the form of bronchitis of the smaller tubes, or rather, I suspect, a tubercular condition of the

lung with bronchitic signs. This boy has probably had an unsound chest for sometime. He is better, however; indeed, nearly well.

About this time, I thought it right to pay a visit at the Industrial School, as I had seen several cases of feverish disorders from thence, which the head-master sent up for my inspection. I found thirteen or fourteen boys smartly ill with cold of the head or chest, and several of them plainly very feverish. Coughs resounded on every side; and squill mixture, with paregoric and ipecacuanha, were greatly in demand. None of the cases were, however, dangerous.

On November 14th, I directed your attention to a very acute case (Jane H., aet. 21) of bronchitis, or broncho-pneumonia, admitted two days before. The fever was very intense on admission, but had quite subsided, before you saw the patient, under the treatment by considerable doses of tartar emetic, employed by Dr. Yellowlees from the commencement. The patient, a young girl of seemingly sound constitution, recovered rapidly,—the large doses of tartar emetic being replaced by a simple cough mixture, with small doses of antimonial wine, after the lapse of about 48 hours; as soon, indeed, as the fever shewed signs of retreating. No other medicine was required in this case.

Very different was the result of treatment, or rather of the neglect of treatment, in another case (Mary P., aet. 28) in the same ward. A young woman, the mother of a family, was seized with acute bronchitis, and lay many days neglected. She was then seen by Dr. Watson, who

after blistering the chest and administering some internal remedies, sent her into the hospital. In this case, seen by us only at an advanced stage, the fever had assumed a hectic character. Occasional flushes overspread the face; there was marked dyspnœa, with lividity; sweating was very severe every night, and sometimes in the day; and prostration very considerable. She has since had acidulous drinks, antispasmodics and opiates, and is better; but her convalescence is very slow, fever is not subdued, and I greatly fear that the seeds of tubercular disease have been laid in this case. She flushes whenever she is spoken to, and is very nervous. [This patient was lately dismissed, as she felt it necessary to go home to her family; but she is very unfit for household duties, and will probably be so for some time.]

Two other cases of chronic catarrh, with acute exacerbation, were admitted into the male ward, and were seen by you on November 18th. Both of these were street-porters, and men above 60 years of age—by no means temperate in their habits. I will not, however, dwell upon them.

The same day, November 18th, brought under your notice, for the first time, two extremely interesting cases of acute disease, having the imprint of the epidemic tendency.

One of these was a case of acute pneumonia, or pleuropneumonia, in a previously healthy man (James M'N.) of 28 years of age. The disease had run a course of many days previous to admission, having begun in symptoms altogether like simple influenza, succeeded, at

the end of a week, by pain in the right side of the chest, and difficulty of breathing, with shivering fits. We found the whole lower lobe on the right side more or less consolidated, the sputum rusty, and the fever considerable. The night of admission, before treatment had been well begun, pain occurred on the opposite (left) side, at the lower part; and this aggravation was attended with a pulse of nearly 140 in the minute, at one period, and with respirations between 50 and 60 in the minute. So soon, however, as the tartar emetic began to take effect, these symptoms subsided; and next day we noted the pulse at 78, and the respirations at 32, the skin cool and moist, and the general state quite satisfactory; though a certain amount of dull percussion, with some consonating râle, existed at the lower part of the left lung, and the physical signs on the right side were unchanged. In another day, the line of dull percussion in the right lateral region was lower by an inch and three quarters, and from this period the convalescence may be said to have begun. The operation of the grain doses of tartar emetic here was most prompt and satisfactory; and, as the fever and dyspnœa have entirely subsided, the pulse being 72 and the respirations 26 in the minute, I am of opinion that we may now suspend the remedy, and leave the cure to be completed by nature. [The convalescence was uninterrupted. The patient left on 2d December, perfectly well.]

The other case, which we saw for the first time on the 18th, was that of a woman in the fever ward. This patient (Ann M.) a domestic servant, aged 28, of rather

corpulent habit, always enjoyed good health till a fortnight ago. At that time she was seized with pains in the head, back, and limbs, with a feeling of lassitude and exhaustion, which confined her to bed for three or four days; but at the end of that time she was somewhat better, and tried to resume her ordinary work—to very little purpose, as in a day or two more she took to bed again, and has been feverish ever since.

Enteric Fever, Influenza.

I believe that this case is one of enteric fever, or typhoid fever, as it is often called. I make this diagnosis, however, chiefly on the ground that certain rose-coloured spots, which you saw me mark on the skin yesterday, bear a strong resemblance to the characteristic eruption of that fever. Should these spots continue to appear, we shall feel sure of our diagnosis; although there is at present *not a trace of abdominal complication, and all the more prominent symptoms are thoracic*, so that there is little doubt the epidemic tendency is shewing itself strongly in this woman. There are, in fact, the following very formidable symptoms:—great acceleration of the respiration; dingy lividity of countenance, with flushed cheeks; small and very frequent pulse; considerable pain in the chest, not localized; some delirium; and I have little doubt that some peculiar form of broncho-pneumonia is present, as there is a scanty sputum, deeply tinged with purple blood, and we find, on examination, limited dulness on percussion, together with consonating respiration and râle in both backs, at the lower part of the lungs. It is easy to see in this case enteric fever, complicated with

influenza, and with very serious, though ill-defined, acute disease of the lungs—a very ominous conjunction, and all the more so as the debility of the patient forbids the employment of active remedies, and we must confine the treatment to regulated stimulation. I must say, that the state of this woman appears to me perilous in the extreme.

[The sequel of this case justified our fears. On the 21st she was visited, on account of my unavoidable absence from town, by Dr. W. Begbie, who marked out additional rose-spots, thus removing all doubt as to the diagnosis. The chest symptoms, however, still predominated; there had only been one stool, and that a natural one, since admission; and there was no pain or tenderness of the abdomen to any appreciable extent. On the night of the 21st a very loose stool was passed, containing blood. Another followed next day, and another the succeeding night; the blood being in large quantity, notwithstanding the application of ice to the abdomen, and the administration of acetate of lead, with opium, internally. On the 23d, at visit, she was manifestly sinking. She had been very restless and delirious, and had three other stools, largely composed of blood. The tongue was dry and brown, and the pulse almost imperceptible. There was no additional embarrassment of breathing, and I did not examine the back; but over the right front there was marked dulness on percussion, with feeble tubular respiration and consonating râle. She died on the morning of the 24th.

Post-mortem examination shewed numerous enlarged

and congested patches of Peyer in the ileum, in a state of ulceration and sloughing; with enlarged, congested, and softened mesenteric glands. The lungs were in an extremely curious and almost indescribable condition; the right lung almost entirely devoid of air, flaccid, evidently collapsed, but shewing throughout, on section, much congestion, and here and there patches of hemorrhagic condensation; the bronchi loaded with mucus deeply stained with blood. In the left lung there was a good deal of collapse at the base and root; but, on the whole, not much disease. In neither lung was there anything like ordinary hepatization, and the pleuræ were quite smooth, and free from exudation. The spleen, as usual, was large and soft.

It is worth noticing here, that the nurse of the ward, a most careful and attentive person, was under the impression that this patient was menstruating two or three days before death, and that the patient herself had a similar impression. The examination of the uterus and ovaries shewed that this idea was erroneous. The mucous membrane was pale throughout; a gelatinous mass of mucus occupied the cervix uteri, and there was no recent corpus luteum. It is evident that the stains of blood from the bowels had led to a mistake in this particular.]

The only other case worth mentioning in illustration of the epidemic tendency, is that of a little girl, (Mary* P., æt. 11), admitted on the 19th, as she has been several

* The after history of this case will be adverted to in the chapters on Cardiac Murmurs.

times before, on account of disease of the heart. She has, I think, a contracted mitral orifice; and with this there is associated, at present, a great deal of lividity, with feverishness, and marked prostration of strength; the consequences, no doubt, of influenza acting upon organs predisposed to disease. I should think badly of this case if I had not seen it before; but this girl has repeatedly got over attacks considerably worse than the present in a very short time. She has all that elasticity of constitution which appears to be the exclusive endowment of youth; and she is in every respect a very good and hopeful little patient. [She recovered in a few days.]

Summary of Observations.—Let me now review these facts. Here, within the space of less than a fortnight, you have seen admitted into our wards (with an average population under 40) no fewer than 11 cases of febrile disease, associated with pulmonary symptoms of one kind or other. Most of these, no doubt, were complicated cases, and only one of them could be called simple influenza. But this is because simple influenza is usually too rapid and too mild a disease to be admitted to an hospital. We see here, not the disease, but the consequences and complications of the disease. In private and in dispensary practice we see the disease itself.

Of these 11 cases of chest affection,

1 was double pleuro-pneumonia;

1 was pleurisy and pericarditis;

2 were very acute bronchitis, or broncho-pneumonia, in one with a probable tubercular complication ;
1 was sub-acute bronchitis, certainly with tubercular antecedents ;
3 were sub-acute bronchitis, supervening on old emphysema of the lungs ;
1 was sub-acute bronchitis, supervening upon old valvular disease of the heart ;
1 was enteric fever, with very acute pulmonary complication ; and
1 was influenza, pure and simple.

While we have been watching these cases together, I have seen many, and heard of many more, cases of the simple and ordinary form of the disease. Not a few of yourselves have had it, and two or three have been seriously ill. Most of the cases that I have seen, however, have been remarkable for the sharpness and suddenness of the attack, and not less so for the rapidity of the passage from a state of feverish prostration to convalescence. I have found a man with a pulse of 130 at night, and next day he has been up and about. This, of course, only happens with sound constitutions. In one or two instances, it has appeared to me that an emetic, given in time, has anticipated or cut short the attack. Certainly it has been followed by great relief. For the rest, the bed, or in mild cases, the sofa, restricted diet, laxatives when required, and liberal doses of opium when there is much restlessness and exhaustion, seem to me to

comprise all the necessary treatment of ordinary cases of influenza, even when severe. In the complicated cases no rule can be laid down. Some are very amenable to remedies, others run their course in spite of treatment. You have seen illustrations of both kinds in these wards.

The most characteristic symptoms of influenza are intense feverishness, usually with great tendency to chilliness or shivering, until the patient takes to bed, and reaction is fairly established. Then come racking headache, with pains in the back and limbs, which sometimes constitute the principal source of suffering; sensation of extreme debility; total prostration of appetite, with less of thirst than is usual in fever; and with these, coryza or mild catarrh, bronchitis, bronchopneumonia, as the case may be. But though catarrh is frequent, and may be severe, *the disease is essentially a fever, not a catarrh.* Nay, the catarrh may be absent, or insignificant; not unfrequently it is so. In one of the cases I saw among yourselves, there was absolutely no catarrh; in another it was very slight. And I saw two very curious cases, in private practice, a few days since, which enable me to put this point yet more strongly. The catarrh may, in fact, be absent in the very cases in which you would, *a priori*, expect its occurrence. A gentleman (Mr. S.), who has been long afflicted with spasmodic asthma, with intervals, however, of fair good health, and with no appreciable organic disease of the chest, came to me after he had been struggling for several days with debility, prostration, chilliness, and feverish

Influenza without Catarrh.

sensations. These were with him the only manifestations of influenza. [He afterwards, at an interval of ten days, had a slight cold in the head, without fever; in the meantime, his whole family sickened with feverish colds, some of them with chest affection, from which he himself remained exempt throughout.] In another case, a gentleman (Mr. B.), who also suffers from habitual asthma and bronchitis, and in whom I suspect a morbidly enfeebled heart, sent for me in a great hurry on account of the alarming prostration produced by this strange and inexplicable "influence." He was, however, more frightened than hurt; in a couple of days he was convalescent, and the amount of bronchitis in his case never gave me the slightest uneasiness.

Even the complications in influenza are not always of a catarrhal kind, nor yet confined to the chest. Ten years ago, in connection with a great and general epidemic of influenza, I witnessed in this hospital a succession of cases such as I have never seen since that time. In the course of a few weeks there occurred, I forget exactly how many, but upwards of half-a-dozen cases of severe inflammation of all the great serous membranes conjointly—double pleurisy, pericarditis, peritonitis. Most of them were fatal; indeed, they seemed to come into the house only to die; so rapid, so uncontrollable were the symptoms, that no time was given for the application of remedies, even had remedies been clearly indicated.

It is somewhat remarkable, that the great epidemic

influenza of 1847-8 began at the same time of the year with the present one, almost to a week. You will find an account of it in the excellent monograph of Dr. Peacock of London.* That epidemic, however, came upon a population wasted by typhus and other forms of fever, and not yet recovered from the famine and destitution caused by the blight of the potato, and the high prices of grain in 1845-6. Scurvy, dysentery, and fever, preceded the influenza on that occasion, and cholera followed not very long after. Notwithstanding the recent money-crisis, and the distress likely to follow among certain classes of the working population, we may hope that we are at present more favourably situated than we were ten years ago. A short time will shew whether the present epidemic is to bear comparison with the last or not. Hitherto it has been of a very mild character, comparatively speaking. I have myself seen only one fatal case—a professional man much exposed to fatigue, and not of temperate habits, but not past middle age. He had been for some time in very poor health, without any distinctly defined disease; and, in the end, he became suddenly feverish, and died of an acute chest complication not very unlike that of our case of enteric fever. I do not know, indeed, that this can fairly be called a death from influenza, though I believe influenza to have been mixed up with the fatal result.

* *On the Influenza, or Epidemic Catarrhal Fever, of* 1847-8. London, 1848.

(*Lecture, Friday, November* 27, 1857.)

Since I spoke to you about influenza a week ago, there have been only two additions to the list of acute diseases which appear to have had their origin in it—one a case of pleuro-pneumonia, admitted only two days ago, treated both before and after admission by calomel and opium, and already in process of resolution; the other a case of genuine influenza, with all the usual symptoms, and which, like the former one, was sent up to the fever ward, as being under suspicion. I have directed her to be put in the closet, apart from the other patients; and we shall make a point of parting with her as soon as possible. So far as the wards are concerned, the epidemic does not appear to have made rapid progress this week.

Effect of Influenza on the Death-rate.—I have received the Registrar-General's report of mortality in London for the week ending November 21st. It is worth while to compare the indications in this report with those derived from our own observation as regards Edinburgh. For this purpose I have drawn up a table of those diseases whose mortality appears to be notably above the average of the season, and have calculated the existing mortality as against the corrected average of ten years. The correction I speak of is made thus:—The Registrar's table gives the mortality of each disease during the forty-seventh week of the present year, and during the corresponding week of ten previous years; from these he

deduces an average, which occupies a separate column. But before you can use this average as against the numbers of the present year, you must in every case raise it by one-tenth, to make allowance for the increase of population ; which, it is calculated, increases by one-tenth in five years.

Now, the past week has in London been one of unusual mortality *for the season ;* seeing that the corrected average for ten years makes the total mortality of the forty-seventh week of the year 1211 ; while during the past week it has been 1382. This very considerable extra mortality appears to be due chiefly to bronchitis, pneumonia, and phthisis, to which may be added hooping-cough. All of these are 20 or more deaths in excess of the average mortality of the season ; and bronchitis is in excess by the very large number of 123, shewing a mortality much more than double the corrected average of the ten years. These four diseases together have a mortality 188 in excess of the average ; while the entire excess of deaths for the week is only 171 ; the difference being, of course, made by diseases which are below the average, especially typhus, scarlatina, and small-pox, which have at present a low mortality. The other diseases which, though to a smaller extent, have contributed notably to raise the mortality of the past week above the corrected average, are—croup (with which I have included laryngitis), scrofula (the disease of the young), and apoplexy, with paralysis, the diseases of the aged ; to which we may add that somewhat vague condition called atrophy (mostly infantile), and that still more

vague cause of death called age. Both of these are considerably in excess ; and these, with the other causes stated, go to shew that the mortality of the past week in London has fallen heavily on the two extremes of life. This indeed is always the case with influenza.

But are we justified in assuming the existence of influenza as a cause of death in these cases, recorded as they are under such a variety of names ; especially when we look to the fact, that not more than nine deaths are recorded in all London, during the past week, as having occurred from influenza ? I think we are; because we may be sure that an epidemic condition which raises the whole mortality by one-seventh, which more than doubles the deaths from bronchitis, and largely increases those from other acute diseases of the chest, while the aged and the young, the apoplectic, paralytic, and consumptive, suffer out of proportion to the rest of the population—such an epidemic condition, I say, has essentially the characters attributed to influenza, by whatever name it may be called. The small number of deaths under the special head of influenza, therefore, is only one proof out of the many that the Registrar-General need not have been at the trouble of making a separate class of what he calls zymotic, or epidemic diseases. The epidemic tendencies of a given period must be sought, not in any particular class, but in an intelligent consideration of the whole mortality list. Influenza, in particular, is apt to be recorded under other names than its own ; for though well known as a morbid tendency or " influence," it is not very often a direct and

palpable cause of death. Medical men, accordingly, are slow to report a death from influenza when it can be properly placed under any other title. It is, however, the fact (as I know from other sources), that influenza has been unusually prevalent in London.

TABLE DEDUCED FROM THE REGISTRAR-GENERAL'S RETURNS (LONDON), for the week ending November 21, 1857; shewing the Rate of Mortality in the forty-seventh week of the year 1857, in regard to those diseases which are above the corrected average of the same week for ten years:—

	Average Mortality (Corrected.)	Actual Mortality.	Excess.	Excess per cent.
Hooping-Cough . .	33·5	53	20	58
Croup and Laryngitis .	13·4	26	13	94
Influenza . . .	3	9	6	...
Scrofula . . .	6·5	13	7	...
Phthisis . . .	137·6	159	21	15
Apoplexy . . .	25·6	33	7	29
Paralysis . . .	22·7	31	8	36
Bronchitis . . .	103·6	227	123	118
Pleurisy . . .	2·6	7	4	...
Pneumonia . . .	104·2	127	23	22
Atrophy . . .	30·6	38	7	24
Age	49·6	57	7	15
All Causes . . .	1211·4	1382	171	14

Additional Remarks, December 19.—The epidemic mortality in London appears to have attained its culminating point in the week ending December 6, in which the mortality from all causes was 1428, from bronchitis 242, from pneumonia 129, and from phthisis 168. Considered with reference to the season, however, this mortality is by no means so much in excess as that

indicated in the above table; and we may, therefore, possibly conclude, that the epidemic has passed its maximum in London. The next week shews a considerable decline. It is worthy of remark, that all the gentle hints and solicitations of the Registrar-General in the Weekly Reports, have not succeeded in raising the cypher of influenza above 22. In the year 1847, the stated deaths from influenza for the corresponding week were 198, those from bronchitis 343, from pneumonia 306, and from all causes 2454. The epidemic of 1847-8 was, therefore, immensely more fatal than the present one, so far, at least, as we have hitherto gone.

It appears from the returns of the Registrar-General (London) for the quarter ending September 1857, that the mortality from acute diseases of the chest was considerably below the average during the past autumn. It began to exceed the decennial average, however, in the month of October; and during the latter weeks of that month and the beginning of November, the increase was considerable, although not such as to give a decidedly epidemic character to the mortality. It was only in the second week of November that the total mortality began to be decidedly in excess of the decennial average.

In Scotland, the Registrar-General's returns shew a very large advance on the mortality from pulmonary diseases during the month of November 1857, as compared with the preceding month. Thus, in October, the deaths from bronchitis in the eight principal towns of

Scotland were only 76, while in November they were 151, or *almost exactly double.* Pneumonia in the same period increased from 53 to 76 ; while phthisis has only advanced from 212 to 228. The increase, as regards bronchitis, is most marked in Aberdeen, next in Greenock, next in Dundee, next in Glasgow, and next in Edinburgh. Influenza scarcely appears in the returns, numbering only 3 in October, and 7 in November.

Meteorology.—The weather was, on the whole, fine in November, and not very dissimilar from that of the preceding month. The barometric pressure was somewhat higher than in October—viz., 30·143 inches against 29·817 inches. The mean temperature was nearly six degrees less—viz., 45°·1 against 51°·0. The rainfall was somewhat greater, and there were somewhat more of easterly winds. It is worthy of remark, that the mean development of ozone, as tested at Greenock, was decidedly less in November than in October.

It would be interesting to know to what extent the inland districts of Scotland have been affected with influenza, and whether its manifestations have been simultaneous with those in the cities or not. From circumstances which have incidentally come to my knowledge, I am inclined to believe, that in some places in the neighbourhood of Edinburgh the appearance of influenza, in an epidemic form, was considerably later than in the city itself.

VII.

DISTINCTIONS OF TYPHUS AND ENTERIC (TYPHOID) FEVER.

(*Lecture, June 5th*, 1860.)

I AM going to say a few words to you upon a subject of great importance, which we have not, till now, had a proper opportunity of discussing for many months past —the distinction of the Enteric, or, as it is often called, "Typhoid" Fever, from the form so long and so well known here in Edinburgh, and elsewhere, under the name of Typhus. And as I observe that the doctrines which I have for some years regarded as established, and taught as being beyond reasonable doubt, have quite recently been once more brought into question in the Medical and Chirurgical Society of London,* I will ask you to observe very carefully for yourselves whatever facts our own experience affords as bearing on the subject.

Since the session began (in May) you have seen me pick out four cases from the miscellaneous list of febrile

* In a paper by Dr. Henry Kennedy of Dublin, a physician of large special experience in epidemic diseases, and to whose excellent treatise on Scarlatina I have often been indebted for valuable suggestions.

diseases sent up to the female fever ward; and these four cases you have heard me pronounce to be cases of enteric fever, on the faith of certain diagnostic peculiarities which have been described by various observers, and especially by Louis; but of which I must confess, for my own part, never to have had quite a clear idea until the publication of the researches of Dr. Jenner of London, in 1848. Founding partly on the results obtained by Dr. Jenner, and partly on my own limited experience since 1848, I have never hesitated for some years past to point out the cases of this disease as differing *essentially* from cases of typhus fever—*i. e.*, differing as small-pox differs from measles, or both of these from scarlet fever or from typhus. In other words, I have been led to regard this enteric fever as a *specific* type of disease, not, perhaps, always distinguishable in practice from all others, any more than scarlet fever, and even small-pox, are always distinguishable; but when well-characterized, always observed to follow a law of development and diffusion which marks it out as an independent disease, not confounded *in nature*, and therefore not to be confounded *in name*, with any other fever. Now I wish you to observe particularly, in connexion with these four cases, on what grounds I have placed this diagnosis; what doubts we have admitted in making it, and how those doubts have been solved. I may mention that all the four cases are now convalescent; so that I hope we shall not have in any of them that kind of confirmatory evidence which consists in finding, after death, ulceration of the small intestines, so characteristic of this kind of fever.

Observe, now, that judging from our four cases, the characteristic phenomena of this fever are not to be found either in its duration or in its severity, or in its so-called "typhoid" characters.* The presence or absence of delirium or of stupor, or of dry tongue, affords no sufficient ground of distinction. One of our cases had severe and protracted delirium, with an approach to coma; another had no delirium, but was very deaf and apathetic, with a foul and dry tongue for many days; the other two had hardly any "typhoid" symptoms at all. The heat of skin was equally variable; in one it was scarcely ever much elevated; in all the others it varied greatly at different periods, as also did the pulse. In not one of the cases was there a rapid and decided crisis; but one began to amend about the eleventh day, another about the twelfth day, while the two remaining cases were excessively protracted, and can hardly be said even now, after nearly seven weeks, to be out of danger; the fever having in one of the two degenerated into a kind of hectic type, exceedingly like that of subacute phthisis—a consequence, let me remark, which is very much more to be dreaded in enteric fever than in any other form of specific fever with which I am acquainted.

Nothing, then, can well be more variable or less characteristic than the general symptoms of this fever. I have seen it resolve itself in ten days, with the symptoms of a febricula only, or of a mild remittent fever; I

* Hence I prefer the name "Enteric," already in use in America and elsewhere, to that of "Typhoid," which is apt to cause confusion. See the following article (VIII.) of this series.

have seen it, on the other hand, last nearly as many weeks, and pass imperceptibly into organic disease. It mimics in turn not only all other fevers, but many other general or local diseases—phthisis, pneumonia, meningitis, perhaps more frequently than most others.

Next, as regards complications, we find that in two of our four cases there was intense deafness; in one, if not in both, with disorganization of the ears. In the same two there was pulmonary complication of considerable intensity, and doubtful issue; in one of them resembling broncho-pneumonia, in the other tubercular disease. The other two cases were almost perfectly free from pulmonary or other severe complication, and but for a little diarrhœa at the commencement, might have been said to be free from complication altogether.

But *there was diarrhœa, more or less, in all the four cases.* This, I need hardly say, is the particular symptom from which the name "enteric," as applied to this fever, is derived. Diarrhœa, without known cause, with light-coloured, ochrey, or blood-tinged stools, and with a tympanitic state of the abdomen, gurgling on movement, or tenderness on pressure in the right iliac fossa, are symptoms as nearly characteristic of this fever as any that can be named. And yet I would not have you trust to these symptoms too much; for some of them are absent in the majority of cases, and even diarrhœa may be very transitory, as it was in two, at least, of our four cases (not lasting beyond a few days, or even hours); or it may appear to have been determined by a purgative. Further, there may be no diarrhœa; and even in fatal

cases, with great ulceration of the intestines, diarrhœa may not appear till a very late period.* Besides, in typhus fever, and, indeed, in all fevers whatever, diarrhœa, as an incidental symptom, is by no means uncommon.

Now observe, further, that *in all our four cases, there was a peculiar eruption on the skin.* This, when present, and in sufficient amount to be identified, is the great criterion of enteric fever, as the eruption of typhus is also of that disease. I will not dwell on the character of these eruptions here in the class-room. I will ask you to study them carefully in the wards whenever you have opportunities, and particularly to contrast them with one another. Of the maculated eruption of typhus, you have two excellent examples under observation at present. I will only recall to your attention now the fact, fully demonstrated to you before on many occasions, that in most of our cases of enteric fever the eruption has been very scanty, and that we have not unfrequently hesitated for a day or two to make a diagnosis. Indeed, the first crop of the eruption is rarely quite decisive; but as soon as successive crops, even of two or three spots each, appear, all doubt is removed. But in order to be quite sure, it is usually requisite in this disease, when the eruption is scanty, to mark each individual spot with ink in such a way as to distinguish accurately the period of its appearance; which precaution against error, accordingly, you saw me take in all these four cases. This

* See a case of this kind in the preceding article (VI.) at p. 95; and another case of severe, but not fatal, ulceration at p. 126.

in typhus fever it is usually unnecessary to do, and from the number of the spots it would be impossible, even if it were necessary.

As regards the character of these eruptions, I may refer you for details to the excellent descriptions and illustrations of Dr. Jenner, with which I entirely concur. [See the "Additional Remarks" to this article, pp. 124, 125.]

As regards the characteristic phenomena of typhus, and particularly the relation of the eruption to these phenomena as we have lately observed them in Edinburgh, I have no time to enlarge at present, and I will therefore take the liberty of referring you to a short paper of mine published in July 1859,* on the changes that have occurred in our fevers during the last ten or twelve years. You will there find that typhus, like enteric fever (though certainly not to the same degree), is a disease of exceedingly variable severity and duration; that its general febrile characters, though following on the whole a certain type, are by no means to be depended on in individual cases; that its complications are few, and follow no regular rule; and that therefore, still more decidedly than in enteric fever, we have usually to fall back on the eruption for its diagnosis.

You will remark, then, that in the diagnosis of enteric fever, as compared with our old and familiar (but, happily, not *of late* very familiar) typhus, I rely not so much on any of the characters of the fever itself, though these are often very peculiar and distinctive when taken

* No. VIII. of present series.

all together; but chiefly, and in some cases exclusively, on the eruption. And the eruption I believe to be characteristic thus far, that when sufficiently abundant and well marked it shews forth quite decidedly which of the two diseases is present. When scanty in amount, and present only for a short time, it sometimes leaves us in doubt not only whether the fever present is typhus or enteric, but whether it is an eruptive or specific fever at all. Thus you saw that, in one of our four cases, I remained in suspense for two or three days, but was afterwards quite decidedly enabled to pronounce that the eruption was that of enteric fever. And as regards typhus, some of you may have observed that, in the two cases now in the ward, I did not for a single instant admit of a doubt. The eruption being a well-marked one, was to my mind conclusive evidence of the nature of the disease, without a single inquiry as to the symptoms or antecedents of the cases. The same perfectly distinctive character of the eruption, at the first visit, has often occurred to me in the case of enteric fever.

Now I presume you will ask, and you certainly will do well to ask, what proof can be presented that the two eruptions alluded to (admitting them, as I think you will do, to be distinguishable from each other in the great majority of cases) indicate two diseases, and not mere accidental varieties of one disease? I have already fully confessed to you that, unless the eruption makes the distinction, there is no sufficiently constant distinction in the symptoms ascertainable during life. Of course there is the additional *fact* of the ulceration of the Peyerian

patches—by far the most noteworthy single fact, pathologically speaking, in the natural history of enteric fever. But it will not do to found on a fact like this in the matter of diagnosis ; and, besides, even after you have settled that there are cases of fever with ulcerated intestines, you have still to settle whether these cases differ *in nature*, or only in their accidents, from other cases in many respects similar, in which the intestines are not ulcerated.

Have we, then, here before us *two distinct fevers with two distinct eruptions, or only one fever with two varieties of eruption ?*—that is the question. Let us look at it, first, in the light of the six cases at present in the ward.

Two of these cases, I have already told you, were recognized, not by a careful investigation of symptoms, but simply by a single glance at the eruption, as cases of typhus. What is the history of these cases ? They are two sisters of a family of five—namely, a mother, three daughters, and a son,—who, with a lodger, occupied a house in St. Mary's wynd, in the immediate neighbourhood of a large tan-work. Of these six persons, five* have had fever ; and we know positively that in all of these it has been typhus with eruption. The brother and the lodger you may see for yourselves in the male fever ward ; the mother was the very last case of typhus we had under treatment in our own ward, and it is now more than a month since she died (May 1st). We do

* Another daughter, the only remaining person of this group of six, has since passed through typhus fever with distinct eruption, under our observation. (July 4th.)

not know where this fever originated; there is some ground for suspicion that the mother was visiting a fever case in another ward in the hospital before she was attacked, but as I cannot trace the story distinctly, we had better not insist upon it. Be that as it may, we have here evidently a group of five cases having a common infection; and all of them prove to be typhus with distinct typhus eruption. Excepting this group, there has not been a single case of typhus in the Infirmary, I believe, since the 25th of April, and then only one from the New Town. In March there was only one case—a girl from Dalkeith. In February there was only one case—the last of a little group which, like the present group, was composed of typhus cases only. In January there were one or two other cases. In the Dispensary books I find no case of typhus recorded since March, except one of those which I have noticed as admitted here. So that for three months, at least (we may say confidently), Edinburgh has been almost clear of typhus fever, with the exception of the cases now enumerated.

Now, as regards the four cases of enteric fever, I find that three of the four were apparently quite isolated cases. This is in accordance with what we know of this disease, which has much less tendency than typhus to run into groups of cases, and is, I believe, much less contagious; but in the fourth case we have got hold of a link in a chain or group of cases, and this time the locality is not in Edinburgh, but in Penicuik, a village at nine miles' distance. I can tell you nothing about these cases from my own experience; but, fortunately, they were seen by

Dr. Thin, of Penicuik, who sent in the girl *as a case of enteric fever*, and assures me that her brother had the same fever, with a like eruption, in a severe form ; and that other cases of the disease occurred in the same house. And I know that Dr. Thin's observation is most implicitly to be trusted upon this subject, because he was a most valued pupil of mine a few years since, and had ample opportunities of studying fever during his attendance in the Royal Infirmary. Nothing short of having seen these cases with my own eyes could give me more confidence than this in telling you that they actually were cases of unquestionable enteric fever. Now, besides the cases under observation at present, I find recorded in the books of the fever wards only one case of enteric fever in May, two in April, one in March, one in February, two in January—all, so far as known to me, isolated cases ; and, further, I find the Dispensary books quite void of any reference to the disease.

Here, then, is the kernel of my argument. Fever has not been epidemic in Edinburgh for six months at least ; on the contrary, it seems probable that there have been hardly a dozen cases of typhus in all, and about as many of enteric fever, in the entire town during the whole of that period. But five cases are found in a single household, and these are all, without exception, cases of eruptive typhus. One case of enteric fever is sent in from Penicuik, and forms part of a little epidemic or local visitation there, which, we are assured by Dr. Thin, consists of enteric fever only, and not at all of typhus, as distinguished by eruption. The two diseases

are, therefore, as distinct as they can possibly be. No case of typhus has given rise to anything but typhus; no case of enteric fever has given rise to, or been associated with, anything but enteric fever.

Now this is only a small contribution to a kind of experience of which Dr. Jenner has availed himself with great care and exactness in the much greater field of London. Here, again, therefore, I may refer you to his paper, already mentioned, for further details. But there is this advantage in a small field, that you can be much more sure of exhausting your facts. No one can pretend to have had access to all, or nearly all, the fever cases of London, during however short a period. But in Edinburgh, Dr. Begbie and myself probably have seen, or have had the means of knowing about, very nearly all the fever cases; and therefore, when I declare to you that within my experience for ten years past, no instance has occurred of a decided origin of enteric fever in a group of typhus cases, or of typhus fever in a group of enteric cases, I am entitled to say that I have obtained very strong evidence in corroboration of the view that these two diseases are, in reality, different diseases, and not mere varieties of the same disease.

Last summer I made a very careful survey of the whole fever-field of Edinburgh (if I may call it so) for several months together. It was not an epidemic season; but I gathered about thirty cases of typhus and twelve of enteric fever, and into the whole details of these I inquired with the greatest possible minuteness,

visiting every one of the fever localities, except one or two in which I was quite sure that the cases were isolated. The result was that in no case could I light upon a suspicion that typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever. The details of this inquiry were published in September 1859.*

We had, however, on one occasion, in the infirmary, a melancholy proof that although typhus cannot give rise to enteric fever, or enteric fever to typhus, it is possible for persons who have passed through enteric fever to take typhus very soon afterwards. Last summer we were unable to avoid, during a certain period, the association of the two diseases in the same ward in rather excessive amount. Now, mark what followed. No case of typhus was seized with enteric fever; but no less than four out of a dozen cases of enteric fever were attacked within a few weeks with typhus, and I am sorry to say that the mother of a very interesting family of five (who all, except the father, passed through enteric fever) died of this secondary attack of typhus, caught beyond question from typhus cases in the ward where she was visiting her daughters. Both these daughters afterwards sickened of typhus; but although one of them was excessively debilitated, we succeeded in saving both. Another young girl had the two diseases in succession, but in each case mildly.

I beg you to notice very particularly these facts; for not only are they full of instruction as to the danger of associating typhus cases with enteric fever, or indeed

* No. IX. of present series.

with any other fever, in the same ward, but they form the most conclusive of all possible proofs that the two diseases are distinct diseases. Typhus fever, it is now well known, is not subject to recurrence; it is a curious and indisputable law with respect to it, that it almost never attacks again, at least within a period of years, those who have had it before. Yet here we have three members of one family, within a few weeks, seized with two distinct febrile attacks, each person so attacked having two distinct eruptions, and the attacks being separated the one from the other by a perfectly distinct convalescence. To make these cases out to be mere varieties of typhus fever would require the whole laws of that fever, as ascertained by innumerable observations here and elsewhere, to be set at nought in this particular instance.

ADDITIONAL REMARKS, CHIEFLY ON ENTERIC FEVER, WITH CASES.—APRIL 1862.

THE experience of nearly two years since the delivery of the preceding lecture has afforded much corroborative evidence of the principles of diagnosis adopted in it; and during the same period numerous very interesting researches into the mode of communication of enteric

fever in England,* have given a renewed interest to the subject of its differential diagnosis, as involving questions of great importance in sanitary science.

I have not neglected to consider carefully everything that has appeared in opposition to the "non-identity" of typhus and enteric fever; but my conviction (formed in 1850) of the unassailable character of Dr. Jenner's conclusions, as based on the evidence he presents in their favour, remains unaltered. The very small amount of typhus, and the very occasional, almost sporadic, character of enteric fever in Edinburgh during the two years, have indeed given me curiously distinct proof, had this been needed, of the practical security of the diagnosis; for it has repeatedly happened that cases of the one or the other fever have presented themselves when their occurrence was entirely unexpected, and yet I do not remember on more than a single occasion to have been much at a loss in making a diagnosis, when there was a sufficient amount of eruption. During the course of last summer, for instance, I attended in private practice a case of enteric fever which began with anomalous symptoms, and without the least trace of intestinal disorder, in a well-aired and well-drained house, quite removed from all suspicion of local causes of disease. I have reason, even

* Especially by Mr. Simon, Medical Officer of the Privy Council; Dr. Murchison; and Dr. William Budd of Bristol. The result seems to be that this fever is communicated chiefly by the decomposition of excrementitious matter, often the alvine evacuations of the sick; the products of decomposition contaminating either the air or the water, according to circumstances. I have given a review of some of the evidence in a late work on "Public Health," pp. 199, 256, 260.

now, to believe that this was almost the only case of this fever in Edinburgh at the time; yet there was practically no difficulty in at once recognizing its true character about the sixth day, when the first traces of the eruption appeared. On other occasions, a series of cases of enteric fever has been suddenly interrupted by an isolated or imported case of typhus, which has nevertheless been immediately recognized, without hesitation, through the eruption alone. During the past winter this has happened on two or three occasions. In the beginning of November an isolated case of typhus occurred in the male *general* ward under my care, having been admitted as a case of pneumonia; it was immediately recognized by the eruption, and sent to the male fever ward, under the care of Dr. Warburton Begbie, who, also, had no difficulty about the diagnosis. No other case of typhus occurred in the whole Royal Infirmary, nor, I believe, in Edinburgh, till January, when a case was admitted from Kirkcaldy into Dr. Begbie's ward; this, too, was at once recognized, and treated as typhus. In the meantime, I saw not a single case of typhus eruption from the beginning of November to the 21st of February, when I gave the female fever wards up to the care of Dr. Sanders, with a number of cases of enteric fever in one ward, and of scarlatina in another. Two days afterwards a case of typhus was reported to me, and, after communication with Dr. Sanders, I took the students to observe an eruption, which many or most of them had not seen for upwards of three months, and some of them had never seen before. The eruption was

quite characteristic. It now turned out, on inquiry, that the patient was a nurse in another ward in the Infirmary, into which an isolated case of fever had been admitted some little time before; this case, at first a doubtful diagnosis, was ultimately pronounced to be typhus; from it, there is no reasonable doubt, sprang the case of the nurse, and also that of a patient in the same ward, admitted for another disease, and secondarily infected with fever, in apparent connection with the case before mentioned. Not only so, but a subsequent investigation has proved the connection of this case with a nest of five fever cases in the Water of Leith village, some of which were *at first* supposed to be enteric fever, but not one of which had rose-spots, while one other, besides the case first mentioned, had a distinct typhus eruption. It must be plain to the reader that the testing conditions of a diagnosis could hardly have been carried to a higher degree of precision than in the instances cited.

The confidence which may justly be reposed in the eruptions of these two fevers as a means of diagnosis, will only be felt by those who take the trouble of examining the surface with some care *every day*, until the distinctive facts are clearly ascertained; and in the case of enteric fever at least, it is not only necessary to see the spots, but also, unless they are unusually numerous, to *mark them so as to know each individual spot again.* For, as Dr. Jenner has well pointed out, it is not merely the character of the spots, but their succession, that constitutes the diagnosis; the eruption of

typhus being merely a flush of mottled colour, while that of enteric fever is a more or less frequently renewed *series* of spots, appearing, a few only at a time in most instances, over the chest and abdomen :—" This successive daily eruption of a few small, very slightly elevated rose-coloured spots, disappearing on pressure, each spot continuing visible for three or four days only, is, so far as I know, peculiar to, and absolutely diagnostic of, typhoid fever." * * * " The ordinary duration of each spot is about two days, but it varies from two to six days. Fresh spots appear every day or two from the outset of the eruption, till from the twenty-first to the twenty-eighth day of the disease" (and even as late as the sixth or seventh week, in the case of a relapse). " The separate spots are circular, and of a bright rose-colour ; * * * they are somewhat elevated ; but, although perceptible to the finger pressed lightly on the surface, they possess none of the seed-like hardness of the first day's eruption of small-pox, nor are they so prominent and perceptible to the touch as the papulæ of lichen. Their surface is rounded, lens-shaped, never acuminated. No trace of vesication can be detected in their apices."* With these remarks of Dr. Jenner I entirely concur, and feel satisfied that, with proper care on the part of the observer, and a certain amount of experience, they will hardly ever mislead him.

A few extracts from the cases of last winter will

* Medico-Chirurgical Transactions, vol. xxiii. p. 26. I have seen vesication, however, certainly in one, and I think in two, instances. W. T. G.

serve to illustrate partly the well-marked types of enteric fever, and partly its strange and wide limits of variation, on which I have remarked above (pp. 111, 112), and which make the difficulty so great to many persons of understanding this disease as a separate nosological form.

The first case I shall quote is one in which, though the symptoms were most severe, and the enteric affection extremely well marked, so that I at one time quite despaired of seeing the patient recover, *the pulse only varied from* 72 *to* 88 *throughout the course of the disease.* The convalescence did not begin till about the end of the third week, or probably later, when the patient was in the last stage of emaciation and depression; yet there was hardly any delirium till about three nights before the amendment. The patient's mind, however, was more or less unsettled throughout, the memory and the sensibility to external impressions being dull, though never to the extent of positive stupor, but rather of a kind of mental abstraction or reverie, which, I think, is more characteristic of this fever than of most others. A very exaggerated case of it will be given afterwards.

Enteric Fever. Profuse Diarrhœa, with mesenteric glandular enlargement. Slow pulse throughout.

Bedside, 6th November 1861.—Alexander K., a policeman, aged 24, admitted on the 5th November. He states that he has been ill three weeks or a month, but the person who came along with him says only one week. Patient has rather a languid expression, no marked change of colour, slight heat of skin, rather pungent on the abdomen and thorax, no sweating. Pulse 84. No marked dyspnœa or oppression, slight congestion of conjunctivæ,

eyeballs prominent, pupils large. He has no headache now, but had some at a former stage of the disease. He complains chiefly of pain in the hypochonders and upper part of the abdomen. Tongue coated with light yellow fur; the papillæ rather injected, especially towards the tip. Bowels are stated (by the patient) to be regular throughout, and he says he has no complaint of the chest of the nature of cough. Surface of body moderately dark-complexioned, no distinct mottling, but rose-coloured spots here and there; some rather ill defined, but in considerable numbers, the largest distinctly elevated and disappearing on pressure. No meteorism or swelling of abdomen.

7th November.—General symptoms much the same as yesterday, with the exception of the pulse, which is only 72, slightly irregular at times, and with a tendency to the back stroke. There were two stools last night and four this morning, of the characteristic light ochrey colour. Patient now states on inquiry that he is not sure about his statement of yesterday, as to the state of his bowels, and it is evident that very little reliance can at present be put on what he says. The spots are very numerous this morning, and there are some new ones come out since yesterday. Abdomen is perfectly natural to the touch, and there is no gurgling. Up to this time there has been no medicinal treatment. (Lime-water and milk. Beef-tea. Bread or rice or arrowroot, as relished. Carefully regulated use of wine from this period onwards.)

8th November.—Patient is to-day neither better nor worse, presents slight irritation of manner. Pulse 80, slight flush on cheeks. Five stools, still loose, but without the slightest pain of abdomen. Eruption still copious, but individual spots small. Tongue dry towards the tip.

14th November.—Pulse to-day 88; bowels still loose, there being four stools last night, light coloured. Patient scalded his arm some days ago, which has caused an erythematous blush there. Cheeks flushed. No distinctly new eruption. Tongue apparently cleaning and more moist. Patient chiefly complains that he cannot take his food, except milk and beef tea, but takes his wine regularly.

15*th November.*—Patient is fully more flushed to-day; is apt to talk between sleeping and waking, has had a delirious and restless night. The bowels are open, four stools since yesterday, dark in colour; otherwise the same. Pulse 84, feeble. Tongue very brown in centre, but not dry. There is still no complaint of pain; abdomen is rather more tumid, but not at all remarkably so. Percussion of abdomen pretty natural, but in various parts of it deep palpation detects localized resistance. Splenic dulness rather large, but not inordinately so.

18*th November.*—One stool yesterday and day before. Patient is very much emaciated and languid, but heat of skin is less. Pulse as before. Abdomen less resistant. Tongue moister and cleaner, and patient has a distinct sense of improvement. [From this period the convalescence, though very slow, was quite uninterrupted.]

In lecturing on this man's case on the 15th November, I remarked:* "I fear this man is going to die; in fact, I may say that I think he has almost no chance of recovery. It is not, however, the delirium, or the mere state of typhoid exhaustion that induces me to give you this prognosis. In typhus fever I could see him worse, and even much worse, without thinking so badly of him as I do. Recollect that the fever has now run nearly three weeks, or possibly more, and we might have a favourable change at any time. But this man has enteric fever, not typhus; his severe diarrhœa has not been at all checked by the use of milk diet, and lime-water. His pulse is becoming weaker and weaker, and the use of wine does not appear to tell upon it, though I believe he is not an intemperate man. The pulse has been remarkably slow, for a fever-pulse, throughout; one day it

* Revised from notes taken by Mr. Welsh.

was as low as 72, and I hoped we were to have had a crisis, but the fever goes on, and so we cannot make much favourable account of the pulse. Here, however, is the fact that startles me most of all : there is a very peculiar uneven sensation on pressure, as of tumours below the surface, on the right side of the abdomen, between the iliac fossa and the umbilicus. I made one of you (Mr. Duncan) feel it, but I cannot think right to handle the parts much, for we are certain that the intestine is much weakened, and it might give way under squeezing ; I am of opinion that this peculiar sensation indicates a group of enlarged and softened mesenteric glands below the surface of the intestines. These appearances look very unfavourable, although even now his pulse is hardly above 80. We are supporting him with food that will give as little refuse as possible, such as strong meat juice, animal jelly, etc. ; and we shall give him an increased allowance of wine."

Further on in the session, almost exactly the same condition of the intestine proved fatal to a young girl, Bridget S., aged 18, in the fifth week of her illness, which was attended with several imperfect crises and remarkable variations, including only about 48 hours of decided delirium, neither preceded nor followed by any marked stupor.

The following case illustrates a very different phase of this remarkable disease. In this case the febrile phenomena were extremely characteristic throughout, the physiognomy was quite typical, and the diagnosis was clearly established by the eruption ; but the intestinal

disorder remained entirely in abeyance till about the 24th day of the fever, when a single loose stool was passed, followed in the next two days by three others containing a considerable quantity of blood. The case presents a close resemblance, in these particulars, to one recorded in the chapter on Influenza (Ann M., p. 94); but there was less of pulmonary complication, and the issue was more fortunate. The two cases may, however, be instructively compared, and it is also remarkable that, in the case now about to be narrated, the marked rise in the pulse from the 11th November onwards was the only fact that betrayed the advance of internal mischief, while in the case of Alexander K., recorded above, the pulse was scarcely disturbed at all, though most probably there was a still greater amount of intestinal disorganization.

Bed-side, Tuesday, 5th November.—Christina M'L., servant, aged 22, admitted on the 4th November, and stated to have been ill for 12 days. Patient has a well-marked febrile expression, and is languid, apathetic, faintly flushed, but has no bronzing nor duskiness of countenance, nor suffusion of eyes. Lips are well-coloured. Tongue very moderately coated, but inclining to be dry towards the back. Pulse 108, soft and small. Respiration not laboured, about 30 in the minute. Patient is not quite sure on what day she took ill, but thinks it was on a Monday, and from other statements, it would appear to have been Monday week. Patient had headache at the beginning of her illness, but since then no pain: she had also at the commencement shiverings and cold sweats, and altogether has been more cold than hot throughout the disease. Bowels have been inclined to constipation, especially since

Enteric Fever. No abdominal symptoms till the 4th week. Hectic flushing and rise of pulse-rate for some days, followed by diarrhœa and hemorrhage.

admission, and she states that her bowels have never been loose. Patient has no pain either in the chest or abdomen, and no difficulty of breathing. Surface at present is moderately moist; abdomen not tumid, no gurgling nor tympanitic condition; no tenderness on pressure. Patient has been nursing, and her left breast is tumid and painful; right breast is in the same condition though in a minor degree. On abdomen there are from 9 to 12 spots, of a rose colour, slightly elevated, which disappear on pressure by the finger, and have no surrounding mottling. Two of these spots were marked with ink before, the others since, admission. In all her communications patient appears to be free from cerebral disorder, but she is somewhat deaf; she does not rave at night. Patient repeats very particularly and decidedly that she has had no purging.

Wednesday, 6th November.—Patient complains chiefly of her breast, otherwise she feels better; the left breast, however, is softer than yesterday. She has only been at stool once last night, and the nurse describes the stool as natural. There is no uneasiness nor swelling of the abdomen. Two large new rose-coloured spots since yesterday, some of the others are fading. Expression continues in all respects as yesterday; pulse 100, and of the same character as before.

Thursday, 7th November.—Hardly any change in general symptoms since yesterday. Patient has still the languid expression. Tongue slightly coated, but moist; pulse 104. One stool only, which the nurse describes as well-formed. There is not the slightest swelling or gurgling of abdomen, and no pain. There are from 3 to 6 new spots, not very well defined. The old spots are dying away.

Saturday, 9th November.—Patient complains of being sore everywhere, but no localized pain; no pain in the abdomen, nor any kind of tension; no stool for nearly three days. Tongue has a silvery fur, and is moist. Skin warm, no sweating, there is a new eruption of spots in considerable number on the thorax. Breast still a little sensitive, but much diminished both in size and tension.

Monday, 11*th November*.—Pulse 110. Expression still febrile, but tongue almost perfectly clean, hardly at all dry, with slight fur. Skin warm, not pungent, with little moisture; copious eruption of sudamina. Rose-spots not distinctly increased in number since last note. Left breast still painful on handling; no pain in abdomen, and bowels natural since last report.

Wednesday, 13*th November*.—Sudamina mostly dried in, and there is no renewed rose-coloured eruption, symptoms also seem much as before, so far as deafness permits interrogation, and so far as information derived from nurse goes.

Bowels opened once the day before yesterday, stool natural, abdomen quite free from flatulence, pain, or undue rigidity. Pulse, however, for the last two days, persistently at 120, other characters the same as before. Patient lies quiet, with the exception of slight involuntary sighing or moaning. No delirium even at night. Intellect, so far as can be made out from deafness, unaffected.

Thursday, 14*th November*.—Pulse same as yesterday. Mucous crepitant râles behind at both sides, especially the right, with some dulness on percussion.

Saturday, 16*th November*.—Patient still says, as all along, that she is no worse, and she has evidently no local complaint; but pulse is 130, and smaller if anything than formerly. She is paler and more emaciated, with a slight hectic flush on her cheek, otherwise no marked febrile appearance. Conjunctivæ quite free from injection. Tongue dry and brown, but not much furred. Heat of skin rather elevated, but not remarkably so, now quite without moisture. Intellect apparently unaffected, though she is still very deaf. *To-day, for the first time since admission, the single stool passed is loose in character.*

Wednesday, 20*th November*.—Since last report, patient is in an extremely feeble and exhausted condition, without pain or any kind of suffering, but with greatly accelerated small pulse, and a frequent circumscribed flush (almost exactly similar to that of hectic), which is now very deep and livid on the left cheek. She has also been at times slightly embarrassed in breathing,

with wheezing râle in chest and a little short cough, without, however, any marked dulness on percussion, except at the base of the lungs behind. *Day before yesterday, a single loose stool, containing a quantity of blood. This has been followed by two others similar in appearance, but there has been none since yesterday morning.* Abdomen still entirely free from tension. Tongue and mouth dry and parched, but not much sordes. Some muttering delirium night before last, but at present, as always in the day time, mind seems unaffected.

Thursday, 21st November.—Patient states she is better, and seems free from suffering. Hectic flush extremely well marked on both cheeks. Pulse 136. No stool. No pain of abdomen, but perhaps a little more tension in right side of abdomen than in left. Tongue still cleaning ; very little cough.

Thursday, 5th December.—For about ten days past there has been a very slow but decided improvement, chiefly in the general strength, and in the appearance ; the hectic flush has continued to be present almost daily since last report, but is now nearly gone. Patient sits up a little in bed every day, takes food with relish, has a perfectly clean and moist tongue, and no symptom of disturbance in abdomen. Hearing is also much improved, the improvement coinciding in point of time with other symptoms. No discharge from either ear ; no soreness of back. Pulse, although diminished in frequency and increased in strength, has never been below 100 ; usually between 108 and 112 for the last few days.

[From this period convalescence uninterrupted.]

In lecture on the 29th November, I alluded to this case as follows :—" The first warning of anything like great danger in this case was the rise of the pulse, which took place, most mysteriously, when she was apparently remarkably free from enteric or any other complication. Up to a late period in the fever, we had nothing on which to found a diagnosis except the rose-

spots, taken in connection with the peculiar physiognomy, which I always pointed out to you as distinct from the dingy dull complexion, and oppressed look of typhus. The remittent character of the fever was also a diagnostic mark, but not so much so as in some cases. Sudamina then made their appearance pretty copiously, and the rose-spots ceased; the tongue at the same time had become almost perfectly clean and moist, and it was the nineteenth day of the fever, so that I thought we had got over the worst, when I was struck by the rise of the pulse, first to 110, and then higher. I signalled to you the very first approach of danger, and you may remember that we puzzled ourselves very much to discover the reason of this rise in the pulse, and we questioned both the patient and the nurse daily. There was absolutely no cough, no diarrhœa, no delirium—nothing at all, in fact. I had her raised up in bed, though I thought it hardly prudent, and at the back part of the lungs I heard, as usual in fevers, some slight crepitating râles. We gave her accordingly expectorants and a little turpentine; but she could not take them, and I was afraid to destroy her appetite, so we left her simply to nourishing diet. Meantime the pulse continued to quicken, and was 120 for several days, and then 130. At last, diarrhœa commenced, and there was blood in the stools, a clear proof that mischief had been going on in the intestines to a considerable extent. As she was now extremely emaciated and exhausted, I was afraid it was all over with her; still I did not give up hope. Bleeding from the bowels, in enteric fever, is not quite

so fatal as you might suppose. You can hardly exaggerate the danger of peritonitis; but almost all the other complications may be got over. This woman was very near gone. The continued exhaustion, and the pulse up to 130 and beyond, gave me great alarm; she became intensely deaf, too, and she had a kind of hectic flushing so exactly like phthisis that you might easily have supposed it to be an advanced case of that disease. But she has pulled through all that, and to-day I found her sitting up to dinner. I must beg you not to suppose, however, that this was altogether with my approval. I should have thought it quite too dangerous a venture in her present weak state. She was anxious to sit up, and had over-persuaded the nurse to allow her. Being done, and done safely, I venture to hope that it gives us an earnest of her recovery."

Of two cases which were fatal among fifteen or sixteen under treatment, one has been noticed above as terminating in the fifth week. The other was that of a young girl who took the fever after an exhausting attendance on two of her sisters, and who died of pure febrile exhaustion on the ninth or tenth day of the disease; and, as the *post-mortem* examination gave evidence, just at the beginning of a menstrual period. The intestines were in a rather early stage of ulceration of the Peyerian glands.

Contrasted with these severe cases were several in which the symptoms never at any time rose much above the height of a febricula, and two in which the pulse ranged from 60 to 80, the tongue was almost absolutely

clean, and the skin almost devoid of heat. One of these is under treatment at present, and, beyond a little languor and want of appetite, with occasional feelings of chill and depression, there is hardly a trace of fever at all. Her expression is perfectly natural, and she can with difficulty be persuaded to keep her bed; she meets me always with a smile, and often says that "there is nothing the matter" with her. But the invasion of the disease gave to the physician in attendance upon her at home the impression of "gastric fever," and there have been about two scanty crops of rose-spots. I have little or no doubt as to the diagnosis.

In connection with this much abused name of "gastric fever," I have to mention another very curious case —one in which acute pleurisy, accompanied or followed by embolism of the pulmonary artery, was supposed to be gastric fever by a medical practitioner of the highest character, and of great experience. An extract from another clinical lecture will serve at once for a narrative of the facts, and for a practical commentary on them:—

Supposed Gastric Fever. Pleurisy. Pulmonary Embolism.

Lecture, December 13, 1861.—"A respectable servant from a house in the New Town was admitted on the 10th December to the fever ward, having been sent in specially as a case of "gastric fever" by a medical man. You are aware that I have no liking for this name, and the present case proves that we cannot attach much importance to the disease having been so named. But as many cases of so-called "gastric" fever turn out on in-

quiry to be "enteric," I went to see this patient with the impression that it might be so in this case; all the more, that I had been told she had had a smart diarrhœa. After observing the case, I am inclined to think that it is *not* a case of enteric fever. Here are the facts: The girl (Mary S., æt. 26) was a nursery-maid, and was admitted with a febrile disorder said to be of four days' standing, but it may have been longer. At the beginning, she had breathlessness and pain below the left mamma (she is said by a relative to have been breathless since last summer, but this is not so clear). She was rather an extreme brunette as to complexion, but her skin had a good deal of anæmic pallor, and in the midst of this you could see slight flushing, and a degree of lividity of the cheeks and lips. The feverishness was considerable, the surface rather dry, no sweating; the tongue dry, brown in the centre; the papillæ injected; there was a good deal of *sordes* about the mouth. Pulse 130, regular, soft, not full. Respirations 48 in the minute (which fact, in connection with the pulse, was enough to mark this case as dangerous); the rhythm of the respiration regular, but the separate inspirations quick, gasping, not at all noisy. There was, however, no persistent *laboriousness* of respiration (remember how often I have pointed out to you this refinement of diagnosis); from which I argued that it was neither bronchitis nor asthma. Decubitus dorsal. Conjunctivæ bluish (owing to dark complexion), there being no suffusion of the membrane as in typhus. There had been diarrhœa, but then we learned from questions that the patient had had a pill of

some kind before admission; the diarrhœa followed the pill, we cannot tell whether caused by it or not.* She had one stool the morning of admission, which was natural in colour, but loose. No eruption observed before admission. Some nervous agitation and a suppressed tone of voice (not unnatural in the circumstances). Respiratory murmur on both fronts about normal; percussion also normal. It was not considered right, from her state of exhaustion, to make an elaborate examination, but I thought that in the left front the respiratory murmur was a little less full than in the other; and guided by this fact we found on the left side, on the border of the mammary region, a little fine crepitation (I called it so, not as being exactly proper pneumonic crepitation, but resembling it);† it was very indistinct and scanty. Guided once more by these facts, we had her gently raised, and we examined the whole back extremely rapidly. The left back was dull to percussion from the middle downwards, and absolutely dull from the sixth or seventh rib downwards. Respiratory murmur was puerile above, wanting below. Expectoration said to be little or none, and in particular no trace of coloured expectoration. It was not possible to be absolutely sure what the disease was, but I told you at once that I thought it was probably *not* enteric fever. I supposed it was either pneumonia or pleurisy, inclining most, in my opinion, to pleurisy. But the symptoms were out of proportion to the extent of the effusion, and

* The pill, in fact, contained croton oil, as I learned afterwards.

† As a matter of fact, I *now* believe it to have been friction.

therefore I was not satisfied with that diagnosis. The disease was acute; had I seen it earlier I might possibly have been led to give antimony, but I was afraid of the effect of antimony on the pulse and on the diarrhœa, and not knowing fully the antecedents of the case, I considered it inadmissible. Should opium be given? The dry state of the tongue, and the marked febrile oppression, were against it, although there was no apparent tendency to coma; and so, without prescribing it, I allowed a discretion at the evening visit, as to whether a small dose of morphia might not be given at night, if there was continued diarrhœa or restlessness. It was not given. I ordered a single grain of ipecacuanha every hour, in pill, to promote expectoration, but it was to be carefully watched. Dr. Watson stopped this medicine, too, at the fifth dose, not because of any sickness or positive bad effect, but because the tendency to livid exhaustion had already become apparent. Next morning the case was hopeless, and I told you so. She had wine ordered, but could not take it in any quantity, and sank rapidly soon after the visit. There will be a post-mortem examination immediately after the lecture."

[At the post-mortem examination, I stated briefly that there was probably pleurisy of the left side, *and something more*, as it was extremely rare for unilateral pleurisy to prove rapidly fatal with such severe symptoms. There was found about a pint of turbid fluid, with abundant soft lymph, in the lower part of the left pleura; the left lung being about one-half collapsed, but otherwise normal. The *left* lung was thus only par-

tially disabled; but the pulmonary artery of the *right* lung was found completely obstructed by an adherent plug of fibrin; there was no similar obstruction in the pulmonary artery of the left side. The right bronchus and the lung itself were normal. The intestines were normal. This case may be usefully compared with other instances of pulmonary embolism, to be afterwards narrated in this volume.] *

Another most interesting case, which occurred early in October last, illustrates one of the more rare incidents of enteric fever, viz., the formation of parotid and submaxillary swellings late in the disease. Several of the authorities who have specially treated of enteric or typhoid fever from personal observation, have not even mentioned this complication,† which, as is well-known, was one of the most frequently discussed of all the critical "apostases" regarded by the ancients as of favourable import in the fevers known to them. I have only seen one or two cases of parotid swelling connected distinctly with enteric fever; but in the great epidemic of the year 1847 (when the distinctions between enteric and typhus, in particular cases, were certainly not well

* See the Index of Cases.

† Trousseau is an exception to this remark; he regards the "parotides" as an almost invariably fatal complication, not only in typhoid fever, but in acute diseases generally (*Clin. Med.*, I. 170). Graves notices four cases (most probably in typhus), of which two died and two recovered. The parts infiltrated in the fatal cases were not specially the parotid glands, but the cellular and intermuscular textures (*Clin. Lectures*, 2d edit., I. 193).

understood), I well remember a certain number of very severe lingering cases of what was then called *typhus* fever, accompanied by parotid swellings ; as also a considerable number of cases of fever attended with laryngitis, or copious epistaxis, requiring surgical interference. It is possible that some of these may have been instances of enteric fever ; Dr. Murchison, however, informs me that within his experience parotid swellings are much more characteristic of typhus than of enteric fever, in which, indeed, they very rarely occur.

The remarkable tolerance of the hard work of a sailor's life, exhibited in the following case during the first fortnight or more of the disease, though not unexampled, is well worthy of attention. We afterwards ascertained that the poor fellow had been treated exactly like the other seamen on board, and had never laid himself up in the least till the vessel came into port. I have seen another case in which nearly the same thing occurred, and in which the patient died from perforation of the intestine two days after leaving his ship.

Hendrik K., aged 24, a Belgian sailor, arrived last from Dantzic, admitted October 4.

Bedside, 6th October.—Somewhat exhausted febrile appearance, with dingy, somewhat livid flush ; dry lips and tongue, considerable sordes. Pulse 92, soft. Eyes not suffused, slightly congested. Respirations 24, perfectly calm. No stupor nor delirium at present ; but said to have wandered much during the night. There has only been one stool since admission, and it was not observed. States that his bowels were very loose before admission. Skin of abdomen and chest rather freckly and rough ; several faintly-

Enteric Fever. Parotid swelling.

marked rose-spots disappearing on pressure. No marked tension of abdomen. No gurgling, but if anything, more resistance towards right iliac fossa than elsewhere. Percussion of spleen ill defined, but no manifest enlargement. Liver natural. Perhaps a little undue resistance in abdomen. Through difficulties of interpretation (as he cannot speak a word of any language known to the physicians), the history of the disease cannot be given in detail. Fortunately, however, a convalescent in the ward, himself rather feeble, is also a Belgian, and through him the following brief history is obtained with some difficulty. The patient left Dantzic three weeks ago, having been engaged in repairing the sails of the ship while it lay in the river, and drinking all the time only the river water, which is very impure, and to which he ascribes his illness. Almost immediately after this he became loose in his bowels, and has continued so ever since, but does not appear to have considered himself seriously ill till five or six days ago, when he took to his bed for the first time. [The treatment is mentioned generally below, and was mainly dietetic. The patient was carefully watched, but from pressure of other cases no formal reports were made for six days, when the parotid swelling began to appear. The eruption of rose-spots was unusually distinct and copious.]

12*th October*.—This case has not been noted since the day after his admission. The bowel complaint has been quite moderate, and in fact arrested for some days ; abdomen is also quite natural to the touch. Tongue, however, has been dry throughout, and there is tendency to sore throat with slight swelling of glands on left side of neck immediately at angle of jaw. This morning the swelling over both parotids is rather dense. Cheeks slightly flushed and dingy. Pulse 106. Treatment has been mainly by nourishing soups, with milk, bread, and a little wine. Says that he feels the soup go to his head.

14*th October*.—Pulse 88—never before below 100, and has in all respects the character of improvement—soft, fluctuating, double-beating. Less flushing ; still considerable swelling of parotids, they are on the whole, however, less swollen and ten-

der than at last visit. Two stools since last night, very loose. Tongue is somewhat moistened, though still very dry. No delirium, no pain, but says that he does not feel himself better. The eruption has been so copious that it is impossible to tell whether there are any new spots. [From this period convalescence uninterrupted.]

The last history I shall give here is one which is unique within my own experience of enteric fever, but yet not so far removed from precedents as to be without an important bearing on the general study of this disease, in its relations to other fevers and disorders of the nervous system. For in this part of the symptomatology of typhus and enteric fever, as in others, I suspect that states very different in reality have been confounded under common names, and a misleading notion of identity. To a carefully observant eye, the restless, agitated, often tremulous, sometimes violent, always unquiet delirium of the second week of typhus, passing gradually over into coma or even convulsions, and accompanied throughout by suffused eyes and a contracted pupil, resembles few other states within a medical man's experience, unless it be some combinations of opium and alcoholic poisoning, or cases of febrile delirium tremens; while, on the other hand, the much later developed, and less typical, delirium of enteric fever (even when delirium is present at all, which, in my experience, is comparatively seldom) is apt to be associated with a condition of the consciousness widely different, and marked by an entirely different state of the physiognomy and of the pupil. Perhaps the following case may be only

an extremely exaggerated example of this peculiar condition, which might easily be confounded with hysteric coma, or even with tubercular meningitis (especially if attended with squint, of which Louis and Jenner appear each to have seen at least one example), but hardly, I think, with true typhomania, as we have commonly witnessed it in our epidemics of fever, or with the coma which succeeds the delirium of typhus :—

Winifred M., apparently a healthy and rather good-looking young country girl, æt. 15, a native of Ireland, recently arrived in Scotland. No information as to antecedents. Admitted 8th February 1862.

Enteric Fever, with Cataleptic Stupor. Very dilated pupils. No delirium.

Bedside, 12*th February*.—Patient has been watched for some days past under a feverish affection of extremely ill-defined character, and complicated with nervous symptoms resembling catalepsy. Patient lies generally with her eyes closed, apparently insensible, or drowsy, except when spoken to, but then quite distinctly appreciating every remark. Pupils dilated, or at least very large ; quite equal. Tongue slowly and imperfectly protruded, but evidently voluntarily ; red at edges, furred on surface, brown, and rather dry. Skin rather dry and hot, but not pungently so. Face a little flushed. Colour of lips good ; no marked lividity. General surface not remarkably pale ; no quite distinct eruption on skin, but one or two spots resembling rose-spots—a little more distinct within the last twenty-four hours. Only two of these can be distinctly marked, both on the upper thorax. On back, no distinct eruption.

No sweating since admission. Pulse 124, very soft. Bowels moved once a day throughout, but the single discharge loose. No cough has been heard. Urine not preserved, but stated by nurse to be natural in appearance.

No complaint of pain ; but patient has not, since admission, spoken to any one. No delirium, and nothing like complete unconsciousness. (Cream of tartar drink.)

13*th February.*—The peculiar condition of the nervous system more remarkable than ever. Patient talks inarticulately, but still quite readily, and protrudes the tongue when asked. Shews perfect consciousness when spoken to, muttering in reply to questions something which proves quite unintelligible, but is so immediately following the question as to indicate clearly that it is meant for an answer. When undisturbed, lies either on the back or very far round towards the face ; quite motionless for the most part ; no agitation ; no tremor. Mouth half open. Eyes shut, or half shut. No convulsions of any kind ; expression perfectly apathetic. There is absolutely no delirium, and no pain complained of. Not the slightest trace of paralysis of the face. No squint ; no drooping of either eyelid ; but permanently dilated pupils—dilatation not quite extreme, but nearly so ; with very slight mobility on approach of light. Pupils perfectly equal in size. Nurse has no doubt that she sees quite well, as she follows objects with her eyes when told about them, and reaches out her hand to take them. No stertor. Swallowing quite easily performed, and the voluntary movements have generally the character of spontaneousness. No rigidity of the neck. Breathing perfectly uniform in character, regular, and but little hurried.

Pulse rapid and small. Heat of skin less than yesterday ; absolutely no sweating. A few additional spots apparent, and two on the back appear to leave no doubt of the existence of the true rose spots. Bowels only once moved, but stools still loose, and, according to nurse, characteristically light coloured.

The colour of the face is remarkably natural, perhaps only a little heightened after a fit of coughing occurring during the visit.

14*th February.*—Patient is, on the whole, in much the same state. Replies to questions by inarticulate sounds, which, however, are evidently intended for answers, and seem to denote consciousness ; although this appears rather more obscure than yes-

terday. Tongue decidedly dry and brown. Manner rather restless, but still no delirium. Dilatation of pupils rather less marked ; the left to-day is rather more dilated than right. Still no trace of squint, and patient appears to be at all events able to see an object held opposite the eyes, and to grasp at it, though not very accurately.

Skin cool, no more spots visible, and some doubt possibly thrown on previous ones by the discovery of vermin. One stool, same character as yesterday. Pulse 104, feeble, but regular. (White wine whey ordered.)

17*th February.*—Marked improvement as regards nervous condition. Patient is still peculiar in manner. Still tends to remain in a drowsy or half-unconscious-looking state, with dilated pupils, but is very readily led to answer questions to-day quite articulately, although her answers are often repeated over and over again in an automatic way, and her aspect when spoken to is that of a person startled from a reverie.

The pupils contract readily on the approach of a candle, while yesterday they were quite fixed.

In answer to questions, patient complains of pain in back, not at all in head. Says she is very weak, but has still good colour, and is not very much emaciated.

Spots not more numerous, but better defined, and still retaining character of rose-spots. Pulse about 98, feeble. Tongue moistening a little, still very red at the edges.

Dr. Watson notices that the favourable change in the manner has come on within the last hour.

18*th February*—There is to-day a still more obvious improvement. The pupils have nearly regained their normal contractility, although still rather large. There is not much febrile heat, but pulse 100, and tongue very dry and brown.

Still a stool daily, not so loose. In other respects not much change.

19*th February.*—Feverishness subsiding. Skin almost cool, still no sweat. Pulse 96. Tongue cleaning. About one stool daily, nearly natural. Expression much improved.

Patient now protrudes her tongue instantly on being spoken to, and replies to questions with perfect alertness, though still in a peculiar manner. Pupils still large, but contract almost naturally, only slowly, on the approach of a candle.

[From about this time I gave up the charge of the ward, but the convalescence was uninterrupted. Almost on my last official visit, traces of a roughness were observed about the skin of the wrists, evidently the commencement of a cutaneous eruption, which proved ere long to be scabies, no doubt suppressed during the fever, and appearing afterwards in its usual form. It was plain that this girl had been a good deal neglected before admission during her short residence out of Ireland.]

The clinical lecture of February 14th recalls so accurately my impressions of this curious case at the time when it was nearly at the height of apparent danger (though before the pupils became quite immobile), that I shall here add to the bedside narrative of the facts given above a few extracts from a report of the lecture :

"*Lecture, Feb.* 14.—I think this is a case of enteric fever, and I hope it is so, for otherwise it may prove worse. The girl was admitted on the 8th February, and on the 12th, two days ago, the following note was taken : (Read as above.) At this time I felt very doubtful indeed as to the evidence of enteric fever ; for the spots* were scanty, and not characteristic. I thought, however, that it might be enteric fever modified by a peculiar nervous affection ; and that was the most favourable view, on the whole, to take of it. Now, on this theory of the case there are points about it that

* The doubt was quite removed afterwards.—See Report of 17th February, above.

demand your best attention, as they are of very great practical importance. In *typhus* fever, according to my experience, the delirium precedes the coma, and accompanies it till the unconsciousness becomes complete; there is no such thing, I suspect, in typhus, as *coma without delirium;* but in this case we have a kind of half-unconsciousness, although we have not now, and never have had, any delirium at all. Moreover, there is another distinction of this state from the coma of typhus, which I hold to be *absolute*, as far as my experience goes. The state of the pupils is quite the opposite of that which is characteristic of the delirium and coma of typhus. The danger of the true febrile coma is in fact very much in proportion to the contraction of the pupil; and as for typhomania with dilated pupils (still more, with very dilated and nearly insensible pupils, as in this case), I don't think I ever saw it in genuine maculated typhus. The state of this girl, then, is utterly different from that of the coma of typhus. There are, however, various structural diseases of the brain and its membranes that may lead to dilated pupil. Can this be such a case of organic disease? We had this question fairly before our minds on the 13th, and in the long string of *negatives* in the report you will find the answer that we got to our rigid questioning of Nature in this matter. * * * There is, then, no *evidence* of organic disease, but it is just possible that there may be, after all, meningitis of the base; I hardly venture to give a diagnosis. We have ordered milk diet, and a little stimulant; and we shall carefully watch their effects."

I have only to add, that in one other case during the last winter, which I regarded at the time as one of enteric fever (though with few and ill-defined spots), possibly associated with derangement of the menstruation (this last suspicion, however, turned out, on careful inquiry afterwards, to be incorrect), we had something like the condition observed in Winifred M., viz., very dilated pupils, and a state resembling reverie, without delirium, but with more febrile exhaustion than in the case now narrated. In the case of the policeman, Alexander K., given above, there was an abstracted condition of mind throughout, delirium coming on only very late, and the pupils being large, or at least not contracted. In the absence of a sufficiently extended experience of my own upon this subject,* it is very interesting to me to observe that Louis, throughout his well-known memoir, alludes to the somnolence and the delirium of the fever he describes in terms corresponding accurately with what has been stated above ; though not drawing any contrast between this state and the true typhomania, as we know it in this country, probably because the latter was out of the range of his immediate experience at the time.† It is true that Louis, in his detailed

* Not only from the small number, absolutely, of my cases of enteric fever, but from the small *proportion* of them attended by cerebral symptoms of any degree of intensity. I have, for example, mentioned above *all*, out of sixteen successive cases last winter (with one possible exception), attended by any considerable disorder of mind.

† Especially he says of the delirium—"Il débutait chez presque tous les sujets *apres* la somnolence," "la précédait bien rarement, debutait deux, trois, cinq ou six jours et plus après elle," (ii., p. 152, 1st edition,

description of the physiognomy, does not once mention the pupils, nor do any of the other French authorities, so far as I have read them in reference to this point; but, on the other hand, on appealing to the descriptions and definitions of Drs. Jenner and Murchison, I find that they expressly contrast the dilated pupil of the one disease, *typhoid,* with the contracted pupil of the other, *typhus,* as recognized by Dr. Graves* and other well-known authorities.

1829; see also pp. 3, 4, 7, 9 of the same volume). While on the somnolence, he remarks in one place (p. 9) as follows:—" Chez quelques individus, l'assoupissement dominait, continuait sans interruption, bien qu' à un médiocre degré; il n'y avait point de délire, ou a très peu près; et, malgré les plus graves lesions, le *calme* persistait jusqu'à la mort," and immediately adds—" c'était apres la forme attribuée au typhus;" language which, quite inadvertently, as it were, shews forth the entire absence of any personal experience of *true* typhus on the part of Louis, in 1829.

* See especially his *Clinical Lectures,* 2d edit., vol. i., at pp. 179, 202, 204, 234, and elsewhere.

VIII.

REMARKS ON THE HISTORY OF EPIDEMIC FEVER IN EDINBURGH.*

SINCE the middle of May (1859), there has been a tendency to increase, as compared with the past winter, in the number of cases of typhus fever. The increase is not such as to amount to an epidemic, but may form the foundation of a few remarks. It is made more noteworthy by the fact of the disappearance, for the moment, of the enteric type of fever—the "typhoid fever" of Louis, the "gastric fever" of many old-fashioned physicians, and, of course, of their patients. (The last name, however, is quite out of place, when applied to this fever, as it too often is; for there is nothing specially *gastric* about it.) I am far from supposing, indeed, that enteric fever has taken its departure in consequence of the advent of the other. Nothing is more certain than that these two types do not exclude each other as epidemic forms. The experience of the London Fever Hospital, as recorded by Dr. Jenner, and more lately by Dr. Murchison, fully proves this. It would be more correct to say that enteric fever varies within comparatively narrow limits,

* First published in July 1859.

its latent germs being quickened into life under influences of a remarkably local kind; while typhus is a fluctuating quantity, at one time sweeping the broad masses of our crowded town populations with the destructive virulence of a plague, and at another being almost wholly absent. Typhus (truly the *plague* of Edinburgh in past times) possesses the characters of an *epidemic* disease in much greater perfection than enteric fever. It is a disease especially of the poor, or rather of these when massed together in towns; it follows the great lines of human communication in a marked manner; it is often traced quite distinctly from town to town, from family to family, nay, from person to person; in other words, it is clearly reproduced in, and carried by, the human body—*contagious*, in a word, in the sense in which the term is always used here. Nor do I know any one familiar with typhus who doubts this contagious property. Enteric fever, again, is nearly as common in the country districts as in the towns; perhaps, indeed, relatively more common; and in the towns, it is fully as common in the smaller as in the larger; further, it is not by any means confined to the poorer populations and to the crowded masses. Hence its contagious propagation may fairly be questioned; and as compared with typhus, beyond all doubt it is contagious in a far less degree; though, in the face of facts stated by observers in France and in America, I do not think we are in a position to deny its being contagious. There are many other contrasts between these fevers, on which I might insist, and which, together with the evidence

adduced by Dr. Jenner of London as to the diversity of their origin in particular cases, render it, in my opinion, no longer a matter of doubt that these two types of fever are essentially distinct diseases ;* and not, as they used to be considered, mere varieties of the same disease.

Now this theory of the essential distinctness of the two types, frequently confounded under the name of typhus fever, necessitates a revision of the entire data upon which our general doctrines of the origin and propagation of fever have been founded. If it be true, as seems likely, that certain observers have seen little else than typhus, and certain other observers little else than enteric fever, it cannot be surprising that they should have come to different conclusions. Nor is it wonderful that the confusion caused by the imperfect state of the nosology should have led to an unsatisfactory condition of the doctrine taught on this subject in the schools, even by those who have had ample opportunities of observing both kinds of fever. The records of our hospitals, and the written opinions of our most distinguished authorities, shew that, up to about ten years ago, no general conviction existed in this country that these two fevers were more than mere varieties of the same disease. And even now, it is only in the London Fever Hospital (the scene of Dr. Jenner's labours) that there has hitherto been found a sufficiently wide and accurate basis for an investigation into the laws of these two fevers, separately considered ; an in-

* See the preceding article, in which also new evidence is referred to of the distinct origin, as to locality, of the two fevers.

vestigation, however, which has been carried out by Dr. Murchison in an elaborate paper in the last volume of the *Medico-Chirurgical Transactions*,* in such a manner as hardly leaves anything more to be extracted from the data which he has turned to such good account.

Having been led by the cogent arguments of Dr. Jenner in 1849-50 to abandon the doctrine in which I had been instructed, and of which I had been up to that time more or less an adherent, I have looked forward to an opportunity of adding to the facts bearing on this much-controverted question, by a published summary of my own hospital experience. Fortunately for Edinburgh, however, cases of fever have since this period been singularly few and scattered; and though everything has tended to confirm my convictions of the "non-identity" of typhus and enteric fever, I have hitherto been unable to bring to the solution of disputed questions anything worthy of comparison, in point of importance, with what has been contributed elsewhere. In fact, since the year 1853, the sum of all kinds of fever in the Edinburgh Royal Infirmary has only once reached the number of 200 in a year; and probably not more than seven or eight cases per month, on an average, have come under my own notice, including the numerous anomalous fevers which have prevailed, and which have sometimes quite overborne the numbers of genuine typhus and enteric fever added together. Of

* *Medico-Chirurgical Transactions* for 1858. "Contributions to the Etiology of Continued Fever; or an investigation of various causes which influence the prevalence and mortality of its different forms."

late, especially, it has more than once happened that a considerable portion of an academic session has passed over, without my having been able to shew the students a single really characteristic example of eruptive typhus; and during the last winter session, as also during the preceding summer, several months occurred during which no case of fever of any kind was admitted to my wards. During the past month of May, although the number of fevers, on the whole, was larger than in the preceding month, it twice happened that the only fever ward in the house open for females was very nearly empty. And a similar absence of new fever cases has not unfrequently, I believe, been observed on the male side. Further, I have frequently, of late, made inquiry as to the state of some of those closes and tenements which used to be the almost constant hotbeds of fever; and have almost always been informed that they were free from disease.

The records of the Royal Infirmary, for the twelve years preceding 1849, shew a very marked contrast with these satisfactory details of the public health. On four years only out of the twelve were the admissions of fever cases below 1000; three times they were between 1000 and 2000, twice between 2000 and 3000, twice between 3000 and 4000; and on one year, viz., 1848, the admissions of fever reached the frightful amount of 4693. The epidemic which attained this stupendous climax will not readily be forgotten by any one who had to do with it. It began in March 1847, and continued to increase, at first rather slowly, and almost exclusively

among the Irish families, chiefly in the Cowgate and West Port. About midsummer it took a wider range, ran rapidly up to a height, and then, the disease having outrun all the ordinary and extraordinary means of accommodation, the managers had to avail themselves of the space around the Infirmary, which, by means of tents and sheds, was enabled to afford such shelter as could be provided on the spur of the moment to 628 fever cases at once, besides a considerable number of ordinary cases of disease, and a very large staff of nurses, attendants, and other officials. This epidemic was not only the severest, but also one of the most protracted, that has visited Edinburgh during the present century. It did not subside till late in the succeeding year (1848); and during its progress must have attacked much more than 10,000 persons* in the city and neighbourhood (*i.e.*, counting as separate "persons" separate attacks in the same individual). The cases treated in the Infirmary amounted during the two years to not less than 8381, of which an immense proportion were either typhus or relapsing fever; a small but not easily ascertainable number of cases of enteric fever, however, having occurred at intervals during its whole course.

The *changes of type* which have occurred in epidemic fever, and especially in typhus fever, during the last ten years, or since the cessation of the great epidemic of

* Dr. Robert Paterson estimates the number at 20,000, with a mortality of 2500. It is possible that this may be nearer the truth than the more vague statement in the text.—See *Edin. Med. and Surg. Journal, Oct.* 1848.

1847-8, are not less remarkable than the diminution in the amount of this class of diseases. In making a few observations on this subject, I desire to premise, that there is a partial gap in my personal experience, extending from the autumn of 1848 to the winter of 1855-6, when I assumed the charge of the fever ward for females in the Royal Infirmary; which charge I still retain. During this interval of seven years I was not, indeed, without interest in the subject, nor entirely without opportunities of observation; but for all statements bearing on the characters of fever in the aggregate of cases, I must rely upon the statements of others; and I shall therefore only casually refer to the state of epidemic disease during this period. Suffice it to say, that the epidemic of 1847-8, having reached its acmé about midsummer of the former year, continued to decline throughout the whole of the latter, at the end of which fever was not more abundant than it had been during ordinary seasons for the preceding twenty years or more. During the four years following 1849 it maintained an ordinary, or not more than average, amount of from 520 to 960 cases a year. In 1854 the number of cases admitted diminished to between one and two hundred, and it has never since risen above the latter of these numbers. The remarkable exemption which we now enjoy from epidemic fever, an exemption hardly to be paralleled during the present century, may be said to have commenced about five years ago.

The cases which have been under my observation during the last four years of this fortunate period have

not only differed in aggregate numbers from those of the last epidemic, and of some years before it; they have also differed remarkably in character, as I shall now endeavour to shew.

1. *The Relapsing Fever, or Synocha, which formed so large a part of the epidemics of* 1843-4 *and* 1847-8, *has absolutely disappeared.* At least I can say, that having been perfectly familiar with its characters as witnessed in these two epidemics, I have not seen a single case, distinctly referrible to this type, since 1855. On this very curious fact I shall have more to say presently.

2. *Typhus Fever has become less fatal to those attacked than it was ten years ago.* It is difficult to reduce this conclusion to a statistical form, partly on account of the paucity of cases and the imperfection of some of the records, and partly on account of the suspicion which naturally arises, that in the experience of former years typhus may have been mixed up statistically to a considerable extent with other types, and particularly with the enteric fever. I think, however, that there are still ample grounds for making the assertion of the diminished mortality of typhus in Edinburgh. During several successive sessions, I have pointed out to my students the fact that *eruptive* typhus, as occurring in my wards, has had a very small mortality. The deaths, indeed, are so few that it is unsafe to found an average upon them; but I think I cannot possibly be in error in stating the average mortality at *much less* than 1 in every 10 cases. In this estimate, indeed, I make very large allowance for the chances of error connected with small numbers. Were

I to state more exactly my own personal experience for the last two years, it would be to the effect that I believe I have had during that period only two deaths from typhus ; or, including (but merely for the sake of argument) one questionable case of febrile disease which occurred lately,* and in which eruption was scanty and doubtful, say three deaths at the very most, out of 45 cases which I find marked as " typhus" in the hospital records. In this list of fever cases everything which could justly be called " febricula " has been kept apart both from typhus and enteric fever ; on the other hand, some cases of protracted fever have been classified as typhus, though no distinct eruption was observed ; and one of these appears among the fatal cases. A certain amount of doubt exists, therefore, as to the proper margin to be assigned to typhus ; but I have always refrained from giving this name to cases which appeared at all questionable. The deaths from enteric fever during the same period have been four, or perhaps five, in number. It should be stated that this experience is almost exclusively of cases among females ; who, as will be presently

* M. B., æt. 14, admitted in a state of partial coma, without delirium or local paralysis. Bowels relaxed ; slight tenderness in right iliac fossa ; evacuations involuntary. Tongue furred, dry, with papillae much enlarged. Pulse 120, weak. Skin cool, no flush ; traces of a rose-spot (?) or two. Pupils much dilated ; no marked strabismus. The patient gradually sank, and died perfectly comatose, and *with the pupils widely dilated*. I believe the case to have been one either of enteric fever or tubercular meningitis ; certainly not of typhus. There was no *post-mortem* examination. Excluding this case, the mortality is, of course, under 1 in 20.

seen, have a somewhat smaller average mortality than males. It is possible, also, that the proportion of children may have been somewhat larger than usual. On the other hand, the only male adult included in the number was one of the fatal cases; an old man of excessively drunken habits, who was sent by mistake to the male general ward, at too late a period for removal to the fever ward. If the female fever ward alone, therefore, is to be counted, the total numbers and the mortality must each be diminished by one.

In the epidemic of 1848-9, Dr. Robertson* states the mortality of typhus as 24·72 per cent, or 1 in 4 nearly. It was somewhat different for males and females, being in the former case 26·36 per cent, in the latter 22·11 per cent. This difference in favour of the female sex is observed uniformly in the reports of the Edinburgh Hospital, and even to a greater extent in Stockholm, according to Dr. Magnus Huss. In London, according to Dr. Murchison, the difference is less constant, though, on the whole, nearly similar in amount.

It may reasonably be supposed that the enormous mortality of 1 in 4 was caused by the overcrowding and deficient accommodation, consequent upon the rapid and overwhelming development of the epidemic of 1847-8. That this was one cause of the high mortality there can be no doubt. The late period at which cases were removed to the hospital, and the want of a sufficient staff of experienced nurses, may probably have led, also, to considerable sacrifice of life. Nevertheless, it

* *Monthly Journal of Medical Science*, vol. ix. p. 370.

appears from the Infirmary reports, that in the succeeding year 1849, after the entire decline of the epidemic, and when the cases were not a tenth of the number during its progress, the mortality of typhus and synochus (*i. e.* continued fever, *excluding* febricula and relapsing fever) was 22 per cent, or 1 in 4½ (males, 24·18 per cent; females, 18·91 per cent). It was not until the next year that any considerable abatement was observed; and even then, out of 422 cases 65 died, or 1 in 6½. Nor is it improbable that this apparently better result is in part obtained by the less careful separation of "relapsing fever" and "simple fever," which in this return bear a much smaller proportion to "typhus and synochus" than in the former.

Another statistical difficulty arises from the fact that enteric fever is certainly included to some extent in the Edinburgh returns of 1847-8 and the two succeeding years. From personal recollections, however, I believe I can state that this fever did not prevail to such an extent as very materially to change the average of mortality. Besides, the experience of the London Fever Hospital seems to shew that the mortality of enteric fever, as compared with the number attacked, is, on the whole, less than 1 in 5, or 20 per cent; though in a few exceptional years it appears to have been upwards of 1 in 4.

I think we may fairly conclude that the mortality of typhus fever in Edinburgh at the time of the epidemic of 1847-8, and for some time after its decline, apart from all accidental disturbing causes, was certainly not less than 1 in 5, or 20 per cent. In London, typhus has

maintained, on the whole, a nearly similar rate of mortality over the ten years 1848 to 1857. In three of these years the mortality of typhus approached or exceeded 25 per cent; in five of the others it was somewhat above or below 20 per cent; in the two years 1851-2, however, there were admitted 272 cases, of whom only 30 died; being about 11 per cent, or 1 in 9.

From these facts, as compared with my own personal experience of typhus before 1848 and after 1855, I feel very safe in asserting that its mortality, in proportion to the number attacked, has remarkably diminished, in Edinburgh, during the last ten years.

3. *Typhus not only has become less fatal, but its type, and some of its leading characters, have been remarkably modified during the last ten years.* This will be evident from the following statements of facts coming under my own personal observation: The most characteristic phenomena of typhus fever are—1*st*, An eruption, distinct in a considerable majority of the cases, but occasionally escaping observation altogether, and sometimes so indistinct as to be scarcely, *per se*, characteristic; 2*d*, A course too protracted for febricula or relapsing fever; 3*d*, A *gradual* convalescence, commonly without well-marked critical phenomena; 4*th*, More or less disturbance of the nervous system, with an approach to the character of delirium or stupor. It is not my object to describe these phenomena at length, but only to shew the modifications to which they are subject in the existing type of typhus fever. The last, it should be observed, has been not unfrequently altogether wanting.

The eruption is to me, as to most modern observers, the great criterion of typhus; *i. e.* when the eruption is distinct the diagnosis is easy and complete; but when the eruption is not present, or not characteristic, it is to be regarded as difficult to verify the disease; nothing less than a very marked and characteristic course in other respects, or a close relation to other cases having the eruption, being then sufficient for the diagnosis. When the typhus eruption has been copious, I have never experienced the slightest difficulty in distinguishing it from other fever eruptions; and as regards the distinction of the rose spots of enteric fever from the measly rash of typhus, I would beg to express my entire concurrence in the views of Dr. Jenner, to whose excellent descriptions I have nothing to add. To those who are still sceptical upon this subject, I would commend the careful observation of the manner of development of the two eruptions, rather than the characters of the individual spots in each. Nothing, certainly, can be more contrasted with the well-marked typhus efflorescence all over the body, limbs, and back, than the isolated rose-coloured pimples of the enteric fever, appearing by threes and fours, from day to day, on the abdomen and thorax. Those, especially, who will take the trouble to mark the spots as they arise, in doubtful cases (and in the enteric fever, at least, this should generally be done throughout), will rarely feel themselves much at a loss.

The most remarkable peculiarity which it has occurred to me to notice in the typhus of the last few years, as respects the eruption, is the *earliness of its ap-*

pearance and disappearance. Dr. Jenner mentions the fifth and sixth day as those on which the eruption first appears. It has often occurred to me, however, to observe it, especially in children, quite fully formed on the fifth, and even on the fourth day; and though patients are seldom brought into hospital so early, I have now seen several cases in which it was quite distinct on the third day of the disease; and this, where the symptoms of invasion were so sudden and well-marked as to leave no doubt of the real date of the accession. In some of these cases the eruption has begun to fade after being out only for a few days, or even hours; in others it has continued distinct till the convalescence was far advanced. On the whole, the disappearance of the eruption usually corresponds pretty closely with the period of convalescence; anticipating it, or lingering behind it, according as the eruption is profuse and deep in colour, or the reverse.

The *course* of typhus fever has been also modified of late years; and this fact is of great importance both with respect to the diagnosis and the prognosis. It will be remembered by those who were concerned in the epidemic of 1847-8, how very rarely it happened that anything like a satisfactory crisis was observed before the fourteenth day, or, at the very earliest, before the thirteenth. This fact is very strongly impressed on my memory by the circumstance of an attack of eruptive typhus, which occurred during the height of the epidemic in the person of an intimate friend of my own, and which terminated on the twelfth day by a sweating crisis. The

companions of this gentleman, and the physician who attended him, were all in the habit of seeing fever in the wards of the hospital on the large scale offered by the epidemic; and so unusual did the occurrence of a decided crisis so early as the twelfth day appear to these gentlemen, that doubts were raised, notwithstanding the eruption, whether the case was not really an anomalous one of relapsing fever. No one, however, who has seen much of fever in Edinburgh within the last two years, would have found the fact of a crisis on or about the twelfth day at all difficult to reconcile with his ordinary experience. I have again and again seen, of late, the pulse coming down several beats, the eruption fading, and the tongue cleaning progressively, at every period between the tenth and the fourteenth day; and in the case of children and young persons at least, I am certain that the change has begun quite as often before the twelfth day as after it. I have even observed the favourable change as early as the very beginning of the second week, and had at one time learned to look on the eleventh day as, on the whole, the one most frequently critical. In comparatively few cases has the critical period been later than the end of the second week; although, in this respect, I think the tendency is at present rather again towards retardation of the crisis than towards further abridgment of the fever.

Of course, this early crisis is in all probability one, at least, of the causes of the diminished mortality of typhus fever. Perhaps it would not be too much to call it the principal cause. For (as every one knows who

has watched such cases) even a single day's delay of the crisis in a case of any degree of severity is an immense addition to the risk.

But while the course of typhus fever has somewhat approximated to that of synocha or relapsing fever in respect of its duration, all the characteristic peculiarities of typhus, as regards the individual symptoms and the phenomena of the crisis, are perfectly preserved. I desire to make this observation the more pointedly, because doubts have been expressed, on very high authority, whether relapsing fever or synocha can, after all, be regarded as a nosological form distinct from typhus. If the epidemics of 1843 and 1847 had left me in any doubt upon this subject, I should feel now that these doubts were removed by the observation of the existing type of fever. Notwithstanding its short duration, and small mortality, nothing can well be more unlike the now vanished relapsing fever than the typhus of the last two years. Not only is it *not* a "synocha;" it has scarcely even the characters of a "synochus." The invasion is so far from sudden, that great difficulty is often experienced in fixing the day of attack. The pulse is altogether that of typhus. The heat of the skin, except in very young persons, is commonly moderate; and the surface tends to moisture rather than dryness. The urine is not usually red and scanty; but rather (according to Cullen) "parum mutata." The muscular pains are not of the acute character observed in synocha. The stomach is seldom persistently sick, and is often capable of receiving food throughout. The epigastrium is rarely

tender on pressure. Jaundice is still more rare. The eye is commonly muddy, the brain apathetic, the senses oppressed; delirium is common; the tongue dries early; and the whole series of symptoms called "typhoid" frequently occur characteristically; only cut short, in many cases, by the early crisis. And to crown the list of differences, the crisis itself is of the character peculiar to typhus. It is rarely quite rapid or sudden, usually extending over two or three days, and often barely appreciable till it has been forty-eight hours or more in progress. Nor is it a crisis by sweating, or by any other form of discharge, in the majority of instances. On the contrary, profuse sweating is almost always non-critical, and injurious; and the same may be said of diarrhœa and other so-called "critical" discharges. In all these respects, to say nothing of the eruption, typhus fever differs greatly from relapsing fever; and these differences are as perfectly preserved in the typhus of 1859 as they were in that of 1848; or as they are in the description of Dr. Christison, or even of Cullen.

What we have then, at present, is no new fever, nor any old fever revived, but a somewhat altered type of typhus. Typhus is unchanged in its essence and in its special symptoms; but its mortality has diminished; its course has become shorter; the eruption appears and disappears at earlier periods; the crisis is rarely prolonged into the third week, and not unfrequently takes place before the twelfth day. Hence the disease, while presenting to the skilled eye of the educated physician characters sufficient to distinguish it from all other

fevers, has lost much of its formidable *prestige*, and in many cases is scarcely to be distinguished, except by the eruption, from a mild febricula of rather protracted duration.

[Several of the statements made in this paper as regards typhus fever were received with some apparent hesitation in London, when verbally brought forward by me at the discussion on Dr. Murchison's paper on Fever, at the Royal Medical and Chirurgical Society, at which I happened to be present. I therefore desire to say here, that before the paper was written, but after that discussion, I submitted my opinions to the test of carefully-renewed observations, made with every possible precaution against error, and with the result of confirming in every point what I had stated in London, and now reproduce here. The rarity of typhus since this paper was written has prevented me from renewing the inquiry; but I must not omit to state that Dr. Peacock had noticed the early eruption and the early crisis in London even before I did so in Edinburgh; and that Dr. Murchison has since informed me of observations more or less similar to mine made by him.]

IX.

ON THE LOCAL DISTRIBUTION OF ENTERIC FEVER AND OF TYPHUS IN EDINBURGH.

(*From Personal Observations in the Summer of* 1859.)

Enteric Fever has always been rather an exceptional form of disease in Edinburgh. Dr. John Reid, who was well aware of its peculiarities of localization, and quite familiar with its pathological characters, used to remark, that the greater number of cases of this type occurring in the Royal Infirmary were not indigenous. Dr. William Robertson, who saw a considerable number of cases during the great epidemic of 1847-8, remarked, that a large proportion of them occurred among the railway labourers then employed upon the Hawick line, to the south of Dalkeith. Indeed, Dr. Robertson's remarks go still further, inasmuch as he declares that during three years previously to the date of this paper (*Monthly Journal*, December 1848), "no case of dothinenteritis, authenticated by *post-mortem* examination, and occurring in an inhabitant of Edinburgh," had presented itself in his wards.

Unfortunately, no data exist for determining the

numerical frequency of this fever over any considerable number of years in the Edinburgh Infirmary; but it is, I believe, in accordance with our general experience, that no such immunity as that pointed out by Dr. Robertson, can be said now to exist. My own wards have rarely been many months together unoccupied by enteric fever; and a large proportion of the cases have been indigenous. Not unfrequently, the proportion of enteric cases has exceeded that of typhus; at other times, typhus has been more prevalent. The limited number of my observations does not allow of any trustworthy deductions in regard to the influence of season, or of any other supposed determining cause of the prevalence of enteric fever; but on various occasions I have observed the occurrence of groups of cases, springing up in the same localities; and in all such cases the relation of the type to the locality has certainly been remarkably in accordance with the observations of Dr. Jenner. In no one instance, I believe, has it occurred to me to observe the simultaneous progress of typhus and enteric fever in one house, or even in one "land" (or series of houses entering from a common passage); hardly ever have the two diseases been observed to be present in the same court, or wynd, or street, at the same time. And what makes this the more striking is, that neither fever ever occurs to any great extent without assuming the form of groups of cases, distinctly related to each other.

To take the very latest instances, bearing upon this point, that have occurred, I may observe, that for some

weeks previously to the middle of May, in the present year (1859), my wards were almost devoid of fever of any kind ; and from an inspection of the Dispensary records of home visits, I have reason to believe that very few cases of any epidemic disease, except small-pox, existed in Edinburgh during the month of April. The first development of fever after this was of typhus ; and, as usual, while some cases occurred isolated, or what is called sporadically, others presented themselves in groups of two, three, or more, in the same family or house, or among neighbours or relatives visiting one another. Not a single case of enteric fever, however, occurred among those under treatment in my wards during May or June, even while the ward was full of typhus, and the propriety of opening a new ward was under the consideration of the Managers. In the whole Infirmary, indeed, I believe that only two cases of enteric fever occurred during May or June. One of these was a man from Portobello (about three miles from Edinburgh) ; the other was a joiner, who came from Galashiels to seek work in Edinburgh, and took ill within three weeks after his arrival, while still residing with a relative, in whose family and neighbourhood there has been no trace of the disease. This patient, in all probability, must have brought the seeds of disease with him from his last place of residence.

We had, therefore, almost a clean bill of health in Edinburgh, so far as enteric fever is concerned, during the months of April, May, and June. On the 30th of June, however, a little girl, A. S., was admitted from a house in Milne's Court, Lawnmarket ; it was a case of

perfectly well-marked, though mild, enteric fever, and was recognized as such at once by the eruption. On the 9th of July, three members of another family in Milne's Court were simultaneously admitted. This family I shall call G. ; it consisted of a father, mother, and three children. The mother and two daughters were admitted; a son took ill and was treated at home ; the father escaped. In each case of illness there was no doubt whatever that the disease was enteric fever ; all of them had the characteristic eruption, and more or less of the peculiar complications. Dr. Thom, who visited the family at home, had no difficulty in making the diagnosis, as I afterwards confirmed it in those admitted to the hospital. The mother died ; one of the daughters had a most severe illness, with profuse diarrhœa and pulmonary affection ; she is now in very slow convalescence, and by no means out of danger. I visited these two families to observe the localities. Both of them inhabited rather comfortable, well-placed, and well-ventilated, though rather too crowded rooms, high above the level of the street, and far removed from cess-pools, common sewers, untrapped drains, or any of the ordinary concomitants of what has been called " filth-fever." In the house of the G.'s, however, there was one serious flaw. Though free from all bad odour, and very clean, to appearance, at the time I visited it, I found the roof of the room to be of the most flimsy construction, and in a state of great disrepair ; and the painted canvas, which barely concealed the rafters, was at points completely saturated with liquid abominations which had

soaked through the floor from above. I was informed that the room overhead was tenanted by a family of very disorderly habits, and that frequent complaints had been made to the landlord on the subject ; but that no redress of grievances had been obtained, or was even promised. No direct communication was made out between A. S. and the G. family ; nor was there any suspicion of communication of the disease from A. S., until her young sister was admitted during the present month of August to the fever hospital, with a very indefinite type of mild fever, not unlike enteric fever, but without distinct eruption.

Since this series of cases in Milne's Court, only five cases of enteric fever have been admitted to the hospital ; all of them, so far as distinctly appears, isolated cases. Two of these cases, however, are from the Lawnmarket, not far from Milne's Court, but without any apparent relation to the cases occurring there ; one was from a close in the Canongate, in which no other cases of fever are known to have occurred ; one was a domestic servant in the house of a spirit-dealer, residing in High School Yards, not far from the Infirmary ; and the only remaining case was from Inveresk (about 6 miles from Edinburgh).

Here, then, is a series of ten or eleven cases of enteric fever admitted into the Infirmary within a period of little more than six weeks, after at least three months during which no case is known to me to have originated in Edinburgh. All of these cases, except one, seem to have originated in Edinburgh itself ; and all of them,

except two, originated in the immediate neighbourhood of the Lawnmarket, in that bank of houses that crowns the highest slopes of the ridge abutting on the Castle rock. Four of the eleven cases occurred in a single household; six of them in the population of one court. But not one case of enteric fever can be suspected to have been connected, either as cause or effect, with a case of typhus;* nor did any case of typhus fever, so far as known, except one, to be afterwards mentioned (clearly an imported case), originate in the neighbourhood of the Lawnmarket, or of any of the other localities observed as the seats of enteric fever. And this evidence is the more important, inasmuch as it may fairly be assumed to include by far the greater number, if not the whole, of the cases of enteric fever that have occurred in Edinburgh during the period alluded to.†

* I must notice here the fact, that A. S. has within the last few days been admitted to the fever ward again, with pretty distinctly marked typhus fever; which, however, there can be no reasonable doubt, was caught in the hospital by contagion, notwithstanding every caution on my part to prevent undue communication between convalescents. Further, I regret to say that two of the G. family have been seized with typhus,—one of them during slow convalescence from enteric fever, and while still in the ward; the other within a short period after being dismissed cured of enteric fever.

† Since these remarks were written I have made a further investigation into some of the cases mentioned. The result has been the discovery of two or three additional cases of fever, in all probability enteric, in the immediate neighbourhood of the Lawnmarket. The following facts deserve attention, as shewing the difficulties that attend the inquiry into the sources and mode of propagation of any epidemic or endemic disease. When inquiring into the case of G. M., a young girl admitted on July 20th, and mentioned in the list of cases above given, I found that she had

What was the exciting cause of the fever in these cases? I can only say, that the answer to this question is by no means clear to my mind. I have already remarked upon the suspicious circumstances in the house of the G.'s; but it is at least a remarkable fact, on the theory that the fever was caused by filth soaking through the roof, that none of the originators of that filth in the apartment above were affected. In most of the other cases, the sanitary circumstances of the houses were by no means bad; and in only one of the cases in the Lawnmarket group (that of a man in Blair's Close) was there an offensive open drain, or cess-pool, near the house. All the others were in tolerably well-aired and clean apartments, far above the level of the street, and opening on passages much above the average of width and

left her stepmother's house in the Lawnmarket from Thursday to Monday, and had taken ill in the interval, but had not returned home to her father's house from being ashamed of her absence. Where she had been during the four days, during which she was taken ill, her parents could not even guess. Here, therefore, is a possibility of infection, the particulars of which it is impossible to establish; and which may have been the source, directly or indirectly, of some of the other cases. But further, the stepmother of this girl informed me that she herself had had a "bilious fever" about three weeks before the girl took ill; that she was attended by a medical student, who has now left Edinburgh, and that she was in great danger, and had bowel complaint. Her husband also was ill, but not so severely. If these cases were enteric fever, they must have been among the earliest cases, and in all probability the source of the disease in G. M. In the close immediately above Milne's Court, I also incidentally discovered that there had been two cases of a very protracted fever (called "gastric" by the doctor in attendance): these cases had been kept quiet as much as possible, *because the mother of the family kept a mangle.*

airiness in the Old Town of Edinburgh. That a specific cause, nevertheless, did exist; that this cause was capable of generating enteric fever, and not typhus; and that it was, in some mysterious way, disseminated about the Lawnmarket and its neighbourhood, while typhus was present elsewhere in the city, follows, I think, very clearly from what has been stated.

Not less interesting has been the distribution of the cases of typhus fever, which from May to August have amounted to nearly thirty, exclusive of ten cases of febricula or continued fever of uncertain type. Here, too, the grouping of the cases, the localities, and the perfect separation of the typhus from the enteric fever, are worthy of notice.

The first case of fever admitted in May, was one which appeared to be isolated, and of which no particulars are accurately known. Indeed, it is doubtful whether the patient had fever on admission, or whether, as seems more probable, he had at first pneumonia, and afterwards a febrile attack, possibly caught in the ward.

Setting aside this case, we find, on the 9th of May, a mother and daughter admitted, of the name of P——n. This family was seized with fever when making preparations for removal from a house in the High Riggs, to one in Castle Wynd, Grassmarket. Of a father, mother, and three children all, except the father, were affected; and in three of them certainly, probably in all, the affection was eruptive typhus, easily recognized. The father had been the subject of a similar fever some

years before. Contrary to my recommendation, Mrs. P—n insisted on taking out her children before convalescence was complete; and, for convenience during the removal, one of them was taken to the house of an aunt, in Cowfeeder Row. The consequence was, that a little orphan girl, who was boarded with this aunt, and was also a niece of Mrs. P——n, was admitted shortly after, with the same disease. No other case of fever, so far as can be ascertained, had occurred in Cowfeeder Row and the neighbourhood; nor did the disease spread in this locality. One case of typhus, however, was subsequently admitted from High Riggs (an adjoining street).

The next group of cases is to be found in a family of the name of F——, the first of whom was admitted on May 13. This family had originally consisted of seven brothers (two of whom were married, and had separate houses, while the other five lived, together with the father and mother, in a house in Buccleuch Street); there was also one married sister, who, with her husband and an infant child, resided in the Potterrow. Of these ten persons, thus distributed, eight were affected with fever, viz., the five unmarried brothers, one of the married brothers, the married sister, and the mother. It was ascertained that they had constantly visited one another in sickness; and all the cases under observation proved to be eruptive typhus, which was thus implanted in three separate localities. It is nearly certain that in none of these localities was there any fever at the time of the seizure of this family; nor did the disease spread in any of them. The main body of the F—— family,

however, removed, while several of them were still ill, to the Horse Wynd; and one case of typhus in another house was subsequently admitted from that locality. I afterwards ascertained that the house in Buccleuch Street had been carefully limewashed and ventilated, and was inhabited by a very respectable man and his wife, formerly in the police force. Neither of these took fever.

The next group was a family named P——s, residing in a wretched house in the West Port, and consisting of a father, mother, and five children. Of these, the mother and two children were taken ill in the house in West Port; the first case was treated at home; the mother and one son were removed to the Infirmary, and it was ascertained that both had unequivocal eruptive typhus. This family also removed from the house in which fever had sprung up at the May term; and, on visiting the house soon after, I found it shut up. At the top of a stair, so narrow as to be more like the admission to a stable-loft than to a human habitation, I found three separate houses, or rather small rooms, the doors of which, in almost complete darkness, were within three or four feet of each other. It is rather surprising that no more cases of fever have been admitted from this locality, which seems eminently fitted for its propagation. An attempt had been made to wash the floor of the fever-infected room before it was abandoned, but nothing effective was, I believe, done; and the officers of police, to whom I applied on the subject, considered that they had no authority to interfere.

On removing from the West Port, the P——s family

was, for a time, at a loss for a lodging. The two younger children of those hitherto unaffected, were sent to lodge with a man in Rae's Close, Canongate; while the father and the eldest boy took refuge with a friend in a very good room in Milne's Court, Lawnmarket, of which so much has been said with reference to enteric fever. Here the eldest boy sickened, but was almost immediately sent to the Infirmary, where he had typhus fever in its usual form; and, after careful inquiry on the spot, I cannot ascertain that any other case of typhus fever occurred in connection with this one in Milne's Court. In Rae's Close, however, one other case of typhus fever subsequently occurred; it was the man who had temporarily taken charge of the younger children of the P——s family. After passing a few weeks in Milne's Court, the father of this family found a more permanent residence in Market Street; and, as no more cases of fever have occurred there, it may be hoped that this focus of epidemic disease is now extinguished.

In Conn's Close, High Street, an ill-drained and ill-ventilated narrow alley, there is a very poor and crowded tenement towards the middle of the close. The access to it is by a wooden stair, not broad enough to allow of two people passing each other with ease; and at the top of this stair, in a room of the most limited dimensions, was a family of the name of B., consisting of a father, mother, and five children. Two other girls of this family were in service in the New Town, one in Greenside Place, the other in Northumberland Street. Of these nine individuals, six took fever: viz., the mother, four of the

children at home, and the girl in service in Greenside Place, who had been visiting her mother during her illness. Most of these cases were seen either by me or by Dr. Shearer, in whose statements I place entire confidence: all that were observed were unquestionably cases of typhus. The disease made no progress in Greenside Place; but in Conn's Close there had been several cases of fever before, and I have reason to think there were some after the B.'s were affected. One man only, however, was admitted to the Infirmary, a lodger in the room immediately below the B.'s.

A family in Campbell's Close, Cowgate, four in number, were *all* affected with fever. Only one, however, was under my observation—it was a case of typhus. The family removed at the May term, and I was not able to trace them.

Several cases of severe typhus fever occurred in the Abbey Hill; they were attended at home, and I have not obtained the particulars. A nurse, Mrs. D., residing in Clyde Street, who was called in to dress the bodies of two who died, took fever. Her case also was one of well-marked and severe eruptive typhus.

A girl in service at Stockbridge was admitted to the small-pox female ward in the Royal Infirmary. No fever cases are ever admitted into this ward; but it opens on a landing, close to the principal male fever ward, which contained a considerable number of cases of typhus at the time. About a week after leaving the small-pox ward, this girl was seized with fever, which ran the usual course of typhus. (*Vice versa*—I may re-

mark in passing—one of the cases in this male fever ward became affected with small-pox at so short a period after his convalescence, as to make it nearly certain that the infection must have been caught in the hospital.)

Of late, several fever cases have occurred in the Middle Meal-Market Stair, Cowgate, an old and notorious haunt of fever in former epidemics. Three of these cases have been under my observation, all of them eruptive typhus.

In addition to these groups of cases, the following localities have furnished cases of typhus, which, so far as known, have been isolated: West Salisbury Place, Gifford Park, Hume's Close, Covenant Close, Toddrick's Wynd, Foulis Close, 110 Cowgate, Bell's Wynd, Candlemaker Row, Portobello. Four cases have also been admitted from the Charity Workhouse.

Such are all the facts which I have been able to gather respecting the origin and progress of fever in Edinburgh during the past summer. I have thought it desirable to give these facts in some detail, because the limitation of the epidemic to a few localities presents a more than usual probability of gaining instruction from it.

Without attempting to exhaust the subject, or to insist on points familiar to the mind of every medical observer, I will conclude with the following brief remarks, leaving the facts given, for the most part, to speak for themselves.

1. The facts, as stated, point clearly to two fevers,

and not to mere accidental varieties of one disease. Assuming the principles of diagnosis employed to be correct* it is inconceivable, on any other hypothesis than that of specific difference of type, that typhus and enteric fever should both have maintained themselves in Edinburgh at the same time, and in a certain relation to certain localities and groups of persons, but should not have been observed to be ever interchanged or substituted, the one fever for the other; that typhus should always have been associated with typhus only, and enteric fever with enteric fever only, wherever association could be distinctly traced; and that in no instance should the two diseases have ever crossed each other's path (as it were), so far as to approach within a quarter of a mile of each other, except in the single instance of the boy P——s, sufficiently explained above.

In the elaborate work of Dr. Magnus Huss of Stockholm, one of the latest defenders of the theory of "identity," much is made of a single instance in which a man and his wife, having gone to inhabit an infected house, were seized, one with typhus, and the other with enteric fever. Setting aside the possibility of error and of oversight in an isolated observation of this kind, it may fairly be remarked, that such instances ought to be quite common, on the hypothesis of "identity" of the two fevers. That Dr. Huss has observed epidemics consisting of both diseases; that they have been intimately intermixed, as epidemics; and have occurred even in

* See on this subject Article VII. generally; and especially pp. 113 and 124.

the same localities, nay, that some individuals should have appeared to be affected with both diseases either simultaneously or successively, is far from inconsistent with the idea of two distinct fevers. But that, under these circumstances, the coincidence of typhus and enteric fever in the same family should be so rare as to demand any special notice, seems to me to be a far more serious objection to the views of Dr. Huss than can be overcome by any of his arguments upon the other side.

In truth, it is only when an epidemic of fever is of limited extent, and when it succeeds to a period of exemption, that questions like those discussed by Dr. Huss can be successfully disposed of. When, in a city like Stockholm, four or five hundred cases of fever have occurred within a few months, and when both varieties have been nearly equally prevalent, it would be unreasonable not to expect some coincidences of the kind indicated. That such coincidences have not occurred to others, except in rare instances, appears to me to afford ample ground for maintaining the essential distinctness of the two forms which Dr. Huss would attempt to replace on the footing of mere accidental varieties.

2. When typhus and enteric fever are brought together into the same ward, the risk is not inconsiderable of the one form being succeeded by the other, evidently from contagious propagation in the ward itself. In fact, it appears from the details given above, that notwithstanding every precaution that could be taken, short of separation of the two fevers during the treatment, three of the patients affected with enteric fever have been

subsequently seized with typhus. This fact, while it affords a renewed illustration of the essential difference of the two fevers, is suggestive of grave considerations as to the management of such cases in hospitals. In Edinburgh, great difficulties exist in furnishing separate accommodation for different classes of fevers, inasmuch as all the wards are of large size, and unfavourably disposed for classification. Besides, it would be impossible, practically, to secure the recognition of the type before admission, or even, in some instances, for a considerable time after the admission of the patient. The small amount of epidemic disease, also, and the constant changes to which it is subject, have hitherto prevented the Managers from entertaining the question of further distinctive accommodation. I have no hesitation, however, in stating, as my own personal conviction, that in the case of enteric fever at least, there would be less risk, on the whole, in distributing it through the ordinary wards, than in placing it in the same ward with the much more contagious typhus.*

The habitual admission of the two types of fever to the same wards, may probably form one source of confusion in tracing out epidemic localities, after the diseases have been simultaneously present in any hospital for some time. No doubt, some of the cases of enteric fever admitted into the Stockholm hospital may have carried the contagion of typhus, acquired in the

* Since this was written, I have always insisted on the separation of the two fevers in the hospital wards committed to my care, as far as was possible in the circumstances.

hospital, to their homes; just as it is quite within the limits of probability that A. S. and the two G——s may yet become a focus of typhus fever to a quarter of Edinburgh at present the seat of enteric fever only.

3. A wise policy would assuredly dictate the attempt to anticipate and to prevent fever, not only by a separation of the sick from the healthy in hospitals, but by an efficient system of house-to-house visitation, and the application of medical science to the discovery and removal of its causes. This, indeed, is only a branch of a very wide subject, that of the sanitary regulation of our great cities. It is painful to think that Edinburgh is as yet very deficient in this respect. Though she has participated largely in the diminution which has taken place in typhus fever throughout the kingdom of late years, and though good has been done, in a general way, by the introduction of additional sewers, and by the systematic whitewashing of the closes, as well as by the operation of the Nuisances and Lodging-houses Act, no attempt has been made, as yet, to introduce a system of thorough sanitary inspection. To do this, in Edinburgh as elsewhere, must be the great work of the healing art (using the term in its highest and most general sense), for many years to come.

X.

SCARLATINA AND ENTERIC FEVER—PROGNOSIS AND TREATMENT.

(*Lecture,* February* 21, 1862.)

Up to this period (of the Session) I have brought before you, for the most part, single cases of disease, and almost all my remarks have been directed to these cases, individually considered. We are now in a position to vary this method, where I think this can be done with advantage to you. We can to some extent classify our cases, and place them in groups, with a view to more general observations; and as I am about to give up the fever wards to the care of Dr. Sanders for a time, it is my purpose to-day to give you a brief résumé of the cases of febrile or acute epidemic disease that have come under our observation since the beginning of November.

We have mainly observed, as you will remember, two forms of fever—Enteric Fever and Scarlatina. We have had no other of the Exanthemata under treatment; not a single case of smallpox, for example, and only a doubtful one of measles; measles, in fact, though pre-

* Reported by Mr. Arthur Reid.

sent in town to a certain extent, has been scanty and mild, and in these circumstances it is rarely sent to the Infirmary; smallpox has not been seen, so far as I know, by any one in Edinburgh and the neighbourhood for a considerable number of months past. Typhus fever, also, seems to have vanished from the list of our epidemics for the present.* But both enteric fever and scarlet fever have been unusually prevalent, and we have had a nearly equal number of cases of each under observation.

Now, of these two diseases, scarlet fever is by far the most common (let me add, that it causes also the larger mortality) in family practice; it is, therefore, in every point of view, the more prominent disease of the two, and the one on which I am the more anxious, if possible, that you should receive instruction; yet I have dwelt much oftener, and much longer, on the other in this class-room. The reason is, that scarlet fever can only be studied at the actual bedside of the sick. It will not *wait* for our clinical lectures. Even in the wards it is extremely difficult to generalize from individual cases on a disease so brief, so fleeting, so changeable from day to day, and from hour to hour; and, therefore, though I have omitted no opportunity of shewing you *facts* in detail, I have been obliged to let you take your chance of appreciating principles and motives of action. As regards the treatment adopted, especially, I have hitherto been content, for the most part, to do exactly what I thought right to do in each

* See the statements on this subject at p. 123.

case, making only casual observations at the bedside, and allowing you to find your way to my reasons as you best could. This unavoidable omission of *principles of treatment*, in our lectures on scarlatina hitherto, it is my intention to supply to a slight extent to-day; still keeping close, however, to the facts under our observation. I shall tell you not so much what I think you ought to do in all such cases, as what I have, in fact, done in these cases now or lately before us, and the result.

But first let me remark on the strange contrast presented by these two forms of fever—scarlatina and enteric fever. In the latter, we have usually had the patient for a long time under careful daily observation; we have had to watch doubtfully the turnings and windings of the fever, often for weeks, before we could feel the least degree of security; and this, although the patients had mostly been ill for some time before admission. Scarlatina, again, is short and sharp; it marches rapidly on to death or recovery; the great majority of our patients have changed decidedly for the better, even within the course of the first week. The danger in scarlet fever, excluding accidents, has been commonly quite over before the second week has been well begun; in enteric, on the contrary, we have felt nothing like practical security, in some cases, for four, five, or six weeks; in a few cases the lingering march of the disease, without a positive crisis, goes beyond even this; now and then there is a crisis, and then a relapse, though we had none of these cases this winter. Remember, however, that scarlet fever, too, is not without its accidents; for after the

patient may appear to have got quite well over the fever, he is still liable to albuminuria, dropsy, and in some cases to discharges from the ears and nostrils, to affections of the throat and tonsils, etc.; all of which are very important to be kept in view, especially in children.

Our cases of scarlet fever would have been more instructive to you, if instead of adults they had been children. But we seldom find children affected with this disease in our wards, partly because there is a rule of the hospital excluding the youngest ages; chiefly, however, because mothers will not have this disease treated away from home; they prefer risking the rest of the family to parting with their little ones. The greater number of our patients, indeed I may say nearly the whole of them, have been servant-maids and nurses in families in which the disease has prevailed among the children.* By the circumstance of our patients being adults no doubt the disease is modified, both as to prognosis and treatment; but I can hardly tell to what extent, or in what direction, for I know nothing certain on the subject.

I received the charge of the fever wards in the beginning of November, and since then, sixteen cases of scarlet fever have occurred; viz., three in November, two in December, seven in January, and four in the present month. We cannot, from these data, reason as to

* In one case, however, a domestic servant caught the disease first, and being in the house of a medical man, was removed as soon as the disease was discovered, with the effect of saving the household, including three children who had not had the disease.

the frequency of the disease in private practice; we know, however, from other sources of information, that it has been more or less epidemic all the autumn and winter, and has been even increasing in frequency up to a recent date; this I have learned from several practitioners who see a great deal more of children's diseases than I do, and who all agree in believing that the epidemic is now, but only very lately, beginning to decline.

Our sixteen cases have *all* ended favourably; note the fact, though I do not wish you to found too much upon it. We have not had a single instance even of great protraction of the disease, or of dangerous sequelæ; not one of very obstinate sore-throat, or of abscesses in the neck, or of dropsy; and this, though many of our cases, when in the fever, were really serious-looking, the fever being very high, and the throat affection severe. To this statement let me add that during the period of my practice in this hospital, now extending over a period of about eleven years including the time when I acted as assistant-physician, and between eight and nine years excluding this, there have not, I think, been more than two or three deaths from scarlatina in the wards under my care, though we have had several considerable epidemics during the period.

Now, I do not mean to put forth an extremely high claim for my treatment, but I may surely be allowed to conclude from these statements that it has not been very bad—nay, that it has been fair, or even good treatment on the whole. And as in regard to principles, I have not consciously varied from first to last; further, as

nearly the same principles apply, with certain modifications, to other febrile diseases, I am extremely anxious to impress upon you what these principles are, so as to make them available for your guidance in so far as they may be found to deserve consideration. There is one negative point about the practice we have pursued which I regard as very important, and which must have struck you all—possibly, struck some of you unfavourably. It is the entire absence of that extreme anxiety to be doing *something*—that *fidgettiness* (as I call it) in detail, which you sometimes see practised, and still more advocated in books, as matter of routine. I have, as you have learned by *facts* rather than by direct instructions, no bias whatever for any of the current extremes of practice. I have done almost nothing whatever as a system of routine, either in the direction of depletion, or of evacuants, or of stimulation. And observe, it is not through slovenliness, or from want of thought, or of due anxiety, that I have proceeded thus, but *on principle;* you saw very plainly from the first that I was *resolved* to do nothing without a distinct reason which could be stated and clearly seen ; and this, because I believe that scarlet fever, if left to itself, with rest and careful nursing, will generally go on well. Little matters have not readily made me depart from this watchful, but not too anxious, policy. In particular, you have seen that I am not readily to be frightened by the mere intensity of the fever. You have seen me abstain entirely from active interference, even when there has been a pulse of 140 or 150, with an extremely dry, burning-hot skin, flushed

face, very red tongue, throat excessively sore, etc., because, as I have frequently told you at the bedside in such cases, the mere violence of the fever is not an indication in this disease for great alarm. I firmly believe, and act upon the belief, that in scarlet fever these symptoms, taken by themselves, are not to be treated by active measures ; they are part of the essence of the disease, which you cannot cut short or cut down materially by remedies if you would ; and further, that they are commonly *not dangerous symptoms in scarlet fever.* I repeat, and I cannot repeat it too often, because the opposite belief leads, in my opinion, to fatal errors, that in a short fever, ending mostly within the first week, there is hardly any frequency of the pulse, if it be at the same time full, and of good strength, that should alarm you much. It is very different in typhus, or in enteric fever ; for in these a persistently frequent pulse in the earlier stage means commonly a still more frequent, and a weak pulse, in the second or third week ; and that again means debility and exhaustion, cold sweats, rapid emaciation, and death, whether with or without complications. Attend carefully to this distinction between long and short fevers : scarlatina is one of the short fevers, and the knowledge of this fact should give you confidence, even when the fever runs very high. But you have seen that in some cases worse symptoms than mere high fever have failed to startle me out of my Fabian policy. In one case, in particular, you may remember there was very violent delirium at night ; for this, I have no doubt at all that some would have shaved the head, and ap-

plied leeches to the temples; others, perhaps, would have given opium or antimony. But here is what I told you at the bedside:—"By careful inquiry I learn that it is now the very end of the fourth, or beginning of the fifth, day of the disease; the eruption has been well out, and is receding; the pulse is rather coming down in frequency, and is otherwise good; the throat affection is at least not getting worse, and there is no *bubo in the neck** (remember the importance I attached always to this point); *therefore*, in twenty-four, or forty-eight hours at most, we shall have a complete crisis; *therefore*, the delirium may be safely neglected; it will disappear as soon as the crisis is fully pronounced." And we did neglect it accordingly, and the patient was, in fact, quite better in even less than the twenty-four hours. At the time, I referred you to a remark or aphorism of Heberden (a truly great observer, and one whose lightest sayings are commonly of more value than the most precise and detailed statements of many so-called authorities), to the effect that there is no disease in which the patient is more apt to be delirious, and with less danger, than in scarlatina.† I did not rest, however, on the general

* This *scarlatinal bubo*, as Trousseau very aptly calls it, is, I believe, by far the best and most accurate index of danger in the later period of scarlatina, in so far as the danger depends on the sore-throat, and on the putrid infection of the blood (*septicæmia*) which accompanies it. Probably the same holds true to some extent of *diphtheria*.

† "Haud temere alium morbum repereris, in quo ægri sæpius desipiunt, et cum minore periculo. In cæteris febribus mens raro turbatur, donec ægri jam in malis sunt; ideoque hoc signum non injuste terret: sed in febre rubra ægri vel ipso primo die delirant; atque interdum,

statement of Heberden, but on the particular facts which I noticed to you at the bedside. I should add, that this patient had an emetic on the first day after her admission to the hospital ; I gave her this because she was very sick and oppressed, with a great feeling of suffering at the stomach ; but mainly because she was urgent for it, and I thought it might relieve her, and could do no possible harm. [Where there is bad putrid sore-throat, and emetics can be borne, I even think they are very useful.] But it did not abate the fever, as you observed, and did not save her from the delirium.

I believe that whatever is to be said of emetics, purging is usually bad practice in scarlet fever. Indeed, I think it not improbable that part of the mortality of scarlet fever in some former epidemics has been due to the system of purging at the beginning of all fevers, then in vogue. I strongly advise you never to use even laxatives without a special reason, and to meet a specific indication. Of course, if a patient came under my care with obviously loaded bowels, I should never hesitate to give a purgative ; but only under these circumstances would I employ this class of medicines. They have

licet omni alio periculi indicio vacent, tamen non cessant aliena loqui singulis noctibus ab initio morbi usque ad finem."—*Commentarii de Morborum Historia et Curatione*, cap. VII., "de Angina, et Febre Rubra ;"—a book which never can become old to a true physician, but which, I fear, too many of our students have now forsaken, to run after other, and often worse, text-books. Perhaps a few earnest men among them may appreciate the true wisdom and the exquisite language of the sentence above given, and may then be moved to turn to Heberden, and read him to some purpose ; in the original, if possible.

been rarely, if ever, given in our cases this session; of course they may have been given before admission.

I have not employed the cold affusion, though I hasten to say that I by no means disapprove of this practice, especially in children, when the reaction is high, and when it gives comfort. In adults, it is obviously inconvenient, and it is at least not necessary. In some instances I told the nurse to sponge the body occasionally with cool or tepid water; in other cases, no doubt, the nurse did this of her own accord, and as matter of routine; it is very refreshing to the patient, and a part of necessary cleanliness.

I have not once bled nor leeched the throat, and have hardly ever given what are called febrifuge mixtures; remember, therefore, as a fact, that patients can get on quite well without any of these, even when fever is high, as it usually is in scarlatina. I have very much the same opinion of frictions with lard and other unguents, as practised in Germany; but I don't violently object to that practice, and it is said to give comfort.

Now, to conclude my list of negatives, I beg you to observe that the wine and spirit roll is an almost entire blank. There has been hardly anything of this kind used in our cases of scarlet fever. This is partly to be accounted for by our patients having mostly been young women of temperate life, unaccustomed to alcoholic liquors except in the most moderate quantities. I am of opinion that in such persons stimulants can rarely be required, and that they do positive harm if given with-

out necessity ; this is, of course, even more true, if possible, of young children. I can easily understand the necessity for stimulants in a few exceptional cases, and even as a matter of routine in some worn-out, broken-down constitutions.

The practical result of all this is, that you have really no excuse for being in a state of perpetual activity in scarlet fever. Lay this to heart, and when you meet the disease in private practice, meet it in this spirit. Look well a-head for special dangers, but in general, don't be fidgetty, don't be perpetually dosing your little patients for the sake of *doing something.*

I said that I had no routine practice in scarlet fever. This is not altogether correct. I have had at least one practice which has been with me absolutely a routine practice for years ; so much so, that the nurse employs it without orders in almost all cases of scarlet fever, and in all the stages. I was astonished this morning, on making a little search through the books most at hand, and especially the text-books which are in your hands, to find not a single reference to my routine practice ; for this looks as if it were very little in use elsewhere. It is a very simple matter, notwithstanding, and in my opinion, a very important one. It is this :—*Let the patient inhale the steam of hot water from the beginning to the end of the fever ;* as long, in fact, as the throat is sore. I can assure you that this is good practice, and it requires no qualification on the score either of danger or difficulty ; I tell you frankly that with me it supersedes almost all other local applications. Nothing that

I know of produces nearly so much local relief, or has so much of a disinfecting tendency, preventing, as I believe it does in many cases, the secondary infection—the putridity as the ancients called it, the *septicæmia* as it is now sometimes called—from the sore-throat. We have had little or nothing of this, as I told you before; yet I have hardly ever used gargles, or indeed any other local application than *steam*, in the acute stage; not muriatic acid, nor chlorine, nor nitrate of silver. In a few lingering cases of the throat affection we have used gargles consisting of a very dilute solution of Condy's liquid, or of the permanganate of potash, or of chlorine water. It is not, then, that I neglect these things, but that simple *steam*, employed from the beginning to the end, in my opinion renders most of them unnecessary. [The inhaler most used in the hospital is the form figured in Watson's Practice of Physic (Vol. I., p. 878) as Mr. Hercy's. It is an old Edinburgh invention, introduced by Dr. Watson into the Middlesex Hospital, and long a part of the furniture of every ward in our Infirmary. It answers very well in scarlet fever, and is a convenient instrument, not troublesome to manage with large numbers of cases, and not easily overturned; but in private practice I much prefer another form of inhaler, brought into use by my father many years ago,* which has the great advantage of a spirit-lamp below to keep the water boiling, and is an invaluable instrument to have in a sick-room, in almost all cases of chronic irritability or disease of the mucous membrane of the

* *Edinburgh Medical and Surgical Journal*, for 1823, vol. xix., p. 217.

throat and larynx, or of the upper part of the air passages.*]

In scarlet fever, then, let me say in conclusion, I would have you neither stimulate, nor deplete, nor purge, nor put in force any kind of very active or perturbative treatment. Keep the patient quiet, and steam the throat frequently throughout the course of the fever; let the nourishment be light, or even in some cases almost none if the patient declines it : trust in the shortness of the fever, which makes it of less importance in this than in most other febrile diseases to insist upon nourishment. Even in convalescence, I believe it to be better that patients should abstain from stimulating diet for a good while. It is a very curious fact, but I think a fact nevertheless, and one which cannot be without its lesson, that scarlet fever is fully more fatal to well-fed children than to those that have been half-starved. With many other fevers, and especially with typhus, it is probably the reverse. During convalescence, it is well to insist on the patient being kept fully covered; and he should by no means be exposed to cold, nor even, in many cases, allowed to leave the house, until after the usual period for the dropsy has fairly passed; *i.e.* until three or four weeks after the fever has ended.

Enteric Fever, like scarlatina, has been lingering about for the greater part of the autumn and winter; in fact, we can hardly say that it has ever been absent

* These inhalers are made by Mr. Young, surgical instrument maker, 58 North Bridge, Edinburgh, at a cost of about six shillings.

from Edinburgh and the neighbourhood for the last eighteen months, though it has become decidedly more prominent of late. The following is an account of the cases that have been under my care:—There were two admitted to the ordinary male ward in November, and, seeing that I do not consider this fever to be very infectious, I did not insist on removing them, as I should certainly have done in cases of scarlet fever or of typhus. In the fever ward for women there were three cases admitted in November, two in December, seven in January, one in February. Fifteen cases in all. [One case was admitted shortly afterwards to the ordinary ward for women.] Out of these fifteen [sixteen] cases, two have died. The first death occurred in December, and was at as early a period of the fever as I remember to have seen a death [earlier cases have, however, been recorded]. The girl was young, and apparently healthy; she sank exhausted, without any very special symptoms, about the ninth or tenth day. It is a remarkable fact that age, sex, and position in society, seem to make almost no difference in the proportion of deaths in this fever. A large majority of the cases is among the young, and at least a fair proportion among the well-to-do and temperate; yet the mortality appears to be hardly lessened by these apparently favourable circumstances.* On the other hand, I strongly suspect that in enteric fever, as in scarlatina, there is a family predisposition to the fatal forms of the disease; for I have

* See Dr. Murchison's paper before referred to in vol. xli. of the *Medico-Chirurgical Transactions.*

several times seen two or three deaths successively in one family, a very melancholy and painful circumstance, and one which could not possibly occur frequently did the disease really preserve a nearly uniform rate of mortality. This girl had been nursing two of her sisters, I understand, in enteric fever, and both of these afterwards died ; but one of them, I suspect, was phthisical. The family lived in Penicuik, where this fever has been very fatal; and our only other fatal case was likewise from Penicuik. This other death occurred only last night ; the girl had got over the period of the fever proper, but died of the secondary accidents. There had been, I have no doubt, great ulceration of the intestines, and there was to be felt a very decided enlargement of the mesenteric glands, with pain on pressure of the abdomen: I am not even sure that there was not peritonitis. You will remember my directing your attention to her a week ago, and saying that had we not seen a case somewhat similar to hers (Alex. K., p. 126) this session already, which recovered after being almost past hope, I should have given you the very worst prognosis. I think in this disease the prognosis is more often at fault than in almost any that I know ; after my very gloomy opinion this girl even began to rally, and notwithstanding the swollen state of the mesenteric glands I was beginning to indulge a hope that the worst was past, and that she would go on improving. But now began that terrible symptom, the putrid poisoning of the blood (*septicæmia*) evidently from the ulcerated intestine ; I think, too, judging from the traces

of peritonitis, that the bowel must have been on the verge of perforation; at all events, the pulse became rapid and small, cold clammy sweats appeared, and the body exhaled a putrid odour; and from the combined effects of these conditions she sank yesterday morning. We did not succeed in obtaining leave for an examination of the body.

Looking now to our death-rate you observe that it is about one in seven and a-half [eight]. You can do nothing with such small numbers; but I think, judging from recollection, that this is pretty nearly the usual rate with us; and, so far as I can form an opinion, other hospitals, if not worse off, are generally no better off than we are. Thus in the Fever Hospital at London, the proportion of deaths is stated at about one in five. I therefore hope that our treatment has been, at least relatively, not bad; but I confess there is ample room for improvement, and no temptation to rest satisfied. The treatment of this disease is with me quite an open question, and I have not a sufficiently large field of experience to do much towards its settlement. Take the following remarks then, merely at what they are worth as incidental suggestions:—Most of our patients have had lime-water with milk, especially at the commencement of the diarrhœa; and this they generally state to be agreeable and refreshing: it is, so far as it goes, nourishment, and perhaps the lime-water acts in checking the diarrhœa, though I am by no means confident of this, nor yet quite confident that the diarrhœa *ought*, in all cases, to be checked. The diet of

our patients has been either milk alone, or milk with sago and arrowroot, or with wheaten bread (panado) in the earlier stages of the disease; in the advanced stages, and sometimes pretty early, they have had beef-tea, and occasionally strong meat juice; this last has sometimes appeared to disagree with the stomach. When abdominal pain was complained of, we have applied cloths wrung out of hot water, or turpentine stupes, followed by the simple hot water fomentations. I am not at all sure about the proper treatment of the intestinal affection, when diarrhœa and more or less abdominal distension are present. Some of my personal friends in London and elsewhere, for whose opinion I have great respect, are utterly opposed to the giving of laxatives, and consider the stopping of the diarrhœa as the great point to be attained. My own experience would rather lead me in a different direction; for I don't think the mere diarrhœa, as such, is often a source of danger; while I am very sure that the retention of decomposing matters in the intestine is really one of the greatest dangers we have to contend with. I think, moreover, that there is rather good evidence (better evidence than is usual in such cases) of the favourable results of the French practice of giving saline laxatives in this disease.* But I have not dared to carry out this

* I am also strongly impressed with the facts stated by Dr. Jackson of Boston in favour of emetics, especially in the early stages; but I confess I have hardly yet ventured to try this practice (which has not as yet succeeded in making way among us as perhaps it ought to have done) in my limited field of experience.

practice systematically, in the absence of encouragement nearer home to do so. As a sort of compromise, I have been very diligent in the use of enemata, to unload the bowels from below, where anything like abdominal distension has occurred ; the *enema fœtidum* of the Edinburgh Pharmacopœia has been often used, also occasionally a little aniseed added to the large enema of plain warm water. I think, on the whole, that relief has followed from this practice ; I *almost* feel confident that it has tended to diminish the mortality.

Enteric fever being usually accompanied by much exhaustion, and being often very protracted, you might naturally think that stimulants were among the most appropriate remedies. I have no prejudice whatever against stimulants in such cases, and have used them to a considerable extent, both in this disease and in typhus. I have learned, however, by experience, that this fever does not bear stimulants nearly so well as typhus ; I am sure, indeed, that patients are often greatly injured by stimulants in enteric fever ; the hectic flush is set up, the pulse quickens, the tongue dries, and delirium appears after their use. I have seen this so often that I have nearly abandoned stimulants in ordinary cases of this fever ; and in such cases you have yourselves seen that delirium has rarely occurred in our experience of this winter ; this is also my general experience of enteric fever for some years past.

I refrain, however, from drawing the inference, that we have banished delirium by withholding stimulants ; I don't think we have sufficient evidence on which to

base that inference. [It is, nevertheless, quite clear to me, from my own experience, that Dr. Todd's opinion * cannot possibly be correct, when he says that delirium, in acute diseases, is to be kept down chiefly by giving plenty of wine and brandy; that these are, in fact, the true antidotes to delirium when present, and its proper preventives when absent. Had this view been at all correct, or even near the truth, it is simply impossible that I should have failed to see the bad consequences of refraining from the administration of alcohol in such diseases as scarlatina and enteric fever. Yet among sixteen cases of scarlet fever occurring during the winter session, there was only one that presented delirium to anything like an appreciable extent, and in this case the delirium was only present for a few hours immediately preceding the crisis; although hardly any of the cases of scarlet fever had a single ounce of wine or spirits throughout the whole course of the disease. The facts in relation to enteric fever cannot be stated so simply; for there was more or less marked delirium in four, perhaps in five, cases out of our sixteen. But in nearly all of these the delirium was a very late phenomenon, in one of them occurring only in the third week, in another probably also in the third week, in two others either in the fourth or fifth week, and only in one doubtful case so early as the second week. But further, in two, at

* "I am enabled to enunciate dogmatically, that alcohol carefully administered, from an early period, in small and often repeated doses, is the best preventive of, and antidote to, delirium in acute disease."—*Clinical Lectures on Certain Acute Diseases.* 1860, p. 265.

least, of the cases which had delirium, wine had been given soon after admission, and was continued throughout the fever, on account of the great debility, or the profuse diarrhœa. In one other case the delirium existed at the time of admission, or most probably even before this. In the others the delirium was very trifling. These facts shew, I think, conclusively, that the statements of Dr. Todd on this subject are founded on a misapprehension of the true bearings of the evidence. I quite agree with Dr. Todd's views, however, in considering food and sustenance of all kinds as real preventives of delirium; and to the extent to which alcoholic liquors form part of a good dietetic system in fevers, I have no doubt they may operate beneficially in certain cases.] *

I was induced to try, in several cases, the internal administration of turpentine. Some of the Swedish physicians have expressed a high opinion of this medicine in enteric fever, and we have good accounts of it also from America. But as the indications on which it is prescribed by different physicians seem to differ materially, I have rather viewed it as a kind of specific, and prescribed it when the special symptoms of the fever were more or less threatening. I confess I have not been able to observe any distinct benefit from its use; but neither can I say that it has done any harm; so that I am still rather inclined to give it a further trial.

* See Art. IV., p. 64, for further remarks on this subject.

XI.

PATHOLOGY AND TREATMENT OF CHOLERA.

[This article consists partly of certain pathological observations, briefly reported from much more extended data to be found in the journals of the pathological department in the Royal Infirmary and in part of remarks on treatment. I did not think it necessary at the time, and still less is it so now, to swell the already overloaded literature of cholera with all the details referred to in the first part of the article ; but as the brevity of this paper has caused it to be often overlooked by compilers, I may venture, in reproducing it here, to state in general terms that it is as strictly the result of a careful and often numerical scrutiny of the facts, as if they had all been displayed at length to the view of the reader. The observations on treatment which follow formed part of a review published in the *Monthly Journal of Medical Science* for January 1849, and are founded mainly on a minute personal study of the clinical and pathological characters of the disease in the Cholera Hospital, at that time under the charge of my friend Dr. William Robertson, to whose elaborate researches upon the chemical characters of the blood in cholera* I may be permitted to refer as entirely corroborative of the anatomical results at which I had independently arrived. The most important of Dr. Robertson's conclusions, as bearing on practice, are, that " the changes which the blood undergoes up to the period of the reaction, consist in a concentration of the serum from the loss of water, and a loss of salts almost proportionate to

* See the *Monthly Journal of Medical Science*, May 1849, p. 764.

the amount of water abstracted ;" there being, however, no special loss of salts over and above the other elements. The changes observed during the reaction, again, " are probably due, in part, to the absorption of fluid into the circulation, and consequent dilution of the blood. * * * The fibrin is often increased, and there is reason to believe that blood corpuscles are rapidly formed." The changes characteristic of the collapse Dr. Robertson believes to be entirely secondary—not the direct results of the morbid poison, but of the choleraic discharges from the intestinal canal, by which immense quantities of water, salts, and even albumen are rapidly excreted, leaving behind a concentrated serum, holding in suspension the altered, but not destroyed, blood corpuscles. He also shews that " the formation and retention of urea in the blood is always to be dreaded during the reaction, and hence the use of diuretic remedies, and especially of such as are believed to possess the property of expelling urea from the system, is distinctly indicated." It was on this observation that the attempt was founded by Dr. Robertson to promote the excretion of urea and uric acid by colchicum ; which remedy, however, on the whole, seemed to Dr. Warburton Begbie, in the epidemic of 1854,* to be of inferior value to other diuretics, especially acetate of potash administered in very dilute doses, with abundance of cold water. I have omitted a few unimportant sentences from the article on treatment, and have likewise made a very few additions within brackets, in accordance with later experience ; but, on the whole, I find these brief suggestions on treatment so much in accordance with the whole recorded experience of my two friends above named, that although written pretty early in the epidemic of 1848-9, I trust they may be received as representing, to a considerable extent, the results of the observation of cholera in Edinburgh in both the more recent epidemics.]

* Dr. Begbie was sole physician to the Cholera Hospital in 1854 (as Dr. Robertson had been during the former epidemic) ; he has published an able and complete account of his experience in the *Edinburgh Medical and Surgical Journal* for April 1855.

THE following statement* is founded on the examination of eighty-nine fatal cases of cholera during the late epidemic, in the theatre of the Royal Infirmary of Edinburgh. In the course of my duties as pathologist to that institution, I have had unusual opportunities of contrasting and comparing the appearances in cholera with those presented to my notice in connection with other diseases; and the constant use of this *comparative* method of observation has led me to consider many statements as erroneous, which are generally, and in some cases all but universally, received in relation to this disease. The greatest care has been taken to verify or correct the results obtained by previous observers, more especially where any theory of the disease appeared to be involved; microscopical analysis and chemical tests have been applied wherever they promised to aid in the investigation; and in regard to such doubtful points as appeared likely to acquire precision by numerical analysis, the results have been preserved in the tabular form.

The bodies opened were mostly from the cholera hospital, being taken at random from those who died under the care of Dr. W. Robertson; a few, however, were from the Infirmary. The proportion of females to males was two to one. The average age of the patients was thirty-three, by far the greater number being between twenty and forty. Four of the females were in different stages of pregnancy; but as the greater num-

* Communicated to the Medico-Chirurgical Society of Edinburgh, June 6, 1849.

ber of the bodies of females known to be pregnant were given to Mr. Goodsir by his special request, this number does not give an accurate idea of the large proportion of pregnant cases. Several of the females had been nursing, as was shewn by the distended mammæ, which readily yielded milk on pressure. The great majority of the bodies examined were evidently those of persons previously healthy and vigorous. They presented the external appearances, so well known in cholera, of lividity and collapse, the last being caused by the shrinking of the cellular tissue from the absorption of its watery parts; there was, however, no diminution of the fat, which was usually in very considerable quantity on the abdomen and elsewhere. The muscles were of good colour, and usually in strong tonic contraction from the rigor mortis. The more important facts resulting from the examination of the internal organs are embodied in the following conclusions :—

I. *Previous Diseases of those attacked.*—Cholera appears, during the late epidemic, to have attacked chiefly persons in health, or in the retrograde stages of chronic affections; and to have spared almost entirely those affected with acute or actively progressive disease. Appearances of acute disease were chiefly observed when death took place after more or less distinct reaction, and were evidently the *sequelæ* of the choleraic affection. The chronic lesions were exactly similar in kind to those most commonly found in hospitals and dissecting rooms, but bore a decidedly lower proportion than is usual to

the whole number of cases examined. Thus the lungs had the traces of old disease in only one-sixth of the cases; among which two only (cases of miliary tubercle) indicated progressive disease. The liver presented a chronic lesion in one case; the kidneys in four, of which only two (incipient Bright's disease) were progressive. The uterus and ovaries were frequently more or less abnormal, but the only progressive lesion was ulceration of the cervix, usually quite superficial. The intestines were uniformly exempt from chronic disease. It thus appears that the opinion which has been so extensively prevalent since 1832, that cholera attacks chiefly or exclusively individuals of unsound constitution, or bearing the traces of previous organic disease, is not borne out by the facts of morbid anatomy; and in particular, that there is no evidence whatever that previous disease of the intestinal canal pre-disposes to cholera.

II. *The Blood* is much less affected in its physical characters than is usually supposed to be the case in cholera. Its coagulation within the vessels takes place much as in other diseases. In the majority of instances, firm clots are found within the heart, more or less completely decolorised; and the serum or non-coagulated portion contains the greater part of the blood-corpuscles. The colour of the blood presents nothing unusual, the epithets "dark" and "venous" being in no degree more applicable to cholera blood after death than to that of every ordinary form of fatal disease. The remarkable viscidity of the serum (or portion of the blood not in-

volved in the clot) so often noticed, was chiefly observed in cases fatal during the collapse or early reaction ; and was certainly owing to the removal of the fluid matter by the intestines. The effect of this in modifying the chemical constitution of the blood has been fully shewn in Dr. Robertson's researches ("Monthly Journal," vol. ix., p. 764). The microscopic appearances of the blood presented nothing unusual.

III. Much importance has been attached to *Congestion* in relation to the pathology of cholera. But it is an error to conceive of congestion as an essential or universal condition in this disease ; for, although the lungs and right side of the heart are frequently loaded with blood to a considerable extent, the liver, spleen, and kidneys are, in most cases, paler than is usual in other affections. The intestines present every shade of colour from the palest to the deepest. The uterus and Fallopian tubes are generally loaded with blood. The venous system of the brain, and indeed the great veins generally, are also in most instances full. But such appearances are very common in other forms of fatal disease.

IV. A tendency to *Ecchymosis* in various situations was certainly characteristic of cholera to an extent not common in other fatal diseases. These ecchymoses seldom occurred externally, except in one situation, viz., beneath the conjunctivæ of the eyes, which were very commonly more or less bloodshot. Among the internal organs it was more frequent on the intestinal mucous

membrane, especially of the colon, than in any other situation ; but very frequent also in the form of petechiæ on the posterior surface of the heart, and occasionally in other places, as the cellular tissue surrounding the cervical vessels, and in that around the dura mater of the spinal cord. Perhaps there were other situations in which ecchymoses might have been found on examination ; but in the above it was so frequent, that in only six out of twenty-six cases was it not found in one or more of them.

V. The *Glandular Secretions* in cholera are probably more or less diminished in quantity, and they appear also, from observations on the milk, and on the urine,* to undergo great alterations as to quality. But with the exception of the urinary secretion, which in the

* Dr. J. W. Begbie has made a very extended series of observations [afterwards published in *Monthly Journal*, vol. ix., p. 1207] on the urine voided in the earlier periods of reaction, from which it appears that the urea is generally much diminished, and in some instances entirely absent ; and that albumen is almost invariably present in greater or less amount, together with epithelium, for a variable period [of a few days] after the commencement of reaction. The presence of albumen was noticed at a very early period in the Edinburgh epidemic [according to Dr. Parkes, first by Hermann, in Moscow, in 1830], and has since been extensively observed in the Parisian hospitals. Dr. Begbie also informs me that various other modifications of the urinary secretion have been observed by him ; in particular, a reaction with nitric acid, indicating the presence of bile.

According to Dr. Douglas Maclagan (*Monthly Journal*, vol. ix., p. 394), the milk in cholera was of low specific gravity, containing little or no butter, and an unusually small number of milk granules.

collapse appears to be nearly, if not altogether suspended, there is no evidence that any of the more important and constant glandular secretions are suppressed in any stage of this disease. The milk could be readily expressed in streams from the ducts, even after death, in nursing women. All the other glands preserved their natural appearance and structure, and the lymphatic and mesenteric glands contained a secretion which presented the normal microscopic elements.

Next to the suppression of urine, the suppression or retention of the bile has been assumed (on account of its apparent absence in the dejections) to be one of the most characteristic features of Asiatic cholera. The assertion that the bile is suppressed, however, is obviously incorrect; and it is only wonderful that this idea has not met with more positive discouragement by pathological writers. In the late epidemic, in every instance, except two, the gall-bladder contained more or less of bile, which was mostly of good colour and consistence; and, in the greater number of instances, in quantity sufficient to produce considerable distension. One of the exceptional cases was the one formerly referred to of diseased liver, in which the gall-bladder contained only a thin light-coloured fluid; in the other, abundance of bile was present in the duodenum and stomach. The gall-ducts were most frequently empty, but occasionally contained bile; a probe passed in all cases easily from the duodenum to the gall-bladder, being usually somewhat tightly grasped at the neck of the latter, as is invariably the case, whatever be the

cause of death.* The duodenum contained, in most cases of collapse, no appearance of bile ; but there were, nevertheless, several cases in which the characteristic green colour was present in greater or less amount in the contents of the duodenum and stomach, though not in those of the intestines generally.

It appears, therefore, clear that the non-bilious character of the evacuations cannot be due to non-secretion. It can only, then, be ascribed to retention of the secreted bile in the gall-bladder. But we may well doubt whether this retention can be rightly regarded as any special part of the pathological process in cholera. The quantity of bile which passes into the duodenum, under ordinary circumstances, in the absence of the natural stimulus of food, is probably very small ; and the quantity which appears in the fæces in the normal state, or even under an ordinary attack of diarrhœa, would be quite insufficient to tinge visibly the enormous quantity of fluid thrown off by the intestines in cholera. Further, the examination of the cholera dejections chemically shews that biliary colouring matter can frequently be discovered in them by the nitric acid test, when it is not appreciable otherwise ;† and it is fully

* This constriction at the neck of the gall-bladder is probably due to an elastic tissue. It has been frequently mistaken for *spasm* of the gall-ducts, a condition which I believe, *as far as post-mortem examinations are concerned*, to be purely imaginary.

† Dr. Parkes seems to doubt whether the matter indicated by this reaction is bile ; but neither does he admit it to be uric acid, the only substance which has been alleged to have caused confusion.—See the *London Journal of Medicine*, Feb. 1849, p. 143. [Dr. Parkes has been

proved by the *post-mortem* appearances, that bile is occasionally found in appreciable quantity in the duodenum during the collapse state (though to a more considerable extent during the reaction). From these circumstances it seems probable that the secretion of the liver is discharged into the intestines in cholera to quite as great an extent as in most diseases in which digestion is totally interrupted, or as in a healthy individual when fasting; and that its ceasing during the collapse to colour sensibly the dejections, is merely the consequence of its extreme diffusion through the mass of fluids in the intestines.

Repeated microscopic examinations of the liver revealed nothing unusual. The kidneys, however, appeared in many cases to have undergone morbid changes, the cortical substance being pale and turgid, and the tubuli uriniferi gorged with imperfectly developed epithelium, which was mostly loaded to an unusual extent with oleo-albuminous granules. A similar state of the kidneys occurs after scarlatina, and not unfrequently after typhus fever, and some other acute diseases.*

led, by renewed observations, to adopt exactly the opinion stated in the text in reference to the presence of bile in the alvine evacuations in cholera. No one who knows Dr. Parkes can consider his opinion, so pronounced, as less than decisive.—See *Monthly Journal*, vol. ix., p. 1127.]

* I am indebted to my friend, Mr. Alexander Borthwick, now in Dumfries, who studied along with me very carefully the condition of the different organs in cholera as compared with the miscellaneous cases which came under our notice, for tabular statements which shew quite satisfactorily the much greater comparative prevalence of an excessive quantity of oleo-albuminous exudation in the kidneys of cholera patients, than either in the liver or the heart, which were in this respect below the average. None of these cases, however, with the exception of those

The secretions of the serous membranes appeared to be diminished in quantity, and everywhere more viscid than natural ; an effect probably of the altered constitution of the blood.

VI. *The condition of the Intestinal Canal and its Secretions* demands special notice. The most frequent of all the abnormal conditions of the mucous membrane was the prominence of the intestinal glands, both aggregated and solitary, but especially the latter. This condition, the *psorenterie* of some French writers, was found in about two-thirds of the cases. The great frequency of patches of ecchymosis in the intestines, especially in the cæcum, has been already alluded to ; in these cases the intestinal contents were usually more or less tinged with blood, and presented blood corpuscles among their other elements under the microscope. The ecchymosed patches were distinctly circumscribed, in tint varying from claret colour to the deepest purple, approaching black, but in parts not unfrequently greenish or ash-coloured ; their surface presented a similar appearance, in all but colour, to the rest of the mucous membrane, and was not perceptibly elevated. In three instances, however, a different appearance was observed in the colon, the mucous membrane presenting a few flattened elevations, each over the extent of about a sixpenny piece, of a greyish or leaden colour. These elevated

alluded to in a former part of this communication, presented the granulations of Bright, or any other unequivocal marks of a chronic disorganization.

patches were evidently due to a sub-mucous exudation (probably identical with the diphtheritic exudations described as occurring in cholera by Virchow), and yielded on section a creamy fluid; in this fluid microscopic examination shewed no well-marked pus-corpuscles or complete cells, but a number of nuclei, on which acetic acid produced no effect. In one protracted case, in which the great intestine was much ecchymosed, there were traces of dysenteric lymph on several parts of the mucous membrane. In two other instances, there was distinct exudation of yellowish lymph, with heightened vascularity on the external serous surface of several folds of small intestine.

On the other hand, it was by no means uncommon, especially in cases early fatal, to find the intestines throughout natural in colour and appearance, or even paler than natural; and in many of these cases there was no prominence of the solitary glands.

The intestinal contents resembled closely at first-sight the well-known cholera stools. In their most characteristic appearance they were yellowish-white, but frequently acquired, from blood on the one hand, or bile on the other, various shades of orange and greenish colour. On microscopic examination, the intestinal contents invariably shewed immense quantities of perfect epithelium, sometimes in coherent masses, peeled from the mucous membrane, and preserving the form of the villi and follicles to which it had been attached. The existence of epithelium in the fluids found in the intestines in cholera has been noticed by Boehm (see

Medical Times, June 24, 1848), and other observers, and has been supposed to indicate a complete desquamation of the intestinal epithelium as one of the special pathological conditions of cholera. In reality, however, the appearance proves precisely the reverse; for the presence of epithelium in such large quantities in the fluids found in the intestines after death, is the result of purely mechanical maceration upon a mucous membrane to which, during life, the epithelium remained attached. That this is the case is proved by these facts—1st. That artificial maceration produces a similar result on a healthy mucous membrane; 2d. That the epithelium found in the fluids of each division of the intestines is always that of the particular part where it is found; 3d. That the true cholera stools, *passed during life*, contained so little perfect epithelium, that it cannot be considered as anything more than an accidental ingredient.*

The examination of the cholera-stools leads to much more important and less confusing results than that of the intestinal fluids after death. These discharges separate by filtration, or on standing, into a colourless, or slightly coloured fluid, of an alkaline reaction and a

* A similar conclusion, in regard to the desquamation of epithelium, has been maintained by Dr. Parkes, in an elaborate article on the *Intestinal Discharges in Cholera*, in the *London Journal of Medicine* for February last. Although there are opinions in Dr. Parkes' paper from which I differ [see p. 215, *note*], it affords me much gratification to observe, that the principal facts included in the description of the cholera-masses by himself and his colleagues, correspond so nearly with what I have myself observed.

flaky sediment. The former is usually of a specific gravity from 1005 to 1010, and contains, therefore, a very small proportion of solid matters ; these have been shewn by Dr. Parkes to consist chiefly of salts. Repeated examinations of this fluid have shewn during the late epidemic (as Andral shewed in the former) that albumen, as tested by heat and nitric acid, is not necessarily present in it. Indeed, the presence of albumen usually coincided with that of a small quantity of blood, which, as before mentioned, is frequently present. The fluid, however, contains *constantly* a small quantity of an organic substance which is precipitated by alcohol, by corrosive sublimate, and (when acidulated) by ferrocyanate of potash ; and which presents, in other respects, the chemical reactions of mucus.*

The flakes which form the sediment of the cholera-stools, have likewise the ordinary chemical reactions and physical properties of mucus. They are, however, opaque and turbid, and when submitted to the microscope, shew a hyaline finely-striated basis, involving numerous granules, nuclei, and cells. The granules and minuter molecules require no particular description.

* M. Mialhe calls this substance *albuminose*, and considers it as "the ultimate product of the digestion of albuminous substances."—See "*L'Union Medicale*," 5th April 1849. His pathology of cholera is founded on the alleged presence of albuminose in the blood, which assertion, however, he does not appear to have attempted to establish by experiment. I prefer, therefore, the more familiar term of mucus, which is perfectly applicable to this substance, and was used by Andral, to one which is associated with a very doubtful hypothesis. The nearly allied chemical relations of mucus and the protein-compounds are well-known.

The nuclei are from 1-250th to 1-180th of a line in diameter, circular or slightly oval, and not affected by acetic acid. The cells are comparatively few in number, mostly globular, seldom exceeding 1-150th of a line in diameter. Some of them are but little affected by acetic acid, and single-nucleated; others present all the appearances of perfect pus cells, being two, three, or even four-nucleated, and having the cell-wall rendered very transparent by acetic acid. The most common appearances, however, were the nuclei without cells above mentioned.

Any one who is familiar with the pathological phenomena displayed by mucous membranes in a state of irritation, will readily recognize the similarity of the above microscopical appearances to those of ordinary catarrhal discharges from any of the mucous surfaces of the body; the chief peculiarities of the choleraic fluid being the enormous exaggeration of the watery and saline matters evacuated, and likewise the smaller tendency to the discharge of albumen and the development of pus-corpuscles, so readily formed and thrown off from the mucous membranes in all states of ordinary irritation.

The effects upon the blood of the removal of the watery and saline constituents are well shewn in Dr. Robertson's analyses; and it is probable that a considerable amount of the albumen of the blood also passes away by the intestines in the form of mucus, which is nearly allied to it in composition, and which, as has been shewn, forms the organic basis of the choleraic evacuations.

VII. The *Nervous System* presented no lesion worthy of remark. The pia-mater of the brain and spinal cord were often considerably injected, especially that of the cord, which, from the position of the body after death, almost always presents this appearance. The spine was, however, only opened in four cases; and in one of these, there were found small calcareous plates upon the arachnoid of the cord—the most frequent, probably, of all its chronic lesions. The sympathetic ganglia, and the pneumo-gastric nerves were repeatedly examined; but nothing unusual, except in a few cases slight ecchymosis, was discovered.

REMARKS ON THE TREATMENT OF CHOLERA.

(*January* 1849.)

* * * * * *

It is evident, that for a long time to come, the treatment of cholera cannot be fixed by an examination of records. Till it is so fixed, the practice in individual hands must be regulated in a considerable degree by theoretical or rational considerations; or rather by that peculiar combination of rational and empirical judgment which passes in the world under the name of *common sense;* the tribunal to which so much, besides cholera, affecting the interests of individuals and of the community, is in

these days referred. Hence the importance to practical men of just ideas of the pathology of cholera, and minute knowledge of its phenomena; by which, and by which alone, they can estimate the success of their own treatment, or that of others, in individual cases.

In the meantime we shall allude shortly to a few of those remedies which have acquired, from longer or shorter experience of them, somewhat of the reputation of specifics in cholera; and conclude by inquiring if there be not a few principles of treatment so decidedly borne out by theory and experience, as to be fairly assumed as the basis of future inquiries.

The first and most controverted of cholera remedies to which we shall allude, is *venesection*. In reference to this, the evidence is of the most varied character. It has been employed with alleged benefit in all stages of the disease, and not less in this country than in India. The most satisfactory accounts are of its use in the early stage, before the collapse has occurred; and here it seems to be often most effectual in relieving the feeling of tightness and oppression about the stomach and region of the diaphragm, which are frequently most distressing to the patient. As to the effect on the mortality, it is difficult to form an opinion. It is usually only in the early stage that blood can be procured in quantity, and this is precisely the stage not only when mistakes of diagnosis are most apt to occur, but in which the disease is most manageable under any form of treatment. Notwithstanding this circumstance, the mortality where blood-letting formed a considerable

part of the treatment, is rarely much less in any of Mr. Ross' tables* than 50 per cent, and sometimes more; few of these results, however, relate to blood-letting alone. Dr. Robertson, whose observations on this subject are well worthy of attention,† thinks that he has in several instances prevented the collapse by this measure. He employs it, however, only in the early stage.

Stimulants, especially alcoholic liquors, have been lauded in cholera no less than blood-letting; but there appear to be now grave reasons for doubting their efficacy, and even, we think, of rejecting them in a great measure in the treatment of this disease. Not only are they in many cases most disagreeable to the patient, whose perpetual thirst they do nothing to relieve; but it seems to be most probable that they are often not absorbed, and that their action is therefore purely local. It is important also to observe, that the evidence against them in Mr. Ross' tables is most unequivocal, and that, both alone and in combination with other kinds of practice, they seem invariably to have deteriorated the results wherever they were used. There is not an instance in these tables of a mortality under 50 per cent where stimulants have formed part of the treatment. That by stimulants alone gives in the aggregate 58·8 per cent.

Opium has a very large amount of individual testi-

* *Medical Times* for October, November, and December 1848 "Lectures on Asiatic Cholera," by George Ross, etc.

† *Monthly Journal of Medical Science*, December and January 1849. [The practice did not, however, take root in Edinburgh. It was scarcely heard of in 1854, either in the Cholera Hospital or in private practice.]

mony in its favour, and is indeed apparently so directly indicated by the diarrhœa, that we cannot wonder at its having been extensively used. In conjunction with acetate of lead, it forms the celebrated pill of Dr. Graves, which has had a most extensive reputation in the cholera both of this country and India. There seems no reason to doubt, that, in the premonitory diarrhœa, this remedy has the power ascribed to it; but, in the confirmed disease, Mr. Ross' tables shew that it has not diminished the aggregate mortality below 50 per cent. Those who continue to employ it should certainly do so in the fluid form, in order not to oppose any unnecessary obstacle to its absorption. In the stage of reaction, or where there is a tendency to coma, as is often the case in this country, there is every reason to reject opium as probably injurious.

Mercury, in the form of calomel, and usually in combination with opium, has been widely recommended in India. In this country it has been used to a large extent, but without remarkable success, according to the returns, excepting in the hands of Dr. Ayre of Leeds, and Dr. Peacock, in whose cases the mortality was as low as 31 per cent, and who both used it *without stimulants*. Whether this success is due to the calomel, or to the simplicity of the treatment in other respects, is we think very doubtful, considering the negative results of mercury in other hands, and combined with other modes of treatment. [It is an additional objection to calomel in large and repeated doses, that it is apt, as I have had occasion to observe, to bring on most alarming saliva-

tion after the apparent cure of the collapse. Dr. W. Begbie found, in 1854, that severe salivation occurred in three out of ten recoveries, treated according to Dr. Ayre's method of small and repeated doses of calomel. Out of twenty-seven cases so treated, when more or less completely in the stage of collapse, about one-half died. Notwithstanding this result, Dr. Begbie does not discountenance the use of calomel as he does venesection; he even thought it did good in individual cases.]

Tartar Emetic is strongly recommended by Dr. Billing, on the ground that cholera is a fever, and must be treated by sedatives and fever medicines. He considers the collapse of cholera to be similar to the cold stage of ague, and strongly reprobates stimulants in every form. He allows cold water to be liberally given, and even pushes his theory of the disease so far, as to administer quinine from the beginning. The tartar emetic is given in small doses. Dry friction is the only external application. In Droitwich Lunatic Asylum, where tartar emetic was the staple of the treatment, there were only four deaths in twenty-one cases; but the number of cases is too small to afford anything more than a presumption in favour of the remedy.

Injection of the veins was first introduced by Dr. Mackintosh of Edinburgh; it has been so frequently tried in cholera, and so frequently found wanting, that, notwithstanding its extraordinary effects in the first instance, we should hesitate to recommend it in any case which presented a hope of recovery under any other treatment. Nevertheless, the high mortality ascribed to it in Mr.

Ross' tables (85·7 per cent) is evidently to be ascribed to the trials of it having mostly been made upon moribund cases, in which alone it is by many practitioners thought justifiable. We refer our readers to Dr. Robertson's remarks on this subject. Nevertheless, we think that, if this treatment is to have any chance of success, it must be by being tried earlier in the disease, and *repeated as often as the collapse recurs.*

Emetics and *strong Purgatives* (such as croton oil) have each had their supporters; but neither from theory nor from experience can we gather much satisfactory testimony in their favour.

Chloroform inhalation has been used in thirty-seven cases in Peckham-house Asylum, all of which presented characteristic symptoms.—(See *Med. Gazette*, Nov. 24, 1848, p. 903). The number of cases is too small to enable us to form a decided opinion upon the practice, more especially as the details of symptoms and treatment are not given. In the meantime, the results are superior to most of the methods in Mr. Ross's tables, but inferior to the tartar emetic practice in Droitwich Asylum, before referred to, and very decidedly inferior to the results of upwards of 700 cases treated by cold water and saline medicines internally. In the Edinburgh Hospital, chloroform was found to relieve the cramps while the patient was under its action; but with respect to the restoration of temperature, and amendment of the symptoms of collapse, it was believed to exert a negative, if indeed not an unfavourable, influence.

With regard to several other remedies which have

been faithfully tried in Edinburgh, we must again refer to Dr. Robertson's paper.

On the whole, we think it is clear enough that the specific for cholera remains yet to be discovered; and that none of the more vaunted cholera remedies present evidence in their favour so strong as to command an exclusive attention. On the other hand, it is much to be feared that the *routine heroic* practice of many practitioners, both in this country and in India, has aggravated to no small extent the mortality of this tremendous disease. This is especially the case, as we have already pointed out, with regard to stimulants.

On reviewing the evidence deduced from large numbers, we find that there are two modes of treatment which present so marked an advantage in respect of mortality, as to arrest our attention very forcibly. The treatment by common salt, with cold water given in abundance internally, produced in 607 cases (in various hands) a mortality of 20 per cent; and in Greville Street Hospital, 107 cases treated by a saline mixture, with copious draughts of cold water, gave a mortality of only 14 per cent, the lowest which has yet been recorded from a similar number of unselected cases. Nearest to these stands the treatment by ice alone, given by the mouth; a continental practice, of which the results are 30 per cent mortality. It is a remarkable fact, as pointed out by Mr. Ross, that in all these the administration of cold water by the mouth plays a prominent part; and when we consider the success which this remedy alone appears to have had in the hands of many

practitioners (especially Dr. Shute of Gloucester), it is impossible not to think favourably of it in cholera. Dr. Shute states, "that under this system the state of collapse is prolonged to two, three, or five days; and others have remarked, that during the reaction a paroxysm of raging delirium is apt to take place. It is not, therefore, an inoperative remedy." Add to this, that it is most grateful to the patient, whose burning thirst seems always to point to this as the most appropriate resource for his relief. If it be the case also, as seems every way probable, that the water so administered is either absorbed into the blood to replace the fluid lost, or tends to prevent the loss of fluid from the blood by the intestines, we can have no difficulty in understanding its beneficial effects.

We are most firmly persuaded that cholera, like all other diseases dependent on a specific poison, has a spontaneous tendency to cure after the virus has exhausted itself; and that the treatment will be most securely and successfully accomplished by discarding, in the majority of cases, heroic remedies; by following out the indications afforded by the feelings and desires of the patient; and, as Cullen said of fever, by attending to those conditions and means calculated to "obviate the tendency to death." Now, all that we yet know of the pathology of this disease tends to ascribe the fatal result in the collapse to a slow asphyxia, induced by the imperfect fluidity of the blood. In proof of this assertion, we would refer to pathological facts, as well as to the wonderful, though too transitory, effect of the injec-

tion into the veins. We would therefore endeavour by every means to supply fluid to the blood through the intestines, the skin, the lungs, or at least to prevent, in as far as possible, the fluids of the body from being thrown off by these channels.

This might be accomplished—1*st*, by supplying cold water in abundance by the mouth, as already mentioned, and as dictated by the thirst of the patient; 2*d*, by the use of baths of regulated temperature, at least at the *beginning* of the treatment; 3*d*, by maintaining the body of the patient *throughout* the treatment in contact with fluid media, or at least with fluid vapour, by means of soaked cloths placed around him, and covered by a sufficiency of blankets; 4*th*, by surcharging the air of the apartment with vapour, particularly when the external air is dry or frosty.*

The third of these indications was put in force by Dr. Robertson by means of the hot wet sheet, surrounded by several dry blankets, very soon after the commencement of treatment in the Cholera Hospital; and the advantages of it over the use of heated air, as in the first cases in the Infirmary, was soon apparent. The mortality under the latter practice, indeed, was so considerable as to cause it at once to be renounced in the Cholera Hospital. The hot wet sheet was found, however, to have some disadvantages in the case of adults,

* We are happy to find a corroboration of these views, as to the importance of fluids in the treatment of cholera, in an able pamphlet by Dr. Buchanan of Glasgow.—See "Observations on Malignant Cholera," etc. By Andrew Buchanan, M.D., Professor of the Institutes of Medicine in the University of Glasgow.

from the disagreeable sensations sometimes caused by it, which gave rise to restlessness and struggling, and consequent exposure to the air. We are disposed to ascribe these effects in part to its having been used very warm, and exclusively with the view of producing reaction by artificial heat, and think that many of these inconveniences would vanish if the temperature were studiously regulated by the feelings of the patient. [So employed, it was afterwards found very frequently useful, especially in children, both in 1849 and 1854. The warm bath was also largely used by Dr. Begbie in the early collapse, and found to be " a most valuable adjuvant to every kind of treatment."]

The regulation of temperature is, indeed, a most important means in the treatment of cholera. The extremes both of heat and cold appear to be ill borne. The momentary shock even of the cold affusion (followed by warm wrappings) has been useful in rousing patients from deep collapse ; but nothing has shewn its continued application to be beneficial. On the other hand, the exhausting influence of excessive heat, externally applied, has been noticed by many writers. The supervention of reaction appears to us to depend much more upon the reception of fluid into the blood than upon the application of external heat ; and we have seen it take place, and follow its usual course, where no external heat has been applied. Should the relation of the reaction to the fluidity of the blood be established, it will appear still further how paramount is the indication of treatment by fluids, to which we have alluded.

Meantime a moderate amount of heat, such as is agreeable to the patient, appears to us to have most evidence in its favour in the treatment of the collapse.

The relief of the spasms is the next important indication of treatment during the collapse. In relation to this symptom, further trials of chloroform appear to be requisite, unless it shall be found to affect unfavourably the progress of the case. The use of frictions to the affected parts, and of stimulation of the skin by liniments, or by mustard cataplasms, is universally admitted to be useful. [Tight bandages on the limbs, as recommended by Dr. Wise, were found useful by Dr. Begbie in 1854.]

Finally, in the management of the reaction the most important indication appears to be the restoration of the urine, and of its normal constituents, especially the urea and uric acid, which are often deficient, and appear by their deficiency to lead to coma. [From Dr. Begbie's experience this would appear to be best accomplished by acetate of potash in doses of ℈i, largely diluted, and frequently repeated. Stevens' saline powders were of no use, and were seldom kept on the stomach. Cupping, or dry cupping over the loins, appeared to be of service, in some cases. And the catheter was repeatedly used, with the effect of unexpectedly procuring urine from the bladder.]

XII.

SYPHILIS.

(*Lecture,* May 17th*, 1861.)

THE subject to which I intend to direct your attention to-day, is one of very great difficulty. It is one often claimed by the surgeons as their peculiar field, but we cannot give it up to them entirely, seeing that its relations to medical practice generally are so vast and complicated. I refer to Syphilis, in all but its primary manifestations.

We have many examples of this disease in the wards at present, and as we may not soon have another opportunity nearly so good of speaking about it, I shall take it up to-day.

Syphilis, as seen by the physician, has generally arrived at its secondary or even tertiary stage ; for the cases of primary infection generally find their way either into the special wards, or into the general surgical wards. The result of this is, that in the medical department we see very little of primary syphilis. Another consequence is, that as we see the disease, it is very apt to be

* Revised from report of Dr. Joseph Bell, compared with that of Mr. William Ketchen.

extremely complicated—mixed up with, and overlaid by other constitutional and local diseases.

In many cases the diagnosis is not easy. Sometimes a positive diagnosis is practically impossible, and very often it is not even expedient to attempt to press the diagnosis home. In doing this at all in any case, I need hardly say that great delicacy and discretion are required. And this is more than a matter of mere good feeling and politeness, or even of external decency, in my opinion. I must warn you that it is even morally wrong to insist on getting at the whole facts of every case. I wish you to understand me clearly on this point. There are cases in which you ought to see and know everything that you can see and know; but there are equally cases in which you ought not, and very many of the cases in a physician's practice belong to this class. If the interests of the individual patient really require it, there is nothing that you may not say or do without being guilty even of indelicacy. But to require an examination of the parts in every instance, in order to discover traces of old sores or buboes, as is done in some continental hospitals, is not only an outrage on decency, but a total misunderstanding, in my opinion, of the duty of the physician, especially in advanced cases of the disease. Nay, there are cases of real or supposed syphilis, and these not at all uncommon, especially in women, and most of all in married women, where you may do an irreparable wrong even by hints and suggestions of evil. Keep this constantly in view, and be guided in your inquiries, not by any indiscriminate iron rule, but

by a due discretion exercised for the benefit of each individual patient, both moral and physical. Study the benefit of the patient, and do not be led away by any mere medical or scientific curiosity. In many cases, especially those of long standing, I am content to leave the question of infection practically undecided; for holding, as I do, that syphilis is a disease which in course of time wears itself out of the constitution, and not one which requires to be checked at every turn, and combated by strong specific remedies, I believe that for purposes of treatment we do not require absolute certainty of diagnosis, in all possible cases.

With these preliminary observations, I now proceed to give you a very brief sketch of points to be kept in view in making inquiries into the cases before us.

Our diagnosis rests on an accurate investigation, so far as appears possible or expedient, of the following particulars :—

1. Has a primary sore existed on the genitals, or elsewhere? The *primary* sore of syphilis (the infecting chancre of Ricord) may often be very small, causing at the first little discharge, and frequently leaving, after a time, very slight traces of its presence. The sore is often not at all painful, and it is possible that in some cases it may even really escape the notice of the patient. I was once told by a very old acquaintance of my own, who came to me labouring under manifest constitutional syphilis, and who could have no possible motive for concealment, inasmuch as he did not, in fact, attempt to conceal the truth in the least, that the sore from which

the whole infection resulted was almost imperceptible, and caused him absolutely no uneasiness. Practically speaking, you may say that the primary sore is sometimes really quite overlooked at the time, and forgotten afterwards. No doubt, people often deceive themselves, as well as others, in these matters ; but the fact of such self-deception is *a clinical fact*, and one of great importance, which you must not neglect to take with you in all your inquiries.

2. As a rule, the primary sore is soon followed by a small, painless bubo in the line of the lymphatics. Most commonly this is not at all a prominent or obtrusive phenomenon ; indeed, if it is prominent or obtrusive, it is much more likely to be the result of a gonorrhœa, than of true syphilis. Bubo is simply an enlargement, from syphilitic poisoning, of the lymphatic glands, in a line extending from the primary sore to the lymphatic centres. It is often a very slight enlargement, as I have said, and not at all painful. The glands, remaining loose in the cellular tissue, form a little group, or *pleiad*, as Ricord calls it, rolling freely under the finger ; it is not often that they inflame, or go on to suppuration. As to the true nature of the syphilitic bubo, it must be regarded as still an open question whether, when the glands are indurated, the secondary constitutional infection has already begun, as Ricord believes. Most probably, I think, it is so.

3. After an interval varying from six weeks to six months, or longer, after the primary infection, the symptoms, commonly called *secondary*, make their appearance.

A roseolar, or erythematous rash, which is often succeeded immediately by papular, pustular, or scaly eruptions, may go along with sore throat, or falling of the hair, or enlargement of the cervical glands near the occiput, or tubercles of the mucous membranes, or inflammation of the eyes, especially iritis. The cutaneous eruptions, however named or classified, are more or less peculiar; they are remarkably symmetrical in their distribution, and curvilinear in their grouping; they are very apt to change, as regards the special forms, but yet to be remarkably permanent in their results; leaving behind them stains of colour, or cicatrices, or persistent ulcerations, affecting the true skin, to an extent very unusual in non-syphilitic eruptions.*

4. The sore-throat, beginning early and lasting long, may in some cases be at first little more than a peculiar colour of the mucous membrane, persistent, however, like the staining of the skin, and leading gradually to various forms of disorganization; superficial erosions, or deeper ulcers of the soft palate, or more diffused forms of ulceration, involving great destruction of parts, and spreading in all directions; encroaching on the nasal

* Devergie remarks, on the local distribution of syphilitic eruptions, as follows:—"Their seats of election, in the order of frequency, are the parts around the *alæ* of the nose and the angles of the mouth; the root of the hair at the forehead and back of the neck; the inner angle of the eyes; the centre of the breast; the inner side of the limbs; the neighbourhood of the axillæ, and of the groins. While all parts of the body may be invaded, the eruptions will be found, in eighty cases out of a hundred, limited to those here indicated; and among all these parts, it is the face which is chiefly apt to bear the marks of syphilis."—*Maladies de la Peau*, 2nd edition, p. 678.

fossæ and pharynx, eating away the epiglottis, extending down into the air-passages, and even causing necrosis and exfoliation of the cartilages of the larynx at a later stage.

5. Chronic glandular enlargements of various kinds, other than the proper bubo, accompany syphilis through its whole course in some cases. Of these the most significant probably is the "adenopathie cervicale," as M. Ricord calls it, viz., an enlargement of the chain of lymphatic glands extending up towards the occiput, behind the sternomastoid muscle. This enlargement, being painless, is generally overlooked by the patient, and does not tend to suppurate. It is not a sure sign of syphilis, but is often present in the earlier period of the secondary eruptions, and may last an indefinite time.

6. Among the later secondary or tertiary symptoms, we find rupia, pemphigus, other chronic pustular and scaly eruptions, the interesting and very intractable so-called tubercular affections of the skin, and eroding ulcers resembling lupus. Some of these we may regard as forming a sort of connecting link, bridging across the gulf between the secondary and the tertiary symptoms.

7. *Tertiary* symptoms. On these I have no time to enlarge, as they are exceedingly varied, and apt to be complicated with other diseases as to their diagnosis. The most characteristic are the well-known periosteal nodes and various affections of the bones, sometimes with enlargements of the liver and spleen, or with anomalous affections of the nervous system, originating in the brain and spinal cord. A general character of these disorders is the increase of suffering at night, and the

considerable and rapid relief often experienced from the use of Iodide of Potassium.

I shall now, in illustration of this brief sketch of the history of syphilis, bring several cases before you, some of distinctly syphilitic disease, and others of affections more or less resembling it. I must premise that, in presence of *some* of the patients, I shall put no questions, and offer no remarks, which would lead them to the knowledge of our suspicions regarding their antecedents. Some of the patients I shall present to you are, indeed, happily ignorant of everything connected with this subject; and for obvious reasons, I wish them to be none the wiser through us.

Case I. Janet C., æt. 26 (Remarks while patient was in the room).—A married woman. She is extremely pallid and flabby, but not correspondingly emaciated. She has a cough. There is complete aphonia. Her hair has, to a great extent, come out. A large part of the epiglottis has been destroyed by ulceration. The upper vocal cords can be felt by the finger to be roughened, and the arytenoid cartilages can also be distinctly felt to be thickened; in consequence of this thickening they cannot approach each other accurately, hence the loss of voice.* She has had an old affection

* In this case, as in some others of the series, the diagnosis of the larynx was made entirely with the finger. I am not insensible to the advantages, in certain cases, of laryngoscopic examination, which, after many unsuccessful attempts, I have lately been enabled (through the kindness of my friend and former pupil, Dr. Thomas J. Walker, of Peter-

of the left eye ; the cornea is opaque, and even since her admission she has suffered from a subacute form of iritis, for which she has been leeched. There is no glandular enlargement in the neck or elsewhere at present. With regard to the loss of hair, her history is very striking, and I beg you to attend to it particularly, as it has a bearing on the diagnosis. She states that the hair began to come out a long time since, when she was only three years old, but she has forgotten the circumstances. Now look at the scalp and you will see what I regard as a complete corroboration of this history, for it has all the appearances, almost indescribable, but recognisable by the skilled eye, of a case of *favus* of very old standing which has undergone cure, or rather come to an end spontaneously, with loss of the hair. There are no crusts now. The surface is generally smooth. The hairs that remain are extremely short and stunted. There are still a few small remains of white scab, but the peculiar yellow dry incrustation of vegetable mould, with its mousy odour, and quite characteristic appear-

borough) to practise with entire satisfaction, at least as regards seeing the parts in the line of the glottis, and beyond it, in the healthy subject. But I am still of opinion, that any one who has accustomed himself to the careful and scientific use of the finger carried fairly over the epiglottis, and down to the arytænoid cartilages, in the diagnosis of laryngeal diseases, will but rarely find his knowledge much increased by the comparatively troublesome and difficult method of laryngoscopy. Having scarcely ever omitted, for ten or twelve years past, at the least, to make the digital examination referred to in cases carrying the suspicion of disease of the larynx, I may be allowed to bear testimony to the simplicity of this little operation, and to the confidence it inspires, even at a time when a diagnostic method of so much higher pretensions is coming into use.

ance, is entirely absent. Favus is for the most part a disease of very early life, and this case is a most marked instance of the tenacity with which it keeps its hold, when not carefully managed at the beginning. This woman will give you a history of more than twenty years of successive eruptions on the scalp, each attended by a new loss of the hair, and to a great extent neglected as to treatment. I have no doubt whatever that her history is exactly as she gives it, though there has been nothing like accurate scientific observation of the disease.* (The patient now left the room, and Dr. G. continued.) You will remark that this circumstance of the *favus* entirely destroys the value of any conclusion which you might be disposed to draw, as regards syphilis, from the loss of the hair in this woman. If you examined her somewhat carelessly, or with imperfect knowledge of the facts, you might easily run away with a hasty conclusion, founded upon a very wrong estimate of this particular symptom; and furthermore, you might be right in your conclusion, though wrong in the premises. I think it very likely that this woman has had syphilis. Her nose is flat at the base, she has had ulceration of the throat, and probably of the nostrils; she has had chronic inflammation of the cornea, con-

* This patient returned about a fortnight ago (April 1861) to put herself under my care for a renewed eruption of unquestionable favus on the back part of the scalp. It was in the most wretchedly neglected condition, and full of vermin. The laryngeal affection had in the interval undergone a considerable improvement, but there is a marked fetor, apparently proceeding from the nostrils, and suggestive of diseased bone there.

junctiva, and iris ; she has even now a very marked chronic ulceration of the larynx. These circumstances are very suggestive, but they do not amount to complete proof of syphilis. On the whole, perhaps the chief circumstance which renders it probable that this is a syphilitic case is the laryngeal affection. You will ask, may this not be phthisis laryngea ? She is pale and delicate ; there is a chance, of course, of there being tubercular disease ; but we find in her lungs no unequivocal signs of tubercle. She may have phthisis ; I think it is even not unlikely ; but there is no distinct evidence of it. She has been improving much since admission, especially under the administration of iodide of potassium. Squalid, ill-nourished, ill-cared-for as her childhood must have been (witness the favus, which seems always to require such conditions for its development to any extent), her old eye affection may be merely one of the many forms of scrofulous disease. My belief is, that there is a syphilitic element in the case, but I shall go no further with the diagnôsis. She is a married woman ; she seems to have no suspicion of anything having been wrong. I will ask no questions ; it is far better in such cases, especially when of very old standing, to leave the diagnosis to a certain extent unsettled.

Case 2. Mrs. S., æt. 42.—This patient also has very complete aphonia. Her respiration, when I first saw her, was noisy, with occasional paroxysms of dyspnœa, which, on her admission, made us consider the possibility of tracheotomy being required. She, also, has

thickening of the false vocal cords, with nearly complete loss of the epiglottis from ulceration. You will see, also, the remains of a curious looking old ulceration, now quite cicatrized, round the corners of the mouth, not at all like any common result of herpes labialis, or of any simple eruption that I know. You will observe her pallid complexion, and her evidently deteriorated constitution. [The patient having been removed, Dr. G. continued.] This is, I think, unquestionably a syphilitic case, but the infection is a very old story. She is much better since her admission, having been treated by frequent inhalations of steam, and the internal use of the iodide of potassium, with good diet, and the local application of sulphate of copper in solution. This case has a very curious history. She has been married for eight years, and has had two children, both of whom are said to be healthy. Twenty years ago, however, we have learned by carefully-put questions, that she had an illegitimate daughter. We find, further, that this child of long ago had a skin eruption, and it was then, she says, that she herself had the remarkable ulceration round the mouth, of which I pointed out to you the traces still remaining, and which was, at the time, accompanied by a sore mouth inside. In fact she then had syphilis; I think there can hardly be a doubt on the subject. It is remarkable that in this case the disease, though continuing to affect her own throat during this long period, and though affecting the health of her first child (which died early), has so far lost its virulence that the second family has escaped. This is a case to

set against the doctrine of some great authorities, that syphilis, once implanted, can never be eradicated so as not to affect the offspring. I do not believe that doctrine, at least in any practical sense.

CASE 3. In this case, A. B., æt. 20, we have the advantage of a perfect history, which need not be concealed from you. This man got infected, he tells us, and had a chancre, at the New Year; when he also first became aware of what is still quite evident, a small swelling in the right groin. This swelling, which has all the characters of the syphilitic bubo or *pleiad* of glands, did not inflame much; it gave him very little pain. The primary sore itself did not trouble him much. Lately a rash came out on the skin, which is now gone, and to which, not having seen it, we cannot give a name; but this rash, whatever it was, has left a coppery discoloration on the surfaces on which it appeared. He has most distinct cervical glandular enlargement (*adenopathie* of Ricord), which does not give him the slightest pain or uneasiness, and of the existence of which he was not himself aware, till we found it out for ourselves in examining him. His voice is not gone, but it is hoarse. There is a very small superficial erosion in the middle line of the soft palate, and a slight affection of the vocal cords beyond the reach of sight, but which can be discovered with the finger. This is a most typical case, shewing a combination of symptoms which can result from nothing else but syphilis; and which, even without the history, I should not have had the slightest hesitation in pronouncing to be syphilitic.

Case 4. Mrs. B., æt. 42. You will remember having seen this woman at a former lecture. She is now nearly well. She sought admission on account of a serpiginous eruption between the eyes and on the forehead, which was, when you last saw her, in a very bad state of ulceration, tending to the phagedenic character. She had been drinking hard for some time, and was in a very bad way altogether. She has been cured by good diet, abstinence from whisky (though we gave her, perhaps unnecessarily, about 4 oz. of port wine daily) and the occasional use of a very strong solution of the sulphate of copper to the ulcerated parts. [The patient left the room.] I do not shew you this as a case of syphilis. It is under suspicion, but no more, of being connected with an old syphilitic infection; for as she has obviously led a very irregular life, she may have been in the way of becoming infected at one time. But there is nothing in the history she tells us which gives us a moral right to examine her more closely, the treatment being sufficiently obvious. She is a married woman, and there is no reason whatever to presume *recent* syphilis. As to her past history of years ago, if you asked her, she would probably dissimulate; and at this distance of time (for this eruption is tertiary if anything) we simply cannot, in all probability, know the truth.

Case 5. Mrs. C., æt. 26. This poor woman has, like some of the others, a very distressing sore throat, and her general health is far from satisfactory; she is nursing,

however, and has plenty of milk. There is a deep, angry-looking ulcer (of phagedenic character on admission) right in the middle line of the palate, just above the velum; from its depth, as seen from the front, it must be very nearly through to the other side. This, of course, we are endeavouring to stop—by good diet, hospital regimen, and the use, here also, of the sulphate of copper. She is much better. I wish you also to see her baby. (Go and fetch your baby and shew it to the gentlemen. I know you are proud of it, and you have good reason.)

[In her absence, Dr. G. continued]—This case might easily fall under the suspicion of syphilis on account of the throat affection and cachectic appearance, but she is a quiet respectable young married woman, with no history or even probability of syphilis. She has, too, a most excellent witness in her favour, which I have sent her to produce before you; namely a very fine healthy baby of three months old, with a pure fair skin, and not a trace of cachexia. I may here take the opportunity, however, of observing that in many cases, children with blotchy syphilitic eruptions on their skins may appear in other respects fat, healthy, and well nourished. An instance of this I brought before the class at the end of the winter session. A mother, in whose case the history of syphilitic contagion from her husband was undoubted,*

* If it had been doubtful, a curious coincidence occurred, some weeks after this lecture, which amounts to a complete confirmation of this. The husband came to me for advice, obviously affected with syphilis; and, meeting his wife on the stair of the hospital, he at once told Dr. Bell, the resident physician, who he was. This led to explanations

brought to me two successive children suffering from patchy eruptions on the skin, with alteration of the tissue of the epidermis in the neighbourhood of the anus. They were cured by the use of flannel bandages spread with mild mercurial ointment. In other respects these children were fat and apparently healthy. No doubt appearances are sometimes deceptive, the health of such children not being good in proportion to their appearance.

[The mother and child now returned, and Dr. G. continued]—Mrs. C.'s general health is, as you see, very much below par. Baby, on the other hand, is quite a model baby, very good natured, never fretful, and never shewing the least febrile or other unhealthy symptom. Its skin, you see, is perfectly pure and clean throughout. Its limbs are wonderfully fat and firm. You will test the health and condition of a baby (if you are wise) more by its legs and arms and belly than by the face. This one stands the test well. It is positively overflowing with life and high health and good humour. I hardly ever saw so fine a baby. The patient has two other children as strong and healthy as the present. In fact there has never been a complaint in the family. The mother has improved since admission under good diet and the treatment just mentioned. She may now return to the ward. This I regard not as a case of

by which the whole history of the infection was made quite clear. I am not sure, even now, that he may not have been moved to come to me by his wife, who, though behaving with great propriety and delicacy throughout, had plainly been led by her friends to suspect the true state of the case.

syphilis, but one of hyperlactation in a debilitated subject, which, aided by poor food and probably bad air, has produced a cachectic state of the system. Observe, too, that in this case the throat affection has commenced too suddenly, and advanced too rapidly to phagedæna, to be like syphilis. Syphilis is usually much more cunning and insidious in its mode of attack.

Case 6. Another case I must mention, though, as the patient is in bed in Ward XV., I cannot shew her to you here. —— ——, æt. 28. Confessedly a prostitute. This poor girl suffers from the periosteal results of syphilis. Nodes on her tibiæ; pains, very much aggravated at night, in nearly every bone in her body, especially in the tibiæ and in the hips. She has also a pimply eruption on her face, and on the top of the head. The pimples are large, resembling acne more than lichen. The treatment has consisted of tonics, with iodide of potassium, and occasional doses of morphia at night to procure sleep. The disease is clearly of long standing, but it is nearly uncomplicated. [On May 24th it was remarked on this case:—"The eruption on the face of —— —— has assumed a still more decidedly syphilitic aspect, if possible. It has the appearance of a very chronic acne; the pimples much elevated, and very large. Each individual pimple looks as if it tended to suppurate, but in the end they do not suppurate in this case. There is no accounting for syphilitic eruptions, which defy calculation in regard to their progress. They present themselves in all sorts of mon-

grel forms, and when you have given them a name one day, you find them shortly afterwards resembling something else much more closely. This difficulty of nomenclature and classification has been felt so much by authorities, that they have generally put the syphilitic eruptions altogether apart, in an order by themselves."]

There are two cases under our observation, probably of old syphilitic infection, and where syphilis may probably be responsible for certain obscure cerebral symptoms. I have no time to go into them at length, but very briefly the facts are as follows :—

Case 7 is, like the last, that of a girl who has been a prostitute. She has the " adenopathie cervicale," and there is no doubt, I think, of the existence of constitutional syphilis. She has a curious tremulous motion of the left eye, with occasional squint, and various uneasy sensations in the head, which at first looked very alarming, till it was found that they were under control of iodide of potassium. I will not detain you, however, on her case at present.

Case 8. James B., æt. 42. This man cannot be decidedly pronounced to have a syphilitic history, but the probabilities are all in favour of it. He admits having contracted a venereal affection, but says it was twenty-three years ago, and that it was only a running, with a bubo, and with no chancre. He admits also having had a cutaneous eruption some years ago. He has had rheumatic pains, and has something like nodes on the

tibiæ. No other symptom shewed itself, according to his own account, till a few weeks since. He now suffers from hoarseness. His articulation is somewhat impaired. Amaurosis is coming on. His walk is distinctly paraplegic. He has therefore symptoms of nervous disease, both spinal and cerebral. There is no history of an apoplectic attack, nor of anything like hemiplegia or softening of the brain; nor has he the symptoms of Bright's disease, nor of uræmia. In such a case as this, I think we should suspect syphilis, even without the history. In fact, whenever you have a very irregular series of cerebral or paralytic symptoms, the matter is worth inquiring into. Many of these cases (I don't always know whether syphilitic or not) improve under the iodide of potassium.

XIII.

HYSTERIA. DELIRIUM TREMENS. DIPSOMANIA.

Lecture, Friday, December 7th, 1860.*—I wish to make an observation or two upon hysteria, and some allied forms of nervous disorder, in connection with cases now or lately under our observation in the wards. We have had two very well-marked cases of pure hysteria since the beginning of the session. You did not see much of them, because I do not ever think it right to parade such cases before you ; but you heard of them, and some of you saw them. They were two young girls from a reformatory institution in town—one of those excellent charitable institutions, the idea of which is almost peculiar to our times, inasmuch as they are not, like the older Penitentiary or the Magdalen Asylum, intended for cases of confirmed depravity or crime, but only of bad habits and tendencies, requiring careful moral control and superintendence. Such institutions do a great deal of good, but they are also subject to occasional abuses and imperfections, like everything else in this world. From this particular reformatory we have had not only these two cases of hysteria, but one or two others before the session

Hysteria in a Reformatory Institution.

* From a report by Dr. Duggan, revised.

began ; and, in particular, one curiously deceptive case, where the paroxysms almost exactly resembled acute laryngitis ; possibly there may have been, indeed, a slight degree of laryngitis also. Now it so happens that a medical friend of mine, who, till lately, was the medical attendant of this institution, tells me that during twenty years he had hardly any trouble with it at all. Recently there has taken place a considerable change in the administration, and one of the consequences of the change, I suspect, is this cumulative state of hysteria—this little epidemic, in fact—of which we have witnessed some of the traces. I have not inquired, and do not wish to inquire, too closely into the matter, which is beyond my province ; but I must make this epidemic a part of your instruction, and I therefore tell you plainly, that on inquiry you would probably find that it is due to some form of injudicious moral management—undue fostering of the emotions at the expense of the active powers of the mind—the cultivation of sickly sentimentalisms of one kind or other, instead of practical habits of business-like occupation. I believe that this is so, because the epidemic is so strictly localized, and, also, because it is a new feature in the institution itself. In the three or four cases that were brought here, the mere removal to the hospital went far to cure them ; they were well in forty-eight hours. The treatment here did not consist in administering drugs, but in speaking kindly and firmly, inquiring carefully into all the facts of the case, and then giving a confident assurance that all would be right to-morrow ; and so they all obeyed instructions,

and got well. Hysteria, I may tell you, is a disease almost always the result, in some degree, of imperfect moral management; sometimes, no doubt, it has also, in part, a physical origin, as when it depends on disorder of the menses as a predisposing cause.

Hysterical Mania.

Another case under our care at present is not unlike aggravated hysteria; but I fear it is not to be so easily managed as those from the reformatory. This poor girl has a painful but too common history. She was seduced, lived for awhile with her seducer, had an illegitimate child by him, and was then, I suppose neglected; at all events there was a quarrel. It does not appear that there was any fixed idea of desertion on his part, for the young man has been here since her admission, and seems on good terms both with her friends and with the girl herself. But the result was, at the time, a terrible shock to her nerves; she was found by the police crouching in a doorway in some dark corner of one of the closes in the High Street, in a state of extreme trepidation and excitement, under the delusion that "the devil was coming to take her away," and that she was "going to hell." It appears that she must have jumped out of a window from a considerable height into the street. When I saw her some time after her admission, she seemed in danger of dying, partly from fright and partly from the exhaustion following excitement, from cold, and from want of food. She persisted in the delusion that the devil was coming to take her, and seemed impressed with the

idea that it was to be some time before midnight. She had, also, for some time, refused food ; in fact, there was reason to think she had neither eaten nor slept for several days. I have seen cases of mania where death was imminent from these causes, and from the physical prostration produced by the inordinate excitement. In this case the treatment was substantially based upon the same principles, so far as the mind is concerned, as in the hysterical cases ; but it required to be modified in accordance with the urgency of the case. The first thing wanted, clearly, was bodily sustenance—food, in short, not crammed down her throat, but taken, if possible, *with a will.* We had, therefore, to create, or educe, a will, as it were, out of the wreck of the mind ; and here the true principle is, not to sympathize too much in words, still less to lay yourself open to the charge of harshness, but to steer a middle course between these two, by firm, authoritative, at the same time, sympathetic conduct.

Two errors are often made in such cases, as matter of routine. Shower-baths and other strong impressions on the surface are useful in many cases of nervous disease, but they are often overdone ; in this case they would have been almost certain death. Again, to load the stomach of this patient with fetid antispasmodics would have been to destroy what little appetite she possessed. The true course is to gain the patient's confidence ; and this is usually not difficult up to a certain point, for the insane or hysterical mind, in spite of itself as it were, tends to lean trustfully upon the strong and

healthy mind, provided the circumstances of the case are carefully and delicately probed by the physician in the exercise of his office; a firm and genial tone of authority being at the same time adopted. Here is the sort of impression (put into words) that you want to produce:—"You may trust me to do all that I can to protect you from the danger you fear, and *I can do it; only you must do exactly what I want you to do.*" This sort of impression must be got, however, not so much by words, as by manner, by looks, by pressure of the hand, by carefully considered conduct in detail, by fifty little things which cannot be foreseen or remembered afterwards, but which are naturally suggested by the situation of the case at the moment. In this case we very soon succeeded in giving and getting a certain amount of confidence, and I then told her decidedly and seriously, that I would be responsible for her being perfectly safe, but that she *must* take food, and keep herself alive. She said she would try; she did try, and succeeded every day in taking some. She has been getting decidedly better, and is now out of all immediate danger; in fact, she has ceased to talk about her delusions, but I fear they still exist, and if this condition continues long, we must consider it as a case of mania or monomania, and transfer her to an asylum.

Friday, January 4th, 1861.—The case of hysterical mania which I mentioned in a former lecture has had to be sent to Morningside Asylum, as I anticipated. She will there have the benefit of a more complete sys-

tem of attendance, more entire control, more facilities for occupation, better air, more room for exercise than we can give her here; and very probably she will get well. You will have observed that I discountenance the use of much medicine in this class of cases, and trust almost entirely to hygienic measures. There is another class of cases of which the same thing may be said in a general way, and of which we are now beginning to have some examples in the wards devoted to delirious and noisy patients. I allude to those forms of mental aberration due to intemperance, or what is technically called delirium tremens. We have three of these cases in the ward at present, and may expect more shortly, in consequence of the great amount of ardent spirits usually consumed at this season of the year. Were I addicted to changes of nomenclature (as I am not in general), I should wish for a new and more comprehensive term for this disease; for though delirium and tremor are both frequent enough symptoms, yet you may have all the essential phenomena of nervous disturbance consequent on the use of intoxicating drinks without tremor, and without well-marked delirium. We may have, in fact, every degree and kind of nervous irritability, and almost every kind of cerebral disorder in turn, due to this cause—from the fidgety, dyspeptic, and prostrate condition, vernacularly called "the horrors of drink," to the most exaggerated mania or the most desperate and repeated epileptic attacks.

The cases now under observation are very characteristic. The first is that of a man (W. N——, aged forty-

five) who is already improved since admission. He has been very delirious, but is now comparatively quiet. He addresses me as an old friend, and alludes to many transactions as having passed between us; for my part, I don't remember ever to have seen the man before. His delusions are very decided and insurmountable; they refer chiefly to his business, for he is a publican by occupation, and he fancies he is behind his counter, selling liquor to his customers; nothing can root this idea out of his mind. He had been indulging very freely for some days before admission. Yesterday he was so unsettled as to give a great deal of trouble to the attendants; and though hardly so violent as to be dangerous, he managed to break a window by dashing his hand through the pane, in performing which feat he cut his hand pretty severely. We had him removed, for safety, to one of the padded rooms; though I must tell you that I have a strong objection in general to using these rooms at this season, because we are unable to keep a fire in them, and some of these cases suffer very much from darkness and cold. If I had my choice, indeed, and plenty of space, I would treat almost all cases of delirium tremens by more or less of exercise in the open air; but that is impossible here without giving rise to great annoyance, from the small means at our disposal in the way of exercise-ground. This man did not suffer from being in the solitary chamber; on the contrary, I found him always quite warm, happy, and comfortable, shaking me by the hand, singing, and going back to his prison with the best possible grace.

Delirium Tremens.

I have left him there, therefore, without hesitation, only giving directions to the attendant to visit him every hour or two, so as to see that he does not catch cold. I am told that he has not slept much, but has taken food freely. Now, bear in mind always that the chief danger of maniacal and delirious attacks is exhaustion ; if the patient digests his food, the danger is much diminished. This man certainly appears in no great danger of exhaustion. I am not apprehensive as to his life. There is a risk, perhaps, that the case may degenerate into chronic mania, but I think that in all probability he will come round, and get well without any further treatment.

Another case of the same disease is that of G. S——, aged thirty-six, a cab-driver. He has been drinking hard lately, as usual at the new year. He complained at first of a pain in the right flank, and a doctor in town applied leeches and a blister. He first began to have delirium on New-Year's-day, and was brought here on the 3d inst. He is constantly talking about horses, and sits in his bed holding the reins, as if in the act of driving his cab. The delirium is not so violent as in the case of the publican, but it is emphatically the same restless, happy, good-humoured delirium, bearing largely on the habitual occupation (well called by Dr. Watson a *busy* delirium). It is not always so *sunshiny* as in these two cases—sometimes quite the contrary—but almost always restless and fidgety. Now, let me tell you in general terms (and this I hold to be a very im-

portant doctrine) that *these patients absolutely require, as a rule, nothing but careful nursing—i.e., adequate protection and good food, adapted to the state of the digestion,* which is usually feeble. Very often I give no medicine at all. When the sleeplessness is very prolonged, I sometimes give *moderate* doses of opium ; never the high doses that are often prescribed in this disease, and never cumulative doses, involving the risk of poisoning. When the disease and the excesses that have led to it are of recent origin, I hardly ever think of giving stimulants, at all events in quantity; but when the patients are much debilitated, and the disease is due to very long-continued habits of drinking, and not the immediate effect of a debauch, I sometimes think it right to give a very moderate allowance of whisky, and I believe they take their food all the better for getting it.

The third case (W. M'N——, aged thirty-four) is one in which this treatment might have been requisite, had the disease degenerated into aggravated delirium tremens ; but it was merely an exaggerated case of the " horrors," marked by distinct tremor of the hands and tongue, rapid and feeble pulse, complete loss of appetite, melancholic and hypochondriacal delusions, considerable prostration, and costive bowels. He improved rapidly under laxatives and tonics.

Friday, January 11th. All the cases of delirium tremens are now getting well. I mention this particularly, as illustrating the doctrine I have delivered to

you—viz., that simple treatment, consisting chiefly of good nursing and food, will cure the most of these cases. Since I adopted the plan I mentioned, I believe I have never lost a case that was not complicated with either surgical or medical disorder, such as fractures or other severe injuries, pneumonia, etc. Another remark, however, ought to be made as qualifying this. I do not think that delirium tremens is quite so formidable a disease, on the whole, as it once was. The habits of the population are improved of late years; there are fewer cases altogether, and certainly fewer aggravated cases, such as I used to see a dozen years ago, when, however, I must admit, the usual treatment was such as in many cases to increase the disease. This year there have been fewer cases, I think, up to this date, than I ever saw before at this festive season.

ADDITIONAL REMARKS.

(*April* 1862.)

The treatment here indicated in delirium tremens is similar in principle to that advocated by Dr. Ware of Boston, in 1831, in a most able and philosophical sketch of the natural history of the disease, reprinted in the *British and Foreign Medical Review*, vol. xxiii., p. 603. My own convictions on the subject, however, were de-

rived, not from Dr. Ware, but from the late Dr. Hood of Kilmarnock, an excellent practitioner and most enlightened man, for whose opinion I had great respect, and who told me, a good many years ago, of the great success of an expectant and tonic practice in his hands, in a most extensive and fruitful field for the observation of this disease. Dr. Peddie has since brought together many important observations bearing on this subject, in his valuable memoir "On Delirium Tremens," published in 1854. Dr. Laycock is also an adherent of a practice substantially hygienic, and has written a striking paper on the subject in the *Edinburgh Medical Journal* for 1858.

In general terms, I may say that since my communications with Dr. Hood, above referred to, I have always treated delirium tremens as a spontaneously curable disorder; not by absolutely withholding remedies, but by using them in strict subordination to good nursing and carefully-adjusted diet and regimen. The result has been, as stated, that no case has been lost, unless complicated either with surgical injury, or with internal organic change, or with both together; of these last I have seen a considerable number, as the Infirmary has only one male and one female ward for surgical and medical cases of this kind. I am not able to state the total number of cases of delirium tremens, or of acute nervous affections the result of intemperance, that have been under my care in the Infirmary, with any approach to numerical accuracy; partly because the ward has changed hands often, and partly because the responsibility for the treatment of the surgical and the medical cases re-

spectively has not always been so clearly defined as it ought to have been. But for the last three years I may be presumed to have had at the least my full share of such cases, having had charge of the wards appropriated to noisy and delirious patients at that period of the year which is, above all others in Scotland, apt to be given over to dissipation—viz., New Year's Day, and the week or more before it and after it. Speaking from older recollections, I am pretty confident that there is proof in my experience of the improved habits of the population at large; for in three separate quarters (of three months each), recurring annually for three successive years at this period, there have been only about 30 cases in all * which can be placed under the title of delirium tremens. A considerable proportion of these, too, were what I am in the habit of calling *miniature types* of the disease as we used to know it, when I resided in the Infirmary as resident physician in 1847-48. This fact, of the diminishing severity of the disease, is one which I think probably Dr. Laycock has not sufficiently taken into account in his otherwise very philosophical and useful paper above referred to. I have no doubt at all, indeed, that many of his cases, like many, or perhaps most, of my own, were *miniature types;* and I think it not easy, or rather not possible, to draw exact nume-

* An inspection, since this was written, of the hospital weekly report book for Ward X. gives 47 cases; but this book is, I am afraid, rather lax in its principles of diagnosis (to judge from some particular instances which I remember). All the cases seen by me are there, probably, and also others which it was difficult to classify. I believe the number given above to be the more accurate one.

rical inferences from these cases, as to the probable mortality of delirium tremens in the older periods of the history of the Royal Infirmary, if it had been met in those days by being left to nature, or by a treatment in the main expectant, such as we employ at present.

But agreeing, as I do, closely with Dr. Laycock in essentials, I am little disposed to object to the forms of his criticism on the older practice, or to scrutinize too narrowly the details of his cases, or of their treatment. Beyond all doubt, it must be conceded by every one who has given fair-play to nature, as he and I have all along agreed in doing, that the old routine treatment by spirits, and by large repeated doses of opium and hyoscyamus* was, in all but the most wary hands, positively injurious, and in fact only a little less injurious than the treatment by bloodletting, which it superseded. I think there is little doubt that, in many cases, it simply substituted narcotic poisoning for delirium tremens; and of this fact I firmly believe I have been but too often an eye-witness, when acting under superior instructions in the Royal Infirmary in 1846-48, and under the rule of

* It has often been to me a curious subject for speculation, whether the enormous, and too clearly poisonous, doses we were directed to administer in Edinburgh, were not sometimes prevented from being visibly and manifestly poisonous outright, chiefly by the combination of opium with hyoscyamus which was then insisted on as the correct plan of treatment. Mr. Benjamin Bell and others, who have studied the presumed antagonism of these two narcotics, may be interested in knowing that the *formula* of 1846-47 in the Edinburgh Royal Infirmary was pretty nearly as follows;—℞. Tinct Opii, ʒi; T. Hyoscyam, ʒij., Spt. Commun. (whisky), ℥i. ad ℥ij. haustus p. r. n. sumendus, donec somnus supervenerit.

procuring sleep, at all hazards, by the *continuous* administration of narcotics. Indeed I may say it was the experience thus personally acquired which made me so curiously and minutely inquire into the success of Dr. Hood; for I was thereby predisposed, as it were, to adopt, in the main, his carefully-guarded principles of treatment; to which, I believe, he had been led almost purely by personal experience during a long course of years, possibly fortified (though he did not say so) by the facts in Dr. Ware's paper. I regret very much that the death of Dr. Hood, which occurred a good many years ago, precludes my referring to him for a more detailed account of the facts he communicated to me;* but they were to the effect generally, that delirium tremens was a disease of extremely small mortality in his own practice, and that he ascribed the small mortality mainly to his having for years abandoned all very active

* Dr. Hood was at one time known to practical men, as he still is to those who are curious in the literature of practical medicine, by a memoir on "Spasm of the Glottis from enlarged Thymus," which was based upon the careful observation of nine cases, and being published in 1827, was really the first, as it probably is still the best, of the now rather numerous contributions to that difficult and rather unsatisfactory subject. Dr. Hood's memoir laboured under the disadvantage of being published in a short-lived journal of limited circulation, inaccessible to many or most special inquirers, and hence he has never received the credit due to his intelligent and perfectly sober-minded treatment of the subject. (See Copland's Dictionary, vol. ii., p. 679; and the *Edinburgh Journal of Medical Science*, for January 1827). I am sorry to observe that my friend Dr. West, usually so unassailable in all that relates to the literature of his subject, has not succeeded in finding out Dr. Hood's memoir.

treatment by narcotics and stimulants. This struck me at the time (I think it may have been about 1849 or 1850) as new doctrine; but I am ashamed to say that I did not even know of Dr. Ware's paper till long afterwards.

It is lamentable to observe that the medical journals are even now occupied to a considerable extent with extremely active specifics for delirium tremens, founded on the idea that it is normally a disease of very high mortality; for surely nothing less than this can justify the administration of enormous doses of digitalis, as proposed by the late Mr. Jones of Jersey, or of hardly less extravagant quantities of cayenne pepper given with gin and water every four hours, as recommended by Dr. Kinnear.* It cannot be unnecessary under these circumstances to state that of the thirty or more cases referred to above, as having been under my care in the Infirmary during the last three years, all of which have been under an extremely simple and natural treatment, only one died; and this one had very extensive double pneumonia, involving three-fourths of one lung and about a third or a fourth of the other; the pneumonia having probably been present long before admission, but masked by the delirium. There was also old disease of the right lung, which was the one chiefly involved in the more recent inflammation.† Of

* *Lancet*, March 8, 1862.

† This man had been driving the Dunfermline coach throughout the very severe snowstorm of Christmas 1860, which left the ground for many weeks covered with several feet depth of snow, and almost rendered transit by coach impossible. His wife told me that he was ailing more or less throughout, and was keeping himself up with whisky, of

the cases not numbered, but attended by me in hospital before these cases, and since I became ordinary physician, one only died: and this was a case of very severe delirium tremens, complicated with fracture of a rib, and with pyemic pneumonia. It is briefly noticed in the paper on "Five Years' Experience of Pneumonia" (III., p. 48), and more fully in the *Edinburgh Medical Journal* for August 1856, p. 129, from which I have given an extract in the foot note below.* The only

which he had certainly been consuming large quantities, though she could hardly form a notion how much, owing to his being constantly on the road. This man (W. H., æt. about 40) was admitted on the 13th January 1861; so that his case is neither referred to in the preceding lecture, which was delivered shortly before, nor in the former paper on pneumonia, the details of which are only carried down to 1859.

* "He complained of pain in his side, and was so restless and excited, after admission, as to be placed under restraint until proper attendance could be secured. Next day I found him sweating profusely, more prostrate, and less excited, but still very fidgety and nervous. He was removed to the ward for such cases, had a special male attendant placed over him, and the straight-waistcoat removed. It was plain, from the physical signs, that the lower half of the left lung was in an early stage of pneumonia. After appropriate treatment, however, by antimony and the regulated administration of opiates, he became apparently much better, as far as the head symptoms were concerned. It was afterwards found that a fracture of a rib had existed, which at first was overlooked, from the imperfect history which the patient's state of mind, on admission, enabled him to give of the disease. The surrounding parts, irritated, I suppose, by his perpetual motion during the maniacal paroxysm, became the seat of suppuration, and a low type of fever accompanied this, which terminated the patient's life during my absence in the country. The sterno-clavicular articulation was found to contain pus, and the case was no doubt one of purulent infection of the blood from the fractured bone."

other fatal case of delirium tremens which has occurred to me for at least ten or twelve years past, was one which I saw in consultation about four miles from Edinburgh. The patient was a stout, full-blooded, rather over fed-looking farmer and grazier, who had been at Dalkeith fair eating and drinking constantly for days together, and was seized with very acute delirium tremens, complicated with severe double pneumonia, as in the first case mentioned above. This patient I saw only once, and I must admit that the treatment was extremely puzzling, as it was absolutely impossible to keep him in bed, or even in the house, without subjecting him to a degree of restraint which greatly aggravated his sufferings from the chest. The case was altogether one of the most unmanageable I have ever seen, although I think I may say that every variety of this disease, and almost every possible complication of it, have occurred within my hospital experience, first as house surgeon and resident physician, and afterwards as ordinary physician. I must add, that I have not included in the statement above mentioned some cases of fracture of the skull and other injuries of the head in connection with intemperance, in which it was practically impossible, during life, to say how far delirium tremens was the ruling condition ; nor have I included cases of typhus fever in intemperate habits, a considerable number of which have been fatal within my experience, with symptoms more or less resembling delirium tremens, but always, I think, distinguishable from it, either through the history of the attack or through the

presence of the eruption. It will be seen from the statement given above that pneumonia and old disease of the lungs (chronic induration, perhaps, from a former pneumonic attack in the upper part of the right lobe) are really the only organic complications of any importance that I have met with in cases of fatal acute delirium tremens not arising from accident; and nothing in the natural history of this disease is more remarkable than the rarity of such complications, and especially the rarity of serious organic disease of the liver and kidneys, and of tubercle of the lungs in connection with a disorder so indisputably the result, in most cases, of confirmed habits of intemperance. I am content in the meantime to note this fact, without attempting to discuss it. One branch of the subject has been treated by Dr. Ogston of Aberdeen, Mr. Ancell, and others; but the infrequency of tubercular disease in confirmed drunkards is, in my opinion, part only of a very wide question, far beyond the limits of the present paper.

I am well aware how difficult it is to *stand by* in such a disease as delirium tremens without canvassing anxiously the probabilities of modifying the disease by remedies; and it is on this account, chiefly, that I have been so careful to place in a clear point of view the grounds on which confidence in the spontaneous curability of delirium tremens is to be justified. I am very far from making pretension to such an amount of philosophic indifference as to have adopted "expectancy" *as a system*, in this disease or in any other. The statements of personal opinion here expressed are, on the contrary,

the direct result of experience of remedies; *i.e.*, of having again and again tried, in the course of hospital and other experience, most of the remedies habitually used in delirium tremens; tried them, however, with perhaps more than usual caution and reserve, and always under the feeling that Nature has ways of cure of her own in this disease which are greater and better than our ways. The record of this experience would therefore be incomplete if I did not add to it a few remarks on the details of treatment, and on the limits within which the use of particular means is safe or beneficial.

1. The first element in the safe treatment of delirium tremens is, unquestionably, the protection afforded by a good and thoroughly practical system of nursing; that is, one not guided by any absolute rule of conduct prescribed by the physician, but rather leaving to the immediate attendants of the sick a certain discretion, and insisting only on kindness, cheerfulness, and firmness in carrying out positive orders, with a view to prevent possible mischances. The patient in delirium tremens, though often very troublesome, seldom requires the direct control of physical force; he is easily led by *humouring* him, and is far too much occupied with his own multitudinous, busy fancies, to meditate violence or injury to any one, unless provoked. It is the business of the attendant, therefore, by no means to resist him further than is necessary for protection, but rather to take advantage of his ravings as occasion may offer, inducing him to eat, to lie down, to walk about, to put on or take

off articles of clothing, according to circumstances. Generally speaking, a little good-humoured attention to details will go a long way, even in the most violent and maniacal cases, towards preserving a certain amount of control; and when a sufficient number of qualified nurses, male and female, can be secured, as in a well-regulated asylum, it is commonly quite unnecessary to resort either to solitary confinement, or to any form of mechanical restraint. I have, however, been obliged to adopt both of these methods of treatment as supplementary means, in exceptional cases, especially under occasional pressure, or in the absence of a proper staff of attendants; in some cases, I believe, without the slightest injury, as in the case of W. N——, mentioned in the preceding lecture. A far preferable plan in most cases of excitement would be to allow the maniacal paroxysm to exhaust itself by exercise in the open air, under the eye of the attendant; and in a proper airing court this would, I believe, be quite safe, as patients in this stage of *mania à potu* are rarely suicidal, but rather disposed to enjoy life, after the fashion dictated by the delusion of the moment. Darkness, confinement, and the sense of perpetual obstruction in detail, may indeed develope a state of depression, especially under the influence of cold and want of food; but this it should be the object of good nursing to avoid. I am certainly of opinion, however, that many cases of this disease can be adequately treated only in asylums; and accordingly it has often been to me a subject of regret, that the law does not permit confinement under some compara-

tively simple form, requiring only a report to the Commissioners in Lunacy, in cases of temporary insanity, not extending beyond a week or a fortnight. The result of the present system is simply an irregularity of procedure, by which the most extreme practices of lunatic asylums, and some which are almost unknown in them, have to be introduced into general hospitals and private houses, without any direct responsibility to the constituted authorities.

2. Next to good nursing, especially in the numerous cases attended with debility, stands nourishment. This is sometimes a difficult indication to carry out, on account of the prostration of the digestive functions, and the total absence of anything like appetite. In most cases, however, food will be taken almost mechanically, if offered in a simple form, at the right moment, and in the right way; and it is here that the *bribe* of a small quantity of the habitual stimulant can sometimes be employed with advantage, and I believe without injury, in the stage of sleepless delirium, when the appetite is much impaired. I have so often seen this method effectual, that I should be sorry to abandon it in favour of any positive rule of total abstinence from alcoholic liquors in the treatment of this disease; though I entirely agree with Dr. Peddie* in believing that alcohol,

* On the Pathology of Delirium Tremens, and its Treatment without Stimulants or Opium; Edinburgh, 1854. A reprint, with large and important additions, from the *Monthly Journal of Medical Science*. The evidence adduced in this valuable memoir, from the experience of jails and other public institutions, tends to prove that even the most habitual drunkards may be suddenly and totally deprived of their habitual

when deliberately and systematically conveyed into the system in quantity, is injurious ; while it is certain that the cure of the disease upon total abstinence principles is quite possible in most instances. After the end above mentioned has been attained, and still more, after the crisis of the disease, I am very strict in withholding everything that can suggest a lenient consideration for the destructive habit. The preferable forms of nourishment are soups, beef-tea, panado, sack-whey, and sometimes porridge. Sometimes a laxative, followed by some bitter tonic, has appeared to operate favourably in lingering cases of nervous and dyspeptic exhaustion.

3. In some cases purgatives have appeared to me to be indicated from the first. These cases are known by the flushed, bloated appearance, the very foul tongue, the mawkish peculiar odour of the breath, the fetid discharges from the bowels, and the history of a recent surfeit of eating as well as drinking. In such cases nourishment ceases to be the paramount indication ; and I can well understand, in them, the administration of tartar emetic, even in a high dose, though I have rarely used this remedy, and must consider tartar emetic as, on the whole, probably superfluous, if not in some cases

indulgence without any appreciable bad effects, and of this I have myself no doubt whatever. If I advocate the use of a very limited amount of the stimulant, therefore, in certain cases along with food, it is not from any sympathy with the erroneous pathology which Dr. Peddie has so successfully combated, but strictly on the grounds, and within the limits, stated in the text. (For further remarks on this subject, see p. 277.)

positively injurious by interfering with the already weakened digestive powers.*

4. Opium, the sheet-anchor with many authorities in delirium tremens, has been employed by me, as already stated, only to a limited extent, and in strict subordination to the foregoing more natural remedies. Yet I cannot readily assent to the doctrine, that opium is always injurious, or even useless, in this disease. On the contrary, I believe that it may often be administered with safety and advantage in protracted cases, provided the quantity given in twenty-four hours is never allowed to exceed the full dose which would be considered safe for a healthy person, of the age and sex of the patient. It has been my uniform habit, where the giving of opium has appeared to be indicated in protracted cases, to push it on rapidly for two or three doses, watching carefully the effects ; and to discontinue it afterwards, for at least a good many hours, without regard to the immediate consequences, so soon as a full maximum amount of ʒiss. to ʒij. of the tincture in all, has been reached ; or even sooner, *if the pupils have become at all considerably contracted under its use.*

This rule, of regulating the effect of opium, in cases of protracted sleeplessness, not by the production of sleep absolutely, but by the state of the pupils, is one that I have for years insisted on, not only in delirium tremens, but in many other states of the system in which it is only too easy to push this remedy over the hyp-

* On the other side of the question, however, see Graves, *Clin. Lectures*, 2d edit., i. 205 ; and Peddie, as above.

notic, and into the narcotic stage of its action. Another rule of no small importance, I believe, is, that the opium should always be administered in the fluid form ; otherwise it is apt to accumulate in the bowels, owing to the weakened state of the digestion. For the same reason, its use should often be alternated with laxatives, or even with purgatives when the bowels are confined.

It is quite true that, in many cases of maniacal disorder, febrile and non-febrile, and particularly in delirium tremens, opium often fails altogether to procure sleep in the early stages of the disease. In such cases it is with me a fixed and absolute rule of practice that the limit of safety is reached when the pupils have become contracted ; and if the pupils are contracted from the first, I hold opium to be contra-indicated altogether. Within this limit, however, I am inclined to think that the administration of opium, especially in the liquid form, is in delirium tremens sometimes beneficial, and at all events safe. In obstinate cases, therefore, I am still not disinclined to employ it. I have never continuously used any of the other narcotics, but there are some cases in which I should think belladonna might be worthy of attention, regulated on the same principle of looking to the pupils as the test of its physiological action.

5. Chloroform, according to my experience, is a useful adjunct to opium ; yet I almost hesitate to say a word in its favour, on account of the alarming results I have witnessed, and even the fatal consequences of which I have been informed, as occasionally due to its

use by inhalation in delirium tremens. In one case, which I saw some years ago along with Dr. Gordon, the inhalation of chloroform from a handkerchief, in the usual way, produced, on three different occasions, epileptic convulsions so extreme, and attended with so much lividity and suppression of the pulse, that we were obliged to desist from its further administration; the patient ultimately recovering without further remedies. In other instances it has acted without any unpleasant symptoms, but has been followed by a temporary good effect only, the delirium and maniacal excitement recurring as soon as the anæsthesia was intermitted. Hence it occurred to me several years ago to attempt to combine the action of chloroform with that of opium in some of the more obstinate and protracted cases of delirium tremens, by placing the patient under chloroform, after having induced contraction of the pupils by opium cautiously used, as above mentioned; with the view of securing thereby a period of absolute repose, so as to promote the hypnotic effect of the opium on the system. In two instances in which this method was employed, it appeared to me very successful; in a third the epileptic coma before mentioned put a stop to the attempt. On the whole, I believe that narcotics are safe in delirium tremens only when administered with the comparatively limited object of aiding and seconding the natural cure of the disease; and, therefore, they should be employed with caution, in moderate doses, and only in the later stages. So employed, I have often seen them apparently useful; but often, also, they have failed;

so that I can hardly venture to say more of this class of remedies than that renewed trials seem to be still demanded, to enable us to distinguish clearly between their use and their abuse ; between their employment as part of a *heroic* method, and their more restricted use, according to the principles here laid down, and in subordination to the natural cure by diet and regimen, the sufficiency of which, in most instances, must be considered as fully established by experience.

As a further illustration of the safety in most cases of what may be called the *natural method*, and also of some of the difficulties of diagnosis which occasionally occur in the acute nervous affections of intemperate habits, I will give an extract from the clinical lectures of the past winter (1861-62), embracing a notice of the entire number of cases presented about the period of the New Year.

Lecture, 10*th January* 1862.*—Delirium tremens, a disease of which we have had several examples lately, is characterised, in typical instances, by marked muscular tremor, and by delirium of varying character, but chiefly by a *busy* delirium, or constant fidgety habit of occupation with imaginary business (see the cases of W. N—— and G. S——, *ante*, pp. 225, 227); also by spectral illusions both of sight and hearing, and in bad cases by physical weakness and prostration, the stomach being in very bad order, not often exactly sick, but wholly without relish for food, or the power of assimilating it. The relish even for the habitual stimulant is not unfre-

* Revised from the Report of Mr. J. Thomson Welsh.

quently lost, and there is but little thirst; this marked insensibility to internal conditions is combined with extreme irritability of the external nervous system, and the latter condition is commonly the measure of the former, the external restlessness appearing to exhaust the nervous force, and to prevent its expenditure within. [*Versâ vice*, if the internal susceptibilities can be roused by proper and adequate stimuli, the external irritability will cease; and this is the key, I believe, to almost the entire treatment, philosophically considered.] The tongue is commonly white, coated, sometimes very foul, but commonly not dry as in the delirium of typhus; there is often a mawkish and very disagreeable odour of the breath, indescribable but easily to be recognized, due to the remains of food and alcoholic liquors lingering in the tract of the stomach and intestines. [This is the indication for purgatives, as mentioned above.] The cases which fall to be treated more or less under this title, are not all pure or typical cases, but you will appreciate their general character from these brief remarks, and those which follow.

Case of "the horrors."

——— æt. 37; a most characteristic case of *incipient* delirium tremens, commonly called the horrors of drink. He is a clerk in a mercantile house. He was drinking hard after New Year's Day, and was admitted late in the night of 8th January (two days ago), very much the worse of drink. He shivered intensely all night till the next forenoon. When I saw him, the surface was warm, almost feverish, and sweating, but the trembling con-

tinued as much as ever. He had taken no nourishment, and to get him to take even beef tea, we had to put a little brandy in it. The tremor was gone to-day, and he has even had some sleep ; but I have no doubt this man was on the very verge of true delirium tremens, and a little more excitement and want of food would have brought it on in a well-marked form. It is a most characteristic example of the mode in which the disease begins, and as we seldom see our cases so early, I wish you to note these points in the history. He began at the New Year season, as I told you, and went on for some time drinking hard ; not hard, however, just before he came in, for he has told us himself that his stomach had ceased to receive drink, and he vomited it. Now, *had he not come in*, but continued making occasional ineffectual efforts to keep up the debauch, I think it is quite plain what would have happened. He would not have eaten, he would not have slept, he would have ceased even to drink ; he would have passed in these circumstances into ordinary delirium tremens, *and it would have been ascribed to his having stopped drinking.* The truth is exactly the other way. *A popular error.* The disease does not depend on the ceasing to drink, but the ceasing to drink depends on the commencement of the disease. This fact, or rather this order of events, I have verified in numerous instances, and I am anxious that you should understand it clearly, for it lies at the root of the pathology of the disease, and is constantly mis-stated in medical accounts of it. The treatment was simply nourishment and protection.

Opium in this case would probably have done no good, perhaps harm. Nor have I given him any allowance of stimulants, only the single dose I mentioned. [The sequel of this case follows at p. 281.]

Wm. O., æt. 39, a hopeful subject altogether, and a "seasoned vessel," but hardly a case of true delirium tremens. He has a gonorrhœa and a Hunterian chancre, but no distinct bubo. He has been a habitual drunkard. Began drinking hard at the New Year, or before it, a week before admission, and was in a state of considerable nervous prostration, but without tremor or delirium.

J. K., æt. 31, admitted January 6th, had been drinking only two days. He had no tremor or spectral illusions; there was in his case simple sleeplessness, but no mania. There was no active treatment required in this case. I gave him 3ss. of solution of morphia as an experiment, but it had little or no effect; and I did not care to give him more, as I was sure of the favourable result. This man is a pretty well-known inmate of Ward No. 10; he has been there four or five times before.

Colin M., æt. 30, a highlander naturalized in London, where he has imbibed a taste for London Stout. Admitted January 7th. He says this has been his drink, not whisky. He is not, according to his own account, at all a habitual drunkard, but in coming down in the train from London to Edinburgh, on the night before admission, he got drunk with some companions, and was drunk even when admitted. He was very shaky the day after admission, and had the horrors, but no delirium.

Peter H., æt. 33, admitted 8th January, at the end of a debauch of two days. He has been badly off, and with his dissipated habits has been half-starved lately. He has taken his food pretty well since admission, and we can be quite sure that he will have good sleep if we let him alone. He is therefore to have no active treatment.

Robert M., a habitual drunkard, brought into hospital in a state of great excitement on 6th January. He had no sleep at all the first night after admission ; but nevertheless he is now nearly well. He has had no medicine at all, and no whisky ; nothing but nourishment and careful attendance. [Further remarks on this case follow at p. 284.]

Susan W., admitted just before New Year's Day (31st December), with well-marked delirium tremens, having all the symptoms—delirium, tremor, spectral illusions, etc. No active treatment. Now nearly well.

Christina G., æt. 30, admitted 4th January. This is not a case of delirium tremens, but must be mentioned in this connection. Last June she had a real attack of delirium tremens, and became thereafter a tee-totaller. Unfortunately she took to morphia instead of drink, and she tells us that she was in the habit of consuming about sixpence-worth daily. She felt moved to increase her dose about New Year's time, and got sent here to avert the consequences. She has had no morphia since she came here ; she felt the want of it at first very much, but is now improving.

William A., æt. 42, admitted 7th January. This

is a very interesting case, and a somewhat doubtful diagnosis at present. I have taken some pains to ascertain the particulars of his habits, and of the history of the attack, from his wife as well as from himself. He was a habitual drinker on Saturday nights, but seldom got absolutely drunk. During the week before the New Year he took about a pint of ale or a glass of whisky every night; and we may easily suppose he did not quite abstain at other times, or after the New Year in particular. Still, his wife believes that his present state is not owing to drink; and he himself connects it in some measure with the calamity of the fallen house in the High Street, on 24th November last, which he says affected his mind deeply at the time. He is quite sensible that his mind is wrong. He has no tremors, and no spectral illusions. He is quite happy and cheerful, as is often the case in delirium tremens; but he has not, I think, the special appearance and manner of delirium tremens; not the restless pre-occupation of mind, nor the unconsciousness of internal wants, nor even of his own condition, which is usual in that disease. His case rather resembles acute mania when subsiding. Here is another point in favour of this diagnosis. He was quite a furious maniac when admitted; and since this, though he has not had good sleep, he has had some sleep; at all events he has become much less violent. Now in delirium tremens whenever there is a marked crisis, or when there is sleep, the disease is virtually at an end; but I have doubts about this case; he is still, I think, struggling with the disease. From his wife I learn

that he was never a strong-headed man; but was nervous, and apt to be *put out*, even before his positive illness. We cannot, perhaps, depend absolutely upon all these statements of fact, and therefore I only tell you my suspicions. In a few days we shall see what is the real character of the disease. If it has depended upon drink only or chiefly, it will subside at once. If, as I suppose, it is merely connected with drink accidentally, or perhaps excited by it in a predisposed subject, we have not yet seen the end of the matter. [Further remarks on this case at p. 283.]

[Here follow remarks on treatment in accordance with the preceding part of this paper.]

Lecture, 17*th January*.—The cases in Ward No. 10 have all come to a favourable termination, with two exceptions, about which I will tell you immediately. But first let me mention the issue of the case of ——— (p. 276). It looked like delirium tremens, and I have no doubt that it was incipient delirium tremens, but was arrested in time. From a little cough, and the tendency to feverishness, and the great depression of spirits, I suspected some other bodily affection also, possibly pneumonia; and with this suspicion on my mind, I very carefully examined his chest [though I forgot to mention this at the last lecture] without finding anything wrong. Next day he was a great deal better, and the next again [the day after the lecture] he wanted to be let out, and to return to his business, which was one of some responsibility. His case is a very pitiable one,

but I could not help him out of his lamentable scrape. His employers had threatened to dismiss him from a good position, one requiring a trustworthy person in point of character, unless he could produce proof that his illness had not been due to his own misconduct; and he came to me to beseech me to give him this attestation, by certifying his disease as being *anything* but delirium tremens. He fixed at once on my suspicion of bodily disease, and tried to make something out of that. He also entered into a long explanation, to the effect that drink was not the real cause of his complaint. I will not trouble you with this, as it was only the natural self-delusion of a guilty and remorseful man. I had the greatest possible commiseration for him, but what could I do? I told him the best thing I could say for him was to say nothing at all; and he went away very much dejected, apparently with ruin staring him in the face. These things happen only too often, and I need not tell you how much they try the firmness, and at the same time the temper and discretion of a physician. It was very hard to refuse this certificate, but you must not allow yourself to equivocate, even for a humane purpose. On the other hand, you might even have been doubtful of the diagnosis in a case like this. It is plain that the disease was modified by the mental condition here. It was the fear of losing his situation—the agony of remorse, as well as the effect of the drink—that caused the intense agitation the first day we saw him.

Now, in general, I will remark that almost all our cases in No. 10 at this season have been more or less

connected with intemperance, even if they were not regular and complete delirium tremens. The rule of treatment, as I told you, has been to employ no drugs, but to trust largely to nursing and nourishment. All the cases of delirium tremens have recovered ; few of them, however, were very severe cases. Two cases, as I told you, remain on hand uncured of those I mentioned formerly ; and both of these are instructive.

Wm. A. (p. 279) has been sent to Morningside Asylum. From the first, I doubted if the symptoms in this man were really due to intemperance, though no doubt he had been taking a good deal of drink. [It is difficult to know, sometimes, as the last case illustrates, if statements made on this point are consistent with truth ; and, generally speaking, an allowance must be made for wilful or unconscious error ; but a *moral* diagnosis is not less a part of the duty of the physician than a *physical* diagnosis, and the disentangling of truth from falsehood is not to be done by a mere general scepticism as to statements made in such circumstances. Such indiscriminate scepticism, in fact, defeats its own end, for I am sure that people, who believe everything told them to be a lie, hear in reality many more lies than those that throw themselves frankly upon the remnant of good that exists even in the most degraded human nature. In this case I had the conviction that I was told substantially the truth ; though both the patient and his wife were very coarse, and his language, when in the maniacal paroxysm, was frightful in its obscenity and general vulgarity.] I found that he had been *flighty*

and nervous for some time—at the least ever since the falling of the house in the High Street. It is probable that drink taken about the time of the New Year acted here on a mental organization strongly predisposed to mania. He was raving mad when admitted, and required restraint; afterwards he became somewhat better, but still I had doubts about him, which I expressed to you at last lecture, and which I should not have had if it had been pure delirium tremens. I told you a few days would decide; and a very short time did decide the question, for in twenty-four hours he was quite mad again, in fact worse than ever; we had to put him into one of the padded rooms, and there he lay shouting, and swearing, and roaring all sorts of filthiness without the slightest coherence, as many of you heard. I lingered a little upon it, but from that moment it was plain that he would have to be sent to Morningside.

The other case of delayed cure is also connected with drink, but although the man was very excited on admission, indeed quite frantic from drink, I doubt if it can properly be called delirium tremens. Since he has sobered down it has presented none of the characters of this disease; but on the other hand it is very evidently a case of what is now often called *dipsomania.*

A case of "Moral Insanity" or "Dipsomania."

Remark the particulars, for the case is a type of many others. This man came in mad with whisky, and yet clamouring for whisky; absolutely maniacal in fact, but I suppose merely from the immediate effects of drink. By and by he sobered down, and being told

most absolutely that he was to have no whisky at all, he reconciled himself to what he thought was simply a necessity of the case. In the course of conversation with him about this matter, I thought I detected him in various palpable untruths, and indeed it very soon became apparent to me that he was one of those unfortunate persons who hardly know whether they are uttering truth or falsehood when they make a strong assertion. There was a shamelessness and regardlessness of consequences, and even of decency about his whole manner, that convinced me I had to deal with a very low type indeed of human nature in this case. He had not the slightest sense of regret or of remorse, but wanted always to take me into his confidence, and explain to me how much he needed to have some more whisky. The result of this unsought confidence was, that I learned his antecedents so far—he had been drinking till the money was done, and till he was quite out at elbows ; and then he went and drank at the expense of anybody and everybody who would give him whisky, till he landed himself in the Infirmary. He had not been in the ward two days, moreover, before he developed a new phase of degradation, for the attendant caught him masturbating. He did not deny the fact to me, but said it was only once—that he had never done it before, etc., etc. All this, however, he said apparently with the most perfect indifference as to whether I believed it or not, and I could not but tell him very plainly that I did not believe a word of it. This he received with the same cool indifference as the former charge of habitual masturbation. He is thin, emaciated,

withered-looking, without colour, yet without apparent bodily disease or deformity. There is in all this evidence of long-continued and probably irreclaimable bad habits, founded on a weakening of the moral sense amounting to a kind of *paralysis* of it. You cannot get hold of anything on which to act in the way of making this man ashamed of himself. His aunt, whom I sent for, and who speaks of him, on the whole, with wonderful charity and good temper, as well as good sense, says, without the least reserve, that it was always so. He has *never* been able to do anything for himself, or to turn his mind to good in any shape. He *never* would work, and drank at all times whenever he could get the drink. In fact she says he was always "a perfect gowk"*—that is the climax of her description; and I hold that it is both a charitable and a true description—the more so that it is perfectly simple and natural, having no relation to any ulterior object whatever, for she has plainly none in view. Our patient has had sense enough for the most part, she says, to keep out of the way of the police, and that is about the utmost that can be said for him; but even that cuts two ways, for possibly, if he had been more clever or ingenious, he might have been led more easily into positive crime. As it is, we have pretty clear proof that he is—1. An utterly abandoned, and almost unconscious liar. 2. An almost equally shameless masturbator. 3. A drunkard, quite devoid of self-control, or even of the desire to control himself. 4. A *lazy* and an *incapable*, of the most incorrigible description. 5. "A per-

* "*Gowk*, a fool, a simpleton."—*Jamieson's Dictionary.*

fect gowk"—or, to use another most expressive Scotch phrase—a *ne'er-do-weel*,* *i.e.*, one who not only *does* not do well, but apparently *cannot* do well; who has neither the capacity nor the desire to do well. It is a case not only of degradation, but of positive *degeneration* of the moral instincts; and the degeneration is probably both physical and moral by this time; the machinery of mind has suffered as well as the mind itself. This man, I believe, literally *cannot* do good at present; you can no more expect good conduct and high principle from such an organization than you can from that of a gorilla. A long course of reformatory discipline might possibly indeed, even now, do something to reverse the habits of forty years; but at this moment of time the man is in a state of *moral paralysis*—powerless for good, and a prey to evil, in virtue of his physical and moral organization—his craving appetites and deeply imprinted bad habits. There may have been also a congenital fault or deficiency; but about this we know, and can know, nothing with any certainty.

What is to be done?

What can you do with such cases? You often meet with them in various degrees of urgency in the higher ranks, and then they are especially puzzling. Positions of very high responsibility have sometimes to be filled after a fashion, and in the eye of the world, by such persons; for example, it is quite easy to suppose that the peerage might devolve its

* "*Ne'er-do-weel—ne'er do good*, one whose conduct is so bad as to give reason to think that he will *never do well.*" (As an adjective)—"Past mending."—*Jamieson's Dictionary.*

honours upon the head of such a being as this, or a princely fortune might fall to be spent by him without control; which would be truly, and in the largest sense of the word, a *mis*-fortune, inasmuch as it would merely give free scope to all his base, revolting, and ruinous propensities. Then rises the question of moral responsibility in the eye of the law, or of technical *insanity*—a difficult one to settle, I need not tell you, from the legal point of view, especially when money is plentiful. Such men are not *cretins* or *idiots*, and yet there is something in them plainly deficient as compared with a sound organization. There is a certain loss of self-control, which is not a mere vice, but has become *stereotyped*, as it were, and stamped down upon the habit as an infirmity extending over the whole moral organization. But does it constitute insanity? The legal view of the case is extremely involved, and we have no occasion to discuss it at present, but as a practical question of medical treatment I would put it thus: What can you make of him? Is there anything you can act upon through the ordinary forces of moral discipline, and with reasonable hope of a good result within a moderate time? Is there any *moral leverage*, so to speak, by which you can move the sluggishness, the low tone of this man's whole moral nature? If so, use it by all means; but if not, or if you fail utterly after trial, accept the alternative. I consider this a really *diseased mind*, in a practical sense, as regards the medical and moral question of cure. It is a mind plainly requiring to be under control and coercion; you can make nothing of it otherwise. As to the tech-

nical question of insanity, as affecting legal rights and responsibilities, I would not allow it to be too much mixed up with the other, but leave it to be settled practically also, according to the nature of the interests involved. I don't care in the least about the mere *word* insanity, and I confess I think it quite unnecessary to look too closely into the metaphysics of the matter. It is to me a practical question altogether; in one case, a question of medical treatment; in another, of law and of substantial justice (though sometimes rather rough and ready justice) to the individual and to society.

In this man's case, I should certainly be disposed to recommend, as a matter of treatment, his being put under a certain amount of personal restraint, with due moral and medical discipline; and this probably for a long time. I entirely believe that this man cannot possibly be made a useful member of society, or even otherwise than a nuisance, without such discipline. But I feel the want in these cases of proper support from public opinion, and from the law. I *dare* not certify even this wretched being as insane, without more obvious and striking reasons to carry conviction to every one's mind, as well as my own, than I have at present; so he must be left to cumber the ground. If I could even force him into the workhouse, it would be a point gained; but I cannot *force* him at all; we must simply let him go his way. Better men than he have committed murder or suicide in the like circumstances; but I don't see clearly anything of this kind impending in his case; I think he is too great a coward, and too utterly inert,

to set about doing either the one or the other; and therein, perhaps, lies his immediate safety. But the existing state of the law, and of public opinion, is very unsatisfactory in regard to these cases. As *prevention* is better than cure, I think that society, and the medical man as the organ of society, ought to have a much greater control than exists at present over cases like this.

[The last part of this lecture has been considerably extended, as compared with the form in which it was actually delivered; but the facts and arguments were in all essential particulars exactly as here set down. The great amount of public consideration recently given to this subject appeared to me to demand that strict consideration should be given to the language in which even an isolated case is reported and commented on. The legal difficulties connected with the restraint of the so-called "dipsomaniac," are very ably discussed in an article (published since this lecture was delivered) entitled "Inveterate Drunkenness not Insanity,"* in which it is not difficult to recognize the hand of a friend who has long given much attention to the subject in all its bearings, and who is well acquainted with all that has been written and thought about it, abroad as well as at home. Admitting, however, all that can be said upon the legal aspect of the question, in respect to the danger of extending too much the conventional idea of "insanity," as applied to legal forms and precedents, the strictly practical, and to a great extent medical, question remains; which is, not—How we are to classify the dipsomaniac? but—What we are to do with him? This question, I think, must be kept before the public, medical and general, until it is answered much more completely than our conventional forms at present allow; and, therefore, without in the least anticipating conclusions, or even attempting to discuss the subject at large, I am content to submit, without any important modification, the purely clinical facts and reflections contained in this lecture.]

* *British and Foreign Medico-Chirurgical Review* for April 1861.

XIV.

PLEURITIC EFFUSION. DIAGNOSIS AND PROGNOSIS. QUESTION OF THORACENTESIS.

THE following cases are a selection from a considerable number that have occurred to me at different times illustrating the important, and of late years much debated, subject of pleuritic effusion and its proper treatment. I prefer submitting them very much after the clinical method, with the reflections actually suggested at the time of their occurrence, both because this is more in conformity with the plan of the present volume, and because it is really more instructive as regards the exigencies of medical practice than any formal survey of the whole argument could possibly be. The reader will therefore have the kindness to make allowance for the differences of style and manner presented by these cases, and to remember that several of them have been already published at different times, and that they are now assembled in one article solely with a view to give an unbiassed and true account of the general character of hospital experience in this disease. In the latter part of the article, I have attempted something like a critical estimate of the question of treatment, and have adapted what was formerly written to the latest information in my possession.

CASE I.—*Double acute hydrothorax or pleurisy (fibrinous dropsy?) rapidly improved under diuretics—Leading points of diagnosis—Question of ultimate prognosis—Progress of recovery indicated by the position of abdominal organs and of heart—Rapid cure, as estimated by symptoms; but persistence of some physical signs, and of a slight degree of breathlessness on exertion—Question of remedies versus paracentesis—Ultimate recovery, with downward dislocation of heart and liver; observation fifteen months after the acute attack.*

Bedside, Thursday, February 28th, 1861.—Here is a case of considerable urgency, just admitted (Wm. L——, aged thirty-one). I have sent him to bed that we may examine him together. He is a porter at the railway station at Galashiels, accustomed to carry heavy burdens up to a late period; but he has recently been off duty on two occasions. He states that he has lost flesh, and he is, in fact, a little spare-looking and emaciated in features; but you observe that his configuration is that of an extremely strong robust man; his complexion is good, and everything assures us that he is the subject of a somewhat acute disease.

He complains of cough and breathlessness, with a little expectoration. Breathlessness appears to be the leading symptom. His voice is slightly hoarse; there is some effort in the production of it. His pulse numbers 132, and his respirations 36 (make allowance for the fact that he has only recently gone to bed). The pulse is small and compressible; there is slight wheezing, and slightly laborious respiration, but not so laborious as you might expect, seeing how much it is quickened. There is no appearance of fever; the tongue is coated with a slight silvery fur. The chest is sore from a large blister applied before admission, which is still discharging. There can be little doubt that there is here some considerable lesion of the lungs or pleuræ: let us try to discover what it is.

You observe that I shew you here a dulness on percussion in

the left lateral region. It is very marked; in fact, the percussion is quite dull, and I think also it is somewhat dull in front, above the heart, even as high as the second rib. The percussion is questionable, too, in the right lateral region (we are under the disadvantage, however, of not having the left to compare it with); yes, I think it must be reported as decidedly dull, as high at least as the tranverse line of the nipple. The dulness shades off gradually, diminishing upwards. Dr. Bell will please

Physical signs of Pleuritic Effusion.

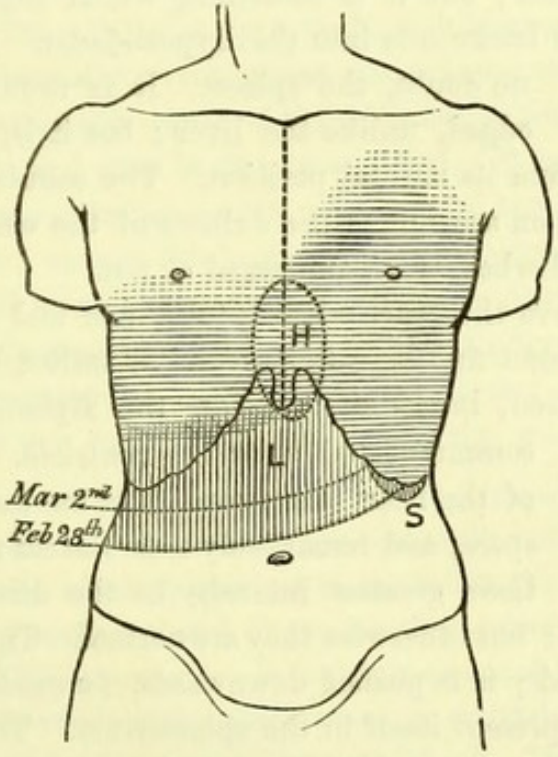

Fig. 1.

Front of thorax and abdomen in the case of Wm. L——, as sketched at the bedside, on Feb. 28th 1861. L, position of liver (the ascent of its edge in two days indicated by the dotted line). H, seat of heart's sounds and impulse in epigastrium. S, edge of spleen felt to the left of the liver. The transverse shading indicates the seat and measure of the pleural effusion.

note these facts in his sketch-book, for assuredly there is here something very interesting and unusual. Now observe what I shew you next: to me it almost fixes the character of the case.

Displacement of Liver,

The edge of the hepatic dulness, and also the free edge of the organ as felt by the hand, pass almost exactly through the umbilicus. You think the liver enlarged? No, it is not enlarged—only pushed down. Observe that there is no appearance of anterior prominence or fulness of the hypochonder; the whole abdomen is perfectly flat. The organ is, in fact, displaced downwards by an effusion in the right thorax. Now trace the hepatic edge up towards the left hypochonder. Just where you lose it, the finger begins to encounter a floating body, giving a sense of slightly increased resistance; but it is something which slips easily away from the touch backwards into the hypochonder. This body is,

and of Spleen.

no doubt, the spleen. It is round and blunt-edged, unlike the liver; but it is, as the liver is, displaced from its natural position. You cannot make it out by percussion, on account of the dulness of the whole left side; you cannot tell whether it is enlarged or not.

Now, observe the heart—listen to it, and feel for the apex-beat. You cannot find it in the natural situation, but there is a distinct pulsation, immediately below the xiphoid, apparently communicated from the ventricle. The sounds

Heart displaced downwards and forwards.

of the heart are heard over an unusually large space, and remarkably near the ear; they acquire their greatest intensity in the direction of the lower sternum; but otherwise they are normal. The heart, then, is also displaced; it is pushed downwards, forwards, and to the right, so as to present itself in the epigastrium. These facts are extremely characteristic of fluid in the pleuræ; effusion is present on both sides, but especially great on the left.

Next notice that the respiratory sounds are natural in both apices, with the exception of a little wheezing, and that they become gradually feebler in the direction of the dull percussion on both sides, without any marked râle. Now turn to the back, which we have not yet examined. There is dulness on percussion, absolute at the bases, and very decided from the fifth dorsal spine downwards. The respiratory murmur is correspondingly

diminished in the dull parts, with hardly any râle. The vocal thrill is also diminished on both sides all over the dull parts. All the facts, therefore, concur to verify the diagnosis which we have just given.

Now I wish to mention for your guidance the fact that double pleuritic effusion is rather rare, except in connection with general dropsy. Let us inquire into this. The patient states that he has never had swelling either of the feet or face ; and you observe that he has none now. He considered himself, in fact, a healthy man until quite lately. His urine is, if anything, scanty, but without albumen ; the specific gravity of the specimen we have here is 1032 ; there is, apparently, a deficiency in quantity, but nothing essentially wrong about the urinary secretion. We cannot, then, fathom the cause of this effusion, at least at present ; we must take it and treat it simply as a fact. It is probably not to a marked extent inflammatory ; at least he has so little pain and fever, that I cannot think of using any antiphlogistic remedies. We shall view it as a dropsy (perhaps a fibrinous, or half-inflammatory dropsy), and we will treat it with diuretic remedies, pretty actively pursued. He will have the cream of tartar electuary * (which you so often see me use, as the most serviceable

* The publication of this case in the *Lancet* produced several applications to me from England to know the composition of this electuary. It is a prescription which I have got so much into the habit of ordering as matter of routine, that I forgot, for the moment, its not being included in the Pharmacopœia. It is simply cream of tartar, mixed in nearly equal proportions with treacle, honey, or marmalade, and in some cases flavoured with a few drops of peppermint oil. The dose is a teaspoonful, repeated as often as the stomach will bear it, or as the urgency of the case demands. I know of no diuretic nearly equal to this in acute cases of effusion ; and also in some chronic cases, if there is not a spontaneous diarrhœa, and if the patient is not exhausted or sick, all of which circumstances interfere materially with its safe action. It is no objection to this medicine that it sometimes acts on the bowels ; in fact, if the kidneys are sluggish, and the bowels confined, I usually prepare the way for it by doses of gr. x. of compound jalap powder, frequently repeated for a day or two.

of diuretics), and in addition, he will have a squill and digitalis pill three times a day.

Bedside, March 1st.—He is better, evidently. The respiration is quiet; its slight laboriousness has disappeared; it numbers 28 in a minute. The pulse is still rapid, but less so, 110; it has, on careful observation, a slightly-marked double beat, the *pulsus dicrotus* of the ancients, often indicating the resolution of acute diseases. The patient has taken his medicine without sickness; he has no well-marked fever, nor flushing of face; there has been no sweating, even in the night. The slight fur is separating from the tongue, which is not in the least red, nor has it the enlarged papillæ of fever. But what is most conclusive as to the effect the remedies have taken is, that *the edge of the liver is half an inch higher than yesterday.* The dulness on percussion in the left lateral region appears also less, its edge, however, not being well defined.

Lecture, March 1st.—We must speak with caution and reserve as to the ultimate prognosis in Wm. L———, because the case is one of a rather unusual kind, and because double pleuritic effusion, when unconnected with general dropsy, as in this case, is apt to arise from some organic cause of even a more insuperable kind, such as a tumour in the posterior mediastinum, or malignant disease of the pleuræ themselves.* Nevertheless, everything looks well for the cure hitherto; the powerful diuretics have had a surprisingly good effect in a short time; the urine is increased in quantity, and all the urgent symptoms have been relieved. (Recapitulation of facts as above.) All the facts shew a formidable

* These casual remarks by no means exhaust the pathology of the subject. The question of prognosis in acute pleuritic effusion is discussed in other parts of this article.

amount of effusion, but they do not shew its cause. There is no ascertainable disease of the kidney, heart, or liver. Unless there be some serious organic affection hitherto undiscovered, all the facts tend to assure us that we are about to obtain a cure in this case.

Bedside, March 2d.—Continued improvement; *margin of liver at least an inch and a half above umbilicus* (see fig. 1, dotted line); dulness on percussion less; still slight pulsation of heart below xiphoid, and sounds unduly distinct towards sternum, with reduplication of second sound, but no murmur.

March 4th.—Patient still improves, especially as regards right side of thorax, the percussion of which is now almost natural in front, and even in lateral region only slightly dull, the respiratory murmur being quite full to about two inches below nipple. *Hepatic edge at least two inches above umbilicus.* In left lateral region there is still considerable dulness, with obscuration of respiratory murmur; indeed these signs appear even more distinct than they were two days ago; in left hypochonder an increased resistance corresponding to the seat of the spleen can still be obscurely recognized. * * * In right back the upper half of chest is quite clear to percussion, but there is considerable dulness in lower third; in left back there is more or less dulness up to the supra-spinous space.

March 12th.—Patient has been continuously improving as regards symptoms, and now walks about with very little feeling of oppression. Dulness on percussion in left lateral region much diminished; on right side almost gone, and above the left nipple there is now hardly any dulness. In both backs percussion continues dull; limits of dulness hardly to be defined, there being no distinct edge. In left back, however, it is almost absolutely dull below the seventh dorsal spine, and in right back below the ninth. Vocal thrill and respiratory murmur still very much diminished in both lower backs and in left lateral region. Limits of liver as last noted. Spleen not to be detected. Heart's impulse

still traceable (but barely traceable) below xiphoid. Patient is quite sure that he has had no pain in the spine or limbs, but

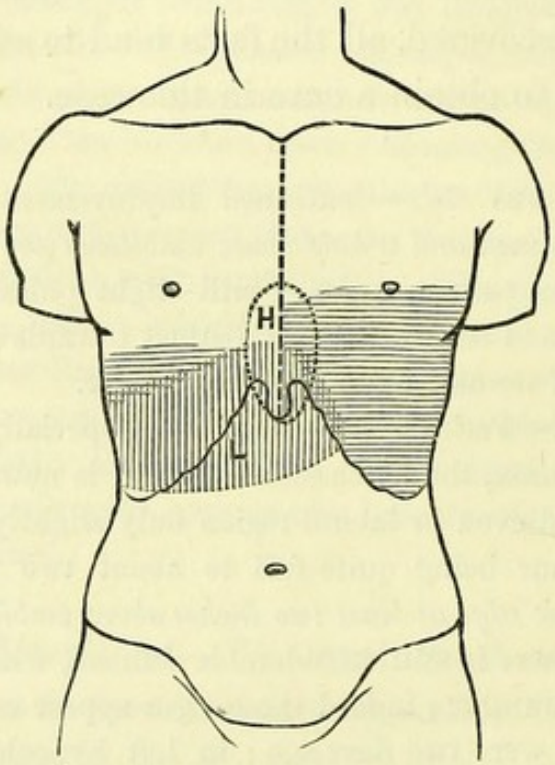

Fig. 2.

The state of Wm. L—— on March 13th 1861. The letters indicate the same facts as formerly; but the heart's impulse is somewhat raised out of the epigastrium, and the edge of the spleen has disappeared; the liver also is higher. The apparent enlargement of the left side to the eye is due to an inaccuracy in the diagram; it did not exist in nature.

admits a little pain in the left lumbar region and groin; admits also a feeling of numbness in his knees. Hardly any cough, and very little sputum; no œdema; no difficulty of swallowing; no vomiting; no affection of the voice."

Further Remarks, April 30th.—The pressure of other matters of interest prevented my alluding to this case again at lecture, and I will therefore state that he was dismissed on the 3d inst., feeling himself so well as

to go back to his work—" cured," in fact, in the conventional sense of the term. Nevertheless, I am not disposed to withdraw altogether the reserve expressed in the prognosis of March 1st. The series of negative facts at the end of the last report indicates both the suspicions we entertained of some tumour or form of organic disease, and the absence, also, of any positive basis for our suspicions. The effusion had almost disappeared upon the right side, except at the lower region of the back; on the left, however, resolution was very far from being complete, or even manifestly in rapid progress. Feeling anxious to know more about him, I wrote to the station-master, and had him sent in to town again two days ago. He is evidently better in appearance; complexion fresh, appetite and strength improved; has gained weight considerably; has no perceptible fever, and is doing his work without much difficulty, *although, on careful questioning, he admits that he is still breathless on exertion.* In the right lateral region there is no dulness on percussion, and the respiratory sounds are normal; in the left lateral it is still more or less dull at all points below the nipple; and in both backs there are dull percussion and diminished respiratory sounds over a space no whit less, perhaps even more, extensive than at last examination. In the left lateral region, and still more distinctly in the front, there is a well-marked shuffling friction-sound with respiration; a trace of friction also is audible on the same side near the spine. The heart's sounds are not so much as formerly heard just below the sternum, but they are still

very distinct there, and there is still a perceptible impulse in the epigastrium. The liver is not higher than at last report. These facts constitute a rather serious counterpoise to the favourable impressions derived from his general appearance, and the absence of any positive signs of permanent organic disease. The case is still under observation, and must probably continue for some time to be viewed with anxiety as regards the ultimate result. The bulk of the pleuritic effusion is, no doubt, removed, but the state of the parts left behind is by no means satisfactory.

It is probably not desirable, in the absence of increasing effusion and of fever, to pursue any active treatment; but in the event of the complete cure being delayed, blistering or the inunction of iodine may be advisable. It is to be remarked that in this case M. Trousseau would probably at once have performed paracentesis thoracis, upon the principles advocated by him in some of his earlier clinical lectures on the subject. I have pretty fully expressed [in the latter part of this article] the view I have been led to take of the expediency of this operation in cases of acute effusion, and will only remark here, that in all probability nothing could have been done by paracentesis in this case up to the present date that was not done by remedies with more than equal safety, and nearly equal rapidity. Should evidence of larger effusion in the left pleura at any future time be present, it may possibly still become a question, after a renewed trial of remedies, if an operation should not be attempted; but he is at

present so well that no one, I think, would be in a hurry to interfere.

Concluding Remarks, May 1st, 1862.—After almost exactly a year's interval since the last report, Wm. L—— has presented himself to-day, not from any sense of his requiring further medical advice, but to fulfil a promise he had made to me to keep me informed of his progress. He considers himself quite well, and able for duty; he also looks well, and has the same fresh colour that he has all along presented, although, critically speaking, I should call him thin, or at least spare, as to flesh. He is about to change his situation for one that he considers more wholesome, though equally laborious. The only uneasiness of which he is ever sensible is referred to the hypochonders and upper part of the abdomen, and he describes this as a *tightness* rather than a pain. I have little doubt that it is connected, partly with the remains of exudation in the left pleura, and partly with the permanent displacement of some of the organs presently to be described.

Physical Examination in the Erect Posture.—The respiratory movements appear free on both sides of the chest; indeed, no difference can be observed between the two sides. In the epigastrium (fig. 3, p. 302) there is a distinct visible movement at each pulsation of the heart, not very defined in its limits, and this corresponds with a pretty distinct impulse to the fingers pressed into the left side of the epigastrium, or over the xiphoid. The percussion-dulness of the heart is low; it can hardly be traced to more than half an inch above the nipple at any point; it is also rather displaced to the right, and transcends the mesial line

by about an inch. It appears to measure rather more than four inches across. The position of the left apex-beat is not so much

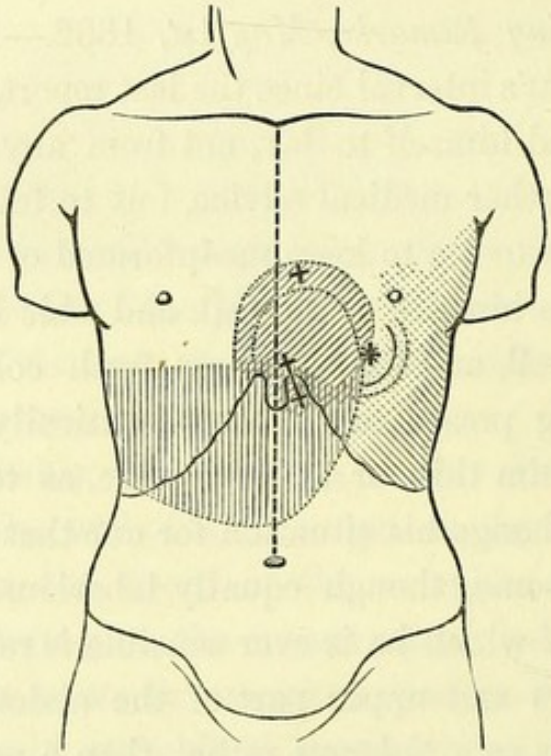

Fig. 3.

The state of Wm. L—— on May 1st 1862. The shading with ill-defined edge in left lateral region indicates the remains of pleural exudation, nowhere amounting to a bulk of fluid. The cardiac and hepatic percussion-dulness are shewn by the vertical and oblique shading merging into each other at the epigastrium. The inner dotted line in the cardiac space shews the restricted dulness in the recumbent posture.

* is the site of the apex-beat, or at least the point of contact of left ventricle with the wall, the two circles beyond this mark shewing the limits of diffusion of the impulse.

‡ is the position of impulse of the right ventricle, as described in the text, and of the corresponding sounds.

\+ is the site of the tactile sensation corresponding with the (pulmonic?) second sound, as also of the greatest distinctness of the sound itself.

displaced as that of the percussion-dulness ; it seems, however, to be fully two inches below the nipple, and a feebler impulse can be traced still farther downwards and outwards. The movement in the epigastrium is quite distinct from this impulse, and must

be referred to the right ventricle. The sounds of the heart are loud over the whole region of the percussion-dulness. The first sound is peculiarly distinct at the site of the apex-beat, and in the epigastrium ; in the latter situation it is prolonged and dull, as well as loud, but in both situations without murmur. The second sound is loudest on a line with the left nipple, three inches from it to the right, and three-quarters of an inch to the left of the middle line. There is no murmur with the second sound, and though decidedly louder than usual, it has no altered character.

The hepatic percussion-dulness is, like the cardiac, low in situation ; its upper edge in front is three inches and a half below the nipple ; its lower edge only an inch and a half from the umbilicus. There is, however, not the slightest protrusion of the hypochonder, or increased resistance to pressure. The vertical measurement of the hepatic dulness is four and a half inches in the lateral region, and five and a half inches between the right nipple and the mesial line.

In the left lateral region, and still more in the left back, there is percussion-dulness rather diffused. (In the right back the percussion is strictly normal). The splenic dulness cannot be defined. The respiratory murmur is obscure in these parts of the left side, but it is not absent anywhere.

In the recumbent posture. The limits of the hepatic dulness are unchanged ; but the cardiac dulness diminishes both in extent and intensity (see fig. 3), and the movement in the epigastrium becomes imperceptible. The position of the apex-beat does not vary, and the only change in the sounds of the heart is that they are less loud than in the erect posture.

The result of this examination is rather curious and interesting, for it proves that even when the apparent cure of acute pleurisy is most nearly complete, and when it has been practically uninterrupted from the moment the disease has been fairly brought under treat-

ment, there may still be left behind organic changes which may be virtually permanent, and yet may interfere very little with the health or even comfort of a laborious life. I am inclined, indeed, to believe that the very active habits of this man, and the necessity for much exertion in the erect posture during the period of the recovery, are the chief reasons of some of these changes, and particularly of the gravitation downwards and forwards of the heart and liver, which are so clearly displayed in the diagram; and which, in the case of the heart, the effect of posture in modifying the phenomena shews to be caused by relaxation of the mediastinal attachments of the organ. It is interesting to observe, and I think amounts to a conclusive proof of the absence of any considerable effusion in the left pleura, that the apex of the displaced heart is not in the very least to the right of its normal position. If there be, therefore, as is likely, some remains of imperfectly organized exudation in the left pleura, it must in all probability be merely a thickening of the parts, with a corresponding resistance to the complete expansion of a portion of the left lung. These changes are no doubt important, if not as affecting life, at all events as a lesson in diagnosis; for in the absence of the well-defined history above given, they might be rather puzzling. The extent of the heart's dulness and impulse, and the character of the sounds, even now lead to a reasonable suspicion that hypertrophy may be impending, if not begun; and I cannot but hold that the cautious prognosis of March 1, 1861 is even now not within justifi-

cation,* although the suspicion of malignant disease, at least, can hardly be longer entertained. To the observations on treatment, as given above, I am also disposed to adhere; and in particular to the remark that at no period of the acute course of this disease could the operation of tapping the chest have been proposed, having due regard to the wonderfully rapid effect of the diuretics in removing the bulk of the effusion.

As a further illustration of the principles of the diagnosis and prognosis in the preceding case, and particularly as shewing the necessity of a cautious prognosis in cases of painless and non-febrile effusions, I insert the following striking case which occurred during the past winter session, and was made the subject of repeated commentary at the bedside, partially reproduced in the report:—

* Dr. Walshe's observations on "double or bilateral pleurisy" would have borne me out even in a stronger statement than this; for it appears that he has seen only four instances of this form of disease in persons quite free from the evidence of constitutional taint, and *these four cases were all fatal.* The pericardium was also apparently involved in all Dr. Walshe's cases; and the peritoneum in one of them. "There is obviously (he remarks) a proclivity to general serous inflammation." I have referred to a series of cases of this kind within my own experience, in the article on Influenza, at p. 101.

But the case of Wm. L——— is evidently different from all of these; and if it should really be found to have been uncomplicated at first, it will be a marked and very rare exception to Dr. Walshe's rule (which I believe to be substantially correct), that "primary pleurisy is essentially unilateral; while the implication of both pleuræ almost surely signifies the existence of diathetic mischief."

CASE II. — *Pleuritic effusion on left side, distinct according to physical signs, very insidious and latent according to symptoms—Suspicion of aneurism, or of malignant tumour of chest—Hæmoptysis to a slight extent—Partial recovery, with greatly diminished effusion—Soon after, abdominal pain, passage of blood by stool (no hæmorrhoids)—On re-admission, evidence of tumour of the greater omentum, and of pulmonary tubercle—Tubercular disease—"Clicking" râle.*

Bedside, November 13, 1861.—Peter B., æt. thirty-five, a brewer's servant, for some years in the establishment of Messrs. Younger and Company. He complains chiefly of difficulty of breathing and cough, with copious expectoration. The cough comes on chiefly at night, in paroxysms, and last night (12th), during and after a fit of coughing, he says that he expectorated about half a spittoon-full (about 6 oz.) of very tough phlegm, which, however, has been thrown out. What remains in the tin at present is chiefly frothy mucus. [As this is literally the whole substance of our patient's complaint to us, I may observe here that it exactly resembles an attack of somewhat asthmatic bronchitis, such as men of his class are subject to, and such as has been not uncommon recently in all classes of the community.]

First Appearances.

The patient is a very large-boned strongly built man ; the framework of his body is remarkably robust, but the muscles are not firm or large in proportion to his size. [I should infer from this that he has probably become emaciated since his illness.] His face is ruddy, especially for an emaciated man ; there is quite a remarkable capillary or small venous redness of cheek and forehead, and a tendency to acne, the papules of which have a kind of rough resemblance, at a distance, to the first traces of a small-pox eruption. The lips are high-coloured, but I cannot say that they are livid, as you might expect from the symptoms. The neck and upper part of the breast are pretty florid, the rest of the surface rather pale, but entirely without undue heat or

sweating. The pulse is 80 to 84; the respiration perfectly quiet at present. The tongue is well-coloured, and in every respect of natural appearance as regards its surface. [In short, there is no existing evidence of fever, hectic or other; the facial redness is a chronic condition, entirely distinct from the true febrile flush.]

[Now you have often heard me say at the bedside, that the question of questions in cases of chronic bronchitis, in all persons before the middle period of life, is—*Are they, or are they not, of tubercular constitution?* I do not pretend, as you know, to decide this at a glance, one way or other; but I may observe in passing, that you could hardly have, at this man's age, a stronger presumption *against* the idea of a tubercular constitution than you derive from the first slight examination into the external facts of the present case. He has been quite a finely developed man in every respect; the evidences of failure, so far as we see them, lie in the direction of the vascular system, not of the general nutrition. There is, indeed, muscular emaciation, probably of recent date; but it is not accompanied by acceleration of the pulse, nor by marked alteration, either of temperature, or of the complexion, as in the true phthisical emaciation. I merely throw out these as hints; keep them in view, but do not attach too much importance to them. They are fair and strong presumptions in favour of our patient's constitution. Now let us hear some of the further facts of the case from the patient himself.]

Question of Tubercle.

He says that his complaint began with a sharp attack of sweating, about a month ago, on a Sunday night, when he had neither been at work, nor indulging in alcoholic liquors (he admits that he takes a good deal of malt liquor, ordinarily, like most of his class). On Monday he was still ill, and was sensible that he had caught cold; from that time he has had a cough, and occasionally what he calls a "grewsin" of cold, *i.e.* chilliness, with tendency to shiver, but no decidedly feverish attack. He had this cold feeling even last night, but thinks it was owing to the cough in the night, which obliged him to sit up uncovered.

Details of History.

He is quite sure he has had no severely feverish turn, and no heat or flushing throughout the complaint. He is also quite sure that he has had no serious oppression at the chest at any time, at least none that disturbed him in the least degree when at rest; for although often obliged to rise at night, he is positive in stating that it was not to get breath, but to cough. I remark to him that he lies on the right side; but this appears to be chiefly from habit, or accident, as he says that he lay last night on the left side without any uneasiness. There is no orthopnœa, then, nor preternatural decubitus. Again, I remark that his voice is hoarse, and deep-pitched; but this, he thinks, has always been so; perhaps it may have been a little more remarkable since his illness, but not much. He never was a singer, so that we cannot test him in this way. He says that a mustard-blister was applied early in his illness to the front of the chest; what pain he had at that time was general over the front, and not either on the right or left side specially. Since then he has had absolutely no pain.

[Now I hope you are all satisfied as to the exactness with which we have ascertained these little points of detail; for the next observation I make is rather a startling one, in connection with some of them. This man, who has had the symptoms merely of an attack of bronchitis, with no local pain, no permanent dyspnœa, and next to no fever, *has nevertheless a very marked and unmistakeable local affection of the left lung;* a pleurisy or pneumonia, probably; at all events, something giving rise to extensive dull percussion in the left lateral region.] Observe that dulness on percussion is quite distinct, from a little above the level of the left nipple downwards; it becomes almost absolute below the level of the heart; behind, it extends from the line of the sixth dorsal vertebra downwards, being in great part absolute from this, and passing the whole way round to the nipple, without any intermediate clear space. Over the upper part of the left lung, on the other hand, both before and behind, percussion is eminently clear, and, perhaps, to a very slight extent

Evidences of Pleuritic Effusion.

tympanitic in quality.* On the right side the percussion is faultless, both above and below ; you may use this side, in fact, as a standard by which to gauge the other. It will help you to appreciate the nature of this percussion-dulness if you now auscultate. You observe that where there is no percussion sound there is also a deficiency of respiratory murmur, and the vocal thrill and resonance are likewise suppressed. It is extremely probable, therefore, that there is pleuritic effusion in considerable amount. But here is an observation which makes us certain that the fact is so ; that *not only there is pleuritic effusion, but it is displacing the heart.* The left margin of the cardiac dulness cannot be made out ; but the right margin is quite distinct, and is quite to the right of the usual position ; in fact, about half an inch to the right of the middle line throughout its whole extent. The upper level of the cardiac dulness is much the same as usual ; the heart is therefore simply displaced rightwards, not either raised or depressed. Observe, in connection with this, that no distinct apex-beat can be felt in the usual situation below the left nipple ; nor is there any distinct impulse in any intercostal space, nor yet in the epigastrium. The centre, both of motion and of sound, is (as nearly as we can place it) at the left margin of the sternum ; and it is probable that the heart is there in exceedingly close contact with the thoracic wall.

The abdomen is mostly natural, with one exception. The left hypochonder is unduly resistent on deep pressure ; you feel

* This tympanitic *quality* of percussion is, as Skoda has pointed out, almost constant in cases of pleuritic effusion at the base, not extending to the summit. It also occurs in some cases of pneumonia. I do not adopt, and do not stop to discuss, Skoda's theory of this peculiar quality of sound ; but the knowledge of it, as a clinical fact, is very important ; for there is no doubt that the earlier observers of it (Dr. Hudson and Dr. Graves of Dublin) were misled into the opinion that it was caused by air in the cavity of the pleura. The tympanitic tone referred to is not necessarily associated with very clear percussion as in this case ; it may coincide with considerable (though of course not complete) dulness ; for an instance of which, among others, see the case of Susan B., farther on.

this on comparing it with the right. We fail, however, to make out any distinct tumour, having form or edge; we also fail to discover any unusual dull percussion beyond the margin of the ribs. The liver is perhaps a little depressed towards the epigastrium [but, on the whole, *the effusion presses rightwards more than either downwards or upwards*]. It is, nevertheless, considerable enough to displace the heart, as we said. The diagnosis, therefore, is *a pleuritic effusion of considerable amount, occupying the lower zone of the chest on the left side; the lungs, in the main, healthy.* The general symptoms have been such that we can hardly call it an acute inflammation; at the same time it is not a dropsical effusion, otherwise we should have had it on both sides, or with other symptoms of general dropsy.

℞ Potassæ Acetatis ℥i.
Potassii Iodid. ℥i.
Syrup. Aurant. ℥i.
Aquæ ad Oi. Solve.

Sig. Two tablespoonfuls three times a day, or oftener.

19*th November.*—To-day the mixture was stopped, as it disagreed with the stomach.

Ordered—℞ Pulv. Jalap co. gr. x.
Sig. One powder three times a day.

24*th November.*—Powders discontinued, as they cannot be borne.

27*th November.*—Patient is to-day complaining of pain in the left side, which to his recollection he never has had before.

Blood in Sputum.

Sputum has also been present of a slight reddish colour for some days past, and has evidently contained a little blood—a mere tinging, but still quite unquestionable. On last examination, coloured sputum was noticed by Dr. Gairdner, but patient would not admit that it was blood. [I infer from this that the patient is unobservant of his own symptoms, and may have had this often enough before without noticing it.] Since then he has

been watching, and has generally noticed it coloured in the morning. No material change in the dulness of percussion since last report. Sounds of heart also much as formerly. The second sound cannot be said to be abnormal, but is certainly both deep-toned and rather loud about the manubrium sterni, especially to the right.

24*th December*.—Dismissed, relieved.

Re-admitted December 27, 1861.

Bedside, 10*th January*.—Says that he was sent out feeling quite well on last occasion, and that he has not since suffered from the chest, although he admits having a little cough and a slight mucous spit. Very soon after he went out, however, he had a discharge of blood from the bowels to the extent of fully a spoonful, of which he took no particular notice, having had something of the kind eleven years before, and having then quite recovered. Previously to the bleeding, had been suffering some pain in the belly, which became accompanied with swelling. When re-admitted, there was found in the abdomen a state of parts very much as at present. Abdomen rather tense and resisting all over, without being remarkably distended; the resistance, however, is greatest in the umbilical and right lateral regions, and to a less extent also in left part of umbilical region towards the hypochonder. The lower fourth of the abdominal wall is decidedly less resistant, especially on left side, and the epigastric region preserves nearly all its natural elasticity and softness. The hardness mentioned is at present quite painless, but was slightly tender on admission. Superficial percussion determines the stomach tympanitic in its natural situation, and the resistant part before mentioned dull; but by stronger percussion a tympanitic tone can be made out all over the resistant part, and a perfectly clear demarcation exists between it and the edge of the liver, which is as nearly as possible in its natural position, or a little higher. In the splenic region there is an increase of dull percussion, but without any marked marginal limit, and apparently

New Symptoms.

Tumour of Omentum.

only the remains of former lateral dulness. The left front of the thorax is universally a little duller than the right, the line of margin corresponding accurately with the manubrium. Towards the base of left lung posteriorly there is considerable dulness in lower fourth, and almost universally throughout left lung there is more or less of a slight clicking râle, which, however, reaches its maximum below the clavicle. Above the clavicle respiration is very tubular. In the right base behind there is also clicking (mucous) râle ; but only over the lower sixth. The rest of the right lung has a full and free respiratory murmur. Patient is considerably more emaciated than on last examination.

"*Clicking*" *Râle.*

23*d January.*—Patient is this morning found very much worse, livid in the face and extremities, which are also inclined to be cold—the hands very much so. Patient has also been delirious since last night, and has vomited a great deal. On inquiry, the nurse states that the vomiting has existed for three days—the other symptoms only since yesterday. The respiration is rather rapid—nearly 40 in a minute, and a little catching. Pulse very feeble, certainly not less than 120, but so soft that it can hardly be counted. Tongue nearly absolutely clean, but a little dry. Patient admits no suffering of any kind whatever, and even states that his pains have all disappeared. Has a little dry cough, otherwise nothing remarkable farther than stated. Bowels, he says, are regular, but they are in fact very loose, and three days ago there was some blood. The last stool is light-coloured and yellow. The tumor of the abdomen is much as before—perhaps a little harder. No trace of spots or any kind of eruption.

Patient died in the course of the afternoon.

I did not formally discuss the case of Peter B. at lecture during the first period of it ; but at the bedside very careful examinations were made into a number of minute details, under the theory that it was probably *not* a case of simple pleurisy, but an antecedent state of chronic

disease determining a remarkably insidious and painless form of effusion. The suspicions expressed at this time bore chiefly in the direction of aneurism or other thoracic tumour, and the absence of distinct hectic fever, or of a tubercular physiognomy, or of any characteristic expectoration, with the exception of a mere trace of blood, unaccompanied by pus, seemed to render tubercular disease less probable than other forms of organic chest affection. The direct evidences of aneurism, however, were wanting; and on lecturing on 29th November on a case of unquestionable aneurismal disease, resembling phthisis in some of its symptoms, I was led to allude to the case of Peter B. as follows:—"I am going to venture an extremely hazardous opinion just now, but it rises to my mind, and I must express it. I do not call it a *diagnosis;* it is merely a *guess,** and may turn out to be a good or a bad guess, according to circumstances. My guess, then, is that in the case of Peter B., which we have observed so often, and in which we can only find out positively a pleuritic effusion on the left side, there may probably be an aneurism; or, if not aneurism, some other form of thoracic tumour determining the pleurisy. Observe the little suspicious circumstances on which I proceed in giving you this view of what appears to be at first sight a simple case enough. It is a pleurisy of some kind, no doubt; but I think it is not a mere pleurisy, and, least of all, an acute pleurisy of the ordinary kind. You have had no pain, little fever,

* See the differences between a diagnosis and a guess, as remarked upon at p. 336.

hardly any dyspnœa. A pleurisy which sets in without any of these is open to suspicion at all times. Again, this is not mere hydrothorax, for in this case it would probably have been double. It is not connected with disease of the kidney, or of the heart, so far as we can discover. It is not, according to the appearances, tubercular; his physiognomy is rather that of vascular disease than of phthisis; there is a peculiar injection of the skin of the face, and he has been a very robust man up to this illness. His sputum contains a little blood, but in no other particular has it had the slightest resemblance to tubercular expectoration. Now, blood-tinged sputum which is not tubercular is, if persistent, more often connected with cardiac and vascular disease than with anything else; and, on very careful and critical examination yesterday, we found what I can hardly realize as abnormal facts, but what, as I told you, I do not think it safe to neglect. There is the slightest possible peculiarity—a deepening in tone—of the second sound over the aorta; the trachea is rather unusually deep in the neck, though not otherwise displaced; and I think we can almost trace a degree of undue resistance deep in the jugular fossa. It is very likely that this man is not cured; and I should not be surprised if he had an aneurism pressing on the root of the lung." [His hoarse voice, also, was in favour of this view].

On the second admission of Peter B., the pleurisy had in great part disappeared, and became replaced by the abdominal disease, as described. The effect of this and

other changes on the diagnosis at the time was thus expressed in the lecture of 24th January, the day after the patient's death :—"Peter B. was admitted with pleurisy. From certain circumstances observed and referred to in a previous lecture my mind ran on aneurism, and I mentioned it to you, but only as a guess. The pleuritic effusion disappeared, and he went out; but by no means well. He came back with a tumour in the abdomen, and I made, at the bedside, the diagnosis that it was in the omentum, but of what pathological nature it is difficult to be sure. Tubercle and cancer are the most frequent causes of omental tumour. Now, in this case, on examining the left lung, where the pleurisy was, we detected a fine clicking râle (I called it "clicking" râle provisionally, but I warned you that I had heard such a râle as the result of absorbed pleuritic effusion where I was convinced it was really extra-pulmonary) over almost the entire left lung. This combination of facts led to a revolution in my views of the case. I had first thought of aneurism, then of cancerous disease; and now, notwithstanding the first appearances, I am of opinion that the case is probably one of tubercular disease."

The *post-mortem* examination took place after this lecture, and the results, as recorded by Dr. Haldane, were as follows :—

Examination of Body, 24th January 1862.

Slight jaundiced tinge of surface.
Larynx, trachea, and other cervical organs natural.

Thorax.—Pericardium and heart healthy.

There were some old cellular adhesions on the right side of the chest. The substance of the right lung was, on the whole, healthy; but scattered through it were a few small groups of opaque miliary tubercles, none of which had undergone softening.

The left lung was generally adherent; but the adhesions were pretty readily separated, and were found to consist of tolerably recent lymph, in which were seen numerous opaque yellow tubercles. The substance of the lung contained scarcely any tubercle.

The bronchi in both lungs contained a good deal of gelatinous mucus, and their lining membrane was somewhat congested.

Abdomen.—The parietal was found adhering to the visceral peritoneum; the adhesions were separated without much difficulty; a few ounces of bloody serum were found in the non-obliterated portion of the sac.

The great omentum was converted into a thick cake of a greyish-yellow colour; it extended to the spleen, to which it was adherent; its greatest depth (from above downwards) was about four inches, while towards the left it tapered away, and where in contact with the spleen, it was not more than an inch and a half. The thickened omentum felt firm, and, on section, presented a somewhat mottled appearance, little opaque fatty masses being imbedded in a greyish, tough, translucent material.

The coils of intestine were adherent by moderately firm lymph, in which were small yellow opaque masses of tubercle. The coats of the intestine were entire, and their mucous surface was every where healthy.

The upper surface of the liver was adherent to the diaphragm by pretty firm lymph. The gastro-hepatic omentum was thickened, and coated with lymph. The liver, on section, was of a bright yellow colour, with a somewhat granular appearance, and softer than natural. On microscopic examination it was found to contain much fat; many of the hepatic cells seemed broken down, others contained numerous yellow granular masses, and there was also free biliary matter.

The gall-bladder was moderately distended with rather thick dark-coloured bile.

The spleen was healthy.

The right kidney contained two or three small masses of yellow tubercle, otherwise the renal tissue was natural.

The chief remark that falls to be made regarding the results of this examination is, that the direct evidences of tubercle in the left lung, as derived from the "clicking" râle, and the tubular respiration at the apex, were plainly untrustworthy, as it seems probable that the small amount of tubercle actually present in the substance of that lung had little or nothing to do with the physical signs, which must have been to a great extent the consequence of the absorbed effusion. I have so frequently observed this close resemblance of the extra-pulmonary to the intra-pulmonary râles under such circumstances,* that I fully anticipated this in my diagnosis, both at the bedside and in the pathological theatre; in fact, a curious and interesting point was raised at the *post-mortem* examination, as to the real character of this so-called clicking râle, the resemblance of which to the indistinctly mucous râle of some of the earlier stages of tubercle in the lung was very striking, and possibly ruled in part the nomenclature employed. The diagnosis, indeed, was founded on other facts, and was sufficiently clear and accurate for practical purposes.

* See the case of Mr. A. B. A. (Case VIII. of this article, p. 341), and the commentary at p. 349, for further remarks on this subject.

CASE III.—*Pleurisy on the left side, of two years' duration, in a healthy subject, tapped three times before admission (twice ineffectively)—Very large effusion, with extreme displacement of organs—A fourth and a fifth tapping, the latter followed by progressive amendment under diuretics.*

Bedside, 29th January 1862.—Colin S., æt. 22, discharged soldier, sent into the Infirmary by Dr. Figg of Bo'ness. Has been ill for two years. At the beginning of his illness had difficulty of breathing and pain on the left side. At that time he was serving in the army in India. Was on the sick list twelve months, and was seven or eight months at the hills, but did not get better. Was tapped twice in India, but only three or four ounces of fluid were got away. The difficulty of breathing gradually increased, and he was completely knocked up two months ago. Was sent home from India during last summer.

History of Symptoms.

Three weeks before admission, paracentesis thoracis was performed by Dr. Figg, and about three-fourths of a gallon of fluid were drawn off.

First effective Paracentesis.

Has no appearance of feverishness. Complexion good. A diffused resistance is felt in the left hypochonder, and at one part there is in addition a sensation as if the spleen came down on the hand with each inspiration. Complete dulness of percussion over the left front. Cardiac sounds heard most distinctly on the right side. Apex is about two inches below and rather external to the right nipple, and the second sound can be "felt"* distinctly above and somewhat internal to the same nipple. (Observation to be continued.)

Displacement of Spleen (?)

30th January 1862.—Left front remarkably full, as also the lateral region, but the front more than the lateral region; the intercostal spaces are scarcely distended in the lower part of the lateral region, while in the front they are more widely separated, and the sixth intercostal space, below and to the left side of the nipple,

Evidences of Pleural effusion.

* I have not hitherto been able to devise any modification of ordinary language to get over the awkwardness of this (with me) quite habitual

presents considerable tension. There is almost absolute dulness of the left front and of the lateral region down to the stomach, the tympanitic percussion of which, however, is quite distinct up to the level of three and a half inches below the nipple. Above

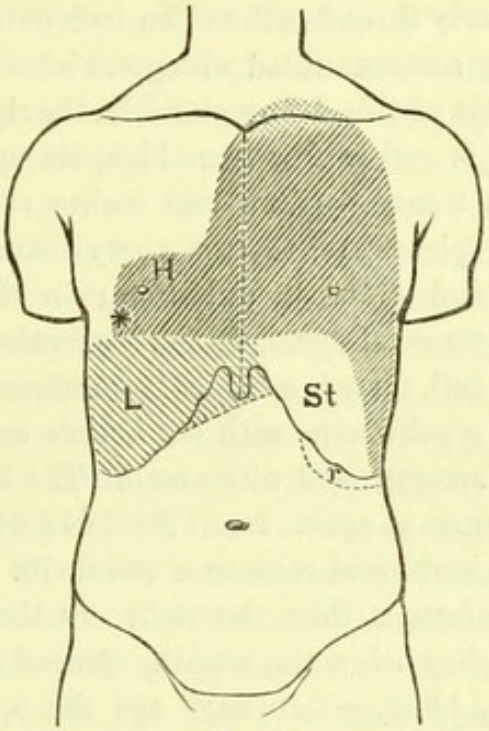

Fig. 4.

The front of thorax and abdomen in the case of Colin S., when the effusion in the left pleura was at its height, previous to paracentesis. The upper oblique shading represents the distended pleural sac, with the lung floated up to the summit.

H, the heart displaced to the left.

* Apex, or point of contact of left ventricle.

L, liver not much, if at all, displaced.

St, stomach, also little displaced.

r, undue resistance in left hypochondrium, probably from old thickening of pleura with adhesions.

the clavicle on the left side there is very faint remaining clearness, possibly tracheal. Dulness crosses the middle line in front to the extent of almost two inches. In right front the percussion is clear, with the exception of the inner part just mentioned, from

expression. The second sound is often, as every auscultator knows, coincident with a tactile sensation over the pulmonary artery, which is neither an impulse, nor yet a vibration in the sense of the "frémissement cataire."

the clavicle down to the third rib. Below this it is more or less dull (obviously owing to cardiac displacement). The heart is felt distinctly to pulsate in the third intercostal space, and also in the fifth; in the latter, nearly three-fourths of an inch outside the nipple—the sounds being communicated with great intensity and nearness through the whole of this dull region. In the right lateral region hepatic dulness is rather low than high, its upper margin corresponding to a transverse line two inches at least below the nipple. In the epigastrium there is a very distinct, though vague and diffused, pulsation; in the sixth left intercostal space, formerly mentioned as being too full, there is an indistinct, diffused impulse plainly synchronous with the heart's action, and hardly accompanied with sound. The left back dull, except a little clearness at apex. In all the dull parts there is greatly diminished or absent vocal resonance and thrill. The left side is greater in circumference than the right by three-fourths of an inch, at three inches below the nipple. (Saline diuretics given).

Displacement of Heart.

Pulsations of Heart communicated through the effusion.

5*th February* 1862.—Two days ago the operation of paracentesis was performed with Bowditch's instrument,* in the sixth left intercostal space, and eighty-eight ounces of fluid, considerably blood-tinged, but not to the eye containing pus, were drawn off. During the operation had a good deal of coughing, apparently determined by the rapid exhaustion of the chest, and owing to this no attempt was made to draw off the last portions procurable. Cough has continued somewhat, but otherwise he has felt much better since the operation. Pulse to-day is sixty-four, quite normal. Immediately after the operation the dulness was observed to have limited itself to the mesial line, and there was even some clear percussion towards the apex on left side. The heart's beat disappeared from the right side without reappearing

Second effective operation.

Modifications of physical signs by the operation.

* See further on, for a description of the peculiarities of this operation.

on the left. The clear percussion continues and the stethoscope gives a little respiratory murmur towards the apex. He says that after the last operation he heard for some weeks a noise in his chest like the rumbling of water in a cask, but this has not been present since the tapping of Monday.

March 10.—The continuous administration of diuretics having failed to prevent reaccumulation, and the chest being in much the same state as on admission, the breathing also being again much oppressed, and the patient anxious for relief, another operation was performed precisely in the same manner as the former, and with a like result as regards the fluid drawn off, the quantity, however, being somewhat less. Diuretics resumed.

April 28.—Not a single bad symptom since the operation. The patient has gained flesh considerably, is in good health and spirits to appearance, and lately took a walk up to the top of Arthur's Seat, 822 feet above the sea level, and a pretty steep ascent. [He went this walk, curiously enough, along with another patient in the ward, affected with a large chronic pleuritic effusion on the right side, from Bright's disease of the kidney.* Both patients are improving, the latter without operation, after a tediously protracted illness, and both are in much the same state as regards the effusion ; this comparative trial was made unknown to me, as I only discovered the fact after the departure of Colin S., who managed the whole ascent, but had to leave his companion behind at a point short of the summit, but I should think, from his own statement, not very far]. The left side is still very dull over the lower part, but the heart has very nearly returned to its place, and over the upper part, and the upper and inner part of the back, there is a good deal of percussion-tone. It is quite evident that considerable absorption of the effusion has taken place, and he is accordingly sent home, much relieved, and not without good prospect of recovery, though still to be under observation.

The satisfactory result of the case of Colin S., thus

* See p. 329, case of Christian M.

far, is certainly a most unequivocal testimony to the value of thoracentesis; for not only had remedies entirely failed up to the period of the different operations, but the relief from the last operation has been succeeded by considerable further diminution in the bulk of the effusion, and great improvement in the general health. It is further to be observed that this favourable change has taken place even after four operations followed by renewed effusion; the first two being, indeed, entirely useless for any practical purpose, apparently from imperfections in the mode of performance; while the third, though quite effectual for relief, seems to have been accompanied by the admission of air into the cavity.* I suspect also that in one or both of the two earliest operations there had been some bleeding into the sac which had permanently tinged the effusion; but it is peculiarly interesting and satisfactory to record, that under all these rather unfavourable circumstances the produce of the operation of March 10th was no more purulent than that of February 5th, while the improvement in all the symptoms tends still further to modify the fears that might be entertained of a slow progress towards empyema. I record these facts the more carefully, that I by no means participate in the opinions of Dr. Hamilton Roe† and others, who regard the admis-

* See the patient's statement under date Feb. 5, p. 321.

† *Med. Chirurg. Transactions*, 1844. Between 1833 and 1844 Dr. Roe operated in twenty-four cases. He says—"In every case which has fallen under my observation a considerable quantity of air entered the pleura during the operation, and in some of them so freely as to excite all the physical signs of pneumo-thorax, but in none of them did it pro-

sion of air into serous sacs full of fluid effusion as a matter of indifference. There exists, indeed, as this and other cases shew, a power in nature to repair this injury by the rapid absorption of the air, even while the fluid may remain for some time unabsorbed; but it by no means follows that air will always be quickly reabsorbed in such cases, and the experiments of Speiss* (referred to by Dr. Roe) on the rapid removal of air from the healthy thoracic cavity, prove absolutely nothing as regards the consequences of leaving it, even for a limited period, in contact with fluids on which it must necessarily act in the way of chemical decomposition. Not to anticipate, however, what follows as regards the operation of thoracentesis, I will only state here that I am fully persuaded both of its usefulness and of its safety, where internal remedies fail, and where effusion is considerable and increasing; while, on the other hand, I am of opinion that its safety as regards the ultimate result is partly dependant on the exclusion of air, and hence I shall always, hereafter, employ the method of suction by Dr. Bowditch's instrument, which has proved so satisfactory in this and other cases. The diagnosis

duce any permanently evil effect, a few hours being sufficient for its spontaneous removal: in one instance only did it cause even temporary inconvenience." This is certainly a very remarkable statement, and very difficult to reconcile with the observations of others, which shew that air may remain very long in the pleura in connection with fluid, and may lead to decomposition of the fluid effused, or at least to such modification of its characters as is highly unfavourable to recovery.

* "De vulneribus pectoris penetrantibus." I have not been able to find this work.

of the effusion in the case of Colin S. is made so evident by the ordinary physical signs, as represented in the report and the accompanying diagram (Fig. 4), that I will not dwell upon it further than to remark briefly upon—(1.) The position of the heart and of the mediastinum. (2.) The fulness of the left front. (3.) The comparatively slight protrusion in the lateral region, or expansion of the intercostal spaces there. (4.) The absence of displacement *downwards* of the left lobe of the liver and of the stomach. (5.) The existence, nevertheless, of undue resistance in the outer part of the left hypochonder (which remained to the very last); and, (6.) The distinct fluctuation-movement communicated from the heart to the fluid, both in the epigastrium, and quite across the entire bulk of the effusion into the sixth left intercostal space. These circumstances, when taken in connection with each other, amount to pretty complete evidence of an effusion closely pent in between the displaced mediastinum and the outer wall of the chest, but restricted in its pressure downwards by old adhesions and thickening of the membrane, to which, I have no doubt, the resistance deep in the left hypochondrium was due. The bulging of the front wall of the thorax in recent pleurisy is usually much less evident than the lateral protrusion and the downward pressure of organs, as was observed in the case of William L., above mentioned, and in others to be afterwards narrated. But in pleurisies of longer standing it not unfrequently happens that the gravitation downwards of the fluid effusion is restrained by the great accumulation of solid products

in the lower part of the pleura, and the spontaneous obliteration or contraction of the cavity in that direction. It was very probably this abundance of imperfectly organized lymph that led to the failure of the first two tappings, which might have been more successful if they had been performed a little higher up.

The pulsations of the heart, if communicated to a tense pleural effusion, and causing thereby a movement of fluctuation remote from the cardiac apex, may simulate aneurism, as was first pointed out by Dr. M'Donnell of Dublin.* I shall afterwards give cases, shewing how the cardiac pulsations may react upon an air-filled cavity, causing very peculiar auscultatory phenomena, liable to be mistaken for aneurismal or cardiac murmurs.

Case IV.—This case (Wm. C.) resembles the last in so many points, that it seems quite unnecessary to treat of it here at any length, more especially as I have only seen it on a single occasion, along with my friend and former pupil, Dr. Ballantyne of Selkirk. The patient is a young man of tolerably healthy appearance, in whom there may, nevertheless, be a reasonable suspicion of tubercular disease from certain signs in the lungs and in the general history; the effusion has been gradual, and has oppressed the breathing so much, that Dr. Ballantyne some weeks since thought it right to draw off about 75 ounces of fluid, which proved to be of pale colour, slightly opalescent, and of specific gravity 1026 (only a little lower than the average of the blood-serum). The fluid has

* *Dublin Journal of Medical Science* for March 1844.

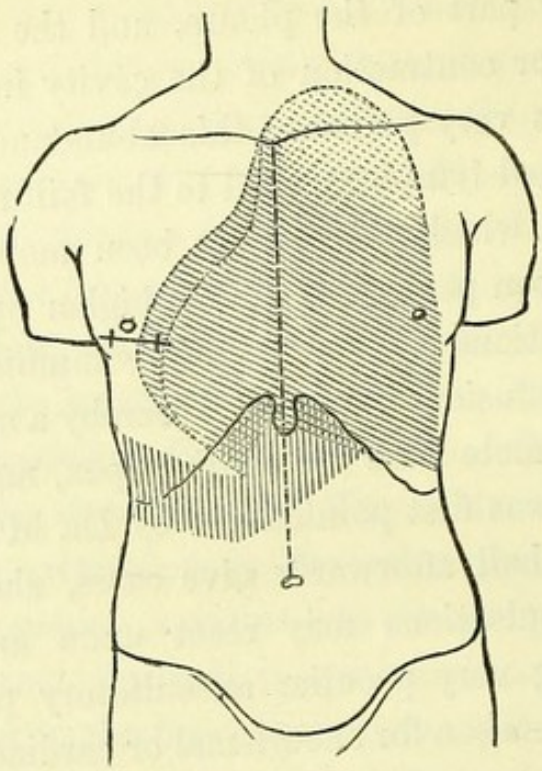

Fig. 5, A.

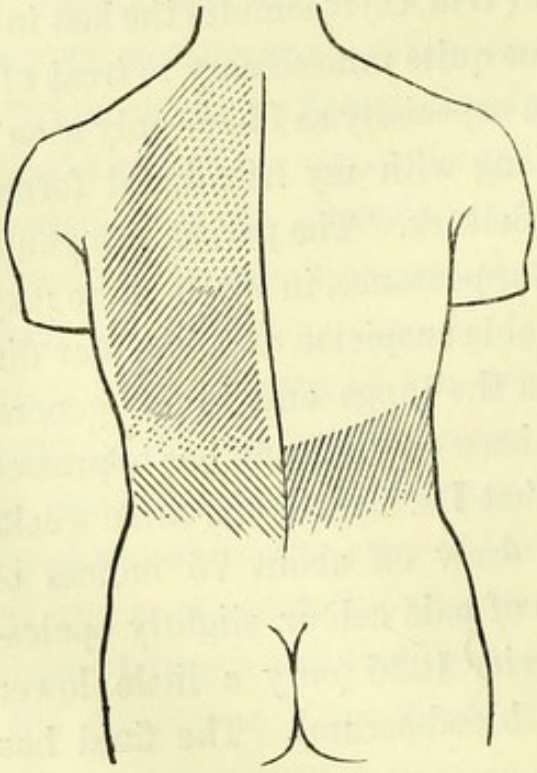

Fig. 5, B.

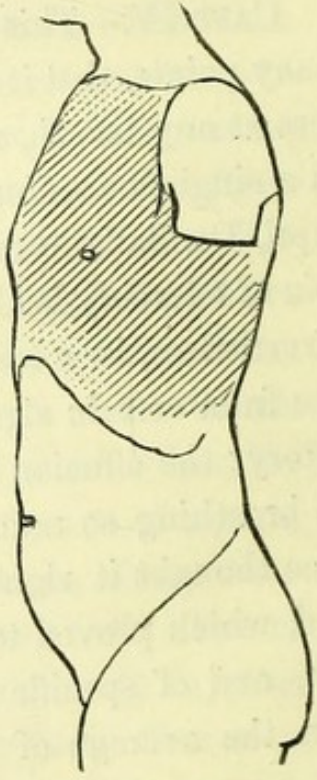

Fig. 5, C.

now reaccumulated, and the patient is desirous of being again relieved; he has accordingly been sent into town in order to allow of Bowditch's syringe being used on the next occasion. The circumstances of the case, and the constitution of the fluid drawn off, give some ground for the fear that it may pass more rapidly towards suppuration* than in the case of Colin S., although I feel perfectly well assured, from Dr. Ballantyne's description of the previous operation, that it was conducted in accordance with principles to be afterwards mentioned,

EXPLANATION OF DIAGRAM.

Fig. 5, A.

Front of thorax and abdomen in case of Wm. C., as described in the text. The markings are for the most part like those in Fig. 4, except that the cardiac and hepatic percussion-dulness are more separate, and that the seat of impulse is indicated by ‡, being less defined in position than in the former case. The inner dotted line near the margin of the effusion indicates the subsidence of the fluid towards the back part of the chest in the recumbent posture, the outer line having been drawn when the patient was erect.

Fig 5, B and C.

Back and side-view of the percussion-dulness in case of Wm. C. The clearness of stomach and colon modify the tone below, and the slight remaining pulmonary clearness above.

* While this sheet is passing through the press, the opinion in the text is verified by the event. The patient presented himself to-day (May 9th), absolutely determined to have the operation done, if possible; and although I had intended to wait somewhat longer, his state not appearing to me urgent, I did not in the circumstances see any good reason for delay. It was found, however, that not more than 38 ounces could be drawn off without inconvenience, the canula becoming obstructed, apparently by the closing in of solid lymph. The fluid is now quite evidently purulent in character, and no doubt will soon be perfectly pure pus. There was not the slightest fetor, however, or other evidence of decomposition; the patient's health and spirits seem improving, and I trust he will continue to be relieved by the repeated operations which will probably now become necessary. A careful examination by succussion, to-day after the operation, shews that there is no air in the chest.

and so as entirely to prevent the possibility of the entrance of air into the pleura.

The case is introduced here chiefly in order to give an additional diagram of the physical signs, which in many respects resemble those in Colin S., but differ in the relative position and amount of the cardiac and hepatic percussion-dulness, and in some other minor particulars. As it is impossible to explain these differences altogether at present, it seems sufficient to record them as facts for the guidance of careful observers. The liver is obviously pushed downwards and also backwards in this case, and the pleural effusion is more affected by position than in the former case, as is shewn by the retraction of the edge of the dull percussion in the recumbent posture, as shewn in the diagram (Fig. 5).

CASE V.—*Bright's disease of kidney, subacute at first, but becoming chronic, and leading to a very insidious and obstinate pleuritic effusion on the right side of the chest; displacement of liver and heart; after about two months' treatment by diuretics (often interrupted on account of diarrhœa), absorption of the greater part of the effusion, and restored function of the lung, with marked improvement of the general health, but persistence of the albuminuria.*

The particulars of this case are extremely interesting, but as it is introduced here chiefly with a view to the diagram of the physical signs, as a contrast to the preceding cases, I shall not enlarge upon them. The patient was a Dane (Christian M., æt. 20), who, at the time of his

admission to the hospital (October 25, 1861), was unable to speak a word of English, French, or German; our communications with him were therefore very much

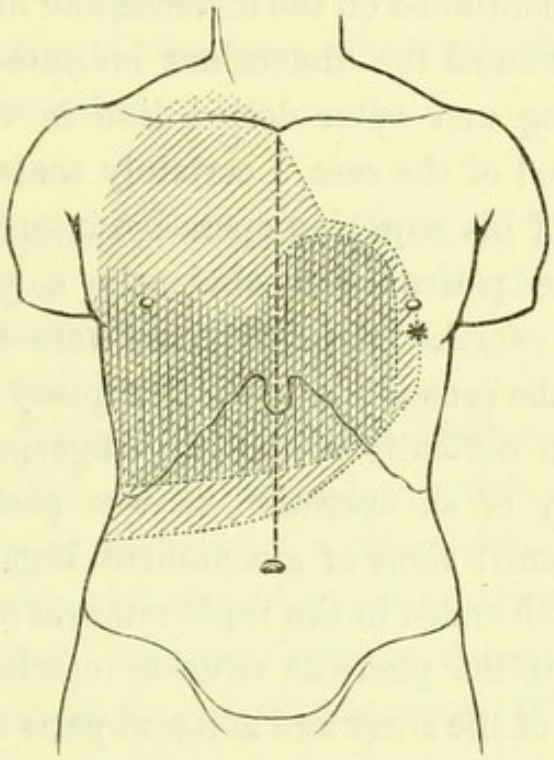

Fig. 6.

Front of thorax and abdomen in case of Christian M. The extreme boundary of the outer oblique shading indicates the limits of a large pleural effusion on the right side extending up to the clavicle, and displacing the liver downwards and the heart leftwards. The dulness is replaced by faint tympanitic tone near the summit of the chest.

* is the site of the apex-beat, an inch or more to the left of the usual position.

The darker shaded part, with vertical and oblique shading, indicates the improved state of the percussion-dulness some time before the patient was dismissed from the hospital.

restricted, and the pleuritic effusion, which was quite painless, had made considerable progress before it was discovered. The general dropsy was from the first extreme, and proved very little under the control of diu-

retics; the use of these remedies, indeed, having often to be suspended altogether on account of the very loose state of the bowels. After the employment, both of saline and other diuretics, with iron in various forms, the effusion continued on the increase, and finally (March 19, 1862) assumed the dimensions indicated in the oblique shading and outer dotted line in the diagram. At this period of the case I certainly contemplated the probability of his requiring operative interference; but, although the patient had obviously a good deal of dyspnœa on exertion, the symptoms were by no means extreme in the sense of immediate urgency; and owing chiefly to the difficulty of finding language to explain the propriety of an operation, it was postponed from day to day until signs of amendment began to be discovered, which ended in the rapid removal of the general dropsy and of the pleuritic effusion together. The improved state of the chest and adjacent parts on April 2, is indicated in Fig. 6 by the perpendicular shading crossing the oblique, and by the inner dotted line, which shews, perhaps, a somewhat hypertrophied heart, but only a slight remaining effusion in the pleura. The satisfactory character of the improvement is shewn in the fact that this is the patient formerly mentioned (p. 321), whom Colin S. chose as his companion to ascend Arthur's Seat; and although he did not quite reach the top, he was evidently in very good spirits, and perfectly pleased with his exploit so far as it went. This patient was dismissed on May 5, 1862, after more than six months' residence in hospital.

As a contrast to these observations, illustrating almost every variety of pleuritic effusion when seen in the advancing condition, I will now give an interesting case of retrograde empyema, which proved fatal from a sudden effusion in the opposite lung, and in which some obscurities of diagnosis were made clear by a *post-mortem* examination.

CASE VI.—*Empyema, with symptoms of extreme choleraic collapse—Difficulties of diagnosis—What is a diagnosis?—Treatment—Question of tapping.*

Lecture, Friday 23d November 1860.—A case came into the hospital on Saturday last which is extremely interesting as an example of a rather difficult diagnosis in chest disease, and also as one of a patient rescued for the moment from a state of extreme danger.

S. B., aged 42, was admitted on the 17th of November. Nothing could be more apparently urgent than her state on admission; bloodlessness and coldness of the whole surface; pallor and collapse of features; eyes sunk in the sockets; almost no pulse in either wrist; cold, clammy sweat; lips a little (but only a very little) livid; expression of anxiety and suffering; not much cough; no expectoration. Of course we could not examine her much in this state, nor inquire at great length into her history or symptoms; but we ascertained that she had twins fifteen years ago, since which time she has never menstruated; also that the present is not an acute attack, and has not

First appearances deceptive.

been preceded by any considerable pain; she says also that throughout the disease she has had no expectoration, at least none that she can remember. Her account of herself, however, is not to be trusted too much, for the very fact of her being brought here in such a state of urgent distress without being able to give any distinct account of its origin and progress shews that she is extremely unobservant of her own condition.* She lay on the right side, but had no orthopnœa or even apparent dyspnœa of any kind; neither was there any severe pain. The symptoms were purely those of collapse—very much indeed those of the collapse of cholera; and as she probably had purgative medicine before admission, the existence of those symptoms may be in part accounted for. But observe how deceptive was all this! Nothing in all these symptoms, and nothing in her history as given by herself, would have led you to the truth, which is, that this woman has unquestionably been suffering for a long time—an unascertainably long time—from disease of the right lung or pleura, or both. There is hardly any respiratory murmur in any part of the right side, and there is a great dulness on percussion all over—greatest in front, but great also behind and in the lateral region. (These facts we ascertained at once;

* She stated amongst other things that on the morning previous to admission she had been surprised to find that her urine was "all blood." She therefore applied at the dispensary, and the medicine she got there purged her so severely that she was obliged to leave it off. She had had a little pain in the right side, which she attributed to "wind." There was no blood whatever in the urine passed after admission.

the more minute examination had to be postponed on account of her feeble state on admission.)

On further examination on the 19th, when she had only a little recovered, under food, stimulants, and warmth, from her extreme exhaustion, we found that there was just a trace of tympanitic percussion (*tympanitic* and at the same time *dull;* the word "tympanitic" refers to the quality, not at all to the degree, of the sound)* below the right clavicle; there was almost a doubtful trace, also, of remaining percussion-sound in the lateral region; the hepatic dulness was not distinctly separable from the pulmonary, but the lower edge of the liver was exactly in the normal position. There was absolutely no evidence of distension of the right side, or protrusion of the intercostal spaces; nevertheless the dulness (as I shewed you particularly by an ink marking in the ward) appeared to cross the mesial line in front by nearly half an inch, or, at all events, ran so sharply up to the mediastinum as to give the impression of a degree of displacement of it. The opposite lung was quite clear throughout to percussion, and—to conclude the details so far as the left lung is concerned—it had puerile respiration, without râle, all over. On the diseased side the respiratory murmur was everywhere either very feeble or *nil* (there is in these cases generally a communicated sound from the opposite side and from the root of the lung, so that absolute deficiency of sound is not common). About the scapular spine alone was

Physical Diagnosis.

* See p. 309, *note.*

there anything like distinct sound with respiration, and there it was of tubular quality; in that region also there was a pretty distinct crepitus, not abundant, and rather coarse than fine. It may be friction, or may be intra-pulmonary—I don't know which.* The vocal resonance was pretty distinct in the upper part of the right side, both before and behind; elsewhere it was much less distinct; nowhere, perhaps, absolutely suppressed, and nowhere exaggerated or ægophonic.

Now this is a case which you will find it very difficult to resolve according to your accustomed rules of diagnosis. Is it a consolidation of the right lung, or an effusion into the pleura, or both together? I regard this question as practically insoluble at present, and will venture on it only a few remarks.

I think a *large* effusion in the pleura (which is suggested by the very remarkable dulness in the right front overlapping the mediastinum) is quite out of the question; because if the right pleura were distended with fluid to this extent, we should certainly have bulging of the side, probably with protrusion of the intercostal spaces; or, at all events, the liver would be pushed down towards the abdomen. It is very difficult to reconcile the entire absence of all these signs with a considerable pleural effusion, but I do not think we can exclude the idea of pleural effusion from the diagnosis; there may be fluid, but not in distending amount. Is the great and general dulness, then, due, in part at least,

* See the case of Peter B., pp. 315, 317; also case of Mr. A. B. A., further on.

to pulmonary consolidation? My opinion leans, on the whole, in this direction; but against this view we have to set the absence of expectoration throughout the disease—a strong fact, if we can really trust the patient's statement, which I fear we cannot do. If a pulmonary condensation, of what kind? Here we are quite at a loss. You know that you may have consolidation of the lung, or at least extensive and great dulness on percussion, from pneumonia, tubercle, cancer, hydatids, or other tumours. Of these, I think the first is virtually excluded by the history; for unless we find the facts to be otherwise than as stated, we have here not one of the clinical facts of pneumonia—not the acute development, not the pain, not the fever, not the rusty or any other expectoration; only crepitus at the back, which is compatible with almost any other form of consolidation. Infiltrated tubercle is not so improbable; but there is no sign of tubercle in the opposite lung, and, what is more singular still in this view of the case, no sign of softening in the tubercles of the affected lung. Cancer is also possible; but there is no evidence of cancer in any other part of the body. There is nothing to support the view of hydatids, and primary hydatid of the lung is extremely rare (though I have seen and recorded one case). We must conclude, then, that the diagnosis is very obscure, at least till we obtain more facts. In the meantime the treatment is plain enough. We must support and nourish the patient, and also keep her warm. The tendency to coldness, and the low vitality altogether, are the prominent and dangerous circumstances in the case. We

have met them, so far, successfully; she is decidedly improving, but by no means out of danger.

Friday, 14th December.—You remember the case of obscure disease in the right side of the chest, in a woman (S. B——) whom I mentioned to you before. (Recapitulation of facts.) This case has terminated fatally, after lingering longer than I could at first have believed possible in her exhausted condition. You remember the diagnosis; and observe, in relation to this very difficult case, the distinction which I always make between a *diagnosis* and a *guess*. A guess may be happy or the contrary—glaringly right or glaringly wrong; the element of chance or luck enters into it, and I do not deny that great reputations have been founded on happy chances. But a diagnosis, in the true medical sense—namely, a safe and just appreciation of all the elements of a case, with a view to treatment—is the highest art of the physician; and it has this characteristic as opposed to a guess, that it is *never wrong*. We shall not be afraid, I hope, to confess our mistakes: but whenever you or I make a mistake, depend upon it we have been trying our luck at guessing; we have not been making a diagnosis. What I mean by a diagnosis is this: *such a view of the facts of a case as excludes no important circumstance in it, and gives in few words the opinions that can be reasonably entertained with respect to it, and the grounds for those opinions.* And a diagnosis in this sense can never be far

A Diagnosis and a Guess.

wrong, if you accurately observe facts and correctly reason upon them.*

I am not ashamed of my diagnosis in the case of S. B——. You will recollect I admitted that the case was an obscure one; and I think now that we singled out every-

* These sentences were spoken and written too, before I had the opportunity of knowing the beautiful bit of illustrated metaphysics in the second series of the "Horæ Subsecivæ," by my friend Dr. John Brown, in regard to the "Happy guessing,"—the εὐστοχία of Aristotle; which is very truly and justly pronounced (in the paper I refer to) to be a high and noble faculty, akin to genius, but only to be perfected by cultivation; requiring, indeed, for its safe exercise, the "long and painful training of the reason in the sphere in which the guesses are to be made." With all that Dr. Brown has written on behalf of this faculty, I can most cordially agree, as also with the following, by his "Balliol friend:"—"Εὐστοχία is a hitting the *mark* successfully—a reaching to the end, the rapid, and, as it were, intuitive perception of the truth." This is quite in accordance with what has gone before; for the archer or rifleman does not hit the mark successfully till he has virtually trained himself so as to exclude all possible causes of failure, in particular instances, by careful and deliberate study of the allowance to be made for each; so much for gravitation, so much for the resistance of the air, for the wind, and even for his own individual and known specialties of arm or of eye. This study to exclude error is none the less true and precise that it is almost wholly practical; but in the matter of hitting the mark successfully with heavy ordnance, we find mathematical calculation, and theory, too, brought into play, just as medical theory is in the art of diagnosis. The same conditions, if I mistake not, preside over εὐστοχία, when brought into action. It is in such circumstances that ἀγχίνοια—the "nearness of the Νοῦς," or presence of mind (as it is commonly called) becomes apparent; not altogether as a new faculty, but rather, as Aristotle calls it, "a sort of εὐστοχία"—*happy guessing* reduced to practice, and applied, on the instant, to the necessities of the occasion. What I have striven to indicate again, in the sentences above recorded, is the ideal διάγνωσις,—*i.e.* not merely *knowing* in a general way,

thing about it that was of importance, and that could have been ascertained during life. I said that if the dulness on percussion was owing to effusion on the right pleura, the effusion was not large enough to distend the pleura. The absence of expectoration was, as I told you, a strong fact in favour of its being pleural; but we had reason to distrust the details of the history, and our convictions were still more unsettled upon this point afterwards, for while she continued in the ward she had expectoration—not much, indeed, nor yet very characteristic. On the

but *thorough knowing*—the *knowing through and through* of the facts of a case, and of all the inferences from the facts; which is so far from being inconsistent with "happy guessing," that it aims at including within one comprehensive judgment all the possible, or rather all the reasonable guesses which can be made; distinguishing the happy from the unhappy, and assigning mentally the real amount of confidence to be placed in each. This, I suspect, is truly the *εὐβουλία*, or good deliberation, of Aristotle. I am sure that Dr. Brown will agree with me in thinking that it is a matter of some practical importance to our students of medicine to keep in view the distinction between a diagnosis and a guess. He will readily admit that it is only through the cultivation of *exactness* and *certainty* of diagnosis, that any one can hope to arrive safely, either at the power of *happy guessing*, or at the *nearness of the νοῦς*, when it is wanted in matters of high medical import. He has, indeed, himself said as much. "Instead of this view of the healing art discouraging us from making our principles as precise as we should make our observations, it should urge us the more to this; for, depend upon it, that guess as we may often have to do, he will guess best, most happily for himself and his patient, who has the greatest amount of true knowledge, and the most serviceable amount of what we may call mental cash, ready money, and ready weapons." With which admirable and wholesome reflection on "happy guessing," I heartily commend Dr. Brown's remaining remarks, and their very characteristic illustrations in the original paper, to the consideration of all concerned.

other hand, it was extremely difficult to name any form of pulmonary consolidation which harmonized with the facts of the case ; but I should not have been greatly surprised if it had been tubercle or cancer. It was in fact a pleural effusion—an empyema ; but, as I supposed, it did not distend the side at all ; it was evidently of very old standing, and had caused such a condensation of the pleura as to render it almost fibro-cartilaginous in appearance ; in fact, the disease had long passed the stage of effusion, and the contents of the pleura were undergoing slow absorption, but had not become absorbed sufficiently to cause retraction of the side. As to the treatment, there is little to be said ; it consisted entirely of sustenance and warmth. When I first saw the patient I did not expect her to live twenty-four hours. She did, however, by the help of stimulation and food, get something like a pulse, and I then began to think she might have survived. Latterly, an œdematous effusion took place in the left lung, which was previously altogether healthy ; and this carried her off rapidly. Now was there a chance that tapping the effusion would have saved her? I do not think it could possibly have done so, because the fluid was not in anything like a distending amount ; had you attempted to draw it off, you would probably not have got more than two or three ounces. Besides, there never was any serious dyspnœa ; and even at the last the mode of death was not dyspnœa, but absolute sinking. It is right, however, to mention that tapping might have been useful at an earlier stage of the disease, when there was more fluid.

Had there been any doubt, it would have been right to use an exploring trocar.

In more than one of the previous observations I have remarked upon the difficulty, in certain stages of the absorption of pleuritic effusions, of distinguishing the intra-pulmonary from the extra-pulmonary varieties of râle—friction-sound, in fact, from crepitation, and even, in some cases, from mucous bubbling or clicking râle. I fully believe that experienced auscultators will not deny this difficulty; for which, nevertheless, I find students, and even practitioners of some standing, every day unprepared until they learn it by experience of error, committed through over-confidence. It is, I think, a grave omission in most of the regular text books and systematic works, not to state the fact of this difficulty in plain and clear terms; and I would therefore endeavour to contribute towards the true practical knowledge of the subject by placing distinctly before the mind of the reader some of the doubts that may arise. The importance of the subject will at once explain, and be the proper excuse for, the form in which the following case is presented. It was one that interested me much at the time, so that it stands recorded in my private note-book, not, indeed, with a view to publication, but in a form which I trust will be at least intelligible, and will carry the impress of reality more effectually than even a more carefully worded document. The few interpolations that seem absolutely requisite are introduced within [ ], and the omissions are

simply details of a personal kind, without any important bearing on the scientific part of the narrative. It may be as well to explain that with one of the three physicians here referred to, I was, and am, in frequent, if not daily communication; I trust he will fully recognize the general faithfulness of this narrative when he sees it. I suppress his name (which I am quite sure would be cheerfully given, if asked) chiefly because it would hardly be fair, at this distance of time, to burden him with any share of responsibility in regard to the statements of fact or of opinion here presented to the reader almost exactly as recorded in my case-book.

CASE VII.—*An obscure case of chronic pleurisy, simulating disease of the right lung, and marked by a peculiar constant râle, supposed to indicate "capillary bronchitis," but probably in great part extra-pulmonary. Muco-purulent expectoration. Question of tubercle? of empyema opening into the lung? Suspicions, gradually developed, of vertebral disease, ultimately confirmed by evidence of curvature of spine. After a lingering illness, death apparently from paralysis of the respiratory muscles. Vertebral abscess, nearly empty, communicating with right lung, and with spinal canal. Old dense adhesions of right lung; a small amount of retrograde tubercle in the left lung, and a still smaller amount in the right.*

January 29, 1855.—Mr. A. B. A., æt. 25. This young gentleman is a nephew (sister's son) of ———. Healthy family, no trace of hereditary tubercle [this fact rests on the authority of very particular statements made to me by the relative alluded to in the blank; and also, as regards the mother's side, on my own personal knowledge of three members of the family]. Has consulted Drs. X. (Edinburgh), Y. (Manchester), and more lately Z.

(Edinburgh). [These are, all of them, men of very high professional standing and large experience. Their opinions, however, were not submitted to me until I had given my own to a medical relative of the patient, who explained to me afterwards the different views which had been taken of the case by his previous medical advisers ; as will appear in the course of this narrative]. Employed in the business of a railway in Manchester, in which, it is believed, he has been overworked.

Almost exactly a year ago (end of January 1854), Mr. A. was seized with pain in the small of the back, gradually increasing. Towards the end of March, expectoration, breathlessness, loss of appetite. No one consulted, and complaints concealed till the spring. May 16th, arrived in Edinburgh, and on May 25th placed under the care of Dr. X. After cupping and blistering, pain of back shifted to front and right side of chest, then slowly decreased. Breathing relieved ; expectoration not affected ; some feverishness ; indifferent sleep. June 12, pain was almost gone ; returned to Manchester ; caught cold on journey ; increase of all the symptoms. Weight at this time was 10 stone ; original weight fully 12 stone.

History of early Symptoms.

June 15, under Dr. Y. in Manchester, was ordered blistering every third or fourth day, with relief to chest, but strength much reduced. Under nitrate and acetate of potash some diarrhœa. Sleep not procured without opium.

July 26, went to Malvern (weight 9 st. 7 lbs.), and after a fortnight, began to improve. Before leaving (on September 25) able to walk six miles a day, and in every respect felt much better. Weight, however, only slightly increased (9 st. 11 lbs.). Returned to Manchester.

Foggy, damp weather in Manchester brought back cough, rawness in throat, feverishness, chilliness, diminished appetite.

November 27, arrived in Edinburgh. After resting from fatigue of journey, began to improve again, and weight to increase. Improvement continues to present date (January 29) ; weight 10 st. 3 lbs.

At present complexion good, tongue clean; slight sour odour of breath, probably from expectoration; no bad taste; appetite good; sleep unbroken; walks several miles a day without fatigue; bears the cold well (hard frost, snow on ground); feels no distress in chest except in damp foggy weather. Expectorates a few ounces daily of muco-purulent sputum, not characteristic [of phthisis]; what I saw was much mixed with saliva. *Breathing permanently quickened, with a kind of habitual hitching motion, which appears to me to be nervous, and does not correspond to actual dyspnœa.* Lips perfectly free from lividity. Alveolar margin pale and flat. Evacuations regular and normal.

"Nervous" breathing.

On the right side of chest there is slight dulness on percussion; and respiratory murmur is much impaired, least so at top. *Over the lower two-thirds of the right lung, there is everywhere a loud, but not easily definable sound, chiefly with inspiration—something between creaking and clicking, with a share of both.* This sound is not affected by position. Both sides of chest move pretty freely. Vocal thrill nearly equal. Vocal resonance perhaps greatest on the right side; whispered resonance on the left side. *The auscultation and percussion of left lung quite unexceptionable.*

Right lung. Peculiar râle.

Note of Opinion, January 29, 1855.—This case may possibly be tubercular; but there are many favourable points which incline me to the contrary opinion. Integrity of left lung—seat of maximum disease in right [base]—appearance of patient—family history, etc. All that the physical signs *prove* is an impaired and partially compressed right lung, which is now heard in its imperfect play through the medium of adhesions, and possibly of slight fluid effusion. I feel certain that *some* of the râle heard is friction; whether *all* is so I cannot say—very probably.

Question of Tubercle.

What is the Râle?

Prognosis *in the meantime* is tolerably hopeful. Is he to leave Edinburgh, and to go to Rothesay, England, abroad? [Questions submitted to me by his friends]. No, not unless the weather here disagrees more than hitherto. He is not of those who by inclination or habit "trans mare currunt." Domestic, retiring, regular, shy. His mother, too, would be anxious and dissatisfied; and no change of climate, even if useful *per se* (which I doubt) would compensate for these disadvantages. * * * A short holiday of travel in spring may come under consideration by and by. So may the question of permanent occupation here or elsewhere. Meantime he is to amuse himself in Edinburgh. * * *

Various Opinions.

These opinions, delivered to ——, have met with assent. I am told [by the medical relative above indicated] that Z., who was consulted on Saturday, gave a very unfavourable prognosis, founded on the idea of a *capillary bronchitis*, which, he said, is in young persons rarely disengaged from tubercle, and is commonly incurable. Can Z. have taken the râle [in right side of chest], which I believe to be pleuritic, for one exclusively mucous?

X. considered the first attack [as observed by him May, 1854] pleuritic. Y. looked on it as empyema, destined to open outwardly. From this view X. dissented. The discrepancy of opinions led to my being consulted. The mother's leanings led to Z. being called.

I have no doubt that X. was correct [as to the existence of pleurisy at first]; but is it possible that Y. may have been also right so far [in anticipating empyema]?

that an empyema may have actually formed, and have opened into the lung [thus accounting for the expectoration]? This must remain doubtful. There was, indeed, no *sudden* gush of expectoration, but this I do not consider necessary [to make out the case of empyema].

January 31.—X. [to whom I had reported generally the preceding opinions] tells me this day that friction was quite unequivocal at first; that it was afterwards superseded by percussion-dulness, and returned after the Manchester visit. Further, that the râle now present has been of long continuance unaltered; that it has never disappeared, nor become modified by the expectoration; and that *he always regarded it as in great part extra-pulmonary.*

Additional Facts of History.

April 14, 1855.—Mr. A. called on me this morning. Since last report continued to feel well till the week before last, when he noticed a slight loss of weight. Had gained from 10 st. 3 lbs. to 10 st. 6 lbs.; is now 10 st. 4½ lbs. Six days ago felt increase of cough and oppression—no pain—strength somewhat declined—expectoration somewhat increased. No feverishness or shivering, cold sensation, etc.; no flushing; but during examination this morning felt faint, and pulse 120-130 (room very warm—too much so). Says his pulse is commonly 78-80 when quiet.

Re-examined chest—found almost everything as at last examination. The râle possibly somewhat feebler; the percussion scarcely differs on the two sides; neither does the vocal resonance; if anything this is greater on the right, but not at all ægophonic. A faint reflection of the râle can be heard on left side near the lower dorsal spines; and here *respiration is rather feeble,* elsewhere perfectly full and good over left side; particularly so at apex, where there is no trace of râle.

First suspicions of Spinal disease.

Careful measurement of the two sides shews extremely slight difference; if anything, the right is larger. I think the heart's apex a little displaced leftwards, but I am not sure (pulsation feeble owing to faintness).

Respiration still hurried. *I think that the lateral movement of the chest is impaired on both sides as compared with superior and diaphragmatic.* I am quite sure that there is no very appreciable difference on comparing the two sides. I think also that there is *a trace of angular projection of the lower dorsal spine.* It is so very slight and ill-defined, that little can be said about it; but this point must be kept in view for re-examination.

Opinion, April 14.—It would be an important modification of our views of this case could it be ascertained or clearly surmised that the beginning of the whole was spinal disease, causing a secondary empyema, and perhaps discharge of the vertebral abscess through the lung. The history of the case, the absence of tuberculous diathesis, the original site of the pain (see p. 342), the long duration of the disease, the [hitching, hurried, nervous] character of the breathing [carried on chiefly by the upper part of the chest and by the diaphragm] (see pp. 343 and 345, passages in italics), and finally the appearances of the spine itself, with the faint breathing near it, even on the left [the healthy] side, concur in giving probability to this view.

Guesses at Truth.

The actual state of the [right] lung itself is very obscure and doubtful. Why is there no marked dull percussion? Can there be air in the right pleura at its lower part? Why is there not relatively impaired movement [on the right side]? Is the râle heard friction?

or a mixed râle from air in pleural adhesions [modifying friction]? or an intra-pulmonary râle? I think the first or the second view the most probable. I am sure it is not a "capillary bronchitis," or a purely mucous râle of any kind. The lung, moreover, cannot be far from the surface.

Progress of Symptoms.

July 12.—Mr. A. has been in the country for some time. Weight 10 st. 3 lbs. [slightly declined since April, same as January]. Feels well and stronger, but for some days past pain in the *left* [the healthy] side, from near spine, round lower ribs to middle of side. On auscultation, nothing beyond the communicated murmur of right side, close to spine, as noted (p. 345). Pulse and system less excitable, all other things as formerly. Breath-sound good *all over left side;* on right side I think murmur decidedly fainter, and there is more respiration in upper part. Expectoration less, but he thinks yellower.

October 11.—Has returned from ———; seems improved in health and complexion; says that he lost weight during the heat of summer, and has just regained it; but feels decidedly better. Respiration seems much more free. Says that pain of back [pain of left side, above mentioned, had settled into back] was at first severe at ———; but he has now been rid of it for a month.

Sounds in right chest diminished, but not altered. I think there is more respiration; *left lung has perfectly free breathing throughout*, and there is no trace of the friction râle heard at left base on July 20 (but not noted).

May 6, 1857.—This poor lad died two days ago, having maintained a struggle with his disease for more than three years.

Since last note (October 11, 1855) I have seen him at intervals varying from weeks to months, but not with any degree of regularity. During 1856 he was for the most part in Glasgow,

where he was attempting to do some business for a few hours daily in a bank. He did not complain, and, indeed, took kindly to the work, having felt very much the idleness of his life for some time before. He had an instinctive shrinking from going abroad, founded, I have no doubt, on the feeling of insecurity as to his own life.

I saw him in October 1856, when he came to spend a day at ———. It was quite evident he had not gained ground, but neither did he seem considerably worse. On examining him, however, I found *antero-posterior curvature very distinctly present in the middle of the dorsal region*, and, as it appeared to me, *a limited region of dull percussion on either side of the curvature.* In other respects, the organic condition was unaltered. Of course he stooped more ; his breathing, though not decidedly worse, was no better. He sometimes felt fatigued with his work, but was unwilling to give it up in the absence of anything more suitable.

Rather more than a month ago, his mother told me of his feeling ill, and too weak for his work. I suggested his coming to Edinburgh on a visit. He did so after some delay, and I then found him so considerably weaker that I opposed his return to Glasgow.

On the day of his death his mother happened to be alone with him in the house, his other relative, before mentioned, with whom he resided in Edinburgh, being at a distance. He had passed a bad day, followed by a bad night, and I was sent for in the morning. As soon as I saw him (about 10 A.M.) it was plain to me that he would die. His countenance was dingy and livid, skin cool, respiration hurried and imperfect, evident distress and anxiety. A telegraph message was despatched for two of his relatives, but he died about 4 P.M., a little before their arrival.

Examination of the body.—Evident dorsal curvature. Heart normal. Left lung collapsed [recent] in lower part, even anteriorly, to a remarkable degree; also œdematous. A few very scattered miliary tubercles in left lung, with two very small cavities in upper lobe, quite retrograde and in midst of crepitant

tissue. In right lung even less tubercle than in left, but universal adhesions, extremely firm towards the base. This lung could not be separated at the base [behind] without breaking into a cavity formed between the tissue of the lung and the vertebral column. The cavity contained [only] a small quantity of pus; the finger passed readily down to the bodies of the vertebræ, and a probe passed down into what seemed to be the spinal canal, as it could be moved both upwards and downwards [in the direction of the canal] pretty freely. The other organs were normal.

The foregoing record might almost be left to be its own interpreter; I will, however, very briefly direct attention to its more important practical lessons, as conveyed in the language of facts, and of opinions written down at the time. It will readily be understood that the ultimate question here is not one of opinion at all, but of the real teaching of nature in this case, as a matter of science, apart from individual opinion. Having endeavoured throughout the record to do every justice to all the various opinions expressed, and having nothing new to add upon that head, I shall now address myself solely to the question of facts.

The râle described as constantly existing over the right lung in the greater part of its extent, and *only* over the right lung (except where evidently transmitted through the spine to the root of the left lung), is undoubtedly the most prominent diagnostic fact in the case. Notwithstanding the resemblance of this sound at times to the mucous râle of "capillary bronchitis," or the click of pretty early tubercle, notwithstanding also the pre-

sence of expectoration (which was undoubtedly a most embarrassing circumstance), and of a very little tubercle as found in this lung after death, I apprehend that the facts, when interpreted with the necessary care and exactness, preclude the idea of the râle having been tubercular, or even formed within the right lung or its bronchi. For, *First*, The râle was heard over nearly the entire right lung, and more or less from first to last; while the tubercle, in the right lung especially, was in exceedingly small amount. *Secondly*, The râle referred to was *never* heard over the left lung (with the exception referred to), although the tubercle was here both more abundant, and more in progress towards excavation, than in the right. *Thirdly*, The râle grew out a distinct friction-sound, and continued long to present, to the ear of the physician who had heard it from the first, some of its original character. *Fourthly*, The adhesions, gradually becoming denser as the fluid matter was absorbed, were exactly of a character to give rise to a sound universally heard, passing very slowly and gradually towards extinction, and never becoming completely extinguished. *Fifthly*, The cavity at the root of the right lung, in connection with the vertebral column, may possibly have intensified this râle or altered its apparent character, and may even, at times, have contributed to it a liquid element which would be very deceptive. *Sixthly*, The "clicking" element in this râle, by far the most deceptive element, is precisely what I have heard in other cases of absorbed pleurisy, in certain stages of the absorption. I lately saw, with Dr. Christison, a

typical case of this kind, in a young gentleman who came to me with pleuritic effusion, altogether displacing the heart from the left side of the chest. He was treated successfully by diuretics pretty actively pursued; and as *he had no expectoration throughout*, and had been seen by Dr. Christison at the height of the effusion, I watched the state of the chest during resolution with some interest, and was not at all surprised to find a râle, exactly resembling a mucous click, developed at a certain period of the resolution. I sent this patient back to Dr. Christison, accordingly, simply directing his attention to the râle, in connection with what he knew of the case; and was much pleased at discovering afterwards that his experience and convictions on the subject precisely coincided with my own. *Seventhly*, It was the occurrence of cases of this kind that led to the guarded diagnosis of the "clicking" râle in the case of Peter B. before recorded. The circumstances of that case are therefore not only corroborative of the present one, and receive corroboration in return from it, but both cases concur in suggesting the doubt, whether the "clicking" râle of ordinary phthisis is *always* intra-pulmonary? A similar doubt, in my opinion, extends to some kinds and some stages of so-called "crepitating" râle, as heard in pneumonia. But this wide subject would require a quite separate discussion to do it anything like justice.

On the whole, I would submit as the ultimate result of this case and the others narrated, that *the diagnosis of râles* (like that of cardiac murmurs to be afterwards discussed in this volume) *is often not a matter of*

mere acoustics, even as to the determination of their intra-pulmonary or extra-pulmonary character; but of careful consideration applied to the whole circumstances of their development. In other words, I would maintain that both pulmonary and cardiac physical diagnosis are to be placed, as a rule, more within the domain of the reason, and less within the domain of the senses alone, than is commonly represented in works having authority; the want of recognition of this truth, in relation to pulmonary râles and cardiac murmurs, having a tendency to mislead the student in certain cases by no means of rare occurrence, and on points of great practical moment. I think there is little doubt that the apparent simplicity of our text-books often leads in reality to confusion and error, by insisting on distinctions as of primary importance, and easy to be made, which not rarely fail us at the bedside, and therefore never can be *safe* in practice, unless guided by a highly cultivated faculty of rational and exact diagnosis applied to all the facts of the particular case under consideration.

[The following pages (353 to 360) belong to the summer session of 1856, and are given here nearly as published in the *Edinburgh Medical Journal* for August in that year; although it will be observed that the opinions expressed require some modification, in accordance with later experience.]

Two cases of empyema afford an interesting contrast as regards the circumstances which rule the performance of the operation of paracentesis.

CASE VIII.—*Tubercular Empyema, fatal by hectic fever and diarrhœa.*

One of the patients alluded to was a shoemaker, æt. 29, affected for several months with cough, dyspnœa, and hectic fever, and in an advanced stage of emaciation. He was unable to lie upon the affected side (the right), which was intensely dull on percussion, and prominent. There was a degree of cracked-pot sound under the right clavicle, and suspiciously, though faintly cavernous breathing. At the left apex, the respiratory sounds were also not pure, and the percussion was somewhat dull. The patient was expectorating much pus, and the individual sputa were like those proper to phthisis. He was under treatment about six weeks, and finally died exhausted by hectic fever, and by diarrhœa. The whole disease was tubercular; there was a cavity in the right apex, tubercular infiltration of left, and ulcers of the intestines. It is clear that, in this case, paracentesis could not have been counted on as likely to afford any relief, while there would have been a probability that the patient might either have sunk from the operation, or, at all events, immediately after it.*

The other case was that of a robust young man from the country, where he had followed the rather wholesome occupation of a currier up to the time of his seizure. The case is worth quoting in detail, as reported by Mr. Yellowlees, clinical clerk:—

* Dr. Bowditch's method of operation, however, affords increased room for palliative interference in such cases.

CASE IX.—*Empyema, not apparently tubercular, but with a questionable history (Qu., preceded by pneumothorax ?)—Signs of large effusion on right side, displacing the heart and liver —Marked relief from thoracentesis, followed by remedies; but no permanent improvement—Death from hectic fever some months afterwards—Question of operation.*

P—— J——, æt. 27, currier, admitted April 28, 1856. The patient is of apparently healthy constitution, and of robust appearance. He says that he has had a cough ever since June last ; that about six months ago he was seized with a severe cold, which produced great aggravation of the cough, pain in the right side, and considerable dyspnœa. In about six weeks he recovered from this attack (under appropriate treatment) so far as to be able to walk about. He states, positively, that for about a month during his slow convalescence, a splashing sound within his chest was distinctly audible both to himself and his physician, on any sudden change of position, and that it was readily elicited by shaking him. The splashing sound disappeared about three months ago. There has been dyspnœa, more or less, since this time, but not commonly considerable, except after exertion of any kind. About four weeks ago the cough and dyspnœa became worse, and both continued so urgent as to require his admission to the hospital.

Splashing Sound.

May 8th.—The physical signs are those of chronic pleurisy of the right side, with very considerable effusion ; they have not changed in the least degree since his admission, and are as follows :—Marked bulging of the right side, with obliteration of the right intercostal spaces. Less motion of the affected side in respiration ; and, when measured two inches below the nipple, its circumference is two inches greater than that of the opposite side. Percussion anteriorly gives absolute dulness, except at the very apex, where it is *almost* absolute. This dulness extends one inch across the middle line, towards the opposite side. The

heart is considerably displaced to the left, and its apex beats between the fifth and sixth ribs, one inch and a half external to a line drawn vertically from the nipple. The hepatic dulness, measured in the course of the same line, extends for three inches below the margin of the hypochonder. Posteriorly there is marked dulness on percussion on the right side ; and the respiratory sounds heard, at least those towards the base, are probably only the sounds transmitted through the spine from the opposite side. On the left side posteriorly the percussion and respiratory sounds are apparently normal. Since his admission, diuretics have been fairly tried, but without any effect in reducing the amount of effusion. Blisters and mercurials had been used for the same object, before his admission, and with a like result. The operation of paracentesis thoracis was therefore judged expedient, as alone holding out any prospect of cure.

May 10.—The operation was performed to-day, and about seventy ounces of yellowish sero-purulent fluid withdrawn, by a very small canula. The patient immediately experienced marked relief, and the breathing became much freer.

Vespere.—The physical signs have undergone a change corresponding to the relief which the patient experiences. The dulness of the right side anteriorly is still complete, except at the very apex, where, too, it is still great. It extends towards the left side almost as far as before, but it is now incomplete, and not absolute as before, for an inch beyond the middle line. The heart's apex beats between the fourth and fifth ribs, a little beneath, and to the outside of the nipple. The hepatic dulness does not extend more than two inches below the hypochonder.

℞. Acetat. Potassæ ℥i.
Inf. Scoparii ℥viii. m.
Sumat ℥i. ter in die.

May 15.—Continues the diuretic mixture, and makes about the normal quantity of water. He is in all respects as at last report. Expresses himself as much relieved by the tapping.

May 20.—The diuretic still continued. Urine free from al-

bumen, 68 ounces in twenty-four hours. All his symptoms have been greatly relieved by the tapping, and he feels himself much better. The vocal thrill is much greater on the left than right side, but greater on the latter than it has been since admission. Respiration is heard at the right apex, very harsh, and probably in part transmitted from the trachea and larynx. At the left apex the respiration is loud, inspiration wavy, and expiration slightly prolonged. There is absolute dulness on right side, except at apex, as far as the middle line, and slight comparative dulness for half an inch beyond it. The measurement of the right side, two inches below the nipple, is exactly the same as it was the day after the tapping. Since the tapping he has been taking 3 grs. of the proto-iodide of mercury, thrice daily, in pills.

May 26.—In every respect as at last report; urine abundant, and free from albumen. Continues the diuretic, and the pills of iodide of mercury.

June 1.—The measurement of the right side is a quarter of an inch, and that of the left side one inch less than at last measurement. This seems to be owing in part to absorption of the subcutaneous fat. He has lost about 9 lbs. in weight since his admission to the hospital. He has taken pills of the proto-iodide of mercury, 3 grs. thrice daily ever since May 12th, and they are still continued. Diuretic to be stopped, and cod-liver oil ordered. Urine abundant, free from albumen.

June 9.—The gums have become affected by the pills, and they are ordered to be stopped. The diuretic mixture has been resumed, as the urine became scanty as soon as its use was discontinued. He takes the cod-liver oil three times daily, and is otherwise as at last report.

June 13.—No change.

June 15.—The operation of paracentesis thoracis was repeated yesterday, with a larger trocar, and 72 ounces of a somewhat purulent and very albuminous fluid were drawn off. The measurement of the right side is less by half an inch, and that of the left side is less by fully a quarter of an inch, since the tap-

ping. He says his waistcoat is very loose, and he feels greatly relieved. The diuretics and cod-liver oil are continued. The respiration seems to be somewhat freer at right apex, and dulness less ; otherwise as before.

June 18.—The relief obtained by tapping continues. He is making plenty of water, and in other respects as formerly.

June 21.—No change. Continues the oil and diuretic.

June 28.—Has continued in exactly the same condition until this date. His general health is very good ; there is no dyspnœa, except on exertion ; and he is greatly improved since admission.

Dismissed relieved.*

Although there is no sufficient evidence of disease of the left lung in this case (in which respect it contrasts strongly with the preceding), I am far from certain that there is not a tubercular taint. This suspicion is liable to arise in every case of pleurisy in which the disease becomes chronic, and is attended with loss of flesh ; for non-tubercular chronic pleurisy, though not of the last degree of rarity, is far less common than the opposite kind. On the other hand, this patient's case has many points in favour of the view that his pleurisy is simple ; and the loss of flesh may be merely the consequence of restricted exercise and of suppuration slowly advancing in the pleural effusion. The alleged splashing sound at one period, which has now disappeared, raises a number of curious questions. One would wish to have had

* This patient continued pretty well for many months after his dismissal, but the effusion was never absorbed. Ultimately he died under the care of Dr. Bell, then of Leslie, who was kind enough to write me about him. I was consulted about the propriety of tapping him again, shortly before his death ; but the progress of hectic fever rendered this plainly inexpedient.

medical evidence as to the fact; yet I do not much doubt his own statement, which is very explicit. Was the pleurisy, then, originally caused by the bursting of a cavity, of which some obscure hints are, perhaps, still discernible at the apex of the lung? Or, did a pleuritic effusion make its way into the lung, by an opening through which air regurgitated into the pleura? The absence of expectoration in any quantity throughout the disease negatives the latter supposition, and leaves, therefore, a strong probability of the previous existence of excavation.

The practical question that we had to decide, was the propriety of performing paracentesis thoracis; and I never entertained any doubt upon the subject. Whether tubercular or not, the patient was young, he was active, he was restrained from exercise, and kept in great discomfort, by the oppression of his full chest; he had one healthy lung, and notable displacement of the viscera and mediastinum. These circumstances appeared to me to make the operation at once imperative and safe. Much has been written on the statistics of empyema, with the view, on the one hand, of discountenancing, and, on the other, of recommending the operation. Accordingly, we find, that while some practitioners decline to perform it except in the last extremity of suffering, M. Trousseau recommends the removal of the fluid even in acute simple pleurisy, when the quantity, as ascertained by percussion, is supposed to exceed a certain limit; and this, without any reference to symptoms

whatever.* M. Trousseau's results are said to be very favourable ; but the real question is, if the operation in these cases be at all necessary? Knowing the general safety with which simple acute pleurisy is treated without operation, I am at a loss to see the advantage which it is proposed to obtain by the latter method. But, on the other hand, it is equally plain that the deaths after *paracentesis thoracis*, in the hands of many practitioners, are deaths not from the operation, but from the antecedent disease. Had we operated in the case of tubercular empyema recorded above, the list of the fatal cases would have been increased, and no advantage would have been gained. But the safety and satisfactory character of the operation in properly chosen cases, is shewn in that of P—— J——.

Mr. Cock and Dr. Hughes of Guy's Hospital, have laid much stress on the employment of a very small trocar and canula, as conducing materially to the success of the operation. It is undoubtedly desirable not to make an unduly large opening into the chest ; and I have drawn off pint after pint of serous fluid, by a fine exploring canula, which, moreover, is often a most useful instrument in cases where the character and exact position of the fluid effusion is doubtful. But I appre-

* This statement requires qualification, having regard to M. Trousseau's opinions as given at large in his *Clinique Medicale*, vol. i. The remark in the text was founded on the perusal of individual clinical lectures, reported, I think, in the *Gazette des Hopitaux*, but to which I cannot now refer. The subject is more fully discussed afterwards, (p. 369 *et seq.*)

hend that it is possible to go too far in the direction of smallness. The real object to be attained is, to draw off the fluid, of whatever consistence it may happen to be, in a fine uniform stream, so that there may be no great risk either of stoppage of the canula, or of the sudden ingurgitation of air during inspiration. For this end, it should be allowed to flow as long as it flows freely; but whenever the stream flags, from any other cause than accidental obstruction of the canula, the point of safety has been reached; the instrument should then be withdrawn, and the wound closed. If this, which is the only really important caution, be attended to, it is, I believe, a matter of comparative indifference whether the canula be larger or smaller; and it ought, for the comfort of the patient, as well as of the operator, to be always large enough to give free vent to the fluid, whatever be its consistence.*

In short, the object in paracentesis thoracis is merely to unload, and not to empty the over-distended side; when, however, it is done in properly selected circumstances, it seems to be an operation of very small danger in itself, and may not only be freely performed, but repeated as often as seems necessary.

* It is of importance that the canula should be closed during coughing, on account of the sudden forced inspiration by which it is followed. The operator should therefore have his thumb ready to close the opening, and the patient should be directed to give notice when he is about to cough. With this precaution, and in the circumstances above mentioned, mechanical contrivances to prevent the entrance of the air are quite unnecessary. [See further remarks at p. 379, on Dr. Bowditch's plan of operation.]

CASE X.*—*Acute Pleuritic Effusion, filling the left side, and displacing the heart to the middle line. Doubtful antecedents, and patient probably not of temperate habits. Recovery under remedies, without operation. Metallic tone of returning crepitus (friction), in neighbourhood of stomach. Tympanitic percussion in lateral region (gastric).*

H. P., æt. 30 ; single ; joiner ; native of Scotland ; admitted 20th June 1859.

Patient has been ill for a fortnight. Has suffered from cough, with pain on coughing more than on inspiration, and not referred particularly to either side. Of late, says he has had pain in left shoulder ; also considerable difficulty of breathing.

Patient says he was a healthy man up to a fortnight ago ; in fact, never had a day's illness. Expectoration very slight throughout ; at present as much as it has ever been ; it does not cover the bottom of the vessel. It is very tenacious, almost pure mucus ; and presents a single trace of blood, which he says is from the gums.

Patient is a joiner by trade, and previously to his illness was employed in a baker's shop, where he perspired a great deal, and caught cold by going out into the open air. Says he had pain in both sides, and cough, some days before he gave up work. He lost his appetite, became very weak, and used to sweat profusely at night, but continued at work till four days ago.

On admission, patient did not present the symptoms of a severe illness ; but there was considerable heat of skin, and the pulse numbered 102.

22*d June*.—Examined by Dr. Gairdner. Left chest is dull throughout ; most remarkably so in lower part, where dulness is absolute, except in lateral region, and there tympanitic resonance of stomach extends up to 6th intercostal space. Respiratory murmur much diminished and all

Physical Signs.

* Reported by Dr. Shearer.

but suppressed on left side. No râles are heard. Vocal thrill and resonance very much diminished on left side; indeed, quite suppressed below, and towards spine assuming a very faint ægophonic character. Little or no bulging of left intercostal spaces. Dulness of percussion does not pass beyond the middle line in front, but proceeds very closely up to it. Apex-beat of heart cannot be distinguished; sounds, on the whole, are loudest over the sternum, and are fully as loud 1½ inch to the right as same distance to the left of sternum. Left hypochondrium perhaps a little more tense than right, but this doubtful. Respiratory murmur of right front is puerile, but in all other respects quite unexceptionable. Measurement of left and right sides about equal, apparently less than a quarter of an inch being in favour of left side. Tongue slightly furred, not dry; margin of gums very slightly spongy. General aspect that of a robust man, without cachectic appearance. Pulse 84; skin quite cool. Ordered:—

℞ Liq. Corrosivi Sublim. . . ℥iij.
A teaspoonful thrice a-day.

℞ Potassii Iodidi . . . i.
Potassæ Acetatis . . . ℥ss.
Aquæ . . . . . Oi. m
A wine-glassful three or four times a day.

22*d June.*— *Vespere.* Pulse 88, soft. A fly blister applied to affected side.

23*d June.*—Pain in side considerably relieved by the blister; breathing not any easier. Pulse 92.

24*th June.*—Patient is not relieved in his breathing. Pulse 104. Complains that the medicine in the small bottle burns his mouth though taken in water. Says, on being asked, that his gums are a little sore, but no mercurial fetor perceptible. Has taken ℥vij. of the Liq. Corrosivi Sublim. To take, instead, 1-6th of a grain of the Iodide of Mercury thrice daily.

25*th June.*—Patient has been somewhat feverish since application of blister, and to-day, though skin not hot, the pulse is 100.

Dulness passes the mediastinum by about an inch.* Heart's sounds distinctly louder to right of sternum, and there can even be observed an indistinct impulse in one of the intercostal spaces on that side. An inch below the point where the measurements were formerly made, the difference between the two sides considerably greater, being not less than an inch in favour of left side; and in lower part of left lateral region there is distinct bulging.

Increase of Effusion.

℞ Calomelanos, gr., v.
Pulv. Jalapæ Co. gr., xxv. m
ft. pulv. tales tres.

One immediately (3 P.M.), the second to-night, and the third to-morrow night.

27*th June.*—Patient says his breathing is not any better. Pulse 88. Respirations 28. No lividity. No obstruction of swallowing. Trachea seems rather deep in the neck, but quite mobile. Limits of percussion-dulness unchanged, but amount of tension in lateral region apparently rather less.

28*th June.*—Patient certainly not worse; thinks breathing much the same. Measurements almost exactly the same as in previous note; but tension appears less in lower lateral region, and dulness on percussion a little less intense on right side of mediastinum. Fever inconsiderable. Ung. Hydrarg. ordered to be rubbed into the groins, as he cannot take the mercury in any form without inconvenience and sickness.

Beginning of Improvement. Remedies continued.

30*th June.*—Dulness less marked to right of sternum. Another blister 9 × 6 in. to be applied to side.

9*th July.*—Patient has been several days out of bed, and has been trying his breath upon the stairs. Patient admits improvement; but, on the whole, is not so much better as to give him much confidence. Still feels dyspnœa, both on movement and at night. Left chest remains perfectly dull except at apex, but here there is quite distinct percussion sound, of somewhat tym-

* Compare with notes of June 22d and of 18th July.

panitic character, especially above the clavicle, and upon direct percussion of the clavicle itself. Above clavicle, respiratory murmur is pretty well heard ; and also immediately below clavicle and at upper part of back, where, however, it has a faintly tubular character. There is now no distension of intercostal spaces, and no trace of dull percussion to the right of middle line. Heart's sounds still very faint in normal apex-region, and sounds almost equal on either side of sternum.

18*th July*.—Distinct tympanitic percussion audible as high as left nipple, and in left lateral region distinct, though faint, up to 2 inches from axilla, and as far back as margin of latissimus dorsi in recumbent posture. This tympanitic percussion inseparable, both as to character and distribution, from the strong and well-marked gastric tone of whole epigastrium and left hypochondrium. In left apex there is well-marked tubularity of respiration, and below the second rib everywhere a coarse crepitus, heard down to and below nipple, and into left lateral region, where it assumes a character more resonant and hollow, approaching very closely at some points to a metallic quality.

Evidence of Absorption of Effusion.

Dull percussion of left lung is at no point traceable past the middle line ; but on the contrary, clear percussion of opposite front seems to transcend the middle line.* The tympanitic percussion, noted on the 9th, at apex of left lung is more extended, traceable as low as second intercostal space ; in fact, the only considerably dull part of left front in the recumbent posture, is between the last-mentioned point and the nipple. Intercostal spaces not in the slightest degree bulging on the left side. Vocal resonance exaggerated over the apex of left lung in front, gradually diminishing downwards, and disappearing about the third intercostal space, not distinctly ægophonic anywhere. In back, vocal thrill and resonance weakened throughout, considerably weaker below spine of scapula, with faint trace of ægophonic character. Sounds of heart now quite distinct below left nipple, and very indistinct to right of sternum, though the apex-beat is still not

* Compare with note of 25th June.

traceable in the normal situation. No expectoration has occurred throughout.

16*th August.*—Beat of heart distinct in fourth left intercostal space rather within the nipple; less distinct, but appreciable, in fifth; of medium distinctness in third. Sounds quite distinct in all these situations; indistinct on right side of chest. Left front somewhat flattened; percussion not so pure as in right, but everywhere there is considerable resonance, inclining to tympanitic quality. Below nipple-region, and below a line from nipple obliquely outwards and downwards, nearly halfway between xyphoid and axilla, percussion is tympanitic, and undistinguishable in character from the unquestionably tympanitic percussion of epigastrium. Respiratory murmur of right front full, of left front comparatively deficient and tubular, but still abundant. Similar distinction everywhere between right and left back, but at no point respiration entirely wanting unless in lowest part of lateral region. No râles. Quasi-metallic phenomena formerly observed are now gone. Expectoration hardly appreciable; a little mucus, he thinks from his throat. Patient is quite sure that he is gaining strength, he is also free from pain, and has been improving in regard to the dyspnœa on exertion. Complains, however, of sweatings at night. These sweatings happen every night, and are so profuse as to force him to change his night-shirt. They have been totally unaffected by quinine, with sulph. acid, which he has been taking for a fortnight without effect. (The weather, however, was very warm.)

Effusion mostly Absorbed.

There can be little doubt in this case that, long before the patient was dismissed, the lung had again resumed perfect contact with the wall of the chest over the whole of its extent, while the general strength was much improved and the respiration became much more free. It is true that the lung had not expanded fully to its former bulk, as is shewn by the displacement of

the stomach upwards into the lateral region, and the imperfection of the respiratory murmur ; but this is a common result of large effusion, much more time being necessary before the ultimate result of such a case can be stated with confidence. It is evident, however, that between the 27th June, when the distension of the pleura reached its height, and the 10th of August (little more than six weeks), several quarts of fluid must have been removed from the chest by the gradual operation of remedies, acting together with nature. The first impression on the disease seemed to be made by the purgatives administered on the 25th June. It is extremely doubtful, I think, if the mercury had any share in the result, as it never affected the mouth, and was not regularly taken, owing to its producing sickness. For several years, indeed, I have all but abandoned faith in mercury in cases of this kind, using it only as an experiment.

I had very nearly been led to perform paracentesis in this case. In fact, on the 25th of June, when the disease became rather aggravated under the remedies administered, the consent of the patient was obtained to the operation ; and I had some difficulty afterwards in persuading him to trust to the healing effect of time, as the oppression continued very considerable for several days.

In the following case a still larger effusion existed than in the former ; and on its proving rebellious to remedies, paracentesis was performed with a temporary good result. The fluid, however, re-accumulated, and the operation was again in contemplation, when the

disease took, somewhat suddenly, a turn for the better ; and absorption of the fluid took place with considerable rapidity :—

CASE XI.*—*Acute Pleuritic Effusion in a robust and temperate man, distending the left side, and displacing the mediastinum. A single operation, to relieve distention; recovery completed under internal remedies.*

R. L., æt. 24 ; single ; journeyman gardener ; admitted July 9, 1859.

Patient has been ill for 18 days with difficulty of breathing. The disease commenced with shivering and pain in the left side of the chest. Patient continued at work for a week, when the increased difficulty of breathing compelled him to desist. Dry cough was throughout very severe, but there was not the least trace of expectoration. After being laid up some days, applied to a medical man, who advised him to come into the Infirmary, and prescribed no remedy. Has always been a healthy man. About three years ago was a week off work with a sore throat, but has never had any complaint of the chest previously to this.

He is a well-formed and strong man, not emaciated, and when recumbent has no trace of dyspnœa in his appearance, except a somewhat exaggerated movement of the clavicles. The difference between the expansion of the sides is not striking, but on the whole the right expands better than the left.

The left thorax is absolutely dull on percussion, almost from base to apex ; the only remaining trace of sound being above the clavicle. The lower intercostal spaces are manifestly distended, the hypochondrium not so. Left lobe of liver depressed to within two inches of umbilicus.

Dulness on percussion passes about one inch beyond the mediastinum into the right front. Respiratory murmur greatly im-

* Reported by Mr. Swanston and Dr. Shearer.

paired or lost on the left side ; on right side normal. Vocal thrill and resonance in great part absent on the same side. Cardiac beat distinctly apprehended in the third and fourth intercostal spaces on the right side, about one or one and a half inches from the sternum, and sounds audible, as far as margin of right nipple, with greater intensity than at corresponding part of left side. Tongue moderately clean, slight silvery fur general. Pulse and respiration both rather accelerated. Skin perhaps a little elevated in temperature, not greatly so. Patient can lie easier on left side, and before his illness it was indifferent. Lies quite easily in recumbent position, and can lie on right side without much inconvenience. Slept last night on right side.

℞ Potass. Iod., ʒi.
Potass. Acet., ℥i.
Aquæ, Oi.
Sig. A wineglassful three or four times a-day.

Iodine ointment, with one part of lard, to be rubbed on affected side.

July 11*th.*—Ordered four grains of blue pill every second night.

July 13*th.*—As patient felt very uneasy and oppressed, and was not at all better since admission, paracentesis thoracis was performed to-day. Twenty ounces of muddy-yellow, highly albuminous fluid were drawn off with manifest relief to the patient, but it was found impossible to procure more without risking the admission of air. Continue remedies.

Operation.

Aug. 15*th.*—Up to the last two days no important change took place in the amount or character of the effusion (which seemed to have re-accumulated after the operation) ; and as the patient complained much of dyspnœa and inability to lie on the right side, his pulse being frequently quickened, it was intended to have performed paracentesis again so soon as the blistered surface had healed. (The treatment during the interval since last report has been blistering,

Progress.

occasional purges of Comp. Jalap powder, and a blue pill every second night.)

Last night and the night before, patient states that he was able to lie on the right side, and that he was less oppressed. On re-examination, bulging of lower intercostal spaces on left side obviously less, and there is now no dulness to be detected to the right of middle line. It is also observed that the pulsation of the heart, on the right side of sternum, is less defined than before, though still appreciable, and although the sounds are still somewhat more distinct to right of sternum than to left.

Aug. 30*th.*—Patient's breathing and strength have much improved. Says, however, he has lost four pounds within the last fortnight. Complains of flying pains in his sides and of loss of appetite.

℞ Acidi Nitro. Muriatici., ʒij.
Infusi Calumbæ, vij.
Tr. Gentianæ Co. ʒi.
ʒi. ter in die. Port wine 4 oz.

Sept. 20*th.*—Patient's strength is still improving, and there is now no doubt that he is in better condition than at last report. The heart is now in the normal position, or very nearly so, although the respiratory murmur is still feeble in the left lung generally, and there is still marked dulness in the lower part of the left lateral region over about a hand's breadth, with a slight tendency to fulness of one or two intercostal spaces. Elsewhere there is so much respiratory murmur and vocal resonance as to shew that the lung is probably in contact with the surface.

Question of thoracentesis, and the mode of its performance.—Notwithstanding all that has been written of late years in favour of reviving the operation of paracentesis thoracis in cases not otherwise desperate, it is an unquestionable fact that physicians of the greatest experience, and of the most unquestionable

sagacity, continue to regard the operation as one attended with great risk under any circumstances, and one, accordingly, which can hardly be recommended unless in cases immediately threatening life. It would appear, for example, that the late Dr. Addison,* unquestionably a most eminent and most faithful observer of nature, although originally a decided supporter of the operation, had come round by experience to be one of its greatest opponents; and this in the face of facts specially adduced and published by some of his colleagues in Guy's Hospital in support of its performance. On the other hand, the experience of many French and German authorities recorded with great care, and apparently justified by numerous successful results, has been tending to bring the operation into use not only in cases of chronic pleurisy or empyema which have resisted other remedies, but as a preferable method of treatment in acute cases, even before the failure of internal remedies has been established, and before the circumstances are such as to justify operative procedure as a mere palliative means. A full discussion of so intricate a question is not within the scope of these "Clinical Notes;" but a few remarks on the present cases, in their bearings upon it, may not be out of place.

M. Trousseau (or perhaps I should rather say some of his friends writing on his behalf) appears to me to have set up a somewhat too artificial distinction, in claiming credit for introducing, or re-introducing, the operation in cases of *acute* pleurisy, as con-

* Report of clinical lecture in *Lancet*, November 1855.

tradistinguished from chronic empyema;* for I apprehend that the difference between him and his more intelligent opponents arises, not on the question whether the disease is acute or chronic, but on the far more important question, whether the cases treated by him were curable more or less easily by other means than by the operation. No one, I suppose, would decline to plunge the trocar into the chest, were the alternative between operation on the one hand, and suffocation on the other, placed clearly in his view; and this without reference to the period of the disease, or

* Since this passage was first published, M. Trousseau's matured opinions on this and other subjects have become much more accessible on this side of the channel, through the publication of the first volume of his most instructive lectures, under the title of "*Clinique Medicale de l'Hotel-Dieu de Paris*, 1861;" and as the best method of putting to rights whatever wrong I may have inadvertently done him in the sentence given above, I will quote his own words, which are assuredly worthy in all respects of his character as a high minded and truly practical physician: "On me rendra, j'espère, cette justice que je parle rarement de moi et que, pour ma part, j'attache généralement peu de prix aux questions de priorité. Je puis donc, une fois en passant, revendiquer ce qui me revient pour la paracentèse de la poitrine. Je n'ai pas la prétention de l'avoir imaginée; je n'ai inventé aucun instrument spécial pour faciliter cette opération, je n'ai conseillé aucun procédé opératoire qui ne fût parfaitement connu auparavant; mais je crois avoir, sinon le premier, du moins l'un des premiers et en même temps que plusieurs praticiens étrangers à notre pays, formulé nettement la nécessité de la paracentèse dans les pleurésies avec épanchements excessifs; j'en ai établi avec précision, peut-être avec plus de précision que cela n'avait été fait avant moi, les indications; je crois, enfin, avoir popularisé une méthode qui, maintenant, est à peu près généralement adoptée, et, à ce titre, je pense avoir fait faire quelques progrès à la thérapeutique de la pleurésie." p. 622.

the violence of the fever.* But it is not unfairly urged, that in a large proportion of the cases claimed as successful results by the partisans of the operation, the disease may have been really quite curable by internal and external remedies; and when M. Trousseau argues, as he did at one time argue, for performing the operation *according to the state of the physical signs*, and without regard to the dyspnœa, he only furnishes additional materials for the objection, that his operations may possibly have been unnecessary, and may, therefore, form no sufficient basis for the conclusions he draws from them. Were the question limited to the saving of life from immediate danger, my own personal experience would compel me to join, to a certain extent, with the opponents of paracentesis in these objections to M. Trousseau's practice; for, since I became an hospital physician, it has occurred to me only once to have had under my care a case of acute simple pleurisy terminating directly in death; and in this case (Janet D., admitted September 1861), paracentesis *was*, in fact, performed, though late, to relieve an enormous acute empyema of the right side, threatening suffocation, in a young woman. I am sorry not to be able to give details of this case in a narrative form, but the facts are unhappily only too simple. The poor girl was rapidly exhausted by continuous hectic fever, and by respiratory oppression, over which neither the operation, nor the internal reme-

* From certain statements made in detail by M. Trousseau, I am inclined to think that this statement would require to be modified as regards the practice in France at the time he began to "popularize" the operation.

dies which preceded and followed it, seemed to have more than very slight and temporary power. Of course the more extreme advocates of thoracentesis will say, that the operation was in this case not performed early enough. My reply is, that it was performed as soon as the operation of internal remedies had been fairly tested; and there is no shadow of evidence to my mind that it would have permanently controlled the effusion, had it even been performed some days before the patient's admission to the hospital. It was plain to me at the time that this had been throughout a practically unmanageable case ; the effusion rapid, tending early to suppuration, accompanied from the first by consuming fever and rapid emaciation, so as to resemble a case of acute tuberculosis. A somewhat similar, but a much less rapid case, is recorded above (p. 353), in which tubercular disease actually was present, overruling the empyema ; and I am sure every physician of experience will admit that, in such circumstances, a diagnosis as to the presence or absence of tubercle must often be very doubtful and the inducement to interfere by operation, except in extreme effusion, not great. Perhaps the series of cases here recorded, and the remarks which follow, may entitle me to the credit of writing without prejudice as regards the operation in this instance. At all events I have to state (and this statement, deliberately made, is surely of some importance as regards the practical question), that in the midst of a very large, though not special experience of cases of severe disease, *I have only twice, in eleven years' hospital practice, felt*

myself called upon to perforate the chest in acute pleuritic effusion; *i.e.*, in effusion, accompanied from the first by decided symptoms having their origin within six weeks from the date of the beginning of treatment. This statement requires to be accompanied by another, as given above; viz., that *in no case of acute pleuritic effusion, during these eleven years, with the exception above referred to, has the disease ended in death.* In other words, there has been no single case in which I could make good the certainty, or even the reasonable probability, that the operation, performed within the first six weeks, would have saved life; and in the single case in which death occurred, it appeared to me clear that the operation actually had, and could only have had, however early performed, a very limited and palliative result. It is not too much to infer, that for the mere saving of life in the acute stage of the disease, it is not necessary, in any but a very small minority of the cases, to operate; for I have in fact operated in every such case within my experience, which has not yielded to remedies, and in which the necessity appeared to me at all urgent. I am very far indeed from wishing to convey the impression that cases fitted for operation do not occur in the acute stage referred to; but I am fairly entitled to say that they cannot be common, and that they will be rare in proportion to the diligent use of remedies in the earliest period.*

* The very sudden deaths *from pure syncope*, referred to by M. Trousseau, and so important to his argument, have not occurred within my experience of acute pleurisy of one side. I fear it is impossible to

The saving of life from immediate danger is not, however, the whole question. The advocates of the operation may very fairly plead that many lives are lost through allowing an acute pleurisy to degenerate into the far more unmanageable chronic empyema; and that the ultimate prospects of the patient are seriously compromised, if the lung be allowed to settle into the state of collapse without relieving it, at an early period, from the superincumbent pressure of fluid. To this the opponents of the operation may reply that, admitting the premises, it is only a question between two methods of relieving the lung; a question, too, not to be settled by the immediate and direct relief experienced, but by the ultimate result. Does the operation *cure* (in the sense of *permanently* removing the fluid, and allowing the lung to re-expand) quicker and better than ordinary remedies? or is it safer in the end to trust to the apparently slower, but in many cases not less effectual, methods in ordinary use? This question is one not to be answered by statistics, but rather by the careful consideration of individual cases; and I am not sorry that the state of my convictions has allowed of so evenly-balanced an experiment as is contained in some of the cases above recorded. The cases of H. P. and of R. C., taken in connection with those of W. L., of Christian M., and of the young girl mentioned above, appear to me to present much

avoid the suspicion in some of these cases, as reported, that the death was due to the severity of the diet, or to the treatment by depletion and digitalis rather than to the disease. See *Clinique Medicale*, I., 638; and the reference in p. 379 of this article, *note* on case of R. L.

instruction on this point, on which I will venture to add a few further remarks, suggested by a wider experience.

For some years I held and taught at the bedside the opinion,* that while the operation of thoracentesis might be *safely* enough practised, provided it is managed so as entirely to avoid the admission of air into the chest, yet that the very necessity of avoiding this great source of danger restricted, to a great extent, the utility of the operation as a strictly curative means, and reduced it to the rank of a palliative remedy, whether in acute or in chronic pleurisy. While, therefore, I am quite sure that I have never withheld the operation in any case of great distension, when other remedies have had a fair trial without effect, I think I have commonly avoided performing it in cases of partial and moderate effusion, both because the symptoms in such cases have not appeared to me usually to warrant its performance, and because in such cases it has appeared to me practically impossible, by any means excepting one presently to be mentioned, to draw off a considerable quantity of fluid without the admission of air. A very small amount of familiarity with the operation of thoracentesis, as performed in chronic cases, or even a careful consideration of the mechanical conditions to which fluid in the pleural cavity is subjected, will suffice to shew that nothing short of powerful suction can possibly remove the bulk of a pleuritic effusion. All that can be done by the operation as commonly conducted, is to remove that portion of the fluid which distends and bulges the

* See p. 358, *et seq.* for opinions of 1856.

cavity ; and all that can be done *with safety*, is to let the fluid flow from the canula as long as it will flow without intermission. I have little doubt that the disastrous effects observed in so many cases from the operation of thoracentesis, have resulted from the absurd and dangerous attempt to empty the thorax completely ; an attempt which must always be followed (unless suction is employed) by the admission of air in proportion to the amount of fluid evacuated ; or, if air is prevented from entering (by the method of Schuh or otherwise), must end in expelling the distending portion of fluid, and that only.

Guided by these principles, I determined upon performing thoracentesis in the case of R. L., not because it appeared to me that the operation could not have been dispensed with, but because, from the considerable amount of distension, it seemed probable that some present relief might be obtained without danger as regards the ultimate result ; and further, because remedies had been fully brought into action without the desired effect. The result was, so far, favourable ; for decided relief followed the operation, and no bad effects whatever were observed. But, on the other hand, re-accumulation was not prevented, notwithstanding that internal remedies were freely employed. And the quantity of fluid drawn off was not much more than a pint, although pressure on the hypochondrium was carefully maintained, and every means was taken to evacuate as far as was deemed safe. So that the effect of the operation, as regards the real cure of the disease, was probably but small, either way. It acted as a pal-

liative means, and without serious danger gave temporary relief from distressing symptoms; but that is all. In the case of H. P. (in which, however, the effusion was at no time quite so great as in that of R. L.), a few days more of patient endurance, with the continued employment of remedies, led to precisely the same result; while the removal of the fluid was certainly, to say the least, quite as rapid in the case not operated upon as in the other. On the whole, the case of H. P. (the one not operated upon), undoubtedly presented fully the more rapid convalescence of the two, while in hospital. On the other hand, I am not sure if H. P. kept his advantage over R. L. I suspect he did not take care of himself, for the only time he returned to the ward, he was tipsy, and there are questionable accounts of him. R. L. made a most excellent recovery, and on his coming to see me in the summer of 1860, a full year after the operation, it was hardly possible to distinguish the side that had been affected, except by a little comparative deficiency of movement; the respiratory murmur was quite natural, and the health and appearance were excellent.*

* Later accounts (May 1862) are not quite so good. I heard the other day from his former employer to the following effect:—"R. L. left me to go and carry a pack in Lancashire a year ago. He was then strong and fat. One of my men heard from him six months ago, when he complained that his new calling was too severe for him, and that he feared he would have to give it up. This is the last that we have heard of, or from him." I cannot but remark here upon the unavoidable deficiencies in the histories of disease, as recorded in hospitals, from the want of a sufficiently extended period of observation of the individual patient. The continued narrative of hospital "cures," in particular, would often form a most instructive chapter in the history of chronic

While some of these cases were under treatment, I had the pleasure of a visit from Dr. Bowditch of Boston. Dr. Bowditch is not only one of the most distinguished physicians of Massachusetts, but he has devoted more attention than any other American authority to this subject; and it was therefore with peculiar interest that I submitted to him my doubts and difficulties with regard to thoracentesis, in connection with the cases of R. L. and H. P. I believe I do not misrepresent his opinions, when I ascribe to him a certain amount of reserve in the performance of the operation in acute cases, as compared with M. Trousseau in a certain period of his practice; that is to say, Dr. Bowditch operates chiefly in cases of considerable distension, and to relieve very urgent symptoms; and only, except when hard pressed, in cases in which internal remedies have been first em-

disease. I have endeavoured to work out this aspect of the subject as faithfully as was possible in the circumstances, but it is greatly to be desired that some of our older practitioners, who have really outlived and carefully watched the varying phases of intermittent structural disease, would give us more of the results of their life-long experience.

This case of R. L. has a remarkable resemblance, as regards some of the details, to M. Trousseau's *first* case of cure by paracentesis (*Clinique Medicale*, vol. i., p. 625) in which 800 grammes only, or about 26 ounces, of fluid were withdrawn. I think there must be a degree of unconscious inaccuracy in the unqualified statement in p. 626, that "the organs had resumed their place, and the respiratory murmur had re-appeared," after the operation. It is physically impossible that this can have been true to the full extent suggested, considering the small amount of fluid removed. The ultimate cure was (as in the case of R. L.) due to diuretics, and the question, therefore, cannot fail to arise, why were these remedies not employed also *before* the operation, instead of bleeding, calomel, and a "large blister on the back of the chest?"

ployed. But a far greater distinction between Dr. Bowditch and the other advocates of thoracentesis, consists in the mode in which he performs the operation. Thoroughly appreciating the real difficulties which I have attempted to point out above, Dr. Bowditch boldly solves them by the use of an exhausting syringe in all cases; and by the peculiar construction of his instrument (the invention of Dr. Morrill Wyman of Cambridge, U.S.) he is enabled at once to evacuate the chest much more completely than by any other method, and to prevent entirely (and with absolute certainty) the admission of air. Had I been fully aware of the peculiarities of this operation, and of the great success which it has had in the hands of Dr. Bowditch in upwards of 120 cases, I should certainly have employed it in the case of R. L., if not also in that of H. P. It appears to me to be in every respect an improvement so important, that it may be said to open up a new history for the operation of paracentesis thoracis; and I trust it will henceforth receive in this country the attention which is due to it. For the description of the instrument employed by Dr. Bowditch, I may refer the reader to his original communication on the subject to the Society of Medical Observation in Boston.*

* *American Journal of Medical Sciences*, vol. xx., October 1850, p. 325. Since the remarks in the text were published in October 1859, I believe that most of the operations on pleuritic effusions in the Royal Infirmary have been performed with the aid of this instrument, which I have had manufactured by Messrs. Kemp and Co., 12 and 13 Infirmary Street, Edinburgh. I can now, therefore, bear ample testimony to the fact, that it has fulfilled my expectation of rendering the operation of thoracentesis both more secure from risk, and more satisfactory as regards the result.

XV.

PNEUMOTHORAX.

In the course of hospital and private practice I have seen numerous cases of pneumothorax, both from the bursting of a pulmonary cavity during inspiration, and from the opening of a pleural effusion into the lung. None of these hospital cases, however, though often very full of instruction, forms nearly so good a basis for the few remarks I have to make upon this subject, as the following example, by my friend Dr. Thorburn of Manchester, of a pneumothorax ending in recovery, with a very remarkable absence of bad symptoms. This case was brought under my notice, though at a comparatively late stage of its progress, and in the curiously exceptional character of its phenomena, as reported, it might fail to secure the attention it so well deserves, as a simple record of facts. I shall, therefore, take the liberty of reproducing it here from Dr. Thorburn's very careful report,* in order to add to it briefly what my own experience suggests as bringing it into harmony with other facts, and with the doctrine commonly

* *British Medical Journal*, June 2, 1860. The case was read to the Medical Section of the Royal Manchester Institution.

taught in works of reference. I shall have occasion to refer briefly to numerous cases of pneumothorax, reserving, however, for the next article, a very remarkable example of metallic transmission of the cardiac sounds through an air-filled cavity, in pneumothorax from empyema.

"Mr. J. C., aged 22, was in perfect health up to the date of the present attack; that is to say, he was not himself conscious of any ailment, bodily weakness, or wasting; he had had no cough, hæmoptysis, pain, or other recognizable symptom of internal disease, and, being an intimate personal acquaintance of my own, I had every reason to consider him up to that time as of sound constitution. My reason for thus insisting on this point must be evident. His only previous illness had been a severe fever about ten years ago. He was on the eve of a voyage to America, and had consequently for some days had rather more exertion than usual. He had also, three days previously, indulged freely in rowing, to which he was not recently accustomed, though in former years he had been; he felt, however, little or no inconvenience at the time.

"*July* 19, 1859.—Early in the morning he was awakened from a sound sleep by a sharp pain in the right side, accompanied by dyspnœa. It continued thus for two or three hours, and then, becoming less severe, he rose, dressed, and went to business in the city. In the afternoon he first called upon me, and stated that he still felt considerable pain in the right breast and shoulder. It was experienced chiefly on deep inspiration. He had no cough, nor, when sitting in my consulting-room, any apparent dyspnœa, though he had rather a feeling of breathlessness. The pulse was quiet and regular (76). The skin was cool and moist; the tongue very slightly furred. As there seemed to be no evidence whatever of any acute inflammatory disease, I merely auscultated the top of the chest anteriorly. The percussion was fair on both

sides; the respiration rather weak on the right, but without other alterations of quality or rhythm. Everything pointed to a simple attack of pleurodynia, consequent perhaps on the heat and exercise of rowing three days before. I prescribed a saline aperient, and a sinapism to the seat of pain.

"*July* 20.—He called again, and expressed himself as a good deal better. The pain had moved about, and was now felt somewhat on the left side.

"*July* 21.—I called on him in the morning, and found him almost quite free from pain, and considering himself well again. He had just come down stairs, however, and his breathing seemed to me to be much too hurried. Bearing in mind the comparatively feeble respiration at the right apex, which might possibly be of tubercular origin, though a pleurodynic pain in the side could equally account for it, and knowing of his intended voyage, I desired him to allow me carefully to examine his chest. On doing so, I found to my intense surprise the following conditions. The left side was perfectly normal, except that the respiratory sounds were rather exaggerated. On the right side the percussion-sound in front was at the apex comparatively, though not to a marked degree, more resonant; lower down, it was very evidently so, and the increased resonance extended quite to the border of the false ribs. Behind and laterally the percussion-note was quite tympanitic, and the tympanicity completely obscured the natural dulness of the hepatic region. On auscultation, the vesicular breathing was completely lost over the whole side, and was replaced at the apex by a clear hollow tone, with an equality of the inspiratory and expiratory periods; lower down, it gradually merged into a clear amphoric ring. The same loud amphoric sound was evident over the whole back and side. In addition there was audible with almost every inspiration a clear ringing click, as of water dropping into a deep well—the most beautifully marked *tintement métallique* imaginable. The impulse of the heart was also occasionally accompanied by the same sound, audible chiefly at the right back. The vocal resonance was perfectly amphoric, both spoken and whispering. Change

of posture made no difference either in the percussion or auscultation. It was clear that he was labouring under complete pneumo-thorax of the right side, although, nevertheless, the pulse was not above 76, either now or at any other time; the tongue was clean and moist; the skin cool; there was no apparent dyspnœa when in bed; not a trace of cough; and hardly the slightest personal discomfort. I at once advised a second opinion, and Sir James Bardsley saw him with me in the afternoon. In spite of his long experience, he was equally astonished with myself at the great clearness of the physical signs and the total absence of concomitant bad symptoms.

"The subsequent history may be given very briefly. Any active treatment was out of the question; and the only and self-evident measure consisted in preventing all unnecessary action of the part. This was done, and slight counter-irritation was applied by means of iodine.

"For a day or two the signs continued much the same, and the chest became a little bulged, as if the accumulation of air in the pleural cavity was increasing. In ten days, however, the metallic tinkling had quite disappeared, the percussion-note also becoming less tympanitic gradually, commencing *from above*. The respiration and vocal resonance became *pari passu* less amphoric. He had never during this time one bad general symptom, and I may state again, that he was not aware of having once coughed during his whole illness.

"*August* 31.—It was deemed advisable for him to go down to Scotland, having been going about a little for a fortnight without injury. At this time the tympanitic percussion was quite confined to the lower and lateral part of the side. The rest was still rather comparatively over-resonant, and the breathing was nowhere yet quite normal. At some points it was faint and distant, though vesicular; at others it had still rather a blowing character. He came now under Dr. W. T. Gairdner's care, at Edinburgh, who kindly informed me from time to time of his straightforward progress.

By "*Nov.* 8.—Dr. Gairdner became convinced that the lung was almost everywhere again in contact with the chest-wall, except

perhaps at the extreme base ; and that, in spite of the improbability, there was no perceptible evidence of tubercle whatsoever.

"*Jan.* 20, 1860.—He had come back to Manchester, and I examined him very carefully. I must say that, if unaware of his previous history, I should have set his chest down as a very healthy one, rather better than the average. As it is, I can discern nothing more than a slight faintness of respiration at the base, and a rather too low extension of the thoracic resonance when in the recumbent position. He is strong and well, has gained flesh, can go through a long day's shooting without resting or fatigue, and may therefore, I think, safely be set down as perfectly recovered."

The commentary of Dr. Thorburn on this most interesting case is, I believe, quite in accordance with the existing state of professional doctrine when he records it as probably a unique example of pneumothorax, occurring in a previously healthy man, running its course without fever, as well as with singularly little pain or dyspnœa, and ending in a recovery virtually complete. And although another somewhat similar observation is briefly referred to in the paper, not only is the latter case much less complete as a record of facts than that of Dr. Thorburn, but the facts, as recorded, are every way much less remarkable. I can personally bear witness at least to the highly *probable* accuracy of Dr. Thorburn's diagnosis; for although the metallic phenomena, which alone could have given me the absolute personal assurance of pneumothorax, had disappeared before I saw the patient, there remained a state of the right lung which could hardly have had any other source than either air or fluid compressing it for a time, and afterwards gradually re-

moved, or in process of removal. The following extracts from my notes made at the time will, I think, tend to confirm Dr. Thorburn's opinion of the case, as above stated :—

Mr. C., from Manchester, consulted me on 7th October 1859. Patient has no cough, nor spit, nor has he ever had. He is a very tall, rather pale young man, with signs of some degree of emaciation, but apparently always of spare habit. Has no fever, and is able to walk about. Under medical directions, has been exceedingly chary of exertion in walking, but has ridden considerable distances without fatigue. He is, however, conscious of breathlessness, and of impaired power. No pain now, but had at one time pains in the chest generally, and more especially on the right side.

The physical signs are those of impaired action of the right lung ; they are present in the most marked form below. The respiratory murmur, however, is nowhere absolutely suppressed, and, except in the extreme base and lateral region, seems pretty close to the ear. No metallic phenomena. Vocal resonance is rather stronger generally on right than on left side, especially in the upper part, over the scapular spine ; it is perhaps feebler below than on right, but this is doubtful. No change in quality of vocal resonance. No râles of any kind.

November 30. — Re-examined after a residence in the country, during which he has been pheasant-shooting, and accustoming himself gradually to undergo considerable exertion. Pains have altogether disappeared ; and breathlessness, except on going up a very steep hill, is hardly perceptible. No cough nor spit. There is still a deficiency in the breath-sound of the right side of chest, from third rib in front downwards, and in the entire lateral region ; in the latter, fully less breath-sound now than in the back. Strong inspiration, however, brings the breath-sound more or less into every part of the lung, even to the very lowest part behind. In apices, breath-sound natural in quantity, and free of all râle. Vocal resonance as formerly.

No history of phthisis, hereditary or other. Patient has gained 8 lbs. in weight; and is evidently stronger and improved in complexion.

January 6, 1860—Still keeps well, and is going home.

The acute and terrible symptoms so characteristic of pneumothorax in typical cases, are generally admitted to be occasionally absent individually, or at least not of diagnostic value absolutely. Thus, pain may be only moderate in amount, or not characteristic; dyspnœa may be merged in that of a more chronic affection of the chest; fever may hardly be observed amid the hectic of phthisis, or of empyema; while the more special sensation of rupture alluded to by some authorities, and the suddenly-felt rush of fluid or cold air into the chest, as described by others, are certainly quite as often absent as not. Dr. Stokes has given an admirably descriminating and appreciative view of the symptoms of pneumothorax, which, though written more than a quarter of a century ago, can hardly be improved upon even at the present day. Dr. Walshe reduces the typical symptoms to three, viz., sharp, often agonizing pain; sensation of rupture; and dyspnœa. He adds, "the second is habitually wanting; and I have known perforation occur, as proved by physical signs and inspection after death, without any one of the three announcing its occurrence." The observations of Laennec and Louis are well known, and require no remark. Skoda has added to our knowledge of the physical signs, but not of the symptoms, of perforation. Dr. Walshe records the very remarkable case of a dragoon, who, in the third

stage of phthisis, and after the occurrence of pneumothorax, marked only by "slight pain below the right nipple of some hours' duration," * * * "had walked without difficulty upwards of a mile to the hospital, and yet the right chest was so full of air as to have pushed the heart to the left axilla." * Dr. Stokes has "known several instances where the first symptoms (of pneumothorax in phthisis) were not more violent than what we often see in phthisis from a new attack of irritation;" and says in general terms that "the disease may set in with violent symptoms, or be so latent that we cannot determine the date of its invasion." † Instances of this *practical latency* (so to speak) of pneumothorax, amid the sufferings caused by another disease, are so common as to be quite well known to every hospital physician of some experience. Several such instances have occurred to myself. One case, in particular, recurs to my memory in writing these sentences; it is distinguished from others, because the absence of a well-marked period of invasion became, at the time, the subject of a most critical inquiry, on account of a very puzzling diagnosis, there being evidently great disease of the chest, and also other symptoms of much interest. The patient was a young girl (Elizabeth A., aet. 19), admitted to the Infirmary in the winter of 1859-60. She was in a state of great febrile exhaustion, and the history

Enteric Fever. Acute Tubercle. Pneumothorax.

* Walshe, Diseases of Lungs, third edition, p. 310. The probability, however, here, I think is, that the pneumothorax may have had an older date, and may have escaped detection at the time.

† Stokes, Diseases of Lungs, pp. 527, 530.

bore that she had passed, about six or eight weeks before, through an attack of what was called "gastric fever," latterly complicated with chest affection. The signs, as detected on admission, were those of metallic cavernous râles about two inches below the left clavicle ; but from her exhausted state it was not considered expedient to examine the back. A most minute scrutiny was made of every detail of the history, owing to the entire uncertainty in which I believed the diagnosis to be involved as regards the alleged attack of "gastric fever." It was quite certain, however, that she had become feverish, and had passed through a lingering attack of some febrile disorder attended with diarrhœa, from which she had become partially convalescent, and then apparently relapsed into acute fever, attended with pulmonary affection ; but neither the patient herself nor her nearest relatives could assign any precise date to the relapse, or even could be quite sure that the disease was not one and undivided from first to last. She had been getting better, and got worse again ; that was all. Everything like acute pain or fever was merged in the symptoms of a rapidly progressive tubercular disease, and even the dyspnœa, with the sense of oppression and general suffering, were not greater than is often witnessed in cases of more than usually acute phthisis. It was far from clear to me, during the life of the patient, that there had been any separate attack of "fever," in the ordinary sense of the term ; but after communicating with the previous medical attendant, I became satisfied that most probably it was so, and that a tubercular disease, either absent or

little noticed before, had been developed with extraordinary rapidity immediately after an attack of enteric fever. On this theory the case was treated in the hospital; and I must confess that the pneumothorax remained undetected to the last, the metallic râles being supposed to be the result of a cavity *within* the lung, instead of *external* to it, and the persistence of some respiratory sound in the upper part of the lung, with bubbling râles, tending to keep up the illusion; while the back and side, not being examined, failed to afford the necessary key to the truth. The left lung was found, after death, adherent all over the apex, but containing a good deal of very recent yellow tubercle, partially softened; the pleura was perforated in two distinct places towards the upper and middle part of the lung, and both openings had been subsequently sealed up by very soft lymph. There was about a pint of sero-purulent fluid in the left pleura, and it was so far distended with air as to depress the diaphragm and left lobe of the liver a good deal, but not to give the sensation of extreme tension. The cicatrices of several intestinal ulcers in the ileum appeared both to Dr. Haldane and myself to correspond exactly with the theory of enteric fever at the date assigned, or even further back; and there were also traces of enlargement of the mesenteric glands. Even after these circumstances were known, it was found impossible to elicit from the father and mother of the patient, who had carefully watched her throughout her illness, any fact tending to mark precisely the date of the perforation of the lung.

In additional evidence of what I have called the *practical latency* of pneumothorax in some cases, I may state (what I have no doubt concurs with the experience of others) that I have been repeatedly called in consultation, in cases of phthisis most carefully watched, on account of aggravations which have not been supposed to be in the least degree out of the ordinary course of the disease, and have found the pleura of one side more or less full of air. In one such case, which I saw with one of the most entirely trustworthy and habitually careful practitioners in Edinburgh, it was utterly impossible to assign a date to the pneumothorax, which, nevertheless, had attained such proportions, that within two days I was led to puncture the side with an exploring trocar, and thus to give issue to a large quantity of air, with great, but of course only temporary, relief. On the other hand, I feel satisfied that in my own hospital practice similar accidents have occurred more than once; pneumothorax being detected evidently at a period more or less remote from its actual occurrence, and when the symptoms had not led to any clear inference that the more ordinary course of phthisis had been departed from.

Latent Pneumothorax in Phthisis.

One case is very remarkable, and though not precisely in the position now suggested, I will mention it here, as illustrating the general doctrine of pneumothorax and its symptomatic history.

In November 1858, a very respectable and intelligent married woman, Mrs. C., was admitted into the Royal Infirmary, being, at the time of admission, in the 7th month

of pregnancy, and complaining of cough, with expectoration, accompanied by rather urgent vomiting. The pulse was 118, the respiration considerably quickened, but not very laborious. The tongue had a slight yellowish fur, the patient sat up in bed, or lay on the left side, and there was a history of an acute attack of pain ten days before, while cough and some of the general symptoms of phthisis had attended the whole duration of the pregnancy, and had even preceded it. On examination, there were found signs of pneumothorax over the lower lobe of the left lung, viz., marked deficiency of respiratory murmur, without dull percussion, and with metallic phenomena and even hippocratic succussion-sound in a slight degree. The upper lobe of the left lung was evidently adherent all over, dull to percussion, but receiving a certain amount of air in inspiration. The opposite lung presented slighter indications of tubercular disease. It was in many respects a typical case of tubercular pneumothorax from perforation ; and the question only remained if anything could be done to save the life of the patient, or if not her life, that of her infant? I confess it hardly occurred to me as possible that she could survive long enough to bring her child nearly to the full term ; and as it appeared very improbable that she could live to see it born, even supposing her delivery to occur prematurely, I was at this time chiefly intent on prolonging life for a week or two, in the belief that abortion would probably occur spontaneously, when it might become desirable, both for the sake of mother and child, to

Pregnancy, 7th month.
Tubercular disease.
Pneumothorax.

Prognosis.
Practical questions.

hasten the delivery by turning, or otherwise. Directions were also given as to what was to be done, in the event of a sudden fatal event to the mother, with a view to the preservation of the life of the child, if that should appear to be still possible. Meantime, and rather as a forlorn hope than with any well-founded expectation of ultimate success, the administration of opium in pretty large doses was resolved upon, to relieve the vomiting and prostration, which were by far the most urgent symptoms in the case. On the 10th December, I had the pleasure of referring to the case of Mrs. C. in a clinical lecture, as one of a series of examples then under observation, of the very beneficial effects of opium in relieving suffering, and prolonging life. "In Mrs. C.'s case, the patient was suffering very intensely, on admission, from constant sickness, vomiting, and prostration, connected with pneumothorax occurring in the 7th month of pregnancy. A pill, containing gr.j of opium, was given every second hour; after taking eight pills the pain had entirely gone, and the extreme sickness and prostration were also in great part gone. The dose of opium was therefore gradually diminished, but we had to resume it afterwards in the high dose again, on account of a renewal of the pain, when a like good result again followed."* I now watched anxiously for indications of resolution of the pneumothorax; but although the metallic phenomena and the hippocratic succussion-sound became indistinct, and at last ceased to be heard, there

Opium.

* From brief but accurate notes of lectures in the Session 1858-9, by Mr. John Lowe.

was but little sound of breathing to replace them. The patient, however, bore up well, and on the 14th of January I had to report her somewhat premature delivery, the child being born alive, though puny and weak. The delivery was rapid, and did not in any respect differ from a normal labour in a woman who had borne many children. As too often happens, it was followed by a considerable increase of the symptoms of tubercular disease; the mother was quite unable to suckle her child, and gradually became exhausted; the infant, too, though fed with all possible care, and exciting great interest in the ward, became feebler and feebler; and at last, exactly six weeks after the delivery, the mother and the child breathed their last on the same day. A dissection was not allowed.

Delivery.

Death six weeks after delivery.

There may be other cases more or less similar to this in the voluminous annals of medicine or of midwifery, but I have not happened to meet with them. I have thought it right, therefore, to place the present instance on record, more especially as it seems not unlikely that similar cases may occasionally present themselves without their true character being detected. This woman certainly survived an attack of pneumothorax, occurring during the later months of pregnancy, long enough to give birth to a living child without accident, though the pneumothorax was probably only partially removed; and there seems to have been no reason, except the gradual advance of the tubercular disease, to prevent her ultimate recovery from this unusual and apparently most hope-

less complication. I think there can hardly be a doubt that, as a matter of doctrine, we must assume that a smaller amount of pneumothorax (other things being equal), will prove fatal to a pregnant woman, than to a patient under other circumstances; and the simple fact of a patient so affected having passed safely through the usual period of child-bed, without *special* risks due to the perforation of the pleura (as distinguished from the tubercular disease), appears to have an important bearing, both on the history and general doctrine of pneumothorax, and on that of the puerperal state. This latter part of the subject, however, though of great practical importance, I must leave in the hands of professed obstetricians.

As regards the prognosis of pneumothorax in general, I am hardly yet prepared to give complete and substantial proofs of the views which I shall now briefly put forward, and which have been for years assuming form and character in my own mind, on the basis of a large number of separate facts observed both during life and after death. The conclusion to which I have been gradually and almost insensibly led is, that while pneumothorax affecting the whole, or nearly the whole, of one side in tubercular disease, is usually fatal after a period varying from minutes to weeks, *limited* pneumothorax is both more common, and less fatal, than is represented in the ordinary doctrine of authorities on the subject. This is true, I believe, even of tubercular pneumothorax, but true also, and in a less restricted sense, of those forms of perforation of the lung (rare indeed, but

by no means so rare as is commonly stated) which are non-tubercular, and end in recovery ; of which, I believe, I have seen a number of instances, although Dr. Thorburn's is the only instance which appears to me sufficiently complete in its evidence to be here adduced in detail. What happens, I believe, in cases like Dr. Thorburn's is this :—The lung, previously affected or not with tubercle, emphysema, or other form of lesion, weakening, at points, its power of resistance, gives way by a mere *pin-hole* perforation ; through which, however, the pleura would rapidly enough become quite distended with air, were it not for the pre-existence in some cases, and the very rapid formation in others, of pleuritic adhesions sufficient to limit the consequences to a part only of the cavity. In most cases, too, there occurs a new pleuritic attack, the result of the pneumothorax ; by which the opening, when small, is often sealed up long before the pleura has become full of air to a degree productive of the most extreme form of suffering from dyspnœa. In some instances, again, and especially in those of tubercular cavities proceeding to perforation, I have seen reason to believe that a little air may gain the cavity of the pleura by transudation, without an actual perforation ; the serous membrane having its vitality weakened so as to exercise little more control than a dead tissue over this mechanical transudation. I base this inference chiefly on a single instance, in which it appeared to me evident that a small quantity of air had so escaped ; the case being one of tubercular disease, in which pneumothorax was not suspected during life, and

death seemed to have caught the disease *in the very act of perforation*, as it were by a mere coincidence of time. I am fully aware that no amount of evidence will convey to any other person, under these circumstances, the conviction that occupied my own mind—1*st*, That there actually was a *small* amount of air in the pleura (a thing extremely difficult to *prove*, except by opening the body under water, and perhaps not even thus); and, 2*dly*, That the air was there during life. I shall content myself, therefore, with simply stating, that such was my belief at the time, and continues to be my belief now, after consideration of the whole circumstances of the case. In other instances, I have seen two and even three successive perforations in the same lung, the last of which only has proved fatal; all the others having been sealed up, shortly before the death of the patient, by the effusion of soft lymph; surely a most noteworthy illustration of the *vis medicatrix naturæ*, which in many or most of these cases has evidently acted by exciting just enough of pleurisy to cause adhesions, and to obliterate the traces of perforation; in other words, at once to limit the consequences, and to shut off the sources, of the extravasated air.

I am led to regard pleurisy, then, in its relation to pneumothorax, as (in by far the greater number of cases) *not a fatal complication, but a healing power.* And the true value and importance of this doctrine, in its relations to pneumothorax in general, can only be estimated by considering the ordinary course and progress of the disease that most commonly gives rise to pneumothorax

—viz., tubercular phthisis. In the course of pulmonary tubercle, we constantly, it may be almost said normally, see perforation of the pleura anticipated, and only just anticipated, by the formation of adhesions over the tuberculated portion of the lung. It is a curious and

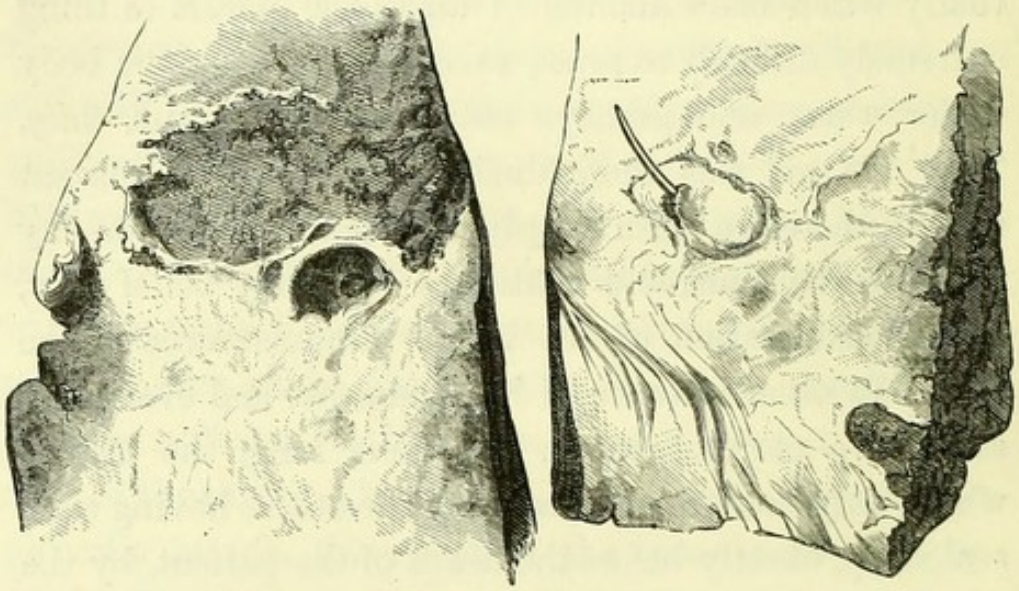

Fig. 7.

A perforation of the pulmonary pleura in phthisis, anticipated by adhesions, as described in the text. There is a large excavation in the upper part of the lung, communicating with the surface through a comparatively narrow sinus, distinctly seen in the woodcut. The adhesions have been torn through on removing the lung. From a preparation.

Fig. 8.

Perforation partially sealed up by soft lymph, after the occurrence of pneumothorax. A probe is seen passed through an accidental rupture in the veil of lymph covering the perforation, the margins of which can be distinctly traced by the rough granulations thrown out in their neighbourhood. From a preparation.

beautifully conservative arrangement, that in these cases the pleurisy external to the lung usually keeps pace pretty accurately with the tubercle within; or rather, that the pleuritic adhesions are often in advance of the actual deposit of tubercle near the surface; and still more, of course, in advance of its softening. Were this

not so, it could hardly be otherwise than extremely common in phthisis pulmonalis for the patient to perish from perforation of the lung. A disease which consists essentially in the formation of a multitude of abscesses in all parts of the lungs could have hardly any other termination, were it not for those little attacks of adhesive pleurisy, so well known to the pathologist as forming a hardly less essential part of the disease than the tubercles themselves. These "little pleurisies" no doubt contribute largely to the symptomatology of pulmonary phthisis by causing the well-known "flying pains" about the shoulders and arm-pits, and below the clavicles, which patients occasionally insist upon more than the really formidable symptoms of the disease. They are also of no small importance in relation to the physical diagnosis, and are, I believe, often the source of râles, and of irregularities in the respiratory murmur, which are described and commonly thought of only as connected with the tubercular deposit.* But by far the greatest importance of these minor pleurisies is in relation to prognosis; for it is not too much to say that, without them, phthisis could hardly ever pass into the third stage, or perhaps even beyond the first, without destroying life.

All this will be readily admitted, probably, by some who will still feel more than doubtful as to the function of pleurisy in limiting the consequences of pneumothorax, after it has already taken place. But I must here observe that, according to the view I am endeavouring

* See the cases of Peter B. and of Mr. A. B. A. in Art. XIV. pp. 306, 341.

to illustrate as, at least, probable, the perforation and the pleurisy which is commonly connected with it are not to be regarded simply as cause and effect, but rather as co-ordinate and simultaneous results of a common cause. And this relation holds good as respects the result, even when, from some accident, a rupture of the lung has taken place before adhesions have fully prepared the way for it. The adhesions in such cases may not have been formed immediately over the point of rupture; but there have been adhesions forming, nevertheless, over some part of the lung not far off, and these adhesions tend to limit the escape of air, and thus to prevent the utter collapse, and consequent destruction of function, of the entire lung. In the case of Mrs. C., for instance, the whole upper lobe was the seat of such limiting adhesions; the perforation having taken place, probably, only a little way beyond the limit within which it would have been absolutely secured by the adhesions from all immediate bad consequences whatsoever. In the case of Elizabeth A., the upper lobe was also maintained by adhesions in a certain degree of activity; although, from the deposit of tubercle there, it was very unfit for the performance of function. And in many other instances I have seen illustrations of the same facts. It has even occurred to me, though much more rarely, to observe a portion of the lower lobe maintained in contact with the thoracic wall, while the upper lobe was collapsed from perforation; and in one case in which the lung was partially adherent above and below, the middle part of the organ was connected with the lateral wall by a long,

tense, round ligament of adhesion, about the thickness of a moderate sized goose quill, broadening out at both ends. From the constant action of the thoracic expansion upon this curious ligament, probably during many months together, it had assumed all the properties, and much of the structure, of an elastic texture ; closely resembling, indeed, in its quite remarkable elasticity, a piece of india-rubber.

The preceding remarks apply chiefly to cases of pneumothorax fully known to be such. Rejecting all doubtful clinical evidence, I have hitherto been content to infer the general laws of the disease from a somewhat extended view of its phenomena as observed in the dead body. But it is evident that the attempt to demonstrate the healing powers of nature in any disease from observation of its pathological anatomy, must always be made at a disadvantage. The fatal facts predominate immensely over the more favourable instances, in virtue of the very conditions of the inquiry ; unless, indeed, the disease be of such a kind as to leave behind it, after complete cure, unmistakeable traces of its former presence. And in the case of pneumothorax, no such traces have ever been demonstrated ; nor can they be even supposed possible, from the nature of the lesion.

General theory of the cure of Pneumothorax.

We are driven back, then, for the most part, upon purely clinical observation for the further evidence that may exist of cured pneumothorax. In pathological anatomy we see, as might be expected, only too many instances of its more extreme and fatal results ; but I trust I have shewn

also, that we catch occasional glimpses of a more favourable issue. For I have endeavoured to prove, 1*st*, That pleuritic adhesions, in the vast majority of cases, anticipate or prevent the occurrence of rupture of the pleura, where pneumothorax would otherwise be a common or almost certain occurrence; 2*dly*, That where adhesions fail to prevent altogether the escape of air, they often limit the space within which it is extravasated; 3*dly*, That pleurisy coinciding with, or closely following pneumothorax, is not to be viewed as a wholly destructive agency, inasmuch as it tends to the still further limitation of the mischief in many cases, by the rapid sealing up of small perforations, and the formation of new adhesions. I am much inclined to believe, moreover (though it is, of course, difficult to prove), that pneumothorax is fully more often fatal from the absence of inflammation, or the delayed occurrence of the inflammation which normally may be said to coincide with the rupture, than from the gravity of the pleurisy, considered merely as a complication. I have, in fact, seen one case of pneumothorax,* probably at least of several weeks' standing, in which there was absolutely no vestige of recent inflammation of the pleura, but in which, nevertheless, the air had accumulated to such an extent as to overcome the resistance of somewhat firm adhesions evidently of older date, in the lateral region; these were found stretched, in the form of organized fibrous bands, to the

Pneumothorax without Pleurisy.

* Case of William B., æt 21; died 27th March 1855. Report of examination after death in Dr. Haldane's Register, vol. xvii., No. 18.

length of about two inches, somewhat as in another very chronic case of pneumothorax, mentioned above. But what is perhaps most curious of all in this instance, is, that although there was not pleurisy enough to form a serious complication, there was yet sufficiently ample evidence of the *vis medicatrix naturæ*; for not only had the upper part of the lung become adherent, so that it followed the movement of the chest to a certain extent; but the perforation, "three lines in length by a line and a half in breadth," had become completely sealed up by "softish yellow lymph," apparently at no long period before the death of the patient; and accordingly, when Dr. Haldane inflated the lung, having previously filled the side of the chest affected with water, in the first stage of the dissection, "not a bubble of air escaped" through the fluid. In this case, moreover, I think we must admit that the *vis medicatrix* failed chiefly or entirely through the insufficiency of the pleurisy on the perforated side; for, on the opposite side, as often happens in cases of fatal pneumothorax, there was a much more abundant deposit of tubercle than on the perforated side; accompanied by general adhesions, and considerable thickening of the pleura.

This case is certainly unique, within my experience, as regards the entire absence of evidence of pleurisy simultaneous with perforation; but I have seen several others in which the amount of pleurisy in connection with pneumothorax appeared to have been rather within, than beyond, the bounds of what was requisite for the cure; in particular, one case of

rather acute tubercular disease, which I saw in consultation with the late Dr. James Balfour, several years ago. The patient was a young man of temperate habits, and who gave a perfectly intelligent account of his symptoms throughout the disease. He had suffered much from breathlessness, and from flying pains, but not much more than is quite usual in this type of phthisis. On several occasions, in examining the left side of the chest, we were struck with the local feebleness of the respiratory murmur, and on at least two occasions I detected perfectly unequivocal metallic phenomena, in connection with râles which could hardly be strictly defined, but did not seem to resemble the ordinary cavernous râles of phthisis. On examination after death, we found in the left pleura pretty firm though very partial adhesions; *but absolutely no recent lymph or recent adhesions, and no visible perforation.* In two places, where the adhesions were deficient over a space of some inches, there was a distinct interval between the costal and pulmonary pleuræ containing air, though by no means distended with air. The utmost care was taken, in separating the adhesions with the finger, to verify these facts in such a way as to preclude fallacy; for the metallic phenomena demanded explanation, and I had formed during life the impression that there were no considerable cavities within the lung, as was indeed found to be the case. I think it impossible to avoid the conclusion, in this case, that there had at one time been a perforation, which had become subsequently involved in, and

Tubercular Pneumothorax in process of cure.

sealed up by, adhesions ; the progress of the case being very plainly towards cure, in so far as the pneumothorax was concerned, by the absorption of the air ; the cure being arrested, however, by the advance of the tubercle in the lungs to a fatal issue.

What else I know of cured pneumothorax must be told subject to the doubts that rest upon all obscure clinical facts, not elucidated by *post-mortem* investigation. But I trust the preceding statements will give an aspect of credibility, if not of probability, to the following speculations. No one, indeed, can be more sensible of the deficiencies of the evidence than I am ; for it is extremely difficult to say precisely what, in the present state of science, constitutes complete evidence, in a clinical sense, of the existence of air in the cavity of the pleura. The combination of *very marked* deficiency of respiratory murmur, with *very marked* amphoric respiration or metallic tinkling, would probably be accepted by the most sceptical critic ; but then these phenomena can only be had where the pneumothorax is very extensive, and sometimes not even then. Dr. Thorburn's case is, as I have said, unique in respect of the distinctness of the clinical facts, unless it be matched with a case recorded long ago by Dr. Graves,* but afterwards involved in doubt, from the objections raised by Dr. Williams to the theory of diagnosis of the Irish physician. There can hardly be a difference of opinion now-a-days as to the insufficiency

Clinical evidence necessarily defective.

* Clinical Lectures, second edition, vol. ii., p. 72. Case seen with Dr. Dwyer.

of the evidence of pneumothorax in most of Dr. Graves' cases; and it is perhaps no less difficult to accept his theory of the simple secretion of air into the pleural sac; yet I am much disposed to think that this one case, at least, is safe from criticism as to the diagnosis, in respect of the evidence stated to exist of displacement of the heart quite to the right nipple, and its recovery afterwards of its normal position. As there is no mention of metallic phenomena, however, and as the case is somewhat imperfectly related, considering its vast importance, it may be, on the whole, safer to consider it as being not quite conclusive.

The difficulty, however, of the inquiry is this :—Pneumothorax of such an extent as to displace the mediastinum or diaphragm, and to cause the most characteristic type of metallic tinkling, or of amphoric respiration, rarely occurs, I believe, except in conjunction with tubercular disease;* in which combination it is only too often fatal, more or less directly. Pneumothorax followed by recovery, again, is usually much more limited in extent, and is more commonly, though I believe, not always, found apart from tubercular disease; still, as it depends upon,

* I have, nevertheless, seen one well-marked example of fatal and general pneumothorax, without tubercular disease, in a young girl (Isabella G., æt. 8), who died of typhoid fever under my care in the beginning of January 1857. I was fully persuaded she must have died of internal hemorrhage; but a hurried *post-mortem* examination performed by Dr. Spasshatt, then my resident physician, disclosed a number of very minute bronchial abscesses, one of which had perforated the lung. Both Dr. Haldane, who saw the lungs afterwards, and Dr. Spasshatt, were satisfied of their perfect freedom from tubercular disease.

and is associated with, *some* disease of the lung, we can hardly ever feel altogether persuaded—1st, that the evidence of cavity exists at all ; and 2d, that the cavity is in reality external to the lung. I have seen numerous cases of what I am disposed to believe may be examples of cured pneumothorax ; but few of them, I must admit, would be proof against objections or doubts raised on these grounds. For this reason, also, I am unable to say how many cases of this kind have come under my notice ; all I shall venture to assert is, that in at least six or seven cases I have witnessed phenomena which, with a full knowledge of all the objections that may be raised, I am disposed to consider as really instances of cured pneumothorax.

One instance will illustrate the whole series. A medical student was seized with symptoms of what would naturally enough be called pneumonia, or pleurisy—viz., pain in the left side, cough, feverishness, and a certain amount of difficulty of breathing, but with very little expectoration. On examination, there was found at the lowest point of the left lung, behind, the most extreme faintness of the respiratory murmur, with *obscurely* metallic character ; no râle, unless the most insignificant form of crepitus, and absolutely no dulness on percussion from first to last ; the percussion, indeed, tympanitic in quality nearly throughout the disease. Now, every one will admit, that if these phenomena had been more diffused, *and if the patient had died*, they would have indicated pneumothorax ; but with what we now know of the occurrence of tympanitic percussion-sound in limited pneu-

monia and pleurisy, we must simply admit the doubt that exists, whether these symptoms and signs together did actually indicate pneumothorax in this case. I have again and again observed more or less similar facts, but am quite at a loss, even now, to separate the true from the false instances ; only I feel pretty well convinced that in some of these instances the metallic phenomena were not doubtful, and that they could hardly have been connected with a cavity in the lung. In two cases the disease was connected with an injury to the chest.

Cured Pneumothorax after injury.

In one of these the symptoms—viz., those of pneumonia, without dull percussion, and with remarkably feeble respiratory murmur, and indistinctly metallic râles, came on immediately after a blow, which there was reason to think might have broken a rib ; but no fracture could be detected, and the case, originally admitted to a surgical ward, was accordingly sent to the medical hospital. In the other case, a young man, when in a state of drunkenness, stabbed himself with a long iron skewer which was lying by him, in the front of the chest, immediately above the heart. He was taken to the surgical hospital, but the external wound was exceedingly slight, and required absolutely no treatment ; under these circumstances he was remitted to my care, and I found all the evidences of a moderate and limited pneumothorax ; including pretty distinct, but still not perfectly distinct, metallic crepitus audible for some distance around the seat of the injury. This man was hardly even in bed throughout his disease ; the metallic phenomena disappeared in about forty-eight

hours, and gave place to abundant and loud ordinary friction-sound; the percussion, however, never at any period of the disease became dull, and it retained throughout a certain obscurely tympanitic quality, which very gradually disappeared. On this case, and the preceding, I will make only one remark. If they were not cases of *limited pneumothorax*, what were they? If they were cases of limited pneumothorax, then I have the complete conviction, that I have observed as nearly as possible the same series of phenomena in other cases, not connected with external injury, and yet ending, like these cases, in a recovery virtually complete. Further than this, I think it is impossible to proceed at present, in the investigation of the subject.*

* There are two subjects of great interest connected with pneumothorax, and indirectly involved in the considerations adduced in this article, on which I prefer at present, having regard to the materials before me, to refrain from expressing any decided opinion. The connection of empyema with pneumothorax (in the sense of the former becoming a cause of the latter, through the opening of the pleuritic effusion into the lung) is so well known as to require no remark. But may it not sometimes be the case that the converse is the order of events? rupture of the lung taking place in the first instance, and empyema supervening on this, while at the same time the traces of the pneumothorax became obliterated, and the air in great part or wholly absorbed? I have detailed one case which seemed to have this history (P— J—, p. 354); and I have seen others in which it appeared more or less probable that an empyma might have this origin. May it not have been so, for instance, in the case of R. G. at p. 419, in which the expectorated matter was fetid from the first? The influence of *cured* pneumothorax on the progress of phthisis is likewise a subject of much interest and difficulty, on which I will only refer in the meantime to p. 417, and to some other incidental remarks in future articles of this volume.

XVI.

PHTHISIS PULMONALIS. EMPYEMA AND PNEUMOTHORAX. HYDATID TUMOUR OF LUNG. EMPHYSEMA OF LUNGS. REMARKS, CHIEFLY ON PHYSICAL DIAGNOSIS.

CASE I.—*Phthisis Pulmonalis. Displacement of Heart, Stomach, and Right Lung, consequent on Atrophy of the Left Lung. Peculiar Sounds accompanying Action of the Heart.*

Bedside, November 1856.—I ask your particular attention to this case of pulmonary phthisis, on account of certain interesting and unusual phenomena presented by the physical exploration of the chest.

The first fact which comes under observation, on looking at the chest, is the rather considerable flattening below the left clavicle, and the comparatively small respiratory movement in the same situation. On percussion, the left front is not only comparatively, but absolutely, dull. I need not tell you that these signs indicate a seriously damaged lung.

Now, observe the action of the heart, as seen by the eye. In the second intercostal space, and more slightly in the third, there is a very strongly marked undulating movement. Three inches from the sternum, in the second intercostal space (H, Fig. 9), there is a dis-

tinct protrusion of the parietes at each pulsation. Feel it with the hand, and you will readily recognize this as

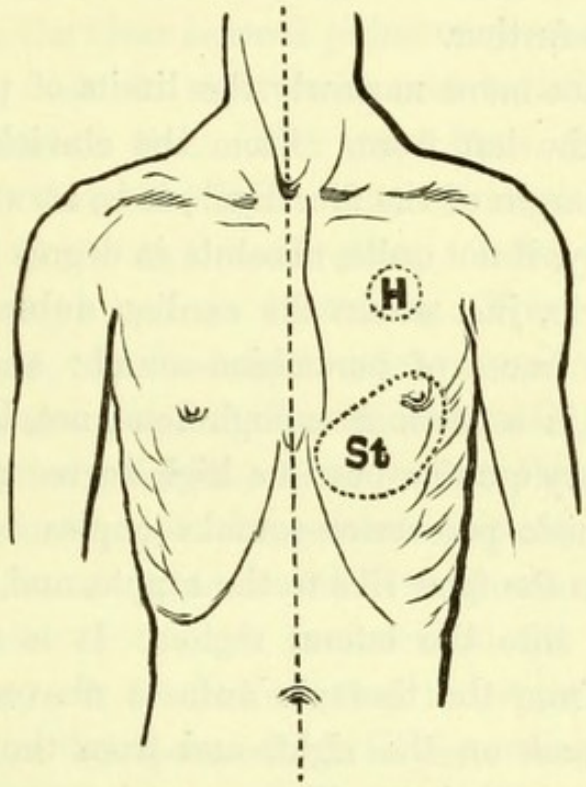

Fig. 9.

the site of the apex-beat; the punctuate impulse is, in fact, much stronger and better marked than usual. You cannot, in this case, employ percussion to check your conclusions in regard to the position of the heart. The general dulness of the left front prevents this; but, by the stethoscope, you can realize the sounds of the organ, and observe that the first sound is conveyed to the ear with more than ordinary sharpness and articulation, just at the spot which the eye and the finger jointly recognize as that of the apex-beat. There can be no doubt, that the heart is, in this case, very greatly dis-

placed upwards. I don't know that I ever saw it so high up before. To understand this strange dislocation of the heart, we must pursue our examination further.

Heart displaced.

Now, trace more narrowly the limits of percussion-dulness in the left front. From the clavicle down to the upper margin of the fourth rib, it is, as we observed before, nearly, if not quite, absolute in degree ; but, over the fourth rib, just where the cardiac dulness *ought* to be, we find traces of percussion-sound ; and, a little lower, there is a distinct enough tone, not, however, of the pulmonary quality, but in a high degree tympanitic. This tympanitic percussion-sound occupies a space extending from the false ribs to the nipple, and, from this, dying away into the lateral region. It is easily distinguished from the thoracic dulness above, from the hepatic dulness on the right, and from the intestinal percussion note below. There can be no reasonable doubt that it belongs to the stomach, which is dragged out of the epigastric region, and into the lower part of the left thorax, by the same force that has carried the heart upwards. As the heart rests upon the diaphragm, and the stomach is immediately under the diaphragmatic concave, you can readily understand how displacement of the heart should sometimes involve dislocation of the stomach.

Stomach displaced.

Next, examine the thoracic percussion-dulness towards the right. You observe that there is no difficulty in defining it. The line which I trace with ink (see

diagram) almost along the left border of the sternum, or even a little farther from the middle line than this, cuts off abruptly an absolutely (or nearly absolutely) dull space from the clear normal pulmonary sound, which is present all over the right front, and to this extent beyond the middle line. You must have already begun to suspect that the right lung exceeds its proper limits in this direction. Take the stethoscope and see if it is so. You observe that, on the right side of our ink-line the respiratory murmur is full and free, while, on the left, it is absent or altered. And, in fact, the full and free respiratory murmur is the general character of the right front, just as much as impaired respiratory murmur is the character of the left front within the line that we have drawn. It is obvious, that the right lung is encroaching on the space which, in the ordinary course of things, should be the portion of the left.

Right lung displaced.

Let us next see if the left lung, hemmed in as it is from every side in front, has extended its limits in the backward or downward direction. On percussing the back, we find, once more, dulness over the left lung, from the supra-scapular space downwards to the seventh or eighth rib, and here the sound begins to have a clear quality, obscurely tympanitic. It is possible that there is pervious lung here ; but, from the paucity of respiratory sound, I greatly suspect there is not much. A little below, we have the unquestionably tympanitic sound of an abdominal viscus—probably the colon. The lateral region shews a similar state of the percussion.

Can anything be plainer than the state of the left lung in this case? It is densely packed into the upper and back part of the left thoracic cavity, from the suprascapular space to the sixth, seventh, or eighth rib. The rest of the space properly belonging to the left lung is occupied by a small portion of the right lung, which crosses the middle line to the average breadth of an inch or more; by the heart, which is pushed or dragged upwards several inches; and by the stomach and colon, perhaps also the spleen, displaced in like manner. That the atrophy of the lung is the cause of these manifold displacements of the viscera, as it is of the flattening and immobility of the left thoracic wall, is plain. In fact, whenever a change takes place in the volume of any structure within the thorax, you always find that the most mobile of the surrounding parts accommodate themselves to the change, either by yielding (as in the case of pleuritic effusion, so as to produce displacement of the heart or liver), or by pressing in, so as to take the place of an atrophied viscus. Had a *part* only of the left lung been atrophied, the neighbouring parts would probably have enlarged to such an extent as to fill the void. This is a frequent source of emphysema. But, as there is atrophy of the entire lung, it follows that all the surrounding viscera must, to some extent, be dragged out of position. The heart is more displaced than usual, which I take to be owing to its having contracted adhesions, through the pericardium, to the upper lobe of the lung.

State of left lung.

Results of atrophy of lung.

But we have not yet exhausted the facts of this remarkable case. If you will examine carefully over the spine of the scapula, you will find a rather faint, but unequivocal tubular respiration, especially at the outer part. In the axilla there is the same. The resonance of the whisper is in both places increased. Between the axilla and the heart's apex there is, in addition to these phenomena, a crackling râle with respiration, having that peculiarly articulate and hollow tone, called by Laennec "cavernous;" and by Skoda "consonating." In this case, though by no means always, or even generally, I am disposed to believe that this sound, in reality, indicates a cavity, and probably a pretty large cavity. But there is a far more curious indication of a cavity than this. If you listen carefully to the heart, an inch below and to the outer side of what seems the apex, you will find that there is a very peculiar crackling or grating noise, absolutely synchronous with the movement of the heart, both in systole and diastole. This noise has the same hollow and articulate, or, as Skoda calls it, "consonating" quality, as the respiratory crackling; and, what is very curious, this quality disappears as you approach the heart, the movement of which is evidently the source of the sound. Over the greater part of the heart itself, nothing is heard but the faintest possible approach to a murmur, or, as I would rather call it, a reduplication of the first sound—a hint of what the French call a "bruit de rappel." What is the meaning of these facts?

Evidence of cavity.

I am pretty certain that the heart is here resting

upon the wall of a considerable cavity in the lung. Owing, perhaps, to adhesions of the pericardium to this cavity on the one side, and to the heart on the other, the motion of the organ is not accomplished without some disturbance of the surrounding parts, and this disturbance gives rise to sound. You may call the sound thus produced a friction sound, or you may call it what you please ; provided we understand the mode of its production, the name we give it matters little. But this sound would be a very small affair, were it not for the pulmonary cavity in the immediate neighbourhood, the walls and contained air of which catch up the minute vibrations, reflect and magnify them, and convey them, thus magnified, to the ear, at a certain distance from the site of their production ; just as the whispering gallery of St. Paul's carries the faintest vibrations in a concentrated condition around the whole circle of the dome. This is a very marked example of the effect of a cavity in reflecting and increasing sound. I have heard exactly the same phenomena as in this case, by listening over the air-filled stomach to the sounds of the heart, when they have been slightly altered, and when the stomach has been in very close proximity to the heart.

Diagnosis.

I have indicated an opinion, that the pericardium is adherent in this case. I think so, because of the very marked movement of the intercostal spaces over the heart ; and because the organ has followed so closely the retraction of the upper lobe of the lung. There is, however, one circumstance which some would consider con-

clusive against the idea, at least of general adhesion. This is the distinct and punctuate beat of the apex against the finger. According to Skoda, this localized beat is always obscured in adherent pericardium; and if Skoda is correct in establishing this as a rule, then the present is not a case of adherent pericardium. I am very strongly persuaded that Skoda's rule on this point is too general; but it is impossible not to receive with respect a statement from so accurate an observer and so great a man. My own observations (which I believe to be opposed to the rule in one or two cases) are not *absolutely* to be trusted; and, in the meantime, we had better wait for some new facts. I am not prepared, therefore, to declare either for or against the adhesion of the pericardium in this man.

Were I to construct a hypothetical history of this case, from the facts now before us, I should say, that this patient has been affected, in the first instance, with tubercle of the left apex—that he then became the subject of an effusion into the left pleura, which distended the side and compressed the lung; not, however, until adhesions had been formed between the upper lobe and the thoracic wall; perhaps also between the upper lobe and the heart. [Since the preceding sentence was written in 1856, I have become convinced, or at least acquired clearer convictions, of the possibility of pneumothorax having to do with the result in some of these cases of compressed lung in phthisis, as well as in tubercular empyema. See p. 409, *note;* as well as the entire article (XV.) on Pneumothorax in its more general

aspects.] At a later period the effusion in the pleura was absorbed, when the lung, instead of recovering its volume, was incapacitated from expanding, by the tubercular deposit in its substance. The other changes would follow naturally from this state of the lung; and the advance of the tubercular disease would account for the cavity in the apex, as well as for the slighter deficiency in the respiration at the apex of the right lung.

I will now give you the actual history, as recorded by Mr Hardie, clinical clerk :—

J—— C——, æt. 25, born in Ireland—occupation a tailor—admitted Oct. 9, 1856—ward 4—complains of pain in chest and cough.

Patient states that, four years ago, when in Greenock, he received a severe blow on the chest and head, which was followed soon afterwards by slight expectoration of blood. He continued to spit blood, at times, for a day or two after the accident, when it ceased altogether, and he got quite well again. Shortly after this he went to New York, where, about eighteen months ago, he had an attack of "pleurisy" (?) which lasted about ten days. After his recovery from pleurisy, he was troubled with a cough, accompanied with profuse expectoration of a white frothy character, but, latterly, varying in colour, being sometimes yellow and sometimes white. He has never had any palpitation, but complains of sickness, generally when walking after meals. He sweats a good deal at night. There is almost complete aphonia, and he is much emaciated. He has been taking cod-liver oil for some time back, and, since his admission, has been continuing the same, and rubbing the chest with cocoa-nut oil.

History. Blow. Hæmoptysis.

"Pleurisy."

Sickness.

Night Sweats. Cod Oil.

CASE II.*—*History of a three years' illness, apparently arising from a strain, and at first supposed to be Rheumatic Fever. Symptoms of Empyema gradually developed; apparent convalescence; sudden expectoration of fetid pus in large quantity, and afterwards external opening. Dropsy of legs, emaciation, diarrhœa, albuminous urine, of low specific gravity. Observation of very loud metallic echo, communicated from first sound of the heart. Sudden death of the patient from uræmic convulsions, without premonitory symptoms. Simple Empyema, non-tubercular, with pneumothorax. Waxy degeneration of liver, spleen, and kidneys.*

The following case is in many respects an interesting one; indeed, the peculiar alteration in the cardiac sounds, which it illustrates, was in some respects new to me, as well as to many others who observed the case, or who have heard of it since its termination. I shall therefore submit the facts in some detail, exactly as they were reported under my direction, during the life of the patient, by Mr. (now Dr.) Andrew Pow, then clinical clerk; and afterwards give some additional particulars, in the form of remarks tending to illustrate the view taken of the phenomena at the time, and render their bearing upon diagnosis more apparent.

R. G., æt. 37, a labourer from Berwick-on-Tweed, admitted into the Royal Infirmary, Ward 4, on 30th June 1857. Ill three years. Gives the following history:—

Whilst at the herring fishery off the coast near Sunderland

* Published in October 1859.

in the latter part of August 1854, patient was suddenly attacked by severe pain in the back, which was so intense as to make him fall down. The pain was situate in the lumbar region, not exactly in the mesial line, but on both sides of the vertebral column. This pain seized him on a Thursday, without any known cause, and, after remaining for a short time, left him; but notwithstanding its entire absence, he felt himself unwell, and was very anxious to get home as soon as possible. On the Saturday, in consequence of the dilatoriness of his men, he, in a fit of passion, raised the mast of the boat himself, a weight which it generally required the strength of three men to lift. From this exertion he felt no immediate injury, although he thinks he may have strained himself in some way. He arrived at his own house that day, still feeling unwell with "gruising" of cold, but not so badly as to oblige him to remain in bed. In this state he continued, getting gradually worse, for eight or ten days, when he was obliged to go to bed. A fever then set in, which at the time was called by the doctor rheumatic fever, but which seems not to have presented the ordinary symptoms of rheumatic fever, there being no pain nor swelling of the joints. Of this, however, the patient cannot be quite positive, as during a considerable period of the fever he was delirious and insensible. During the course of the fever, he was once very suddenly attacked by a pain along the vertebral column, from occiput to sacrum, so acute as to make him believe he was going to die. The pain, however, lasted only a short time, and disappeared as suddenly as it had come. He does not recollect having any pain in left side during the course of the fever, but he believes that he must have had some, as two or three blisters were applied.

History of Lumbago and Rheumatic Fever.

During the latter period of the fever just described, and the first period of his convalescence, he found his breathing rather difficult, and discovered that he could only lie on his left side. Nearly three months had elapsed before he was fairly convalescent from this attack. One day, a short time after he had risen from bed, when he was

Symptoms of Empyema.

walking about, he was very suddenly attacked by most severe cutting pain in lower part of neck, and at the very same time by a violent fit of coughing, accompanied by a profuse fetid purulent spit. The expectoration was so abundant, that during that afternoon he thinks he must have spit up nearly two pints of matter. On turning his head the matter sometimes gushed from his mouth. He is pretty sure that he had no cough during the fever, and positive that he had none during his convalescence till this sudden attack. Three months after the accession of the cough, a swelling appeared between the ribs, or over the ribs, on the left side. This after a short time was opened, and discharged above a pint of the same fetid matter as came through the mouth. The opening soon closed up ; the matter again collected ; and in little more than a fortnight another opening was made, giving vent to about two quarts and a pint of the same matter. Before the openings were made, patient had great difficulty of breathing, with sharp pain in left lateral region, increased on taking a deep breath, and very much on coughing. The openings made the second time have remained patent till now, and through them and through the mouth fetid matter has been continually discharged till the present time. Occasionally, when matter tinged with blood was discharged from the side, the same passed by mouth. Patient is not aware that he ever passed blood or matter through any other channel. For the last two years his legs all below the knees have been liable to swell, and at present the ankles are considerably swollen, the swelling hard and fibrinous. No swellings in calves.

Patient is emaciated to a very considerable extent, as compared with his former condition. His colour, however, is by no means bad, possibly heightened by some flushing of cheeks. The nails are strongly curved, and the fingers clubbed. The abdomen is moderately full, not rigid, and seems to have lost less flesh on its parietes than the rest of the body. The alveolar margin quite smooth and normal. Bowels frequently loose for a fortnight or three weeks at a time, but not so at present. Appetite very bad, and has

Examination on admission.

been so for a long time. Thirst considerable ; does not sweat at night. Skin dry. Sputum is pretty copious, but variable in quantity—would appear, from his own account, to be from 3 to 12 oz. or more, fetid, composed to a great extent of pus mixed with mucus. Urine acid, pale in colour, somewhat turbid, with very little deposit—of Sp. Gr. 1008, containing albumen.

July 1*st*.—Some tenderness along left hypochonder, and still more in epigastric region. Liver descends within an inch of um-

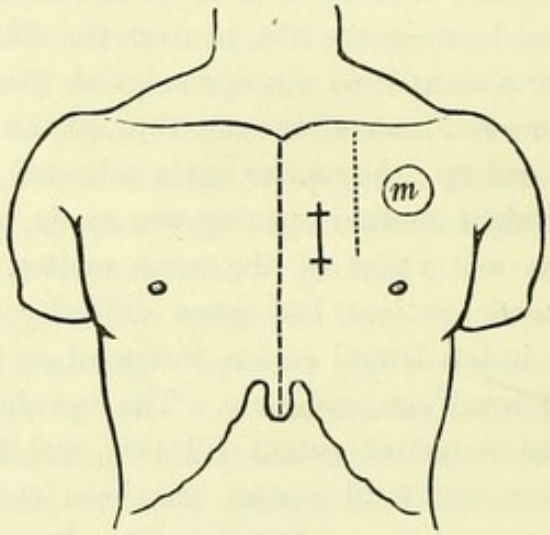

Fig. 10.

Diagram of case of R. G. *m*, seat of the metallic echo of cardiac sounds ; ‡, seat of reduplication of second sound. The finely-dotted line in left front shews the limit of pulmonary adhesions, outside of which the pleura contained air, while on the margin of the adherent lung the signs resembled those of a cavity in the lung itself.

bilicus, but more in epigastric than in right lateral region. Expansion of left side of thorax visibly less than that of right, breadth also less ;—this comparative immobility reaches to the summit. From one to three inches below nipple there are several depressed cicatrices evidently connected with ribs and periosteum by fibrous tissue, and surrounded by slight blush of discoloration. Percussion of left front, generally, is dull, with faint tympanitic sound over upper third ; that of right front perfectly normal. Respiratory murmur throughout left front is diminished. *In the upper and outer portion of left front there is heard, after*

each beat of the heart, a loud echoing, hollow, and very peculiar murmur. There is no murmur at apex of heart, and none over aorta. Over the third or fourth left costal cartilage, there is a reduplication of the second sound. Nearer the sterno-clavicular articulation expiration is accompanied by a somewhat hoarse blowing, and inspiration by a coarse resonant crackling.

July 2.—(*Examination resumed*)—On back of chest there is dulness all over left side, with great general feebleness of respiratory murmur, which is absent, or nearly so, in lateral region, and faintly tubular towards root of lung. The cough, as listened to at back, and also the deep inspiration, together with the crackling that accompanies them, are harsh and doubtfully echoing, but can scarcely be called metallic. This character is most distinct with expiration ; and is, perhaps, nothing more than strong tubularity. Vocal resonance not notably altered over the upper part of left back ; but about lower edge of scapula, and towards root of left lung, at middle and lower third, it acquires an approach to the ægophonic character, most marked at the sixth or seventh dorsal vertebra. Auscultation and percussion of right lung quite satisfactory.

He was ordered the following :—

℞ Tinct. Humul. Lup. . . ℥j.
Inf. Serpentariæ . . . ℥viij.
Sp. Ammon. Aromat. . .
Tinct. Opii Camph. . . āā ℥ss. M.
Sumat. ℥ss. ter in die.

July 6*th.*—The sputum, being in a clean vessel to-day is not nearly so fetid.

July 10.—This morning, about 7 A.M., this patient died suddenly. Last night, about 9 o'clock, he had a fit of insensibility with convulsion, during which the nurse thought he was dying, and in which he remained some minutes. He was seen by the resident physician, Dr. Spasshatt, while coming out of the fit, with stertorous breathing, but no convulsion, a full pulse, slight congestion of face. After a quarter of an hour he became par-

tially sensible. Purgatives and enemata were given ; but in the night he had four or five similar attacks, and died in the midst of a paroxysm this morning. The insensibility during the fit appeared to be progressively greater from first to last, with the exception mentioned above.

The death of this man, at the time it occurred, was undoubtedly very unexpected, and even now appears rather mysterious ; though I think there can be little doubt that it was owing to a modification of that remarkable, and still too vaguely interpreted, condition of the system, commonly termed uræmic poisoning. I regret that it was omitted to examine the blood and serum of the brain for urea ; this was intended, but by an accidental neglect the parts were not preserved. The patient was, before the fatal attack, a remarkably intelligent man, and had no indication of an epileptic tendency. His urine was, however, permanently albuminous, and of low specific gravity ; while a diarrhœa, to which he was subject, had been spontaneously suppressed ; and to these circumstances I am disposed, without affecting further to explain it, to ascribe the fatal attack.* The most careful inquiry was instituted as to the medicines administered; but there is no ground for supposing that the patient had, either by accident or design, taken anything beyond the slightly sedative and tonic mixture which appears among the prescriptions in the ward-book. The case was indeed regarded as one for tonic regimen rather than for active treat-

* This case may be compared with one recorded in Art. I., p. 11, of this volume.

ment, and illustrates very well a class of accidents which sometimes take the practitioner by surprise, and which may bring him, unless fortified by character and protected by circumstances, into trouble. The epileptic seizure was in this instance distinguished from ordinary epilepsy chiefly by its rapidly fatal termination; from ordinary uræmic poisoning by the absence of premonitory symptoms and progressive coma; from strychnine poisoning the symptoms differed in the presence of marked coma during the attacks; and any narcotic poison appeared to be totally out of the question; although, had there been grounds for suspicion on the head of administration, it would, perhaps, have been difficult to feel assured that death, following with such rapidity, and with such remarkable symptoms, had taken place from natural causes. On these grounds the very sudden fatal termination of the case, however imperfectly explained by science, appears to be not devoid of instruction for the practitioner.

The circumstances, however, which chiefly demand attention, are those connected with the signs of the chest-disease; and these appeared to me at the time so curious, that many students and practitioners (among the latter Drs. Laycock and J. W. Begbie, who were at the time on duty in the medical wards of the Royal Infirmary) were requested to examine the case, as being one of a very unusual, if not unique character.

That this man was the subject of empyema, and probably of pneumothorax, appeared evident from the history and the symptoms, as well as from the physical signs.

There appeared, also, to be some circumstances in favour of the idea of tubercular disease, and, if so, of a cavity within the lung at the left apex; but my own convictions were by no means strong on this point; and in consideration of the perfect integrity (to physical diagnosis) of the right lung, my opinion even inclined towards the negative. I was rather, indeed, disposed to suspect, from the history, that disease of the vertebræ, or of a rib, might have been the real origin of the mischief. The general aspect of the patient was not tubercular, but rather gave the impression of a robust constitution worn down by long-continued discharge from the suppurating pleura; while the diarrhœa was regarded as the joint effect of renal disease, and of the absorption of fetid matter from the pleura into the blood.

Had it not been for the peculiar sounds connected with the heart's action, the diagnosis would probably have rested here. But the singular ringing murmur which attended the first sound of the heart, at a point midway between the base of the heart and the left coracoid process, could not fail to raise many curious questions. Was it a variety of bellows murmur? Was it generated in the heart, in the pulmonary artery, in the aorta and its branches, or in an aneurism? Or was it, finally, an extra-cardiac, extra-arterial murmur, due to some peculiar combination of conditions in the pleura and pericardium, or in a pulmonary cavity; the heart itself, and the great vessels, being normal? I will detail the circumstances which led me to adopt this last opinion.

In the first place, the sounds of the heart, as heard over the precordial region, were perfectly normal, with the exception of the reduplicated second sound, at the left margin of the base, which was undoubtedly not of a character to indicate any considerable disorder. Nor was there any symptom of heart disease ; the organ had a firm and regular beat, and there was neither palpitation nor uneasiness referrible to the heart : much less was there any trace of aneurismal pulsation over the site of the abnormal murmur.

In the second place, the murmur itself, so curiously circumscribed in its locality as to correspond neither to the site of a cardiac nor arterial bellows murmur, was also, to my ear, distinct in its character from both of these. I ought, indeed, to remark, that opinions were divided as to the possibility of the sound heard being a bellows murmur ; and in case it were so, as to its being communicated from the heart or from the subclavian artery. My own opinion, almost from the first moment, was that the sound closely resembled the hollow musical resonance which succeeds a loud noise of some kind resounding in a vaulted room or cavern ; that it was, in short, a cavernous and metallic echo of the first sound, communicated by the heart or aorta to the wall of an air-filled cavity in the neighbourhood. That such a cavity existed, in this case, at the site of the murmur, was rendered very probable by the history of the case, although physical examination failed to detect unequivocal signs of a cavity, and there were no other clearly defined metallic phenomena excepting those presumed to be produced in connection with the heart.

The production of metallic sounds by the influence of the heart upon a cavity in its neighbourhood, is by no means unexampled ; but in none of the cases within my knowledge or experience have these sounds presented, as this one appeared to me to do, the character of a *simple metallic echo* of the heart's sound, like that which sometimes accompanies the sound of the breath or voice in pneumothorax. I have many times heard metallic phenomena over the air-filled stomach, accompanying the motion of the heart. Such a case I saw a good many years ago in a patient of Dr. Begbie (senr.), a country schoolmaster, who had discovered the anomalous sounds for himself, and was, in consequence, deeply hypochondriacal. But in this case, and in the others more or less similar which have occurred to me, the abnormal sound was clearly owing to the motion of the contents of the stomach itself ; the heart being only the source of an impulse, not the starting point of the sounds. In like manner, in a case first published in the *Edinburgh Medical Journal* for January 1857,* a number of curious metallic sounds were produced in the walls or in the contents of a pulmonary cavity moving along with the heart. The sound in the present case was quite different in character from these, and was clearly due, in my opinion, to the simple reflection, or reverberation of the first sound, as formed in the heart or aorta. It was simple, homogeneous, and entirely destitute of the crackling or tinkling character ; it resembled much more closely, indeed, the type of the

* Case I. of this article, p. 410.

amphoric echo, as heard along with the spoken voice or cough, in cases of dry pneumothorax.

In most of the cases in which I have observed metallic phenomena in connection with the heart's sounds, it has appeared to me probable that the pericardium has been more or less adherent, as in this case, and perhaps in Case I. of this article (see pp. 416, 417). I have not indeed, usually been able to test this view by *post-mortem* examination; but, in the present case, I ventured to infer it as probable, partly from antecedent observations, and partly from the peculiar reduplication of the second sound, and the propagation of the first to the air contained in the pleura in such a remarkable manner. I do not know if the experience of others, on this point, corroborates the idea that adhesion of the pericardium is favourable, if not necessary, to the production of cardiac metallic phenomena (unless, indeed, in the case of pneumo-pericardium); such, however, is my impression at present.*

I shall conclude with the account of the *post-mortem* examination, from Dr. Haldane's register of dissections.

Sectio cadaveris.—Fifty hours after death.

Considerable emaciation, contraction of left side of chest, and slight lateral curvature of the spine. There was the opening of a sinus between the third and fourth left ribs, in the external lateral surface of the chest.

* I have seen one or two additional cases since the publication of this one bearing more or less distinctly in the same direction as the argument in the text; but none of them has been corroborated by *post-mortem* examination, and they are therefore of little value.

The larynx and trachea were quite natural. They contained a little frothy-looking purulent matter.

The pericardium was universally and pretty firmly adherent. Externally it was adherent also to the left pleura.

The heart was a little enlarged, and weighed 14 oz. Valves normal. Muscular substance natural.

The right pleura was not adherent. Right lung voluminous and healthy.

The left pleura was most densely adherent for a short distance from the middle line anteriorly. When these adhesions were broken down, a cavity, bounded by pleura, was found, which contained about a pint of thick purulent matter. When the left lung was removed from its adhesions, it was found to be very much compressed, was about six inches long, and not more than an inch and a half to two inches broad. The upper half was found to crepitate partially; the lower half was completely carnified and non-crepitant. The lung contained no deposit of any kind.

The pleura on this side was generally much thickened, and in some places of almost cartilaginous consistence.

No disease of the rib could be discovered.

The liver was large and waxy, weighing 6 lb. 10 oz.

The spleen weighed 18 oz., waxy.

The kidneys were enlarged and mottled. They weighed 16 oz., and were found to have also undergone the waxy degeneration.

No other lesion was found in the abdomen.

The brain and membranes were normal.

CASE III.—*Tumour of Right Lung; expectoration of portions of Hydatid Membrane; Gangrenous suppuration; Death. A large solitary Hydatid in the upper lobe.*

Cases of Hydatid originating in the lung are not so frequent as to make the following account superfluous.

Most of the cases of recovery after the discharge of hydatid cysts by expectoration, leave a presumption that the liver was the source of the parasitic growth. Some of them are still more questionable, and probably apocryphal, or founded on delusive representations. In the fatal cases, on the other hand, the relation between the symptoms and the *post-mortem* appearances is rarely given with such detail as to be really instructive. In connection with this rare pathological incident, I may remark, that the acephalocyst, even in its usual site, the liver, would appear to be extremely uncommon in Edinburgh ; as among many thousand dissections, which I have either performed or seen performed during my connection with the Royal Infirmary, there has not been a single instance of acephalocystic hydatid or echinococcus, either in the liver or in any other organ, with the exception of this one.

H. L., a furnaceman, æt. 27, complaining of cough and expectoration, was admitted to the Royal Infirmary on July 20, 1856. The symptoms were stated to have been of long standing, but to have attracted attention particularly only about two years before admission, when he was confined to bed for a week with pain in the back and right shoulder. About six months afterwards, this pain recurred, in connection with cough, and the symptoms were ascribed to cold, caught by working in the snow. He was unable to move the right arm for a week, and after entering the Newcastle Infirmary, to which he

Early Symptoms.

resorted for relief, he spat up about half-a-pint of pure blood. After this he spat up blood occasionally, but, on the whole, continued well till a month before admission, when his symptoms returned, and he lost appetite and flesh considerably. He had the impression, at this time, that an abscess was forming in his right side, and that it would burst at the back, below the scapula ; where he described the sensation as being that of a cold spot, on which he could have laid the finger.

Diagnosis.

For some time after this man's admission, his case resembled strongly one of ordinary tubercular disease. There was dulness of percussion over the right front ; respiration was faintly audible, and vocal thrill and resonance were much impaired. After repeated examinations, however, I was strongly impressed with several circumstances tending to throw doubt on the diagnosis of tubercular disease. It appeared to me that the expectoration (which was copious and almost constantly more or less mixed with blood) was not characteristic of that stage of phthisis, which the history and the physical signs might be supposed to indicate. The pus was scarcely ever in large proportion, and never presented the appearance of the well-known masses of softened tubercle. The patient had much more suffering and distress than is usual in tubercular disease ; the dyspnœa was somewhat spasmodic ; there was, at times, a harsh laryngeal respiration ; the voice was also affected in a similar paroxysmal manner ; and there was even a trace of dysphagia. Hectic sweats, though not absent, were neither so profuse nor so fre-

quent as in the like stage of phthisis. Finally, some time after admission, the following very peculiar physical signs were noticed :—

Physical Signs.

The right front was completely fixed in respiration, and there was a distinct fulness of this side from the second to the fifth rib, with obliteration of the intercostal depressions. The dulness on percussion was absolute all over this prominence, and all the respiratory and vocal phenomena were absent ; above it, there was faintly tubular respiration. The lower zone of the chest was dull on percussion, but not visibly enlarged ; and breathing (faint and tubular) was audible at the lower part of the back. The left lung presented a few mucous râles, but no other evidence of disease.

The alternative views present to my mind at this period, and frequently expressed to those who saw the case with me, were cancer of the right lung, in its middle part ; or a very peculiarly placed pleuritic effusion ; in which last case it was to be presumed that the fluid was confined to the part of the right front above mentioned, by firm adhesions over the upper, lower, and back part of the right lung. It was considered impossible to form a decided diagnosis without further evidence. Dr. Warburton Begbie saw the case with me at this time, and I believe he concurred in these opinions, as also in thinking that the progress of the case would probably make clearer what then appeared to us difficult and doubtful.

No important change took place till the 16th No-

vember, although palliative treatment was constantly administered for the cough, and various tonics, including chalybeates, cod-oil, and oil-inunction, were employed, as well as repeated blisters, from which he always said he felt temporary relief. At the last mentioned date, he was sitting up at the fire, when hæmoptysis occurred. Blood was copiously rejected, and a pint or more of watery discharge came away along with the blood, which was believed at the time, on the faith of the nurse's statement, to have come from the stomach. On the 20th November, the blood had entirely disappeared from the sputa, under the use of aromatic sulphuric acid and small doses of morphia. The expectoration at the same time became much more purulent than before, and acquired a very marked gangrenous fetor.

Hæmoptysis, etc. (Rupture of Cyst.)

On the 24th November (no examination of the chest having taken place since the 10th), he was very weak and prostrate, and complained much of cough. The case had now again much of the character of tubercular disease, or of gangrenous excavation of the lung, and, on a cursory examination, it was noticed that the bulging of the right front had entirely subsided. His suffering, however, continued to be great, the cough had a paroxysmal character, a croupy tone, and an extreme violence, which were very striking, and constantly suggested the idea of a tumour pressing on the nerves. The voice continued hoarse and low-pitched, yet there was no evidence of laryngeal disease. At this time, a portion of the sputum brought to Dr. Spasshatt, the

resident physician, in the evening, was observed to contain some very peculiar membranous shreds, which were shewn to me next day before lecture. The largest of these shreds would have covered a shilling. It was thin, but tolerably consistent—it had a homogeneous surface and a pearly-white semi-transparent appearance. Under the microscope, it presented much of the character ascribed to "basement membrane," but, in parts, had an appearance of fibrous structure, which recalled the fenestrated membrane of the arteries rather than any other tissue of the normal body. There was no trace of any globular cyst in the expectoration; every portion of membrane found was capable of being laid out perfectly flat. Notwithstanding the difficulty of coming to a clear decision, I now ventured the opinion that the case was one of hydatid disease of the lung. That it was *primarily* of the lung was probable from the physical signs; but a considerable enlargement of the liver, appreciable by examination in the hypochonder, made the difficulties of the case still greater. It was impossible to be quite sure that it was not a case of hydatid of the liver, opening into the lung.

First expectoration of Hydatid Membrane.

On now re-examining the chest more carefully, the greater part of the space formerly occupied by the bulging was found to be faintly tympanitic on percussion, and to communicate to the stethoscope a highly tubular breath-sound, with occasional hollow crackling, strongly suggestive of

Signs of Cavity, etc.

a cavity. The opposite lung had still a few mucous râles, but seemed in the main healthy.

The patient was now prostrated to the last degree. Emaciation was extreme. There were cold sweats. The appetite for food was very low, and diarrhœa had set in. The urine had for some time been highly albuminous; the breath and the sputa were very fetid; the latter more and more purulent, besides containing gangrenous debris and larger portions of membranes like those already described. Some of these appeared to be brought up with extreme difficulty, and several times there was heard in the trachea, without the intervention of the stethoscope, a kind of flapping sound with the cough, not unlike the description of Laennec's "souffle voilé." The 29th of November brought his sufferings to a close, his senses having remained entire to the last, or nearly so.

"*Souffle Voilé.*"

The following account of the *post-mortem* appearances was laid by Dr. Haldane before the Medico-Chirurgical Society, along with the illustrative preparation :—

"The specimen was interesting as presenting an instance of a disease of the lung, rarely met with in Edinburgh, though it occurred much more frequently in other quarters, where certain local causes could be traced in its production. During six years in which Dr. H. acted as Pathologist in the Infirmary, and during other six or seven years over which his observation had extended, he had not previously met with a similar case. The right pleura was very densely adherent throughout, except over the base of the lung, where the adhesions consisted of pretty recent lymph, which readily broke down. When the lung was removed, it was found to be voluminous, its central part fluctuated, and was evi-

dently occupied by a large cavity ; towards the apex and base, the pulmonary tissue felt firm and dense. On making an incision into the fluctuating portion, the knife at once entered a cavity containing a white membranous substance, as well as some fetid purulent matter. The cavity was of a tolerably regular circular form, about six inches in diameter. Superiorly, it commenced two inches below the apex of the lung, and descended to about the same distance from the base. There was more destruction of the tissue of the lung anteriorly than posteriorly, so that the anterior wall of the cavity was much thinner than the posterior. Anteriorly, the wall of the cavity consisted entirely of thickened pleura ; the visceral layer was rather more than a tenth of an inch thick, and of perfectly fibrous consistence, the parietal layer, to which the former was so closely adherent that the two could not be separated, was of about the same thickness, but of looser and more cellular structure. Adherent to the inner wall of this part of the cavity, were a few minute shreds of pulmonary tissue. The posterior and lateral walls of the cavity were composed of condensed pulmonary tissue. The right bronchus opened directly into the cavity. The cavity was lined by a false membrane, to which small portions of the hydatid cyst were here and there closely adherent. The inner surface of the cavity had evidently been intensely inflamed, being highly congested, and having flakes of recent lymph adherent to it. There was, besides, some fetid purulent matter in the cavity. The cavity was partly filled by a white membrane, which had formed the wall of a hydatid cyst, but which had been broken down. The greater portion of this membrane was of a dead white colour, like the boiled white of an egg, presenting at some places a granular, at others a smooth and glistening surface. The membrane was pretty tough, but tore readily ; it was about the twentieth of an inch in thickness. Other portions of the membrane, which appeared to have lined the other thicker layer, were clear and transparent, quite resembling clear coagulated albumen, or the capsule of the crystalline lens. Scattered through it, however, a few opaque white dots could be seen. At one place, projecting from the inner sur-

face of the thicker portion of the membrane, was a rounded growth, apparently of exactly the same structure, and about the size of a pepper corn.

"*Microscopically*, the membrane was found to be absolutely structureless. The white dots seen in the thinner portion were due to the accumulation of what appeared to be fatty matter. No trace of echinococci, or hooklets, could be detected.

"The tissue of the lung around the cavity was much condensed, and when cut into was found to be of a greyish colour, and of almost fibrous consistence. The only part of the lung not so condensed was a layer, about an inch thick at the base ; this portion, except being a little œdematous, seemed normal.

"There was recent pleurisy of the left side, and the lung was moderately emphysematous.

"The principal morbid appearances found in the abdomen were peritonitis, a dysenteric condition of the great intestine, and an early stage of waxy degeneration of the liver, spleen, and kidneys."

CASE IV.—*Emphysema of lungs, with intermittent attacks of catarrh, producing marked aggravations, the last attack ending in death. In the intervals of the catarrh, observations in regard to physical diagnosis of emphysema. Displacement of organs of abdomen, of heart, dilatation of right ventricle and murmur of tricuspid regurgitation ; peculiar râles, with a corresponding impression communicated to the hand, over emphysematous parts of lung. Comparison with observations of Laennec—"rale crépitant sec a grosses bulles"—"frottement ascendant et descendant," etc. Rale of pneumonia and of emphysema compared. Question of pleurisy as causing the rale described by Laennec. Examination of the body in the present case as compared with observations during life.*

Lecture, 14*th March* 1862.—One of the cases ending in death, which we have lately had under observation,

is peculiarly interesting as having been the subject of many conversations in the wards and elsewhere. It tends, I think, to settle some difficult and hitherto questionable points in the physical diagnosis of emphysema of the lungs. In addition to the report of the case, which I hold in my hand, I am able to shew you in these diagrams the facts as noted during the progress of the case ; and I shall now connect the statement of these facts in a more brief form with the speculations which we raised upon them, and with the result as revealed by post-mortem examination, and recorded in Dr. Haldane's very exact report in the " Register of Dissections."

Thomas B., æt. 38, was admitted pretty early in the winter session (December 5, 1861) : he improved very much under treatment, and was dismissed (January 20, 1862), at his own wish, not nearly well, but fit for a certain amount of exertion and enjoyment of life. When he was under treatment at this time, I repeatedly pronounced his case to be a typical one of emphysema of the lungs, with dilatation of the right side of the heart ; and in pointing out to you his somewhat livid lips and his feeble systemic circulation, I frequently told you of the extreme danger to which such patients are exposed by what would be in others almost insignificant attacks of catarrh. [This man was in fact in a most critical state on admission, and was several times threatened with a relapse.] Accordingly it was scarcely a matter for surprise to me that he returned in the end of last week in a dying condition ; the

Danger of Catarrh in Emphysematous Persons.

amount of catarrh was very trifling, the expectoration slight, the râles not at all abundant; but they were more than enough *for him;* his pulse was gone, his aspect deathlike, his skin cold, and it was easy to see that he could hardly live. Notwithstanding the urgency of the symptoms, which prevented me from examining him on admission, Dr. Watson managed to make a rapid physical exploration of the chest and to assure himself that the signs marked in this diagram so long ago as December 9th, were still quite distinct in the right front. The treatment was by expectorants and diuretics, and latterly by stimulants; we need hardly, however, insist upon what is so familiar. Now here are the physical facts observed nearly throughout this case more or less distinctly, and accurately noted on December 9th, but tested over and over again by renewed examinations and by a number of observers. [The respiratory murmur was much less altered than it often is in emphysematous cases, the diagnosis resting chiefly upon the general symptoms, added to the facts presently to be mentioned.] The liver was considerably below its normal position in the chest; there was displacement downwards, and perhaps very slight enlargement of it. See Fig. 11, p. 443. [We were obliged to admit the possibility and even probability of enlargement by congestion; but the careful consideration of the facts, the extent and character of the percussion-dulness, and the degree of anterior prominence, led me to suppose that it was at least not much enlarged.] I

Physical signs of Emphysema.

Altered position of Liver.

pointed out to you that in cases of displacement of the liver downwards by emphysema of the lungs the organ revolves as it were on its posterior attachments; the posterior aspect of the organ may even remain fixed or elevated, while the anterior is pushed downwards and forwards. [The percussion-dulness, accordingly, is commonly small in these cases in the lateral region, and unusually great and low down in the anterior and epigastric. The edge of the liver is also felt to be depressed, but, unless there is much congestion or other enlargement, it is not very easily felt, the organ being quite free from induration or prominence of any kind, and yielding readily to moderate pressure on the abdominal wall.] We found the heart, also, manifestly displaced downwards, as in fig. 11; and not only so, but it, too, was somewhat revolved on its own axis, so that the left ventricle was thrown into the background, while the right ventricle presented unduly in front. We ascertained this partly by means of percussion, and partly by examination of the impulse. There was hardly any sense of a proper apex-beat to indicate the *pointing* of the left ventricle; but on the other hand there was a *diffused* impulse all over the seat of the lowered percussion-dulness, and extending from this into the epigastrium.* This impulse and this percussion-dulness, accordingly, we interpreted as arising from a prominent,

And of Heart.

Peculiarities of Heart's impulse.

* See farther remarks on this and the other cardiac phenomena in the chapters on cardiac diagnosis.

enlarged, and dilated right ventricle, displaced downwards and forwards. Now *in the very centre* of this percussion-dulness, and concurring with the centre of impulse and sound (for the natural sounds of the heart were not in the least obscure, as they are often stated to be in emphysema of the lungs, but only displaced downwards and forwards, like the percussion-dulness), in the very centre of what we presumed to be the right ventricle of the heart (Fig. 11, *), we found a very distinct murmur with the first sound; a murmur concurring with the ventricular systole, heard over the right ventricle; and, therefore, as I at once interpreted it, a murmur of *tricuspid regurgitation*. [We afterwards found a feeble pulsation in the veins of the neck, but this was not always present, nor always equally distinct; the murmur on the other hand, was constant.] Now these are among the *normal* phenomena (as you might almost call them), at all events among the more distinctive and typical phenomena of a high degree of emphysema of the lungs. We had also others, which may be called ordinary phenomena; the respiration was feeble at some points; at others, and occasionally, there were bronchitic râles of various kinds, mucous and wheezing râles especially. But here are some facts which we at once fixed upon as not ordinary, and as demanding a great deal of attention. We had frequent discussions about them, and you will recollect that I did what I seldom think it necessary to do; I brought you straight from the ward into the

Sounds of Heart.

Murmur of Tricuspid Regurgitation.

lecture-room, in order to converse freely about these facts and to represent to you fully my opinions. The opinions were, to a certain extent, doubtful; not so the facts, which were as follows: — 1. There was a very peculiar râle; 2. A sensation felt by the hand during inspiration and expiration, but most marked during inspiration.

Peculiar physical signs in Emphysema.

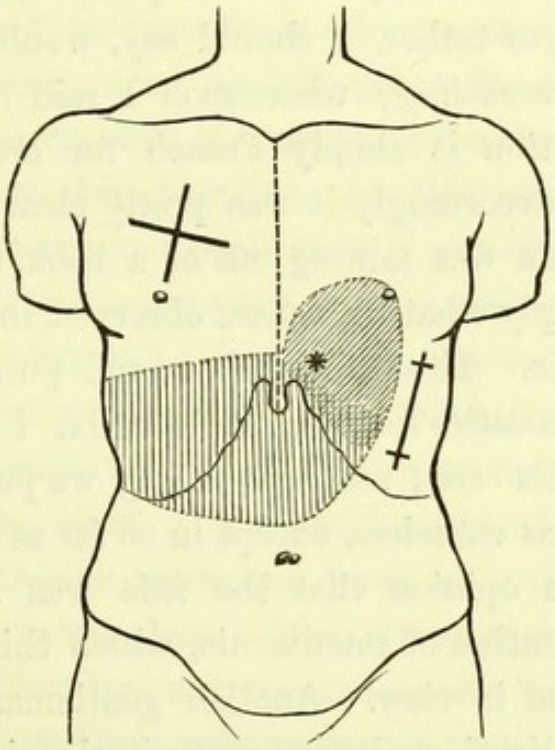

Fig. 11.

Front of thorax and abdomen in case of Thomas B. The shading indicates the dull percussion of heart and liver, displaced downwards and forwards by the emphysematous lungs. * Seat of murmur of tricuspid regurgitation. × and ‡, seats of peculiar emphysematous râle, and of tactile sensation, as described in the text.

The râle and the tactile sensation were found in two situations, viz., on the right front, and in the left lateral region (fig. 11, × and ‡); but the tactile sensation was much less distinct, even doubtfully present in the lateral

region; while both the râle and the tactile sensation were noticed over and over again in the right front. Now as to the râle, you will recollect that I fixed upon it at once as a sound not to be too easily disposed of, and I asked a considerable number of you to listen to it, and to give it the name that seemed most suitable to its character as heard by the ear. One of you immediately called it *crepitation*, but hesitated at calling it, or rather, I should say, would not call it *crackling;* whereupon I said that crepitation is simply French for crackling, and accordingly it was pretty clear to me that this gentleman was talking out of a book, and not expressing simply what his senses observed, independently of authorities. This independent and purely physical record of acoustic sensations was what I particularly wanted in this case; and accordingly we put down that observation as valueless, except in so far as it may have indicated an opinion that the râle was actually the special crepitation of pneumonia, which this gentleman no doubt had in view. Another gentleman called the râle "crumpling"—a better name in the circumstances, because not a hackneyed one. A good many said it was *more or less* crackling. Dr. Watson and I exactly concurred; and we did so independently. We both said the râle had a "shuffling" character, primarily; but we both said that it had something of "crackling" in it, as well. It was a combination of shuffling with crackling, as it were. This refers to the right front; in the left lateral region the râle was somewhat similar, but hardly

The râle.

Various opinions.

so abundant or distinct, and the shuffling character predominated. It was here, you will recollect, that we failed to realize distinctly the tactile sensation which was so evident in the right front; and the whole of the phenomena were less distinct and constant in the lateral region. This was apparent enough to all of you, and it was hardly possible to mistake the facts. Now as to the character of the tactile sensation. There was a jerking movement, as of something rubbing up and down against the walls of the chest. This at once suggested pleurisy, and the shuffling sound tended also in this direction; on the other hand the crackling quality (or crepitation, if you like a French word better) was suggestive of pneumonia. We discussed this point, and I told you that as we were getting into the realm of opinions here, we had better walk warily. We made careful inquiries into the antecedents, and failed to elicit a distinct history of pleurisy or pneumonia, though there had been some degree of ill-defined pain in the chest. Now it was upon this that I expressed to you my own opinion on the whole subject of this case; and you will remember that in order more clearly to discuss the matter we came into the lecture room, as I do not like to have these elaborate discussions in the wards. I said something to this effect—"It is quite impossible in this case to say absolutely that there is not pleurisy, or even pneumonia; pleurisy, especially, may exist, or its consequences may be there, without any symptom of a characteristic kind. But my *opinion* is, having regard to all the facts

The tactile sensation.

of the case, that this shuffling, or crackling, or crepitating râle, and this rubbing or jerking tactile sensation, are due not to pleurisy or pneumonia, but to the emphysema of the lungs, of which the other evidence in this case is complete and irresistible." Now many of you had never heard before of such signs as these in emphysema ; and I am not surprised at this, for some great authorities mention these signs only to say that they do not acknowledge anything of the kind, and most of your text-books, and particularly your *little books*, say nothing whatever about these signs, which nevertheless were very particularly described by no less an auscultator than Laennec himself. I brought you the book, accordingly, next day, and I read you over, carefully, Laennec's description. I shall do this again now, and pray keep in view, that while I do not accept all Laennec's theories as to the mode of production of these phenomena (which are apparently implied in his description of them), it is simply impossible to doubt for a moment that the sound we heard, and the sensation we felt with the hand, are the sound and the sensation heard and felt by Laennec. Of that I have no doubt at all, nor had I from the first, remembering, as I did, his description. Laennec said that this râle was a peculiar one, characteristic of emphysema ; he called it " râle crépitant sec à grosses bulles" —crackling râle *with large bubbles ;* but you must try to get rid of the impression, that by the word " bulles," or " bubbles," he means, as you would do, anything moist in its quality ; for he expressly says that this is a dry

Laennec's description.

râle (râle crépitant sec), and distinguished by its dryness from the " râle crépitant humide ;" with which latter he identifies, to some extent, in his description of it, the well-known crepitating râle of pneumonia. [Many of our authorities describe the râle of pneumonia as characteristically dry, which is no doubt a correct description, so far ; but some of them, Dr. Williams especially, will not allow that there is any moist element at all in the râle of pneumonia ; while Laennec's statements shew that he was of a different opinion ; for he first describes the crepitating râles generally, as moist ; then he picks out this "râle crépitant sec" of emphysema as distinct in kind from all the other crepitant râles, and distinguished by its dryness ; then he afterwards describes particularly *the crepitant râle of pneumonia*, about which so much has since been written ; and he describes it, evidently, *as belonging to the series of the moist crepitant râles, though a peculiar râle in that series*, in respect that " it presents the character of very small bubbles, very equal among themselves, *and it appears very little moist.*"* Observe,

* " Il présent alors l'image de bulles très-petites, très-égales entre elles, et il parait très-peu humide." *Auscultation Médiate*, t. 1, p. 417. Compare p. 96, where he uses, in respect to the " râle crépitant humide," the well-known similes of the crackling of salt, and of the blown-up dry bladder ; and says further, that this very râle is the pathognomonic sign of pneumonia in the first stage : and that over and above the crepitation, " il porte avec lui une sensation d'humidité bien marquée." I do not mean to attempt to reconcile this partial apparent inconsistency ; to me it simply shews that the dryness or moisture of the " râle crépitant " was, in the mind of Laennec, a distinction in degree, more than a distinction in kind, as maintained by Dr. Williams.

that he never once hints at the râle of pneumonia as resembling the *dry crackling râle* of emphysema, which it is evident he wishes to place in a different order altogether.]

Now here is Laennec's description of the "râle crépitant sec à grosses bulles, *or* craquement"—*cracking* (instead of *crackling*), as we might render this last word in English. [It is the only occasion, I believe, on which Laennec uses the word *craquement*, in reference to a sound in the chest.]—

"The *râle crépitant sec à grosses bulles*, or *craquement*, hardly exists except in inspiration ; it gives the sensation of air distending the dry and very unequally dilated pulmonary cells ; or even of air penetrating the cellular tissue surrounding the lung. The sound is altogether like that of a dry bladder which is being blown up." [The same similitude is used in a more modified sense, by Laennec, when describing ordinary moist crepitation at p. 87, so that it is quite clear that the inventor of mediate auscultation did not regard the distinction between dry and moist sounds as quite absolute, but rather used these words by way of comparison and contrast, according to the varying impressions which occurred to him in each special case, to aid the mind of the observer in apprehending his meaning. The same remark applies to the first sentence of the description above, which is certainly open, strictly speaking, to the charge of being somewhat fanciful, and too much pervaded by theory to be a safe guide for the mind, in appreciating the facts.] "This phenomenon is the pathognomonic sign of pulmonary emphysema, and of interlobular emphysema of the lung ; it is ordinarily much more marked in this last case. A like impression is experienced in sub-cutaneous emphysema, on applying the stethoscope over the affected part, and pressing with the ear in an interrupted manner, or compressing the surrounding parts with the finger in the same manner. This sign may be

employed even so as to recognize very deep intermuscular emphysema, in doubtful cases." *

"When pulmonary emphysema is very well marked, it can be discovered by means of a sign altogether pathognomonic, *i.e.* by a sort of dry crepitation which I have described in the first part of this work, under the name of *râle crépitant sec à grosses bulles* (see the passage above quoted); there is heard in these cases, when the patient inspires *or coughs*" [observe that, according to this extract, the râle may be heard in expiration], "a sound like that which would be produced by air blown into a cellular texture half-dried. This sound, like that of the ordinary crepitant râle, is very easily distinguished from it by this character, that the "râle crépitant sec" *carries with it the impression of dryness, while the other gives the impression of moisture;* and further, the *bullae* of the crepitant râle" [evidently he has here in view the râle of pneumonia], "appear small and equal as among themselves, while those of the 'râle crépitant sec' are large and unequal." [I must beg you to remark here in passing, that you could hardly have clearer evidence than this passage affords, that Laennec considered the râle of pneumonia as one of the moist râles, notwithstanding his comparing it elsewhere to the blowing up of a dried bladder, and the decrepitation of salt in a heated vessel.] "This phenomenon is rather rare, and of short duration in pulmonary emphysema" [*i.e. vesicular* emphysema, as he himself described it]; "it is ordinarily heard only for some instants, at remote intervals, and over limited spaces. It is, as we shall see, much more common and more durable in *interlobular* emphysema. I have seen some patients who experienced the sensation of a *cracking* (craquement) at the point and at the moment when the dry crepitating râle becomes audible; I have also, but very rarely, in lean persons, *felt in these cases an evident crepitation on pressing with the finger the corresponding part of the lung*, during inspiration or cough." †

"Interlobular emphysema is to be recognized by a sign alto-

* Auscultation Médiate, vol. i. p. 106.

† Idem, pp. 308, 309.

gether pathognomonic ; it is the *râle crépitant sec à grosses bulles*, very manifest and almost constant. I do not think that this sign is ever wanting in interlobular emphysema, and it is always more decided (prononcé) than in pulmonary emphysema. There is commonly, at the same time, a certain impression perceived" [*by the ear*, as appears afterwards] "*as though one or more bodies were moving up and down during inspiration and expiration, and rubbing along the ribs.* These phenomena present rather remarkable varieties ; they are commonly associated ; but one of them may exist alone, or they may alternate. The *upward rubbing* (frottement ascendant) occurs during inspiration, and it is at this moment also that the *râle crépitant sec à grosses bulles* is most commonly heard, *often completely masking it.* The *downward rubbing* (frottement descendant), which accompanies the expiration, is on this account much more commonly heard ; it is sometimes a single momentary rub, in other cases it is composed of two or three successive jerks (saccades) at distinct times, often heard quite close upon the expiration, or when it is nearly finished ; it seems, then, as if something descended and slipped back into its place. * * * These phenomena, furnished by mediate auscultation, *are frequently accompanied by a crepitation felt by the hand.* This last sign, however, is often absent, and commonly disappears before the auscultatory signs. In some cases, again, it is more easily perceived than these, or at least this is the case at intervals." *

I have quoted the greater part of what Laennec has written on these peculiar signs of emphysema, because I believe that you will readily recognize the facts we observed in this case, even through the medium of these somewhat imaginative expressions, founded on observations of half a century ago. I am no advocate, as you know, for authority in opposition to the study of nature ; but in this instance I firmly believe that Laen-

* Idem, pp. 343, 344.

nec was right, in the main, as to the presence of these signs of emphysema in certain cases. The distinction between interlobular and pulmonary emphysema is not always so clear as he makes it, and in very chronic cases I even hold that it cannot be made; very probably, therefore, Laennec unconsciously exaggerates the constancy of the signs in describing this latter disease. The doctrine of Laennec on this subject has been very generally disallowed by auscultators; but for some years I have been of opinion that the schools and the text-books were wrong, and that Laennec was right. [Let me add, as the result of a pretty close study of Laennec, that I find he is almost always right in what he asserts as simple fact, even when his theory is defective, or his description prompted by rather loose analogies.] Nothing can be more clear to me than that this case amply vindicates the reality (which has been doubted) of Laennec's observations on the signs of emphysema; the only question that remains is whether the signs are really pathognomonic, *i.e.*, whether these sounds and tactile sensations are produced, as Laennec supposed, by the emphysematous vesicles, or by some adventitious and accidental condition. Dr. Stokes (I should tell you) is one of the objectors to Laennec's views, and justly one of the most weighty and influential; but he does not, apparently, doubt the mere observation of Laennec, especially as to the *frottement ascendant et descendant;* he thinks this was owing to pleurisy concurring with the emphysema; and points out (what is curious enough, certainly), that Laennec,

while noticing this sign in emphysema, overlooked it in pleurisy, in which we now chiefly know it.* Now, this is to some extent a matter of theory, and it is very difficult to get cases bearing upon it which are free from objection; but I think this is almost such a case. I shew you here the lungs, but I must remark that the emphysema is now not nearly so distinct as at first; for since the lungs were cut into, two days ago, the bullae have mostly collapsed. By comparing accurately the seat of the chief emphysematous lesions with that of the physical signs as shewn in the diagram, and by inflating the lungs to make sure that we overlooked nothing, we were able to make observations which Dr. Haldane has carefully and accurately reported, and which, I think, leave no doubt whatever upon the subject; for although there were old pleuritic adhesions over a part of the right lung, the seat of these adhesions did not at all correspond with the seat of the emphysema, or with that of the phenomena observed during life. You can look into this matter for yourselves, however, after lecture. You will find also that our other observations upon the state of the heart and liver are fully justified in all points by the report. There can be no doubt, in particular, considering the great size of the tricuspid orifice, that we rightly judged the murmur to be one of tricuspid regurgitation.

Extract from Dr. Haldane's report (Register of Dissections, XX. No. 518. Case of Thos. B., 12th March, 1862). "There were some very limited loose cellular adhesions over several

* Stokes' Diseases of the Lungs, etc., pp. 193, 194.

points of the lateral surface of the right lung, and some, rather more extensive, over the posterior surface and over the anterior part of the base of the organ. The pleura surrounding the lower third of the lung, on its anterior and external lateral aspect, was smooth, thickened, and semi-opaque. There was a moderate degree of vesicular emphysema of the apex of this lung, as well as of the anterior surface of the upper lobe (where there were a few small bullæ), and of the base of the organ. There was also some interlobular emphysema immediately below the apex, but still more over the anterior surface of the lung, in a space comprehended between the upper margin of the third and fifth ribs, where there were pretty numerous bubbles of air below the pleura. At the base of the lung were two or three patches of imperfect collapse, which disappeared on inflation.

"There were no adhesions on the left side. There was vesicular emphysema of the upper part of the anterior margin of the upper lobe, and, still more marked, of nearly the whole of the lower lobe, which scarcely collapsed on removal from the chest.

"The right side of the heart was evidently enlarged. The organ weighed 14½ ounces. The left ventricle was about the natural size; the right dilated and evidently thickened. The pulmonary artery was wider than the aorta; the semilunar valves in each vessel were natural. The tricuspid orifice was considerably dilated; the mitral orifice and valve were natural. The measurements were

Circumference of Aorta at origin . .	3.3	inches.
„ „ Pulmonary artery, at do.	4.1	„
„ „ Mitral orifice . .	4.7	„
„ „ Tricuspid do. . .	6.9	„

"There was a very considerable amount of fatty degeneration of the muscular substance of the heart in both ventricles.

"The liver and spleen were somewhat congested; the other organs of the abdomen normal."

XVII.

ANEURISM.

THE following cases are a selection from a considerable number that have occurred to me as illustrating different aspects of diagnosis, and also some points of prognosis and treatment, in one of the most distressing and puzzling of diseases. Their general bearing is towards illustrating the great importance of a very exact and truly *physiological* study of the symptoms in all forms of disease which may possibly have this solution; and it has therefore appeared to me expedient to preserve to a certain extent the commentaries made at the time in publishing some of these cases, especially those in which the diagnosis, as actually made, was defective. Where the statements of opinion, however, have appeared to me to require modification in accordance with later experience, I have not hesitated to make a few alterations, not affecting the general principles involved, or their application to the particular case under discussion. The original references will be found in the note below.*

* Monthly Journal of Medical Science, vol. x., p. 83; vol. xiii., p. 137; vol. xvi., p. 114. Edinburgh Medical Journal, vol. i., pp. 71, 143, 429. Medico-Chirurgical Transactions, vol. xlii., p. 189. Additional references to the subject in Edin. Med. and Surg. Journal, vol. lxxxii. Case-book, p. 12. Monthly Journal, vol. xix., p. 79; vol. xx., p. 71. Edin. Med. Journal, vol. ii., p. 87.

CASE I.*—*Symptoms of laryngeal disease; aneurism of the aorta, arising from the back part of the arch, involving the left recurrent laryngeal nerve, and fatal by suffocation—Rust-coloured expectoration due to continuous slight hemorrhage from the sac; no considerable hemorrhage—Questions of diagnosis—Question of tracheotomy.*

Thomas O'B——, æt. 46, a robust labourer, was admitted into the hospital at the hour of visit on the 30th May 1851. He complained of great dyspnœa, which, in the recumbent posture, was so extreme as to threaten suffocation. The breathing was sonorous, with a distinctly stridulous character on inspiration. The countenance anxious and flushed; no fever or pain complained of. Expectoration considerable; the chest was examined as well as his state permitted, and revealed only slight bronchitic râles, the harsh laryngeal breathing being heard all over the bronchi. The voice was evidently produced with effort, scarcely husky, but having a somewhat muffled character; there was no tenderness over the larynx; the epiglottis and throat were natural. Shortly after his removal to a ward, the paroxysm subsided to some extent. I then learned that this was only an accidental exacerbation of a state which had existed for some months, and for which he had undergone active treatment. A blister was applied to the nape of the neck; and he was ordered ipecacuan wine ℥ss every second hour.

Laryngeal Dyspnœa.

On the 31st he had slept well; but the dyspnœa had returned in the morning; at visit he was better, but not able to lie down. The chest was examined with more care; no abnormal percussion at any part; the respiratory murmur everywhere abundantly audible, and natural; some coarse mucous râle in both backs, and a few dry bronchial râles elsewhere; the sounds over the region of the heart and of the great

* Read to the Medico-Chirurgical Society of Edinburgh 18th June 1851.

vessels strictly normal. The pulse was hurried, but natural in character.

He continued in much the same state till June 4th; paroxysms of extremely difficult breathing occurring three or four times a day, and lasting generally from twenty to thirty minutes. He expectorated daily from six to eight ounces of frothy mucus, tinged of a distinct rusty colour, with streaks of purple. Repeated examination of the lungs, however, shewed that they admitted air abundantly in every part, and were free from every physical sign of disease. The treatment was not altered; the operation of tracheotomy was proposed, and, though urged as a certain means of relief from the paroxysms, was objected to in the strongest terms by the patient, who said he would die rather than submit to it.

Hemorrhagic Expectoration.

Operation proposed.

On the morning of the 4th June, between 7 and 8 A.M., he had an attack of laryngeal suffocation of peculiar intensity, accompanied by dull pain in the lower part of the chest. He was seen by the resident clerk, and again refused the operation; he seemed to obtain relief by being supported in the erect position, and walking up and down the ward, friction being also applied to the front of the chest. Another paroxysm, not so severe, occurred in the afternoon. At half-past 7 P.M. there was a return of the paroxysm. He was seen at 8 o'clock, when he was livid and exhausted; he expressed a desire to be bled, and again refused to permit tracheotomy. At half-past 8 the dyspnœa was intense, the lividity of lips very great; the face generally pale; the skin covered with cold sweat. The operation was performed by the resident surgical clerk in attendance, but the patient was nearly asphyxiated before the tube was introduced. He continued after the operation to breathe slowly and at long intervals; the pulse continued perceptible for about ten minutes, but he did not rally, and died about a quarter of an hour after the tube was introduced. Artificial respiration was employed without effect

Tracheotomy.

A small quantity of blood was lost during the operation, some of which entered the trachea, and was apparently expelled again with considerable force.

Post-mortem examination, 6*th July*.—The body unusually robust; post-mortem lividity considerable; rigor mortis well marked. No emaciation either of fat or muscle.

Pleuræ containing little fluid; adhesions at apex of right lung, corresponding to a few encysted cretaceous concretions, little larger than a barley-corn. A little emphysema in the anterior parts of both lungs, and slight collapse of the tissue posteriorly, otherwise they were healthy. The greatest bronchi had the mucous membrane slightly congested, and contained a considerable quantity of tough mucus and muco-purulent matter rather deeply tinged with blood; but nowhere any distinct coagula.

The heart weighed 12½ oz.; its muscular tissue much congested. On the aortic valves, which were quite competent, and not at all deformed, there were one or two very minute granulations, and a few similar ones on the inner membrane of the vessel near its origin. The other valves perfectly normal.

The thoracic aorta had its inner membrane throughout uneven and thickened, but with little distinct abnormal deposit. The arch presented no general dilatation; it was, however, slightly dilated upwards at the root of the innominata; and this vessel, as well as the origin of the right subclavian, was uniformly large relatively to the vessels on the opposite side. The two carotids were of equal size; but both of them, as well as the left subclavian, were very slightly expanded at their origin.

At the back part of the arch, half an inch below and between the origin of the innominata and left carotid, was an oval opening, through which a hazel nut might be passed lengthways. Its edges were tolerably smooth and rounded; and it was three-quarters occluded by a mass of firm granular coagulum, which passed from this opening into the aneurismal sac beyond. This was of the size of a walnut, and was situated between the aorta and the trachea, being adherent to the perichondrium of some of

the tracheal rings; the sac was nearly full of laminated, decolorized coagula, with a little fluid blood.

The left recurrent nerve, emerging from below the aorta, passed immediately to the left of the sac, and rather behind it, being bent over it, and at one point almost imbedded in the thickened cellular tissue which surrounded it; at this point there were also one or two indurated lymphatic glands around the nerve, dark from carbonaceous deposit. The pneumogastric nerve on both sides, and the recurrent on the right, had their normal relations, excepting that the subclavian artery, where it was surrounded by the right recurrent, was, as before mentioned, somewhat dilated.

The tongue rather brown, and dry in front. Its root, and the fauces natural.

The epiglottis normal in size and form; its mucous membrane faintly rose-coloured on the posterior aspect, and displaying a somewhat granular surface, from prominence of the mucous follicles, especially in the neighbourhood of the arytænoid cartilages. Ventricles of larynx and vocal cords natural.

The cricoid cartilage and three upper tracheal rings divided by a perpendicular incision in the middle line.

The mucous membrane in the larynx and upper fourth of the trachea nearly natural in colour and appearance. Below this the mucous membrane presented rose-coloured vascularity, deepening towards the bifurcation, on the left side, into purple. The mucous membrane slightly granular throughout this injected part from hypertrophy of the follicles.

About an inch and a quarter above the bifurcation on the left side there was a circular opening, admitting readily a crow-quill, and passing into the aneurismal sac before mentioned which lay in contact with the outside of the costal cartilages.

Nearer the bifurcation there were three or four small points slightly elevated, and of an opaque yellowish colour, as if the mucous membrane were stretched over some abnormal deposit. The cartilages of the two tracheal rings immediately behind the

opening were entirely separated from their perichondrium at the part opposite the aneurismal sac.

The abdominal viscera were congested as usual in asphyxiated persons, but had no other morbid appearance. The abdominal aorta was not so uneven internally as the thoracic, but presented more distinctly atheromatous opaque deposit in its inner membrane.

The first question which suggests itself in connection with this case is, *What was the cause of death?* On this point, I think, a consideration of the whole circumstances will leave no doubt that the patient died chiefly from laryngeal suffocation, induced by pressure of the sac on the recurrent nerve of the left side. The occurrence of suffocation from this cause is too well attested by numerous cases of aneurism and tumours of the chest now on record, to admit of reasonable doubt. The evidence adduced by Dr. Hugh Ley upon this subject in his work on laryngismus stridulus, although certainly insufficient to establish his exclusive theory of that disease, is strongly confirmatory of the correctness of the views entertained nearly two centuries ago by Willis as to this source of death in some intra-thoracic tumours. The experiments of Legallois, and the far more elaborate and satisfactory ones of Dr. John Reid, have demonstrated, in the most unquestionable manner, the production of laryngeal suffocation by various kinds of interference with the recurrent nerve on one or both sides of the neck. "From the experiments we have detailed," says Dr. Reid, "it is apparent that severe

Death due to Laryngeal Suffocation.

dyspnœa, amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or the trunks of the pneumogastrics. For when both, or *even one recurrent nerve*, was irritated, the arytenoid cartilages were approximated, so as in some cases to shut completely the superior aperture of the glottis."* Section of the vagi, also, according to Dr. J. Reid, produced "sudden and violent attacks of dyspnœa, which generally went off in the course of a very few minutes, when they did not terminate in suffocation;" leaving, however, the animals liable to renewed paroxysms on the occasion of a violent struggle, or any exertion tending to hurry the respiration. It is unnecessary to enter into the physiological details and principles connected with these curious results; it is sufficient for the present purpose to observe, that they fully explain the numerous cases recorded in pathological and practical works from the time of Bonetus, in which tumours involving these nerves (in the great majority of cases aneurismal) have been shewn to produce death by sudden orthopnœa, often independently of any pressure directly on the air-tubes. Indeed it is worthy of remark, that spasmodic dyspnœa is a cause of death in a very considerable proportion of cases of aneurism of the aorta.† It is sufficient to refer, in illustration of

* Physiological, Anatomical, and Pathological Researches, p. 120. See also pp. 167 and 272.

† It may even be said that this symptom is rarely absent in those aneurisms which spring from the back part of the arch of the aorta. See Dr. Greene's collection of cases of this kind in the Dublin Quarterly

this point, to the cases by Drs. Graham and Alison, communicated to the Edinburgh Medico-Chirurgical Society in 1835,* in which aneurisms of the aorta were accompanied by marked laryngeal dyspnœa from this cause, in Dr. Graham's case altogether simulating a primary laryngeal affection; to a similar case under the care of Dr. Todd,† in which the recurrent nerve of the left side, and all the muscles to which it was distributed, had undergone atrophy from the pressure of the tumour; to the case of aneurism of the innominata, detailed by Mr. Lawrence,‡ in which death took place from suffocation, tracheotomy being proposed but not performed; and to several examples of this form of dyspnœa detailed in a paper by Dr. Henderson,§ and in the work of Dr. Ley before referred to.‖ I shall only say farther, that the violent paroxysms of dyspnœa experienced by my patient on many occasions before they were actually fatal, the highly stridulous respiration, the difficulty he evidently had in producing vocal sounds, and their altered character, pointed unquestionably to the glottis as the source of his danger; and the absence of any physical signs or morbid appearances indicating serious pressure of the aneurism on the trachea render it not

Journal, No. 3, new series; and Mr. Crisp's Table of aneurisms, Treatise on the Blood-vessels, p. 235; for numerous instances bearing on this point.

* Edin. Med. and Surg. Journal, vol. xliii., p. 292, *et seq.*

† Lancet, June 1841, p. 400.

‡ Medico-Chirurgical Transactions, vol. vi.

§ Monthly Journal of Med. Science, 1841, p. 10.

‖ On Laryngismus Stridulus, etc., p. 453, *et seq.*

probable that death can be ascribed to this cause. There remains only one other possible cause of suffocation, and to this I am willing to allow its due influence. The exhaustion consequent upon the numerous attacks of dyspnœa during the last days of life evidently told severely upon his strength; and the considerable and increasing quantity of mucus in the bronchi must have been expectorated far less freely in the last hours of his existence. The accumulation of this mucus, which was found in the larger tubes after death, fully explains why the operation of tracheotomy performed *in articulo mortis*, was followed by so imperfect a result. It is worthy, however, of remark, that up to the last visit which I paid him, the evidence of obstruction in the bronchi continued to be not greater than in cases of very slight bronchitis, and fully warranted the idea, that the greater part of the mucus expectorated came from the upper part of the air-passages. This idea corresponded also with the morbid appearances in the dead body.

The muco-purulent matter found in the larger bronchi after death was considerably more tinged with blood than that expectorated at any period during his fatal illness. But it is very doubtful whether this increased hemorrhage was from the sac; as I am told that a good deal of blood was drawn into the trachea during the operation. At all events, it is clear that hemorrhage was not connected with the fatal event, nor did it ever form a serious complication,—never amounting to more than was

Death not due to hemorrhage.

sufficient to give a purplish, and often only a rusty tinge to the expectoration.

With this absence of material hemorrhage, it is important to ask, *how long the communication of the sac with the trachea had existed before death*? The lungs having presented no symptoms of disease, and being found after death free of all serious lesion, it is difficult to suppose that even the small quantity of blood in the sputa had any other source than the aneurism; and yet we have evidence that, if this be so, the opening must have continued for months, yielding only these small quantities, as the patient distinctly stated that he had at no time coughed up clots of blood. When we consider the nearly complete occlusion of the sac by coagula, this phenomenon will appear less difficult to understand; at all events, it is far from rare in the histories of aneurisms opening upon mucous surfaces, and especially into the air-passages, to find, even after one serious hemorrhage has given evidence of a rupture of the sac, that the hemorrhage reduces itself to a very trifling amount, and sometimes is altogether suspended. The case of Mr. Liston at once suggests itself as an illustration of this fact. In the only record published of the fatal illness of this distinguished member of our profession, it appears that the first hemorrhage was followed by a period of exemption from symptoms, and that when these recurred, it was in the form of a cough attended with expectoration, which was "difficult, small in quantity, and of a rusty colour;" no further material hemorrhage oc-

Though hemorrhage long-continued.

curring till his death, which was from orthopnœa.* In the remarkable case of abdominal aneurism which follows in another page,† it is much more difficult, owing to the situation of the first opening into the duodenum, to judge of the amount of blood that may at different times have been ejected, and it is highly probable that some bleedings may have taken place unobserved; but it seems in every way probable that no considerable hemorrhage occurred during twenty-two months, from an aperture which had evacuated gallons of blood in a few days, and which was found very nearly, if not entirely, sealed up after death.

Could the aneurism have been discovered during life? On this point we have the following data:—no dull percussion, abnormal pulsation, or tremor at the upper sternum; no abnormal sound over the heart and great vessels in front or behind; normal and symmetrical percussion over the lungs in every part; no abnormal respiratory sound over the trachea in front or at the root of the lung behind; abundant and symmetrical respiratory murmur in both lungs and over every part of them, mixed with slight mucous râles behind, and a very little sonorous râle in front. These physical signs, in regard to which full and careful examination may be relied on, form the elements of a tolerably complete negative diagnosis of aneurism, the suspicion of which was certainly entertained at the second examination of

* *Lancet*, December 11th, 1847.

† Case vi., p. 495. See also further remarks on this subject in connection with case vii., at p. 514, *et seq.*

the chest, but soon dismissed, the case being treated as one of laryngeal affection. Perhaps it is still doubtful if more than a bare guess could have been formed under the circumstances; but the event shewed that the suspicion thus negatived by physical diagnosis was allowed too hastily to be driven from the mind by the apparently greater probability of an ulcerative lesion of the larynx and trachea; and during the short period he was under my care no time was given for considering the case in all possible points of view, especially as the indications of practice appeared sufficiently distinct. After a careful consideration of the diseased parts, however, I am led to think it probable that while no sure sign of aneurism of the aorta could have been made out, a very close and accurate examination at the root of the neck, and in the course of the arteries, *might possibly* have discovered the dilatation of the innominata and subclavian vessels. Circumstances which did not come to my knowledge till after the patient's death were also calculated to arouse suspicion. The long persistence even of a trifling amount of blood in the expectoration would certainly have justified the belief in something more than an ordinary ulceration in the larynx; and a pain which he is said to have suffered at one time at the upper part of the sternum, but which was not complained of during the last part of his illness, would have confirmed the diagnosis of some fault in the thoracic region. The absence of any marked tenderness on pressure over the larynx, and of swelling of the epi-

Diagnosis erroneous.

Corrections in detail.

glottis, was calculated to attract, and did attract, attention from the first; but this negative circumstance was considered as outweighed by the rest of the evidence.

It is worth while to remark, although it is difficult to obtain any accurate data on the subject, that the combination of symptoms presented by this case may probably be expected not unfrequently to occur in chronic or acute ulceration of the laryngeal mucous membrane. Local pain and tenderness are by no means of constant occurrence in these cases; neither can alterations of the epiglottis and upper vocal cords be recognized in all cases, though some kind of local symptom will doubtless be accessible in the great majority. On the other hand, the presence of blood in the sputa, though of course a suspicious circumstance when long continued, is neither universally present in aneurism, nor always absent in laryngeal ulceration.* I have lately seen a case almost precisely parallel to this one in every important feature, in which paroxysmal laryngeal dypsnœa, apparently very little under the influence of remedies, and accompanied for a considerable period by blood in the expectoration, is probably due to a primary laryngeal affection, of which the local symptoms have lately become more distinct, while physical signs of aneurism remain, after repeated and careful examination, undiscoverable. If this man remains under

* A more critical estimate of the value of this symptom in aneurism, will be found in connection with Case IV. See also case of Peter B., Article XIV., where the blood really depended on tubercular disease, though at one time suspected to be from aneurism.

observation for a sufficient length of time, it is probable that a more secure diagnosis may be formed; but at first it would have been impossible to act on an assured conviction either of thoracic or laryngeal disease, while the state of the patient has been, and continues such, as may render a recourse to tracheotomy an extremely necessary expedient for his security, or even his rescue from impending death.*

In reference to diagnosis, the practical conclusions which follow from the above remarks are no less evident than important. The mistake of an intra-thoracic tumour for a laryngeal affection is one of those accidents which has probably occurred in practice far more frequently than it has been accurately recorded; although a sufficient number of instances have been published to shew that it may readily occur in the most careful hands, in the absence of stethoscopic exa-

Practical Cautions.

* After the above was published in the *Monthly Journal*, this case terminated fatally by profuse hæmoptysis and consequent suffocation; the source of the symptoms having been shewn on dissection to be an aneurism, arising at the back part of the innominate artery, and bursting into the lower third of the trachea. The case was most sedulously and accurately examined by three physicians, under the express suspicion of aneurism, and every known physical sign of that disease was sought for in vain. The patient was likewise seen by a fourth physician, not of Edinburgh, but of large special experience in regard to affections of the throat and larynx, who unhesitatingly pronounced the epiglottis and neighbouring parts ulcerated and thickened,—a diagnosis which I am by no means prepared to justify, but which, with some slight redness of the mucous membrane, plainly enough discernible to every one concerned, gave a bias to the opinion expressed in the text. See p. 470 for further particulars of this case.

mination. It cannot, therefore, be too strongly insisted on, that a physical examination of the chest should take place in all cases of supposed laryngeal disease. This is indeed an invariable rule with all careful practitioners, on many grounds; although it may be doubted whether the lungs and air-passages do not often too exclusively absorb attention in such examinations. But the present case, while it proves still more strongly that no amount of care in the examination of the chest, and especially of the great vessels, is superfluous, also shews, I think, conclusively, that the absence of the physical signs of aneurism or tumour should not suffice to remove completely the suspicion that they may be concerned in the affection of the larynx. It is obvious that the part of the aorta most apt to be affected in these cases is the middle or transverse portion of the arch, and particularly its posterior or inferior surface, where it is most removed from the possibility of physical diagnosis. It is also evident that a very small tumour in these situations is enough to give rise to all the symptoms of laryngeal obstruction. Dr. Todd correctly remarked, in his clinical lecture upon the case above alluded to as having occurred under his care, that "most observers had attributed these symptoms (those of chronic laryngeal affection) to compression of the trachea and bronchi, and had overlooked the condition of the recurrent nerve." In the present case, as in that of Dr. Todd, "there was the most ample evidence that the pressure upon that nerve occasioned the laryngeal distress."

With regard to the treatment of such cases, the present narrative seems also not devoid of instruction. Had an aneurism been discovered or strongly suspected in this case, it seems probable that general blood-letting, together with such remedies as would have contributed to control the heart's action, might have been pursued farther with advantage to the patient; whereas the chronic nature of the supposed laryngeal affection, and the active treatment to which he had already been subjected, were accepted as sufficient reasons for foregoing these remedies and trusting to blistering, ipecacuan, and the performance of tracheotomy. This operation was absolutely refused by the patient while he had sense and vigour, and the case accordingly adds one more to those in which the operation was performed too late to be of any service; but I think it is impossible not to admit that it would probably have prolonged life had it been performed at any period before the final agony, and that the patient's sufferings throughout his illness would have been greatly less severe had advantage been taken of one of the earliest threatening paroxysms of dyspnœa to place a tube in the trachea. In a clearly ascertained case of aortic aneurism, such a proceeding could of course only be proposed as a temporary relief from immediate and pressing danger; and even in this point of view it could only be prudently recommended after careful examination had ascertained the freedom of the lung and of the air-tubes from any considerable pressure; but under these circumstances, I should certainly not hesi-

Tracheotomy.

tate in offering to the patient the benefit, even though temporary, which this operation is calculated to afford. Much less should I feel justified in withholding it, on the ground of the uncertainty of diagnosis, in cases like the present, where an obvious laryngeal spasm exists, the source of which cannot be discovered, but which is unconnected with any other ascertainable affection of the respiratory passages.

CASE II.*—*Symptoms of laryngeal disease, with continuous slight hemorrhage in the expectoration. Negative result of physical examination of chest. Unfounded diagnosis of ulceration of epiglottis, and error thence arising. Aneurism of the innominate artery, involving the right recurrent nerve. Death by hæmoptysis, after repeated threatenings of laryngeal suffocation.*

Shortly before the preceding case was read to the Medico-Chirurgical Society, and while the impressions derived from it were in full force, a robust labourer, æt about 35, came under my observation in the Royal Infirmary, during the absence from town of the late Dr. Andrew, for whom I was then acting as assistant-physician. He had very much the same history and symptoms as the former, the symptoms of his case being frequent attacks of dyspnœa, evidently spasmodic, and accompanied by laryngeal stridor, but without fever, tenderness over the larynx, or any ascertainable local laryngeal affection, except slight and doubtful redness about the arches of the palate. The expectoration had been at various

* This case is the one referred to in the note, p. 467. It was not recorded at length during the life of the patient, but the facts were carefully minuted soon after the termination of the case, and the state of the parts on dissection is to be found in the Pathological Register of the Royal Infirmary.

periods considerable, muco-purulent, and intermittingly tinged, streaked, or stained with blood; there was no trace or sign of any considerable affection of the lungs or bronchi, and the heart and great vessels appeared in all respects healthy. The examination was in this case made with the express suspicion of aneurism on the mind, and every known physical evidence of thoracic aneurism was sought for with the greatest care. In particular, the vessels at the root of the neck were explored with the fingers, by the stethoscope, and by percussion, without giving a trace of anything abnormal; and the trachea and bronchi, examined both at the front and over the root of the lung behind, shewed the air-passages to be free from material obstruction, except at the larynx. Being thus foiled in discovering proof of an aneurism, and yet by no means satisfied of its absence, while, at the same time, the laryngeal symptoms appeared frequently to approach the point of threatening life, I told the patient that the last resource for him would be to open the windpipe, but that all other means would be tried first. This conclusion was steadily kept before him for some days; and though strongly protesting against the extreme measure, he submitted very attentively and quietly to sedative and antispasmodic treatment, which, in a week or two, seemed to have somewhat diminished the violence of the spasms, and reduced the frequency of their occurrence. On one occasion I requested Dr. William Robertson to see him with me, with a view to the complete re-investigation of the question of aneurism; and the physical examination of the chest and root of the neck was again made with the utmost care by Dr. R., with a negative result. The sputa were still sanguinolent, though often only slightly so, and very slight tenderness could be apparently elicited over the larynx. Tracheotomy was never insisted on with urgency, owing partly to the strong suspicion which still remained, that the laryngeal affection was dependent on an aneurism within the thorax, and partly to the slight abatement in the symptoms, which removed the principal apparent reason for urging the operation on an unwilling patient. On handing over the patient to Dr. Andrew on his return, the same course was

pursued, and the chest was again repeatedly examined without any further discoveries to which importance could be attached as indicating aneurism. Shortly afterwards, I requested a physician,* who was at that time on a short visit to Edinburgh, and who has a large special practice in diseases of the throat and larynx, to examine this patient, telling him, at the same time, the doubt which appeared to hang over the case. This gentleman at once pronounced the epiglottis and the neighbouring parts to be ulcerated and thickened; and his opinion was given so confidently, as the result of examination by touch, and even by sight, that the lingering doubt of aneurism was for the time dissipated; and I believe Dr. Andrew was even induced to make a few trials of the solution of nitrate of silver, applied with a sponge to the glottis. No very material change took place in the symptoms; but the difficulty of breathing underwent no diminution; the sputa remained purulent and sanguinolent, and a few weeks afterwards the patient expired suddenly from profuse hæmoptysis.

Dissection discovered an aneurism, the size of a large date communicating with nearly the entire length of the trunk of the innominata, and projecting from its orifice backwards towards the lower third of the trachea, into which it opened near the mesial line. The aneurism pressed on the right recurrent nerve, which was flattened and involved in the sac. The internal muscles of the larynx, on the right side, were slightly atrophied. The mucous membrane of the larynx and epiglottis was perfectly pale and normal, unless a very slight and scarcely appreciable development of its mucous follicles be considered a disease. There was certainly no trace of ulceration, even the most superficial. The right carotid and subclavian were of normal calibre; the arch of the aorta was not dilated; the heart was nine and a half ounces in weight, and normal; the aneurismal sac was nearly full of coagulum; and this circumstance, together with its deep position, may probably account for the expansion being so slight as not to be appreciable at the root of the neck during life.

* Dr Horace Green, of New York.

Remarks, May 1862.—This case can hardly fail to make clear to many, as it did to me at the time of its occurrence, the extreme importance of the functional symptoms of thoracic aneurism. Like Case I., but all the more strongly from the circumstances in which it occurred, it shews that the direct physical evidence of aneurism may be most deficient in the very cases in which it would be of the greatest value and importance; in other words, that life may be threatened from day to day, and serious questions of treatment may arise and press for settlement, even where the true character of the disease may elude the most careful physical diagnosis. The lesson was not, indeed, lost upon me, and I trust it has made me more careful and sensitive (so to speak) in weighing all the most minute details of evidence in all doubtful cases of thoracic disease; but even now I cannot see how we could have avoided the performance of tracheotomy in this last case, as in the former, had the laryngeal symptoms attained the same degree of intensity, or had they even remained as threatening as they were for a short time after the patient's admission. Moreover, from the anatomical relation of the aneurism in this as well as in the former instance, there is little doubt that the recurrent laryngeal was the only important nervous trunk implicated in either case; and it seems highly probable that the frightful and agonising dyspnœa which attended both cases, and especially the former, would have been materially relieved, and the tenure of life rendered for a time less precarious, had the operation been performed. I am still, therefore, of

opinion that it might have been performed with propriety, though not, of course, with a permanently good result.

CASE III.*—*Symptoms of laryngeal disease treated at first as laryngitis; ultimate diagnosis of aneurism. Operation of tracheotomy in urgent circumstances. Prolongation of life for twelve days.*

(Communicated by Dr. Gibson of Dundee.)

W. R., an athletic seaman, aged thirty-four, on the 31st of October 1851, fell to the ground from a height of several feet, suffering what appeared, from a careful examination which I made shortly afterwards, to be simply a severe muscular contusion of the right shoulder and side of the chest.

From the effects of this accident he had to all appearance, with the exception of some remaining stiffness and weakness of the limb, quite recovered at the end of three weeks, when he returned to duty; and I accordingly lost sight of him till he called on me again, on the 22d of January 1852, to consult me respecting what he thought a bad cold, which had been hanging about him, he said, for ten days or a fortnight; and I confess that, at the time, I thought he was right. He had the muffled husky voice, the sharp ringing cough, the dyspnœa, and stridulous inspiration, of a sufferer from acute laryngitis. He confessed to an uneasy sensation at the top of the windpipe, which pressure over the larynx increased, though it scarcely amounted to pain; and

* This interesting record of a case occurring shortly after the publication of Case I. in the *Monthly Journal of Medical Science*, was kindly placed at my disposal by Dr. Gibson, to whom it occurred; and as it was published in this connection in a subsequent number of the Journal, I retain it here as part of the evidence, although it is in no sense of the word an observation of mine. The narrative speaks for itself so clearly that I omit all commentary on this case.

as a careful examination, by percussion and the stethoscope, failed to detect anything wrong with either the heart or the lungs, and he had no pain of chest, I at once concluded laryngitis to be the disease under which he was labouring.

He told me, moreover, that he had, again and again, while at work, been suddenly seized with a feeling of instant suffocation, which he referred to his throat, and under which he had more than once come to the ground in a momentary state of unconsciousness. All this seemed still further to point to the glottis as the chief seat of the mischief; and as his pulse at the same time was hard, frequent, and jerking, my treatment was correspondingly active.

Into any details on this point, however, it is needless to enter. Suffice it to say, that it was only after the successive adoption and failure of every remedial measure suggested by the belief that the case was one simply of laryngeal disease (a view of its nature in which my friends Drs. Munro and Matthew Nimmo entirely concurred), that I began to entertain serious doubts of the accuracy of my diagnosis; and that the suspicion that this was perhaps, after all, an instance of aortic aneurism, simulating, through its pressure on the recurrent nerves, disease of the larynx, now first struck my mind on my meeting, in the course of my reading during this my dilemma, with a paper by Dr. W. T. Gairdner, on an analogous case, and the details of another by Dr. W. H. Gooch; the former in the "Monthly Journal of Medical Science" for August 1851, the latter in the "Provincial Medical and Surgical Journal" for February 1852. These together threw quite a new light on the case. Nor did the universally acknowledged obscurity in the diagnosis of intra-thoracic aneurism during life in some instances appear to me to render less probable the accuracy of this view of the case. For, though auscultation still failed, at the end of two months of close observation, to detect in the chest of my patient the least signs of aortic disease, it was nevertheless easy, with the evidence of similarly obscure cases before me, both to surmise its existence, though too deep for detection, and to find a likely cause in the accident he had so recently met with.

I ought to add that Dr. Alison, whom my patient now consulted in Edinburgh, took, on the whole, the same view of the case.

The treatment was now for some weeks longer directed accordingly, in the hope that, under the effects of complete bodily and mental repose, extremely low and spare diet, the occasional abstraction of blood by venesection and cupping, and the administration of such sedatives as digitalis and opium, the disease might be at least kept in check, and time afforded for Nature's attempts at a cure. But his condition, on the contrary, became progressively worse ; his dyspnœa, and stridulous breathing, and cough, more distressing and constant, and the paroxysms of laryngeal suffocation in particular, so frequent and alarming upon the slightest exertion, that, with the concurrence of the two professional friends whom I formerly mentioned, I determined on opening the windpipe as the only means left of prolonging his life ; though indications which the stethoscope had latterly given of pressure upon, and consequent narrowing of, the left bronchus especially, made this hope somewhat doubtful. I ought also to mention, that a very slight *whizz* was now to be detected, we thought, with each systole of the heart, at the upper part of the sternum.

Such was the patient's state when, at midnight of the 6th of last May, and with the kind assistance of these gentlemen and Dr. James Drummond of Edinburgh [now of Glasgow] I performed tracheotomy ; the suffocative agony, and the whole aspect of the patient indicating that there was no time to be lost.

It saved his life at the moment, and he lived for twelve days thereafter ; for although, probably owing to pressure at the root of the lungs, respiration continued still very laboured and panting, yet he managed to breathe through the opening, and while he also got rid of large quantities of ropy mixed purulent matter by coughing, he remained free from those paroxysms of laryngeal suffocation under which, previous to the operation, he had so often nearly expired. He at last sank from sheer exhaustion apparently, scarcely having closed his eyes in sleep for a fortnight or more.

Permission was given us to open the body ; but I deeply re-

gret to say that, having carefully removed the whole parts involved with a view to their being examined with more leisure at home than we then could command, and unavoidable delay having occurred in so doing, they unfortunately went so fast into a state of complete putrefaction in the keeping of another professional friend, who was present, and who undertook to preserve them, as to be found quite unfit for the purpose. Nevertheless, we saw enough at the time of our first somewhat hurried inspection to satisfy us, that a tumour of about half the size of a fist, which we found closely attached to the under part of the aortic arch, was aneurismal ; the left recurrent nerve being, at the same time, seen passing into the condensed areolar tissue, with which it was closely invested.

CASE IV.—*Paroxysmal dyspnœa, of purely asthmatic character (not laryngeal) ; expectoration blood-stained ; physical signs of pneumonia ; aneurism of descending aorta, opening into left bronchus, laryngeal nerves not involved ; physiological diagnosis.*

W. G., porter, robust, full-blooded, aged about sixty. Admitted into the Royal Infirmary, August 9th, 1851, at three P.M., suffering under excessive dyspnœa.

Has been ill since Monday last (five days) with cough, attended with little or no difficulty of breathing until three days ago, when he was compelled to give up working. He ascribes his illness to carrying a heavy load, for a great distance, last week. He never had any pain in the chest (he was repeatedly questioned on this point with the above result ; pain in the back was not specially inquired after.) His cough was accompanied by expectoration, mixed with blood, ever since it came on. He never brought up blood in any quantity ; and has always up to this illness enjoyed uninterrupted good health. Habits not temperate.

Orthopnœa extreme ; marked lividity of face and lips ; sur-

face of body covered with large drops of perspiration. Pulse tolerably full, regular, hurried. Has an expression of extreme anxiety. Sputa consist of mucus, rather viscid, deeply and uniformly stained with blood. Chest expands pretty freely on both sides. Vocal thrill and resonance feebler on the left side. Percussion on left side, both in front and behind, almost absolutely dull. On right side, respiration is sonorous and wheezing; on left side, comparatively feeble. Inspiration accompanied by numerous rather coarse mucous râles; and throughout the chest sibilant and sonorous râles are audible. Heart sounds appear natural.

Orthopnœa. Blood in sputa.

Bled to ℥xx., with the effect of producing an approach to faintness. Blood slightly buffed. During the venesection, perspiration, in excessively large drops, poured from the forehead and the whole body.

Bloodletting.

Vespere.—After an hour, breathing became easy, lividity of lips diminished; face was slightly flushed; pulse about 90, full. Had still cough, sometimes in paroxysms. Ordered sulphuric ether, in ʒss. doses, with a little whisky. At half-past six P.M., Ipecac. gr. iij., and Squill gr. ij. in powder, every four hours.

August 10th.—*Vespere.*—Slept well last night, and continued well during this day. Took his meals heartily, and made no complaint. Sputa as before.

Nine P.M.—A new attack of orthopnœa; condition as on admission.

Again bled to ℥xx., without relief, and died between one and two A.M. on the 11th.

Supplementary Note on the 11th.—(From memory.)—A very plethoric man, robust, neither emaciated nor corpulent.

Chest large, well developed. Dulness of percussion (as above-mentioned) nearly absolute over left side; traces of pulmonary resonance only over upper lobe; respiratory murmur much enfeebled, but still audible at back; vocal resonance and thrill present over greater part, but enfeebled in lower lateral region. On right side percussion good; respiration very loud and wheez-

Facts bearing on diagnosis of Aneurism.

ing, obscuring both the heart's sounds and those of the left lung. On this account the examination of the heart was not satisfactory; but nothing abnormal could be detected over it, or over the great vessels. No pulsation of veins, nor abnormal pulsation of arteries in neck.

Expansion of the two sides appeared not visibly different, but was not accurately estimated. The right side exceeded the left slightly (quarter of an inch) in circumference below the nipple. Heart's sounds not displaced. Position of apex indistinct.

Skin cold on admission. Afterwards no trace of fever. Countenance livid and congested on admission; lividity had almost disappeared, though some congestion remained, at second visit.

On the 10th, was observed eating freely and heartily, without apparent dysphagia.*

* *May* 1862.—The diagnosis in this case was correctly delivered previously to the *post-mortem* examination, at which Dr. Wm. Robertson and others were present. It was founded on the spasmodic character of the symptoms, the absence of a distinct history of pneumonia or of pleurisy, with the signs of condensation of the base of the left lung; also upon the peculiar character of the hemorrhagic expectoration, which I had by this time learned to recognize as probably aneurismal, and of which more will be said hereafter. See Case VII., p. 514. The absence of signs of cardiac disease, and the peculiar, robust, full-blooded habit of the patient, with his occupation, also entered, beyond all question, into the diagnosis. It was the belief in aneurism which led to the note *from memory* made immediately after the death of the patient, of circumstances which the short duration of the case and the urgency of the symptoms did not permit of our recording at the bedside. It is right, also, to state that the practice was regulated by the diagnosis of aneurism, not by that of pneumonia, as might perhaps be inferred from the blood-letting. I had at that time the belief (now, I confess, much shaken) that in fitting cases, blood-letting might be of service in aneurism, after the method of Valsalva; and at all events there seemed to be no other course open in this case. With my present convictions, I

Examination of the Body, August 12th.—On opening the thorax, the right lung appeared voluminous and distended ; the left retracted, and comparatively small. In the left pleura there were about twenty-four ounces of serum, mostly clear, with a few flakes of lymph. The right pleura contained only a few ounces of fluid. No adhesions in either pleura, except at the root of the left lung at its upper part. The heart was not displaced.

The whole of the organs in the neck and thorax were removed together, when it became evident that there was a tumour pressing upon the bodies of three or four dorsal vertebræ, and connected with the descending aorta. The pharynx and œsophagus being slit open, it appeared that the œsophagus, about its middle, was in the close neighbourhood of the tumour. The muscular coat of the œsophagus was at this point slightly atrophied, and there was a little sub-mucous ecchymosis, but the mucous membrane was normal. The larynx and trachea were normal, except that the latter contained a quantity of blood-streaked mucus.

The heart weighed fourteen ounces ; all the valves normal. The aorta at its commencement was not enlarged, but its inner membrane was rough and uneven, with much opaque deposit not calcareous. The arch of the aorta was slightly dilated near the origin of the innominata, and this vessel was also slightly enlarged. The carotid and subclavian arteries on the right side were normal in size, the former studded internally with yellow opacities. The left carotid and subclavian also contained slight atheromatous deposit, but were otherwise normal. The internal coat of the whole thoracic aorta abounded in atheromatous deposit, and was extremely uneven, but without any induration or calcareous matter. In the descending aorta, there were two considerable abnormal openings in the wall of the vessel. One was situated about an inch beyond the origin of the left sub-

think I should have refrained from large blood-lettings in this case ; though I still think small blood-lettings may now and then be useful in emergencies. Valsalva's treatment has been fully tried in Edinburgh, and, in my opinion, without any favourable result.

clavian artery ; it was oval, about an inch by three quarters of an inch in diameter ; its smooth and rather sharp edge formed by the internal coat of the vessel. This opening led into an aneurismal sac, about the size of a large orange, which, arising from the posterior wall of the artery, projected upwards and backwards, and was adherent to the periosteum of the third and fourth dorsal vertebræ, to their left transverse processes, to the articulations of the ribs, and, through the medium of the parietal pleura, to the inner and back part of the upper lobe of the left lung. The bodies of the above-mentioned vertebræ were considerably deformed by atrophy of their anterior part. This sac was about half-full of stratified and decolorised coagula.

Another aneurismal sac arose from the second opening above mentioned in the wall of the descending aorta. This opening was round, somewhat less than a sovereign, and quite smooth-edged, like the former, from which it was distant about three-quarters of an inch downward and inward. The sac connected with this opening arose from the right wall of the artery, and passed inwards towards the left bronchus, to which it was firmly adherent, and into which it opened by a very wide communication. This aneurism was likewise half-full of laminated coagula, and was about the size of a large plum. The two sacs above described were so close together as to form what at first sight appeared as a single tumour. They did not, however, communicate at any point.

The left bronchus had its posterior wall deficient for about an inch ; and its calibre was almost completely occupied at this point by a firm, grey, coagulum of blood, which projected out of the aneurismal sac.

The left lung was almost completely condensed ; crepitation remained only at a few points in the apex, and still fewer in the lower lobe. See Fig. 12, A and B, pp. 482, 483. Its tissue had a nodulated and dense feeling externally, and presented a remarkably variegated appearance, some portions being violet-coloured, and others of an almost sandstone-grey tint. The interlobular spaces were everywhere well marked, depressed, and often dividing

sharply the red from the grey parts. On section the same varieties of colour prevailed throughout the lung; the tissue was dense, opaque, resistant, very obscurely granulated to the eye, and here

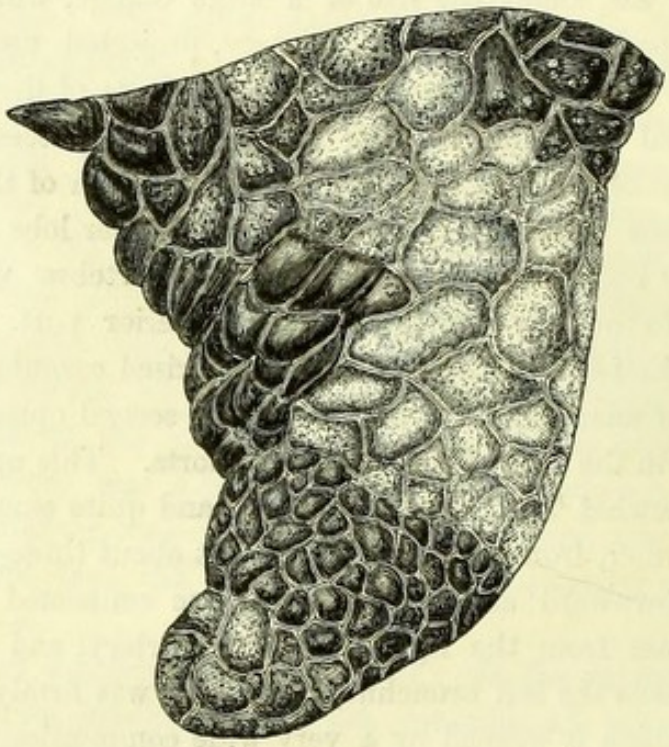

Fig. 12, A.

Surface of left lung in case of W. G., shewing lobular condensation, from old and recent hemorrhage into left bronchus. The differences of colour and of prominence of the lobules is due to the differences of date of the hemorrhagic condensation; the older hemorrhage being nearly decolorized, and in part absorbed, or converted into a puriform matter, which is seen at points forming small abscesses below the surface. (From a coloured drawing.)

and there dotted with distinct yellowish points, which presented somewhat the appearance of tubercles, but on examination proved to be the smaller bronchial ramifications, completely plugged with opaque, viscid, muco-purulent matter. The greater bronchi contained a similar stringy mucus, more or less mingled with pus and blood; their mucous membrane was stained, but little congested.

The right bronchus and its principal branches contained a little blood-coloured mucus, but were otherwise natural. The corresponding lung was distended, and almost emphysematous in

appearance ; on section, its tissue stained here and there with blood, but highly crepitant throughout.

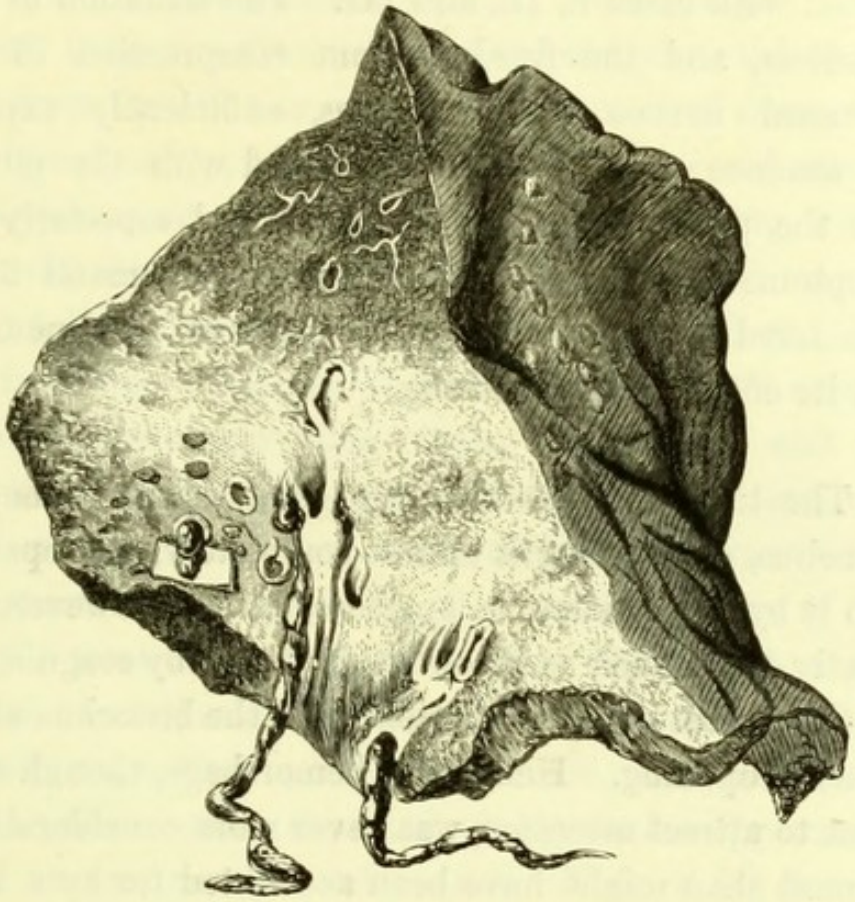

Fig. 12, B.

Section of lung, the surface of which is shewn in Fig. 12, A. The varieties of colour and the bronchial abscesses are less clearly defined. The bronchi are seen to be full of blood in different stages of alteration. (From a coloured drawing.)

The only other morbid appearances of importance were, incipient cirrhosis of the liver, atheroma, with calcareous degeneration of the abdominal aorta, and slight opacity of the cerebral arteries in some points.

The case just related is an excellent example of aneurism giving rise to spasmodic dyspnœa of an *asth-*

matic character, without the slightest tendency to laryngeal spasm. In this last particular it is strongly contrasted with cases I., II., and III. The situation of the aneurism, and the freedom from compression of the laryngeal nerves on both sides, sufficiently explain the absence of phenomena connected with the glottis. But the positive facts of the case, and especially the symptoms of disorder of the respiration, demand to be considered in relation to the position of the aneurism and its effect on the left lung.

The tumour pressed on the greater part of the left bronchus, obstructing it almost completely, and opening into it by a considerable aperture, which, however, was greatly diminished as an outlet for blood by coagula, and by the almost complete flattening of the bronchus at the point of opening. Hence the hemorrhage, though sufficient to attract attention, was never more considerable in amount than might have been accounted for by a more ordinary cause, such as hemorrhagic condensation of the lung, or even ordinary acute pneumonia. The blood, confined in its passage outwards, appears to have pressed backwards towards the lung with considerable force, and was found in all the bronchial ramifications of the left lung, mingled with a large quantity of glairy, viscid mucus, which completely obstructed the greater number of the air-passages. It was a very striking, peculiar, and rare form of condensation, equally distinct from hepatization and hemorrhagic condensation in their more ordinary forms, as well as from that simple collapse of the air-

cells which I have elsewhere described * as supervening on bronchial obstruction in other circumstances. Yet it had characters in common with all these lesions. That the left lung was diminished in volume was evident, not only from the effusion of a certain amount of serum into its pleural cavity, but from the voluminous and almost emphysematous condition of the opposite lung, which was evidently over-distended, and had crossed the mesial line of the chest from the collapse of its fellow. Yet this incomplete collapse of the left lung was not accompanied by an empty state of the air-vesicles ; and the pulmonary tissue, although perfectly non-crepitant, was by no means flaccid, dry, and smooth on section, as usual in bronchitic collapse. On the contrary, many of the air-cells were filled with blood, and others with a soft greyish exudation, being a mixture of fibrin, in various proportions, with blood and pus, which, however, did not give to the section the distinctly granular appearance usual in pneumonic condensation, although in density and resistance the lung resembled the condition of that organ in hepatization or hemorrhagic condensation from disease of the heart. In fact, I believe that this curious and unusual pathological condition of the lung, of which I have preserved a very accurate drawing, can only be explained as follows :—The obstruction of the main bronchus produced, in the first instance, a considerable degree of pulmonary collapse ; and after this condition had lasted some time, the bronchial hemorrhage forced

* Papers on the Pathology of Bronchitis,—*Monthly Journal of Medical Science*, 1850-51.

itself into the collapsed air-vesicles, much in the same manner as an artificial injection through the bronchi in a partially atrophied lung ; the lobules thus assuming a distended appearance, the intervening septa remaining depressed. The remarkable distinctness with which the surface of the lung shewed the various lobules in their different degrees of distension (Fig. 12, A), and the very firm plugs in most of the bronchi (Fig. 12, B), seem to prove the correctness of this solution of the anatomical phenomena.

I have next to consider the *dyspnœa*. That a certain considerable amount of dyspnœa is fully accounted for by the condition of the left lung, it is impossible to deny. But the peculiarly exquisite character of the symptom in this case, its very perfect intermission within an hour or two after the first bleeding, as well as during the whole of the second day of observation, and its equally sudden re-accession on the next evening, are phenomena much more allied in character to spasmodic asthma than to any other disease of the respiration, and certainly not due to the condensation of the lung, or to the permanent obstruction of the air-passages. Bearing in mind the analogy of the cases of compression and irritation of the laryngeal nerves, and the experiments of Volkmann* and others, which shew that the nervous system has a powerful influence over the contraction of the bronchial muscles, it is impossible to avoid the conclusion, that the spasmodic asthma in this case was due to the pressure of the aneurism on some of the branches of

* Handworterbuch der Physiologie, vol. ii. p. 586.

the left pulmonary plexus of nerves, which must have lain in great part between the tumour and the bronchus. The main trunk of the pneumogastric was not involved by the tumour, and accordingly there was no dysphagia ; nor were any of the other important nervous structures in the thorax at all implicated. The facts of the case, therefore, both positive and negative, seem to be perfectly in harmony with the morbid appearances as thus explained.

In relation to *diagnosis*, this case is not a little instructive. The idea that the patient was the subject of aneurism was strongly presented to my mind at the first examination, and was mentioned at the dissection as the most probable solution of the case. Notwithstanding the absence of the elements of a physical diagnosis, this opinion seemed to be justified by the following circumstances :—The almost complete dulness on percussion over the right lung, and the feebleness of respiration, vocal fremitus, etc., on that side of the chest, could only be produced either by considerable pleural accumulation, or by pulmonary consolidation, accompanied by nearly complete obstruction of the bronchi.* The former supposition was excluded by the absence of dilatation of the left side of the thorax ; the latter was entirely in harmony with the supposition of an aneurism of the descending aorta. The presence of spasmodic dyspnœa, and the absence of laryngeal spasm, seemed equally to

* I have been for several years in the habit of observing, that pneumonic consolidation so accompanied will give rise to all the signs of a large pleuritic effusion, except distension of the side. Of this I have now seen many instances besides the present.

point to this as the probable situation of the aneurism, if present. The presence of blood in the sputa lent strength to the idea of aneurism, as it has been shewn in numerous other cases that this disease does not necessarily lead to profuse hemorrhage by bursting on a mucous surface. Although the amount of blood observed in the expectoration in this case was not greater than is often observed in heart disease with pulmonary hemorrhage, yet as evidences of valvular disease were wanting, and as the consolidation of the lung in pulmonary hemorrhage is usually double, I was induced to regard this idea as less probable than that of aneurism ; and was thus led, among various circumstances suggestive of doubt, to the correct diagnosis. Admitting the idea of aneurism, there could be little doubt as to the situation of the tumour. The absence of laryngeal symptoms and of dysphagia, the signs of obstruction of the left bronchus, the absence of dull percussion or abnormal sounds on the front of the chest, the normal condition of the vessels in the neck, combined to give a correct idea on this point. If a more accurate history had been procurable, the presence of pain between the shoulders would have probably formed an additional element in the diagnosis.

This case illustrates very clearly a respiratory symptom dependant on aneurism, and probably due, like the laryngeal dyspnœa before mentioned, to nervous irritation, but of a kind beyond the reach of surgical relief. It may easily be believed that these two classes of symptoms may be often combined in one case, especially if the aneurism be of large size ; and in such instances

little or no relief would be procured from tracheotomy. I do not, however, regard the possible existence of this, or of any other unascertained complication, as a sufficient reason for withholding the operation in cases of aneurism associated with laryngeal symptoms very dangerous to life ; but only as suggesting the propriety of a rather guarded prognosis, great care being taken in all cases to ascertain as nearly as possible the relations of the tumour when tracheotomy is proposed. [The strictly practical result has been, however, that retaining as I do the same convictions, I have not met with any case of aneurism since the one first recorded, which has appeared imperatively to demand tracheotomy.] The cases which hold out most encouragement to the operation are evidently those of small tumours of the innominate artery, or of the lower and back part of the arch ; and every part of the air-passages ought to be accurately explored, with a view to the discovery of evidences of obstruction.

The catalogue of symptoms, due to the pressure of intra-thoracic aneurisms on the nerves in their neighbourhood, is not exhausted by those to which I have hitherto alluded. Various kinds and degrees of uneasiness or positive pain arise from this cause ; although the most severe and constant painful sensations produced by aneurism are no doubt due to the erosion of the bones and cartilages on which, as in the above case, they so often exercise deleterious pressure. Dysphagia, although frequently due to direct pressure on the œsophagus, is

sometimes so distinctly spasmodic in character as to suggest the idea of its having its source in pressure on the pneumogastric nerve ; and the respiration sometimes suffers (as I believe I have seen in one instance) from lesion of the phrenic nerve in large aneurisms. But there is one symptom of by no means unfrequent occurrence, and which I believe will be found to be almost confined to aneurisms of the ascending aorta, especially in the immediate neighbourhood of the heart. This is a greater or less degree of *angina pectoris*, as described by Heberden, a form of nervous irritation, which may well be referred to compression of the great plexuses of nerves ramifying on either side of the ascending aorta, and communicating freely with the cardiac ganglia, and plexuses of the ventricles. The following case affords a good illustration of this symptom in an almost uncomplicated form :—

CASE V.—*Severe pain, with angina-like paroxysms, and sense of impending death. Death without serious dyspnœa or hemorrhage. Aneurism of the ascending aorta, with dilatation of the whole arch ; laryngeal nerves not involved.*

A man, aged about 45, affected with manifest signs of aneurism of the ascending aorta pressing on the costal cartilages, came under my notice repeatedly at intervals during last summer. He had been ill for many months, when first seen ; had been formerly under medical observation ; had borne his fatal complaint to Ireland and back, and by watching the gradual course of his disease had acquired spontaneously so complete and intelligent a conviction of its nature and certain termination, that his whole manner and habit of life conveyed the impression of a man pre-

pared for instant death. As he was very patient and calm, he often related to me, at considerable length, and with great clearness, the narrative of his sufferings. These were very variable in amount; he had almost constantly a certain degree of gnawing pain, corresponding to the site at which the aneurism had produced a tumour at the front of the chest. He had no dysphagia, and little if any feeling of dyspnœa; the lips were, however, somewhat livid, and the venous return by the superior cava was manifestly impeded. The most severe symptoms arose from a feeling, which seized him from time to time, and the minor degrees of which were present, to a greater or less extent, every day. It was a sensation of pain rarely amounting to agony, but attended by extreme distress, and an indefinable species of uneasiness, which he could only describe as driving him to desperation, and as being quite unrelieved by change of posture, or by any kind of repose or exertion. This sensation was not distinctly localized within the chest; he often compared it to the approach of death; it usually passed off, at least in its intense form, after a few hours, but a certain degree of it was almost constantly present with him. The pulse was little affected by this condition; the respiration always free. Percussion revealed a large tumour, occupying the position of the ascending aorta, and projecting to the right side of the sternum; a double murmur existed over the tumour, and the heart presented signs of hypertrophy. This patient died in the middle of August, during my temporary absence from Edinburgh. Many remedies had been used, but none were of any avail as palliatives, except the exhibition of large doses of opium.

Pain.

Angina Pectoris.

The dissection was performed by my friend Dr. Sanders. A large aneurismal sac was found adhering to the parietes on right side at the fourth and fifth costal cartilages. The pericardium contained some serum, deeply stained with blood; and it presented reticulated lymph on both its surfaces. The aortic valves were incompetent; the heart much hypertrophied.

The aneurismal dilatation, on being laid open, shewed a large

pouch, consisting of the entire vessel, dilated from its very origin to the descending portion, where it resumed nearly its normal diameter, but presented much disease from atheromatous and calcareous deposits. From this dilated arch of the aorta there opened laterally on the right side, through a ring-shaped aperture of two inches and a half in diameter, another sac, lined only by thin fibrous walls, and adherent, as above mentioned, to the third and fourth right costal cartilages. The pneumogastric and recurrent nerves were carefully traced, but neither of them appeared to have been involved in the tumour or stretched over it.

Physiological analysis of the preceding facts.—In the preceding part of this article I have adduced illustrations of three distinct kinds of spasmodic or paroxysmal suffering liable to be connected with aneurism of the thoracic aorta, and of its branches within the chest. *First*, Spasmodic *laryngeal* dyspnœa or *laryngismus*, attended frequently, but not invariably, by alterations of voice or cough, and sometimes by habitually harsh stridulous respiration. *Secondly*, spasmodic *bronchial* dyspnœa or *asthma*, which may be accompanied by disease of the lung, or by signs of pressure on a bronchus. *Thirdly*, Paroxysmal suffering referred to the heart, of the character now well known to practical physicians by Heberden's name of *angina pectoris*. The view, that these affections have their origin in the interference of aneurisms with the thoracic nerves, seems to have much theoretical probability ; and the very decided manner in which observation has corroborated this view in the instance of the laryngeal spasms, suggests the propriety of an increased accuracy of anatomical and physiological investigation in relation to this subject. As the in-

stances which have occurred to myself have not presented a sufficient basis for induction, I endeavoured to obtain a broader ground for generalization, by consulting the preparations of aneurism in numerous museums here and elsewhere, and the records of older observation on this subject; but the result was far from satisfactory, and it soon became evident that very little was to be added to our knowledge in this way. The museums to which I have had access abound in preparations of aneurism in which every relation of the slightest physiological interest has been laboriously cleared away by the knife of the dissector; and the guesses that might have been formed as to these relations are, in the vast majority of instances, rendered valueless by the negligence which has allowed the records of the case to perish. On the other hand, the literature of aortic aneurism has not appeared to me to yield many cases in which the symptoms I have described above, admit of precise analysis in connection with anatomical details of the position of the aneurism. It is sufficiently evident that the subject requires renewed observations, conducted with a view to the elucidation of these symptoms; and I trust this paper may prove the means of attracting additional attention to the relations of aneurism with the nervous system, especially in the thorax, where results of considerable importance to diagnosis and treatment have been shewn to depend upon these relations.

Not a few cases are now on record, in which the sufferings caused by aneurism have been of so variable and capricious a character, as to suggest the idea of a purely

nervous affection; and the complete remissions which occurred in such cases, naturally surprised the older anatomists, who could not conceive "but that the effects of a permanent cause, such as an aneurism is, must be permanent."* These paroxysmal attacks are in some cases described as resembling suffocation or orthopnœa, the breathing being sometimes accompanied with stertor (stridor?), or with a sensation of a cord binding the trachea; in other instances, they have evidently some of the characters of angina, being accompanied by palpitation of the heart, swooning, etc. Frequently these symptoms are mixed, and very often details are wanting to distinguish between them. In the 17th and 18th Letters of the second book of Morgagni's work, the reader will find many interesting cases and most able discussions, which reflect the entire information of his age upon the subject.† In a former part of this paper I have alluded to the later observations of compression of the recurrent nerves by aneurism: on the other departments of the subject I have failed to discover any more precise data. Anatomical knowledge would lead

* Morgagni, de Sedibus et Causis Morborum, Lib. 2, Epist. xvii. 27.

† See especially Epist. xvii. Sect. 14 (angina and spasmodic dyspnœa); Sect. 25 (spasmodic dyspnœa); Sect. 26 (do., interesting remarks on treatment by warm water fomentations of arms; in this case there was probably angina; Epist. viii. Sect. 17 (spasmodic dyspnœa, probably with angina, in a physician of Modena). He was seen by Ramazzini and Malpighi, with the exception of the latter, no one who saw him suspected an organic disease. A case of great interest; the dissection, however, very imperfectly recorded. With these examples contrast the case in Sect. 25, in which dyspnœa, non-paroxysmal, accompanied an aneurism pressing directly on the lung.

us theoretically to look to compression of the cardiac nerves by aneurisms of the ascending aorta immediately above the valves, as the probable cause of angina ; and on the other hand, to refer the asthmatic paroxysm to aneurisms having the relations of the one in Case IV. So far as my reading extends, I have found those views corroborated by the few cases from which any approach to precise deductions can be formed. It is certain, at least, that aneurisms of the ascending aorta are found not to produce dyspnœa in the same large proportion of cases as those of the descending and transverse part of the arch, except where this symptom occurs as a sequel of cardiac hypertrophy and valvular disease ; and these are, again, more frequent in connection with aneurisms of the ascending aorta than with those of any other part of the vessel. I desire, however, to avoid laying too much stress on these imperfect observations, and to leave the subject to more mature and careful investigation.

CASE VI.*—*Aneurism of the superior mesenteric artery, opening into the duodenum twenty-two months before death, and causing repeated and very copious hæmatemesis, with symptoms and history closely resembling gastric ulcer. Remarkably complete convalescence, followed by a protracted surgical treatment for an ulcer of leg, which bled at the menstrual periods. No return of hæmatemesis ; amenorrhœa, dyspepsia. Several months afterwards, a sudden attack of syncope. Death by hemorrhage into the peritoneum, the duodenal opening being entirely or nearly closed.*

M. C., æt. 16, servant, was admitted into the Royal Infirmary January 4, 1848. During twenty-four hours previous to her ad-

* Read to the Medico-Chirurgical Society of Edinburgh, Dec. 5, 1849.

mission, she had brought up, by vomiting, large quantities of blood, on six different occasions. The vomited matter consisted of clotted blood, with a good deal of fluid, and might have amounted in all, according to her statement, to half a gallon. The vomiting had ceased on admission.

History of Illness.

She dated her complaints from the summer of 1847, when she had jaundice, accompanied by some sickness and vomiting, and by pain across the chest and back. She was not confined to bed; but became afterwards sensible of diminished strength. About six weeks before admission, she had suffered from the prevailing influenza, during the progress of which she frequently had vomiting of a sour acrid fluid, sometimes to the extent of half a gallon at a time. The vomiting occurred usually in the evening, after dinner; never after breakfast. She also suffered from constant pains, extending from the back to the pit of the stomach, and from a feeling of tightness in the chest, especially when she had on stays, or heavy clothing. She had little or no cough at any period of the complaint.

She was admitted in a state bordering on syncope; the surface was very pale; the circulation hurried. The slightest exertion seemed to cause fainting, accompanied by the peculiar pain in the abdomen from which she had been suffering. When seen next day, she had vomited about half a pint of blood since admission; the blood was coagulated. She fainted at the time of the vomiting. She was free from sickness; but there was tenderness in the hepatic region, with slight extension of the dull percussion. The breath fetid; the tongue with a slight grey fur; circulation hurried. (Digitalis and ipecacuan). She continued in the same state for twenty-four hours more, unable to rise without a feeling of vertigo. The pulse, on the 6th, was 103, soft and quick. From this time to the evening of the 7th she improved steadily. On the evening of the 7th (third day from admission), she had repeated vomitings of blood, preceded by headache and sickness. The quantity vomited was not exactly known. She was more anemic than before, and the epigastric tenderness

was more considerable, but chiefly on the right side. A *slight pulsation of the abdominal aorta* is noted in the report, and an impairment of percussion on the right side of the epigastrium. From this period (four days after admission) she had no return of any bad symptom ; and, by rest and careful regimen, she became rapidly convalescent. She remained in the house till February 7th (one month) when she was dismissed "cured." After this, I saw her several times in the surgical house. She had been admitted on account of a weak ulcer on the back of the left leg, which usually bled at the menstrual period, and was slow to heal. She was still pallid and languid, and suffered from dyspeptic symptoms, with amenorrhœa, but had no return of vomiting. She was sent out with her ulcer nearly healed. I heard nothing more of her till I learned that she had fallen down suddenly in the street, and had been found by the officers of police in a fainting state. She died before she could be removed to the hospital, whither the body was immediately brought. This was on November 28, 1849 (twenty-two months from first occurrence of hæmatemesis).*

Convalescence.

Sudden Death.

The dissection was performed next day (November 29th).

The body was pale, but not at all emaciated.

On making the first incisions through the parietes, the immediate cause of death was at once revealed. A large quantity of clotted blood was found in the peritoneal cavity. The coagulum, on being removed, weighed 2½ lbs.; so that I think considerably above 3 lbs. of blood must have been extravasated.

The thoracic organs were healthy ; the left side of the heart was small and firmly contracted ; the thoracic aorta was also small.

The great abdominal glands were all healthy, but very

* After these notes were written, I learned that she was, for a few days, in another ward in the house, in July 1849 (four months before death), affected with deep jaundice; and also that she had led an irregular life, having, at a previous period, suffered under syphilitic ulceration of the ear.

anemic. The liver and spleen were bound up to the diaphragm by rather loose but dense adhesions [thus accounting, in part, for the tenderness during life in the right hypochondrium]. In the angle between the lower part of the duodenum and the head of the pancreas, there was found a ragged lacerated opening through the serous membrane. The edges of this opening were not all thickened. The pancreas was normal in size and structure, but appeared to be displaced forwards and somewhat stretched over a small tumour, situate behind it, on the front of the vertebræ and great vessels. To ascertain the nature of this tumour, the biliary ducts (which passed very close to it at its right side, but had apparently no connection with it) were cut across, close to the liver. The aorta and vena cava were then divided below the diaphragm, and a considerable portion of these vessels, with the stomach, duodenum, and pancreas, were removed *en masse*.

Being strongly prepossessed with the idea that there had been some ulceration of the stomach, giving rise to the profuse vomitings both of blood and alimentary matters to which she had been subject, I slit open this organ, and examined every part of the mucous membrane with great care, but without discovering anything abnormal. I then continued the incision into the duodenum, and carefully looked at the mucous membrane, at first with a similarly negative result. The coats of the intestine, however, were at one point very thin, and slightly ecchymosed; this part was found to be in the immediate neighbourhood of the opening before mentioned through the peritoneum, which had given rise to the fatal bleeding. On again carefully inspecting the mucous membrane, I now found a very small ecchymosed spot, slightly elevated, and perforated in the centre by a minute opening, from which a very little bloody fluid could be squeezed by gentle pressure. A moderately fine probe could be passed a line or two into this opening, but was there arrested; and no attempt was made with a finer instrument. The cicatrix was situate about two inches lower in the gut than the opening of

Evidence of a healed Duodenal Opening.

the biliary vessels, and very close to the site of the external peritoneal opening.

On slitting up the aorta from behind, it was found to be, like the part of the vessel examined in the thorax, nearly, if not absolutely, free from disease or deposit. The opening of the superior mesenteric artery, however, was a little irregular in form ; and, on passing a probe through it, a considerable dilatation was discovered in the line of the vessel, occupying the whole first portion of its trunk, and corresponding in situation with the tumour above described. A little further manipulation enabled me to push the probe downwards, through the ragged opening in the peritoneum.

The sac was now divided by an incision to the left of the mesial line, passing through the peritoneal opening. It was seen to be composed of a thick and strong fibrous cyst, slightly oval in form, and not larger than a hen's egg, somewhat flattened antero-posteriorly, and with its long diameter in the axis of the artery. This cyst had evidently ruptured at its lowest point, and the blood had made its way through the cellular tissue between the coats of the duodenum, breaking up the muscular coat into two layers, and finally perforating the serous coat by a ragged opening. The sac contained a number of irregular and half-decolorised coagula.

Description of the Aneurism.

The condition of the coats of the mesenteric artery itself, at the point of the origin of the aneurism, was not investigated, the parts being sent to the University for further dissection and for preservation. The continuation of the artery was also not examined. The cœliac axis was healthy, but perhaps rather larger than usual ; the other branches of the abdominal aorta appeared all normal. The splenic vein, which lay close upon the side of the tumour, but, so far as could be observed, without any communication with it, was somewhat dilated, and contained a very firm coagulum of decolorised fibrine.

It was now evident that the disease was an aneurism of the superior mesenteric artery, which had opened primarily into the duodenum, giving rise to copious vomitings of blood twenty-two

months before death ; that this opening had become entirely or nearly closed, and that death finally took place from a second opening, not far from the first, into the peritoneal cavity.

In the view of the case brought to light by the *post-mortem* examination, the whole of the symptoms observed become of easy explanation. The occasional jaundice (the amount and character of which were not known till after the patient's death) was evidently owing to the pressure exerted by the tumour, when at its extreme point of distension, on the biliary duct ; while the sickness and vomiting of sour matter, after a full meal, so evidently relieved after each hemorrhage, may have been owing to a similar pressure on the duct of the pancreas interfering with the duodenal digestion. The nearly constant pain produced by exertion, and the feeling of tightness and oppression caused by the use of stays, or by any other article of dress which compressed the abdominal organs, may be obviously explained by the injurious pressure effected in this way on the tumour and on the great blood-vessels which lay beneath it. The tenderness of the epigastrium and right hypochondrium were probably not directly connected with the deep-seated lesion, and rather seem to have resulted from the inflammation in the serous lining of the liver, which produced the adhesions found after death. The slightly increased size of the liver, as shewn by the extended dull percussion in the hypochondrium, probably was caused by the obstruction of the vessels and ducts of the organ. The lassitude, dimin-

ished strength, anæmia, and amenorrhœa, were evidently owing to the great and repeated losses of blood ; and the obstinate continuance of the anæmic state during the long interval between the attacks of hemorrhage, may have been caused by the imperfection of the digestive process, or possibly by minor hemorrhages by stool, which may have existed without being particularly observed. The existence of pulsation in the epigastrium needs no explanation.

At the same time it is to be observed, that the whole of the phenomena under observation at the time of the first attacks of hæmatemesis, were such as to lead directly to the supposition of a chronic ulcer of the stomach. The frequency of this disease in young females, the whole progress of the case, and, finally, the apparent cure, by simple remedies and careful regimen, were calculated to confirm this diagnosis; and even now, on reviewing the recorded facts of the case, I do not think that any of the prominent symptoms can be considered as opposed in any way to this opinion. The following symptoms, at least, appear sufficiently equivocal in character :—

1st. The hæmatemesis. Profuse hemorrhage, as a consequence of chronic ulcer of the stomach, involving the coronary artery or one of its branches, has been repeatedly observed, as in the cases of Dr. Craigie* and

* *Edinburgh Medical and Surgical Journal*, vol. iv., p. 262. [Compare on this and all other points bearing on the phenomena of gastric ulcer, the very complete monograph of Dr. Brinton, "On the Pathology, Symptoms, and Treatment of Ulcer of the Stomach." London, 1857.]

others; [this, indeed, may almost be called one of the regular modes of fatal termination of the disease].

2d. Sickness and vomiting after eating, with dyspeptic symptoms of various kinds, are nearly constant phenomena in chronic gastric ulceration.

3d. Dull pain, increased by exertion or repletion, with tightness and oppression at the epigastrium, are equally characteristic symptoms of the disease in question.

4th. Anæmia was the obvious consequence of the loss of blood; the tendency to syncope, the lassitude and the diminished strength, were equally so.

5th. Lastly, in the category of the equivocal symptoms, I must also place the slight epigastric pulsation which existed in this case, unaccompanied by any appreciable tumour, from which its true nature might have been inferred. The extremely frequent occurrence of such a pulsation in connection with dyspepsia, would of itself have been, in the present instance, a sufficient reason for the absence of any suspicion; but the probabilities in favour of its being what is so well known as "nervous pulsation" of the aorta, were greatly increased, when it is considered that the whole arterial system presented the vibratile pulsation which so often follows profuse hemorrhage.

There remains, then, of the actually observed symptoms, only the jaundice. Now jaundice is, to say the least, far from being a characteristic symptom of aneurism of the abdominal vessels, while, in the present instance, the tenderness in the hepatic region and extension of the dull percussion, seemed to point to an accidental affection of the liver itself as its source.

But is there any symptom or collection of symptoms, which, in another case similar to the present, might lead to the establishment of an unequivocal diagnosis? On reviewing the whole case it appears to me that the following points merit consideration:—

1st. The stethoscope might have revealed a bruit, single or double. If the sound had been double, the nature of the case would have been no longer doubtful; but if a single sound only had been heard, as is commonly the case, it would have been still open to question, whether it was produced by aneurism, or by some other deep-seated tumour pressing on the aorta. Nay, in the pulsation which is independent of organic disease, I have occasionally [often] heard a distinct bruit, on applying moderate pressure with the stethoscope over the vessel.

But, farther, there might have been no aneurismal bruit. This peculiar phenomenon depends, in great part, upon the dilatation of the aneurismal sac at each impulse of the heart; and there is reason to think that an aneurism so closely bound down as the present, would [might] be accompanied by but little murmur. In fact, in a case related by Dr. Hope, where an aneurism considerably larger than this one sprang from the right side of the aorta, half an inch below the cœliac artery, there was no aneurismal bruit, only a superficial whiff, which proceeded from the superior mesenteric artery, stretched over the front of the tumour. This is the only case I can find bearing on the question. In the present instance, I do not remember that a stethoscopic examination was made; but if so, the results must have been negative.

2d. The pain and vomiting, after taking food, might have presented a peculiar character, had inquiry been made. If these symptoms proceeded, as is most probable, from impediment to the duodenal digestion, they would occur an hour or two after food had been taken ; whereas, in the case of ulceration of the stomach itself, the pain would rapidly follow the meal.

3d. The jaundice, if its intermission and recurrence had been the subject of frequent and continued observation, might probably have been attributed to some cause of occasional obstruction of the ducts, rather than to a disorder of the liver itself ; and this circumstance would probably have led to a closer examination of other symptoms and signs.

The most practical conclusions to be drawn from this remarkable, and so far as I know, unique case, appear to be the following : That the combination of jaundice with symptoms indicating imperfect duodenal digestion (cardialgia, pain and vomiting some time after taking food), should, in all cases, lead to the strong suspicion of a tumour pressing on the ducts of the liver and pancreas, near their duodenal termination ;—that the co-existence of these symptoms with fixed pain or oppression in the epigastrium, pulsation in the same region, and hæmatemesis, would very probably indicate aneurismal tumour, even in the absence of more unequivocal signs ; and that this diagnosis would not be invalidated by the arrest of the hæmatemesis (even after repeated recurrence), or by the apparent cure of the affection ; while, on the other hand, it would be rather confirmed if the remission of the pain

and other rational symptoms immediately after each bleeding, were as marked as in the present instance.

I think, however, it may also be said, with truth, that none of these symptoms or phenomena, would suffice absolutely to point out the true state of the case, unless unequivocal signs of an aneurism were presented on stethoscopic examination.

Aneurism of the superior mesenteric artery is exceedingly rare in man. Since the above case was read to the Society, however, my attention has been called to four cases of it.

In the "Lancet" for 1835 an instance of this affection is described, in a patient who died of scarlatina, under Dr. Elliotson. It was as large as a human heart, and had formed during life a pulsating tumour above the umbilicus. It was attended by severe pain in the lumbar, epigastric, and umbilical regions, and also with occasional nausea and vomiting after taking food. The sac remained entire up to the period of death.

Two interesting cases of superior mesenteric aneurism are related by Dr. J. A. Wilson, in the "Medico-Chirurgical Transactions," vol. xxiv., p. 221; and it is very remarkable that one of these cases ended by jaundice, while the other was accompanied by vomiting of large quantities of blood. The blood, however, in this latter case, does not appear to have come from the aneurism, but from the lung, the patient being affected with profuse hæmoptysis from phthisis. No symptom of digestive derangement is recorded in this case, except obstinate

constipation. The aneurism was large, easily felt in the epigastrium, and attended with pain.

In the other case the jaundice was very marked while the patient was under observation. There was also pain between the shoulders, and in the line of the dorsal vertebræ, as well as occasionally in the epigastrium and hypochondrium; exhaustion, loss of muscular power, depression of mind, and loss of appetite, but no tumour or pulsation, although frequent examination was made. Neither vomiting nor sickness are mentioned. The aneurism was also large, and in the trunk of the artery, about an inch from its origin; it was closely in contact with the *ductus communis*, which, however, was pervious.

In the "Medical Gazette" for 1842 (Feb. 25), Mr. James Douglas relates a case of thoracic aneurism, complicated with small aneurisms of the cœliac and mesenteric arteries. These latter presented no symptom during life, except vomiting, which occurred when exertion was made after taking food.

Aneurisms of the cœliac axis and its branches are somewhat more common than those of the superior mesenteric. In one case alluded to by Mr. South (Translation of Otto's Pathological Anatomy, vol. i. p. 320), in the Museum of St. Thomas's Hospital, the aneurism "by its motions against the stomach, produced vomiting whenever food was taken, and the patient died of consequent starvation." The museum of the College of surgeons of Edinburgh contains a preparation (Catalogue, No. 1152) of aneurism of the hepatic artery, in which the superior mesenteric is also considerably thickened

and dilated; but no particulars of the case are given. In the same museum (No. 1146) is an aneurism of the abdominal aorta involving the cœliac axis; the superior mesenteric artery issues from the lower border of the sac, but is very slightly involved in the disease. In this case the sac burst into the cellular tissue, and the extravasated blood became encysted, forming a secondary sac, the rupture of which caused death. The existence of the aneurism was not suspected during life, and the patient was treated for hepatitis. We may, therefore, presume, that jaundice was probably present in this case.

An interesting case of aneurism of the hepatic artery, recorded by Dr. Stokes, will be found in the *Dublin Journal*, vol. v., p. 401. The tumour was bound down by the capsule of Glisson, and therefore in close connection with the ducts, which were singularly dilated throughout the liver, forming projections on its peritoneal surface. In this remarkable case, the first symptom was copious hæmatemesis; and, from this fact, together with the singular dilatation of the biliary ducts, I cannot help suspecting, that the aneurism had opened into them—a circumstance which might easily have been overlooked. In a letter to Dr. Stokes, cited in this paper, Dr. Harrison incidentally notices having seen an aneurism of the mesenteric artery. He also observes that hæmatemesis frequently accompanies abdominal aneurism; and that in one case, where examination after death took place, the aneurismal sac had no communication with the stomach.

The museum of the College of Surgeons of Ireland contains two instances of aneurism of the abdominal

aorta bursting into the duodenum (Dr. Houston's Catalogue, B. c. 268, 269). The history of the cases is not given, nor are the anatomical relations of the aneurism stated.

In the museum of St. Bartholomew's Hospital (thirteenth series, 68), there is an aneurism of the abdominal aorta, extending from the superior mesenteric artery to the bifurcation, which ruptured into the duodenum *four days before the death of the patient.* I am indebted to Mr. Paget for some particulars of this interesting case, copied from the books of the hospital. The man had been sensible for two years of a pulsation just below the scrobiculus cordis, which he perceived for the first time after a fall from a scaffold. In the two days before death, he had repeated discharges of blood *per anum*, preceded by severe pain, which was relieved by the discharge. The opening into the duodenum was found, after death, to be regular and smooth-edged.

In this, and most of the other cases of abdominal tumour I have referred to, the coats of the vessels were diseased. In the case I have narrated, however, the nearly healthy state of the aorta seems to render it probable that the aneurism was the result in some way or other of violence applied to the artery. The sac was very strong and dense, but its relation to the coats of the vessel could not be distinctly made out without destroying the preparation.*

* Additional references to cases of abdominal aneurism will be found in an article by Dr. Haldane, Edin. Med. Journal, vol. iv., p. 349. I have also recorded two cases in Edin. Medical and Surgical Journal, vol. lxxxii., *Case-book*, p. 12, in which the symptoms resembled, in the one case, nephritis, in the other tubercular peritonitis; the diagnosis, however, being successfully made in both.

CASE VII.*—*Aneurism of the thoracic aorta, opening into the trachea and left bronchus; two attacks of hæmoptysis, four years and eight months before death; afterwards marked improvement as to symptoms, but occasional appearance of blood in the sputum during the whole remainder of life.—In the end, paroxysmal pains, emaciation, epileptoid attacks, nausea, suppression of respiratory murmur in the left lung; death by a small hæmoptysis, apparently producing suffocation.—Remarks on the mode of rupture of aneurisms, and on aneurismal hæmoptysis.*

The subject of the following observations was Mr. J. B——, a merchant, of a robust frame, and more than average intelligence, who, at the period of his death, was about forty years of age. I was first consulted by him on the 9th of March 1854, and he remained more or less constantly under my observation till his death, which occurred on the 19th of April 1858. The mass of details regarding his symptoms, which came to my knowledge during this period of four years, would occupy much more space than can be given to them here, and I must therefore bring within very narrow limits the statement of many facts to which the intelligence of the patient, and the anxieties of his relatives lent much interest at the time of their occurrence.

From many conversations with the patient, I am enabled to carry back the history of his complaint to nearly six years before the date of my first visit to him, *i.e.* to ten years before his death. The earliest symptoms were pains in the left side and shoulder, sometimes pretty severe, but without any distress of breathing or palpitation. He was at this time accustomed to take a good deal of exercise, and used to experience relief from his pains on walking till he was thoroughly heated. He also found that the use of stimulants frequently removed the pain, which was always

Early symptoms.

* Read to the Royal Medical and Chirurgical Society of London, April 26, 1859; published in *Med. Chir. Transactions*, vol. xli.

lessened after a free perspiration; and partly, no doubt, on this account, the pains were regarded as "rheumatic." He afterwards became subject to more or less of difficulty of breathing, and, under the advice of his ordinary medical attendant, underwent a considerable amount of active treatment.

In October 1852 it was for the first time intimated to the patient that there was a fault in the great vessels, indicated by a murmur, of the precise character and site of which I have, however, no information. He was forbidden to use severe exertion, which up to this time he had permitted himself without restraint; he was also directed to use palliative remedies, and was allowed moderately good diet, with a proportion of stimulants. Previously to this time, but at what precise period in the history of the case I am unable to state, he was sensible of two distinct aggravations in his complaint, one after a fall in shooting, the other connected with an injury which he received while descending from the top of a stage-coach. After this he always acted on the advice he had received, and was very chary of exposing himself to the least risk of injury.

In March 1853 a pulsation became apparent in the upper part of the left front, and in the course of the summer he became subject to pretty severe cough. With the cough came a mucous expectoration, and though nothing that was distinctly of blood colour was observed, the sputum was slightly coloured, or "bilious," as he called it.

It was in August 1853 that the first gush of blood occurred. The circumstances were remarkable, and served to make known Mr. J. B——'s precarious state of health to a wide circle of his townsmen. He was giving evidence as a witness in a court of justice, under some degree of mental excitement, when, without the least warning, his mouth suddenly filled with blood; and apparently without either coughing or vomiting he brought up such a quantity of pure blood as made him quite faint. He was immediately conveyed home, and there the hemorrhage was repeated to nearly the same extent as before. For several weeks

Profuse Hæmoptysis.

after this occurrence Mr. J. B—— was confined entirely to his room, and for the most part to the sofa; he also took sedative remedies to a considerable extent. After a time, however, being dissatisfied with the results of treatment, he changed his medical advisers more than once; and ultimately placed himself under my care, as already stated, in the spring of 1854, about six months after the first hemorrhage, and rather more than four years before his death.

It is unnecessary to go in detail into the history of the case after this. I found the patient with every sign of a large aneurism of the aorta presenting itself in the left front immediately below the clavicle, and passing backwards and upwards so as slightly to involve the left subclavian artery. The radial pulse on the left side was a little weaker than on the right (this sign became afterwards much more distinct, the pulse being ultimately almost entirely suppressed). There was less of dulness on percussion than of tumour and impulse; there was only a trace of the murmur formerly heard. For the rest, the patient was in tolerable condition, sleeping better than he had done for some time after the serious accident of the preceding August; suffering little positive pain, and having had no considerable expectoration of blood, although he had had a slight cough, with occasional coloured sputa. The pulse was ordinarily 70 to 80. He at this time took some exercise, but as yet very little. His hopes of life had obviously been deeply shaken, and his behaviour was that of a man doomed to a speedy and probably a sudden death.

I directed him to take gentle exercise, and after a time to go for two hours daily to his place of business, but not to lean over the desk; to take light, nourishing, solid food; to abstain from medicine; now and then, when pain was severe, to put one or two leeches over the tumour; and in general to think as little as possible about his complaint, except in the way of caution against violent movement. Under this regimen his condition improved considerably. In the beginning of May he removed to the sea-side, and before the end of the same month he took lodgings at a quiet railway station on the Gala Water, on the banks of which he

Improved general health.

spent the summer with very considerable enjoyment. He became a brother of the angle, and with the help of his wife and a light rod, managed to do a considerable amount of execution among the Gala trout. His mind, too, found great relief from suffering in a resigned and intelligent view of his condition. From having abandoned all idea of an ultimate cure he seemed to derive decided comfort as regards the present no less than the future.

Continued small expectoration of blood.

But in the midst of this improved state of health, he continued occasionally to bring up a more or less tinged expectoration; sometimes rusty, sometimes purple, almost never of anything approaching pure blood. This, I believe, continued to be the case, with intermissions of, at most, a few weeks, during the remainder of his life. The two succeeding summers were spent, in part at least, on the Gala; but, although the progress of the disease was slow, I had no difficulty in recognizing a distinct progress, chiefly in the direction of the left lung, of which the sounds gradually became more and more impaired. Occasionally he had attacks of severe pain, and now and then paroxysms closely resembling *angina pectoris*. He had also one or two attacks during the night, which, according to the description, I believe to have been of an epileptic character, but which lasted only a few minutes, and were not accompanied by marked convulsions. In the winter of 1857-8 it was evident to me that the end was approaching; the patient was worn and haggard in appearance; he had lost flesh to a great extent; he breathed with difficulty, and had a somewhat hoarse, rather laryngeal, inspiration, and an altered voice; he occasionally complained of difficulty in swallowing; he frequently brought up blood rather more copiously than he had done since the first attack of hæmoptysis, but never in large quantity, or pure. He also had sickness of stomach to a distressing extent, often lost his sleep at night, and was altogether in a pitiable condition. At length the respiration of the left lung became completely suppressed, the percussion at the same time becoming dull all over

Death by Asphyxia.

the left side; and on the 19th of April 1858, a small gush of blood, probably not exceeding eight or ten ounces, occurred, which terminated life by suffocation in a few minutes.

A careful *post-mortem* examination was performed by Dr. Haldane; and the result of it, so far as the interest of the preceding facts extends, is now before the Society. The aneurism involves the descending aorta, from the left subclavian, which is barely free of the sac, to several inches lower down. The sac rests on the vertebræ behind, on the ribs above, on the left lung below and in front. To the left lung it is firmly adherent over a space of many inches square. The left bronchus is stretched over the sac, and has its posterior wall absorbed throughout its whole length. The sac is filled with firm coagulum, which is freely exposed to view from the interior of the bronchus. Exactly at the bifurcation of the trachea there exists another opening into the sac; it is not larger than will admit a probe, and from its smooth rounded edges has evidently been a long time present. This opening, too, rests upon a firm, solid, laminated clot. The left pneumogastric nerve, and the corresponding recurrent, are deeply involved in the wall of the sac. The left lung is much collapsed, and infiltrated with a considerable amount of purple blood. The right lung was quite normal, and has not been preserved.

I am led to believe the preceding case not unworthy of the attention of the Medical and Chirurgical Society —1st, because of the very long period which existed between the occurrence of rupture of the sac and the ultimate fatal event; and, 2dly, because of the occurrence of hemorrhage, in a modified form, at intervals during the whole of that very long period. There is not much room for remark on the diagnosis, which, from the time that I first saw the case, was too clear to admit of a doubt. Nor did the case illustrate any new symptom

or combination of symptoms in thoracic aneurism, such as to call for remark. But as it presents an undoubtedly very rare, if not unique, example of the prolongation of life after rupture of the sac, and as this particular class of cases has not been made the subject of any very formal investigation, though incidentally discussed by most authorities, I have thought it desirable to accompany the statement of the facts with a short commentary, illustrative of some points which may have escaped the attention of members of the Society.

It is well known that sudden death by hemorrhage in the case of aneurisms of the thorax and abdomen, opening on mucous surfaces, is very generally preceded by minor discharges of blood, and sometimes by more or less considerable gushes, occurring many hours, and sometimes many days or weeks, before the fatal event. This fact, indeed, though apparent to all who have studied the records of aneurism, was not duly appreciated until a comparatively recent period; these slight hemorrhages being often either allowed to pass unnoticed, or being referred to some other cause than the true one. The celebrated case of Mr. Liston probably did much to diffuse among the members of his own profession a knowledge of the variable character of the hemorrhage from aneurismal sacs, and of the remarkable intermissions to which that symptom is occasionally subject. It is well known that between July 1847, when hæmoptysis occurred to the extent of many ounces, and October of the same year, when cough recurred, accompanied with a rust-coloured expectoration, Mr.

Liston enjoyed an almost complete immunity from symptoms; an immunity so remarkable, that all the warnings of his medical advisers were inadequate to lead him to consider himself an invalid. Mr. Liston died in December 1847, five months after the first gush of arterial blood, and without any new considerable hæmorrhage. The trachea was found perforated in three or four places, and portions of the aneurismal clot were discovered projecting through the openings, and partially blocking them up. It is evident, therefore, that in Mr. Liston's case the rupture of the aneurismal sac had actually taken place at least five months before death; and that, notwithstanding the unclosed openings, no discharge of blood such as to cause serious alarm to the patient himself had occurred in the interval.*

Mr. Liston's case was probably not unique even at the time at which it was published. Very few recorded cases, however, exist in the literature of medicine, in which death was postponed so long as five months after a serious hemorrhage from a mucous surface in aneurism. In proof of this assertion, I may mention that Dr. Edwards Crisp's laborious collection of cases, added to my own reading, and to a list with which I have been kindly furnished from the manuscripts of Dr. Sibson, whose very elaborate and careful study of this subject is known to the members of the Society, furnish in all only nine, or perhaps ten, instances in which an interval of a month or more existed between hæmoptysis and death, and in very few of these was there an

* See *Lancet* for December 1847, p. 633.

interval nearly so long as in Mr. Liston's case between a considerable bleeding and the fatal event. In one case, indeed, pointed out to me by Dr. Sibson's manuscript,* hæmoptysis is stated to have taken place seven years before death; the connection of the hæmorrhage with the aneurism, however, does not appear to be clearly made out by the history. Of all intervals less than a month the examples appear to be numerous enough.

It has been my fortune to observe and describe two cases of aneurism in which very copious bleeding occurred on a mucous surface, and in which the interval between the first bleeding and death greatly exceeded that observed in the case of Mr. Liston. One of these cases is the one just narrated; the other was a case of aneurism of the superior mesenteric artery (Case VI.), remarkable as a specimen of a rare disease, and a curious, though incorrect, diagnosis, but still more remarkable as shewing a minute opening (from the aneurismal sac into the duodenum), which was nearly closed at the time of the patient's death,

* Guy's Hospital Museum, Preparation 1489[50]. I have had an opportunity to-day (April 25th 1859) of inspecting this preparation, along with Dr. Sibson. The aneurism forms a three-fold dilatation, involving the greater part of the arch, and communicating by separate openings with the ascending and the descending aorta. The anterior part of the aneurism is extensively adherent to the right lung, while the posterior sacculated dilatation communicates with the left bronchus. The patient was a woman of fifty-four years of age. She had suffered for some months from palpitation, with pain between the shoulders, at times extending down the left arm; but "it was ascertained after death that she had been the subject of hæmoptysis six or seven years before." She died of hæmoptysis shortly after admission.

but had led to profuse hemorrhage twenty-two months before the fatal rupture into the peritoneum.*

When aneurisms open into serous cavities it is rare to find death long delayed. Dr. Stokes, however, has referred to one case, where some days probably intervened between rupture into the pericardium and death. I have seen one instance in which "hemorrhagic pericarditis" was caused by aneurism, and where I was led at the time very strongly to suspect that what appeared to be an inflammatory effusion was in reality a hemorrhage, the blood having been churned about in the course of the movements of the heart so as to decolorize its fibrin. In a case of aneurism fatal by rupture into the peritoneum, also, it has occurred to me to observe adhesions which appeared to have confined the hemorrhage, during some days at least, to the posterior part of the sac. But these cases are extremely few.

Several cases have been recorded in which, in aneurisms opening on the external surface of the body, a considerable interval existed between the opening and the fatal event. One of the most curious of these is a case communicated by Mr. Ramsay, surgeon at Broughty Ferry, to Mr. Syme.† A man affected with aneurism of the arch and of the innominate artery, lost a very large quantity of blood from a rupture opposite the cartilage of the third rib. The stream of blood is reported as being "somewhat larger than a quill;" and, strange to say, the patient, "nothing alarmed, got hold of a bowl,

* See case vi. p. 495.

† *Monthly Journal of Medical Science*, vol. x. p. 89.

and held it at arm's length to receive the red arch, which he supposed was the contents of a "bloody boil," pressing the tumour with his chin to effect a more speedy clearance. After about a quart of blood had gushed out, he fainted, and the bleeding stopped." The patient lived for four months, without any new bleeding, and died in the end, not of the aneurism, but of "typhus fever." Not less curious is a case communicated by Dr. Neligan to Dr. Stokes.* A ship-carpenter, aged 56 years, had all the signs and symptoms of an aneurism of the aorta, opening externally about the second rib on the right side, in front. For more than a year the tumour discharged at intervals, sometimes copiously and in a continuous stream, a quantity of blood sufficient to cause alarm, and occasionally arrested with difficulty. Two of these hemorrhages occurred under Dr. Neligan's observation. After the first of them, which was by far the most considerable, the tumour diminished considerably in size, and became much more dense, losing the fluctuating character it had previously had. The cough and dyspnœa also subsided, and three weeks after the last hemorrhage the patient left the hospital, "stating that he felt quite well." This is assuredly one of the most remarkable cases on record. The only flaw in it, as an observation, arises from the very circumstance that makes it so interesting—viz., that no opportunity occurred of ascertaining, with absolute certainty, that it was an aortic aneurism. I agree, however, with Dr. Stokes, in thinking that the early signs and symptoms leave no reason-

* *Diseases of the Heart and Aorta*, p. 582.

able doubt of this diagnosis. And it is possible, therefore, though perhaps hardly probable, that this case may have ended in a more or less permanent cure of the disease ; or, at all events, in long-continued exemption from external hemorrhage, and death from some other cause.

Cases are not very uncommon, in which aneurisms of the aorta, after opening on one mucous surface and leading to hemorrhage, are actually fatal by hemorrhage in another direction, or by some other complication. I have several times seen an aneurism open nearly simultaneously into the trachea and œsophagus; the fatal event having been probably delayed for days after both openings. Similar cases have been recorded, and preparations illustrative of such double openings will be found in many museums. Rupture of an aneurism into the œsophagus, and into the alimentary canal generally, is probably rarely detected until the hemorrhage is very large, indeed fatally large ; because small bleedings, discharged downwards into the stomach and intestines, are almost sure to pass unnoticed. Hence we rarely obtain the opportunity of observing closely the process of rupture on the œsophageal mucous membrane. In the air-passages it is different ; very small discharges of blood being here easy of detection. Sometimes, indeed, the true nature of the disease is overlooked, and the hæmorrhage is ascribed to pneumonia, pulmonary hemorrhagic condensation, malignant disease of the lung, or, perhaps quite as commonly, to ordinary tubercular phthisis. I have seen each of these mistakes made by physicians

nowise incompetent or inattentive ; indeed, where large gushes of blood occur in connection with obscure physical signs, it is not unfrequently all but impossible to decide between aneurism and phthisis, unless the history, age, and appearance of the patient constitute a ground of decision. Even these grounds of diagnosis, carefully investigated, sometimes fail ; and I have notes of one case, where large quantities of cod-liver oil were ordered, not unreasonably, nor without benefit, to an aneurismal patient, under the idea of his being consumptive, for several months before the true diagnosis was made.

But the cases in which aneurism is most apt to be overlooked after rupture of the sac are those in which no large hemorrhage occurs ; but in which, for weeks together, perhaps for months, an inconsiderable leakage occurs into the air-passages, assuming the form of—1st, a frothy bronchitic sputum *streaked* with blood ; 2d, a rusty sputum very like that of pneumonia, but usually more abundant, more frothy, and less viscid ; 3d, a deeply dyed purple or brownish-purple sputum, like the so-called "prune-juice" expectoration, characteristic of the third stage of pneumonia, and of certain forms of pulmonary hemorrhagic condensation from valvular disease of the heart ; 4th, any of the preceding, alternating with small discharges of pure, unmixed, but generally imperfectly coagulated blood. All of these forms of expectoration I have seen on several occasions ; most of them occurred at different periods in the preceding case. In general, it may be remarked, that the bronchitic

varieties of sputum, either stained or streaked in different proportions with blood, occur chiefly in tumours pressing directly on the trachea and larger bronchi, and not producing consolidation of any part of the lung; while the "prune-juice" sputum, and the varieties more truly resembling the expectoration of pneumonia, occur when the lung is directly involved in the tumour; or when by long-continued flattening of a bronchus, perhaps with extensive ulceration of its mucous membrane, and necrosis of its cartilages, secondary changes have been induced in the pulmonary texture. Such changes are rarely of the nature of inflammation, at least in the first instance. They partake more of the nature of collapse of the air-cells, which is sometimes the result of mere pressure on a bronchus, producing obstruction to the evacuation of the bronchial and pulmonary excretions; while occasionally we find a chronic and continuous infiltration of the lung with blood, either from the aneurismal sac through the bronchus, or from rupture of the aneurism directly into the pulmonary air-cells. The "lobular" character of these lesions is often very clearly demonstrable, and shews that they spring, not from inflammation proper, but from some derangement of the mechanism of the air-passages. In the more advanced stages, however, inflammatory changes are apt to occur; and I have repeatedly seen an entire lung, or some considerable portion of it, ulcerate and break up into suppurating cavities, under the continued pressure of an aneurism upon the bronchus. In one or two cases this has been attended with many of the symptoms and

signs of tubercular ulceration, and the morbid appearances after death have also to a remarkable degree resembled those of softened tubercle; confined, however, absolutely to the side on which the aneurismal pressure took place. Possibly some of the cases alleged to be "aneurism associated with phthisis," may have been of this kind.

In systematic works, it is customary to ascribe the minor hæmorrhages to which I am now alluding, not to rupture of the sac, but to "congestion of the lung" from pressure on the veins, and consequent impediment to the return of blood. I am certainly not prepared to maintain that the pressure of aneurisms on the pulmonary veins never causes hemorrhage. But that this is the chief cause of minor hemorrhages cannot, I think, be admitted; 1st, because even in cases in which pressure on the veins may have occurred, hemorrhage (at least continuous or repeated hemorrhage) is almost always associated either with direct pressure of the sac upon the lung, or upon an ulcerated bronchus; 2dly, because some of the most characteristic cases I have observed of slight and continuous hemorrhage have been from aneurisms in which no pressure on the pulmonary veins was possible; but in which there was undoubtedly pressure upon, and opening of the sac into, the trachea. I may refer in particular to two cases of aneurism with laryngeal symptoms, which I recorded some years ago with a view chiefly to other points of interest, but in one of which the patient died of dyspnœa after weeks of constant though inconsiderable hemorrhage; while in the

other a fatal hemorrhage occurred after many days of very slight tinging of the sputum. In both these cases there was no reasonable doubt, from the condition of the mucous membrane of the trachea, that the blood must have come from the sac.*

While, therefore, I will not venture to say that blood in the discharges of a patient affected with aneurism *always* indicates the communication of the sac with a mucous membrane, I believe it *generally* does so ; and more especially is this the case in aneurisms accompanied by hæmoptysis, if the pressure of the tumour be on the trachea, and if it be unaccompanied by the indications of pulmonary change. The importance of this view, if correct, both as regards the diagnosis of obscure cases of aneurism, and the prognosis of this disease in well-marked cases, it is unnecessary to point out at length. I may be permitted, however, to detain the Society over one view of diagnosis, which, if it be as generally applicable as my own experience would lead me to affirm, must be one of considerable importance. In cases characterised chiefly or exclusively by laryngeal symptoms, it is often extremely difficult to arrive at a satisfactory conclusion as to the cause of the very distressing dyspnœa. Now it is precisely in this class of cases that the repeated presence of even small quantities of blood in the sputum becomes a most valuable means of diagnosis. For, if there be laryngeal dyspnœa and stridulous respiration (which are seldom present to any marked extent in mere laryngeal phthisis) ; if the epiglottis be not thick-

* Case I. p. 455; and II. p. 470.

ened; if the mucous membrane of the larynx, in so far as it is within reach of the finger, be sound; and if, with these signs, positive and negative, there be a persistent tendency to even the slightest amount of blood in the sputum, while auscultation and percussion give negative results both as regards the lungs and heart, I believe that aneurism may be predicated with as near an approach to certainty as is possible without the physical signs of tumour; and further, the aneurism will be small; it will arise from the back part of the arch, or from the commencement of the innominate artery; and it will be so placed as to entangle either the left or the right recurrent nerve. These considerations have more than once led me to the diagnosis of aneurism under circumstances where, without them, it would have been impossible to give a decided opinion; and hitherto they have not led me wrong. In fact, there is but one form of disease which, in any considerable number of instances, leads to laryngeal stridor and to hæmoptysis, without positive ulceration of the larynx. Malignant tumours very closely resemble aneurism in their diagnosis in these respects. But I have never yet seen malignant disease of the chest leading to hæmoptysis, without manifest physical signs of disease in one or other lung. So that I am inclined to believe that the rules of diagnosis mentioned above, will hold good in the great majority of instances.

The wood-cut on the next page illustrates the usual mode of opening an aneurism upon a mucous membrane. It is from the case of a man who died, not of hemor-

rhage, but of suffocation ; but in whom, nevertheless, an opening into the trachea existed, which had yielded blood in small quantities for some time before the fatal event. The rupture has been arrested by death at its earliest stage ; and it will be observed that while five or six minute papillary eminences, with distinct pale apices on a congested membrane, are to be seen, only one of these has been perforated ; the actual opening not exceeding the size of a pinhole. The mucous membrane as viewed from within, opposite a necrosed cartilaginous ring, is puffy and thin ; and in all probability a larger rent would ere long have formed there. But it is very easy to understand, on looking at this drawing, how these small pinhole openings should sometimes heal up ; especially after a sudden removal of pressure, such as occurs after a copious hemorrhage. Such would appear to have been the course of events in the case of Mr. J.

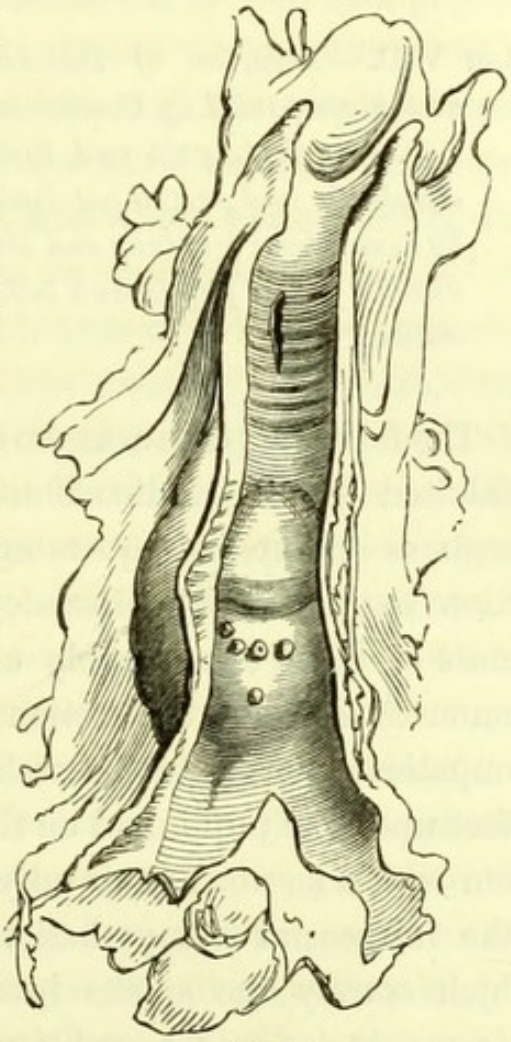

Fig. 13.

Perforation of Aneurism in a mucous membrane, as described in the text.

B—, and also in that of Mr. Liston ; as well as in the case of aneurism of the superior mesenteric artery referred to above.

CASE VIII.—*Aneurism of the Aorta projecting into the neck, and accompanied by Contraction of the Pupil on the affected side. At first, pain and tumour only; at last, hoarseness, dysphagia, and partial paralysis of fingers of left arm. Death by hemorrhage into lung and stomach simultaneously. Experiments on the pupils with belladonna. Post mortem examination.*

The following case was narrated, and the patient was exhibited to the Medico-Chirurgical Society of Edinburgh, as an interesting example of a pathological condition explicable by physiological laws. The experiments of Petit, considerably upwards of a century ago, demonstrated that the section of the united vagus and sympathetic nerves in the neck of the dog has a marked effect upon the pupil, and on the conjunctiva of the eye; from which he not unnaturally drew the inference, that "the intercostal (sympathetic) nerves furnish branches, which convey the spirits into the eyes."* Although this conclusion was a sufficiently startling one, and although Cruickshank, Dupuy, and others performed experiments tending more or less distinctly in the same direction, the first exact investigation of the subject was due to the late Dr. John Reid, whose attention was attracted to it in the course of his researches on the vagus

* *Histoire de l'Académie Royale des Sciences*, An. 1727.

nerve, and who succeeded in proving distinctly the dependence of contraction of the pupil upon section of the sympathetic in the neck, independently of every other source of disturbance.* Dr. Reid did not fail to anticipate for this inquiry a pathological as well as a physiological importance, and he refers to "a case described in the *Medical Gazette*,† where the right carotid, the vagus, and surrounding parts are described as being enveloped in a large morbid tumour, and where, consequently, the sympathetic could hardly be supposed to escape," in which "the pupil of that side is described as becoming smaller in the course of the disease." Valentin, from further experiments, and from a consideration of the whole arrangement of the nerves involved, concluded that the pupil derives its nervous supply from two sources; the nerves, which act on the radiating fibres of the iris, from the spinal system, *through the sympathetic,* and those which supply the circular fibres (or those which contract the pupil) from the inferior branch of the motor oculi nerve. The section of the sympathetic trunk in the neck, according to Valentin, paralyses the former nervous filaments, and resigns the pupil to the exclusive influence of the circular fibres, which keep it in a state of permanent contraction. The more recent experiments of Budge and Waller tend to confirm the views of Valentin, and to shew that while the circular fibres are supplied from the third (oculo-motor) and also the fifth (trigeminus) cerebral nerves, the radiating fibres

* *Edinburgh Medical and Surgical Journal*, January 1841.

† September 29, 1838.

receive filaments from the sympathetic, which join the ophthalmic branch of the fifth after it has passed through the Casserian ganglion. Hence, stimulating the sympathetic in the neck dilates the pupil, and cutting it causes contraction of the pupil, more or less permanent. These experiments further appear to shew that the whole of the sympathetic fibres which go to the pupil from the cervical ganglia, are originally derived from the anterior roots of the spinal nerves, and, consequently from the spinal cord in the lower cervical and upper dorsal region. If the spinal cord be destroyed between the fifth cervical and the sixth dorsal vertebrae, contraction of the pupils occurs. If one side only of the spinal cord be destroyed in this region (which MM. Budge and Waller call the *regio cilio-spinalis*), or if the emerging spinal nerves or their anterior roots be cut, a similar effect is produced. Further, a consideration of the different distribution of the nerves in different animals goes far to explain the discrepancies which have been met with in former experiments.

These investigations seem to be very clearly applicable to the explanation of the case to be presently detailed. They are mentioned here, because the subject does not seem to have attracted the attention which it deserves from physicians. At the time I introduced this patient at the Medico-Chirurgical Society, I was not aware that the occurrence of contracted pupil, as a symptom of thoracic or cervical aneurism, had ever been placed on record. I remarked, indeed, that accidental differences in the size of the two pupils were not un-

common; and that in the present, or in any other isolated case, the observation would be very apt to be disregarded, as a mere coincidence, unless the physiological explanation were present to the mind of the observer. I have, however, had my attention directed to a case of aneurism at the root of the neck, noted by Dr. Walshe in the last edition of his work on *Diseases of the Lungs and Heart*, p. 759, in which one pupil was observed to be "very notably smaller than the other, where no cerebral symptom of any kind existed." Unfortunately, the state of the nervous structures in the neck does not appear to have been investigated; and I am informed that no recorded details exist, as to the position of the aneurism, sufficiently precise to allow of an accurate appreciation of its relations. The previous observation of this case, however, together with the one above noticed in the *Medical Gazette*, will, I trust, concur with the facts adduced in this communication, in fixing the attention of physicians on the state of the pupils in similar cases of disease.

Physical Signs, etc.

J. W., æt. 40, quarryman, very robust. Was seen first by me in autumn 1854. He had at that time all the signs of a considerable aneurism at the root of the neck in the left side. The expansion of the sac was chiefly above the clavicle, but there was dulness on percussion below it for about half an inch. There was no bruit—only a strong double vibrating shock; second sound natural over aorta. The circulation not interrupted either in subclavian or carotid, if anything, rather feebler in left subclavian than right. Never any oedema of left arm, but numbness and pain frequently experienced. More pain in arm than at site of

tumour; considerable darting pain, too, around the back of shoulder; heat of left side of face and head, but face never seemed to flush. No evidence of hypertrophy or other disease of heart.

He did not at first ascribe his disease to any particular accident; but afterwards gave the following account of it to Dr. Inglis, resident-physician:—

History.

"On one occasion, two or three years ago, he recollects distinctly lifting a smith's anvil 22 stone in weight. He had a bad grip of it, and to hoist it up was obliged to rest the greater part of the weight on his left arm. He felt himself very much strained, and after completing the work he was quite blind for a time; for long after he had pain in his left side. He applied to a doctor, but got no relief. He continued at work till the day before entering the hospital, although not able to do as much as formerly. The pain of the arm going down to the fingers began about a year after the strain. He has no recollection of any later accident. For twelve months he has had difficulty of breathing on going up a hill. Never noticed that there was a swelling, until it was pointed out to him in the hospital."

Observations on Pupils.

The difference in size of the pupils, as shewn to the society last December, was made the subject of particular observation throughout the progress of the case. At the time of my first seeing the patient, the difference was quite as great as it ever was at any subsequent period; indeed, for a good many weeks before death it had become scarcely recognisable. Both the pupils were of small size, but the left very remarkably so, generally not more than a line in diameter, in the light. Both pupils dilated and contracted under different degrees of light, but the dilatation of the left, even in deep shadow, was very slight. This observation was made repeatedly during six weeks, during which nothing was applied calculated to affect the pupils. Once or twice, it was thought that the left conjunctiva was slightly congested, but this cannot be stated with confidence.

In December several experiments were made with atropine and belladonna, of which the following are the results.

Experiments with Atropine.

1. The affected pupil was susceptible of dilatation under a solution of atropine placed on the conjunctiva. The dilatation was perhaps scarcely complete, but very nearly so. It did not commence, however, till about three-quarters of an hour after the atropine was applied. The dilatation continued nearly two days, and for several days more the original inequality of the pupils did not return.

2. The pupils being in the usual condition of inequality, extract of belladonna was given internally in repeated doses, till both pupils were dilated. In doing so, it was observed that throughout the experiment the left pupil continued smaller than the right.

The tumour did not undergo much enlargement till the end of the year. Considerable relief was derived from the application of freezing mixtures, although the pains were scarcely ever absent. No internal remedies were applied. Various liniments were tried, and given up in favour of the application of cold.

Progress.

On December 31, in consequence of increased pain and throbbing in the tumour, local depletion by leeches was employed for the first time, and the patient was put on a very restricted scale of diet. He had always been a moderate eater, and found, as he told me, no difficulty of restraining his appetite, as the pain often took away the desire for food. For a time, too, he thought he derived benefit from this system. I allowed him to have leeches repeatedly applied, and he decidedly lost flesh and strength. The tumour, however, extended very decidedly during January and February. Freezing mixtures were continued. On February 27, 10 oz. of blood were taken from the arm, but I was not induced to repeat this experiment. By this time the local applications had lost their effect, and the pains had become greatly aggravated. Repeated doses of opium were required to procure sleep. On March 27, it is noted, that "he takes 80 minims of solution of morphia every night." About

this time I observed him seated near the fire with both hands grasping the edge of a shelf almost as high above his head as he could reach. He said, that in that position the pain was somewhat alleviated. He was much weaker at this time from want of sleep and general distress, but never lost his appearance of being a strongly made muscular man.

In the course of the month of March, a number of sedatives were tried, aconite, cannabis, opium applied by Dr. Wood's method. He always begged, however, to be allowed to return to the internal use of morphia in large doses, which he found to be, on the whole, the most effectual way of procuring rest. The tumour continued to become more and more diffused, occupying a great part of the posterior triangle of the neck, but not tending at any point to become superficial. Two new symptoms were noticed, or at least became much more distinct during this interval; slight cough, with mucous expectoration, and diminished power of motion of the fingers of the left arm, which were usually bent half-way into the palm. A little dysphagia, and slight hoarseness of voice, had been present from the first time he came under observation; and these too, increased at this time. On April 12, he expectorated a little blood. His general uneasiness, and all the chest symptoms, were likewise increased. He lay mostly in bed. On April 22, he brought up two or three teacupfuls of arterial blood, and suddenly expired.

New Symptoms.

Hæmoptysis.

Death by Hæmorrhage.

On examination after death, an aneurism was found arising, by an opening about an inch in diameter, from the upper part of the aortic arch. The sac involved the origin of the left carotid, which vessel was slightly dilated for about three-eighths of an inch above its origin, but elsewhere normal. The left subclavian artery was normal throughout, and lay in front of the aneurismal sac at its connection with the aorta. The sac, which was irregularly oblong, and appeared to be of the size of a very large lemon, occupied the subclavian space and root of the neck on the left side, and passed deeply backwards to the 5th, 6th, and 7th cervical, and to the

Post-Mortem Examination.

1st and 2d dorsal vertebræ, with which it was in contact. In front, the deep fascia of the neck, the carotid artery, jugular vein, and pneumogastric nerve were slightly stretched over the sac,

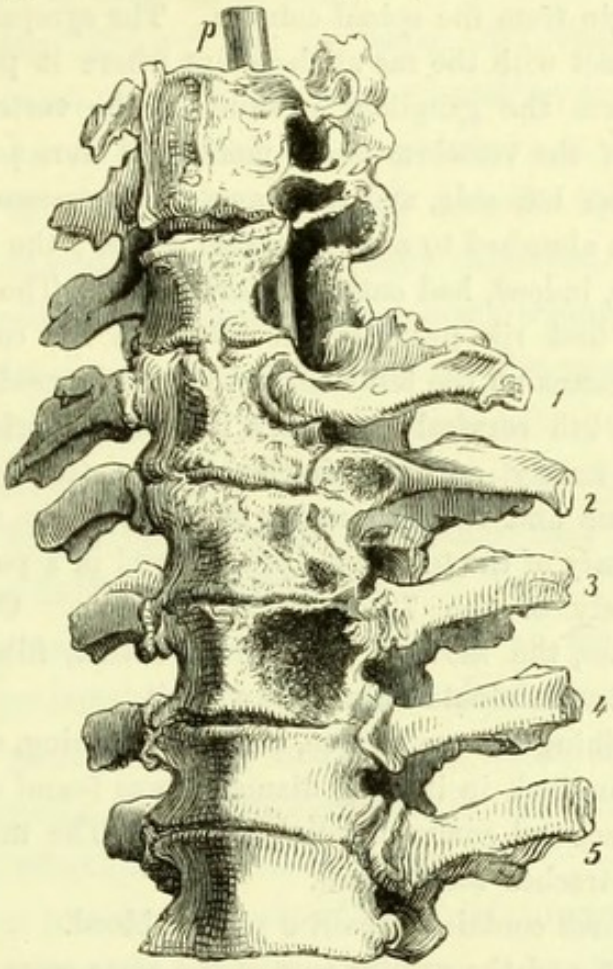

Fig. 14.

The left side of the lower cervical and upper dorsal spine, in the case of J. W., shewing erosion of the bodies of several of the vertebræ, extending quite into the spinal canal at one point, as is shown by a metallic director passed into the canal from above. The ribs are indicated by numbers at the side, and guide the eye sufficiently to the anatomical relations.

which, however, exerted its chief pressure in the opposite direction. The subclavian artery and vein, and the scaleni muscles were also very slightly displaced forwards and outwards, but were not adherent to the sac. The inner and back part of the tumour adhered closely to the vertebral column, and to the œsophagus ; it may also have exerted slighter pressure on the trachea and the

left recurrent nerve, but these did not appear to be much displaced. The lower divisions of the brachial plexus were stretched over the sac at its upper part, where it came in contact with their point of origin from the spinal column. The sympathetic nerve came in contact with the sac at the point where it passes downwards to form the ganglion in front of the vertebral artery. The bodies of the vertebræ above mentioned were pretty deeply eroded on the left side, and the transverse processes of the 6th and 7th were absorbed to a considerable extent; the latter transverse process, indeed, had entirely disappeared. The upper surface of the first rib near its tubercle, and the corresponding transverse process on the left side, were slightly eroded. At the level of the 7th cervical vertebra, a large oval aperture of communication, nearly an inch in length, existed between the aneurismal sac and the dura mater of the cord, in consequence of the deficiency of the transverse process, and of a portion of the arch and body of the 7th cervical vertebra. Opposite the diseased bones, the sac was, to a great extent, filled up with moderately firm laminated fibrinous deposit.

On examining the œsophagus, a ragged opening, about three-quarters of an inch in its long diameter, was found opposite the aneurismal sac, and communicating with it. The mucous membrane of the trachea was normal.

The stomach contained about a pint of blood.

The heart, and the greater part of the aorta were normal.

The apex of the left lung was firmly adherent to the aneurismal sac, over more than a square inch. The sac was at that point very thin. The lungs, elsewhere, were normal, as were the other organs.

The pupils were in the usual slightly dilated condition observed after death, and there was no appreciable difference between the two sides in the amount of dilatation.

The occurrence of this examination during the holidays, when my friend Dr. Struthers was absent from Edinburgh, prevented my securing his co-operation

in a plan which I had contemplated, of having the whole dissection conducted under his eye, and in his anatomical rooms. The interest which he took in the case during life, led me to hope that the anatomical relations of an aortic aneurism, presenting so remarkable a character as that which I pointed out on a former occasion to the Society, would receive that full and complete investigation which they deserved. As it was, the impossibility of retaining the body when not claimed for the school, and my own want of the necessary skill, have combined, I fear, to render this account of the dissection less satisfactory than was to have been desired. Still, the following facts and inferences may be regarded, I think, as fully established :—

1. The aneurism did not, as I at first supposed, involve the inner portion of the sub-clavian artery, but lay on a plane entirely behind it. This fact explains the extremely slight impediment to the circulation of the limb, whether arterial or venous.

2. The aneurism exerted its chief pressure in a direction backwards and inwards ; more on the emerging roots of the spinal nerves, and their connection with the sympathetic, than on the carotid, pneumogastric, or recurrent. This was fully anticipated from the symptoms, and from the situation of the tumour, as I indicated in December. Since that period, however, the marked hoarseness of voice, and evidences of œsophageal pressure, shew that the aneurism had, at a late period, been extending so as to involve structures originally not much affected.

3. The vertebral artery, and the sympathetic ganglion lying upon it; a portion, at least, of the brachial plexus; the anterior roots of several of the lower cervical nerves, with the branches given off by them to the sympathetic; the inferior attachments of the longus colli muscle, must have been either entirely sacrificed. or very much altered in their structure and relations. In addition to these extensive encroachments, the aneurism may have exerted a certain amount of pressure on the spinal cord; and especially on that region of it described by Budge and Waller as the cilio-spinal. The localized character of the paralysis, however, and particularly the small amount of paralysis of sensation in the left arm, render it probable that the spinal cord had not been actually disorganized to any appreciable extent, and that the chief pressure of the aneurism was on the anterior roots of the spinal nerves. In these facts the symptom shewn to the Society, the permanent contraction of the pupil, finds ample explanation.

4. It is uncertain whether the equality of the pupils, in the last few weeks of life, depended upon the assimilation of the condition of the left to that of the right, or of the right to that of the left pupil. In the latter case, it is open to question whether the contraction of both pupils was a result of interference with the cord, or of the opium which the patient took so largely as an anodyne.

Finally, it may be supposed that the equalization of the two pupils, in the latter part of the history of the case, was due to some compensating or collateral nervous

influence, which had arisen to supply the deficiency caused by the pressure of the aneurism. It is certain that the dilating power of the pupil was not in this case (at least at the time of the experiments with belladonna) destroyed, but only weakened.*

5. Death took place by hemorrhage into the œsophagus, from which the stomach and a portion of the intestinal canal appear to have been filled with blood before any was ejected by vomiting. The comparatively small quantity of blood which came up with the sputum, may possibly have been hawked up from the œsophagus; but it seems more probable that it was the result of direct bleeding of the sac into the apex of the left lung. Three fatal terminations, therefore, were simultaneously impending: 1*st*, hæmorrhage into the œsophagus; 2*d*, hemorrhage into the lung; 3*d*, pressure on the cord and paralysis. Perhaps we may also consider that serious symptoms connected with the respiration, were not far off; as the recurrent nerve and the trachea, would very soon have been involved.

I have thus attempted to indicate a new source of functional disturbance in thoracic aneurism, as connected with the interference of such tumours with the nervous system. In this point of view, the present case may be advantageously considered in connection with those

* It would appear from one of Dr. Reid's experiments, that the contraction of the pupil in these cases is not absolutely permanent. "In a cat . . . the pupil was nearly natural a month after portions of the sympathetic and par vagum on one side were removed. *Loc cit, in a note.*"

which I have laid before the Society on former occasions. It results from the whole series, that aneurisms of the aorta may, in virtue of their pressure on different portions of the nervous system, produce four different classes of symptoms: 1*st*, angina pectoris; 2*d*, spasmodic laryngeal dyspnœa; 3*d*, spasmodic asthma, or bronchial dyspnœa; and 4*th*, permanent contraction of the pupil on the affected side. On the important considerations connected with diagnosis and treatment involved in these four aspects of aneurismal disease, I need not now detain the Society by any further remarks.

The case narrated above, of aneurism at the root of the neck, accompanied by contraction of the pupil on the affected side, forms an illustration of the results of injury to important nervous structures in that situation. Taking the case in connection with the physiological data there mentioned, I believe this conclusion to be irresistible; but as others may be led to suppose that the interference of the aneurism with the circulation of the left carotid had, by its influence on the cerebral circulation something to do with the contraction of the pupil, I am led to advert again to the subject, in order to shew that this was probably not the cause. I take pleasure in referring to the elaborate and interesting inaugural dissertation of Dr. Kussmaul,* "On the Influence of the Circulation on the movements of the Iris, and other parts of the Head," as bringing to the test of experiment all that can be said on this view of the subject.

* Wurzburg, 1855.

Dr. Kussmaul's researches shew, that the result of suddenly cutting off the flow of blood through the carotid arteries is to produce a certain amount of contraction of the pupil, *followed*, however, *after a short interval, by dilatation.* On the other hand, an increase in the flow of blood is usually succeeded by dilatation of the pupil. But, admitting that these results are clearly and unquestionably as stated, it does not appear that a really permanent and considerable contraction is, under any circumstances, the consequence of interference with the circulation alone. I am led, indeed, by the extreme care and delicacy with which the measurements in Dr. Kussmaul's experiments were conducted, and his great and praiseworthy caution in endeavouring to avoid collateral sources of error, to the conclusion, that the amount of effect produced was by no means such as to be easily ascertained, or to carry confidence to his mind by any means short of those which he employed. It could not, therefore, have been in any degree comparable with the contraction of the pupil produced by interference with the sympathetic trunk, or with that observed in the case of aneurism narrated above.

But it is always best to let Nature answer the questions which she herself proposes. The future observation of cases of aneurism, with special reference to this point, will furnish ample data for deciding to what extent, and in what manner, the pupil is affected by the permanent suppression of the circulation through the carotid, on one or other side. In the meantime, I beg to

contribute to this inquiry the following abstract of a case which seems (so far as a single carefully observed instance may be trusted) to prove that the influence exerted by an aneurism upon the pupil, through the medium of the arterial circulation, is not considerable or even permanently appreciable. The case is in other respects, also, worthy of being recorded.

CASE IX.—*Aneurism of the aorta, occupying the arch, and obstructing the left carotid and subclavian arteries; with lesser aneurisms, one of which opened into the left auricle. Aortic and mitral regurgitation; murmurs, probably valvular; pain in paroxysms (angina pectoris); husky voice; no affection of pupils.*

T. G., tailor, æt. 45, a bloodless, feeble, but not greatly emaciated person, was at intervals under my care for many months during the end of last year, and during the present spring and summer. He complained chiefly of a sensation, which he termed "breathlessness," but which, on examination, proved to be more similar to the angina pectoris of Heberden. His voice was husky and high-pitched; nevertheless he had no severe laryngeal symptoms, and no dysphagia; his chest expanded readily and fully, and there was no lividity. He complained of pain about the left arm and shoulder, very much increased by stretching the left arm upwards, or by bending the neck towards the right side. In sitting he stooped very considerably; when recumbent, he was often seized with paroxysms of suffering. Angina-like paroxysms were also readily brought on by agitation, and even by any kind of examination into his case. The feet were œdematous. The following facts were ascertained on physical examination:—

General Symptoms.

In the main trunks of the left arm, and in the left carotid,

the pulse was entirely suppressed. In the *superficialis volae* of the left side, pulsation could sometimes be discovered. The carotid and subclavian of the right side pulsated with extraordinary strength and fulness, the pulsation being attended by a vibrating thrill and a whiffing murmur.

Physical Signs.

At the upper sternum, from the middle of the clavicle on the left side to one and a half inches beyond the sterno-clavicular articulation on the right, there was dull percussion, merging into the cardiac dulness at the third left costal cartilage. The cardiac dulness at the level of the nipple was upwards of four inches by light percussion, and extended to the right of the sternum. At the upper sternum there was a short systolic murmur, of a somewhat rasping character, but distant and inarticulate. The second sound was suppressed, or nearly inaudible, over the whole upper sternum. At the lower sternum there was heard, with each sound of the heart, a very loud, distinct, and articulate murmur, —that of the second sound, however, predominating, both in length and loudness, over the first. The second sound itself was entirely lost in the murmur. At the apex of the left ventricle, the first sound was heard, accompanied by a very loud and distinct murmur,—the second sound being inaudible, and without murmur.

The vertebral column was considerably bent in the cervical and dorsal region, but nowhere angular unless at the sixth and seventh dorsal, where it was a little more prominent than elsewhere. No localized dulness of percussion along the spine ; but the left back, near the spine, was generally a shade more dull than the right.

At the left apex, before and behind, the respiration was much diminished ; elsewhere, natural. A blowing murmur was heard along the aorta, in the left back.

The pupils throughout the disease, were found, on repeated examination, perfectly similar in size, perfectly contractile, and apparently quite natural.

After a protracted illness, this patient finally succumbed to

the ordinary symptoms of valvular heart disease. He had little or no cough or expectoration, but the dropsy increased and gained the chest, causing great orthopnœa. During the last week of life, the right side of the face was very dropsical, as also the parietes of the chest and back. The conjunctiva of the right eye was so much relaxed, as to form folds infiltrated with serum, which protruded between the eyelids, and gave the face an exceedingly distressing appearance. Ultimately he sunk exhausted, in the course of July 1855.

Progress of Case.

The body was examined on the second day after death. The majority of the organs were free from considerable disease. The lungs were highly œdematous, and the pleuræ contained fluid on both sides. The heart was enlarged and dilated; it may probably have weighed 20 ounces or more; but being removed along with the aneurism and the half of the sternum, it was not weighed. The upper sternum, where dull percussion existed during life, was closely attached to a large tumour which arose from the aortic arch, and passed backwards so far as to have caused slight erosion of the bodies of two or three dorsal vertebræ. The tumour was also closely attached to the apex of the left lung, but neither the air-passages nor the œsophagus were in the slightest degree involved in it.

Post-Mortem Examination.

On laying open the large tumour, by an incision parallel to the sternum, and from above downwards, it was seen to involve the whole upper part of the arch, which, from its commencement at the aortic valves to its termination in the descending aorta, was dilated and atheromatous. The left half of the dilated portion was filled with very firm laminated clots of pale fibrine, which passed from the sternum to the back part of the sac, completely overlaying and blocking up the openings of the left carotid and subclavian trunks. The innominate artery was perfectly free, but did not appear enlarged in proportion to the force and fulness of its pulsation during the life of the patient.

The aortic valves were incompetent. On examination, their

defective action was found to proceed from a separation, to the extent of about an eighth of an inch of the two posterior segments, which were a little thickened at this part, but otherwise normal. The divergence of the segments appeared to be the result of a yielding of the arterial wall at this point; immediately above and immediately below the segments in question, an opening led into a small aneurismal sac. The upper of these aneurisms would have held a large filbert; it communicated with the aorta by an oval opening half an inch in diameter. The lower sac, which had a somewhat smaller orifice towards the endocardium, passed directly backwards to the left auricle, with which it communicated by an opening which admitted an ordinary catheter with ease.

The mitral valve was obviously incompetent, owing to the rigidity of that portion of its curtain nearest the aortic orifice. Here, also, a small sacculated dilatation of the endocardium was formed, bordered on the one side by the muscular substance of the heart, and on the other by the rigid portion of the mitral valve.

The rest of the heart, though enlarged, presented no valvular or other deformity.

In commenting upon this case during the life of the patient, I repeatedly called the attention of the students, who saw it with me in the Infirmary, to the state of the pupils in connection with the position of the aneurism, and the obstruction to the circulation in the left carotid. That no appreciable *permanent* effect upon the pupil is produced by an interruption to the carotid circulation *per se*, is, I think, sufficiently proved by this single instance.

In regard to the murmurs, I remarked, that while it was by no means impossible that one or both of them might be generated in the aneurismal sac, there was a much greater probability that they were due, in this in-

stance to valvular disease of the heart itself. The existence of such disease seemed presumable from the great amount of hypertrophy and dilatation of which there was evidence; while the ordinary rules of physical diagnosis permitted us to refer the systolic murmur in great part to the mitral orifice. That the diastolic murmur heard at the lower sternum, was due to the aortic regurgitation, appeared probable from its great intensity and prolongation as compared with the systolic bruit, which is usually the louder of the two murmurs occasionally heard in aortic aneurism. Lastly, the faintness of the sounds over the sac, as compared with their loudness and distinctness at the lower sternum, I presumed to afford proof that some thick mass of substance, of a kind calculated to intercept the vibrations of sound, lay between the current of blood in the sac and the surface. This condition was found to be fulfilled in the fibrine, which lay in layers over the upper sternum, to the thickness of more than an inch, and which, as I presume, acted as a muffler to the sounds diffused in that direction. Of the facts, at least, as above stated, there can be no doubt.

The sources of the collateral circulation in this case would have formed an interesting subject for anatomical research; but this inquiry could not, under the circumstances, be undertaken.

The opening of the small aneurism into the left auricle had produced, apparently, no distinctly appreciable clinical result.

The following case is the only one which I think it

necessary to give here at length from my later experience of thoracic aneurism, though the number might have been easily extended. It affords a useful point of comparison with Case II. as regards the diagnosis; and affords a striking example of another, and a not very uncommon, mode of death in aneurism.

CASE X.—*Aneurism of arch of aorta and innominate artery, compressing the trachea and right recurrent nerve. Laryngeal spasm, threatening suffocation, but permanently relieved by inhalation of steam. Remarks on diagnosis. Persistent aphonia and cough, with unclosed glottis; extremely purulent expectoration; signs of condensation of lungs, and suspicion of cavity. Occasionally diminished size of right pupil, and of right radial pulse. Great emaciation, occasional hæmoptysis, sweating, colliquative diarrhœa, but no marked return of dyspnœa. Death as from laryngeal phthisis.*

William J., a printer, æt. 40, and stated to have been ill five weeks, was admitted into the surgical hospital in the beginning of September 1860, with very threatening symptoms of laryngeal obstruction, marked by total loss of voice and by spasmodic attacks of dyspnœa, which for about a week before admission had, on various occasions, appeared likely to end in suffocation. The question of tracheotomy being very obviously suggested by the symptoms, Mr. Spence requested me to examine carefully the chest, with a view to regulate, in some degree, the surgical procedure. In consequence of this examination, and of the opinion given, that the laryngeal spasm depended on thoracic disease, it was

decided to refrain from operating, and to employ palliative measures, under which, fortunately, the urgency of the symptoms completely subsided in the course of a few days. He was then removed to ward No. 4, medical hospital, where he continued free from all the severer forms of laryngeal suffering till his death, more than three months afterwards, although the voice was completely lost, and the cough, with its peculiarity of tone and want of sharpness and articulation, clearly indicated an imperfectly closed glottis from first to last. The grounds on which the diagnosis proceeded, as to the existence of an intra-thoracic cause for the laryngeal symptoms, were as follows:—1. There was no ascertainable structural disease of the larynx itself, in so far as it could be examined by the finger passed down to the arytenoid cartilages and their connections. 2. There was a slight want of fulness of the respiratory murmur in the apex of the right lung, before and behind, indicative of pressure impeding the entrance of air. 3. Very deep in the jugular fossa, and a little to the right, there was a very obscure, but distinctly abnormal pulsation, accompanied by an undue sense of resistance suggestive of a solid tumour. I regarded it, accordingly, as very highly probable that the patient was affected with aneurism of the arch, involving the root of the innominate artery; but the obscurity of the pulsation, and the absence of all collateral signs of obstructed circulation, made it absolutely impossible to exclude from the diagnosis the chance of a tumour of some other kind, deriving pulsation from the aorta, and involving

the nervous system of the larynx in the same way as an aneurism might have done. There was absolutely no substernal dulness on percussion, and no murmur over any part of the arch; the heart's sounds were normal; the lungs generally were free from all signs of disease.

The necessity for active practice being apparently in abeyance after his admission to the medical wards, the patient was abandoned to the influence of rest, warm air, good food, expectorants, occasional opiates, and the inhalation of hot vapour, which proved very soothing, as usual in cases of laryngeal spasm. He was harassed a good deal by cough, at first nearly dry, but afterwards accompanied by a good deal of expectoration, which ultimately became intensely purulent. About the same time with this latter change, signs of condensation became distinctly developed at the base of both lungs, predominating, however, in the right, in which they were so marked, and accompanied by so remarkably tubular a breath-sound, as to lead to the suspicion of a cavity. In the apex the signs were less distinct, though bubbling râles were frequently present in all parts of the right lung. Two or three times I particularly remarked the large quantity of perfectly pure pus that appeared to be brought up by a single fit of coughing, and apparently almost in an instant; on one occasion between one and two ounces were thus discharged immediately after slight pressure in the jugular fossa with the finger. I had now serious doubts as to whether the tumour might not, after all, prove an abscess in the mediastinum, opening

into the lung ; but its physical characters remained unchanged, and its size rather increased than diminished. There never was any appreciable difficulty in swallowing ; but the aphonia and the imperfectly toned cough continued, characteristic of pressure on the laryngeal nerves. Two or three times a pretty well-marked comparative diminution of the right pupil, and a smallness of the right radial pulse, were observed ; but these phenomena were hardly so constant or so distinct as to be beyond the limits of physiological variation. At last the patient became extremely emaciated and cachectic, being utterly exhausted by long-continued irritation and purulent discharge ; his state closely resembled that of a person in the last stage of laryngeal phthisis, except in the greater degree of orthopnœa, and in the presence of something like angina pectoris, but unaccompanied by any irregular action of the heart, or any sign of obstructed circulation, or of deficient aeration of the blood. The resemblance to a case of phthisis was maintained to the last, and in almost every point ; there was occasional hæmoptysis, and much sweating ; at last a colliquative diarrhœa supervened, and under this accumulation of disorders he sank, and died on December 23.

Abstract of Dr. Haldane's Report of the Examination after Death (Register of Dissections, xx., No. 233).—The aorta at its origin was of the natural size ; but almost immediately above the coronary arteries the right side of the vessel bulged out into a marked dilatation, which affected the whole of the ascending portion of the arch. The inner margin of the vessel was, however, quite normal, the dilatation only affecting the right side.

An aneurism arose from the transverse portion of the arch, commencing a little before the origin of the innominata, and extending to the origin of the left carotid. This aneurism was of an oval form, two and a half inches long, and having a circumference fully equal to that of a turkey's egg. [It was recognized

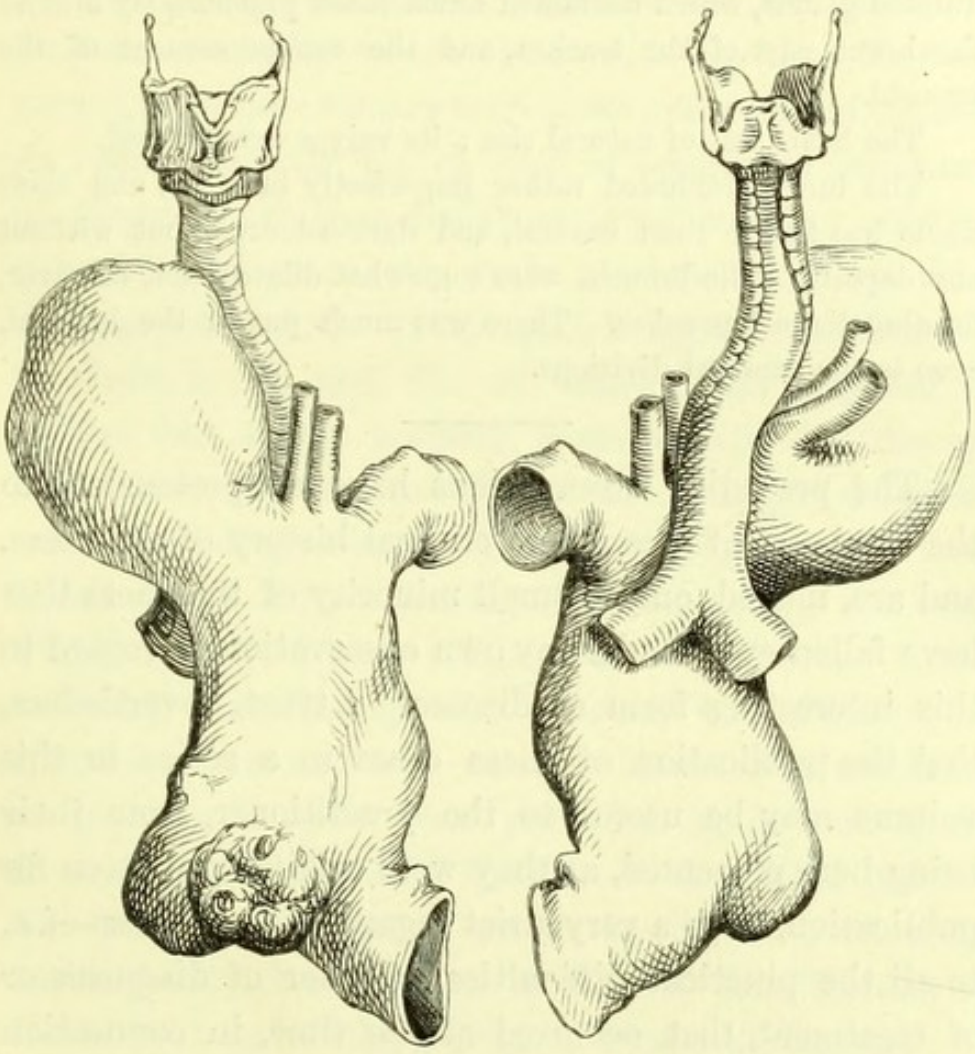

Fig. 15.

The front and back of the aneurism and adjoining parts in the case of William J. (as described in the text). The drawing is from a dried preparation in the possession of Dr. Haldane; and the mode of preparation tends to represent the tumours as more full and prominent than they were in nature.

during life as having very much increased in size during the period of observation of the case; and especially as having come much more distinctly than at first within the range of the finger.]

The innominata was very much shortened, so that the right subclavian and carotid appeared to arise separately from the back of the sac, about its middle. The inner surface of the aneurism was closely applied to the trachea, which, in consequence, was somewhat twisted and compressed. A mass of enlarged and indurated glands, which contained much black pigment, lay around the lower part of the trachea, and the commencement of the bronchi.

The heart was of natural size ; its valves were natural.

The lungs crepitated rather imperfectly in parts, and their tissue was firmer than natural, and dark-coloured, but without any deposit. The bronchi were somewhat dilated, not, however, forming distinct pouches. There was much pus in the bronchi, even to the smallest divisions.

The preceding observations have no pretensions to the character of a complete clinical history of aneurism, and are, indeed, only a small minority of the facts that have fallen even under my own observation in regard to this interesting form of disease. I trust, nevertheless, that the publication of these cases in a series in this volume may be useful to the practitioner, from their being here presented, as they were originally written for publication, with a very strict regard to usefulness—*i. e.*, to all the practical difficulties, whether of diagnosis or of treatment, that occurred at the time, in connection with each of them. I have, accordingly, been even more anxious to bring forward cases which occurred some years ago than those of later date, although not insensible to the fact that some of the difficulties and errors faithfully represented in these pages might have been avoided, had it been possible to arrive at the fruits

of an enlarged and more carefully studied experience in any other way than by passing through the ordeal suggested by these details. It will be evident, however, to the reader that most of the points of interest raised by these cases are of a kind in regard to which no sufficient guidance could have been obtained, at the time they occurred, from the ordinary text-books and works of authority; and hence the record, even of errors and omissions of diagnosis which actually occurred, was not only instructive and practically useful, but nothing less than an imperative duty. This remark applies particularly to Cases I., II., and VI., in which I am disposed to believe that a more accurate conception of the disease might have been arrived at, had the truly physiological method of diagnosis, to which these papers tend, been followed out with the scrupulous exactness, and with the attention to minute details, which it requires. I can truly say that through the constant watchfulness inspired by these and other more or less similar incidents of practice, I have been protected again and again from errors which would have been inevitable but for this previous experience; and it is, therefore, not without a profound conviction, at once of the difficulty of the subject, and of the power of a careful diagnosis to penetrate many of its obscurities, that I embrace the opportunity of summing up this article with a few very brief remarks on the general subject, in so far as illustrated by the cases adduced.

It must be evident, from the consideration of these cases, that physical diagnosis (commonly so called), al-

though it can hardly be said to take subordinate rank among the means of discovery of aneurismal tumours, is in many cases inadequate to their discovery, unless aided by the careful study of symptoms; while, on the other hand, the interference of even very small aneurisms with the nerves and vessels of the thorax and abdomen may determine physiological changes which can be at once appreciated by a careful physician from a diagnostic point of view, and the study of which is at the same time of the greatest importance as guiding the prognosis, and even the treatment. Not only the individual symptoms, therefore, but *the correlation of the symptoms* as indicating a possible tumour, should be carefully studied in all cases of obscure chronic disease of the internal cavities; the object being to determine not only the existence of the tumour, if present, but its exact relations to the surrounding parts, and the probabilities of its encroachments becoming dangerous to important structures. Thus, I have endeavoured to shew—

1*st*, That aneurism, when accompanied by well-marked angina pectoris, is probably situate in the ascending portion of the arch, and near the cardiac plexus of nerves. The natural course of such aneurisms is to burst into the pericardium; or to compress, perhaps open into, the auricles, or the pulmonary artery, causing in many cases cyanosis and sudden death.

2*d*, That internal aneurism, when attended by laryngeal symptoms, is likely to be so placed as to in-

volve the right or the left recurrent nerve, *i.e.*, either in the innominate artery, or on the posterior and inferior aspect of the arch; in either of which situations, but especially in the latter, an aneurism may cause death by laryngeal suffocation before it is large enough to be readily detected by physical diagnosis.

3*d*, That aneurism, characterized chiefly by bronchial asthma and orthopnœa, is probably situate in the commencement of the descending portion of the arch, or, at all events, so as to compress the pulmonary plexus of nerves; and that its consequences may be looked for in the obstruction of one or other bronchus, at first with the symptoms and physical signs of asthmatic bronchitis, and afterwards of pneumonia or pleurisy.

4*th*, That aneurism, producing permanent and well-marked contraction, or perhaps (in rare instances) dilatation of the pupil on one side, may be expected to arise from the upper and back part of the arch or its primary branches, the sac projecting backwards in the direction of the sympathetic trunk, or of its ganglia, and of their communications with the spinal system.

5*th*, That dysphagia indicates pressure either on the œsophagus, or on the pneumogastric nerve, and a corresponding situation of the tumour. To these principles I would add another, as applicable to the diagnosis of thoracic aneurisms, viz.—

6*th*, That all aneurisms coming within the range of physical diagnosis, and not attended by any of these symptoms, must necessarily arise either from the descending aorta, below the range of the pulmonary plexus,

or from the upper part of the arch, projecting upwards and forwards; as it is in these situations alone that a thoracic aneurism can attain sufficient bulk to be discoverable, without involving important internal structures, and leading to very marked functional disturbance.

In the case of abdominal aneurisms the modes of functional interference are both more complex and more obscure, insomuch that I can hardly hope to make the principles of their diagnosis apparent without a much more extended consideration of the subject than is possible in this article. The mere enumeration of possible symptoms, indeed, would be sufficiently easy; but the investigation of them in relation to practical difficulties is a task of great complexity, and I must in the meantime be content to refer to the single case here published, and to two others in the Edinburgh Medical and Surgical Journal for January 1855, in illustration of this subject.

The influence of the sympathetic nerve upon the pupil, besides having been the subject of the very careful physiological experiments adverted to above, has, since the date of publication of the case of J. W. (p. 529), (in August 1855) been investigated from the clinical point of view by Dr. Ogle, of St. George's Hospital, in a remarkably interesting and exhaustive memoir in the Medico-Chirurgical Transactions, vol. xli.* I communi-

* On the Influence of the Cervical Portions of the Sympathetic Nerve and Spinal Cord upon the Eye and its Appendages; illustrated by Clinical Cases. By John W. Ogle, M.D. 1858.

cated to Dr. Ogle a brief account of the whole of my further experience up to that time (1858); and as his paper is easily accessible, and is by far the most complete and satisfactory source of information upon the whole subject, I shall be enabled to dispense with any further account here of the more recent facts contributed by Dr. Williamson of Leith, Dr. Banks of Dublin, Dr. Seaton Reid of Belfast, and others, as confirmatory of my own observations with respect to this symptom of aneurism. It is due to Dr. MacDonnell of Montreal, however, to state that he had observed, in 1850, a case of malignant tumour producing pressure on the sympathetic, in connection with contracted pupil, ptosis, and epistaxis, all on the same side with the malignant growth. The remarks of Dr. MacDonnell shew clearly that he fully appreciated the importance of the observation, and is therefore entitled to the credit of being really the first to follow out the physiology of the subject to its legitimate pathological consequences.* It is true that Testa, in his strangely confused way, had noticed something about blindness, and something about the pupil, and something about the sympathetic nerve; but the connection is so obscure, that I must content myself with a mere general reference to the chapter.†

* This paper "On Contraction of the Pupil, a Symptom of Intra-Thoracic Tumours," appeared in the Montreal Medical Chronicle for June 1858. A copy was kindly sent me by the author, which I immediately transmitted to Dr. Ogle, who has referred to it in his paper, pp. 412, 432, *notes*. My own case, I need not say, was observed and discussed several times at the Medico-Chirurgical Society of Edinburgh, before I knew of any observations of a similar kind, except the very imperfect one noticed by Dr. John Reid.

† Malattie del Cuore, vol. ii., l. ii., c. 9.

Since the observations here referred to, I have seen so many cases of more or less considerable contraction of the pupil in connection with tumours at the root of the neck, that the observation may be said to be almost a habitual one. But although slight variations, and sometimes possibly transient dilatation, as well as contraction, are not uncommon, a permanent and marked form of the lesion, so extreme as to be quite beyond the limits of physiological divergence, is certainly among the more rare phenomena of aneurismal disease. The number I have seen of this marked kind, in connection with equally marked symptoms of aneurism, can hardly be said to exceed six or seven; although probably not less than two or three times that number have occurred to me in which more or less inequality of the pupils was observed in aneurismal cases. In one of these, in which the symptom was thus ill-defined and questionable, an aneurism was found, as was expected, at the root of the neck; but it did not directly involve either the nerve or its connections, so far as could be observed. Add to this, that in a minority of persons (perhaps one out of every six or eight) the pupils present a very slight degree of physiological difference of size; and the necessity for caution in the interpretation of the symptom will be at once apparent. In order to shew the characters of a well-marked case, however, I have had an accurate drawing made of a patient affected with evident physical signs of aneurism of the innominata, or arch of the aorta, who presented herself at the hospital within the last two months, for an attack of subacute bronchitis, from which she re-

covered under treatment. The right pupil is extremely contracted, and though not quite immobile, dilates very little indeed even in the most dim light. The left pupil, on the contrary, is permanently somewhat dilated, or at

Fig. 16.
A well-marked case of inequality of the pupils in aneurism, as described in the text.

least decidedly large ; and it is probable that this, too, is an abnormal fact, due to an old injury of the left eye, which has produced a tremulous state of the iris, and a slight degree of amaurosis. The result is a very remarkable contrast.

The curious symptom of strictly unilateral sweating, stopping short quite abruptly at the middle line, and

occurring (in one case almost constantly) over the face and scalp of the affected side, has been observed by me in two cases only, in each of which it concurred with a contracted pupil. One of these cases ended fatally in 1858, after a protracted illness, extending over more than two years, during which the whole of the phenomena were quite constant. I reported this case to Dr. Ogle, in December 1856, and also shewed the patient to the members of the Medico-Chirurgical Society of Edinburgh. On the death of this patient, however, it was found quite impossible to obtain the consent of his friends to an examination of the body.

In one case, I was enabled to form a well-founded suspicion of aneurism, from a very marked contraction of *both* pupils, concurring with certain other symptoms. I was summoned one evening about three years ago to the bedside of a respectable tradesman, who was rightly supposed by some of his friends to be in considerable danger. It happened, very curiously, that the friend who accompanied me was the brother of Mr. J. B—— whose case I have already narrated (case VII. p. 509); and I had therefore no difficulty in making him understand what I suspected to be the truth, although the examination was necessarily an extremely hurried and unsatisfactory one, owing to the absence of the ordinary medical attendant, who was ill in bed at the time, but with whom a consultation was appointed for the next day. The patient had been inhaling quite enormous quantities of chloroform to relieve his suffer-

ings, and could hardly be persuaded to give it up even for a moment. I found the pulses extremely feeble in all the arteries, great pains in the back and shoulders, extending down the arms, and a very contracted state of both pupils, as though from opium poisoning (there being given me the most absolute assurance that he had taken no opium). The patient died within a few hours after I saw him of hæmoptysis, and after death there was found an enormously elongated aneurism applied closely to the vertebrae almost throughout the whole dorsal region of the spine, which had destroyed the bones to a very great extent and had led to great obstruction of the systemic circulation.*

Several cases of aneurismal disease are mentioned in the remaining articles, for which see index of cases.

* I am tempted to mention further, though the observation can hardly be brought into connection with any of the others in this series, a case of what I presume to have been aneurism of the descending aorta, which I saw along with Professor Miller. The patient was a farmer from Dumfriesshire, originally of robust frame, but very much emaciated from obstruction of the œsophagus. He had for many weeks been unable to swallow more than the most minute particles of solid food, and was plainly dying gradually by starvation, which was the more painful to himself and to his friends, as his appetite was strong, and his spirits were good. None of the ordinary sizes of œsophagus-bougie could be passed; but with great care an instrument, specially made for the purpose, and about the size of a No. 5 or 6 urethral bougie, was guided past the obstruction on a single occasion. The question referred to me was the evidence of complication, either cancerous or aneurismal; and after an extremely minute examination into all the symptoms and physical signs, it appeared to me scarcely possible to avoid the conclusion, that there was a tumour in front of the spine, very slightly, but still evidently, encroaching on the left lung. In consequence mainly of this opinion, it was deemed expedient to desist from further surgical procedure; and shortly afterwards I learned that the patient had perished suddenly from hæmoptysis.

XVIII.

CARDIAC MURMURS.*

In estimating the importance of a cardiac murmur, we must take care not to be misled by its mere *acoustic quality*, so to speak. I need hardly tell you that it is not the loudest or the roughest murmurs in all cases that are really the most significant, or the most fraught with danger to life. Often, indeed, quite the opposite of this is true; it not unfrequently happens that as the prognosis of a case in which a well-defined murmur has existed gets worse, the murmur itself becomes less and less, or even vanishes altogether. And the *same* murmur may at different times appear to be blowing, sawing, grating, rubbing, or even musical in character, while its real value to the auscultator, as respects diagnosis and prognosis, may remain unchanged through all these changes of quality. On the other hand, the quality of the murmur may remain the same; it may be constantly very rough and loud, and the prognosis may be good, or constantly very soft and low, and the prognosis bad. In

* A portion of a lecture on "Modern Cardiac Pathology and Diagnosis," delivered at the request of the Council of the Royal College of Physicians of Edinburgh, in July 1861, in the hall of the College.

short, *the mere fact that a murmur exists, and has a certain acoustic quality*, tells very little indeed as regards the character of the case.

For, in the *first* place, the existence of a murmur does not tell that there is organic disease, or even any disease at all.

In the *second* place, it does not tell, in many cases, whether the disease, supposing organic disease to exist, is external or internal to the heart.

In the *third* place, the mere existence of a murmur, apart from a careful and detailed scrutiny of the collateral facts, does not tell if the disease is old or recent, increasing or diminishing in intensity, requiring or not requiring treatment, likely or not likely to affect greatly the duration of life.

Generally speaking, I would say that the tendency of half-instructed auscultators is to over-estimate the importance of the murmur *as a fact*, and to under-estimate its importance *as a means of investigation*,—to pay too much attention to the mere existence of the sound, and too little to the circumstances in which it occurs. And from this springs another tendency, which is, to take too grave and sombre a view of cardiac murmurs generally, and especially of such as are loud and obtrusive. I could give you numerous instances of this tendency, but the following may probably be sufficient.

I was made acquainted some years ago with the case of a medical practitioner of great eminence, and of special reputation as an auscultator, who, at a period at least a quarter of a century before his death, and when

he was quite a young man, discovered a cardiac murmur in himself, referrible to the aortic valves. He at first made himself very anxious and unhappy about it, and used flexible stethoscopes to follow its variations; but finding the murmur persist, while he felt himself none the worse, he gave up thinking about the matter, and in the end died of a rare form of tubercular disease, without any distinct symptoms connected with the heart. There was found in his body a lesion of the aortic valves somewhat like this (shewing preparation): two of the valves were united by their edges over about a third of an inch in length; the current of blood was therefore obstructed, or at least broken, and the obstruction produced murmur; but it was not sufficiently obstructed, as the result shewed, to interfere with the circulation, or to precipitate a fatal result. Nay, it did not interfere even with the size or form of the heart, which was in every other respect normal, or even rather small.*

I know another case at this moment in which a regurgitant mitral murmur has persisted in a medical brother for nearly half a lifetime, without seriously interfering with the functions, or shewing any obvious tendency for many years past to shorten life. [This gentleman recognized his own case in the brief description given

* The following record was made by me at the time:—"*May* 4, 1853. Heart not enlarged, or deformed; aortic valves competent, but two segments adherent, *as if glued together*, without other deformity, so as to make a decided obstruction of orifice. Mitral valve and all other parts of heart normal." The record generally, though very interesting, has no relation to the present subject.

above, and in calling upon me some time afterwards to be re-examined, informed me that the murmur had arisen during an attack of rheumatism when he was a very young man, and that at this time he was bled largely on account of dyspnœa, probably caused by œdema of the lung. He now lives temperately and carefully, but is able for a great deal of duty and enjoyment of life; and it is very satisfactory to observe, as I did particularly on this last occasion, that, notwithstanding a murmur of so many years' standing, there is not the slightest indication of permanent functional disturbance or hypertrophy of the heart.]

Again, a young and active man once applied to me on account of a cardiac murmur which had been detected by a medical friend some time before, and had caused him considerable uneasiness. I examined, and found a quite decided aortic murmur with the first sound; and from its permanence and constancy of character, after several examinations, I was convinced that it was valvular. But, keeping in view the insignificant character of the symptoms, and the absence of evidence of hypertrophy, I thought myself justified in giving merely a cautious, but not a very discouraging, opinion. After losing sight of him for some months, I met my patient accidentally on the banks of Loch Tummel, in Perthshire. He was in the midst of a walking tour in the Highlands, and had been pacing it over the hill from Blair-in-Athole, along with a stalwart and kilted companion, whom I knew well as a man in the full vigour of an extremely robust frame and excellent

constitution. He told me that he had suffered nothing, and was, in fact, quite well.* To this case, too, I know almost a parallel one in another young man, who has had a very well marked aortic murmur for several years, but is able to perform without uneasiness all the ordinary duties of an active life. [He is now in India, and in apparently excellent health, to judge from the last

* In regard to the case here alluded to, although my own opinion in regard to it is quite decidedly as expressed in the text, I am aware that it may reasonably be suspected by some that the murmur heard was merely functional. In order not to run any risk of misleading the reader as to the facts, I transcribe them, with all the qualifying circumstances, as noted in my case-book:—

"*May* 1855. Mr. P. came with —— (another patient). I took the opportunity of examining his chest for an aortic systolic murmur which I detected eighteen months ago. I found it *in statu quo*, short, but quite distinct, and prolonged into the vessels. He complains of the heart, but I think it is from his attention being directed to it. There is nothing unnatural in the action of the organ; size natural. Mr. P. had a smart attack of acute catarrh in winter, which went off readily under treatment."

The following remarks on the case, in addition, bear date October 27, 1857:—"This case of Mr. P. was a very remarkable one. There is no doubt whatever that the murmur had every character of the aortic obstruction. *Permanent, well-marked;* heard over base and prolonged into vessels; attended with symptoms which may easily be construed into organic disease. On the other hand, it is to be observed, that the symptoms were *subsequent* to the discovery of the murmur by ——; that Mr. P. passed through several attacks of catarrh and incidental illness without the development of the disease of the heart; that in the autumn of 1855, I met him at the Bridge of Tummel inn, with Mr. ——, with whom he had been performing all sorts of pedestrian feats without distress. Finally, Mr. P. was disposed to be lazy, and was occasionally hypochondriacal, which may have accounted for most of the symptoms. Compare, however, case of Dr. ——."

accounts of him received by his friends, of which I heard some particulars not many weeks ago.]

Once more: The young son of a merchant in this city was brought to me by his father on account of a very singular and loud murmur which had been observed almost over the pulmonary artery, and with the first sound. There had also been certain symptoms, but not at all aggravated symptoms, of cardiac disturbance. I do not profess, even now, to be quite sure as to what is the character of this murmur; but I certainly was far from entertaining so favourable an opinion of the case as in the others mentioned before. For, not to dwell on the fact that there was a certain amount of cardiac uneasiness, it is consistent with my experience that valvular diseases formed during the period of growth of the body are commonly much sooner fatal than those established for the first time at a later period of life. I mention this case to you, not as a parallel one to the others, but simply to note as a remarkable fact that this boy has grown up to manhood not only without increase, but with a positive diminution of the symptoms; while the murmur remains, or remained when I last examined him, at least as loud as ever. [I met this young man the other day, along with his father, and I found upon inquiry that his health continues as above described. I did not, however, examine the murmur.]

I will give you only one other instance of life preserved, and a certain degree of bodily activity maintained, long after the occurrence of a cardiac murmur, and one of a kind usually more formidable than any of the fore-

going. There is a man in this city, whom I meet frequently on the street, and whom I know to have been for *at least* the last five or six years affected with aortic obstruction and regurgitation, indicated by a double murmur over the base of the heart, and in the great vessels at the root of the neck. This man discovered the disease for himself, as, indeed, there was no difficulty in doing; for the murmur with the second sound was at one time of highly musical quality, and could be heard at almost any distance. He is a highly irritable and nervous person, dyspeptic, thin, and disposed to be fidgety about himself; but so far from having got worse since I knew him, I am confident he has been much better; and he now works as a light porter in an office, with apparently quite as good a tenure of life as he has had at any period since the murmur began. [He has apparently risen in life since this lecture was delivered, and looks altogether more comfortable, portly, and well-to-do than formerly. He avoids speaking to me, and I do not choose to inquire after him; but there can be no reasonable doubt that the preceding statements are more than justified by his appearance, and yet, from the length of time the murmur was under observation before, it is nearly certain that the valvular disease can have undergone no important change.]

Of course I do not adduce these facts to infer from them that the prognosis of a cardiac murmur is not usually grave. I only wish to shew by instances that there are good grounds for qualifying the gravity of the prognosis in certain cases, and that hopes may be

held out, where the symptoms are not such as to forbid them, of considerable comfort and extension of life even in persons affected with incurable organic lesions of the valves. And hence it follows that the determination of the mere fact of a cardiac murmur, and even of its character, to the ear, leads a very small way indeed in the thorough appreciation of a case of cardiac disease. It is necessary to follow up the inquiry by a great number of considerations of detail, some of which, but only a very few, I shall try to overtake at present.

The first question in diagnosis that follows on ascertaining the mere fact of a murmur is—What are its character and seat? in other words—How is it pathologically related to the structures which produce it? Generally speaking, I would say that of every murmur, unless its acoustic character and history are very well defined, you must assume, in the first instance, that it *may* be exocardial or endocardial, old or recent, from disease, malformation, or mere functional disturbance; it may be caused by the smallest fragment of obstruction to the passage of the blood, or by the most minute leakage of a valve, or by the slightest possible bit of old rough deposit on the surface of the heart; or it may, on the other hand, be the result of a ruinous injury of structure, which will certainly bring the circulation into an unmanageable condition in a very short time. You have to work out these questions as well as you can from the evidence, taking care not to lose sight of any part of it; and

the first part (only the first and smallest, but still an *essential* part of the inquiry) is—What is the pathological character of the murmur?

To determine this it is necessary to observe particularly two points, which form the finger-posts (as it were) of the whole inquiry, and in many cases serve to direct it to a safe conclusion. We have to study carefully—1*st*, The Rhythm; 2*d*, The place of the Murmur. And as the great majority of murmurs are valvular in origin, it is on the whole a good rule to try all murmurs which require detailed investigation by the tests of the valvular murmurs; to exhaust the hypothesis of a valvular origin before proceeding to any other.

I. The first point we have to look to, then, is *Rhythm*. What has to be ascertained under this head, is the relation of a murmur to the different physiological acts which constitute a complete cardiac pulsation, viz., the contraction, dilatation, and rest of each of the cavities. You have to define the murmur as occurring during this or that portion of the heart's action, or during the pause which intervenes between two periods of activity. To do this, you have to watch carefully its relation to the normal sounds, to the impulse, and to all the other externally appreciable phenomena which attend upon the action of the heart.

Now, these phenomena are partly audible, and partly conveyed by the sense of touch. We may set aside, in the meantime, the visible phenomena as of minor importance. I have found it desirable in teaching to use a diagram

(Fig. 17, p. 570), by which you will be able to see at a glance the whole audible and tangible phenomena of the heart's action, and their relation to the physiological movements which cause them. If I venture to dwell on such elementary facts for a little, it is because I have often observed that great confusion results, not so much from ignorance of them, as from the want of such an accurate and instantaneous appreciation of them as is required for purposes of diagnosis. It is necessary not only to know the rhythm of the heart as a matter of theory, but to have such a vivid conception of it as calls up immediately, in connection with any single phenomenon, the whole of the others with which it is in relation. And it is to communicate such useful and practical conceptions that I have been in the habit of employing the diagrams that you see here.

In Fig. 17, the physiological action of the heart, apart from its external manifestations, is indicated by the inner circle and its divisions; the external rim is occupied by marks corresponding to the sounds; and the different pulses or impulses are portrayed by lines projecting from the circumference of the outer circle. You observe the succession of actions, or physiological facts, which constitute a cardiac pulsation, beginning with the contraction of the auricles, then that of the ventricles, then the rapid dilatation of the ventricles, and then the pause, succeeded by the contraction of the auricles again. You observe also, that in this succession of actions, the phenomena which we can appreciate externally are a little later than the real commencement of the heart's action;

they do not correspond to the very first beginning of move-

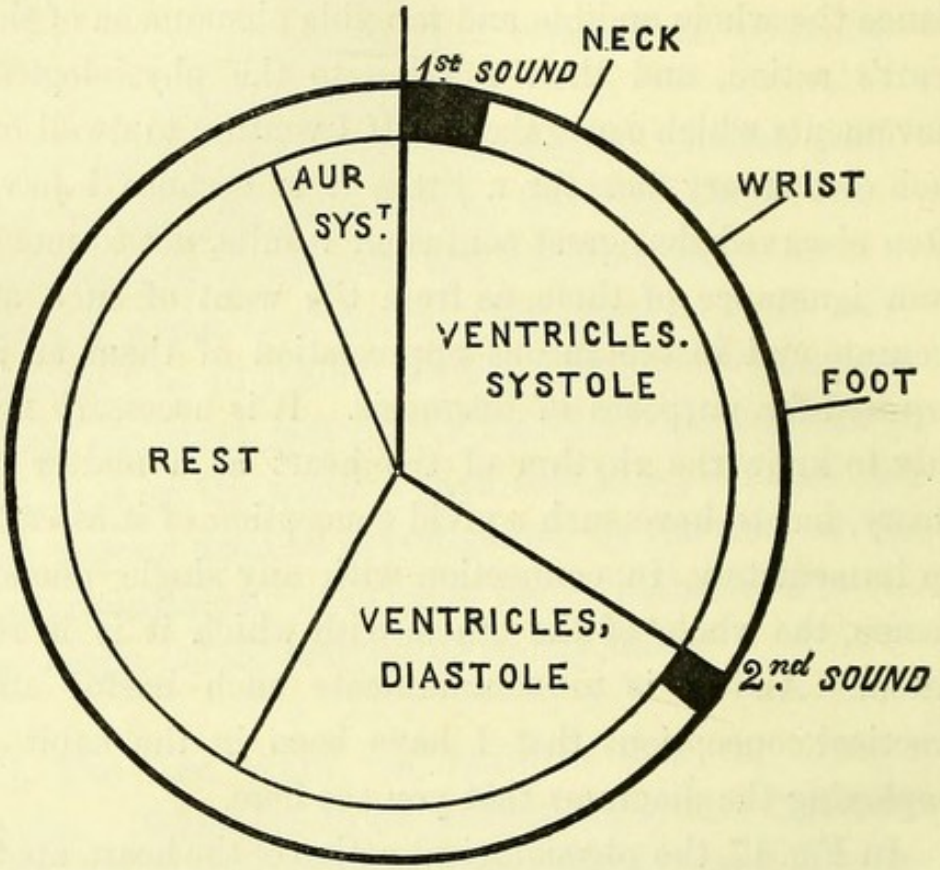

Fig. 17.*

ment, for, *before there is either sound or impulse, the con-*

* The notation of the rhythm, in so far as regards the sounds and the pulses, must be taken in this figure as only a very rude approximation to the truth in particular cases; 1st, because there are considerable varieties in nature; and 2d, because few attempts have been made (and those few not very successful in my opinion) to appreciate either the exact rhythm in particular cases, or the extent of the normal varieties. Dr. Brinton has been kind enough to point out to me that, according to Valentin's experiments, which are undoubtedly of the highest authority, the pulse in the neck ought to have been noted at least 45° from the beginning of the first sound. Dr. Brinton also suggests that the distance between wrist pulse and foot pulse in the diagram is too great. The relative lengths of the various cardiac movements and of the pulse also vary so much that it is impossible to give effect to their differences in a diagram. See on this point Fig. 18, and its explanation in the text.

traction of the auricles has already taken place. This is exceedingly important in relation to certain forms of murmur which precede the first sound of the heart and the apex-beat by a minute but distinctly appreciable interval of time; for such murmurs clearly correspond to one period of the heart's action only, viz., the contraction of the auricles; and whatever their pathological origin or seat may be, they have to be explained in accordance with this fact. So, too, the murmurs which immediately succeed the first sound and the impulse, whatever their origin or seat, correspond to the period of the ventricular contraction; and those which succeed the second sound, to the ventricular dilatation. Of these facts I shall make use presently in the further definition of murmurs.

Let me, however, in the first place, advert to certain difficulties that are apt to occur in estimating the normal rhythm of the heart, and therefore of its murmurs in disease. The entire period of the heart's action may be divided into a period of motion and one of rest; the former being again subdivided into the three distinct stages or periods indicated in the diagram (fig. 17). Now, it is important to observe, that when the heart's pulsations follow one another with great rapidity, the period of rest is reduced to a minimum; and when, on the contrary, the heart's action is slow, the period of rest is much lengthened in proportion to the period of movement. The consequence of this is, that the normal sounds, which occur during the contraction and dilatation of the ventricles, change their relation to one another according as the pulsations are in rapid succes-

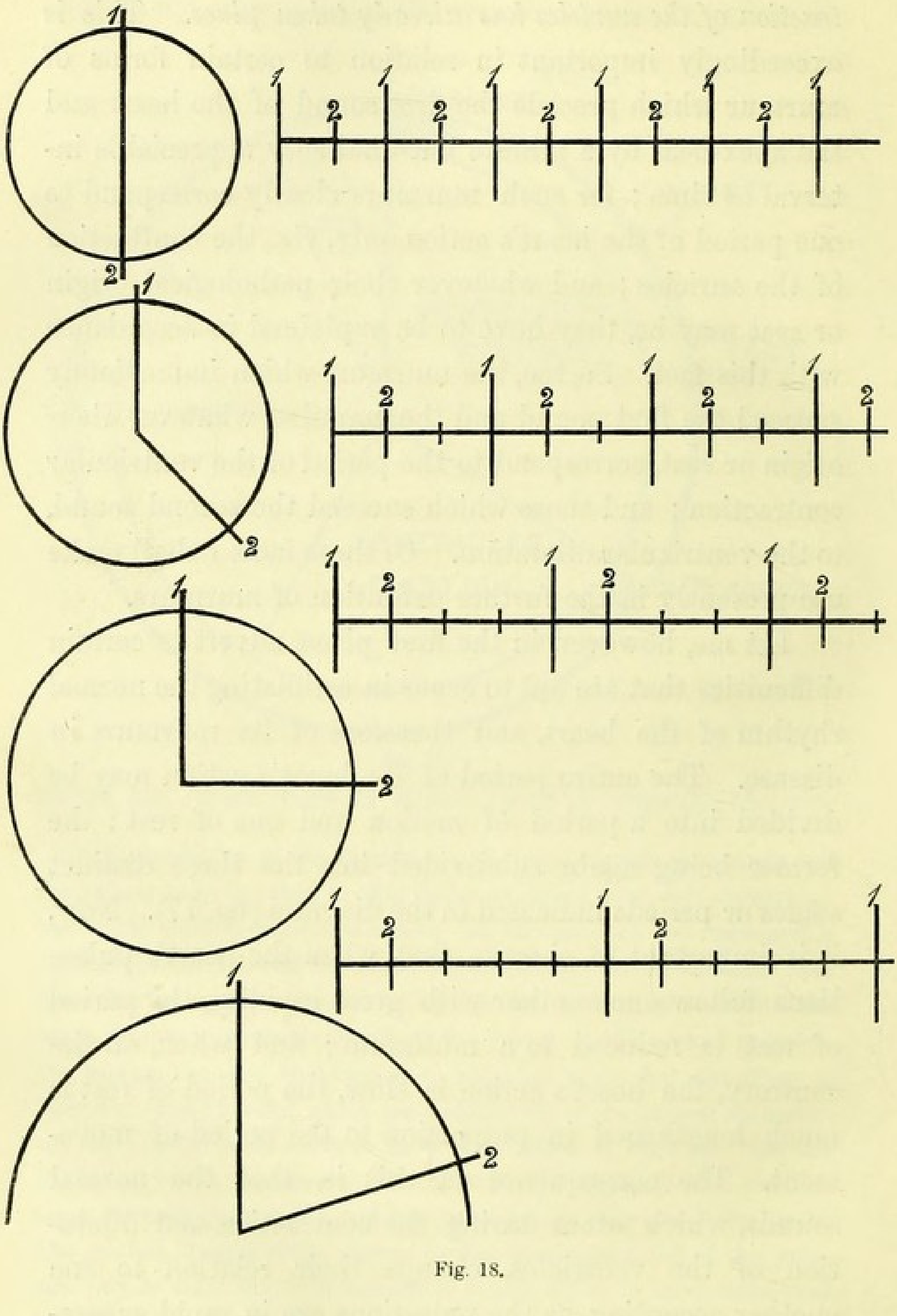

Fig. 18.

sion, or the contrary. In the former case, the interval between the second and the first sound (which includes the period of rest and of the contraction of the auricles) is very short; in the latter, it is very long. Hence the altered relation which is indicated to the eye in fig. 18, and which is often very embarrassing to the beginner. The larger circumference of each successive circle indicates the lengthening of the pause; and, accordingly, you have the interval between the *first* and *second* sounds occupying a less and less arc of the circle, as the heart's action gets slower, while the interval between the *second* and *first* sounds is correspondingly lengthened. In the first and smallest circle (indicating the most rapid action) the two intervals are nearly alike, and each occupies about one-half the circumference; in the last or largest circle (indicating very slow action) the interval between the *second* and *first* sounds is four times as long as that between the *first* and *second*. Hence it is that when the heart is acting rapidly, it is difficult to distinguish the *first sound* from the *second*, and *vice versa;* while with the slowly-acting heart this difficulty does not occur. Attention to these varieties (physiological varieties they may be called) in the rhythm of the sounds is of very great importance in determining the attributes of a cardiac murmur—for the first step in the inquiry is to determine which is the second sound and which is the first; and this, as I have said, is sometimes not quite an easy matter. Generally speaking (and in all cases when the action is slow and regular), there is no difficulty; you have only to remember that the

longer interval is between the *second* and *first* sounds, and the shorter interval between the *first* and *second;* but when the action is rapid, or irregular, and when the first sound is indistinct at the apex, or cannot be identified with the apex-beat, and also when the second sound is indistinct, or when it is audible only at the base, the first sound being audible only at the apex (as sometimes happens), the difficulty of recognition of the two sounds is very considerable. It is in such circumstances that I have found Dr. Scott Alison's double or bin-aural stethoscope useful in cardiac diagnosis, for it enables you to identify the sounds at the point at which you can hear them best, and to bring them into accurate relation with each other, by means of the two ears receiving at the same time the sounds from two points of the precordial region. I beg to commend this instrument to your best attention.

Now, let us suppose that you have identified the two sounds, and traced their relation to the impulse, as shewn in the diagram (fig. 17), you will then find no difficulty in the next step, which is to determine the rhythm of the murmur. All valvular murmurs which are not of complex origin have one or other of these three relations to the sounds and impulse of the heart:—

1. *The murmur precedes and runs up to the first sound*, ending at the moment of this sound and of the apex-beat. In this case the murmur is simultaneous with the contraction of the auricles (see fig. 17), and I call it *Auricular-Systolic* (fig. 19). The interpretation of such a murmur depends on the consideration that it

occurs *only when blood is being expelled from an auricle*, and when the ventricle is passive. With very rare ex-

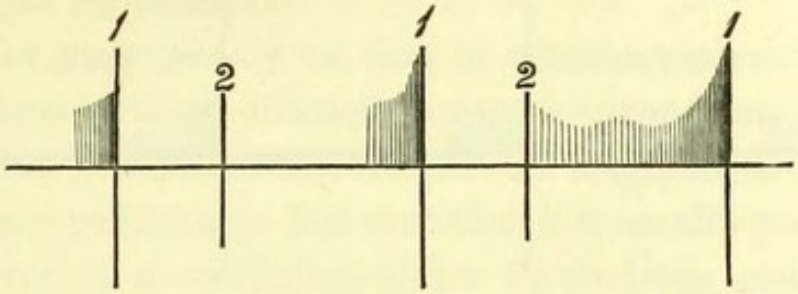

Fig. 19.

Auricular-systolic murmur, preceding the first sound of the heart, and the apex-beat.

ceptions, therefore, such murmurs depend upon contraction of the auriculo-ventricular orifices, and consequent interruption to the flow of blood out of the auricle during its contraction.

The auricular-systolic murmur may merely precede the first sound — *i.e.*, it may follow the pause of the heart's action ; or it may appear to be prolonged out of, or even quite through, the period of rest, being in this last case necessarily associated with a degree of the ventricular-diastolic murmur, presently to be described. Its essential character, however, is preserved in every case, as above defined, and as represented in the diagram.

2. *The murmur follows and runs off from the first sound*, ending somewhere between the first sound and the second, or close to the second sound. In this case the murmur is simultaneous with the contraction of the ventricles (see Fig. 17), and may be called *Ventricular-Systolic* (fig. 20). A ventricular-systolic murmur, being coincident with the emptying of the ventricles, must of

course be caused (if valvular in origin) by blood flowing outwards from the ventricle, either in the natural on-

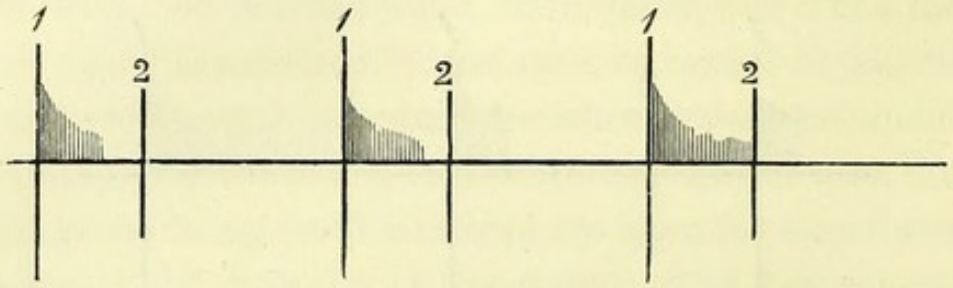

Fig. 20.
Ventricular-systolic murmur, following the first sound of the heart, and the apex-beat.

ward direction, or backward, by regurgitation, through the auriculo-ventricular orifices.

3. *The murmur follows and runs off from the second sound,* ending somewhere during the interval between the second and first sounds (in some instances, however, prolonged through the period of rest). In this case the murmur is simultaneous with the dilatation of the ventricles (see fig. 17), and may be called ventricular-diastolic (fig. 21). A ventricular-diastolic murmur is coincident

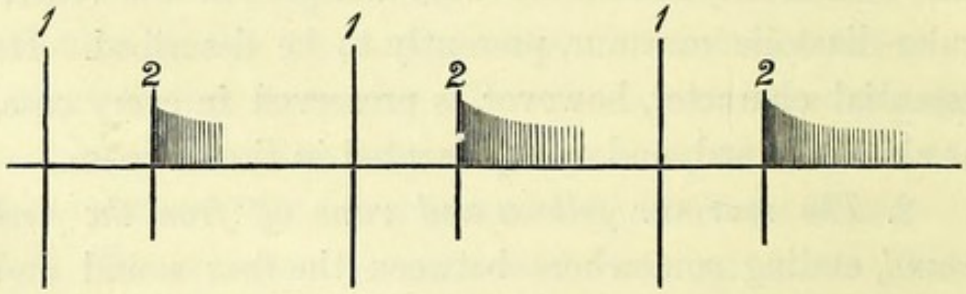

Fig. 21.
Ventricular-diastolic murmur, following the second sound of the heart.

with the filling of the ventricles by their rapid expansion movement. It is therefore always due (if of valvular origin) to blood entering a ventricle, either from the

auricle or from the artery; and in this last case of course the semilunar valves must be deficient, so as to admit of regurgitation.

Not unfrequently we find in practice various combinations of these different murmurs in the same case; and this, I need not say, renders the diagnosis so much the more perplexing. But even then it is usually possible to arrive at a conclusion, either by studying carefully the whole murmur in its relations to the rhythm of the heart, or by finding that it undergoes certain changes in tone or quality, which can only be explained on the supposition of its being complex. For instance, it is not unusual to have an auricular-systolic and a ventricular-systolic murmur combined, as in fig. 22; and they may even appear to be so combined as to constitute but one murmur; you will commonly, however, be able to detect the first sound in the middle of this murmur, splitting it, as it were, into two; and you then know absolutely (see fig. 17) that the part preceding and running up to the first sound must be auricular-systolic, and the part succeeding the first sound must be ventricular-systolic. In like manner a ventricular-systolic and a ventricular-diastolic murmur are very frequently combined in one case (fig. 23, over the leaf) but here the second sound intervenes, and makes the rhythm quite plain. The greatest degree of difficulty

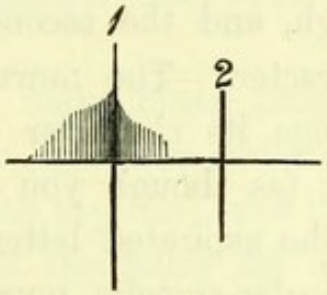

Fig. 22.

Auricular-systolic and *Ventricular-systolic* murmurs in combination; the first sound of the heart dividing and distinguishing the one from the other.

is when the normal sound is merged in the murmur, as it often is when an auricular-systolic and a ventricular-

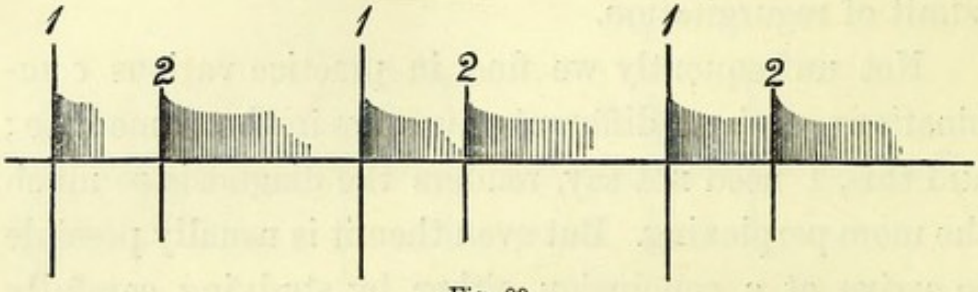

Fig. 23.

Ventricular-systolic and *Ventricular-diastolic* murmurs, in combination, the second sound of the heart being interposed.

systolic are combined ; but even in this case you will often find that the first part of the murmur is very rough, and the second part much more of a bellows character. The murmur, in fact, will often abruptly change its character about the moment of the apex-beat (as though you were to imitate it by a change of the aspirated letter, thus, r-r-r-r-r-f-f-f-f, where the auricular-systolic murmur, imitated and characterized by the harsh rattle of the r-r-r, even increases in harshness up to the first sound, and then melts away suddenly into f-f-f) ; or you may find that one element of the complex murmur is heard more purely at the apex, and another at the base, or over the right ventricle ; and in this way you are led to distinguish the one from the other. As a matter of fact and observation, I can tell you that I have been able to distinguish accurately three, and to suspect even four, sources of murmur in a case in which no period of the heart's action, or even of the pause, was free from abnormal sound ; and in which, in fact, all the valves of the

heart were more or less diseased. You will obtain some idea of the complication I mean, by consulting the diagram in fig. 24.

Let me add, before leaving this part of the subject,

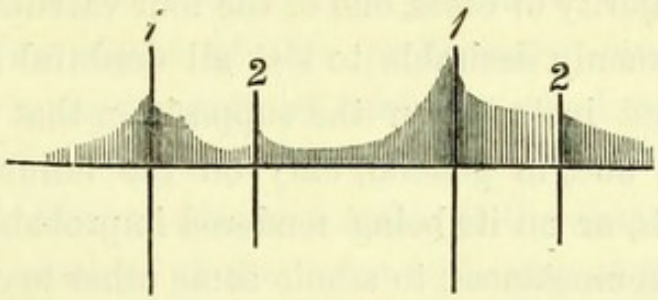

Fig. 24.

Prolonged murmurs accompanying all the three periods of the heart's movement, as described above, and also extending through the period of rest.

that I believe it is impossible to make much progress in the exact diagnosis of cardiac murmurs, without a system of notation founded on their rhythm ; and in all cases of interest, I have been in the habit for some years of using diagrams, similar to those before you, at the bedside. They are exceedingly simple and effective ; they save a great deal of tedious description ; and they bring the facts at once before the mind in a way that no description can do, at least in the case of those who have any difficulty in understanding cardiac diagnosis. They are therefore very favourable to accuracy in the recording of facts.*

* A convenient mode of indicating the character of cardiac murmurs is by imitative combinations of consonants, chiefly labials and linguals. But this, though used to a considerable extent, and habitually employed in the lecture-room, I have found to be rather cumbrous and subject to misapprehension on paper.

II. Having determined the rhythm of a murmur, the next step in the investigation is to fix, within as narrow limits as possible, the *place of its origin.*

Now, the point at which a murmur is produced being, in the majority of cases, one of the four valvular orifices, it is commonly desirable to test all doubtful murmurs, in the first instance, on the supposition that they are valvular; and, in general, only on the failure of this hypothesis, or on its being rendered improbable by collateral circumstances, to admit some other to consideration. The first branch of the inquiry as to the seat of origin of a murmur is therefore commonly this: *At which of the four valvular orifices is it produced?*

To this question a very satisfactory answer can commonly be obtained, if the murmur is not too complex, and if there be no remarkable deformity or displacement of the heart or great vessels to mask its true character. To avoid this last source of fallacy every means should be taken at the outset to determine, in the particular case under examination, the actual size and position of the heart, together with its relations to the thoracic wall, and to the surrounding organs.* In particular, percussion should be accurately performed, so as to define, with as much exactness as possible, the limits of that portion of the heart which is in contact with the thoracic wall, and uncovered by lung. In making this observation, the presence of any abnormal state of the lungs or great vessels, affecting the percussion, should be observed; and, in particular, the presence or absence of

* See the cases in the latter part of this article.

substernal tumour. The exact point of the apex-beat is next to be determined, if possible, and the characters of the impulse, both of the right and left ventricle, should be at the same time carefully studied. Lastly, the sounds are to be used as an aid to the other phenomena. The object of this preliminary investigation (and it is a most important one) is to avoid mistakes in the rest of the inquiry, for it is plainly very necessary to have as clear an idea as possible of the exact position, size, and relations of the heart with respect to the surrounding parts, before proceeding to speculate on the relation of the murmurs to the superficial areas over which they are heard.

Having proceeded thus far, we next endeavour to determine, by careful stethoscopic observations, the exact seat, and the limits of diffusion of the murmur actually under observation. If the murmur is very loud, or very generally heard over the cardiac region, there may be some difficulty;* and still more, if there are several murmurs interfering with, or crossing each other in the field of observation;† but, in ordinary circumstances, the stethoscope will enable the observer to fix on a few points, or a few restricted spaces, over which each murmur is audible with special distinctness; areas within which murmur is heard, there being no murmur elsewhere; or, if not so, then areas within which murmur is heard decidedly more loud, more articulate, and apparently nearer to the ear, than elsewhere. Modern diagnosis localizes the murmur (and therefore the disease)

* Case of Harriet M'D., Appendix. † Case of Wm. K., p. 605.

chiefly from the observation of these areas of transmitted sound.

Now, as there are four valvular orifices, so there are four distinctive areas to which murmurs arising at these orifices may be propagated. This is a fact thoroughly established by modern inquiry, although the exact definition of these areas is a matter of some difficulty, owing to the great difference in the diffusion of murmurs in different cases, and the numerous exceptional circumstances interfering with the diffusion in many instances. I have been accustomed to use a diagram (fig. 25, p. 583), as indicating these areas in a general way; and when due allowance is made for causes of variation, I think it will be found more in accordance with the facts than most of the descriptions. The following rules will also be found useful in recognizing these areas in actual practice :—

1. *Area of the Mitral Murmur.*—The mitral murmur corresponds generally with the apex of the left ventricle, to which point this murmur is readily conducted, as being the only part of the left ventricle which is in close contact with the wall of the thorax. To find this area with precision, it is requisite to determine the exact seat of the apex-beat; the patient lying a little towards the left side, or even on the face. If there is no distinct apex-beat, find the most remote point, downwards and leftwards, at which the impulse of the heart is discernible; test this point by percussion, to observe if it corresponds with the margin of the cardiac dulness; test it also by auscultation, to hear if the first sound is con-

veyed thither with special distinctness. If a murmur concurs in position with the seat of these different phenomena, and if its seat of diffusion is *round this point*

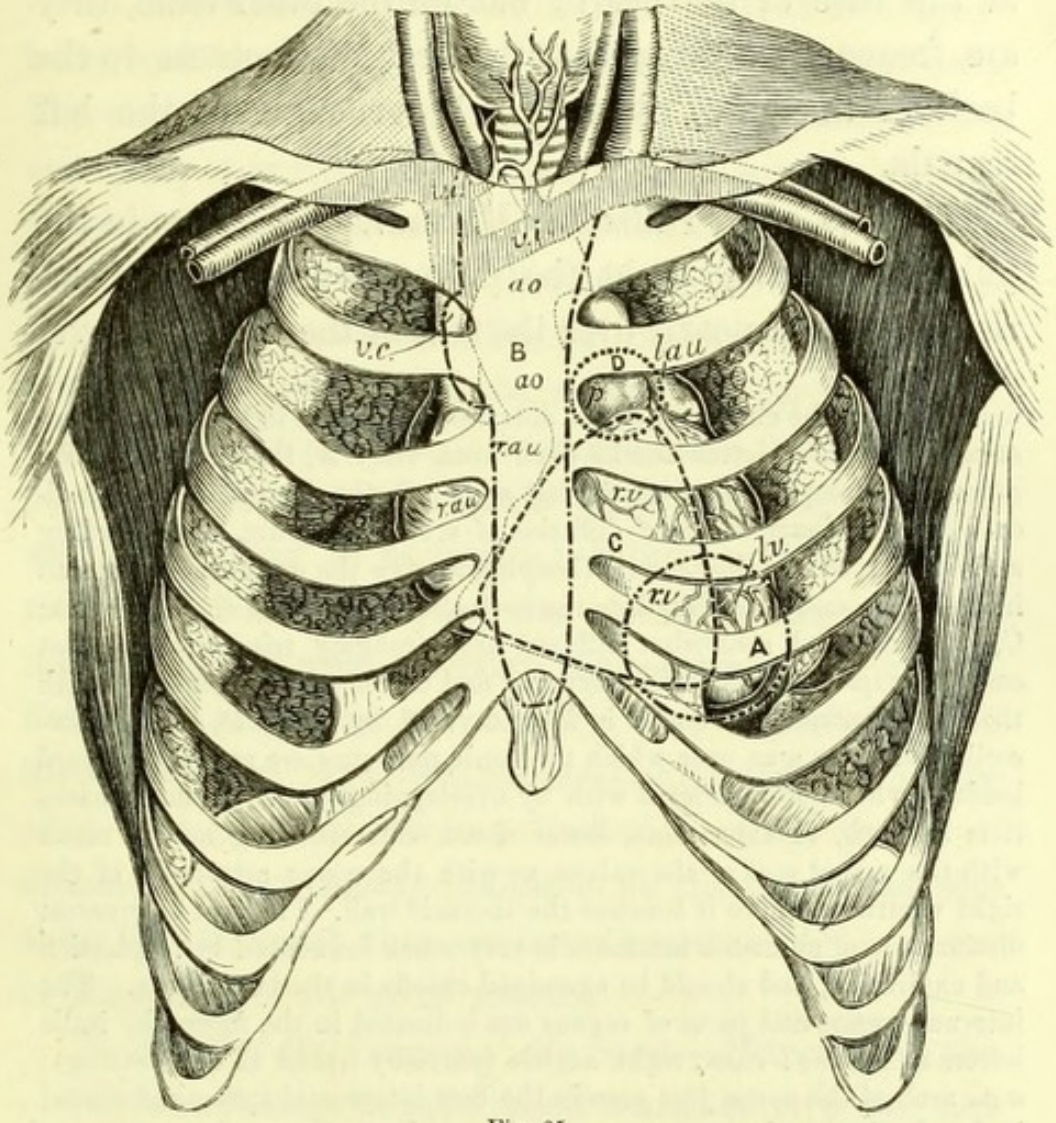

Fig. 25.

Explanation of Fig. 25.—In this figure the drawing of the heart and great vessels, in their relation to the front of the thorax, has been accurately copied, on a reduced scale, from the large folio work of Professor Luschka of Tubingen. ("Die Brust-Organe des Menschen in ihrer Lage." Tubingen, 1857.) The outlines of organs, which are partially invisible in the dissection, are indicated by very fine dotted lines; while the areas of propagation of valvular bruits, as described in the text, have been roughly marked out by additional much coarser, and more visible

nearly in a circle (A), it is probably of mitral origin. Mitral murmurs are often heard over a very limited space in front of the thorax; they are mostly inaudible at the base of the heart; but on the other hand, they are frequently conveyed with great distinctness to the back of the chest, about the lower angle of the left scapula.

2. *Area of the Pulmonic Murmur.*—Murmurs in the pulmonary artery, or at the pulmonary valves, are carried to the ear nearly over the seat of the valves, or over

dotted lines: the character of the dots being different in each of the four areas. A capital letter marks each area, viz.: A, the circle of mitral murmur, corresponding with the left apex; B, the irregular space indicating the ordinary limits of diffusion of aortic murmurs, corresponding mainly with the whole sternum (keeping in view the qualifications stated in the text), and extending into the neck along the course of the arteries; C, the broad and somewhat diffused area (roughly triangular in most cases) occupied by tricuspid murmurs, and corresponding generally with the right ventricle, where it is least covered by lung; D, the circumscribed circular area over which pulmonic murmurs are commonly heard loudest, when not interfered with by overlapping lung. In many cases it is an inch, or even more, lower down, corresponding not so much with the actual seat of the valves, as with the conus arteriosus of the right ventricle, where it touches the thoracic wall. The seat of greatest distinctness of pulmonic murmurs is very much influenced by inspiration and expiration, and should be examined chiefly in the latter state. The internal organs and parts of organs are indicated in the figure by italic letters as follows: *r.au.*, right auricle (partially traced in fine dotting); *a.o.*, arch of the aorta, just seen in the first intercostal space, and traced in fine dotting on the sternum; *v.i.*, the two innominate veins; *v.c.* vena cava descendens; *p*, pulmonary artery, where it lies near the thoracic wall, but often overlapped by a thin edge of lung; *l.au.*, left auricle, always covered by lung; *l.v.*, left ventricle, deeply overlapped by lung, except at the apex, within circle A; *r.v.*, right ventricle, to a considerable, but variable, extent in contact with the thoracic wall; in disease, often extending downwards nearly to the xiphoid cartilage, and propagating its murmurs in this direction beyond the limits of the area C, as shewn in the figure.

the upper part of the right ventricle. Their situation and their distinctness vary, however, to some extent according to the position of the left lung, which sometimes covers deeply the base of the heart, as in emphysema, and in other instances leaves it nearly uncovered. The circle D indicates the most elevated position of the murmur; it is frequently heard more distinctly an inch or even an inch and a half lower down. It coincides in position with the greatest distinctness of the pulmonic second sound, as contradistinguished from the second sound heard over the aorta, a little higher up, and to the right of the sternum; frequently also it coincides in position with a certain tactile vibration, difficult to describe, but easy to recognize; being perfectly characteristic, and accompanying the second sound, of which it conveys, as it were, an exact impression to the finger.* Pulmonic murmurs are usually very superficial, and therefore often very distinct, and apparently near the ear; they are nevertheless limited in their power of diffusion, being usually inaudible at the apex, and also along the sternum; they are never heard in the neck, nor in the course of the great vessels.

3. *Area of the Tricuspid Murmur.*—Murmurs at the

* See case of George M., p. 613; and of Wm. K., fig. 27, p. 605. Like all the other cardiac murmurs, the pulmonic murmur may likewise be accompanied by a more prolonged tactile vibration, the *frémissement cataire* of Laennec, or purring tremor. This, however, accompanies the first sound fully more often than the second; it is, in fact, simply the impression of the vibrations of the murmur on a less sensitive organ than the ear. The auricular-systolic mitral murmur is very often easily recognizable in this way, and may be perfectly well distinguished even without the aid of the ear.

tricuspid orifice are usually represented as very rare; this is not in accordance with my experience, at least as regards the murmur of regurgitation, which is often confounded with (indeed not unfrequently is associated with) the mitral murmur. The tricuspid murmur is heard over the right ventricle, where it is uncovered by the lung; *i.e.*, at the lower part of the sternum, and over the whole space between this and the seat of the mitral murmur. It is usually, like the pulmonic murmur, very distinct and superficial in its character; little audible, however, above the level of the third rib, and thus distinguished both from the pulmonic, and still more from the aortic murmur. The communication of the tricuspid murmur to the surface directly through the substance of the right ventricle is so obvious as to require no explanation. The area of this murmur, in ordinary circumstances, is indicated by the triangular space C; but in cases of considerable hypertrophy and dilatation of the right side of the heart, especially in connection with emphysema (when the ventricle pulsates in the epigastrium) the murmur is heard loudest towards the xiphoid cartilage, and along the margin of the sixth or seventh left costal cartilage.*

4. *Area of the Aortic Murmur.*—The law of diffusion of the aortic murmur is rather mysterious; for not only is it found in great intensity over the base of the heart and the *manubrium sterni*, which are in the immediate neighbourhood of the seat of its production, but fre-

* See the case of Thomas B., p. 438, also case of Wm. K., p. 605, with the remarks, p. 601, and Harriet M'D., Appendix.

quently, and not less distinctly, along the whole length of the sternum ; rather oftener than not, too, it is absolutely louder close to the xiphoid cartilage than at many points nearer to its origin. This fact, though not ordinarily stated in the text-books, is one of some importance, and rests (with me) upon very numerous observations. The aortic murmur is distinguished from all the other valvular murmurs by being propagated (though sometimes very faintly in the case of diastolic murmurs) into the arteries of the neck. Over the *manubrium sterni*, also, it is generally more distinct towards the left of the bone, and has not unfrequently (though not always) a quite special distinctness over the sternal end of the second right costal cartilage. It is the most widely-diffused of all the cardiac murmurs, and can sometimes be traced to great distances along the spine, and even along the bones of the extremities. I have heard it at the occiput, at the sacrum, and even at the elbow. It is in many cases undistinguishable from the murmur of aneurism of the arch, or of the innominate artery ; and also from certain purely functional murmurs following the first sound.

By due attention to these peculiarities of localization, a large amount of knowledge may be acquired of the special characters of valvular murmurs. I have not alluded hitherto (for I was anxious to avoid too wide a range in my observations) to the collateral phenomena by which some of these diseases are further distinguished, to the characters of the pulse, to the swelling and pulsation of the veins in the neck, to the alterations of the sounds apart from murmur, in certain cases. In the

meantime, I must be contented with a very brief *résumé* of what I have already stated, with a few important additions in regard to the distinguishing characters of exocardial, aneurismal, and functional murmurs.

Murmurs (and especially valvular murmurs) are judged mainly by their *rhythm*, and by their *limits of diffusion or area.*

1. An *auricular-systolic* murmur (fig. 19, p. 575), *i.e.*, one preceding and running up to the first sound of the heart, is in all probability produced in one or other of the auriculo-ventricular orifices ; inasmuch as it coincides with the forcible emptying of the auricles into the ventricles through these orifices. Its reasonable interpretation therefore is *obstruction to the current of the blood entering a ventricle.* If the left auriculo-ventricular orifice is affected, the murmur will be found to have the characters of a mitral murmur, and to be localized at A (fig. 25, p. 583) ; if, on the contrary, the tricuspid orifice be obstructed, the murmur will occupy the area C.

2. A *ventricular-systolic* murmur (fig. 20, p. 576), *i.e.*, one succeeding and running off from the first sound, may be produced either in the auriculo-ventricular, or in the arterial orifices. In either case, it coincides with the *emptying* of the ventricles; and therefore,

a. If *auriculo-ventricular* as to its origin, it is necessarily a murmur of *emptying backwards into the auricles*, or of *regurgitation.*

b. If *arterial* as to its origin, it is necessarily a murmur of *emptying forwards into the arteries*, or of *obstruction.*

A ventricular-systolic murmur may thus have four dis-

tinct solutions among the organic valvular diseases ; and here the consideration of the area of diffusion of the murmur comes to solve the difficulty. For if the area be (according to fig. 25, p. 583)—

A. Mitral—it is *mitral regurgitation :*

B. Aortic—it is *aortic obstruction :*

C. Tricuspid—it is *tricuspid regurgitation :*

D. Pulmonic—it is *pulmonic obstruction.*

3. A *ventricular-diastolic* murmur (fig. 21, p. 576), *i.e.*, one succeeding and running off from the second sound, may be produced either in the auriculo-ventricular, or in the arterial orifices. In either case it coincides with the *filling* of the ventricles ; and therefore,

a. If *auriculo-ventricular* as to its origin, it is necessarily a murmur of *filling forward from the auricles*, or of *obstruction :*

b. If *arterial* as to its origin, it is necessarily a murmur of *filling backwards from the arteries*, or of *regurgitation :*

A ventricular-diastolic murmur may thus have four distinct solutions among the organic valvular diseases ; and here the consideration of the area of diffusion of the sound comes to solve the difficulty. For if the area be (according to fig. 25, p. 538)—

A. Mitral—it is *mitral obstruction :*

B. Aortic—it is *aortic regurgitation :*

C. Tricuspid—it is *tricuspid obstruction :*

D. Pulmonic—it is *pulmonic regurgitation.*

4. One, two, or even three of the murmurs above mentioned may be found in combination in the same case. The most frequent combinations are—

a. Aortic obstruction and regurgitation, indicated by the ventricular-systolic and ventricular-diastolic murmurs (figs. 20 and 21), heard over area B (fig. 25) ;

b. Mitral obstruction and regurgitation, indicated by the auricular-systolic (fig. 19) and ventricular-systolic (fig. 20) murmurs, heard over area A (fig. 25) ;

c. Various combinations of the two preceding forms, the aortic and mitral valves being both diseased ;

d. Mitral obstruction, with dilated right ventricle, and consequently tricuspid regurgitation, indicated by the auricular-systolic murmur (fig. 19), heard over area A (fig. 25), and the ventricular-systolic murmur (fig. 20), heard over area C.

The rarest of all the murmurs proceeding from positive valvular deformity are the pulmonic, and the murmur of tricuspid obstruction ; and these murmurs are still more rarely observed singly, being usually in combination with diseases causing murmur on the left side of the heart.

5. Pericardial murmurs are frequently more or less present with both sounds of the heart, and when they are present with one sound only, it is almost invariably the first. They are to be distinguished in part, but by no means chiefly, by their special acoustic character of friction, or of irregular shuffling or grating.* In general terms they may be said to be deficient in precision of rhythm, and especially in what may be termed (in the technical language of music) *accentuation.* The *staccato* quality is wanting to them ; they are altogether more *cantabile*, less exact, or (to use a common phrase) more *slip-*

* See Case of Christina M., p. 621.

slop in their relation to the physiological sounds, than the valvular murmurs are. They are also more liable to change both in rhythm and position, from day to day, and even from hour to hour; this, however, applies chiefly to the early stages of disease. They are more often, more considerably, more unexpectedly and irregularly affected by the position of the patient, than in the case of the endocardial murmurs generally; and they are also more considerably, and especially more *essentially* altered in their character, in some cases, by pressure with the stethoscope.* Pericardial murmurs are sometimes heard along the left margin of the heart, or at the apex; but on the whole, they most frequently occur over the right ventricle and at the mid-sternum, and are not carried into the arteries of the neck, or in the direction of the xiphoid cartilage. They differ, therefore, in their usual localization, for the most part, both from aortic and mitral murmurs, which they sometimes resemble closely both in tone and rhythm.

6. Aneurisms of the arch, especially of its ascending part, give rise to murmurs which are with difficulty distinguished from those of aortic valvular disease. Sometimes, indeed, the distinction is practically impossible; where possible, it is to be made chiefly through the superadded signs and symptoms of aneurism, or through

* This fact, pointed out by Dr. Sibson, is undoubtedly of diagnostic value within the limits stated in the text. Endocardial murmurs, and indeed all murmurs whatever, are apt to be more or less increased under stethoscopic pressure; but a murmur which not only increases in amount, but *develops a new character or rhythm under stethoscopic pressure*, is, I believe, almost certain to be pericardial.

some minor irregularities in the localization of the murmur, or through its relation to some abnormal percussion-dulness of the sternum or left front, more or less easily distinguishable from that of the heart.*

7. Anemic and functional murmurs, as heard over the heart and great arteries, are always ventricular-systolic in rhythm, and they almost always simulate aortic or pulmonic murmurs as regards their area (B and D, fig. 25). They are to be distinguished chiefly by the circumstances in which they occur, and by the absence of the symptoms of valvular disease. A murmur which is generally diffused over the base of the heart, and over the great vessels, and which is not accompanied by vascular turgescence (or which is accompanied by anæmia and by venous murmur in the neck), is almost certainly functional if it alters in position and in intensity from day to day, or from week to week; and especially if it alters and diminishes, or disappears, under treatment by chalybeates, and under a tonic regimen.

8. Mitral and tricuspid murmurs of regurgitation frequently occur without primary valvular deformity. Such murmurs arise either from dilatation of the orifice, or from dilatation of the cavity of the ventricle, and from the want of adaptation of the muscular walls of the ventricle to the columnæ carneæ and chordæ tendineæ.†

9. A metallic echo of one or of both cardiac sounds may simulate a valvular murmur, and may be produced by an air-filled cavity in the neighbourhood of the heart (possibly aided by pericardial adhesions), the heart itself

* Case of Mary M'D., p. 611. † See p. 600, *note*.

being in other respects perfectly normal.* Metallic echo is pretty easily distinguished, by its peculiar hollow ringing or booming character; and also by its relation to the sound, which it seems to succeed not instantaneously, or so as to be identified with the rhythm of the movement, but rather after a brief interval, rising to a climax, and then falling away again.

I have observed a somewhat similar phenomenon in very large aneurisms near the heart; and also in one very remarkable case of aneurismal varix (or perhaps of what is often called aneurism by anastomosis), affecting all the vascular structures of the right side of the head and neck, and deriving a pulsation, with a marked thrill to the hand, from the carotid artery or some of its branches. The loud, hollow, prolonged, *roaring* murmur of this case, however, only resembled the metallic echo, as above described, in a few of its acoustic peculiarities, and could hardly have been mistaken for any ordinary cardiac or arterial murmur, by the most careless observer.

THREE MONTHS' HOSPITAL EXPERIENCE OF CARDIAC MURMURS.

Lecture,† *February* 28, 1862.—I think it may be useful to you to have, collected into a group, all the cases of cardiac valvular murmurs that we have hitherto had under notice this session, although many of them have

* Case of R. G. p. 419; and remarks on the murmur, p. 426.

† Revised from the report of Mr. J. Thomson Welsh.

been pretty fully discussed before. I wish you to observe in the aggregate the principles of their classification, and the mode in which these cases tend to arrange themselves in nature. I wish you also to be aware of some facts not generally known, or (not to presume too much) let me say some peculiarities in my own views of this subject which are well brought out by the cases of this winter. There have been a good many cases altogether, as you will recollect; but we always have a good many cardiac diseases under treatment in this hospital; for unhappily these diseases are only too common. Our experience this session is very much the experience of other years; and I propose to place it before you at one view.

First, then, recollect that at the beginning of the session we had several cases of what we pronounced to be *aortic valvular disease*. We had no doubt of it at all in these cases, except in so far as some of them were open to the question of aneurism of the aorta. [It is not always easy to distinguish the aortic valvular murmur from the aneurismal, if the aneurism is near the heart, or even in any part of the ascending aorta; the truth being that the two conditions are often associated, and that even if not associated, they are often quite identical as to the characters of the murmur. The existence of aneurism may indeed be inferred from other facts; but even then the question remains—is the murmur aneurismal or valvular? for you may have an aneurism entirely devoid of murmur; and you may have a murmur, in a case of

Aortic and Aneurismal Murmurs.

aneurism, which is nevertheless not aneurismal, but proceeds from the valves. There was a case under observation in November, which ended fatally, and which illustrates this point. The case was, as regards the murmur, one of aortic valve-disease; while as regards the physiological symptoms it was one of aneurism; and it proved to be, in fact, both valve-disease and aneurism.*] The case of John H., also, was, according to physical diagnosis, one of aortic valve-disease of the ordinary kind; though he complained, on admission, remarkably little of the heart. I think I might even say that he complained of nothing at all but of pain in the back; and you will recollect that this symptom had been so decidedly *the* symptom of the case, that before I examined him he had Corrigan's cautery repeatedly applied. On examining the back with great care, we found evidence of a limited dull percussion on each side of the spine, from the 9th to the 12th dorsal vertebra, and of a corresponding feebleness of the respiratory murmur; and the hepatic dulness was considerably depressed behind as well as in front. I have no doubt at all, therefore, that there was an aneurism of the descending aorta, pressing on the vertebræ. In the end, the pain became more of the ordinary character of angina pectoris, due to the cardiac disease. The case of Violet M'P. was a case of primary aortic valve-disease followed by dilatation of the left ventricle and secondary mitral regurgitation. This case ended fatally. John

Aneurism of Descending aorta, with Valve-disease.

* Case of Mary M'D., p. 611.

K. had also aortic valve-disease, and in this case there was quite distinct angina pectoris, and a hypertrophied heart. After this we had no more cases of aortic disease till the other day, when three cases presented themselves almost at once. Two of these are perfectly pure cases of aortic obstruction and regurgitation, and are quite typical cases as regards the murmur. [Description of characters of murmur.] There is a very great degree of enlargement of the heart in both these cases, and in both of them the enlargement is chiefly *downwards and to the left.* The lung expands freely over the base of the heart in both, so that it is difficult to estimate the length of the organ [diagrams shewn and measurement stated in detail] ; but in the case of David H., in which the transverse percussion-dulness is enormous (7½ inches), I doubt much if it is all cardiac dulness, especially the part to the right of the sternum, which is rather vague and ill-defined.* Now, I beg you to remark that every one of our cases of aortic valve-disease has had more or less of that peculiar kind of *anguish*, which I call in general *heart*-suffering, as opposed to *lung*-suffering. It is closely related to the angina pectoris of Heberden, but less clearly defined as regards local pain, and sometimes a little difficult to distinguish from dyspnœa or breathlessness. None of our cases have had much real dyspnœa as yet, and all of them are devoid of lividity ; most of them, on the other hand, are apparently anemic,

Heart-suffering and Lung-suffering.

* This doubt proved well-founded. See further remarks on case of David H., with diagram, at pp. 617, 618.

owing to the failure of the systemic circulation to reach the capillaries in full force. [This is, according to my experience, the special physiognomic character of cases of aortic disease; sleepless anxiety, and the sense of impending death; pallor, and a peculiar frightened or agitated look, with but little dropsy or lividity, at least in the earlier stages of the disease.] Not one of the cases at present under observation has had much dropsy. Most of them have had the well-known "pulse of unfilled arteries" of Dr. Hope; but in some the pulse has been feeble, in others not remarkable. I have called these cases aortic disease; but several of them have had mitral regurgitation also. We cannot be sure, but I am of opinion that this was probably a secondary phenomenon, due simply to dilatation of the ventricle, not to primary deformity of the mitral orifice. [In the case of David H. the mitral murmur afterwards supervened under observation.] There were in all *seven* cases of aortic valve-disease, including the two cases of aneurism.

Physiognomy of Aortic Disease.

Secondary mitral regurgitation.

Next, as regards *mitral valve-disease*, I find that we have had *seven* cases (or possibly eight) in which the mitral valve was affected *primarily, i.e.*, excluding the murmurs of mere dilatation, and retaining only those in which I am sure there are vegetations and positive deformity of the valve. How can I possibly be so sure of this? you will ask. There is only one way to be sure of anything in medicine, and that is to accept only positive evidence; in this case I think we may very pos-

sibly have left out some cases which ought to have been included; but we can hardly have erred much in the opposite direction, for *all that I have ventured to call cases of primary mitral disease have had the special murmur of mitral obstruction, i.e.*, the murmur preceding the first sound, *auricular-systolic* as I call it, which you will remember I have demonstrated to you over and over again in the wards as perfectly distinct from the murmur of mitral regurgitation, the *ventricular-systolic*, sometimes produced by mere dilatation of the orifice. *I hold that without the auricular-systolic murmur we have no security at all that the disease is primary mitral disease;* and I confess I am surprised at the extraordinary confusion that prevails in your accustomed text-books as to this refinement of diagnosis, which is to me one of those perfectly plain, almost mathematically demonstrable, facts about which there is hardly the possibility of a mistake. I cannot explain the constant assertions of authors that this murmur is of rare occurrence, except upon the presumption that almost all of them have confounded it habitually with the murmur of mitral regurgitation. [Indeed, it has occurred to me to make quite sure of this in one instance; for a very excellent and well known physician, who has investigated cardiac diseases with much more than ordinary attention, and is justly esteemed for his numerous and valuable contributions to scientific medicine, has favoured me with a private letter in which, among other things, he expresses an entire scepticism as to the very existence of the *auricular-systolic* murmur, as I have observed it. He

is of opinion that what I have so described in the paper on cardiac murmurs is in reality only a variety of the mitral regurgitation murmur.] Now here is my view of the matter, and I beg you to note it, and to hold me to the proof on all proper occasions. Auricular-systolic murmurs are certainly not rare; for here are seven of them, at least, in three months, as a counterpart to the same number of aortic murmurs, which nobody will assert to be rare. To me they are among the commonest and the most easily detected of all the cardiac murmurs; and seeing that I regard the auricular-systolic murmur as all-important in the diagnosis of primary mitral disease, you may trust me when I tell you that in many years' hospital experience, I have not seen a single instance in which an auricular-systolic murmur, being of mitral origin, has been produced by *mere* regurgitation; not a single instance in which such a murmur has occurred without either vegetations or contraction of the orifice. On the other hand, I have seen many cases of widely-dilated mitral orifice with evident regurgitation, but without obstruction or deformity; and in every one of these cases the murmur (if present at all) has been ventricular-systolic. In our seven cases, therefore, I feel as sure as I can well be of anything in medicine, that we have to do not only with mitral disease, but with mitral obstruction. The cases are William S., George M., Janet A., Margaret D., Jane R. (also regurgitation), Eliza T. (obstruction-murmur slight, regurgitation-murmur loud), William K. (complex) [see diagram

Diagnostic value of the Auricular-Systolic Murmur.

and description at p. 605]. There have been only two deaths among these cases, and only one dissection; but in this case our diagnosis was verified, although the obstruction-murmur was very slight and transient, and the regurgitation murmur tended to obscure it. I am a little doubtful about Janet A.'s case. I feel almost sure it is mitral obstruction, but I admit the possibility also of its being an exocardial murmur. If exocardial, I do not hold out absolutely for my diagnosis; but if not exocardial, I think it must be mitral obstruction and nothing else. [Diagnosis afterwards verified. See p. 607, No. 10.]

In addition to these seven cases of mitral obstruction (two of which were of regurgitation also), there have been five other cases of mitral regurgitation, having the ventricular-systolic murmur only. We cannot be sure that there *is not* obstruction in these cases; we can only be sure that there *is* regurgitation; there is no *evidence* of obstruction, but of course there may be obstruction without the characteristic murmur. The cases are James F., Eliza C., Jane D., John H., and Violet M'P. [the last two (afterwards also David H.) with aortic disease]. The regurgitation may, as I said before, be in some of these cases quite independent of any deformity of the valve; it may even occur without dilatation of the orifice. It may in these circumstances depend on a dilated ventricle, causing derangement of the mechanism by which the valve is closed in the normal condition.*

* See a memoir by the author "On the Mechanism and Sounds of the Dilated Heart," in Edinburgh Medical Journal, July 1856. I could

I have nothing to say to you about *disease of the pulmonic valves*, as no single case of this really rare form has presented itself.

Of *tricuspid valve-disease* indicated by murmur we have had, I think, not less than six or seven cases. Some of you may possibly be surprised at this; for you will find in most of your books statements that these murmurs are rare; and some very high authorities even assert that tricuspid regurgitation scarcely ever gives rise to murmur. In my opinion these positions are quite incorrect. Tricuspid murmurs you will find, if you examine carefully, to be nearly as common as other murmurs; I admit that they are more difficult to detect with certainty, because they are very apt to be confounded both with mitral and with exocardial murmurs; indeed they are very frequently associated with mitral murmurs. In the great majority

Tricuspid Murmurs not rare.

have wished to have included this memoir in the present volume; but the anatomical details which would have been necessary to its completion, though I have demonstrated them on many occasions to my students, and also brought them before the Medico-Chirurgical Society of Edinburgh, and the Physiological section of the British Association of Science (in Dublin), have never been fully published, and would have been somewhat unsuitable for this series of papers. Of the clinical fact, that a dilated ventricle often causes incompetency of the auriculo-ventricular valves, there is now no longer any doubt whatever. Of the anatomical and physiological relations of that fact, no satisfactory account, to my mind, has ever yet been given. I long hoped that my imperfectly expressed ideas would have been caught up by some professed anatomist or experimental physiologist, and worked out in detail with opportunities that are not at my command; but the most recent contributions to the physiology of the auriculo-ventricular valves make little or no progress in the direction that appears to me to be the true one."

of cases, however, the tricuspid murmur is one of regurgitation; *i.e.* it is *ventricular-systolic* in rhythm. This corresponds with what we know of the pathological origin of tricuspid murmurs, which very rarely proceed from deformity of the valve, but only from dilatation of the orifice or of the right ventricle, with secondary regurgitation. And if you keep in view what I told you about mitral murmurs, you will see how exactly the tricuspid murmur accords with my theory; for while the ventricular-systolic murmur of regurgitation is quite an ordinary phenomenon, which we observe, both over the mitral and tricuspid area (fig. 25, p. 583), and while the *mitral* murmur of obstruction is very common also, the *tricuspid* murmur of obstruction, the auricular-systolic, is among the rarest of clinical facts in my experience. I cannot even remember to have heard it, so as to be quite certain of its occurrence, more than once, or perhaps twice at most; I suspect, indeed, that I have heard it oftener than this, but it has been so mixed up with other murmurs as not to be easily distinguished. [I have, indeed, heard (but once only), an *auricular-systolic* murmur over the tricuspid orifice, absolutely uncomplicated, and free from the suspicion of mistake. The patient is an Irish labourer (Patrick M., aet. about 20), known to Dr. Greig of Dundee, where he is still living, and, happily, likely to live for some time. He suffers no very great amount of inconvenience from his disease, except from a very remarkable undulating movement in his neck, for which he came

But almost always regurgitant.

Auricular-systolic Tricuspid Murmur. (Obstruction).

over to Edinburgh, about two years ago, to consult Mr. Syme, supposing that it was something that might be cured by surgery. He afterwards came under my care, and remained a good while in my ward on two occasions, but more, I must confess, with a view to my scientific curiosity than to his own advantage, as there is little excuse for keeping him as an hospital patient. He is rather pallid, and, perhaps, not very strong; but of firmly-built frame, tolerably active, and neither livid nor dropsical. The undulation is beyond all question in the jugular veins on both sides of the neck; and it is quite evident that these veins are much dilated or enlarged permanently, without being much distended with their contents. The cardiac murmur begins immediately after the second sound, continues *(diminuendo)* throughout the pause, and then goes on *(crescendo)* up to the first sound, at which it stops abruptly. I think tricuspid contraction may in this case be predicted with all but mathematical certainty; the fact, however, of having witnessed this typical instance, only serves to make me more entirely confident that I cannot have overlooked the fact in many other cases. In one other instance I *must*, and in yet another I *may*, have heard this murmur. In both these cases death occurred, and the hearts, now in my possession, and shewn by me at the time to the Medico-Chirurgical Society, have a contracted tricuspid orifice, as part of a complex morbid condition of the valves. In one of them (Ann D., æt. 25, Register of Dissections, Aug. 9, 1859), the murmur covered nearly every part (at some examinations *quite* every part) of the heart's sounds and their interval; and I thought I could distinguish

the right side of the organ as being affected. In the other the murmur was quite unequivocally on the right side, but appeared to be a murmur of tricuspid regurgitation, which condition, no doubt, existed in addition to obstruction. This last was the case of the young girl Mary P., mentioned at p. 97 ; and I think there is good evidence that the disease began in the mitral orifice, and extended at a much later date to the tricuspid].

Cases of Tricuspid Regurgitation-murmur.

Our cases of tricuspid regurgitation this session form a most instructive series, as bearing upon the usual mode of origin of that form of disease. The first was that of Thomas B. (fig. 26), who had emphysema of the lungs, with a very

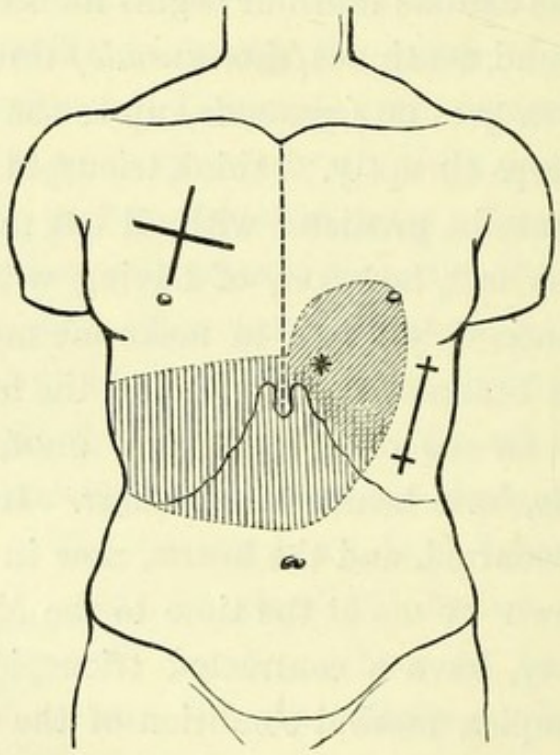

Fig. 26.

Explanation at p. 443. * Seat of Murmur.

distinct and permanent ventricular-systolic murmur heard over the very middle of the right ventricle. [The case is

fully recorded at p. 438]. In the case of Elizabeth C., I feel almost sure we had a similar murmur, but her weak state prevented much examination. In several of our cases of mitral obstruction, we had also a murmur of regurgitation which I fully believe to be of tricuspid origin; though it is a little difficult to prove this, owing to the probability of there being mitral regurgitation also present. The cases are Eliza T., Jane D., and

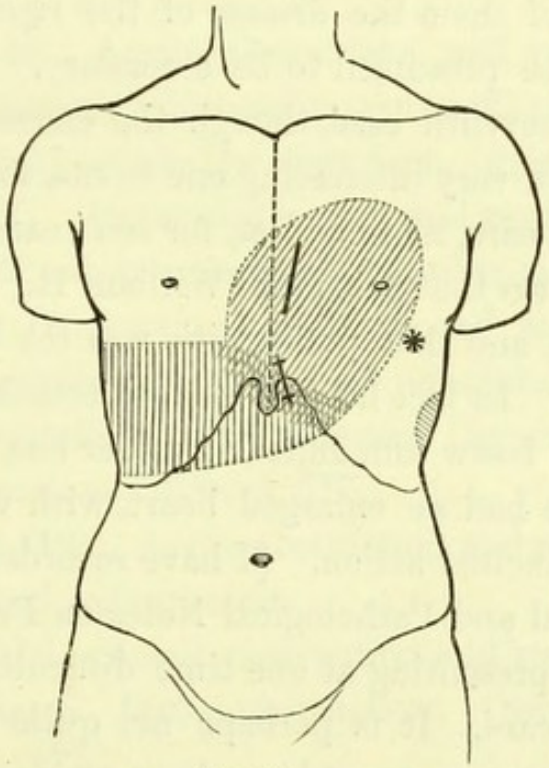

Fig. 27.

Case of William K., as described in text. The limits of dull percussion (liver, spleen, and enlarged heart) defined by shading. At *, the apex-beat of left ventricle is distinctly felt by the hand, and here the murmur is almost exclusively auricular-systolic. At ‡, there is very distinct pulsation in epigastrium, and here the murmur is ventricular-systolic. There is no murmur at the base, but the second sound is very loud over the space indicated by the bar.

Margaret D. But by far the most convincing case of this kind, is that of Wm. K. (fig. 27), where, I think,

there are unquestionably present both a mitral obstruction and a tricuspid regurgitation-murmur, the two being quite clearly separate both in position and rhythm; so that with only a little care it is impossible, I think, to make a mistake; and accordingly I made you study this case very attentively. These four cases of mitral disease, added to the two of emphysema of the lungs, make up six instances of tricuspid regurgitation; and you observe that in all of them the disease of the right side of the heart must be presumed to be secondary. The same is true of the seventh case, though the cause is different. This case is a very interesting one to me, for I have had it under my care, more or less, for ten years. I have no doubt whatever that this man (William H.) has adherent pericardium, and that pericarditis was the beginning of his disease. In fact he had several attacks of pericarditis before I saw him suffering under one, and even at that time he had an enlarged heart, with very irregular and rather excited action. [I have recorded his case in my "Clinical and Pathological Notes on Pericarditis," p. 13, as one presenting at one time difficulties about the murmurs heard. It is perhaps not quite certain, even now, that there may not have been endocardial disease also; but there has been no murmur corresponding in rhythm to the one now heard, at least for many years past, during which I have seen this man from time to time.] His heart is now, of course, considerably more enlarged than when I first knew him; it is, in fact, enormously large. There is a murmur of regurgitation exactly over the right ventricle, and I have no doubt it is simply

a leaking of the tricuspid orifice, due to the great dilatation of the ventricle.

Synopsis of Cases of Valvular Murmurs referred to above as having been under treatment between November 1861 *and February* 1862.

1. Mary M'D. Aortic regurgitation, aneurism of arch. (See p. 611.)
2. John H. Aortic obstruction and regurgitation, aneurism of descending aorta, mitral regurgitation (?)—probably secondary. (See p. 595.)
3. John K. Aortic obstruction and regurgitation.
4. John B. Aortic obstruction and regurgitation.
5. David H. Aortic obstruction and regurgitation; afterwards, on a second admission, mitral regurgitation, with increased hypertrophy and dilatation. (See p. 617.)
6. Violet M'P. Aortic obstruction and regurgitation, mitral regurgitation.
7. Jane M'L. Aortic obstruction and regurgitation.
8. William S. Mitral obstruction. (See remarks on No. 17.)
9. George M. Mitral obstruction. (See p. 613.)
10. Janet A. Mitral obstruction; afterwards tricuspid regurgitation, from dilatation of right ventricle. (See p. 600.) After death (March 13th), heart, 13 ounces; mitral orifice, admitting only the thumb; circumference, 24 inches.
11. Margaret D. Mitral obstruction; afterwards

tricuspid regurgitation, and possibly also mitral regurgitation.

12. Jane R. At first mitral obstruction only ; afterwards mitral regurgitation, which became the predominating murmur, obscuring the other.
13. Eliza T. Mitral obstruction and (tricuspid ?) regurgitation. The latter murmur was much the more constant, and before this girl's death it greatly obscured the former, which, however, had been duly recorded at a previous stage. After death, great dilatation of right ventricle, left side and aorta small. Heart weighed 7 ounces (æt. 14). Mitral orifice admitted only the little finger ; circumference 1.6 inches.
14. William K. (Not in the hospital, but submitted to careful examination several times at the ordinary visits), mitral obstruction, tricuspid regurgitation ; the two murmurs remarkably distinct, with very marked signs of hypertrophy of right ventricle. (See Fig. 27, p. 605.)
15. James F. Mitral regurgitation.
16. Eliza C. Mitral regurgitation.
17. Jane D. Mitral regurgitation, with a possible element of tricuspid regurgitation or of pericarditis. The same doubt arose here as in the case of Janet A. (No. 10, see p. 600); but I regard it as still unsolved. In cases 15 and 16, similar doubts arose at one period or another of the investigation ; and in case 15, which was one of sub-acute rheumatism, it is even

likely that there was pericarditis with slight effusion, indicated both by symptoms and by physical signs. In Jane D. and James F. there had been several attacks of rheumatism before admission, and the history of the cardiac murmur was obscure. None of these cases had the murmur of mitral obstruction. Ultimately it appeared to me clear that in James F. and Eliza C. there was *at least* mitral regurgitation, whatever else ; but in the case of Jane D., I must confess a difficulty in diagnosis, amounting to a permanent uncertainty as to whether there is endocardial disease at all, and still more as to its character and seat. In the case of Janet A., as beforementioned, this doubt arose, though only at a late period of the case ; it was not, however, nearly so difficult a case as that of Jane D., and ultimately I reverted to the original opinion, which was justified by the post-mortem examination. It is worth while here to remark, that at one time (about two years ago) I had indicated the case of William S. (No. 8) as one probably of exocardial murmur, though doubtful. I have no exact note of the facts or of the difficulties, but from general recollection I have no doubt that they were of the same character as in these cases, and they leave, in my opinion, the case of William S. still open to a suspicion of pericarditis. In

Nos. 2, 5, and 6, there were also murmurs of mitral regurgitation, along with aortic disease.

18. Thomas B. Tricuspid regurgitation, from emphysema of lungs. (See pp. 438 and 604.)

19. Elizabeth C. A nearly similar case, but only a very short time under observation.

20. William H. (See p. 606.) This case is one of the greatest possible interest, and not without difficulty; but the murmur of tricuspid regurgitation, which has arisen at a late period of the history, is, in my opinion, the least doubtful part of the physical diagnosis, and it probably depends upon dilatation of the right ventricle. The case is particularly recorded, as stated above, in my papers on pericarditis; and there can be no doubt, I think, that its primary character is exocardial disease. It is possible also, but not proved, nor even, I think, probable, in the circumstances, that there may have been some degree of mitral obstruction in this case. A very instructive case, involving similar doubts, and ending fatally in the summer of 1858, is briefly noticed in pp. 14 and 15 of my "Clinical and Pathological Notes on Pericarditis;" and the comparison of these facts with others mentioned in that paper, will fully explain the doubts adverted to in the remarks under No. 17 of this series.

In addition to these three cases of tricuspid regurgitation, there were at least three, possibly five, others,

in which this murmur was developed in connection with mitral disease, being in all probability secondary to the disease at the left side of the heart. Nos. 10, 11, 13 (?), 14, 17 (?)

SELECT CASES IN ILLUSTRATION.

CASE I.—*Aneurism of the Arch of the Aorta, with Murmurs of Aortic Valve-Disease—Symptoms Characteristic of Aneurism, but generally resembling Laryngeal Phthisis.* (*No.* 1 *of Synopsis, p.* 607.)

Lecture, November 29, 1861.—Mary M'D., æt. 55. This poor woman, who died a few days ago, was a very long time in the hospital. She had been an illustration of our clinical lectures for nearly a year past. She was emaciated and pallid to an extreme degree, and had cough, with a great deal of expectoration; occasional fits of breathlessness, aphonia, rapid, weak pulse, and diarrhœa; in addition to which symptoms of phthisis, she had the clubbed finger-ends, commonly supposed to be characteristic of tubercular disease, in a high degree. In fact, I am sure that nine out of ten of you must have mistaken her case for one of phthisis, in passing through the ward, had I not been careful to point out the differences. These were as follows:— In the first place, the dyspnœa was remarkably spasmodic; more like the dyspnœa of asthma, or of chronic bronchitis with emphysema, than that of phthisis. There was also a degree of angina pectoris, I think, mixed with the dyspnœa, and very certainly there was a laryngeal element in it, for there was stridulous breathing

distinctly proceeding from the glottis; and also imperfectly closed glottis during cough; besides these, there was very persistent aphonia. These symptoms might, of course, have proceeded from laryngeal ulceration in phthisis, but then the epiglottis and glottis, as examined with the finger, were sound. *Whenever you have laryngeal symptoms with what seems to be a sound glottis, you will do well to suspect aneurism, or tumour of the mediastinum.* Next, observe this point, connected with the history of a very protracted case. During a long illness, with many symptoms of phthisis, this patient never had a habitually purulent expectoration. Now, *whenever, either in catarrh or in apparent phthisis, the expectoration continues abundant, without becoming purulent in due course, you are justified, in my opinion, in suspecting organic disease not tubercular, and most probably aneurism.* [See the case of Roderick R., p. 651, *et seq.*; also that of Peter B., p. 306, *et seq.*, where this rule, however, when too rigidly applied, tended to mislead. Nor can it be argued from the purulent character of the sputum, that the disease is *not* aneurismal. See case of William J., p. 545, *et seq.*] Lastly, the diagnosis in the case of Mary M'D. was fixed, in my opinion, by a murmur with both sounds of the heart, as of aortic valve-disease, at the upper sternum; and by an occasional very slight dulness on percussion about the right sterno-clavicular articulation; which, however, proved not to be the part of the tumour that was most important functionally; for that was quite beyond the reach of physical diagnosis. [Cardiac murmurs with the second sound very rarely concur with tubercle, and frequently with aneu-

rism, and therefore the *principle* here was quite correct; but the murmur was, in fact, not aneurismal in Mary M'D.'s case; the tumour arose from the extreme back part of the arch; it compressed the left recurrent, and lay close to the vertebræ. The aortic valves were incompetent, and no doubt this was the true source of murmur. The heart was but little enlarged. To complete the *possible* fallacies which attended an *actually* correct diagnosis in this case, the slight and occasional hæmoptysis, which was particularly remarked upon as bearing on the question of prognosis, was in fact neither tubercular nor aneurismal, but in all probability due to congestion of the lungs from the valvular lesion of the heart. It hardly ever amounted to more, however, than mere streaks in the sputum. In the case of Peter B., a similarly slight hæmoptysis, suspected at one time of being aneurismal, depended on incipient tubercle.]

CASE II.—*A characteristic case of Mitral Obstruction, arising from Rheumatism; interpretation of the Collateral Phenomena, as bearing on Prognosis.* (*No.* 9 *of Synopsis*, p. 607).

Lecture, 6th December 1861. George M., æt. 28, clerk. From looking at this man's physiognomy you can learn nothing; it is quite impossible to tell that there is anything wrong with him. From the history we have the symptoms of a cardiac disease, referred to acute rheumatism; in fact, he has had three severe attacks of rheumatic fever, the first and most severe three years ago. Before this he had no uneasiness of

any kind, but for the last two years he has suffered from breathlessness and palpitation.

Now, on examining the heart, we note that there is nearly equal movement over the right and left ventricle. This of itself is an abnormal fact, and an important one. It shews that the right ventricle is both too prominent and too powerful. The apex-beat is moderate, and seemingly more diffused than natural ; it is rather low and far to the left, but not extremely out of position. The heart's dulness is also increased transversely, being about six inches across at the nipple. At the apex there is a murmur, and it is with the first sound. But as a murmur may either immediately precede or immediately follow the first sound, I beg you to observe that this murmur precedes the sound, running sharply up to it, and then coming quite abruptly to a stop. It is a very rough murmur, grating in character, and ending thus, r-r-r-b. This is the usual character of the murmur now under consideration ; the murmur which succeeds the first sound is usually much more soft and blowing. The murmur heard in this case is not heard during the pause or rest of the heart's action ; it has, therefore, no connection with the second sound ; it is separated from this by the pause, and as the heart's action is quite regular and slow, there is no difficulty in defining it. This is the simplest of all the murmurs to define. Except in rare instances, or where the facts are confused, it may be taken for granted that a murmur of this kind is mitral, and that it depends upon obstruction of the orifice. In such cases there is generally a permanently overloaded pulmonary circulation, and a hypertrophied right ven-

tricle, and so it is in this case. The proof is to be found in the increased impulse over the right ventricle, and in a peculiar tactile sensation over the pulmonary artery, which concurs with the second sound, and gives to it the effect of a sudden impulsive *snap*, as indeed it is. The second sound is heard also by the stethoscope placed over the third left costal cartilage, and it is manifestly increased in distinctness and sharpness, as well as in depth of tone. These are the clear and unmistakeable signs by which you estimate the condition of the right ventricle and of the pulmonary circulation, and this is of the greatest importance as regulating practice. [When the tricuspid orifice becomes dilated, you may have these signs lost or weakened, and this, with or without the additional tricuspid murmur ; you then get lividity, cyanosis, distension and pulsation of the veins in the neck, etc. ; but there was nothing of all this in the case of George M. Even when there is tricuspid murmur, on the other hand, you may have these signs of increased force in the pulmonic circulation, and on the other hand few signs of overloading of the systemic veins, as in the case of William K., see p. 605 ; the interpretation is, that the amount of regurgitation is small, compared with the amount of murmur. The same facts may be often verified on the left side of the heart, by observing the variations of the pulse in connection with mitral regurgitation.] Now, to sum up, we have evidence in this case, notwithstanding the man's favourable appearance, of considerable enlargement of the heart. It is especially a broadening of the organ to the right, and concurs with hypertrophy and dilatation of the right ventricle from frequent

overloading of it. Probably the pulmonary artery is permanently enlarged, and the tricuspid valve might at any time give way, if it has not done so already. There is also evidence, which I did not dwell upon, of enlargement of the entire liver ; and this man has repeatedly spit up blood. The prognosis is regulated by the whole of these collateral phenomena, not by the mere fact of the murmur. The diagnosis of the murmur is only the first stage in the study of a cardiac case. The questions of most direct and immediate importance lie behind. How far has the disease gone ? what are the special dangers impending ? These are the questions that regulate practice, and to answer them you must survey the case broadly, and study it with a wide range of inquiries. In this case, I fear the prognosis is not so good as it looks at first ; though dropsy, the most familiar symptom, has been absent, I cannot exactly tell why. In fact, since our report of him was written, I have learned that just at the time of admission he seemed in extreme danger ; he had great dyspnœa, with irregularity and smallness of the pulse. These symptoms rapidly disappeared under diuretics ; and being much occupied with other matters, I saw little of them. This man, in my opinion, lives upon the brink of a precipice ; he is in much greater danger than the patient with mitral regurgitation now in the same ward ; though mitral regurgitation, on the average of cases, is a much more immediately dangerous form of disease than mitral obstruction.

[The case of Janet A. (No. 10, p. 607), was closely similar to this one, but was not discussed so fully at lecture. In her case a tricuspid murmur occurred very

late in the disease, and evidently from secondary dilatation of the right ventricle; as a post-mortem examination, performed 15th March, shewed the mitral valve alone to be diseased. The diagram in the case of Wm. K., p. 605, serves in some measure to illustrate both of these cases.]

CASE III.—*A characteristic case of Aortic disease, followed by Secondary Mitral Regurgitation and Pulmonary Hemorrhage. Effect of these secondary phenomena on the physical signs.* (*No. 5 of Synopsis.*)

At the time of the first admission of David H., he had all the ordinary signs of aortic obstruction and regurgitation in the highest degree. The heart was enormously enlarged, its percussion-dulness $7\frac{1}{2}$ inches, measuring from the upper right border, near the manubrium sterni, to the site of the apex-beat. The murmur was with both sounds, and fulfilled every condition of the aortic valve-murmurs, as laid down in the preceding paper. The radial pulses were highly undulating and full, though soft; there was great pallor, and marked angina-like suffering; the systemic circulation was evidently not carried on into the capillaries, and the left ventricle was consequently permanently overloaded. The liver was somewhat enlarged, but as yet there was little evidence of pulmonary complication. The diagram made at that time (February 25th), is not here reproduced, as it represents merely a less degree of the signs presently to be described.

On the second admission, the diagram, Fig. 28, was

executed (April 19th). All the sufferings were greater,

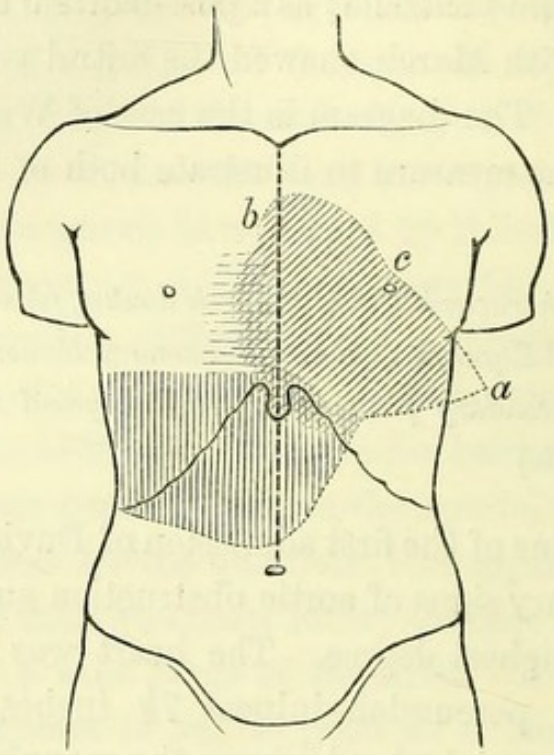

Fig. 28.

Case of David H. as described in text. The dotted outline of cardiac percussion-dulness is carried out to *a*, so as to indicate the extent to which the apex passes into the lateral region. *b* to *a*, 8½ inches; xiphoid to *c*, 5½ inches; greatest vertical measurement of hepatic dulness, 5 inches. The transverse percussion-dulness of heart uncertain, as stated in text.

and, in addition *hæmoptysis had begun.* There was now *a very distinct mitral regurgitation-murmur* in addition to the aortic obstruction-murmur, though occupying the same period of the heart's rhythm. Of course this apparently new murmur may possibly have existed before, but not loud enough to be heard as separate from the other. The heart's apex was felt beating over a wide space, and quite in the extreme lateral region; the distance from apex to base was fully 8½ inches; the uncertainty as to the right border of the cardiac dulness still

existed, as mentioned at p. 596. [This uncertainty was explained afterwards by the hemorrhage into the anterior border of the right lung, which had caused local condensation there]. The liver, enlarged and at the same time pushed downwards by the undue expansion of the right lung in inspiration, extended nearly to the umbilicus, and yet its left edge fell very much within the line of the cardiac dulness (compare the relations in cases of Thomas B. and William K., pp. 604, 605). These changes were the precursors of death. The hæmoptysis now became constant, and at the same time extensive dulness on percussion was observed at the bases of the lungs. The radial pulses became much weaker, and the heart's beat more feeble and diffused ; angina and orthopnœa made progress, and the patient became much emaciated. In the end the murmurs were nearly inaudible.

At the post-mortem examination a third diagram was made of the percussion, from which it appeared that the liver had receded almost to the right hypochonder, evidently from the ascent of the diaphragm.* The heart had also considerably diminished in apparent bulk, and this in all its dimensions. Either, therefore, the lungs had been expanding much more freely than before over the heart ; or (as I think more probable), the overcharged left ventricle had been emptying itself very freely back-

* On the first diagram, February 25th, the upper margin of the hepatic dulness is marked as $1\frac{3}{4}$ inches below the right nipple, while on April 19th the distance was $2\frac{1}{2}$ inches. After death (evidently, as stated from the ascent of the diaphragm, an almost constant phenomenon in the last hours of life, owing to the collapse of the lungs), the distance of the hepatic dulness below the right nipple was only $1\frac{1}{2}$ inches.

wards, through the permanently open mitral orifice, into the lungs, the bases of which were thus rendered useless; while respiration was carried on chiefly by the anterior and upper parts, in a state of unusually complete expansion. Owing to these causes, the apparent bulk of the heart, as estimated by percussion after death, was at least a third less than appears in fig. 28.

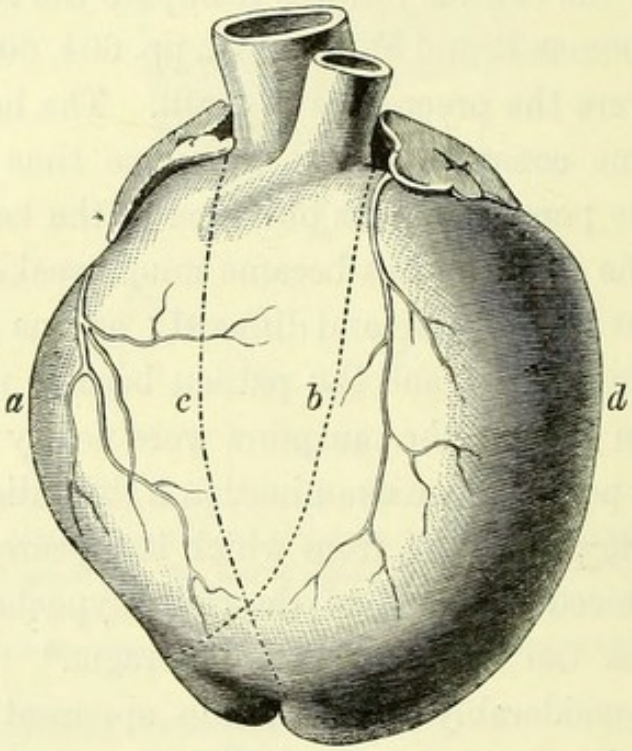

Fig. 29

Heart of David H. The pulmonary artery in front, and the aorta behind it, are seen emerging from the right and left ventricles, and the auricles are seen at the sides. The outline at *a*, and the dotted line at *b*, enclose the right ventricle; the dotted line at *c*, and the outline at *d*, shew the limits of the greatly enlarged left ventricle, the rounded apex of which gave rise to the extremely diffused apex-beat. It is to be kept in view that the dilatation of the left ventricle had diminished before death, as compared with fig. 28.

The heart, of which a drawing is given in the margin (Fig. 29), weighed 23 oz. The left ventricle was enor-

mously enlarged and dilated relatively to the right ; the aorta was also much larger than the pulmonary artery, and the septum ventriculorum was convex towards the right ventricle. The aortic valves were incompetent to a great degree ; the mitral valve was normal, and the mitral orifice little, if at all, dilated.

CASE IV. *Case of Pericardial Effusion in connection with Bright's Disease of the Kidney. Question of Exocardial or Endocardial Murmur. Paracentesis Abdominis, followed by great improvement.**

Lecture, Friday, February 22*d*, 1861.—Amongst the numerous urgent cases recently admitted is one of more than usual interest as bearing on some points of cardiac diagnosis ; and although we have as yet only partially examined the case, and there may possibly be a difference of opinion about it, I hold it good to bring it thus early under your notice. I have just come from her bedside, having seen her with you for a very few minutes only both yesterday and to-day ; in fact, she cannot bear any lengthened examination, and we are obliged to take very summary views of her case on this account. You must often be content in practice to remain ignorant of things which you might by possibility know, but which you could not know without doing more mischief than the knowledge is worth ; and I think I shall rest satisfied in the meantime with what I have been able to gather in these few minutes about Christina M. She

* This case is not included in the synopsis at p. 607, as it belongs to the experience of a previous session. The lecture was reported by Dr. Duggan.

is a woman about thirty-eight years of age, and has evidently been ill for some time. She suffers under great dyspnœa; she has to be propped up in bed, and the breathing is hurried and anxious; at the same time there is not any lividity, but rather extreme pallor—a sort of cachectic pallor, with a slight tendency to œdema of the eyelids; the lower part of the body is dropsical to a considerable extent; there is a good deal of wheezing in the chest, but not so much as to account for the very serious dyspnœa; finally, there is diarrhœa, and it has lasted about three months. These are the most obvious facts, and it does not take many moments for the skilled eye and ear to discover them almost without a question. When I was introduced to this case as one of valvular disease of the heart, I thought there was some mistake, and I still think that the cardiac phenomena are probably altogether secondary to the disease of the kidneys, and perhaps of other excretory organs. The assimilating and blood-making function is deeply involved in this case, and you observed accordingly that almost on the first glance I asked at once after the state of the urine. It was found to be highly albuminous; there is Bright's disease, and this is, if I mistake not, the leading fact in the diagnosis. The state of the breathing was, however, suggestive enough of cardiac disorder, and I was not much surprised when Dr. Bell told me he had detected a murmur the evening before last. I examined the heart accordingly, and found—no murmur. Dr. Bell repeated the examination after me, and was himself satisfied that the murmur which he had

heard so distinctly the evening before, and regarded as one of aortic valve-disease, was gone. Now, from Dr. Bell's careful previous study of these subjects, and from what we know of his experience and skill, I think we can place entire confidence in his statement as to the fact of the murmur. I don't say we should take for granted his conclusion, and, frankly, I am of opinion, speaking for myself, that his conclusion was wrong; but I think we must give full effect to his account of the audible phe-

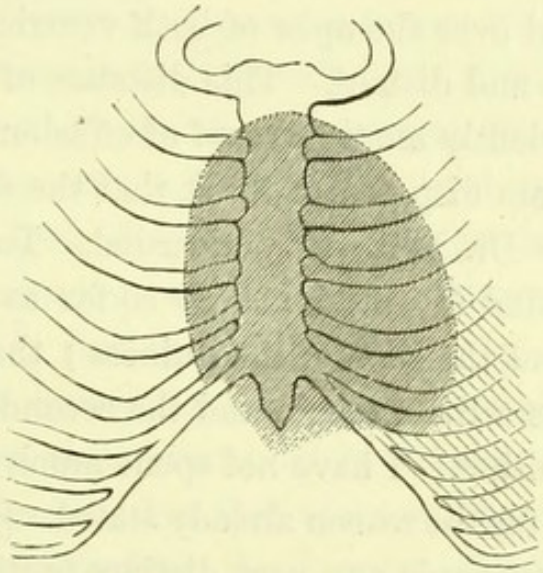

Fig. 30.

Limits of the præcordial dulness in the case of Christina M. The lower and left margins are undefined, owing to their being inseparable from the dull percussion of the abdomen and of the left pleura.

nomena. There was a murmur, then—a double murmur, too—last evening but one, and it was gone the next day. From its rapid disappearance, I think it could hardly have been an aortic murmur. What was it, then? I noticed to you at the first visit the peculiar

character of the dyspnœa in this woman ; the breathing rapid, not laborious, but gasping, and apparently checked by some half-voluntary impulse. I began instantly to suspect pericarditis, such as we often have in Bright's disease ; and on percussion of the front of the thorax we easily detected evidence of large pericardial effusion, which, however, is evidently complicated with pleuritic effusion on the left side, obscuring the limits of the dulness. The heart's sounds, too, are pretty distinct at the manubrium sterni, but lower down they rapidly become indistinct, and over the apex of both ventricles they are quite obscure and distant. This distance of the heart's sounds is evidently another proof of effusion in the pericardium. I am disposed to think that the double murmur heard by Dr. Bell was pericardial. To-day I hear an altered sound (I would not go so far as to call it a murmur) about the edge of the dulness ; the first sound of the heart is a little rough, and the second is not altogether right either. I have not spent much time on the examination, for the reason already stated. [There never was heard afterwards any very distinct friction murmur, but repeatedly an alteration of the sounds as stated above.]

You may think that it is too much to suppose that an exocardial was mistaken for an endocardial murmur ; or at all events that it is a strong thing for me to set a judgment based upon inference against a direct judgment of the ear upon audible facts. But I am not in the habit of speaking without book in these matters, and accordingly I will give you a few particulars of a case that occurred to me in 1853, and which, amongst others,

has convinced me that very well trained ears may mistake an exocardial for an endocardial murmur after a much greater amount of observation than Dr. Bell was able to give to this case. The case I refer to was under the care of a very able physician for a month some time before I saw it, and many curious and diligent ears besides his had been engaged upon the diagnosis. After I had made up my mind that there was pericardial effusion and a friction murmur, I was told for the first time that there had been a valvular and endocardial murmur before. The controversy remained open till the patient's death, and several of those who had formerly seen the patient examined her in my presence, the results of the different observations being curiously at variance. After the patient died, there was the greatest interest shewn in the examination of the body. Many of those who had seen her during life attended; and in the end I succeeded in convincing all who were present that there had not been, and never could have been, an endocardial valvular murmur; but that there had been two distinct attacks of pericarditis, the former of which probably corresponded with the first admission of the patient into the hospital, and the latter with her fatal illness. Ever since this case (and for some time before), I have been slow to admit the infallibility of even well-educated ears in regard to the distinction of exocardial and endocardial murmurs.

* The full details of this case, and of others bearing on the diagnosis in question, will be found in the memoir on pericarditis formerly referred to, pp. 11-19; and particularly p. 15, case of A. P.

[The progress of Christina M.'s case justified the diagnosis. Contrary to my expectations, this formidable pericardial effusion subsided pretty rapidly under diuretics (squill and infusion of digitalis); although these had to be managed very charily on account of the tendency to diarrhœa. I have no doubt, indeed, that the effusion was in the main dropsical, though attended probably with enough of fibrin to give rise to an evanescent murmur. For a long time, however, after the pericardium had been relieved, the abdomen and legs continued more distended than ever; the latter, indeed, became quite enormously enlarged, and extremely dense and indurated from effusion. Accordingly, after trying every possible form of diuretic and also acupuncture of the limbs ineffectually, it was determined to perform tapping of the abdomen. The operation removed a large quantity of rather turbid fluid, with very favourable result; for the kidneys, being relieved from pressure, began to act again under diuretics, and the dropsy of the limbs was surprisingly diminished, considering that they had become so enormous and so indurated as to resemble closely the state represented as elephantiasis. In the end, after a second tapping of the abdomen, the patient left the hospital in greatly improved health and spirits, and nearly, if not altogether, free from embarrassment in her breathing. I think it exceedingly probable that there is disease of the liver, as well as of the kidney, in this case.]

XIX.

RETROSPECT OF 200 CASES UNDER TREATMENT IN THE ROYAL INFIRMARY DURING THE WINTER SESSION 1859-60.*

THE season has been, on the whole, a healthy one, although the month of January was attended by an increase in the rate of mortality. Erysipelas was rather prevalent during the autumn, and also diarrhœa occasionally assuming a dysenteric character. Two of the nurses in the female wards were laid up at the beginning of the winter with slight dysenteric symptoms. Epidemic fevers, with the exception of small-pox (which is not admitted to the wards under my care), have been few, except at the beginning of the period. Acute inflammations of all kinds have been few, and, on the

* The cases here mentioned were the entire number of those admitted from the beginning of November to the middle of February. This article was at first published in the Edinburgh Medical Journal for April and May 1860, and all the facts were carefully revised by Dr. Shearer (now of Liverpool), who was then resident physician, and gave me most important assistance throughout in preparing this report. Some additions have been made within brackets and otherwise, derived from more recent experience of several of the cases referred to.

whole, of moderate character. A considerable number of cases of acute nervous disorders, most of them the result of intemperance, presented themselves about the beginning of the year. A great majority, however, of the cases admitted to treatment were instances of severe chronic diseases, often with subacute exacerbations; and of these, a large proportion were cases of organic disease, admitting only of palliative treatment.

It is necessary to remark, at the outset, that the following classification of the cases is not intended to subserve any precise statistical or systematic purpose, but only to form a foundation for a few remarks on the more interesting occurrences of the period under review.

I. *Phthisis Pulmonalis* (including remarks on *Empyema*).—Tubercular phthisis carried off more than its usual large proportion of victims. Of twenty-two cases of well-marked phthisis pulmonalis—*i.e.*, cases with an unequivocal diagnosis of excavated tubercle in the lungs—exactly one-half have proved fatal. But in order to make the list of presumed or possible tubercular cases complete, it would be necessary to add an uncertain proportion of the catarrhal cases, and perhaps also of those of diarrhœa, and of various organic diseases. One case of unquestionable tubercle of the lungs (not included in the number above-mentioned) occurs among the diseases of the nervous system. Of the twenty-two well-marked cases, fifteen were females and seven males. The left lung was mainly or exclusively affected in three; the right lung in six; both lungs in twelve. In seven of

the cases there was diarrhœa, so marked and persistent as to form a leading feature of the disease.

Rapid Phthisis.

Four of the cases of phthisis were very acute in their progress; so much so, indeed, as to resemble, more or less, the inflammatory affections of the chest, or the continued fevers. In one of these cases (Elizabeth A., æt. 19), the diagnosis was complicated by the fact that the tubercular disease was rapidly developed during the progress of cicatrization of the ulcers of enteric fever; and that it was attended by pneumothorax and empyema, the symptoms of which were unusually obscure. [See p. 388 for further remarks on this case.] In another of the acute cases (David C.) the history and symptoms were very much those of pneumonia of the upper lobe of the right lung. The patient was a young man, who had presented no symptom of illness or of delicacy of constitution till six weeks before admission. On admission, however, the whole upper lobe of the right lung was found greatly condensed, and probably excavated; and the lower lobe appeared also partially condensed, probably from pleurisy. The disease in this case ran a course of less than three months without involving the other lung till the very end; the signs of excavation becoming more distinct, and the strength and flesh gradually declining throughout, notwithstanding a carefully regulated diet and the administration of tonics. Cod-liver oil could not be taken. At last an acute exacerbation (probably pleuritic), attended with severe pain, cut him off in a few hours. [It is not without interest, as a sequel to this

case of acute phthisis, to remark that his wife, a remarkably fine-looking and healthy young woman, to whom he had been recently married, gave birth a few months after his death to a child which died within eight or nine months afterwards.]

Extreme Disease of One Lung.

Two cases were remarkable on account of their chronic character, and the extreme disorganization of one lung. In one of these (Jane E.) a tubercular affection of two years' standing ended in an immense excavation of the left lung, no part of which was capable of sustaining respiration. There were also suspicions of pneumothorax ; and, at all events, there had certainly been an attack of pleuritic effusion ending in complete consolidation of the left lung about a year before death. Notwithstanding this the patient lingered on, frequently improving very much under treatment. In the end she succumbed to the combination of pulmonary and enteric phthisis, with continuous albuminuria having all the characters of Bright's disease. In another case (Janet M'D.) the patient has, some weeks ago, left the hospital much improved, for the third or fourth time, and was, when dismissed, actually better than she had been for some years, although there have long been signs of a very large cavity at the back part of the left lung,—large enough, indeed, to give rise to metallic tinkling, and *almost* amphoric breathing. The left side is also extremely retracted, and the lung seems packed up into the upper and back part of the chest, having displaced the heart upwards, so that the apparent apex-beat

Repeated partial Recovery.

is under the third rib instead of the fifth. This phenomenon was also present in the case of Jane E. It must be added, as bearing upon the diagnosis and prognosis of these cases, that there has almost certainly been empyema, followed by absorption of the fluid and contraction of the side. It is therefore not absolutely certain that the cavity is intrapulmonary and tubercular, though the tubercular character of the case is extremely probable on other grounds.* I am also of opinion that there is adherent pericardium in this case; or at least considerable roughness of the pericardium, the result of pericarditis, which probably originated at the same time as the empyema. [This patient still survives (June 1862), and I think there is even evidence of contraction of the cavity. She still suffers a good deal, but the general health is not materially worse than in 1860, and she prefers to remain at home, where she does a little sewing.]

Displacement of Heart.

This is perhaps the most convenient place to advert to another case of *empyema*, which I have not, however, included among the phthisical cases. Bridget K., a married woman with a family, received some ill-usage from her husband during her last pregnancy, the result of which was a miscarriage, followed by considerable hæmorrhage, for which she was treated in the poor's-house for ten weeks in the beginning of 1859. Within a fortnight after leaving the poor's-house, being much debilitated, she fell ill with a pain in the

Case of Empyema.

* Compare cases I. and II. of art. XVI., p. 410; and remarks in Appendix on Articles XIV. and XV.

side, and after struggling with this for some days was admitted into the Infirmary almost exactly a year ago. At that time she had severe fever, with a rapid small pulse, dry tongue, and great prostration; not much difficulty of breathing in the recumbent posture, but still a degree of pain; she was emaciated, and had some cough, but without any expectoration. In the lower part of the right lateral region (the seat of pain), over about a hand's-breadth, there was dulness on percussion; respiration was here absent: elsewhere it was weak, but not suppressed; it was especially weak in the back, which was more or less dull on percussion throughout. There was a little indistinct crepitus about the borders of the dull part; elsewhere no râle. The dull space was slightly prominent, but there was no general distension of the side, nor protrusion of the intercostal spaces; the movement of the right side was restricted, as compared with the left; the liver was a little depressed. The apex of the right lung was carefully examined, but gave no sign of a cavity; the left lung was normal to auscultation and percussion throughout its whole extent. The patient took various remedies, including mercury and opium, without effect upon the disease. Accordingly, in lecturing upon the case, I indicated my opinion that there was a limited empyema, which would probably find its way either outwards to the surface, or inwards towards the lung; but I said also, that from the limitation of the effusion, and the absence of urgent symptoms of respiratory oppression, I was induced to refrain from active interference, and that the ultimate issue of the

disease would probably be ruled more by the presence or absence of tubercle, or of disease of the bones, than by the precise mode of opening : at all events, that an operation might probably be followed by very imperfect evacuation of the fluid, and by putrefaction of what remained ; on which grounds I thought it not advisable to operate. Within a fortnight the occurrence of an immense and sudden expectoration of pus shewed that an internal opening had in fact occurred. Since this period the patient has had repeated returns of purulent expectoration ; pint after pint of pus has been brought up, at first with intervals of relief, and periods of reaccumulation accompanied by sense of increasing oppression. Of late the flow has become more uniform, the alternations of oppression and relief having given way to a condition of great exhaustion, but of comparatively little suffering ; the patient has become emaciated to the last degree ; the fingers are clubbed at the ends, and the nails extremely curved ; she has frequent returns of feverishness, but has a pretty good appetite, and no considerable dyspnœa. *The sputum has never been in the slightest degree putrid, or even of disagreeable odour, neither has it been at any time bloody.* The left lung remains apparently normal. The right is pretty extensively lost to respiration, especially at the back part ; but a little breath-sound may still be heard over the greater part of it, and there is no sign of cavity, even over the seat of the former dulness on percussion in the lateral region, which has now become rather indistinct. At no period in the history of the case has there been the slightest

approach to metallic phenomena, or to succussion-sound. I therefore infer that in this case a large and continuous, though limited, suppuration in connection with the pleura, is finding its way through the lung *without the regurgitation of a single bubble of air into the suppurating cavity.**

To return to the phthisical cases. Another life was prolonged under the most unpromising circumstances, but in this case by an operative proceeding of the most decided character.

Case of Laryngeal Phthisis.

Elizabeth S., a girl of very emaciated appearance, exhibiting hardly any indications of puberty, and stating that she had never menstruated, came under my care for the first time in the spring of 1858, affected with ulcerative sore-throat, which had much of the appearance of being syphilitic, but of which no distinct history could be procured at the time. She was at that time about 17 years of age, and had been ill for more than a year. She said that she had never had any eruption on the skin, and seemed to consider the sore throat as the result of "a cold." Under a course of local and general treatment, chiefly by iodine and cod-liver oil, she improved in some degree, and left the hospital—only, however, to return fourteen months afterwards, in a far worse condition. On the 3d of June 1859, she was admitted at the hour of visit. Her appearance indicated the last degree of prostration: the

* I doubt if this inference can be sustained. The facts are as stated; but it seemed probable, nevertheless, from the examination of the body on this woman's death some time afterwards, that the absence of metallic phenomena was not due to the absence of air in the suppurating cavity, which was freely open to the lung.

pulse could scarcely be felt at the wrist; there was extreme pallor of the surface, with a trace of lividity of the lips; the respiration was much embarrassed, with distinct laryngeal stridor; there was orthopnœa; the patient breathed with open mouth, and with heavy effort. Notwithstanding this, the chest hardly expanded, and respiratory murmur was all but inaudible; cough was not severe, but performed with imperfect closure of the glottis; there was pretty copious expectoration of intensely purulent and globular sputa.

There was only too much reason to fear that an operation in this case would be unsuccessful. In fact, the characters of the expectoration, and of the case in general, were those of advanced phthisis. But, on the other hand, there was a positive and manifest source of immediate danger in the state of the larynx; and the state of the chest was rather doubtful, there being no positive physical signs either of condensation or of excavation of the lung. I therefore determined at once to sanction tracheotomy, if Mr. Spence was willing to perform it; and accordingly, at a quarter past one, Mr. Spence saw the patient with me, and, after a careful examination of the throat, decided to operate immediately. There was, indeed, no time to be lost.

Tracheotomy.

The operation was performed at half-past one. It was a narrow escape from instant death. The heart's action was almost exhausted before the first steps of the operation were over; and although very little blood was lost, the small quantity which necessarily made its way into the trachea was not coughed

up again until the mucous membrane was tickled with a feather. Hardly any efforts to breathe followed for some seconds; the pulse fluttered and was lost; the countenance was deadly pale. At last, under compression of the thorax, tickling of the tracheal mucous membrane, and the administration of strong beef tea and wine by enema (as she was unable to swallow), we had the satisfaction of seeing the patient revive sufficiently to breathe freely, and to cough up a large amount of pus, mixed with a little blood. She was not considered safe, however, from the recurrence of fainting for several hours. During the afternoon she was disposed to sleep, and at a quarter past ten P.M. I was greatly relieved to find that she had been enjoying quiet and refreshing, though light slumbers, for several hours, broken only by short paroxysms of cough, each of which was accompanied by the free expectoration of pus.

From this time onwards to the 25th August, when she was dismissed in a greatly improved state of health, there was absolutely no unfavourable symptom. No treatment was employed beyond a nourishing diet, with a proportion of wine. A degree of purulent expectoration continued, but the respiratory murmur in the apices was pretty good, and no distinct signs of cavity could be discovered anywhere, although bubbling râles were pretty general.

On the 4th of January this patient returned for the third time, wearing the tracheotomy tube, but apparently breathing a good deal also through the larynx. Now, however, it was very evident that the thoracic disease

had made great progress. There was copious expectoration of pus, which had almost a gangrenous fœtor; there was also exhausting diarrhœa; and emaciation had increased, even as compared with the most extreme degree it had formerly reached. The respiration, however, was not at all difficult. The percussion of the front of the chest was good on both sides; the breath-sound full on the left front, less so on the right, in both fronts accompanied by bubbling and snoring râles. In the supra-scapular space on the left side there was slight dulness on percussion; but the chief alteration was about the middle of the right back, near the spine, where there were very distinct signs of a considerable cavity. In the lower part of the right back the respiration was suppressed.

It was now very evident that this patient was sinking under her obstinate and uncontrollable disease. Yet I did not by any means feel quite sure that the disease was tubercular. The comparative soundness of the upper lobes of the lungs, the gangrenous character of the excavation, and the history of the throat affection, seemed to throw doubts on the diagnosis in this respect. The *post-mortem* examination, in fact, shewed that the tubercles had an extremely peculiar distribution through the lung; and further, that tubercle, though present, and though possibly present throughout the whole course of the disease, had not had the chief share in bringing about the fatal termination. The posterior part of the right lung adhered very firmly to the mediastinum and to the œsophagus; a large gangrenous cavity, close to the surface,

having almost opened into the cellular tissue. The disease in the larynx had mostly subsided, leaving cicatrices along the margin of the epiglottis, and some thickening of the mucous membrane in the ventricles of the larynx; the trachea also was the seat of two semi-cicatrized ulcerations. The other organs were mostly quite normal.

The genital organs were very carefully examined, with a view to the evidence of syphilis. The hymen was gone, but there was not a trace of ulceration, nor of any visible cicatrix on the labia, vaginal mucous membrane, or os uteri. The uterus itself, and the ovaries, were those of a girl before puberty. There was no bubo, or induration of any kind, in either groin.

Mr. Spence is under the impression that this girl had previously been under his care on account of condylomata. But whatever be the pathology of the case, I have thought it worthy of being recorded in detail, as an encouragement to the performance of tracheotomy, even under certain very unfavourable circumstances, as I think there can be no reasonable doubt that in this case the operation prolonged life fully more than seven months.

A few remaining particulars will close the narrative of the phthisical cases.

Two cases of phthisis were complicated by severe hæmoptysis. Of these one has temporarily recovered; the other died. This patient (Peter R.), a lad of nineteen years of age, had been ill for eighteen months, and was evidently far advanced in the disease on admission. He was extremely emaciated and pale, and the expec-

toration indicated a rapid softening and excavation of the tubercles. On the occurrence of severe hæmoptysis for the first time, I prescribed Gallic acid in ℈j. doses every hour. It failed altogether in producing any effect upon the bleeding. It was then replaced by acetate of lead and opium, which, for a time, appeared to be very successful. In the end, however, the hemorrhage again gained the mastery, and produced a somewhat sudden fatal result.

Two cases (one of which is mentioned above) were complicated with well-marked Bright's disease. Several had more or less enlargement of the liver.

In one case the disease was the result of diabetes mellitus.

Two only shewed any notable disturbance of the intellect or nervous system. One of these was a woman (Bridget M.), in no very advanced stage of the disease, who was affected with a low form of maniacal derangement, and had to be sent to Morningside Asylum on account of a suicidal tendency. The other was a very remarkable case, which will be noticed further on in connection with the disorders of the circulation. (Case of Margaret M'K., p. 656.)

I have little to say about the treatment of phthisis in general. No special therapeutical experiment has been in progress during the last three months. I will therefore pass by this subject for the present.

II. *Catarrhal and Pneumonic Affections.*—Very few cases of typical acute pneumonia have presented them-

selves during the three months. Indeed, with the exception of one well-marked case of pneumonia of the upper lobe in a boy (Robert N.), of ten years old, very rapidly resolved under a few doses of antimony, followed by an expectant treatment, there is hardly a case of acute pulmonary condensation not obviously complicated either with other pulmonary affections or with constitutional disease. In one patient a slight pleuro-pneumonia of the right base occurred as a complication of gonorrhœa, with double bubo, and orchitis, and was easily subdued under simple treatment. In two other patients similarly slight condensations occurred without obvious complication, but were altogether mild in character. In one or two cases typhus fever was complicated by very severe chest affection, having more or less of the pneumonic character. One patient (a cattle dealer), an extremely intemperate man by his own confession, had contracted a very acute affection of the chest, which, aggravated by intoxication and extreme neglect of the most ordinary precautions, became at an early period accompanied by intensely fœtid expectoration. He had the extraordinary imprudence to attend Hallow Fair during the second week of this illness, which proved to be a very extensive gangrene of the base of the right lung, of four weeks standing at the time of his admission to hospital. This case was fatal by profuse hemorrhage. Several cases of pulmonary condensation, mostly hemorrhagic, occurred in connection with heart disease. Finally, tubercular diseases have sometimes, as already

Case of Gangrene of the Lungs.

remarked, taken on to a marked extent the pneumonic form. One case, which I believed to be of this kind, though I have refrained from including it among the cases of phthisis pulmonalis (as not being beyond question a tubercular case), is a remarkable instance of a partial recovery under circumstances of the most unpromising character. The patient (Janet O.), having been not long before under treatment for pretty severe bronchitis, was re-admitted almost *in extremis*, with a cold sweat on the brow, and an almost inappreciable pulse, numbering 124. The face was flushed and livid, the rest of the body deadly pale and cold. There was severe cough and much expectoration of a frothy character, muco-purulent, with a good deal of rusty colour. There were signs of general bronchitis, as before; but, in addition, the upper lobe of the left lung was entirely condensed, and signs very suspiciously cavernous in character were heard under the left clavicle, about the third intercostal space, and also at the apex behind (viz., the highest degree of tubular breathing, almost amphoric, behind; and coarse crackling râle, very loud, with a distinct metallic after-tone, and with nearly perfect pectoriloquy in front, together with cracked-pot sound on percussion in the greatest perfection). These signs continued unchanged for a time, even while the patient was manifestly improving under treatment. Ultimately they have almost all diminished in intensity, especially the râles, which have become comparatively insignificant; breath-sound has also returned, though feebly,

Case of Tubercular (?) Pneumonia.

over a considerable portion of the left upper lobe, which has also become less dull on percussion, especially in the supra-scapular space. The cracked-pot sound has ceased to be educible, except, perhaps, on extremely strong percussion. On the other hand, the limits of dulness in front are unaltered, though its degree is somewhat less; there are still metallic, or almost metallic, phenomena about the level of the second rib; and the breath-sound is still very tubular there, and in the neighbourhood of the spine behind. The vocal resonance has lost much of its extreme exaggeration and pectoriloquous character, but is somewhat ægophonic over the second rib in front.*

Without presuming too much upon stethoscopic refinements, I think I am warranted in regarding this case as one, not only of condensation, but of partial excavation of the upper lobe of the left lung, supervening on a pulmonary affection of some standing, with catarrhal signs. Had the patient died within three or four days after admission, no one could have hesitated in regarding it as a tubercular lesion. The recovery, however, even though partial, and the considerable amount of positive restoration of the disorganized texture, will induce a cautious physican to suspend his

* The lapse of time enables me to add a very interesting fact to this curious history — viz., that more than a year afterwards this patient presented herself in a complicated state of bad health, and having still traces of chest affection, but without a single sign of excavation, or very marked condensation, over the seat of the changes described above. I still suspect tubercular disease; but it has become nearly latent as to physical signs.

opinion. And yet I do not doubt, *as a matter of opinion* (setting aside the question of individual cases), that I have often seen a pneumonic affection, implanted on a nucleus of tubercular disease, undergo, even after excavation, as good a cure as this case. I believe, in fact, that (especially in children) the apparent completeness of the recovery after a tubercular attack often shuts our eyes to the true nature of the disease.

Use of Antimony.

In this case, as in several others of similar character, which have occurred to me, I ventured, notwithstanding the extreme weakness and exhaustion of the patient, upon the administration of tartar-emetic in small doses, along with diffusible stimulants; and was rewarded by seeing the remedy produce its best effects, viz., a therapeutic, without the least trace of a physiological, action. The dose should rarely exceed $\frac{1}{10}$ or even $\frac{1}{12}$ of a grain to begin with in such cases, sometimes even less. But, on the other hand, it is sometimes so well borne, that I have given $\frac{1}{2}$ gr. and even 1 gr. doses to patients who were so weak as to be utterly unable to raise themselves in bed; and this without the slightest disagreeable effect of any kind. In general, I regard the ordinary physiological effects of antimony as *quite opposed to its therapeutic action;* and whenever they occur, I make it a rule either to suspend the remedy or to diminish the dose — believing it to be, on the whole, much safer to forego the possible advantage of the antimonial medication, than to run the risk of superinducing the least degree of poisonous action.

Among the catarrhal affections of the chest but few cases require any special notice. I have records of twenty-three or twenty-four cases which may be placed under the head of Bronchitis, or Chronic Catarrh, with or without Emphysema of the lungs. As I have said, however, an uncertain proportion of these cases may probably have been tubercular. Among others, the case of Michael D. recurs to my recollection. This man was a hawker, thirty-five years of age, and had been all his life extremely addicted to whisky, which he took regularly, when in tolerable health, to the extent of several gills a day, apparently without the least idea that he was doing himself anything but good; being, as he remarked, "a seasoned vessel." The symptoms and physical signs were those of bronchitis, modified by the peculiarities of the spirit-drinker's habit; but there were suspicious indications near the spine of limited condensation of the lungs, and the history shewed that he had been subject to chest disorders from an early period; also that he had on two occasions had extremely violent hæmoptysis, and that he had not unfrequently had attacks of diarrhœa. I kept him in the ward for some time after his recovery, chiefly in order to convince him that he could both get well and keep well without drinking three gills of spirits a day.

Bronchitis.

Of our twenty-three or twenty-four cases of bronchitis all recovered, at least partially, except two. Seven of the cases were complicated with very marked emphysema; nevertheless, they all made wonderfully good recoveries with one exception,

in which there was also a very much weakened heart. This case proved fatal. The other fatal case of bronchitis was that of C. P., a hunchback, with an extremely circumscribed thoracic and abdominal cavity, as compared with the size of the head and extremities. This man was twice admitted during the three months; on both occasions with an amount of catarrh which would hardly have interfered with the avocations of a well-formed man, but which was sufficient in him to cause the most extreme lividity, and almost entirely to annihilate the pulse at the wrist, while the heart, and especially the right ventricle, continued to beat with great force immediately beneath the costal cartilages. There was no evidence of emphysema in this case. I repeatedly pointed out at the bedside the contrast between the weak cardiac sounds and moderately good pulse of emphysematous patients, and the strong cardiac sounds and weak oppressed pulse of this man, labouring as he did, under pure cyanotic acute bronchitis. It was quite evident that the danger was simply mechanical. The diaphragm and ribs could not act effectively, as there was no room for the organs. Fetid injections, however, relieved the bowels of some flatus; and diffusible stimulants, with diuretics (especially cream of tartar carried up to purgative doses), brought about a rapid cure on the first occasion of this patient's admission. He was dismissed with a caution, but nevertheless neglected himself so much in his second attack, that when admitted, very late in the disease, it was quite plain he

Case of Bronchitis with spinal Deformity.

had only a few hours to live. The deformity proceeded from old healed caries of the spine, with extreme angular curvature. All the organs were quite normal, except the lungs, and perhaps the heart, which was very slightly hypertrophied on the right side, and much distended with dark blood.

III. *Diseases of the Heart and Great Vessels, including Aneurisms.*—Sixteen cases in all may be fairly included in this series, without reckoning cardiac murmurs unattended by special symptoms of disorder of the circulation. Of these, five were fatal. The leading details of the sixteen cases may be thus stated:—

In three cases, the mitral orifice was chiefly or exclusively the seat of a murmur: two of these cases being regurgitant, and one obstructive disease. The patient who is the subject of mitral obstruction (William L.) has the characteristic auricular-systolic murmur (pp. 575, 599) in its most marked form. He has also moderate hypertrophy of the right ventricle; but, on the whole, the disease is remarkably free from grave complications, and is productive of only slight suffering; it is of long standing, and very probably rheumatic. [William L. died in September 1860, under an accidental attack of bronchitis. The heart weighed 13 ounces. The right ventricle and the tricuspid orifice were dilated. The left ventricle was small. The mitral orifice just admitted the point of the little finger, but its margins were smooth and the valve appeared capable of closing.] Neither of the two cases of mitral regurgitation has

proved fatal, and one of them went out, after a short residence, much improved. The other patient, a very anæmic and enfeebled woman (Barbara C.), is still under treatment, and suffers exceedingly from palpitation and hæmoptysis. There is also considerable anasarca, and the kidney is affected with advanced Bright's disease. The patient has been accustomed to take large quantities of opium, and finds it necessary at present to have from one to three ounces of solution of morphia daily to procure rest from her distressing symptoms. [Barbara C. died very shortly after this report was drawn up. The heart weighed 16 ounces. The mitral orifice was contracted and rough with vegetations, which prevented its closure. It is difficult to be sure whether the obstruction-murmur existed in this case ; ordinarily it was lost in the regurgitation-murmur, which was loud and obtrusive. Possibly there may also have been tricuspid regurgitation, but it was not distinctly indicated. (Similar cases at p. 605.)]

In two cases there was a murmur chiefly referrible to the right side of the heart (tricuspid regurgitation), but complicated in one, probably in both, with mitral regurgitant disease. [One or two instances of indistinct or temporary murmurs of this kind were not included in this report.] In both these cases there was copious pulmonary hemorrhage. In one of them there was extreme cyanosis, with moderate venous pulsation ; in the other, the cyanosis was moderate, but the venous pulsation intense. Both cases ended fatally: one of them, however, only after repeated partial recoveries under diuretics

and expectorants. [Of one of these cases I am unable to record the result. The other was a very remarkable instance of tricuspid insufficiency from vegetations on the valve, referred to at p. 604. The patient, Mary P., æt. 12, had originally been affected with mitral disease, from which she had partially recovered, notwithstanding numerous attacks of bronchitis, one of which is referred to in the chapter on Influenza (p. 97). She had also repeated and considerable hemorrhages from the lungs in the course of her disease, which lasted for several years. Ultimately, she became extremely cyanotic and permanently dropsical; and about the same time the murmur assumed the characters of tricuspid regurgitation.]

In three cases there was a double murmur referrible to the aortic orifice, or aorta; in one, if not two of these, there is aneurism of the ascending aorta. In the third case (John W.), the murmur is of very recent origin (probably not more than eight weeks' standing), not due to rheumatism. I think it not improbable that there has been a rupture of the valve in this man. The symptoms, on admission, were extremely threatening. The patient believed he had "caught cold," but was disappointed at not having received the speedy relief he expected, and was advised to come into the Infirmary. He was found to have a fluttering pulse, with extreme irregularity of the heart's action, especially under excitement, and most alarming paroxysms of suffocative angina recurring every half-hour, sometimes every few minutes. In addition, the left back was absolutely dull to percussion

Recent Aortic valve Disease. Angina Pectoris.

over the lower third of the lung, and the patient was expectorating a considerable quantity of blood mixed with frothy mucus. The heart was not materially enlarged. Shortly after admission dropsy occurred in the feet and limbs, and the suffocative paroxysms rather increased than diminished in severity. By the persistent use of opium, however, with chlorodyne, and afterwards chloric ether and diuretics, all these symptoms have been greatly relieved. The patient now walks about the ward with considerable comfort, and has lost altogether the feeling of impending death. It is probable that he may soon be dismissed with safety for a time, though assuredly he will return at no distant period, with his heart more manifestly enlarged. [I have seldom seen a more manifest rescue from impending death than in this case. He continued for some time in an improved state of health, but I am almost positive that I heard of his death recently, without any particulars.]

There were two cases of disturbed action of the heart, without murmur, in regard to the precise character of which I did not feel warranted in coming to any conclusion.

Aneurism of Aorta.

Six cases presented such symptoms and signs as led me to infer aneurism of the aorta—in some with certainty, in others as an extremely probable, though not absolutely certain, diagnosis. Of these cases, not less than two were in women—an unusually large proportion. Two only ended in death; one had no dangerous symptoms even on admission, having been supposed (erroneously)

to labour under fever, and being dismissed in a few days; the other three were cases of great urgency at the time of admission, and all of them received great and striking, though, of course, probably only temporary relief. The following particulars may probably be found interesting:

Mary M., æt. about 24, admitted as a case of fever, affected with cough, and some slight degree of palpitation. Physical signs of dilated aorta with probable aneurism. No urgency. Patient dismissed in a few days.

Mary L., æt. 40, a rather feeble and emaciated woman, admitted 2d December, with hollow-toned noisy respiration, such as is commonly observed in cases of pressure on the trachea. More or less dyspnœa on exertion had existed for about six months; latterly much aggravated. Pain was complained of in back and shoulders, sometimes severe; uneasy sensation also, not amounting to pain (nor distinctly definable), about the upper part of the sternum, and in direction of throat. Cough moderate; voice a little choked. Inspirations laboured, 20 to 25 in the minute; expansion of chest perfect; percussion a little dull at left apex behind, and perhaps also at left sterno-clavicular articulation. Obscure pulsation, deep in jugular fossa; trachea deep in the neck, but quite mobile. No decided alteration of cardiac or vascular sounds. Expectoration of frothy, rather tenacious mucus, streaked with blood. Under rest, expectorants, and chlorodyne, which in this case answered well, the patient improved so much as to be able to leave the house on the 3d of January.*

* Compare cases I. II. III. and X., art. XVII.

Roderick R., æt. 52, a man of robust frame, but somewhat debilitated, admitted 18th April 1859, for an illness of ten weeks' standing, supposed by himself to be a cold. Dismissed, much improved, on June 10th. Re-admitted October 31st. The symptoms and signs in this man were very gradually developed, but in the end left almost no doubt of the existence of aneurism or tumour in the chest, which was the view taken by me of the case on his first admission. The earliest symptoms were — paroxysmal, excessively hoarse cough, with dyspnœa, at first without expectoration, afterwards with a frothy mucous sputum, slightly streaked with blood. The physical signs were negative, except that the radial arteries were twisted and rather rigid, and that there was slight *arcus senilis*, the right pupil being just perceptibly smaller than the left. On the second admission these symptoms continued; there was, however, an increase of dyspnœa, and more expectoration, still with a trace of blood, and frothy, not purulent. The respiration was slightly laryngeal, and the voice was perceptibly, though not greatly affected. The cough was more decidedly paroxysmal, and was followed by marked lividity. About the beginning of November, it began to be observed that the cough was *imperfect* (the glottis not closing completely), and that the voice was liable to sudden though momentary suppression. This was not explained by anything in the larynx, which to the finger appeared perfectly normal. Pain, which was severe in coughing, was referred mostly to

Latent Aneurism.

the hypochonders and lower sternum. About the middle of November, the symptoms were still becoming aggravated; in addition, the patient was almost constantly sick, vomited most of his food, and had sour eructations; he could only obtain rest when propped up in bed, and sometimes when lying on the right side. The expectoration was very copious, sometimes amounting to more than half-a-pint in the day, and almost constantly contained more or less of blood. It now also began to be more decidedly observed (what had, however, been noted from the beginning of November), that the right lung admitted air less freely than the left, throughout; and that the percussion was rather flat, and the expansion of the chest less full, on the right side.*

I now regarded the diagnosis as established; it was a tumour, in all probability an aneurism, in contact with the right bronchus or lung. The state of the patient was obviously perilous in the extreme, as the symptoms were increasing in violence, in spite of a great variety of palliative treatment; when he derived sudden and well-marked relief from the administration of a remedy which might, perhaps, have been regarded as a dangerous one in the circumstances—viz., an emetic of tartrate of antimony and ipecacuanha. The emetic was given by Dr. Shearer at an evening visit, with a view of relieving the state of supposed congestion of the tracheal mucous membrane. The result fully justified the proceeding. The

Emetics in Aneurism.

* Compare cases I. IV. VII. and VIII. in art. XVII; also case of Mary M'D., p. 611.

relief was immediate; and on two occasions the patient has repeated the remedy, with the effect of producing a marked and continued abatement of cough and expectoration. He was dismissed from the ward much relieved, on December 31st, and has been seen a few days ago continuing pretty well. Blood, however, was never long altogether absent from sputa. [This man died in July 1860 (after another long residence in the hospital) at his own house. Unfortunately, dissection could not be obtained, but the circumstance of his death by profuse hæmoptysis leaves no reasonable doubt that the disease was aneurism. There was no new symptom.]

Matthew C., æt. 26. Symptoms of more than four years' standing. This is a complicated case, mentioned above as one of those with an aortic double murmur. The heart is enormously enlarged, the apex beating as low as the 8th rib, and far to the left. There is likewise, so plainly as to admit of no doubt, great dilatation of the arch of the aorta, and probably a sacculated aneurism. The right pupil is persistently a little smaller than the left, though sometimes the difference is just perceptible. The murmur has been of a distinctly musical character in the course of the disease, but only for a time. The symptoms are chiefly those of angina pectoris, occurring in paroxysms, and are found to be most effectually kept in check by repose, warmth, stimulants, dry cupping, antispasmodics, and occasional opiates. The patient is still under treatment. He died (February 28th). On examination, enormous hypertrophy with dilatation, chiefly of left ventricle. Dilatation of arch of

aorta. No sacculated aneurism. Aortic valves incompetent; two of them united together at their roots.

James S., æt. 38. This was an extremely interesting, but by no means an obscure case. It had been repeatedly under treatment, and at last terminated fatally. The aneurism, which was evidently of large size, pointed externally at the mid-sternum, and when first seen, was so soft and thin in its coats (having entirely perforated the bone, and presenting to the finger somewhat the sensation of a pulsating abscess), that an external rupture seemed to be impending. By means of a carefully adjusted soft compress of cotton wadding sewed into a linen bag, and applied by cross strips of soap plaster, and a figure-of-eight bandage round the shoulders, the development of the tumour outwards was held in check until a firm coagulum had formed. The patient wore this bandage for several months, and I feel assured it was the means of saving his life at the time. Its application would have been difficult but for the perfect absence of all respiratory oppression at this period. The very slow and gradual increase of the tumour internally, was accompanied, however, in the end by very severe sufferings from angina and oppression of the breathing. Ultimately, there was great obstruction to the venous circulation in the upper half of the body, and during the last hours complete suppression of the pulses, and intense cyanosis. There was no rupture of the sac. The pericardium was found to be firmly adherent.

Aneurism, threatening Rupture; Treatment by Compress.

Peter C., æt. 46. A case of dissecting aneurism bursting into the pericardium. The patient, who had only been seriously ill for a fortnight before admission, and could give no distinct history of his symptoms, except his having caught cold, had caused himself to be brought in a cart from Dunfermline, in the midst of deep snow, and arrived at the hospital in a state of great exhaustion. He revived somewhat (though subject to paroxysms of severe dyspnœa, and cough), for two days, during which he had no expectoration. A distinct double murmur over the base of the heart, and a marked though ill-defined pulsation, with sense of undue resistance in the jugular fossa, justified the gravest prognosis. On the third day after admission, he expectorated some blood in the midst of a paroxysm of orthopnœa, with extremely cold surface, suppression of the pulse, and manifestly obstructed and noisy respiration. This was at the hour of visit, and it seemed probable that he would die immediately. He partially recovered, however, for five days more, during which he had little or no sleep, but sat in a chair opposite the fire. His feet and legs became decidedly dropsical. He evidently indulged in strong hopes of recovery, but in the end he perished quite suddenly. The inner coat of the aorta, which was only slightly atheromatous, was found torn across, immediately above the valves, for more than an inch in a horizontal direction. The valves themselves, and the heart generally, were normal; but it is probable that the valves were

Dissecting Aneurism.

rendered incompetent by loss of their support in the vascular wall. The outer coat of the aorta was separated from the middle for more than three square inches. The rupture into the pericardium had occurred at a point between the aorta and the left branch of the pulmonary artery. The pericardium contained about a pint of perfectly recent blood, the fibrin of which had coagulated in a mass involving the corpuscles. The lungs were much congested, œdematous, and contained extravasated blood in a few places. The mucous membrane of the trachea was likewise congested. The other organs were normal.

It remains, in connection with the disorders of the circulating system, to notice briefly two remarkable cases of the form of disease so ably and thoroughly illustrated by Virchow under the name of *Emboli* or *Embolism*—the obstruction of the arteries and capillaries by fibrin and debris of coagulated blood, washed onwards in the course of the circulation. Though neither case can be said to have been the subject of a complete diagnosis during life, the difficulties felt with respect to each were instructive, and in one the presence of fibrinous clots in the pulmonary artery was correctly *suspected*, as the cause of a considerable divergence from the usual course of phthisis pulmonalis.

Embolism.

Case of Pulmonary Embolism, with Phthisis and Cerebral Softening.

Margaret M'K., æt. 33, was admitted with well-marked signs and symptoms of phthisis pulmonalis; the upper lobes of both lungs, but especially the right, being extensively diseased, and the expectoration ex-

ceedingly purulent. Two peculiarities, however, marked the case. The earliest in date was an affection of the brain, marked by an almost entire loss of memory of facts and of words, and often by the mistake of one word for another. The other was an extraordinary amount of lividity, developed in connection with paroxysms of dyspnœa and suffering, but also becoming to a considerable extent permanent for some weeks before death. On admission, the loss of memory, and the complete helplessness thence resulting, with the intelligence evidently considerably impaired, produced all the effect of complete fatuity. I believe, however, that the patient was to some extent conscious of her own condition, and she readily enough answered simple questions requiring only "Yes" or "No." She had no severe suffering at this time; she generally sat up in bed, living the life of an automaton. The affection of the intellect rather improved; but the pulmonary symptoms became slowly worse from the period of admission, and took the form, as stated above, of cyanosis quite out out of proportion to the amount of lung involved, or to the advance in the general symptoms. On these grounds I hazarded a guess (for it did not pretend to be any more) that there might be some obstruction in the course of the pulmonary circulation; and as no murmur existed over the heart, it seemed most probable that it would be found in the branches of the pulmonary artery. The examination after death shewed the right auricle quite impacted with adherent coagula, most of which were either in *debris*, or soft and puriform in the centre. The

greater branches (beyond the primary division) of the pulmonary artery contained several similar clots, blocking up a considerable portion of its area. The valves of the heart were normal. The brain presented a very peculiar form of red softening at numerous points of the grey matter of the convolutions, close to the pia-mater, and never passing further inwards than the mere surface of the white matter. The arteries at the base of the brain were normal; nor could any clots be found in the smaller vessels, in so far as they could be easily traced with fine scissors. The character of this softening was not subjected to a more minute examination, owing to want of time.

Case of Pulmonary Embolism, with Dropsy and Cerebral Symptoms.

Jane H., æt. 26, was admitted in a state of aggravated suffering from dropsy, accompanied by some vague form of uneasiness, of which no good account could be procured, as she constantly stated that she had no pain, and referred to no part in particular as the source of her distress. Notwithstanding this, she lay constantly moaning and disturbing the ward with aimless cries, over which, apparently, she was totally unable to exert any control. Her brother, who came to visit her, was not more able than the nurses and myself to get at the cause of her suffering; and he, as well as I, became quite persuaded ultimately that the cries were the result of a cerebral disturbance bordering on fatuity, but without either delirium or any distinct delusion. This state of the patient made it necessary to remove her to the ward devoted to noisy patients. Meantime, a great

variety of diuretics, and at last compound jalap powder and croton oil, had been tried, with little or no effect on the dropsy. Careful examination gave evidence of moderate enlargement of the heart, but neither in the state of the sounds of that organ, nor in the kidney and liver, could an adequate explanation of the extraordinary severity of the dropsical symptoms be found. The urine was always free from albumen, scanty, and of high specific gravity. Finally, the patient became worn out by agitation and sleeplessness, and bed-sores formed upon the back. She died about two months after admission. The heart weighed 12 oz., and was somewhat dilated, though by no means greatly so: the valves were normal. The right auricle and ventricle were half full of decolorized clots firmly adherent to the endocardium, and having all the appearance of the "globular polypi" of Laennec, although mostly broken up. The muscular fibre of the heart was decidedly fatty. Both branches of the pulmonary artery contained fragments of similar clots, so placed as to cause very marked obstruction. The lungs were œdematous and somewhat collapsed. The kidneys congested, solid, and large, otherwise normal. The liver not diseased, though dense. The brain normal, except that, at the base of the cerebellum, the membranes were somewhat white and opaque.

Singularly enough, these two cases terminated almost exactly at the same time, and were examined after death on the same day. As it is not my intention to discuss the subject of embolism in its pathological relations, I will only remark that the cyanosis, which was so striking

a feature of the first case, was only slightly present in the second; while the dropsy of the second case was not present in the first. The symptoms in the two cases were not altogether dissimilar, but the cerebral morbid appearances had nothing in common; and it is perhaps premature to venture an opinion, in the present state of our knowledge of the subject of embolism, whether the peculiar punctuate form of red softening of the grey matter witnessed in the case of Margaret M'K. had any connection with an obstruction of the capillaries of the brain, occurring without the formation of any distinct clots in the larger vessels.

In connection with this subject, it may be right to refer here to a case to be detailed further on,* in which a marked dilatation of one pupil, occurring the day before death, was found to be associated with a fragment of decolorized clot in the corresponding internal carotid, where it passes through the cavernous sinus.

IV. *Diseases of the Stomach, Intestines, and Peritoneum.*—To the large class of diseases indicated by this title (but excluding enteric fever), there were referred twenty-seven cases; ten of which, however, were complicated either with phthisis pulmonalis, or with Bright's disease of the kidney.

Sixteen of the twenty-seven cases were marked by diarrhœa as one of their principal features. In two of

* See Case of puerperal phlebitis, p. 682.

these the diarrhœa was simple; in two, it was a symptom of Bright's disease; in eight, it was a prominent characteristic of tubercular phthisis; in four cases, the disease had the characters of dysentery. With the exception of the cases complicated with organic disease of the lung and kidney, all of these terminated favourably; but in one case of apparently simple diarrhœa, the disease was exceedingly obstinate, and it is probable, or rather certain, that temporary relief only has been afforded. The patient John W., æt. 68, had been subject to indigestion and attacks of diarrhœa for more than twelve years, and had acquired an inveterate habit of taking opium, which every effort during his residence in the hospital failed to induce him to break off.

Diarrhœa.

Of the four cases of dysentery, three recovered quickly under the use of large injections of warm water, and moderate doses of opium, chiefly administered by injection. One case (James A., æt. 56) was exceedingly severe, and required a protracted treatment. The patient had been ill for several months before admission; he was very much reduced, and extremely pallid. The stools, no longer bloody to any considerable extent, were passed in small quantities at a time, with great suffering, and were extremely fœtid. I had great apprehensions as to the result in this case; for, from the slight effect produced by opiates and injections, and the severe tenesmus, with tenderness on pressure along the descending colon, I formed the opinion that considerable disorganization of

Dysentery—Its Treatment.

the lower bowel existed. I determined, under these circumstances, to try a remedy which I have frequently found useful in subacute and chronic dysentery—viz., the addition of creasote to the large emollient and detergent enemata, which I employ in all cases of dysentery whatever, and which I believe to be by far the most important element in the treatment of that disease in all its forms. As I am not aware that creasote is in general use in the treatment of dysentery, I may state that I was led to its employment some years ago, by the statements of Dr. Wilmot* as to its efficacy in an epidemic prevailing in the Union Workhouse of Pembury. I have employed it, however, somewhat differently from Dr. Wilmot, who administered it in large doses (3i.) merely suspended in gruel. It appeared to me more likely to do good, and with less risk, if employed in smaller doses in solution, so as to be applied uniformly and certainly to the whole surface of the diseased mucous membrane. I have, therefore, usually employed the mistura creasoti of the Pharmacopœia, and have added one to two ounces of this, according to circumstances, to each large injection. It has been followed in several cases by the best possible effects; and I can safely recommend it as a valuable addition to the resources of the physician, in cases of dysentery attended by great irritation and fetor of the evacuations. In the present instance, I employed a solution of creasote in glycerine, prepared for me by the Messrs. Smith of Duke Street, to whom I applied to

Creasote in Dysentery.

* *Monthly Journal of Medicine*, May 1855, p. 423.

find a neutral solvent for creasote, miscible in all proportions with water.* The relief afforded in the case of James A. was very remarkable. The injections were repeated twice or thrice a day, the dose of creasote in each being from ℳ. v. to ℳ. x., and the patient experienced so much benefit from them, that he proposed of his own accord to take the remedy by mouth also. The intense irritation subsided, and the stools became much less offensive. It is right, however, to remark, that the remedy appeared to lose some of its power after a time, and that this man was finally restored to greatly improved health under considerable doses of ipecacuanha, which was well borne by the stomach, and seemed to have a very decided effect in removing the remains of a very dangerous and exhausting disease.

In a case of obstinate diarrhœa from Bright's disease, and also in the case of John W., alluded to above, creasote was also tried as a *dernier ressort;* but only with the effect of shewing that its good effects are probably limited to cases of genuine dysentery, accompanied by ulceration of the great intestine. It failed, in the others, to afford even temporary relief.

Seven cases were attended by well-marked symptoms of *disorder of the stomach.* One of these was probably a case of gastric ulcer; three were cases of chronic

* The proportions used were Creasote m. xx. to Glycerine ℥j. The materials require to be pure and to be well rubbed up together in a mortar. The result is what appears to me a perfect solution, though Mr. Smith doubts its being so.

vomiting, probably or certainly without ulceration; three were instances of severe dyspeptic suffering, without proof of structural disease. All recovered except one, a very melancholy case of fatal vomiting consequent on pregnancy, in a patient having (as the *post-mortem* examination shewed) a large amount of fatty deposit in the liver and kidneys. [The urine had been repeatedly examined during life, and was found quite free from albumen.] This case caused me much anxiety, as it is impossible not to feel, looking back on the result, that a more active interference might have had a chance, though perhaps not more than a chance, of saving the patient. A consultation was held, after all palliative means seemed to have been exhausted; and it was decided, on obstetric grounds, not to interfere by inducing abortion. The pregnancy appeared to be in the fourth month.*

The case indicated as *gastric ulcer* was in no respect peculiar, as regards the symptoms on admission or the treatment; but the history of the disease was rather unusual, and tended to puzzle the diagnosis, and even to throw doubt upon it to some extent.

Question in Diagnosis. Gastric Ulcer? Fever?

The patient, a most intelligent man (Joseph A., æt. 40), was disposed to refer the greater part of his symptoms to a "gastric fever," occurring rather less than two years before his admission; although he had unquestionably had dyspeptic symptoms of a less striking kind, and especially costiveness and more or less irritability of

* Reported in Dr. Haldane's Register of Dissections, vol. xx., No. 2.

stomach, for ten years or more. This (so-called) gastric fever forming the starting-point of a class of symptoms altogether similar to those of gastric ulcer—viz., vomiting and purging of dark blood, anæmia, excessive sensitiveness of the stomach to very small quantities of food, localised pain of epigastrium, vomiting shortly after meals, impeded nutrition, and consequent emaciation. But what was especially singular was, that the acute attack in question was attended from the third or fourth day by marked jaundice ; and, further, that the patient's daughter, at the time about six years old, was similarly affected with jaundice, fever, and vomiting, with purging of very dark matter (evidently, from the description, discoloured blood). These statements, made with great clearness by the patient, were confirmed in all respects by his wife, whom I examined separately, and who had no doubt that her husband's disorder and the girl's were similar in character, and were, in fact, considered by the doctor in attendance to be the same febrile disease. The recovery from the fever was in both cases slow, the daughter being confined to bed for eight or nine weeks, and the father nearly double that time.

I shall in the meantime refrain from speculating on the curious history of which this is a very brief abstract, merely remarking that no type of epidemic fever, with which I have been familiar for several years past, presents the slightest resemblance to the disease here described. Had the father only been the subject of the acute attack, it might have fairly been considered as probably having been no fever at all, but an acute exa-

cerbation of the chronic disease of the stomach. But even in this case the jaundice would be a very curious and rare symptom, and the concurrence of this symptom with hemorrhage from the stomach and intestines, in two members of the same family at the same time, is surely (if correctly stated) a fact of the most singular and anomalous character, at least in any part of the world not visited by yellow fever.* This patient improved very much under a carefully regulated diet, with moderate and cautious use of laxative medicines. [When I last heard of him he continued well.]

In three other cases belonging to this series (Thomas L., Thomas M'G., Christian S.) there are evidences of serious organic disease, of obscure and complicated character. The details of these cases, however, are too long and complex to be narrated here with advantage.

One patient (Margaret F.) died from acute peritonitis, probably, I think, the result of perforation, perhaps from enteric fever admitted late in the disease. Another (Rachel S.) died from cancerous disease of the peritoneum and abdominal viscera generally.

V. *Diseases of Kidney and Urinary Function.*—These cases were twelve in number. Two of them were dia-

* In the relapsing fever of 1843-4, jaundice was very common, and something like black vomit occasionally occurred. But relapsing fever had been extinct in Edinburgh for years before this attack in Joseph A. and his daughter took place. Nor was the extremely lingering character of the illness in the least degree like relapsing fever, as observed either in 1843 or 1848.

betes mellitus, still under treatment; one was a case of abscess of the kidney, chronic and probably scrofulous, dismissed without improvement. All the rest were cases of Bright's disease, or of albuminuria tending in the direction of Bright's disease.

Of the nine cases of Bright's disease four died; one of these (Walter B., æt. 44) had extreme and uncontrollable diarrhœa, and was probably otherwise complicated; another had waxy kidney and liver, ulcerated larynx, traces of nodes, with exhausting diarrhœa, and a constitution probably deeply contaminated with syphilis; the two others were merely complications of fatal heart disease and phthisis. One other case was complicated with heart disease, certain to be fatal at no long distance of time. The remaining four improved very much under treatment, and some of them might fairly be said to have been cured, were it not for the well-known liability of this disease to recur. The most important facts of these cases are stated very briefly below :—

Acute Renal Desquamation, with Dropsy, and threatened Suppression of Urine.

Christina D., æt. 47, sempstress. Admitted Nov. 25th. A most formidable case of acute and exceedingly abundant renal desquamation, with dropsy, threatening at one time to end in suppression of urine, and complicated also with severe bronchitis, as well as with enlargement of the liver, probably chronic. The urine at first varied from sp. gr. 1027 to 1038; it was very scanty, of deep brownish-red colour, loaded with blood, and excessively muddy from tube-casts and epithelial debris; there was dull pain in the loins, and the dropsy,

though nowhere extreme, was universal and tending to increase, while the stomach rejected both food and medicine. Cream of tartar alone, in moderate doses, having been tried ineffectually in this case, and the symptoms being urgent from the extreme diminution of the quantity of urine (which was almost entirely suppressed for twenty-four hours), I directed the use of ℈j. doses of compound jalap powder, repeated sufficiently often to maintain brisk purgation. In a very few days the secretion of urine was restored much improved in quality; the dropsy diminished simultaneously with this improvement, and the sickness disappeared altogether. Free diuresis was afterwards kept up by saline diuretics and infusion of digitalis, and the warm bath was repeatedly used; there was no relapse, and the patient was dismissed on January 16th, perfectly well as regards symptoms. I find on one occasion before her dismissal the urine noted as non-albuminous; on other occasions the albumen was "slight," "a mere haze," etc.; I believe, however, that in this patient the kidney has sustained serious injury, and it is more than probable that albumen will continue to be present, at least for some time, if not permanently. Of the disabled condition of the kidney I think there is evidence in the specific gravity of the urine, which decreased rapidly as the secretion became more abundant, and for some weeks before the patient's dismissal varied from 1010 to 1017, never rising above the latter figure.

Use of Purgatives and Diuretics.

Betty M., æt. 55. Affected on admission with rather acute bronchitis, accompanied by œdema of ankles

and lower limbs generally. It is probable that the renal affection in this case was of some standing, as the specific gravity of the urine was persistently rather low than high. On admission, diaphanous tube-casts were present, but no blood ; albumen was also present in small quantity. Under treatment by active saline diuretics, chiefly cream of tartar and expectorants, the albumen in the urine was reduced to a mere trace, and the tube-casts disappeared. She was dismissed on December 5th, almost exactly three weeks after her admission, quite cured as regards symptoms ; but probably she will continue to have albuminous urine from time to time. The specific gravity varied from 1015-19.

Renal Desquamation, Bronchitis. Diuretic Treatment.

Alexander S., æt. 37, was admitted on December 7th, affected with dropsy and albuminuria. On inquiry, it appeared that he had been under treatment for a similar attack about a year before, in another ward, and was dismissed cured, *i.e.*, relieved of the more manifest symptoms—for it does not appear that there is any evidence of the absence of albumen from the urine, at least on more than one occasion. [The albumen was, in fact, still present when the patient was dismissed, though in small quantity. Only ten days before this, albumen was noted as separating in flakes on boiling, the urine having " a dim, smoky tint, reaction acid, sp. gr. 1018." Amaurotic symptoms had been present within three weeks, muscæ volitantes within a few days of the patient's dismissal; the leading symptoms in hospital

A Second Attack of Acute Dropsy. Renal Desquamation. Diuretic Treatment.

having been dropsy, with well-marked uræmic convulsions. It is not without importance in a clinical point of view to remark, that these facts are recorded apparently without any suspicion as regards the ultimate result; the object being to prove (as respects Bright's disease) "how, by judicious treatment, it sometimes terminates in recovery."] The patient's own statements went to shew that his general health had been considerably impaired since the former attack; he had, however, remained free from dropsy, and the only serious illness in the interval had been an attack of vomiting (?) of blood, which was attended by discoloured spots on the arms and legs. He had also had some degree of cough and shortness of breathing on exertion occasionally. On admission, dropsy was pretty general, though not extreme in amount; the urine was of a smoky tint, specific gravity 1012, and contained blood-discs and tube-casts, with albumen in rather small quantity. Under a diuretic treatment, the blood-discs and colour disappeared in five days; tube-casts, however, continued to be visible till the 5th January, when the dropsy had quite disappeared, and the albumen was reduced to a mere trace, the urine being usually of specific gravity 1012. On several occasions albumen was not precipitated at all on the addition of nitric acid to the heated urine, but after an hour or two there was found a small sediment of flocculent matter, evidently a trace of albumen. The patient went out feeling quite well, but it is evident that in this case the function of the kidney is still more or less impaired.

Anne W., æt. 23, admitted with a first attack of dropsy, amounting only to moderate œdema of limbs. The urine scanty, specific gravity 1020, moderately albuminous. The dropsy disappeared in a few days under diuretics, and the albumen also disappeared; but as the girl insisted on leaving the house at the end of a week, and did not return, no opportunity has been afforded of ascertaining the permanency of the cure.

Renal Dropsy. Rapid Cure (?) under Diuretic Treatment.

These cases, though not numerous, nor very unusual, are full of instruction. They shew, in the first place, that the objections still entertained by many (partly, no doubt, founded on Dr. Bright's original statements) to the use of diuretics in the acute and subacute forms of renal albuminuria, are quite unfounded. In the case of Christina D., indeed, I was obliged to have recourse to the employment of drastic purgatives; not because diuretics did harm, but simply because, in the height of the disease, they failed to act. In all the other cases, the cure was trusted to diuretics alone; and even in Christina D.'s case, so soon as the kidneys were relieved from the oppression, which at one time threatened to end in ischuria, the use of diuretics was resumed with manifest advantage as regards the alleviation of the general symptoms, and with improvement in the quality as well as quantity of the urine. From very careful observation of numerous cases of renal dropsy in all stages, I am fully convinced of the accuracy of Dr. Christison's remark, made public so long ago as 1839, but too much overlooked amid the

Remarks on Treatment of Renal Dropsy.

speculative refinements of a more modern, but by no means more correct, pathology, that "diuretics do not increase the coagulability of the urine in the early stage; in many instances they appear to diminish it."* I am even prepared to go further, and to say that where diuretics fail, it is only in rare instances that other remedies will be found of material service. Moreover, though diuretics in dropsy are not unfrequently got to act with difficulty, it is certainly not (as is commonly asserted) in renal dropsies that they are most apt to fail. They are far more likely to be found wanting in the advanced stages of heart disease, and in the complex forms of dropsy depending primarily upon heart disease, with stagnation of the venous, and especially of the portal, circulation. In simple renal dropsy, on the contrary, whether acute or chronic, I have generally found the free employment of saline diuretics, sometimes aided by chalybeate preparations or by digitalis, and by the use (as a secondary or subordinate means) of the warm bath, the true key to the safe and efficient treatment, whether of the dropsy or of the albuminuria. And looking to the accumulated evidence of my own experience and that of others on this subject, I confess I am quite at a loss to understand the modern bias in favour of diaphoretics and purgatives, as opposed to a diuretic treatment, except upon the ground of a theoretical prejudice, adopted without due consideration of the facts of clinical experience.

The other remark suggested by these cases is, the

* *On Granular Degeneration of the Kidneys*, p. 140.

necessity of extreme caution in pronouncing upon the cure of persons apparently freed from the symptoms of acute or subacute renal dropsy. The case of Alexander S. is peculiarly instructive in this respect. When such cases are kept carefully in view for some time after the apparent cure has been effected, it is often found that the specific gravity of the urine remains permanently rather low, and that a minute amount of albumen is either always present, or occasionally occurs without any marked derangement of the general system. Care should be taken, therefore, not merely to test the urine repeatedly by heat and nitric acid in the ordinary way, but to allow it, when so tested, to stand for several hours, before pronouncing on the absence of albumen. The patient should also, if possible, be kept in view for some time, that the permanence of the recovery may be carefully verified; for in this way alone is it possible to be assured of the reality of the cure.

Alleged cures.

VI. *Diseases of the Nervous System.*—On these cases, though several of them are important and interesting, I shall avoid lingering in the present paper, as they are too few and of too miscellaneous a character to be useful for the exposition of any general principles either of diagnosis or of treatment. Excluding the cases admitted into the special ward for delirious and noisy patients, there were in all 15 cases under treatment referrible to this general designation. They may be thus classified:—

Hysterical symptoms of various kinds . .	3 cases.
Local nervous affections, not paralytic or hysterical	5 cases.
Muscular Tremors	1 case.
Apoplexy (hemorrhagic) . . .	1 case.
Hemiplegia	2 cases
Paraplegia	2 cases.
Tubercular Meningitis . . .	1 case.

Of these, three were fatal: one of apoplexy; one of hemiplegia (cancer of brain); and one of tubercular meningitis, which presented the usual characters on examination after death. The rest were in most cases considerably improved under treatment; but the period of observation was usually too short to allow of satisfactory statements as to the ultimate result, whether favourable or otherwise.

As regards the cases admitted into Ward X. (the special ward above alluded to), they were of considerable interest, and formed a majority of the cases of nervous disease under my care during the three months. They amounted in all to about 40 cases, some of which, however, were under the care of the surgeons; and others were cases of poisoning, treated for the most part in my absence, by Dr. Shearer. There were about 20 cases of acute delirium, of which 12 were distinctly connected with intemperance, and had, in the majority, the character of delirium tremens. In most of these the delirium gave no anxiety, being mild, and requiring nothing but careful watching; two were rather more severe. I am, however, happy to record my conviction, that, owing probably to the improved habits of the lower classes, severe delirium tremens has become

Cases of Delirium.

of late years comparatively quite a rare disease. Two cases were admitted, of very inveterate habits of intemperance, amounting to positive dipsomania: one in an exceedingly clever workman, a tailor, earning high wages in the "cutting" department of his business; the other in a clerk, admitted for the 21st time, and giving very little room for hope that he is at all likely to be less frequently a visitor in future!

Dipsomania.

A married woman, living apart from her husband, was a complicated example of the mischief of intemperance superinduced upon hysteria. It was a complete example of hysteric coma with cataleptic rigidity in an aggravated form, which, however, passed off in a very short time without treatment, the patient being left, by my directions, entirely to herself, and being merely watched from a distance, in case of mischief or injury. The other cases of delirium were comparatively uninteresting, with the exception of three cases of acute mania, all of which had to be sent to Morningside Asylum. It is remarkable that two of these were characterized by extreme excitement of the religious emotions; brought on, I have little doubt, in one case, by injudicious moral treatment in a young girl undergoing the discipline of a reformatory institution; in the other, the result of a morbid spiritual pride, developed into the delusion of a mission directly from the Holy Ghost.

Hysteric Coma.

Religious Mania.

There were five cases of poisoning admitted into Ward X., not including eight others of intoxication with spirits, so deep as to come fairly under that denomina-

tion. One of the latter was a curious instance of pre-
Cases of Poisoning. cocious intemperance. A little boy, seven years old, was induced, during the excitement of a wedding-party, to swallow three teacupfuls of pure whisky, and was admitted in a state of very deep and alarming coma, from which, however, he recovered under appropriate treatment. One child swallowed a quantity of green paint, supposed (from the symptoms) to be arsenical; it also recovered. Four persons took poisonous doses of opium, one of whom died. Most of these cases, including those of delirium tremens, occurred shortly after the saturnalia of the New Year.

VII. *Diseases of the Skin.*—Cutaneous diseases, and external diseases generally, form an extremely interesting class of cases; but an hospital such as ours does not fairly represent the prevalence of such diseases among the population, inasmuch as their comparatively slight urgency and great number causes them to be only admitted under peculiar circumstances, and in virtue of a selection guided by very special considerations. For this reason I shall pass them by briefly, with a very few words of commentary. The total number under treatment of skin diseases was 12, of which

1 was papular (Prurigo).
4 were vesicular or pustular (2 Eczema, 1 Eczema impetiginodes, 1 scabies).
3 were squamous (1 Lepra alphoides, 2 Psoriasis).
1 was a case of Rupia cachectica.
1 was an inveterate case of Favus.

In addition, there occurred a well-marked example of ringworm on the neck (Herpes tonsurans), and one of Pityriasis versicolor, in patients admitted for other diseases, and entirely without reference to the cutaneous affection, which was only discovered by accident, and was productive of little or no inconvenience.

All of these cases were either cured, or are in progress towards cure, with the exception of one patient affected with syphilitic psoriasis, who absconded on the day after admission; and the case of prurigo, which had to be sent to the small-pox ward, the patient having unfortunately contracted that disease while under treatment. The case of Eczema impetiginodes was of very unpromising appearance, from its affecting the entire surface occupied by an exceedingly stiff beard and whiskers. After a comparative trial of various local applications, including various lotions carefully applied, and several varieties of ointment, it was found that the unguentum oxidi zinci fulfilled every necessary purpose of protection, with far more soothing effect as regards the smarting pain than any other; the cure closely coincided with the development of the physiological action of arsenic, and was exceedingly rapid and satisfactory. Equally unpromising in appearance was the case of Rupia: the eruption was of long standing, and had very much disfigured the face, especially the upper lip and forehead; it was present in a slighter degree over the trunk, and was evidently of constitutional origin. It subsided, however, rapidly under the administration of hydriodate of potass inter-

Eczema.

Rupia.

nally, with good nourishing diet; the external treatment being at first poultices, and afterwards gently stimulating lotions, especially black wash, with occasional touches of sulphate of copper, and of tincture of iodine, on the ulcerated surface. A case of syphilitic psoriasis, with sore-throat, was practically instructive, inasmuch as it yielded rapidly to alterative doses of mercury after being quite ineffectually treated for some time with iodine. A case of simple Lepra, very widely diffused, got well rapidly under the arsenical treatment with pitch ointment and warm baths. Two of the cases of Eczema were rather obstinate, but are improving. The case of Favus was not devoid of instruction, though no one, who has carefully studied the disease, will venture to pronounce it cured for a long time to come. I presented the patient to the class at a clinical lecture, with the head covered with yellow crusts of long standing. Exactly *four days afterwards,* I again shewed the patient in the ward: there was not a vestige of a crust to be seen, nor even any broken surface; though the patches of absolute baldness, and the stunted and diminutive hairs in many places, shewed clearly the deep hold the disease had taken. The change was entirely due to *three successive poultices of linseed meal.*

Squamous Eruptions.

Favus.

After so much has been written about favus, and so many perfect cures have been recorded in periods varying from six weeks to several months, I am almost afraid to state my conviction that the satisfactory result above mentioned,

Remarks on Treatment of Favus.

obtained in four days under linseed-meal poultices, was quite as much entitled to the name of a cure as most of those that I have yet seen or heard of either in nature or in the records of medicine. To speak of a cure of this disease, with opportunities of observation extending over less than a year or two, is, in my opinion, evidence of nothing else than the most entire ignorance of its habits.* I do not, however, doubt the cure of favus. Soap and hot water, with abundant scrubbing, the hair being kept short, will commonly keep the yellow crusts indefinitely in abeyance; as will also, perhaps more thoroughly and effectually, the simplest oil inunction. There seems no reason, therefore, to believe (though hospital physicians can but seldom hope to witness the result) that these simple means, *long and perseveringly used*, will not finally effect the cure of a disease which owes its origin and perpetuation to nothing else than want of cleanliness.

Under ordinary circumstances, what takes place after an apparent cure of favus is this:—So long as the hair is kept shaved, and an alternation of oily applications with soap and water is maintained, the disease does not reappear; but on neglecting these precautions for a few weeks, yellow dots begin to crop up, and these rapidly extend so as to become distinct favus crusts, which in no long time, if uninterfered with, will cover the whole head. I have repeatedly kept cases under observation after the head had been completely cleared, in order to observe the first beginnings of the eruption after the sus-

* See a most marked instance of this inveteracy of the disease, in p. 239 of the present volume.

pension of treatment; and I have also employed a great variety of medicated ointments and lotions, including sulphurous acid, iodine and sulphur ointments, empyreumatic oils, mercurial ointments, and mixed medications of various kinds. After most of these, I have seen the disease reappear about as quickly as under the simpler treatment by oil and soap. If there is any of them in which I have faith more than another, it is in empyreumatic oils, as the juniper tar oil or the common pitch ointment. But the inveteracy of the disease evidently depends, not on the difficulty of removing its visible traces, but on the complete infiltration (so to speak) of the scalp with the sporules of the fungus in all old-standing cases; and no treatment will be of the slightest avail towards a radical cure that is not deliberately and carefully pursued until a complete new growth of scarf skin has been obtained, perfectly free from all traces of the noxious germs. This must, of course, be the work of a considerable time; just as it is a work of time, and of unwearied attention to simple details, to rid a virgin soil of ragweeds and whins, or even of stones. No application of a specific can be expected to meet the one case, any more than the other.

One point, not always observed by those who have written on this subject, is, that favus is often, perhaps even in the majority of cases, implanted on the basis of a previous eruption; in other words, that the fungous crusts, or vegetable mould, are sown on a soil already the seat of impetigo, eczema, or some other variety of disease of the skin. Sometimes the original disease has died out

when the favus first comes under treatment; at other times it still persists, and requires separate treatment. In the course of a considerable and varied experience of true favus, however, I have not seen a single case that did not at once yield to local treatment to the extent I have indicated above; and I am very far from believing that any constitutional disorder has to do with the production of the fungus, further than that favus and other diseases may arise simultaneously, under exposure to the same causes of filth, neglect, and hygienic errors of every kind, in every variety of bodily constitution.

VIII. *Miscellaneous Cases not elsewhere noticed.*—It is hardly possible, considering the length to which this retrospect has extended, to do much more than enumerate these cases. They were as follows:—

Continued Fevers—Typhus, 10 cases; of which two were fatal. Enteric Fever, 3 cases; all cured.

Rheumatism—8 cases; all recovered, more or less completely: one was syphilitic, one gonorrhœal, one with a suspicion of gout.

Venereal diseases—4 cases, besides those already noticed under skin diseases; cured or improved.

Erysipelas—6 cases, all cured. One, however, a very severe case of phlegmonous erysipelas of the forearm, ended in considerable deformity and stiffness of the wrist.

Laryngitis and Cynanche—4 cases, all cured.

Puerperal disorders and special diseases of women—5 cases (one noticed under chronic vomiting). One of

the remaining four was fatal—a remarkably interesting and melancholy case of puerperal phlebitis, accompanied by marked hectic fever, and symptoms resembling pyæmia; also by an obscure and distant murmur with the first sound over the base of the heart, which gave rise to a suspicion of pericarditis or pericardial adhesion. The patient had been subject to varicose veins during her pregnancy; and, simultaneously with the origin of the grave symptoms, which came on within a few days after the delivery, the varices opened externally, giving rise to considerable hemorrhage. At last, notwithstanding every precaution, sloughs formed over the sacrum and trochanters, and the patient sank exhausted. The examination after death shewed the iliac and femoral veins impacted with softened adherent clots, in close connection with abscesses of the cellular tissue. The heart was normal, with the exception of an immense open foramen ovale more than an inch in diameter (query, Was this the cause of the indefinite murmur above mentioned?) The lungs, notwithstanding great expectoration of pus during life, shewed no abscesses, but the bronchi filled with nearly pure pus in many parts. In the left internal carotid artery, within the cavernous sinus, there was a minute decolorized granular clot; but no further traces of embolism could be discovered. In connection with this last fact, it is interesting to notice that the pupil of the same side presented a very remarkable dilatation (as though from belladonna) for some hours before death. The

Case of Puerperal Phlebitis.

Patency of Foramen ovale.

Embolism?

texture of the oculo-motor and optic nerves seemed unaltered.

I shall conclude this report by noticing a remarkable case of pure anæmia, fatal without any explanatory organic complication. Agnes T., æt. 47, had for some time been labouring under depression of spirits, ascribed to the loss of an attached friend. She was stated (but I think the fact is doubtful) to have been affected with jaundice three or four weeks before admission. She was admitted in a state of great debility, blanched and sallow in appearance to an extreme degree, and obliged to maintain the recumbent posture to avoid faintness. There was a well marked though short murmur with the first sound over the heart and great vessels: the venous murmur was present to a great extent in the neck. Great exhaustion was produced by any movement, and even by the effort of speaking. The stomach almost entirely refused food, except milk and a little bread. No disease of any organ, however, could be discovered. There was a little tendency to œdema. The urine was non-albuminous. Notwithstanding the diligent use of tonics and of iron in every form that appeared likely to be of service, all these symptoms increased. The blood presented an extreme deficiency of red, and certainly no increase of white, corpuscles. After death, which occurred from pure exhaustion, every organ was carefully examined: the only morbid appearances were in the heart, liver, and kidneys, which were all more or less occupied by fatty granular deposit.

Case of fatal Anæmia.

I have only to say, in regard to this most mysterious

form of disease, that it has no real relation with the chlorosis of young women. In fact, the only two cases which I have seen exactly resembling this one, occurred in men, and at middle age. The colour of the skin is essentially different from that of chlorosis; having, in fact, a tendency to sallowness, which, I have little doubt, was in this case mistaken for jaundice. The general appearance is much more that of malignant disease than of any other condition with which I am familiar.

IX. *Summary of Cases, with remarks on the Mortality.* —In concluding this retrospect, it may be useful, for the sake of comparison with the experience of others, to condense into a few lines a summary of the more important forms of disease I have mentioned; and at the same time to exhibit in detail the sources of the entire mortality from these and other causes. The population of the wards under my care during the quarter has been precisely 200; of these—

Catarrhal Affections have furnished 23 cases, or 11·5 per cent; of which 2 were fatal.

Phthisis Pulmonalis—22 cases, or 11·0 per cent; of which 11 were fatal.

Inflammatory Acute or Sub-acute Condensations of the Lungs (usually complicated)—9 cases, or 4·5 per cent; of which 1 (a case of gangrene of the lung) was fatal.

Cardiac Disease, Aneurism, etc.—16 cases, or 8 per cent; of which 5 were fatal.

Bright's Disease of Kidney, or Renal Albuminuria with Dropsy—9 cases, or 4·5 per cent; of which 4 were

fatal (one of these was complicated with cardiac disease, one with tertiary syphilis and disease of the liver, one with advanced phthisis, and one with intense and uncontrollable diarrhœa, attended by questionable symptoms and signs of tubercle).

Disease of the Stomach, organic and functional—7 cases, or $3\frac{1}{2}$ per cent; of which 1 was fatal.

Diarrhœa and Dysentery—16 cases, or 8 per cent; of which 8 were fatal (all the fatal cases were complicated; six with phthisis, two with Bright's disease).

Diseases of the Skin (including six cases of erysipelas)—18 cases, or 9 per cent; none fatal.

Fevers (typhus and enteric)—13 cases, or 6·5 per cent; of which 2 were fatal.

Rheumatism—8 cases, or four per cent; none fatal.

Diseases of Women—6 cases, or 3 per cent; of which 2 were fatal.

Venereal Affections—7 cases, or 3·5 per cent; of which 1 was fatal (mentioned under Bright's disease).

Sequelæ of Intemperance—15 cases, besides 8 cases of direct alcoholic poisoning. The proportion of these is large; but it is to be remembered that the entire number of cases of this kind admitted to the Infirmary, at the season of the New Year's Day festivities, have passed through my hands on the present occasion. Among these cases there were no deaths.

The entire mortality during the quarter amounted to 33, or very nearly *one* in *six* of the population of the wards during the period. This proportion is undoubtedly very high as compared with that of most hospitals,

and even of the medical department of the Edinburgh hospital generally; but it is amply explained by a reference to the details given in the body of this paper and in the preceding summary. The season has been a severe one; and large numbers of the phthisical cases, as well as of other cases of organic disease, have been brought to a termination. One-third of the mortality (11 cases) is, indeed, accounted for by phthisis alone; and somewhat less than one-sixth (5 cases) was due to disease of the heart; bronchitis (complicated) caused two deaths; gangrene of the lung, one death; peritonitis, abdominal cancer, chronic vomiting, each one death; disease of the kidney, two deaths; apoplexy, cancer of brain, tubercular meningitis, fracture of skull, opium poisoning, each one death; typhus fever, two deaths; puerperal phlebitis, one death; anæmia (as above recorded) one death. The mortality from acute uncomplicated inflammations and local diseases was, therefore, absolutely *nil;* for the death from peritonitis was, I think, almost certainly the consequence of perforation in enteric fever, although complete evidence could not be obtained to justify the case being placed in that list. Nothing can possibly shew more conclusively than these facts the absurdity of estimating the success of hospital practice by the absolute proportion of mortality.

The freedom of access afforded in the Edinburgh Royal Infirmary to cases of organic or, as it is often called, *incurable* disease, is, in my opinion, one of the most valuable and excellent features in the administration of that institution. The principle adopted is, to admit

patients *daily*, and to admit with an almost exclusive regard to the urgency and serious character of the cases making application; or at least, to reject no serious case, whether supposed to be curable or not, while there remains an available bed in the hospital. While in many other hospitals the admission of patients is a matter of choice or favour, here it is simply a matter of course that the poor, who feel themselves in danger of being neglected at home, apply and are admitted to the benefits of the Infirmary; the probability or possibility of cure not entering at all into the calculations of the admitting physicians, and the ordinary physicians being obliged to undertake the relief of all who apply in such circumstances.

The fruits of this policy are abundantly displayed in the preceding pages. While a small margin only is left for the record of brilliant successes, in comparison with the results of hospitals conducted on a different plan, I feel well assured that a great and beneficent lesson is taught by our experience, on perhaps a larger scale than in any other hospital in the kingdom. That there is almost no conceivable combination of adverse circumstances which can be regarded as altogether excluding the hope of amendment; that medical treatment, careful nursing, good diet and regimen, are often powerful for good, even in the most apparently desperate cases, is surely a truth of some importance to humanity; and such is undoubtedly the result of a simple and not overstrained deduction from every week's experience in the Edinburgh Royal Infirmary. Nay, I believe it is in

cases of organic disease that the results of treatment are, on the whole, most easily appreciated, and least liable to fallacy. I trust, therefore, that if the present report gives evidence in some respects of the weakness of the medical art, it will also be found not deficient in facts calculated to wean the earnest student from the gloomy feelings of scepticism and discouragement too often suggested by a superficial view of medical practice, whether in connection with acute or chronic, curable or incurable, diseases.

XX.

ON THE STUDY OF CLINICAL MEDICINE.

(*A Lecture delivered in the Royal Infirmary of Edinburgh at the commencement of the Winter Session*, 1861-62.)

It is my duty to begin, on the part of my colleagues and myself, this course of Clinical Medicine. Now, the object of clinical medicine is to enable you to study disease, and also the medical management of disease, for yourselves. This is the special business of clinical medicine, as contrasted with those systematic courses which you have elsewhere, and in which the leading doctrines of medical science have to be instilled into your minds chiefly through the opinions and words of your teachers. Here, in the wards of the hospital, teacher and student are sent to school together. It is our business here to study disease, not with a view to a body of doctrine, but with a view to the management of individual cases. We study those cases with the professed intention, first, of treating them medically; secondly, of learning from them as much as we can. And the duty of the student, on the one hand, and of the teacher, on the other, may be deduced from the careful consideration of this double point of view.

As I have very little time for preliminary remarks,

and must compress them into about half a lecture, I wish to submit to you only a very few important considerations bearing upon this matter. We have, as I said, to study disease, and also the medical management of disease. Do not forget that both of these objects are essential. If our object here were simply to study disease, or individual cases of disease, considered altogether apart from the management of them, we might proceed in a different way. I might in that case take you into the wards, and make them a scene of disputation and controversy ; discussing with you at the bedside, in presence of the patient, the whole of the medical doctrines bearing upon his case, as laid down in books and in the schools. And as I said we are sent here to *school*, so I might make the wards a *schoolroom*, and nothing more ; I might run the risk of forgetting what is due to the patient, in making his case matter of instruction for you. I recognise here a serious danger, amply illustrated in some of the foreign systems of clinical instruction. I need not tell you, therefore, that this is not the way we are about to proceed ; because, as I remarked at the beginning, we have a double object. We have to study disease, and we have to study also the medical management of disease.

Now, remember that the medical management of disease is, to you, nothing less than the business of your lives. If I fail to teach you that, I teach you nothing. If I take you into the wards and teach you physical diagnosis, for example, but forget to teach you the medical management of disease, I teach you nothing that will be

of permanent service to you. If I even omit to teach you those little courtesies of the sick-room which are often all in all to the physician, I have forgotten not the least part of my duty. I must teach you, not only how to know diseases, but how to treat them; and among the little details of treatment and management which are of no small importance—I had almost said, which are of the greatest importance—are the manners and habits, the way of thinking and speaking and acting, proper to the sick-room. I must teach you—not always by words or in lectures, it is true—how to approach your patient, how to communicate with him, and how to leave him. The rules of the hospital, and the necessities of clinical teaching, permit us to approach the patients together; they permit us to surround the beds in numbers; they give us free access to the sick-room, where, as a general rule in private practice, few are present. That is a necessity of the case, and it is, on the whole, a useful necessity. I do not think that, with proper precautions, it is one likely to be abused in our wards. But this very necessity of the case appeals only the more strongly to our good feeling, and requires of us not to let slip from our minds the central idea from which we are to proceed; which is, that the teaching of clinical medicine is not alone the imparting to you certain doctrines about disease in connection with the individual patient, but also the communication, by precept and by example, of everything bearing on the treatment of the sick; by which I do not mean, of course, the medicines and prescriptions only, but a

great deal of what is both more difficult and more important.

The first thing we have to learn, then—and let everything be throughout kept subordinate to that—is the management of the sick-room. We have to look upon the hospital ward simply as a large sick-room; we have to do everything in it, as far as possible, just as we should in a private sick-room. You will observe at once that this view of our duty imposes certain limits on our instructions at the bed-side; but if you have rightly understood me, you will perceive that it does so as much in your true interest as in that of the patient. I wish this to become an element in your instruction; I wish it to become an all-pervading feeling in our clinical course. One thing is certain, you never can become physicians, in any high sense of the word, till you are pervaded by this feeling; and therefore the sooner you begin so to feel, and so to act, the better alike for you as students and for us as teachers; the better, too, for your patients and for ours, whether now or in the far future.

Now, then, here is another thing that I wish to say to you at the outset of this clinical course. My colleagues and I are, according to the regulations of this hospital, wholly and exclusively responsible for the treatment of our patients in the several wards. I wish you to understand that this is a responsibility of which I, for one, cannot possibly divest myself. I should simply give way to a fallacy if I allowed you to suppose, even for a moment, that your opinion, formed from books or from your other teachers, or, indeed, any

one's opinion, except in so far as it may influence my individual judgment, is to have anything to do with the treatment of the patients in my wards. It is I that must treat these patients, and no one else; unless indeed the resident physician in my absence. It is I that must decide in my own mind all about the cure of the sick, and so maintain inviolate my own responsibility. How am I to do this—for that is a feeling I cannot lay aside—and at the same time make the treatment of the sick matter of instruction to you? Some of the modern systems of clinical teaching attempt to accomplish the double object of teaching the student and treating the sick by more or less completely shifting the responsibilities of the physician to the shoulders of the student. The student is held responsible for part of the investigations on which treatment is based—he notes the facts, he in some degree forms the opinions; the physician stands by, and is a kind of "guide, philosopher, and friend" of the student; but in the main he is content to devolve upon his assistants, as it were, a good deal both of the labour of observation and the responsibility of treatment, which must, of course, be based upon observation. The student is called up to the bedside; he states his opinion, and defends it; he proposes a treatment; it is accepted or not; a discussion takes place, and the treatment ultimately employed emerges, as it were, from the joint reflections of the student and of his teacher, acting upon one another at the bed-side.

Now, I have tried this plan fairly, and I tell you frankly that it does not do. It is, in my opinion, a

showy and specious, but not a genuine or truly practical method of clinical teaching; for it leads, in the first place, to an almost unlimited waste of the time properly due to the sick and their concerns; and in the second place, to a sort of conversation, and to habits of thought and action, unfitted for the sick-room at all times, especially in cases requiring delicate and careful moral management. I might add, that my experience of the histories of disease recorded according to this method is not favourable. I have found such records to be too often unfaithful records, even as to the facts—pictures of disease drawn according to the fancy of the recorder, and vitiated by the confusion of the patient, or by suggestions arising at these bed-side discussions, and converted into apparent facts by being insisted on in presence of the sick.* You cannot expect to get a clear

* It was the observation of these anomalies, and particularly of the errors arising from suggestion on the part of unskilful reporters, that led me in the first instance to diverge from the established practice in the Edinburgh Royal Infirmary, of having the histories of disease regularly reported by the "clinical clerks" before the first visit of the physician. Probably no part of the record of a case, not even the more recondite facts of physical diagnosis, is more apt to be insensibly biassed by preconceived opinions, than what is technically called the "previous history" or "anamnesis." To commit this to the ready and willing pens of assistants, however able and well trained in a system, is not, in my opinion, a satisfactory way of disentangling the intricacies of mingled fact and theory which generally enter into a patient's story. For several years past, accordingly, so far from demanding the record of the cases on admission at the hands of my subordinates, I have usually dictated personally at the bed-side, at the very first visit to a new case, the whole of the more essential facts of the history, as given to me by the patient, and in the very manner and order in which the facts have been elicited. These

narrative of facts out of a controversial discussion of opinions; it is not according to human nature that you should do so. But the main objection I have to this way of teaching is, that it is inconsistent with the primary relation of physician and patient, and therefore with the habits of the sick-room, which it is my duty

records are the basis of the clinical lectures, as delivered on succeeding days; and I have constantly found the advantage of being able to refer to them as conveying, not only the very facts observed, but the very order of observation of the facts; and thus, by implication, the degree of their actual relative prominence, the extent to which they were brought out by leading questions, etc., etc. All these details enter into the actual narrative of the case, as thus recorded, and all of them have been again and again useful in determining points of theory or of opinion afterwards. This mode of case-taking was adopted by me, more or less completely, from the very first; and is distinctly indicated, in principle, in the fragment of a written lecture (p. 703), which I find to have been delivered on various occasions since the commencement of my duties as a clinical teacher, and which contains the elements of the method of clinical observation and instruction more fully set forth in the present article. It is perhaps right to add, that my innovations, though very unobtrusively and gradually introduced, were at first viewed with distrust by some of the very ablest of my resident physicians, who feared that the method here referred to might prove unpopular with the students, by taking up too much time at the ordinary visit. To say the whole truth, I was also not without some fears on that subject; but as the matter was with me one of principle, and not of expediency, I persevered; and I believe the result has been, that most of the objectors, actual and possible, are now convinced of the manifold advantages of my method, not indeed as a mode of getting up voluminous "cases" with the least amount of personal labour to the physician; but as the best way of securing really faithful and vivid pictures of disease, on which to found instructions in clinical medicine. The cases introduced into this book are, with a few exceptions, transcripts or abstracts of my own notes thus dictated at the bed-side, and afterwards referred to in the lectures.

to teach you, whatever else I teach you. The discussion of opinions at the bed-side, to the extent required by this method, assuredly tends to a feeling of uncertainty on the part of the physician as well as of the patient, unfavourable to the proper management and cure of disease. I am content, therefore, to sacrifice whatever of apparent advantage there is in this method of teaching. And I tell you frankly that I will always, as far as is possible, observe for myself the essential facts of every case ; and that I will, from my own observations for the most part, determine what is to be done, without respect to what you think or have to say in the matter. In many cases I will not engage even to tell you the whole process of my reasoning, though as far as possible I *will* endeavour to do this, either at the bed-side or in the lecture-room.

This must be my method, for the reasons I have already told you. But I hope to make it instructive to you in this way. You come here to learn, by contact with an educated mind in connection with the sick, how disease is actually viewed as a matter of treatment; and the way in which I can best enable you to do this is simply by what I will express in two words—by *thinking aloud.* My object throughout will be to think aloud —to have no mental reservations—to make everything come before you just as it comes before my own mind—to tell you, or to convey to you without formally telling you, what I think of every individual fact—every individual symptom, at the moment it emerges ; and therefore, as a part of this plan, to unfold, as far as possible,

both the histories and the theory of the cases before you, by writing down the facts, for the most part, in your presence, causing you to observe them, and to know and feel, as it were, their true significance as we proceed. You will find that this idea pervades our whole system of case-taking. You will see in it, if I mistake not, my anxiety for your instruction—my anxiety that the whole conduct of the investigation should be, as much as possible, under your eyes—my wish to make the very statement of the facts, as far as may be, a *thinking aloud* in your presence.

Now you will say,—"Is not this, too, a violation of the primary idea of conduct in the sick-room? Are there not things very proper to be spoken between student and teacher, which must not be spoken in the presence of the patient?" No doubt there are. There are certain things, for example, in medicine which you can easily understand, but which a patient cannot understand. There are certain things which may be stated in language such as will enable you to appreciate their true value, but which will convey exaggerated or erroneous impressions to the patient. These difficulties we must carefully keep in view in all that we say at the bed-side. You must endeavour to understand and follow me when I am trying to soften a painful impression to the patient, or to prevent him from taking an unduly exaggerated view of a fact in his case. You must endeavour to enter into the spirit of my teaching—into the feelings with which I am speaking, as well as into the mere words that I speak. But when that is done all

the rest is plain sailing, so to speak. I want to have no concealments, no mental reservations, no shirking or speaking round about the plain facts of a case. I wish also, for your sake as well as my own, to have no double language; one for the bed-side, and one for the class-room; one full of exaggerated sympathy, the other in the ordinary, hard, indifferent tone of scientific investigation. I wish, in speaking to you and to the patients, to preserve the same character throughout; and whether in the class-room or at the bed-side, I must beg of you all to aid me in this. Let us all endeavour to fulfil the idea beautifully expressed by Sydney Smith in one of his brilliant conversational sayings—so full at once of wit and of wisdom, of the finest humour and the most genuine practical philosophy. "Here," he said, "we live with open windows." Let us study, in general, so to speak, that every one about us may know exactly what we are and what we think—our patients amongst the rest. And, trust me, that is the best way in general to deal with the sick. Have no mental reservations with them that you can possibly avoid. I trust that we shall all do our best to make that the pervading spirit of the clinical teaching in this hospital; and, if we rightly understand the spirit, the details of teaching will emanate naturally, without any fixed method of proceeding, from the consideration of each case as it comes under our notice in the wards.

A word or two only as to some things which it is especially desirable to avoid. There is a bustling, pretentious manner which springs from the wish to produce

an impression—and which often does produce a false impression—of superior knowledge. Avoid this by all means, for it is very offensive at the bed-side. We have none of us occasion, you will find, to plume ourselves on what we know; far oftener we have to lament our deficiencies; to feel humbled that we know so little, especially so little of what is practically useful. Remember always that here we are all alike students. Though officially your teacher, I am in this hospital, and at the bed-side, a teacher only in an indirect and limited sense; and the limitation is not of my making, nor is it really to your disadvantage. My place here is to be a student like yourselves, only invested with a responsibility that you have not.

Another thing that we must try to avoid is the unnecessary use of technical terms. I do not mean that we can avoid them altogether; but we must try not to use them in an absurd and pedantic way, because the practical effect of that is that the patient is apt to take what is unknown for something very terrible; and if you use technical terms too much, he will be apt to suppose that you have something to conceal from him, and that he is labouring under some very unusual and tremendous form of disease. To sum up this argument in a few words; let us all try to keep up in the wards as much as possible of the appearance, and manner, and language, and conduct that we should like to see at the bed-sides of our own private patients, or at our own bed-sides for that matter. Let us try to exhibit the same candour, the same simplicity of pur-

pose, the same spirit of sympathy and cordiality with the sick here that we would maintain in private life.

Nevertheless I must not forget that you are to be instructed about some points which are strictly technical, and which require a great deal of minute and careful consideration as to the best mode of communicating them. For this purpose, amongst others, we meet once a week, at least, here in the lecture-room; and I may occasionally bring you here at other times when I have something particular to say. But in general, a great deal of this may also, with due preparation and selection of cases, be done without offence or injury at the bed-side. From experience, too, I find that the best way of proceeding is usually suggested, to a great extent, by the consideration of the particular circumstances of the case. You will find that at the bed-side I shall now and then call you up individually, and make you observe things for yourselves. If there is anything new and curious to be noted, I will always endeavour to give as large a number of you as possible the opportunity of observing it. And with regard to physical diagnosis, and especially auscultation, which is often a difficult matter to manage in accordance with the feelings of the patient, I will try as much as possible, even in recording the facts, to make you observe them along with me in every instance. Where this cannot be done with the whole of you who may be present, I will usually take up one or two of you and make you observers for the rest; and you will find that I very rarely indeed write down anything as a fact which is not observed by some

of you, as well as by myself, to be a fact. I do not say that I shall be able to let you all hear and see everything, because that is impossible; but I will let some of you, and as many of you as possible, hear all that is to be heard and see all that is to be seen.

I need not tell you that with regard to such subjects as the examination of the urine and some of the more ordinary elements of physical diagnosis, in which the illustrations are constantly at hand, we shall be able to give you tolerably complete instruction. We shall give ourselves carefully to this early in the session, and a special series of instructions will be given on these points both by my colleagues and myself. I trust that in the study of physical diagnosis you will find considerable advantage in recording your observations by means of those outline figures or clinical diagrams which I have employed more or less for a good while past, but which have now been issued by Messrs. Maclachlan and Stewart in a form available for all of you, and by no means expensive.* I strongly advise you to obtain these diagrams, and to employ them habitually in the wards. They are to be had in the form of loose sheets; also in the form of books, most of which are interleaved, to allow you to make notes of the case upon the intermediate leaves. The use I intend to make of these outlines is to note down, by a set of conventional marks which I will explain to you in the

* Outline Figures for recording Physical Diagnosis, for the use of Students and Practitioners of Medicine. Edinburgh: Maclachlan and Stewart.

wards, as much as possible of the physical diagnosis; and I intend that you shall keep a similar series of notes, and employ them as an aid to the memory in future observations.

Now, I believe that I have said all that is necessary in the way of general introduction to this course. In conclusion, I wish you fully to understand the advantages that you will derive from the system introduced into this hospital some years ago by the common consent of the ordinary physicians, and in regard to which I think I may fairly claim for myself a considerable share in its introduction, namely, the system of making one ticket give admission to the clinical instructions of all the ordinary physicians together.* By means of this arrangement we have been enabled to throw open to you a very large field of very various information, both general and special, under the management of different physicians, each of whom of course will have, in some points, his own way of thinking and acting. You will soon, I doubt not, find out for yourselves in what order it will be best for you to attend the different wards, and what time you ought to give to each clinical visit.

* The system sanctioned by the managers for many years before the period referred to was for the first and second ordinary physicians to give a course of clinical instruction in alternate years, the remaining physicians not being allowed to lecture.

REMARKS ON "CASE-TAKING."*

Much has been written, and much has been spoken, in books and lectures accessible to you all, upon the subject of clinical study, and particularly on the method of examining patients with a view to diagnosis and treatment. It is a subject that may be expanded to any extent, so that I might readily occupy half of this course of instruction in enunciating precepts for the conduct of the other half. I need not say that I shall not do so; for the proper object of this course is not so much to tell you what *ought to be* done when you are placed face to face with a patient as to let you hear, and see, and know, what *is* being done with patients actually before you. We have much to do, and little time to do it in; vast opportunities, but few hours for their improvement. You must not be surprised, therefore, and still less disconcerted, if I take you into the wards and enter on our studies with what may appear to you but slender preparation for the task. I tell you beforehand that it is of set purpose that I do so, being satisfied from personal experience that the best method of clinical investigation is that which will most readily occur at the bed-side, and in the actual presence of disease. To any man having that combination of good sense and good feeling which is necessary for every kind of dealing with

* First delivered, I think, in 1856 or 1857, and repeatedly since. See p. 695, *note*.

the sick, a fixed method of clinical study is an obstruction and an annoyance. To the patient it appears invariably unintelligible and mysterious; sometimes terrible and portentous; sometimes, on the other hand, merely tedious and insignificant. In private practice no patient who has the smallest amount of common sense will endure a methodical examination according to any system hitherto invented; and hence when clinical medicine is taught by a fixed method, the first thing that has to be done in private practice is to unlearn many of the lessons, and (what is still more difficult) to do away with many of the habits acquired in the hospital and the clinical class. I have no hesitation in saying that this is a very unfortunate necessity, if necessity it be; and that the nearer hospital practice can be made to resemble private practice on a large scale, the better for clinical instruction, as well as for the patient and his physician. It is needless to point out that for treatmen, and even for diagnosis, this is the proper way; but what I contend for is, that it is the proper way for teaching too; that anything which interrupts the flow of ideas naturally arising between the patient and his physician when placed in easy, familiar, and unreserved communication, is to be avoided, and this especially in a first examination. There can be no doubt that the following of a rigid and inflexible method is an interruption to such unreserved communication, and gives, from the very first, a dry and pedantic character to a physician's inquiries. Much more is it so when the method is a very complicated one, so that neither its details nor

its results can be contained within the compass of the brain; and the clinical inquirer has to be constantly travelling in idea from the patient and his symptoms to some manual or written scheme, either kept *in retentis* or spread out before him. Such a process is most destructive to the confidential and sympathetic character of the intercourse that ought to reign at the bed-side. If you make use of such clinical aids at all, therefore, you should read them and digest them chiefly at home. Bring the *results* of your reading, if you please, into the wards; let your ideas be carefully stored, digested, and arranged, when you proceed to the examination of a patient; read at home upon the particular subject of his case, and return to make your inquiries as often as is necessary; but accustom yourselves to do all this quietly and unobtrusively, without any preconcerted plan or any unnecessary display of the machinery of knowledge.

There is a short and uniform rule for "case-taking" in hospitals, and it is the same that I would recommend to you for examining cases in private practice. Go about the matter simply and quietly, just as you would in eliciting any ordinary information.

Address the patient clearly and intelligibly, as taking a real interest in him. Ask him what he complains of; place him at his ease; and let him, in the first instance at least, tell his own story. Allow me to assure you, as the result of my whole personal experience, that you will very rarely indeed be baffled in getting at the more significant facts of the case in this way. It is in the nature

of the heart of man, all the world over, to open itself freely to inquiries made in a spirit of genuine, unaffected candour and simplicity. Few patients come into hospital without having made up their minds to speak frankly to their medical attendants; though it must be admitted that, through shame, through self-love, through exaggerated ideas of small matters, through sheer stupidity, and last, not least, through long-continued perverse and irrational doctoring, their own ideas may have become so confused, as to try the patience of the physician not a little. There is but one method of getting over these difficulties; *gain the heart of your patient*, and you have gained the key to his case; fail in this, and you may labour in vain with all the systems and all the notes in the world to help you. Nay, it is precisely when your patient is most perverse, or fanciful, or stupid, that your note-book, and your fixed and formal questions, do most mischief. The least appearance of art on your part—the faintest trace of what may be construed as impertinent curiosity or skilful management, suffices to baffle your inquiries. The patient's mind is on the rack, and he either takes refuge in sullen silence, or indulges in answers made to suit what he fancies to be your particular whim. He is all attention to your words, and gives much more consideration to them than to his own feelings. If you are busy with your pen or pencil, so much the worse; the unhappy patient believes that every word he utters may be brought up in judgment against him; and like a witness under cross-examination in a court of justice, he studies to say as little as

possible, and to work up what he says into a consistent story, at all risks ; or perhaps he declines to say anything at all.

" Are ye axing me as a magistrate, Monkbarns," says Edie Ochiltree to the antiquary, " or is it just for your ain satisfaction ?"

" For my own satisfaction solely," replied the antiquary.

" Put up your pocket-book and your keelivine pen,* then, or I downa speak out, an' ye hae writing materials in your hands —they're a scaur to unlearned folk like me. Odd, ane o' the clerks in the neist room will clink down in black and white as muckle as wad hang a man, before ane kens what he's saying."—*The Antiquary.*

The novelist tells us that the antiquary complied with the old man's humour ; and depend upon it that you, too, had better consult the humour of your patients, even when it is not so unmistakeably manifested as by Edie Ochiltree. Many people who are far less clever and amusing than the old bluegown, are apt to be quite as cautious and reserved in presence of the keelivine pen, and the other accessories of a formal examination.

There is, however, a wide difference between the multiplication of teazing and formal interrogatories, and the authoritative short questioning which is often necessary for determining the facts of a case. As a general rule, the patient should be led himself to see the object, and to understand the course of your inquiries ; but he should never be allowed to tyrannize over you with minute impertinencies of detail, or vague theories and notions as to his own case. There is usually no real

* "*Keelivine pen*—a black-lead pencil."—*Jamieson's Dictionary.*

difficulty in making a patient understand, in a general way, what you want to be at; that *you* are responsible for making out everything as to the cause of his disease and the proper treatment of it; and that *he* has nothing to do but to give you a plain unvarnished story, detailing not what he *thinks*, but what he *feels* and *knows* to be true. It is worse than useless to wrangle or to debate with a patient who makes an imperfect statement; the true way is to convince him that it is imperfect, and at the same time to make him feel that he is trifling with his own interests, as well as with your time. Those who proceed in this way will, I think, very rarely fail.

The truth is that those physicians are generally much to blame themselves, or, what is worse, they are totally unfitted for their business, who find their patients habitually either very stupid or very prone to deceive. For if a patient comes to you for advice, and clearly understands that you have nothing but advice to give him, he is not only a rogue but a fool if he does not come prepared to tell you the truth. Of course, in hospital practice, and still more in pauper practice, there are often inducements to malingering and deceit; but making every deduction on this account, the amount of positive dishonesty shewn in well-conducted establishments is not large; and self-deception, which on the other hand is very common, is generally as much a symptom of the disease as any of its bodily characteristics. A little temper, intelligence, experience, and the undefinable quality called *tact*, will carry you easily over every common difficulty.

APPENDIX.

A.—Sequel of a Case of Intermittent Phthisis (p. 6).

In the Retrospect of Cases treated during the Session 1855-56, I noticed two very similar cases of phthisis, as presenting a marked contrast in regard to their progress, under as nearly as possible the same conditions of regimen and treatment. I accidentally omitted in the proper place to give the sequel of the story, which was recorded in the *Edinburgh Medical Journal* for August 1856, p. 130. The survivor of these two patients was a friendless lad, Archibald M'K.; and as he was of good character, and anxious to work, I got him admitted, not without misgivings, as a servant of the house; part of his duties being the work of the pathological theatre, which he performed remarkably well under the direction of Dr. Haldane. For many months he was so ruddy and stout-looking, that no one would have supposed him an invalid, unless from previous knowledge, or from observing the care which he took of himself, and the feeling of personal insecurity which he betrayed occasionally in his voice and manner when questioned about himself. In March, as stated in the text, he broke down again, the physical signs in the lung being almost exactly the same as on the previous admission to my wards. From the month of May onwards to July 16th, when he died, his downward progress was exceedingly rapid, and large quantities of cod oil, with oil-inunction used externally, and other tonic measures, were found inadequate to check the disease, even in the slightest degree. Within the short space of ten weeks, the lung underwent a truly colliquative suppuration; the sputa being in the end nothing

but pure pus, with debris of tissue. The hopes that I at one time entertained of a permanent improvement in this case, therefore, only shew more forcibly than ever the difficulty of anticipating the course of tubercular disease in any particular instance.

B.—Sequel of a case of Enteric Fever (pp. 130-133).

I am indebted to my colleague Dr. Sanders for the information that Christina M'L., who passed safely through a very severe attack of enteric fever in the end of 1861, was readmitted in May 1862, and died of acute pneumonia, which could not be traced as having any apparent connection whatever with the state of health left after the fever. It will be observed, on referring to the case, that it was one of those in which the abdominal symptoms were singularly latent, there having been not even the slightest trace of diarrhœa, or of other disturbance of the alimentary canal, until the fourth week of the fever, when four loose stools, three of them containing blood, were passed in the course of four days; the bowels then reverting to their natural, or rather confined state, as at first. The disease was perfectly defined by the eruption; and although the local complications were indistinct at first, it was regarded all along as a case of considerable danger, owing to the rise in frequency of the pulse, and the long-continued hectic flushing and emaciation. The convalescence was slow, but uninterrupted.

Under these circumstances, the following brief notes of the *post-mortem* examination, by Dr. Haldane, will be read with interest:—

Christina M'L., examined 15th May 1862. Appearances in thorax, those of pleuro-pneumonia of left lung, involving the whole lower lobe, and at least half of the upper. A thin layer of lymph, also, coated the pericardium.

In the lower part of the ileum there were 12 or 14 cicatrices, of rounded or oval form, from the size of a shilling to less than that of a split pea, very superficial, so that they were rather indistinct unless the intestine was held up against the light; the cicatrized surfaces and

edges quite smooth, and covered by what had a general resemblance to mucous membrane, but without the characteristic velvety appearance, and without villi. These cicatrices had not given rise to even the slightest contraction of the intestine. The mesenteric glands and spleen were natural in appearance.

C.—Scarlet Fever (p. 190).

It must be added to what is here said of the favourable result of sixteen cases, that one of these patients, a respectable young servant girl, returned to me about two months after her dismissal, complaining in general terms that she found her work too hard for her; the only local complaint being of some dull pain in the back. On examining the urine, it was found to be very decidedly albuminous, and yielding a sediment of tube-casts, but no blood or evidences of acute desquamation. I fear there is little doubt that in this case the foundation has been laid of chronic disease of the kidney; for although this girl looks well, and would hardly be taken for an invalid, there is a tendency to depreciation of the urea, and at times even to great diminution of the absolute quantity of the urine, which remedies carefully used have failed to overcome. There is no dropsy, but the stomach has become very easily disturbed; and this symptom I should view as a very unfavourable one, were it not that there is reason to suppose it due in part to a hysterical constitution, which has been manifested in various ways since she came again under notice. It is just possible that there may have been a renal affection antecedent to the attack of scarlatina in this case.

D.—Sequel of a Case of Hysterical Mania (pp. 252-255).

In making some inquiries lately in regard to cases sent from the Infirmary to Morningside Asylum, I was sorry to find that this poor girl's case terminated fatally within a fortnight after her admission to the Asylum; the immediate cause of death, however, not being cerebral, but an

unmanageable form of ulceration of the rectum and nates. The insanity had assumed the characters of melancholia, the maniacal paroxysms having entirely subsided before she left the hospital.

E.—Sequel of a Case of Mania, simulating Delirium Tremens (See pp. 279-281, 283-284).

I am indebted to my friend Dr. Yellowless for the following particulars of the rapidly fatal termination of this case; which appears to have presented, on the whole, the characters of a rather exceptional and peculiar variety of general paralysis, marked by intercurrent mania.

Abstract from the Records of Morningside Asylum.

* * * * On admission, 11th January 1862, A. seemed very weak, was maniacal and incoherent, and had a slight occasional hesitancy of speech, which suggested general paralysis. His general appearance, too, corroborated this suspicion.

As the excitement gradually subsided, this affection of the speech disappeared, and about ten days after his admission there was nothing to justify the diagnosis of general paralysis except inequality of the pupils.

Ere long, however, he began to manifest the delusions so characteristic of general paralysis; he said that everything in his room was made of gold, and began to collect any trifles he could find, supposing them to be of great value.

His health now began to fail rapidly; general anasarca came on, and he gradually sank till the 3d February, when he died.

On examination of the head after death, the arachnoid was found to be adherent in several places both to the brain and to the dura mater, and at these places it was much thickened; its sac contained about 1½ ounces of fluid. On section, the outer layer of the grey matter had a dull opaque look, quite different from the inner layer; the white matter

generally seemed œdematous ; the membrane lining the ventricles was firmer, or tougher, than usual, and had many small transparent granulations on its surface ; these were largest in the fourth ventricle. The encephalon weighed 52 ounces.

The general anasarca was explained by the condition of the kidneys.

On enquiry at a brother who was present at the examination, it was found that A. had occasionally manifested similar delusions while at home—imagining that common things were made of gold, carefully hoarding and setting great value on them. He had never thought of telling this until directly asked about it.

F.—Sequel of a Case of Pleuritic Effusion, treated by Paracentesis (Case IV., p. 325).

This man (Wm. C.) still remains under treatment ; the physical signs, however, have undergone a considerable change since the passages in the text were written, and these changes have led to renewed inquiries as to the earlier history of the case, which shew that the record at pp. 325-328 was written in some respects under an erroneous view of some of its details. The progress of the fluid towards suppuration between the first and the second tapping (which I remarked upon in the note at p. 327), although giving a somewhat unfavourable bias to my opinion, did not, in the absence of direct evidence, suggest that there might have been air in the pleura at a former period ; and, as already stated, there was certainly no direct evidence of this, on most careful examination, both before and after the second operation. Within a few weeks, however, the physical signs underwent the rather remarkable changes represented in the diagrams here given (Fig. 31, A, B, C, which may be compared with Fig. 5, A, B, C, in p. 326, and the explanation in p. 327). At the same time, the patient became distinctly sensible of the sound of jumbling of fluid and air together in the chest ; and similar sounds were quite apparent to every one on examination. It was

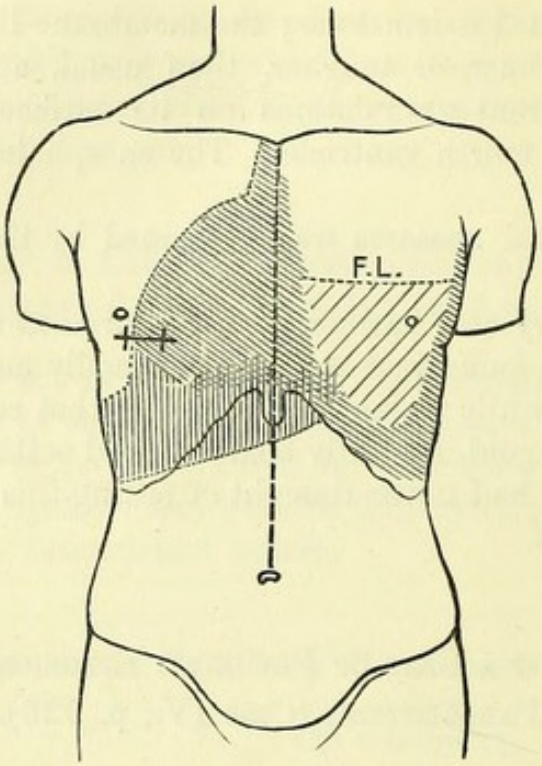

Fig. 31, A.

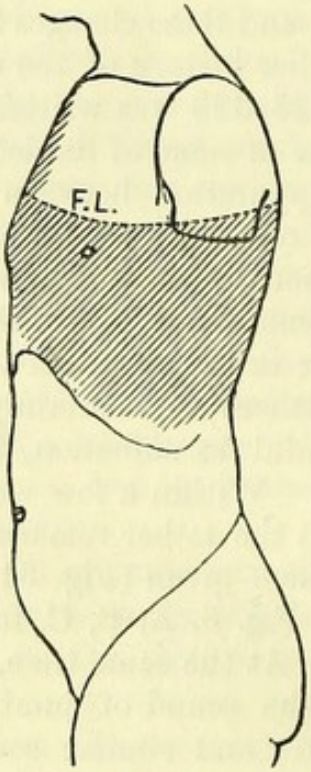

Fig. 31, B.

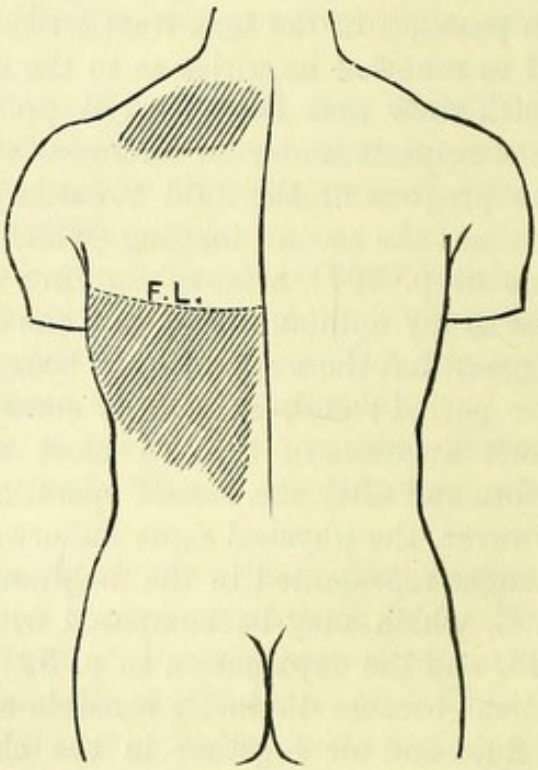

Fig. 31, C.

now that the patient told me for the first time a history almost precisely similar in its details to that of P. J. at p. 354 ; that after the first symptoms of pleurisy, and before the first operation by Dr. Ballantyne, he had been sensible at various times of the splashing or jumbling sound in the chest on shaking or walking about ; that it had also been observed, though less distinctly, in the interval of the two operations, and had then ceased, until it re-appeared a few weeks after the second tapping, in accordance with the physi-

Explanation of Diagram.

Fig. 31, A, compare Fig. 5, A, p. 326.

Front of thorax and abdomen, in case of Wm. C., as examined on May 26, 1862. The closer shading in this fig. represents the limits of dull percussion in the recumbent posture ; the unshaded portion of the left side in front (including in this description the wide shading below the line F.L.) indicates a space almost uniformly tympanitic on percussion, and which had replaced the former dulness. The line F.L., limiting the wide shading alluded to, corresponds with the fluid level in the erect posture ; in this position all below F.L. was dull, while all above was tympanitic. The heart and right lung are in much the same position as in Fig. 5, A ; but the liver has risen somewhat since the second operation. It ought to be added that immediately after the second tapping the heart's apex approached the middle line ; but was evidently protruded once more towards the right by the air which accumulated in the pleura within a few weeks after the operation.

Fig. 31, B and C. Compare Fig. 5, B and C, at p. 326.

Back and side view of the percussion dulness in Wm. C. ; corresponding to Fig. 31, A, in so far as it represents the results of examination in the erect posture. The fluid level, F.L., is seen continued around the chest. A little comparative dulness at the summit and close to the sternum correspond in all probability with the seats of dense pleural adhesions.

cal signs, as observed by me. On learning these facts, I wrote to Dr. Ballantyne, with whom I had previously had only a single very hurried interview in reference to the case. The following clear and satisfactory letter of explanation will place the whole facts before the reader in the same way in which they became known to myself. The case is plainly one of those, not very uncommon, in which it is impossible to be certain whether the pleurisy preceded the pneumothorax ; or whether, on the other hand, a limited escape of air from a tubercular cavity may have been the exciting cause of

the pleurisy, or at least a nearly coincident fact. Like several others of the same kind which I have seen, this case is extremely interesting in its bearing on the theory of pneumothorax, as discussed at large in Art. XV., and in Appendix C.—

SELKIRK, *9th June* 1862.

"I am afraid I did not give you a proper history of Wm. C.'s case when I was in Edinburgh; indeed, I can scarcely recall what I did say; but the facts are that C., when I first saw him, was suffering from a tubercular affection of the upper part of the left lung; some months afterwards, he had a distinct attack of acute pleurisy, accompanied with effusion. Some time after this, I heard succussion-sound, not very distinct, but plainly enough; there were, however, no metallic phenomena, and there was no great amount of preternatural clearness in any part of the left side. A few weeks after this, the succussion-sound disappeared, after which he was tapped. I had then the idea it would prove an empyema, but the fluid drawn off was thin and without pus. I stopped the withdrawal of fluid before the stream began to intermit, and was perfectly sure there was no air admitted through the canula. Then after this a little "succussion" again was audible, but not very plainly. It again disappeared on the collection of a greater amount of fluid, and there was nothing of it when I sent him to you. I am only sorry I did not keep a report of his case, but what I have stated above are the principal facts."

Since the preceding paragraph was written I have once more (June 27th) minutely examined the physical signs in this case with the following curious result:—The large amount of air which must have been present on May 26th, and which is indicated by the tympanitic percussion in Fig. 31 (it can hardly have been less than several pints), has been so far absorbed that when the patient is carefully examined in the recumbent posture, there is now only a very limited tympanitic space in the front 3 by 4 inches in diameter, which obviously is floated upwards according to the position of the patient, and is only sufficient to give rise to an almost doubtful succussion-sound heard under peculiarly favourable circumstances, and which might easily be simulated by the conveyed sounds of the stomach. Several good ears to-day were unable to detect succussion-sound at all.

G.—Letter from Dr. Bowditch of Boston, U.S., in reference to Thoracentesis.

The following letter, received since the chapter on Pleuritic effusion was printed off, will, I am sure, be acceptable to the reader. A careful perusal of the cases published in the text will be found to furnish corrobative evidence of some of its positions, and the large experience of the author will secure for his opinions all the attention they so well deserve :—

Boston, U. S. A.
May 22, 1862.

My dear Sir,—I gladly avail myself of the opportunity afforded me, by your letter of May 2, of writing to you upon the subject of paracentesis thoracis. Nothing has given me more sincere pleasure than the prompt manner with which you and Dr. Budd of London recognised the peculiar value of this (Dr. Wyman's) method of operating. I have no doubt that many valuable lives would be saved, if medical men would lay aside their theories and fears, and simply *try* the operation. In order to aid in this most desirable change in the sentiment of the medical profession, permit me to lay before you a brief sketch of some data which I gave, from my private medical records, to the students of our Harvard medical school during the last winter's course.

They have never been published, although I intend to present the chief results to the Boston Society for Medical Observation, and the paper *may* appear during the summer. Meanwhile you may make whatever use you choose of the data thus afforded.

Between April 17, 1850, and December 17, 1861, eleven years and eight months, I operated 150 times on 75 persons, and saw ten operations of the same kind by others, making 85 persons operated upon, and 160 operations. One lady I tapped nine times during eight months and a half, commencing when she was four and a half months pregnant, and when the orthopnœa was threatening death. She was delivered of a living, though puny, child at the full term.

She is now well. I operated very recently on an elderly physician eight times in about six weeks! In his case there was organic disease, and the orthopnœa was excessive. The relief he obtained was so great that he demanded a frequent repetition of the operation. He playfully called it his "luxury."

29 of the 75 patients got wholly well, and the operation was apparently the first means of arresting the serious nature of the symptoms. At times the recovery was very rapid, and there was no return of the fluid. One of the most remarkable was a youth who had had obscure symptoms for nine months. I recognised, by the physical signs, "latent pleurisy," or "idiopathic hydrothorax," as some may style it. I removed over four pints at one time, and the lung was fully expanded in forty-eight hours—and in three weeks the patient seemed well, and has continued so.

In a few of the earlier operations no fluid was obtained, and in which either (as in one case) a tumour resembling pleuritic effusion, or the lung itself was punctured. No evil resulted. Strange as it may seem, all these patients were brighter after the operation than before. From the character of the fluid drawn, I am able now to make a partial prognosis of the result of any case.

In 26 out of 75, *serum* was drawn, and 21 of the 26 got well. If after one or two operations, the fluid becomes purulent, an almost certain fatal prognosis is to be made. I have seen six such cases—four died, two were lost sight of, but, when last seen, were failing. *Pus* was found in twenty-four patients. Once it was of the consistence of honey, yet I was able to draw it easily through the small tube. Eight got well, seven died, nine were *relieved* one or many times, but they had either a long tedious illness, terminating usually in phthisis, or fistulous opening, or a doubtful result.

A sanguinolent fluid, and by that I mean a dark red thin fluid, and evidently *chiefly bloody*, though not coagulating at the *first* operation, I consider almost certainly fatal. There were seven of these—six died, one had a doubtful result, but fatal tendencies were arising when last seen. If a *bloody*

coloured fluid appears at the second or any later operation, I do not deem it so necessarily fatal in its indications. A mixture of *bloody and purulent fluid* is usually fatal. There were three cases all fatal. Only one case of very fœtid *gangrenous* fluid was met with. Infinite relief from horrible dyspnœa was procured by the operation, so that the patient and friends were equally gratified. No return of the dyspnœa, but the patient sank in about four days with gangrenous pleurisy.

I have operated once in *pneumothorax* with temporary relief, so that comfort was obtained for several days. I have never had but one opportunity for trying the operation in such a case. I know that a great many *theoretical* arguments might be used against it. I have only to say I shall *try* it when I see a fitting occasion. Finally, in seven cases *I could get no fluid.* These cases occurred in the earlier operations, and were owing, 1st, to my fear of plunging the trochar in boldly, the consequence being, I presume, that the false membrane covering the pleura costalis was not perforated, or was pushed before the end of the trochar; 2d, Failure to make an accurate diagnosis, owing to the dulness caused by a membrane and unexpanded lung, resembling much that caused by fluid; 3d, in one case by mistaking an immense tumour occupying one pleura throughout, and resembling fluid effusion very exactly.

Accomplished surgeons often ask me questions which seem to me the result of *antique* notions on this subject, and which my experience teaches me are absurd. These surgeons say—make an incision first and then puncture so as to have a valve produced over the internal opening. I plunge the trochar boldly and directly in without making any incision or taking the least care for pushing up the skin so as to make a valvular opening. Neither of these precautions are necessary, and to make either causes extra pain to our patient. I operate usually between the ninth and tenth, sometimes tenth and eleventh ribs behind, in a line let fall from the lower angle of the scapula. I tap at any point, however, where, from careful examination, I am satisfied that fluid exists, but of course in the most depending part. I never wait till *pointing* takes place; for then I am sure pus exists.

If pointing has commenced, I pay little attention to its position or progress, but puncture if need be in a better place. I now never operate unless I find some distension or rounding out of the chest, and filling up some of the intercostal spaces so that the chest presents a uniform curve, and not alternate depressions and elevations as in the healthy chest. I operate under the following circumstances where I feel certain there is fluid.

1st, When there is *severe, permanent* dyspnœa, orthopnœa, however acute the disease, if I find fluid *filling* one pleural cavity or nearly filling it.

2d, When there are occasional attacks of orthopnœa, threatening death, even if there be not sufficient to fill more than half of the cavity. If the fluid seems to be the cause of the dyspnœa, I operate because, occasionally, I have lost a patient, when waiting for more extensive physical signs. This rule I apply to acute and chronic cases.

3d, I use the trochar after three or four weeks of ineffectual treatment without any absorption being produced.

4th, In chronic idiopathic hydrothorax or latent pleurisy with simply physical signs to indicate *extensive* effusion, but when the rational signs are either very slight or none at all save a general *malaise* and weakness, etc.

A word upon the objections to the operation. We may puncture the lung; we may let air into the pleura; by strong suction we may tear the lung by forcing it to expand; we may excite pleurisy; we may strike the intercostal nerves or arteries. All these objections fade away before a few trials of the operation. Some oppose it by saying that all cases non-tubercular will get well without it, and with almost any treatment; while, of course, the existence of tubercles contra-indicates the tapping. I take the negative of both these propositions because I have seen facts to disprove them. 1st, Many patients die of simple pleuritic effusion. 2d, I have seen signs of tubercular disease of one lung lessen and almost disappear after the removal of a large quantity of fluid from the other side of the chest. Evidently the presence of fluid caused irritation to the tubercles. Removal of the fluid removed that irritation.

Forgive me if in my desire to popularize this operation among the profession I have taxed your patience unduly by this long communication. Do what you will with it.—Yours, with the greatest esteem and respect,

HENRY J. BOWDITCH.

Dr. W. T. Gairdner.

H.—CASES OF TRICUSPID AND PULMONIC VALVE-DISEASE.

In p. 602 I gave a brief notice of a very curious, if not unique observation of a case of apparently uncomplicated tricuspid obstruction (Patrick M.), in which a very well marked auricular-systolic murmur has existed probably for some years with obvious signs of venous reflux to a great degree, but with comparatively insignificant effect upon the general health of the patient. I have just received a letter from my friend, Dr. Greig of Dundee, which enables me, almost at the last moment before publication, to add a well authenticated account of the present state of this patient. Dr. Greig writes, on the 26th June 1862, as follows:—"I am sorry that I could not see Patrick M. before now. When I visited him yesterday I found him in better health than he has enjoyed for years. He is constantly at work (weaving); but not able to do as much as his companions. The venous pulsation in the neck is as strong and full as ever; and as far as I could judge, the sounds of the heart were the same as when you saw him." Dr. Greig has kindly offered to endeavour to keep the case in view, in so far as this can be done with so independent and so little self-conscious an invalid.

In addition to the cases mentioned in the text (pp. 601-607), another very interesting example of valvular disease primary, or at least greatly predominating, on the right side of the heart, has presented itself to me since the article in the body of the work was completed; and although it is perhaps at present impossible to arrive at a perfectly exact diagnosis, I have thought it expedient to have a diagram cut upon wood, representing the precise facts as they have been

carefully noted at the bed-side. The patient is a highly respectable servant girl, Harriet M'D., æt. 26, who came to Edinburgh from the north country within the last twelve months, supposing herself to be in good health. So far as can be ascertained she has never had rheumatism, nor has she suffered from any severe constitutional disease, nor from cough or difficulty of breathing before her going into service in Edinburgh; and it was only from observing the gradual failure of her health and strength, not from being made aware of any decided acute illness, that her mistress was led to apply for her admission into the hospital. When admitted, she was evidently in very considerable danger; the pulse was feeble, the lips and cheeks extremely cyanotic; there were copious bronchitic râles in both lungs, and evidence of œdema at their bases posteriorly; the patient was unable to lie down, and paroxysms of mingled dyspnœa and angina occurred from time to time in a severe form, the urine being also very scanty. On examination of the heart by percussion and with the hand, it was found much enlarged in all directions, especially towards the right and upwards; the left apex, however, being displaced towards the left and a little downwards, so that the dull percussion of the heart, as indicated in the diagram, was really enormous. The left apex-beat was perfectly distinct, and in the diagram its seat is shewn by an asterisk *; while nearly over the middle of the cardiac dull space, about the level of the nipple, there was felt by the hand a distinct impulsive thrill, corresponding closely in time with the apex-beat, and in position with the point, or small superficial area, represented in the diagram by the sign ×. Around this last point, as a centre, the diagram shews a number of concentric circles or ellipses, which are intended to indicate in a general way the seat and mode of diffusion of an extremely loud blowing, or rather more than blowing, murmur with the first sound of the heart. This murmur, closely scrutinized on various occasions, and at intervals of days and weeks, has always been of exactly the same character; and as it is perfectly homogeneous and very prolonged, I think there is at present no evidence of its having more than one source; further, as

it is purely *ventricular-systolic* in rhythm, it must be formed in some way or other during the *emptying* of the right ventricle. There are, however, in this case, none of the

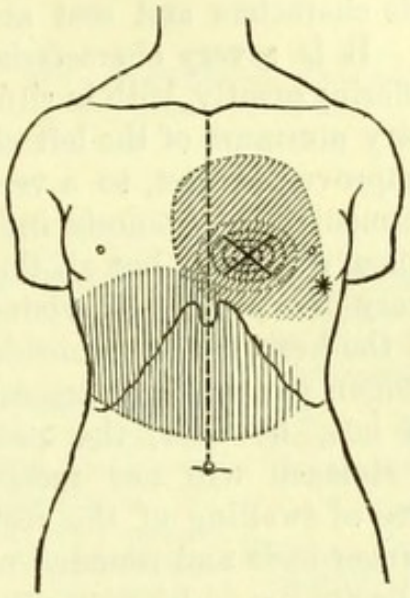

Fig. 32.

Front of thorax and abdomen in case of Harriet M'D. as described in the text; the enlarged heart and liver, the seat of the apex-beat, and that of the impulsive thrill and ventricular-systolic murmur over the right ventricle (tricuspid regurgitation ?) being marked out by the usual mode of notation employed in this work.

clinical incidents (so to speak) usually associated with tricuspid regurgitation, as described in the preceding pages; neither emphysema of the lungs, nor evidence of mitral obstruction, nor of pericardial adhesion. The history of the case is strongly opposed to the idea of congenital malformation; and aneurism of the great vessels (or perhaps an aneurism pressing on the auricles or pulmonary artery, which has occurred to me as a possible solution of the facts of this case) is, to say the least, rendered improbable by the seat of the murmur, and by the absence of any positive facts tending in the direction of such a diagnosis. Nevertheless it is not impossible that the source of the murmur may be complex; and therefore it is not safe, for the present, to exclude the pulmonary artery, or even the aorta, from

a share in its production. The difficulty is increased by the immensely wide diffusion of the murmur, which is heard with great distinctness all over the chest, and even far beyond it in various directions; but repeated examinations have convinced me that its characters and seat are quite constantly as here described. It is a very characteristic example of a murmur plainly differing greatly, both in situation and rhythm, from all the ordinary murmurs of the left side of the heart.*

This patient improved at first, to a very marked degree, under diuretic treatment. The cyanosis, indeed, with a certain amount of orthopnœa, remained, but all the other distressing symptoms were very remarkably alleviated. After a time both the liver and the heart became considerably diminished in size, the latter organ ceasing to be traceable to the right of the sternum. Of late, however, the patient has become worse again; the stomach will not receive medicine, and there is some degree of swelling of the feet. I should add that the clubbed finger-ends and rounded nails, so often seen in such cases, are beginning to be very apparent.

* In my previous experience of cardiac diseases, I can only remember three or four cases, which impress my mind as giving equal evidence with this one, that the primary seat of the disease is on the right side of the heart; but I hold that, with the possible exception of the *perfectly pure and unmixed pulmonic double* murmur (ventricular-systolic and ventricular-diastolic), which is one of the very rarest of pathological incidents, there is absolutely no amount of clinical evidence in such cases which entirely removes the possibility of errors, arising from the want of a sufficient number of facts and from the frequency of unexpected complications. In the case of Mary P., briefly alluded to at pp. 97, 604, the disease probably began in the mitral orifice, but ultimately predominated greatly in the tricuspid. In the case of Patrick M., above noticed, the murmur is far more clearly indicative of primary valvular deformity (see pp. 598, 602) than in that of Harriet M'D., and the leaping movement of the veins (which in Harriet M'D. is much less, indeed hardly appreciable) renders the diagnosis all but certain. On the other hand the marked cyanosis in Harriet M'D.'s case contrasts strongly with the pale anemic habit of Patrick M.; and so far as it goes in evidence, indicates a much greater amount of imperfection of the circulation in the capillaries of the lungs. It is not unlikely that in Harriet M'D.'s case the pulmonary artery may be compressed or obstructed in such a way as to determine tricuspid regurgitation.

I. INDEX OF NAMES AND SUBJECTS.

II. INDEX OF CASES AND FACTS OBSERVED.

THE END.

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