union, or even any sign of reparative action, could be excited. The patient became very weary, and begged to have his limb amputated. On January 4th, 1876, with the assistance of my colleague Mr. Banks, I performed the following operation, the patient being placed under the influence of ether. I made an incision on the outer aspect of the thigh, about six inches in length, and sufficient to enable me to reach the fractured ends of the bone. By means of a small saw and a gouge, I completely bared the lateral aspect of both fragments, each of them to the extent of what they overlapped. After the opposing lateral surfaces of this oblique fracture had been most completely bared, I then passed round both fragments a stout piece of ordinary copper bell-wire, and a second piece at an interval of about an inch and a half. Having placed the wires in their proper relative position, I gradually screwed them up until the bones came into a position similar to that of a splice in a fishing-rod. Having thus screwed up the wires, I had the satisfaction of finding that they held the ends of the fracture in close and firm apposition.

To prevent any slipping, I then passed through the bones, midway between the two wires, by means of an Archimedean drill, a drill-head; this had the effect of completely fastening the two opposing portions of the fracture together. The wound was closed by wire sutures, the other end of the drill-head projecting through the wound. The limb was then placed on an outside interrupted splint, and the patient removed to bed.

Bleeding was entirely controlled during the operation by means of an Esmarch's bandage placed round the groin. The operation was followed by some slight febrile reaction; but, considering the severity of the operation, it was remarkable how little the patient suffered. The chief pain was referred to the position of the drill-head, the slightest touching of which occasioned severe and deeply felt pain. On January 8th, the pain referable to this drill-head was so great that I removed it, fearing that it might set up in the bone an undue amount of inflammatory action. The removal of this was followed by complete comfort. The suppuration was not excessive, and, in the course of a few weeks, the wound was completely filled in with healthy granulation. The wound was not dressed antiseptically. On March 8th, as the wires appeared now to be sources of irritation, I determined to remove them. I had the patient placed under ether. I then passed my finger through the mass of granulations down to the wires, and, by means of a curved pair of cutting pliers, severed and withdrew each wire. This was done without difficulty, and I had the satisfaction of feeling that union was complete. The patient was kept in bed till May 4th, when he was allowed to get up, union being perfectly complete. The amount of shortening was not greater than that observed on his admission; viz., very nearly three inches.

The patient was sent to the Convalescent Hospital at Woolton, where some very small fragments of bone were from time to time exfoliated. On July 10th, 1877, the patient called at the Infirmary to see me. His limb was perfectly firm. He informed me that he had been out to Callao as a seaman, and that he was again leaving on the following day for the same place. Except the shortening, which now appeared to give him little inconvenience, the movements and form of the limb were all that could be desired. The case, in fact, turned out as successfully as I could wish.

In commenting upon it, I would first observe that recourse to operation was not had until it was evident that it was useless to expect union from the employment of rest, position, and other means which are found successful in instances where union is merely delayed. In a seaport like Liverpool, cases of delayed union are not uncommon. Sailors and others are often sent for treatment who have been rolling in ships with fractured limbs, sometimes for weeks; in these, rest and proper treatment are speedily followed by firm union.

In this instance, all ordinary means for securing union had failed, and therefore it was necessary to consider carefully what proceeding offered the best chance of success. It was suggested that a portion of the fractured ends be sawn off, and the case treated as one of compound fracture. This plan, however, did not recommend itself to me, inasmuch as, without some means which would secure apposition and immobility of the fractured ends, it was hardly reasonable to expect union to take place.

As it appeared impossible, under any circumstances, to prevent shortening, I thought it better to avail myself of the overlapping of the fragments, and, by baring the sides of them to this extent, in this way to obtain a much larger bony surface from which union might spring than if section of the ends of the bone were done. For the purpose of holding the fragments thus bared together for a time sufficient to allow union to take place, I determined merely to encircle the bones with wire. I was anxious to avoid retaining the wires in the substance of the bone, as I have seen the plan followed by most violent

inflammation, resulting in extensive necrosis, and thus defeating the object in view. I therefore contented myself with wrapping the wire round the bones.

For the purpose of adding to the security of the union thus effected, and guarding against any slipping of the bones by the spasmodic action of the powerful muscles of the thigh, which is not uncommon for the first two or three days after such an operation, I introduced a drill-head, which firmly "nailed" the fragments together. At the end of four days, I removed this without difficulty, as the pain referred to it became intolerable, and I was fearful of setting up an excessive inflammatory action in the bones; besides, by this time, the drill-head had fulfilled the object I had in view in its introduction. The wires were not removed until union was complete, when, for the first time, Nature began to indicate they were no longer necessary. On their removal, the wound healed kindly, and beyond, from time to time, the throwing off of small necrosed portions of bone, no further inconvenience was experienced by the patient.

The degree of shortening was not greater than three inches, and I was surprised to find how little it interfered with the patient's movements.

I shall not trouble you with any further observations on the case. The treatment of so unfortunate an occurrence as an ununited fracture claims from us all, when opportunity offers, contributions to the literature of the subject.

DIABETIC COMA: ACETONÆMIA.*

By BALTHAZAR FOSTER, M.D., F.R.C.P.,

Professor of Medicine in Queen's College; Physician to the General Hospital, Birmingham; Consulting Physician to the West Bromwich Hospital; etc.

THE sudden occurrence of grave symptoms, and death following in a few hours, are startling events in any chronic disease. They are especially startling in a malady like diabetes mellitus, which usually runs a long and comparatively uneventful course. Prout, with his usual keen observation, noted some instances of this sudden ending in diabetes, but has simply referred to them as interesting illustrations of the frail tenure on which the life of a diabetic is held. Later writers have advanced hypothetical notions to account for the phenomena observed in their cases. By some, myself included (*Clinical Medicine*) they have been regarded as uræmic; thickening of the blood to a degree sufficient to render it unfit to circulate, from excessive loss of water, has been suggested by others; the nervous and respiratory systems have been invoked as the centres on which these grave symptoms depend; and, lastly, chemical transformations of the diabetic sugar, leading to the formation of poisonous compounds within the body, have been supposed by some German and Italian authors to be associated with sudden death in diabetes.

There are cases, I have no doubt, to which each of these hypotheses may be justly applied; but my more recent experience strongly inclines me to regard the last mentioned view as the most satisfactory explanation of the hitherto obscure and alarming condition which has been aptly termed diabetic coma. In this communication, I propose to record some cases of this kind, and to inquire how far their clinical aspects support the chemical view.

One morning about twelve months ago, when teaching in the wards, I was called away to see a boy who had been admitted two days before for diabetes. We found the boy in bed, tossing about restlessly, with a very anxious expression of face and slight duskiness of the skin. He had very remarkable dyspnoea. Each inspiration was deep, ample, and chest-filling. Large volumes of air passed in and out of the chest thirty-two times a minute, but seemed to have no power to satisfy the patient's craving for air. On examination, the chest was everywhere resonant, and loud and harsh breathing was heard all over the chest. No odour beyond the ordinary diabetic smell was observed in the boy's breath. The pulse was small and weak, but regular, 136; the tongue was moist, with whitish fur; the temperature in axilla was 97 deg. The boy was somewhat dull mentally, but, when roused, answered correctly. The symptoms had begun the evening before with sickness, which had occurred again early in the morning, and been associated with sharp pain in the epigastric region and rapid breathing. The abdomen

* Read in the Section of Medicine at the Annual Meeting of the British Medical Association in Manchester, August 1877; and also before the Birmingham and Midland Counties Branch.

was distended, especially in the epigastric region. When I noted these symptoms, I recognised the beginning of diabetic coma, and expressed to my class the opinion that the case was likely to end fatally in twenty-four hours. An injection to relieve the bowels, which had been confined for two days, an effervescing draught, with half-drachm doses of compound spirits of ammonia every two hours, and brandy in small doses, were ordered. The boy gradually grew worse; the surface grew colder, the face dusky; the rapid breathing lasted for twelve hours, and then grew short and catching; the pulse became feebler and more rapid; the restlessness gave place to coma; the surface became cyanotic; and he died early the next day, less than twenty-four hours after my visit. The urine was passed freely in the bed during the last twenty hours of life.

Now, this boy had been diabetic for more than twelve months, and was admitted to the hospital, not on account of any urgent symptoms, but for the purpose of observing the effect of systematic treatment. On the day of admission, he had come by train ten miles to the hospital, as he was accustomed to do as an out-patient; and, when it was decided that he was to remain as an in-patient, he fretted a good deal, and continued to do so up to the time of the grave symptoms coming on. He was placed on partly restricted diet, and had no medicine, as I wished to estimate the sugar-excretion before beginning specific treatment. He had been passing some 100 to 120 ounces of water daily before admission, with a high specific gravity (1035 to 1038), and containing 2,700 to 3,000 grains of sugar. On the day after admission, the urine fell in quantity and specific gravity (84 ounces, specific gravity 1026), and never rose above 1028 till his death. The sugar was not estimated.

In this boy we had a typical example of the form of sudden death in diabetes, for which we have hitherto lacked any adequate explanation. All who had observed the case, in common with myself, looked forward with great interest to the necropsy, in the hope that it might unravel the mystery.

The *post mortem* examination was made by Dr. Saundby twenty-six hours after death. The brain and membranes were pale and anæmic, and there was no discoverable coarse or microscopic change except hypertrophy of the muscular coat of the arterioles of the pia mater. The lungs and heart were quite healthy; the muscular structure of the heart was pale, but not fatty. The abdomen showed no sign of any inflammatory mischief. The spleen was small and soft. The liver was pale in colour, but normal in structure under the microscope, and weighed 3 lb. 3 oz. A decoction showed the presence of sugar in small quantity. The kidneys were healthy. The stomach showed patchy congestion and catarrh. So far, the search had been fruitless. The blood, however, had not escaped observation. In the brain-sinuses, and throughout the body, it was pale and cream-like. One observer compared it to grumous pus. It was fairly fluid, and did not clot, but, on exposure to the air, became pinker and brighter in colour, assuming a magenta-like tinge. The microscope showed the creamy condition to be due to the presence of a large quantity of molecular matter, which looked like fat, but did not dissolve in ether. There was a small quantity of sugar in the blood.

These appearances were very striking, and pointed to the peculiar blood-change as the one abnormal condition to account for the patient's symptoms. Its nature still remained to be explained. While pondering over this problem, I came on a reference to the ideas of Petters and Kaulich on the development of acetone in the system of diabetics. Acting on this, I at once determined to test the effects of acetone on healthy blood. Dr. Saundby willingly and ably assisted me. We found that, when acetone was added to the blood, it became paler and creamy-looking, presenting the same appearances as the blood of the boy. On exposure to air, the acetonised blood assumed after a time the same pinkish red coloration that had been seen in the *post mortem* theatre. Under the microscope, the blood-corpuscles were seen to break down into granular debris, reproducing exactly the state observed in the blood of the patient. Roughly examined, it looked like blood containing a good deal of fat; but neither the artificially acetonised blood nor that of the boy owed its appearance to fatty matter. The addition to fresh blood of chloroform, alcohol, or ether, failed to produce similar effects. In these results, then, we had a clue to the explanation of the case. Assuming for a moment that acetone was developed in the boy's system, its destructive effects on the blood would be amply sufficient to account for the great dyspnoea, and the cyanosis, steadily increasing, in spite of the full and frequent inflation of his lungs. The blood-cells were so destroyed that they no longer were able to absorb and fix the oxygen drawn into the air-cells by the vigorous respiratory action of the patient.

Before recording other cases, I propose to sketch very briefly some of

the observations made on this subject by others. Petters (*Prager Vierteljahrschr.*, 1857) was the first to show that acetone is developed in the system of diabetics, and Kaulich (*Ibid.*, 1860), in confirming this discovery, endeavoured to sketch clinically the state of acetonæmia. This state, he attempted to show, was not peculiar to diabetes, but was also met with in chronic affections of the stomach and digestive organs, and accounted for the depression of the whole nervous system observed in such cases. Betz (*Memorabil. für pract. Aertze*, 1861) followed, referring the presence of acetone in the breath to catarrh of the throat, etc.; and, some years later, Cantani (*Il Morgagni*, 1864; *Syd. Soc. Biennial Retrospect*, 1865-66), endeavoured to show that there were several forms of acetonæmia, occurring mainly in conditions of gastric catarrh. Alcoholic and acetic fermentations of grape-sugar, and possibly of other organic matters in the stomach, was assumed by these authors to be the sources of acetone. The following formula represents acetone: C_3H_6O . Since then, Bechamp (*Comptes Rendus*, 1872) has shown that alcoholic and acetic fermentations occur in the stomach under favourable conditions, such as catarrh. Rupstein (*Centralblatt für die Medicinischen Wissenschaften*, 1874) believes that the acetone may also be formed in the blood. It has been found there after death, and has also been obtained from the solid organs of a diabetic dying of coma by Berti (*London Medical Record*, September 1874). Acetone is frequently found in the urine of confirmed diabetes, and is often developed in the urine after it is passed; an important fact to bear in mind in estimating the sugar. I have, however, myself detected it in urine immediately after it was passed. I refer last of all to the important paper of Kussmaul (*Deutsches Archiv für Klin. Med.*, Band xiv, 1874),* who has observed cases of this kind and experimented on animals with acetone. The results of these experiments, which are most interesting, show that acetone is an anæsthetic less potent than ether and chloroform, and acting more like alcohol. It produces great muscular feebleness, quickens the pulse, and causes deep slow breathing, and in large quantities brings on stupefaction. The breath of the animals smelt strongly of acetone. Altogether, the results obtained by Kussmaul's experiments closely resemble the symptoms of diabetic coma. Acetone produces partial anæsthesia, and in large quantities coma. It causes great muscular feebleness generally, but deep and energetic respiratory action. In man, before the coma comes on, there is partial unconsciousness, broken by a happy delirium, in which the patient laughs and jokes when roused, reminding one very much of the effects of alcohol. Summed up, the results so far go to show—

1. That acetone has been found in the breath, urine, blood, etc., of patients who have died of diabetic coma.
2. That grape-sugar may be converted in the stomach by alcoholic and acetic fermentations into acetone.
3. That the changes in the blood observed after death from diabetic coma can be artificially produced by the addition of acetone.
4. That the administration of acetone in large quantities to animals produces similar symptoms to those observed in diabetic coma.

I will now pass on to consider other examples of this interesting affection.

In October last, I was summoned in haste to see a case with Mr. F. Hollinshead of Selly; and, on my arrival, I heard that it was a patient suffering from diabetes who had suddenly got worse, and was rapidly becoming insensible. As we drove to the house, I learned that he had suffered from diabetes some two and a half to three years. He had never lost flesh, but had always been a stout vigorous-looking man, in spite of an excretion of eight quarts of urine daily, of specific gravity 1037. He had been subject to boils, but had usually had no great trouble with them till six months before my visit, when he had suffered from a bad carbuncle on his neck. He recovered from it, however, and regained his usual health, and had been able to perform his daily duties as formerly. About October 10th, signs of another carbuncle showed on the nape; this had increased enormously, and was the beginning of the illness for which I was called to see him. The carbuncle had opened and all had gone on fairly well till forty hours before my visit. His temperature had then risen to 104 deg., and his pulse to 120, while the breathing became a little quick. On the next day, he had a sharp pain in the right hypochondrium, was not sick, but had free eructations of gas, and his breathing quickened somewhat. In the evening (October 24th), he became drowsy and wandered in his talk.

When I saw him on the afternoon of October 25th, I found a man in the prime of life (41) lying in bed on his back, breathing loudly and

* There is also an excellent account of acetonæmia, with reference to most of the papers on the subject, in Lecorche's *Traité du Diabète*. Paris: 1877.

noisily, but with no stertor. Each inspiration was long, deep, and chest-filling, but seemed to have no influence on the air-hunger; for one rapidly succeeded another, reaching fifty to sixty a minute. His countenance was dusky; his expression was anxious, and there was a good deal of jactitation. The pupils were equal and a little dilated; the pulse small, quick, regular, 150. The hands were cool and moist; the skin moist all over, and perspiration acid. The feet were colder than the hands, and both were dusky in colour. When spoken to, the patient answered, but not to the point, and soon wandered off into a happy kind of delirium. When told, he put his tongue out; it was moist and a little furred. On examination, the lungs were found to be resonant all over the chest, and the air entered freely and fully everywhere. There was a systolic aortic murmur, which he had been known to have for some years. The abdomen was rather tympanitic, but not tender. While I was present, frequent eructations of a gas of peculiar odour occurred. The liver was not much enlarged; the spleen appeared natural in size; the bowels had acted well, but the quantity of urine had decreased during the last twenty-eight hours. The temperature in the axilla was 104 deg. Fahr. The patient got out of bed in my presence and passed urine (about a pint); but he was greatly exhausted by the effort; his muscular strength was almost gone, and there was partial anæsthesia of the limbs.

This man's breath had a most peculiar odour, which reminded me strongly of a case seen some years ago. It was strongly impregnated with acetone. I asked one lady who was with the patient, and who had visited him from time to time during the day, if she had applied any vinegar-cloths to the patient's head. I asked this question with a purpose. She answered: "No; but I thought the nurse had; for, when I came in this morning, the room smelled of stale vinegar"; but she added: "The smell comes from his breath." So it was; the room was saturated with this vinegar-like smell, likened very aptly by Mr. Hollinshead to that of stale beer. So strong was it, that it gave all who stopped long in the room headache and slight drowsiness. The smell was unmistakable; every expiration, and every eructation of gas from his stomach, poured forth volumes of acetone, thus freely volatilised, on account of the high temperature of the patient.

Drowsiness and coma gradually superseded the delirium, the patient grew quieter and more comatose, the breathing became less vigorous and more broken, the muscular feebleness and anæsthesia increased, and he died insensible seven hours after my visit. For this end, we were prepared; the case was a hopeless one; acetonæmia killed him, in spite of any remedies we could administer.

The urine had fallen in quantity, as it does in most instances of this kind; the sugar also is usually lessened, from the fact of its conversion in the system into acetone. The urine I obtained had only twenty grains of sugar per ounce, a much less quantity than he had formerly passed most probably, and showed a distinct reaction with the acetone test. The blood of this patient I should have very much liked to examine; but my colleague Mr. Hollinshead was unable to procure any for me, or to obtain a *post mortem* examination. If we had been able to procure a specimen of the blood, we should, I have no doubt, have found the same disorganisation of blood-corpuscles that I have described in the former case. In such a condition, we have an explanation of the coincidence of the deep vigorous breathing and the general cyanosis of the surface: a duskiess worthy of death by asphyxia, associated with full and frequent inflation of the lungs.

The attempt was made in this case to arrest the disorder by remedies. Carbolic acid and sulphurous acids were administered in water as drinks; but the case ended too quickly to be of any value as a test of the treatment.

Must this fatal ending always occur when symptoms of poisoning by acetone once arise in a diabetic patient? I think not. Many milder cases pass unnoticed, and, in many instances, the slighter effects of acetone are put down to temporary brain-disturbance, and no more account is taken of them. In other instances, symptoms less grave than those I have described, but yet sufficiently serious to cause considerable danger to the patient, result from acetonæmia; but the process is arrested either naturally or by treatment, and the life is saved. A very striking example of this kind I saw not long since with Mr. George Jones of this town.

The patient was a gentleman aged 64, who had suffered from diabetes for upwards of two years, but had greatly improved under opium treatment. About a week before I saw him, he had become affected with bronchial and gastric catarrh. The attack had been preceded by a good deal of mental worry, which had lowered his state of health. The chest-troubles had decreased; but his breathing had confined it to rapid, his temperature high (101½ deg. Fahr.), and drowsiness and delirium, with a pulse intermitting every third beat, were the conditions which led to a consultation. I found a strong-looking man

in bed breathing thirty a minute, drowsy and restlessly excited by turns. He wandered at times like a man in drink, and was cheerful and good-humoured in his fancies. The pupils were equal and a little dilated. There were slight dulness and coarse moist sounds at the right lung-base, but the air entered all the rest of the chest freely. There was tenderness over the liver, which was full in size. The abdomen was rather distended by gas; the tongue was moist but loaded; the breath had a faint smell of acetone; the urine, which was not measured for the twenty-four hours, was 1034 specific gravity, contained twenty-five grains of sugar per ounce, and gave a distinct reaction with test for acetone. Suitable remedies were prescribed for the heart and lung-conditions, and, on the third day, when I again saw him, they had greatly improved. The pulse was again regular, 108; the temperature was 99½ deg. Fahr.; the physical signs at the back of the chest were almost gone, but the breathing, though slower, was very deep and almost sighing in its character, and the head-symptoms had decidedly increased. The patient was more frequently unconscious; he could be roused; but, when left alone, he wandered more in his talk, being very restless and excited. This state had lasted twenty-four hours, and was associated with alarming muscular feebleness. He had passed only eighty ounces of urine in the twenty-four hours; it contained thirty-one grains of sugar per ounce, and showed more distinctly than the last the presence of acetone. The breath also was more decidedly impregnated with acetone. The tongue was dry and red. My colleague took, with me, a grave view of the case, and we decided to alter the treatment by giving carbolic acid in a drink, so that the patient could take two grains every hour at first, and then every two hours. We gave opium also in larger doses, as Mr. Jones had convinced himself that it possessed to a high degree in this case the power of checking the formation of sugar. By this, we hoped to lessen the amount of diabetic sugar formed, while by the carbolic acid we trusted to prevent the conversion of the sugar that was formed into acetone. The next day, the patient had improved; the nervous symptoms were all less marked, and, on the second and third days, he had improved still more. After this, he took the opium alone, and, when I saw him on the tenth day, on account of a new complication, in the shape of an aphthous condition of the mouth and pharynx, the symptoms of acetone-poisoning had all passed away; his breath had still a faint odour of it, and the urine gave a slight reaction. After this, he made a good recovery, and, when I last examined his urine, it was free from acetone, and contained only nine grains of sugar per ounce.

The above cases, in addition to others which I have seen, have led me to conclude that we have in the acetonæmia hypothesis the best explanation of diabetic coma. Patients who are seriously diabetic have, in all probability, a small quantity of acetone constantly formed in their economy. It is this which gives the odour to the breath and to the urine. It would appear that, under conditions of nervous depression and exhaustion, and more especially conditions favouring stomach-catarrh, the quantity of acetone formed may rapidly undergo great increase and give rise to the acute symptoms which lead to sudden death; a chronic poisoning, as Kussmaul remarks, suddenly taking on an acute form, like delirium tremens in habitual drinkers. The similarity in the symptoms of some of these cases to the effects of alcohol make it highly probable that alcohol is also formed in the system. I find that Senator, in his valuable treatise on Diabetes in *Ziemssen's Cyclopædia of Practical Medicine*, vol. 16, also holds this opinion.

In the discussion which followed the reading of this paper at Manchester, the difficulty was raised that, in some of these cases, there is no smell of acetone in the breath. This is so. In the first case here recorded, there was no marked odour. This is to be accounted for by the fact that acetone requires a high temperature for free volatilisation. In some experiments recently made, I find that from urine, for example, very little acetone is given off till the temperature reaches 100 deg. Fahr.; but, above this point, it is given off quickly. The cases in which I have observed the smell of acetone most distinctly have been cases with high temperatures, as in the second case in this paper. The temperature of diabetics is not, as a rule, high enough to send it off in any quantity.

Finally, if the explanation of diabetic coma here supported be the true one, we may look forward to more precision in its treatment. Transfusion has failed in this country and abroad; the inhalation of oxygen has been tried in vain; elimination of the poison by ordinary methods can hardly be trusted in these sudden attacks; I therefore used carbolic acid in two cases narrated, in the hope of checking the fermentative process which develops the poison. So far as it goes, the history of one case supports the practice. Salicylic acid and its salts

are remedies also worth trying on this view, and especially thymol, which has been suggested to me by Dr. Burdon Sanderson as a more powerful and pleasant agent of the same class. The remedy will not long be wanting when the morbid process is fully understood.

BATH THERMAL MINERAL WATERS: THEIR USES AND ABUSES.

By A. B. BRABAZON, M.D.,
Physician to Bath Mineral Water Hospital.

NOTWITHSTANDING the title with which I have prefixed the purely practical remarks which I am about to offer to the readers of the JOURNAL, I fear some may consider the subject a dry one.

In many aspects, the subject has been well nigh worn out; and the deservedly highly extolled natural beauties of Bath, its environs, its antiquities and its salubrious climate, its fauna and flora, have all been eloquently described in language which I dare not and could not emulate. The task which I have set before me is one which, both for writer and reader, is bereft of all such charms and devoid of all such interest. My object is purely practical, as will be my observations; and in this sense alone do I venture to place them before my professional brethren, for the following reasons.

1. Because I am not aware of any attempt, founded on statistical information combined with personal experience, to classify, arrange, or tabulate, in any systematic form, the effect of the Bath mineral waters on the patients submitted to treatment therewith, either as regards age or sex, whether in hospital or in private practice.

2. I feel assured that the want of some such information must occasionally, and indeed frequently, be felt by my professional brethren, living, possibly, at a great distance from Bath, when called upon to decide, in any given case, whether the chances of cure or improvement are such as to render it advisable for the patient to undertake, possibly, a long and tedious journey.

3. I know, from personal observation, and I think my colleagues in connection with the Bath Mineral Water Hospital will bear me out in the statement, that the want of some such information is occasionally made painfully apparent in the character of the recommendation of candidates for admission to hospital, which are submitted to the medical staff. Moreover, it is not only in hospital cases that one finds such want of correct ideas as to the therapeutical powers of the waters displayed, but also, and probably more frequently, in private practice. I have, in my own experience of sixteen years, frequently met with patients sent to Bath, I might say to die, to whom the long journey must have been a cruel and painful infliction, and to whom even alleviation of their sufferings was almost an impossibility, unless the Bath waters were a veritable "Pool of Siloam". Such are the cases, fortunately rare, which bring such bitter disappointment to those concerned, and are calculated to cast doubt and discredit on the efficacy of these mineral waters, which are, I confidently believe, second to none in their curative powers in cases suitable for their administration.

In making this humble attempt to arrive at even something like definite conclusions on the subject in hand, I must beg my professional brethren to remember that the conditions under which such an attempt is made differ widely from those which obtain in reporting or relating circumstances connected with the history of disease otherwise treated. The cases on which are founded any observations, statistical or practical, which I may venture to offer, are, as a rule, subacute or chronic in their character. There are no hourly or daily changes to be reported; no frequent variations of pulse, temperature, or even treatment, to excite any special interest. The process, by which alone we can arrive at anything approaching to definite conclusions is the tedious and comparatively uninteresting one of observation, not of individual, but of aggregate cases of disease, as they are presented to our view in hospital or private practice. Thus, it becomes necessary to arrange, in some tabulated or statistical form, those cases from which we wish to derive therapeutic information of any scientific or practical value.

1. I propose to tabulate, under certain heads, the diseases which have been treated in the Bath Mineral Water Hospital during the past three years.

2. I shall append statistical tables representing the results of treatment in these various forms of disease as to sex and age, and giving the percentage as regards results: first, respecting sex; secondly, respecting age—these percentages being averaged on a given number of patients discharged from hospital within the several periods of life recorded.

3. I shall endeavour, as far as possible, from hospital statistics and from general observation, to delineate these diseases, in their general

aspect as well as in their particular forms, which positively or comparatively are successfully treated by the use of the Bath mineral waters, whether used internally, or applied externally, or administered in both ways.

I may here mention that, although the general rule adopted in almost all health-resorts famous for mineral springs—viz., that of restraining patients from taking any kind of medicine while making use of the water—is followed, yet, under certain circumstances, the most experienced physicians in Bath do not hesitate to supplement the waters with additional therapeutical means when they consider it judicious to do so. On the present occasion, it is impossible for me to individualise those cases so treated, but at some future time I may be able to do so. At present, I can only, in my observations, allude generally to those cases in which my individual experience would lead me to adopt some such supplemental treatment.

The diseases admitted or admissible into the Bath Mineral Water Hospital may be divided, for convenience of description, into four classes.

1. *Constitutional Diseases*; 2. *Local Diseases*; 3. *Diseases from Specific Causes*; 4. *Miscellaneous Diseases*. These four classes will include, each respectively, certain subclassifications, which I shall tabulate as follows.

1. *Constitutional Diseases*, producing local or general effects: A. Rheumatism (including muscular, articular, and chronic rheumatic arthritis); B. Gout (pure and so-called rheumatic gout); C. Sciatica (gouty, rheumatic); D. Lumbago (gouty, rheumatic); E. Coxalgia (gouty, rheumatic, or arthritic).

2. *Local Diseases*, with or without constitutional causes: a. Hemiplegia; b. Paraplegia; c. Chorea; d. Neuralgia; e. Paralysis after Injury; f. Hysteria (simple and complicated); g. Wasting Palsy; h. Locomotor Ataxy; i. Skin-Diseases (including eczema, lepra, psoriasis, impetigo, acne).

3. *Diseases from Specific Causes*, such as effects of metallic poisoning, including dropped wrist, colica Pictonum, mercurial poisoning.

4. *Miscellaneous*: Amenorrhœa, dysmenorrhœa, anæmia, debility, and others of various types which occasionally crop up among patients who may have been admitted for other forms of the diseases above mentioned.

The results of treatment I have divided under four different heads: 1. *Cured*; 2. *Much Better*; 3. *Better*; 4. *No Better*. As this classification may be considered somewhat arbitrary, I must beg leave to explain that, according to a time-honoured custom, all patients discharged from the hospital are discharged under one of the above-named divisions. I shall, as far as possible, define the meaning to be attached to these four terms or divisions. Under the head of "Cured", I include all those patients who leave the hospital free from all symptoms (constitutional or local) of the disease for which they have been admitted, and who are discharged able to follow their special trades or avocations. Under the head of "Much Better", I include all those patients who have obtained relief from pain and other prominent symptoms, and who are most of them sufficiently recovered to return to their usual occupations. Patients discharged as "Better" I may describe as simply temporarily relieved of their most painful and prominent symptoms. Under the head of "No Better", I comprise all cases in which, after sufficient trial, the mineral waters, however administered or supplemented, have been perfectly inefficacious.

I shall take the several diseases enumerated above into consideration in order, commencing with that of which the greatest number has passed out of hospital—viz., rheumatism in its various types and kinds.

During the year ending May 1877, I find 415 cases of rheumatism were discharged from hospital; that number being composed of 269 males and 146 females. Distributed according to periods of life, I find the following results:

	Males.	Females.	Total.
Under 20	23	24	47
20 to 40	111	69	180
40 to 60	107	43	150
Over 60	28	10	38
	269	146	415

In explanation of the appended tables, I beg to state that Table I gives the results of treatment under four divisions, as above described, and within four periods of life; it also gives the total of males and females regarding these separate results of treatment, according to ages above mentioned.

Table II gives the percentage of results of treatment in proportion to numbers discharged under ages mentioned and according to sexes. As an illustration, under 20 years, 13 cases (males) were cured; total number of males discharged under 20 = 23; therefore, the percentage