

scription, but he assured him that the medicine was all right, and said that she should continue to take it, but advised that she should diminish the dose. This the patient did, being encouraged by the fact that she felt her arm improving daily. At the end of a week, while she was still taking the medicine, the eruption began to disappear; and when she presented herself, for the second time, at the hospital, the blisters had dried up, and only the rings still remained. A very extraordinary feature in the case was the fact that the catamenial flow, which had been absent for twelve years, reappeared and continued during the greater part of the week during which she was taking the iodide.

I visited the patient at her own house on Saturday, February 2nd, and found her almost quite well. The paralysis had quite disappeared, and she was evidently much impressed by the fact that her arm, after being paralysed and useless for fourteen weeks, had apparently been quite cured by one bottle of the medicine. The skin had almost resumed its normal appearance. The rings had quite disappeared, and, except a little roughness and reddening of the skin, nothing remained of the formidable eruption which had caused the patient such great and not unnatural alarm.

REMARKS.—The first remark I have to make on the above case, is that I think there is no reason to doubt that the eruption was due to the administration of the iodide. There was no trace of eruption present when the patient first presented herself. She had always been healthy, and was perfectly well, except as regards her paralysed arm. Then the eruption came out after two doses of the medicine, and quickly attained its maximum, began to decline when the dose of the drug was reduced, and disappeared altogether when it was entirely stopped. We have the further facts to consider that many varieties of cutaneous eruptions have been universally attributed to the use of the iodide, and that recently several cases of an eruption, closely resembling that present in this case, have been described by such an eminent authority as Mr. Jonathan Hutchinson.

It is more important to describe the physical characters, and to understand the clinical history of this eruption, than to give it a definite name. It is perhaps best simply to rest content with pronouncing it a bullous eruption due to iodide of potassium, rather than to identify it with any specific variety of dermatitis. It was evidently quite distinct from the ordinary iodide eruption, which, so far as physical characters go, is a variety of acne. It was not an urticaria—the large size of the bullæ, the encircling rings, and the severe constitutional symptoms being sufficient to distinguish it from that disease. Probably some of the cases which were described some years ago by M. Bazin of Paris, under the name of “hydroa,” were really identical with the eruption which appeared in this patient.

The practical deduction to be drawn from the severer forms of iodide-eruptions is that their appearance shows a marked susceptibility to the action of the drug, and calls either for greatly diminished doses, or for its entire suspension. Mr. Hutchinson has seen several cases where persistence in the drug was the cause of death, but it is more usual for tolerance to be quickly established. Mr. Hutchinson believes that those patients who quickly show signs of extreme susceptibility to the iodide will be found to derive the same benefit from very minute doses as others do from the usual three, five, or ten grain doses, and adds that he has repeatedly cured tertiary ulcerations by doses of half or one-third grain of iodide in patients for whom iodine had been believed to be a poison even in the smallest quantities. The whole question of dosage in reference to iodide of potassium is one on which very eminent authorities have gravely differed. It is remarkable that, in the present case, the patient had taken only seven and a half grains of the drug before violent symptoms of poisoning set in. When we remember how hundreds of patients go on taking ten, twenty, or even sixty grains of iodide of potassium daily for weeks and months without an unpleasant symptom, except perhaps a little touch of gastric irritation, the case teaches us a lesson as to the important part idiosyncrasy may occasionally play.

This patient had evidently other symptoms of iodism, in addition to the eruption; but on these I need not dwell.

It is a singular fact that in this case the drug brought back the menstrual flow after a lapse of twelve years. Whether this was merely a result of the profound constitutional disturbance present, or whether the iodide can be credited with any emmenagogue properties, I must leave for others to decide.

PROPORTION OF DOCTORS TO POPULATION.—The *Siglo Medico* gives the following proportion of Medical men in different nations, to population: France, 9.91 to 10,000 inhabitants; Germany, 3.21; Austria, 3.41; England, 6.; Hungary, 6.10; Italy, 6.10; Switzerland, 7.06; United States, 16.24.

REPORTS OF SOCIETIES.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, MARCH 25TH, 1884.

GEORGE JOHNSON, M.D., F.R.S., President, in the Chair.

Seventy Cases of Dupuytren's Contraction of the Fingers.—Mr. NOBLE SMITH read a paper on this subject. Upon examination of the hands of 700 elderly people, 70 cases of indurated and contracted palmar fascia were found. The points to which attention was chiefly directed were: 1. the supposed immunity of females from the affection; 2. the cause of the malady; 3. its treatment. 1. Among 440 women were found 15 cases of indurated, thickened, and contracted fascia alone, and also 11 cases of well marked Dupuytren's contraction of the fingers. Drawings of some of these cases were shown. 2. As the disease had been attributed to gout or rheumatism, inquiries were directed as to these diseases, and the evidence was against their being the cause. The majority of the individuals were free from common complaints, and seemed tolerably robust; they were mostly old people, the average age being 73; twelve were over 80. Most of them attributed the contraction to some particular kind of work. The much greater frequency with which the right hand was found affected led one to suppose that use had a material influence. In some cases, the hands had, however, been very little used. Some cases occurred from injuries in which the contraction and thickening were exactly similar in appearance to the others. In many cases the hands were affected symmetrically, indicating some other than a local cause. Contraction of the palmaris longus was almost constantly present, and it was thought possible that this might indicate a condition of nerve-irritation in some instances. 3. With regard to treatment, the author of the paper recommended operation upon the contracted bands of fascia by as few incisions as possible, as opposed to the recommendation that many incisions should be made; the object being to separate the cut parts as much as possible, so that recontraction should be prevented. Cases were recorded which prove the value of this recommendation. It was thought probable that section of the palmaris longus tendon might be beneficial in the early states of contraction.—The PRESIDENT thanked Mr. Smith for his communication, and directed attention to a dissection he had brought from the King's College Hospital Museum, showing that the real seat of contraction was in the fascia and not in the muscles.—Mr. WILLIAM ADAMS was very glad to find that more attention than formerly was now being given to this class of cases; for it was not so very many years ago since they were generally regarded as incurable, and amputation sometimes submitted to as the only possible remedy. On May 22nd, 1877, he had himself brought the subject before the Society, but no attention had been paid to his paper, and the Society had declined to print it among their *Transactions*. Since then it had been published, and he had been glad to find considerable interest aroused. Dr. Keen, of Philadelphia, had taken up the subject, and had collected the largest number of cases in the *Journal of the Pathological Society of Philadelphia*, in 1882, and added the most complete analysis of the foreign literature. He should be glad to learn from Mr. Noble Smith whether the 440 women he spoke of in his paper were included in the total 700 [Mr. NOBLE SMITH rose at once to say that it was so], and why, if it were so, so many more women had been examined than men. Further, he remarked two classes of cases distinguished among the women, viz., those with “indurated, thickened, and contracted fascia alone,” and “also cases of well marked Dupuytren's contraction of the fingers;” he wished to ask why this division, to which he did not know what importance was thought to attach, had not been carried out among the men. Mr. Noble Smith's cases showed that 10 per cent. of the subjects examined had Dupuytren's contraction. That that was the case with the general population could not for a moment be supposed, and he had no satisfactory account of how Mr. Smith had selected his cases for examination. He had himself said, when speaking in 1877, that he had seen no true case in a female; since then he had seen six and operated on four. But he still considered it roughly true, as Keen's tables showed, that the disease was ten times as common among men as among women. As to the cause; Dr. Keen thought it largely due to gout, in which he himself agreed; and, as arguments, he would mention that it was frequently hereditary, in several cases for three generations; that it affected the right and left sides about equally, which showed that it was probably constitutional and not local in origin; that it was found much more frequently in the middle and upper classes than in the lower; in this it agreed with gout. It was common in the clergy, who never used their hands; in barristers also; but commonest in officers of the army. As to treatment, he could not

agree to limiting the number of incisions to two or three, as Mr. Smith suggested. His most successful cases had, as a rule, been those where most incisions had been made, viz., one in which there were eighteen incisions. He quoted details of several cases, and showed many casts of hands, before and after operation. In one of them the operation had been done twenty years ago, and the hand operated upon had remained quite well though the other had become slightly affected. He thought it very satisfactory that the disease had been brought into notice again, and its curable nature demonstrated.—Dr. HUMPHRY remarked that as to the question of the cause between constitutional gout and local irritation, the truth probably was that it was due to both of them; that local reasons determined this exhibition of a gouty condition. It was generally observed first in the ring-finger, and he associated that with the fact that in handling a stick or umbrella the pressure was felt chiefly in the ball of the ring-finger, as compared with the other fingers. To such pressure its local determination was due. The metacarpo-phalangeal was first affected, the others secondarily and less; the palmaris longus probably had very little to do with it, for it was a very small muscle, not having much effect on the fascia, and any effect it might have would influence all the fingers. It was worth notice that similar contractions were extremely rare in the plantar fascia; and that he attributed to the greater and more constant pressure upon the metacarpo-phalangeal joints, keeping them in extension, so that the plantar fascia had hardly the opportunity of contracting that the palmar had. The motion of a toe was hardly ever due to the contraction of the fascia, but to the tension of a tendon. That the ring-finger should be specially affected, was due to the fact that its extensor powers of range were considerably less than in any of the other fingers.—Mr. JOHN CROFT admitted that his experience was not large, but in the majority of cases with which he had had to do, had found a gouty history; but the gouty symptoms were ill marked and the limits of gout ill understood. At any rate, that some cases were entirely free from gout was a fact that should be attended to. Mr. Smith's idea that the palmaris longus was affected he thought very improbable, for the first and chief action of that muscle was to flex the wrist, and not the palmar fascia; it was sometimes completely absent, and Mr. Smith had not even shown its presence in his cases. He agreed with Mr. Adams that treatment by multiple incisions was to be preferred.—Mr. W. J. WALSHAM wished to add his testimony to the value of multiple incisions, which, as he pointed out, were best made in the furrows of the contracted fascia. In the cases under his treatment, he had not met with gout, or been able to distinguish any other cause.—The PRESIDENT observed that what he had heard had reinforced his own impression, that the gouty theory of causation was insufficient. The five old women whom Mr. Smith had brought to the Society that evening had no gouty sign about them. He had noticed with interest Dr. Humphry's remark, that the disease was very rare in the foot; but gout certainly was very much commoner in the foot than anywhere else. Modern fashion was in the habit of attributing more to gout than its just share of the obscurer diseases.—Mr. NOBLE SMITH said that the cases he had examined had been taken from the oldest inhabitants of workhouses. He had asked the medical officers to collect cases where the hands were at all deformed, and had been surprised to find a crowd with deformed hands gathered together, sometimes without any case of Dupuytren's disease. That such cases should be overlooked he thought very probably due to the fact that there was very little inconvenience felt, and no complaint made. Dr. Keen and Mr. Adams had probably chiefly seen cases in the middle and upper classes. He preferred few incisions in treating the contraction, simply because he found a few were sufficient. As to the causation, he wished to point out that he felt himself that the original cause was obscure, and that he had expressed very little opinion of his own.

Three Cases of Xeroderma Pigmentosum (Kaposi), or Atrophoderma.
—Dr. RADCLIFFE CROCKER read a paper on this subject. The disease, of which the three cases brought forward were examples, was first described by Kaposi in *Hebra's Diseases of the Skin*, in 1870, and, last year, he gave a more extended account of it. These were the first and only cases of the kind known in England, and consisted of two sisters, aged respectively twelve and ten years, and their brother, aged nine, out of a family of four. Besides the above cases, there were thirty-one others known, all bearing to one another a striking resemblance. The disease usually commenced in the first or second year without apparent cause, affecting the exposed parts of the face, neck, and extremities. It spread slowly, reaching as low as the third rib in front and up to the middle third of the upper arm. In the first stage, red blotches or spots appeared, which faded, but left lentiginous pigment-spots; or the freckles might be the first noticed, and tended to increase in number, size, and depth of colour. Soon the skin became very dry, and

white atrophic spots appeared between the lentiginous, which coalesced into larger areas. The skin in part peeled off in thin lamellae, and subsequently became contracted and parchment-like. Some years after the onset, superficial ulcerations covered by crusts appeared, and verrucose projections could be felt in the position of some of the pigment-spots. These terminated the second stage. From the fourth to the sixth year the third period began. The verrucose projections and the sores became the starting-point of fungating epitheliomas. The patients, whose general health had previously been entirely unaffected, became marasmic and died exhausted, or, in a few cases, from generalisation of the cancerous tumours. Death usually occurred before puberty, but both onset and termination might be deferred. Of the whole thirty-four cases, the number of males and females affected were equal; but if attacking several members of a family, the disease usually limited itself to the members of one sex only; seven boys having been affected in one family, and five girls in another. Other etiological factors were few. Congenital but not hereditary exposure to the sun had been suggested as a possible exciting cause in a few, but direct evidence was wanting on this point. Treatment, whether to cure or even arrest the fatal course, had been completely futile. The disease was *sui generis*, apparently a primary atrophy of the skin; and, if the history and all the characters of the disease were considered xeroderma pigmentosum could hardly be mistaken for any other affection. In illustration of the paper, many coloured plates were shown of other cases, and some drawings of microscopical specimens, as well as the three children who were the subject of the paper, and who were somewhat changed since they had appeared before the Society in January. The removal, by Mr. Marcus Beck, of the fungating epithelioma, near the right ear of the eldest girl, had considerably improved her appearance. There was a little keloid in the cicatrix.—Mr. MORRANT BAKER had been exceedingly interested in the cases, and was glad that Dr. Crocker was ready to propose some new form for this malady, to which he thought Kaposi's name of *xeroderma pigmentosum* singularly inappropriate. It was clearly allied to states of atrophy of the skin.—Dr. T. COLCOTT FOX, after some careful microscopical examination, had not been able to distinguish any difference between specimens of this disease and of rodent ulcer. On the essential nature of the disease no two observers agreed, and that it was primarily an atrophy it was almost impossible to say, when the earlier stages were so imperfectly known. He would have been glad to discuss the matter more at length had time permitted.—Dr. RADCLIFFE CROCKER very briefly replied that he had not alleged that atrophy was the primary cause of the disease, only that it was a prominent symptom; and he was glad that some attempt at a new name, such as atrophoderma, should be approved. The sections of the tumours removed in his cases were on view on the table, and showed them to be papillomatous.

MEDICAL SOCIETY OF LONDON.

MONDAY, MARCH 24TH, 1884.

ARTHUR E. DURHAM, F.R.C.S., President, in the Chair.

Facial Erysipelas with Low Temperature.—The full text of Dr. CAVAFY'S paper on this subject will be found at p. 599.—The PRESIDENT made a few remarks on the importance of the diagnostic question raised by Dr. Cavafy.—Mr. R. W. PARKER had met with cases in children which presented all the appearances of erysipelas without pyrexia; further, he had observed cases which seemed to show that the severity of the constitutional symptoms did not bear any direct relation to the extent of the local lesion.—Dr. CULLMORE mentioned certain examples of inflammation without pyrexia.—Dr. C. H. ROUVE referred to the occurrence of scarlatina and measles without pyrexia. He had recorded an epidemic in which patients, under the same influences, suffered, some from a contagious ("putrid") sore-throat, others from an erysipelatous affection, and where the temperature rose in some cases and not in others. He thought that Dr. Cavafy had given an instance of a wide law, which was, that the zymotic diseases might or might not be accompanied by pyrexia.—Sir JOSEPH FAYRE had met with several cases of hepatic abscess without pyrexia.—Dr. WYNN WILLIAMS believed that rise of temperature was often to be referred to nervous shock. He had met with a case of pelvic abscess without pyrexia.—Mr. A. P. GOULD observed that all Dr. Cavafy's cases were instances of idiopathic erysipelas, and inquired whether he had seen any cases of traumatic erysipelas running an apyrexial course; idiopathic facial erysipelas differed in so many respects, that many believed that it belonged to a different category. He suggested that, when abscess occurred in the liver or pelvis without pyrexia, it partook of the nature of a cold or chronic abscess.—Sir JOSEPH FAYRE said he wished to be

understood to have said that liver-abscesses occasionally formed without discoverable rise of temperature, but with some pain and constitutional disturbance, though not so great as in acute liver-abscess, to which he did not refer; neither did he refer to chronic abscess.—Dr. CAVAFY replied.

Case of Opium-Poisoning Treated with Atropine.—Dr. FINLAY read notes of a case of opium-poisoning. The patient, a stableman, aged 36, was admitted into the Middlesex Hospital on the night of November 17th, 1883. Twenty minutes before admission he had swallowed a mixture containing a quantity of tincture of opium and compound tincture of camphor, equal to seventeen grains of dry opium. His pupils were contracted. Before the stomach-pump could be used his respiration ceased, and he became deeply cyanosed, but he was restored by faradism and artificial respiration. The stomach-pump was then passed, and the contents of the stomach evacuated. Failure of respiration again occurred, and rendered recourse to artificial respiration necessary a second time. Faradism was also employed again, with compulsory exertion, and flicking the face and chest with wet towels. The stomach was also again washed out, and strong coffee introduced. An enema of brandy was administered, and as, after half an hour's compulsory exertion, it was found that his pulse was failing, he was given a subcutaneous injection of ether. The respiration sank now to two in the minute, and it was decided to try the effect of atropine. Accordingly, $\frac{1}{8}$ of a grain of the sulphate was administered subcutaneously, and repeated in half an hour. The respirations, a few minutes later, were found to be fully doubled on the average, although they continued irregular for some time. The pupils also were soon noticed to be less contracted. A few hours later the respirations reached ten per minute, and he gradually recovered, suffering, however, from headache, vomiting, and slight albuminuria. The pupils became considerably dilated. It seemed that the case had been favourably influenced by the atropia injections. It was certain, at all events, that a marked improvement almost immediately followed them. The patient was in danger more from apnoea than from cardiac failure, and was consequently in the state most likely to be benefited by the antagonistic action of atropia on the respiratory centre. It was worthy of note, in connection with this view, that artificial respiration was not required after the injections.—Dr. CHOLMELEY had met with one case where the use of atropine had had a remarkably beneficial effect in a severe case of opium-poisoning; the quantity of atropine used was so considerable that the patient, a woman, passed into a condition of atropine-poisoning, but though, when admitted, apparently moribund from opium-poisoning, she was out of danger in some three hours.—Dr. ISAMBAIRD OWEN thought that the antidotal action of atropine might be confined to its action on the respiratory centre.—Dr. C. H. F. ROUTH made some remarks, and Dr. FINLAY replied.

The Climate of Davos.—The SECRETARY (Dr. Kingston Fowler) read a paper by Dr. WATERS, giving an account of meteorological observations at Davos. The author believed that Davos was decidedly suitable for some, though by no means for all, cases of phthisis. It appeared from the paper that the weather does exercise an influence on the temperature of the body of a large number of the patients—the temperatures being taken three times a day; secondly, that the healthy individuals also showed this influence, though in a less marked manner than the invalids; and that the most diseased were most affected by meteorological conditions. It was also found that there were individual differences. It was also further gathered from the paper that the warmer the weather, the higher was the temperature of the patients. The paper concluded with remarks on the incompleteness of the observations, and the consequent necessity for caution in drawing inferences therefrom. Curves and charts illustrated the points of the essay.—Dr. FOWLER commented on the necessity for more elaborate sanitary precautions to provide for the wants of a rapidly increasing invalid population.

EPIDEMIOLOGICAL SOCIETY OF LONDON.

WEDNESDAY, MARCH 12TH, 1884.

NORMAN CHEVERS, M.D., C.I.E., President, in the Chair.

The Etiology and Pathology of Cholera.—Dr. GEORGE GULLIVER commenced his paper by observing that, though the mode of origin and propagation of cholera had been raised during the debate on Sir Guyer Hunter's Report, it was in reality a side issue, that paper being concerned only in showing that the disease was not imported into Egypt, the story of its having been so imported having no foundation in fact, and the disease being endemic in the country at a period long antecedent to the outbreak. At the request of the President of the Society, he had brought forward a few of the facts bearing on the etiology of cholera generally, without special reference to Egypt. He thought that few would deny that cholera is a specific disease; pro-

bably no great number would object to its being called a specific febrile disease. The ordinary symptoms and pathological appearances necessary to complete the definition were not dwelt upon, as being well known to all. The value of a knowledge of the anomalous and latent cases in investigating the etiology of specific diseases was then dwelt upon, and it was held to be great cause for regret that we knew so little of such cases of cholera, the reason for this being obvious. The consequence was that our knowledge of the etiology of cholera was in a very much more backward condition than of other specific diseases. Assuming that cholera was a specific disease, he had no hesitation in saying that it depended on a specific poison which entered the body from without. The principal views on the mode of propagation of the disease were then enumerated: (1) that it was contagious; (2) that it was never contagious, and that the poison was generated and propagated independently of man; (3) that of Pettenkofer, who considered that it was contained in the excreta, but required the soil to act as an intermediary host before re-entering the body of man; and (4) certain views less definite than the foregoing. Dr. Gulliver then insisted on the great importance of the predisposing and exciting causes of the disease, which were so important that one might almost be in danger of mistaking them for the specific poison itself. Probably, the rôle played by them was without parallel in the history of specific diseases in which the poison entered the body from without. A comparison was made with what takes place in gout where the exciting causes play such an important part in rousing into activity the poison latent in the blood, the difference being that in the case of cholera the poison was not in the blood but in the surroundings, only wanting a predisposing or exciting cause to enable it to enter the system. The question of a definite particulate contagium was then dwelt upon, in entering upon the discussion of the first view, which was that the poison was communicated from man to man. For the present, at any rate, Dr. Gulliver considered that we were not in a position to say that this had been demonstrated, but it was pointed out that an apparent apathy existed in the search after similar organisms in enteric fever, the reason being that the contagious nature of that disease was firmly established, apart from the demonstration of any organism. Unwholesome conditions had performed for us many experiments on human beings, and it remained to be asked whether they had not done so in the case of cholera. It was held that they had done so, and the well-known outbreaks in Broad Street and at Theydon Bois, and other facts in relation to its spread in Europe and in America, were adduced as evidence, it being pointed out that one positive fact was worth a great many negatives. The second hypothesis, which denied the propagation of the disease from man to man, was then passed in review; and Dr. Gulliver said that those who held it seemed to regard the poison as of a partly atmospheric, partly telluric, nature, some apparently claiming for it an affinity with the malarial poison. In his opinion no such relation existed, inasmuch as the one was the type of an epidemic, and the other of an endemic poison. The subject of an "epidemic constitution" was then discussed, and it was freely admitted that there were cosmic conditions which at times greatly modified the character of epidemics, but this was just as true of epidemics of diseases which were virulently contagious, such as small-pox, as of those diseases which are less contagious. It was further stated that no hypothesis except that of an epidemic constitution would account for the spread of cholera in 1870-73 through Europe. No doubt such conditions played a very important part in the spread of cholera, which being admitted, prevented the author from believing that the theory of its spread by contagion was inadequate. He thought that the difficulties in explaining outbreaks in the East arose largely from the simplicity of the manner in which the natives lived, the complexity of the problem which presented itself to the sanitary investigator, increasing in exact ratio with the primitiveness of the customs prevailing. For this reason our knowledge of the etiology of enteric fever would probably be still on an unsatisfactory basis if it rested on observations made in Eastern countries alone. In conclusion, the author briefly referred to the Egyptian outbreak, and expressed the opinion that, though it afforded no conclusive evidence on the question to be discussed, yet such evidence as it did afford by no means weakened the opinion that cholera was a contagious disease.—In the discussion which followed, Sir Guyer Hunter, Sir Joseph Fayrer, Sir William Smart, Drs. Murray, Lawson, Scriven, Balfour, Acland, and Dennis took part.

SOCIETY OF MEDICAL OFFICERS OF HEALTH.

FRIDAY, FEBRUARY 16TH, 1884.

T. O. DUDFIELD, M.D., President, in the Chair.

The Examination of Water for Sanitary Purposes.—A paper on this subject, by Mr. C. E. CASSAL and Dr. B. A. WHITELEGE, was read. The authors said that the object of the analyst

was to determine the presence or absence of impurities, especially organic, in water, their nature and quantity, and, on the evidence obtained, to form an opinion as to wholesomeness. Every sanitary analysis of water should include the estimation of nitrates and chlorides, and microscopic examination, the latter being far too frequently neglected. The two most important processes to consider as regards estimation of organic matter were the Frankland and the Wanklyn processes, generally looked on as rival ones. As regards the alleged antagonism between these two processes, the authors agreed with Dr. Fox that, when the circumstances were the same, analyses of the same waters made at the same time by the two processes yielded results on which precisely similar opinions were based by the different analysts. The Wanklyn process made the ammonia yielded by the water on boiling with alkaline permanganate of potash a measure of the amount of nitrogenous pollution; the Frankland process consisted essentially in burning the solid residue of a water by measuring the carbonic acid and nitrogen evolved, thus arriving at the quantities of carbon and nitrogen present. The authors dealt with some of the points in favour and against these processes, and pointed out that results from either one or the other could not alone justify the formation of an opinion. They also strongly insisted on the estimation of the nitrates in every case. Dealing with the criticisms to which water-analysis had recently been subjected, they called attention, first, to the report of the Local Government Board for 1881-82 (medical officer), and to Dr. Buchanan's words therein, namely, "That it was necessary to go beyond the laboratory for evidence of any drinking-water being free from dangerous organic pollutions;" secondly, to the assertion that, in cases where the amount of pollution of a water is known, analysis is found to give discrepant results with equal or proportionate amounts; and, thirdly, to the statement that the "cultivation" processes were destined to supersede present methods. As regards the first point, it might be admitted that chemical analysis failed to distinguish between dangerous and "innocent" pollution, if the latter ever occurred; but no merely chemical process could be expected to do this unless it distinguished principles characteristic of injurious and "innocent" pollution respectively. The chemist can only report on the quantity or proportion of organic impurity, be it noxious or not. But, if analysis could tell of specific pollution, it could tell that a water was polluted, and, in such cases, specific pollution may occur. Practically, no cases occurred where specific poison was present without much appreciable filth. The authors considered the conclusions generally drawn from the history of the Caterham case by no means valid, pointing out that it was unlikely that the small amount of poison introduced into the reservoirs of the company was uniformly distributed through them; and that, in the absence of analyses of the actual samples of water that produced disease, no conclusions could be drawn as regards the validity of water-analysis. They also denied that one grain of enteric fever-stool per gallon could be called a large befoulement, admitting, however, that it might be dangerous. As regards the discrepancy in analytical results, the authors called attention to Dr. Cory's report on his experiments with purposely polluted waters. Some of Dr. Cory's results were very fairly proportionate, others were very discrepant; and the authors, therefore, repeated some of his experiments. They pointed out the objections to Dr. Cory's use of enteric stools, healthy stools, and urine for polluting the waters; and indicated the inaccuracy of his plan of further polluting an already dirty water, and calculating results from the increments of ammonia obtained. They maintained that the ammonia obtained in the Wanklyn process was a measure of nitrogenous organic matter, and that it represented a constant proportion for varying quantities of the same pollutant. They then pointed out the necessity of a limit for ammonia, etc., yielded, and the way in which such limit was to be used, objecting further to Dr. Cory that a process capable of differentiating between two and three parts of polluting material could not do so sharply between, e.g., 52 and 53. The details of some of Dr. Cory's experiments were then examined and criticised. In repeating experiments with egg-albumen as a pollutant, the extreme difficulty of getting an uniform solution, even by repeated filtration, was noticed, as also the liability to error, probably impossible to prevent altogether, and requiring the greatest care in working, arising from coagulation of the albumen; and, further, the fact that, as decomposition of the albumen-solutions advanced, when equal amounts of them were used, the yield of albuminoid ammonia increased. The figures obtained by the authors are in no case far wide of those due to the actual proportions present—these proportions having been in every case unknown to the analyst. The discrepancies in Dr. Cory's albumen-results would be due to the fact that the quantities to be measured were often very abnormal, and determined an already impure water by increments (added to the latter's yield). The cultivation-processes were prematurely credited with the

capability of supplanting present methods. All waters contain organisms; the commonest being easiest to cultivate; and, no means of distinguishing between harmless specific organisms being known, the process at present merely forms an additional test for organic pollution. The authors then combated Dr. Cory's conclusions as regarded the combustion or Frankland process, stating that this process could not achieve more than the ammonia-process accompanied by subsidiary analytical evidence could achieve. They considered that nothing in the way of evidence or criticism had been brought forward to detract from the great sanitary value of water-analysis; and that, as regarded absolute safety, that was a thing unknown and unknowable.—In the discussion which followed, the President, Drs. Thurstfield, Stevenson, Tripe, and Edmunds, Mr. Wynter Blyth, and Mr. Shirley Murphy, took part.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

WEDNESDAY, FEBRUARY 27TH.

H. D. LITTLEJOHN, M.D., President, in the Chair.

Catheter-Fever.—Sir ANDREW CLARK, Bart., delivered an address on this subject. The speaker, after eloquently reminding the Society of his former connection with the Edinburgh Medical School, ably expounded his ideas on the subject of catheter-fever, which were materially the same as those contained in his address published in a former number of this JOURNAL.—Dr. PATRICK HERON WATSON, after thanking Sir Andrew Clark, said that it seemed to him that catheter-fever, as described by Sir Andrew, had always one antecedent, and that was the introduction of an instrument, possibly only once; on the other hand, however, the patient might have passed through some weeks of catheter-life, before any unfortunate symptoms manifested themselves. Of this fever, the initial symptom was undoubtedly a rigor, which occurred usually upon the first micturition after the catheter had been withdrawn. Then followed a varying degree of febrile disturbance and diaphoresis, and, under favourable circumstances, a more or less complete return to the condition before the occurrence of the rigor. In his experience, this fever did not occur in the female. As regards character, the febrile condition was usually slight and ephemeral; but it might be typhoid in nature, with the temperature sometimes as high as 105° Fahr. Students had been frequently warned never to pass a catheter on a patient in the erect posture, when the passage of the instrument through the prostatic canal often gave rise to a storm in the patient's nervous system, inhibited his heart's action, and produced unconsciousness, often attended by unilateral epileptiform convulsions. In such cases, the patient usually recovered when he reached the horizontal level. These symptoms might be followed by fever or not. Such a condition pointed to the element in the disturbance of the nervous system, which Sir Andrew had indicated, working in such a manner as to disturb the circulation, respiration, and nervous consciousness. Sometimes, for twenty-four hours after the passing of a catheter, there was no disturbance of any kind; then slight rigors, followed by steadily advancing diminution of urine. After the first rigors, death might be very rapid, and that with marked decrease of urine, the urine being perfectly normal, except that it contained a little blood, which, *post mortem* examination showed, probably came from the Malpighian corpuscles. Dr. Watson recited several clinical examples of these conditions: among others, that of a nobleman who died on the day following the passage of a catheter. In this case, the *post mortem* examination revealed a fatty heart and calcareous aorta; and the fatal issue was probably due to the fact, that the entrance upon catheter-life had caused a degree of nervous irritation, in a man with a heart and aorta in a condition incapable of standing a reflex cardiac inhibition. Another symptom, which he had frequently observed in connection with this urethral fever, was a herpetic eruption on the face and lips. Dr. Watson was inclined to deny that the passing of a catheter was essential for the development of any portion of them. Other conditions also give rise to it; e.g., the passage of a calculus along the urethra; the passing, in large quantities, of uric acid, oxalates, a mixture of urine with blood-clot, or of air or gas in the urine (and that without any decomposition of the fluid). Fever-symptoms, ague-like in character, often occurred in cases of advanced stricture. Gonorrhoeal rheumatism also was not far dissociated from the subject of consideration; it specially occurred in those in whom the prostatic urethra was involved. Urethral fever was not due to rupture or injury of the urethral canal, nor to absorption of urea, or even to a quasi-pyæmia. Rupture of the bladder was seldom accompanied by such symptoms; nor were these results altogether avoided when an antiseptic catheter had been employed, where, in fatal cases, the condition of so-called interstitial nephritis had resulted. He was inclined to attribute the

first step in this condition of urethral fever to the irritation of the prostate acting upon the spinal centre, and producing reflected changes which had most injurious effects upon the Malpighian glomeruli and capsules, and the capillary plexus passing from these towards the renal veins, in which, after death, there were found minute clots, and not only cells of the leucocyte-type occupying the spaces between the tubules, but blood extravasated in this region, and corresponding to the presence of blood in the urine, with marked diminution of the urinary excretion. He preferred to call this fever, not "catheter," but "urethral" fever. It essentially depended upon something which irritated the prostatic urethra. When its effects were trivial and evanescent, he believed it was because it found no diseased internal organ on which to tell. When a fatal or serious result ensued, the patient had been suffering from some serious lesion of some important internal viscus, and that the dangers were chiefly found in connection with a feeble heart, diseased arteries, and kidney-disease, either original or secondary. When this urethral fever was set up as the result, the turning-point in that result was the influence it exercised upon the glomeruli and venous capillary system of the kidney. The prognosis depended on the degree of rise of temperature, and on careful daily examination of the patient's urine. As regarded treatment, Dr. Watson advised that soft instruments should be preferred to hard metallic ones. For some days before the introduction of the catheter, quinine, with bromide of potassium or opium, should be administered, and continued for some time afterwards; and this followed by iron and digitalis during the whole treatment (if fever were established). The vapour- and lamp-baths in suitable cases were also useful auxiliaries.—Dr. GRAINGER STEWART stated that, in connection with this subject, he had certainly seen three conditions. 1. Acute rigor followed the introduction of an instrument into the bladder. This attack could not be conceived to be due to anything except reflex nervous influences. 2. In some cases, after such interference, and often from long standing disease of the urinary tract, extensive sloughing, or, at all events, a nasty form of cystitis, occurred, accompanied by the other features of surgical kidney. 3. This class occupied a position intermediate between the other two, when the kidneys were normal, although the bladder was enlarged and inflamed, and the ureters were dilated and congested. He believed also that the causes leading to this febrile condition were various. While, in some cases, it was due to reflex action, and in others to direct inflammatory extension upwards along the urinary tract, yet, in most cases, septicæmia was the cause. He had never seen anything he would call uremia. Another very important element in such cases was where long habitual overdistension had been removed, and the parts fell together, there must be great congestion and tendency to inflammation, and parts of mucous membrane normally apart coming into contact with each other would afford a ready nidus for the development of such bacteria, etc., as might have been introduced. He heartily advocated rest, quiet, and low diet for those beginning catheter-life. Opium and bromide of potassium were often useful.—Mr. CHIENE did not deny that there might be other causes of death, as from irritation of the urethra, etc., but he believed that the great majority of those cases were septicæmic; and he was very pleased indeed to find, in the report of Sir Andrew Clark's London address, what was to him a comforting statement, *i.e.*, "The urine of catheter-fever is always loaded with micro-organisms of various kinds." The septic matter might enter the bladder in four different ways: 1, by an impure instrument; 2, occasionally and rarely by air rushing in along with the catheter which had been introduced; 3, a condition of septic urine, by passing along an inflamed urethra, irritated by the prolonged use of instruments; 4, it might pass between the membrane and the catheter, when the latter had been tied in. He believed it was infinitely safer and better thoroughly to purify the catheter. For this purpose he had used a carbolic solution (1 to 20) until the present year, when, after Koch's experiments, he preferred corrosive sublimate. He had not yet found an antiseptic solution which would irritate the urethra so slightly that they could use an instrument for a long period. He disagreed with Sir Andrew Clark's statement that this subject had not received adequate expression, either in surgical works or in teaching. This point would be found shortly stated in the small University textbook for the first part of the course of surgery, and he himself had spoken of it as a form of septicæmia, ever since he commenced lecturing on surgery in the School of Medicine.—Mr. ANNANDALE wished to direct attention to the four causes which Sir Andrew had mentioned as probably producing this condition; and, as a practical surgeon, to give a few hints which might tend to prevent its occurrence. The alleged causes were: 1, local irritation from passing of an instrument; 2, local irritation acting on the general system; 3, septic matter introduced; 4, sudden emptying (in some cases) of the bladder and

ureters by the drawing off of urine. A catheter should never be passed on a patient without first inquiring somewhat into his condition and history. The instrument should be properly warmed. The second cause depended mainly on how they treated the first. To obviate the third one, antiseptics had to be employed. He was inclined (with reference to the fourth cause) in some cases of retention from altered prostatic or urethral conditions, to aspirate rather than catheterise the patient when he came for the first time.—Dr. WYLLIE had given much attention to the subject since 1868. He would merely say a word or two as regarded the introduction of germs into the bladder. The common method by which this was brought about was their introduction in the oil which anointed the catheter. A second possible cause under certain circumstances was the suction of air into the bladder during the catheterisation—as by withdrawing the pressure of the hand over the pubes before the catheter had been removed. He also detailed a case where this occurred from the patient coughing at the close of the operation.—Mr. JOSEPH BELL said that, excluding septic conditions and all causes of supposed kidney-disease, there were still some cases, which had peculiar symptoms after the introduction of a catheter, which could not be explained very well by any of these things mentioned. Ever since Mr. Banks's paper on the subject, he had been extremely cautious about introducing catheters into unknown urethra, and he had even had a regular drill for all cases in which it was to be employed. In patients treated in the wards, antipyretic doses of quinine were given early in the morning, and the patient put to bed after the operation. In casual out-patients, if swollen eyelids or other suspicious symptoms were present, their catheterisation was postponed until a bed had been got. Bad results had been much fewer since he had adopted these precautions. As Sir Andrew had indicated, there was a personal equation in these cases. Some persons could stand any amount of catheterisation without bad effects, while others would have very rapid effects, ending in pyrexia and even in death. A patient ought to be prepared for catheterisation, and treated as if about to undergo a serious operation.—Dr. BYROM BRAMWELL said that, in many cases, before the catheter had been introduced at all, he believed the kidney was in an unhealthy condition. He thought that the fact of one-third of Mr. Marcus Beck's cases presenting well marked structural lesion in the kidney, went conclusively to show that, in many cases of prostatic enlargement with distension of bladder, and probably of ureters also, there must have been some structural change in the kidney before the passage of the instrument, although in the earlier stages it might not perhaps be present. He agreed with Sir Andrew Clark and Dr. Stewart, that the sudden withdrawal of a large quantity of urine sometimes produced a distinct alteration in the structure of the kidney (*e.g.*, acute hæmaturia). The fact that the urine was often copious, too pale, and of low specific gravity, showed that at least the kidney was incompetent. That organisms were present in all these cases seemed to him a point of great importance. Sometimes, as Sir Andrew admitted, the urea excreted in the urine was diminished in amount, and then he took it that there must be some alteration in the local condition, or in the kidney, to account for it. In some cases death was probably due to uremia, and where a weak or diseased kidney existed, he believed that a slight amount of septic absorption in that person, would be much more serious than it would in a patient whose kidneys were perfectly healthy.—Dr. JOHN DUNCAN said that the two questions which Sir Andrew Clark had introduced were—1, Why does a patient die after the introduction of a catheter? 2, What can we do to prevent it? With regard to the first question, he brought all the cases into two categories, one neurotic, the other inflammatory. The extremely important nervous connection of the urinary organs, which was shown even in the well known shiver which often accompanied the last drops of urine, was illustrated in slight cases by instances of evanescent rigors, beginning almost immediately after the passage of the catheter, and, in severe cases, ending fatally. The neurotic cause might act in various ways. He doubted the cause from removal of undue pressure. All the other cases he was strongly inclined to class under the head of inflammatory. The causes of these were generally septicæmic, although, in some instances, an inflammatory affection independent of septicæmia might exist, and even kill the patient, *e.g.*, prostatitis in an old man, with perhaps cystitis, perhaps pyelitis, and then uræmia, although this last was rare. He attached more importance to the impurity of the catheter, and the entrance of air with it, than to the other two causes to which Mr. Chiene had referred. The introduction of air was very important, especially when cystitis existed. He thought that there was another class of septic cases to be considered, *viz.*, those to which Sir Andrew had referred in a word as coming from within. Local inflammation did a good deal to determine the locality in which wandering germs in the body might breed. It was most likely

that these and other causes of septic inflammation might occur, and then lead to death in various ways by inflammatory fever, by passing upwards to the kidneys, or by septicæmia and pyæmia. Possibly other forms of toxæmia of a rheumatoid character existed, as in gonorrhœa. Independently of the general points of treatment already stated, the two points to be attended to locally were the most absolute gentleness in the use of instruments, and the greatest possible care being taken to prevent the entrance of organisms through the catheter.—Sir ANDREW CLARK said that the only point in the address of Dr. Heron Watson which he wished to notice, was Dr. Watson's quarrel with the name catheter-fever. He (Sir Andrew) gave it up entirely. It was only used provisionally and temporarily, and he himself should be very sorry if the name remained in connection with the subject. In replying to Dr. Bramwell's contention that in many cases of catheter-fever, there was previously existing renal disease, and that specially in cases of enlarged prostate and distended bladder, they could know that the kidneys were diseased by a study of the urine, he perfectly agreed with that gentleman. He must add, however, that such cases did not always occur in middle age. In a case recited by him of a fatal issue in the case of a young person, the only thing found after death was the so-called purulent urethritis and the commencement of a slight catarrhal, or, if they pleased, catarrhal and interstitial, nephritis. In many of these cases, the urine assumed the ordinary characters of a febrile urine. Referring to Dr. Bramwell's point in regard to the presence of urea in these cases, he did not mean to suggest that in septicæmic cases there would not be an increase of urea. In many instances, the urea was normal in amount, although the density of the fluid was low, but certainly in other cases there was a deficiency of urea; and in these cases—and he had called attention to this over twenty years ago—any surgical operation was liable to be followed by dangerous consequences. He had called this condition renal inadequacy, and had always endeavoured to teach his students at the London Hospital never to attempt any surgical operation without first examining as to this. Sir James Paget had stated a similar warning. In these cases of renal inadequacy, he had only been able to determine functional inadequacy in consequence of the state of the urine, and no structural change. In acknowledgment, he had more to say than he could express. The first acknowledgment was one of sincere gratitude to the Society for the kindly, he might even say friendly, way in which they had been pleased to receive his imperfect communication. The next he had to make was, to thank the members of that important Society for the great advance which had that night been made in the discussion of the subject. Even to himself, who had thought of it not a little, it had assumed surer proportions, and a clearer position in its relations to other kindred affections, than he himself knew at the beginning; and he felt assured that that discussion would exert a happy and favourable effect upon the mass of the profession throughout the country, and they would no longer be open to the reproach that the body of the profession was ignorant of that which the leaders of it had so well known.—The PRESIDENT, in closing the meeting, expressed the hope that the distinguished Scotchman who had gone to the South would return more frequently than he had hitherto done, and give them the benefit of his experience.

ACADEMY OF MEDICINE IN IRELAND: PATHOLOGICAL SECTION.

FRIDAY, FEBRUARY 29TH, 1884.

A. H. CORLEY, M.D., President, in the Chair.

Stricture of Rectum.—Mr. MCARDLE exhibited a specimen of malignant stricture of the rectum taken from a patient who died of peritonitis. The stricture was situated at the junction of the middle and lower third of the bowel, and extended downwards to within two inches of the verge of the anus. The intestine below the seat of disease was smooth and dilated. The stricture with difficulty admitted a No. 9 gum-elastic catheter. The upper opening of the strictured part was surrounded by a large ulcer, which perforated the anterior wall of the bowel, which at this point was attached by bands of adhesion to the recto-vesical fold of the peritoneum.

Connection of Acute Diabetes with Pancreatic Disease.—Dr. DUFFEY reported a case of diabetes occurring in a farm-labourer, aged 24, the duration of which, from the first recognition of the characteristic symptoms of the disease until his death, was two months. There was a sudden onset of abdominal pain, vomiting, and diarrhœa, a week before death, the mode of which was, as is usual in acute cases, by coma. Nothing remarkable was observed by the naked eye at the *post mortem* examination, with the exception of the condition of the pancreas. This gland was hypertrophied, and felt extremely indurated, the hard-

ness being due (as the microscopic sections made by Mr. Abraham showed) to carcinomatous infiltration. It was well known and remarkable that a diseased state of the pancreas should so frequently be found associated with diabetes. In these cases, the pancreas seemed to be more frequently atrophied than hypertrophied. In acute forms of the disease, there might not be sufficient time before the death for the atrophic change, which generally followed the primary inflammatory enlargement of the gland, to occur. There were, on the other hand, numerous records to show that the pancreas might be diseased, apart from any glycosuria. At the same time, there was evidence to show a connection between certain morbid lesions of the pancreas and diabetes constituting a particular type of that disease. He discussed the views of Klebs and Lauder Brunton in explanation of the assumed co-existence, and referred briefly to the subject of carcinoma of the pancreas.

Intestinal Concretions.—Dr. CHARLES B. BALL showed a number of calculi from the intestine of a horse. Two of these were examples of the oat-hair concretion. One was rough and tuberculated on the surface, and, on section, was found to be composed of concentric layers of vegetable hairs, with a considerable quantity of phosphatic matter and portions of the husk of the oat. The second was smooth and uniform both on the surface and in section, and was composed almost entirely of oat-hairs, closely felted together. Microscopic slides of both these calculi, with the hair of the oat-seed for comparison, were exhibited. The author also brought forward a collection of fourteen concretions, composed chiefly of ammonio-magnesian phosphate, the nuclei of which were very various, including iron-nails, small stones, vegetable matter, a piece of leather, etc. Some were much faceted, and illustrated distinctly the two ways in which this appearance was produced. One of the concretions was concavo-convex; and sections showed that this was evidently a portion of a calculus which had undergone spontaneous fracture within the animal. Additional evidence of this was to be found in the extreme brittleness of its structure. Microscopic sections of these calculi were exhibited, which showed well the tubular structures found abundantly in intestinal as well as in urinary concretions, and supposed to be *conferva*. Dr. Ball showed several beautiful examples of argagropoles and bezoars from the museums of Trinity College and the Royal College of Surgeons.—Mr. VALENTINE BALL said he had, at his brother's request, brought a specimen, the first impression having been that it might be a fossil calculus, belonging to some extinct animal. But he had obtained information which showed that, within the last year, a number of rounded masses of tribasic phosphate had been introduced into the country for the manufacture of manure. Six months ago, he received two specimens, and he had the sections made which were now exhibited. He noted the absence of concentric rings, but it occurred to him that they might have been obliterated by crystallisation. Having exhibited the specimens to the Geological Society, he learned that masses of the kind were abundant in Podolia and Bessarabia, but unaccompanied by any fossil mammalian bones. They resembled specimens of calculi in the Museum of the College of Surgeons; and he had heard that, twelve or thirteen years ago, similar specimens were described in the *Transactions* of the Geological Society of Vienna as coming from Silurian rocks, thus excluding the hypothesis of any connection with mammalia. They might have been derived from original spherical masses of carbonate of lime, and some of the analyses made showed that they contained carbonate of lime. They might represent sponges or spherical masses of coral, in which the carbonate of lime had been replaced by tribasic phosphates. Or they might be spherical conglomerations of iron pyrites. When obtained from the mother-rock, the specimens were rough; but, when they had been rolled in rivers, they presented a polished surface.—Dr. MOORE had, some years ago, got possession of the remains of a crocodile 11 feet long; and, on examination, he found pendant from the exterior of the intestines, and encapsuled, stones and other refuse which the reptile had evidently swallowed, and which, instead of having been passed out in the ordinary way, had been extruded through the lining membrane of the bowel, lying between it and the peritoneum, apparently without having caused any irritation.—Dr. FRASER said that several years ago he attended an old woman, a cook, for a tumour in the cæcum, the size of his two hands. From time to time she passed what she thought were the joints of a tapeworm, but on examination they were found to be portions of the ligamentum nuchæ of a calf, which had collected, forming the tumour, and which was got rid of by doses of castor-oil. Some years afterwards he attended another female for a tumour in the same region, and purgatives produced a ball about the size of a wall-nut. She had been in the habit of eating her own hair, and the ball was proved to consist of rolled-up hair. Afterwards he attended a young man employed in a shop, who had been in the habit, every time he tied a parcel, of eating a bit of twine. He got the tumour

away, and it proved to consist of pieces of twine. Concretions found in cattle were not uncommon in Ireland. These concretions were the result of the animal licking its own skin. Calves did so more than cows, and unless they vomited the concretions so formed, they grew emaciated and died. In the old Natural History Society, Archbishop Whateley read a paper on the licking of bones by cows, in which he stated that perfectly healthy cows did not lick bones. In the old days when Scotchmen ate a great deal of strabout, the formation of intestinal concretions made of the oatmeal-husk was much more common than now. He had seen numerous specimens of these concretions in the old Hunterian Museum. The nearest approach he had ever seen in Ireland to the bezoar stone of the East, was taken from the body of a lunatic who had been employed in French polishing, and imbibed habitually methylated spirit, containing shellac, which formed a concretion in the stomach. Straw was sometimes swallowed by lunatics and formed concretions. As to the curious phosphatic masses mentioned by Dr. Ball, he had himself obtained specimens from a large manure manufacture, and came to the conclusion that they were metamorphosed tissue of some kind or other, but the curious thing about them was that they were phosphatic.—Dr. BENNETT observed that the essential difference between the concretions which were of animal origin, and those which arose under geological influences, and which they had good authority for believing to be the result of chemical or mineralogical action was, that the latter did not present the concentric arrangements that were observable in the former.—Dr. BENSON mentioned that he saw in the Whitworth Hospital a ball of the size of a pill-box, which was taken from the stomach of a girl 10 years old, and which proved to consist of the pickings of blankets she had eaten.

Fatal Meningitis after Enucleation of the Eye.—Mr. ARTHUR BENSON showed the brain, and read the notes of a case of meningitis which had occurred in a girl aged 17, after enucleation of a shrunken eyeball, the result of second purulent inflammation excited in an old blind staphylococcal eye. Headache, vomiting, etc., began on the day after the operation. On the fourth day a bright erysipelas-like blush occurred on the eyelids, nose, and both cheeks; it disappeared in thirty-six hours. There was no discharge from the socket of the eye which had been removed; but on the day that the red blush was first seen, there was slight secondary hæmorrhage from the wound. Death by coma occurred on the eighth day, and the necropsy confirmed the diagnosis of purulent meningitis, the whole surface of the pia mater being covered with lymph and pus. A summary of nine other cases, of all of which the author could find records, was given; and, of the nine, only two were known to have occurred after enucleation of the eyeballs in a state of purulent panophthalmitis, while four were known not to have been purulent, and in three the condition of the eye was not stated.—Mr. P. S. ABRAHAM had made sections of the optic nerve and surrounding tissue, taken at a distance of about one cubic centimetre from the foramen. The nerve was profoundly altered. The space which the interfibrillar prolongations enclosed was occupied by a continuous granular material freely interspersed with nuclei. The connective tissue of the framework was itself filled with proliferating cells, massed together in some places. The sheath of the nerve was thickened, and towards the outer boundary the fibrous bundles were separated by spaces containing numerous small cells and nuclei; and, further out still, these were sufficiently abundant to be looked upon as purulent deposits. Some of the sections were treated with methyl-blue, and by this means he thought he could detect microorganisms among the pus-cells and between the fibrous bundles. A complete examination, however, had not yet been made. Sections of the kidney showed marked hyperemia, the vessels being largely distended with blood, and bulged in places. No extravasations had been seen. The urinary tubules were quite occluded by the swollen and granular epithelial cells, which had lost their marginal boundary or contour. The ureters were studded with collections of inflammatory cell-growth.—The PRESIDENT asked if erysipelas was present in the hospital at the time.—Dr. BENNETT said the evidence of erysipelas in the case depended largely on the appearance of the patient's face, which prevailed one evening and disappeared next morning, without any desquamation following. His opinion was, that the affection was an erythema, but not an erysipelas erythema. The rash on the face was due, probably, to nerve-disturbance. He did not think there was any other case of erysipelas in the hospital.—Dr. STONY agreed that there was no erysipelas in the case; nor had he thought so at first, until the enormous red swelling and blood appeared after the occurrence of the blood-clot. After the rash disappeared, he came to the conclusion that the case was one of meningitis without erysipelas. Dr. Stoney of Abbeyleix had stated that there were some cases of scarlatina in a building next door to where this case had been, and that made them think that this case was the commencement of scarlatina; but that hypo-

thesis was disproved. When the girl came to the hospital, she was dull and stupid; and she put her hand to her head, complaining of headache; so it was possible there might have been cerebral affection independent of the operation altogether. The case was remarkable, there being but few instances of affection of the brain occurring after enucleation of the eye. The operation was a perfectly safe one; he never saw but one case of trouble after it.—Dr. J. W. MOORE saw the patient two days after the operation, and he was struck by the strange combination of symptoms presented—an exceedingly high temperature and a pulse of only 80. He at once suggested meningeal trouble—a view borne out by repeated vomiting and violent cardiac action. The throat was œdematous; it was livid, rather than red, and was covered with an exudation, and very like the throat of a patient suffering from malignant scarlet fever. The tongue also resembled that of a scarlatina-patient; but, on the other hand, the slowness of the pulse was a symptom opposed to the supposition of scarlatina. Again, the respiration was such as to prove there was nothing whatever wrong with the lungs. On all these symptoms, he expressed the opinion that the case was probably one of meningitis, and that at any moment the rate of the pulse might double. Subsequently, the pulse rose from 155 to 160. He could understand the probability of the girl, from the time she came to town, suffering from the initial stage of meningitis. There was a good deal of erysipelas in Dublin at present; but, when he saw the girl, she had no trace of erysipelas swelling, and he thought the meningeal symptoms sufficient to exclude the idea of erysipelas.

Diabetes Mellitus: Microscopic Sections.—Mr. P. S. ABRAHAM gave a demonstration of sections from two cases of diabetes mellitus.

REVIEWS AND NOTICES.

RUPTURE OF THE URINARY BLADDER. By WALTER RIVINGTON, B.A., M.B., M.S. (Univ. of London), Surgeon to the London Hospital. Pp. 156. London: J. and A. Churchill. 1884.

This is a publication, in a revised and extended form, of papers on "Rupture of the Urinary Bladder," read by Mr. RIVINGTON before the Hunterian Society, and subsequently contributed, with some additions, to contemporary medical journals. The work in its latest form presents us with a complete treatise on this lesion, and shows great industry on the part of the author, and a careful and profitable study of the many points relating to its clinical features and treatment. The subject is introduced by reports of four cases, treated by Mr. Rivington; and the statements and views embodied in the work are based on an analysis of a total number of three hundred and twenty-two cases. The value of a long series of carefully observed and reported instances is, probably, more evident in the study of rupture of the bladder than in most other injuries and surgical affections; for, though any practitioner might be called on to deal with such a case, the accident, as Mr. Rivington points out, is rare; and it is only by the recorded experience of many observers, combined into one view, that the want of personal experience in diagnosis and treatment can be remedied.

The author insists much on the fact, that distension of the bladder is a necessary condition of the occurrence of an uncomplicated intra-peritoneal rupture. It is very probable, he holds, that rupture of the viscus, when empty, can only occur in connection with fracture of the pelvic bones, or when such violence is applied to the hypogastric region as to strip off the peritoneum from the anterior abdominal wall. In idiopathic cases, rupture is often preceded by some morbid change in the bladder itself. Rupture from over-distension, it is stated, has certainly occurred in the living body; but, in these cases, either the viscus was not in its normal condition, or the over-distension had been assisted by the action of the abdominal muscles. Ability to micturate has been frequently observed in cases both of intra-peritoneal and extra-peritoneal rupture. In the former lesion, the rent may be closed soon after the accident; and in the latter, the opening may either be small, and become closed for a time, or, being placed in the anterior wall, may allow the urine to return through the bladder into the urethra.

The most important point in connection with rupture of the bladder is its diagnosis. Can we always distinguish rupture of the bladder from some other variety of severe abdominal injury? And, again, in any case of undoubted rupture, can it always be determined whether the lesion be extra-peritoneal or intra-peritoneal? Mr. Rivington, recognising the fact that occasionally some, and, in exceptional cases